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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.

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

THURSDAY, NOVEMBER 6, 1890.

PRIESTLEY, CAVENDISH, AND LAVOISIER.

THE *Revue Scientifique* of the 25th ult. contains a translation of the address which I had the honour of delivering to the members of the Chemical Section of the British Association at the recent meeting in Leeds, to which, on the invitation of the editor, M. Charles Richet, M. Berthelot prefixes a letter, of which the following is a translation:—

"I have no direct concern in the republication of Mr. Thorpe's address which you purpose making in the *Revue*. Personally, I have not any reason to complain of his courtesy, and I should have been silent so far as he is concerned, holding that one is not bound to enter into a controversy which is purely critical, where no new fact is alleged, and where the judgment of public opinion suffices to set things in their true place; however, I comply with your request to let your readers know what my opinion is.

"To my mind, nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. All civilized nations are at one in proclaiming the glory of Newton, the greatest of astromers, and yet the majority of English men of science, refusing to treat his rivals with equity, are not agreed to recognize Leibnitz's rights to the invention of the differential calculus: they are as prejudiced in this respect as was Newton himself. Something analogous occurs in regard to the discoveries which created modern chemistry a hundred years ago.

"Unquestionably, Priestley and Cavendish are recognized by all as great discoverers. I have myself taken pains to describe Priestley's discovery of the principal gases in terms of admiration ('La Révolution Chimique,' p. 39), and especially that of oxygen, which I unreservedly attribute to him (pp. 61–62). I have also detailed, with the encomiums which they merit, the investigations of Cavendish, 'one of the most powerful scientific minds of the last century,' and particularly his fruitful research on (to use Blagden's phrase) the artificial generation of water. But the well-merited praise accorded to these English *savants* does not prevent some of their countrymen from persistently denying the right of Lavoisier to the discovery and co-ordination of those general ideas on which rest our actual conception of matter, more especially in relation to the composition of air and water. This, I venture to repeat, is an incident in the long-standing feud, continually being renewed in the history of science, between the sagacious discoverers of particular facts and the men of genius who frame general theories. The opinion of most Continental men of science seems, however, to be decided on this special point, as may be seen from the judgment given, not only by Dumas, but by Hoefer, in his 'History of Chemistry,' by H. Kopp, in his careful account of the discovery of the composition of water, and by many others. I have merely concurred with them.

"It was in this spirit that I had sought to trace the history of the discoveries which constituted the doctrine of modern chemistry, by faithfully reproducing all its phases, whilst at the same time indicating the continuity of sequence in the facts and the paternity of the ideas. I did this with an impartiality which has brought upon me the reproach that I have been indifferent to the reputation of my countryman—the very opposite to the accusations which are now directed against me.

"As regards the composition of air, it is easy to separate facts from ideas. It is certain that the discovery of oxygen is due to Priestley. But, said Lavoisier: 'If I am reproached for having borrowed my proofs from the works of this celebrated philosopher, at least none will contest my right to the conclusions, which are often diametrically opposed to his.'

"Priestley, obstinately adhering to the theory of phlogiston, regarded his new gas as consisting of the very substance of air deprived simply of its phlogiston; whilst nitrogen, according to him, was formed also of this same substance combined with a complementary portion of phlogiston. He remained faithful to this doctrine, which obscured the true nature of the greater number of chemical phenomena, until the moment when, like Lavoisier, persecuted by his countrymen, who now proclaim his fame, driven from home, his laboratory burnt by a mob, and threatened with death, he fled to America, where he died in sadness and in solitude. Even more unfortunate was Lavoisier!

"But whatever may have been the personal fate of these two great men, if it is true that Priestley discovered oxygen, it is not the less certain that the true theory of the nature of air is due to Lavoisier.

"The history of the composition of water is more complicated. In reality, the discovery of the facts belongs neither wholly to Cavendish—who undoubtedly played a most important part, inasmuch as he gave the impetus towards the definitive solution—nor to Lavoisier, who first established a knowledge of the facts by his public experiments and his published writings—nor even to the two combined. They had predecessors, and at the moment even when the light came, Monge played an essential part in the rigorous demonstration of which Mr. Thorpe apparently has no suspicion. Thus each man's share in this history cannot be settled by a word: we require to follow exactly the gradual progress of experiment and publication. But here, again, if Lavoisier is not the principal discoverer of the facts, it is he who has the incontestable merit of having furnished the exact interpretation of the phenomena, freed from the mists of phlogiston, to which Cavendish seems to have remained faithful to the day of his death.

"I have elsewhere laid bare all these facts, and I have no intention of reproducing here the details of a controversy exhausted even in Lavoisier's time, and in which Mr. Thorpe does no more than reproduce the unjustifiable imputations of Blagden, who, impelled by passion, went so far as to interpolate and falsify, with his own hand, the manuscript memoirs of Cavendish, in order to gain arguments in support of his accusations.

"Moreover, nothing more decisively establishes the part played by Lavoisier, and his right to the institution of our modern theories, than the letter of a contemporary English *savant*, Black, as celebrated for his discoveries in physics as in chemistry, and who might have put forward claims on his own account. In 1791 he wrote to Lavoisier, in a letter equally honourable to both :—'The numerous experiments which you have made on a large scale, and which you have so well devised, have been pursued with so much care and with such scrupulous attention to details that nothing can be more satisfactory than the proofs you have obtained. The system which you have based on the facts is so intimately connected with them, is so simple and so intelligible, that it must become more and more generally approved and adopted by a great number of chemists who have long been accustomed to the old system. . . . Having for thirty years believed and taught the doctrine of phlogiston as it was understood before the discovery of your system, I, for a long time, felt inimical to the new system, which represented as absurd that which I had hitherto regarded as sound doctrine; but this enmity, which sprang only from the force of habit, has gradually diminished, subdued by the clearness of your proofs and the soundness of your plan.'

"We can but hope to see the day when the scientific men of England will conform to the opinion of one of the most illustrious of their countrymen.

"M. Berthelot,
"of the Institute."

I quite agree with M. Berthelot that nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. It was for that reason that I felt compelled, in the Leeds address, to protest against the spirit and bias of the accounts of the discovery of the facts relating to oxygen and the composition of water given in "La Révolution Chimique." Although M. Berthelot's letter somewhat confuses the issues, there is, in reality, but small difference between us. What I ventured to criticize was the general tone and tendency of M. Berthelot's argument, which seems to palliate, and even to justify, Lavoisier's pretensions to a discovery in which he has no right even to be considered as a participator. M. Berthelot now tells us, in his letter that he attributes the discovery of oxygen unreservedly to Priestley. So far so good. It is something gained to have thus secured such an unqualified statement from one who occupies the position of authority in the world of chemistry in France that is enjoyed by the present Perpetual Secretary of the Academy. We may well hope, therefore, that this particular question has been finally set at rest.

M. Berthelot need not ask British men of science to conform to the opinion of Black. They already do so. That to Lavoisier, and to Lavoisier alone, belongs the merit of having effected the overthrow of the theory of phlogiston, and of having to that extent laid the foundation of modern chemistry, is not questioned on this side of the Channel. So far as I know, it has only been among Lavoisier's own countrymen that any doubt on this point has been raised. We all remember the passionate scorn with which Lavoisier repudiated and protested against the attempts of his compatriots to rob him of his rights: "Cette théorie n'est donc pas comme je l'entends dire—la théorie des chimistes français; elle est la *mienne*, et c'est une propriété que je réclaime auprès de mes contemporains et de la postérité." It is true, as M. Berthelot implies, that Black has claims. Lavoisier himself admits as much. It would be easy, if it were not beside the points at issue, to match the letter which M. Berthelot quotes, by others from Lavoisier in which he ascribes to Black the germs of his doctrine. M. Berthelot, I repeat, confuses the issues. This particular point was never raised by me in the address. What I said was:—"Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen, and the determination of the compound nature of water. M. Berthelot's contention is, that not only did Lavoisier effect the overthrow, but he also discovered the facts." I, in common, I venture to assert, with every British chemist, admit unreservedly that Lavoisier effected the overthrow, but we deny that he discovered the facts. It is altogether beside the question for M. Berthelot now to say in effect:—"Have I not praised your men of science, and thereby drawn down upon myself the wrath of my countrymen? And yet you are not satisfied!' We are sorry for M. Berthelot: he is in the position of the man with many friends, and his friends for the moment are a little angry. He has either not the courage of his convictions, or he has halted between two opinions—with the usual consequences.

With respect to the discovery of the compound nature of water, M. Berthelot now takes up a different position from that which he occupies in "La Révolution Chimique." His contention there was that by every legitimate canon the experiment of June 24, 1783, gives to Lavoisier the priority of discovery. He now admits that Cavendish played "un rôle capital—car il donna le branle aux esprits vers la solution définitive." But how was this possible when Cavendish's memoir was not published until January 1784? There is really only one answer—it was given simply by the intervention of Blagden. I repeat that Blagden told Lavoisier of Cavendish's researches and of his conclusions, and that it was in the light of that knowledge that the experiment of June 24, 1783, was made. There can be no question of this. Blagden's testimony, as given in the letter to Crell, is as direct and decisive as it is damning. It was never contradicted by Lavoisier, nor by Laplace, Vandermonde, Fourcroy, Meusnier, or Legendre, who were present on the occasion when Lavoisier himself admits that he received the information.

M. Berthelot does not contradict it, but, instead, he asperses the moral character of Blagden. This method of treating a witness whose evidence cannot be rebutted is apt, when unsuccessful, to recoil on him who attempts it. It is perfectly true that Blagden interpolated the famous passage in Cavendish's memoir:—

"During the last summer, also, a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them. . . . But at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."

This passage, however, was inserted with Cavendish's knowledge and consent, and by his assistant and amanuensis, who happened to be the very man who had a personal knowledge of the facts. Assuming the statement to be true, where is the immorality of the proceeding?

Everything that we can learn authoritatively concerning Blagden goes to show that he was an upright and honourable man. Sir Joseph Banks has testified to his abilities and integrity. Dr. Johnson spoke of his copiousness and precision of communication, with the characteristic addition: "Blagden, sir, is a delightful fellow." Laplace, Cuvier, Berthollet, and Benjamin Delessert, were among his friends.[1] He was rich, and was understood to have speculated to profit in the French funds. For thirteen years he was a Secretary of the Royal Society, and in 1792 he was knighted for his services to science. Every year he spent a considerable time on the Continent, and was frequently in Paris. The gossip of the period states that he aspired to the hand of Madame Lavoisier, who preferred Count Rumford. He died in Berthollet's house at Arcueil, on March 26, 1820. In an obituary notice in the *Moniteur* of September 22, 1820, M. Jomard testifies to his benevolence and generosity, and states that "none of his countrymen have done more justice to the labours and discoveries of the French, or have contributed more than he to the happy relations which have subsisted for six years (1814–20) between the *savans* of the two countries." By his will he provided liberally for his scientific friends: Berthollet, the daughter of Madame Cuvier, and the daughter of Count Rumford, each received £1000; and Laplace £100, "to purchase a ring." M. Berthelot asperses the character, not only of Blagden, but also of his countrymen by his insinuations. Would he have us believe that men like Berthollet, Cuvier, and Laplace, would extend their friendship to, and receive pecuniary benefits from, one whom they believed had foully stabbed their compatriot in the back? It is surely incumbent on M. Berthelot, on every ground, either to substantiate his implications or to withdraw them.

M. Berthelot makes the gratuitous assumption that I am ignorant of the work of Monge. Whether I am or not is altogether beside the mark. There is, indeed, no question of Monge. Monge distinctly disclaims priority to Cavendish, nor did he attempt to establish a right to be considered an independent discoverer of the true nature of water. In his memoir, "Sur le Résultat de l'Inflammation du Gas inflammable et de l'Air dephlogistiqué dans les Vaisseaux Clos," he tells us that the experiments recorded in it were made in June and July 1783, and repeated in October of the same year. "I did not then know," he adds, "that Mr. Cavendish had made them several months before in England, though on a smaller scale; nor that MM. Lavoisier and Laplace had made them about the same time at Paris in an apparatus which did not admit of as much precision as the one which I employed." I fail to see what M. Berthelot gains by his reference to Monge.

M. Berthelot reproaches Priestley and Cavendish for their adherence to phlogistonism. I say it with all respect, but is it seemly for M. Berthelot, of all men, to cast this stone? Is not he himself an exemplification of that conservatism which he deplores? A generation ago the doctrine of Avogadro became the corner-stone of that edifice of which M. Berthelot asserts that Lavoisier laid the foundations. Indeed, the introduction of that doctrine effected a revolution hardly less momentous than that of which Lavoisier was the leader. But what has been M. Berthelot's consistent attitude towards this teaching? We can illustrate it by a single example. He is the sole teacher in Europe of any position who continues to symbolize the constitution of that very substance of which he claims that Lavoisier discovered the composition by a formula which is as obsolete as any conception of phlogistonism.

T. E. THORPE.

[1] Many of the letters of Berthollet to Blagden are still in existence. In one of these, dated "19 Mars, 1785," he writes from Paris:—"L'on s'est beaucoup occupé ici ces derniers tems de la belle découverte de Mr. Cavendish, sur la composition de l'eau: Mr. Lavoisier a tâché de porter sur cet objet toute l'exactitude dont il est susceptible. . . . Mr. Lavoisier veut repéter l'expérience en faisant brûler l'air dephlogistiqué dans le gas inflammable, et il y a apparence qu'alors on n'aura point d'acide nitreux, sel n les belles observations de Mr. Cavendish." Is this language consistent with the belief that Berthollet, who must have known the facts, regarded Lavoisier as the real discoverer of the compound nature of water?

A HAND-BOOK OF PHOTOGRAPHY.

Handbuch der Photographie. Part I. Fourth Edition. By Prof. Dr. H. W. Vogel. (Berlin: Robert Oppenheim, 1890.)

THIS is the latest edition of a work which has been known in Germany for ten years, and of which the author is the Director of the Photochemical Laboratory of the Imperial Technical High School in Berlin. The existence of such a post as that occupied by Dr. Vogel in one of the foremost technical schools of Germany is as much an indication of the advanced state of technical education in that country as the non-existence of such specialists in the technical schools of this country is a sign of our comparatively backward condition in the field of chemical technology. The subjects comprised under this heading are so wide in their range and so difficult to grasp, excepting by actual personal contact with the chemical industries, that no instruction likely to be of any great value to those preparing for, or engaged in, the latter can be given, unless the instructor has this qualification. Nor can the student properly avail himself of the instruction thus offered, unless he on his part is well grounded in the general principles of the science which underlies his subject. When such a ground-work has been laid, and the student thus equipped is passed on to the specialist, the result is a chemical technologist who is likely to be of real use to his country. The Germans have realized this long ago—the machinery exists both for laying the foundation and for raising the superstructure of specialized knowledge. In this country, so far as chemical technology is concerned, we have not yet advanced very much beyond the stage of furnishing the appliances for the general training—the real technical or special training has been allowed to take care of itself, and the student is supposed to have finished his education at the time when he ought really to be beginning it.

These ideas naturally suggest themselves in having brought under one's notice the various special works on applied science which reach us from time to time from the German press, and of which Dr. Vogel's book is a not unfavourable specimen. The present volume, which is the first part of the work only, contains some 350 pages, and the advancement of the subject since the last edition is indicated by the fact, which the author states in the preface, that the subject of photochemistry alone has been increased from 8½ to 22 pages. The whole volume consists of an introductory portion, three chapters, and an appendix. We propose to give, in the first place, a brief analysis of its contents.

The introduction consists of a history of photography, followed by some remarks on the study of the subject. The scientific aspect of photography forms the subject of the three chapters which constitute the main portion of the work. The first chapter deals with the physical action of light, and comprises such subjects as phosphorescence, phosphorography, the photophone, telephotography, the action of light on polished surfaces, Crookes's radiometer, and the action of light on ebonite, including an account of Edison's tasimeter, which, the author thinks, may become serviceable as a chemical photometer.

The second chapter occupies over 200 pages, and is divided into two sections, dealing with the action of light on non-metals and metals respectively. The subdivisions of this portion of the subject are well planned, but are not carried out with logical consistency. The action of light on inorganic compounds brings in allotropy and the photochemical combination of hydrogen and chlorine, &c. The action of light on organic compounds begins with the remarkable subject of the photopolymerisation of such compounds as vinyl bromide, anthracene, quinine, chloral, and asphalte, the latter, of course, leading to the original heliographic process of the elder Niepce. The author gives good reasons for the belief that the change in this last case is due to polymerisation, and not to oxidation. Instances of combination between organic compounds under the influence of light are then discussed, the formation of a compound from phenanthrenequinone and acetaldehyde being compared with the synthetical processes which go on in plants by the action of this same agency.

The photochemical decomposition of organic substances is dealt with at considerable length. The remarks concerning the action of light on cellulose in the form of wood and paper should be carefully studied by those interested in the technology of paper-making. The action of light on colouring-matters, both organic and mineral, is also treated of very completely, and the tables given will be found valuable to dyers, colourists, and painters. This portion of the subject is covered to some extent by Russell and Abney's Report on the fading of water-colours, of which the author has evidently availed himself. With respect to the fading of organic colouring-matters, it is of interest to note that all the artificial yellows are faster than the natural vegetable yellows. The section treating of the action of light on the vital processes of plants and animals brings the author into contact with such physiological subjects as germination, the formation of chlorophyll and other plant colouring-matters, the respiration of plants, and Aimé Girard's observations on the formation of sugar in the beetroot. Under the action of light on animals, the author gives an account of Engelmann's experiments on *Bacterium photometricum*, but Lubbock's analogous experiments with the Daphnidæ are not alluded to.

Passing to the second section, we find 167 pages devoted to the action of light on metallic compounds. The logical sequence is here broken by the introduction of a large amount of ordinary chemistry, *i.e.* the formulæ and properties of the most important compounds of the metals used in photography. This is very well in its way, and it is essential that the scientific photographer should be familiar with this portion of his subject, but it may be suggested that in future editions these paragraphs should be relegated to the third chapter, which deals with the chemistry of photographic materials. The author's use of formulæ, we may here point out, is somewhat antiquated, capricious, and inconsistent. Thus in some places chloracetic acid is written $C_2\left.\begin{matrix}H_3\\Cl\end{matrix}\right\}O_2$, ferrous tartrate, $C_4\left.\begin{matrix}H_4\\Fe\end{matrix}\right\}O_6$, ferric tartrate, $C_{12}\left(\begin{matrix}H_{12}\\Fe_2\end{matrix}\right)O_{18}$, ferrous acetate, $C_4\left.\begin{matrix}H_6\\Fe\end{matrix}\right\}O_4$; while on the same page, or in other parts of the book, we find ferric citrate written, $(C_6H_5O_7)_2Fe_2$, silver tartrate, $C_4H_4O_6Ag_2$, and silver citrate, $C_6H_6O_7Ag_2$. In the same equation, on p. 152, sodium thiosulphate is written $Na_2S_2O_3$, and the double silver salt, $\left.\begin{matrix}Na_4\\Ag_2\end{matrix}\right\}3S_2O_3$. Then, again, some compounds of very definite composition, such as sodium nitroprusside, Prussian blue, &c., are not favoured with formulæ at all; while in other cases, such as under the salts of ammonia, the alkalies, and the alkaline earths (third chapter), the formulæ suddenly rise to the dignity of thick type.

Under the action of light on iron salts, we have a description of the various printing processes depending on the use of these compounds, such as Willis's platinotype, the negative and positive blue processes and other less widely-known methods. Under chromium compounds we have an account of the chromatized gelatine processes, including that of Pretzsch (relief process), Fox Talbot (steel etching), pigment printing, Woodburytype, photogalvanography, lithography, and zincography, and a multitude of other processes, which form quite a special feature among the recent developments of applied photography. The salts of uranium and copper are dealt with in due order, but the chief interest, of course, centres in the compounds of silver. After a brief description of the pulverulent metal, the use of silver as a developing agent in acid and alkaline developers is discussed. Speaking of the black compound which is precipitated when ammonia is added to a solution containing silver nitrate and a ferrous salt, the author uncompromisingly gives the formula, $Ag_4O + Fe_3O_4$, which at least may be said to require confirmation. The chemical principles of intensification and toning, and the transformations of silver pictures by substitution, are well dealt with, and a long account of Carey Lea's researches on the allotropic modifications of silver is given. When treating of silver nitrate, Dr. Vogel gives way to patriotic bias in the form of a footnote:—

"Als Thatsache erwähnen wir, dass das in Deutschland fabricirte Silbernitrat das reinste ist, welches geliefert wird. Ueberhaupt übertreffen ALLE deutschen Chemikalien die ausländischen weit und werden daher von allen Pharmazeuten des Auslandes hoch geschätzt. Selbst in China, Japan, Indien, Nordamerika werden deutsche Chemikalien allen übrigen vorgezogen. Wenn zuweilen in ausländischen Zeitungen Verdächtigungen derselben versucht werden, so laufen diese auf Concurrenzneid hinaus" (p. 147).

Our chemical manufacturers had better see to this!

Of fundamental importance in photography are the silver haloids, and to these we naturally turn with the greatest interest. The author admits the existence of a subchloride, but justly expresses reserve as to its formula. The "photosalts" of Carey Lea are described, and the oxychloride theory referred to, but the author omits to mention that these coloured compounds, produced by chemical methods, were discovered by the British Association Committee of 1859. Photochromy is treated of briefly, and in an earlier portion of the work (p. 9) the author distinctly asserts that permanent photography in natural colours has never been accomplished. Of course we knew this before, but it is well that the public should have the statement from such a recognized expert as Dr. Vogel. Under silver bromide will be found an account of Stas's modifications of this haloid, and after discussing the ripening of the salt in emulsions, and the action of ammonia thereon, the author arrives at the conclusion that these modifications represent different states of physical aggregation—a conclusion which most scientific photographers will endorse. The discussion of this part of the subject is, we may add, very thorough. Silver iodide is similarly treated of, and then comes a section on the influence of different substances on the sensitiveness of the silver haloids.

This last subject leads to the action of sensitizers, and some very useful tables, giving the results of experiments with all kinds of sensitizers, are here given. The subject of development, which the author divides into chemical and physical, is dealt with under this same section, and the different compounds which have been used for this purpose are enumerated. Sensitizers are again discussed after development has been disposed of, and we are sorry to see that Dr. Vogel still classifies these as chemical and optical sensitizers. The latter comprise those colouring-matters which confer on the silver haloid film a special sensitiveness for certain rays of low refrangibility, and the author gives a long account of this discovery, with which his name will always be associated. We confess to being somewhat disappointed with this section. The theory of orthochromatic photography is in a very unsatisfactory state, and we should have liked to know the author's views on this subject. By his still retaining the term "optical sensitizer," he leads us to infer that he has not abandoned the physical theory. In this case a discussion of Abney's results and a repetition of his experiments are most urgently needed. We shall, however, perhaps, be more enlightened in the practical portion of the work which is to follow the present part.

In reviewing the history of orthochromatic photography, the results of Eder are given almost *in extenso*, but the experiments of Abney and Bothamley in connection with this subject are not referred to. Perhaps these, again, are being reserved for the succeeding parts of the book. Dr. Vogel can certainly score against those who discredited his discovery in 1874, but he is hardly correct in stating that the Berlin Academy of Sciences alone recognized its importance. In NATURE, vol. x. p. 281, the value of the discovery was pointed out, and an account, of the early experiments was given. The treatment of the subject of solarisation, which follows that of "optical sensitizers," is somewhat meagre, and from the scientific point of view we should have been glad to have a more complete discussion of a topic of such fundamental importance to the theory of the photographic image. The author accepts the explanation of reversed chemical action, but he does not, we venture to think, lay sufficient stress on the important part played by the vehicle or sensitizer in this phenomenon.

After dealing with the salts of silver, we are led in due order to the compounds of mercury, lead, gold, platinum, and allied metals. The only comment we have to make is that the ordinary chemical properties of these different salts would be more in place if described in the third chapter, so that this portion of the work might be restricted to what its title indicates, viz. the chemical action of light. Nor do we understand why the author should retain the old name "platina," as his own countrymen have now generally dropped the terminal letter, while here and in America the names of all the metals of the group are made to end in "um." It certainly seems strange to a chemist of the present time to read: "hierher gehören Platina, Iridium, Palladium, Osmium, &c."

The third chapter is devoted to a description of photographic chemicals, and is more or less of the nature of an ordinary descriptive manual of chemistry, having special reference to the elements and compounds used in photography. Among the metalloids, oxygen, hydrogen, and the halogens are alone treated of. Under solvents, we have the compounds water, ethyl and methyl alcohols and some of their homologues, glycerin, ether, chloroform, benzene, &c. Then follow the acids, inorganic and organic, and bases and salts, beginning with the compounds of potassium, sodium, and ammonium, and ending with those of the earthy metals. The salts of the heavy metals, having been taken (out of order) in the preceding chapter, are not dealt with here. The author, in explaining the theory of salts, clings to the old water type, *e.g.* $\left.\begin{matrix}SO_2\\H_2\end{matrix}\right\}O_2$, $\left.\begin{matrix}NO_2\\H\end{matrix}\right\}O$, &c. Under ammonia (p. 269) we are told that in the salts of this base ammonium (NH_4) is present, "das mit Sauerstoff das Ammoniumoxyd (NH_4O) bildet." On p. 261 potash alum is written $K_2SO_4 \cdot Al_2(SO_4)_3 + 24H_2O$, and on p. 269 ammonia alum is formulated $(NH_4)Al(SO_4)_2 + 12H_2O$. The section on reducing agents and developers treats of hydroxylamine, tannin, gallic acid, pyrogallol, hydroquinone, pyrocatechol and resorcinol, paraphenylene-diamine, phenylhydrazine, and eikonogen.

Following this section we again have some ten pages devoted to optical sensitizers, the compounds described being eosin and allied colouring-matters, cyanin, quinoline red, and chlorophyll. There are some amusing footnotes attached to this section, one of which we cannot refrain from quoting, as illustrating the author's method of dealing with the sceptics at home and abroad. After attempting once more to make clear his definition of an optical sensitizer, he admits that this definition can only be made intelligible to those

"welche von farbigen Strahlen, d. h. Spectralfarben, und von optischer Absorption derselben, also Absorptionsspectralanalyse, eine klare Vorstellung besitzen, die leider bei sehr vielen Empirikern (und auch Wissenschaftern) die in dieser Sphäre arbeiten, vermisst wird."

Then comes the note :—

"Wie übel es in dieser Hinsicht bestellt ist, geht daraus hervor, dass sogar ein Professor der Chemie und Physik in Berlin das Spectrum von Eosin und Eosinsilber, welche total von einander verschieden sind, als gleich erklärte, dass der gerichtliche Sachverständige Prof. Spiller in England die Behauptung aufstellte, Eosin und Chinolinroth seien identisch, und dass sogar Carey Lea meinte, ein Sonnenspectrum lasse sich durch eine Anzahl farbiger Glasstreifen ersetzen."

We do not regard it as "good form" in this country to make horrid examples of our co-workers in a book intended for the use of students. If any remarks of a polemical character had to be brought forward, there were other arenas, both here and in Germany, where Dr. Vogel might have broken a lance with his adversaries.

Under the heading "Bildträger" (image-carriers or film-producing materials), we have an account of cellulose, starch, pyroxylin, albumin, gelatine, and paper. The description of the various cellulose nitrates and their preparation is fairly complete. Under gelatine we do not find, either in this chapter or in the historical portion, any reference to the name of Maddox, who first made the use of this vehicle practicable, and laid the foundation of our modern gelatino-bromide emulsion processes. Some miscellaneous subjects which are not included in the text are added in an appendix. It must be mentioned that there are numerous prints and thirteen plates inserted in the work, some of them very beautifully executed, and introduced with the object of illustrating the various photo-etching, engraving, and printing processes, the difference between orthochromatic and ordinary plates, spectrum photography on dyed films, &c.

With respect to the work as a whole, it will be seen that it covers to a large extent the same ground as the "Ausführliches Handbuch der Photographie" of Eder, which is also a recognized standard work. In some respects Dr. Vogel's book offers advantages over the latter, but in other respects it is inferior. We miss the splendidly complete lists of references with which Eder's work abounds, and in neglecting to supply this information the author often unconsciously does himself injustice, for there is much original work included in the volume which many readers would desire to refer to in the original papers. Dr. Vogel has contributed so largely to the advancement of photography that any observation or experiment of his is entitled to the fullest consideration.

The criticisms which have been offered in the course of this notice are on minor points, but taken in their *ensemble* they indicate certain weaknesses which are to be regretted. It is obvious from the examples given that pure chemistry is not the author's strong point, and it would have been better, seeing how largely the subject is connected with this science, if he had consulted some of his chemical colleagues. The inconsistencies of formulation and classification which have been pointed out might thus have been avoided, and the work made more logically coherent. Another defect is the retention here and there of passages which look like survivals from an earlier edition. For example, we read on p. 7: "Der Collodium process verbreitete sich allgemein, wurde im Laufe der Zeit immer mehr und mehr vervollkommnet und ist jetzt der ausschliesslich angewendete." Again, on the same page: "Collodium für den Negativprocess, Albuminpapier für den Positivprocess bildeten die wichtigsten Grundlagen unserer photographischen Bilder." This may have been true at the time of the last edition (1878), but is certainly not the case now.

In concluding this notice we can only express regret that the author should have fallen into the habit, now, unfortunately, becoming only too common on the Continent and across the Atlantic, of allowing insufficient credit for, or, worse still, of ignoring altogether, work done outside his own country. The historical portion of the book hardly does justice to the labours of Fox Talbot when it is stated that "nach dem Bekanntwerden der Daguerre'schen Entdeckung suchte Talbot auch Camerabilder auf Papier aufzunehmen." If the introduction of the collodion process by Archer and Fry is considered worthy of historical record, surely the gelatino-bromide emulsion process of Maddox is at least of equal importance. When dealing with the action of light on selenium (p. 23), the author gives a description of Shelford Bidwell's experiments on telephotography, but the experiment of Ayrton and Perry having for its object the electrical transmission of moving images (*telopy*) is not referred to. When treating of optical sensitizers, he tells us that von Baeyer discovered fluoresceïn, that Caro discovered eosin, and that Jacobsen discovered quinoline red, but the reader is not informed that cyanin, one of the best special sensitizers, was given to science by Greville Williams as long ago as 1860. These and the other blemishes which we have felt bound to indicate have only to be remedied in future editions to make Dr. Vogel's book take that high position to which it is justly entitled, both on account of the vast body of useful and often original information which it contains, and the deserved reputation of the author as one of the foremost of German scientific photographers.

R. MELDOLA.

CONTRIBUTIONS TO INDIAN BOTANY.

Annals of the Royal Botanic Garden, Calcutta. Vol. II. Pp. 110; with 104 Lithographed Plates. (Calcutta: Bengal Secretariat Press, 1889.)

THE whole of this volume, like the first, with the exception of a part of the Appendix, is the work of Dr. G. King, the Director of the Calcutta Botanic Garden. It contains a monograph of the species of *Artocarpus* indigenous to British India, and a monograph of the Indo-Malayan species of *Quercus* and *Castanopsis*, both fully and excellently illustrated. The genus *Artocarpus* was founded by the Forsters (father and son, who accompanied Captain Cook on his second voyage, not brothers, as inadvertently stated by Dr. King) for the bread-fruit tree, with which they became familiar in the Pacific Islands. This they called *Artocarpus communis*, though most subsequent botanists have adopted the later Linnean name, *A. incisa*; and the younger Forster published a separate illustrated memoir on it, in German, entitled "The History and Description of the Bread-fruit Tree." Dampier, however, appears to have been the first to make this valuable tree known to Europeans. The only other familiar species of the genus is the Jak fruit (*Artocarpus integrifolia*), a prominent cultivated tree in the Malay peninsula and archipelago, and recently collected by Colonel Beddome in a wild state in the forests of the Western Ghats in the Deccan Peninsula, South India. Exclusive of this, Dr. King now describes and figures seventeen species found within the limits of British India, seven of which are described for the first time. Many of them are very handsome trees, but their wood is of little value, and, as far as their history goes, none yields an edible fruit.

The Indo-Malayan species of *Quercus* and *Castanopsis* number 82 and 22 respectively, besides some imperfectly-known species. As Dr. King remarks, there is no reason, except convenience and the desirability of not adding to the already overloaded synonymy, why all the species described under *Castanopsis* should not be placed in the section *Chlamydobalanus* of *Quercus*. Generally speaking, *Castanopsis* differs from *Quercus* in the involucre, which answers to the cup of the acorn, being prickly or tubercular, and completely inclosing the nut, and when ripe splitting irregularly to free the nut. But this distinction completely breaks down in the long series of species illustrated in the present monograph, the cup or involucre varying, in the species referred to *Quercus*, from two or three series of scales, or a discoid form, to an ovoid or globose receptacle completely enveloping the nut, and sometimes more or less prickly. In *Castanopsis*, on the other hand, the involucre is sometimes quite smooth. In foliage there is nothing to distinguish them, yet, taking the whole series of species, these Asiatic oaks exhibit a wonderful and beautiful variety in foliage and fruit, especially in the latter, being in many of them exceedingly elegant in shape and structure. There are about half-a-dozen species of the same group (*Lepidobalanus*) as the British oak, but none has quite the kind of foliage characteristic of this, and some have leaves more like an apple-tree, others almost exactly the same as the sweet chestnut. In other groups the leaves are often very large, thick, and leathery, having entire margins. Like some of the oaks of Central America, some of the Indian species have acorns of enormous size. One of the handsomest trees of the Eastern Himalayas, at elevations of 5000 to 8000 feet, is *Quercus lamellosa*. It grows from eighty to a hundred feet high, and in young vigorous specimens the conspicuously veined leaves are as much as a foot long, and the depressed spheroidal cups or involucres are 2½ inches in diameter, enveloping all but the apex of the nut, and built up of broad concentric plates, thin towards the usually fimbriate edge. The figures illustrating this species in Dr. King's monograph are partly copied from Hooker's "Illustrations of Himalayan Plants," though this fact is not mentioned, which is apparently an oversight, as all other copies that we have noticed are acknowledged. Running through the plates from the beginning, we will indicate a few of the more striking. Thus, *Quercus semecarpifolia* (Plate 15), has globose acorns with the cup reduced to a small disk at the base; *Q. serrata* (Plate 16) is remarkable for having the acorn almost buried in a cup of long narrow scales; *Q. oidocarpa* has an ovoid acorn with a closely-fitting cup, at least two-thirds of its length, and consisting of a few elegantly notched broad plates; *Q. Kunstleri* (Plate 31) has the long narrow acorns in spikes, and seated in shallow cups similar to those of the common oak; *Q. grandifrons* (Plate 35), has broad leaves sometimes 15 to 18 inches long, and *Q. Scortechinii*, on the same plate, has a mossy cup containing a huge obovoid acorn; *Q. platycarpa* (Plate 65) has very flat acorns half immersed in the thin cup, and about an inch and a half across; *Q. cyclophora* (Plate 67) is somewhat similar, but the cup is very thick, and composed of very numerous rounded scales; in *Q. reflexa* (Plate 72) the cone-shaped acorns are borne in clusters, and entirely inclosed in a thin cup completely beset with short recurved prickles; *Q. Junghunii* (Plate 73) is so like a *Castanopsis* in its unsymmetrical prickly fruit as to be undistinguishable from it; and, finally, *Q. Beccariana* (Plate 78) presents a fruit more resembling the nest of a solitary wasp than an acorn, being an oblong body about three inches long by two broad, the cup entirely inclosing the acorn, and consisting of about four very broad overlapping layers of scales with thin edges.

Apart from the practical use of Dr. King's monograph, this long series of figures of the Indian oaks offers a most interesting opportunity to the student of evolution, especially if it be remembered that the figures are portraits of individual specimens, not embodiments of "species," and that another series of specimens of the same "species" would probably exhibit many intermediate modifications.

It is fortunate for science that Dr. King, supported by the Indian Government, should devote his time and talents to the elucidation of such large and difficult arboreous genera as *Ficus* and *Quercus*. These monographs are of the greatest value to botanists generally, and one would say specially valuable to the officers of the Forest Department of India. The drawings by native artists, are, as already stated, with few exceptions, original, very faithfully executed, and mostly sufficient for purposes of identification. The lithography, too, by the Government School of Art, Calcutta, deserves praise, comparing favourably with much of the work done in this country. We may perhaps be permitted to call attention to the fact that the copies we have seen of the present volume are printed on smaller paper than the first, which detracts from their appearance on the shelves.

W. BOTTING HEMSLEY.

OUR BOOK SHELF.

Elementary Text-book of Trigonometry. By R. H. Pinkerton, M.A. New Edition. (London: Blackie and Son, Limited, 1890.)

THIS is a new and enlarged edition of this excellent elementary text-book. An account of the method of proportional parts and of its application to logarithmic and trigonometrical tables has been added, together with a collection of questions in trigonometry which have been set during the last ten years in examination papers of the Science and Art Department in mathematics, second stage, and of the London University Intermediate Examination in Arts.

Instead of each paper being given separately, the questions in them are arranged under headings, and the source from which each is taken is indicated; and, to avoid the necessity of a book of tables, the logarithms required for their solution are given in a table at the end.

Throughout the work the author has explained most clearly and fully every part that might in any way prove difficult to the beginner, and he has added numerous well-chosen examples at the conclusion of each chapter.

Higher Geometry. Containing an Introduction to Modern Geometry and Elementary Geometrical Conics. By W. J. Macdonald, M.A. (Edinburgh: James Thin, 1890.)

Many of the more advanced theorems in geometry, which are not very often treated to any extent in elementary books, are here dealt with. The author's idea seems to have been to connect the theorems together as much as possible in a continuous and graduated series; and this, together with the fact that they are worked out in a neat and concise form, will greatly add to the utility of the book.

The latter part of the work treats of geometrical conics. Although it does not contain so many propositions as many of the elementary works on the subject, yet the author has included in it all the most important propositions, thus making it a brief course for those who are about to attack the subject for the first time. Many problems have been put in here and there among the propositions, and an index to definitions, which has been added at the end, ought to prove handy for reference.

Nautical Surveying. By the late Vice-Admiral Shortland, LL.D. (London: Macmillan and Co., 1890.)

This volume, which is published by the late Admiral's widow and children, relates chiefly to the errors to which surveyors and their instruments are liable. It shows how these errors are to be found and corrected. The book is not one for beginners, but appears to have been written rather for surveyors themselves, after they have become thoroughly acquainted with the more practical and simple surveying. Every branch of surveying is thoroughly discussed, but at such length that the work would be of little practical use to a beginner. An index would greatly improve the book.

An American Geological Railway Guide. By James Macfarlane, Ph.D. Second Edition. (New York: D. Appleton and Co., 1890.)

The first edition of this book appeared in 1878, and the object of the compiler was to provide travellers with a hand-book from which they might learn the geological structure of every district in America intersected by railways. Many changes and additions have, of course, become necessary since 1878; and at the time of his death, in 1885, Dr. Macfarlane had made extensive preparations for a new edition. His work has been completed by his son, Mr. James R. Macfarlane, who has had the advantage of being aided by various competent contributors and advisers. The idea of the book is excellent, and has been carried out with great care and intelligence. It relates to the Dominion of Canada, as well as to the United States, and anyone travelling in these countries may find out at once, by turning to the proper page of this volume, the geological significance of phenomena that may happen to attract attention during the journey. The work ought to do much to encourage a liking for geology in the New World; and even professional geologists may find it useful for occasional reference.

LETTERS TO THE EDITOR.

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

Araucaria Cones.

I should be glad to know through any of your correspondents whether the Araucaria is often known to bear cones in the British Islands?

A plant of the common Araucaria in my garden here was blown down in a severe gale two years ago. It was a well-grown plant about 20 feet high, and very healthy. I replaced it on the spot, supporting it by ropes well pinned down.

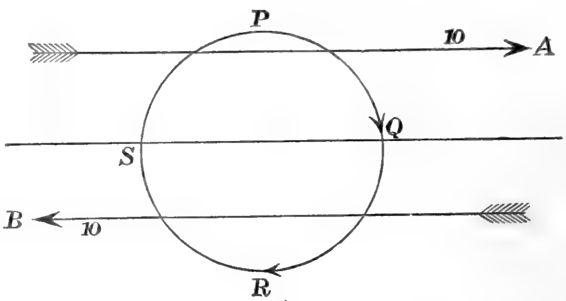
This autumn it has come out covered with cones all over the top branches. I have never seen them before, and I think they must be rare. They are terminal on the branches which bear them—sit upright upon them—and are of a very handsome ovate form. No scales are visible—the actual seed-vessels being covered and concealed by a thick coating of modified leaves or needles, narrowed, elongated, and terminating in hooked bristles.

Argyll.

Inveraray.

On the Soaring of Birds.

It is a pity that so many of your correspondents on this subject fail to grasp the elementary and self-evident fact that no common horizontal movement, relatively to the surface of the earth, of the air in which a bird is immersed can by any possibility enable it to soar. Upward convection-currents and upward slants may have something to do with the question, as may also the existence of different horizontal currents.



Thus, let there be two horizontal currents, A and B, in opposite directions, of 10 miles an hour each, and let a bird arrive at Q, down the path PQ, moving through the air at Q with a velocity of 5 miles an hour. On passing into the current B, it has a velocity relative to B of 20 miles an hour in the direction of current A, and of 5 miles an hour in a perpendicular direction. By proper adjustment of the wings, this relative velocity can be converted into work, and spent in lifting the bird to a higher altitude, so that, on arriving at S, its velocity relatively to the A current is again reduced to 5 miles an hour, and the circumstances at Q are exactly reproduced. When Mr. Magnus Blix began his communication (August 21, vol. xlii. p. 397) I expected that he was going to suggest this explanation; but though he commences with the supposition of the bird passing from one current to another, he goes on as if the bird afterwards remained constantly in one current.

An upward convection-current, as suggested by Mr. O. Fisher (September 4, p. 457), is, no doubt, a *vera causa* for a bird's being assisted in floating; but has Mr. Fisher reflected or calculated whether it is an adequate cause for actual soaring? Natural convection-currents can seldom have a rate of more than a few feet per second, whereas a velocity of 20 feet per second would be required to support a bird which weighed 1 pound for every square foot of supporting surface—wing, tail, and body. It is true that, in soaring, the rapid horizontal motion probably increases the horizontal support of the air, just as the transverse motion of the sails of a windmill through the air-current propelling them increases the horizontal pressure, as is proved by the fact that windmills evolve more work than they would be calculated to evolve if the pressure were the same as when they are at rest. The great difficulty, however, in all these explanations is that birds soar under circumstances which render these explanations inapplicable. In the open ocean, where a steady wind is blowing, where there are certainly no upward convection-currents, and where, equally certainly, there are no cross currents or diverse horizontal currents, where from the smoke of the steamer it is obvious that the air is moving *quasi* rigidly—that is, without perceptible internal motion—birds nevertheless soar to perfection.

As definite instances, however, are of more value than abstract statements, I subjoin copy of an actual observation made by myself and a friend some years ago. As the eyes become tired and dazzled in following the same object intently, we took it in turns to watch the bird, exchanging watch when the bird was so situated as to be easily identified.

"*Walmer Castle*, May 4, 1876" (on voyage home from the Cape).—"Observed a common gull for 11 minutes by the watch, following the ship without flapping a wing. The wings were used occasionally, but with a slow motion, as if for balancing or changing direction. The tail was occasionally altered in position when the bird ascended or descended.

"During the time of observation, the bird sometimes followed the ship with a steady motion, without apparently changing its height or velocity. Occasionally it veered to right or left a distance of a hundred yards or so, it rose and fell in altitude, changed its direction, fell back or overtook the vessel, without any apparent muscular effort. In overtaking the ship it seemed rather to rise than to fall" (though this is very difficult to judge of, because one cannot allow with any exactness for perspective or parallactic change of position).

"The direction of the wind, relatively to the ship, was E. by N., its velocity, as estimated by Captain Webster, was 7. The course of the ship was N.E. ½ N., its rate 8 knots.

"While following the ship, the bird was often within 15 yards, at which time there was no perceptible inequality in the horizontal angles of its wings, and no perceptible angle made by the axis of its body with the ship's keel.

"In moving to right or left out of the ship's course, there was generally a change of level." F. GUTHRIE.

South African College, Cape Town.

The Value of Attractive Characters to Fungi.

THE importance of attractive colours and odours and of modifications of form to flowering plants is now perfectly understood; but the value of attractive characters to fungi has received comparatively little recognition. At first sight it would seem unnecessary that a plant, insusceptible of fertilization, should possess characters apparently designed to enlist living creatures in its service: there is no pollen for them to carry, and no ripe seeds for them to distribute, and yet attractive characters, such as colour, taste, and odour, are extremely well marked.

The colours which fungi exhibit include almost every hue from white to black. We have the brilliant red of the Peziza cups; the orange-scarlet of the *Amanita muscarius*, with its cap gaily speckled with white; the crimson of the *Russula emetica*; the rich yellow of the *Cantharellus cibarius*; the blue of the bruised *Boletus luridus*; the amethyst of the *Agaricus laccatus*; and the dark green of the bruised *Lactarius deliciosus*, with every possible shade to the deepest jet. But not only have fungi colours that are attractive by day; some, like the *Agaricus olearius*, are phosphorescent by night. Many tropical species light up the jungle in the hours of darkness; and in this country the coal-mines are often found illuminated by one of the polypores which propagates itself on the timbers of the workings.

The tastes and odours of fungi are equally varied and attractive. Many Agarics have an odour of fresh meal; the *Hydnum repandum* rejoices in the flavour of oysters; the *Armillaria mucidus* in that of nuts; the yellow Chanterelle in that of apricots; others have the scents of various flowers, such as the violet and woodruff; or of aromatics like anise; while a large number have an indescribable damp cellar or fungus smell, such as slugs delight in. Many, like the shameless stinkhorn, *Phallus impudicus*, emit an intolerable stench which so strongly resembles

"the carrion of some woodland thing"

that blow-flies and ravens quickly find it out.

There can be little doubt that these are attractive characters. What, then, can be the service which these characters induce animals to perform for fungi? To answer this let us review briefly the life-history of any fungus possessing characters of an attractive kind.

The common mushroom, *Psalliota campestris*, is particularly agreeable to sheep and oxen, and is abundant in autumn in rich pastures. Although there is still much in our knowledge of its life-history that is incomplete, yet it is evidently composed of two main periods: first, a parasitic period passed in the body of an animal host; and secondly, a saprophytic period passed on some suitable organic soil. Let us sow the spores of a ripe mushroom as carefully as we may, none of them will grow: the first stage of the mushroom's existence must be passed in the body of an animal host; and as horses, sheep, and oxen are all readily attracted by its taste and mealy smell, it has never any difficulty in finding a host to take it in.

When once the spores have passed from the body of the host, they produce a mycelium, from which the future mushroom is formed. The connection between fungi and animal droppings is a matter of very early observation, and our forefathers were wont to believe that certain evil species came from the body of the Wicked One, and familiarly called them Tode's-stools, or Devil's droppings.

In this division of the life-history of fungi I believe we have the key to the value of attractive characters. Horses, oxen, sheep, foxes, squirrels, moles, birds, snails, and insects are all attracted by appropriate scents, tastes, and colours; and the forms and habitats of fungi are those which have best succeeded in attracting their particular hosts. There is no living being either great enough or small enough to escape the attentions of these plants in their ceaseless endeavours to attract; and among fungi, just as among flowering plants, every variation of form, scent, and colour has been perpetuated and developed, because it has been successful in attracting and in thus securing the multiplication of the species.

The subject is one, I think, that requires the gathering together of much individual observation in all parts of the world; and it would be well if those who have the opportunity would note at the time the name of the fungus and its observed host, and if students of biology who possess facilities for laboratory work would follow the matter still further by artificial cultures, and so determine the changes that take place in the body of the host, and the course of the alternating sexual and agamous generations. CHARLES R. STRATON.

Wilton, Salisbury.

Extraordinary Flight of Leaves.

MR. SHAW'S letter (NATURE, vol. xlii. p. 637) is a curiously corroborative fact in support of Mr. Wallace's theory of the wind being an agent for the dispersion of seeds, which he so strongly urges in his book, "Darwinism," to account for the universal distribution of many plants. For if, as the letter intimates, such weighty objects as oak-leaves can be conveyed through the air in such vast numbers as to cover an area two miles long by one in width, then it would not require a wide stretch of imagination to conceive that miniature objects like seeds, delicately winged for flight by Nature as so many are, might travel by thousands for hundreds of miles in favourable winds.

Speaking of leaves, on the morning of that severe frost last week, I observed a horse-chestnut, in full foliage, showering down its leaves with extraordinary rapidity, so that in three hours the tree was bare—half the leaves were yellow, the rest still quite green. The gardener gathered twelve large barrow-loads from beneath it. R. HAIG THOMAS.

November 1.

Kœnig's Superior Beats.

THE interesting experiments of Dr. Kœnig at the meeting of the Physical Society on May 16, and described in NATURE (vol. xlii. p. 190), have induced me to offer your readers the following view of Dr. Kœnig's superior beats and beat-tones.

If we look for the physical cause of the superior beats, we find nothing but the inferior beats themselves to build upon. But if we can admit that inferior beats may be in an ascending and descending scale of intensities, we have at once the structure of a beat which may be appropriately termed a superior beat, viz. the beat resulting from the periodic recurrence of a maximum and minimum inferior beat, in the same manner as the inferior beat is the result of the periodic recurrence of a maximum and minimum vibration.

Now, all inferior beats can be divided into two classes—similar and dissimilar beats. The four beats given by 128 vibrations, with 124 vibrations in the second, are perfectly similar beats, because the ratio $\frac{128}{124}$ is $\frac{4 \times 32}{4 \times 31}$, *i.e.* the ratio in its simplest form is $\frac{32}{31}$, giving one beat under precisely the same conditions of phase coincidence, at each quarter of a second. But the four beats given by 127 vibrations, with 123 in the second, are dissimilar beats; because the two S.H.M.'s, though together four times in the same phase in the second, these coincidences are at different parts of the wave-length; just as the hands of a watch are twelve times together in one complete revolution of the hour hand, but at different parts of the dial.

Another example will complete my meaning. The example is an experiment described by Dr. Kœnig in his valuable work, "Expériences d'Acoustique," with the two tuning-forks making 75 and 40 vibrations in the second, English measure. Five distinct beats were heard along with the beat-sound of 35 beats in the second. Here we have the interval-ratio $\frac{75}{40} = \frac{5 \times 15}{5 \times 8}$ showing (15 – 8) or 7 dissimilar beats recurring five times in the second. And we have apparently no other physical quantities to deal with. So that it appears a necessary conclusion that the seven dissimilar beats are in an undulatory order of intensity, in order to account for the five superior beats of Dr. Kœnig. The construction is then simple. Each recurrence of the 7 dissimilar beats is marked by a beat, and there are 5 of these in the second. These are the 5 superior beats formed out of the 5 × 7, or 35 inferior beats.

For this reason I have introduced the term cycle for the resultant generating curve formed by two S.H.M.'s of unequal periodic times. The curve or cycle is traced by the extremity of one generating radius revolving about the moving extremity of the other as a centre, and is complete when the two radii return to their original position. The cycle may be of long or short period. In the octave interval it is very short. With a nearly perfect unison it is very long. In every cycle there is at least one beat. There may be many dissimilar beats, but there cannot be any similar beats in the same cycle. The superior beat, number is, then, the number of cycles in the second; and the inferior beat-number is the product of the number of cycles into the number of dissimilar beats in the cycle.

According to this theory, therefore, the superior beats heard on the occasion referred to should be accounted for as follows:—

(1) When the two tuning-forks were executing 120 and 64 vibrations respectively in the second, eight superior beats were heard, because $\frac{120}{64} = \frac{8 \times 15}{8 \times 8}$ gives 8 cycles of 7 dissimilar beats, in each second of time.

(2) When the forks were making 96 and 64 vibrations, or the fifth interval, 3/2, it would be more correct to say that there were no superior beats than to say that "the inferior and superior beats agree in frequency." Because the ratio $\frac{96}{64} = \frac{32 \times 3}{32 \times 2}$ shows 32 cycles of only one beat in each.

In like manner all the other experiments given in detail in the text of June 19 (p. 190) can be accounted for. But there is an explanatory example given by Dr. S. P. Thompson, which, if verified by experiment, must be fatal to my theory of Kœnig's superior beats. This illustration gives 92 inferior beats and 8 superior beats in 492 and 100 vibrations of the primaries; while, according to the theory just sketched, $\frac{492}{100} = \frac{4 \times 123}{4 \times 25}$, shows only 4 superior beats with 392 inferior beats. Only Dr. Kœnig's valuable instruments could satisfactorily decide between them, and I should be glad to know if my building cannot stand. On the other hand, Dr. Molloy's elegant experiment, described in NATURE (vol. xlii. p. 246), is in favour of the cycles of dissimilar beats, inasmuch as these account for his three beats directly from the primaries without the aid of secondaries. In this experiment the primaries were 384 and 255 vibrations in the second; and the ratio $\frac{384}{255} = \frac{3 \times 128}{3 \times 85}$ shows 3 superior beats with 43 dissimilar beats in 129 inferior beats.

Stonyhurst College. WALTER SIDGREAVES.

THE CELL THEORY, PAST AND PRESENT.[1]

I.

IN taking the chair at the first general meeting of the Scottish Microscopical Society, I would offer to the members my hearty thanks for having done me the honour to choose me as the President under whom the work of the Society is to be inaugurated, and during whose incumbency the Society is to begin to substantiate its claim to have an existence amongst the scientific Societies in Scotland.

As myself engaged in biological studies, it is only natural that my attention should have been more particularly directed to the use of the microscope in connection with them, and to the influence which it has exercised on their advancement. Since the time of Hooke, Grew, Malpighi, and Leeuwenhoek, this influence has been continuous and progressive. The improvements in the instrument during the present century have led to discoveries of the utmost value in the structure of plants and animals, and to generalizations of a wide-reaching importance.

One of, if not the most fundamental of these discoveries, was the recognition of the anatomical unit, which we call a *cell*, as a common element in the structure of organisms. Our conceptions of the structure of cells, of the relative function of their constituent parts, and the mode in which cells are developed and multiply, has varied very materially from time to time. I purpose to pass in review those aspects of the subject which have attained prominence, and have influenced the course of investigation.

Dr. Robert Hooke was one of the first men of science to employ the microscope in the study of the structure of plants and animals. A chapter in his "Micrographia" (London, 1665) is entitled "Of the Schematisme or Texture of Cork and of the Cells and Pores of some other such frothy Bodies." This is probably the first use of the word cell in histological description. In the course of this chapter he refers to the lightness of cork, which he compares with froth, or an empty honeycomb. Its substance, he says, is wholly filled with air, which "is perfectly enclosed in little Boxes or Cells distinct from one another." Further, he gives an idea of the dimensions of these cells by stating that about sixty could be placed endways in the $\frac{1}{18}$th part of an inch, and that 1,166,400 could be placed in a square inch. He thinks that they are the channels through which the juices of the plant are conveyed.

The term cell was also employed to express a definite morphological unit by Dr. Nehemiah Grew,[2] who shares with Malpighi the glory of being one of the fathers of vegetable physiology. When describing in his "Anatomy of Plants" the skin of the root (p. 62), he says the parenchymous material is "frequently constructed of exceeding little *Cells* or *Bladders*, which, in some Roots, as of Asparagus, cut traverse, and, viewed through a Microscope, are plainly visible. These Bladders are of different sizes; in Buglos larger, in Asparagus less, and sometimes they coincide and disappear."

In his account of the parenchyma of the bark he again uses the word cells (p. 64), and says that "each is bounded within itself, so that the *Parenchyma* of the *Barque* is much the same thing as to its conformation, which the Froth of *Beer* or *Eggs* is as a fluid, or a piece of fine *Manchet* as a fixed body." These cells are so small as "scarcely, without the microscope, to be discerned;" more usually, however, Grew applies to them the term bladders or vesicles. In the chapter on the vegetation of roots he speaks of the sap swelling and dilating the bladders, and

[1] The Inaugural Address delivered to the Scottish Microscopical Society, by Sir William Turner, F.R.SS. L. and E., President of the Society.

[2] "The Anatomy of Plants," London, 2nd ed., 1682. The several books into which Grew divided his treatise were presented to the Royal Society of London at various dates between 1671 and 1675.

as being fermented therein, as transmitted from bladder to bladder, and leaving certain of its principles adhering to them. He thus recognized that the cells or bladders played an important part in the nutrition of the plant. Almost, indeed, he seemed to have grasped the idea that they exercised a selective or secreting influence; for, in describing the parenchyma of the fruit of the lemon, he speaks (p. 180) of "those little *Cells* which contain the essential Oyl of the fruit," whilst, he says, in other bladders "lies the acid juyce of the limon."

Malpighi, whose work on the anatomy of plants ("Anatome Plantarum," London, 1675) was almost cotemporaneous with the treatise of Grew, had also seen the structures which Grew named cells or bladders, and had designated them *utriculi*, and believed that they could be separated from each other. In a subsequent treatise ("Opera," vol. ii. p. 41, 1686) he described the lobules of fat in animals as consisting of adipose vesicles.

Leeuwenhoek, in the course of his microscopic inquiries into the structure of plants, gave the name of *globules* to many of the objects which we now term cells, though he expressly states that they were not perfect spheres.[1]

Clopton Havers, in his treatise on the skeleton, described ("Osteologia nova," p. 167, 1691) the vesicular structure of the marrow, and compared it, when seen under the microscope, to a heap of pearls.

Alex. Monro, *primus*, in his work on the bones ("Anatomy of the Humane Bones," Edinburgh, 1st ed., 1726; 2nd ed., 1732), when writing on the medullary structure, stated that it is subdivided "into communicating vesicular Cells, in which the Marrow is contained. Hence it is that the Marrow, when hardned and viewed with a Microscope, appears like a Cluster of small Pearls. This Texture is much the same as what obtains in the other cellular parts of the Body where Fat is collected, only that the Cells containing the Marrow are smaller than those of the *Tunica adiposa* or *cellulosa* elsewhere."

Caspar F. Wolff[2] also recognized that fat was contained in small vesicles, surrounded by a fine membrane. He conceived also that the developing organs, both of plants and animals, consisted of a viscous substance which contained cavities, cells, or bladders which communicated with each other.

Fontana figured the fat vesicles, both free and surrounded by the fibres of the areolar tissue.[3]

Mirbel, in his botanical writings,[4] published at the beginning of the present century, stated that vegetables were composed largely of cells. He described *le tissu cellulaire* as composed of *les cellules*, which were contiguous with each other, so that the walls were in common. These walls were extremely thin and translucent, and sometimes riddled with pores. The term cells was also used both by his contemporaries and immediate successors in their writings on the anatomy of plants.

But anatomists experienced much greater difficulty in distinguishing the presence of cells in the textures of animals. It is true that from the time of Malpighi and Leeuwenhoek, the globules or particles had been recognized in the blood, but it is only within a comparatively recent period that their cellular structure was determined. Both Bichat ("Anatomie générale," Paris, 1812) and Béclard ("Élémens d'Anatomie générale," Paris, 1823), in their important treatises on general anatomy, made no reference to cells as elements of the tissues. Both these authors had chapters *du tissu cellulaire* or *du système cellulaire*, a term which had been in use from the early part of the last century. But by the tela cellulosa or cellular tissue, anatomists meant that form of tissue which we now more appropriately call areolar tissue; the so-called cells of which are not microscopic closed vesicles, but areolæ or spaces bounded by the fibres or laminæ of which the tissue is chiefly composed.[1] Béclard, in his description of the adipose tissue, stated that the lobules of fat consisted of microscopic vesicles $\frac{1}{100}$ to $\frac{1}{800}$ of an inch in diameter. The vesicles, he says, have walls, but they are so thin as to be indistinguishable. The presence of organized vesicles or globules in the tissues of animals had thus been recognized, but it needed further observations and facts in order to bring them into association with the cells of vegetable tissue.

This was supplied by the discovery in 1831 by the great English botanist, Robert Brown, of the "nucleus" or "areola" in the cells of the epidermis, and other tissues in Orchideæ and many other families of plants.[2] Following closely upon this discovery were the observations of Schleiden, published in 1838 ("Beiträge zur Phytogenesis," *Müller's Archiv*, p. 137, 1838), that the nucleus was a universal elementary organ in vegetables. Schleiden also came to the conclusion that the nucleus must hold some close relation to the development of the cell itself, and he consequently called the nucleus a "cytoblast."[3] Schleiden further discovered that the cytoblasts contained one or more minute circumscribed "spots," or "rings," or "points," which he considered to be formed earlier than the cytoblasts, and which were regarded by him as hollow globules, and were subsequently named by Schwann "nucleoli."

The cellular structure of some of the animal tissues had also begun to be recognized. Turpin had noticed the resemblance between the epithelium corpuscles found in vaginal discharges and the cells of plants. Johannes Müller had discovered that the chorda dorsalis of fishes was composed of separate cells provided with distinct walls, though he did not detect a nucleus in them. Purkinje, Von Baer, Rudolph Wagner, Coste, and Wharton Jones had seen the germinal vesicle within the animal ovum. E. H. Schultz had observed the nucleus in the blood globules, and Valentin and Henle had seen it in the cells of the epidermis. The way was thus prepared for a fuller recognition of the essential correspondence between the elementary tissues of plants and animals and for a wider generalization. Science had not long to wait for an observer who could take a comprehensive grasp of the whole subject; and in 1839 Theodore Schwann published[4] his famous researches into the structure of animals and plants, in which he announced the important generalization that the tissues of the animal body are composed of cells, or of materials derived from cells:—

"That there is one universal principle of development for the elementary part of organisms, however different, and that this principle is the formation of cells."

Both Schleiden and Schwann entertained the idea, which had long before been present in the mind of Grew, that a cell was a microscopic bladder or vesicle. In its typical shape they regarded it as globular or ovoid, though capable of undergoing many changes of form. This vesicle possessed a cell-membrane or wall, which enclosed

[1] Samuel Hoole, who translated many of Leeuwenhoek's writings (London, 1799, part 2, p. 178), when describing Fig. 11, on Pl. vi., says that the globules of meal are enclosed as it were in cells, and that some of those cells are represented at H in the figure. Leeuwenhoek, himself, however, in his description of the same figure ("Epistolæ physiologicæ," Delphis, 1719, p. 25), does not use the word cellula.

[2] "Theoria Generationis," editio nova, 1774; Commentary "Ueber die Nutritionskraft," by Blumenbach and Born, St. Petersburg, 1789.

[3] See his essay "Sur la structure primitive du corps animal" in his "Traité sur le vénin de la Vipère," Florence, 1781 (Pl. viii., Figs. 19, 20).

[4] "Traité d'Anatomie et de Physiologie végétale." t. i, Paris. An. x.; "Exposition de la Théorie de l'organisation végétale," Paris, 1809. Ch. Robin, in the article "Cellule," "Dict. Encyclop. des Sciences médicales," Paris, 1873, credits Mirbel with having introduced the term "cellules," but the extracts given in the text show that its English equivalent, cells, had been in use for upwards of a century before Mirbel wrote.

[1] The term cellular tissue was originally applied to this texture from a fancied resemblance to the proper cell tissue of plants; the walls of the cells of which were believed to be formed of a framework of fine fibres.

[2] "Organs and Mode of Fecundation in Orchideæ and Asclepiadeæ," Trans Linn. Soc., vol. xvi., 1833; reprinted in "Miscellaneous Botanical Works," vol. i. p. 511. Ray Society edition.

[3] Fontana (*op. cit.*) figured the "globules" or scales of the epidermis, in which he recognized the nucleus, but he neither gave it a special name, nor knew its importance (Plate i., Figs. 8, 9, 10.)

[4] "Mikroskopische Untersuchungen," 1839; and preliminary notices in Froriep's *Notizen*, 1838.

contents that were either fluid or somewhat more consistent. Either attached to the wall or embedded in it was the nucleus, which in its turn contained the nucleolus. Schwann, however, recognized (p. 176 of Sydenham Society's translation of Schwann's memoir) that many cells did not exhibit any appearance of a cell-membrane, but seemed to be solid, and had their external layer somewhat more compact. As showing, however, the importance which Schwann attached to the cell-wall, I should state that he regarded the chemical changes or metabolic phenomena as he termed them, as being chiefly produced by the cell-membrane, though the nucleus might participate. He explained the distinction between the character of the cell contents and the cytoblastema external to the cell, to the power exercised by the cell-membrane of chemically altering the substances which it is either in contact with or has imbibed, and also of separating them so that certain substances appear on the inner and others on the outer surface of that membrane. In this way, he accounted for the secretion of urea by the cells lining the uriniferous tubes, and for the changes which not unfrequently take place in the cell-membrane itself by thickening or deposition of layers on or within it.

Schwann described the nucleus as either solid or hollow and vesicular, in the latter case being surrounded by a smooth structureless membrane; whilst the contents of the nucleus, other than the nucleoli, were in his view either pellucid or very minutely granulous.

Both Schleiden and Schwann conceived that in the formation of a nucleus a nucleolus was first produced, that around it new molecules were deposited for a certain distance, and then a nucleus was formed. When the nucleus had reached a certain stage of development, new molecules were deposited upon its exterior so as to form a stratum, which when thin was developed into a cell-membrane, but when thick only its outer portion became consolidated into a cell-membrane. Immediately the membrane became consolidated its expansion proceeded by the progressive reception of new molecules; the cell-wall separated from the cell nucleus, and a vesicle was formed; the intermediate space at the same time became filled with fluid, which constituted the cell contents.

Schleiden contented himself with little more than a simple statement of what he conceived to be the process of cell formation in plants; but Schwann entered into an elaborate survey of cell-life both in animals and plants, and founded on it a theory of cells applicable to all organisms.

Schwann conceived that there existed in organized bodies a solid amorphous or fluid substance to which he gave the name *cytoblastema;* this substance might be contained either within cells already existing, or else be situated in the interspaces between cells; and he believed that the cytoblastema for the lymph and blood corpuscles is the fluid lymph-plasma and liquor sanguinis in which these corpuscles float. He held that in the cytoblastema new cells are formed in the manner just described. In animals he says it is rare for cells to arise within pre-existing cells; more usually they arise in a cytoblastema external to the cells already present. Schleiden, on the other hand, maintained that in plants new cells were never formed in the intercellular substance, but only within pre-existing cells. The idea obviously present in the mind of Schwann was that the process of cell formation in a cytoblastema had some affinity with that of crystallization. He figuratively compares the cytoblastema to a mother-liquid in which crystals are formed. He speaks of molecules being deposited around a nucleolus to form a nucleus; of a nucleus growing by a continuous deposition of new molecules between those already existing; and of the cell being formed around the nucleus by a progressive deposition of new molecules; and in more than one passage he indicated that this deposition is a precipitation. He obviously considered the principle of formation of the cell around the nucleus as the same as that of the nucleus around the nucleolus, a process which Valentin subsequently described as heterogeneous circum-position.

But Schwann at the same time showed that, with reference to the plastic phenomena, cells differed from crystals in form, structure, and mode of growth; for whilst a crystal increases only by the external apposition of new particles, a cell grows both by that method and by the intussusception of new matter between the particles already deposited. The difference, he says, is yet more marked in the metabolic phenomena, which he conceived to be quite peculiar to cells. Cells and crystals, however, he considered resembled each other in this point, that solid bodies of a definite and regular shape are formed in a fluid at the expense of a substance held in solution by that fluid, for both attract the substance dissolved in the fluid. Schwann concluded his memoir by advancing, as a possible hypothesis, the view that organisms are nothing but the form under which substances capable of imbibition crystallize; and although this hypothesis involved very much that is uncertain and paradoxical, yet he considered it to be compatible with the most important phenomena of organic life. Schwann inclined, therefore, to a physico-chemical explanation of cell-formation and cell-growth.

Shortly after the publication of Schwann's famous memoir, Henle, who had for some years been engaged in microscopic investigations on the tissues, published his well-known treatise on general anatomy.[1] He attached great importance in cell formation to extremely minute particles, $\frac{1}{8000}$ to $\frac{1}{12000}$ of an inch in diameter, which he called *elementary granules*. He conceived that these appeared in a blastema, that several aggregated together to form a nucleus, in connection with which he thought it not improbable that a cell subsequently formed. He looked upon the elementary granules as the first and most general morphological elements of the animal tissues, and he regarded them as vesicles consisting of excessively minute particles of oil coated with a film of albumen. It should be stated that Henle's observations on cell formation were conducted to a large extent on the products of inflammation, and on the lymph and chyle, in all of which fatty and granular particles abound.

As regards the part which the nucleus plays in the process of cell formation, both Schleiden and Schwann regarded it as of prime importance, though in the subsequent life of the cell they considered that its function terminated. Schleiden stated that, subject to certain exceptions which he enumerated, it is rare for the cytoblast to accompany the cell through its entire vital process—that it is often absorbed either in its original place, or cast off as a useless member, and dissolved in the cavity of the cell. Schwann, whilst contending for the exceedingly frequent, if not absolutely universal, presence of the nucleus, yet held that in the course of time it usually became absorbed and disappeared, so that it had no permanent influence either on the life of the cell or the reproduction of young cells, though he recognized that it remained in the blood corpuscles of some animals. Henle, again, maintained that, as there are nuclei without nucleoli, so also cells exist without nuclei, and that new cells may arise without the least trace of cytoblasts.

At about the same time, and also immediately after the publication of the important investigations by these eminent German observers, a young graduate of medicine of the University of Edinburgh, Dr. Martin Barry, stimulated, he says, by the researches and encouraged by the friendship of Johannes Müller, Ehrenberg, Rudolph Wagner, and Schwann, undertook elaborate researches into the structure of the ovum, more especially in mammals. His results were published in a series of memoirs printed in the Transactions of the Royal Society

[1] "Allgemeine Anatomie," Leipzig, 1841; also French translation by Jourdan in "Encyclopédie Anatomique," vols. vi., vii., Paris, 1843.

of London from 1838 to 1841.[1] In these embryological memoirs, Barry announced several important discoveries. In his first memoir (1838) he pointed out that the germinal vesicle which had been discovered in the mammalian ovum by M. Coste and Mr. Wharton Jones was the first part of the ovum to be formed both in mammals and birds, and he thought that this was probably the case throughout the animal kingdom. In his second memoir (1839) he extended to the mammalian ovum an observation which had been made by Prevost and Dumas on the ovum of the frog, and by Rusconi on the ovum in osseous fish. He described the formation within the rabbit's ovum of the body which he named, and which has been known since his time as the mulberry-like structure. This body arose at first as two vesicles, then as four, and so on in multiple progression, so that Barry was the first to recognize in the ovum of mammals the process which we now know as the segmentation of the yelk. He showed that the vesicles of the mulberry body were cells, and that each contained a pellucid nucleus, and that each nucleus presented a nucleolus. Further, these vesicles arranged themselves as a layer within the zona pellucida.

Barry's third memoir was published in 1840, and as he gave it the subsidiary title of "A Contribution to the Physiology of Cells," it is clear that he regarded his embryological inquiries as having an important bearing on the facts of cell-formation and function. He repeated his observations on the formation of the mulberry-like body, and now recognized that its component cells had been derived from the germinal vesicle, the contents of which entered at first into the formation of two cells, each of which presented a nucleus which resolved itself into other cells, and by a repetition of this process, the cells within the ovum became greatly augmented in number. Further, he stated that the whole embryo at a subsequent period is composed of cells, filled with the foundations of other cells. Although we may not agree with all the details given by Barry in his account of these observations, yet there can be no doubt that he had early recognized the important fact, that in animals new cells arose within pre-existing cells, as Schleiden had affirmed to be the case in plants, and that the nucleus acted as an important centre for the production of young cells. In recognizing the endogenous reproduction of young cells in animals, Barry made an important advance on the view entertained by Schwann, who regarded the endogenous production of cells as quite exceptional amongst animals.

In this same memoir Barry incidentally mentioned that he saw in the ovum of the rabbit a cleft or orifice in the zona pellucida, and that on one occasion he observed what he believed to be the head of a spermatozoon within the orifice. Two years afterwards he read to the Royal Society (Phil. Trans., vol. cxxxiii.; read December 8, 1842) a short paper, in which he announced that he had seen a number of spermatozoa within the ova of the rabbit, and in October 1843 he published a figure of an ovum with spermatozoa in its interior ("On Fissiparous Generation," *Edin. New Phil. Journ.*, October 1843).

In a memoir on the corpuscles of the blood, published in 1841, Barry announced a still more definite conception of the function of the nucleus. He directly traversed the statement of Schleiden, that the nucleus, after having given origin to the cell-membrane, has performed its chief office, and is usually cast off and absorbed; as well as that of Schwann, who had never, except in some instances in fat-cells, observed anything to be produced by the nucleus of the cell. Barry stated that the nucleus is a centre for the origin, "not only of the transitory contents of its own cell, but also of the two or three principal and last formed cells destined to succeed that cell; and in fact, that by far the greater portion of the nucleus, instead of existing anterior to the formation of the cell, arises within the cavity." Further, he says, "young cells originate through division of the nucleus of the parent cell, instead of arising as a sort of product of crystallization in the fluid cytoblastema of the parent cell." He regarded the division of the nucleus in pus corpuscles as not artificially produced by the agency of acetic acid, as was held by Henle and Schwann, but as a part of the process by which cells were produced, and apparently universal in its operation.

In a paper published in 1847, Dr. Barry summarized his observations on the nucleus of animal and vegetable cells, and whilst expressing certain opinions on the mode of formation of the nucleolus and nucleus and the growth of cells which cannot now be accepted, he continued to maintain that cells are descended from an original mother cell by cleavage of the nucleus, and all subsequent nuclei are propagated in the same way by fissiparous generation. Every nucleus, therefore, was a sort of centre, inheriting more or less the properties of the original nucleus of the fecundated ovum, which he conceived to be the germinal spot, and exercising an assimilative power. Dr. Barry's contributions to a correct conception of the development of cells are of the highest importance when viewed in the light of modern observations.

But another Edinburgh inquirer, Mr. John Goodsir, afterwards as Prof. Goodsir the distinguished occupant of the Chair of Anatomy in the University of Edinburgh, was engaged between the years 1842 and 1845 in studying the processes of cell-life, both in healthy tissues and in certain pathological conditions.[1] In his important memoir on secreting structures, published in 1842, he demonstrated from a variety of examples that secretion is a function of the nucleated cell, and he gave, as one of his many illustrations, the cells of the testis containing spermatozoa which were derived from the nuclei of these cells. In the original memoir he was inclined to believe that the cell wall was the structure engaged in forming the secretion; but in a reprint of it in 1845, he modified that view, and gave as his opinion that the secretion would appear to be a product of the nucleus. Goodsir also stated in the memoir of 1842 "that the nucleus is the reproductive organ of the cell, that it is from it, as from a germinal spot, that new cells are formed," and he cited cases in which it became developed into young cells. He subsequently, in a short paper on centres of nutrition, extended this view to the tissues generally. He defined the nutritive centres as minute cellular parts, existing, for a certain period at least, in all the tissues and organs. They drew from the capillary vessels or other sources nutritive material, which they distributed to the tissues and organs to which they belonged. He regarded a nutritive centre as a cell, the nucleus of which is the permanent source of successive broods of young cells, which from time to time fill the cavity of their parent. He called this central or capital cell the mother of all those within its own territory or department. Goodsir also showed that cells were important agents in absorption, ulceration, and inflammation. In inflammation of cartilage, for example, he described and figured the cells in the area affected as increased in size, modified in shape, and crowded with a mass of nucleated cells in their interior, through the agency of which the walls of the corpuscles and the hyaline matrix became absorbed. He also gave illustrations of the multiplication of nuclei within cells in the course of formation of cysts. Corroborative observations on endogenous formation within

[1] Phil. Trans., vols. cxxviii.–cxxxi. The value which was attached to these memoirs at the time may be estimated by the fact that the Royal Society of London awarded to their author in 1839 one of the Royal Medals. The neglect into which Dr. Barry's writings have since fallen is largely due to the disbelief in his subsequent descriptions of the spiral structure of muscular fibre, of blood-corpuscles, and indeed of the elements of the tissues generally.

[1] "On Secreting Structures," Trans. Roy. Soc. Edin., 1842; "On Peyer's Glands," *London and Edinburgh Monthly Journal*, April 1842; "On Structure of Human Kidney," *ibid.*, May 1842; "Anatomical and Pathological Observations," Edinburgh, 1845; also, his collected papers in "Anatomical Memoirs," Edinburgh, 1868, edited by W. Turner.

animal cells were also given by Mr. H. D. S. Goodsir, as confirmatory of the doctrine propounded by his brother on the cell as a centre of nutrition, secretion, and production of young cells. In a research into the structure of the testis in Decapodous Crustacea, Henry Goodsir observed that the head of the spermatozoon corresponded with the nucleus.

The conception entertained both by Martin Barry and John Goodsir of the process of cell-formation and of the function of the nucleus was in the main very different from that propounded by Schleiden and Schwann. Whilst agreeing with Schleiden in holding that new cells were formed within parent cells, they did not look upon the process as one of deposition, in the first instance around a nucleolus and then around a nucleus, but they regarded the nucleus as the prime factor by the division of which new cells were formed. With regard to the free formation of cells, as it was not unfrequently called, by deposition in a cytoblastema situated externally to existing cells, to which Schwann and Henle attached so much importance in animals, they gave no concurrence. Both Barry and John Goodsir had grasped and advocated the fundamental principle, both of the endogenous development of cells from a parent centre and of an organic continuity between a mother cell and its descendants through the nucleus; and the brothers Goodsir had applied this principle in their anatomical, pathological, and zoological researches.

As regards the physiological action of cells, Mr. (now Sir William) Bowman had expressed the opinion[1] that there was a strong presumption that the epithelium of glands assimilated the secretion from the blood; that the secretion might be separated, either by the passage of its elements through the cells, or by the cells undergoing solution or deliquescence, or by the cells being cast off entire with their contents. Mr. (now Sir John) Simon also expressed, in 1845, some important general conclusions on the physiological action of cells ("Essay on the Thymus Gland," London, 1845). He looked upon the cell wall as of secondary importance and of inessential formation, and he regarded the nucleus with the material developed around it as constituting the sole physical evidence of activity in the part. He saw bile and other secretions within cells, and stated that when the products of secretion can be seen within a cell, they are accumulated in the portion which corresponds to the nucleus as though it were the true centre of attraction. Simon also observed the development of spermatozoa within cells, and had seen one end adhering to the relique of a cell, probably its nucleus.

Histologists elsewhere had made isolated observations on the development in the animal body of young cells within parent cells. Even before the publication of Schwann's immortal treatise, Turpin had stated that the corpuscles which he found in vaginal discharges contained a new generation in their interior, and Dumortier had described secondary cells as formed in the ova of snails. These observations exercised, however, no influence on the progress of thought; and Schwann, though referring to them in the preface to his treatise, yet appeared to question their accuracy.

In 1841, Robert Remak published (*Medicinische Zeitung*, p. 127, July 7, 1841) an account of what he saw in the blood corpuscles of the chick, some of which were biscuit-shaped. At each end was a nucleus, and the two nuclei were connected together by a thin stalk which traversed the intermediate part of the corpuscle. He thought it probable from these observations that a multiplication of blood corpuscles through division occurred. He obtained also similar evidence in the blood of the embryo pig, and saw, both in the blood of the horse and of man, red blood-cells formed in the interior of large mother cells. It is customary in Germany to credit Remak with being the first to recognize the division of the nucleus within the cell as a stage antecedent to, and associated with, the division of the cell itself; but from what has already been stated, it will be seen that Martin Barry had preceded him by some months[1] in the recognition of the importance of division of the nucleus in the production of young cells.

In 1843, Albert von Kölliker published (*Müller's Archiv*, 1843) an interesting memoir on the changes which take place in the fertilized ova of various parasitic worms. He described and figured the production in regular progression of young cells within the ovum, and observed that in some cells the nucleus was elongated; in others constricted in the middle, as if about to divide; in others two nuclei were present, each smaller than the single nucleus of adjoining cells, as if they had arisen from the division of a larger nucleus. A legitimate inference from these observations was that, in the formation of young cells, the nucleus of the parent cell divided into two, and that each of these gave origin to a new cell.

The endogenous multiplication of animal cells by division of the nucleus now began to be more widely recognized. It was described by Kölliker and by Mr. (now Sir James) Paget in the blood corpuscles of the embryo, by Kölliker in cartilage and in the giant cells of the marrow of bones, and by various observers in the fertilized ovum. It acquired, therefore, much more importance as a mode of origin of animal cells than was accorded to it by Schwann.

At the time when I began the study of anatomy and physiology in 1850, the current teaching of the schools embraced two methods of cell-formation—the one through the intermediation of existing cells, which might be either by endogenous production within a mother cell through division of the nucleus, or by fissiparous division, or by budding off of a part of a cell; the other by a process of free cell-formation outside existing cells and within a blastema. When I came to Edinburgh in 1854 to act as Demonstrator of Anatomy, I found that the biologists were divided into two hostile forces—the one was presided over by Prof. John Goodsir, whose views on the intracellular origin of new cells I have already explained, and which he systematically expounded in his lectures; the other was led by the then Professor of the Institutes of Medicine, Dr. Hughes Bennett. Dr. Bennett, whose investigations into cell-formation and cell-life had been largely based, like those of Henle, on the study of pathological processes, was led to attach great importance to the granules or molecules which abound in the so-called inflammatory exudations and in purulent fluids. Bennett held that molecules arose in an organic fluid, and that an aggregation of molecules produced nuclei, upon which cell walls may be formed; that the molecule was the primary, elementary, and most simple form of organized matter, and that an aggregation of molecules might even form fibres and membranes without the agency of cells. His views were almost a reproduction of those of Henle, and he advocated them with great vigour and persistency, especially in regard to the production of pus and other products of inflammation.

Pathologists had indeed very generally supported the

[1] Article "Mucous Membrane," in Todd's "Cyclopædia," date probably 1842 or 1843.

[1] Barry's later memoirs were read to the Royal Society of London, May 7, 1840; January 7, 1841; June 17 and 23, 1841. They are illustrated with numerous beautiful figures, in which the division of the nucleus and the endogenous production of young cells are shown. Further, it should be kept in mind that Remak's observation was on a single tissue, the embryonic blood corpuscle; whilst Barry's was a generalization based on a large series of researches on the ovum, blood and mucous corpuscles, epithelium and other cells. John Goodsir, in a footnote to his important paper "On Centres of Nutrition," already referred to, says:—"For the first consistent account of the development of cells from a parent centre, and more especially of the appearance of new centres within the original sphere, we are indebted to the researches of Dr. Martin Barry." Remak subsequently extended his observations, on the multiplication of cells through division of the nuclei, to the ovum, and the cells of the tissues generally. See *Müller's Archiv*, 1852, p. 47, and "Untersuchungen über die Entwicklung der Wirbelthiere," 1855.

theory of the free formation of cells in exudations; but this view, however, was not universally entertained by them. Prof. Goodsir (*op. cit.*, 1845), and Dr. Redfern[1] had shown its inapplicability in inflammation and ulceration of articular cartilages. Prof. Virchow, in a series of papers in his *Archiv*, commencing with vol. i. in 1847, had described the endogenous formation of young cells in pathological structures. In his "Lectures on Cellular Pathology," published in 1858, Virchow, like Goodsir, announced his belief in the mapping out of the body into cell territories. Virchow's conception of the territory was the intercellular substance immediately surrounding a cell, and subject to its influence.[2] He maintained that in pathological structures there was no instance of development *de novo*, but that where a cell existed, there one must have been before. He called it the law of continuous development, which could be formulated in the expression *omnis cellula e cellula*. He adduced a great variety of specific instances to show the diffusion throughout the tissues and organs of nucleated cells, and he established, by a variety of proofs, the important part played by the cell elements, more especially those of the connective tissue, in the inflammatory process and in the production of new formations. He advanced, indeed, such a mass of evidence in support of this position, that the theory of free cell-formation was shortly after abandoned in connection with pathological processes, as it had been some time previously by most observers in normal histogenesis.[3]

(*To be continued.*)

THE CAUSES OF ANTICYCLONES AND CYCLONES.

A MEMOIR presented to the Vienna Academy of Sciences on April 17 last by Prof. J. Hann, giving the results of his study of an anticyclone which lay over Central Europe from November 12 to 24, 1889,[4] brings to a climax one of those investigations that rank as landmarks in the advance of science, and compels us to modify in some important particulars the views now generally current on some of the leading phenomena in meteorology. Next to the facts of the general circulation of the atmosphere, which, in recent years, have been treated of more particularly by Ferrel, Hann, Siemens, Sprung, Oberbeck, and Pernter, the relations between areas of high and low pressure, or anticyclones and cyclones, have played a chief part in the science of atmospheric movements; and indeed in that large and popular department that deals with the weather and its vicissitudes, they may be said almost to monopolize the field. Hitherto, however, excepting in so far as the movements of the clouds afford us any information of the changes in progress in the higher atmosphere, our experiential knowledge of cyclones and anticyclones has been almost restricted to what can be observed within a small distance of the general land-surface. As a rule, a region of high barometer, especially in the winter, is one of low surface temperature, while cyclones, which originate in regions of low pressure, are fed by warm southerly winds. Interpreting these facts by the light of well-known physical laws, it has become the common teaching of our text-books that the former are due to the low mean temperature and therefore increased density of the superincumbent air column, while the latter are brought about by the opposite conditions. The correctness of these views, in so far, at least, as regards anticyclones, was challenged by Dr. Hann as long ago as 1875, in a paper published in the Vienna *Zeitschrift* (vol. x. p. 210), wherein he showed that, as a result both of theory and observation, the cold that prevails in a region of high barometer in winter is really due to terrestrial radiation under the clear skies that are characteristic of such an area, that it is restricted to a stratum of very moderate thickness, and that above this the compression of the sinking atmosphere must induce a high temperature, and consequently greatly reduce its density.

In a subsequent, very suggestive paper, published in 1879 in the fourteenth volume of the same periodical, he discussed more fully the causes of anticyclones, and concluded that they are essentially the same as those which give rise to the two sub-tropical zones of high barometer—viz. the congestion of the upper or anti-trade currents, directed polewards and eastwards, which, owing to the rapid contraction of the circumpolar zones in high latitudes, are partially arrested and forced to return in a lower stratum of the atmosphere. He also expressed the opinion that these areas of congested currents determine the formation of travelling cyclones in the intervals of relatively low pressure, instead of being themselves caused by the overflow of the upper currents from the latter (which is Ferrel's view). Hence that both anticyclones and cyclones have their origin in the circumstances of the general atmospheric circulation, and are, in neither case, primarily due to the heating or cooling of that part of the earth's surface which they temporarily occupy. Some further consequences of high importance were pointed out in this essay. Since the general circulation of the atmosphere is determined by the expansion of the air over the equatorial zone, and the consequent tilting of the planes of equal pressure to form a gradient between the equator and the two poles, the greater frequency of stormy weather in the higher latitudes in winter was shown to follow from the increased activity of the higher or anti-trade currents; the difference of temperature between equatorial and polar and sub-polar regions being at that season at their maximum; and not merely to the contrasted conditions of continents and oceans. Also that any cause tending to increase the heating and expansion of the equatorial atmosphere must intensify both the anticyclonic and cyclonic movements of the temperate and sub-polar zones. In another paper, published in the fifteenth volume of the *Zeitschrift*, he pointed out that this last view received confirmation from the fact, then recently ascertained, that those years in which the barometer ranged below the average in the Indo-Malayan region were years of excessive barometric pressure in winter (but not in summer) in Western Siberia and Russia.

From time to time, as occasion has served, Prof. Hann has continued to verify these views, by investigating the temperature conditions of anticyclones, on the evidence afforded more particularly by the high Alpine and other mountain observatories. He has thus shown that the relatively high temperature prevailing at high levels during periods of intense winter cold at low levels is no exceptional occurrence, but a constant and characteristic feature of anticyclonic conditions. Moreover, that, as a general fact, the temperature at mountain observatories in winter rises and falls directly with the barometric pressure at those elevations, while the reverse holds good at the general ground surface. In summer, the lowest temperatures at mountain observatories coincide also with the lowest pressures at the ground surface; and this he explains partly by the fact that a low barometer is accompanied with stormy and rainy weather, and with snow at the greater elevations, and partly by the dynamic cooling of the ascending air over the region of minimum pressure. The Alpine observatories are, however, less favourably situated for

[1] "Abnormal Nutrition in Articular Cartilages," *Edinburgh Monthly Medical Journal*, August 1849: and separate memoir, Edinburgh, 1850.

[2] He first used the term "Zellen Territorien" in his *Archiv*, Bd. iv., 1852, p. 383.

[3] In a lecture which I delivered before the Royal College of Surgeons, Edinburgh, in 1863 (*Edinburgh Medical Journal*, April 1863), I summarized the evidence of the derivation of pathological cell formations from pre-existing cells, and adduced additional examples from my own observations.

[4] "Das Luftdruck Maximum von November 1889 in Mittel Europa, nebst Bemerkungen ueber die Barometer-maxima in Allgemeinen," von J. Hann, W.M.K. Akad.

observing the conditions of cyclones than of anticyclones, since the Eastern Alps lie away from the ordinary tracks of these storms, and are but seldom traversed by their vortices.

In his latest memoir, Dr. Hann describes and compares two striking instances: one of a prolonged period of barometric maximum (from November 12 to 17, 1889), and one of a barometric minimum, on October 1 of the same year. Both of these included the Eastern Alps, and thus afforded an unusually favourable opportunity for contrasting their accompanying temperature conditions up to an elevation of over 3000 metres. The result is that, notwithstanding that the anticyclone occurred six weeks later in the year than the barometric minimum of October 1, the mean temperature of the anticyclonic air-column up to a height of 3 kilometres, was certainly more than 2° C. higher than that of the antecedent minimum. The further conclusions may be given in the words of the memoir.

"That this result holds good, not only for the barometric minimum of October 1, 1889, but as a general fact (the temperatures in both cases being regarded as deviations from the normal of the season), is in itself probable, and has been fully established in my investigation of the temperature of the summit of the Sonnblick during periods of high and low pressure at the earth's surface. One result of this was that the cyclones of the summer half-year bring about a great cooling of the air-column of at least considerably over 3000 metres in height, and cause the greatest depressions of temperature that occur in the summer generally. The mean temperature of the whole air-column in a summer cyclone, from the ground up to a height of certainly over 5000 metres, is lower than in an anticyclone. It is probable that this holds good for winter cyclones also, if their temperature be compared with that in the centre of an anticyclone. The warmth which accompanies winter cyclones at the earth's surface, and which was assumed to characterize the whole air-column, is restricted chiefly to the lower atmospheric layers; the observations on high mountains show that the greatest warmth is always brought by anticyclones; the higher the mountain, the more pronounced is this result.

"At very great elevations, above the cirrus level for instance, the temperature differences of cyclones and anticyclones may again be reversed; possibly, however, not so. For either of these alternative views plausible grounds may be assigned. But this much is certain, that the theory of the causes of cyclonic and anticyclonic movements of the atmosphere must take count of the fact that up to heights of at least 4 or 5 kilometres, the air temperature in the heart of an anticyclone may be (and perhaps always is) higher than that in the centre of a cyclone.

"Thus fall to the ground the views of those who have sought for the cause of these movements in the different specific gravities of the air in cyclones and anticyclones; in the 'upcast' to which the air must be subject in a cyclone. . . .

"So long as the observed temperatures were those only of the earth's surface, one fell almost necessarily into this error, which was so natural and apparently explanatory. Where cold air lay on the earth's surface, there we found high pressures, and *vice versâ;* what could be more self-evident than that the temperature of the air-column was the determining cause of the pressure? It was the observations of mountain observatories, those of peak-summits, that first set us free from this error; and we must now conclude that the temperature conditions of wandering cyclones and anticyclones are the effect and not the cause, that they are the consequence of the movements of the air-masses, of the ascents and descents of the vertical circulation of the atmosphere. There can no longer be any doubt that the pressures in barometric maxima and minima generally are to be explained mainly through these movements of the air. The forces which set up the atmospheric circulation of the higher latitudes, especially in winter, have their origin in the warmth of the tropics—that is to say, in the difference of temperature between the polar regions and the equatorial zone. Cyclones and anticyclones are but partial phases in the general circulation of the atmosphere. The air-currents that set towards the poles as a consequence of the upper gradients are partially resolved in vortices in the higher latitudes, and their progressive movement is chiefly determined by the prevailing westerly direction of the wind currents. The influences of variations of the terrestrial surface, of the heating and cooling of continents and oceans, as well as of the local influx of water-vapour and its condensation, are but of secondary importance. They may however strengthen or destroy the ascending or descending eddies, and modify their paths and their rate of progression.

"These views are such as I have always enunciated (for a long time, indeed, without any apparent result) in opposition to the then prevalent theories of the local origin of barometric minima through the agency of condensing water-vapour (as contended by Mohn, Reye, Loomis, and Blanford). They now begin to make way and to prevail. Most clearly is this seen in the case of Loomis, who, in the course of his own persistent study of the behaviour of barometric minima and maxima, has been compelled by degrees to give up the 'condensation theory' to which he formerly adhered so strongly, and to ascribe the origin as well as the progressive movement of cyclones to the general circulation of the atmosphere."

After a cursory recapitulation of some of the leading demonstrations of his previous writings, to which brief reference has been made above, Prof. Hann concludes:—

"This theory is not merely deductive, nor is it put forward simply as a speculation. On numerous occasions I have demonstrated step by step how it agrees with observation in all its details, so that I may fairly claim the right of priority for its establishment."

This claim will doubtless be readily admitted, and it adds one more to the many great services which its author has already rendered to the cause of physical meteorology, and which have long since won for him his universally acknowledged place in the forefront of modern meteorological science. As regards the genesis of anticyclones, for the study of which the Sonnblick, Hoch Obir, and Santis Observatories have afforded him numerous opportunities, which he has turned to the best account, Prof. Hann's conclusions appear to be unassailable. And in respect of the cyclones or barometric minima of the temperate and sub-Arctic zones, although the evidence is perhaps less decisive, and its conclusiveness may possibly yet be challenged in some particulars, it must at least be conceded that his arguments are entitled to much weight; and the facts adduced greatly weaken, if indeed they do not altogether destroy, the validity of the views hitherto prevalent. For my own part, I am quite prepared to admit the probability that these barometric minima are, as he contends, in their origin, great eddies in the higher atmosphere, and are not determined by the high mean temperature of the air-column over the spot in which they first appear.

But I cannot admit that these conclusions can be extended to the case of tropical cyclones. Prof. Hann does not indeed expressly claim such extension; but, on the other hand, he does not expressly limit their application to the storms of extra-tropical latitudes, and from the fact that, in a paper recently published in the *Meteorologische Zeitschrift*,[1] he discusses the conditions of both these classes of cyclones without insisting on any fundamental distinction between them, it must, I think, be in-

[1] "Bemerkungen über die Temperatur in den Cyclonen und Anticyclonen," *Met. Zeitschr.*, Heft 9, September 1890.

ferred that he contemplates as at least a high probability that they originate from like causes. Moreover, in the paragraph quoted above, he refers to myself as an upholder of the condensation theory, in terms that seem to imply that he regards me as an opponent, the fact being that I have never contended that this theory is applicable to the case of other than tropical cyclones, or, to be exact, of other than those of Indian seas. To this contention I must still adhere; but as the discussion of the question would unduly extend the limits of this notice, I reserve it for another occasion. HENRY F. BLANFORD.

ON THE ANATOMY AND DEVELOPMENT OF APTERYX.

IN a paper read before the Royal Society on April 17 of this year, Prof. T. Jeffery Parker, F.R.S., of Otago University, gives an account of his researches on the anatomy and development of the Kiwi (*Apteryx*) which are of especial interest, as so few detailed observations have been recorded on the development of any of the flightless birds (*Ratitæ*). Moreover, the comparisons which are given of the different species of *Apteryx;* the account of the sexual differences, and the variations seen within the same species; and the tables showing the relative proportions of the various regions of the body in different stages of development, illustrating the "law of growth," add greatly to our knowledge of this remarkable genus. A number of new terms are proposed in the description of the skeleton, and a new method of writing the vertebral formula of birds is adopted. Notes are given with regard to the presence of uncinate processes and to the structure of the foot in *Dinornis*.

The chief materials on which the investigation is based consist of a number of embryos of the three common species of *Apteryx*, which naturally group themselves into ten stages (A–K); an eleventh stage (L) is furnished by a bird a few weeks old, a twelfth (M) by the skeleton of an adolescent specimen, and a thirteenth (N) and fourteenth (O) by odd bones of young birds; the adult may be considered as constituting a fifteenth stage. The embryos were, for the most part, well preserved, but not sufficiently well for the purposes of exact histological study. The single embryo belonging to stage A corresponds in most respects to a chick of the fourth day.

The paper is illustrated with over 300 figures, and gives so many technical details as to the structure and development of the skeleton, and as to the muscles of the wing, the brain, and the eye, that it is impossible here to give anything approaching a satisfactory abstract of the whole, which will appear shortly in the Philosophical Transactions. The chief results of more general interest, as bearing on the phylogeny of *Apteryx* and of the Ratitæ generally, may be briefly summarized as follows.

In stage A, the limbs have already attained their permanent position, so that, if the backward shifting of the appendages so noticeable in the chick occurs in *Apteryx*, it must take place at an unusually early period. In stage C, corresponding with a sixth-day chick, there is a well-marked operculum growing backwards from the hyoidean fold, and covering the third (? and fourth) visceral cleft. A rudiment of this structure is seen in the preceding stage.

From the first appearance of the feather papillæ there are well-marked pterylæ and apteria, most of which can be made out with tolerable distinctness in the adult.

The wing of the adult has a well-marked pre- and post-patagium, and amongst its feathers may be distinguished nine or ten cubitals, two or three metacarpals, one mid-digital, and a row of tectrices majores. The barbicels of the feathers are slightly curved. The fore-limb passes through a stage in which it is a tridactyle paw with subequal digits, followed by one (stage F) in which it is a typical wing with hypertrophied second and partially atrophied first and third digits. The variability of the muscles of the wing is noteworthy, and the evidences of degeneration are very clear; a number of wing-muscles, not mentioned by Owen, are described.

The nostril has acquired its final position at the end of the beak in stage E; up to the middle of incubation the whole respiratory region of the olfactory chamber, from the anterior nares to the commencement of the turbinals, is filled with a solid mass of epithelial cells, through which a passage is formed at a later period. The turbinals are unusually well developed. A pecten is present in the eye during late embryonic life. At no stage is there any trace of the caruncle or "egg-breaker" at the end of the beak.

As regards the skull, it may be mentioned that the head of the quadrate is provided with two articular facets; no intertrabecula could be observed; there is no interorbital septum; Jacobson's cartilages are present; and the hyoidean portion of the tongue-bone ossifies late, and is obviously degraded.

The vertebral column and hind-limb are typically avian, both as regards structure and development, and these typical characters appear early in the pelvis. There is a pygostyle. The sternum and shoulder-girdle, as well as the wing, are very variable, indicating degeneration; their position in stage E resembles that seen in Carinate birds. Vestigial acromial, procoracoid, and acrocoracoid processes are present, the procoracoid being well marked in comparatively late embryonic life. There is no trace of clavicles. A vestigial keel is occasionally present in the sternum; but before considering the peculiarities in the development of the sternum as of fundamental importance, it will be necessary to study that of the flightless Carinatæ, and especially of *Stringops*.

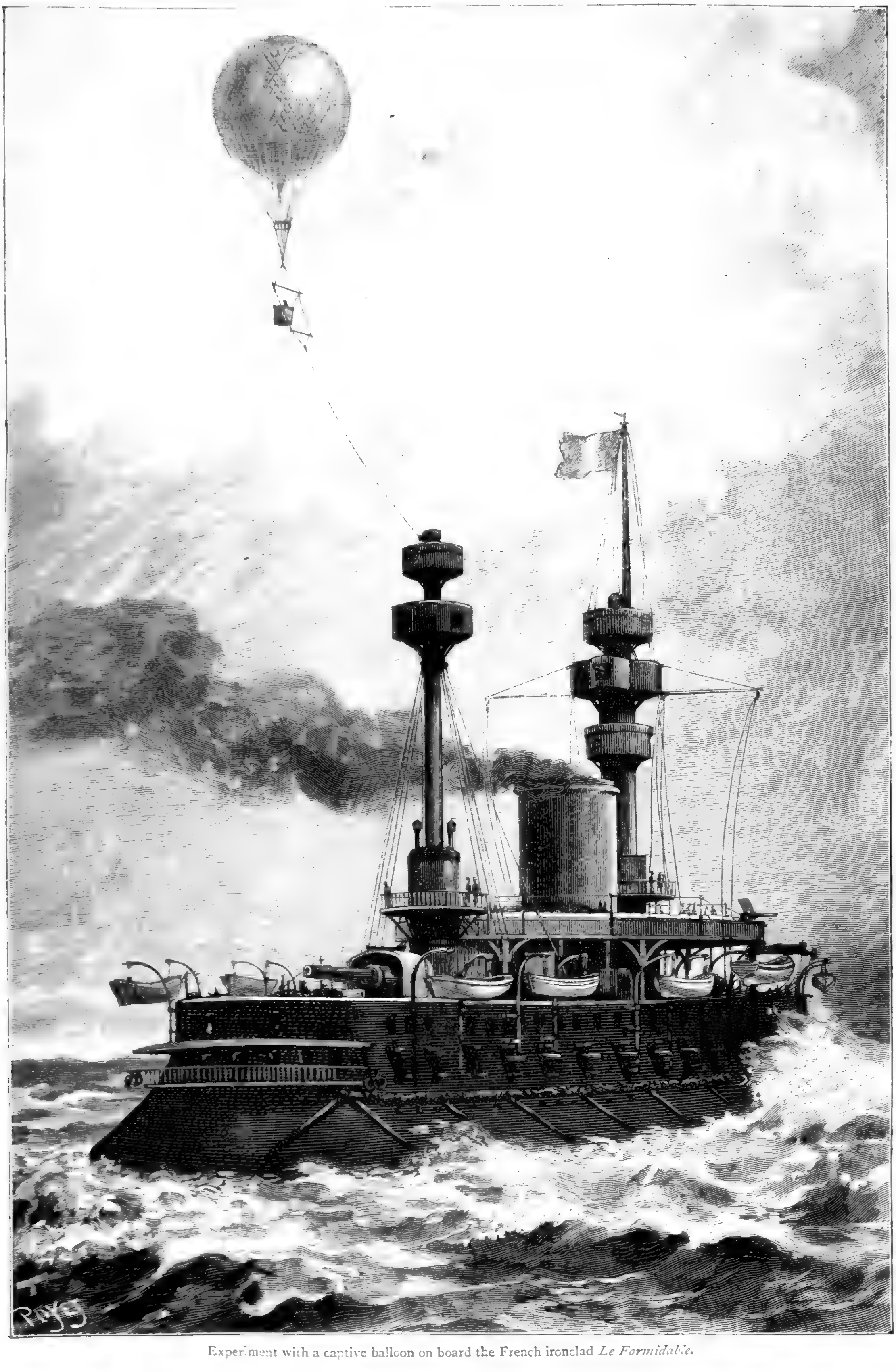
The brain passes through a typical avian stage with lateral optic lobes. The mesencephal is unusually small from the first; in stages D—F the optic lobes are dorsal; in G they become lateral by the transverse extension of the optic commissure or median portion of the roof of the mesocœle; in H they are already ventral, although larger proportionally than in the adult. The diencephal becomes tilted backwards in later stages, its dorsal wall becoming posterior, and the foramen of Monro postero-dorsal instead of antero-dorsal. The anterior commissure and corpus callosum are large. The cerebral hemispheres are of unusual proportional length, and partly cover the cerebellum.

The greater number of the characters enumerated support the view that *Apteryx* is derived from a typical avian form capable of flight;[1] but on the other hand, the total absence of rectrices tells against this theory. Many characters, again, indicate derivation from a more generalized type than existing birds; while in other points *Apteryx* exhibits greater specialization than other birds. The general balance of evidence seems to point to the derivation of both Ratitæ and Carinatæ from an early group of typical flying birds or *Proto-Carinatæ*.

CAPTIVE BALLOONS.

SOME important experiments with captive balloons have lately been made in the Mediterranean squadron of the French navy. By the courtesy of the editor of *La Nature*, we are enabled to give, on the next page, a representation of the most interesting of these experiments, which was made on board the ironclad *Le Formidable*. All the officers who mounted in the car declare that it afforded an excellent point of observation. In clear weather they could distinguish, from Lagoubran, all the details of the coast from the entrance to Mar-

[1] The attitude assumed during sleep also supports this view.



Experiment with a captive balloon on board the French ironclad *Le Formidable*.

seilles to the eastern extremity of the Islands of Hyères. No ship within a radius of from 30 to 40 kilometres could have escaped observation. With a cable of silk, the balloon could rise in calm weather to a height of 400 metres.

It is evident from the success of these experiments that captive balloons may be a most important aid to those who hereafter make use of them in naval warfare. The subject has attracted the attention of the naval authorities in Germany, and at Wilhelmshaven a captive balloon was sent up recently from the *Mars*. We are glad to learn that the English Admiralty has taken up the question.

THE COCO-DE-MER IN CULTIVATION.

WITH only one exception, the palms of the Seychelles have long since proved amenable to cultivation in our tropical plant-houses. The genera *Stevensonia*, *Verschaffeltia*, *Roscheria*, *Latania*, *Dictyosperma*, *Acanthophœnix*, *Hyophorbe*, and *Chrysalidocarpus*, which are peculiar to this small group of islands, and which rank amongst the noblest of a noble family, are all well known in European collections of palms, their cultivation presenting no more difficulty than that of tropical plants generally. The coco-de-mer or double cocoa-nut (*Lodoicea seychellarum*) has, however, so far proved unmanageable under artificial treatment, notwithstanding that many attempts have been made to establish it at Kew and elsewhere. So long ago as the year 1827, Sir William Hooker published a series of figures and a description of the coco-de-mer in the *Botanical Magazine*, and recorded the arrival of living nuts of it at Kew, where, he says, "we cannot doubt of soon seeing them flourishing in our stoves." But they failed to grow, and although dozens of nuts have since been tried at Kew, not one ever got beyond the first stage of germination.

The absence from our collections of living examples of this most remarkable palm is most disappointing to all students of the order. At Kew we have lately been successful in establishing living plants of the Ita (*Mauritia flexuosa*) and Bussu (*Manicaria saccifera*) palms of the Demerara swamps, and the Doum (*Hyphæne thebaica*) and Palmyra (*Borassus flabelliformis*) palms of Africa. These successes stimulated the desire once more to obtain a living plant of the coco-de-mer.

Application was therefore made in January last year, through the Secretary of State for the Colonies, for a supply of fresh nuts from the Seychelles, and at the same time directions for packing and forwarding the nuts were sent to Mr. C. Button, the Conservator of Forests at those islands. The Administrator, Mr. T. Risely Griffith, took a warm interest in the matter, and through his kind exertions several consignments of nuts were received, of which four germinated. Two of these are probably too weak to live, but the other two are in a most promising condition. The strongest has a radicle 3 feet 8 inches long, and 12 inches in circumference at the end where the plumule is developed. This is now a foot long, and is pushing a perfect leaf.

In a note by the late General Gordon on the germination of the double cocoa-nut, it is stated that the nut is planted horizontally, without the husk, when it sends out a sprout some 12 feet long, which pushes up the young plant at a distance of 12 feet from the nut. The longest "sprout" we have had at Kew has not exceeded 4 feet. Nor can it be made to grow horizontally, the point turning down perpendicularly however often its position may be altered. At Kew the nuts were planted in a bed of cocoa-nut fibre, and kept at a temperature of 80°–85° F. They were planted in June 1889.

Mr. Button had kindly undertaken to plant a nut in a Wardian case, and treat it according to our instructions until it had germinated and developed the plumule before despatching it to Kew. A nut thus treated arrived in July last in the most promising condition. The radicle is 1 foot 10 inches long, and the plumule is 7 inches in circumference at the base. It has a stout sheath-leaf, and a normal leaf 3 feet 2 inches long, 3 feet wide, with thirty-six folds. The midrib is curved, and the blade at present folded double. The texture is exceptionally firm, and the colour a deep green.

Full-sized trees of the coco-de-mer attain as much as 150 feet in height, with a smooth trunk about a foot in diameter. The leaves form an immense crown on the top, and each leaf is 20 feet long and 10 or 12 feet wide. The male and female flowers are on separate plants: the male inflorescence is shaped like a huge willow catkin, its length being 5 to 6 feet by 4 inches in diameter; the female is from 2 to 4 feet long, and it bears from six to ten fruits, each of which weighs from 25 to 30 pounds. They take seven years to mature, and sometimes hang two years on the tree after they are ripe. The process of germination extends over about two years. According to General Gordon, the trees begin to fruit when about forty years old, and attain maturity in 120 years.

Royal Gardens, Kew. WILLIAM WATSON.

[The coco-de-mer is at present confined to Praslin and Curieuse, two of the islands of the northern group of the Seychelles Archipelago. It undoubtedly runs some risk of extinction from the long period which the nuts take to germinate, and from the fact that, the trees being of different sexes, isolated females may easily escape fertilization. Its cultivation in the Botanic Gardens of the tropics is therefore of considerable importance.

Plants have long flourished in the Royal Botanic Gardens at Peradeniya, and the following extract from a letter from the Director, Dr. Trimen, F.R.S., to Kew, records the interesting circumstance of a male plant having flowered:—

"*Peradeniya*, August 12, 1890.

"You will be interested to hear that one of our Lodoicea palms put out a ♂ inflorescence last month. The tree is thirty-nine years old. To my great disgust, when the spike was about 6 inches long, some visitor cut it off with a blunt knife, and I found it on the ground. The flowers were all formed, and the structure exactly as described by Sir W. Hooker in the *Botanical Magazine*. I hope my other tree will prove ♀, but that is much younger."

Sir John Kirk also succeeded in establishing the palm in his garden at Zanzibar.

The Government of the Seychelles has long watched with care the preservation of the existing groves of the palm, and pains are now taken to fertilize the female plants artificially, and to plant the seeds.—W. T. T. D.]

NOTES.

WE have to announce the death of Pierre de Tchihatchef, which took place at Florence on the 13th ultimo. This gentleman was perhaps best known as a botanist, though his principal literary work, "Asie Mineure: Description Physique, Statistique, et Archéologique de cette Contrée," took a much wider range. Prior to 1857, he travelled ten years in Asia Minor and Armenia, and, besides the work named, he published a large number of separate papers on a variety of subjects, chiefly however on botany and geology, commencing in 1840. Like so many Russians, he appears to have been an accomplished linguist, and wrote German and French with equal facility. He resided some years in France, and was one of the original members of the Botanical Society of France, founded in 1854. His "Botany of Asia Minor" forms the third part of the work named above, and consists of two volumes of letterpress, and a volume of plates by Riocreux. Pierre de Tchihatchef was also the author of an admirable French translation of Grisebach's "Vegetation der Erde." But this was something more than a translation, for it was cast in a better mould than the original, and contained much new matter, including an essay on the geological formation of oceanic islands.

WE regret to have to record the death of Dr. Alexander John Ellis, F.R.S. We reprint from the *Times* the following notice of his career:—Dr. Ellis, whose original name was Sharpe, died at his residence in Auriol Road, West Kensington, on October 28. He was born in Hoxton in 1814, and educated at Shrewsbury, Eton, and Trinity College, Cambridge, of which he was elected a scholar in 1835, and graduated B.A., being sixth wrangler and first in the second class in classics, in 1837. He was elected a Fellow of the Cambridge Philosophical Society in 1837, of the Royal Society in 1864 (being a member of the Council for 1880–82), of the Society of Antiquaries in 1870, of the College of Preceptors in 1873, and a life governor of University College, London, in 1886. He was President of the Philological Society during 1872–74, and also 1880–82. He was also a member of the Mathematical Society of London, of the Royal Institution, of the Society of Arts, and honorary member of the Tonic Sol-Fa College. Dr. Ellis was a voluminous author, his works including "The Alphabet of Nature," 1845; "Essentials of Phonetics," 1848; "Plea for Phonetic Spelling," 1848; "Universal Writing and Printing," 1856; "Early English Pronunciation, with special reference to Chaucer and Shakespeare," 1869–86; "Glossic," 1870; "Practical Hints on the Quantitative Pronunciation of Latin," 1874; "On the English, Dionysian, and Hellenic Pronunciation of Greek," 1877; "Pronunciation for Singers," 1877; "Speech in Song," 1878; together with numerous other works and tracts on music and phonetics. He received the silver medal of the Society of Arts for three papers in connection with the "Musical Pitch" at home and abroad.

DR. F. R. JAPP, F. R.S., Assistant Professor of Chemistry in the Normal School of Science, South Kensington, has been elected Professor of Chemistry at the University of Aberdeen.

AT the Royal Institution of Great Britain, on Monday, Mr. Victor Horsley, F.R.S., was elected Fullerian Professor of Physiology for three years.

THE Secretary of State for India has appointed Mr. Arthur W. Thomson, C.E., B.Sc., of the Glasgow and West of Scotland Technical College, to be Professor of Mechanism and Applied Science in the College of Science, Poona.

WE are glad to learn that the accommodation at the disposal of the Botanical Department in the University College, London, has been greatly augmented. Hitherto, all the botanical work, other than lectures, has been confined to the single general laboratory in the north cloister. In the adjacent Birkbeck building, from which the school of technological chemistry has been transferred to another portion of the College, several rooms have now been set apart for the various branches of botanical teaching; and the room in the north cloister has been fitted as a museum and general botanical laboratory. During the building operations, just concluded, the workmen found three large chests in which Prof. Lindley (who died in 1865) had stowed away a series of fossil types, representing the chief genera of plants occurring in the Coal-measures. This collection is, of course, a valuable accession to the botanical museum.

ON Monday the Corporation of Brighton obtained formal possession of the museum in Dyke Road, containing the collect tion of British birds formed by the late Mr. E. T. Booth, and bequeathed by him to the town. A large assembly, including some specially invited men of science from London, gathered on the occasion. The key of the building having been handed to the Mayor of Brighton, Alderman Manwaring, he said that he trusted the collection was the beginning of such a natural history museum as no other town in the kingdom could boast of possessing. The gathering was addressed by Prof. Flower, of the British Museum, who said the collection was in many respects unrivalled in the kingdom. The homes in which the birds dwell were carefully and accurately reproduced in a manner that had never before been achieved. In that museum some of the specimens of taxidermy were very fine. All were above the average, and he did not believe there was a single bad one among them. It would have been a national calamity for such a collection to be dispersed or destroyed, and when it was offered to the British Museum he should have advised the Trustees to take it over had it not been intimated to him that the Corporation of Brighton were willing to take it and maintain it for the future benefit of mankind. Though it would have been a great privilege to him to be its official guardian, he rejoiced to find it was going to remain in Brighton, where it was formed, and in the neighbourhood of which many of the specimens were obtained.

THE following have been elected as officers of the Cambridge Philosophical Society for the ensuing year: President:—Prof. G. H. Darwin, F.R.S. Vice-Presidents: J. W. Clark, Trinity; Prof. Babington, F.R.S., St. John's; Prof. Liveing, F.R.S., St. John's. Treasurer: R. T. Glazebrook, F.R.S., Trinity. Secretaries: J. Larmor, St. John's; S. F. Harmer, King's; E. W. Hobson, Christ's. New Members of Council: Dr. Alex. Hill, Downing; Dr. A. S. Lea, Caius; A. Harker, St. John's; L. R. Wilberforce, Trinity.

THE Meteorological Council have published the meteorological observations made at stations of the second order (*i.e.* observations taken at 9h. a.m. and 9h. p.m. each day) for the year 1886. The present volume differs in several important particulars from those of previous years: the distribution of stations is much more complete, for, although the number for which observations in detail are published has been reduced, on the other hand the summarized observations have been considerably increased, and include the records from the observatories in connection with the Office. Some alterations will also be found in the tables, both as regards the information given and the form in which it appears: the barometer observations are no longer reduced to sea-level, owing to the uncertainty which attaches to the formula for reduction when the height of the station is considerable. In the hygrometrical values, the differences between dry- and wet-bulb readings are given under the heading of "Depression of Wet Bulb." In other respects, the general plan of the publication is the same as in previous years, and includes observations made at some selected stations of the Royal and Scottish Meteorological Societies.

IN the *Bollettino Mensuale* of the Italian Meteorological Society for September, Prof. P. Busin publishes the results of his discussion of the diurnal probability of rain, calculated from long series of observations for three of the principal Italian cities, obtained by dividing the number of days of rain in a given period by the number of years of observation for the same period. He concludes that the tables show that barometric depressions do not bring rainy weather, or anticyclones fine weather, so frequently as generally supposed, and that such tables are more to be relied on by agriculturists than telegraphic forecasts of rain, owing to the variability of this element in adjacent localities. Although we cannot entirely agree with this view, there can be little doubt that such investigations may be valuable aids to the study of weather changes, if used in connection with telegraphic information of existing conditions.

WE have received two pamphlets on the "Aurora" and "Forces concerned in the Development of Storms," by Mr. M. A. Veeder. In the former he finds that the phenomena depend on the rotation of the sun, and he mentions the remarkable coincidence in time of their sudden appearance and gradual fading with the solar disturbances that appear on the sun's eastern limb. He also suggests that thunderstorms "may be a reciprocal or alternative method of manifestation of forces, which, under other conditions, find their expression in the aurora." In the latter pamphlet, the working hypothesis adopted is that the distribution of atmospheric pressure as a whole may be determined to an important extent by the fact that the earth is a magnet, and that its magnetic properties are variable. In fact, convection-currents are of secondary importance, for he says that "the bringing of warm air from the tropics, or the bringing of cold air from the polar regions, is the effect, and not the cause, of the redistribution of pressure." Thus, if these magnetic forces, associated with magnetic induction from the sun, influence the atmosphere and mass it together in any particular way, equilibrium is maintained as long as these forces do not vary, but, as soon as they do, readjustment sets in, and eddies, storms, &c., are the result.

THE latest Report on the economical condition of Switzerland, from the British Legation at Berne, refers to the subject of technical education in that country. Subventions to the amount of £12,854 were granted by the Federal Government during the past year to the various technical schools existing in the different cantons. Among the more important of these schools is the Technikum at Winterthur, the various silk and cotton weaving schools in the cantons of Zürich, Basle, &c., and the schools of horology in the cantons of Geneva, Berne, and Neuchâtel. The Federal Government have, moreover, on more than one occasion been invited to consider the question of subsidizing schools of commerce, and applications for financial assistance have been addressed to them by the cantons of Geneva and Zürich, in which schools of this nature are already established. The Federal Council, while admitting that it is their duty to encourage in every way the establishment of schools in which youths may be trained for the various branches of commercial life by an education especially adapted to that end, have nevertheless decided that they would not be justified in thus adding to the expenses of the confederation until the equilibrium has been restored in the Federal budget.

WITH reference to our note, last week, about scientific guide-books, a correspondent writes:—"For Switzerland I should like to recommend the 'Botanist's Vade-Mecum,' published by the 'Librairie Saudoz' of Neuchâtel. It was compiled by Mr. Paul Morthier, Professor of Botany at the Neuchâtel Academy, and President of the International Society of Botanists. It has already passed through two or more editions, and the price is about 2*s.* 6*d.*"

MESSRS. CASSELL have issued, for the National Association for the Promotion of Technical and Secondary Education, a "Guide to Evening Classes in London." This compilation ought to be of great service to what is now, happily, a large class of students. Full information is given as to all the chief classes, elementary and advanced, that are to be held during the coming winter in the capital.

A SUPPLEMENT to the third volume of the *Internationales Archiv für Ethnographie* has been issued. It consists of some interesting ethnographical notes, by Dr. Max Weber, on Flores and Celebes. The paper is admirably illustrated.

A WEEKLY review, entitled *L'Université de Montpellier*, is about to be issued at Montpellier. It will contain information concerning the University; an abstract of the most interesting lectures in science, literature, and art; and original papers by men of science in the town and neighbourhood.

THE following is a list of the edible fungi exhibited for sale in the market of Modena during 1889:—*Amanita cæsarea, A. ovoidea, A. strobiliformis, Lepiota excoriata, L. naucina, Armillaria mellea, Pleurotus ulmarius, P. glandulosus, Entoloma Rhodopolius, Pholiota mutabilis, P. Aegerita, Psalliota campestris, Morchella esculenta, M. conica, M. rimosipes, Helvella monachella, H. crispa, Peziza vesiculosa, P. cerea, P. Acetabulum, Tuber magnatum, T. æstivum, Balsamia vulgaris.* By far the greater number of these species are also natives of Britain.

M. KUZNETSOFF, who has spent several years in the study of the flora of the Caucasus, sets forth in the last issue of the *Izvestia* of the Russian Geographical Society the following interesting conclusions:—The flora of the Kutais and Tchernomorsk regions, on the eastern coast of the Black Sea, belongs, as already known, to the Mediterranean region of evergreen trees. Next comes the region of West European flora, characterized by the extension of the beech-tree, and offering on the slopes of the mountains the very same subdivisions as we are accustomed to see in the Alps. That region extends over the provinces of Kuban and Terek as far east as the water-parting between the Terek and Sulak rivers. The territory to the east of it was formerly thought to have a flora more akin to that of Asia, but a distinctly European flora appears again on the eastern slopes of the Daghestan plateau turned towards the Caspian Sea; while the dry Daghestan plateau itself has a flora decidedly recalling that of the highlands of Central Asia. M. Kuznetsoff explains these differences by the moister climate of the Caucasus highlands, due to the proximity both of the Black and of the Caspian Sea. But it may also have a deeper cause. In fact, the plateaus of Daghestan cannot but appear to the orographer as a continuation of the geologically oldest plateaus of Asia Minor, now separated from the main plateau by the relatively much younger chain of the Caucasus. Referring to the vegetation of the Caucasus during the Tertiary epoch, when the Caucasus was a vast island surrounded by Tertiary seas, M. Kuznetsoff considers that the flora of Daghestan has undergone the greatest change since the Tertiary epoch. The floras of both the Western and the Eastern Caucasus have maintained more of their old characters, owing to less change having gone on in their climate, which has remained moist; and the vegetation of the Black Sea coast, which has a climate very much like that of the Japan archipelago, has retained still more of the aspects it had during the Tertiary epoch. Further exploration will be necessary to show how far climate alone can account for the present characters of the flora of Caucasus.

FURTHER details are given by Prof. Curtius in the current number of the *Berichte* concerning his new gas, hydrazoic acid, N_3H. Since the first announcement, a better and much readier mode of preparing the gas has been discovered. Instead of reacting with hydrazine hydrate upon benzoylglycollic acid, it is found much more convenient to commence by preparing the hydrazine derivative of hippuric acid,

$$\begin{array}{l} C_6H_5 \\ | \\ CO \\ | \\ NH \\ | \\ CH_2-CO-NH-NH_2 \end{array},$$

a substance much more readily obtained. This compound is converted into its nitroso-derivative,

$$C_6H_5-CO-NH-CH_2-CO-N\begin{matrix}\diagup NO \\ \diagdown NH_2\end{matrix},$$

by treatment with sodium nitrite and acetic acid at a temperature about 0° C. Nitroso-hippurylhydrazine is much more permanent than the corresponding nitroso-compound of benzoylhydrazine, used in the earlier experiments, and the yield is 90 per cent. of the theoretical. The well-washed crystals of this nitroso-compound are next dissolved in dilute caustic soda, and the solution warmed for a short time upon the water-bath. The alkaline solution is afterwards placed in a flask connected with a condenser and fitted with a dropping funnel. Dilute sulphuric acid is now allowed to slowly drop into the liquid, which is maintained at the boiling temperature. Under these circumstances an aqueous solution of hydrazoic acid distils over. The distillate is allowed to flow into a solution of silver nitrate, when the silver salt, silver azoate, N_3Ag, is precipitated. The silver salt is afterwards dried at 60°–70°, at which temperature no danger attends the operation, and decomposed by sulphuric acid diluted with eight times its volume of water, when hydrazoic acid gas is liberated, contaminated with only a trace of moisture. It appears that the aqueous solution of the free acid is almost as explosive as the silver and mercury salts. Upon one occasion, when attempting to fuse the drawn out end of a tube containing about 2 c.c. of a 27 per cent. solution, Dr. Curtius had a very narrow escape of serious injury, the whole exploding with a fearful detonation, and shattering the glass tube into dust. Several of the azoates explode when a beam of coloured light is thrown upon them; thus barium azoate, BaN_6, explodes when exposed to a strong green light, as does also the still more explosive silver azoate. A concentrated solution of hydrazoic acid appears to be able to dissolve gold, with formation of a red solution of gold azoate.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. Charles E. Flower; an Azara's Fox (*Canis azaræ* ♂) from South America, presented by Mr. H. M. Dodington; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. A. Schafer; two Black-faced Spider Monkeys (*Ateles ater* ♀ ♀) from Peru, deposited.

OUR ASTRONOMICAL COLUMN.

THE ROTATION OF VENUS.—M. Perrotin, the Director of Nice Observatory, presented a note on the rotation of the planet Venus, at the meeting of the Paris Academy held on October 27. The observations described in the note were undertaken for the purpose of testing the conclusions recently arrived at by Signor Schiaparelli. They extend from May 15 to October 4. In the interval the planet has been observed on 74 days, and 61 maps made of its appearance. The whole of the observed facts leads M. Perrotin to the following conclusions:—

(1) The rotation of the planet is very slow, and is made in such a way that the relative position of the spots and terminator do not experience any notable change during many days.

(2) The time of rotation of the planet does not differ from its sidereal period of revolution (about 225 days) more than thirty days. My observations will easily accommodate themselves, however, to a rotation of which the period is from 195 to 225 days.

(3) The axis of rotation of the planet is almost perpendicular to the plane of its orbit. The displacement of the white region observed at the northern edge indicates that the difference does not exceed 15°, as was admitted by Schiaparelli.

These conclusions, therefore, support those deduced by Schiaparelli from an extended discussion of all the observations of the planet.

SPECTRUM OF THE ZODIACAL LIGHT.—Prof. C. Michie Smith has published a series of observations made at Madras of the spectrum of the zodiacal light (Proc. Roy. Soc. Edinburgh, April 7, 1890). He used a spectroscope specially designed for observing and photographing this spectrum, and records:—"In all my observations, which have been carried on at intervals since 1875, the spectrum has appeared continuous and free from bright lines except during the spring of 1883, and even then the lines were not seen with sufficient distinctness to make their existence certain. The estimated position of the supposed line, wave-length 558, differs but little from that of the auroral line (wave-length 556·7) which was observed by Angström in the zodiacal light spectrum in 1867. He was, however, observing at Upsala, where the auroral spectrum can often be seen in almost all parts of the sky, even when the aurora itself cannot be detected. . . . There would seem to be very little risk of obtaining the auroral spectrum in Madras, and I think that if the bright line seen was real, and not imaginary, it must have been due to the zodiacal light."

These observations indicate a periodic appearance of the 558 line in the zodiacal light spectrum. They also support the idea that the origin of the line is the first fluting of manganese at λ 5576.

D'ARREST'S COMET.—This faint comet (*d* 1890), re-discovered by Mr. Barnard on the 6th ult., may be observed near the following positions:—

Ephemeris for Greenwich Midnight.

1890.	R.A. h. m. s.	Decl. ° ′
Nov. 8	21 24 31	−27 13·0
12	39 15	26 40·3
16	53 32	26 1·4

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON Wednesday and Thursday evenings of last week, the 29th and 30 ult., a general meeting of the Institution of Mechanical Engineers was held. The chief business was the reading and discussion of the following two papers: on tube-frame goods waggons of light weight and large capacity, and their effect upon the working expenses of railways, by Mr. M. R. Jefferds, of London; and on milling cutters, by Mr. George Addy, of Sheffield.

Mr. Jefferds is an American engineer who has come over to this country with a view to introduce the tube-frame waggon into England. It should be explained that the tube-frame differs from the ordinary frame of an English railway truck chiefly in the fact that, in place of the timber sole-bars with which we are acquainted, there are used eight wrought-iron tubes, 2⅞ inches in diameter, each pair forming one sole-bar, and suitably connected and supported by malleable cast-iron parts. The boldness with which these castings are used in a structure upon which so much depends bears testimony to the superiority of American foundry practice and to the courage of American designers in perhaps about equal proportions. Certainly no Great George Street engineer would venture upon putting annealed castings in such a position; and, if he did, he would, no doubt, meet with disaster. The tube-frame waggons have, however, been largely built and extensively used in America, and we understand from Mr. Jefferds that there is no reason to think that the castings are not suitable for the work.

The interest in the paper, to judge by the channel into which the discussion was turned, did not centre so much in the constructive details of the waggon described as it did upon the general policy of the American as against the English methods of handling railway freight. In the United States, as most people know, they go upon the principle of having large goods waggons, some even as long as 40 feet and capable of carrying 40 tons. These, however, would be of extreme dimensions, the more usual length being 32 to 34 feet, with a carrying capacity of 30 to 32 tons—that is, American tons of 2000 pounds to the ton. These waggons are mounted on a pair of bogies, each having four wheels. Our own goods trucks are something about 20 feet long, and are mounted on wheels with axles which are fixed with their axes parallel to the ends of the trucks. The English truck will carry 10 tons, and weighs, according to Mr. Jefferds, 8 tons. Mr. Jefferds is, however, a little out here. No doubt some 10-ton trucks weigh 8 tons, but Mr. Williamson, of the Great Western Railway, and Mr. T. Hurrey Riches, of the Taff Vale Railway, state the average weight of their 10-ton trucks to be 5 tons 5 hundredweight and 4 tons 17 hundredweight respectively. Still, making every allowance for errors of this nature, and the possibility of Mr. Jefferds having placed his case in the most favourable light, there is no doubt but that the Americans carry their merchandise over their railways with a far lower proportion of tare to paying load than is the case in England. It has been notorious for years that American railway rates are far below those of this country. We will, however, let Mr. Jefferds speak for himself by making selections from his paper, merely first pointing out the great importance of this question upon our national well-being.

The supremacy of Great Britain—indeed her existence as a Power—is founded upon cheap ocean carriage. We can carry goods across the sea at a lower price than any other people.

Were we to lose that advantage to-morrow, a large part of our population would be in want of bread within a few months, and there would hardly be an individual in the country whose wealth and comfort would not be lessened. Railway carriage is of next importance, and it is only our insular position and the small size of the country which renders it secondary. There is an impression, well or ill founded, that railway goods carriage might be conducted with more economy in England; and when an American expert comes to us to show how, in his opinion, an improvement may be made, he is worthy of our best attention.

Mr. Jefferds begins his paper by pointing out that the present build of goods trucks on English railways differs nothing in principle, and but little in construction, from the truck made by George Stevenson to carry the barrel of water required for replenishing the *Rocket's* boiler. This, perhaps, is rather an exaggerated statement, but there is more truth in it than we find it pleasant to acknowledge. In America, however, such vehicles, as we have already said, are no longer seen. "Since 1865, the railway rates of the United States have," the paper says, "been reduced fully 79 per cent.; so that the railways are now rendering for £21 the same service for which in 1865 they charged £100. The reason they have been able to make so great a reduction is that they have gradually improved their goods waggons, which would formerly carry loads of their own weight only, but will now carry three or four times as much. . . . In 1889 the average rate charged for all descriptions of goods on all the railways of the United States, including terminal charges, was only 0·488*d.* per ton mile, while the average cost to the railways was only 0·311*d.* The average dividend on highly inflated shares was 3·3 per cent." Turning to individual instances, Mr. Jefferds selects three prominent American lines—the New York Central, the Pennsylvania, and the Philadelphia and Erie. The working expenses per ton mile on these were 0·28*d.*, 0·201*d.*, and 0·176*d.* respectively. The working expenses per ton mile on our London and North-Western Railway are 0·65*d.* per ton mile, or three times as much as the great American line, the Pennsylvania. When one thinks how many millions two-thirds of the cost of goods carriage in this country amounts to, one begins to grasp the magnitude of the question. According to Mr. Jefferds, all this vast sum may be saved by the use of his carriage, although he only claims a modest 9 per cent. for his particular tube-frames.

The average Englishman often wonders how it is American farmers can send wheat right across the Atlantic and undersell British growers comparatively on the spot, and that more especially since agricultural rents have so gone down that farms can be got at purely nominal rents. Here, however, is a fact, according to Mr. Jefferds's paper, which may help to throw some light on the question:—"At the present time, for every hundred tons of grain he sends to London, a farmer living 1000 miles inland in the United States has an advantage of £30, after paying both land and ocean transit, over a farmer living at Stirling in Scotland, only 420 miles from London."

The benefits promised by Mr. Jefferds, if we use his big bogie waggons, are, indeed, immense, but the price we shall have undoubtedly to pay for these benefits is immense also. In the first place, it would be very difficult—practically, we think, impossible—to run these long waggons in mixed trains with the English trucks. The difficulties are mechanical—the principal one being the system of buffing—but we have not space to enter upon them here. Therefore these long bogies could only be brought in by a very sweeping change. What would this involve? Nearly the whole of the usual appliances on the permanent way would have to be entirely reconstructed. Sidings and platforms would be too short, points would have to be altered, locking bars and switching apparatus would have to be replaced, turntables would be too small, hydraulic hoists not sufficiently powerful, even if large enough, and weighbridges would have to be replaced, coal-tips rebuilt—in fact, English railway lines would want reconstructing so far as the appliances for dealing with goods traffic are concerned.

There is, however, another salient feature to consider before we can take Mr. Jefferds and his big bogies to our bosom.

The goods traffic of America is more in bulk than that of England, as might be expected in comparing a comparatively new and sparsely peopled country with one older and more crowded. A 30- or 40-ton waggon can be loaded at St. Louis, Chicago, or elsewhere, and sent through to New York. The journey is long enough to make a big car worth filling. In Britain the conditions are different. During the discussion, one English railway manager said the average lading of general merchandise on his line was not much above 2 tons; another, Mr. Williamson, of the Great Western Railway, gave 2½ tons as a fair average. Mr. Jefferds retorts to this that no one expects a truck to go with only one parcel; the trucks can be filled even if it takes the goods of twenty consignees to make a load. Here again another question arises—Do the Americans pay for cheapness by delay? In England a merchant or manufacturer expects goods given over to the Company one afternoon to be delivered the next day (perhaps his expectation is not always fulfilled); but in America, we are told, no such expedition is observed. May not this be due to the fact that a big waggon is often waiting for the last hundredweight or two to make up its load?

The fact is, the question wants treating quantitatively, and for this purpose a vast mass of statistics must be accumulated; for Mr. Jefferds has only touched the fringe of the question. The arguments he has advanced are, however, sufficiently powerful to have made out a very strong *primâ facie* case—most distinctly a case for inquiry. The railway authorities of this country are the only persons who can supply the details by means of which the problem can be adequately discussed.

The discussion on Mr. Jefferd's paper occupied the greater part of the two evenings of the meeting. Mr. Addy's paper on milling cutters was, however, read and discussed. The author gave analyses of the steel used for the purpose, which appeared to approximate closely to razor steel, and by means of wall diagrams explained the mechanical principles which he considered should govern the construction of milling tools, and the machines in which they are used. Without the aid of these diagrams it would be impossible to make the subject clear, and for these we must refer our readers to the volume of the Transactions. The discussion which followed the reading of the paper turned chiefly on the speeds of cutting by milling in use respectively in this country and America; the fact that the American machinists are in advance of us in this respect being fully acknowledged by those present.

The next meeting of the Institution will be held in London early next year.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Vice-Chancellor, the Marquis of Hartington, LL.D., of Trinity College, Lord Walsingham, M.A., of Trinity College, Dr. Morgan, Master of Jesus College, Dr. A. S. Lea, Prof. Browne, Prof. Liveing, Prof. Foster, Albert Pell, M.A., of Trinity College, J. D. Dent, M.A., of Trinity College, W. Aldis Wright, M.A., of Trinity College, L. Ewbank, M.A., of Clare College, F. Whitting, M.A., of King's College, R. F. Scott, M.A., of St. John's College, J. R. Green, M.A., of Trinity College, have been appointed a Syndicate to consider the subject of the letter, dated July 25, 1890, addressed by the President of the Board of Agriculture to His Grace the Chancellor, on the subject of Agricultural Education in the University, and to report to the Senate before the end of the Lent Term, 1891.

At the annual election, on November 3, three Fellowships out of five were awarded to students of Natural Science:—Mr. R. A. Sampson, B. A. (Third Wrangler, 1888, First Smith's Prizeman, 1890), Lecturer in Mathematics at King's College, London, for researches in Hydrodynamics; Mr. L. E. Shore, M.A., M.B., B.C. (First Class Natural Sciences Tripos, 1884–85), (Senior Demonstrator of Physiology in the University, for researches in Physiology; E. H. Hankin, B.A. (First Class Natural Sciences Tripos, 1888–89), Junior George Henry Lewes Student in Physiology, for researches in Bacteriology.

Mr. Walter Heape, M.A., of Trinity College, has been elected to the Balfour Studentship in Animal Morphology, in succession to Mr. William Bateson, Fellow of St. John's College.

Mr. E. E. Sikes, Scholar of St. John's College, has been appointed by the Vice-Chancellor to hold the Newton Studentship at the British School of Archæology in Athens.

The Board for Biology and Geology propose to take power to appoint to the University Table of the Marine Biological

Laboratory at Plymouth, a student of either sex not a member of the University, failing a suitable University applicant.

The proposed new statute affecting the contributions to the University of financially depressed Colleges, passed the Senate on October 30, by 72 votes to 30. The opposition was headed by Prof. Humphry, Prof. Liveing, and Mr. W. N. Shaw. Under the new statute, which has yet to receive the consent of the Queen in Council, depressed Colleges may withhold part of their contribution to the University, and, instead thereof, elect University teachers to Fellowships.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, October 27.—M. Hermite in the chair.—Observations of the planet Venus at Nice Observatory, by M. Perrotin. (See Our Astronomical Column.)—On the reduction to the canonical form of differential equations for the variation of arbitrary constants in the theory of movements of rotation, by M. O. Callandreau.—The neutral meridian of Jerusalem-Nyanza, proposed by Italy to fix the universal hour, determined by its horary distance from 120 observatories, by M. Tondini. A list is given of the time-intervals of sixteen important observatories from the Jerusalem meridian, which cuts the equator about 75 kilometres east of Lake Nyanza. This list is part of a larger one giving the latitudes and time-intervals of 120 observatories from the same meridian. —On the developments in series of the integrals of certain differential equations, by M. R. Liouville.—Periodic visibility of interference phenomena when the light source is limited, by M. Ch. Fabry.—Thermo-electric researches, by MM. Chassagny and Abraham. It is well known that if thermo-electric couples be formed from three metals, A, B, and C, the electromotive forces obtained at a given temperature in each case may be expressed by the following equation :—

$$E(AC) = E(AB) + E(BC).$$

The authors have found the following results in some researches on this relation :—

Couples.		Electromotive Forces. Calculated.		Observed.
Iron-Copper	...	0·0010925 volt	...	0·0010926 volt.
Iron-Platinum	...	0·0016842 ,,	...	0·0016842 ,,
Copper-Platinum	...	0·0005917 ,,	...	0·0005917 ,,

—Electrolysis of aluminium fluoride by igneous fusion, by M. Adolphe Minet. The author has previously shown that he had produced aluminium by electrolyzing its fluoride. He now describes the composition and properties of the bath used, and the relation between the constants of the current and those of the electrolyte—(1) when the salts which make up the bath are chemically pure ; (2) when the electrolyte is mixed with other salts.—On amylamines, by M. A. Berg.—On the arteries and veins of nerves, by MM. Quénu and Lejars.—On the changes of colour of the common frog (*Rana esculenta*), by M. Abel Dutartre.—On the anatomy of the grasshopper and lizard, by M. Ch. Contejean.—The rot of the heart of the beetroot, by M. Prillieux.—Seismic motions at Chili : earthquakes of May 23, 1890, by M. A. F. Noguès. Of the eighteen movements recorded, five took place during the spring in the southern hemisphere, one in the summer, four in autumn, and eight in winter. Of the six of which the direction of motion has been exactly determined, three had an east to west direction, one south-west to north-east, one from north to south, and one from south to north.—Experiments on sedimentation, by M. J. Thoulet.—Theory of sedimentation, by M. A. Badoureau.

DIARY OF SOCIETIES.

London.

THURSDAY, November 6.

Linnean Society, at 8.—A Contribution to the Study of the Relative Effects of different parts of the Solar Spectrum on the Assimilation of Plants: Rev. Prof. Henslow.

Chemical Society, at 8.—The Magnetic Rotation of Saline Solutions : Dr. W. H. Perkin.—Note on Normal and Iso-propylparatoluidine : E. Hori and H. F. Mosley.—The Action of Ammonia and Methylamine on the Oxylepideus : Dr. F. Klingemann and Dr. W. F. Laycock.—Condensation of Acetone Phenanthraquinone : G. H. Wadsworth.

FRIDAY, November 7.

Geologists' Association, at 8.—*Conversazione.*

SATURDAY, November 8.

Royal Botanic Society, at 3.45.

Essex Field Club (at Loughton), at 7.—Essex Meteorological Records : Rev. T. A Preston. (Communicated, with some Notes on Dr. Derham's Early Records, by Prof. G. S. Boulger.—Some Notes on Dipsacus sylvestris and D. pilosus, and their Natural Relationship : J. French.

SUNDAY, November 9.

Sunday Lecture Society, at 4.—Why and how we Eat our Dinner (with Oxy-hydrogen Lantern Illustrations): Dr. Andrew Wilson.

TUESDAY, November 11.

Mineralogical Society, at 8.—Anniversary Meeting.—Election of Officers.—Twins of Marcasite in Regular Disposition upon Cubes of Pyrites: Dr. C. O. Trechmann.—Tetartohedrism of Ullmannite : H. A. Miers.—Notes on Cassiterite : R. H. Solly.

Institution of Civil Engineers, at 8.—Steam on Common Roads : John McLaren.

WEDNESDAY, November 12.

Geological Society, at 8.—On the Porphyritic Rocks of the Island of Jersey : Prof. A. De Lapparent. (Communicated by the President.)—On a New Species of Trionyx from the Miocene of Malta, and a Chelonian Scapula from the London Clay : R. Lydekker.—Notes on Specimens collected by W. Gowland in the Korea : T. H. Holland. (Communicated by Prof. J. W. Judd, F.R.S.).—Further Notes on the Stratigraphy of the Bagshot Beds of the London Basin (North Side): Rev. A. Irving.

THURSDAY, November 13.

Mathematical Society, at 8.—The Influence of Applied on the Progress of Pure Mathematics : the President.—Spherical Harmonics of Fractional Order : R. A. Sampson.—Proofs of Steiner's Theorem relating to Circumscribed and Inscribed Conics : Prof. G. B. Mathews.—On an Algebraic Integral of Two Differential Equations : R. A. Roberts.—Some Geometrical Theorems : Osher Ber.

Institution of Electrical Engineers, at 8.

FRIDAY, November 14.

Royal Astronomical Society, at 8.

CONTENTS.

THURSDAY, NOVEMBER 13, 1890.

THE CURE OF CONSUMPTION.

WHEN we read the announcement of some fresh scientific discovery, the first impulse, perhaps, is to ask, "Who has made it?" and the second, to say, "Does it sound right?"

The amazing declaration that Prof. Koch had discovered the sure means of arresting the growth of the bacillus of tuberculosis, *i.e.* of "consumption" without further imperilling the lives of its countless victims, must have made many ask themselves these questions, and in all cases probably with unanimously satisfactory replies.

The man who has made this discovery has been known for more than the last decade as the foremost technologist in bacteriology. The discoveries and really infinitely wide generalizations of M. Pasteur had cleared the way and pointed out the broad path of investigation in this branch of pathology. When Dr. Koch began his remarkable research into the life-history of the bacillus of anthrax or splenic fever, the completeness and perspicuity of that work obtained for him the opportunity of devoting his whole energies and time to this department of pure science, and now not only its votaries, but the whole mass of mankind, will applaud the magnificence of what has been achieved by patient toil in a laboratory.

Many of us doubtless remember the eloquent account given by Prof. Tyndall in the *Times*, some eight years ago, of Koch's discovery of the direct cause or virus of consumption, viz. the *Bacillus tuberculosis*, and, possibly, some may remember the ignorant contempt with which this announcement was received by the enemies of science and truth—the anti-vivisectionists. In 1884, Koch published, in his customarily complete manner, the whole chain of his investigations into this, the most general of specific diseases, and it was in this paper that he set at rest, by final judgment and demonstration of the infective character of the disease, a debate of 40 years. On looking back to that time, it is easy to see (in answer to the first question) that the character of the man's work insured, if his health permitted, the accomplishment of a gigantic step forward, such as that which is now announced on the most trustworthy authority. Were we not in eager anticipation of the publication of Dr. Koch's experimental facts, it would be out of place to ask the second question. But, as is well known, the bearing which the general drift of bacteriological research possesses at the present time is, in this connection, of the utmost interest.

Already we have learnt that, as of course seemed necessarily the case, Koch early abandoned the search for an antidote among pharmacopœial remedies, and looked for the means of arresting the functional activity of the bacillus among the biological waste-products of the organism. Pasteur's fundamental discoveries in fermentation had established the law that every micro-organism produces, from the substances which it katalyzes as a result of its biological activity (and especially multiplication), a material or materials, which, on accumulation, inhibit its growth, and finally, indeed, arrest its vitality. The actual discovery that this principle is applicable in its entirety to micro-organisms which are pathogenic, *i.e.* causative of disease, is probably due to M. Charrin, whose investigations into the properties and growth of the *Bacillus pyocyaneus* have shed much light on this difficult area of investigation. But M. Pasteur's teaching has been productive of similar results in other terribly common diseases, notably in diphtheria, at the hands of Drs. Roux and Yersin. In all cases albuminous substances have been found, which, while toxic in certain doses, are nevertheless capable of giving immunity to animals from the disease which produced them. Such albumins, as was first shown by Wooldridge, are not only to be obtained as a result of the specific katalysis of artificial cultures by the pathogenic microbe, but exist already formed in some animal tissues. According to the latest investigations of Hankin in this direction, and according to the observations of Reichert and Weir Mitchell and Wolfenden on snake poisons, and of Martin on anthrax, &c., these toxic, and yet protective albumins seem to belong almost exclusively to the class known as globulins.

Should these generalizations prove to be true, one most important step alone will be gained, viz. a definite advance into the unknown desert of biological chemistry. But, to return to the arrest *intra vitam* of an infective virus, there is another factor in the successful extinction of a parasitic poison, such as the tubercular microbe, and that is the specific resistance of the tissues to their invasion by bacilli, which has been so strikingly elucidated by Metschnikoff and others.

This factor has a specially important bearing in tuberculosis, since the bacillus, as is well known, grows with vastly different rapidity in various individuals, and even in the same patients exhibits great differences of vigour according to the tissue it has attacked. This makes the practical application of Prof. Koch's discovery so much the more striking as well as important. Already it is stated that the most chronic forms of tuberculosis, viz. caries of bones and joints and lupus, yield most readily and rapidly to the preventive treatment, whereas a greater difficulty is experienced in dealing with advanced lung disease.

There are many instructive parallels which might be drawn between this last fruit of experimental science (absurdly called vivisection) and the direct application by M. Pasteur of his labours to the prevention of a far more horrible, if fortunately relatively much rarer, disease—hydrophobia; but the storm of incredulity and abuse with which Pasteur's single-minded labours were received are replaced in the present instance by respectful appreciation and admiration.

It may be that this is a sign of the wider public knowledge of the scientific facts concerning infectious disease, or it may be that the irresistible effect of the truth of M. Pasteur's labours is clearing away the obstruction of ignorance and folly. In either case it is a happy augury of the future. But we fear much of the very different feeling with which this last gift of pure science has been received is a sad testimony to the frightfully wide extent to which this distinctive disease, tuberculosis, prevails, and that really there is no family but feels what a priceless result Prof. Koch has attained if an extended trial should confirm his early successes.

Whatever be the explanation, we earnestly hope that the public interest which is thus awakened in a practical exposition of the extreme importance of supporting scientific research, although its objects may not be at first sight apparent, will receive no disappointment or check, but develop into a true appreciation of the great principles which underlie human happiness, health, and wealth.

CLERK MAXWELL'S PAPERS.

The Scientific Papers of James Clerk Maxwell. 2 Vols. Edited by W. D. Niven, Director of Studies at the Royal Naval College, Greenwich. (London: Cambridge University Press, 1890.)

THE gratitude with which we receive these fine volumes is not unmingled with complaint. During the eleven years which have elapsed since the master left us, the disciples have not been idle, but their work has been deprived, to all appearance unnecessarily, of the assistance which would have been afforded by this collection of his works. However, it behoves us to look forward rather than backward; and no one can doubt that for many years to come earnest students at home and abroad will derive inspiration from Maxwell's writings, and will feel thankful to Mr. Niven and the committee of friends and admirers for the convenient and handsome form in which they are here presented.

Under the modest title of preface, the editor contributes a sketch of Maxwell's life, which will be valued even by those who are acquainted with the larger work of Profs. Lewis Campbell and W. Garnett; and while abstaining from entering at length into a discussion of the relation which Maxwell's work bears historically to that of his predecessors, or attempting to estimate the effect which it had upon the scientific thought of the present day, he points out under the various heads what were the leading advances made.

In the body of the work the editor's additions reduce themselves to a few useful footnotes, placed in square brackets. Doubtless there is some difficulty in knowing where to stop, but the number of these footnotes might, I think, have been increased. For example, the last term in the differential equation of a stream-function symmetrical about an axis is allowed to stand with a wrong sign (vol. i. p. 591), and on the following page the fifth term in the expression for the self-induction of a coil should be $-\frac{1}{3}\pi \operatorname{cosec} 2\theta$, and not $-\frac{1}{3}\pi \cos 2\theta$.

To a large and enterprising group of physicists, Maxwell's name at once suggests electricity, and some, familiar with the great treatise, may be tempted to suppose that this book can contain little that is new to them. It was De Morgan, I think, who remarked that a great work often overshadows too much lesser writings of an author upon the same subject. In the present case it is true that much of the "Dynamical Theory of the Electro-magnetic Field" was subsequently embodied in the separate treatise. Nevertheless, there were important exceptions Among these may be noticed the experimental method of determining the self-induction of a coil of wire in the Wheatstone's balance. By adjustment of resistances, the steady current through the galvanometer in the bridge is reduced to zero; but at the moment of making or breaking battery contact, an instantaneous current passes. From the magnitude of the throw thus observed in comparison with the effect of upsetting the resistance-balance to a known extent, the self-induction can be calculated. The letter to Sir W. Grove, entitled "Experiment in Magneto-electric Induction" (ii. p. 121), will also be read with interest by electricians. It gives the complete theory of what is sometimes called "electric resonance."

There can be little doubt but that posterity will regard as Maxwell's highest achievement in this field his electromagnetic theory of light, whereby optics becomes a department of electrics. The clearest statement of his views will be found in the note appended to the "Direct Comparison of Electro-static with Electro-magnetic Force" (vol. ii. p. 125). Several of the points which were then obscure have been cleared up by recent researches.

Scarcely, if at all, less important than his electrical work was the part taken by Maxwell in the development of the Dynamical Theory of Gases. Even now the difficulties which meet us here are not entirely overcome; but in the whole range of science there is no more beautiful or telling discovery than that gaseous viscosity is the same at all densities. Maxwell anticipated from theory, and afterwards verified experimentally, that the retarding effect of the air upon a body vibrating in a confined space is the same at atmospheric pressure and in the best vacuum of an ordinary air-pump.

Besides the more formal writings, these volumes include several reviews, contributed to NATURE, as well as various lectures and addresses, all abounding in valuable suggestions, and enlivened by humorous touches. Among the most noticeable of these are the address to Section A of the British Association, the lectures on colour vision, on molecules, and on action at a distance, and, one of his last efforts, the Rede Lecture on the telephone. Many of the articles from the "Encyclopædia Britannica" are also of great importance, and become here for the first time readily accessible to foreigners. Under "Constitution of Bodies," ideas are put forward respecting the breaking up of but feebly stable groups of molecules, which, in the hands of Prof. Ewing, seem likely to find important application in the theory of magnetism.

A characteristic of much of Maxwell's writing is his dissatisfaction with purely analytical processes, and the endeavour to find physical interpretations for his formulæ. Sometimes the use of physical ideas is pushed further than strict logic can approve;[1] but those of us who are unable to follow a Sylvester in his analytical flights will be disposed to regard the error with leniency. The truth is that the limitation of human faculties often imposes upon us, as a condition of advance, temporary departure from the standard of strict method. The work of the discoverer may thus precede that of the systematizer; and the division of labour will have its advantage here as well as in other fields.

The reader of these volumes, not already familiarly

[1] "With all possible respect for Prof. Maxwell's great ability, I must own that to deduce purely analytical properties of spherical harmonics, as he has done, from 'Green's theorem' and the 'principle of potential energy,' seems to me a proceeding at variance with sound method, and of the same kind and as reasonable as if one should set about to deduce the binomial theorem from the laws of virtual velocities or make the rule for the extraction of the square root flow as a consequence from Archimedes's law of floating bodies."—Sylvester, *Phil. Mag.*, ii. p. 306, 1876.

acquainted with Maxwell's work, will be astonished at its variety and importance. Would that another ten years' teaching had been allowed us! The premature death of our great physicist was a loss to science that can never be repaired.

RAYLEIGH.

SAP.

Sap: Does it rise from the Roots? By J. A. Reeves. (London: G. Kenning, 1890.)

THE object of this book is clearly stated in the following words from the introduction (p. 4):—

"Facts will be advanced to show there is no evidence to support any of the following theories, viz.:—That the sap in trees rises at any time; that inorganic matter rises from the soil; that the soil is exhausted by the growth of vegetation; that sap is elaborated in the leaves," &c.;

and the style is exemplified in the following quotations from the conclusion (p. 82):—

"Instead of water ascending and gases descending; the facts (which are open to the observation of any person disposed to give unbiassed attention to the subject) go to prove that the water *descends* to the roots, and the gases *ascend* to the leaves, both actions being in strict conformity with the Laws of Gravitation."

"Let the reader witness a monster forest tree during a Summer shower, after a long drought, and then calmly consider—Whether the CREATOR IN HIS INFINITE WISDOM ordained that the thirsty leaves should be refreshed and invigorated by drinking in the genial rain falling upon them. Or,—Whether each leaf was designed to *resist such moisture*, but, at the same time, to draw the water it needs from the soil, which is often hundreds of feet below, and as DRY AS DUST."

The italics and large and small capitals are the author's, and we have now to examine how he proceeds to justify the extraordinary statements quoted.

Starting with a number of extracts from Sachs's "Textbook of Botany," which refer to several different things, and are in part misquoted or mutilated, and of which the most remarkable is as follows,[1] "It is not known how water reaches the tops of trees, but probably by the *formation of dew*," the author concludes that much difference of opinion exists. This conclusion is not without warrant, but the nature of the diverging opinions is by no means illustrated by his statements, and is not to be understood without an acquaintance with much more modern literature than he seems to have any knowledge of. At any rate, he might have obtained even more conflicting statements by judicious culling from the writings of Böhm, Elfving, Westermaier, Vesque, and other modern authorities. Granted, however, that much difference of opinion has been expressed on the subject, let us see how the author proceeds to clear up the matter. He suggests as an alternative theory that the leaves of plants obtain their water and mineral substances from the air.

"It is suggested that the foliage of plants by absorbing the moisture in the air also absorbs the impalpable dust which it contains."

[1] At p. 684 of Sachs's "Text-book," second edition (English translation), this statement runs: "It is not known how this water has reached the higher parts of the trees, though it is possibly by the formation of dew," &c., and it bears a very different signification from that given.

"It seems quite possible that, in dry weather, a portion of the dust alluded to, which comes in contact with the leaves of plants, may, with the dews of night, pass through the leaf-cells into the downward flow of sap. If so, such inorganic matter becomes a constituent of the sap, to be chemically acted upon in the formation of new cells effecting the mysterious operations called growth."

This kind of thing is sufficiently startling, and what its effect may be on the minds of those insufficiently acquainted with the elements of botany need not be discussed. For the information of those who expect to find such views supported by new and adequate evidence, however, the following illustrations may suffice:—

"The oil which rises through the cotton wick of a lamp to support the flame is constantly referred to as an apt illustration of the transpiration theory."

"The leaves of the weed anacharis contain a large proportion of silica, although the plant has no root, and it grows whilst flowing with the stream."

"Seeds sown in flannel, moistened with distilled water, will grow (although not to maturity) and produce as rich green foliage as if grown in alluvial soil. [N.B.—Iron cannot be supplied from flannel.]"

If it were not that the book contains internal evidence of the deadly earnestness of the writer, we should have regarded these (and other paragraphs, adduced to show that the roots do not absorb the water and minerals of the transpiration current) as quaint jokes of the Max O'Rell or Mark Twain type. Moreover, the work teems with such funny statements. Speaking of trees (p. 40),

"If the pendent ends of the branches be embedded in the soil, the descending sap will be *drawn out* and and [*sic*] roots will be formed of the discharged sap."

Although the superfluous "and" might suggest that even the pen of the author gasped and stammered, as it were, at this monstrous statement, we fear it must be regarded as an innocent misprint, for the idea that tissues and roots can be formed by the mere hardening of sap is gravely expressed in several places, *e.g.* pp. 37, 48, and 54.

Other notions of sixteenth century value are to be found serving as the foundation stones for the curious superstructure which the author dignifies as a theory. Thus, on p. 32:—

"The germination and growth of a seed seem to be controlled by the same law of gravitation as the growth of a mature plant. Water *descending*, gas *ascending*. . . . When, however, the seed is placed in moist warm soil, water is absorbed, and a kind of fermentation or decomposition commences, the contents are expanded, the gas is necessarily evolved. This expansive operation continues until the skin of the seed is broken. The heavy watery parts exude first, and cell to cell of atomic matter becomes united with the embryo-radicle, and gravitates downwards, forming the root; while the gases or vapours, which result from the fermentation in the seeds, press upwards and cause the plumule to form."

We could not resist quoting this rather lengthy joke, for the sake of the climax: it may be doubted whether the days of a belief in levitation could have produced a statement to equal the last sentence.

This must suffice to show the tenor of the production before us, and we can only conclude by expressing our wonder that any writer could be found to invent the text and any publisher to produce it.

INDOOR GAMES.

The Hand-book of Games. Vol. I. Table Games. (London: Bell and Sons, 1890.)

THIS work was originally published in the year 1850. In spite of the antiquity of the information, there is a steady demand for the old edition, and the publishers, having resolved to reissue the book, decided that it should be thoroughly revised, and many of the articles entirely re-written. In consequence of the recent development of old games and the invention of new ones, the present edition will fill two volumes of about 500 pages each, while the first edition consisted of only one volume of 600 pages.

The games included in the first volume are known as table games. The first one treated of is billiards, and is written by the noted amateur Major-General A. W. Drayson; the well-known professional Mr. W. J. Peall has read through the proof-sheets, and, in his own words, "endorsed, in nearly every case, the author's advice, both theoretical and practical." Billiards is one of the many games that have made remarkable progress in the last fifty years. Of its origin very little is known: some consider that the French invented it; others that the Germans originated the idea, the French only improving on them. Bouillet gives the English as the originators of it. "Billiards," he says in his first work, "appears to be derived from the game of bowls. It was anciently known in England, where perhaps it was invented. It was brought into France by Louis XIV., whose physician recommended this exercise." In his other work we read, "It would seem that the game was invented in England." Whatever may have been the origin of the game, it was originally played on the floor or on a table, and consisted in trying to send a ball through a ring which revolved on a pin or stick fixed firmly on the floor or table. A few years before 1674, billiards must have been well known, especially in England, for in a work published in that year, where it is described as a "most gentle, cleanly, and ingenious game," the author says that in England there were few towns of note "which had not a public billiard table, neither are they wanting in many noble and private families in the country." From those days up to the present time the game has gradually been raised from the "disreputable to the highly respectable," and it is now ranked among the first-class ones.

The game as described in this article is for amateur players only, and, as stated by Mr. Peall, "there are few amateurs who may not gain some useful hints from a study of this book." Although the author gives some very good explanations of the various strokes, describing the best ways of making them and *the effects* produced on the impact of balls, yet in some cases it would be advantageous to the reader to know the reasonings from which the results are drawn. On pp. 34 and 35, in discussing the effect produced by "side" on the striker's ball, the example he gives serves to illustrate our point.

Following these explanations, the games of pool, black pool (commonly known as "shell out"), pyramids, and snooker are described, the last of which is an extension of the game of pyramids, but not as yet generally known.

We now come to the game of chess, by Mr. R. F. Green, who gives a condensed but capital account of it as it is played to-day. Of all games chess is the most difficult, and in consequence of the riddance of the element of chance, great skill is required to play it well. In the study of this game a great expenditure of time is necessary—in fact, "no knowledge or proficiency, easily acquired, could be held in such high and general esteem; and the time involved may, especially in the case of young students, be looked upon as well spent. It constitutes a mental training of the greatest possible value, and promotes a taste which can only be elevating." Lovers of this game will find much in this article that will interest them, and the hints, technical terms, rules, moves, &c., should form a good foundation for beginners.

The remaining games include draughts, backgammon, dominoes, solitaire, reversi, go-bang, rouge et noir, roulette, E. O., hazard, and faro, all of which are thoroughly explained and illustrated by "Berkeley."

In conclusion, a work of this sort becomes a necessity to those who wish to understand the scientific principles which form the bases of many of our games, while for those who play occasionally it will serve as a most handy book for reference. It is thoroughly to be recommended.

W.

OUR BOOK SHELF.

Elementary Algebra. With numerous Examples. By W. A. Potts, B.A., and W. L. Sargant, B.A. (London: Longmans, Green, and Co., 1890.)

IN this book the principles of algebra as far as quadratic equations only are dealt with. The authors have explained them in plain and simple language, and have worked out numerous examples in order to illustrate the methods adopted. In an elementary treatise like this, intended for those who are preparing for public school entrance examinations, great importance must be attached to the working out, briefly and neatly, of examples, and here we have a good and copious collection, together with some papers set at former entrance examinations.

Heat and Light Problems. By R. Wallace Stewart. University Correspondence College Tutorial Series. (London: W. B. Clive and Co., 1890.)

THIS work forms a supplement to the author's "Elementary Text-book of Heat and Light," and is an expansion of the chapters on calculations in that book. The mathematical sides of both these subjects only being dealt with, the fundamental formulæ in each case are clearly worked out and illustrated by many examples. At the heading of each chapter a short summary is given, for convenience of reference, of the many formulæ that may have been proved in previous chapters, but which are used in the one under consideration. This method causes a saving of time and patience, and facilitates the solving of the problems. Students who find special difficulties with regard to the questions on the quantitative relations of either of these two branches of physical science, cannot do better than study this work. By so doing, they will find themselves very much enlightened, and their difficulties considerably diminished.

Annalen des k.k. naturhistorischen Hofmuseums, Wien, Bd. V., Nr. 2 and 3, 1890.

THE above-named (445 pp. in all) contain six original contributions and notices. Of the former, one, by Dr. Karl Fritsch, is botanical, and deals with the flora of Madagascar; another, by Ludwig Hans Fischer, is devoted to personal adornments among the native Indians;

and, of the four which remain, three are zoological, the fourth, by Dr. Felix Koerber, being meteorological. Dr. Fritsch's communication, *apropos* of collections made by Dr. Paulay on the voyage of the "*Saida*," adds to the genera *Blepharis* and *Walleria* each a new species, while new varieties of *Hibiscus vitifolius* and *Cynorchis fastigiata* are described. Dr. Fischer's paper gives an account of his journeys and collections made on behalf of the Vienna Museum. It is a most interesting and carefully executed work of 30 pp., with six plates and 51 admirable woodcuts. The author's notes on ear ornaments are especially commendable, but when all is done as by him it becomes difficult to particularize. This monograph bears the same stamp of excellence as those of Dr. Otto Finsch and Prof. Hein which have preceded it (cf. NATURE, vol. xlii. p. 157), and ethnologists owe the authorities of the Vienna Museum a debt of gratitude for the manner in which they have enriched the literature of their subject. Of the zoological treatises, one (by Dr. Gottlieb Marktanner-Turneretscher) is a Report upon the Hydroids in the Museum collection. The *Gymnoblastea* and *Calyptoblastea* are chiefly dealt with; several new forms are described, localities and donors' names are sufficiently recorded, and a Report upon the *Hydrocorallines* is promised. The two remaining contributions are entomological. One, by Dr. J. Kreichbaumer (13 pp.), is a continuation of the author's previous Report on the *Ichneumonidæ* in the Museum; new genera and species are described. The other is a lengthy monograph of the Linnean genus *Sphex* (266 pp., with 5 plates) by Franz Friedr. Kohl. The author acknowledges his indebtedness to the collections of other Museums and of private individuals, from many of which types have been lent him, and he makes a point of excepting "the material in the London Museum, which contains the greatest number of types." This is greatly to be regretted, in consideration of the pretentious nature of his work, which purports to be a revisionary monograph of the genus; he promises a companion treatise on the allied genera *Ammophila* and *Sceliphron*, and we sincerely hope that, in preparing this, arrangements may be made whereby he shall consult our national cabinet.

Exercises in Practical Chemistry. By A. D. Hall, M.A. (London: Rivingtons, 1890.)

THE author states in his preface that an opinion has been growing latterly that chemistry, as usually taught, is a subject lacking in educational value, and that this is especially the case inpractical chemistry. These exercises are suitable for boys beginning practical work, and they are intended "to exemplify the exact nature of chemical reactions, and to illustrate some of the great principles and fundamental laws of the science." Consequently many of the exercises are quantitative. The first experiment is a verification of Boyle's law by means of a straight barometer tube and mercury. The second is a determination of the coefficient of expansion of air. After a few such preliminary exercises, the more usual chemical experiments follow. The author has not attempted to give "details of craftmanship," as he states that they can be better obtained from the teacher.

An Elementary Geography of India, Burma, and Ceylon. By Henry F. Blanford, F.R.S. Macmillan's Geographical Series. (London: Macmillan and Co., 1890.)

DR. GEIKIE, the editor of Macmillan's Geographical Series, could not have entrusted the subject of the present volume to a more thoroughly competent writer than Mr. Blanford. In the course of a long service in India, as Mr. Blanford himself notes in the preface, he had occasion to visit most parts of the Empire, so that his knowledge of the geography of India is incomparably more exact, extensive, and vivid than if it had been derived merely from books. Traces of this fact are to be found in every section of his excellent manual. In the preparation of a volume of this kind one of the chief difficulties of the writer is to decide how much shall be omitted; and this question Mr. Blanford seems to us to have settled with admirable tact and judgment. Nothing he introduces would, if properly understood, tend simply to burden the memory. The facts he has selected are both important and interesting; and they are presented in so simple and clear a style, while their relations to one another are so distinctly brought out, that they cannot fail to arrest the attention of young learners, and to foster the growth of individual intelligence. The illustrations—for the most part taken from photographs—are in every way worthy of the text.

LETTERS TO THE EDITOR.

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

Araucaria Cones.

I AM happy to say that I can inform the Duke of Argyll of at least two instances within my own personal observation of the fruiting of Araucaria. The cones were seen, and one of them handled, by myself and many members of the Geologists' Association five years ago, when we were entertained at luncheon by our excellent Rural Dean, the Rev. J. T. Brown, Rector of St. Paul's, Wokingham. The fact will be found recorded in the Proc. Geol. Assoc., vol. ix. p. 223. The other instance was a few years earlier, and the tree which bore the cones is, I believe, still standing on the lawn of Sandhurst Rectory. My attention was drawn to them, as to something somewhat rare, by the present Bishop Suffragan of Reading, who was Rector of Sandhurst at that time. A year or so later, a geologist, who has written much on palæobotany, when on a visit to this neighbourhood for the purpose of making the acquaintance of the London Bagshots, had the assurance to inform me that the Araucaria never blossomed or fruited in this country, because it was a diœcious tree! In order to convince him of his error on so fundamental a matter, I took him to Sandhurst Rectory and pointed out the tree to him; but I cannot recollect if any fruit-cones were on it then.

I may add that the fruit is large and the bract-scales very succulent. It resembles most nearly the fruit of the Alpine cedar (*Pinus Cembra*), but the fruit is three or four times as large.

Wellington College, Berks, Nov. 7. A. IRVING.

IT is quite a common occurrence for the Araucaria to bear cones in this country, when the tree is healthy and of fair size. Very possibly it may be necessary that some check to its development shall have taken place before a large number of cones are formed, as I have never seen more than three or four upon a single tree.

Not only are the cones formed, but seed is ripened, and I have now in my possession some plants about three years old, which I have reared from English-grown seeds picked up under a fruited Araucaria. Apparently about a year elapses between the appearance of the cone and the shedding of the seed, and no doubt towards the end of the summer of 1891 the tree belonging to the Duke of Argyll will yield many thousands of fertile seeds. These do not seem to keep well through the winter, but are best sown in the autumn without delay. Artificial heat is not necessary to their germination. JOHN I. PLUMMER.

8 Constitution Hill, Ipswich, November 11.

IN reply to the Duke of Argyll's inquiries (NATURE, November 6, p. 8), we have an Araucaria in our garden, now about 20 feet high, which bore barren cones first in the summer of 1889. We have also a seedling Araucaria, one of several grown from seeds from large seed-bearing cones gathered from the avenue of Araucarias in the late Lady Rolle's grounds at Bicton Park, near Budleigh Salterton, in 1878, at which time they were abundant on the splendid trees forming the avenue.

Further Barton, Cirencester, Nov. 9. E. BROWN.

Squeaking Sand *versus* Musical Sand.

ALLOW me to use your columns to thank Mr. Henry C. Hyndman for the reference in NATURE of October 2 (vol. xlii. p. 554) to a locality of sonorous sand in the interior of South Africa. Its occurrence in the interior is new to me, though it has been reported from the west coast at Liberia, and at Cape Ledo, from which latter place my friend, Mr. L. Harold Jacoby, a member of the American Eclipse Expedition, recently brought me specimens.

Dr. Alexis A. Julien and myself quite agree with Mr. Carus-Wilson in his remarks (NATURE, October 9, vol. xlii. p. 568) that there is no scarcity of sonorous sand, and only observers are lacking. This we established in 1884, when we announced at once seventy-four localities on the Atlantic coast of the United States, although at the time we began our researches its occurrence at Manchester, Massachusetts, was thought to be unique in America. The localities were in part reported by the keepers of life-saving stations to whom we had sent circulars.

The old theory adopted by Mr. Carus-Wilson, that the sounds are produced by "rubbing together of millions of clean sand grains very uniform in size," is, we think, insufficient to explain *musical* sand, but well adapted to explain *squeaking* sand. Two distinct classes of sounds are produced by disturbing sand, both undoubtedly due to vibrations; the more common sound is caused by attrition of the particles, and has a well-known harsh character by no means musical; this in rare cases becomes a loud squeak. The second is caused, we believe, by oscillations of the particles themselves protected from actual contact by elastic air-cushions, and this is decidedly musical in tone. Musical sand yields notes by friction only when *dry*; squeaking sand yields a harsh, shrill squeak (reminding one of the cry of a guinea-fowl), best when *moist*. This latter variety is very rare; we have collected by correspondence and in person over 500 samples of sand from around the world, and musical sand seems to be comparatively common, but only two localities of squeaking sand are known to us, both in so-called boiling springs—one in Maine, and the other in Kansas. A very small quantity of squeaking sand pressed between the thumb and forefinger produces, when wet, a peculiar shrill squeak—a phenomenon which we think well explained by the attrition theory. The magnificent acoustic displays which I have witnessed in the desert of Sinai (NATURE, vol. xxxix. p. 607) and on the coast of Kauai (NATURE, vol. xlii. p. 389) are, however, manifestly due to greater freedom of oscillatory motion than is possible if the particles merely scrape against each other.

Dr. Julien and I await with interest the second edition of Mr. Carus-Wilson's paper, and shall be very much obliged to him for giving a large circulation to the results we have obtained by extended travel and years of study, though we had planned to present the results to the scientific public in our own way.

H. CARRINGTON BOLTON.

University Club, New York City, October 27.

Honeycomb Appearance of Water.

THIS afternoon, while ascending a mountain pathway adown which water was trickling, after the torrents of rain that fell in the morning had ceased, I observed an appearance of the surface of running water so exactly like the hexagons of the bees' cells that I looked at it carefully for some time. Little air-bells of water seemed to issue from under the withered leaves lying in the tract, which rushed towards the hexagons, occupying an irregular space about four inches by five. As soon as these air-bells arrived at the hexagons, they arranged themselves into new cells, making up, apparently, for the loss occasioned by the continual bursting here and there of the cell-walls. No sooner had these cell-walls burst, than others closed in and took their places. The worst-formed hexagons were those at the under or lower side of the surface—the part of the surface farthest down the hill; here they were larger, and more like circles. By an ingenious mechanical theory, Darwin accounts for the hexagonal structures of the cells of the hive-bee so as to supersede the necessity of supposing that the hive-bee constructed its comb as if it were a mathematician. But here the blind forces of Nature, under peculiar conditions, had presented an appearance, on running water less than half an inch in depth, so entirely like the surface of a honeycomb, that it would be a startling result could it be reproduced in a laboratory.

J. SHAW.

Tynron, November 7.

On the Soaring of Birds.

MR. GUTHRIE has suggested (November 6, p. 8) one more *vera causa* of soaring. Like all its predecessors, this seems to the last degree unlikely to occur to an extent adequate to the explanation of soaring in the sense in which the term is commonly used, viz. floating at a constant height without motion of the wings.

May not the true cause be that birds do not soar at all in this sense, but only seem to soar because the movement of the wings is too rapid for our imperfect eyes to detect? Is it not possible that birds which to our eyes seem to soar would betray themselves to the camera? Is it not also possible that in some cases the motion may be too rapid to be discovered even by photography?

Whether this be the whole truth or not, I venture to protest against such statements as that a bird followed a ship for 11 minutes "without flapping a wing." If Mr. Guthrie had said, "without any flapping which my eyes could perceive," I should not have had a word of criticism to offer. But that would be an entirely different statement. What would be thought of one who should say that he had seen a conjurer with hands a yard apart take a card with the right hand out of the left without any movement of either hand? Yet many people have seen or seemed to see this common trick.

G. W. H.

A Bright Green Meteor.

AN exceedingly bright green meteor was seen here on the 8th inst. at 5.30 p.m. It passed from north to south under α and β Aries, which would give it an altitude of 19°. The path was parallel to the above stars and about 5° in length. This indication may serve to determine the height of the meteor if it was seen from elsewhere.

J. P. MACLEAR.

Cranleigh, Guildford.

Weighing by a Ternary Series of Weights.

IT has been shown in NATURE (vol. xlii. p. 568) that any number of pounds may be weighed with weights, the numbers of pounds in which form a geometrical progression with 1 for first term and 3 for common ratio. The following method of treating the same problem may serve to illustrate some remarks made by the President of the Mathematical Section of the British Association at the recent meeting in Leeds. One of these remarks had reference to the fascinating interest attaching to such inquiries into the properties of series of numbers, another showed that the adoption of special systems of notation for different problems was often of great service, and a third remark alluded to the attainment of one and the same result by diverse methods of procedure. In the present case the interest attaching to the subject may be left to speak for itself; the notation suitable for the problem requires elucidation. It is well known that by means of only two figures, 1 and 0, any number may be expressed if we agree that the value of the 1 shall be doubled every time it is removed one place further to the left, so that, for example, 11111 would denote the number $1+2+4+8+16$, and that any number not greater than 31 would be denoted by means of five figures or less. It follows that if we had five weights of corresponding value to the above five numbers we could weigh any number of units of weight from 1 to 31. Now, the present problem only differs from this in two respects—namely, in that the 1 increases threefold in value on being removed one place to the left, and that the value denoted by it may in any position, except the place on the extreme left, be taken negatively. Let us agree to denote the negative value by using a different type, and we may then indicate all values from 1 to 40 as follows:—

1	1	1**11**	5	1**111**	14	10**11**	23	11**11**	32
1**1**	2	1**1**0	6	1**11**0	15	10**1**0	24	11**1**0	33
10	3	1**1**1	7	1**11**1	16	10**1**1	25	11**1**1	34
11	4	10**1**	8	1**1**0**1**	17	100**1**	26	110**1**	35
		100	9	1**1**00	18	1000	27	1100	36
		101	10	1**1**01	19	1001	28	1101	37
		11**1**	11	1**1**1**1**	20	101**1**	29	111**1**	38
		110	12	1**1**10	21	1010	30	1110	39
		111	13	1**1**11	22	1011	31	1111	40

In the extreme right-hand column of this table, where 1 denotes a single unit, the figures 1, **1**, 0 are written each once and then repeated in the same order, and so on to the end. In the second column, where 1 denotes three units, each figure is repeated three times, and then again three times, and so on to the end, but this column begins at the value $\frac{3+1}{2}$. The third column, where 1 denotes nine units, begins at the value $\frac{9+1}{2}$, and the figures are repeated nine times. The fourth column, in which 1 stands for 27, begins at the value $\frac{27+1}{2}$, and contains only the figure 1, twenty-seven times repeated. Hence it will be found that, in order to weigh all the pounds from one to forty, we shall have to make use of *each* weight twenty-seven times.

If, instead of four, seven weights were used, we might, by using each weight $3^6 = 729$ times, weigh any number of pounds from 1 to $729 + \frac{729-1}{2} = 1093$[1] pounds.

Further, we may, for any given number of weights, construct tables, one for each weight, showing the numbers of pounds for which it will be used positively, and also indicating by different type the numbers for which it has to be subtracted. Thus, with five weights we should have for the first four the following tables, whilst the fifth would contain the 81 consecutive numbers beginning with 41 and ending with 121, all to be used positively.

1	4	7	10	13	16	19	22	25
28	31	34	37	40	43	46	49	52
55	58	61	64	67	70	73	76	79
82	85	88	91	94	97	100	103	106
109	112	115	118	121	**2**	**5**	**8**	**11**
14	**17**	**20**	**23**	**26**	**29**	**32**	**35**	**38**
41	**44**	**47**	**50**	**53**	**56**	**59**	**62**	**65**
68	**71**	**74**	**77**	**80**	**83**	**86**	**89**	**92**
95	**98**	**101**	**104**	**107**	**110**	**113**	**116**	**119**

2	3	4	11	12	13	20	21	22
29	30	31	38	39	40	47	48	49
56	57	58	65	66	67	74	75	76
83	84	85	92	93	94	101	102	103
110	111	112	119	120	121	**5**	**6**	**7**
14	**15**	**16**	**23**	**24**	**25**	**32**	**33**	**34**
41	**42**	**43**	**50**	**51**	**52**	**59**	**60**	**61**
68	**69**	**70**	**77**	**78**	**79**	**86**	**87**	**88**
95	**96**	**97**	**104**	**105**	**106**	**113**	**114**	**115**

5	6	7	8	9	10	11	12	13
32	33	34	35	36	37	38	39	40
59	60	61	62	63	64	65	66	67
86	87	88	89	90	91	92	93	94
113	114	115	116	117	118	119	120	121
14	**15**	**16**	**17**	**18**	**19**	**20**	**21**	**22**
41	**42**	**43**	**44**	**45**	**46**	**47**	**48**	**49**
68	**69**	**70**	**71**	**72**	**73**	**74**	**75**	**76**
95	**96**	**97**	**98**	**99**	**100**	**101**	**102**	**103**

14	15	16	17	18	19	20	21	22
23	24	25	26	27	28	29	30	31
32	33	34	35	26	37	38	39	40
95	96	97	98	99	100	101	102	103
104	105	106	107	108	109	110	111	112
113	114	115	116	117	118	119	120	121
41	**42**	**43**	**44**	**45**	**46**	**47**	**48**	**49**
50	**51**	**52**	**53**	**54**	**55**	**56**	**57**	**58**
59	**60**	**61**	**62**	**63**	**64**	**65**	**66**	**67**

Supposing, now, we wished to determine how any number of pounds, less than 122, may be weighed by means of the weights 1, 3, 9, 27, 81, we seek the number in question, say, for example, 115, first in the upper part of each of the first four tables, as well as in the fifth, and find in this way that we shall require to place all the weights except the weight 3 on one side of the balance; and as the same number is found in thick type in the second table, we see that the weight 3 must be placed in the opposite scale of the balance, and we have $1 + 9 + 27 + 81 - 3 = 115$. In like manner $70 = 1 + 81 - (3 + 9)$ and $38 = 3 + 9 + 27 - 1$. It is manifest that these tables, like the similar tables founded on the binary series, 1, 2, 4, &c., may be used to discover what number up to a certain limit a person has thought of, on being informed first in which of the tables the number occurs in the upper part of the table, and then in which, if any, of the remaining tables it is found in the lower part. For this purpose it is only necessary to add together, not the lowest numbers at the head of the tables, as for the binary series, but to take in each table a number one less than the double of the lowest number, both in the positive and in the negative group, and then to subtract the second sum from the first.

Many facts respecting such tables as the above may be ascertained from the following rows of numbers, which are formed, a column at a time, beginning on the left with 1, 0, 1, and adding the last number to each of the former two, which gives 2, 1 and their sum, 3, for the second column; from these 3 + 2, 3 + 1, and their sum, for the third, and so on.

1 .	2 .	5 .	14 .	41 .	122 .	365 .	1094
0 .	1 .	4 .	13 .	40 .	121 .	364 .	1093
1 .	3 .	9 .	27 .	81 .	243 .	729 .	2187

The upper row contains the lowest numbers in the several tables; the middle number in any column shows the highest number of units that can be weighed with the weights shown in the last row as far as the preceding column. The first two rows, moreover, show, in reverse order, how many times each weight will be used positively or negatively. The *highest* of the weights, for example, will be used only positively, as shown by the numbers 1, 0 in the *first* column, the weight next to the highest will be used twice as often positively as negatively, as shown by the numbers 2, 1 in the *second* column, and so on. Thus, for five weights we should have in the *first* table, as shown by the *fifth* column, 41 numbers in the upper part of the table, 40 in the lower part; in the second table the corresponding numbers are 3 × 14 and 3 × 13; in the third 9 × 5 and 9 × 4, and in the fourth 2 × 27 and 27, as we have already found above.

J. WILLIS.

Bradford, October 22.

[1] The coincidence of this number with the number of NATURE in which this subject was introduced is, of course, purely fortuitous.

THE CELL THEORY, PAST AND PRESENT.[1]

II.

THE continued investigations into the structure of cells, both in plants and animals, led to modifications in the conception of their morphology. Hugo von Mohl announced that he had discovered (*Botanische Zeitung*, translated by A. Henfrey in Taylor's "Scientific Memoirs," vol. iv., 1846) in the vegetable cell, after being acted on by alcohol and iodine, a thin nitrogenous membrane distinct from and applied to the inner surface of the cellulose wall of the cell, which he named the *primordial utricle.* He regarded it as forming a vesicle within the cell wall, and containing the contents and the nucleus. By subsequent observers it has been shown that the primordial utricle is nothing more than a thin layer of protoplasm lying close to the cellulose wall, and inclosing the sap cavity of the cell.

Prof. Huxley, in an article on the cell theory (*British and Foreign Medico-Chirurgical Review*, October 1853), criticized the views of Schleiden and Schwann, and introduced the terms *endoplast* and *periplast* into histological description. He regarded the primordial utricle as the essential part of the endoplast in the plant, and as homologous with the "nucleus" of the animal cell; whilst the protoplasm and nucleus are simply its subordinate modifications. The periplast, on the other hand, consisted in plants of the cellulose cell wall; whilst in animals the cell wall and matrix of cartilage, the cell walls and intercellular substance of connective tissue, the calcified matrix of bone, and the sarcous elements of muscular fibre, were all examples of periplast which had passed through various forms of chemical and morphological differentiation. Huxley maintained that the periplast was the metamorphic element of the tissues, and by its differentiation every variety of tissue was produced, owing to intimate molecular changes in its own substance. The endoplast again might grow and divide, as in the process of cell multiplication; but it frequently disappeared, and underwent neither chemical nor morphological metamorphosis; and so far from being a centre of vital activity, he held that it exercised no attractive, metamorphic, or metabolic force upon the periplast.

[1] The Inaugural Address delivered to the Scottish Microscopical Society, by Sir William Turner, F.R.SS. L. and E., President of the Society. Continued from p. 15.

But about this time it began to be more distinctly recognized that many anatomical units which were to be regarded as cells, as Schwann had indeed admitted in a few exceptional cases, possessed no cell wall or investing membrane, and that the analogy with a bladder or vesicle could no longer be sustained. Thus in 1856 (" Lehrbuch der Histologie," 1857; preface dated October 1856), Leydig gave as his idea of a cell a more or less soft substance, approaching in its original state to the globular in form, which inclosed a central body, the nucleus. Subsequently, the cell substance might harden into a more or less independent membrane, and the cell would then consist of membrane, contents, and nucleus. Leydig's conception, therefore, of what were the essential parts of a cell closely corresponded with the opinion expressed some years previously by John Simon. Brücke again maintained (" Elementarorganismen," *Wien. Sitzbericht*, 1861) that the constancy of the presence of a nucleus was subject to certain limitations, especially in the cells of cryptogams, and that there was no positive information either respecting the origin or the function of the nucleus. He further showed that the soft contents of the cell were of a highly complicated nature, and that they frequently exhibited spontaneous movements and contractility. In 1861 and also in 1863, Max Schultze published (*Müller's Archiv*, 1861, p. 1; " Das Protoplasma," Leipzig 1863), most important papers on the properties of cells. He adopted the term protoplasm which Von Mohl had employed to designate the contents in vegetable cells which surround the nucleus, and applied it to the substance which had the corresponding position in animal cells. He completely discarded the view that a membrane was essential to a cell, and defined a cell as a nucleated mass of protoplasm. He identified the protoplasm of the animal and vegetable cell as essentially the same substance as the contractile sarcode which forms the freely moving pseudopodia of the Rhizopoda, and he looked upon it as possessing great physiological activity. The conception of the functions and relative importance of the constituent parts of a cell had now undergone a material change. The suggestive ideas of Simon and Leydig had been distinctly formulated by Max Schultze. Instead of the cell membrane being regarded as a necessary part of a cell, and the active element concerned in the formation of the cell contents, as Schwann believed, it now became universally recognized as only a secondary structure formed by a differentiation of the superficial part of the protoplasm. Schultze also maintained that the appearance of the membrane might be looked upon as a sign of commencing loss of activity, for a cell with a membrane can no longer divide as a whole, but the division is restricted to the protoplasm contained within it. A cell with a membrane is, he says, like an encysted Infusorian. Taking the embryonal cell as a type, he believed that both the nucleus and the protoplasm were derived from the corresponding constituents of another cell. The protoplasm was the substance especially endowed with living force; the nucleus, he thought, played an important *rôle*, though its exact function could not be defined. The only structural character which Schultze recognized in the protoplasm was a finely granular appearance throughout the somewhat jelly-like, contractile material in which the granules were embedded. Although the name of protoplasm was now given to this substance, yet it obviously corresponded morphologically with the blastema which both Schleiden and Schwann had recognized within the cell, between the nucleus and the cell wall; though it now assumed in the minds of observers a different physiological import.

The reign of protoplasm had now been inaugurated. Not only was the cell membrane believed to be a product of its differentiation, but the matrix of cartilage and of connective tissues, and the other intercellular substances, were thought to be produced not as a secretion, but by a conversion of the protoplasm of the cells into their respective forms. But, further, Max Schultze (" Organis. der Polythalamien," 1854) described a non-nucleated Amœba; and Haeckel (*Zeitsch. f. wiss. Zool.*, 1865, Bd. xv.) and Cienkowski (Max Schultze, *Archiv*, 1865) other non-nucleated organisms, simple in their structure. These organisms were believed to consist solely of a clump of soft protoplasm, which might either be naked, when they were called *simple cytodes;* or encased in a wall or envelope, and then termed *encased cytodes.* Haeckel named these—the most simple of all organisms—Monera, and referred them to a group on the confines of both the animal and the vegetable kingdoms, which he termed Protistæ. Stricker (" Allgemeines über die Zelle," in " Handbuch der Lehre von den Geweben," Leipzig, 1871) also excluded the nucleus as necessary to our conception of an elementary organism. He went so far as to say that the historic name of cell might be applied to the morphological elements of the higher animals, or to independent living organisms, even if they were only little masses of animal sarcode or protoplasm. He was not, however, disposed to extend the definition to isolated fragments of living protoplasm, unless the whole group of phenomena characteristic of an independent organism could be recognized. Stricker held that protoplasm may be fluid, solid, or gelatinous. It exhibited the phenomena of movement, of nutrition, of growth, and the capability of reproducing its like, *i.e.* the sum of the phenomena which are characteristic of living organisms.

The doctrine that a nucleated mass of protoplasm was the structural unit common to organisms generally, both plants and animals—though at the very bottom of the scale the phenomena of life could be manifested by a particle of protoplasm without a nucleus—received its most popular expression, in this country at least, in a well-known address by Prof. Huxley.[1] In this address he stated that protoplasm, simple or nucleated, is the formal basis of all life, and that all living forms are fundamentally of one character. His views, therefore, had undergone some modification, as to the element of the tissue in which vital activity was more especially centred, since the publication of his previous article on the cell theory.

But contemporaneous with these researches on the protoplasmic theory of cell structure and activity, an English physiologist, Dr. Lionel Beale, was conducting investigations into the structure of the simple tissues from an independent and somewhat different point of view. He considered that the elementary tissues of every living being consisted of matter in two states (" Structure of the Simple Tissues," London, 1861)—the one an active, living, growing substance, composed of spherical particles, capable of multiplying itself, and coloured red by carmine, which he named *germinal matter;* the other, named by him *formed material*, was situated peripherally to the germinal matter from which it was produced; it was passive, non-living or dead, incapable of multiplying itself, and not coloured red by carmine like the germinal matter. In adapting these terms to the ordinary nomenclature of the cell, Dr. Beale states:—

" In some cases the germinal matter corresponds to the 'nucleus'; in others, to the 'nucleus and cell contents'; in others, to the matter lying between the 'cell wall,' and certain of the 'cell contents'; while the formed material in some cases corresponds exactly to the 'cell wall' only; in others, to the 'cell wall and part of the cell contents'; in others, to the 'intercellular substance'; and, in other instances, to the fluid or viscid material which separates the several 'cells, nuclei, or corpuscles' from eachother."

According to this theory of the tissues, all the elementary parts of the body consist of two substances—an

[1] " On the Physical Basis of Life," a Lay Sermon delivered November 8 1868, *Fortnightly Review;* also in " Lay Sermons and Addresses," London, 1870.

active, living, germinal matter, an inactive, non-living, formed material. Every living elementary part is derived from a pre-existing living elementary particle. The nuclei of the germinal matter, though remaining for a long time, perhaps, in a comparatively quiescent state, may become active and give rise to new nuclei. Dr. Beale held that the cell wall was by no means constantly present in cells, and that, when present, both it and the intercellular substance were formed or produced by, or a conversion of, the germinal matter. In a subsequent work, Beale ("Bioplasm," London, 1872) substituted the term *bioplasm* for germinal matter, and included in it the nucleus, nucleolus, and some forms of protoplasm. It is, he says, from the bioplasm that the formed material is produced.

An important advance was made in the conception of the structure of the constituent parts of the cell when it was ascertained that protoplasm was not the structureless, granulated jelly, or slime, which it was originally supposed to be, but that it consisted of two parts, viz. a minute network of very delicate fibrils and an apparently homogeneous substance which occupied the interstices of the network. Stilling and Max Schultze recognized the fibrillated character of the protoplasm of nerve cells and axial cylinders, but Frommann, Heitzmann, Klein, and other histologists applied the observations to the structure of protoplasm generally.

The subject made yet a greater step forwards when it was ascertained by Strasburger and Flemming that the nucleus in its passive or resting stage consists, in addition to the nucleolus, of threads or fibres, some finer, others coarser, formed of *nuclein*, and arranged in a reticular network, so as to form little knots at the points of intersection of the fibres. In the interstices of the network an apparently structureless intermediate substance, nuclear fluid or *nucleoplasm*, is situated; and the nucleus is surrounded by a membrane.[1] By some observers the threads are regarded not as forming a network, but as a greatly coiled single thread. From the affinity which they have for colouring-matter, so that they easily stain with dye, Flemming has named them *chromatin fibres*.[2] But the whole question of the relation of the nucleus to the life of the cell, more especially in connection with the production of young cells, assumed a much more definite form when it was discovered that the chromatin nuclear fibres took a primary part in the division of the nucleus in the process of cell multiplication. The nucleus was now reinstated in its place as of primary importance in the structure of cells, and as an essential factor in the formation of new cells. The movements of the fibres within the nucleus, and their rearrangement so as to form definite figures, which changes precede the act of division, were named by Schleicher *karyokinesis*,[3] or nuclear movement, a term which has now been generally adopted.

Waldeyer states that Schneider, of Breslau, was the first to recognize these movements of the nuclear fibres, and to describe them in connection with the division of the ova, the sperm cells, and also the tissue cells of a flat worm, *Mesostomum*; but Bütschli and Foli made the process more generally known. The publication of their researches excited the greatest interest, and a host of observers, amongst whom I may especially name Strasburger, Flemming, Hertwig, Balbiani, E. van Beneden, Johow, Heuser, Pfitzner, J. M. Macfarlane, Carnoy, and Rabl, demonstrated the process in a number of plants and animals, and the literature of the subject is now very extensive. In order to express the appearances presented, and the changes which take place both in the nucleus and in the cell in the process of division, a new nomenclature has been introduced, and we now read of cytaster, monaster, dyaster, equatorial plate and crown, pithode or cask-shaped, spindles, ellipsoids, coils, skeins both compact and loose, pole radiations, spirem, and other terms. From the range of the literature it would be a work of considerable labour and time to make an analysis of the different observations so as to associate with the name of each observer the particular set of facts or opinions which he has made known. Fortunately, this is unnecessary on my part, as admirable *résumés* of the whole subject have recently been published both by Prof. McKendrick of Glasgow (Proc. Phil. Soc., vol. xix., Glasgow, 1888) and Prof. Waldeyer of Berlin (*Archiv für Mikros. Anat.*, Bd. xxxii., 1888).

[1] This membrane is perhaps nothing more than a somewhat differentiated layer of the protoplasm of the cell arranged around the nucleus.

[2] The chromatin fibres appear to be composed of granules or spherules, named "microsome-disks" by Strasburger.

[3] Flemming proposed the term *karyomitosis*, or nuclear threads, to express the thread-like figures formed in the process. M. Carnoy gives the name *enchylema* to the apparently structureless material which occupies the interstices of the network in both the nucleus and cell protoplasm.

Without entering into a detailed description, it may be sufficient for my present purpose to say that four stages may be recognized in connection with nuclear division.

The *first*, or *spirem stage*, exhibits several phases. At its commencement the finer threads, which connect the primary or coarser chromatin fibres of the resting nucleus together, and which give the network-like character, have disappeared along with the knots at their points of intersection and the nucleoli. The primary chromatin fibres, or *chromosome* as Waldeyer calls them, form a complex coil, the spirem or ball of thread, which divides into loops, about twenty in number, and forms a compact skein. The loops are placed with their apices around a clear space called by Rabl the "polar field," whilst their free ends reach the opposite surface of the nucleus or the "antipole." The nucleus also increases in size contemporaneously. The loops next become not so tightly coiled, and form the loose skein though the individual fibres thicken and shorten. A most important change then occurs, which was discovered by Flemming, and which consists in a longitudinal splitting of each loop or primary chromatin fibre into two daughter threads. A spindle-shaped figure, first seen by Kowalevsky, next appears in the nucleus; it consists of threads that stain much more feebly than the chromatin fibres.[1] The spindle has two poles and an equator, and it finally occupies a position in the deeper part of the nucleus; its equator lies in the plane through which division of the nucleus is about to occur. The loops of chromatin fibres group themselves in a ring-like manner around the equator of the spindle with their angles inwards, whilst from each pole of the spindle a radiated appearance (*cytaster*) extends into the protoplasm of the cell. The membrane of the nucleus has now disappeared, so that it is directly invested by the protoplasm of the cell; and it is possible, as Strasburger thinks, that there may be a direct flow of the protoplasm into the nucleus, and that the spindle may be produced by it. At the pole of the spindle, from the point at which the cytaster radiates, E. van Beneden has seen a small, shining, polar body, which Strasburger says is not found in vegetable cells.

The *second*, or *monaster stage*. When the chromatin loops have arranged themselves about the equatorial plane of the spindle with their limbs pointing outwards, and the angle of the loop towards the centre of the spindle, a single star-like figure (*monaster*, *equatorial plate* or *crown*) is produced. The two daughter threads, into which each primary chromatin thread had previously split longitudinally, now separate from each other, and, according to Van Beneden and Heuser, pass to opposite poles of the nuclear spindle, where they form loops. These changes are known as the process of *metakinesis*.

In the *third*, or *dyaster stage*, the chromatin loops at each pole of the spindle arrange themselves so that the angles of the loops, though not touching each other, are close together at the pole, and the limbs of the loops are bent towards the equator of the spindle. Two stars are thus produced (*dyaster*), one at each pole, and each star

[1] Owing to the feeble staining of the spindle figure and of the nucleoplasm, the substances which compose them have been named *achromatin*.

is formed of one of the daughter threads into which each chromatin fibre of the monaster divides by its longitudinal splitting. Each star is sometimes called a daughter skein; around each daughter skein a membrane appears at this stage, and a daughter nucleus is then formed.

In the *fourth* or *dispirem stage*, the chromatin threads thicken and shorten, and the loops of each star arrange themselves with the angles towards the polar field of the nucleus, and the limbs to the antipole.

The division of the mother cell into two new daughter cells is now completed by the cell protoplasm gradually constricting in the equatorial plane until at last it is cleft in twain, and each daughter nucleus is invested by its own mass of protoplasm. The chromatin threads of the daughter skein then form a network of coarser and finer fibres, a nucleolus appears, and the resting nucleus of the daughter cell is completed. Two daughter cells have thus arisen, each of which possesses its own independent vitality. Owing to the very remarkable longitudinal splitting of the fibres of the chromosome, and the distribution of the daughter threads from each fibre to the opposite poles of the spindle, it follows that each daughter nucleus contains about one-half of each chromatin fibre, so that whatever be the properties of the chromosome of the mother cell, they are distributed almost equally between the nuclei of the two daughter cells. As regards the cleavage of the protoplasm, there is no evidence that such a rearrangement of its constituent parts takes place as to give to each daughter cell one-half of the protoplasm from each pole of the mother cell. It is probable that each daughter nucleus simply becomes invested by that portion of protoplasm which lies in proximity to it at the time when the constriction of the protoplasm begins. The young daughter cell, seeing that it is composed both in its nucleus and protoplasm of a portion of each of these constituent parts of the mother cell, possesses therefore properties derived from them both.[1]

Owing to the disappearance of the nuclear membrane at the end of the spirem stage of karyokinesis, at least in cells generally (though it is said to persist in the Protozoa during the whole process of karyokinesis), it follows that the nucleoplasma and the cell protoplasm cease for a time to be separated from each other, and an interchange of material may take place between them in opposite directions—both from the protoplasm to the nucleus, as Strasburger contends, and from the nucleus to the protoplasm, as has in addition been urged by M. Carnoy. In every case it should be remembered that the nucleus, being surrounded by protoplasm, can only obtain its nutrition through the intermediation of that substance, and thus there is always a possibility of the protoplasm acting on the nucleus, and in so far modifying it.

Having now sketched the progress of knowledge of the structure of cells and their mode of production, I may, in the next instance, state the present position of the subject. We have seen that the original conception of a cell was a minute, microscopic box, chamber, bladder, or vesicle, with a definite wall, and with more or less fluid contents. This conception was primarily based upon the study of the structure of vegetable tissue; and, as regards that tissue, it holds good to a large extent to the present day. For the cellulose walls of the cells of plants, with their various modifications in thickness, markings, and chemical composition, constitute the most obvious structures to be seen in the microscopic examination of vegetable tissue. Within these chambers is situated the active, moving protoplasm of the cell, and in it the nucleus is embedded; the cell also contains the sap, crystals, starch granules, or other secondary products. The cell wall is to all appearance produced by a conversion of, or secretion from, the protoplasm. But even in plants a cell wall is not of necessity always present; for, in the development of the daughter cells within a pollen mother cell, there is a stage in which the daughter cell consists only of a nucleated mass of protoplasm, prior to the formation of a cell wall around it by the differentiation of the peripheral part of its protoplasm. Again, the so-called non-cellular plants or Myxomycetes, before they develop their spores,[1] consist of masses of naked protoplasm, on the exterior of which, in the course of time, a membrane of cell wall is differentiated; in the substance of these masses of protoplasm numerous nuclei are situated.[2]

In animal tissues the fat cell possesses a characteristic vesicular form, with a definite cell wall, but neither in it nor in the vegetable cells does the cell wall exercise any influence on the secretion either of cell contents or of matters that are to be excreted. In animal cells a cell wall is frequently either non-existent, or doubtful, and when present is a membrane of extreme thinness. Animal cells, therefore, do not have as a rule the chamber-like form or vesicular character of vegetable cells.

The other constituents of the cell, and the only essential constituents, are the nucleus and the material immediately surrounding it, in which the nucleus is embedded. It is of secondary importance whether this material be called protoplasm, or bioplasm, or germinal matter. The term protoplasm, however, is that which has received most acceptance. In adopting this term, it should be employed in a definite sense to express the translucent, viscid, or slimy material, dimly granular under the lower powers, minutely fibrillated under the highest powers of the microscope, which moves by contracting and expanding, and which possesses a highly complex chemical constitution. The term ought not to embrace either the cell wall of the vegetable or animal cell, or the intercellular substance of the animal tissues. For although these have in all probability been originally derived from the protoplasm, by a chemical and morphological differentiation of its substance, or as a secretion from it, they have assumed formal and special characters and have acquired distinct functions. Protoplasm, as above defined, is a living structure endowed with great functional activity. It possesses a power of assimilation, and can extract from the appropriate pabulum the material that is necessary for nutrition, secretion, and growth. Growth takes place not by mere accretion of particles on the surface, but by an interstitial appropriation of new matter within its most minute organized particles. In cases, also, where the media in which the cell lives are suitable, as in the freely moving Amœba, or the white blood corpuscles, portions of the protoplasm may separate by budding from the general mass of the cell, and assume an independent existence; but the conditions under which the budding off of protoplasm can take place are exceptional in the higher organisms. Protoplasm, therefore, according to this definition, in addition to being a moving contractile substance, is the nutritive and secreting structural element of the tissues, and is always found relatively abundant where growth and the nutritive processes are most active.

In the fertilized ovum, after the process of segmentation has begun, and in the earlier stages of development of the embryo, the cells are nucleated masses of protoplasm, without cell-walls, and with no intercellular material. In the course of time, in animals more especially, an intercellular substance arises apparently by a

[1] Dr. J. M. Macfarlane has described as constantly present within the nucleolus of vegetable cells a minute body, which he terms *nucleolo-nucleus* or *endonucleolus*. He considers it, as well as the nucleolus, to become constricted and divided before the nucleus and the cell pass from the resting into the active phase of cell multiplication. See Trans. Bot. Soc. Edin., 1880, vol. xiv., and Trans. Roy. Soc. Edin., 1881–82, vol. xxx.

[1] "Lectures on the Physiology of Plants," by Julius von Sachs; translated by H. Marshall Ward, Oxford, 1887.

[2] The opinion for long entertained that the simpler algæ and fungi and cryptogams generally are destitute of nuclei has been shown by Schmidt and others to be incorrect.

differentiation of, or secretion from, the protoplasm. In many of the tissues this substance acquires such characters, magnitude, and importance as to overshadow the nucleated masses of protoplasm which it lies between and surrounds. The intercellular substance is the principal representative of the "formed material" of Dr. Beale. I cannot, however, agree with him in regarding it as passive and non-living or dead; for morphological and functional changes take place in it long after its original formation. Thus the hyaline matrix, or intercellular substance, of the young costal cartilages becomes converted into a fibrous matrix in the later period of life, and the striated substance of muscular fibre is one of the most physiologically active tissues in the animal body. In the general economy of the tissues, in the fitting of each to discharge the function for which it is specially intended, the intercellular substance plays an essential part. It gives strength to the bones, toughness and elasticity to ligaments and cartilage, motor power to muscles. It wastes by use and needs repair. But it is probably to the nucleated protoplasm within its substance that we are to look for the structural element which attracts to it the pabulum required for its nutrition, so that the interstitial waste which is consequent on its use may be made good.

The nucleus is also an active constituent of the cell, and in young cells is proportionately larger than when the cell is matured. It is doubtful if it plays a special part as a centre of attraction in secretion, or in the nutrition of the cell generally, an office which is most probably discharged by the protoplasm; but it undoubtedly acts as a centre for its own nutrition. Numerous observations, moreover, clearly prove the truth of the generalization originally propounded by Martin Barry, and confirmed by Goodsir, that the nucleus is intimately associated with the production of young cells. The karyokinetic phenomena which have been observed during the last fifteen years have established this on a firm basis, beginning with the segmentation of the yelk and nucleus within the fecundated ovum down to the latest period of cell formation.

But, along with the karyokinetic changes within the nucleus and its cleavage, there is also a cleavage of the protoplasm of the cell, so that the daughter cell consists of portions of both the nucleus and the protoplasm of the mother cell. The question therefore has been put whether the division of the protoplasm is a consequence or a coincidence of the division of the nucleus. I am inclined to think that the cleavage of the cell protoplasm is consequent on the nuclear changes; for it must be kept in mind that certain of the movements in and rearrangement of the chromatin fibres of the nucleus precede any rearrangement of particles in the cell protoplasm so far as yet observed, and, still more, the process of cleavage. Applying, therefore, to the cell the well-known economic principle of division of labour, and that differentiation of structure carries with it differentiation of function, I regard the protoplasm as the nutritive and secreting element of the cell, and the nucleus as its primary reproductive factor.

The present position of the cell theory differs therefore in many important respects from the doctrine advocated by Schwann and his immediate successors. Cells are no longer regarded as of necessity bladders or vesicles. A cell wall is not constant but of secondary formation. A free formation of cells within an extracellular blastema by deposition around a nucleolus to form a nucleus, and then around the nucleus to form a cell, does not take place. Young cells arise from a parent cell by division of the nucleus, followed by cleavage of the cell protoplasm, so that each cell is directly descended from a pre-existing cell. Although in so many of its details, therefore, the theory of Schwann has been departed from, yet the great generalization of the cellular structure of plants and animals holds good, and his work will continue to mark an epoch in the progress of biological science.

The study of the very remarkable series of karyokinetic phenomena above described has given an impulse to speculation and thought in connection with some of the most abstruse problems of life and organization. The question of the hereditary transmission of properties, both as regards the constituent tissues of the organism and the individual as a whole, has been put on a more definite physical basis. The penetration of the ovum by the spermatozoon, originally seen by Martin Barry in the rabbit, and extended to other animals some years afterwards by Newport, Bischoff, and Meissner, has been completed by the recent researches of Bütschli, Auerbach, Fol, Hertwig, and E. van Beneden. The conjugation or incorporation of the male pronucleus or head of the spermatozoon with the female pronucleus derived from the germinal vesicle, and the consequent formation of the segmentation nucleus, has been demonstrated. The segmentation nucleus is built up of chromatin fibres and nucleoplasm, derived from both the nucleus of the male sperm cell or the spermatozoon and the nucleus of the female germ cell. It is therefore a composite or hermaphrodite nucleus, and represents both parents. The cells derived from the segmentation nucleus in the early stage of segmentation contain chromatin nuclear particles, which are in direct descent from the chromatin fibres of the segmentation nucleus, and through it from the corresponding fibres of both the sperm and germ cells. From Nussbaum's and E. van Beneden's observations it would seem that each nucleus of the first pair of segmentation cells contains one-half of the chromatin threads of the male, and one-half of those of the female pronucleus. It is possible that an equal division of the male and female components of the nuclei takes place in every subsequent nuclear division, in which case the nucleus of every cell would be hermaphrodite or composite—that is, would represent both parents. The segmentation cells arrange themselves to form the blastoderm, which, in the more complex organisms, by the continuous subdivision of the cells, forms three layers; from which, by a prolonged process of cell division and differentiation, all the tissues and organs of the adult body are ultimately derived. Karyokinetic changes mark the process of cell division throughout, and each daughter cell receives from the mother cell chromatin nuclear material derived from both parents, which, without doubt, conveys properties as well as structure.

In the division of the segmentation nucleus within the ovum a cleavage of the protoplasm of the egg also takes place, and each daughter nucleus is enveloped by the protoplasm of the maternal egg. If during the period of nuclear division there is no interchange of matter between the nucleus and the protoplasm which incloses it, the cell protoplasm would then be derived solely from the ovum, and would represent maternal characters only, whilst the nucleus would possess characters derived from both parents. But if, as is most likely, during the process of karyokinesis, when the nuclear membrane has disappeared, an interchange of matter takes place between the nuclear substance and the cell protoplasm, the latter would then become, if I may say so, inoculated with some at least of the nuclear substance, and be no longer exclusively of maternal origin. Again, it should be stated that, as E. van Beneden has described, when the spermatozoon enters the egg it takes with it a portion of the protoplasm of the sperm cell. This apparently blends with the protoplasm of the egg itself. With Waldeyer, therefore, I would ask the question, Is this altogether without significance? It would seem, therefore, as if the whole of the cells of the body and the tissues derived from them are, as regards both nucleus and cell protoplasm, descended from material originally belonging to both parents.

Although ova in different organisms differ materially from each other in size, shape, the relative amount of food yolk which they contain, the mode of segmentation, and the presence or absence of a segmentation cavity, they all agree in this that the primordial cells of the egg are nucleated masses of protoplasm. Notwithstanding the general resemblance of the morphological units which thus mark the first stage in the production of young organisms, each fertilized ovum gives rise to an organism resembling that in which the egg itself arose. Hence the offspring resemble the parents, and the species is perpetuated by hereditary transmission, so long as individuals remain to keep up the reproductive process. During sexual reproduction the substance of the segmentation nucleus undergoes karyokinetic changes during the act of segmentation, and the question arises if the process of karyokinesis is the same for all organisms, whether plants or animals, or if there are specific differences. As the fertilized ovum is potentially the organism which is to arise from it, specific differences not unlikely exist in the minute structure of the segmentation nucleus, which may perhaps be expressed by modifications in the arrangement of the chromatin fibres and in the number of their loops. The varieties which have been described in the forms of the karyokinetic figures and polar radiations in different plants and animals may perhaps mark these specific differences.

But there is another question which merits consideration. Are the karyokinetic phenomena which show themselves in the cells of a given tissue characteristic of that tissue? and, if so, would it be possible to distinguish one tissue from another in the same organism by differences in the process of cell division? On this point a commencement seems to have been made towards obtaining some positive knowledge. Strasburger and Heuser think that they have obtained evidence in certain plant cells that such is the case; Rabl concludes, from observations on the epidermic cells of salamander, that the loops of chromatin fibres are constantly twenty-four in number in the same kind of cell in the same species of animal.

But in considering the different kinds of tissue, and the possibility of each kind possessing its characteristic karyokinetic process, it has to be kept in mind that more than one kind of tissue, each of which has its characteristic structure and function, arises from each layer of the blastoderm, so that there is a stage in development —a stage of indifferentism, if I may use the expression —when the blastoderm represents several tissues which have not yet differentiated. From the epiblast, for example, tissues so diverse in structure and function as cuticle and nerve tissue arise. Now, if there be a special karyokinetic process for the epidermal cells, and another for the nerve cells, does either of these correspond with the process of nuclear division in the cells of the epiblast in their stage of indifferentism, or do they both differ from it? When does the impulse reach the layers of the blastoderm, so as to produce in their constituent cells changes which so alter the characters of the cells as to lead to a differentiation into various forms of tissues, and to what is that impulse due? In the development of each species there seems to be a definite time within certain limits when the differentiation shall begin, and when the process of development of the tissues and organs shall be completed. This is an hereditary property, and is transmitted from parents to offspring. Is the impulse derived from the nucleus or from the cell protoplasm, or do both participate? As already stated, the nucleus is the element which is immediately descended from both parents, and which may therefore be supposed to be the primary morphological unit through which hereditary qualities are transmitted. But, as is most probable, the nucleus reacts on the cell protoplasm—on the element of the cell through which the ordinary nutritive functions are discharged. As a consequence of this reaction, when the appropriate time arrives in the development of each species for the commencement of the differentiation of the protoplasm of a cell, or group of cells, into a particular kind of tissue, the necessary morphological, chemical, and physiological changes take place. When once the differentiation has been effected, it is continued in the same tissue throughout the life of the organism, unless, through some disturbance in nutrition, the tissue atrophies or degenerates. Every multicellular organism, in which definite tissues and organs are to arise in the course of development, has therefore a period, varying in its duration in different species, in which certain of the properties of the cells are as it were dormant. But, under the influence of the potent factor of heredity, they are ready to assume activity as soon as the proper time arrives. When the process of differentiation and development is at an end, the organism has attained both its complete individuality as regards other organisms, and its specific characters.

Every organism, therefore, has to be viewed from both these points of view. Its specific position is determined by that of its parents, and is due to the hereditary transmission of specific characters through the segmentation nucleus. Its individuality is that which is characteristic of itself, and arises from the fact that in the course of development a measure of variability within the limits of a common species, from the organic form exhibited by its parents and by their other offspring, is permitted. In all likelihood the variability, as Weismann suggested,[1] is, to a large extent, occasioned by the bisexual mode of origin of so many organisms. By the expulsion of the polar bodies, during the maturation of the egg, portions of the ancestral germ plasm may be removed, and as corresponding molecules need not be expelled from each ovum, similar ancestral plasms would not be retained in each case, so that diversities would arise. There is also a possibility of the molecular particles of the segmentation nucleus and of the nuclei of the cells descended from it, having a method of arrangement and adjustment, and a molecular constitution characteristic of the individual as well as of the species. On this matter we have, however, no information. It is as yet a mere hypothesis. When we consider the extreme minuteness of the objects referred to, and recollect that it is only about fifteen years since karyokinetic phenomena were first recognized, it is astonishing what progress in knowledge has been made within this limited period. We owe this great advance to the much more complete magnifying and defining power of our microscopes, to the improved method of preparation of the objects, and to the acute vision and clear-thinking brains of those observers who have worked at the subject. By continuing the work, and extending it over a wider area, we may hope in time to be able to solve many questions to which we cannot now give an answer.

The nuclear material which makes up the substance of the male and female pronuclei, by the fusion of which the segmentation nucleus is formed, has been termed by Prof. Weismann the *germ plasm*. In a series of elaborate papers he has developed a theory of heredity,[2] based upon the supposed continuity of the germ plasm. He believes that in each individual produced by sexual generation a portion of the germ plasm derived from both parents is not employed in the construction of the cells and tissues of the soma, or personal structure of that individual, but is set aside unchanged for the formation of the germ cells of the succeeding generation—that is, for reproduction and the perpetuation of the species.

[1] See his essays, "The Significance of Sexual Reproduction in the Theory of Natural Selection," and "On the Number of Polar Bodies and their Significance in Heredity," translated in "Essays on Heredity" (Oxford, 1889).

[2] Translations of these papers have been published by the Clarendon Press, Oxford, 1889.

According to this theory, the germ plasm, more especially through the chromatin fibres, is the conveyer of hereditary structure and properties from generation to generation. Further, he holds that the cells, tissues, and organs, which make up the somatic or personal structure of the individual, exercise no modifying influence on the germ or reproductive cells situated in the body of that individual, which cells are also, he thinks, unaffected by the conditions, habits, and mode of life. In its fundamental idea Weismann's theory is in harmony with one propounded a few years earlier by Mr. Francis Galton (Proc. Roy. Soc. London, 1872; and Journ. Anthrop. Inst., vol. v., 1876).

In an address delivered at Newcastle in September last to the Anthropological Section of the British Association (NATURE, September 26, 1889), I reviewed this theory of heredity, and, whilst finding in it much with which one could coincide, I directed attention to points to which, I thought, objection might be taken. More especially I took exception to the idea that the germ plasm was so isolated from the cells of the body generally as to be uninfluenced by them, and to be unaffected by its surroundings.

On this occasion I propose to say a few words on the bearing of this theory on the development of the tissues and organs of the individual. If we examine the development of the embryo, say of one of the Vertebrata, we find that it makes a certain advance, varying in its time and extent according to the species, without any differentiation of a reproductive organ with its contained germ plasm being discoverable. I shall not enter into the much-disputed question of the layer or layers of the blastoderm from which the reproductive cells take their rise. But I may say that in the chick, both in the third and fourth day of incubation, a layer of germinal epithelium may be seen in close relation to the Wolffian duct and the pleuro-peritoneal cavity. At the end of the fourth day or in the fifth day this epithelium becomes thickened, and the primordial ova appear in it as distinctly differentiated cells. In the rabbit a corresponding differentiation does not appear to take place before the twelfth or thirteenth day. Up to the period of differentiation of the primordial ova, no isolation or separation of the reproductive cells and germ plasm has taken place; and, so far as observation teaches, there is nothing to enable one to say which cells of the blastoderm may give rise to primordial ova, or which may differentiate into cells for other histogenetic purposes. But before the germ cells appear, the rudiments of the nervous, vascular, skeletal, muscular, tegumentary, and alimentary systems, and the Wolffian bodies or primordial kidneys have all been mapped out. Up to this time, therefore, in all probability, a more or less complete diffusion of the germ plasm throughout either one or more of the layers of the blastoderm has taken place. The hereditary influence conveyed by the germ plasm would thus be brought to bear upon the cells of the blastoderm generally, so as to impart to them the power of undergoing at the appropriate period the morphological and chemical differentiation to form the several tissues. As the tissues and organs are derived through division of the nuclei from the cells of the blastoderm, the continuity of the hereditary influence exercised over them is kept up, even after the germ plasm has become isolated, and the entire organism is moulded so that it acquires its specific and individual characters.[1]

But the diffusion of the germ plasm throughout either the whole of the blastoderm, or a part thereof, of necessity so intimately associates it with the formative cells of the tissues generally, that it is difficult, if not impossible, to comprehend how in its turn it can be unaffected by them. Before, therefore, it again becomes stored up or isolated in an individual, in the form of ova or sperm cells, it has in its stage of diffusion been brought under precisely the same influences as those which in the embryo affect the formative cells of the whole body.

From the observations and reasoning of Wilhelm His (Proc. Roy. Soc. Edin., April 2, 1888), there can, I think, be no question that the layers of the blastoderm are affected by pressures and other mechanical conditions. These pressures would produce or modify flexures, or occasion a diminution in dimensions in some directions and an increase in others; and in this way would tend to affect either the form of the entire organism, or the form and relations of its constituent parts, or perhaps both. Should such modifying influences come into operation either before the isolation of the germ plasm, or when it was in a plastic or impressionable condition, one could conceive that it might be affected by them. Molecular changes might thus be induced in the germ plasm of such a kind as to modify the properties of the chromatin constituent of the nuclei, so as to induce in it and the germ plasms descended from it corresponding modifications, which would become hereditary. If such an hypothesis be granted, it would follow that the external conditions would exercise a perceptible influence on the germ plasm of the reproductive cells, both in the individual in which they first manifested their effect and in the generations which are descended from him.

If the germ plasm, from the first stage of development of each organism, were completely isolated from the cells from which all the other cells of the body were produced, it would be possible to conceive its transmission from generation to generation unaffected by its surroundings. But as in each individual a stage of diffusion or non-isolation precedes that of differentiation into the special reproductive apparatus, it follows that the conditions which would secure the germ plasm and the soma cells from mutual interaction are not complied with. On this ground, therefore, as well as for the reasons previously advanced in my Newcastle address on heredity, I am unable to accept the proposition that there can be no transmission to the offspring, through the reaction of the soma on the germ plasm, of characters which may be acquired under direct external influences. But in questioning the accuracy of the proposition that somatogenic "acquired characters" are incapable of being transmitted, I do not of course contend that all the characters which may be acquired during the lifetime of an individual are perpetuated in his descendants.

THE LABORATORY OF VEGETABLE BIOLOGY AT FONTAINEBLEAU.

THIS Laboratory, the establishment of which we have already announced, has now been in full working order, as far as the present buildings will permit, since May 15. We are enabled to furnish our readers with the following account of its scope and design, with the accompanying sketches, from an article supplied by M. Jumelle to the *Révue Générale de Botanique*.

The Laboratory was established at the suggestion of M. Liard, the Director of Higher Instruction in France, and

[1] On the question of the influence exercised by the germ plasm on the tissues, I may refer to some most suggestive remarks by Sir James Paget, published forty years ago, in the *London Medical Gazette*, 1849, in one of his lectures (VI.) on "The Processes of Repair and Reproduction after Injuries:"—"In every impregnated germ we must admit that properties are implanted which, in favourable conditions, issue in the power to form, of the germ and the materials it appropriates, a being like those from which it sprang. And, mysterious as it may seem, yet must we conclude that a measure of those properties is communicated to all the organic materials that come within the influence of the germ; so that they, being previously indifferent, form themselves in accordance with the same specific law as that to which the original materials of the germ are subject. So through every period of life the same properties transmitted and diffused through the whole organism are manifested in the determination of its growth and maintenance in its natural degeneration, and its repair of every part, in accordance with that type or law which has prevailed in every individual of the species." See also a lecture "On the Formative Process," in "Lectures on Surgical Pathology," vol. i., London, 1853.

is regarded as an *annexe* of the Botanical Laboratory of the Faculty of Sciences at the Sorbonne, being under the direction of M. Gaston Bonnier, the Professor of Botany at the Sorbonne. Its foundation was due to a consideration of the difficulties which necessarily attach to culture-experiments and physiological researches when carried on in large towns. For these purposes an advantageous locality presented itself in Fontainebleau, with the rich herbaceous and arborescent flora of the neighbouring forest, an abundant supply of water, and ready access to Paris. A portion of the forest (see Fig. 1) is inclosed within the territory attached to the Laboratory.

It is intended in the future to make important additions to the building at present constructed. The room for experimental researches is represented by S in Fig. 2; it will accommodate 24 workers. Those who are engaged in microscopical researches, or the study of the lower forms of vegetable life, work in galleries placed at about one-half the height of the room; the floor is intended for physiological experiments, which, with the necessary apparatus, require a larger space. Here are placed the instruments required for the study of vegetable chemistry—furnaces, balances for delicate work, glycerin-troughs, apparatus for sterilization, &c.; others will be added as needed. Opening out of this hall (B and P, Fig. 2) are the director's room and the library. Other rooms (A, A, Figs. 2 and 3) are occupied by M. Duval, the director of cultures; and on the upper story are chambers for some of the students. In the grounds attached are frames (C, Fig. 1), and a greenhouse (S)

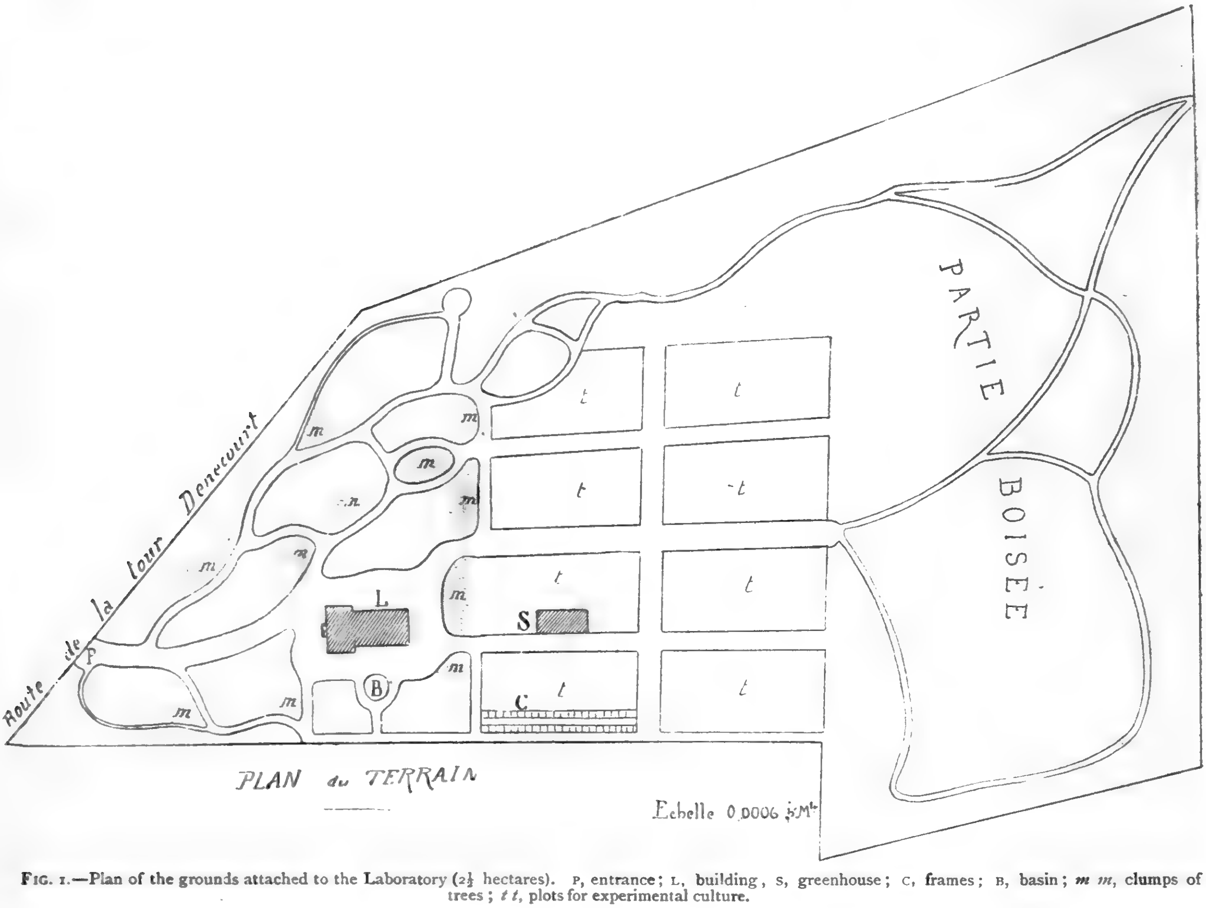
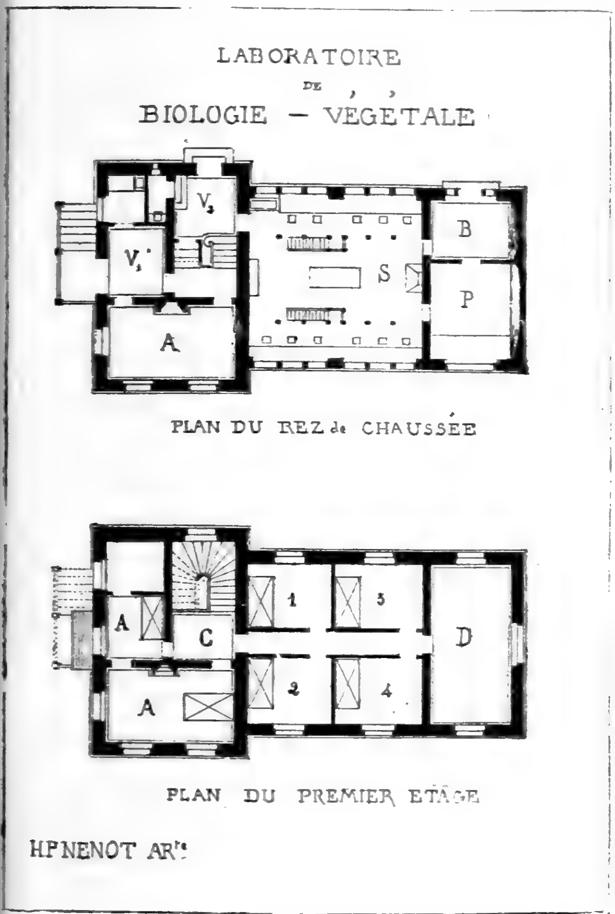


Fig. 1.—Plan of the grounds attached to the Laboratory (2½ hectares). P, entrance; L, building, S, greenhouse; C, frames; B, basin; *m m*, clumps of trees; *t t*, plots for experimental culture.

divided into a hot and cold portion; in the latter is placed apparatus necessary for physiological experiments.

One object aimed at by the establishment of the Laboratory is to provide facilities, already afforded in the case of animals in zoological laboratories, for the study of plants in the localities and under the conditions in which they are found in Nature. Hitherto, experiments in vegetable physiology have been mainly carried out on specimens which have been transplanted for the purpose, and which are therefore subjected to unfavourable or unnatural conditions of light, air, soil, &c., which may materially invalidate the results. This objection applies largely to experiments carried on in botanic gardens. To meet this difficulty, a portion of the actual forest (see Fig. 1) has been inclosed within the precincts of the Laboratory; and numerous plots (*t*, *t*) are also set apart for experimental cultures. In the former are a number of trees and herbaceous plants growing in their natural habitat in all stages of development. It is hoped that in this way much light will be thrown on the diseases, whether due to the attacks of parasites or to unfavourable vital conditions, to which plants are subject. It is intended that the culture-plots shall not be devoted exclusively to the use of students in the Laboratory, but that other experiments shall be carried on by M. Duval at the desire of, and under the conditions prescribed by, outside workers. The study of vegetable pathology, and especially of the diseases which attack vines and forest-trees, is one of the special objects for which the Laboratory has been established; and, to aid in these researches, relations have already been opened with horticulturists and viticulturists, and with the Administration of Forests; and the occupiers of neighbouring estates have placed their resources at the disposal of the director.

In addition to the Laboratory at Fontainebleau, a new one is also being built at the Sorbonne itself, with a large lecture-room, galleries for collections, a library, a greenhouse, a photographic room, and twelve workrooms.



FIGS. 2, 3.—Ground floor: V V, vestibules; S, hall for research; P, Director's laboratory; B, library. First floor: C, landing: D, 1, 2, 3, 4, chambers for students; A A, rooms of the director of cultures.

The Director of the Laboratory at Fontainebleau offers a cordial invitation to foreign botanists who may desire to avail themselves of the facilities for work afforded by it.

BENJAMIN FRANKLIN.

THE Official Report, issued by the American Philosophical Society, of the celebration of the centennial anniversary of the death of Benjamin Franklin, April 17, 1890, has just been sent to us. The Society, in January last, held a stated meeting, at which it was resolved to form a Committee of five members, to be appointed by the President, who should be empowered to take all necessary action. The following was the form of the resolution that was drawn up by the Committee:—"That we commemorate in a becoming way the approaching centennial anniversary of the death of Benjamin Franklin, and that a series of short addresses upon his life, character, and work be delivered before the Society upon this occasion."

The Society is to be congratulated on the success with which the idea was carried out. It is right that the world, on fitting occasions, should be reminded of the achievements of so great an investigator. Franklin had all the qualities of mind which are necessary for the extension of knowledge by means of observation and experiment, and his career marked one of the most important stages in the development of science. Englishmen regard his work with hardly less pride than Americans, and the memory of what he accomplished must always be one of the links that serve to unite the two peoples.

In the evening of the anniversary day, members and their guests assembled at the Association Hall in the city of Philadelphia. Mr. Talcott Williams began the proceedings with a few remarks on the occasion of the gathering. He then informed the audience that the Society had summoned the biographer of Franklin; it had called upon the historian of the land in which he served his country, upon the man of science, upon one who was both the man of science and of letters, and lastly upon the President of the Society, to represent the civic and associated activities in which Franklin was engaged.

In the following notes we shall give brief extracts from some of the speeches that were delivered. Franklin, the youngest son of a family of seventeen, began his career at Boston. He wished to go to sea, but his mother thought it would be better for him to become a minister. His father, having tried hard to make him a tradesman, bound him over to an elder brother to learn printing. The apprenticeship did not last long, as the brothers disagreed, and what with insults, quarrels, and blows, they parted—"the one to drag out a humble existence, the other to become the most illustrious American of his day." Travelling about looking for work, Franklin came across William Keith, who sent him to Boston, and then to London. On reaching the latter place he found out that "Keith was a knave, and himself a dupe." During his stay in London he wasted his substance, misused money, and kept bad company; but after some time he made the acquaintance of a merchant, who gave him a situation. Going back to Philadelphia with him, he commenced to "keep books, sell goods, and learn the secrets of mercantile affairs." Owing to the death of the merchant, Franklin was once more at large, and this time, with the help of a friend, he set up a "new printing office in High Street near the market."

From that hour he prospered, bought out his partner, married, founded one of the best newspapers, published the famous almanac, and was made Postmaster-General. Having now become wealthy by a strict adherence to the maxims of "Poor Richard," he sold his shop, newspaper, &c., and gave his time to the study of science. Before he had reached the age of fifty he had won for himself the Copley Medal, and membership of the Royal Society.

He now dropped scientific studies and returned to politics, and was loaded with public duties. Among other things he was appointed Postmaster-General of the Colonies, sent by the Assembly with its Speaker to hold a conference with the Indians at Carlisle, and then to the Albany Conference, where he presented his famous plan of union. On his return to Philadelphia in 1761, his intention was to settle down again and study, but he was again drawn into politics by the conspiracy of Pontiac, and the massacre of the Conestoga Indians by the men of Donegal and Paxtang.

After this he wrote many articles on American affairs for the English newspapers, and it was about this time that he republished a London edition of the "Famous Letters," and brought out "The Votes and Proceedings of the Freeholders and other Inhabitants of

Boston"; he also sent over the "Hutchinson Letters," and underwent the memorial examination before the Privy Council.

Having been abroad ten years and six months, he turned his face homewards, and found that great changes had taken place; the city had grown considerably, and the people were more prosperous. The greatest change seems to have occurred in his family; his wife was dead, his daughter married, and his son estranged in politics. Luckily, Franklin was soon absorbed again in public affairs, so that he had no time to consider his misfortunes.

The day after he landed, he "was chosen a member of the Continental Congress, took his seat four days later, and served for 14 months; was on eleven committees, was made Postmaster-General; was sent on one mission to Washington at Cambridge, and on another to Arnold at Quebec; was despatched after the disastrous battle of Long Island, to confer with Lord Howe; and in September 1776, was sent out to join Arthur Lee and Silas Diane in France."

There he was received with great enthusiasm, and "became the sensation of the hour." He concluded the treaty of alliance with France, the treaty of amity and commerce, negotiated loans for great sums of money, and in 1783 signed the treaty of peace with Great Britain.

In 1785, old and loaded with honours, he returned to Philadelphia. Here he was made a Member of Council, and the Council and Assembly made him President of the State, and while holding this office he was sent to the Convention that framed the Constitution of the United States. It was about this time that his fame was at its highest, and everyone honoured and looked up to "the venerable Dr. Franklin, our illustrious countryman and friend of man," "the father of American independence." The closing years of his life were passed among his friends and admirers, and he died on April 17, 1790.

The epitaph that was placed over his tomb was written by himself, and is quite worth repeating:—

"The body
of
BENJAMIN FRANKLIN,
Printer
(like the cover of an old book,
its contents torn out,
and stript of its lettering and gilding),
lies here food for worms.
Yet the work itself shall not be lost;
for it will, as he believed, appear once more
in a new
and more beautiful edition
corrected and amended
by
The Author."

Franklin was a voluminous writer, and wrote with expression. To use the words of Mr. Brown Goode, who spoke on "the literary labours of Benjamin Franklin," "he wrote habitually with a single eye to immediate practical results. He never posed for posterity. Of all the writings to which he mainly owes his present fame, it would be difficult to name one which he gave to the press himself or of which he saw the proof. Yet he never wrote a dull line, nor many which the century of time has robbed of their interest or value."

The literary remains of Franklin may be classified as follows:—

(1) "The Autobiography," from 1706–57.

(2) "Poor Richard's Almanac," in twenty-six annual issues, 1732–58, culminating in "Father Abraham's Speech at the Auction."

(3) Essays upon manners, morals, and the science of life, including the so-called "Bagatelles"; in all sixty titles or more.

(4) Tracts and papers upon political economy, finance, and the science of government; in all about forty titles.

(5) Essays and tracts, historical and political, concerning the American Revolution and the events which immediately preceded and followed, 1747–90.

(6) Scientific papers from 1737–90; in all 221 titles, and nearly 900 octavo pages.

(7) Correspondence, diplomatic, domestic, and literary, 1724–90; in all some twelve hundred letters, while many still remain unpublished.

Dr. J. W. Holland gave a very interesting account of the scientific labours of Franklin in the various branches of natural philosophy. Considered in their general character, they fall into a few groups, such as labour in sanitary science, in the art of navigation, in meteorology, and in electricity.

The science of electricity in Franklin's time was in its infancy. The only facts known about it were that some substances could be electrified, that the zigzag sparks which could be drawn from a rude machine resembled the lightning-flash, and that the Leyden jar enabled the experimenter to "imprison the fiery spirit and perform many remarkable tricks with it." Having reached middle age and just retired from business, he set to work with the frictional machine and the jar.

He first formed "a coterie for mutual suggestions and encouragement" with three of his friends who were very interested in these experiments; and between them they made a great advance, constructing their own machines, and making "new demonstrations of attraction and repulsion, and of the power of electricity to produce light, heat, mechanical violence, nervous shock, and even death." As a result of these and other experiments, he invented the lightning-conductor, which at that time was accounted "the most brilliant application of science that had been known," and projected his apt and simple theory of an electric fluid, explaining the cause of positive and negative action, which "held sway for so many years that to this day its nomenclature is retained in spite of defects revealed by recent advances in knowledge."

In concluding these brief extracts, we cannot do better than quote the words of one of the speakers:—

"Such men are few in any age; their number is not great in all the combined centuries that together make up the short life of our race upon this planet. It is only meet that we should cherish their names with respect, and gratefully hand down to posterity the story of their honourable and meritorious deeds."

NOTES ON THE HABITS OF SOME COMMON ENGLISH SPIDERS.

SOME years ago I sent to NATURE (vol. xxiii. p. 149) an account of the behaviour of the common small garden spider when a sounding tuning-fork is brought near. If the fork is made to touch any part of the web, or the twigs or leaves by which the web is supported, the trembling of the web completely deceives the spider, so that, after rapidly finding which radial line is most disturbed, she runs along this one and attempts to secure the tuning-fork. She fails to discover in the cold and polished steel anything different from her usual food; or rather, being led by instinct to eat that which buzzes, she struggles in vain to find a soft place in the armour of her prey.

On the other hand, if the tuning-fork is brought near one of these little spiders while she is waiting in the centre of her web, she generally drops instantly, but will climb up again as quickly as possible if the vibrating fork is made to touch the web.

More recently Mr. and Mrs. Peckham, who have made an elaborate study of the mental powers of spiders (*Journal of Morphology*, vol. i. p. 383), have repeated these experiments, and have confirmed them in every essential particular.

They found that many geometrical spiders would drop when a vibrating tuning-fork was brought near them, but that after much teasing in this way they would sometimes learn to take no notice. They conclude that this dropping habit is of direct service to them, in enabling them to escape from birds or wasps which prey upon them.

While staying recently with Mr. Romanes, in Ross-shire, I made some observations in this connection which are possibly worth recording.

The small geometrical spiders which abounded on the gorse bushes near the sea behaved as described above, while, as I have noticed many times before, the diadema spiders, which also were abundant, were affected in a totally different manner. If the tuning-fork is held near them, they throw up their four front legs, either perpendicularly or even further back, and as soon as the fork is within reach strike at it so violently that the blow may be plainly heard. A buzzing insect carried near is caught by the diadema spider in this way, and speedily wound up.

There were a number also of small brown geometrical spiders, which I believe were young diademas; these dropped when a sounding tuning-fork was brought near them even more readily than the full-grown little spiders.

Instead of bringing a tuning-fork near the spiders, I made a sudden and high-pitched shout, taking care that my breath should not complicate the situtation. The effect, when a great number of spiders were resting on their webs near together, was sufficiently striking. The diademas threw up their legs simultaneously, and struck in the air at the imaginary insect, while the full-grown little spiders, and what I believed to be the young diademas, all dropped out of their webs into the branches below.

The suggestion of Mr. and Mrs. Peckham, that this habit is a protection against wasps, is made the more probable by the difference in the behaviour of the full-grown diadema, which would certainly not be afraid of a wasp, and the little spiders. However, the tactics of a wasp that I watched left no doubt in my mind that this explanation is correct. The wasp, when I first saw it on a gorse spray, was evidently intent on something. It ran up the spray until it came to the silken tube in which the little spider dwells when not on the web. The spider retreated further into the tube, while the wasp was struggling among the spines and the silk to dislodge her. After a short time the wasp gave up the attempt, and flew away for a few yards. It then very suddenly darted at another spider, seized her before she had time to drop, and carried her off to a branch close by. This was done so quickly that I could not follow the details of the attack; but it is certain that the wasp, which did not carry a spider a moment before, had without alighting taken the spider off her web. It would appear that the dropping habit of the spider has reacted on the wasp, and has developed in it a speed of attack sufficient to counteract the spider's only means of escape.

I have not found that the little spider is less attracted by low notes than by high; a variety of forks, forceps from a box of chemical weights, or a carpenter's square banged on the knee, all seem to deceive her equally well, but a vibration of great amplitude causes her to retreat to a place of safety. The spider seems to judge of the necessity for prudence by the violence of the insect rather than by the natural note of its wings. She is terrified by a heated tuning-fork, which is not too hot to hold.

Mr. and Mrs. Peckham have formed a low estimate of the spider's intelligence as distinct from instinct. They found that a spider which has the habit of carrying its cocoon was quite satisfied with a lead shot slipped into the silk covering of the eggs, and laboriously carried about. The following are a few of many experiments which I have made, which lead to the same conclusion. A large diadema which had just caught and wound up a large fly, and had carried it up to its retreat, left it hanging by a short line while she proceeded, according to the usual habit of this kind of spider, to carefully clean herself before the meal. Meanwhile I managed to replace the fly by a piece of cork without disturbing the spider. When the toilet was complete, she pulled up the line from which the supposed fly was suspended, and tried to eat the cork. She was a long time trying every part of the cork before she finally let it drop. A piece of an india-rubber ring was twisted up until it had acquired a state (well known to school-boys) of spasmodic recoil. This was placed on the carpet-like web of a large black house-spider, which Mr. Pocock tells me is known to naturalists as *Tegenaria atrica*. These, like other house-spiders, appear to be far more wary than the geometrical sort. The india-rubber was made to move slightly by being pinched from below, and then the spider pounced upon it. I did not allow the spider to carry it off, but made it seem to struggle and resist by manipulation with a pair of forceps under the web. The spider became more and more desperate, and at last, when the web was much damaged by the battle, I dragged the rubber away; but the spider could not allow this, and clambering through the hole made in the web, and hanging by her fourth pair of legs, seized the escaping insect. I then let go, and the spider carried the piece of india-rubber away to her den, perfectly satisfied. However, she did not seem to appreciate her meal, for, after biting it on every side, she was obliged to take it to the edge of her web and drop it. I then picked it up, and was surprised to find the spider willing to be similarly deceived again.

These spiders will come to a tuning-fork once or twice perhaps, but the moment they touch it they fly terrified, as they do from a common bluebottle with mica on its wings. They seem generally thirsty, and will drink water placed upon the web, and if it is scattered in drops they are able to find the drops, but by what process I do not know. The diademas, too, especially when old, and only able to mend old webs, not to spin new ones, are always ready to drink. They will hold a piece of wheat straw six or eight inches long which has a drop of water upon it until they have drunk the water; but while the little spider is so insensitive in taste as not to entirely reject a fly that has been soaking in a parafin lamp, especially if it is made to buzz with a tuning-fork, the diadema has a strong objection to alcohol, even well diluted, and rubs her mouth against anything near by after tasting it, so as to get rid as quickly as possible of the noxious fluid. Is it possible that the numerous spiders which are found in secondary batteries have been killed by the acid when attempting to drink, or are they destroyed by accidentally meeting the acid in their ordinary descents? The *Tegenaria* is aware of the shout which causes the diadema to strike and the little spider to drop, but the effect is a jump such as is executed by anyone when suddenly startled.

It would appear that the only sense which is developed to any extent, and that most marvellously, is the sense of touch; hearing, taste, and smell to a small degree; but sight, as we understand the term, in spite of their numerous eyes, seems to be absent. The *Tegenaria* will stand within half an inch of a fly feigning death, without being able to find it; while the geometrical spiders, under like circumstances, gently pluck line by line until the effect of the inertia (not weight) of a motionless object guides them to the proper place.

These remarks do not apply to the hunting spiders.

C. V. BOYS.

NOTES.

The following is the list of names recommended by the President and Council of the Royal Society for election into the Council for the year 1891, at the forthcoming anniversary meeting on December 1:—President: Sir William Thomson. Treasurer: Dr. John Evans. Secretaries: Prof. Michael Foster, the Lord Rayleigh. Foreign Secretary: Archibald Geikie. Other members of the Council: Prof. William Edward Ayrton, William Henry Mahoney Christie, Prof. W. Boyd Dawkins, James Whitbread Lee Glaisher, Dr. Hugo Müller, Prof. Alfred Newton, Sir William Roberts, William Chandler Roberts-Austen, Prof. Edward Albert Schäfer, Sir George Gabriel Stokes, Bart., Lieut.-General Richard Strachey, R.E., Prof. Joseph John Thomson, Prof. Thomas Edward Thorpe, Sir William Turner, Prof. Sydney Howard Vines, General James Thomas Walker, C.B.

The recent delimitation of the spheres of influence of Great Britain and Germany in Tropical Africa will probably give a considerable impulse to the systematic investigation of its flora by the travellers and botanists of either nation. An arrangement has, therefore, been made between Kew and the Royal Botanic Museum at Berlin, for the regular exchange of African collections. In accordance with it, the extensive herbarium of Marocco plants formed by the late John Ball, F.R.S., after the selection for Kew of all unique speci mens, is about to be transmitted to Be rlin.

The Geological Photographs Committee, appointed by the British Association to arrange for the collection, preservation, and systematic registration of photographs of geological interest in the United Kingdom, have issued a circular inviting the co-operation of geologists in their work. At the Leeds meeting of the British Association, the Committee exhibited, as the result of their first year's operations, a collection numbering upwards of 270 photographs, many of which illustrated geological sections, and other features of considerable scientific interest. The Committee, however, point out that, so far as the work has yet proceeded, only a small proportion of the counties in the British Islands have been represented; and they urge upon geologists the desirability of further assisting the scheme, with the object of forming a national collection of photographs illustrating the geology of our own country, which will ultis mately be deposited in some convenient centre. The Committee wish to receive copies of suitable photographs, which will be duly numbered and registered, or to be favoured with the following information, viz.:—(1) Lists and details of photographs of geological character already in existence. (2) Names of local societies, or persons, who may be willing to further the objects of the Committee in their own district. (3) Particulars of localities, sections, boulders, or other features which it may be desirable to have photographed. A circular of instructions has been drawn up, copies of which will be supplied to any persons who may desire to have them. The Secretary of the Committee is Mr. Osmund W. Jeffs, 12 Queen's Road, Rock Ferry, Cheshire.

The American Association for the Advancement of Science will hold its session for 1891 at Washington. Among the subjects on which papers and discussion are especially invited are: the absorption of gases and of fluids by, and the movements of water in plants, the aëration of aquatic plants, and transpiration.

The eighth Congress of the American Ornithologists' Union will meet at Washington on November 18. The meetings will be held at the U.S. National Museum.

The Society of Arts has issued its programme of "sessional arrangements." The following are the Cantor Lectures, which will be delivered on Monday evenings at eight o'clock: Prof. Vivian B. Lewes, "Gaseous Illuminants" (five lectures), November 24, December 1, 8, 15, 22; A. J. Hipkins, "The Construction and Capabilities of Musical Instruments" (three lectures), January 26, February 2, 9; Gisbert Kapp, "The Electric Transmission of Power" (three lectures), February 16, 23, March 2; Prof. R. Meldola, F.R.S., "Photographic Chemistry" (three lectures), March 9, 16, 23; Hugh Stannus, "The Decorative Treatment of Natural Foliage" (four lectures), April 13, 20, 27, May 4.

One lamentable result of the very rainy summer and autumn has been the partial submergence of the Botanic Garden at Prague, by which incalculable damage has been done to the very fine collection of plants under the care of Prof. Willkomm, the work of many years' careful and untiring labour. The fires in the hot-houses were extinguished, and one glass-house entirely destroyed. The unique collection of succulent plants, including 200 species of *Sempervivum* and 320 of *Cactus*, suffered irreparable injury. The splendid collection of models and preparations only recently made has been rendered almost valueless; in the lecture-room, where it was placed, the water stood at a height of about 5 feet 6 inches.

A quaint custom, dating back to Anglo-Saxon times, known as payment of "wrath silver," was recently observed at Knightlow Hill, a tumulus between Rugby and Coventry. It consists of tribute payable by certain parishes in Warwickshire to the Duke of Buccleuch. The silver has to be deposited at daybreak in a hollow stone by representatives of the parishes, the penalty for default being forfeiture of a white bull with a red nose and ears. The representatives afterwards dined together at the Duke's expense.

The Deutsche Seewarte and the Danish Meteorological Institute have just issued two more quarterly volumes of synchronous daily weather charts of the North Atlantic, bringing the series of this valuable publication down to November 1886. These volumes show very clearly the difference of conditions existing over that ocean between summer and autumn; the difference of atmospheric pressure is much less in summer, and consequently the winds are lighter. A cursory glance at the low-pressure areas shows their important effect upon the weather of their vicinity, and the history of the severe gale which visited this country on October 15–16, 1886 (a discussion of which appeared in the Quarterly Journal of the Royal Meteorological Society for January 1887), may be clearly traced on the charts, as well as several other disturbances which reached our coasts from the Atlantic. The importance of such work can hardly be over-estimated, but its value would be further enhanced if the publication could be made nearer to current date.

The year ending last April was remarkable in point of weather in St. Petersburg. Thus (as Dr. Woeikof points out) only once in 130 years was the mean temperature of the seven months October to April warmer than in this year—viz. in 1821–22, when the excess was 4°·2 C. (this year 3°·6). The normal order of the months was not disturbed, as so often occurs in warm winters. It looked as if St. Petersburg had moved into a warmer and more continental climate. In the number of warmest days of the 130 years series, October and April (1889–90) are unequalled. Reckoning as summer days those with 16° (the mean of summer) or over, it appears that between the last and the first summer day the number of days in 1889–90 is 24 less than the shortest period hitherto observed, two months shorter than the mean, and 112 days shorter than in 1821–22. And it is curious that the period in question is longest in the year in which October to April was warmest (1821–22), but shortest in the next warmest (1889–90).

The following details of the earthquake in Persia in June, last have been received from Dr. Jellisew, a Russian doctor who, on the evening of the 28th inst., was at a considerable distance from Teheran, on his way from Meshed to the capital. The centre of the earthquake seemed to be Tasch, a village on the side of a mountain, and close to a deep precipice. The noise of the shocks was so loud that Dr. Jellisew was awakened from his sleep, and in a few minutes several houses fell together. Others had great cracks in their walls. Many people rushed into the fields; most of those who remained in the houses were killed. Large blocks of rock are said to have burst asunder.

A STALACTITE cave has been discovered recently in one of the State forests near Bissingen, in Würtemberg. The entrance is said to be 15 metres high, and 3 or $3\frac{1}{2}$ wide.

PROF. J. MARK BALDWIN contributes to *Science*, October 23, a note of some interesting experiments relating to the "origin of right- or left-handedness." The subject of them was his own child, and they extended over the greater part of the first year. The following are the points recorded:—(1) He found no trace of preference for either hand as long as there were no violent muscular exertions made (based on 2187 systematic experiments in cases of free movement of hands near the body, *i.e.* right hand 585 cases, left hand 568 cases, a difference of 17 cases; both hands 1034 cases; the difference of 17 cases being too slight to have meaning). (2) Under the same conditions the tendency to use both hands together was about double the tendency to use either (seen from the number of cases of the use of both hands in the statistics given above), the period covered being from the child's sixth to her tenth month inclusive. (3) A distinct preference for the right hand in violent efforts in reaching became noticeable in the seventh and eighth months. Experiments during the eighth month on this cue gave, in 80 cases, right hand 74 cases, left hand 5 cases, both hands 1 case. In many cases the left hand followed slowly upon the lead of the right. Under the stimulus of bright colours, from 86 cases, 84 were right-hand cases, and 2 left-hand. Right-handedness had accordingly developed under pressure of muscular effort. (4) Up to this time the child had not learned to stand or to creep; hence the development of one hand more than the other is not due to differences in weight between the two longitudinal halves of the body. As she had not learned to speak, or to utter articulate sounds with much distinctness, we may say also that right- or left-handedness may develop while the motor speech-centre is not yet functioning.

AT a recent meeting of the Paris Société de Biologie, M. J. Kunckel d'Herculais announced that he had been able to trace the entire development of the parasitic *Mylabris schrebersi*, which, like the *Epicauta* studied by C. V. Riley, live parasitically on different *Acridia*, on *Stauronotus maroceanus* particularly.

WE learn, from the *Oesterreichische botanische Zeitschrift*, of interesting botanical results obtained from Herr J. Bornmüller's expedition to Asia Minor. From Amasia he had paid a five-weeks' visit to the mountain district of Siwas, Kaisarieth, and Jusgat, where he found, at altitudes varying from 1800 to 2500 metres, a flora differing widely from that of Amasia; many Alpine species with showy flowers, belonging to the genera *Primula*, *Gymnadenia*, *Papaver*, *Ranunculus*, *Globularia*, *Geranium*, *Centaurea*, *Bupleurum*, *Fritillaria*, &c., being found in great abundance. The ascent of the steep cone of Yildiz-dagh, 2520 metres, occupied 12 hours. Four days were required for the ascent of Mount Argæus, the higher elevations of which were covered with immense masses of snow. The mountain is 13,000 feet high, and for the last 1500 metres nothing but snow and glacier was traversed; the actual summit of 150 feet was found inaccessible. At a height of 2900 metres all traces of trees had disappeared, and the only shrubs found were starved specimens of *Juniperus nana* and tragacanth in the fissures of the rocks; but the pastures were covered with an abundant and brilliant vegetation. In the snow-covered regions the night-temperature was $+ 2°-3°$ R. Several new species were collected, among them a beautiful orange-yellow *Fritillaria*. On his return to Amasia he found the herbaceous vegetation almost entirely scorched up by the heat of the sun. Herr Bornmüller was then starting for the sources of the Gök-Irmak in Paphlagonia, whence he hoped to bring rich collections, the only botanist who has hitherto visited that district being Wiedemann, in 1840; and his collections are to be found only in the herbarium at St. Petersburg.

WITH reference to our Notes on scientific guide-books to Switzerland, a correspondent sends us the following list:—(1) "Scientific Guide to Switzerland," by J. R. Morell, H.M. Inspector of Schools (Smith, Elder, and Co., London, 1867), 396 pages and index (same size as International Scientific Series). It treats of orography, geology, hydrography, meteorology, glaciers, fauna and flora of Switzerland. (2) "Le Monde des Alpes," a picturesque description of the Alps, and particularly of the animals which inhabit them, translated by O. Bauritt from the German of Dr. F. von Tschudi, "Das Thierleben der Alpenwelt," published by H. Georg, at Geneva and Bâle, 864 pages (same size as Prestwich's "Geology"), and 24 full page woodcuts, tinted, published in 1870, from the second German edition. (3) "Les Alpes," par H. A. Berlepsch (same size as Prestwich's "Geology"), with 16 tinted full plate cuts, published by H. Georg, at Geneva, 1870. The original is in German, with 441 pages. It contains an excellent description of the natural phenomena of Switzerland and the life of the inhabitants of the mountains. 10 francs unbound, 14 francs bound. It has been translated into English.

MM. G. ROUY AND J. FOUCAUD have undertaken the preparation of a "Flora of France," intended to take the place of that of Grenier and Godron. In order to render the work as accurate and as complete as possible, they earnestly request the co-operation of those who are acquainted with the botany of any portion of the extensive region which their task will cover.

MESSRS. OLIVER AND BOYD publish the introductory lecture to the Agricultural Class at the University of Edinburgh, delivered by Prof. R. Wallace on October 22. The subject is, "American Cattle, and the American Export Trade in Beef and Live Cattle to Great Britain."

A PAMPHLET on "The Philosophy of Cycling," by Mr. W. K. Fulleylove, has been issued by Messrs. Curtis and Beamish, Coventry. It is the first of a "popular series of pamphlets on every-day science."

THE latest work issued in the series entitled "Smithsonian Miscellaneous Collections," is a valuable "Index to the Literature of Thermodynamics," by Dr. Alfred Tuckerman. The work is similar to Dr. Tuckerman's "Index to the Literature of the Spectroscope," published in the same series. All the titles are given in full in the author-index, but in the subject-index, to save useless repetition, only the authors and the places where their works are to be found are given—except in the case of books. The work has been brought down to the middle of the year 1889.

A USEFUL summary of progress in mineralogy and petrography in 1889 is presented in the form of a pamphlet compiled by Mr. W. S. Bayley (Waterville, Me.). It consists of a series of monthly notes which appeared originally in the *American Naturalist*.

THE Frankfort publisher H. Bechhold is issuing what promises to be a valuable "Handlexikon" of the natural sciences and of medicine. The editors are A. Velde, W. Schauf, V. Loewenthal, and J. Bechhold. Especial attention is paid to the practical applications of the sciences.

THE seventh volume of the Proceedings and Transactions of the Royal Society of Canada for the year 1889 includes, among other papers, the following: the cartography of the Gulf of St. Lawrence, from Cartier to Champlain, by W. F. Ganong; trade and commerce in the Stone Age, by Sir Daniel Wilson; expeditions to the Pacific, by S. Fleming; notes on mathematical physics, by J. Loudon; on the variation of the density with the concentration of weak aqueous solutions of certain salts, by J. G. McGregor; on new species of fossil sponges from the Siluro-Cambrian at Little Metis on the lower St. Lawrence, by Sir J. W. Dawson.

MR. C. C. VEVERS, Leeds, has published an illustrated catalogue of photographic apparatus, materials and catalogues, and optical lanterns and accessories.

MESSRS. R. FRIEDLÄNDER AND SON, Berlin, have just issued two catalogues of books which they have for sale, one relating to comparative anatomy, the other to *Arachnida*, *Myriopoda*, *Crustacea*, and *Rotatoria*.

BY authority of the Consul-General of Uruguay, London, a catalogue has been issued of the minerals from Uruguay shown at the recent International Exhibition of Mining and Metallurgy at the Crystal Palace. Associated with the catalogue are various allied documents, one of which is the mining code of the Republic.

COLONEL HOLABIRD, of Los Angeles, has lately returned to San Francisco from an exploring expedition in the cañons of Colorado. He penetrated districts never before explored, and found in an almost inaccessible cañon 100 miles north of Williams, and near the Grand Cañon of the Colorado, the Yava Supai tribe of Indians, who had never seen any white man, except Lee the Mormon, who was shot for the Mountain Meadows massacre. Colonel Holabird, in relating his experiences to a correspondent of the *New York Tribune*, said:—"These Indians are of the Apache family, but of ancient origin. The men are magnificent specimens. The valley in which the tribe has lived for many years in seclusion has only two ways of approach. It contains 2000 acres, and is inclosed by almost perpendicular walls 4000 feet high. We travelled over 15 miles along a cañon—a lifeless country. Suddenly we came to two boiling springs under cotton-wood trees. From these springs a river starts, which winds its way through a luxuriant valley. The water in the river is clear as crystal, and so strongly impregnated with lime that it petrifies everything it touches. There are three immense cataracts in the cañon. These look as if centuries ago a huge cotton-wood tree had fallen across the stream and lodged. Mosses, ferns, and creepers formed a barrier. All these have been turned to limestone. The grass caused the deposit to increase until the barricade extended across the cañon, making a fall of 250 feet. Along the front of these high cataracts, limestone ridges have formed 20 to 50 feet one above the other. Over all these the water falls like a sheet of glass. Underneath, between the ridges, thousands of plants, with flowers in full bloom, are seen, while millions of humming-birds dart in and out. The chief of the strange tribe is an old man of sixty—'Captain Tom.' I found these Indians in a starving condition, subsisting on berries and grass-seed. I appealed to the Government for them, but the Indian Department said it could not help wandering people."

THE November number of the *American Journal of Science* contains an account of the preparation of pure metallic cadmium, by the process of distillation *in vacuo*, recently carried out by Mr. E. A. Partridge in the laboratory of the University of Pennsylvania. The successful experiments described a few months ago by Messrs. Morse and Burton, in which it was shown that metallic zinc could be obtained in a state of purity by distillation at a high temperature in a vacuum constantly renewed by means of a Sprengel pump, led Mr. Partridge to make a similar attempt in the case of cadmium, with the ulterior purpose of utilizing such a sample of the pure metal for a redetermination of its atomic weight. The process has proved perfectly successful, and yields a metal of such purity that even spectroscopic examination fails to detect any traces of admixed foreign metals. The retorts employed were of glass—naturally of the hardest kind that could be procured. They were made of tubing of 20–25 mm. diameter, and at a distance of about 11 cm. from the closed end were drawn out into a neck of 12 mm. diameter, through which the volatilized metal passed into the receiving portion of the tube. This latter portion was finally drawn off in such a manner that it could be tightly connected with a treble-fall Sprengel pump. About a hundred grams of cadmium were introduced into the retort before drawing off, and after the latter operation had been performed the apparatus was exhausted by means of the pump, which was kept working during the whole period of the distillation. The retort was then heated in the combustion furnace in which it was supported, the temperature being so regulated that most of the liquefied metal ran back into the retort, only the more volatile vapours passing through the neck into the receiver. The distillation was continued until about sixty grams of metal had passed over, the operation lasting about an hour. The volatilized cadmium condensed partly in the bottom of the receiver in the form of a bar, and partly upon the upper portion and sides in the form of brilliant perfectly-developed crystals. The product was redistilled in a similar manner in order to minimize the risk of traces of foreign metals being carried over. The precaution, however, appears to be unnecessary, for the residue left after re-distillation of the greater portion was found to contain no trace of impurity when examined spectroscopically. The atomic weight of cadmium was determined by three distinct methods, the salts employed being prepared from this redistilled metal. In the first series the loss of weight on converting the oxalate into the oxide by ignition formed the basis of calculation, in the second series the loss of weight on converting the sulphate into sulphide by ignition in a stream of sulphuretted hydrogen, and in the third series the loss brought about on similarly converting the oxalate into sulphide. The mean values obtained for the atomic weight, when $O = 16$, from ten experiments in each series were respectively 111·80, 111·79, and 111·80. The final value deduced from the whole of the experiments is thus 111·8.

THE additions to the Zoological Society's Gardens during the past week include two Squirrel Monkeys (*Chrysothrix sciurea*) from Guiana, presented by Mr. E. Leech; a Brown Bear (*Ursus arctos*) from Russia, presented by Mr. W. H. Stuart; an English Wild Cow (*Bos taurus*, var.) from Bangor, North Wales, presented by Mr. G. W. Assheton-Smith; two Masked Weaver Birds (*Hyphantornis personata* ♂ ♀) from Abyssinia, a Short-winged Weaver Bird (*Hyphantornis brachyptera*) from Africa, presented by Commander W. M. Latham, F.Z.S.; a Viverrine Cat (*Felis viverrina*) from India, a White-crested Touracou (*Corythaix albo-cristata*) from West Africa, deposited; a —— Shrike (*Lanius lahtora*) from India, purchased.

OUR ASTRONOMICAL COLUMN.

MEASURES OF LUNAR RADIATION.—The Proceedings of the American Academy of Arts and Sciences (vol. xxiv.) contains an account of some measures of lunar radiation made by Mr. C. C. Hutchins by means of a new thermograph. This instrument consists of a single thermal junction of nickel and iron placed in the focus of a small concave mirror. The author finds that the condensing mirror fulfils the functions of multiplied junctions, while the single union rapidly attains thermal equilibrium. A comparison was made between the thermograph and a thermopile of forty-eight couples, the result being that the former was found about twelve times as sensitive as the latter. Measures of the radiating power of some rocks, mostly of volcanic origin, show a remarkable uniformity. If the radiation from a blackened surface of quartz be taken as 100, the lowest radiating power is possessed by common white pumice, and is represented by 71·3. The temperature at which the measures were made was near 100° C. The measures of lunar radiation were made with an arrangement similar to that of a Herschel's telescope with the thermograph in place of an eye-piece. The results of the experiments indicate that the heat which our planet receives from the moon is to that of the sun as 1 is to 184,560. Some observations were made during the lunar eclipse of January 28, 1888, for the purpose of determining whether our atmosphere allowed radiations from the heated lunar surface to pass through it. When the moon was in the penumbra, the reading of the galvanometer scale was 254·4, nineteen minutes before totality it was 11·2, eight minutes before totality it was 7·3, and the mean of thirty readings taken during the total phase gave the value 2·09. The inference drawn from these observations is, that all but a minute portion of the rays from the lunar soil and rock are cut off by our atmosphere, for it seems impossible to conceive that a surface like that of the moon, upon which the sun has been shining for many days, should suddenly cease to radiate when the sun's light is withdrawn. A comparison of lunar rays with solar rays reflected from various rocks shows that the selective absorption by the rocks is altogether insufficient to explain the great absorption of the lunar rays observed during the eclipse. An attempt has been made to construct a curve representing the change of transmission of lunar rays by our atmosphere with changes in the altitude of the moon. The measures show that our atmosphere, at the ordinary pressure, transmits 89·25 per cent. of the vertical lunar beam.

THE STAR D.M. +33° 470.—The Rev. T. E. Espin announces, in *Wolsingham Observatory Circular*, No. 27, that this star (R.A. 11h. 28m. 16s., Decl. +33° 38′, 1855), magnitude 9·2, was observed on November 7 as 7·5 magnitude. Hence the star is probably variable. The spectrum is that of Group II.

THE NYASSALAND REGION.

ON Tuesday evening, at the opening meeting of the new session of the Royal Geographical Society, Mr. H. H. Johnston read a paper on his recent visit to the region lying between Lakes Nyassa and Tanganyika. While Mr. Johnston dealt largely with matters bearing on British interests and the industrial development of the region, he was also able to make additions to our knowledge of its geography. Mr. Johnston, in H.M.S. *Stork*, sailed up the Chindé mouth of the Zambesi, and for some distance up the Shiré River, where he was transferred to the Lakes Company's steamer *James Stevenson*. He visited the well-known station at Blantyre, then sailed up the lake to Karonga, the British station on the north-west shore of the lake. After bringing the hostile Arabs to terms, Mr. Johnston went on across the plateau to the south end of Lake Tanganyika, visiting, by the way, Lake Hikwa or Rukwa, first seen by Mr. Joseph Thomson on his first expedition into Africa. Of the navigation of the Zambesi, Mr. Johnston said :—

"The navigation of the Zambesi from its mouth to Vicente is by no means an easy matter to those unacquainted with the intricate windings of the river's navigable channel. The great stream, which is, on an average, three or four miles broad, is studded with islands and beset with sand-banks. Vast stretches of the river are covered by scarcely more than six inches of water. To the eye of a man accustomed to the study of great rivers, the existence of these shallows is at once apparent by the mirror-like calm of the water that covers them, and the warm, pinkish tone of the sandy bottom which subtly permeates the blue reflections of the sky. On the other hand, the course of the deep channel is marked by the swirling water, the tiny whirlpools, and the sharply-cut sides of the bank, which, instead of tapering off into the stream, look as if they had been recently sliced with a large knife. There is a crying need for what at present does not exist, or, if it does, is not known to the outside world—a good, accurate, and detailed chart of the course of the Lower Zambesi. Although the course of the deep channel varies and alters, as it does in all great rivers, it does not generally change so quickly but that a little careful supervision might easily keep such a chart up to date."

The following account of what is to be seen on the Shiré is of interest :—

"Continuing the ascent of the Shiré, we skirt the strikingly picturesque range of the Pinda Mountains, all jagged peaks and sugar-loaves, on the east, and the Matunda Mountains on the west, while in the far, far distance northwards there rise the vast dim outlines of higher and higher peaks, culminating in Mount Tshiperoni (or 'Clarendon,' as it was named by Livingstone). The scenery on this stretch of the Shiré is really very fine. In the foreground there are the serpentine windings of the broad river through the great Morambala marsh, which is here and there dotted by little lakelets of clear blue water, but for the most part covered with wide stretches of tall reeds. These reeds bear large heads of creamy-white flower-tufts, almost as big as those of the pampas grass, and as the wind blows across the marsh it sways the reeds into wave-like undulations, wherein the great white heads of blossom appear like fluctuating foam cresting the billows of shining green leaf-blades beneath. Rising above this white-flecked sea of glistening grass are the abrupt ranges of fantastically-shaped hills and mountains, which girdle in the Shiré valley with great semicircles of blue mountain wall. Occasionally a glaucous-green Borassus fan-palm rises on a column-like stem from an island in the river or a dry patch in the marsh. These landscapes are drawn in large traits, and their harmonies are simple, and not complicated by the admixture of any human habitation or cultivation. It is not until one is within a relatively short distance of the Ruo that the banks of the Shiré begin to be inhabited again, and the marsh yields to thin forest and plantations of maize, tobacco, millet, and pumpkins.

"A short distance above the Ruo one enters the Elephant Marsh, a district of great grassy flats, flooded occasionally when the Shiré overflows its banks, but ordinarily a dry level stretch of prairie dotted with pools of water.

"At the close of the dry season, when the tall grass has been burnt down, and there is little or no cover for the game to hide in, it is really a remarkable spectacle, as seen from the deck of a steamer, to watch the great herds of big animals wandering over these savannahs in search of the young verdure springing up amid the charred stubble of the old grass. With an opera-glass you may distinguish water-buck, gnu, buffalo, eland, pallah, reed-buck, and zebra, and occasionally some dark blue-grey blobs, much larger than the other specks and forms which are in their vicinity, turn out to be elephants. Occasionally a lion has been known to come down to the river and stare at the steamer, and on one or two occasions these beasts have actually been shot from the deck in passing. Both in the Elephant and Morambala marshes, and in the Upper Shiré, the hippopotamuses are a real source of danger and inconvenience to any boats of ordinary size which are not propelled by steam. The hippopotamuses are particularly dangerous at night, but even during the day they will deliberately chase and endeavour to upset boats and canoes which enter their domain ; and in the development of the Shiré navigation it is essential that the hippopotamuses should be mercilessly exterminated."

Mr. Johnston described as follows the fine country on the north of Lake Nyassa :—

"Here there are no fewer than nine perennial rivers, some of them of considerable volume, which descend from the lofty mountain ranges of Buntali, Wukukwe, and Ukinga, and enter the lake between Karonga and Parumbira Bay, the moisture which percolates from them through the soil giving the Konde plain an appearance of perpetual spring. The land at the north end of the lake is a veritable African Arcadia. You may walk for miles and miles through banana plantations ; then you may emerge on wide-stretching fields of maize and millet and cassava. All the oozy water-meadows are planted with rice ; but, above all, the great wealth of the country is in cattle, which, elsewhere by no means common in Nyassaland, thrive remarkably in the Konde district, and consequently milk and beef are cheap and abundant. The inhabitants of this happy land are a contented, pleasant-dispositioned folk, who knew no trouble until the Arabs sought to subdue them a few years ago."

From a geographical point of view probably the most interesting part of his paper was that describing his visit to the remarkably desolate region around Lake Rukwa lying on the south-east of Lake Tanganyika. Rising from the fertile plateau are the

Wungu Mountains, some 6000 feet high, and Mr. Johnston thus described what he saw on ascending them :—

"We looked down on what I thought at first was a very broad sheet of water, surrounded on three sides by high ranges of mountains. But by degrees, with the aid of a field-glass, I discovered that what appeared to be a spacious lake in reality consisted of a narrow contorted strip of water on the one side, and between us and the water a wide extent of absolutely flat plain, so uniformly covered with blue-grey forest that from those heights above it was hard to distinguish it by its colouring from the real lake. When I had taken a number of angles from our camp on the mountain crest, we began a most arduous descent from the heights into the plain below. As we descended, our impressions and forebodings became of a somewhat dismal character. Everything around us bore witness to the dearth of water. On the other side of the mountain range we had left a country in the fully beauty of spring, intensely green with the gentle showers of the commencing rainy season, but here on the slope facing Rukwa, the farther we descended the more arid the country became. At the base of the mountains we crossed a three-mile stretch of level plateau, covered with the dismal grey growth of innumerable thorn-trees, so gnarled and contorted in shape, and provided with such cruelly ingenious hooks and barbs and stiletto-like thorns, that they might have been the enchanted forest round some wizard's lair.

"This plateau came to a sudden and abrupt termination, and from its edge we made a precipitous descent along a blood-red path into a blood-red ravine, the sides of which were fantastically planted and festooned with clumps of purple-green aloes, and those weird candelabra euphorbias with grey spectral stems, the segmented stalks of which looked like the tails of innumerable scorpions. Down through the dark gloomy depths of this cleft of the earth we floundered, slipped, and fell into the gorge of a dry river, cut deeply in a winding channel between precipitous red walls, the sides of which were scoured and polished and striated as if by glacial action. There were scattered stagnant pools of water in the red, red rocks and sand, and water oozed from places in the river bed when our porters dug below the surface. The trees clinging to the sides of the ravine were emerald green, with a metallic-tinted harsh verdure. Evidently this dried-up stream had once been an important river and a powerful torrent, and nothing is more remarkable in the vicinity of Rukwa than to observe the traces of a once abundant rain supply, which, from some unexplained cause, has—so the natives say—suddenly ceased during the last two or three years, as though the country had been literally blasted by some terrible curse.

"Crossing the dry bed of this river we entered on another level stretch of country gently sloping northwards, its surface uniformly clad with a forest of grey thorn-trees, but with the ground at their bases bespangled in a strange contrast with gorgeous flowers, which were almost unaccompanied by leaves, just the vividly-coloured petals rising from the hard grey soil.

"These consisted chiefly of purple, yellow, and white anemones, arborescent lilies, with white star-like flowers springing from a grey branching stem, and great heads of pink crinums resembling the 'kafir lily' of South Africa.

"We passed an occasional dry water-course choked with grey-green life-in-death vegetation, and then at length reached a broader dried-up stream-valley, with shadier trees and a stockaded village, the first we had met with in the land. . . . As soon as we got out of the broad, shaded stream-valley where the village was situated, we entered the frying, blazing heat of the parched plain, and found ourselves in a white, light, bright hell of dazzling sunshine. The shadeless acacias with their cruel thorns, the dry yellow grass, and the yellow withered Borassus palms, in no way mitigated the pitiless glare of the vertical sun, while a raging wind, hot as the breath of a furnace, swept over the plain and burnt the skin of our faces, so that we felt as if we wore tight masks. Every quarter of an hour the wretched caravan had perforce to stop and pant under the thin film of shade which might descend from the skeleton branches of a dead tree. At length, after frequent halts and protestations from the sun-stricken men that they could go no farther, we saw ahead of us an emerald-green line in the grey wilderness, which marked the presence of water. This turned out to be a welling, brackish pool thronged with bulrushes and reeds, a kind of circular spring of overflowing water apparently connected with the lake by a long lane of rush-choked marsh, very distinct from the arid plains of baked mud. We camped here, where the scenery was a little less ghastly in its dead ugliness. The acacias exhibited a little green foliage among their thorns, and they were frequented by thousands of cooing doves, while the scanty bushes on the ground served as cover for many francolin and guinea-fowl. Game, in the shape of antelopes and buffaloes, was evidently abundant, and no doubt was attracted to the vicinity of this brackish pool by the flakes of salt which remained on the soil where the water had evaporated; and the game in its turn was followed by hyenas, lions, and vultures. The hyenas laughed and the lions roared outside our camp fires, and the next day I noticed many scattered fragments of bones and skulls in the vicinity, which were the relics of previous feasts on the part of these Carnivora.

"I was anxious to proceed direct to the lake from here, as we were only about three or four miles distant, but the Wuñgu people would not allow us to do so until we had first seen their Sultan, so we travelled in a north-easterly direction, always through this scorching, glaring wilderness, till we reached the bank of the Soñgwe River. Here I camped so that the men might be close to fresh water, but it appeared to us that even the water of the Soñgwe was brackish, though it was a running river. It seemed to have no effect in quenching one's thirst, and contained some irritating property which occasioned diarrhœa, and even dysentery. Leaving my men at the Soñgwe, I went with Mr. Nicoll and Dr. Cross to visit Mwinyi-Wuñgu, who lived about a mile from its banks in a stockaded town. I can hardly describe the heat of the atmosphere in walking thither; it was like passing through fire. When we got into the town, we eagerly crept under the shade of the overhanging eaves of the houses, which extended so near the ground, for the sake of coolness, that one had to get down on all fours to get under them."

As there was really a famine both of food and water in this dreadful wilderness, Mr. Johnston and his large party of men were compelled to hasten out of it, without his actually being able to get to the edge of the lake itself. What he has told us about this region makes us desirous of knowing more. It is a remarkable fact that, while in the Nyassa-Tanganyika plateau there had been no lack of rain, in the lake basin itself not a drop had fallen for three years.

THE BOTANICAL MYTHOLOGY OF THE HINDOOS.

At a recent meeting of the Anthropological Society of Bombay, Dr. Dymoke read a very interesting paper entitled "The Flowers of the Hindoo Poets," in the course of which he referred to the mythical conceptions which have gathered round trees and plants in the minds of the Hindoos. The ancient Eastern poets saw in the tree a similitude with the heavens and with the human form; in the "Gitagovinda" a comparison is drawn between the clouds and the thick dark foliage of the *Tamala*. These fancies gave rise to the numerous poetical myths concerning the tree of life, of knowledge, of the *Amrita* or Ambrosia, as well as those concerning cosmogonic and anthropogonic trees. The *Soma* or *Amrita* is represented as the king of plants, the eternal essence which constantly sustains and renews the life of plants and animals; it is the symbolical drinking of this eternal essence as a holy ceremony to which constant allusion is made in the Vedas :—

"We've quaffed the Soma bright,
And are immortal grown;
We've entered into light,
And all the gods have known."

—*Rigveda*, viii.

The Amrita appears in various forms in stories and legends. A famous poet says that the drop (*Svedavindu*) which fell into the shell became a pearl; in the mouth of the black snake it became poison; and in the flower of the plantain, nectar. Several plants bear this name, and are supposed to be endued with an extra particle of the eternal essence; among others, the *Neem*, on which account the Hindoos, on their New Year's Day, eat the leaves of this tree upon the supposition that the Amrita contained in them will insure longevity. In Hindoo flower lore the large black bee (*Buramara*) plays an important part: he is the inconstant lover who delights in gathering sweets from every flower. The queen of Indian flowers is the lotus: the Hindoos compare the newly-created world to a lotus flower floating upon the waters, and it thus becomes symbolical of spontaneous generation. The golden lotus of Brahminic and Buddhistic mythology is the sun, which floats in the waters which are above the firmament, like an earthly lotus in the deep blue stream below. From it distils the Amrita, the first manifestation of Vishnu. Brahma and Buddha (the supreme intelligence) were born of this heavenly lotus. Lakshmi, the Indian Venus, is represented sitting on this flower. The Hindoos see in the form of the lotus the mysterious symbol, *Svastika*. The allusions to this flower by Indian poets are innumerable. No praise is too extravagant for it; it is the chaste flower, and its various synonyms are bestowed as names upon women. The red lotus is said by the poets to be dyed with the blood of Siva that flowed from the wound made by the arrow of Kama, the Indian Cupid. The face of a beautiful woman is compared by the poets to a lotus blossom, the eyes to lotus buds, and the arms to its filaments. The bee is represented as enamoured of the lotus. Although a humble little flower, the *Tulasi* is almost as great a favourite as the lotus; it is addressed to the goddess Sri or Venus. The heart of Vishnu is said to tremble with rage if a branch of his beloved is injured. The plant must be gathered only for medicinal or religious purposes, such as the worship of Vishnu or Krishna, or the wife of Siva. It is a kind of amrita, symbolical of the eternal essence; it protects the worshippers and gives children to women. The plant is often worshipped as a domestic deity, and its branches are placed on the breasts of the dead. The Champa is chiefly celebrated for its overpoweringly sweet odour and golden colour; so strong is its perfume that the poets affirm that bees will not extract honey from it; but they console it for this neglect by dedicating it to Krishna, who loves garlands of yellow flowers as becoming to his dark complexion. One of the greatest favourites of the poets is the Asoka; its flowers, which are yellow when they first open, gradually change to red. In March and April it is in its glory, and at night perfumes the air with its delicate odour. The tree is the *kul* or anthropogonic tree of the Vaisya caste, who call it Asupala. The Kadamba (*Anthocephalus cadamba*) is sacred to Kali or Parvati, the consort of Siva; it has many synonyms, such as "protecting children," "dear to agriculturists," &c. It blossoms at the end of the hot season, and its night-scented flowers form a globular orange-coloured head, from which the white-clubbed stigmas project. The flowers are fabled to impregnate with their honey the water which collects in holes in the trunk of the tree. In Delhi the goldsmiths are fond of imitating the flowers. The well-known prickly gold beads so often seen in Delhi jewellery are meant for kadamba flowers. In this part of India the Marathas will not gather the flowers for profane purposes as it is their anthropogonic tree. The Kadamba Rajas claim their descent from it, as recorded in the following legend:—"After the destruction of the demon Tripura, a drop of perspiration fell from the head of Isvara into the hollow of a kadamba tree, and assumed the form of a man with three eyes and four arms. He became the founder of Vanavasi or Jayantipur." There are other versions of the story, but all agree in connecting the origin of the family with this tree, a branch of which is necessary to represent the Kai at a Marathi marriage ceremony.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—At the biennial election to the Council of the Senate held on November 7, the following were nominated (the * indicates retiring members):—Heads (2 seats)—*Dr. Atkinson, Clare, *Dr. Ferrers, Caius, Dr. Hill, Downing; Professors (2 seats)—*Dr. Cayley, Trinity, Dr. Sidgwick, Trinity, Prof. Ryle, King's; Members of the Senate (4 seats)—*Dr. D. MacAlister, St. John's, Dr. Forsyth, Trinity, *Mr. Whitting, King's, Mr. R. T. Wright, Christ's, Mr. E. H. Morgan, Jesus, Mr. C. W. Moule, Corpus, Mr. C. H. Prior, Pembroke. The voting was as follows:—Dr. Ferrers, 184, Dr. Atkinson, 137; Dr. Cayley, 191, Dr. Sidgwick, 127; Dr. D. MacAlister, 158, Mr. Whitting, 156, Dr. Forsyth, 153, Mr. Wright, 117. These were elected. Dr. Hill received 109 votes, Prof. Ryle, 103, Mr. Prior, 111, Dr. Lea, 82, Mr. Morgan, 81, Mr. Moule, 71. The newly-elected members hold office for four years. The result is interpreted as a gain for those who favour the modern development of the University.

It should have been stated that the election of Fellows referred to in our last number took place at St. John's College.

Mr. Frank McClean, M.A., of Trinity College, has offered securities of the value of £12,000 to be held in trust for the University by Trinity College, for the purpose of founding three "Isaac Newton Studentships" in Astronomy, Astronomical Physics, and Physical Optics. The students are to hold their emoluments for three years, to be Bachelors of Arts, and of high mathematical attainments.

R. S. Cole, B.A., of Emmanuel College, has been appointed a Junior Demonstrator of Physics at the Cavendish Laboratory, in the place of Mr. L. R. Wilberforce, promoted to be Demonstrator.

The General Board of Studies propose the foundation of an additional Demonstratorship in Physiology, under Prof. Michael Foster, without stipend from the University Chest.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Microscopical Society, October 15.—Dr. C. T. Hudson, F.R.S., President, in the chair.—Mr. G. F. Dowdeswell's note on a simple form of warm stage was read, and the apparatus exhibited.—The President said he had with great regret to record the deaths of two honorary Fellows of the Society—Prof. W. Kitchen Parker, F.R.S., and Mr. J. Ralfs. In place of these two gentlemen Dr. H. B. Brady, F.R.S., and Prof. W. C. Williamson, F.R.S., were nominated.—Mr. Mayall said he must ask the indulgence of the meeting to enable him to clear himself from possible ambiguity. In notifying the fact that at the first photographic trials of the new objective of 1·6 N.A. the visual and actinic foci had been found by Mr. Nelson and himself to be not coincident; and that when the objective was returned to Jena immediately after, Dr. Czapski found the foci were coincident; he had hazarded what he had imagined would appear a mere playful admission of the state of general puzzlement of both sides by suggesting that "the transit of the objective from London to Jena had somehow got rid of the 'chemical' focus." That sentence had unhappily been construed both in England and abroad into a reflection upon the good faith of Dr. Czapski, or Dr. Abbe, or the firm of Zeiss. Whatever blame was due to himself for the ambiguity of the expression, he must, of course, accept. At the same time he thought the Society would be interested to learn that upon his conveying his explanation to Dr. Czapski and Dr. Abbe, those gentlemen had expressed their complete satisfaction with it. He believed that the existence of a "chemical" focus was probably due to a slight difference in the adjustment of the front lens, especially, as Dr. Abbe had pointed out, in view of the fact that with an objective of such large aperture the colour correction was, as it were, "balanced on a needle-point" in the matter of an alteration in the distance of the front lens from the posterior combinations; and that a very minute alteration in that distance, though producing no perceptible difference in the visual image, was quite competent to lengthen or shorten the focus of the violet rays to such an extent as to exhibit a "chemical" focus non-coincident with the visual focus when tested photographically.—The President gave formal notice that a special general meeting would be held in the Library at 5 p.m. on Wednesday, October 22, for the purpose of considering alterations in the byelaws, the terms of which he read.—Mr. G. C. Karop exhibited and described an improved students' microscope, made by Swift and Son. The new instrument embodied Mr. Nelson's "horseshoe" stage for convenience of readily seeing the condenser, and for estimating by the touch the approximation of the focus on the slide, and on which the Mayall mechanical stage was easily applied, together with a centring sub-stage focussed by sliding on the tail-piece, the whole of superior workmanship and design, and supplied at a moderate outlay.—Prof. J. W. Groves communicated a note by Mr. P. C. Waite on a new method of demonstrating intercellular protoplasmic continuity. A specimen in illustration was exhibited.—Mr. J. D. Aldous exhibited some early forms of microscope slides made of boxwood, similar to those formerly made of ivory, with the objects between pieces of talc.—The President called attention to some original drawings of a new Rotifer by Mr. W. B. Poole, of South Australia: also to a specimen of *Œcistes mucicola* exhibited by Mr. G. Western.—Mr. E. M. Nelson exhibited upon the screen a series of thirty-one photomicrographs, which he described.—Dr. H. B. Brady's paper on a new type of Foraminifera was taken as read.

—Dr. Maddox's paper on the structure of Spermatozoa was postponed until the next meeting in consequence of the lateness of the hour.

PARIS.

Academy of Sciences, November 3.—M. Duchartre in the chair.—Notice of the works of M. Pierre de Tchihatchef, by M. Daubrée. M. de Tchihatchef died at Florence on October 13. He was born at St. Petersburg in 1815, and elected a correspondent of the Academy in the Geographical Section when about thirty years of age. An account is given of his many scientific works.—A photo-chronographic apparatus that may be used to analyze every kind of motion, by M. Marey. A photographic film is caused to move across the focus of the lens of a camera. The motion is imparted by an electric motor, and with the arrangement described the film may be arrested fifty times a second for the production of as many views of the object being photographed. A plate giving six views of a trotting horse accompanies the note.—On the relation of gangrenous septicæmia to lock-jaw, with special reference to the associations of virulent microbes, by M. Verneuil. From a series of surgical and chemical experiments, the author is led to believe that the co-existence in man of certain forms of mortification and lock-jaw is not accidental, but results from the simultaneous production in the wounds of two microbes well known to Pasteur and Nicolaïer.—On the movements of a double cone, by M. A. Mannheim.—On the periodic functions of two variables, by M. Appell.—On a particular case of Lamé's equation, by M. V. Jamet.—Undulatory pressures produced by the combustion of explosives in a closed vessel, by M. Vieille.—On Bunsen's photometer, by M. R. Boulouch.—The rotation of the earth on its axis produced by the electro-dynamic action of the sun, by M. Ch. V. Zenger. The author has caused a hollow sphere to rotate under the action of the two poles of a Wimshurst machine, and thence argues that the planetary motions in our solar system have an electro-dynamic origin.—Action of borax in alkaline developing baths, by M. P. Mercier.—On the affinities of iodine in the dissolved state, by MM. Henri Gautier and Georges Charpy. The authors have studied the chemical behaviour of solutions of iodine in different media. Shaking the solutions with a lead amalgam, they find the colours of the mixture of iodides obtained in each case—that is, the proportions of the iodides of lead and mercury respectively—depend on the kind of solution employed.—On the γ-cyanoacetoacetic ethers and the hydrochlorides of the corresponding imides, by MM. A. Haller and A. Held.—Researches on the conditions of the reactions of the isopropylamines: limit to the production and development of propylene, by MM. H. and A. Malbot. The authors have studied (1) the action of isopropyl iodide on very concentrated aqueous ammonia in equi-molecular proportions, at the ordinary temperatures; (2) the same at 100°; (3) the same above 100°; (4) the action of isopropyl chloride upon aqueous ammonia at 140°. From the results of experiment, they have summarized their conclusions as to the character of these reactions between aqueous ammonia and isopropyl iodide and isopropyl chloride.—The Hanneton parasite, by M. Le Moult.—On certain formations on copper and bronze, by M. Raphael Dubois. The author has observed and studied some white mycelium flakes, very similar to those of *Penicillium* and *Aspergillus*, in solutions of concentrated copper sulphate neutralized by ammonia and used for the immersion of the gelatine plates employed in photogravure. Similar formations have been observed on bronze.—On some rocks from the Lunain valley, supposed to have been used to polish stone implements in Neolithic times, and on the action of water in the Stone Age, by M. Armand Viré.—On the formation of abrupt escarpments of earth that interrupt the slope of valleys in the north of France, where they are known as *rideaux*, by M. A. de Lapparent.—Experimental contribution to the history of the dendrites of manganese, by M. Stanislas Meunier.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, November 13.

Mathematical Society, at 8.—The Influence of Applied on the Progress of Pure Mathematics: the President.—Spherical Harmonics of Fractional Order: R. A. Sampson.—Proofs of Steiner's Theorem relating to Circumscribed and Inscribed Conics: Prof. G. B. Mathews.—On an Algebraic Integral of Two Differential Equations: R. A. Roberts.—Some Geometrical Theorems: Osher Ber.

Institution of Electrical Engineers, at 8.

FRIDAY, November 14.

Physical Society, at 5.—On Certain Relations existing among the Refractive Indices of some of the Chemical Elements: Rev. T. Pelham Dale.—Tables of Spherical Harmonics, with Examples of their Practical Use: Prof. Perry, F.R.S.

Royal Astronomical Society, at 8.

Amateur Scientific Society, at 8.—Geological Travels in France, Spain, and Algeria: G. F. Harris.

SUNDAY, November 16.

Sunday Lecture Society, at 4.—Captain John Smith, the Heroic Pioneer of English Colonization in America: Willmott Dixon.

TUESDAY, November 18.

Zoological Society, at 8.30.—A Catalogue of the Reptiles and Batrachians of Barbary (Morocco, Algeria, Tunisia), based chiefly upon the Notes and Collections made in 1880–84 by M. Fernand Lataste: G. A. Boulenger.—Remarks on the Chinese Alligator: G. A. Boulenger.—On some New Species and Two New Genera of Araneidea: Rev. O. P. Cambridge, C.M.Z.S.—On some Upper Cretaceous Fishes of the Family Aspidorhynchidæ: A. Smith Woodward.

Royal Statistical Society, at 7.45.—Inaugural Address: Dr. Frederic John Mouat, President.

Institution of Civil Engineers, at 8.—Steam on Common Roads: John McLaren. (Discussion.)—The Vibratory Movements of Locomotives: Prof. J. Milne, F.R.S., and John McDonald.

THURSDAY, November 20.

Royal Society, at 4.30.—The following papers will *probably* be read:—On the Determination of the Specific Resistance of Mercury in Absolute Measure: Prof. J. V. Jones.—The Spectroscopic Properties of Dust: Profs. Liveing and Dewar, F.R.S.—On the Specific Heats of Gases at Constant Volume; Part I., Air, Carbon Dioxide, and Hydrogen: J. Joly.—Magnetism and Recalescence: Dr. Hopkinson, F.R.S.

Chemical Society, at 8.—Ballot for the Election of Fellows.—The Estimation of Cane-Sugar: C. O'Sullivan and F. Tompson.—New Method of Determining Specific Volumes of Liquids and their Saturated Vapours: S. Young.

Linnean Society, at 8.

Zoological Society, at 4.

SATURDAY, November 22.

Royal Botanic Society, at 3.45.

CONTENTS. PAGE

THURSDAY, NOVEMBER 20, 1890.

KOCH'S CURE FOR CONSUMPTION.

DURING the last week Koch has made a further communication regarding his treatment of tuberculosis, which has been received with intense interest and perhaps with a certain amount of disappointment. For he has described his mode of applying the new remedy with sufficient accuracy to allow medical men to use it in the treatment of their patients, but he has left us still completely in the dark regarding the nature of the remedy itself.

A certain quantity of the remedy can be obtained from Dr. Libbertz, who has undertaken its preparation with the co-operation of Dr. Koch himself and Dr. Pfühl, but their stock is at present small, and larger quantities will not be obtainable for some weeks. In exercising this reserve regarding the nature of the remedy he employs, Dr. Koch has probably done very wisely, for it is evident that the substance he uses is very powerful and any inaccuracy or imperfection in its preparation might prove injurious to patients, and bring discredit upon the mode of treatment.

The remedy is a brownish, transparent fluid which is much too strong for use until it has been largely diluted with water. The diluted solution is applied by subcutaneous injection. There is a remarkable difference between the action of this remedy upon guinea-pigs, healthy men, and phthisical patients. Two cubic centimetres of the undiluted liquid produces no sensible effect when injected under the skin of a guinea-pig; but, calculated by body weight, the fifteen-hundredth part of this quantity produces cough, difficulty of breathing, sickness, vomiting, and fever, lasting for about twelve hours, in a human being. The presence of tubercle in the organism appears to render it extremely susceptible to the action of the remedy, for while the hundredth part of a cubic centimetre, *i.e.* one twenty-fifth part of the dose just mentioned as producing fever in healthy subjects, has no effect upon them, it will cause high fever along with coughing and sickness in tuberculous patients. Nor is this to be wondered at, for the remedy exerts its action upon the tissues which have been infiltrated by the tubercle bacillus and not upon the bacillus itself. It causes these tissues to die and be thrown off along with the bacilli they contain. This process is accompanied by fever, which the dose of the remedy used would not produce in health. In consequence of this the remedy may be used as a means of ascertaining the presence of tuberculosis as well as of curing it. If it causes more fever than it ought to do, in a doubtful case of phthisis, the presence of tubercle may be assumed, and when it ceases to produce fever in a patient under treatment the cure may be regarded as well-nigh complete. The effect of the remedy upon the diseased tissues can be seen in cases of lupus, where the tubercle bacillus infiltrates the skin instead of attacking the lungs as it does in consumption. A few hours after the remedy has been injected into the skin of the back, the spots of lupus on the face, far away from the seat of the injection, begin to swell and redden, and then become brown and dead, while the healthy tissue around becomes red and inflamed. The spots then become converted into dry crusts, which fall off in one or two weeks, leaving a clean red cicatrix behind. This result is very much like that produced by the direct application of a strong arsenical paste to the lupus spots. The tubercle bacilli weaken the vitality of the tissues which they infiltrate. The arsenic finishes the process they have begun, and kills the weakened tissues altogether, while it does not destroy the healthy tissues, and the once diseased but now dead part withers up and falls away from the living and healthy parts around. But arsenic can only do this when applied to the lupus in quantities sufficient to poison the patient many times over if it were absorbed into the blood, and consequently is quite out of the question as a cure for tuberculosis. Koch's remedy, on the other hand, seems to seek out tubercular tissue wherever it may be, and almost certainly produces changes in tubercular lungs and joints similar to those in lupus spots, although its action cannot be seen in the former as it can be in the skin. The fact that the new remedy does not destroy the tubercle bacilli, but only the tissues in which they are embedded, is distinctive of Koch's new method, which promises to effect a radical change in the treatment, not only of tubercle, but of many other diseases.

Since the discovery by Metschnikoff six years ago that the cells of the blood and tissues can eat up and digest the bacilli which constitute the germs of many diseases, the view has been constantly gaining ground that recovery or death in infective diseases is simply a question of the victory of the cells of a living organism or of the bacilli by which they have been attacked. In seeking for methods of cure men have tried either to find a way to weaken and destroy the bacilli, or to strengthen the resisting power of the tissues. When an organism is attacked by a new disease, it succumbs like an untrained army in the presence of an enemy. But the tissues appear to acquire a power of resistance, and if the first attack be recovered from, the organism is in many instances insusceptible to subsequent infection, as is shown in the case of small-pox, whether this has been accidentally acquired or intentionally inoculated. But an army may be trained to military tactics by sham fights instead of actual warfare, and the cells of an organism may become endowed with the power of resisting any infection, however virulent, by successive inoculations of a virus, weak at first but gradually increasing in power. This was the plan followed by Pasteur in his inoculations for anthrax. In these he employed at first anthrax bacilli so weakened by cultivation in an unfavourable medium that they produced nothing more than a slight indisposition, and then a virus less and less weakened until the virus of full strength could be successfully resisted. This process was one of protection rather than cure, but in his researches on rabies Pasteur turned his genius to the discovery of a means of cure. As he informed the Commission which went from this country to report on his method, he was himself not exactly aware how he arrived at his mode of treatment, but the conclusion had formulated itself in his mind that the disease produced a substance which was a protective against its own action. The treatment which he based on this conclusion has been so successful as to lead to many attempts to separate disease-germs and protective. These attempts have hitherto been fruitless in the case of rabies, but have been successful in other diseases. By cultivating disease-germs in a suitable liquid, and afterwards killing the germs themselves by heat, or removing them by filtration through porcelain, solutions have been obtained quite free from germs, and containing only the substances they have formed during their life and growth. Such solutions have been shown by M. Pasteur's pupils and others to have the power of protecting from septicæmia, anthrax, typhoid fever, and diphtheria; and Prof. S. Dixon, of Philadelphia, has succeeded in rendering animals resistant to inoculation with tubercle by previously inoculating them with the products of growth of the tubercle bacillus.

Most interesting experiments on the nature of these protective substances have been made by Wooldridge, Hankin, and Sydney Martin, who have shown that they are probably either globulins or albumoses rather than alkaloids, although Martin has obtained an alkaloid which produces all the symptoms of anthrax, and possibly may protect against it.

Koch's direction that his new remedy is to be prescribed under the name of paratoluid seems to indicate that it belongs to a class of bodies more nearly akin to alkaloids than to albumoses. Para-acet-toluid has been investigated by Jaffe and Hilbert, and found, like Koch's remedy, to be innocuous to the lower animals. It is possible that the new remedy may belong entirely to that class which has furnished us lately with so many valuable antipyretics and analgesics. But it seems more probable that it consists partly at least of a lymph containing the products generated by some microbe. One's first thought would naturally be that the substances formed by the tubercle bacillus itself would be chosen by Koch for curative as they have been by Dixon for protective purposes. But tubercle differs much from many other infective diseases, for anthrax, typhus, scarlet fever, or measles, run a definite course, and if they do not kill the patient at once, they protect him from a subsequent attack. But the attack of hectic fever which daily recurs in a patient suffering from phthisis confers no protection on him at all, but rather hastens the progress of the disease to a fatal termination.

The case is sometimes different when disease due to another microbe attacks a patient suffering from some form of tuberculosis. Thus lupus has been seen to shrivel and die after an attack of erysipelas or measles, and peritonitis, supposed to be tubercular, has disappeared during recovery from diphtheria. Judging from the resemblance between the effects of Koch's remedy and those of erysipelas, measles, or diphtheria, we should be inclined to suppose it to consist of a filtered culture of the germs of one of these or of some such infective disease, probably mixed with some kind of paratoluid.

It may perhaps seem idle to speculate on the composition of a remedy which its discoverer will probably describe fully ere long, but it must be remembered that tuberculosis, though one of the most frightful scourges of mankind, is not the only ill that flesh is heir to. There are others, with a less mortality perhaps, but even more feared by the sufferers themselves, and we may trust that the lines of research just indicated, whether they be exactly those on which Koch has been working or not, may lead to the discovery of certain cures for scarlet fever, diphtheria, gummata, and that most dreaded of all—cancer.

T. L. B.

THE CHEMISTRY OF IRON AND STEEL MAKING.

The Chemistry of Iron and Steel Making. By W. Mattieu Williams, F.C.S., F.R.A.S. (London: Chatto and Windus.)

THIS work differs materially from the ordinary technological text-books with which we are so familiar. Throughout it is evident that the author has thoroughly utilized his varied experience and great ability to their fullest extent, resulting in the production of a mass of suggestive information valuable to the ordinary student, and affording matter for reflection, even to those who have for years made the chemistry of iron and steel a specialty.

The introductory portion of the work is interesting, but chiefly briefly historical; the same may be said as regards chapter ii., on the ores of iron, which includes some speculations on the possible meteoric origin of iron. The wide diffusion of iron in the form of dust is quoted as indicating in part at least the meteoric origin of the metal.

Chapters iii. and iv., on reduction and dissociation, fluxing, roasting, and calcination, are thorough, giving concisely and forcibly the necessary knowledge in a manner well worthy of imitation in more pretentious works. The author's study on the analogy of dissociation to evaporation, commencing with water, followed by illustrations of the dissociation of the metals from their oxides, &c., shows that he not only clearly understands his subject, but also has the faculty of communicating his ideas clearly to others.

It is difficult to do justice to chapters v. and vi., on the blast-furnace, without entering into details which would here be out of place. The descent of the charge in the blast-furnace is first discussed, and the importance of clear ideas as regards the *rationale* of blast-furnace charging is insisted upon. It is not a simple process: (1) the heterogeneous impurities of the ore have to be removed; (2) the iron must be reduced to the metallic state; (3 and 4) the reduced metallic iron must be fused, as also the earthy impurities. All must proceed in due sequence. A definite statement of the problem of modern iron smelting must be before us ere we can follow rationally the reactions which occur. The evolutionary process from the old bloomeries or Catalan forge, next the small blast-furnace up to the huge structures of to-day is clearly set forth. Credit is also given to the practical worker, who, the author acutely remarks, must have possessed more practical scientific knowledge than we have been inclined or able to perceive.

The apparent irrationality of the modern system of smelting, by which iron ore is not only reduced to the metallic state, but the resultant metal becomes loaded with silicon and carbon, &c., is fairly discussed. It is shown that although what is termed the direct process, *i.e.* the simple reduction of the metal from its oxide, may be so conducted as to produce a nearly pure wrought iron, yet such methods are not economical, and are of limited application.

The direct process (p. 85) demands rich ores; a great expenditure of fuel is incurred; simple and direct as it appears, a greater cost is involved; on the whole, it is better to resort to the ordinary purification of crude cast-iron by the puddling process. The relative merits of the direct process *versus* the ordinary practice of smelting and puddling, are, however, still under consideration. Recent investigations, combined with improved methods, indicate that at any rate in some localities the direct process may be advantageously applied. The modern blast-furnace is clearly described; the form, viz. a circular column swelling in diameter at both extremities, is commented upon, and sound reasons are adduced why this approximate form must be adhered to. Next (pp. 88–93) follows a brief but graphic picture of the behaviour of the mixed charge of ore, coke, &c., during its descent, from the charging to the final fusion of the iron in the crucible or hearth, which deserves careful reading.

The preliminary preparation of ores, *e.g.* drying, roasting, &c., previous to charging into the furnace, is strongly and rightly advocated. Iron manufacturers, however, are averse to anything entailing extra trouble and labour. It is the general opinion that, on the whole, little is gained practically, although theoretically the ordinary practice of charging raw material must, as the author says, entail considerable uncertainty during the subsequent smelting. We agree with Mr. Williams that the reactions of carbonic oxide and carbonic acid gases in the blast-furnace are even now not thoroughly understood; further, the part which solid carbon plays in the direct reduction of ores seems to us still somewhat mysterious. It is admitted that, when using soft coke, carbonic acid may be deoxidized, forming carbonic oxide. This occurs in the upper region of the furnace, and it is quite as probable that ore in contact with carbon may be also directly reduced in this part of the furnace. We have ourselves ascertained that, when intimately mixed oxide of iron and carbon are heated together, the reduction commences at a lower temperature than is usually supposed.

The theory of puddling is well discussed, and the working processes are lucidly explained. In addition the chemical reactions whereby purification is effected are given; also, as regards the elimination of sulphur and phosphorus, the work of Percy, Bell, and many others is ably summarized. The author also quotes his own results, derived from his experience and study, which, however, are not always quite in accordance with accepted ideas, but which nevertheless should be carefully considered.

Many pages are devoted to the question of what is steel; also fallacies concerning steel. Previous to the introduction of the Bessemer process, steel might be defined as a compound of carbon and iron, capable of being hardened, tempered, and welded. Steel containing $\frac{3}{4}$ per cent. of carbon was termed mild steel. The author states that by the Bessemer process steel is made as low as $\frac{2}{10}$ per cent. of carbon; really, however, large quantities of Bessemer steel, or rather metal are now cast with only $\frac{1}{10}$ per cent. carbon. The author does not mention this. One gathers that there is only one true steel, *i.e.* a compound of iron and carbon free from other constituents.

The old definition of steel, *i.e.* a compound of iron and carbon, is as true as ever, when applied as in the past to a material used for special purposes, viz. tools with cutting edges, &c.; but it would be perhaps more rational to say that the Bessemer product cannot in this sense be termed steel at all. It may be granted that a true steel is simply composed of iron and carbon, but such a material has its own particular uses; and should not be compared or confounded with the Bessemer product, where the very properties of being tempered, hardened, &c., with facility must be avoided. In proof of this it has recently been shown that Bessemer metal may be improved by the addition of small quantities of silicon, nickel, or even copper; also that for certain purposes a large quantity of manganese may be added to iron with advantage. Indeed, a well-known writer has gone so far as to hint that carbon may be eliminated altogether, and some other element advantageously substituted. It appears, therefore, that the author's definition of what is steel may in one sense be strictly true, yet may be inapplicable to the Bessemer product for the above reasons.

In chapter x., on fallacies concerning steel, the author reiterates that steel can only be manufactured from pure iron, showing clearly enough that many novel, promising processes have failed solely from the non-recognition of the necessity of freeing crude iron from certain objectionable elements.

The hardening, tempering, and testing of steel under varying conditions, also the cementation process, are discussed; as briefly put, they may be considered complete. The author's own investigations should be carefully studied. It is suggested that during the cementation process carbon itself is not transmitted, but that a soaking in of iron carbide occurs. A film of carbide is first formed on the surface of the metal; this unites or alloys with the layer of iron below; a lower carbide is formed which reacts in like manner, and so on to the interior; and borings taken from next to the surface, also at different depths below, indicate, as might be expected, varying properties of carbon. The gases occluded in the charcoal used for cementation appear to assist the reaction of carbon on iron. The writer has been informed that charcoal thus used becomes in time inert, having apparently lost occluded gas. Some metallurgists assert that carbon does not combine directly with iron, but recently this has been questioned, and it has been experimentally demonstrated that iron may combine with pure carbon. Discussing the *rationale* of cementation, it is suggested that possibly iron at a welding heat is really not a solid, but rather in a viscous, semi-fluid condition like sealing-wax or melted glass. Iron is thus rendered more porous to gases, and probably solids are thus absorbed. The experiments of Graham and others on the absorption of gases by iron seem in conformity; in fact, this absorption may be termed a species of cementation.

The Bessemer process is well described and its early history given; a clear explanation is afforded showing how the inventor's early anticipation that wrought or malleable iron could be manufactured direct was not realized, although the chemical reactions, viz. oxidation, &c., were similar in degree to those in the puddling furnace and refinery. Credit is given to Mr. R. Mushet, who first suggested the addition of spiegeleisen to the blown metal, thus converting a worthless material into the valuable material now termed Bessemer steel.

The chemical reactions of both the Bessemer and puddling processes are compared, and the necessity of using with the former a pure pig-iron free from sulphur and phosphorus clearly demonstrated. The primary reason given by Mr. Mushet for the addition of spiegel was that blown Bessemer metal contained an excess of oxygen in some form or other; and the triple compound of iron, manganese, and carbon was added with the sole object of removing oxygen, a slight excess being added to insure the necessary proportion of carbon. The author appears to have laid sufficient stress on this; the primary cause, according to Mr. Mushet, of the redshortness of the metal; and which is even now a difficulty, for it is known that oxygen cannot be entirely eliminated without the addition in many instances of an injurious excess of spiegel—the addition of silicon, and recently aluminum has been suggested, both favouring the elimination of oxygen.

P. 294. Viewing the Bessemer method as a purely chemical process, it is urged that chemical compounds, or solutions in admixture, will not always arrange themselves in the order of their chemical affinity; where a transfer of constituents can produce a solid compound, such transfer occurs as though the physical attraction of cohesion overpassed that of chemical affinity. Following this general rule, on which all chemical analyses are based, silicon (forming a solid) must go first; carbon follows; the rapid disappearance of manganese is due to its great affinity for oxygen; it also forms a solid silicate of manganese. Sulphur and phosphorus are not eliminated, owing doubtless to the small quantities present. This is hardly conclusive. The author says but little of the rapidity of the chemical reactions; and does not appear to have realized that ordinary chemical reactions may be modified in consequence of the abnormally high temperature attained during the Bessemer blow.

The influence of what may be termed mass has not been noticed; chemical affinities may thus be modified. Of two bodies in solution one may be greatly in excess of the other; on adding a precipitant it will be found that the element in excess is first precipitated, although the chemical affinity may be about the same or even greater for one element than the other. Barium sulphate is slightly soluble in acidified water, and it will be found that either barium or sulphuric acid may be precipitated at will by adding to the solution for the former a slight excess of sulphuric acid; for the precipitate of sulphuric acid it needs only to add a slight excess of a barium salt in lieu of sulphuric acid. As regards phosphorus, there can be no doubt that its presence is objectionable, and it is correct to say it injuriously affects the quality of any kind of steel. Sulphur is not so injurious.

The use of the spectroscope for controlling the blow is practically condemned; the author's experience, however, is not in accordance with that of other metallurgists. The basic process is shortly noticed, and the basic reaction summarized in a few words.

The part played by manganese as a steel improver is thoroughly discussed, the various theories are well ventilated, and the great affinity of this metal for oxygen insisted upon and emphasized. Manganese, however, when alloyed with iron in small quantities, can scarcely improve the quality. Mr. Hadfield's new alloys of manganese and iron cannot strictly be termed steel; the alloy possesses some of the properties of steel, and it may be compared to brass, which is not copper, but is, nevertheless, a substitute for copper, as candidly acknowledged by the author. His views as regards the mischievous action of small quantities of manganese contained in steel cannot be accepted in their entirety: they are certainly contrary to results of every-day practice in Bessemer steel works.

A theory of steel may be accepted as it is written; there can be little doubt as to the existence of a definite carbide of iron which alloys with metallic iron; although we have the alternative of assuming that carbon is simply difficultly soluble in molten or highly heated iron, yet the evidence seems in favour of the existence of the carbide. The properties which distinguish steel from crude iron are marked.

Alluding to the use and value of spiegeleisen, the author, curiously enough, confirms the old, nearly forgotten theory of Mushet, *i.e.* that for the steel manufacture, the triple compound of iron, manganese, and carbon is absolutely necessary. The carbide of iron, Fe_4C, must be more fusible than metallic iron, and some acute observations are made on the part which this fusible compound plays in the heating and cooling of steel under varying conditions. In other words, it is probable that the properties of hardening and tempering are thus conferred on iron, for this compound must be in a semi-fluid or plastic condition, whilst the iron is comparatively infusible. As liquids in cooling contract to a greater extent than solids, it is easy to imagine variations in temper or hardness due to the more or less rapid cooling of the steel. Sudden cooling must produce a violent molecular tension by the resistance of the rigid iron to the greater contraction, and variations in cooling, greater or less, must produce corresponding differences in the hardening or temper of steel. It is noted that, as regards solids, the coefficient of expansion is less than that of liquids; also, that the more fusible solids have a higher coefficient of expansion than the less fusible. This also plays a part in the production of the final product; and steel may be termed a *heterogeneous solid*, not homogeneous as usually assumed. Sir Lowthian Bell and Prof. Abel, however, assert that on heating piles composed of alternate layers of steel and wrought iron, the carbon contained in the former slowly diffuses, and the iron absorbs carbon from the steel. It is, therefore, probable that the heterogeneous solid of the author must, after repeated heatings, become practically homogeneous, and it follows that the so-called carbide, Fe_4C, may thus suffer decomposition.

In conclusion, after discussing the properties of alloys, particularly the alloy of sulphur and phosphorus with iron, the author ventures to state a general law, that the hardness of an alloy does not depend on the mean hardness of its constituents, but is harder than either of them, quoting numerous instances in support of this law. It is difficult to gainsay all this, and we recommend the theory of steel to the careful consideration of metallurgists. Other theories may be broached; for instance, carbon may, as before said, be soluble with difficulty in iron, and the proportion left in solution may depend on the rate of cooling, a portion remaining insoluble, showing its presence in the form of graphite or simply intermixed carbon. Prof. Akerman holds that carbon exists in iron in three different forms which may be distinguished; other well-known metallurgists confirm his views.

The following chapters, on iron *versus* steel for structural uses, and the stability of iron, are apparently ably written, and are recommended for the consideration of experts. The article on occlusion of gases in iron and steel is up to date. The researches of Graham, Deville, and Troost are quoted, and prominently those of Dr. Muller, to which latter the author appears to attach some importance. Iron, however, occludes other elements just as it does hydrogen—such as zinc, cadmium, magnesium, &c.; the same may be said even of carbon during the cementation process. Dr. Muller's method of collecting the gas by drilling gives no information as regards the gas actually occluded, which latter, it is evident, can only be extracted by heating in vacuum, as recommended by Prof. Roberts-Austen, and practised by Graham and Troost, also recently worked by Parry, Stead, and other chemists. JOHN PARRY.

THE THEORY OF LIGHT.

The Theory of Light. By Thomas Preston. (London: Macmillan and Co., 1890.)

MR. PRESTON has written a valuable book on an important subject, one which will, as he hopes, be suited to the reading of junior students, and yet sufficiently full to meet the requirements of many who desire a more special acquaintance with the subject. At the same time it is difficult to avoid expressing the wish that he had carried the mathematical development of some parts of his subject a little further, and, if space required it, had omitted some of the more elementary details; though the work, within the limits laid down, is so well done, that criticism may seem ungenerous.

The historical method adopted in some parts of the book adds greatly to its interest, and the account given of the development of the subject, from the days of the Greek philosophers to the present time, will lead many to study the original sources of Mr. Preston's information with profit to themselves. It is a good thing for us to read how the first masters of the subject expressed themselves; to know what were the difficulties they felt, and what the problems which appeared important to their minds. We, who, thanks to Young and Fresnel, have had the difficulties that surrounded the wave theory in the time of Newton cleared away, are less apt than we might be at recognizing their magnitude, and at grasping the ingenuity and skill with which Newton treated the emission hypothesis, and the marvellous manner in which, in his hands, it was made to explain many of the phenomena of light.

In the earlier chapters of the book, after an explanation of the rectilinear propagation of light, a good deal of space is devoted to the explanation of phenomena usually dealt with under geometrical optics. The ordinary formulæ for prisms and lenses are deduced from the principle that the time from a point to its image is the same by all paths possible for the light—a principle which, in Lord Rayleigh's hands, has led to important results in the theory of optical instruments.

The argument given by Lord Rayleigh in his article in the "Encyclopædia Britannica," for showing that the effect of a wave is equivalent to half that of the first Huyghens zone, and that the phase of the disturbance is a quarter-period behind that from the pole, might with advantage have been given in greater fulness in Article 54 (the reference at the end of that article should be to Art. 154, Ex. 3).

The book is very complete so far as it goes, but the limits imposed by the author on himself do not allow him to show any very great originality in treating his subject, at least until we come to chapter ix., section iii., where he deals with the graphic method of solving problems in diffraction. In this section Cornu's beautiful method is employed, and many problems, usually only solved by analysis, are completely worked out by it.

The analytical solutions are, perhaps, a little hardly dealt with, as the methods of evaluating Fresnel's integrals given by himself, Gilbert, and Knochenhauer, only appear as examples. The theory of the diffraction gratings strikes us as being also rather brief. Rowland's concave gratings are best treated from the consideration that the waves from all the bright spaces arrive in the same phase. Possibly Bessel's functions are outside the limits of the mathematical treatment allowed by the author: if not, a reference to them would improve the treatment of the diffraction problems arising out of the case of a circular aperture.

Fresnel's theory of double refraction is given clearly in chapter xii., and the difficulties of finding a dynamical explanation of it are well stated. It is here, however, and in the chapter on the dynamical theory of reflection and refraction, that we think the limitation of the mathematical development unfortunate. The elementary theory of elastic solids is given sufficiently for optical purposes in several works accessible to students. The author might easily, if he had liked, have introduced a few pages of it in his own book. He would then have been able to give and discuss the theories of refraction and double refraction of both Green and Neumann, or McCullagh, and thus have added greatly to the value of the work.

The book is brought up to date in a satisfactory manner. The last chapter contains an account of the modern work on the electro-magnetic theory of light, including the recent experiments of Hertz.

ENGLISH PATENT LAW.

The Law and Practice of Letters Patent for Inventions; with the Patents Acts and Rules annotated, and the International Convention, a Full Collection of Statutes, Forms, and Precedents, and an Outline of Foreign and Colonial Patent Laws, &c. By Lewis Edmunds, D.Sc., (Lond.), F.C.S., F.G.S., of the Inner Temple, Esq., Barrister-at-Law; assisted by A. Wood Renton, M.A., LL.B., of Gray's Inn, Esq., Barrister-at-Law. (London: Stevens and Sons, Limited.)

THIS is a work of considerable pretensions. We see by his preface that Mr. Edmunds claims to have produced a comprehensive treatise, dealing exhaustively with Patent law and practice, and when we mention that the book runs, in this its first edition, to upwards of 900 pages, our readers may be disposed to think that the author must have achieved his purpose. On a closer inspection, however, the formidable proportions of the work become greatly diminished. We find that the actual text of the book is only some 425 pages, the remaining 515 pages being supplied by statutes, Patent rules, international regulations, voluminous forms, and an index. We have always understood that a legal text-book ought to be in form as concise, and we might almost say as condensed, as possible, consistently with the importance of the subject-matter. Mr. Edmunds does not appear, however, to pay much regard to this wholesome rule. We rarely remember to have seen a legal text-book more gratuitously padded out than the work now before us. To take one illustration, the author devotes nearly 200 pages to printing the Patent Acts 1883–1888, twice over, for what good purpose we are at a loss to understand. The notes which are appended to what we must, under the circumstances, call the first edition of the Patent Acts 1883–1888, are of considerable value, but we cannot approve the system of cross references, by means of which the author seeks to incorporate, under various sections of the Acts, passages from the preceding text. By this device, Mr. Edmunds seems to have attempted to combine in one volume two inconsistent methods of text writing—the method which constructs a book by noting the sections of an Act, and the method which, relegating the statutes to an appendix, makes the body of the text a continuous treatise. There is much to be said for each method. Mr. Lawson's admirable work on Patent practice is an excellent illustration of the first; and the now old but well-known work of Mr. Hindmarch on Letters Patent is a felicitous adoption of the second. But we do not think a cross between the two can ever be satisfactory. Considering how fully Mr. Lawson's work meets the needs of practice, and how much more convenient it is in point of size than the book now before us, we think Mr. Edmunds would have done better to have devoted himself to the production of a treatise on substantive law only. A new work on Patent practice was not required by the legal profession, but a new work on substantive Patent law has long been a public desideratum; and we think the present author, with his industry and evident ability, might well have supplied that want. We are afraid, however, that that want still remains to be supplied.

Coming to what is the text of the book—Part I., Patent Law and Practice—we notice that Mr. Edmunds gives in his first three chapters an interesting historical account of the origin of English Patent law. But we are disappointed to find that the very important question of subject-matter is but scantily treated in a chapter of thirty-four pages. This in a work of nearly 1000 pages, claiming to be an exhaustive treatise, is a surprising deficiency. In this part of his book, Mr. Edmunds has, in fact, limited his space far too much, and betrayed a tendency to huddle important cases into footnotes—a tendency the more to be regretted considering the size to which the book has otherwise been allowed to grow. In his chapters on specifications and infringement, the author has been much more successful, and these show great care and considerable merit. The chapters on foreign and colonial Patent laws are interesting, but necessarily short, and where a statement of law has to be so condensed its utility must be very doubtful. The table of cases is very complete, and it is a useful addition to the usual citations to add, as Mr. Edmunds has done, the dates of the decisions. The appendix of forms is a very full one, and the index seems to be well compiled. The immense increase in the number of patents granted by the Crown in recent years has given to this department of our law a greatly enhanced importance, and while we have not scrupled to point out what we regard as the defects of Mr. Edmunds's work, we doubt not that the book will have a large circulation amongst those whose professional duties lead them to consult works on this branch of English law.

OUR BOOK SHELF.

Lessons on Health. By Arthur Newsholme, M.D., D.Ph. (Univ. Lond.). (London: W. H. Allen and Co., 1890.)

UP to the year 1889, the Science and Art Department of South Kensington required that candidates for the examination in hygiene should at some previous time have passed the Departmental test in physiology. Since that date, however, the Science and Art authorities have decided that the hygiene paper shall contain questions on physiology, embracing the general structure of the human body, the forms, positions, and uses of the more important organs, more especially the construction and action of the circulatory and respiratory systems, and of the digestive and excretory organs; and that a separate examination in this subject shall be dispensed with. Dr. Newsholme's "Lessons on Health" is a manual designed to cover the requirements of the elementary stage of the hygiene examination under the altered regulations. Writing for elementary readers, the author wisely begins by devoting a chapter to the chemistry of the chief elements which enter into the composition of the body. The next four chapters are taken up with histology and physiology, but here we do not think the author has entered sufficiently into detail to enable beginners to grasp the full meaning of what they are reading. Our objections have special reference to the histology. For example, the author tells us that the tissues, when examined microscopically, are found to consist of cells, which, in the case of muscular and connective tissues, have become transformed into fibres; and that the original appearance of cells is best seen in the cells of connective tissue, brain, and epithelium. No explanation, however, is given as to what is meant by a cell; nor even a brief account of the appearances and structure of the other tissues of the body; so that, when the reader comes to learn such facts as that the stomach is composed of four different coats, or that there are three layers in the wall of an artery, the latter differing from a vein in possessing more elastic tissue, he cannot form any adequate idea as to the meaning of these words. Again, in the description of the skeleton, the sterno-clavicular articulation is mentioned, but no allusion is made to the joint between the clavicle and scapula; the ulna is said to articulate with the humerus, but no mention is made of the fact that the head of the radius enjoys the same privilege.

The matter in the hygiene section of the book, both in arrangement and description, is excellent, and may be cordially recommended for the purpose intended.

J. H. E. BROCK.

Practical Inorganic Chemistry. By E. J. Cox, F.C.S. (London: Percival and Co., 1890.)

THIS is a volume of 51 pages, consisting of "the necessary notes, reactions, and analytical tables constantly required for reference" by students preparing for the elementary stage examination of the Department of Science and Art in practical inorganic chemistry. As only seven bases and four acids are included in the syllabus, and the mixtures given are soluble in water or dilute acids, the scope of the volume is very limited. The author begins by stating the possible number and character of the constituents of mixtures that come within the range of the syllabus, and then gives a list of all the substances available for the examiners to make the mixtures from. Then follow lists of reactions and tables of methods. After these is a quotation from the published description of that part of the examination that consists of questions to be answered, and as the examiners state that "the value of the answers will be greatly enhanced by neatness and clearness of sketches," the author proceeds to give "the sketches required," a series of 21 figures all duly labelled, and which presumably includes every sketch that can possibly be needed. The student is recommended to practice copying the figures until he "can draw the apparatus neatly and accurately."

Notes on Trigonometry and Logarithms. By Rev. J. M. Eustace, M.A. (London: Longmans, Green, and Co., 1890.)

THIS work is not like an an ordinary text-book, but consists of a series of well-arranged notes on the elements of trigonometry and logarithms. The subject is treated so that it may be useful to beginners, and to those working it up by themselves. The book-work will be found fully worked out, and, in each chapter, examples on it are given to demonstrate the methods of solution.

Great care has been bestowed on the explanations of the various manipulations to which logarithms can be applied, and the author has reprinted some pages of the mathematical tables published by Messrs. W. and R. Chambers, giving a full explanatory account of the method of using them, which to a beginner will prove most serviceable. Two excellent chapters on solutions of triangles and heights and distances give the student a good insight into the more common problems that are generally worked out in this way.

Miscellaneous propositions and examples are dealt with in the last two chapters: in the former, such propositions as the nine-point circle, distance between centres of circumscribed and escribed circles of triangles, &c., are discussed; while in the latter we have a series of well-selected examples taken from the usual sources.

Elementary Statics. By the Rev. J. B. Lock, M.A. (London: Macmillan and Co., 1890.)

MANY are the treatises which deal with the subject of elementary statics, but few can rival in clearness the present stereotyped edition of Mr. Lock's work. The alterations that have been made have not necessitated any considerable change in the character of the book. By the addition of some fully worked out illustrated problems, and of a carefully graduated set of interesting examples for the student to solve, the author has slightly enlarged the scope of the treatise. The number of the miscellaneous examples at the end have been greatly increased by the insertion of problems that have appeared in the Cambridge examinations in the last two or three years. The subject throughout is treated in the author's best style, and the book can be cordially recommended for the use of beginners.

Die photographische Retouche in ihrem ganzen Umfange. By Wilh. Kopske. (Berlin: Robert Oppenheim. 1890.)

IN order to remove the defects incidental to photographic pictures, a process of "touching up" has to be resorted to, and the present pamphlet of 80 pages in length offers instructions in this subject, which will be found of use by practical photographers. The amount of valuable information compressed within the compass of the little work before us is quite remarkable, and shows that the author is thoroughly familiar with this branch of his art. We can commend the book to photographic artists.

LETTERS TO THE EDITOR.

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

Photographs of Meteorological Phenomena.

AT the Leeds meeting of the British Association a Committee, consisting of Mr. G. J. Symons, F.R.S. (Chairman), Prof. Raphael Meldola, F.R.S., Mr. John Hopkinson, and myself, was appointed to report upon the application of photography to the elucidation of meteorological phenomena, and to collect and register photographs of such phenomena.

The success with which these instructions can be carried out necessarily depends in a great measure upon the voluntary co-operation of others.

Will you therefore allow us to appeal to photographers through the medium of your columns, and to ask all those who have in their possession negatives of clouds, lightning, hoar-frost, hailstones, or any other meteorological phenomena, or of damage done by whirlwinds, tornadoes, or storms, to communicate with me?

We shall be grateful for copies of any such photographs, but shall especially welcome offers of future assistance in the shape of photographs taken in accordance with some simple instructions which will be supplied on application.

ARTHUR W. CLAYDEN.

Warleigh, Tulse Hill Park, London, S.W., November 18.

Some Habits of the Spider.

THE experiment given by Mr. Boys can be successfully made with a common table-fork. The spider will seize the handle and grapple with it in a ridiculous fashion, but it soon tires of the performance. The prongs will continue vibrating for some little time if struck smartly on a wall.

A curious habit of the spider has perhaps been recorded, but I have never seen it noted. A large, dark spider is sometimes seen in the centre of a strong and regular web. Blow the spider with a slight puff, and if it does not fall or run away, it will shake itself violently for a quarter of a minute. These oscillations are not natural, as the spider will only produce them once or twice, and the natural oscillations are slower. The motion is circular and very rapid, so that the outline of the spider disappears and a blurred appearance three or four times as large as the spider is produced.

This habit is probably protective. Birds would be puzzled rather than frightened, and would find it difficult to make a good shot at the spider. The species of spider is fairly common in gardens and hedges, and is abundant in parts of Norway. It is dark, with a few light spots.

A. S. E.

Newton's Rings.

WHILE arranging some experiments on the interference of light for class illustration at the Working Men's College, Melbourne, with a friend, Mr. Wilfred Kernot, of this city, we came across a method of showing Newton's Rings which I have not seen described, and which may be new to some of your readers, though probably any who have had to arrange the experiments for themselves will have come across it.

The apparatus used was a pair of glass plates, $2\frac{1}{2}$ inches square by $\frac{3}{16}$ inch thick, squeezed by a pair of clamps at the centres of a pair of opposite edges. A beam from an electric lamp (900 candle-power) was sent through the plates so as to be partly reflected and partly transmitted, and the images formed by these two beams were received on a pair of screens about 5 feet from the plates. Holding the plates at an angle of about 10° with the incident beam, the complementary colours are shown with great brilliancy on the screens; by varying the inclination of the plates to the beam the colours can be changed at pleasure.

In this form the experiment is well suited for class illustration; care is necessary to avoid irregular reflection at the edges of the plates; we covered ours with ordinary gum paper.

B. A. Smith.

Working Men's College, Melbourne, October 10.

Mutual Aid among Animals.

Recent discussion of this subject has recalled my attention to an observation made some time ago, while studying the animals of Casco Bay, on the coast of Maine.

Among the specimens brought back from one excursion were four of the common Echini (*E. drobachiensis*). The last one taken had been left exposed to the sun some time before it was noticed and properly cared for.

These four animals were placed alone in a small aquarium, and, as we wished to study the action of the ambulacræ, each was turned mouth up. Soon the action began, with which every naturalist is familiar, and three of the captives slowly rose on edge, and then deliberately lowered themselves into the normal position. The fourth, the injured one, made much less rapid progress: all it could achieve was a slight tipping of its disk. The two nearest Echini, from six to eight inches distant, now moved up and stationed themselves on opposite sides of their disabled comrade.

Fastening their tentacles for a pull, they steadily raised the helpless urchin in the direction in which it had started. As soon as it was possible, one of the helpers moved underneath the edge of the disk on the aboral side, and, when the half-turn was accomplished, the other took station on the oral side, gradually moving back as the object of so much solicitude was very gently lowered to the position nature had made most convenient.

This is the best instance of "giving a lift" I have ever met with among animals of so low a grade. It may not be without interest to others.

Wm. Elder.

Colby University, Waterville, Maine, U.S.A.

The Chrysanthemum.

This being the centenary year of the introduction of the Chrysanthemum into England, a word on the subject from its native place, Peking, may not be out of place. It is not generally known that the Chinese grow the Chrysanthemum as a standard tree especially for selling. They graft them on to a stalk of Artemisia. There is a species of Artemisia that grows wild and covers the waste ground round Pekin; it springs from seed every year, and by the autumn attains to a tree 8 or 10 feet high with a stem 1½ inch thick. The Chinese cut it down, and, after drying it, use it as fuel; the small twigs and seeds are twisted into a rope, which is lighted and hung up in a room to smoulder for hours; the pungent smell of the smoke drives out the mosquitoes. This plant, after being potted, is cut down to about 3 feet and used as the stock, the twigs of Chrysanthemum are grafted round the top, and it quickly makes a fine tree, the flowers grow and open, and as the stock soon withers the whole tree dies, and folks say, "another ingenious fraud of the Chinamen."

A favourite style of growing Chrysanthemums is in the shape of a fan, with eight or ten flowers in different parts of it. If the flowers are not grown on the plant, they are tied on, which also does for selling.

The winters in Peking are very cold, and last about four months, and having no glass houses the Chinese gardeners do not have the chance of producing such a variety or such fine flowers as their European brethren, but in the case of Chrysanthemums they have many curious and beautiful varieties.

Thos. Child.

Dispersal of Freshwater Shells.

I am putting together such instances as I can find of dispersal of freshwater bivalves by closure of their shells so as to cling to the toes of birds, amphibia, water-beetles, &c., and of univalves by adhesion to the wing-cases of water-beetles, &c., and venture to ask for co-operation. Any notes or references which your readers may have the kindness to send to the undermentioned address will be welcomed and carefully acknowledged.

H. Wallis Kew.

5 Giesbach Road, Upper Holloway, N.

The Common Sole.

The post-larval flat-fish obtained in 80 fathoms off the west of Ireland, which in Nature (vol. xlii. p. 520) I referred to as common sole, have turned out, on closer examination to be the fry of *Pleuronectes cynoglossus*, called "white sole" in the Dublin markets.

I shall feel obliged by your finding space for this correction.

Dublin, November 15. W. Spotswood Green.

The Scientific Investigations of the Fishery Board for Scotland.

In the review of the "Eighth Report of the Fishery Board for Scotland," which appeared in Nature (vol. xlii. p. 653), the reviewer, misled by the private information to which he refers, makes an inaccurate and baseless statement, reflecting upon me personally, and which I therefore crave leave at once to correct. In dealing with my report on immature fish, which, by the courtesy of the Secretary for Scotland, was placed in the hands of the delegates of the recent International Fisheries Conference, and which has already been referred to in your columns by Prof. McIntosh, F.R.S., the reviewer states: "We have certain information that the *original discoveries which led to* this report were made" by Mr. T. Scott; and that "it is only fair that the credit which is Mr. T. Scott's due, *and which is denied him there*, should be acknowledged here."

Had your reviewer disregarded his private information, and looked at p. 161 of the paper which he has reviewed, he would have found there the following footnote to the statement that "nearly 13,000 food-fishes" had been "carefully measured, and the condition of the reproductive organ registered," viz., "*This has been mainly done by Mr. Thomas Scott, F.L.S., one of the naturalists of the Fishery Board, and partly by Mr. Peter Jamieson, assistant naturalist.*"

What Mr. Scott and Mr. Jamieson did was precisely what is stated—namely, to measure the length of the fish and record on the form provided whether the milt or roe was mature or not. The subjoined note from Mr. Thomas Scott, which I request you to publish along with this, shows that he considers this acknowledgment sufficient. The study and elaboration of these daily records, nearly 13,000 in number, mainly in my private time, was only a part, and a small part, of many months of labour bestowed on my report on immature fish; and the results occupy less than three pages of the fifty-four devoted to the subject. No other person had any part or share whatever in the conception or composition of that report, and this attempt to deprive me of the credit of my work, solely on the strength of private and erroneous information, is not, I think, either usual or creditable.

The reviewer is equally in error as to what I wrote in the Report for 1887, and which he only partially quotes. The entire sentence is as follows: "We have organized a series of extensive and systematic inquiries into the condition of the reproductive organs of the various kinds of fish throughout the year, with particulars as to their sizes, the nature of their food, &c., which will help to clear up the hitherto obscure problems as to the minimum size of sexually mature individuals, the commencement and duration of the reproductive period, the spawning places, and many other points of great interest." If the reviewer will now peruse p. 8 of the Seventh Report, he will find it there stated that these inquiries were "*devised by Dr. Wemyss Fulton*" (in 1887), which is the fact.

T. Wemyss Fulton.

20 Royal Crescent, Edinburgh, November 3.

14 *Lorne Street, Leith, November* 1.

Dear Sir,—I have read the article in Nature of October 30, and desire to say that I consider the footnote at p. 161 of part iii. of the Board's Report for 1889 a sufficient acknowledgment of my work in connection with the immature fish investigation.

You have always from the first acted towards me in a very friendly manner, and would be the last to detract from any credit belonging to me.

Thomas Scott.

Dr. T. Wemyss Fulton, Secretary Scientific Investigations.

Araucaria Cones.

Having been away from home, I have only now seen the Duke of Argyll's letter in Nature of November 6 (p. 8), relating to the cones of Araucaria. Doubtless before this some of your correspondents have answered the Duke's inquiry.

It is not very unusual for the *Araucaria imbricata* to produce cones. The first I myself remember to have seen were on the old tree in the Royal Gardens, Kew, in the summer of 1851 or thereabouts. The female cones are large globular masses, the constituent scales of which are not (superficially) very different from the ordinary leaves. What the Duke describes are evidently the male catkins. The trees are ordinarily diœcious, but I have once seen and figured an example in which male catkins and female cones were borne on the same tree.

London, November 14. MAXWELL T. MASTERS.

IN the garden of the house Bleckley, Shirley Warren, Southampton, there is an Araucaria that for many years past has produced annually a large number of cones. The cones are from 40 to 100 in number, and very large, so that their breaking up and falling on to the lawn is a serious inconvenience, it being difficult to sweep them up. No fertile seeds have been produced by this tree, which from all I have been able to learn is the finest Araucaria in England; the trunk is over 6 feet in circumference some 2 feet above the ground. There is no history of the tree.

Cambridge, November 15. D. SHARP.

IF the Duke of Argyll refers (November 6, p. 8) to ovule-bearing cones, which are spherical and about 7 inches in diameter, these have been plentifully produced in almost every part of the British Isles.

Male or pollen cones (catkins), of cylindrical shape and 3 inches long, are, however, extremely rare, although they have been produced in the Bicton Pinetum and on one of Earl Derby's Kentish properties. A tree at the latter place bears annually, and has done so for some years, a heavy crop of perfectly-developed pollen cones; indeed, so great is the quantity that at a short distance away the tree has quite an unusual and remarkable appearance. A. D. WEBSTER.

Holwood Estate, Kent, November 17.

ATTRACTIVE CHARACTERS IN FUNGI.

THIS subject, which has been introduced by a letter from a correspondent (November 6, p. 9), is one of considerable interest, but it is one also of great mystery and difficulty. In dealing with fungi of the mushroom type we are in contact with a class of plants so different from Phanerogams that it is at once evident that we must not draw the same conclusions from a similar series of initial facts. It is well known that certain fungi possess strong and characteristic odours, and others very conspicuous colours, both of which features are presumed to have some value in the biography of the plant, but *what* influence and *what* value it is not so easy to determine as in the case of plants in which cross-fertilization has to be effected. It is by no means certain that there is any special act of fertilization at all; it is even doubtful if any fertilizing element exists. For nearly a century it has been thought possible to find a fecundating element in Agarics, but all efforts at demonstration have failed.[1] Most of these investigations have been directed to the cystidia, large cells which are recognized as projecting, more or less, on the surface of the hymenium, but these could not be identified with any known process of fecundation.[2] M. de Seynes, after patiently investigating the hymenium of the *Hymenomycetes*, arrived at a negative result, and this has not since been disturbed. "The hymenium," he says, "has not yet offered an organ which we may suppose in reality to be the male organ;" and he adds, "one sole and self-same organ is the basis of it, according as it experiences an arrest of its development; as it grows and fructifies, or as it becomes hypertrophied, it gives us a paraphysis, a basidium, or a cystidium; in other terms, atrophied basidium, normal basidium, hypertrophied basidium: these are the three elements which form the hymenium. Does it develop either outside the hymenium or on the hymenium, at a time, or in a part which has not yet been discovered, organs which yield pollen, spermatia, antherozoids, or any other fecundating agent? This is what remains to be discovered."[1]

[1] De Bary, "Morphologie und Physiologie der Pilze," cap. v.
[2] See *Grevillea*, vol. i. (1873), p. 181.

Amongst British mycologists Mr. Worthington Smith has been the most persistent in belief that Agarics are subject to hybridism, which implies cross-fertilization, but he has not contributed much towards the establishment of the proposition that fertilization really exists, except perhaps to emphasize the suggestion that the cystidia are male organs. In his paper on the reproduction in *Coprinus radiatus*,[2] he remarks: "I consider it quite possible that the mere contact of the threads (or fluid) from the cystidia with the threads from the unpierced spores may be sufficient for the production of a new plant." In more direct reference to the question of hybridism he writes:—"On a dung-heap, which will produce *Coprinus radiatus*, other species, as *C. nycthemerus*, &c., are sure to appear; and not only do allied species come up in company with *C. radiatus*, but every intermediate form between one and the other may be gathered any morning. These latter plants belong to no species described as such, but are natural hybrids, doubtlessly produced by the spermatozoids of one plant piercing the spores of another. Amongst the larger species of Agarics similar forms are quite common, and they prove sore puzzles for those men who only want *names* for the fungi they find."

No one with any extended experience in field work can gainsay that individual Agarics are often met with which strongly suggest hybridism. These forms are so intermediate between more typical forms, with which they were perhaps growing, that it is difficult to get rid of the idea altogether that they are modifications due to some such influences as in higher plants we attribute to hybridization. It would be very unphilosophical to deny absolutely that they are possibly hybrids; but, on the other hand, it would be as bad to declare them hybrids until some sort of impregnation can be demonstrated.

Admitting that hitherto all efforts to discover any process of fertilization in Agarics, which will stand the test of examination, has failed, the difficulty is increased in speculating upon the "why and wherefore" of the phenomena of odour, taste, and colour, in the larger fungi. Yet, notwithstanding this, we may approach nearer the desired end by endeavouring to collect facts, which may some day, by accumulation, serve as a basis for hypothesis.

Why do certain fungi possess very strong odours, which to our olfactory nerves are agreeable or disagreeable? There is a small whitish Agaric, not uncommon amongst grass in woods, which has such a strong and peculiar odour that it is named *Agaricus* (*Clitocybe*) *fragrans*. It is not more than about an inch in diameter, is mild to the taste, very pleasant to eat when cooked, and the odour remains after the plant has been dried for some time. Some persons detect in it a resemblance to anise, others to melilot, or the Tonquin bean, and others again regard it as an odour peculiarly its own. Two or three other species, to be found in similar localities, might, at a glance, be confounded with it, but that they are destitute of the odour and pleasant flavour. The novice could at once distinguish this fungus from its associates by its odour, but wherefore it should smell so sweet whilst the others do not is at present an unsolved mystery. It is certainly not specially attractive to insects, and we have never found it attacked by slugs; perhaps the odour is disagreeable to them.

Another Agaric may be found amongst dead leaves, which is twice as large, and of a singular pale verdigris-green colour (*Agaricus* (*Clitocybe*) *odorus*). It possesses very nearly the same odour, possibly a little stronger, and the same agreeable taste. This, again, we have always observed to be free from any indication of attacks from slugs. We have failed to detect the same odour, except

[1] *Grevillea*, vol. ii. p. 41.
[2] *Grevillea*, vol. iv. (1875), p. 53.

perhaps very faintly in one or two instances, in any other species of Agaric. There is, however, a *Lactarius* which resembles an Agaric in form, but contains a copious supply of white milk somewhat acrid to the taste, and an odour not much unlike but rather more camphoraceous than the two Agarics. This is *Lactarius glyciosmus*, which has a reddish-cream colour when dry, but is more ruddy when moist. In addition we may mention a densely tufted fungus growing on stumps, *Lentinus cochleatus*, with a fainter but similar odour to the *Agaricus odorus*. One species of *Hydnum*, in which the gills of the hymenium are replaced by spines, has but a faint smell of melilot when fresh, but in drying this odour is intensified, and remains persistent for three or four years. This is known as *Hydnum graveolens*, but is somewhat rare in Britain. Finally, we have two species of woody *Trametes*, found growing on trees, which possess an odour of the same type. These are *Trametes suaveolens* and *Trametes odora*, and quite resembling them is *Trametes inodora*, which has no distinct odour at all. The chemical character of this odour has never, to our knowledge, been investigated, but the point now in question is the reason for its existence, and for this all conjectures hitherto offered are weak. In several of the fragrant species it will be remembered that there are similar and allied species which have no perceptible odour; it is possible that this fact may have some value in the investigation.

Passing over other types of odour which prevail in fungi, we will take as a final example a pungent odour as of nitric acid, which is by no means uncommon. It is rather rare with white-spored Agarics, but is often met with in pink-spored species, many of which are either doubtful or poisonous. The majority of instances amongst white-spored species will be found in the subgenus *Mycena*, wherein the species are small and delicate, such as *Agaricus* (*Mycena*) *alcalinus*, and *Agaricus* (*Mycena*) *ammoniacus*, to which may be added also *Agaricus* (*Mycena*) *metatus* and *Agaricus* (*Mycena*) *leptocephalus*, although in a less degree; but the culminating examples will be found in the *Hyporrhodii*. One of the commonest of woodland Agarics in the autumn is *Agaricus* (*Entoloma*) *nidorosus*, which the nose will always determine if the eyes should fail. It is a pale mouse-coloured species, generally about 2 or 3 inches in diameter, which the odour would be sufficient to deter anyone from feeling desirous of tasting, if it had not also a very suspicious appearance. It may be taken for granted that where this odour prevails the species are not edible, even should they escape being positively poisonous. There is a suggestion of the same odour in *Agaricus* (*Hebeloma*) *elatus*, but a full development of it in *Agaricus* (*Hebeloma*) *nauseosus*. It becomes faint in *Agaricus* (*Pholiota*) *heteroclitus*, as it is also in *Cortinarius nitrosus*; but in all there is more or less of the same pungency, which recalls to mind the fumes of nitric acid. It may be inferred from the name of *Hygrophorus nitratus* that it possesses the same odour, and others might be named which partake of it in a less degree, so that this may be accepted as another type of odour to be found in many species of fungi. Here, again, the same question arises as to what value this peculiarity may be supposed to possess for the plant itself, because it is no symptom of decay, since it is present with the plants named above in their youngest and in their most healthy condition. Nothing that we have observed would suggest protection from, or attraction for, insects or mollusks.

Thus much for odours, which must be taken as suggestive, and not by any means exhaustive. The subject of taste may be passed over as of doubtful value in estimating attraction or repulsion, or at least it is of secondary importance, and should not stand in the way of a few suggestions on the subject of colour. It may be premised that, although such a large number of species of Agarics flourish amongst grass, very few possess a green colour. We have alluded to one which is most commonly found amongst dead leaves; *Russula virescens* is seldom seen amongst grass, and the colour of *Agaricus æruginosus* is not in the least concealed when growing amongst grass, until it has lost the greater part of its green gluten and exhibits the dirty yellow cuticle. Dull-coloured species, of various shades of olive, brown, and grey, are common enough, so as readily to be confounded with the soil, stones, and dead leaves, upon or amongst which they are growing; but what excuse can be made for the bright red and yellow species appearing in such gaudy attire? Reds verging upon deep orange, as in *Agaricus* (*Amanita*) *muscarius*, and passing through all stages of vermilion and carmine to deep purple, are by no means uncommon, especially in the genus *Russula*. And here it may be remarked that the species of *Russula* are to be seen in greatest plenty and perfection during those months when flowers are exhibiting their brightest colours, and before the mass of proper Agarics make an appearance. A red *Russula* in October or November would be a far more conspicuous object than if it occurred in June or July, when *Russula* puts in an appearance. *Hygrophorus*, on the other hand, is a late genus, containing some very bright red and yellow species; but these are small, and commonly so immersed in the grass, on lawns and pastures, that they are not conspicuous. Their "season" is October and November. It is easy enough to comprehend the advantage of coloration to such species as *Hygrophorus psittacinus*, *Hygrophorus conicus*, *Hygrophorus Wynniæ*, and even of *Hygrophorus chlorophanus*, when seen growing amongst autumnal grass. The large species of *Russulæ* would, under the same conditions, be most conspicuous. Out of forty British species there are not less than twenty-five which are of some tint of red, or have varieties of those colours. What protective value can there be in expanded disks of bright scarlet 4 or 5 inches in diameter? If it should be contended that they are attractive, and not protective, then it becomes a question as to what they may attract. Slugs are fond enough of devouring not only red, but white and dull-coloured species, not refusing a meal upon one of the most poisonous (*Russula emetica*), and still it is the top of the pileus they devour, apparently in preference to the gills, so that they cannot be regarded as intelligent workers in the distribution of species.

A careful examination of the plates in any good illustrated work on the Agarics will show that in the whole of the old genus *Agaricus*, adding also *Coprinus* and *Marasmius*, the number of brightly coloured species are remarkably few. In the genus *Cortinarius* the colours are brighter, but they are not conspicuous when growing, because violet is not a demonstrative colour, and pale yellow, or lemon colour, is not observable amongst dead leaves. If space permitted it could be shown that, in a majority of instances, the colours of Agarics are protective, inasmuch as they harmonize remarkably with the matrix that supports them. With *Russula*, *Lactarius*, and *Hygrophorus*, the case is different, for there are many very obtrusive species, notably the red ones, for which we cannot formulate an excuse.

A word or two, in conclusion, on the intermediaries, or agencies, for the diffusion of fungi. It must be understood that in this communication we are confining ourselves to the large, or pileate, fungi, principally of the mushroom type. The agents named in the letter already alluded to are: "Horses, oxen, sheep, foxes, squirrels, moles, birds, snails, and insects." Squirrels are very fond of *Boleti* especially, but they eat the top of the pileus in preference to the pores, or spore-bearing surface; snails and slugs are undoubtedly mycophagous to a considerable extent. Of birds we have no evidence, as far as I am aware; ducks will eat fragments, when thrown to them, of such known virulence as *Agaricus muscarius*, and *Boletus luridus* without subsequent inconvenience, but it is doubtful if birds seek fungi, except to beat them in pieces and pick out the larvæ. Whether horses, oxen, and sheep really *eat* the common mushroom, we venture to call in question, but they *do* eat the grass upon which fungus spores have fallen. We have observed horses, cattle, and sheep eating the grass all around where mushrooms have been growing, and seen them pass on, leaving the mushrooms for us to gather on our own account. This does not show much animal predilection for fungus food, and hardly bears out the paragraph that "horses, sheep, and oxen are all readily attracted by the taste and mealy smell" of the mushroom. Without venturing to throw doubt upon the old faith that the spores of the mushroom are doomed to pass through the entrails of a horse, or that a horse or cow may sometimes even eat a mushroom if one comes in its way, still we have great hesitation in accepting as an article of belief that horses are really so fond of fungi that they seek them out, and devour them bodily, for the sake of the preservation of the species. Mushroom gatherers by preference go into meadows and pastures where horses and cattle are feeding in order to fill their baskets, but this could hardly be the case if the animals themselves were so fond of the delicacy, and hence it may be inferred that it is not wholly true that mushroom spores pass through their host because that host recognizes the mealy smell and pleasant taste of the mushroom itself, but rather that they are swallowed unwittingly with the grass over which they are dispersed.

The general question still remains unanswered: "What can be the service which the presumably attractive characters of fungi induce animals to perform for them?" In the case of the *Phalloideæ* there need be little hesitation in furnishing an answer. The fœtid odour of *Phallus*, *Clathrus*, and their allies, undoubtedly attracts flies in great number, and these latter suck up the slimy mass, which contains the spores, with such avidity that scarce a speck is left. These spores are all most remarkably small,[1] so as to leave no doubt as to their being ingested whole, and probably excreted in the same condition, but how, when, and where, is a mystery still. The inference would be that, if true in this instance, why not similarly in others? and hence the inquiry. Unfortunately the data are too few for generalization, and all we can do is to demonstrate that the subject is worthy of investigation, and, as Mr. Straton has observed, "one that requires the gathering together of much individual observation in all parts of the world." Few people hitherto have considered fungi of sufficient interest or importance for any other effort than to kick them over whenever encountered, but in this respect a reform would be imminent, if, by reiteration of the questions here set down, and a wider distribution of suggestions as to the kind of observations required, a larger number of persons could be interested in looking for and recording them. If there are no sexual elements to be discovered, it is still desirable to ascertain what conditions are requisite to secure the successful germination and growth of the agamospores, and how intervening agents might aid the process. The least glimmer of light is always welcome in a dark place.

M. C. COOKE.

LUMINOUS CLOUDS.

LUMINOUS clouds, which were first seen in 1885, are now acknowledged to have so much importance that it may be worth while to present a brief survey of the phenomenon and the facts established by the observation of it.

On June 23, 1885, about 9.50 p.m., local time, I noticed an extraordinary brilliance produced by light-clouds in the north-western sky. I had always previously directed great attention to clouds, and on this account these bright clouds appeared to me the more surprising and puzzling. About 9.50 p.m. the north-western and northern sky was covered, to the height of about 20°, with a layer of bright cirrus-like clouds, which reached from about N.W. to N.N.E. In this layer, the lowest part of which was concealed from me by houses and trees, three horizontal zones could be distinguished. The lower zone was without lustre, and had a yellowish appearance; higher up there was a strip, several degrees in breadth, which shone with an extremely beautiful, white-gleaming, silver-like light; above this strip was another like it, but not quite so brilliant, of a bluish tint. The light of the central zone was comparable to the light of the nearly full moon, when it stands at sunset at about 10°, more over the eastern horizon. About 10.30 p.m. the height of the upper limit of the phenomenon had been somewhat lowered; the three zones were still there, but had become —especially the uppermost one—somewhat narrower.

The position of my place of observation—Steglitz, near Berlin—is 52°·5 N. lat.; about 9.50 p.m., local time, the depth of the sun below the horizon was about 9°. It is well known that, at this depth of the sun, ordinary clouds cannot any longer be affected by direct sunlight.

The same phenomenon appeared pretty often in the course of the following weeks; and I had, therefore, repeated opportunities of studying its peculiarities. I have never seen anything of the same kind at the time of sunset. As a rule, the phenomenon began to appear from 15 to 20 minutes—but sometimes 40 minutes, or more—*after* sunset. Several times I remarked that almost the whole sky—with the exception of a segment in S.E. at the height of from 10° to 20°—was covered by the gradually increasing brilliance. In all these cases a gradual, progressive extinction of the phenomenon, proceeding from S.E. to N.W., was observed. The luminous clouds, when they first shone, generally gave forth only a feeble light. As the sun sank deeper, a gradual, but in the end complete, extinction of the phenomenon took place from the south-eastern side; but at the same time the light of the remaining part became steadily stronger, until it reached its highest degree of strength, when the upper limit in the N.W. had a height of about 12°. From that point onwards the strength of the light decreased.

On some evenings the phenomenon was specially striking, less in consequence of the light than in consequence of an occasional want of light. Several times I observed—the sky having been perfectly clear when the sun set—that about an hour after sunset an absolutely impenetrable black wall, like a threatening thunder-cloud, appeared in the N.W., from the horizon to a height of from 5° to 20°. Higher up, on the contrary, the silver-bright shining showed the presence of the phenomenon. Gradually the black shadow disappeared, from above downwards, and gave place to the intense shining.

Towards the end of the month of July 1885 the luminous clouds disappeared, and it seemed as if the phenomenon had come to an end. It was therefore the more surprising when, towards the end of May 1886, the phenomenon again presented itself suddenly. As in the preceding year, it remained visible, with some interruptions, until the beginning of August. The phenomenon has since been repeated from year to year, always at the same season.

As the result of incessant efforts, I have succeeded in establishing that luminous clouds migrate in the atmosphere of the earth in such a way that during the months of December and January they are to be found in the southern hemisphere at the latitude of from about 48° to 60°. No information with regard to the phenomenon in equatorial regions has yet been received. This suggests the possibility that in passing through these regions it is not visible; but when we consider that also in the temperate zone there are extensive districts in which

[1] Not more than 3 micromillimetres in diameter.

the phenomenon must certainly have presented itself, but from which no record of observations has hitherto come, the fact that it has not yet been observed at the equator will not lead us to conclude that it is not visible there in intermediate times.

The above-mentioned decrease of the apparent height of the upper limit of the phenomenon, coinciding with the deeper sinking of the sun, causes us to recognize that it is due mainly to direct illumination by the sun. Starting from this assumption, we may readily find the principles for the determination of the height of the phenomenon. During the first years, therefore, I frequently made measurements of the apparent height of the loftiest point of the arc which limits the phenomenon towards the S.E.; and, having regard to the time of the measurements, I found that the distance of the phenomenon from the surface of the earth was from about 50 to 60 kilometres.

The knowledge of this extraordinary height excited in me the most intense interest, and my aim now was to determine the height by a more trustworthy method. For the ultimate success of my efforts I am especially indebted to the co-operation of Prof. Förster, Director of the Berlin Observatory. On the evening of July 6, 1887, Dr. Stolze and myself (the former having taken up his position in Berlin, while I observed the phenomenon from the Potsdam Observatory) succeeded in each getting two simultaneous photographs of the luminous clouds. The calculation made in accordance with these photographs gave a height of about 75 kilometres. But this estimate was not perfectly satisfactory, so far as precision was concerned; for, in the first place, the basis of about 26 kilometres was small; secondly, the direction of the basis was such that it formed with the direction towards the luminous clouds too small an angle; and, thirdly, the photographic apparatus employed had not been worked with sufficient exactness.

FIG. 1.

In the year 1889 the luminous clouds were at last repeatedly and simultaneously photographed, with improved apparatus, at Steglitz and Nauen, which are distant from one another about 35 kilometres, and lie with regard to one another in the direction from east to west. At Rathenow also, 70 kilometres west from Steglitz, photographs were taken. These were not exactly simultaneous with the others, but the time differed only by a few seconds; and they are useful at least as a means of checking the results obtained from the photographs taken at Steglitz and Nauen.

From these photographs it is inferred with great certainty that the distance of the luminous clouds from the surface of the earth on July 2, 1889, was 81 kilometres, and that it was 82 kilometres on July 31, 1889. For June 12 there is an estimate of 90 kilometres, but this is less certain than the other two.

These results follow from the measurement of 108 different points, corresponding to one another, which are distributed on six pairs of plates; and it is interesting, from the remaining errors of the single groups, to test more closely the question what part is taken in these errors by, say, the thickness of the cloud-layer in a vertical direction.

It is well known that there is a certain law relating to the probability of the distribution of errors in accordance with their greatness. According to this law, it is to be anticipated that errors which lie between the triple and the quadruple value of the mean error, occur *once* among 80 different points which have been measured in the photographs of July 2 (in which the conditions of accuracy were the most favourable); and, further, that of errors which lie between the double and the triple value of the mean error *six* are to be expected. In reality, the calculations agreed very well with the number of observed cases—viz. 2 and 5 respectively.

These figures show very plainly that the differences of the results with regard to the height are essentially a consequence of errors of measurement, and that the thickness of the cloud-layer itself was very small, perhaps only the fraction of a kilometre. With this agrees the almost exactly similar aspect of the phenomenon at the two places of observation.

Figs. 1 and 2 represent the phenomenon as it appeared on July 2, 1889. The photograph reproduced in Fig. 1 was taken in Steglitz at 13h. 21m. os., Berlin mean time, and Fig. 2 simultaneously at Nauen. It is interesting to observe the parallactic shifting of the same cloud-points, in the two illustrations, in a fixed direction. In each of the illustrations, two stars, α and β Aurigæ, appear. On account of the enormous distance, the lines of direction, in which one and the same star is seen simultaneously from different points of view, are parallel to one another. Hence the deviation of two corresponding cloud-points, in the illustrations, with regard to one and the same star, gives a measure for the parallax of those cloud-points, on the supposition that the focal distance of the photographic apparatus is known. The focal distance of the two sets of apparatus was precisely determined by the photographing of stars, and proved to be almost exactly 200 mm. In accordance with this the above-mentioned height of 81 kilometres was found.

The following peculiarities, which observation of luminous clouds has firmly established, are of great interest:—

(1) Luminous clouds had in general a very rapid movement from north-east to south-west. In some cases movements also took place in the opposite direction; but these were always much slower—and they were also much less frequent—than those first named.

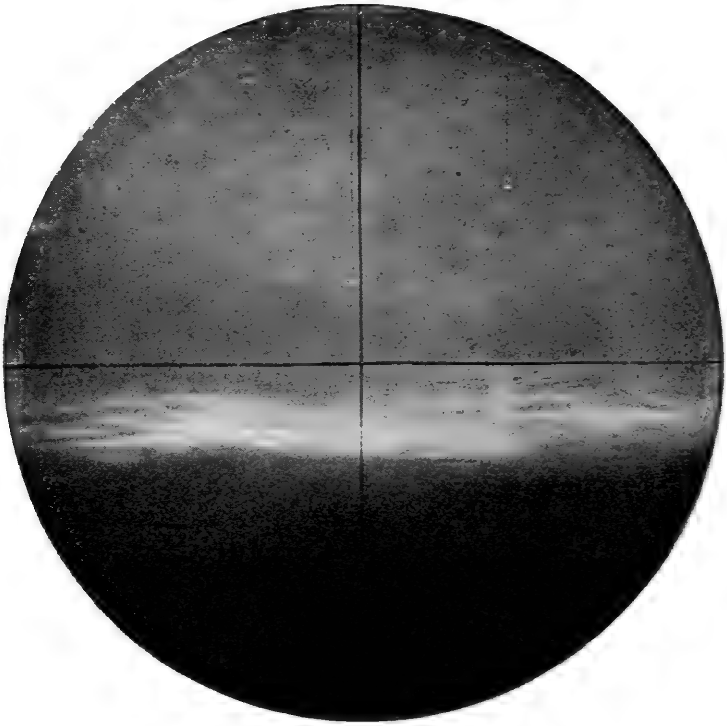


FIG. 2.

(2) Since their first appearance, luminous clouds have to a considerable extent waned. In the year 1890 they have displayed a beautiful brilliance during only about three nights; at other times the light was for the most part very feeble. Very probably we must connect with this decrease of light the fact that the apparent height at which the clouds have been seen, has been very much smaller in the last years than it was formerly.

(3) Luminous clouds present themselves generally much more brightly—therefore are more frequently visible—after than before midnight. While in the first years they appeared before midnight very frequently, they have done so in the last years very seldom. After midnight they still appear pretty often. Whether this distinction existed during the first years, was unfortunately not established, because the regular observations were then usually limited to the time before midnight.

O. JESSE.

NOTES.

THE anniversary meeting of the Royal Society will this year be held on Monday, December 1, St. Andrew's Day falling on a Sunday. The medals are to be given as follows:—The Copley Medal to Prof. Simon Newcomb, for his contributions to gravitational astronomy; the Rumford Medal to Prof. Heinrich Hertz, for his work in electro-magnetic radiation; a Royal Medal to Prof. David Ferrier, for his researches on the localization of cerebral functions; and a Royal Medal to Dr. John Hopkinson, for his researches in magnetism and electricity; the Davy Medal to Prof. Emil Fischer, for his discoveries in organic chemistry; and the first Darwin Medal to Mr. A. R. Wallace, for his independent origination of the theory of the origin of species by natural selection. The anniversary dinner will take place at the Hôtel Métropole.

LORD RAYLEIGH has been appointed an honorary member of the Bavarian Royal Academy of Science.

PROF. J. A. EWING, F.R.S., has been elected Professor of Mechanism and Applied Mathematics at Cambridge, in succession to Prof. Stuart.

MR. WILLIAM LEADBETTER CALDERWOOD, who was for several years naturalist to the Fishery Board for Scotland, has been appointed Director of the Laboratory for Marine Zoology at Plymouth.

SOME time ago the Phi Beta Kappa Society proposed that the four hundredth anniversary of the discovery of America should be signalized by a memorial history of American literature and science. Two prizes of 3000 dollars each were to be offered for the best general survey of American literature and science respectively; and, in addition to this, it was proposed that the preparation of an extensive and detailed account of scientific achievement should be prepared, the work for each department being entrusted to a specialist in that department. The New York *Nation*, which calls attention to the matter, speaks of this last undertaking as "the really serious task, and the only part of the scheme which possessed in a high degree the monumental character." We learn from the *Nation* that a committee having the project in charge is about to meet and confer upon the initiation of the work. The committee consists of the presidents of six of the most important American Universities, together with one or two other gentlemen of equal eminence.

IN the *Kew Bulletin* for November many interesting facts with regard to the cultivation of Liberian coffee are brought together. The same number contains an excellent account of the cola nut (*Cola acuminata*, R. Br.). In early times, cola nuts were supposed to be used merely as a means for rendering water sweet and palatable when drunk before or after meals. "But," says the *Bulletin*, "it was soon evident that they possessed other properties, and that they had been selected as if by intuition on account of the property which undoubtedly they did possess of supplying a necessary stimulus to those who have to endure an occasional or prolonged deficiency of animal food; for in West Africa, as in other parts of the tropics, the flesh of animals is often scarce and difficult to procure. The use of cola nuts to render water palatable may be compared to that of olives in European countries. The latter are well known to enhance the flavour of whatever is eaten after them. On the other hand, the power said to be possessed by cola nuts of staying the cravings of hunger, and of enabling those who eat them to endure prolonged labour without fatigue, is comparable to that ascribed to the leaves of the coca plant of Ecuador and Peru. In fact, cola nuts in Western Africa play the same part that *Erythroxylon Coca* does in South America."

ON Monday, the Master (Sir James Whitehead), the Wardens and Court of the Fruiterers' Company, and others, had an interview with the Lord Mayor at the Mansion House, to seek his aid in a project for the encouragement of the culture of British fruit. Sir James Whitehead, addressing the Lord Mayor, said that, having regard to the great success which had attended the recent exhibition of fruit at Guildhall, the Fruiterers' Company were now proposing to increase their operations in the same direction. Their idea was to have fruit shows in different parts of the country, similar to the annual shows of the Royal Agricultural Society of England, in co-operation with the local horticultural societies. During these shows there would be lectures on various subjects connected with the cultivation of fruit. It would be the aim of the Company to give the warmest encouragement to the various horticultural societies, and to stir up a spirit of emulation among the local and parochial organizations. The Company had arranged that the committee of experts who recently assisted them in connection with the Guildhall exhibition should meet once a month, and receive and answer questions on the subject of fruit culture. It was estimated that to carry out these objects a fund of about £20,000 would be necessary. It was believed that landed proprietors and the City Companies would be willing to aid this effort, and they might even obtain the assistance of a Government grant. What they asked the Lord Mayor to do was, first, to allow a great public meeting to be held at the Mansion House, and, secondly, to organize a Mansion House fund with the view of raising the necessary sum. The Lord Mayor cordially agreed to convene and preside over such a meeting as was suggested, but reserved his decision as to the raising of a Mansion House fund.

MR. SHIRLEY HIBBERD, the well-known horticulturist, died at his residence at Kew on Sunday. He was for many years editor of the *Gardener's Magazine*, and was the author of many works on horticulture. Mr. Hibberd was in his sixty-sixth year.

ON Saturday evening several shocks of earthquake were felt in the north-east of Scotland. The *Daily News* says that the first shock was experienced at 5.50 p.m. in Inverness, and that it lasted about 30 seconds. A good deal of damage was done to property by the falling of gables, chimneys, &c. The inhabitants were very much excited. Half an hour afterwards a second shock was experienced, but was not of so severe a character. About 6 o'clock a sharp shock of earthquake was felt at Forres. The disturbance was accompanied by a rumbling noise, with heaving and convulsion, which lasted from 15 to 20 seconds. It was felt over a radius of several miles. The *Times* says the earthquake was felt at Beauly, Inverness-shire, where many shops and houses were severely shaken, and furniture was thrown down. In Western Lovat a chimney-stack was knocked over. We learn from a correspondent at Nairn that "a slight vibration" was felt there and in the surrounding district "about 6 p.m."

AT the fourth annual general meeting of the Anthropological Society of Bombay, an address was delivered by the retiring President, Mr. Denzil Ibbetson. Speaking of the valuable contributions that might be made to anthropology by native inquirers, Mr. Ibbetson pointed out that sources of information are freely opened to them to which Englishmen can gain access only with difficulty. "It is through their agency alone," he said, "that we can hope to learn the rites and customs particular to women, rites and customs which I believe to possess a very special significance, as being in many cases handed down directly from the aboriginal women of the country, with whom the subsequent immigrants intermarried. And their facilities of communication with the masses are so infinitely greater than our own, that I look forward to the most valuable results so soon as we have a body of native gentlemen intelligently studying the anthropology of India. At present, in Upper India at any rate, a native who is sufficiently educated to understand the nature and object of our inquiries is too often hampered by his religious education, which causes him to describe the religion of the peasantry as it should be rather than as it is, and by his pride of caste, which prevents him from interesting himself in those whom he considers beneath his notice."

A PASSAGE in an appendix to Mr. Scott's last report on the administration of the Northern Shan States shows the extraordinary mixture of peoples and tongues in this region, and shows also what a task there is for the ethnologist of the future to unravel the tangled skein presented to him by the Shan State of Mainglin. The report says the population consists of Shans, Las, Was, Kachins, Shan-Taloks, Myen, and a tribe known as Mutso. The Las men dress like Shans, but their clothes are of black or dark-blue stuff. The women wear black coats and black *tameins*. The Was, both men and women, wear black coats and a striped black and white *lungi*. The Myen wear clothes like Shans, but black, and are said to shave the head, leaving only a pigtail like the Chinese. They are represented as speaking the same dialect as the Chinese Shans. The Mutso tribe is so called by the Shans from their being hunters. They, like the Las and Was, have a separate language, but in every tribe there are a good many men who understand Shan. The Myen tribe are said to be *nat*, or spirit, worshippers, but among the Las and Was there are a good many Buddhists.

IN the September number of the *Bulletin of the Italian Geographical Society*, Colonel C. Airaghi gives an account of an exploration in Dembelas, a region of the plateau of Northern Abyssinia, watered by the River Mareb. The memoir deals with the topography, geology, and natural history of the district, with the aid of diagrams. Considering the proximity to the equator, the climate is very mild, though the changes of temperature are often rapid. A general map, and the notes of his fellow-explorer, Captain St. Hidalgo, will appear in the next issue. The most important paper is Dr. Arthur Wolynski's on the population of the Caucasus. This is estimated at 6,171,400, of whom 1,217,400 are Mongols, mainly Tartars. Of the whites 1,854,000 belong to the native Caucasian races—these are classified carefully; 41,000 are Semites, mostly Jews; the remaining 3,059,000 are Aryans, almost all groups being represented. The Russians number nearly two millions, while Dr. Wolynski estimates the Armenians at 750,000, and the Persian group at 339,000.

THE Meteorological Sub-Committee of the Croydon Microscopical and Natural History Club is doing good work by collecting and publishing daily rainfall values at a number of stations in the counties of Kent and Surrey, together with brief general notes on the weather of each month. The Report for 1889 shows that the year began with a staff of thirty-eight observers, superintending forty-five stations.

IN the *Meteorologische Zeitschrift* for October, M. Nils Ekholm gives an account of a method on trial at the Meteorological Office of Stockholm, which seems likely to throw some light upon what has hitherto been a difficult matter to deal with, namely, the determination of the path taken by storms. He calculates, from the telegraphic weather reports, tables of the density of the atmosphere, and constructs from the data synoptic charts of this element, and finds that they give a better clue to the movements and origin of cyclones than the usual method of a comparison of the isobars and isotherms alone. He finds that storms move in the direction of the warmest and dampest air, parallel to the lines of equal density, leaving the rarer air to the right-hand. A few empirical rules are quoted from about a hundred cases which have been investigated.

MR. T. TUHLIN has recently published in the *Nova Acta* of the Royal Society of Sciences of Upsala, a paper on the nocturnal temperature of the air at different heights up to 24 feet, from hourly observations taken during the winters of 1887 and 1888, in the grounds of the Upsala Observatory. The observations were made mostly while snow lay upon the ground, both with thermometers with and without screens, and were intended to form a sequel to the series made by Mr. H. E. Hamberg during the summer season. The first part of the paper contains a *résumé* of the experiments made since 1778. The following are some of the chief results arrived at in the second part of the paper. The decrease of temperature by radiation from unprotected thermometers over snow remained almost constant at heights above half a metre. During clear nights the temperature increased with height, from two or three hours before sunset until two hours after sunrise, and the lower the temperature, the greater was the increase. During cloudy or foggy nights the temperatures at different heights were nearly equal; but if the clouds were high and thin, the increase of temperature with height was only slightly hindered. The surface of the snow was found to be colder than the surrounding air.

A CORRESPONDENT of *Die Natur* describes the following incident, which he himself observed. On the branch of a tree was a sparrow's nest, in which were some young sparrows, and not far off sat the mother. A male sparrow, coming along that way, was attracted by her, and began to make advances, which were steadily rejected. By and by he rested on a neighbouring branch, and the mother flew away in search of food for her young. No sooner had she departed than the disappointed suitor pounced down upon the nest, caught one of the young sparrows in his bill, went off with it a little way, and then dropped it, apparently rejoicing in its death.

PROF. F. W. PUTNAM lately brought under the notice of the Boston Society of Natural History some fresh evidence of the fact that man was in America contemporaneous with the mastodon and mammoth. This evidence is afforded by a rude figure unquestionably representing a mammoth, scratched on a portion of a Busycon shell found under peat in Clairmont County, Delaware. Around the shell were human bones, charcoal, bones of animals, and stone implements.

ALTHOUGH the adult crab-eater, cobia, ling, or coal-fish (*Elacate Canada*), as the species is variously designated, is well known, the young has escaped notice until recently. In August, 1887, Dr. T. H. Bean caught two specimens in Great Egg Harbour Bay, New Jersey; and these he has described in a Bulletin of the U.S. Fish Commission. Dr. A. K. Fisher, in some notes he has just contributed to the Proceedings of the U.S. National Museum, mentions that, in June 1876, he received a young fish of this species, measuring 95 mm. in length, from a fisherman who caught it in a minnow seine about a mile north of the village Sing Sing, New York, in the broad and shallow cove formed by the expansion of the Croton River as it enters the Hudson. Nothing could be learned of the habits of the young fish except that it was alone. This was true also of Dr. Bean's specimens; "so, presumably, the young must soon separate and lead a solitary life, as the adults are said to do." The subject is of some practical interest, because the crab-eater, in Dr. Fisher's opinion, is "entitled to prominence as a food-fish, not only on account of the delicate flavour of its flesh, but also for its suitable size."

Apropos of our notes on scientific guide-books for Switzerland, Messrs. Wesley and Son send us from their catalogue the titles of the following works:—"Tourist's Guide to the Flora of the Alps," by K. W. v. Dalla-Torre, translated by Bennett, 1886, 4*s*. 6*d*. "Tour of Mont Blanc and of Monte Rosa," by J. D. Forbes, 320 pp. text, with engravings and maps, Edinburgh, 1855, 6*s*. 6*d*. "Flore Analytique de la Suisse," by A. Gremli, Genève, 1885, 7*s*; also in English, 1889, 7*s*. 6*d*. "Flora der Schweiz," by J. Hegetschweiler, edited by Heer, with 8 plates, Zürich, 1840, 4*s*. "Sketches of Nature in the Alps," by F. v. Tschudi, 1856, 2*s*. 6*d*. (Contents: General Characteristics; Vegetable Life; Hunting of the Vulture, Chamois, Lynx, Fox, Wolf, Bear, Alpine Cattle, Glaciers, &c.). "The Tourist's Flora: a Descriptive Catalogue of the Flowering Plants and Ferns of the British Islands, France, Germany, Switzerland, Italy, and the Italian Islands," by J. Woods, with index, 586 pp. and plate, 1850, £2 15*s*.

A SECOND edition of Mr. Frederick E. Chapin's "Mountaineering in Colorado" (Sampson Low) has been published.

There are some most interesting illustrations in this book. With few exceptions, they have been made directly from negatives taken during the author's various expeditions. The reproductions are the work of the Boston Photogravure Company.

We have received from the publishers, Messrs. Macmillan and Co., a small book of "Illustrations and Diagrams" from the works of Dr. Geikie and others, that has been arranged by Mr. Cecil Carus-Wilson to illustrate, in the Oxford University Extension lectures, the geological courses delivered by him. In order to give elementary lectures on this subject to students, one must have a most liberal supply of diagrams, &c., and they must all be placed so that they may be conveniently seen. Again, when slides are used students have not time to copy them while on the screen, and in this way many important facts which could be remembered by the presence of a diagram are forgotten. By means of Mr. Carus-Wilson's hand-book these disadvantages will disappear, and the student will have good and trustworthy illustrations from which he will be able to draw more comprehensive and accurate conclusions than he could from his own rough sketches made at the time. The diagrams are printed on excellent paper, and at the foot of each is a short description, with the reference to the work from which it has been taken. The book is sold at cost price.

The Indian Press, Allahabad, has published a very good series of geographical text-books for Indian schools, by the late Prof. S. A. Hill, of Muir Central College, Allahabad. The series consists of three little volumes, the first two of which have reached a third edition. The third volume, which is new, treats chiefly of mathematical and physical geography.

The fifth edition of Prof. M. Foster's "Text-book of Physiology" (Macmillan) is being published. Part iii., which has just been issued, deals with the central nervous system. The work has been largely revised.

Messrs. Longmans and Co. are issuing the tenth edition of Quain's "Elements of Anatomy." The task of editing the work has been entrusted to Prof. E. A. Schäfer and Prof. G. D. Thane. We have received the first part of vol. i., and the first part of vol. ii. The former deals with embryology, and is edited by Prof. Schäfer; the latter with osteology, Prof. Thane being the editor.

The second edition of "The Fuel of the Sun," by W. Mattieu Williams, has been issued. As the work was originally published twenty years ago, the author contributes a preface to the new edition, giving "a brief outline summary of the bearings of the growth of knowledge upon the subjects of the several chapters."

Messrs. Baillière, Tindall, and Cox will publish in a few days an octavo volume (360 pages and 52 figures) entitled, "Researches on Micro-Organisms," by Dr. A. B. Griffiths. The work gives an account of recent researches in various branches of bacteriology.

The Naturalists' Publishing Company, Birmingham, have issued "The Naturalists' Annual and Directory for 1891," edited by the editor of the *Naturalists' Gazette*. It consists of a number of short scientific articles, by various writers, and a directory indicating "the Lepidoptera of the months."

The University College of Nottingham has published the Calendar for its tenth session. In a supplement the facts relating to the engineering department of the College are brought together.

Benzoyl fluoride, C_6H_5COF, has been prepared for the first time by M. Guenez in the laboratory of M. Moissan at the Parisian École de Pharmacie. It was obtained by the general reaction lately proposed by M. Moissan, heating silver fluoride with the corresponding chloride in a sealed tube. Equal molecular proportions of silver fluoride, AgF, and benzoyl chloride, C_6H_5COCl, were heated together for six hours in a sealed tube to a temperature of 190°. After allowing the tube and contents to cool, the drawn out sealed end was opened at the blow-pipe in order to permit of the escape of gaseous silicon tetrafluoride, which is formed in considerable quantity owing to the energetic action of benzoyl fluoride upon glass. The tube was then drawn out about the middle of its length, and bent over in a V shape; the benzoyl fluoride was thus readily distilled off from the residual silver chloride, the second limb of the V acting as a condensing tube. The product so obtained was found, as might be expected, to contain admixed benzoyl chloride. It was therefore reheated in a second sealed tube with a fresh quantity of fluoride of silver, and the product distilled in the same manner as before. The resulting benzoyl fluoride was found to be practically free from chloride. Benzoyl fluoride is a colourless liquid possessing an odour analogous to that of benzoyl chloride, but much more irritating, the least trace of its vapour producing a copious flow of tears. It boils at 145°, and readily ignites when heated in the air, burning with a flame bordered by a blue halo. It sinks in water, which liquid slowly decomposes it in the cold, with formation of hydrofluoric and benzoic acids—

$$C_6H_5COF + H_2O = C_6H_5COOH + HF.$$

In contact with solutions of caustic alkalies it is rapidly converted into fluoride and benzoate of the alkali, the reaction being almost instantaneous when the temperature is slightly raised. It attacks glass very vigorously, with liberation of gaseous silicon tetrafluoride; benzoic anhydride, containing a deposit of potassium fluoride, is found remaining in the corroded vessel.

$$6C_6H_5COF + K_2SiO_3 = SiF_4 + 2KF + 3(C_6H_5CO)_2O.$$

Benzoyl fluoride appears, therefore, to fulfil the expectations concerning it in resembling very closely its nearest analogue, benzoyl chloride, in properties, the differences being only those due to the more active nature of the halogen fluorine, and to the remarkable affinity of the latter element for silicon.

The additions to the Zoological Society's Gardens during the past week include an Indian Chevrotain (*Tragulus meminna* ♂) from India, presented by Mr. Greenberg; a Globose Curassow (*Crax globicera* ♂) from Mexico, presented by Mr. R. M. Pryor, F.Z.S.; two Long-eared Owls (*Asio otus*), British, presented by Mrs. Twickline; an Eyed Lizard (*Lacerta ocellata*, var.) from Southern Spain, presented by Mr. Francis Napier; a Black-headed Gull (*Larus ridibundus*), British, presented by Miss Lanze; an Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. C. J. Owen; a Cryptoprocta (*Cryptoprocta ferox*) from Madagascar, purchased; two Crested Porcupines (*Hystrix cristata*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Duplicity of α Lyræ.—At the meeting of the Royal Astronomical Society on November 14, Mr. A. Fowler exhibited some photographs of the spectrum of α Lyræ which indicate that it is a spectroscopic double of the β Aurigæ and ζ Ursæ Majoris type. The photographs were taken with the 10-inch refractor belonging to the Royal College of Science, with two prisms of $7\frac{1}{2}°$ each in front of the object-glass, and form part of a photographic study of stellar spectra recently commenced at Kensington by Prof. Lockyer with a special object. The evidence of duplicity of this kind of binary star depends upon the fact that when the two components are moving in opposite directions in the line of sight, the lines that are common in their spectra are displaced towards opposite ends of the spectrum, in accordance with Doppler's principle, and therefore appear double. When the motion is at right angles to the line of sight, there is, of course, no such displacement, and the lines therefore appear single. Hence, during a complete revolution the lines will twice reach a maximum separation and twice appear single. The principal lines in the spectrum of α Lyræ are due to hydrogen. These do not exhibit a duplication, because the separation is less than their thickness. A variation in their width, however, is very obvious. The K line of calcium is the next strongest, and is sufficiently fine and distinct to render the duplicity very apparent. Fourteen photographs of the spectrum have been taken from October 3 to November 4. The maximum separation of the K line was recorded on October 8 as 7·8 tenth-metres. On October 17, 28, and November 1, 8 p.m., the same line appeared single. At 8.30 p.m. and 10 p.m. on the last-named date, the separation was respectively 2·3 and 3·8 tenth-metres. A discussion of the data obtained from all the photographs shows that they are fairly satisfied by assuming a circular orbit, the plane of which passes through the sun, and the remarkably short period of revolution of about 24·68 hours. This period does not appear inconsistent with the relative orbital velocity of 370 miles per second indicated by the photograph taken on October 8, and is confirmed by the three photographs taken at short intervals on November 1. If 370 miles per second be taken as the maximum relative orbital velocity, the distance between the components is about 5,000,000 miles. The total mass will therefore be about 22·5 times that of the sun, and as there is no appreciable difference in the intensity of the K lines, the masses of the components are probably about equal. In the cases of β Aurigæ and ζ Ursæ Majoris, Prof. Pickering found, respectively, periods of 4 and 52 days, and maximum orbital velocities of about 150 and 100 miles per second.

Parallaxes of Nebulæ.—Prof. Holden proposes to determine the parallaxes of nebulæ by taking short exposure photographs of their nuclei, and measuring their positions relative to neighbouring stars, in a similar manner to the method adopted by Prof. Pritchard for the photographic determination of the parallaxes of stars. A short exposure photograph taken at Lick Observatory, of Σ 6 and the stars near it, indicates that the method is one from which good results may be obtained.

Catania Observatory.—Prof. Riccò has been appointed Director of the new Catania Observatory. The work of the Observatory will be principally connected with astronomical physics, celestial photography, meteorology, and seismology.

Washington Observations, Appendix II.—This consists of an account of the work done by Mr. Asaph Hall, on Saturn and its ring, extending over the years 1875–89. Besides giving details of every observation made by him, he sums up afterwards the results obtained, from which we gather the following information. From a series of observations of the white spot on the equator of the planet, he finds the time of rotation of Saturn to be 10h. 14m. 23·8s. ± 2·30s. mean time: this differs slightly from that obtained by Sir William Herschel, who gave 10h. 16m. 0·4s., and said that his result could not be in error by so much as two minutes. The ball of the planet during the fourteen years of observation has undergone very slight changes, with the exception of the white spot which broke out on December 7, 1876. Although a careful examination was made to find if a notch could be seen in the outline of the shadow of the ball on the ring, no such phenomenon was recorded. Of the principal rings, no division was recorded in the inner ring. The Cassini division gave the impression "of not being a complete separation, or that small particles of matter remain in this partly dark space." The Encke division of the outer ring, although specially looked for could not be definitely stated as a "real and permanent division," although a slight marking was seen at times.

In addition to numerous tables of measurements of the ball and of the various rings, the author adds three plates of the planet as it appeared under favourable conditions in the opposition of 1884.

BIOLOGICAL NOTES.

Rate of Growth in Corals.—But little is as yet known as to the rate of growth of corals under different conditions. In the third edition of Dana's "Coral and Coral Islands," a *résumé* will be found of all that is known about this subject up to 1890, but some very interesting details have been published by Alexander Agassiz in the August number of the *Bulletin of the Museum of Comparative Zoology at Harvard College*. A series of specimens are figured, which have been taken off the telegraph cable laid between Havana and Key West, in June 1888, from a portion that was repaired in the summer of 1881; so that the growths could not be more than about seven years. It is to be observed that this portion of the cable was laid at a depth of only from six to seven fathoms, and that the district in which it was laid was most favourably situated as regards food supply to the corals. Some of the specimens belong to species whose rates of growth have not yet been recorded; they are as follows:—*Orbicella annularis*, figured on plates 1 and 2. Verrill mentions that the thickness of this coral formed in sixty-four years was not more than about 8 inches; the specimens from the Havana cable grew to a thickness of 2½ inches in about seven years. *Manicina areolata*, Ehrenb., figured on plate 3, has grown to a thickness of 1 inch; while *Isophylla dipsacea*, Ag., figured on plate 4, shows a still more rapid growth, projecting 2½ inches above the cable. Of course it is quite possible that these corals are of less than seven years' growth, but it is not probable that more than a short time passed before some of the swarms of pelagic coral embryos which must have floated past the cable found a place of attachment thereon.

Tuomeya fluviatilis, Harvey.—Over three-and-thirty years ago the late Prof. W. H. Harvey bestowed this name on a rare and curious freshwater Alga sent to him from the United States by Prof. Tuomey, of Alabama. Harvey's description was published in the third part of his "Nereis Boreali Americana," but Harvey, as was his wont, sent a scrap of his plant to Kuetzing, who at once described and figured it as *Baileya america*. Prof. Farlow gives the priority to Harvey's name, and we believe he is right. The systematic position of this little Alga is a matter of some importance. Harvey says, "The plants referred to Batrachospermeæ naturally group themselves into two sub-orders, distinguished from each other by the habit of the frond, but closely related in structure and fructification, and these seem to me inseparably connected by the genus Tuomeya, which unites in itself the characters of the seemingly so dissimilar genera Batrachospermum and Lemanea;" but the specimens at his command were too imperfect to enable the complete structure to be made out, and until the other day no light was thrown upon the subject. In December 1888, Mr. Holden found it in some quantity in a brook near Bridgeport, Conn., and since then it has turned up in several distant localities in the United States. Mr. W. A. Setchell has lately published the results of a very detailed investigation of the structure and development of this form, as the result of work done in the Cryptogamic Laboratory of Harvard University. It grows on smooth rocks or stones, in brooks or small streams, preferring places where the current is accelerated; in a few rare cases it has occurred upon aquatic grasses; it seems to be easily cultivated, one batch of specimens, even under such treatment, producing the reproductive organs; the plant seldom exceeds 5 cm. in height, and is bushy and rigid, in this latter respect presenting a marked contrast to the larger species of Batrachospermum, which in size, colour, and manner of growth it much resembles, and with which it very frequently grows. When dried, it is non-adherent to paper, resembling in this some of the Lemanea. Each main filament is composed of a single row of cells placed end to end, the apical cell being in a state of active growth, here and there at intervals the older cells of the filament branch out into a di- or trichotomously branched ramellus, of which several may arise from the same node, and the branchlets from these, intertwining as they increase, form a dense mass of cells round the central filamentous axis, and finally make up a hollow cylindrical frond composed of two sets of cells, somewhat resembling the structure seen in some forms of Lemanea. Before, however, this hollow cylinder is fully formed, filaments are given off from the basal portions of the inner cells of the ramelli, which differ from the ramelli cells in being cylindrical and simple. These grow downwards, forming a dense cortical layer around the axis; some, however, grow obliquely outwards, even protruding themselves beyond the outer limits of the frond, and may possibly become detached, forming new plants. Both the antheridia and procarps are to be found on the same plant, but generally on separate portions of the frond; the antheridia in shape, colour, and character of contents exactly agree with those of Batrachospermum; they are borne at the tips of branches which arise from or near the nodes. These mostly pass out horizontally to the surface of the frond. The antheridial branches are at first unbranched; their ends just beneath the outer surface of the frond are two or three times branched in a corymbosely dichotomous fashion; the antheridia are apical, spheroidal; the antherozoids are solitary; for a short time after escape they are of an irregular shape, exhibit slight amœboid motion, but soon become globular and motionless. The female organs are borne upon especially modified procarpic branches; in the axil of one of the ramelli, arise one, sometimes more short branches strikingly different from any of the other outgrowths, made up of broad, stout cells. As these increase in length they generally become spirally twisted, and the cells on their convex sides develop short branches. The terminal cell of the branch produces the procarp, which in Tuomeya consists of a broadly ovoid trichophore, surmounted by a trichogyne several times longer than itself. After fertilization the trichophore begins to bud forth cells, which gradually become arranged in more or less concentric rows upon the surface of the trichophore; the further development was not seen, but it is surmised that strings of spores are then formed, as in Batrachospermum. Unfortunately the germination of the spores has not been witnessed, nor have very young plants yet been seen. Some further investigations as to the structure of cystocarps is also desirable, but we know that Mr. Setchell is continuing his researches, and we trust these will not end until the whole life-history of this very interesting fresh-water Alga is thoroughly understood. (Proceedings of the American Academy of Arts and Sciences, vol. xxv. p. 53, with a plate.)

Marine Algæ of Berwick-on-Tweed.—Mr. Edward A. L. Batters has recently published, from the Transactions of the Berwickshire Naturalists' Club for 1889, a most useful list of the marine Algæ of Berwick-on-Tweed. Many years have elapsed since the appearance of Dr. Johnston's list, and very numerous have been the alterations in nomenclature since those days. This new list is therefore all the more welcome, as it comes as a contribution towards a revision of the British Algæ, which the great advance in our knowledge that has been made since the publication of Harvey's splendid "Phycologia Brittanica" renders absolutely necessary. Mr. Batters's list contains 271 species. Comparing this with a few local lists conveniently at hand, we find in Le Joli's well-known "Cherbourg Algal Flora" 350 species; Debray, "Catalogue of the Algæ found at Dunkirk," gives 189 species; while Flahault's interesting account of his collecting for some six weeks at Croisic details 230 forms. Of those enumerated in the Berwick list no less than 78 species have been added to the British flora since the publication of Harvey's work. One minute form found presenting the appearance of tiny yellow-brown patches has been made by Reinke a new genus, Battersia; it belongs to the Sphacelariaceæ, and has doubtless been often overlooked. From a small encrusting thallus little patches of fruit bearing filaments arise. Several of the Algæ lately described by Bornet and Flahault as perforating species, chiefly in the substance of marine shells, such as *Gomontia polyrhiza* and *Mastigocoleus testarum*, are recorded. There is an index of genera and also figures of most of the new species.

DR. KOCH ON TUBERCULOSIS.

A SUPPLEMENT to the *British Medical Journal* contains "A Further Communication on a Remedy for Tuberculosis," translated from the original article published in the *Deutsche Medizinische Wochenschrift*, November 14, by Prof. Dr. Robert Koch, Berlin, as follows:—

Introduction.

In an address delivered before the International Medical Congress I mentioned a remedy which conferred on the animals experimented on an immunity against inoculation with the tubercle bacillus, and which arrests tuberculous disease. Investigations have now been carried out on human patients, and these form the subject of the following observations.

It was originally my intention to complete the research, and especially to gain sufficient experience regarding the application of the remedy in practice and its production on a large scale, before publishing anything on the subject. But, in spite of all precautions, too many accounts have reached the public, and that in an exaggerated and distorted form, so that it seems imperative, in order to prevent all false impressions, to give at once a review of the position of the subject at the present stage of the inquiry. It is true that this review can, under these circumstances, be only brief, and must leave open many important questions.

The investigations have been carried on under my direction by Dr. A. Libbertz and Stabsarzt Dr. E. Pfühl, and are still in progress. Patients were placed at my disposal by Prof. Brieger from his Poliklinik, Dr. W. Levy from his private surgical clinic, Geheimrath Dr. Fräntzel and Oberstabsarzt Kohler from the Charité Hospital, and Geheimrath v. Bergmann from the Surgical Clinic of the University.[1]

Nature and Physical Characters of the Remedy.

As regards the origin and the preparation of the remedy I am unable to make any statement, as my research is not yet concluded: I reserve this for a future communication.[2] The remedy is a brownish transparent liquid, which does not require special care to prevent decomposition. For use, this fluid must be more or less diluted, and the dilutions are liable to decomposition if prepared with distilled water; bacterial growths soon develop in them, they become turbid, and are then unfit for use. To prevent this the diluted liquid must be sterilized by heat and preserved under a cotton-wool stopper; or more conveniently prepared with a ½ per cent. solution of phenol.

Manner of using the Remedy.

It would seem, however, that the effect is weakened both by frequent heating and by mixture with phenol solution, and I have therefore always made use of freshly-prepared solutions. Introduced into the stomach, the remedy has no effect; in order to obtain a trustworthy effect, it must be injected subcutaneously. For this purpose we have used exclusively the small syringe suggested by me for bacteriological work; it is furnished with a small india-rubber ball, and has no piston. This syringe can easily be kept aseptic by absolute alcohol, and to this we attribute the fact that not a single abscess has been observed in the course of more than a thousand subcutaneous injections. The place chosen for the injection—after several trials of other places—was the skin of the back between the shoulder-blades and the lumbar region, because here the injection led to the least local reaction—generally none at all—and was almost painless.

Effect of Injections in Healthy Individuals.

As regards the effect of the remedy on the human patient, it was clear from the beginning of the research that in one very important point the human being reacts to the remedy differently from the animal generally used in experiments—the guinea-pig; a new proof for the experimenter of the all-important law that experiment on animals is not conclusive for the human being, for the human patient proved extraordinarily more sensitive than the guinea-pig as regards the effect of the remedy. A healthy guinea-pig will bear two cubic centimetres and even more of the liquid injected subcutaneously without being sensibly affected. But in the case of a full-grown healthy man 0·25 cubic centimetres suffice to produce an intense effect. Calculated by body weight, the 1500th part of the quantity, which has no appreciable effect on the guinea-pig, acts powerfully on the human being. The symptoms arising from an injection of 0·25 cubic centimetre I have observed after an injection made in my own upper arm. They were briefly as follows:—Three to four hours after the injection there came on pains in the limbs, fatigue, inclination to cough, difficulty in breathing, which speedily increased. In the fifth hour an unusually violent attack of ague followed, which lasted almost an hour. At the same time there was sickness, vomiting, and rise of body temperature up to 39°·6 C. After twelve hours all these symptoms abated. The temperature fell, until next day it was normal, and a feeling of fatigue and pain in the limbs continued for a few days, and for exactly the same period of time the site of injection remained slightly painful and red. The lowest limit of the effect of the remedy for a healthy human being is about 0·01 cubic centimetre (equal to 1 cubic centimetre of the hundredth solution), as has been proved by numerous experiments. When this dose was used, reaction in most people showed itself only by slight pains in the limbs and transient fatigue. A few showed a slight rise of temperature up to about 38° C. Although the dosage of the

[1] Dr. Koch here expressed his thanks to these gentlemen.

[2] Doctors wishing to make investigations with the remedy at present can obtain it from Dr. A. Libbertz, Lueneburger Strasse 28, Berlin, N.W., who has undertaken the preparation of the remedy, with my own and Dr. Pfühl's co-operation. But I must remark that the quantity prepared at present is but small, and that larger quantities will not be obtainable for some weeks.

remedy shows a great difference between animals and human beings—calculated by body weight—in some other qualities there is much similarity between them. The most important of these qualities is the specific action of the remedy on tuberculous processes of whatever kind.

The Specific Action on Tuberculous Processes.

I will not here describe this action as regards animals used for experiment, but I will at once turn to its extraordinary action on tuberculous human beings. The healthy human being reacts either not at all or scarcely at all—as we have seen when 0·01 cubic centimetre is used. The same holds good with regard to patients suffering from diseases other than tuberculosis, as repeated experiments have proved. But the case is very different when the disease is tuberculosis: the same dose of 0·01 cubic centimetre injected subcutaneously into the tuberculous patient caused a severe general reaction, as well as a local one. (I gave children, aged from 2 to 5 years, one-tenth of this dose—that is to say, 0·001 cubic centimetre; very delicate children, only 0·0005 cubic centimetre, and obtained a powerful but in no way dangerous reaction.) The general reaction consists in an attack of fever, which, generally beginning with rigors, raises the temperature above 39°, often up to 40°, and even 41° C.; this is accompanied by pain in the limbs, coughing, great fatigue, often sickness and vomiting. In several cases a slight icteric discolouration was observed, and occasionally an eruption like measles on the chest and neck. The attack usually begins four to five hours after the injection, and lasts from twelve to fifteen hours. Occasionally it begins later, and then runs its course with less intensity. The patients are very little affected by the attack, and as soon as it is over feel comparatively well, generally better than before it. The local reaction can be best observed in cases where the tuberculous affection is visible; for instance, in cases of lupus: here changes take place which show the specific anti-tuberculous action of the remedy to a most surprising degree. A few hours after an injection into the skin of the back—that is, in a spot far removed from the diseased spots on the face, &c.—the lupus spots begin to swell and to redden, and this they generally do before the initial rigor. During the fever, swelling and redness increase, and may finally reach a high degree, so that the lupus tissue becomes brownish and necrotic in places. Where the lupus was sharply defined we sometimes found a much swollen and brownish spot surrounded by a whitish edge almost a centimetre wide, which again was surrounded by a broad band of bright red.

After the subsidence of the fever the swelling of the lupus tissue decreases gradually, and disappears in about two or three days. The lupus spots themselves are then covered by a crust of serum, which filters outwards, and dries in the air; they change to crusts, which fall off after two or three weeks, and which, sometimes after one injection only, leave a clean red cicatrix behind. Generally, however, several injections are required for the complete removal of the lupus tissue. But of this more later on. I must mention, as a point of special importance, that the changes described are exactly confined to the parts of the skin affected with lupus. Even the smallest nodules, and those most deeply hidden in the lupus tissue, go through the process, and become visible in consequence of the swelling and change of colour, whilst the tissue itself, in which the lupus changes have entirely ceased, remains unchanged. The observation of a lupus case treated by the remedy is so instructive, and is necessarily so convincing, that those who wish to make a trial of the remedy should, if at all possible, begin with a case of lupus.

The Local and General Reaction to the Remedy.

The specific action of the remedy in these cases is less striking, but is perceptible to eye and touch, as are the local reactions in cases of tuberculosis of the glands, bones, joints, &c. In these cases swelling, increased sensibility, and redness of the superficial parts are observed. The reaction of the internal organs, especially of the lungs, is not at once apparent, unless the increased cough and expectoration of consumptive patients after the first injections be considered as pointing to a local reaction. In these cases the general reaction is dominant; nevertheless, we are justified in assuming that here, too, changes take place similar to those seen in lupus cases.

The Diagnostic Value of the Method.

The symptoms of reaction above described occurred without exception in all cases where a tuberculous process was present in the organism, after a dose of 0·01 cubic centimetre, and I think I am justified in saying that the remedy will therefore, in future, form an indispensable aid to diagnosis. By its aid we shall be able to diagnose doubtful cases of phthisis; for instance, cases in which it is impossible to obtain certainty as to the nature of the disease by the discovery of bacilli, or elastic fibres, in the sputum, or by physical examination. Affections of the glands, latent tuberculosis of bone, doubtful cases of tuberculosis of the skin, and such like cases, will be easily and with certainty recognized. In cases of tuberculosis of the lungs or joints which have become apparently cured we shall be able to make sure whether the disease has really finished its course, and whether there be not still some diseased spots from which it might again arise as a flame from a spark hidden by ashes.

The Curative Effect of the Remedy.

Of much greater importance, however, than its diagnostic use is the therapeutic effect of the remedy. In the description of the changes which a subcutaneous injection of the remedy produces in portions of skin changed by lupus I mentioned that after the subsidence of the swelling and decrease of redness the lupus tissue does not return to its original condition, but that it is destroyed to a greater or less extent, and disappears. Observation shows that in some parts this result is brought about by the diseased tissue becoming necrotic, even after one sufficient injection, and, at a later stage, it is thrown off as a dead mass. In other parts a disappearance, or, as it were, a melting of the tissues seems to occur, and in such case the injection must be repeated to complete the cure.

Its Action on Tuberculous Tissue.

In what way this process occurs cannot as yet be said with certainty, as the necessary histological investigations are not complete. But so much is certain, that there is no question of a destruction of the tubercle bacilli in the tissues, but only that the tissue enclosing the tubercle bacilli is affected by the remedy. Beyond this there is, as is shown by the visible swelling and redness, considerable disturbance of the circulation, and, evidently in connection therewith, deeply-seated changes in its nutrition, which cause the tissue to die off more or less quickly and deeply, according to the extent of the action of the remedy.

To recapitulate, the remedy does not kill the tubercle bacilli, but the tuberculous tissue; and this gives us clearly and definitely the limit that bounds the action of the remedy. It can only influence living tuberculous tissue; it has no effect on dead tissue, as, for instance, necrotic cheesy masses, necrotic bones, &c., nor has it any effect on tissue made necrotic by the remedy itself. In such masses of dead tissue living tubercle bacilli may possibly still be present, and are either thrown off with the necrosed tissue or may possibly enter the neighbouring still living tissue under certain circumstances. If the therapeutic activity of the remedy is to be rendered as fruitful as possible this peculiarity in its mode of action must be carefully observed. In the first instance, the living tuberculous tissue must be caused to undergo necrosis, and then everything must be done to remove the dead tissue as soon as possible, as, for instance, by surgical interference. Where this is not possible and the organism can only help itself in throwing off the tissue slowly, the endangered living tissue must be protected from fresh incursions of the parasites by continuous application of the remedy.

The Dose.

The fact that the remedy makes tuberculous tissue necrotic, and acts only on living tissue, helps to explain another peculiar characteristic thereof—namely, that it can be given in rapidly increasing doses. At first sight this phenomenon would seem to point to the establishment of tolerance, but since it is found that the dose can, in the course of about three weeks, be increased to 500 times the original amount, tolerance can no longer be accepted as an explanation, as we know of nothing analogous to such a rapid and complete adaptation to an extremely active remedy. The phenomenon must rather be explained in this way—that in the beginning of the treatment there is a good deal of tuberculous living tissue, and that consequently a small amount of the active principle suffices to cause a strong reaction; but by each injection a certain amount of the tissue capable of reaction disappears, and then comparatively larger doses are necessary to produce the same amount of reaction as before. Within certain limits a certain degree of habituation may be perceived.

As soon as the tuberculous patient has been treated with increasing doses for so long that the point is reached when his reaction is as feeble as that of a non-tuberculous patient, then it may be assumed that all tuberculous tissue is destroyed. And then the treatment will only have to be continued by slowly increasing doses and with interruptions, in order that the patient may be protected from fresh infection while bacilli are still present in the organism.

Whether this conception, and the inferences that follow from it, be correct, the future must show. They were conclusive as far as I am concerned in determining the mode of treatment by the remedy, which, in our investigations, took the following form.

The Treatment Applied to Lupus.

To begin with the simplest case, lupus; in nearly every one of these cases I injected the full dose of 0·01 cubic centimetre from the first. I then allowed the reaction to come to an end entirely, and then, after a week or two, again injected 0·01 cubic centimetre, continuing in the same way until the reaction became weaker and weaker, and then ceased. In two cases of facial lupus the lupus spots were thus brought to complete cicatrization by three or four injections; the other lupus cases improved in proportion to the duration of treatment. All these patients had been sufferers for many years, having been previously treated unsuccessfully by various therapeutic methods.

The Treatment Applied to Tuberculosis of the Bones and Joints.

Glandular, bone, and joint tuberculosis was similarly treated, large doses at long intervals being made use of; the result was the same as in the lupus cases—a speedy cure in recent and slight cases, slow improvement in severe cases.

The Treatment applied to Phthisis.

Circumstances were somewhat different in phthisical patients, who constituted the largest number of our patients. Patients with decided pulmonary tuberculosis are much more sensitive to the remedy than those with surgical tuberculous affections. We were obliged to lower the dose for the phthisical patients, and found that they almost all reacted strongly to 0·002 cubic centimetre, and even to 0·001 cubic centimetre. From this first small dose it became possible to rise more or less quickly to the same amount as is well borne by other patients.

Our course was generally as follows:—An injection of 0·001 cubic centimetre was first given to the phthisical patient; on this a rise of temperature followed, the same dose being repeated once a day, until no reaction could be observed. We then rose to 0·02 cubic centimetre, until this was borne without reaction; and so on, rising by 0·001, or at most 0·002, to 0·01 cubic centimetre and more. This mild course seemed to me imperative in cases where there was great debility. By this mode of treatment the patient can be brought to bear large doses of the remedy with scarcely a rise of temperature. The patients of greater strength were treated from the first, partly with larger doses, partly with rapidly repeated doses. Here it seemed that the beneficial results were more quickly obtained.

The action of the remedy in cases of phthisis generally showed itself as follows:—Cough and expectoration generally increased a little after the first injection, then grew less and less, and in the most favourable cases entirely disappeared; the expectoration also lost its purulent character, and became mucous.

As a rule the number of bacilli only decreased when the expectoration began to present a mucous appearance; they then from time to time disappeared entirely, but were again observed occasionally until expectoration ceased completely. Simultaneously the night sweats ceased, the patients' appearance improved, and they increased in weight. Within four to six weeks patients under treatment for the first stage of phthisis were all free from every symptom of disease, and might be pronounced cured. Patients with cavities, not yet too highly developed, improved considerably, and were almost cured; only in those whose lungs contained many large cavities could no improvement be proved objectively, though even in these cases the expectoration decreased, and the subjective condition improved. These experiences lead me to suppose that phthisis in the beginning can be cured with certainty by this remedy.[1]

[1] This sentence requires limitation in so far as at present no conclusive experiences can possibly be brought forward to prove whether the cure is lasting. Relapses naturally may occur; but it can be assumed that they may be cured as easily and quickly as the first attack. On the other hand, it seems possible that, as in other infectious diseases, patients once cured may retain their immunity. This, too, must, for the present, remain an open question.

Effect in Advanced Cases of Phthisis.

In part this may be assumed for other cases when not too far advanced; but patients with large cavities, who almost all suffer from complications caused, for instance, by the incursion of other pus-forming micro-organisms into the cavities, or by incurable pathological changes in other organs, will probably only obtain lasting benefit from the remedy in exceptional cases. Even such patients, however, were benefited for a time. This seems to prove that, in their cases, too, the original tuberculous disease is influenced by the remedy in the same manner as in the other cases, but that we are unable to remove the necrotic masses of tissue with the secondary suppuration processes.

The thought suggests itself involuntarily that relief might possibly be brought to many of these severely afflicted patients by a combination of this new therapeutic method with surgical operations (such as the operation for empyema), or with other curative methods. And here I would earnestly warn people against a conventional and indiscriminate application of the remedy in all cases of tuberculosis. The treatment will probably be quite simple in cases where the beginning of phthisis and simple surgical cases are concerned; but in all other forms of tuberculosis medical art must have full sway by careful individualization, and making use of all other auxiliary methods to assist the action of the remedy. In many cases I had the decided impression that the careful nursing bestowed on the patient had a considerable influence on the result of the treatment, and I am in favour of applying the remedy in proper sanatoria as opposed to treatment at home and in the out-patient room. How far the methods of treatment already recognized as curative—such as mountain climate, fresh-air treatment, special diet, &c.—may be profitably combined with the new treatment cannot yet be definitely stated, but I believe that these therapeutic methods will also be highly advantageous when combined with the new treatment in many cases, especially in the convalescent stage.[1] The most important point to be observed in the new treatment is its early application. The proper subjects for treatment are patients in the initial stage of phthisis, for in them the curative action can be most fully shown, and for this reason, too, it cannot be too seriously pointed out that practitioners must in future be more than ever alive to the importance of diagnosing phthisis in as early a stage as possible. Up to the present the proof of tubercle bacilli in the sputum was considered more as an interesting point of secondary importance, which, though it may render diagnosis more certain, could not help the patient in any way, and which, in consequence, was often neglected. This I have lately repeatedly had occasion to observe in numerous cases of phthisis which had generally gone through the hands of several doctors without any examination of the sputum having been made. In future this must be changed. A doctor who shall neglect to diagnose phthisis in its earlier stage by all methods at his command, especially by examining the sputum, will be guilty of the most serious neglect of his patient, whose life may depend on this diagnosis, and the specific treatment at once applied in consequence thereof. In doubtful cases medical practitioners must make sure of the presence or absence of tuberculosis, and then only the new therapeutic method will become a blessing to suffering humanity, when all cases of tuberculosis are treated in their earliest stage, and we no longer meet with neglected serious cases forming an inextinguishable source of fresh infections. Finally, I would remark, that I have purposely omitted statistical accounts and descriptions of individual cases, because the medical men who furnished us with patients for our investigations have themselves decided to publish the description of their cases, and I wish my account to be as objective as possible, leaving to them all that is purely personal.

ON THE INCUBATION OF SNAKES' EGGS.[2]

Most Reptilia are oviparous, but certain of the Lacertilians, and many Ophidians, especially vipers and sea-snakes, are ovo-viviparous—that is to say, the eggs are hatched within the mother, or, as sometimes occurs, during the process of parturition. This is the case with the English viper.

[1] As regards tuberculosis of brain, larynx, and miliary tuberculosis, we had too little material at our disposal to gain proper experience.

[2] By Walter K. Sibley, M.B., B.C., B.A. Camb., Assistant Physician to the North-West London Hospital. (Substance of a paper read before the Biological Section of the British Association at Leeds, September 1890.)

There has not been much written on the hatching of snakes' eggs. Almost the first literature of the subject appears to be some observations of M. Valenciennes made in 1841, at the Jardin des Plantes in Paris, and published in the *Comptes rendus*, on a Python (*Python bivittatus*) which was about 10 feet long. This reptile, in the beginning of May, deposited fifteen eggs: it coiled itself up over them for fifty-six days, after which period eight of the eggs hatched, and the young snakes came out, each measuring about half a metre in length. With regard to the temperature of the mother, he says, there was a marked increase of temperature during the whole period of incubation, which was highest at the commencement, and gradually diminished till the close.

In 1862, Sclater, in the Proceedings of the Zoological Society, described a Python (*Python sebæ*) which incubated her eggs, one hundred in number, for eighty-two days, at which period they were removed, none having hatched, and on examination it was found that five or six contained embryos. With regard to the temperature, he states that, in every observation, the female was several degrees warmer than the male; both being kept under similar conditions. Double observations were always made, one with the thermometer placed on the surface of the body, and the other by placing it between the folds. The differences in the temperature of the snake's body and of the surrounding air was higher by from 2°·8 to 12°·4 F., taken on the surface of the animal, and from 6°·8 to 20°·0 F., taken between the folds of the skin.

In the same year Colonel Abbott recorded that he had in his possession in India a female boa, which incubated her eggs, forty-eight in number, for three months, at the end of which period, on opening one of the shells, a live fully-formed young one was found.

In 1880, Forbes carried on some investigations with the eggs of *Python molurus* in the Zoological Society's Gardens. The snake was some 12 feet long, and on the night of June 5-6, deposited about twenty eggs, and then coiled herself around them, almost completely concealing them from view. She continued to cover the eggs for a period of six weeks, never eating during the whole time, and apparently only once leaving the eggs for a few hours one morning early in July.

On July 18—that is, forty-three days after—the eggs revealing evidences of decomposition they were removed, and one or two were found to contain embryos.

From June 14—that is, nine days after the eggs were laid—till they were removed on July 18, careful temperatures were taken every two or three days between the hours of 12 and 2 p.m. The temperature was taken both of the incubating female and also of the male, which was kept under similar circumstances in the next cage, the same methods being employed as in Sclater's experiments just described. The temperature was always found to be highest when that of the air in the cages was also highest. The temperature of the female was higher and more constant than that of the male. The greatest difference between that of the snake and that of the air was 8°·3 F. for the male, and 9°·6 F. for the female, taken on the surface; and 11°·6 F. for the male, and 16°·7 F. for the female, taken between the coils of the skin. It will be noticed that Valenciennes, who kept his snakes much colder,[1] records a difference of as much as 38°·7 F. (21°·5 C.), and some of his eggs hatched, which was not the case with Forbes's; also the later observer does not record a steady fall of the temperature throughout the incubation as did the former.

The common English snake (*Natrix torquata*) lays its eggs, varying in number from about fifteen to thirty, principally in manure-heaps, but also in holes and crevices between or under stones. It is usually stated they are laid in the autumn, but I have frequently found them towards the end of July, and one snake I had in confinement laid them as early as the 11th of that month. The shells being very thin and soft they require very delicate handling, the eggs being readily injured.

When laid under stones or in crevices, they are protected from the immediate pressure of the earth, and when deposited in manure which is much bound together by leaves and straw, &c., they are not individually subjected to much pressure. It must also be noticed that the eggs are usually found very close to the surface, and if laid deep in the manure heap, they, as a rule, do not hatch, although if examined late in the season the young snakes are found in them often completely formed, but dead. When first laid, the eggs are swollen out and full, and many or all the eggs are firmly bound together by adhesive medium. The egg-shell, if examined with the microscope, is found to consist of peculiar glistening fibres arranged in many separate layers. Between the outer layers only a small quantity of calcareous matter is present, and these fibres appear to be closely fitted together. There are seen in fresh, or better in specimens hardened in chromic acid, ten or more layers laid very closely one upon the other. The outer layers differ from the others in that they contain many rather club-shaped bodies of very different thicknesses and appearances placed between the other fibres.

The eggs are at first of a whitish straw colour. As time goes on, they become somewhat darker and then of a brown colour, and finally very dark; but these colour changes do not occur evenly over the whole shell, but in patches, and to a very varying extent. At the same time the regular outline is gradually lost, the shell shrivels, loses its original elasticity, and so at this stage impressions made upon the surface remain permanent. The diminution in actual bulk of the egg is probably due to evaporation of water from its substance. It is chiefly the extreme delicacy of the eggs, also the difficulty of keeping them in the requisite amount of moisture, that makes them so very hard to hatch artificially. But all these difficulties may with care be overcome, as I will proceed to describe.

On July 22, 1889, I found, in the manure of a cucumber-frame in Surrey, some seventy snakes' eggs in two masses, close together, and probably deposited by at least two snakes. The eggs were apparently of recent date, but showed great differences in the stage of their development—even those which were clearly laid by the same female. After removing some eggs for immediate examination, the rest were covered up again by the manure and left.

On September 8 they were again uncovered, and some were removed and taken to London. One of the eggs being opened at this period, the embryo snake was fairly well formed, and movements were visible but feeble. The eggs were brought to London on September 8, and on the 9th some were placed in an ordinary bacteriological incubator regulated for a temperature of 32° C. (89°·6 F.). The eggs were placed in open glass dishes with a small quantity of dung laid both at the bottom of the dish and also partially covering the eggs. Some of the dishes were left open, and others were protected by a piece of glass loosely laid over, but allowing the air to freely circulate, and were kept perpetually moist by cotton wool soaked in water. At the same time some eggs were placed under similar conditions in the atmosphere of the room, the temperature of which was maintained at about 17° C. (62°·6 F.). On September 17, the incubator leaked and had to be repaired, and during this period the eggs from it were left in the room temperature—namely, about 17° C.; the eggs being replaced in the incubator again on September 25.

On September 27, two of the eggs which had been kept all the time in the room temperature, showed signs of hatching—that is, the heads of the young snakes had broken through the shells, temperature 19° C. (66°·2 F.). These were noticed at 10 p.m. The next morning at 10 a.m., the hatching eggs presented the same appearances—that is, the heads only were out of the shells. The dish with these eggs was then placed on the top of the incubator at about a temperature of 24° C. (75° F.). At 1 p.m. the condition was unchanged, and at 10 p.m. both snakes were quite out of their shells.

On the same day, that is, the 28th, at 10 p.m., the head of one of the snakes inside the incubator was seen; at 10 the next morning this snake was quite free from its shell.

During the next few days several more eggs hatched, both those inside and those outside the incubator.

Some eggs, which were kept in a tin of manure in the room atmosphere of about 18° (64°·4 F.) since September 8, were on the 25th placed outside the window. During the night the temperature registered a minimum of 1°·5 C. (35° F.). On the 26th, they were brought inside again, and on the 27th they were placed in a temperature of 26° C. (78°·8 F.). In the course of time some of these eggs hatched with the others. The eggs in the incubator were placed at first on their sides, but on the 28th some were placed on their ends, and in both positions they appeared to hatch equally well.

The period of incubation of the eggs was thus about seventy-five to ninety days; the python, as described by Valenciennes,

[1] The extreme temperatures of the air recorded by Valenciennes, who took his observations when the cages were coldest, *i.e.* before the fresh hot water was put in, were 17°, and 23° C. (62°·6 and 73°·4 F.) respectively. The temperature of the two cages in which the animals were kept was only on three occasions less than the highest in Valenciennes' series.

being fifty-six days. It was noticed that many days often elapsed between the hatching of the eggs of the same lot—even those kept under similar circumstances. The differences in the actual stage of development of the eggs when first laid may possibly explain the apparent differences in the dates of hatching.

On July 11, 1890, a snake I had in confinement laid eighteen eggs. Some of these were placed at a temperature of 16°-20° C. (61°-68° F.): at the end of October, not being hatched, they were opened, and found to contain fully-formed young ones, but these were all dead. Other eggs from the same lot, which was laid on July 11, were sent into the country and placed in a manure-heap; on September 9, an egg being opened, the embryo snake was nearly formed, but there were no movements visible; on September 24 these eggs began to hatch—that is, after an incubation of seventy-five days.

From the first set of experiments it did not appear that the actual temperature influences to any great degree the period of incubation, or at least not after the first few weeks. (In the cases described it would appear that the eggs had been deposited some seven weeks before they were removed, and then kept artificially from three weeks to a month before they hatched.) Also, that exposure to the atmosphere does not destroy their vitality, provided they are kept fairly moist, some having hatched after several days' full exposure to the air of the room; and that they may be exposed to rather low temperatures, at least for a few hours, and yet finally hatch. As might be expected, some eggs which were placed in small glass pots and hermetically sealed did not hatch.

The process of hatching was very interesting to watch. At first a slit appeared in the uppermost part of the egg, whether the egg was placed on the side or on one end; most usually the slit rapidly became a **V**-shaped one, which in shape and position corresponded to the snout of the young reptile—that is to say, the apex of the **V** corresponding to the tip of the lower jaw. The snakes would often remain for some hours in this position, with just their snouts out, and, when disturbed, would withdraw these into the shells again. In a state of nature I have seen them when completely out of the shell, retreat into it again when disturbed. When first out of the shell, the young snakes were very smooth and velvety to the touch; there was usually some opacity about the cornea, which disappeared after a few hours; the yellow ring on the neck was well marked from the very first. They were about 15 cm. (6 inches) in length, and weighed about 3 grammes (45 grains); the eggs themselves weighed about 6 grammes (80 to 90 grains). One cast its skin within a few days after birth, and died. Occasionally they were hatched with the yolk-sac adherent, and in these instances always died. From the first the snakes were very lively, and within a very few days produced the characteristic hissing noise when provoked.

Many problems in connection with the subject of the incubation of eggs might be mentioned. It would be interesting to ascertain definitely what are the maximum and minimum temperatures at which the vital processes can take place in an incubating egg. There is probably an optimal temperature, or one at which the process proceeds most rapidly or most favourably. So also it might be asked, Is the optimal temperature the same for all kinds of eggs—those, for instance, of various forms of birds and those of snakes and lizards? Is the increase of temperature, both of the incubating bird and of the incubating python, essential to the hatching of the eggs? What is the reason of the differences in the incubation periods between different birds? Why, for instance, do pigeons' eggs hatch in fourteen days, hens' three weeks, turkeys' a month, and swans' six weeks?

We know that if a hen's egg be maintained for some twenty-one days at a temperature of about 40° C. it will hatch; but I am not aware of any experiments to ascertain if they will hatch at a temperature considerably under or much over this, and what is the minimum temperature at which they will hatch at all? In the case of many of the micro-organisms, bacteriologists have found the actual limits of temperature within which the various species grow, and also that most of them have an optimal temperature—that is, one at which these lowest forms of vegetable life grow most luxuriantly.

Literature.—Valenciennes, *Comptes rendus de l'Académie des Sciences*, 1841; Sclater, Proceedings of the Zoological Society, London, 1862; Abbott, Proceedings of the Zoological Society, London, 1862; Lataste Fernand, Paris, 1877; Forbes, Proceedings of the Zoological Society, London, 1880; Fisher, *Der Zoolog. Garten*, Bd. 26, 1886.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Among the lectures which are being delivered this term, we notice the following:—Electricity, Prof. Clifton; Physical Optics, Mr. Walker; Ureas and Uric Acid, Prof. Odling; Surfaces of the Second Order, Prof. Sylvester; Disturbed Elliptic Motion, Prof. Pritchard.

In the Morphological Department Prof. Ray Lankester is giving a general course on Animal Morphology, and Mr. Minchin, who has been appointed Junior Demonstrator, is lecturing on the Porifera.

The arrangements for the instruction of medical students in physiology have been considerably improved.

The Burdett-Coutts Geological Scholarship has been awarded to Mr. F. W. Howard, of Balliol.

In the Report of the Visitors of the University Observatory it is stated that Prof. Pritchard will shortly publish an enlargement of his lectures on Disturbed Elliptic Motion.

The following examiners have been appointed for next year:—Physics, Mr. Baynes and Mr. H. G. Madan; Chemistry, Prof. Tilden; Animal Morphology, Mr. Poulton and Mr. Bourne; Physiology, Mr. F. Gotch.

The statute respecting the admission of women to examinations in medicine, which has formed the subject of a good deal of controversy, has been rejected in Congregation by 79 votes to 75.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for August contains:—On the origin of vertebrates from Arachnids, by W. Patten (plates xxiii. and xxiv.). As a full description of the author's observations could not be published without considerable delay in this article of sixty pages, he gives a short account of the facts bearing directly on the subject, and at the same time presents his theoretical conclusions. Recognizing the "Annelid theory" as sterile, the author thinks that since vertebrate morphology reflects, as an ancestral image only, the dim outlines of a segmented animal, but still not less a vertebrate than any now living, it is clear that the problem must be solved, if at all, by the discovery of some form in which the specialization of the vertebrate *head* is already foreshadowed. While, since of all invertebrates, concentration and specialization of head segments is greatest in the Arachnids, it is in these, on *a priori* grounds, that we should expect to find traces of the characteristic features of the vertebrate head. Finding, from time to time, confirmation of this preconceived idea, as the unexpected complexity of the Arachnid cephalothorax revealed itself, he feels justified in formulating a theory that *Vertebrates are derived from the Arachnids*.—On the origin of vertebrates from a Crustacean-like ancestor, by W. H. Gaskell, F.R.S. (plates xxv. to xxviii.). This paper is but chapter one of a very important memoir, which approaches the subject of the ancestry of the vertebrates from a different standpoint from that of Dr. Patten. In previous papers the author had pointed out that the vertebrate nervous system is composed of nervous material grouped around a central tube which was originally the alimentary canal of the invertebrate from which the vertebrate arose, and that the physiology and anatomy of this system both best fit in with the assumption that the invertebrate ancestor was of the Crustacean, or at least of a proto-Crustacean type. In both these papers the author promised to point out the confirmation of this theory, which is afforded by the study of the lowest vertebrate nervous system, viz. that of the Ammocœtes form of Petromyzon. This promise he redeems in this paper, in which, to bring out as prominently as possible the theory, he discusses the nervous system of the Ammocœtes in terms of the Crustacean. Taking separately the prominent features of the alimentary canal and central nervous system of a Crustacean-like animal, he indicates how each one exists in the nervous system of the Ammocœtes. In a second chapter it will be pointed out how the present alimentary canal arose by the prolongation of a respiratory chamber.—On the development of the atrial chamber of Amphioxus, by E. Ray Lankester, F.R.S., and Arthur Willey (plates xxix. to xxxii.). The period of development was that before Hatschek's well-known work stops short. Series of sections were prepared in order to ascertain the mode in which the atrial chamber takes its origin, and the subsequent history of the gill-slits, viz. as to how the slits on the left side of the pharynx originate. The relation of the larval to the adult mouth, and the details of the curious process of the movement of the mouth from a unilateral to a median position, were also included in the scope of the author's inquiries.

American Journal of Science, November 1890.—Further study of the solar corona, by Frank H. Bigelow. The author has made a series of measures upon photographs of coronal streamers taken in 1878, July 29, by Prof. Asaph Hall. This has been done with a view of testing the validity of the equation that he has assigned to the coronal curves in a discussion of them by spherical harmonics.—Superimposition of the drainage in Central Texas, by Ralph S. Tarr.—A description of the "Bernardston Series" of metamorphic Upper Devonian rocks (continued from October), by Prof. B. K. Emerson.—Analysis of rhodochrosite from Franklin Furnace, New Jersey, by P. E. Browning.—A re-determination of the atomic weight of cadmium, by Edw. A. Partridge.—On the occurrence of nitrogen in uraninite, and on the composition of uraninite in general; condensed from a forthcoming Bulletin of the U.S. Geological Survey, by W. F. Hillebrand. It is found that nitrogen exists in uraninite in quantities up to over 2·5 per cent., and seems generally to bear a relation to the amount of UO_2 present. This is the first discovery of nitrogen in the primitive crust of the earth.—Anthophyllite from Franklin, Macon County, North Carolina, by S. L. Penfield.—Pre-glacial drainage and recent geological history of Western Pennsylvania, by P. Max Foshay.—On the so-called perofskite from Magnet Cove, Arkansas, by F. W. Mar.—Experiments upon the constitution of the natural silicates (continued from October), by F. W. Clarke and E. A. Schneider.

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, November 4.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the months of June, July, August, September, and October, 1890, and called special attention to several of them. Among these were a young male example of the Wild Cattle of Chartley Park, Staffordshire, presented by Earl Ferrers; a Water-Buck Antelope (*Cobus ellipsiprymnus*) from the Somali Coast, presented by Mr. George S. Mackenzie; an example of the Horned Screamer (*Palamedea cornuta*), obtained by purchase; and a young female of Speke's Antelope (*Tragelaphus spekii*), presented by Mr. James A. Nicolls.—The Secretary exhibited, on behalf of Dr. A. B. Meyer, a coloured photograph of a singular variety of the Rose-coloured Pastor (*Pastor roseus*), with a red head, obtained near Sophia; and read a note from Dr. Meyer on this subject.—Mr. G. A. Boulenger made some remarks on an early reference to the Syrian Newt (*Molge vittata*) in Shaw's "Travels," published in 1738.—Mr. J. J. Lister gave an account of his recent visit to the Phœnix Islands, Central Pacific, and exhibited specimens of the birds and eggs obtained there.—Mr. Smith Woodward exhibited and made remarks upon the calvarium of an adult male *Saiga tatarica* from the Pleistocene deposits of the Thames Valley. The specimen had been obtained by Dr. J. R. Leeson from recent excavations in Orleans Road, Twickenham, and was the first trace of this Antelope discovered in Britain.—Mr. W. T. Blanford read a paper on the Gaur (*Bos gaurus*) and its allies, with especial reference to the exhibition of the first living Gaur ever brought to Europe in the Society's Gardens. He described the characters and geographical range of the three allied species of flat-horned taurine Bovines—the Gaur or Sladang (Bison of Indian sportsmen), the Gayal or Mithan (*Bos frontalis*), and the Banteng (*Bos sondaicus*); and he discussed the question whether *B. frontalis* is ever found in the wild state.—A communication was read from Dr. A. B. Meyer, containing the description of a new species of Squirrel from the Philippine Islands, which he proposed to call *Sciurus cagsi*.—Mr. R. Lydekker read a paper on a Cervine jaw from Pleistocene deposits in Algeria, which appeared to indicate the former existence in that country of a large Deer allied to *Cervus cashmirianus*. For this form Mr. Lydekker proposed the name *Cervus algericus*.—A communication was read from Dr. A. Günther, F.R.S., on the skull of the East African Reed-Buck. In this paper Dr. Günther described the skull of an Antelope obtained by Mr. H. C. V. Hunter in Masai Land, which he identified with *Cervicapra bohor* (Rüppell) from Abyssinia. He pointed out the differences from the skull of the South African species, for which the name *Cervicapra redunca* (Pallas) is generally employed.—Mr. P. Chalmers Mitchell described a graphic formula, designed for the purpose of representing geographical distribution. The regions were indicated by lines, the sub-regions by symmetrically placed numbers. This formula could be drawn rapidly, and printed without engraving.—Mr. W. L. Sclater read the description of a Jerboa from Central Asia, which he proposed to refer to a new genus and species of Dipodinæ under the name *Eucoreutes naso*.

Entomological Society, November 5.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—Lord Walsingham announced the death of Mr. Atkinson, of the Indian Museum, Calcutta.—Mr. A. H. Jones exhibited a number of Lepidoptera collected in June last near Digne, Basses Alpes, including *Papilio Alexanor; Parnassius Apollo*, larger and paler than the Swiss form; *Anthocharis tagis*, var. *Bellezina; Leucophasia Duponcheli; Thecla spini; Thecla ilicis*, var. *cerri; Lycæna argiades*, var. *corretas; Melitæa deione*; and *Argynnis Euphrosyne*.—Mr. W. E. Nicholson also exhibited a collection of Lepidoptera, formed near Digne last June, which included very large specimens of *Papilio Machaon; P. Podalirius; Thais rumina*, var. *medesicaste*, larger and redder than the Mediterranean specimens; *Apatura ilia*, var. *clytie; Argynnis adippe*, var. *cleodoxa; A. Daphne; Melanargia galatea*, var. *leucomelas*; and many others.—Mr. C. O. Waterhouse exhibited the wings of a large species of *Attacus*, split in halves longitudinally so as to show the upper and lower membranes.—Dr. D. Sharp, F.R.S., exhibited a photograph he had received from Prof. Exner, of Vienna, showing the picture obtained at the back of the eye of *Lampyris splendidula*. He stated that this picture is continuous and not reversed, and shows the outlines of lights and shades of objects at a distance as well as of those closer to the eye.—Mr. H. Goss exhibited a specimen of *Zygæna filipendulæ*, var. *chrysanthemi*, which he had taken at Rhinefield, in the New Forest, on July 15 last. Dr. P. B. Mason said this variety was known on the continent of Europe, and was figured by Hübner in his "Sammlung," a copy of which work he exhibited. He added that he possessed a similar specimen of this variety taken in Wyre Forest, Worcestershire. Colonel Swinhoe stated that he possessed a similar variety of a species of *Syntomis*.—The Rev. Dr. Walker exhibited seven varieties of *Melanippe thuleana*, nine of *Coremia munitata*, and a few of *Noctua conflua*, illustrating the varied forms of these species occurring in Iceland. Dr. Mason said that the only British specimens of *N. conflua* which he had seen resembling the Iceland form of the species were taken at Wolsingham, Durham.—Mons. A. Wailly exhibited and remarked on a number of Lepidoptera from Japan. The collection comprised about thirty species, eleven of which, it was stated, were not represented in the British Museum collections.—Mr. A. C. Horner exhibited a number of rare species of Coleoptera, including *Homalota crassicornis*, Gyll., *H. humeralis*, Kr., and *Euryporus picipes*, Pk., collected at Church Stretton, Shropshire; and also *Amara nitida*, Sturm., *Oxypoda amæna*, Fair., *Homalota testaceipes*, Heer, and *Lithocharis apicalis*, Kr., from the neighbourhood of Tonbridge.—Herr Meyer-Darcis exhibited a specimen of *Termitobia physogastra*, Gangelb., a new genus and species of *Brachelytra* obtained in a white ants' nest from the Congo. Dr. Sharp commented on the interesting nature of the exhibition.—Colonel Swinhoe exhibited a collection of moths from Southern India, which comprised about forty species. He also read a paper, describing these species, entitled "New Species of Moths from Southern India."—The Rev. T. A. Marshall communicated a paper entitled "A Monograph of British Braconidæ, Part IV."—Lord Walsingham read a paper entitled "African Micro-Lepidoptera," containing descriptions of seventy-one new species, and of the following nine new genera, viz. *Autochthonus* (type *A. chalybiellus*, Wlsm.), *Scalidoma* (type *Tinea horridella*, Wkr.), *Barbaroscardia* (type *B. fasciata*, Wlsm.), *Odites* (type *O. natalensis*, Wlsm.), *Idiopteryx* (type *Cryptolechia obliquella*, Wlsm.), *Microthauma* (type *M. metallifera*, Wlsm.), *Licmocera* (type *L. lyonetiella*, Wlsm.), *Oxymacharis* (type *O. niveocervina*, Wlsm.), and *Micropostega* (type *M. æneofasciata*, Wlsm.). Several European and American genera were recorded as new to the African fauna; and the occurrence of one Australian and two Indian genera was also noted.

Linnean Society, November 6.—Prof. Stewart, President, in the chair.—Mr. E. M. Holmes exhibited and made remarks on some little-known seaweeds, including *Monostroma Blythii* and *Capsosiphon aureolus*, collected at Taymouth, *Oscillaria Corallinæ* from Weymouth, and *Schizothrix lardaica* from Paignton. Mr. Holmes pointed out that *Spermothamnion intricatum* gathered near Swanage showed gradual transition to *S. Turneri*, of which it should henceforth be considered only a vegetative form.—Mr. George Murray exhibited and described the peculiarities of some galls of *Rhodymenia* formed by a crustacean.—Prof. G. B. Howes exhibited a specimen of *Lima hians*, with a byssus "nest" which it had spun in 21 days in a vessel of sea-water in which it had been placed. Although constantly watched by day and night, the act of spinning had not been observed.—On behalf of Mr. J. W. Willis Bund, Mr. Harting exhibited and made some remarks upon a South Pacific Petrel (*Œstrelata torquata*, Macg.), which had been shot in Cardigan Bay in December last.—On behalf of Prof. Martin Duncan, who was unable to be present, Mr. P. W. Sladen exhibited two microscopic preparations of the ambulacral ampullæ of Echini, showing that each ampulla is supplied by one offshoot from the main ambulacral water-vessel.—Mr. Harting exhibited a specimen of the Baltimore Oriole (*Icterus Baltimore*), which had been lately obtained at Balta Sound, Shetland.—A paper was then read by Rev. Prof. Henslow, entitled "A Contribution to the Study of the Relative Effects of Different Parts of the Solar Spectrum on the Assimilation of Plants." The paper was illustrated by diagrams, and a discussion followed, in which the President, Prof. H. Marshall Ward, Dr. D. H. Scott, and others took part.

Paris.

Academy of Sciences, November 10.—M. Hermite in the chair.—New researches upon the synthesis of rubies, by MM. E. Fremy and A. Verneuil (fourth communication). The anthors have succeeded in producing rubies on the large scale, and of greater size than heretofore. The artificial stones are as good as the natural rubies for the purposes of the watchmaker.—Study on the fluor-spar of Quincié, by MM. Henri Becquerel and Henri Moissan. The following conclusions concerning the fluor-spar from this locality, long considered of special interest on account of the peculiar odour it evolves on being broken into fragments, have been formulated in this paper: (1) that the fluor-spar from Quincié contains an occluded gas, which is seen to be disengaged when fragments of the spar are broken under the microscope; (2) that all the reactions furnished by the fluor-spar from Quincié may be explained simply by the presence of a small quantity of free fluorine in the occluded gas.—On the approximate representation of a function by rational fractions, by M. H. Padé.—On the analysis of hypophosphorous, phosphorous, and hypophosphoric acids, by M. L. Amat. These acids or their salts may be analyzed either by means of mercuric chloride or potassium permanganate, provided the conditions given are adhered to. The most precise method is that in which mercuric chloride is employed, but the time required is much greater.—Combinations of mercuric cyanide with salts of cadmium, by M. Raoul Varet. Bodies having the following formulæ have been described—

(1) $HgCy_2$, $CdCy_2$, HgI_2, $7H_2O$.
(2) $2HgCy_2$, $CdBr_2$, $4{\cdot}5H_2O$.
(3) $HgCy_2$, $CdBr_2$, $3H_2O$.
(4) $HgCy_2$, $CdCl_2$, $2H_2O$.

—On the preparation and properties of benzoyl fluoride, by M. E. Guenez.—Synthesis of citric acid, by MM. A. Haller and A. Held.—Experimental study of the *rôle* attributed to lymphatic cells in the protection of the organism against the *Bacillus anthracis*, and in the mechanism of adventitious immunity, by M. C. Phisalix. It is shown that the lymphatic ganglion plays both a mechanical and chemical *rôle*: it is an organ that arrests and considerably modifies the form and virulence of the *Bacillus anthracis*.—Experimental production of white tumours in the rabbit by the inoculation of attenuated cultures of Koch's bacillus into the veins, by MM. J. Courmont and L. Dor. The experiments indicate: (1) primitive local tuberculosis appears to be due to the action of an attenuated tubercular virus; (2) after having penetrated directly into the blood, no effect may be observed for some months; (3) the articular synovials, at least in young subjects, favour the implantation of the attenuated tubercular virus more (but without local traumatism) than the visceral organs.—On the development of *Dondersia banyulensis*, by M. G. Pruvot.—New researches on the spores of *Myxospongial* (structure and development), by M. P. Thélohan.—Observations of Norwegian salmon, by M. J. Kunstler.—The Coleopterous parasites of Acridia: the metamorphoses of Mylabrides, by M. J. Kunckel d'Herculais.—On the means (1) of recognizing sections of felspar parallel to the 010 plane, in thin slabs of rocks; (2) of utilizing their optical properties, by M. A. Michel Lévy.

Amsterdam.

Royal Academy of Sciences, October 25.—Prof. van de Sande Bakhuyzen in the chair.—Mr. van Diesen, having referred to his note of January 27, 1872, concerning the disturbances to which the River Maas is liable in times of flood, explained the plan by which it is proposed that the river shall be directed through a new mouth in the Amer, the present mouth near Woudrichem being closed. This work, which is now in full progress, will diminish the said disturbances. He gave a general view of the details of the scheme, and compared it with the project proposed, in 1822, by General Krayenhoff, whose plan for the separation of the Rivers Waal and Maas will be executed more thoroughly than he intended.

CONTENTS.

THURSDAY, NOVEMBER 27, 1890.

THE UNITED STATES CENSUS.

WE are surprised that so little attention is being given here to the totals of the United States census which are just being published. There is no more important record in the life of a people than its census. Accounts of progress are impossible without it. In the United States, also, special attention is given to the census, partly because it is a requirement of the Constitution, which assigns representatives to States and districts in accordance with the population figures. Every ten years, in the United States, there is a vast outlay on the business, with which outlay nothing spent in Europe can compare. But now there are loud complaints all over the United States that the record for 1890 is *wrong;* that the population of cities and places has been miscounted grossly. If these complaints are true, the United States might almost as well have had no census at all. All the elaborate work which is to be based on these population figures is rendered useless before it is begun Apart from the direct loss to the United States people themselves, who lose the information about their own affairs the census might have given them, the whole world sustains a loss in being deprived of comparisons of many kinds with so remarkable a progress as that of the United States. Has a colossal blunder, then, been made? and what can be the reason of it?

That there is a huge blunder, or worse, somewhere, appears quite unmistakable. Those who are interested have only to cast their eye over the following table to see that something unusual happened to the 1870 census, and has again happened to the 1890 census :—

Population of the United States, since 1800, and increase in each decennial period.

	Population (in thousands).	Increase. Amount.	Per cent.
1800	5,308	—	—
10	7,239	1,931	36
20	9,634	2,395	33
30	12,866	3,232	33½
40	17,069	4,203	33
50	23,191	6,122	36
60	31,443	8,252	35½
70	38,558	7,115	23
80	50,156	11,598	30
90	62,481	12,325	24½

In all this long period the increase of population in the United States has been at the nearly uniform rate of a *third* every ten years, with the two exceptions of the 1870 census and the 1890 census, in which the increase is respectively 23 and 24½ per cent. only. What can have happened, first between the 1860 census and that of 1870, and next between the census of 1880 and that of 1890, to make the results so different from those of all the other periods?

Now, what happened between 1860 and 1870 is partially explainable. These were the years of the great Civil War, in which privation and disease, with death and injuries on the battle-field, had their thousands and millions of victims. Such causes are well known to check the growth of population. Unfortunately, also, there is another partial explanation. It has been admitted on the highest authority, that of General Walker, who supervised the census of 1880—and the admission is now repeated by the superintendent of the 1890 census—that the figures of the 1870 census were in some States falsified, with the result that in 1880, when the census of that year was taken, an impossible increase of population appeared to have occurred in those States. As far as *amount* is concerned, however, the former explanation has always been understood to be the more serious.

Can any such explanation be given of the small increase between 1880 and 1890? The answer is obvious. There has been no war or the like occurrence since 1880 to check the growth of population. There is absolutely nothing to suggest why the United States population, having increased most rapidly from the beginning of the century down to 1880, excepting during the war decade of 1860–70, should have suddenly had its rate of growth arrested.

More than this, the period between 1880 and 1890 has been one in which, according to past experience, owing to the special magnitude of the immigration, the rate of growth should have been as great as in any of the previous periods. The amount, and proportion to the population at the previous census, of the immigration into the United States since 1820, has been :—

Census period.	Amount.	Proportion to population at the previous census.
1820–30	128,000	1·3 per cent.
1830–40	538,000	4·2 ,,
1840–50	1,427,000	8·4 ,,
1850–60	2,814,000	12·2 ,,
1860–70	2,264,000	7·2 ,,
1870–80	2,707,000	7·0 ,,
1880–90	5,275,000	10·5 ,,

Thus, between 1880 and 1890, as far as the element of immigration is concerned, the growth of population in the Unites States was as much stimulated as in any previous decade, with the single exception of the 1850–60 period. There was absolutely no reason, then, why the rate of growth should fall off between 1880 and 1890, but a special reason why it should not fall off.

When we compare the figures in amount, we are still more bewildered. Between 1870 and 1880, with an immigration of 2,707,000 only, the increase of population is 11,598,000; so that, deducting the immigration, the increase which is due to the excess of births over deaths appears to be 8,891,000. Between 1880 and 1890, with an immigration of 5,275,000, the total increase of population is 12,225,000, and if we deduct the immigration, the increase which is due to the excess of births over deaths appears to be 6,950,000 only! The excess of births over deaths which was nearly 9,000,000 between 1870 and 1880 falls to less than 7,000,000 in the following decade, although the population at starting was 25 per cent. greater in the later than in the earlier decade. Making any reasonable correction for the under estimate in the 1870 census itself, which is now admitted, we still find these figures most startling. Even if we were to increase the population of 1870 to 40,000,000, as the superintendent of the 1890 census now suggests, thus reducing the apparent increase between 1870 and 1880 from about 9,000,000 to about 7,500,000, we should still be confronted by the fact that, starting from a larger population, and with a larger immigration, the excess of births over deaths in 1880–90 would have been from 25 to 30 per cent. more than in the previous decade, or at least 9,500,000, whereas it appears to be under 7,000,000. The figures of the 1890 census are, therefore, quite incredible.

The superintendent of the 1890 census has issued an explanation, which does not, we fear, amount to very much. He makes a great deal of the errors in the 1870 census, which we have already glanced at, and asserts that the rate of growth of population, when proper corrections are made, was much less than 30 per cent. in the 1870–80 period, so that the rate of 24½ per cent. between 1880 and 1890 does not show a great falling off. But while he makes too much of the 1870 errors in amount, he makes no mention at all of the much larger immigration between 1880 and 1890 than in the previous decade, which should have made a difference of at least 3½ per cent. in the rate of growth in favour of the latter as compared with the former period.

A farther explanation is that there is a permanent tendency for the rate of growth of the population of a country like the United States to fall off. But this is not confirmed so far by any figures of a completely trustworthy kind, while the falling off in the rate of growth to be here accounted for is too great and sudden to be explained in such a manner.

The blunder is thus left quite unexplained, and the people of the United States, we may hope, will not fail to see to it. It concerns wider than merely national interests that the blunder should be seen to. For purposes of comparison, every census in the world is thrown out. Looking at the causes of the errors in the 1870 census, viz. an attempt to understate the numbers of the people in Democratic States, and at the special complaints of under-statement which have now come from New York and other Democratic centres, we have altogether too much reason to fear that the cause of the blundering is political. But, whatever the cause may be, it should be stringently looked into.

SPIDERS' WEBS.

American Spiders and their Spinning Work: a Natural History of the Orb-weaving Spiders of the United States, with Special Regard to their Industry and Habits. By Henry C. McCook, D.D. Vol. II. pp. 1–479, with 5 Coloured Plates and 401 Woodcut Figures. (Philadelphia: Allen Lane and Scott, 1890.)

IN a notice of vol. i. of the above work (NATURE, vol. xlii. p. 244), its object and scope were explained. Vol. ii., now before us, fully justifies what was there stated as to the thoroughness with which the available materials on the subject have been brought together from all sources, and for the first time presented to the world as a whole. A similar popular treatment also of this interesting and most important part of the subject is again here observable. Vol. i. was occupied with the snares and web-spinning of orb-weaving and some other spiders, principally in relation to the getting of their livelihood. Vol. ii. treats of these spiders in respect to the propagation of their kind, and web-spinning as subservient to this. Vol. i., in fact, presents us with spiders safely arrived at maturity, and forming their snares and webs with all the diversity and perfection peculiar to each species; while vol. ii. takes them up at that point, and shows them to us in all the different peculiarities pertaining to the performance of the ultimate object of their existence. Naturally, therefore, the volume before us begins (part i., chapter i.), with an account of the sexes in their relation to each other preparatory to actual pairing. This latter and the points arising out of it form the staple of chapters ii. and iii., which complete part i. A certain air of sentimental allusion, which appears to pervade the author's method of presenting this part of his subject, is perhaps merely a matter of taste, and so beyond the province of scientific criticism. It may be, however, that this, while it certainly adds nothing to scientific accuracy or progress, does add to the popularity of the subject, which is evidently throughout the work one of its author's great objects. On the small size of many male spiders compared with their bulky females, Dr. McCook does not appear to accept the views of a former writer upon it, in which it should be observed that the primitive equality in the size of the sexes is by no means implied; the general rule being that the male is the smaller of the two. But if it be granted that the female had a propensity for attacking and devouring the male, those males which happened to be the smallest and most active would be the most likely to escape, and perform the functions of the sex; natural selection would then come in, and operate gradually in the direction of lessening the size of the males. That there are numerous spiders, and groups of spiders, in which the sexes are nearly equal in size, or live in amity together, or in which the males are furnished with some protective armature against the ferocity of the female, proves nothing against the theory of the action of natural selection in lessening the size of the male in such cases as those where a devouring propensity existed and was otherwise unprovided against; for in groups where any approach to equality in size existed and became protective, or where some other protection became developed, there would be no need, in fact no case, for natural selection in the direction of diminished size, there being no advantage to be gained under it. The drift, however, of the author's reasoning on this subject (p. 7) is not very apparent.

Part ii. treats of the "Maternal Industry and Instincts of Spiders"; embracing the formation of cocoons for the reception of their eggs, and the bringing into existence of the young, which naturally leads (part iii.) to the consideration of the life of the young while still engaged in the struggle for existence necessary for the survival of the fittest. The dispersion of spiders on the approach of maturity leads to an account of their method of locomotion on gossamer lines and flakes, completing part iii.

Part iv. enters into the subject of the senses of spiders, and their relation to habit. The structure of the eyes, and their functions are gone into in considerable detail. The remarkable position of these organs in the males of some species—such as *Walckenaera acuminata*, Blackw., in which they are seated near or at the top of a very long slender kind of footstalk—is mentioned; and it is supposed (p. 298) that this might give the male spider an advantage when in search of the female; but, apart from this explanation not being warranted by any known facts as to the habits of the spider, it would seem to be well enough accounted for as the result of excess of vigour or vital force belonging to the stronger or male sex. The action of natural selection would operate here also, indirectly, and these and other similarly excessive developments would only be checked when they tended to become, or became (as in some cases they indeed appear to have nearly become), positively detrimental, or at any rate disadvantageous, to the sex. The senses of spiders—"smell," "hearing," and "touch"—are then gone into. In connection with the sense of "hearing," various stock stories about spiders and the effect of musical sounds upon them are detailed; but such small credit is attached to them as relating facts on which any scientific conclusions can be based, that it would hardly seem to have been worth while to swell an already bulky volume by their repetition. Chapter x. of part iv. ends with the details of the stridulating power of some spiders and its probable purpose. Chapter xi., on the colour, and colour-sense, brings part iv. to a conclusion. The more brilliant colouring and ornamentation of the male spider, in some groups, is accounted for by the preference supposed to be given on the part of the female to males thus ornamented. But it does not appear that this preference is yet proved in any instance as a fact; nor can it be fairly argued that, because sexual excitement often leads the male to display it in curious antics and contortions, it therefore follows that the female is in the least influenced by it; whereas in fact, as she is stated to be (p. 63), the female is generally an unmoved spectator. Doubtless the male frequently succeeds in his purpose after such displays—*post hoc* certainly, *sed non propter hoc*. The author having come to the conclusion that the female prefers the male for his bright colours, we are not surprised to find it argued, conversely, that in those groups where it is the female sex which is the largest and bears the brightest hues, it is the less gaudy male who is helped and influenced in his choice by the increased size and excessive coloration of the female. The argument here also does not appear to have more real weight than in the former case, if even so much. On the subject of adaptation of the colouring of spiders to their surroundings and its beneficial effect, the opinion is expressed that, considering the great exposure to enemies of numbers of brilliantly coloured and conspicuous spiders, no generalization is yet warranted. No weight seems here to be given to the supposition that some of these exposed spiders may be distasteful as food; while it is admitted that many, as those of the genera *Gasteracantha* and *Acrosoma*, are protected by their spiny armature, and, it may be added, by their generally hard integument. The theory of "warning colours" is dismissed as inapplicable to spiders. Adopting the experiments of Sir John Lubbock and Mr. and Mrs. Peckham, spiders are considered to possess a sense of colour; but when we are told that a test case was afforded by a spider whose eyes had been purposely blinded with paraffin, our confidence in the result of these experiments will perhaps be a little shaken; since it is gravely argued from this test case that, because a blind spider exercised no apparent choice of one colour over another, this proves that the apparent preference of a spider before blinding was a true choice, and that there exists a colour-sense in certain spiders. Chapter xii. of part v. is on that most interesting part of natural history in all its departments—resemblance to other objects both inanimate and animate, with the causes and consequences of the resemblance. Space will not permit us to follow the author in the details of this part of his subject. The chapter will be read with pleasure and interest by most observers; but we may perhaps remark that there is at times an apparent tendency to take inability to perceive any resulting advantage from the resemblance as a proof that the resemblance is not the result of natural selection.

Chapter xiii. of part v. treats of the enemies of spiders, and their influence on habit. This subject is, of course, closely connected with protective resemblance; and by no means the least interesting part of this chapter is the account of some truly parasitic spiders—*Mimetus interfector* and others. These take up their abode in the webs of other spiders, and after eating the rightful owner, regale themselves at leisure upon its eggs and young. Part v. ends with chapter xiv., on "Death and its Disguises; Hibernation, and Death feigning." Natural death may be almost said to be an unnatural event in the history of most of the creation, excepting man; but perhaps more frequent instances of it are seen by the entomologist and araneologist, than by other naturalists. Some of our British *Drassidæ*, in the genus *Clubiona* for instance, may often be found sewn up in a nest of leaves drawn together, with their egg-sac, brooding over it, in various stages of lethargy, sometimes so shrivelled and comatose as to be almost incapable of movement. These spiders probably drop to the earth with the opening of the nest for the exit of the young, and at once die when their progeny begin to live. The author gives interesting details of a similar kind in reference to spiders of the family *Epeiridæ* far removed from the *Drassidæ*, and we have also noted it in another family equally remote from both, *Thomisidæ*. "Death feigning" is considered to be perfectly voluntary, though perhaps developed out of an original state of "fright-paralysis"; contrary to the opinion and explanation given by Darwin of this habit. The concluding part of the book, part vi., contains an account of fossil spiders, a subject which, though bearing very little upon the "spinning work of spiders," has a very strong and an increasing interest of its own. It appears to be well treated in the twenty-three pages here devoted to it. We cannot leave this necessarily very incomplete notice of Dr. McCook's bulky volume without drawing especial attention to the numerous (401) woodcuts, and the coloured plates, with which it is so profusely and usefully illustrated. The greater part of these are engraved from the author's own original drawings, and evidence a skill as well as a power of patient observation scarcely equalled in any contemporary work on natural history.

O. P. C.

MR. BASSET'S ELEMENTARY TREATISE ON HYDRODYNAMICS AND SOUND.

An Elementary Treatise on Hydrodynamics and Sound. By A. B. Basset. (Cambridge: Deighton, Bell, and Co. London: George Bell and Sons. 1890.)

WHEN the Senate of the University of Cambridge decided to adopt the suggestion of the Special Board for Mathematics to include the elements of hydrodynamics and the theory of sound among the subjects of Part I. of the Tripos, teachers of these subjects naturally looked to Mr. Basset to provide a text-book that should meet the wants of students preparing for the examination, and he has responded to the demand with great promptitude. The present treatise is designed, he tells us, for those who are reading for this examination and others in which a knowledge of these subjects is required. If the purpose of the book had been different—if, for instance, it had been written as a purely scientific training for hydraulic engineers, or for use in a physical laboratory—it would have had to be conceived in a very different vein. We should have looked for full explanations of elementary concepts, frequent appeals to experiment, constant arithmetical interpretation of the analysis, and a large proportion of physical reasoning. If, however, the book is to be judged by the standard it aims at, it must be regarded as an admirable specimen of an examination book. The propositions are clearly set out in a methodical order. They are isolated from each other as much as possible, and proved individually by the use of appropriate principles. The examples are for the most part well chosen, and calculated to initiate the student into a great variety of the tips and dodges with which the examiners are likely to be familiar; and no more is generally given than would be useful in writing out bookwork and solving problems.

We proceed to a detailed account of the work.

The treatise is divided into two parts, of which the first deals with hydrodynamics, and the second with the theory of sound. In the first part there are five chapters. Chapter i. treats of the kinematics of fluids and of the general equations of motion. We are glad to see that the author has given prominence to the "flux method," and has had the courage to restore the elementary parallelepiped which Prof. Greenhill affects to despise; for the purposes of an elementary treatise the value of this artifice is too great to be lost. Very welcome also is the proof of the important principle at the foot of p. 11, first stated exactly in the larger treatise, vol. ii. p. 234, and apparently due to Prof. Greenhill ("Encyc. Brit.," Art. "Hydromechanics"). We could wish that the theory of the bounding surface had been as fully explained. The same chapter i. contains a short account of sources, doublets, and images, and electric and magnetic analogies are given which add much to the usefulness of these sections.

Chapter ii. treats of the motion of a sphere and of the motion of certain cylinders in an infinite fluid. The descent of the sphere under gravity is very nicely worked out, the usual ambiguity being avoided by explicitly introducing the distance of the centre from a fixed horizontal plane. The resultant pressure on the sphere is calculated, and the equations of motion deduced from Newton's second law. The chapter concludes with an interesting account of the resistance of a liquid to the motion of a spherical pendulum. This chapter contains an exception to the general plan of the work. Mr. Basset appears to write hydrodynamics *con amore*, and cannot always be restrained from trying to teach the student something which he will not be called upon to write out. We refer to his account of recent researches in the theory of the resistance of viscous fluids.

Chapter iii. is occupied with the theory of the motion of a single solid in an infinite liquid. This part of the subject is less elementary than the others which are treated in the book, inasmuch as the machinery of moving axes has to be introduced. The author has cleverly avoided the use of Lagrange's equations and Routh's method of ignoration of co-ordinates, but it must be confessed that it is sometimes a little difficult to see how some of the terms in the equations are obtained by means of the principles invoked. The student may well be puzzled to account for the term $-M'g$ in the equation of motion of the sphere on p. 69. The most interesting problem discussed in the chapter is that of the motion of an elliptic cylinder. Drawings of the path of the centre of gravity in the three cases of oscillation, revolution, and just complete revolution, are given. In the first case, the path looks something like an orthogonal projection of a curve of sines, and the cylinder moves so as to have turned through the maximum angle when the centre is at an inflexion; it then turns back, and the angle described goes through a periodic oscillation while the cylinder moves over a wave-length. In the second case, the path looks like a nodal trochoid, and the cylinder makes one complete revolution in the time taken to pass from a node to the next consecutive node but one. In the third case, the path looks like a nodal cubic with an inflexion at infinity. The cylinder moves from infinity with its major axis initially parallel to the asymptote; at the furthest extremity of the loop it has turned through a right angle, and it then goes off to infinity in the opposite direction, and only turns through two right angles in the whole length of its path. Other interesting things in the chapter are the application of the theory of helicoidal steady motion to explain the necessity of rifling guns, and the theory of the motion of a cylinder in a fluid bounded by a fixed rigid plane, leading to the suggestion of the realization of "action at a distance" by means of fluid pressure.

Chapter iv. is devoted to liquid waves. All the elementary problems are treated very elegantly. "Long waves" are in the first place regarded as a particular case of progressive harmonic waves, and the "exact theory" of long waves in a canal comes afterwards. This seems to us the most natural order. Another point of interest is the discussion of a case of instability, due to Lord Rayleigh. Mr. Basset has done well all through to insist upon the importance of investigations relating to stability.

Chapter v. is occupied with the theory of rectilinear vortices. The vortex line is treated as an ideal limit of a vortex cylinder, and some cases in which the cylinder is of finite section are discussed. We think it unfortunate that in treating the elliptic vortex cylinder the value of the constant D is not given, as it is directly proportional to the circulation; but the student reading the section is certain to take it to be an arbitrary constant. The simplest cases of motion of a straight vortex in a bounded space are treated by the method of images. The chapter concludes with new proofs of Helmholtz's celebrated "laws of vortex motion." A brief account of Sir W. Thomson's theory of "vortex atoms" would have been of interest here.

Part ii. of the treatise deals with the theory of sound, and contains five chapters. The first of these (chapter vi.) is intoductory, and explains the relation of musical notes to the vibrations of bodies, and the connection between sound and the propagation of waves in air. It is admirable rather for conciseness than for completeness of exposition.

Chapter vii. is occupied with the vibrations of strings and membranes, nearly the whole of it being devoted to transverse vibrations of strings. The method of acoustics is adopted, as developed in Lord Rayleigh's treatise. It consists in assuming the motion to be periodic, and dependent upon a function called a normal function, in a series of which arbitrary functions can be expanded. Mr. Basset has changed the nomenclature, so that the "normal co-ordinate" of Lord Rayleigh is here called a "normal function." Is this intentional? The method adopted dispenses with the necessity of proving Fourier's theorem. Other things in this chapter worthy of note are the formation and discussion of the equation of motion for a string subject to viscous resistance and under the action of a periodic force, leading to a particular illustration of the well-known theory of forced vibrations.

Chapter viii. is occupied with the theory of the vibrations of bars. It opens with an account of the stress in a bent bar, and we have here the first hint in an English book on acoustics of any difficulty in that subject. In a footnote Mr. Basset clearly states the nature of the assumption usually made, and further expresses his conviction that it is not rigorously true, describing the character of the change in the equations of motion if a more exact theory were adopted. It is a good thing to have mentioned this. The rest of the chapter is devoted to the lateral vibrations of bars. The differential equations are obtained by the use of the stress equations, the idea of the method of formation being taken from a paper by Dr. Besant, and the frequency equations are given for the various cases of ends supported in different manners. Although, perhaps, a discussion of them may be beyond the purpose of the book, we cannot help thinking that a few numerical results would assist in the comprehension of the subject

The two last chapters deal with the theory of waves in air. Chapter ix. contains the formation of the differential equation of vibrations, and a discussion of the value of the velocity of sound as given by Newton and Laplace's suppositions respectively. To explain the latter an account is given of the thermodynamics of gases, leading to the relation of pressure to density when the changes are isentropic.

Chapter x. contains an account of some of the simpler problems of plane and spherical waves. We find here the theory of the notes in a doubly closed pipe, and a short discussion of the forced vibrations produced by attributing an arbitrary periodic motion to a disk at one end of the pipe. The interest of vibrations in pipes lies rather, as it seems to us, in the cases where the pipe has one or both ends open. Even when these are treated as "loops," something may be done towards a theory of organ pipes. The relation of the notes in a "stopped" to those in an "open" pipe is not even given as an example, nor do we find any account of the interesting problem of reflexion of waves in a pipe at the stopped or open ends. The next subject treated is the reflexion and refraction of plane waves at a surface of separation between two gases; this is very nicely worked out, the equations and conditions being very clearly given. The rest of the chapter is occupied with spherical waves. We have here Lord Rayleigh's solutions for (1) purely radial disturbance within a rigid spherical envelope; (2) the vibrations in a conical pipe; (3) the resistance of the air to the motion of an oscillating sphere, and the theory of the scattering of plane waves by a small sphere. The last problem, as here solved, requires the expansion of the velocity potential in a series of spherical harmonics—the only instance in the book of the use of these functions.

The book is well printed and nicely bound. The few blemishes we have had occasion to notice will not seriously diminish its value, and Mr. Basset is to be congratulated on having produced a work that certainly ought to achieve success. A. E. H. L.

LOWNE ON THE BLOW-FLY.

Anatomy, Physiology, Morphology, and Development of the Blow-fly (Calliphora erythrocephala). Part I. By B. Thompson Lowne, F.R.C.S., F.L.S., &c. (London: R. H. Porter, 1890.)

MR. LOWNE'S new work on the blow-fly, of which Part I. has just appeared, requires the serious attention of all who occupy themselves with insect anatomy. It contains the results of a diligent and protracted inquiry, and will teach even specialists a good deal which they did not know before.

Mr. Lowne has written before on this subject. His earlier book (1870) on the blow-fly was the manual of what we may now call a past generation of insect-anatomists, who studied it zealously, and, we fear, often got hopelessly puzzled with its many difficult passages. The present publication is not a new edition, but a totally new work, and the author has only thought it worth while to mention his earlier memoir quite casually in one or two places. The interval of twenty years has enabled Mr. Lowne to make great advances in the knowledge of his subject, and his old treatise on the blow-fly may now cease to be read.

This increase of knowledge is due to laborious microscopic investigation, and to a lengthy, though not exhaustive, study of a literature which is copious, technical, and very largely German. The student has to thank the painstaking author for many new facts, and also for a good deal of information, which, though not new, was previously accessible only to specialists.

Perhaps the most interesting remarks which we have found in Part I. relate to the imaginal disks (it would be better to call them imaginal folds), those curious inward growths of the larval epidermis from which nearly the whole body of the fly is ultimately fashioned. Mr. Lowne's account should be carefully examined by those who have hitherto been content with text-book information, or the descriptions of Weismann, which, original and brilliant as they were, required rectification on a number of special points. We can look forward to a discussion of these imaginal folds far more interesting, and at the same time simpler, than any that present knowledge has produced. Such a type as Corethra (which, by the way, is misquoted as Chironomus in the note to p. 77) shows a slight telescoping of the imaginal antenna within the larval head. Other types, as yet imperfectly described, exhibit deeper and more complex invaginations, while the Muscidæ form the extreme term of the series. At this moment we want above all *comparative* studies, and till they are supplied, minute descriptions of highly special cases are hardly intelligible. Few biologists seem to be aware of the interesting research which lies open to any competent student in a well-selected series of Dipterous larvæ and pupæ. It will ultimately be necessary to include other insect-orders, for imaginal folds are not peculiar to Diptera. Many Lepidoptera are instructive in this connection. Pieris, for example, as J. Dewitz (*Biol. Centralblatt*, Bd. iii. p. 582) points out, exhibits that connection between the sutures of the clypeus and the antennary folds which the author (quoting Mr. Hammond) has noticed on p. 43. The subject is not sufficiently worked out for popular treatment, and the beginner who takes up Mr. Lowne's account of the development of the fly has many a hard nut to crack.

The new work contains many interesting particulars concerning the life-history and minute structure of the blow-fly; there are not a few useful figures, and the bibliographical references are tolerably extensive. If the succeeding parts are equally full of matter, the treatise will make a really considerable addition to our knowledge.

Nevertheless there are some faults to be pointed out. The arrangement of the matter is not always convenient or luminous; see, for example, the place (p. 12) chosen for the definition of an insect and the definition of morphology. Now and then an ill-considered remark, perhaps having no close connection with the subject in hand, distracts or misleads the reader. Why should the author go out of his way to speak of mammals, birds, and reptiles as "separate and divergent genetic series" (p. 26), a proposition by no means so evident that it can be thrown in as a passing illustration! Surely Mr. Lowne, by thinking twice, would have saved his readers from puzzling over that strange remark (p. 7) that in all insects (this is apparently the sense) there is no cœlom or "distinct continuous body-cavity."

We shall await with much curiosity the promised proof of the hepatic function of the Malpighian tubules. Meanwhile we can only wonder in what sense the author uses the word *hepatic*. We shall also be glad to learn the reasons for believing in a "metenteron," as defined on p. 17.

No one interested in insect anatomy is likely to adopt all Mr. Lowne's views, but no such person can hesitate to admit that he offers us a substantial contribution to his favourite subject.

L. C. M.

OUR BOOK SHELF.

A Treatise on the Diseases of the Sheep; being a Manual of Ovine Pathology especially adapted for the use of Veterinary Practitioners and Students. By John Henry Steel. (London and New York: Longmans, Green, and Co., 1890.)

SHEEP and their diseases have had but little attention from the veterinary profession, and consequently this work, just published by Mr. J. H. Steel, one of the most astute and successful veterinary practitioners and teachers of the present day, must be looked upon as the result of an important step in the right direction. Though we admit the author's ability and the probable usefulness of the book, yet the most conspicuous feature connected with it is the evidence which it gives of how very little of a sound, practical, and useful nature is known by scientific men in relation to sheep. No claim is made to originality in the subject-matter produced, and indebtedness to the various authorities quoted is freely acknowledged. There is, however, a distinct want of discrimination between those who are the leading authorities on certain subjects and those who are not. One who could write the three names, Walley, Williams, and Gamgee, in the order presented, shows he has no regard for precedence in virtue of merit. Gamgee in his day was perhaps the greatest genius who had ever adorned the veterinary profession, and Williams is unquestionably the most successful living author of veterinary works of a high order. Errors of an extraordinary kind appear where the writer, who is not himself familiar with the subject, attempts to enlarge upon the statements of others from whom he quotes; for example, in writing of the tick, ked, or fag (*Melophagus ovinus*), he says "the animal buries its head and proboscis in the skin, and once fixed hangs on for months. It is nimble and active, and sometimes as large as a horse-bean." Any entomologist familiar with sheep cannot fail to see that the author has mistaken the grass-tick (*Ixodes*) for the sheep-tick (*Melophagus*), which belongs to a different genus, and has confounded the habits of the two creatures in an extraordinary manner. Apart from errors of this kind the work is far from complete, but if due care were taken to correct mistakes and to consult as additional references such recent works of a superior kind as "The Animal Parasites of Sheep," by Dr. Cooper Curtice, published at Washington by the United States Government during the present year, the second edition might be made a most serviceable, interesting, and valuable volume.

Wild Beasts and their Ways. By Sir Samuel W. Baker, F.R.S. Two Vols. (London: Macmillan and Co., 1890.)

NO one who has read any of Sir Samuel Baker's books of travel will need to be told that he has all the instincts and aptitudes which, under favourable conditions, make a man an eminent sportsman. He is, however, much more than a sportsman; as he himself says, he has never hunted without a keen sense of enjoyment in studying the habits of the animals pursued. In the present work he records some of the experiences he has had in various parts of the world, and students of natural history will find in his narrative much that cannot fail to interest them. The book is not, of course, in the strictest sense scientific; but it has points of contact with science, and these will make it as welcome to zoologists as it is sure to be, for other reasons, to general readers. Sir Samuel confines himself to wild animals which he himself has had opportunities of watching, so that all the incidents and scenes he brings before us have that kind of freshness and vividness which can belong only to descriptions that embody the results of direct personal observation. The work is admirably illustrated by reproductions of drawings prepared by Mr. Dixon.

Properties of Matter. By P. G. Tait. Second Edition. (London: Adam and Charles Black, 1890.)

THIS is a revised and considerably extended edition, and the author has paid special attention to points in connection with which difficulties had been found. Among the more important additions are the results of some of M. Amagat's splendid and hitherto unpublished work relating to the compression of liquids exposed to enormous pressures. This in itself, when completed, will, as the author remarks, "form a singularly interesting and practically new branch of the subject."

Our Fancy Pigeons. By George Ure. Cheap and Enlarged Edition. (London: Elliot Stock, 1890.)

THE title of this book does not give quite an accurate idea of the contents, for in the first part there is a good deal about fishing and things in general, and the third is a collection of "rambling ornithological notes." In the second part, however, the author deals systematically with the pouter and other "high-class breeds," and offers some remarks on minor varieties of fancy pigeons. Mr. Ure is a lively writer, and his facts and opinions are presented as the results of long-continued personal study and experiment.

Alexis and His Flowers. By Beatrix F. Cresswell. (London: T. Fisher Unwin, 1891.)

THIS pretty volume is intended for boys and girls, and, as it is brightly written, ought to be read by them with pleasure. It contains much quaint and interesting "flower-lore."

LETTERS TO THE EDITOR.

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

Dr. Romanes on Physiological Selection.

IN his two latest articles dealing with this subject, Dr. Romanes has made certain statements as to my position in regard to it which call for a brief notice on my part.

In his original paper, and in the summary of it published in NATURE, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection. A few quotations will show this. He says: "It becomes almost impossible to doubt that the primary specific distinction (meaning sterility) is, as a general rule, the primordial distinction" (NATURE, vol. xxxiv. p. 339). Again, he enforces this as against Darwin's view that sterility was a consequence or concomitant of other differences, as follows: "My theory, on the other hand, inverts this order, and supposes the primary distinction to be likewise (in most cases) the primordial distinction" (*l.c.*, p. 363). This is very clear, but to show that he limited the term "physiological selection" to the results supposed to arise from this phenomenon, we have his reply to Mr. Galton, who urged a fact also dwelt upon by Darwin—the psychological disinclination to mate between many varieties—as an important factor in the differentiation of species: "Now I have fully recognized this principle as one amongst several others which is accessory to, although independent of, physiological selection" (*l.c.*, p. 407). A little further on he again states his fundamental fact thus: "If my theory is true, it must follow, as Mr. Galton says, that such unions would be more or less sterile, and, as this sterility is itself the only variation which my theory supposes to have arisen *in the first instance*, *ex hypothesi* we can have no means of observing whether or not the individuals which present this variation 'consort with outsiders,' or with those individuals which do not present it" (*l.c.*, p. 407). As if to leave no possible doubt as to the special point of his new theory, he again enforces it in the following passage: "And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in all other respects resemble its parent stock, and so be able to compete with it on equal terms" (*l.c.*, p. 408).

Now surely all this makes it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, was founded on the supposition of sterility-variation alone, arising in an otherwise undifferentiated species; and he claimed that such variations "cannot escape the preserving agency of physiological selection," and that "physiological selection must be quite as vigilant as natural selection, and it seizes upon the comparatively unuseful variation of sterility with even more certainty than natural selection can seize upon any useful variations" (*l.c.*, p. 364).

These last statements, by the truth of which alone the use of the term "selection" can be justified, I showed by two carefully considered cases to be absolutely unfounded, and the exact opposite of what must really occur (*l.c.*, p. 467; and "Darwinism," p. 182). Having thus proved that "physiological selection," in the only form claimed by Dr. Romanes as original, does not exist, and that the only modes by which degrees of sterility between distinct species can arise are those discussed or suggested by Darwin himself, with the addition of the possible action of natural selection in increasing incipient sterility between slightly differentiated forms, will it be believed that I am accused of having appropriated the theory of physiological selection without acknowledgment! In the *Nineteenth Century* (May 1890, p. 831), Dr. Romanes says of me: "He presents an alternative theory to explain the same class of facts. Yet this theory is, purely and simply, without any modification whatsoever, a restatement of the first principles of physiological selection, as these were originally stated by myself." And now, in the October issue of an American magazine, *The Monist*, he has an article entitled "Mr. A. R. Wallace on Physiological Selection," in which the original main point, of sterility-variations alone leading to and constituting "physiological selection," is almost entirely ignored, and the various modes by which isolation is produced between incipient species or in which infertility arises in correlation with other divergent characters, are all claimed as forming part of the theory of physiological selection. He quotes from "Darwinism" my exposition of the effects of partial infertility arising between "two varieties in process of adaptation to somewhat different modes of life within the same area," to show "how unequivocal and complete is Mr. Wallace's adoption of our theory" (*The Monist*, No. 1, p. 11). "Our" refers to Mr. Gulick, who is taken into partnership by Dr. Romanes. And again he speaks of "the peculiar position to which he has eventually gravitated with reference to my views—professing hostility on the one hand, while reproducing them as original on the other" (*l.c.*, p. 19).

I have here confined myself to showing, by Dr. Romanes's own repeated and emphatic statements, what was the essential and original theory to which he gave the name of "physiological selection." The whole of this special doctrine I have argued against as unsound, because, on close examination, it proves to be quite inadequate to produce any such effects as are claimed for it. Whether I was right or wrong in doing so, I did, as a matter of fact, and do still, wholly reject this fundamental and essential part of the theory—the only part which had even a *primâ facie* claim to originality. I also totally reject the two subsidiary doctrines on which Dr. Romanes lays great stress as adjuncts of his theory—that of the inutility of a large proportion of specific characters, and that of the power of isolation alone "without the aid of natural selection" to produce new species; while, so far as I know, the only points in which I agree with him are those in which we both make use of Darwin's facts and adopt Darwin's explanation of them. Yet, notwithstanding this rejection of all that is special in his teachings, Dr. Romanes has the hardihood to assert that I claim them as my own; that I merely restate his theory "purely and simply, without any modification whatsoever"; and that my adoption of his theory "is unequivocal and complete."

I leave it to others to characterize these extraordinary statements in the terms that fitly apply to them.

ALFRED R. WALLACE.

Attractive Characters in Fungi.

I NOTE in your issue of November 6 (p. 9), Mr. Straton mentions the fact of the common mushroom spores being unproductive until they have passed through an animal host, naming horse, sheep, and oxen, but it appears to me it must be rendered equally fertile after passing through the larvæ of beetles, flies, &c., else how could nurserymen supply spawn with mycelium ready for generation? It is possible, therefore, that though larger animals act very often as hosts to mushroom spores, insects are mainly responsible for their reproduction. The soft spongy nature presents but little resistance to the ovipositor, and most mushrooms if examined in a state of decomposition will be found perforated by maggots, the larvæ of Diptera and Coleoptera.

It is possible that a sustained high temperature is necessary to the first stage of development in fungi, which is admirably attained in the living host, but it is probably immaterial whether the mycelium is developed on the excreta of mammal or insect. Heat is evidently a great factor even in the second stage of germination, as the so-called "spawn" will remain dormant for a long period unless it is applied. I well remember a peculiar case in point. A wild hill-top covered with gorse and bracken was to be taken into cultivation; it had been unturfed, the turves and gorse being piled in heaps and burned on the ground (many acres in extent) now ready for the plough. It was in the month of August. While these heaps were still smouldering, there came two days of heavy rain; immediately after, sprang up like magic an immense crop of mushrooms, chiefly close to the ash-heaps. They were unusually large, and the tops were very brown—scarcely to be distinguished from the bare earth they grew on.

These germs must have been gradually collecting under the turf for years, beaten in by the weather, the moss slowly growing over and hiding them from the air and heat. The removal of the turf exposed them, when, forced by the extraordinary heat of the burning heaps, they suddenly sprang into existence. In after years, when the ground was under cultivation, they were seen no more, for the reason, probably, that when the plant-life was all destroyed, a great part of the insect-life went with it, and thus the means of propagation was lost.

Biarritz, November 23. R. HAIG THOMAS.

P.S.—I have never actually seen cattle or horses eat mushrooms, but that goats certainly eat some kinds of fungi I can state positively, as last year, in Norway, I had an opportunity of personally observing the fact. A party of us were walking through the pine forest; one of the peasants was leading a goat down the mountain from a *sæter* to his farm below. My companions called me to look at the goat, which had stopped in the pathway, and was greedily nibbling at a large piece of sponge-like fungus, such as one finds commonly in the woods. She speedily ate it all up. We expressed some surprise, but the peasants told us goats were very fond of and often eat fungi.—R. H. T.

AS stated by Mr. Cooke (NATURE, November 20, p. 57), there is an apparent contradiction between the impossibility of finding out some process of "impregnation" previous to the formation of the spores in Hymenomycetes, and, on the other hand, the occurrence of forms suspected to be "hybrids."

A very remarkable statement in De Bary ("Morphol. u. Physiol. der Pilze," § 1, p. 2), however, may perhaps afford a clue to the mystery, viz. the occurrence of amalgamations between hyphæ originally produced from distinct spores. Might not such a process as this possibly lead to "hybrids," if those spores belonged to distinct species? W.

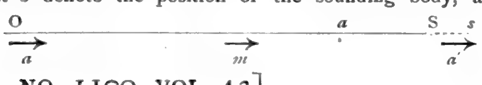
Freiburg, Badenia, November 22.

I WAS shown the other day, in a wine cellar, completely excluding light and fresh air, a remarkably beautiful growth of fungus, covering the wall and floor, to a depth of 4 inches in places, and suggesting cotton-wool in form and colour. When taken up and pressed, it turned brown and emitted the characteristic fungus smell. I should be glad to learn the name, and whether the pure white of the fungus is due to the total exclusion from light. M. H. M.

Doppler's Principle.

THIS subject was referred to in NATURE some months ago, but, although the question is comparatively simple, there is one point of some importance which was not then brought out and to which I have never seen any reference. The change in pitch is, of course, due to the change in the rate at which the cycles of disturbance which constitute the wave-motion fall upon the ear. To determine this change of rate, it is necessary to consider (1) the space occupied by each cycle; (2) the relative velocity of the wave-motion and the observer. Consideration (1) is connected with the velocity of the source of sound, and if wave-length be defined as the shortest distance between two vibrating particles in the same phase, then the space occupied by each cycle may be called the wave-length. If, however, wave-length be defined as the distance which the wave-motion travels through the medium during the "period" of vibration of the sounding body, then the wave-length so defined is unaffected by the motion of the sounding body. It is in connection with this point that there is generally some ambiguity in the usual terms of explanation.

Let S denote the position of the sounding body, and O



that of the observer. If a denote the velocity of the observer, a' the velocity of the sounding body, m that of the medium, and v the velocity of sound, then during the "period" of the vibration of the sounding body the disturbance travels, *through the medium*, a distance $\frac{v}{n}$, where n is the frequency of the vibration. During this period, however, S is displaced to s, a distance $\frac{a'}{n}$; and, owing to the motion of the medium, the disturbance originally starting from S, although traversing a length $\frac{v}{n}$ through the medium, only reaches a point a at a distance $\frac{v-m}{n}$ from S. The distance, $sa = \frac{v+a'-m}{n}$, is thus the actual distance between two particles in the same phase, or gives the *effective* wave-length.

The velocity of the motion through the medium is v, and therefore its velocity relative to O is given by $v+a-m$. Hence, in one second, the number of effective wave-lengths which fall upon the ear is expressed by

$$n' = \frac{v+a-m}{v+a'-m}n.$$

That is, the pitch of the note heard at O is given by n'. This is the formula given by Prof. Everett in NATURE, vol. xlii. p. 81.

Cambridge, November 18. R. W. STEWART.

The Comb of the Hive-Bee.

IN a recent article the Bishop of Carlisle puts forward, as conclusive objections to the perfecting of the cells in the comb of the hive-bee by natural selection: (1) the fact that other kinds of bees continue; (2) that the sterile workers cannot transmit favourable variations.

But (1) other bees, however inferior in comb-making, may have advantages in other respects; thus the humble-bee can reach the nectar of flowers that are not accessible to the common hive-bee. (2) Favourable variations in the workers would presumably or possibly appear in the further swarms thrown off from the hive or home from which these proceeded; and further, seeing that the workers are really females, the queens in the swarms so thrown off may inherit and transmit the favourable tendency. WM. KNIGHT.

Savile Club, 107 Piccadilly, W., November 22.

A Swallow's Terrace?

MR. A. G. VERNON HARCOURT has just shown me, in his boat-house at Cowley Grange, a specimen of swallow's architecture unlike anything I have seen or heard of. The nest, which is itself normal, is placed at the end of a small beam extending from the top of the door to the angle of the building. This beam is about two feet and a quarter in length, and four inches broad. The nest is at the end next the door; the whole of the rest of the surface of the beam is occupied with an adjunct to the nest, which looks as if it had been meant for the family to perch and roost on. It consists, like the nest, of a foundation of dried mud, carefully covered with dry grass; and it is obvious that much care and pains were spent on its construction. Its length (excluding the nest) is nearly two feet.

Mr. Harcourt thinks that the nest was built late last summer, but he did not notice it then, or discover the use of this curious terrace. Can any of your readers parallel or explain it?

W. WARDE FOWLER.

Lincoln College, Oxford, November 20.

Araucaria Cones.

IN answer to the Duke of Argyll's inquiry respecting the coning of the *Araucaria imbricata* in the British Isles, I beg to state that there have been, within my own cognizance, several instances of the same during the last thirty or forty years in this country, notably at Maresfield in Sussex, at Bicton in Devonshire, and especially at Chatsworth. The famous avenue of them at Chatsworth frequently produced cones during the last ten years of the trees' existence prior to 1860, when the memorable severe frost on Christmas Eve completely destroyed the whole avenue, despite the artificial screens of branches of evergreen shrubs that had been annually adopted for their protection from severe winterly weather, so I have been informed by a trustworthy eye-witness, who also stated that the cones produced by the trees in question always proved seedless. The trees, curiously, were all females, and had no opportunity of impregnation. In further reference to the diœcious character of this genus of Conifers, I am informed that the Maresfield trees, as indicated, failed to produce fructiferous cones until males were planted within suitable proximity to them. Pertaining, further, to the sexuality of the Araucaria, I believe that a distinguishing character exists in the size of the foliage, that of the cone-bearer being considerably the larger.

Bearing on the sudden fruition of the Inveraray tree, it may be interesting to relate a parallel case, which occurred upwards of twenty years ago, when I was residing in the neighbourhood of Stratford-on-Avon. A fine specimen of that beautiful Spanish silver fir (*Picea Pinsapo*), on one windy day, became prostrate, and exposed, to my surprise, the greater portion of its main roots in a fungous, diseased condition, thus solving the problem why the tree had for the last few years assumed a stunted growth. Fortunately, however, as two or three of the main roots on one side of the tree remained intact, I resolved to raise it to its former position, after having cut away every vestige of diseased or broken roots; which was successfully accomplished by the aid of a stout rope and pulley-block, and a dozen able men. Subsequently the tree did not appear to suffer materially from the trying ordeal it had been subjected to, and my anticipations of its resuscitation were shortly afterwards justified by a healthy renewed growth, and the interesting appearance, in the course of two or three years, of a crop of beautiful cones, specimens of which I exhibited at one of the Royal Horticultural meetings in 1869, and for which a "Special Certificate of Merit" was awarded. Evidently the cause of this abnormal fruition—as in the case of the Inveraray Araucaria—was owing to arrested growth. In conclusion, I may add that I failed to discover the real cause of the decay of the Picea's roots, but attributed it to something unsuitable in the almost impervious damp subsoil, the fungous condition being only consequential.

WILLIAM GARDINER.

Harborne, Birmingham, November 15.

P.S.—Respecting the sexuality of the Araucaria, it would be instructive as well as interesting could any of your correspondents define any comparative specific character possessed by the plants, such, for instance, as the foliage or general habit, when in their earlier life, and whereby they may be distinguished.—W. G.

EARLY this summer the Araucarias of large size around Terregles House, near Dumfries, were in fruit. Many of the shed cones were lying at the base of the plants. Several years ago I saw a fine Araucaria in fruit in the manse garden, Colvend, Kirkcudbrightshire; but learned from the incumbent that the sight was a rare one. About the middle and end of October, this year, we had numerous trees of the mountain ash from which the leaves had fallen, but which stood glittering, laden with red berries. Clouds of fieldfares arrived, at first noisy and shy, perching on the tops of larch-trees. They devoured these berries, and, getting bolder, invaded my garden, and clustered on a mountain ash in such numbers that there could not be less than 200 at one time. At two visits of one hour each, in one day, every berry disappeared from that tree. Now the flocks of fieldfares are no longer visible, and the berries of the hawthorn and other wild fruit do not seem to attract them, while not a berry of the mountain ash could be picked up for many miles.

JAMES SHAW.

Dumfriesshire.

THE GENESIS OF TROPICAL CYCLONES.

ACCORDING to the views of Dr. Hann, as explained in a previous number of this journal (Nov. 6, p. 15) the storms of the temperate zone originate, not in the convective ascent of warm damp air (an explanation, however, which he appears to admit in the case of tornadoes), but in great vortical movements of the upper air-currents, which commence over the equator as the anti-trades, and set continuously towards the poles, being gradually diverted eastwards in consequence of the earth's rotation. Owing to the spherical form of the earth's surface, these currents become irregularly congested as they necessarily converge on reaching higher latitudes, and thus give rise to anticyclones, or tracts of excessive accumulation and pressure, and to cyclonic vortices in the intervals. Admitting this view as at least highly probable, the question now to be considered is how far similar conditions hold good in low latitudes. Do the cyclones of the tropical zone originate in like manner, or are they not rather primarily due to the conditions of the lower atmosphere, to the production and condensation of vapour over a calm region, and the creation of an upcast current?

In the first place, it is to be observed that in low latitudes those causes which impede the even flow of the upper currents are at a minimum. Their tendency to congestion must vary as the contraction of the degrees of longitude in successive parallels of latitude; and whereas between latitude 40 and 50, for instance, this amounts to 16 per cent. of the length of the degree, and between 50 and 60 to 22 per cent., between 5° and 15 it is little more than 3 per cent. Accordingly, the non-periodic oscillations of the barometer, which, in Europe, frequently amount to an inch in the course of a day or two as cyclones and anticyclones successively sweep past, in the latitude of Madras (13° N.) rarely much exceed a tenth of an inch in the whole course of a month. But cyclones originate certainly as low down as latitude 8°, and instances have been recorded in 7° and even 6°.[1]

On the other hand, the supposed alternative cause, viz. the production and condensation of vapour, is at a maximum in low latitudes, and the facts recorded by Eliot, Pedler, and others who have traced out the early history of Bay of Bengal cyclones, go to show that their formation is determined by the inrush of a saturated current from the equatorial sea, and that this inrush is preceded by at least one or two days of disturbed squally weather in the birthplace of the storm. Moreover, the evident relations of these storms to the features of the terrestrial surface, always in the early stages of their existence, and frequently after they have been maturely developed, seem to admit of no other conclusion than that they are, primarily at least, phenomena of the lower atmospheric strata, even though at a later period the vortical movement may be imparted to the greatly elevated anti-trade, and so be carried forward into higher latitudes. And lastly, as Dr Hann has himself shown, the temperature test, which he rightly appeals to as crucial, and which in his hands has led to the overthrow of the condensation theory of extra-tropical storms, does not fail when applied (as far as the data admit of) to the case of tropical cyclones. On each of these points some further elucidation is necessary.

First, as regards the place of their origin: and in these remarks I shall restrict myself to the storms of the Bay of Bengal and the adjacent Indian continent, which have been more closely studied than those of other tropical seas. A chart given by Mr. Eliot in his recently published "Hand-book of Cyclonic Storms in the Bay of Bengal" shows that they are generated with about equal frequency in all parts of the bay betweeen N. latitudes 8° and 18°. Between latitude 18° and the Bengal coast they are much more frequent, though generally of less intensity. But they are formed very rarely indeed over any part of the Indian peninsula. I can remember but one such case during an experience of many years' daily study of the weather charts. And although they originate somewhat more frequently in Lower Bengal during the height of the monsoon, even these instances are rare in comparison with those of storms generated at the head of the bay during the same season. With but few exceptions, therefore, they are formed only over the sea, and these exceptions are nearly all restricted to the low plain immediately north of the bay. If the original impulse were a vortical movement of the higher atmosphere, it

[1] See the list of storms in Appendix II. to the "Weather and Climates of India."

would be difficult to account for this practical limitation of the storm cradle to the surface of the bay; whereas on the alternative assumption the reason is obvious.

Next, with respect to season and antecedent circumstances. The fierce and destructive cyclones which accompany the changes of the monsoons are generated chiefly in the south of the bay in the spring and late autumn, and further north at the beginning of the summer and in the earlier autumn months; while during the height of the summer monsoon, the less severe storms, which I have elsewhere distinguished as "cyclonic storms," are formed in the extreme north of the bay, and occasionally, though rarely, over the plains of Bengal; in which case they never attain to any great strength. Over the storm cradle at the outset, and everywhere to the north of it, the atmosphere is calm and sultry, or moved only by light variable winds; and at the change of the monsoons, when storms are formed far out in the bay, the atmospheric pressure is nearly uniform all over the bay, and even over the land there are only those slight differences, it may be either of excess or defect, that are due to the normal distribution of the season. Cyclone formation seems to be but little if at all affected by the barometric condition of the atmosphere over the Indian continent. But storms always originate somewhere beyond the northern limit of that saturated equatorial atmosphere which is itself fed by the southern trade winds, and is the reservoir from which is drawn the rainy summer monsoon. In this direction the pressure is always somewhat higher, but until the cyclone has formed, the gradients are gentle.

Thus the average birthplace of storms advances and recedes with the northern limit of the southern monsoon, being always situated beyond it in the region of nearly uniform and relatively low pressure, calms, or light and variable winds, which extends over a greater or less area beyond that limit.

Over the cradle of a storm, the formation of a vortex is always preceded by disturbed squally weather, during which the barometer falls slightly over the disturbed area. In most instances this lasts for two or three days, sometimes longer, and during this period there is but little rain around the coasts of the bay. As this preliminary occurrence of squally weather is a point of some importance, I will quote a passage describing it more fully from Mr. Eliot's recent work:—"The history of all cyclones in the bay shows that they are invariably preceded for longer or shorter periods by unsettled squally weather, and that during this period the air over a considerable portion of the bay is gradually given a rapid rotatory motion about a definite centre. During the preliminary period of change from slightly unsettled and threatening weather to the formation of a storm more or less dangerous to shipping, one of the most important and striking points is the increase in the number and strength of the squalls which are an invariable feature in cyclonic storms from the very earliest stages. First of all the squalls are comparatively light, and are separated by longish intervals of fine weather, and light variable or steady winds, according to the time of year. They become more frequent, and come down more fiercely and strongly, with the gradual development of the storm. The area of unsettled and squally weather also extends in all directions, and usually most slowly to the north and west. If the unsettled weather advances beyond this stage (which it does not necessarily do), it is shown most clearly by the wind directions over the area of the squalls. The winds always settle down into those which invariably occur over an area of barometric depression or cyclonic circulation, or, in other words, are changed into the cyclonic winds of indraught to a central area of low barometer and heavy rain. As soon as the wind directions indicate that a definite centre of wind convergence has been formed in the bay, it is also found that the centre never remains in the same position for any considerable interval of time, but that it moves or advances in some direction between north-east and west, with velocities which not only differ very considerably in different storms, but also at different stages of the same storm."

Such being the facts, as gathered from the detailed study of a great number of storms, their most probable interpretation seems to be somewhat as follows. It may be taken as an established fact that rain is, practically in all cases, the result of the dynamic cooling of ascending air, and that whenever the rain is accompanied by squalls this ascent is irregular and spasmodic. If so, the weather that precedes a cyclonic circulation, as described in the foregoing paragraph, indicates that over a previously calm area the lower atmosphere gradually acquires a spasmodic ascending movement, at first sporadic but gradually becoming concentrated as the influx of the surrounding atmosphere impresses a spiral movement on the general mass. With the influx of the saturated current from the south, this action is greatly accelerated, and the vortical movement which has originated in the lower atmosphere is imparted to the higher atmospheric current, which carries it forwards, at first slowly, and then with increased velocity, as the movement gradually extends to the higher and more rapidly moving current of the general atmospheric circulation.[1] Were the seat of the original disturbance in the bosom of the upper current, it is difficult to see why the disturbed condition of the lower atmosphere should remain stationary during the incubation of the storm, or why it should exist sometimes for two or three days in anticipation of the spiral circulation, which, on this hypothesis, is the determining impulse of the whole phenomenon.

I am not aware that anyone has as yet made a special study of the circumstances under which the storms of the temperate zone originate. Some of them doubtless enter this zone from the tropics. But as the result of a somewhat cursory examination of the Atlantic charts published by the Meteorological Council, others appear to be formed very rapidly either as secondary eddies in the circulation of the North Atlantic atmosphere around Iceland, or in the V-shaped depressions between two neighbouring anticyclones. In neither case does there appear to be that prolonged incubation that characterizes the Bay of Bengal storms; notwithstanding that heat and vapour must be far less active agents in high than in low latitudes. Indeed, this consideration seems to add support to Prof. Hann's views, while it also tends to strengthen the probability that tropical and extra-tropical storms arise from a different class of actions.

Further evidence that tropical cyclones are originally and chiefly phenomena of the lower atmosphere is afforded by the fact that even the most violent storms are often broken up by hills of very moderate height. Notably was this the case with the destructive Backerganj cyclone of November 1, 1876, a very large and violent storm, which nevertheless broke up on reaching the low hills of Tipperah; and perhaps a majority of the cyclones that cross the Coromandel coast from the bay are dissipated by the ghats and hill-groups of the Carnatic, few of which exceed 5000 feet in height. In these cases, a disturbed state of the atmosphere indicated by heavy rain outlasts the cyclone sometimes for two or three days, but the strongly-marked vortical circulation disappears.

It has been suggested to me by Prof. Hann that even

[1] This assumes, of course, that the poleward current of the general circulation exists normally above the calms and variable currents of the monsoons, and such is equally the assumption of the opposite hypothesis. The observations on the progress of the Krakatão dust-cloud indicate only a very rapid westerly current, circulating around the globe in the equatorial zone in August. Those of the movements of high clouds at Calcutta and Allahabad, at a much less elevation than the Krakatão dust-cloud, indicate very variable directions in the summer and autumn, but chiefly southerly at Calcutta, and west or south-west almost exclusively at both stations during the remainder of the year. See the tables in the "Weather and Climates of India," pp. 60, 61.

if the severe cyclones of the transitional periods of the monsoons arise in the way I have above indicated, the milder but more lasting "cyclonic storms" of July and August which are generated in the extreme north of the bay, and which often traverse a great part or the whole of Central and North-Western India before they break up, may nevertheless be formed in the same manner as those of the temperate zone. But this seems to me extremely improbable. With the single exception of the place of their origin, the circumstances of their formation are essentially identical with those of the former class. Moreover, the track which they almost invariably follow seems to be determined by the distribution of the monsoon currents, being along the trough of low pressure which lies between the easterly and westerly branches of the monsoon of Northern India. Although the belt of broken hilly ground running across Central India is generally traversed by the storm vortex, the winds which mainly feed it from the bay have a clear sweep up the great Gangetic plain, and those from the Bombay coast, after surmounting the ghats, have a tolerably unimpeded course across the Deccan plateau, whereas in such cases as the Backerganj cyclone, the whole broad range of the Arakan Yoma, from 5000 to 7000 feet in height, presents an obstacle to the single feeding current from the Bay of Bengal.

I come lastly to the crucial test of temperature—to the question, namely, whether the mean temperature of the air-column in a tropical cyclone is such as to render it specifically lighter than the surrounding atmosphere, and therefore such as to promote an ascending movement. We have indeed in this case no high-level observations to appeal to, such as are furnished to Dr. Hann by the Alpine and other mountain observatories for the storms of the temperate zone. But Dr. Hann has made a rough computation which enables us to bring it fairly to the test, and which in the case of European storms was found to give a result entirely justified by observation. In a paper published in the September number of the *Meteorologische Zeitschrift* he has computed the temperatures of the air-column over a tropical cyclone at different elevations, on the assumption of adiabatic cooling, and has compared these with the average normal temperatures of the atmosphere at the same elevations as deduced from the observations of Newera Eliya, Dodabetta, and Antisana. As the result, he finds that the mean temperature of the former is probably about 2° C. higher than that of the latter, and therefore such as to produce an upward movement of the cyclonic atmosphere, with an acceleration equal to about $\frac{1}{300}$ of the force of gravity. It may indeed be somewhat greater than this, since in his computation Dr. Hann has assumed a temperature of 28° C. or 82°·4 F., and a relative humidity of only 80 per cent. for the atmosphere of the cyclone at sea-level. In point of fact, observation shows that in a cyclone in the south of the bay the temperature at the sea-level is 79° or 80°, but that the air is saturated, or close upon saturation. Making this correction of the data, the mean temperature of the cyclonic air-column will be about 3° C. higher than that of a normal atmosphere, equivalent to an accelerating force of $\frac{1}{200}$ of gravity.

From every point of view, then, whether we regard the place and circumstances of their origin, their behaviour after formation, their physical constitution, or the relative activity of the causes supposed to be concerned in their production, the conclusion seems irresistible that tropical cyclones originate in a manner quite different from that ascribed to the storms of the temperate zone; that they are in their early stages a disturbance of the lower atmosphere; and that the primary impulse is given by the ascent and condensation of vapour.

These remarks apply only to the "cyclones" of the beginning and end of the summer monsoon, and the "cyclonic storms" of the summer months. The storms that traverse Northern India in the winter and early spring, which always travel eastward, and but very rarely descend within the tropic, are of quite a different character, and may not improbably originate in the manner suggested by Prof. Hann.

HENRY F. BLANFORD.

THE DE MORGAN MEDAL.[1]

IN 1869 Lord Rayleigh commenced the long series of papers and memoirs in Mixed Mathematics, which the Council had in view in making the award, with an article (*Philosophical Magazine*, vol. xxxviii., third series) "On some Electro-magnetic Phenomena considered in connexion with the Dynamical Theory," founded on Clerk-Maxwell's celebrated "Dynamical Theory of the Electro-magnetic Field" (Phil. Trans., 1865), the subject being "Examples of Electro-magnetic Problems illustrated by comparison with their Mechanical Analogues." I may add, to complete the key-note, thus struck, of Lord Rayleigh's scientific career, that these theoretical results were followed up in the next year by an account of "An Electro-magnetic Experiment," viz. the magnetizing effect of an induced current as dependent on the self- and mutual inductions of the circuits—an early instance of the author's practice of making theory and experiment, or concrete example, illustrate one another. This combination of experimental with mathematical skill and fertility of resource has been conspicuous in Lord Rayleigh's later memoirs on the "Determination of the Ohm and B.A. Unit of Resistance in Absolute Measure" (Roy. Soc. Proc., vol. xxxiv., and Phil. Trans., vol. clxxiii., 1882.)

Confining myself to those earlier memoirs and papers by Lord Rayleigh in which mathematical investigation predominates, the next which calls for notice is one of considerable length, "On $\int_0^1 Q_n Q_{n'} d\mu$ ($Q_n Q_{n'}$ being Laplace's coefficients of orders n, n'), with an Application to the Theory of Radiation" (Phil. Trans., 1870); in which, instead of the two Q's being multiplied together and integration then effected, the flank of the difficulty is ably turned and the object attained with comparative ease. The mathematical results are illustrated by their application to the problem of "finding the stationary condition when a uniform sphere is exposed to radiation from infinitely distant surrounding bodies."

"A Memoir on the Theory of Resonance" (Phil. Trans., 1871), is the first of Lord Rayleigh's numerous essays in the mathematical theory of sound, which were preliminary, or have been a sequel, to his well-known Treatise, of which the two volumes appeared successively in 1877 and 1878.

The subject of the Memoir is an investigation of air-waves (of the fundamental note) in hollow spaces, whose three dimensions are small in comparison with the wave-length, and which communicate with the atmosphere by necks similarly limited. The theory is shown to be applicable to cases of multiple resonance, as when two or more such hollow spaces are connected by necks and communicate with the external air by necks.

The question of the calculation of c, a factor or coefficient occurring in some of the differential equations, is treated in a noteworthy manner, suggested by the remark that, if the air were replaced by uniform conducting matter of unit specific conducting power, and the sides of the vessel by insulators, c would be the measure of electric conductivity between the interior of the vessel and external space, "an analogy freely availed of, . . .

[1] Address to the London Mathematical Society on the occasion of the presentation of the De Morgan Medal to Lord Rayleigh, November 13, 1890, by the President, J. J. Walker, F.R.S.

much circumlocution being thereby avoided on account of the greater completeness of electrical phraseology."

Having joined our Society in 1871, Lord Rayleigh commenced that series of communications in Applied Mathematics, by which our Proceedings have almost every year since been enriched, with a paper in Physical Optics, "On Verdet's Explanation of Coronas" (vol. iii. p. 267), in which a fallacy of reasoning in his mathematical work is pointed out, but its disappearance in the result explained.[1]

This is the first of many important papers evincing the critical manner in which Lord Rayleigh studies the work of his predecessors or contemporaries, while sympathetically and appreciatingly recognizing the value of their contributions to the progress of Physical Science.

To the fourth volume of our Proceedings was contributed an important triad of papers:—

(1) "On the Vibrations of a Gas contained within a Rigid Spherical Envelope," a problem referred to in the Memoir on Resonance, in which the author indicated the only case of the vibration of air within a closed vessel which had prior to that time been completely worked out, the motion being assumed to be irrotational. The solutions of the differential equations are expressed in terms of Harmonic Functions; and, incidentally, interesting theorems in Operations are proved, if in Laplace's Coefficients μ is replaced by the first or second power of the Operative symbol $1 - d/dy$, the Operand being $1/y$.

I would suggest to those having leisure and taste for such a research the possibility of these theorems being generalized for any positive integral power of the Operator.

(2) The second paper, "On the Disturbance produced by a Spherical Obstacle on Waves of Sound," begins with a series of mathematical lemmas of great interest: the expansion of the velocity potential in a harmonic series, with special consideration of the case of its being finite at the poles, and an independent solution of the case of plane waves; a method leading to an expression in terms of Bessel's Functions of fractional order being explained and applied. A comparison of the two results gives a relation of great elegance between a Laplace's Function of order n and a Bessel's Function of order $n + \frac{1}{2}$.

In an Appendix, the expansion of the Velocity Potential for spherical waves is worked out.

Intimately connected with this second of the three papers are two which appeared in the *Philosophical Magazine* for April and June 1871: "On Light from the Sky, its Polarization and Colour," and "On the Scattering of Light by Small Particles," discussed mathematically. The latter paper, setting out with supposing both density and rigidity of the media variable, shows that there can be no direction in the plane perpendicular to the incident ray in which the scattered light vanishes. Hence, either there is no difference of rigidity (the supposition of the former paper), and the vibrations are normal to the plane of polarization; or no difference of density, and the vibrations are parallel to the plane of polarization—which latter alternative is then disproved.[2]

(3) The third of the triad of papers referred to bears the title, "Some General Theorems relating to Vibrations," with which the Author "had lately become acquainted during the preparation of a work on Acoustics." The first principle proved is that "the natural periods of a conservative system, vibrating freely about a configuration of stable equilibrium, fulfil the stationary condition."

Some applications of the principle are pointed out, and in illustration of one of these—the approximate calculation of a complicated system, but slightly different from one of a simpler nature—the problem of the transverse motion of a stretched string of slightly unequal longitudinal density is worked out. Another instructive example is that of the tones of a square plate when the type of vibration is such that the nodal lines are central and parallel to the edges, the tone being then gravest, and that in which the diagonals are the nodal lines. The former case is here discussed mathematically for the first time, and Chladni's experimental results shown to be confirmed. An important remark on the *possibility* of expansion by Fourier's Theorem, Laplace's Series, or Bessel's Functions, in a large number of cases being inferred from physical considerations, is added.

The second principle is that forces which vary as the component velocities (absolute or relative) of the vibratory motions of the parts of the system may be advantageously treated by the method of Virtual Velocities. The investigation leads to the introduction of a function, F (the "Dissipation Function"), which, like the Kinetic and Potential Energy Functions, is a positive quadratic function of the co-ordinates, and represents the rate at which Energy is dissipated. Its application in the fourth chapter of the Author's great work on Sound will at once occur to those acquainted with it. Finally, a law of reciprocity of a very general character is established, relating to the interchangeability of forces and motions of any two types.

The subject of Reciprocal Theorems in Dynamics has been recently (January 12, 1888) treated in our Proceedings by Prof. Lamb, led thereto by a reciprocal theorem, proved by von Helmholtz in a paper "On Least Action" (*Crelle*, vol. c.), which seems much the same as Lord Rayleigh's, if somewhat more generally expressed.

Arising out of this theorem is "A Statical Theorem," communicated by Lord Rayleigh to the *Philosophical Magazine* (vol. xlviii., 1874), referring to a reciprocal property of a system capable of vibrating, with or without dissipation, if displaced from a position of stable equilibrium. The application of the principle in Acoustics (introduced into the "Theory of Sound") formed the subject of a communication to the Royal Society (Proceedings, xxv., 1876).

In this enumeration of some of Lord Rayleigh's earlier papers should be included two of a pure mathematical character on Bessel's Functions, viz. (1) "Notes" thereon, published in the May number of the *Philosophical Magazine*, 1872, in which their great utility in questions of the conduction of heat or electricity, or of a hydrodynamical nature, with conditions to be satisfied by circular, spherical, and cylindrical surfaces, is pointed out. It is needless to remark how constantly these Functions have since been pressed into service in such cases.

(2) "On the Relation between the Functions of Laplace and Bessel," communicated to our Society (Proceedings, vol. ix., January 1878), in which the Function of zero order is shown to be the limit of Legendre's (P_μ), and the general Function that of an "Associated" Function of Laplace, where n becomes infinite, but n sin . angle cos μ remains finite. A linear relation among three consecutive Associated Functions, which in the limit becomes the well-known one among three consecutive Bessel's Functions, established by their discoverer, follows.

In connexion with the two papers just noticed, allusion may be made to a "Note," also contributed to our Proceedings (June 11, 1874) "On the Numerical Calculation of the Roots of Fluctuating Functions," pointing out how the difficulty of evaluating them when the argument is neither very small nor great may sometimes be met; the

[1] What regretful thoughts are stirred up by the recollection that Clerk-Maxwell joined in a discussion of this paper, commenced by Sir W. Thomson! From 1871 to his death, Prof. Maxwell was a frequent participator in, as well as contributor to, our meetings. "Quis desiderio . . . !"

[2] The volumes of the *Philosophical Magazine* for the same year (1871) contain two other optical papers of great interest:—

"On Double Refraction" (vol. xli.), in which the mathematical treatment of the subject, by supposing density to be a function of the direction of vibration, leads to the replacing Fresnel's ellipsoid by its polar reciprocal. Reference is made to Stokes's Report.

"On Reflexion of Light from Transparent Matter" (vol. xlii.), the object of which is "to see how far the facts might be accounted for by the different hypotheses which have been made as to the condition of the æther in transparent matter."

method, though not limited to, having arisen out of, and being exemplified by, Bessel's Functions.

I pass now to Lord Rayleigh's work subsequent to the award of the medal in 1887, which the Council are enjoined by the rules for their guidance to take into special consideration. In the *Philosophical Magazine* for August 1887 there is a paper "On the Maintenance of Vibrations by Forces of Double Frequency," a sequel to that "On Maintained Vibrations," *ibid.*, April 1883. To this subject Lord Rayleigh tells us his attention had been recalled by Mr. Glaisher's Address to the Royal Astronomical Society in the preceding February, in which he had given an interesting account of the treatment of mathematically similar questions, occurring in the Lunar Theory, to those herein considered, by Mr. Hill, published in the *Acta Mathematica* of the preceding year. The appearance of a "dissipative term" in the equation of motion of the vibrating body prevents it being treated as a special case of Hill's similar equation in the motion of the Lunar Perigee. The results are expressed by determinants, and one of the recent papers of Dr. Muir is referred to, on the relations of the special class of determinants involved to continued fractions. Applications to the case of the vibrations of a laminated medium, in which the mechanical properties are periodic functions of one of the co-ordinates—a problem connected with the colours of thin plates; and to the problem of the stationary vibrations of a string of variable density fixed at two points, follow.

In our Proceedings for November 1887, will be found a paper "On the Stability or Instability of certain Fluid Motions," forming a sequel to a former paper on the same subject in our eleventh volume (1880), to which Lord Rayleigh's attention had been re-directed by a recent work of Sir W. Thomson's. In this the subject is treated with greater generality.

In the following year Lord Rayleigh communicated to our June meeting a short but interesting investigation, "On Point, Line, and Plane Sources of Sound," giving rise to a definite integral the connection of which with Bessel's Functions is worked out by Lipschitz's method (*Crelle*, 1859), referred to in the paper of 1877 on Bessel's Functions.

In the Royal Society Proceedings, December 13, will be found a paper in which Mr. Love's objections (Phil. Trans., 1888, A., p. 545), to the line of argument followed in Lord Rayleigh's paper in the thirteenth volume of our Proceedings (1881), "On the Infinitesimal Bending of Surfaces of Revolution," are replied to with great fulness. The interest aroused by these discussions will be well remembered by those who followed them; and allusion to them has since been frequently made at our meetings.

The same memoir by Mr. Love drew forth another mathematical investigation by Lord Rayleigh (Roy. Soc. Proc., February 26, 1889), "On the Free Vibrations of an Infinitely Long Cylindrical Shell."

At our meeting, April 11, 1889, a paper by Lord Rayleigh was read, "On the Free Vibrations of an Infinite Plate of Homogeneous Isotropic Elastic Matter," a particular case of which "On Waves Propagated along the Plane Surface of an Elastic Solid" had been investigated in a communication to our November meeting of the year 1885.

In the before-mentioned years (1888–89), Lord Rayleigh contributed four important papers in the mathematics of Physical Optics to the *Philosophical Magazine*.

(1) "On the Reflexion of Light at a Twin Plane of a Crystal" (*Phil. Mag.*, September 1888). For the calculation of reflexion at the surface between twin crystals, the electric theory of Clerk Maxwell is found to be the only one satisfying the conditions essential to success—as capable of explaining at once Fresnel's laws of double refraction and those governing the intensity of reflexion when light passes from one isotropic medium to another. Starting with Maxwell's equations connecting the components of the electric displacement and current, magnetic force, electro-magnetic momentum and force, and taking the plane of transition as $x = 0$, Fresnel's tangent formula for isotropic reflexion is obtained; then his law of velocity for propagation in a crystal.

Reflexion at a twin plane is then limited to the cases of the plane of incidence, being (1) coincident with, (2) perpendicular to, the plane of symmetry. In the former case it is shown that the ratio of the amplitudes of the reflected and incident light is equal to the difference of two expressions proved to be equal, whether the vibrations are parallel or perpendicular to the plane of incidence, and no light is reflected whether the incident light be natural or plane-polarized, or elliptically polarized.

The values of the parameters which multiply the exponents in the two reflected waves in the second case are then worked out. Finally, particular cases of these general results are discussed.

(2) "On Interference of Light radiated from Moving Molecules" (*Phil. Mag.*, vol. xxvii. 1889) combats Ebert's conclusions by showing that the maximum admissible retardation is 4·5 times greater than assumed by him (*Wied. Annalen*), whence the width of the spectral lines should be much greater than found, which would involve a blow at the dynamical theory of gases.

(3) "On Complete Radiation at a Given Temperature" (*ib.*), founded on Gouy's "Theory of Irregular Impulses," investigates what type of similar impulses would by their aggregation represent complete radiation. The probability of various amplitudes depends on principles explained in an article in the *Philosophical Magazine*, August 1880, "On the Resultant of a Large Number of Vibrations of same Pitch and Arbitrary Phase." The form of impulse (familiar in the Theory of Errors)—

$$\phi(x) = \text{exponent } (-c^2x^2),$$

which is resolved into harmonic components by Fourier's Theorem, is discussed.

(4) "On Achromatic Interference Bands" (*Phil. Mag.*, vol. xxviii., August and September 1889). A development of notice of in the article "Wave Theory" ("Enc. Brit.," 1888), founded on Cauchy's law of dispersion; with a reference to Mascart, "Achromatism of Interference" (*Comptes Rendus*, March 1889).

These abstracts, imperfect as they are, may serve to convey to those who have not closely followed his work some idea of the great variety of subjects in Mixed Mathematics discussed and advanced by Lord Rayleigh, and on which that distinguished reputation in these domains is founded, their recognition of which the Council of the London Mathematical Society desire to mark by the present award of the De Morgan Medal.

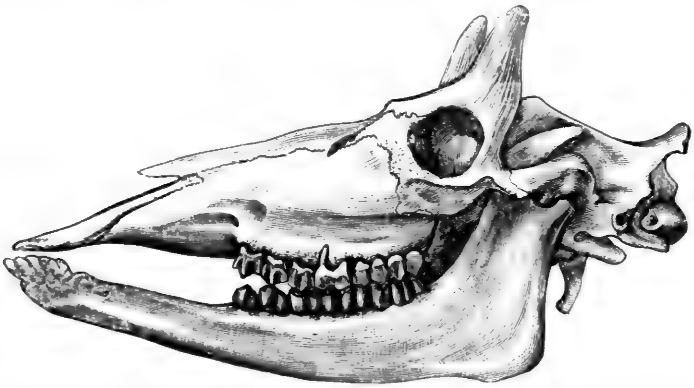
A NEW FOSSIL MAMMALIAN FAUNA.

THE trustees of the British Museum have recently added to the collection at South Kensington a large series of fossil remains of mammals purchased from Prof. C. J. Forsyth-Major, which are of especial interest from several distinct points of view. These remains were obtained within the last two or three years by Prof. Forsyth-Major from a Tertiary deposit in Samos—an island in the Turkish Archipelago, lying immediately opposite the town of Ephesus, and to the south-south-west of Smyrna. This deposit, which has been discovered only quite recently, appears to be absolutely full of the bones of mammals; and in this respect it agrees with the contemporary deposits of the celebrated Pikermi ravine near Athens, the wonderful mammalian fauna of which has been fully made known to us by the labours and writings of Prof. Albert Gaudry, of the Paris Museum, and other palæontologists.

The deposits at Samos have, however, one great advantage over those of Pikermi. Thus, in the latter locality the rock in which the bones are embedded is stained of a brownish-red colour, and very frequently adheres so closely to the bones that they cannot be properly cleaned from matrix; whereas in the case of Samos the rock is of a buffish-white, and can be completely removed from the specimens. This whitish colour of the Samos bones renders them peculiarly attractive objects in a museum; and the contrast between the white bones and the pale-brown of the enamel of the teeth in the magnificent series of skulls now displayed in the Museum is very striking. So well preserved, indeed, are these specimens that many of the skulls are almost as well suited for precise anatomical comparison as those of existing species.

The number of specimens from these deposits acquired by the Museum is no less than 533; the whole of these, with the exception of one bone of a bird, belonging to mammals. As another collection of at least equal extent has been acquired by the Museum at Geneva, the importance of this newly discovered fossil fauna may be readily estimated.

The discovery of this ossiferous deposit, taken in conjunction with that of the equivalent beds at Maragha, in Persia, which were brought to the notice of the scientific world only a few years ago, indicates that there is still hope of much further knowledge of the Tertiary mammalian fauna being eventually obtained by the full exploration of regions lying beyond the European area. As we have already mentioned, the Samos deposits are the equivalents in point of time with those of Pikermi in Attica, and of Maragha in Persia; this identification resting upon the general similarity of the fauna of the three areas, although each locality has some peculiar types not known in the others. The researches of Mr. W. T. Blanford and others have shown that we must assign a Pliocene age to the deposits at Pikermi. And with our present knowledge, the Pikermi fauna may now be traced from Baltavar in Hungary, through Greece, thence to Samos, Persia, Baluchistan, the Punjab, and so to the Siwalik Hills of Northern India, the mammalian fauna of which was the first to be brought to light through the classic labours of Falconer and Cautley. From this fauna, which forms a belt in the regions surrounding the whole of the north-eastern frontier of Africa, it is now pretty certain that the modern mammalian fauna of that continent was derived; and it is noteworthy that the fauna of Samos, and still more that of the Siwaliks, contains the greater number of forms most closely allied to those of Africa. In Pikermi and Samos no true elephants occur, but in the Siwaliks elephants more or less closely allied to the existing African and Indian species are abundantly represented.



Profile view of the skull of *Samotherium*, from the Pliocene of Samos. Much reduced. (From "Guide to British Museum.")

Among the mammals discovered at Samos, a large number are identical with those occurring at Pikermi. Thus, the well-known three-toed horse (*Hipparion*) is especially common in both localities. The rhinoceroses and mastodons likewise appear to have been, in most cases at least, specifically the same. Again, many of the antelopes found at Pikermi, some of which are allied to the African oryx and others to the koodoo, reappear at Samos. A large ruminant from Samos, as yet undescribed, but to which the provisional name *Criotherium* has been applied, appears, however, to be an antelope totally unlike any existing form. In this remarkable animal the horns are set on the extreme vertex of the skull, as in the hartebeest, the gnu, and the ox, but are extremely short, tightly twisted, and bent right in front of the forehead, in a manner totally unlike that found in any existing antelope.

Perhaps, however, the most remarkable of the new mammals discovered at Samos is the large ruminant for which the name *Samotherium* has been proposed. Of the skull of this creature we are enabled, by the courtesy of Dr. Woodward, to give a figure. It will be seen from this figure that the general proportions and contour of the skull are very similar to those of the giraffe; and the molar teeth are practically indistinguishable from those of the latter. The remarkable feature of this skull is, however, the presence of a pair of upright horn-cores, situated immediately over the eyes, and inseparably connected with the frontal bones, of which indeed, as in the antelopes, they form mere projections. This condition is very different from that obtaining in the giraffe, in which, it need hardly be said, the so-called horns are short bony processes, covered with skin in the living condition, and entirely distinct from the frontal bones. The horn-cores of the samothere are, indeed, very similar to those of certain Pikermi antelopes, and were, in all probability, sheathed in horn in the living animal. This ruminant appears, therefore, to indicate a close genetic connection between the giraffes and the antelopes; and since the giraffe itself is very closely allied to the deer, while the extinct Indian sivathere exhibits many points of affinity with the giraffe, but appears to have had deer-like antlers which were never shed, we see how little importance can really be attached to horns and antlers as indicative of want of affinity, or the reverse, between their respective owners. Indeed, there can now be but little doubt that deer, giraffes, prongbucks, and antelopes, are all descended from a common stock; the intermediate and annectant types having mostly died out, although the evidence of their former existence is now slowly but surely accumulating.

The only other mammals calling for especial notice are a species of aard-vark (*Orycteropus*) and a pangolin. The aard-varks, it need scarcely be said, are now entirely confined to Africa and Syria, and the occurrence of an extinct species at Samos may indicate that this group of animals originally reached Africa from the north-east. The fossil pangolin is very considerably larger than any of the existing species, and has been referred to an extinct genus. Since no fossil pangolin has been found in any European deposits, the occurrence of this extinct type is of some interest.

Lastly, we must not omit to mention that the deposits at Samos have also yielded remains of an extinct ostrich, although this species is unrepresented in the collection acquired by the British Museum. The African ostrich is known to have ranged into Persia and Baluchistan within the historic period, and since the genus also occurs fossil in the above-mentioned Siwalik deposits of Northern India, the evidence of its former existence in Samos shows that it once inhabited the whole of that extensive belt of country flanking the north-eastern frontier of Africa, which seems, as we have already mentioned, to have been the original home of the modern Ethiopian fauna.

In conclusion, we venture to express the hope that means will ere long be found by which this magnificent collection of fossil remains will be described and illustrated in a manner worthy of its importance and interest.

R. L.

NOTES.

WE learn that the Weather Service of the United States hitherto under the direction of the Chief Signal Officer, is to be transferred to the Agricultural Department after July 1, 1891. A chief will be appointed at a salary of 4500 dollars a year. The present Signal Corps will be discharged from the army, and will thereafter serve as civilians.

THE first *conversazione* for the season of the Royal Microscopical Society will be held in the Society's rooms, 20 Hanover Square, on the evening of Monday, December 1.

ON Friday last, a marble bust of William Symington was unveiled in the west wing of the Museum of Science and Art, Edinburgh, by Sir William Thomson, who said that Symington was the real discoverer and the practical originator of the steamboat. It was interesting to note that Symington exhibited before the Professors of Edinburgh University a model of a carriage to be moved on the public roads by the power of steam. In 1803, Symington constructed a steamer which took in tow two laden sloops, each 70 tons burden, on the Forth and Clyde Canal. The unveiling of the bust took place amidst loud cheering. Sir R. Murdoch Smith, on behalf of the Museum authorities, accepted the custody of the bust. Amongst those present at the ceremony was Mrs. Dickie, Glasgow, a granddaughter of Symington. The bust is by D. W. Stevenson, R.S.A.

M. TCHIHATCHEF, whose death we lately recorded, left 100,000 francs to the Paris Academy of Sciences, to provide prizes for naturalists who have made noteworthy Asiatic researches.

SOMEWHAT late we learn that Dr. José Jéronimo Friana died at Paris on October 31. The cause of his death is not stated, but the fact that a daughter died in the same house within three days of her father's death points to an epidemic. Dr. Friana was a native of New Granada, born in 1828, and previous to his coming to Europe he was attached to a Survey Commission, and made a collection of more than 5000 species of plants. About the year 1860 he came to Europe with his family, and never returned to his native country. The principal object of his visit to Europe was to determine his plants and prepare a Flora of New Granada. For this purpose he resided partly in Paris and partly at Kew, and spent some years working out his collections. In conjunction with the late Prof. J. E. Planchon, he commenced publishing a "Prodromus Floræ Novæ Granatensis"; but this fell through, partly, we believe, in consequence of a lack of funds, partly in consequence of Dr. Friana's time being occupied by consular and medical duties. For many years he resided wholly in Paris, and filled the position of Consul-General of the Republic of Colombia. Always hoping to be able to resume his favourite botanical pursuit, he kept his large collections at his residence. Among his published botanical work is a monograph of the Melastomaceæ, which appeared in the Transactions of the Linnean Society, and "Nouvelles Études sur les Quinquinas," containing *facsimiles* of Mutis's original drawings. The author visited Madrid, and studied the materials collected by Mutis. Apart from his scientific attainments, Dr. Friana was much loved for his extreme amiability.

HORTICULTURE has sustained a severe and almost irreparable loss in the sudden and unexpected death of Mr. Shirley Hibberd, which we recorded last week. An enthusiastic horticulturist, an accomplished writer, and a fluent and clever speaker, Shirley Hibberd occupied a position that it will be difficult to refill. He was sixty-five years of age, and for more than half that period he has been a constant figure at, and has taken a leading part in, the principal Shows and Congresses in connection with gardening and garden-botany. For upwards of thirty years he was editor of the *Gardener's Magazine*, and he was also the author of many little treatises of considerable literary merit. At the recent Chrysanthemum Show of the National Society of Chrysanthemum Growers, held at the Aquarium, Shirley Hibberd was the most prominent man, both in the lecture-room and as a speaker at the banquet which took place on the Thursday preceding his death. Indeed, it is supposed that he caught a chill in the conference-room, and, bronchitis supervening, the fatal result speedily followed.

IN the Reports on the progress and condition of the Royal Gardens, Kew, from 1862 onwards, there are many useful notes respecting economic and other plants. An index to these notes has just been issued, and will be welcomed by all who have preserved copies of the Reports. The work appears as Appendix III. of the *Kew Bulletin*. In this excellent periodical, to which we have often referred, are now published all notes that may be too detailed for the Annual Report on economic products and plants, to which the attention of the staff of the Royal Gardens has been drawn in the course of ordinary correspondence, or which have been made the subject of particular study at Kew. The preface to the new index concludes with the following statement:—"The *Bulletin*, of which three volumes are already published, and the fourth is in course of publication, may be looked upon as furnishing in a detailed and timely form the special information formerly included in the Annual Reports, but which a necessary economy of space precluded being treated at the length which is possible in the pages of the *Bulletin*. It may be added that the *Bulletin* is published monthly by Her Majesty's Stationery Office, and it may be obtained from Messrs. Eyre and Spottiswoode directly, or through any bookseller."

FROM January 1, 1891, the *Globus* will be edited by Dr. Richard Andree, of Heidelberg, by whose father the journal was established nearly 30 years ago.

THE Council of the University College of North Wales has just purchased for the College library the well-known collection of books belonging to Mr. E. Watkin, of Manchester (formerly of Pwllheli). It consists of upwards of 10,000 volumes, many of which are works relating to botany, chemistry, geology, and other departments of science.

THE recent fire in University College, Toronto, postponed the equipment of the psychological laboratory which Prof. J. Mark Baldwin had in view; but in the plans for the new buildings more ample accommodations are secured. The new laboratory is to be in the restored building in a retired portion of the first floor, immediately over the rooms of the physical department. It will comprise two communicating working rooms, each 16 by 21 feet, a professor's private room, to be used also as a special psychological library under charge of a fellow or instructor, and a dark room available from the resources of the physical laboratory. The first two rooms will be separated by a hall from the latter two. This part of the building will be ready for occupation, it is hoped, in the course of the next academic year. The equipment, apparatus, &c., may be delayed in consequence of the present severe tax upon the resources of the University, but special researches will be prosecuted with the aid of adapted apparatus lent from the very complete collections of the departments of Physics and Biology.

WE learn from the American papers that the provisions of the McKinley Tariff Bill include an exemption of much importance to those engaged in teaching. It permits Universities, Colleges, &c., not only to import books for the institution free of duty, but also for any teacher connected with the institution. All works in languages other than English have been placed upon the free list.

THE efforts which have been made to open commercial communication between England and the heart of Siberia by way of the Arctic Seas have at last been successful. A correspondent of the *Times*, who signs himself, "One who knows all about it," explains the circumstances connected with this remarkable triumph of skill and energy. Two ships and a tug for river work were despatched from London at the end of July and beginning of August. Owing to north-easterly winds the Kara Sea was exceptionally full of ice, so that the ships were detained for some days among ice-floes. Nevertheless, in 39 days the ships and tug reached Karaoul, 160 miles up the Yenissei, without accident. They remained there 19 days, and took 26 days to return. They were thus only 84 days, or two months and 23 days, away from the London Docks. At Karaoul they met the river expedition, which "returned safe to Yenisseisk a few days ago, and is now landing and warehousing there the valuable cargo sent out from England." The same correspondent points out that the real *crux* of the expedition lay in the 160 miles of estuary between Golcheka, at the mouth of the Yenissei, and Karaoul, at the head of the estuary, which the Russian Government had assigned as the port of discharge. Last year the *Labrador* would not ascend to Karaoul, because Captain Wiggins thought there would not be water enough to take him there, and had no steam-launch to enable him to feel his way up. On the other hand, the river ship did not dare to descend because of the gales that then prevailed. This year it was discovered that through the entire estuary there was a channel with sufficient water for ships of any draught, and the ships proceeded up the river to their destination without hindrance. It is unfortunate that Captain Wiggins was accidentally prevented from completing the work with which his name has been so intimately associated, but it was he who showed the way, and to him, more than to anyone, belongs the honour of having provided this new outlet for British commerce. That it may become an outlet of the highest importance is the conviction of no less an authority than Baron Nordenskiöld. In a letter congratulating the promoters of the undertaking, he says:—"I am persuaded that its success will once be regarded as an event rivalling in importance the return to Portugal of the first fleet loaded with merchandise from India. Siberia surpasses the North American continent as to the extent of cultivable soil. The Siberian forests are the largest in the world. Its mineral resources are immense, its climate, excepting the *tundra* and the northernmost forest region, healthy, and as favourable for culture of cereals as any part of Europe." He goes so far as to say that the future of Siberia may be "comparable to the stupendous development which we at present see in the New World."

AT the meeting of the Royal Botanic Society on Saturday, many interesting plants, tropical fruits, &c., grown in the Society's gardens, were exhibited, and the economic value of several were explained by Prof. Bentley, who also noticed a specimen of the fly Agaric on the table from Somerset, presented by Dr. Prior, as containing a very poisonous alkaloid. In some North European countries, where it is common, it is said to be used to increase the intoxicating qualities of liquor. Its name is due to the fact that an infusion in milk is used to poison flies.

PROF. LANGLEY and his assistant, Mr. Very, have been carrying on researches relating to the so-called phosphorescent light of certain insects. They lately brought the subject before the New York Academy of Sciences, and in *Insect Life* (vol. iii., 3) some account of their results is given. The insect principally used in the experiments was the large Cuban firefly (*Pyrophorous noctilucus*). The total radiant heat from the light of one of these insects (heat representing waste) was compared with that transmitted by glass from the nearly non-luminous Bunsen flame, the luminosity from which was very much fainter than that from the insect. The most accurate observations prove that the insect light is accompanied by approximately one four-hundreth part of the heat which is ordinarily associated with the radiation of flames of the luminous quality of those experimented with. Thus Nature produces this cheapest light at about one four-hundredth part of the cost of the energy which is expended in the candle flame, and at but an insignificant fraction of the cost of the electric light, which is the most economic light which has yet been devised. "Finally," the author concludes, "there seems to be no reason why we are forbidden to hope that we may yet discover a method (since such a one certainly exists and is in use on a small scale) of obtaining an enormously greater result than we now do from our present ordinary means for producing light."

THE Division of Ornithology and Mammalogy of the Department of Agriculture of the United States has recently published a Report by Dr. C. Hart Merriam on the fauna and flora of the San Francisco mountain-region of Arizona, in which views are enunciated with regard to the areas of animal and vegetable life on the North American continent different from those usually held. He maintains that there are but two primary life-areas in North America—a northern (boreal) and a southern (subtropical) area, both extending completely across the continent, and sending off long interpenetrating areas; and that the theory commonly accepted by naturalists, viz. that of three life-areas, the Eastern, Central, and Western Provinces, must be abandoned. He further recognizes seven minor life-zones in the San Francisco mountain region, four of boreal, and three of subtropical or mixed origin, the four boreal zones being correlated with corresponding zones in the north and east.

PROF. ALFRED KIRCHOFF, of Halle, contributes to the *Saale Zeitung* an article on "the anxiety with which even scientific men of repute looked forward to the autumn meeting of the International Conference on Degree Measurement, which was lately held at Freiburg." According to an abstract given by the *Times*, Prof. Kirchoff says it had been reported that a series of simultaneous observations, carried on at Berlin, Strasburg, and Prague, went to show that a decrease in latitude was in process, at least in Middle Europe, and further reports from other Observatories showed that a similar phenomenon had been noted in other places in Europe. This implied an alteration in the direction of the earth's axis. That is, the poles and equator, latitude and longitude, are not, as usually assumed, practically fixed data, but are liable to the general terrestrial law of flux. The amount of ascertained decrease of latitude at the end of the six months' period from August 1889 to February 1890 was half a second. But it was notified to the Conference that the Berlin observations for the half-year ending last August showed an increase of latitude amounting to 0·4, or two-fifths of a second. In other words, the fluctuation of the axis is due to a minute oscillation, probably owing to some changes in the internal mass of our planet, and not to be confounded with the precession of the equinoxes.

WE are glad to note that in his address at the opening of the present term at the Johns Hopkins University, Dr. Gilman, the President, was able to speak of the prosperous material condition of the institution after its recent difficulties. Many friends came forward to support the University in its time of trial; and the trustees have been able to make a most advantageous change in a considerable part of the endowment, so that a million of dollars, lately unproductive, now stand invested in an excellent security yielding a fixed and satisfactory income. Dr. Gilman devoted a part of his address to an account of the impressions produced upon him during his recent travels in Europe.

THE new number of the *Internationales Archiv für Ethnographie* (Band iii., Heft 5) opens with a valuable and interesting paper (in German) on Venezuelan clay vessels and figures both of ancient and of modern times, by Dr. A. Ernst, Curator of the National Museum at Caracas. Among the remaining articles is one in English, by Prof. Giglioli, on a remarkable and very beautiful ceremonial stone adze from Kapsu, New Ireland. The illustrations are up to the usual high level maintained by this admirable periodical.

MESSRS. G. PHILIP AND SON have issued a second edition of "The Unknown Horn of Africa," by the late F. L. James. This edition is preceded by an obituary notice of the author, who was killed by a wounded elephant on April 21, 1890, at San Benito, about 100 miles north of the Gaboon River on the west coast of Africa, and within a mile and a half from the shore.

THE Sanitary Institute has published a volume of Transactions, which, as vol. x., continues the series issued by the Sanitary Institute of Great Britain. It contains a full report of the Congress held at Worcester in 1888. Among the contents are papers on the sanitary aspects of the pottery manufacture, by Dr. J. T. Arlidge; on the public health in India, with special reference to the European army, by Sir H. S. Cunningham; sewage disposal, by Prof. H. Robinson; the technical education of plumbers, by Mr. H. D. Matthias; some recent results obtained in the practical treatment of sewage, by Dr. Percy F. Frankland; and the smoke nuisance, under the Alkali Acts, by Mr. H. Fletcher. We may also note a lecture to the Congress, by Sir Douglas Galton; and addresses to the working classes, by Prof. W. H. Corfield, Mr. Henry Law, and Dr. J. F. J. Sykes.

AMONG the contents of the "Papers and Proceedings of the Royal Society of Tasmania for 1889," just received, is an excellent note by Colonel W. V. Legge, R.A., on the Australian curlew and its closely allied congeners. Dealing with the migrations of the Australian curlew, he says it migrates north through the Malay Archipelago, being there met with on passage in Borneo, New Guinea, the Philippines, and other islands; thence northward along the coast of China to Amoor Land, and up to Lake Baikal, in which region it is supposed to breed. In Japan, it has been met with as far north as Hakodadi. New Zealand seems to be its eastern limit.

DR. P. KUBORN, of the University of Liége, has prepared a French adaptation of Prof. D. J. Cunningham's "Manual of Practical Anatomy." The work is called "Guide de Dissection, et Résumé d'Anatomie Topographique." It is published by Marcel Nierstrasz, Liége; and G. Carré, Paris.

MR. WILLIAM HEINEMANN has published the "authorized translation" of Dr. Koch's paper on the cure of consumption—the paper contributed to the *Deutsche Medizinische Wochenschrift*.

A DISCOVERY, which may lead to important results, has been made by M. Chabrié during the course of his experiments upon the properties of the recently isolated gaseous fluorine substitution products of marsh gas. The intimate relation between these bodies and chloroform, and the possibility of their possessing even greater physiological activity, led M. Chabrié to investigate the action of one of them, methylene fluoride, CH_2F_2, upon specific microbes, with the result that in the case of the particular bacillus experimented upon, the gas is found to absolutely destroy them. The bacteria in question, which have formed the subject of these first experiments, were those discovered by M. Bouchard, in 1879, in urine. Two eprouvettes of equal size were taken and filled with mercury over a mercury trough. Equal small quantities of urine containing colonies of the bacteria were introduced into each, and afterwards a mixture of air and methylene fluoride admitted into one of the eprouvettes, and an equal volume of air alone into the other. The two vessels were both maintained at the temperature of the body, 35°, for 24 hours. At the end of this time a few drops of the urine from each of the vessels were introduced into separate flasks containing sterilized culture medium, and both maintained at the same stove temperature for 24 hours, and again for 48 hours. At the expiration of this period the urine which has stood in contact with air alone was found to have given rise to a flourishing colony of the bacteria, while that which had been in contact with the mixture of air and methylene fluoride had not given rise to a trace of a culture. According to MM Albarran and Hallé, twelve hours are ample for the development of this bacillus, hence methylene fluoride had evidently been fatal to the germs. The experiment was again repeated without the use of mercury, in sealed tubes, but with the same result. It appears, therefore, that methylene fluoride possesses the property of destroying the urinary bacteria in question. M. Chabrié has made special experiments in order to determine whether the gas possesses any local irritant action, and the results as far as they go appear to be eminently satisfactory. He is now directing his experiments upon the microbe of the hour, that of tuberculosis, and his results will doubtless be watched with considerable interest. Methylene fluoride is easily prepared by heating silver fluoride with methylene chloride in a sealed tube. M. Chabrié has also succeeded in preparing the higher homologue, $C_2H_4F_2$, ethylene fluoride, by the analogous reaction with ethylene chloride, and is extending his observations to the antiseptic properties of this latter gas. An account of the above experiments is given in the current number of the *Comptes rendus*.

OUR ASTRONOMICAL COLUMN.

A NEW COMET (?).—The following is an account that was received through Reuter, and printed in the *Times* on Monday last, relating to a comet that was visible at Grahamstown. If the statement is correct, we have represented here something that is quite unique in cometary phenomena. It has still to be explained how it was that a phenomenon of such a nature as this was not telegraphed home at the time, and why no confirmation has been received from other sources.

"*Cape Town, November* 5 (*viâ Plymouth*).

"Mr. Eddie, F.R.A.S., reports from Grahamstown that a comet was seen at 7.45 p.m. on the 27th ult., and observed until 8.32 p.m., when the last trace faded in the south-eastern heavens. It travelled from nearly due west around the western and southern horizon at an altitude from about 20° to 25°, and disappeared in the south-east, performing during that very brief interval a journey stretching over at least 100°. It was at its longest fully 90° in length, while in width it did not exceed half a degree, except where it became very faint and slightly spread out at its posterior extremity, and where there were also faint indications of lateral division. The preceding portion was a point in cometary form, but no nucleus could be discerned.

"When first seen, it was inclined at an angle of about 45° towards the south, and was about 30° in length, but as it moved southward it became almost parallel to the horizon, with an altitude of about 20°, till it stretched along the southern horizon an enormously long, narrow, almost parallel, weird-looking riband of gray light moving visibly across the sky. It passed over several bright stars, notably α Centauri and β Argo Navis, but did not appear to dim their lustre. The moon was at the full."

THE STARS 121 AND 483 BIRM.—Mr. Backhouse informs us that these irregular variable stars now appear to be near their maxima. He says:—"They are two of the most splendid red stars that are visible in a moderate-sized telescope; 483 is the deepest-coloured star that I am acquainted with of anything like its brightness, with the exception of R Leporis; 121 is usually nearly as deep, but at present seems not so red as usual." Dunér has described the spectrum of each as Group VI. (Class III.*b*), but, as pointed out by Mr. Backhouse, it is possible that there may be variations with the maximum of luminosity, especially as one of them appears to have changed colour. The positions of the stars are 5h. 39m. 6s., + 20° 39′, and 18h. 58m. 32s., − 5° 51′ respectively.

APEX OF THE SUN'S WAY.—In *Astronomische Nachrichten*, Nos. 2999, 3000, Oscar Stampe gives an extended investigation into the position of the apex of the sun's way. The following are the numbers and groups of stars considered, and the values obtained:—

	Number of Stars.	Yearly Proper Motion.	Co-ordinates of Apex. R.A.	Decl.
Group 1	551	0″·16 to 0″·32	287°·4	+42°·0
,, 2	340	0·32 to 0·64	279·7	+40·5
,, 3	105	0·64 to 1·28	287·9	+32·1
,, 4	58	1·28 and over	285·2	+30·4
		Mean	285°·0	+36°·2

This agrees well with the values found by Boss (*Astronomical Journal*, 213), viz.:—R.A. 280°, Decl. +40°, and does not differ considerably from Struve's values, viz.:—R.A. 273°·3, and Decl. +27°·3, or +37°·7, if Boss's correction be applied. The position of the apex may therefore be taken as somewhere near Vega.

ORBITS OF 61 CYGNI, CASTOR, AND 70 OPHIUCHI.—In the November number of the *Sidereal Messenger*, Mr. N. M. Mann discusses the orbits of these three interesting binaries. The period of 61 Cygni is shown to be 462 years. If this be so, then, taking the parallax of the binary as 0″·55, the mass of the system is 1·45 times that of the sun. In a previous note (*Sidereal Messenger*, vol. ii. p. 22) the author found a period of 1159 years, whilst the combined mass of the connected bodies was concluded to be only one-seventh the sun's mass. New orbits have been calculated for Castor and 70 Ophiuchi. The latter star has made an entire revolution since the first good observation, hence it is probable that the elements computed are correct, and that the places given may be relied upon for many years.

TWO NEW COMETS (*e* AND *f* 1890).—Dr. Copeland announces, in *Edinburgh Circulars* Nos. 10 and 11, the discovery of a rather bright comet, by Prof. Zona, at Palermo, on November 15, at 10h. 24·1m. local time. Its position then was R.A. 5h. 35m. 54·8s., Decl. N. 33° 23′; daily motion *minus* 5m. 32s., and *plus* 17′. This comet was observed at Kiel on the following day at 9h. 1·1m. in R.A. 5h. 30m. 46·3s., and Decl. N. 33° 37′ 6″.

Dr. Spitaler discovered a faint comet, not identical with the above, at Vienna, on the 16th inst., its place at 16h. 32m. local time then being R.A. 5h. 27m. 16·93s., Decl. N. 33° 37′ 16″. It was in the constellation Auriga, and moving slowly towards the north-east, like Zona's comet.

THE STAR D.M. + 53° 2684.—The Rev. T. E. Espin announces (*Wolsingham Circular* No. 28) that this star, 710*a* in the Espin-Birmingham Catalogue, R.A. 21h. 37m. 6s., Decl. + 53° 49′ (1890), was observed in the spring as 7·5–8·0 magnitude, but on November 15 was only of the ninth magnitude. The star is very red, with a magnificent spectrum of the third type (Group II.).

MATABELELAND.

AT the meeting of the Royal Geographical Society on Monday evening, Lieutenant E. A. Maund read a paper on Matabele and Mashona Lands. Mr. Maund was with Sir Charles Warren in South Africa in 1885, when he traversed and reported on Matabeleland. Since then he has spent much time in the country, accompanying Lobengula's two envoys to England about two years ago. He has thus had exceptional opportunities of observing both country and people, and moreover has had access to the official reports of Colonel Pennefather, the leader of the Pioneer Expedition into Mashonaland. After some introductory remarks, Mr. Maund proceeded to give a description of the country:—

"The physical features noticeable in Bechuanaland extend to the high veldt plateau of the Matopo Range, formed by vast sand-belts running east and west, varying in breadth from a few thousand yards to 50 miles, and in elevation, the crest above the trough, from a few feet to several hundred. These belts carry good grass and bush with camel-thorn trees, the bush being invariably thickest on the crest, but necessarily lacking a surface water-supply. This marked feature extends, with a few accidental variations caused by the outcropping of granite, limestone, and basaltic hills, probably from Namaqualand and Damaraland on the west to the Basuto Transvaal and Mashona Mountains on the east, and beyond the Zambezi northwards.

"The cause of these mysterious sand-belts suggests a problem in physical geography which must be left to geology to decide. They must have been raised in their present wave-like formation either by the aid of water or by a constant and powerful wind. The theory that this part of Africa was an elevated basin, which has gradually drained Zambeziward, is the most acceptable, as in the greatest depression about Lake Ngami and along the fertile valley of the Chobe there is still abundance of water. The continual washing backward and forward of the water has disintegrated the old red sandstone upper crust, and left the red sand in this formation like, on a small scale, the sand-ridges left on our sea-shore by the receding tide; while the kopjes of granite, which all have one form, stand out like rocks at low water.

"These kopjes are rocky hills, with the summits apparently denuded, leaving a flat table-top with short cliff-like edge, the *débris* having fallen in slopes at an angle of 45°, as though crumbled off as the tide fell. Beneath the sand formation is generally to be found a limestone sedimentary crust, which in the Kalahari undoubtedly preserves the water underneath from evaporation. Thus at a fountain near Vryburg, between Motito and Takoon, 20 feet beneath the surface there is a running stream 57 feet deep, doing no good to the soil, simply because it wants man, aided by science, to prevent its thus running to waste. The sandstone conglomerates at Kanje and Molopolali, and the banket formation in Matabeleland, were possibly formed by infiltration during this water age. The results of its energetic action is seen in the Matopo range, where you find hills formed of a single block of granite, looking in the distance like our Downs, but on closer inspection this gentle slope is rounded off and polished by the action of the sand-laden water. Detrition has made it as smooth as the shingle-pebbles on our shores. These hills are a favourite haunt of baboons, as immediately they are disturbed they scamper over the steepest and roundest hills, where you cannot follow them. There is apparently no glacial action, but *moulins* I have frequently found of all sizes in the smooth surface, often with the rounded boulder *in situ*. Indeed, for a long time, until I found them large and the boulder there, I had taken them for old Mashona mills, either for crushing corn or quartz, and subsequently I found these people do utilize the smaller for the former purpose. Geologists now by a closer examination will doubtless come across fossils in the limestone crust and sand, which will decide the question as to there having been a large lake since dried up, or one gradually run off, owing to a breach having been made through the outer rim by some convulsion where the Zambezi now flows out. This lake theory was a favourite speculation of Livingstone.

"With regard to the vegetation being thickest on the crest of the belts, I can only suggest that whatever moisture falls quickly finds its way to the valleys; consequently the grass grows more luxuriantly there. The grass in these valleys, after good rains, is often 4 to 6 feet high, and, as the natives yearly burn the grass when it is driest, it naturally follows that the fire is fiercest in the bottoms than over the crest, where grass is sparse from lack of moisture. Bush and trees perish in the dells, but live through the ordeal above, and often ultimately become so thick as to be impenetrable. It is on the high veldt among the Mashona hills that the rich reefs lie, once so well worth working in prehistoric times, as is evidenced by the old workings to be found all over the country; while the rich watered valleys, from whose streams the natives now wash their quills-full of gold, are capable of raising crops and feeding cattle for the support of a large European population.

"Before going into details I would draw your attention to the map of Matabeleland and Mashonaland. It practically lies between the parallels of 16° and 22° S. lat. and the meridian of 27° to 23° E. long., and is certainly the most promising country for colonization in South Africa. Compared with the country south of it, Matabeleland is like Canaan after the wilderness; lying high, generally healthy, and very rich in minerals—gold, copper, and iron having been extensively worked by the ancients with their rude appliances. Its numerous rivers are either running, or have plenty of water in them. The soil is rich and admirably adapted for corn; cattle thrive, and there is an abundance of grass and wood. White children can be reared in the country, which is a *sine quâ non* if it is to be successfully colonized by white men; and, above all, it is sparsely populated.

"The country dominated by the Matabele is nearly as large as Germany, while the territory actually occupied by them is very small, and would compare about as Bavaria does to the German Empire. Their kraals occupy the plateau forming the watershed between the Zambezi and the Crocodile Rivers. They are a Zulu military organization, occupying a rich country which they have depopulated, and live under a despotism of the worst kind. The population may be estimated at about 150,000, and it has become a mixed people of Zulus, Bechuanas, Mashonas, and Makalakas from the incorporation of conquered elements. Their fighting strength is probably not over 14,000 to 15,000 men."

After referring to the history of the Matabele, and to King Lobengula, and giving some account of the visit of the envoys to England, Mr. Maund went on to speak of the country which has just been opened up by the Pioneer Force.

"The country about to be opened up for colonization is," he stated, "an extensive plateau, on the water-parting between the Zambezi and the Crocodile Rivers. There are no mountain peaks. To the east the slope of the land is abrupt and the country broken, many of the hills isolated and very conspicuous, while to the north-west it falls in gentle undulations. The plateau is furrowed by many considerable rivers, and their numerous tributaries. The climate in these highlands, which vary from 3000 to 5000 feet above sea-level, is far more healthy than the now well-colonized sea-board of South Africa. The seasons are well marked, and the rainfall good. For eight months, from April to November, the air is particularly dry and salubrious, and compares well with the Free State. During and just after the rains one must be careful, as in all tropical climates. But, with proper precautions, dwellings placed high, and above exhalations from the marshes left by the subsiding rivers, and above all a judicious abstinence from alcoholic drinks, the new mining and farming communities will be as healthy as are the missionaries who have lived so long there with their families.

"Here let me pay a tribute to these silent workers, whose genial hospitality and kindly attention in case of sickness is bestowed on travellers throughout Africa. In Matabeleland as elsewhere they have been the pioneers of civilization. A heart-breaking up-hill work has theirs been for the past five-and-twenty years among the truculent Matabele, and though their converts are few, their example is beneficial to whites and blacks alike. They have built comfortable brick houses, laid on water from brooks and springs, and irrigated gardens which show the capabilities of the soil. The King, it is true, is the only one at present who dares copy them: he has a large commodious brick house put up by their builder; he has too an irrigated garden after their pattern. Now, let us hope, their harvest will come, for with the advent of a white population the old order of things will quickly change in Matabeleland. The example of their health will also be an incentive to our countrymen to house themselves as quickly as possible, or we shall have direful stories of fever, simply resulting from a lack of those comforts to which they have been used, and which up here will be a necessity.

"During the last rainy season, in the months of November, December, January, and February, the rainfall in the neighbourhood of Baluwayo amounted to upwards of forty inches. Like all tropical rains they are not continuous, but come in terrifically heavy thunderstorms with hot sunshine between. At this time the King is very busy with his witch doctors, rain-making; often painted with medicine charms in bands like a tiger, or making a dreadful concoction, called by the traders 'hell broth,' to please his credulous people, who come to beg rain for their gardens.

"The months of September and October, before the rains, are the hottest in the year. All vegetation appears dried up, and the grass lands are burned off by the natives. Cattle grow thin, and are sent off low down the rivers to find grass and water. The natives have, of course, no knowledge of how to store their generous rainfall. In September I have registered a maximum in the shade ranging between 105° and 111° F., but the atmosphere is so dry that it is more easily supported than 85° near the sea-coast, where the air is saturated with moisture. The evenings and mornings are delightful, and on this high ground the heat is never enervating. During the winter months, May, June, and July, it is very cold at night in these highlands. Even on the Macloutsie River, at an elevation of 2500 feet, I have registered 15° of frost at night, with the thermometer ranging up to 80° in the day (observed with instruments registered at Kew). Mealies—that is, Indian corn—put in soak for the horses over night, have been frozen hard in the morning.

"Notwithstanding this great variation in temperature, the dry season is particularly healthy. What, however, braces the white man withers up the unclothed native. Trek oxen suffer too from this cold, and the dryness of the grass. By these remarks I wish to convey the fact that with ordinary care this country is admirably adapted to colonization by us Anglo-Saxons. Englishmen have lived up there for the last twenty years, and, what is more essential, traders and missionaries have reared large families. There is not the necessity for sending them home as with Indian children. Neither need men, as on the west coast, return home periodically, in order to recruit. Here they may make a permanent home."

After speaking of the immense agricultural capabilities of the country and of its mineral riches, Mr. Maund gave some account of the particular district through which the Pioneer Force passed:—

"Passing out of Khama's country, the B. S. A. Company's expedition found a fair agricultural country, rising only 500 feet in the 150 miles between the Tuli and Lundi. The former river is 400 yards wide at the drift. Half a dozen new rivers, whose euphonious names I need not trouble you with, are reported as running south-east to the Crocodile. At first the road led through a bush and mopani veldt, while the latter 90 miles consists of grazing flats interspersed with granite and sandstone kopjes. It is sparsely populated by Makalakas and Banyai, who are tributary to Lobengula.

"After the Lundi, the elevation gets sharper and the country more difficult; there is a rise of 1500 feet in less than 65 miles to the top of the Providential Pass, the only apparent pass (and that 8 miles long) leading from the low to the high veldt. At the Inkwe (? Tokwe) the height above sea-level is 2700 feet. This is a rapid river with water 50 yards wide and 3 feet deep, even in the dry season. The formation here changes from granite to slate, and the gold indications are very good. We are now among the ancient workings of Benmatapa, Monomotapa, or Quiteve. Twelve miles east in the mountains are the grand ruins of its ancient capital, Zimbabye, or Zimboe.

The many and vast remains of ancient buildings all point, from their propinquity to old workings, to an extensive gold industry, when the means of extraction were crude as compared with modern appliances. The country gradually rises into an undulating plateau ranging from 4530 to 5000 feet above sea-level, with park-like scenery, the eastern edge breaking away into rocky gorges, supplying many tributaries to the Sabi. The water-shed between this river and the Zambezi's tributaries is often very narrow; 100 yards would sometimes only separate the streams running to the two basins. There are apparently no inhabitants on this plateau south of the Hanyani, so cruelly have Matabele assegais done their work.

"The bush is thick and the land boggy at the head waters of these rivers, but beyond the Umfuli there are plains from which Mount Hampden rises, stretching away to the head waters of the Mazoe. The neighbourhood of Fort Salisbury is well wooded, and the petty tribal chiefs welcomed the English force, as promising them security. Prospectors are reporting favourably from all directions, and find old workings wherever they go."

Mr. Maund concluded by referring to the famous Zimbabye ruins, which he seems inclined to think are Phœnician, or at least early Arabian. But Mr. Theodore Bent, who spoke after the reading of the paper, was strongly of opinion that they are of Persian origin.

THE CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES OF THE BRITISH ASSOCIATION.[1]

At the first Conference, held on September 4, the chair was taken by Mr. G. J. Symons, F.R.S. The Report of the Corresponding Societies Committee was taken as read. Subjects of interest to the Corresponding Societies were then discussed in the order of the Sections of the Association.

Section A.

Temperature Variation in Lakes, Rivers, and Estuaries.—The Chairman stated that many thermometers had been distributed, and much information had been collected during the year.

Mr. W. Watts stated that he had been conducting temperature observations in two large reservoirs belonging to the Oldham Corporation, the results of which were included in the Report of the Committee.

Mr. Cushing presented a report of weekly temperature observations taken in the River Wandle, in Surrey. The temperatures were taken between 3 and 3.30 p.m. on Sunday afternoons, and extended from October 1888 to February 1889. The observations were taken at ten different stations. The records were accompanied by a statement of the mean weekly shade-temperature, and the rainfall for the previous week. The observer, Mr. F. C. Bayard, was willing to continue the work.

The Chairman stated that he had recently been reducing experiments on evaporation which had been made for several years at Strathfield Turgiss, in Hampshire, in which the ordinary small evaporators had been compared with a tank 6 feet square and 2 feet deep. Evaporation from the tank averaged 15 inches per annum, while the smaller evaporators (owing to the high temperature of the water) indicated considerably in excess of the truth.

Meteorological Photography.—Mr. Hopkinson alluded to the success which had been achieved by the Committee on Geological Photography, and pointed out the growing importance of photography in other branches of scientific research. He then suggested the appointment of a Committee on Meteorological Photography. Photography, he said, could most advantageously be applied to the investigation of meteorological phenomena, such as the forms of clouds, lightning-flashes, and the effects of storms. The Committee would collect the photographs and keep a register of them. The study of the forms of clouds would be more satisfactory if undertaken by a comparison of photographs than of drawings; methods of overcoming the difficulty of photographing light clouds in a blue sky might be investigated; and the various phenomena connected with lightning-flashes would be a most interesting field for research. Photographs showing the effects of storms should be secured as soon as possible. If a Committee were formed, Mr. Symons and Prof. Meldola would consent to serve on it, and Mr. A. W. Clayden would be willing to act as Secretary.

After some discussion, it was decided that application should be made through the Committee of Section A for the formation of a Committee on Meteorological Photography, and that the application should be supported by a recommendation from the Conference of Delegates.

Section C.

Sea-coast Erosion.—Mr. Topley asked assistance for the Committee investigating this matter.

Erratic Blocks.—The Rev. E. P. Knubley stated that the Yorkshire Naturalists' Union had been recording erratic blocks satisfactorily; twenty-five reports had been presented.

Geological Photography.—Mr. O. W. Jeffs stated that, through the action of the Conference of Delegates at the previous meeting of the Association, a Committee had been appointed for collecting and reporting on geological photographs. Much assistance had been rendered to this Committee by delegates from the Corresponding Societies, many of whom had sent photographs, or lists of those that had been taken. The photographs had been arranged in order to select those which illustrated well-defined strata or sections. The Report showed that many of the counties of England and Wales were as yet unrepresented, and he asked those delegates who had not yet done so to bring the matter before their Societies, and to interest photographers in the work. The object of the Committee was to secure a series of photographs illustrating the features which geologists thought best worth recording in their respective localities. The counties from which photographs had been received were: Dorsetshire, Cornwall, Devonshire, Isle of Man, Kent, Lancashire, Montgomeryshire, Nottinghamshire, Suffolk, Shropshire, and Yorkshire; there were also a few from North Wales, Scotland, and Ireland. The Yorkshire Naturalists' Union had contributed over 100. A large number of the photographs were exhibited in the room of Section C.

Prof. Bonney testified to the zeal and energy of Mr. Jeffs as Secretary to the Committee, and suggested that, when a large collection of photographs had accumulated, some of the more typical examples of geological phenomena might be enlarged for publication.

Mr. Jeffs suggested that the Committee might make arrangements with some photographer for preparing lantern-slides from the photographs for the purpose of illustrating lectures.

Mr. Wm. Gray thought that it would be an advantage if each delegate were appointed the local representative of the Committee in his own district, and authorized to collect the photographs; he would be willing to act in this capacity for the north of Ireland.

Prof. Meldola pointed out that, in taking photographs of geological sections, in which differences in the strata were often indicated only by small differences in colour, it would be an advantage to use orthochromatic plates.

The desirability was then discussed of adopting some means by which members of the British Association, and those who assisted in the work, might be enabled to procure copies of the photographs, either as lantern-slides, prints, or enlargements, and various suggestions to this end were made.

Section D.

Disappearance of Native Plants.—Prof. Hillhouse stated that the third Report of the Committee on this subject had been confined to the north of England, the Isle of Man, and to a few records from South Wales. The bulk of the material had been obtained directly by correspondence with the local Natural History Societies, especially the Yorkshire Naturalists' Union. Next year's Report would probably deal with the whole of Wales, and possibly adjoining counties and the south-west of England. He then gave a *résumé* of the Report, stating that it contained an account of the more or less complete disappearance from certain localities of about seventy species. In some cases the disappearance was due to natural causes, but in most to the action of man.

Mr. Hopkinson stated that nearly the whole of the ferns in his district (St. Albans) had disappeared within the last twenty years. He attributed this to London collectors and dealers, and added that there was danger of even the primrose being exterminated from the neighbourhood of London, as the roots were taken there by cartloads every year.

Mr. M. B. Slater suggested that the best plan to obtain rare plants for cultivation would be to procure a little ripe seed and

[1] Abridged from the official Report which has recently been issued to the Corresponding Societies.

to try to raise it. He had by this means under cultivation some of our rarest British plants.

Investigation of the Invertebrate Fauna and Cryptogamic Flora of the Fresh Waters in the British Isles.—The Rev. E. P. Knubley stated that a Report of the Committee for this purpose had been presented, and that the Yorkshire Naturalists' Union had been steadily working at the subject during the past year.

SECTION G.

Flameless Explosives.—Prof. Lebour stated that the North of England Institute of Mining Engineers had continued experimenting on this subject; the South Wales Society had already helped; and one or two smaller societies had promised assistance. The result of the joint work of the Committee which had been appointed would soon be published.

SECTION H.

Catalogue of Prehistoric Remains.—Mr. Kenward said that the Birmingham Philosophical Society was recording the few ancient remains in its district. He had worked himself, and had induced others to promote the suggestions made at the Conferences at Bath and Newcastle, as well as to assist in carrying out the archæological survey proposed by the Society of Antiquaries.

Mr. Gray stated that the Belfast Naturalists' Field Club had taken the matter up, and would continue to co-operate with the Committee of the Association.

The Chairman remarked upon the advantage of being able to have at hand for reference the publications of the local societies collected by the Corresponding Societies' Committee for the purpose of preparing the catalogue of papers which formed part of their annual Report, and also called attention to the fact that a few of the older and well-known local societies had not yet become enrolled as Corresponding Societies.

The Second Conference took place on September 9, Mr. G. J. Symons, F.R.S., in the chair.

SECTION A.

Temperature Variation in Lakes, Rivers, and Estuaries.—Prof. Meldola read a communication from Dr. H. R. Mill, the Secretary of the Committee, on this subject, thanking the local societies for their assistance, and stating that the work of observers who are members of such societies is, as a rule, more regular and more accurate than that of isolated volunteers. It was desirable, he said, that the societies already assisting should continue to make observations for another year with as much regularity as possible. Additional observers were not required, as sufficient data for the purposes of the Committee were in course of being secured.

Meteorological Photography.—Mr. Hopkinson reported that the formation of the proposed Committee on this subject had been sanctioned by the Committee of Section A, and that the form of application had been forwarded to the Committee of Recommendations.[1]

SECTION C.

Prof. Lebour, representing the Committee of this Section, brought under the notice of the delegates the following Committees recommended for appointment or reappointment:—

(1) *Erratic Blocks.*—The co-operation of the Corresponding Societies which had not yet taken part in the observations was invited.

(2) *The "Geological Record,"* the continuation of which had been recommended, with a grant.

(3) *Underground Waters,* of the work of which Committee the Secretary, Mr. De Rance, would speak.

(4) *Exploration of Oldbury Hill.*—A Committee had been recommended for excavating this ancient earthwork, which was near Ightham, in Kent.

(5) *Geological Photography.*—This Committee, of which Mr. Jeffs was Secretary, had been recommended for reappointment.

(6) *Northamptonshire Lias.*—A Committee for collecting and registering the fossils of this formation had been recommended for appointment, and excavations had already been commenced.

[1] The Committee, consisting of Mr. Symons (Chairman), Prof. Meldola, Mr. Hopkinson, and Mr. Clayden (Secretary), was duly appointed, with a grant of £5.

(7) *Sea-coast Erosion.*—This Committee had been recommended for reappointment.

(8) *Registration of Type-Specimens.*—A recommendation had been sent in for the appointment of a Committee for reporting on type-specimens in museums, an important subject, in which great assistance might be rendered by local societies.

(9) *Earth Tremors.*—This Committee had been recommended for reappointment, with Mr. Davison as Secretary.

(10) *Exploration of Elbolton Cave.*—A Committee had been recommended for the exploration of this cave, which was near Skipton, and in which relics of human occupation had already been found.

(11) *Source of the River Aire.*—The object of the Committee, recommended for appointment for the purpose of investigating this question, was to ascertain, if possible, by means of the coal-tar colouring-matter, fluoresceïn, whether the water which flows out of Malham Tarn and disappears down a "water-sink" to the south of the tarn, is the stream which emerges at Malham Cove or Aire Head, or at both these places. The use of the dye for this purpose had been suggested by Prof. Meldola to Prof. S. P. Thompson, and the latter had brought the subject before Section C. It had been suggested that the method might be found generally useful for investigating the course of underground waters, as a very small trace of the dye produced an intense green fluorescence.

Mr. De Rance, who also represented Section C, remarked on the work of the Underground Waters Committee, that its objects were to study underground water with a view to supply from wells and springs. Questions were asked respecting the quality, quantity, and level of the water. The Committee wished for records of water-level extending over long periods of time, and especially to secure old records. The work of the Coast-erosion Committee had been carried on with important results, and much information had been derived from a study of old charts. The Committee on Erratic Blocks, of which Dr. Crosskey was the Secretary, was appointed in 1871 with the object of recording the exact positions of the more important boulders, and of entering these positions on the Ordnance maps. It was important to have a microscopical examination made of sections of chips from the boulders, so that their probable sources might be ascertained. Another point was that the boulders should not be left to the mercy of the stone-breaker, but should be preserved.

Prof. Meldola stated that he had been requested by Dr. Crosskey to thank the Corresponding Societies for the aid they had already given, and to express the hope that their assistance would be continued.

SECTION D.

Phenological Observations.—Mr. Symons introduced this subject, which, he said, might perhaps be considered to have originated with Gilbert White, but received little attention in England until 1874, when the Meteorological Society obtained the assistance of delegates from several Natural History Societies, who held a number of meetings and drew up a Report. Plants, insects, and birds were referred respectively to the Rev. T. A. Preston, Mr. McLachlan, and Prof. Newton. Of plants, 71 species were recommended for observation, of insects 8, and of birds 17. From 1875 to 1878 the Rev. T. A. Preston prepared, and the Royal Meteorological Society printed, annual Reports embodying the results obtained. Mr. Preston finding it impossible to continue the work, Mr. E. Morley took it up and prepared the Report for 1889. On his suggestion the list had now been reduced to 13 plants, 3 insects, and 5 birds, and the Council of the Royal Meteorological Society desired to enlist as many observers as possible, and to obtain the assistance of the Corresponding Societies of the British Association.

A discussion ensued as to the suitability for observation of the species selected. In concluding it, Mr. Symons said that he had brought the matter forward more as a meteorologist than as a naturalist, and he thanked the delegates for their suggestions.

SECTION E.

Teaching of Geography in Elementary Schools.—Mr. Sowerbutts said that he had undertaken to draw up for Section E a Report on this subject with reference to the action of local authorities, in order to make known how far the Government grant for technical education or allied purposes was made use of for the teaching of geography, and he asked the delegates to assist him by sending in School Board Reports, &c.

SECTION H.

Committee of Aid for Anthropological Excavations.—Dr. Garson called attention to the existence of a Committee of Aid formed by the Anthropological Institute for the purpose of aiding by direction or otherwise the exploration of ancient remains, the Chairman of this Committee being General Pitt-Rivers, the Inspector of Ancient Monuments. Local societies, he said, would find it to their advantage if they would report to this Committee when they were desirous of undertaking explorations.

Prehistoric Remains Committee.—Mr. J. W. Davis said that this Committee, of which he was Secretary, wanted a record of everything that had reference to prehistoric man, his dwellings, implements, pottery, &c.

A discussion then took place with reference to the best method of imparting to the Corresponding Societies through their delegates a knowledge of what had taken place at the Conferences. Mr. Hopkinson suggested that each delegate should read a paper before his Society, giving an account of the work taken up by the various Committees, and that this paper should be published by that Society, so as to be accessible to every member of it. He distributed amongst the delegates a paper on the work of the Committees of the Association which he had brought before the Hertfordshire Natural History Society. Another question raised was the advisability of in some way bringing into relationship with the British Association certain societies which did not come up to the standard of excellence requisite for enrolment as Corresponding Societies.

On the motion of Prof. Lebour, seconded by Mr. J. W. Davis, a vote of thanks was passed to Mr. Symons, Chairman of the Conference, and to Prof. Meldola, Secretary.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Love, Fellow of St. John's College, has been elected Chairman of the Examiners for the Mathematical Tripos, Part I.

Prof. Darwin, Prof. J. J. Thomson, Mr. Pendlebury, St. John's, and Mr. Lachlan, Trinity, have been appointed Examiners for the second part of the Mathematical Tripos.

Mr. E. A. Parkyn, Christ's, and Mr. M. C. Potter, Peterhouse, have been appointed Lecturers in Science at affiliated lectu re-centres.

Scholarships and Exhibitions in Natural Science will be open for competition to non-members of the University in December and January next at the following Colleges: King's, Jesus, Christ's, St. John's, Trinity, Emmanuel, and Sidney Sussex (see *Cambridge University Reporter*, November 18, 1890, p. 237).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Meteorological Society, November 19.—Mr. Baldwin Latham, President, delivered an address on "The Relation of Ground Water to Disease." The pages of history show that when the ground waters of our own or other countries have arrived at a considerable degree of lowness, as evidenced by the failure of springs and the drying up of rivers, such periods have always been accompanied or followed by epidemic disease. In all probability ground water in itself, except under conditions where it is liable to pollution, has no material effect in producing or spreading disease. As a rule, it is only in those places in which there has been a considerable amount of impurity stored in the soil that diseases become manifest, and the most common modes by which diseases are, in all probability, disseminated, are by means of the water supplies drawn from the ground, or by the elimination of ground-air into the habitations of the people. It is found that the periods of low and high water mark those epochs when certain organic changes are taking place in the impurities stored in the ground, which ultimately become the cause and lead to the spread of disease. Mr. Latham defines "ground water" as all water found in the surface soil of the earth's crust, except such as may be in combination with the materials forming the crust of the earth. It is usually derived from rainfall, by percolation; and it is also produced by condensation. In dry countries, ground water is principally supplied by the infiltration from rivers, as, for example, in the Delta of the Nile. The absence of water passing into the ground for a long period naturally leads to the lowering of the free ground water-line, and may lead to the drying of the ground above the water-line; and it is curious to note, with reference to small-pox, that the periods marking the epochs of this disease are those in which there has been a long absence of percolation, and a consequent drying of the ground preceding such epidemics. On the other hand, small-pox is unknown at such periods as when the ground has never been allowed to dry, or is receiving moisture by condensation or capillarity. The study of underground water shows that certain diseases are more rife when waters are high in the ground, and others when the water is low. The conditions that bring about and accompany low water, however, have by far the most potential influence on health, as all low water years are, without exception, unhealthy. As a rule, the years of high water are usually healthy, except, as often happens, when high water follows immediately upon marked low water, when on the rise of the water an unhealthy period invariably follows. Mr. Latham has found that those districts which draw their water supplies direct from the ground, are usually more subject to epidemics and disease than those districts in which the water supply is drawn from rivers supplied from more extended areas, or from sources not liable to underground pollution. In the case of Croydon, one portion of the district (under three-fourths) is supplied with water taken direct from the ground, whilst the remaining portion is supplied with water from the River Thames. It is curious to note that even so recently as 1885 the zymotic death-rate in the districts supplied with underground water was twice as great as in that part of the district supplied from the Thames; and in this particular year forty-one deaths from small-pox occurred in the district, not one of which was recorded outside the district supplied by the underground water. Mr. Latham, in his address, dealt largely with zymotic diseases as affected by ground water, and showed that cholera ordinarily breaks out when there is the least ground water; a high air and ground temperature is also necessary for its development, and as a rule the low-lying districts are favourable to the production of these high temperatures. Small-pox is almost always preceded by a long period of dryness of the ground, as measured by the absence of percolation. Typhoid fever is most prevalent after a dry period, and the first wetting of the ground or percolation from any cause takes place. The condition essential to the development of diphtheria is a damp state of the ground marked by extreme sensitiveness to percolation of rain. Scarlet fever follows the state of the dryness of the ground, which is essential for its development, and it occurs in the percolation period. The conditions that precede small-pox are those favourable for the development of scarlet fever, and, like small-pox, the dampness of the ground for any considerable period in any particular locality, may check its development or render it less virulent, and it is most rife in low water years. Measles are least prevalent at the low water periods, and mostly rife at and near high water periods. Whooping-cough follows the percolation period in its incidence, increasing with percolation, and diminishing as the waters in the ground subside. Diarrhœa is generally more prevalent in a low water year than in other years; that is, with a very much colder temperature in a low water year there is a very much higher death-rate from this disease. Mr. Latham finds that the general death-rate of a district is amenable to the state of the ground water, years of drought and low water being always the most unhealthy.

Geological Society, November 12.—Dr. A. Geikie, F.R.S., President in the chair.—The President referred to the sad loss the Society had sustained since the last meeting, through the death of the late Foreign Secretary, Sir Warington W. Smyth, F.R.S.—The President reported that Mr. L. Belinfante had been temporarily appointed by the Council to the office of Assistant-Secretary.—The following communications were read:—On the porphyritic rocks of the Island of Jersey, by Prof. A. De Lapparent, Foreign Correspondent of the Society. (Communicated by the President.) The author had some years ago described as Permian a series of porphyritic rocks, of which specimens had been sent to him from Jersey. He had since been led to believe that this view of their age, arrived at from what he knew of similar rocks in France, was erroneous, and in a recent visit to the island had satisfied himself that the English observers who had assigned to these rocks a much higher antiquity were in the right. He now found that the igneous rocks in question underlie the Rozel conglomerate, which must be placed at the very base of the Silurian formations. He reserved his detailed statement for a communication to the Geological Society of France; his present object being to do justice to English geologists, whose views he had formerly opposed.—On a new species of *Trionyx* from the Miocene of Malta, and a Chelonian scapula from the London Clay, by R. Lydekker.—Notes on specimens collected by W. Gowland, in the Korea, by Thomas H. Holland, of the Geological Survey of India, late Berkeley Fellow of the Owens College. (Communicated by Prof. J. W. Judd). The southern half of Korea traversed by Mr. Gowland is of a hilly character. The rocks forming the hills are chiefly crystalline schists—gneisses with graphite, garnet, dichroite, and fluor occurring in considerable abundance; and the whole group is probably part of the great Archæan mass of North-East China. The author describes these metamorphic rocks in detail. Stratified rocks, probably of Carboniferous age, lie unconformably upon the schists in the south-eastern part of the peninsula, and petrographical notes of these are given in the paper. Through the crystalline schists and stratified rocks various igneous rocks have been erupted as dykes or in large masses. Amongst these the most conspicuous rock is granite. Biotite- and muscovite-granite are most widely distributed, and in places are cut by dykes of eurite and veins of quartz and pegmatite. The more basic class of rocks is represented by diorites, propylites, andesites, basalts, dolerites, and gabbros. Interesting cases of the gradual passage between the so-called intermediate and basic rocks are found, and various stages in the devitrification and decomposition of andesitic lavas represented. These are described in detail by the author, and compared with similar cases in other regions; and full descriptions of the intrusive rocks are furnished. There are now no active volcanoes; and there is a notable lack of mineral wealth in the southern part of the Korea. Prof. Judd spoke of the value of Mr. Gowland's geographical and geological discoveries, and the enthusiasm with which Mr. Holland had applied himself to the work of examining the specimens brought home, and he considered that the work would prove an important contribution to science. Several points about which difficulties had arisen by examination of European rocks had light thrown upon them by the Korean specimens. The President felt that the Society would agree with him in considering the Geological Survey of India fortunate in securing a petrologist like Mr. Holland.—Further notes on the stratigraphy of the Bagshot Beds of the London Basin (north side), by the Rev. A. Irving.

Mathematical Society, November 13.—J. J. Walker, F.R.S., in the chair.—The Chairman informed the members of the loss the Society had recently sustained by the death of Dr. A. J. Ellis, F.R.S., who was elected a member on June 19, 1865, and had served on the Council during the sessions 1866–67, 1867–68. He gave a brief sketch of Dr. Ellis's contributions to mathematics and other subjects. He next sketched in some detail the numerous contributions made to mathematical physics by Lord Rayleigh, F.R.S., dwelling more particularly upon those memoirs which had led the Society, as announced at the June meeting, to award him the De Morgan Memorial Medal. The medal having been presented, Lord Rayleigh simply thanked the Society for their gift.—The new Council having been duly elected, the new President (Prof. Greenhill, F.R.S.) called upon Mr. Walker to read his address "On the Influence of Applied on the Progress of Pure Mathematics." The author was asked to print the paper in the Proceedings, on the motion of Mr. A. B. Kempe, F.R.S., seconded by Lord Rayleigh.—The following communications were made:—Spherical harmonics of fractional order, by R. A. Sampson.—Proofs of Steiner's theorem relating to circumscribed and inscribed conics, by Prof. Mathews.—On an algebraic integral of two differential equations, by R. A. Roberts.—Some geometrical constructions, by Oscher Ber (communicated by Prof. Hill).—On the analytical representation of heptagrams, by Prof. L. J. Rogers.

Zoological Society, November 18.—Dr. Mivart, F.R.S., in the chair.—Mr. F. Menteith Ogilvie exhibited and made remarks on a specimen of the Red-headed Flycatcher obtained in Norfolk.—Prof. F. Jeffrey Bell exhibited an example of the Cotton-spinner (*Holothuria nigra*), taken off the west coast of Ireland, and sent for determination by Prof. Herdman.—Mr. G. A. Boulenger exhibited a series of skulls belonging to *Distira cyanocincta* and *Chelone midas*.—Mr. G. A. Boulenger read a paper upon the Reptiles and Batrachians of Barbary (Morocco, Algeria, Tunisia), based chiefly upon the notes and collections made in 1880–84 by M. Fernand Lataste.—A second paper by Mr. G. A. Boulenger contained remarks on the Chinese Alligator.—A communication was read from the Rev. O. P. Cambridge, F.R.S., giving an account of some new species and two new genera of Araneidea, mostly collected in South Africa by the Rev. Nendick Abraham.—Mr. Smith Woodward read a paper on some Upper Cretaceous Fishes of the family Aspidorhynchidæ. He offered a detailed description of *Belonostomus comptoni*, from Brazil, and defined a new genus (*Apateopholis*) from Syria. The latter is remarkable as being the only physostomous fish hitherto described exhibiting a spinous armature of the preoperculum.—Mr. G. C. Champion read a paper on the Heteromerous Coleoptera collected by Mr. Bonny at the Yambuya Camp, Aruwimi Valley.

PARIS.

Academy of Sciences, November 17.—M. Hermite in the chair.—Ed. Phillips, by M. H. Léauté. The deceased mathematician was born at Paris on May 21, 1821, and died on December 14, 1889. An account is given of his many works in mechanics and mathematical physics.—On the name of bronze, by M. Berthelot. The author quotes the following from a work of the time of Charlemagne: "Compositio brandisii: eramen partes II., plumbi parte I., stagni parte I.," that is to say, bronze is composed of two parts of copper, one of lead, and one of tin. This appears to confirm the view that the name of bronze is derived from that of the town of Brundusium, or βρεντήσιον, especially as many bronze vessels have been found marked *ars Brundusinum*.—Remarks on some acoustical sensations provoked by certain quinine salts, by M. Berthelot. The author describes certain humming noises that he hears after the ingestion of lactate.—A Chaldæan astronomical annual used by Ptolemy, by M. J. Oppert. A series of lunar and planetary observations have been found in the British Museum among the cuneiform tablets. These have been deciphered, and prove to be among the oldest and most detailed we possess. The phenomena recorded on them took place in the year 522 B.C., and are described in an exceedingly minute manner. An account is given of the results obtained by an investigation into these cuneiform inscriptions, and their bearing upon the dates of certain events.—On the annual variation in the latitude caused by the differences in refractional effects which result from atmospheric tides, by Dom Lamey. The tidal effect of the sun and moon upon the atmosphere is given as a probable cause of the annual variation in latitude.—Rapid development of a solar prominence, by M. Jules Fényi. The prominence appeared on the western edge of the sun, from heliographic latitude $-20° 13'$ to $-30° 21'$, on October 6, at 1h. 18m. Kalocsa mean time. In about half an hour the eruption had reached a height of 327″—that is, 235,900 kilometres.—On one of M. Picard's theorems, by M. Gustaf Kobb.—Note on the construction of plans from views obtained at elevated points in the atmosphere, by M. A. Laussedat. A method is described by means of which photographic views obtained from balloons may be reduced to plan.—Researches in thermo-electricity, by MM. Chassagny and H. Abraham. The authors have already shown that the electromotive forces of thermo-electric couples, of which the junctions are maintained at 0° and 100° respectively, may be determined to $\frac{1}{1000}$ of their value. They find that the following formula, though not representing their measures with entire accuracy, is sufficient to give the tenth of a degree Centigrade in the interval 0° to 100°. The formula

$$E_0^t = \frac{at + bt^2 + ct^3}{t + 273},$$

where $a = 10^{-3} . 3{\cdot}56604$, $b = 10^{-6} . 8{\cdot}3827$, $c = -10^{-8} . 3{\cdot}265$, t = temperature, and E = electromotive force. The value of E_0 at 100° = 0·0010932 volts with an iron-copper couple. The following is a comparison of observed and calculated results at different temperatures—

Temperature.	Electromotive forces. Observed volts.	Calculated volts.
65°·13	0·0007656	0·0007654
32°·49	0·0004043	0·0004045
15°·48	0·0001981	0·0001980

—On the periodicity of undulatory pressures produced by the combustion of explosives in a closed vase, by M. P. Vieille.

The undulations set up at the extremities of a receiver 1 metre long have been photographically registered by the side of time signals. The extremely accurate results obtained indicate that the method may be used for the study of the phenomena of the propagation of waves in conditions of gaseous condensation and temperature above those as yet investigated.—On the electrical resistance of bismuth in a magnetic field, by M. A. Leduc. The author develops a formula which allows the calculation of the resistance of a wire of bismuth placed in a magnetic field at a certain temperature, in terms of its resistance at 0° outside the field.—On the β-pyrazol-dicarbonic acids, by M. Maquenne. Aldehydes react with dinitrotartaric acid in presence of ammonia, giving rise to monobasic acids which, on heating, form β-pyrazol-dicarbonic acids of the general formula

$$\begin{array}{l} CO_2H-C-N \\ \quad\quad\ \ | \quad\quad\quad \rangle CR. \\ CO_2H-C-NH \end{array}$$

Furfural does not act in the same manner, the body obtained being

$$\begin{array}{l} CO_2H-C(OH)-N=CH-C_4H_3O \\ \quad\quad\quad\ | \\ CO_2H-C(OH)-N=CH-C_4H_3O. \end{array}$$

The sugars with aldehyde reactions give no definite combination with nitrotartaric acid.—On a phenol acid derived from camphor, by M. P. Cazeneuve.—Note upon active amylic derivatives, by M. Philippe A. Guye.—On the saponification of halogen organic compounds, by M. C. Chabrié.—The author forms the fluorides by the sealed tube method, and saponifies these bodies by means of milk of lime, *e.g.*

$$C_2H_4F_2 + Ca(OH)_2 = \underbrace{C_2H_6O_2}_{\text{Glycol}} + CaF_2.$$

A reaction of the halogen compounds with B_2O_3 is also indicated.—On a gaseous antiseptic, its action upon the pyogenous bacteria of the urinary infection, by M. C. Chabrié.—On the fixation of gaseous nitrogen by the Leguminosæ, by MM. Th. Schlœsing and Em. Laurent.—On the microbe of the nodosities of Leguminosæ, by M. Em. Laurent.—On some transitory characters presented by *Chelmo rostratus*, Linn., by M. Léon Vaillant.—On the sexual dimorphism of *Enterocola fulgens*, by M. Eugène Canu.—On the sexual differences of *Lepadogaster bimaculatus*, Flem., by M. Frédéric Guitel.—On the antagonistic molecular forces which are produced in the cellular nucleus, and on the formation of the nucleiform membrane, by M. Ch. Degagny.—On the origin of the terraces (*rideaux*) in Picardy, by M. H. Lasne.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 27.

ROYAL SOCIETY, at 4.30.—On the Homology between Genital Ducts and Nephridia in the Oligochæta: F. E. Beddard.—The Patterns in Thumb and Finger Marks; on their Arrangement into Naturally Distinct Classes, the Permanence of the Papillary Ridges that make them, and the Resemblance of their Classes to Ordinary Genera: F. Galton, F.R.S.—Preliminary Note on the Transplantation and Growth of Mammalian Ova within a Uterine Foster-Mother: W. Heape.—The Conditions of Chemical Change between Nitric Acid and Certain Metals: V. H. Veley.—The Variations of Electromotive Force of Cells consisting of Certain Metals, Platinum, and Nitric Acid: G. J. Burch and V. H. Veley.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Efficiency of Secondary Cells; "On the Chemistry of Secondary Cells: Prof. W. E. Ayrton, F.R.S., and E. W. Smith.

FRIDAY, NOVEMBER 28.

PHYSICAL SOCIETY, at 5.—Notes on Secondary Batteries: Dr. Gladstone and W. Hibbert.—An Illustration of Ewing's Theory of Induced Magnetism: Prof. S. P. Thompson.

AMATEUR SCIENTIFIC SOCIETY, at 8.—Aquatic Microscopical Life (with Lantern Illustrations): J. D. Hardy.

SUNDAY, NOVEMBER 30.

SUNDAY LECTURE SOCIETY, at 4.—The Natural Growth of Religion in India: Sir A. C. Lyall, K.C.B., K.C.I.E.

MONDAY, DECEMBER 1.

SOCIETY OF ARTS, at 8.—Gaseous Illuminants: Prof. Vivian B. Lewes.

ROYAL MICROSCOPICAL SOCIETY, at 8.—*Conversazione.*

VICTORIA INSTITUTE, at 8.—On the Geological History of Egypt: Prof. Hull, F.R.S.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

TUESDAY, DECEMBER 2.

ZOOLOGICAL SOCIETY, at 8.30.—On the Antelopes of Nyassa-Land; Richard Crawshay.—On the Suspension of the Viscera in the Batoid Hypnos subnigrum: Prof. G. B. Howes.—Notes on the Pectoral Fin-skeleton of the Batoidea and of the Extinct Genus Chlamydoselache: Prof. G. B. Howes.—On the Presence of Pterygoid Teeth in a Tailless Batrachian (Pelobates cultripes), with Remarks on the Localization of Teeth on the Palate in Batrachians and Reptiles: G. A. Boulenger.

ESSEX FIELD CLUB (at Loughton), at 7.—Some Notes on Dipsacus sylvestris and D. pilosus and their Natural Relationship: J. French.—The Butterflies of Essex: Edward A. Fitch.—The Land and Fresh-water Mollusca occurring in the Neighbourhood of Bishop's Stortford: Edwin G. Ingold.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Ballot for Members.—The Vibratory Movements of Locomotives: Prof. John Milne, F.R.S., and John McDonald. (Discussion.)—The Sukkur Bridge at Benares: F. E. Robertson.—The New Chittravate Bridge, Madras Railway: E. W. Stoney.

WEDNESDAY, DECEMBER 3.

SOCIETY OF ARTS, at 8.—The Chicago Exhibition, 1893: James Dredge.

ENTOMOLOGICAL SOCIETY, at 7.—On the Conspicuous Changes in the Markings and Colouring of Lepidoptera, caused by subjecting the Pupæ to Different Temperature Conditions: Frederic Merrifield.—Notes on the Lepidoptera collected in Madeira by the late T. Vernon-Wollaston: George T. Baker.—A Monograph of the Lycænoid Genus Hypochrysops, with Descriptions of New Species: Hamilton H. Druce.—The Life-History of the Hessian Fly: Frederick Enock.

THURSDAY, DECEMBER 4.

LINNEAN SOCIETY, at 8.—On the Genus of Orchid Brownheadia: H. N. Ridley.—On the Botany of Kandahar: J. H. Lace.—Botanical Visit to Auckland Isles: Thos. Kirk.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—On the Volumetric Estimation of Tellurium: Dr. Branner.

FRIDAY, DECEMBER 5.

GEOLOGISTS' ASSOCIATION, at 8.—Report on the Microscopical Examination of some Samples of London Clay from the Excavations for the Widening of Cannon Street Railway Bridge, 1887: C. Davies Sherborn and H. W. Burrows.—A Short Visit to Ingleton and to Filey Brigg (showing how a Dangerous Reef was converted into a Perfect Breakwater by an Ancient Race): Edwin Litchfield.

CONTENTS.

THURSDAY, DECEMBER 4, 1890.

THE COUNTY COUNCILS AND TECHNICAL EDUCATION.

A CONFERENCE will be held to-morrow under the auspices of the National Association for the Promotion of Technical and Secondary Education, which may prove of the greatest importance to the future development of education in this country. The Marquis of Hartington will be in the chair, and representatives of County and County Borough Councils and other Local Authorities working the provisions of the Technical Instruction Act have been invited to meet the Executive Committee of the Association and delegates from its Branches throughout the country, to confer on the best means of utilizing the new fund placed at the disposal of County Councils under the Local Taxation Act of this year for the purpose of education.

It will be remembered that, by the accident of the abandonment of the licensing clauses of the Local Taxation Bill, a sum of about £743,000, the residue of the proceeds of the new beer and spirits duty after defraying the charges for police superannuation, has been allotted to County Councils in England and Wales, with permission to apply the fund to technical education, or, in the case of Wales, to intermediate education under the Welsh Intermediate Education Act. The Scotch share, amounting to another £50,000, may be used for the purpose of the Scotch Technical Schools Act.

A number of reflections are suggested by this unexpected appropriation. Wales is ahead of England and Scotland in the matter, because a more complete Act is in force in the Principality. The Welsh may use the fund to organize their secondary education under public control, while in England such organization, though, as has been pointed out,[1] not impossible, has to be carried out on a far more restricted scale and under more hampering conditions. This is not because the English share of the fund is inadequate, but merely because when the critical moment for the appropriation came it found Wales provided with the necessary machinery, in the shape of the Intermediate Education Act which is unfortunately not yet in force in the rest of the Kingdom. It was too late in the session to build up the requisite machinery for England, so we are compelled to wait.

Had it not been for the passage of the Technical Instruction Act of 1889, which, as will be remembered, was fiercely contested at the time by many of the more extreme educational partisans, there would have been no machinery at all in England for the utilization of the new fund for education, and the whole would doubtless have been allotted to other purposes, such as reduction of rates. As it is, the fund can be administered under the provisions of that Act, incomplete as it is, until it is duly amended by the Bill of which Sir Henry Roscoe has already given notice, and by the extension to England, under a somewhat modified form, of the Welsh Intermediate Education Act. Thus, had it not been for the previous passage of a confessedly imperfect measure, the greatest chance yet offered for the organization of secondary education would have been missed altogether. We hope this fact will be remembered when heated advocates of one line or other of educational policy are again tempted to forget that "half a loaf is better than no bread." Get public supervision of higher education recognized in an Act of Parliament, and all the rest will surely follow. The Act may be imperfect; parts of it may be bad; but he is a pedant who refuses it on that ground. The Act will be amended as necessity arises; new powers of control will be acquired when any power of control has been once conceded. The new Act gives increased facilities for the organization of education to various parts of the Kingdom in precise proportion to the powers already possessed. "To him that hath shall be given," in education as in other matters.

So much for the Act. The next question that arises is its administration by the local authorities, who, it is to be observed, have power to devote all or any part of the fund to education, and of course to use the rest for the relief of rates. It must be admitted that it is a great temptation, to some County Councils at least, to take advantage of their undoubted right to use the fund otherwise than Parliament intended; but it is a temptation which should be resisted if only for the reason that the renewal of the grant next year may largely depend on the use which can be shown to have been made of it by local authorities. It would be suicidal policy, even for the strictest friend of the ratepayer, to clutch the money this year for the relief of rates, and thus to lose permanently a grant which will be sorely needed before long in order to meet the new charges which, as Mr. Goschen hinted, are likely to be put upon local authorities with respect to agricultural and intermediate education.

So far as the matter has yet come before County Councils, there is no cause for discouragement. At least nine counties have already determined to use the whole or part of the money for technical education; others have appointed committees to inquire into the whole question. We hope that the result of the Conference which is announced will be to decide many wavering Councils to take the same course.

There will be plenty for the Conference to discuss. The work of organizing technical education is new to County Councillors, and many of them are still quite in the dark on many points. Moreover, practical difficulties have arisen in some districts in the working of the Act, on which it will be useful to compare notes. We have received a copy of the provisional agenda, by which we see that among other things the meeting will discuss the nature of the preliminary proceedings which should be taken by County Councils which are desirous of assisting education out of the new fund. This is an important question. Should the Councils simply pass a resolution inviting applications for assistance, and then judge among the various qualified institutions which send in claims? Or should they institute an inquiry into the state of education in their districts, and on the results of that inquiry formulate a comprehensive scheme? Should the money be devoted chiefly to the assistance and development of existing institutions? Or should the Councils initiate schools of their own under the general powers conferred by the Act? On matters like this many

[1] See "Suggestions to County Councils" (National Association for the Promotion of Technical and Secondary Education, 14 Dean's Yard).

Councils are doubtless waiting for a lead such as they may obtain at the Conference. Meanwhile we may commend to their attention the carefully drawn report just presented to the Shropshire County Council by the Technical Instruction Committee of that Council, as a model in many ways of what such a report should be.

The Conference will also discuss the modes in which the fund may be divided among the non-county boroughs and sanitary districts within the county. This again, is a debatable question. Should the basis of the allotment be rateable value, or population, or existing local effort? How far should the convenience and needs of the county as a whole be exclusively consulted in the scheme, independently of the wishes or claims of particular localities? Clearly the Act gives no legal claim to a non-county borough on any particular proportion of the money, though the County Council may, if it please, act through these minor authorities in the distribution. The question is eminently one on which light will be thrown by a friendly conference.

The third point for discussion is the amendment of the Technical Instruction Act. Certain difficulties have arisen in the administration of the Act, which may be removed without raising controversies. For example, many Councils wish to aid agricultural education by founding or assisting agricultural schools in county boroughs in their district. But this they may not do, for the county boroughs are outside their jurisdiction; while the county boroughs themselves are not likely to do the work, since it will, as a rule, be useless to their own inhabitants. Other points, such as the question of the provision of scholarships, especially to schools outside the counties, have been called in question, and need settlement.

The fourth subject on the agenda is the most important of all; viz. the best mode of assisting intermediate and agricultural education. It is hardly to be expected that the members of the Conference will succeed in evolving a complete plan; but they will do a great service if they demand, with no uncertain voice, the extension to England of a measure based on that which is working so admirably in Wales. Meanwhile, the question of the best mode of assisting intermediate education depends very greatly on the chance of the future renewal of the grant. We look to Lord Hartington to give an assurance that there is no possibility of the diversion of any part of the new fund by the Government to other purposes than technical and secondary education.

DREYER'S LIFE OF TYCHO BRAHE.

Tycho Brahe: a Picture of Scientific Life and Work in the Sixteenth Century. By J. L. E. Dreyer, Ph.D., F.R.A.S. (Edinburgh: A. and C. Black, 1890.)

THE time had evidently come for the publication of an adequate account of the life and work of the great Danish astronomer. Popular presentments of the first, and partial studies relating to the second, there have been, it is true, in more languages than one; but Gassendi's was, until now, the only scientific biography of Tycho Brahe; and numerous documents, inedited and inaccessible in 1654, have since been brought to light, clearing up much that was obscure, supplementing meagre with full information, and thus rendering possible the virtually complete narrative now given to the public by Dr. Dreyer. That unstinted pains were bestowed upon his task is obvious from the manner of its accomplishment; and he was equipped for it with a combination, desirable rather than usual, of linguistic, historical, and scientific attainments. Indeed, at times, superabundant information is afforded; the wealth of details tends to congestion; the essential does not decisively enough maintain its supremacy over the superfluous. The book is none the less a most valuable contribution to the history of mediæval astronomy; it is creditably exempt from slips and inaccuracies of memory, pen, or print; abounds with bibliographical knowledge and indications; and—not its least merit—is furnished with a highly serviceable index. It portrays, moreover, with perfect candour, yet full comprehension and sympathy, a vigorous and picturesque individuality.

Judgment in forming, and persistence in holding to, a large and definite purpose, rather than surpassing intellectual ability, caused Tycho Brahe to tower above his scientific contemporaries. Wittich—to say nothing of Viète and Harriot—was his superior in mathematics; he could lay no claim to the inventive ingenuity of Joost Bürgi; his system of the world was less plausible than that of the uncouth Reymers. But he saw clearly what was indispensable to the progress of astronomy, and by the undaunted energy of his efforts to supply it, raised himself to the high level of his great undertaking.

Until observations were begun at Uraniborg, the astronomy of the ancients, as Dr. Dreyer says with truth, reigned practically undisturbed. A speculative impulse of incalculable importance had indeed been given by the promulgation of the Copernican doctrine; but

"No advance had been made in the knowledge of the positions of the fixed stars, those stations in the sky by means of which the motions of the planets had to be followed; the value of almost every astronomical quantity had to be borrowed from Ptolemy, if we except a few which had been redetermined by the Arabs. No advance had been made in the knowledge of the moon's motion, so important for navigation, nor in the knowledge of the nature of the planetary orbits, the uniform circular motion being still thought not only the most perfect, but also the only possible one for the planets to pursue. Whether people believed the planets to move round the earth or round the sun, the complicated machinery of the ancients had to be employed in computing their motions, and, crude as the instruments in use were, they were more than sufficient to show that the best planetary tables could not foretell the positions of the planets with anything like the desirable accuracy.

"No astronomer had yet made up his mind to take nothing for granted on the authority of the ancients, but to determine everything himself. Nobody had perceived that the answers to the many questions which were perplexing astronomers could only be given by the heavens, but that the answers would be forthcoming only if the heavens were properly interrogated by means of improved instruments capable of determining every astronomical quantity anew by systematic observations. The necessity of doing this was at an early age perceived by Tycho Brahe. . . . By his labours he supplied a sure foundation for modern astronomy, and gave his great successor, Kepler, the means of completing the work commenced by Copernicus" ("Tycho Brahe," p. 9).

On August 21, 1560, the young Scanian noble, then a thirteen-year old student at the University of Copenhagen, watched the moon, punctual to prediction, obscure a few digits of the sun's face, and was impressed with it "as something divine that men could know the motions of the stars so accurately that they could long before foretell their places and relative positions." Thenceforward all other pursuits were with him secondary to that of training himself for an astronomer. "Was ein Haken werden will, krümmt sich bei Zeiten." But the career was regarded as compromising to the dignity of his birth, and his uncle sent him to Leipzig in charge of a tutor—the excellent Anders Vedel—whose chief business it was to interpose between him and the heavens. In vain; while Vedel was asleep, Tycho studied his Almegist, learned the constellations from a little globe the size of his fist, and, on the occasion of an important conjunction of Jupiter and Saturn, made his first recorded observation (August 17, 1564) with no other instrument than an ordinary pair of compasses. In the following year he procured a "radius," or "cross-staff," subdivided by the newly invented method of transversals, and gave, in his table of corrections to the results obtained with it, "the first indication of that eminently practical talent which was, in the course of years, to guide the art of observing into the paths in which modern observers have followed" (Dreyer, *l.c.*, p. 20). By this time, too, the intuition had come to him that the planets themselves, consulted with prolonged and patient vigilance, could alone impart trustworthy intelligence as to the system of their motions; and thus, in the mind of a boy of seventeen, the "restoration" of the prehistoric science *par excellence* was conceived and determined upon.

Other interests, however, supervened; and chemical, or, rather, alchemistic, researches might have continued to absorb the attention of one capable of better things, had not his vocation to astronomy been renewed under sanction of the portent (as it were) of the stellar outburst in Cassiopeia. Tycho hesitated no longer, but waited the propitious moment, and sought the appropriate scene for the commencement of his life's labours. He had fixed upon Basle; but the King of Denmark, Frederick II., anticipated his removal thither by the offer of an island-domain in the Sound, together with funds for the erection upon it of suitable buildings.

Their nature and plan are illustrated, both graphically and descriptively, in the work before us. Suffice it here to say that their cost was commensurate with their splendour, and could only have been defrayed by Royal munificence. Yet, after its withdrawal by Christian IV., private means might have sufficed to maintain what they were inadequate to create; for the "Phœnix of astronomers" (as Kepler designated him) was still a comparatively rich man when, rather in disgust than from necessity, he forsook Uraniborg in April 1597. Nor was he free from blame in the transactions which led to his purely voluntary exile. His government of Hveen was nearly absolute, and somewhat arbitrary; his dealings with his tenants, there and elsewhere, were often harsh and tyrannical; he could scarcely be brought, by repeated Royal admonitions, to comply with the conditions of the various grants made to him; and his arrogant bearing made enemies who did not fail to accentuate his shortcomings. A tradition more honourable to him ascribes, it is true, a share in bringing about his disgrace to the King's physician, Peder Sörensen, whose jealousy is said to have been excited by Tycho's unpaid practice of medicine; but Dr. Dreyer saddles the chief responsibility upon the Chancellor Friis, interested, possibly, both through motives of public economy and of private hostility, in the fall of a costly and unsubmissive subject of the Crown. In short, Sir Roger de Coverley's cautious dictum, that "much might be said on both sides," may serve to convey the equitable judgment of posterity as to affairs at Hveen and their lamentable upshot.

Its worst result was in the waste of valuable time. Four years were virtually curtailed by it from the great astronomer's effective career. Yet one compensatory circumstance attended the disaster. Tycho's presence attracted Kepler to Prague; and their connection, formed just at the right moment for progress, put the younger man in possession of a store of facts, from the co-ordination of which he was enabled to deduce the laws constituting the fit epilogue to the work of Copernicus, the indispensable preface to that of Newton.

The results of Tycho's activity during upwards of twenty years at Uraniborg, were summed up by himself as follows:—An improved solar theory; the detection of the third inequality, or *variation*, in the moon's motion, as well as of fluctuations both in the inclination of the lunar orbit, and in the shifting of the nodes; the accurate ascertainment of the places of one thousand fixed stars; the dismissal from astronomy of the supposed "trepidation" of the equinoxes; the collection of a vast mass of materials for the revision of planetary tables; the supply of ample proof that comets are trans-lunar bodies (Dreyer, *l.c.*, p. 262). He might have added the discovery of the lunar annual equation (almost simultaneously perceived necessary by Kepler, as Dr. Dreyer points out); the employment of systematic corrections for refraction; and observations, continuous and exact, of the "new star" of 1572.

Theory was not Tycho's strong point. He attached much more importance than it deserved to his system of the world, which he vainly hoped would outlive himself; nor was he endowed with a penetrating insight into the significance of the phenomena observed by him with such marvellous skill. This may be estimated from the probable errors of his positions for nine standard stars, amounting, as compared with the best modern results, to $\pm 24''$ in right ascension, $\pm 26''$ in declination (Dreyer, *l.c.*, p. 351). His absolute right ascensions were derived from the longitude of the sun through Venus as an intermediate link; his declinations were directly measured. When it is remembered that he was destitute of any kind of optical aid, that his graduated instruments must have been, judged by modern criteria, intolerably defective, that he knew nothing of the effects of aberration and nutation, and commanded only a rough value for precession, the degree of accuracy thus attained must excite our wondering admiration. It should be added that the utter untrustworthiness of his clocks precluded him from the determination of relative places by the easy method of transits.

The frontispiece of the book under review is taken from a portrait of Tycho Brahe painted at Rostock in 1597, and reproduced some few years since in these pages (NATURE, vol. xv. p. 406). Purchased by the Earl of Crawford in 1881, it formed part of his generous gift to the Royal Observatory, Edinburgh. The illustrations in the body of the work, consisting of plans and elevations of Uraniborg, portrayals of the instruments, later residences, and tomb, of its whilom inmate, are both interesting and well executed. The last represents the figure carved in red marble on the monument to Tycho Brahe in the Teynkirche at Prague. Its martial aspect corresponds with the feudal pomp amid which his remains were borne to their last resting-place. Not recumbent, though sleeping, he stands erect, clad in full armour, a plumed helmet on one side, an emblazoned shield on the other, a sword grasped in his left hand. Nothing recalls the genuine quality of his life except the globe upon which his right hand rests, and the inscription from his demolished observatory, "Nec fasces nec opes, sola artis sceptra perennant." A. M. CLERKE.

TWO NEW PSYCHOLOGIES.

A New Psychology: an Aim at Universal Science. By the Rev. George Jamieson, D.D., &c. (Edinburgh: Andrew Elliott, 1890.)

Hand-book of Psychology: Senses and Intellect. By Prof. J. M. Baldwin, M.A., Ph.D. (London: Macmillan and Co., 1890.)

WE are quite ready to allow Dr. Jamieson that, as he claims in his preface, his work is strictly original. We do not think, however, that he need fear that, because it emanates from a clergyman, it will be any the less attentively considered by any competent persons. The reasonings and speculations, in another branch of science, of Dean Conybeare, of Prof. Sedgwick, of Dr. Bonney, or of Mr. Osmond Fisher, have, so far as we can judge, in no wise suffered from this cause. And if there were any value in the reasonings and speculations of Dr. Jamieson, in the realm of psychology and philosophy, they would be as readily and thankfully accepted as those of Bishop Berkeley or Dr. Mansell. But, in truth, however well grounded in theology, Dr. Jamieson's little book gives abundant evidence that he is not very thoroughly grounded in more mundane branches of science.

It is impossible to summarize briefly this "new psychology." We therefore content ourselves with giving two or three extracts which we consider representative, and must then leave such of our readers as aspire to fuller knowledge to worry through the book for themselves.

"It is of enormous moment," we are told, "to understand what it is that really constitutes the soul, as well as to apprehend under what circumstances the soul is constituted. We know of no other substance, as absolute and eternal, than that of Ether. It is absolute in two important respects, *first*, as positive and conditioned, and *second*, as negative and unconditioned."

"If it holds good, as a fact throughout all nature, that material structures have their conditions represented by an immediate encompassment of their delineation in Ether, and if it holds equally good, that in the animal frame generally, and especially in man as the highest exemplification, there is a nerve system in the body known as the 'sympathetic,' and carrying its affections, *i.e.* conducting the states of the living body to one single pole or axis, as the terminal of concentration,—as the pivot whereon the assembled conditions of animal states are manifested, what follows in the first place, as the necessary result of this concentration? Why, surely this, that precise analogy with all that we know, as express revelations in our experience must, in this particular case, be the actual outcome of an Etherial Image or representation of this complex animal self-hood."

"The very fact of our discovery that the pure Etherial Medium, as spirit-substance, is ever present as the fundamental mainspring of operation, in the linking together of conditions in the material world, opens the door of apprehension for us to comprehend the inter-workings of Brain, in producing the phenomena of Mind, as manifested in the Soul or Ego of each personality."

A very different book is the first instalment of Prof. Baldwin's "Hand-book of Psychology," of which a second and revised edition is before us. Well arranged, carefully thought out, clearly and tersely written, it will be welcomed in this country as it has been welcomed in America. That it views psychology from a standpoint somewhat different from that which Mr. Sully takes up in his "Outlines" will render it none the less acceptable to English students.

An interesting and important section is devoted to "consciousness and the unconscious," and Prof. Baldwin brings out very clearly the difficulties that attend the view that mental phenomena may be unconscious. He notes that it is often the case that an association between states of mind is accomplished without conscious links of connection, and that consequently the links of connection must be unconscious. He thinks it unsatisfactory, however, to say that the unconscious links are states of mind. "The missing links in broken chains of associations," he contends, "may be supplied from dynamic connections in the cerebral substance." But there are many people who think that it is as inconvenient as it is unphilosophical to dodge backwards and forwards from the brain to the mind. Either mental states are of a different phenomenal order from physical states or they are not. If they are not, let us at once say that just as the lachrymal gland produces tears so does the brain produce consciousness. If they are, then let us, so far as is possible, avoid hopping backwards and forwards from one to the other. The fact is, this question cannot be discussed without touching on the philosophical basis of psychology. Our own opinion is that we should be aided here by that most reprehensible procedure—the coinage of a new term. Believing, as we do, that mind never has been, and never could be, evolved from matter, the question arises, From what, then, have mental states (psychoses) been evolved? If a similar question be asked with regard to molecular vibrations in the brain (neuroses), the answer is not difficult. Complex neuroses have been evolved from less complex neuroses; these from simple neuroses; these again from organic modes of motion which can no longer be called neuroses at all; and these perchance, once more, from modes of motion which can no longer be called organic. But in the case of psychoses, from what have they been evolved? Complex psychoses have been evolved from less complex psychoses; these from simple psychoses; and these, again, from—what? From other and simpler modes of—something which answers on the subjective side to the organic modes of motion. For this we might use some such term as *metakinesis*. Neuroses are kinetic manifestations; states of consciousness are metakinetic manifestations. But, as neuroses are evolved from kinetic states (kineses) of a lower order, so have psychoses been evolved from metakinetic states (metakineses) of a lower order. According to this view, every mode of organic or neural kinesis has its concomitant mode of metakinesis, and, when the kinetic manifestations assume the form and intensity of neuroses in certain parts of the human brain, the metakinetic manifestations assume the form of human consciousness. Instead, therefore, of jumping across, with Prof. Baldwin, from psychical to physiological states, we would suggest that, from the point of view of psychology, the intermediate states are metakineses, which do not reach a sufficient intensity or complexity to appear in consciousness as states of mind. They are not unconscious states of consciousness, but they belong to the same phenomenal order as consciousness.

In his interesting discussion of the perception of space, Prof. Baldwin says:—

"Two general theories of space perception are held by psychologists, and under them may be classified the various attempts made to explain the origin of this idea. These are the *nativist* and the *empirical* theories. Empiricists hold that presentation of extension (and time) are derived through experience from elements which have not the spacial (and temporal) form. On the other hand, nativists maintain that space and time presentations cannot take their origin in data of consciousness which are simply intensive; consequently that these presentations are primitive data of knowledge, native, innate. In other words, the empiricist asserts the reducibility of presentations of space and time, and the nativist asserts their irreducibility."

Now here it seems to us that, if the empiricist claims to reduce the perception of space to sense-impressions, he is manifestly in error; and that, if the nativist bases his contention on the fact that there are over and above sense-impressions, impressions of relation, he is on firm ground. But it is questionable whether he can carry his claim much further.

Prof. Baldwin has a short but important chapter on illusions. We are unable entirely to agree with him in his treatment of the subject. Speaking of illusion proper, he says:—

"At the outset we find ourselves face to face with a whole class of experiences, in which one mental state is *taken for another*. There are, really, two images involved —one, the rightful image, the presentation, as ordinarily aroused by the stimulus experienced, say the sound of the clock; the other, the image of something different, formed within the domain of the same sense-quality, and usually prominent in consciousness before the time of the illusion, as the alarm of fire, into which the striking of the clock is interpreted."

This does not seem to us altogether satisfactory. Prof. Baldwin, at an earlier stage of his thesis, well brings out the fact that a percept is a mental construction formed at the bidding of a stimulus or sense-impression. Now, an illusion is simply a false and erroneous mental construction. If, to take another of Prof. Baldwin's examples, a man in the half light takes a tree for an Indian, or if a nervous old lady takes a grey donkey for a ghost, at the time of presentation the tree image and the donkey image have no existence. There is one image in each case—that involved in the false and erroneous mental construction. The difference between a true percept and an illusive percept is that the one is confirmable by the process of verification on further examination, and the other is not.

We have left ourselves no space to consider Mr. Baldwin's treatment of reason.

"The rational function," he says, "is contrasted with the apperceptive function in the absence of the element of process, which constitutes the essential nature of the latter. Apperception is a process, through which the material of acquisition passes in preparation for the higher uses of mind. Reason, on the contrary, is not a process, as the more special term, intuition, given to it implies. It conditions and underlies all mental processes. It is the nature of mind itself as it reveals itself in consciousness."

On this somewhat might be said. But we have already said enough to show that we regard the work as a valuable and stimulating hand-book of psychology, so far as the subject is treated in this instalment. C. LL. M.

THE MYOLOGY OF THE RAVEN.

The Myology of the Raven (*Corvus corax sinuatus*): *A Guide to the Study of the Muscular System in Birds*. By R. W. Shufeldt. (London: Macmillan and Co., 1890.)

WITH the exception of the brief descriptions in several of our zootomical text-books of certain of the muscles in birds, there has been up to the present time no special manual treating of this subject in detail. The student of avian myology has therefore had some difficulty in obtaining the requisite directions for his work, more especially if the advantages of a scientific library were not within his reach. Such works as those of Gadow in Bronn's "Thierreich," and of Fürbringer in his monumental "Untersuchungen zur Morphologie und Systematik der Vögel," besides being written in German, are not easily obtainable by the greater number of workers. The present book, from the fertile pen of Dr. Shufeldt, fills up this important gap in a highly satisfactory manner.

At first sight, it might appear an objection that Dr. Shufeldt has chosen such a rare bird as the raven as the subject of his work. But as other species of *Corvidæ* are so easily obtainable almost everywhere, it is perhaps even an advantage that the student in using the book cannot follow out every detail slavishly: by making use of some allied form, which agrees in the main, but differs in detail, from the Raven, he will learn more, and at the same time gain more self-reliance.

Though following, as regards nomenclature, the authority of Owen, Carus, A. Milne-Edwards, Huxley, Garrod, Forbes, Selenka, Coues, Fürbringer, Gadow, and others, the author has in doubtful cases introduced names of his own making, and has quoted very freely from Gadow (Bronn's "Thierreich"), whose synonymy is given fully in footnotes throughout the work. These notes indicate very plainly that we have as yet by no means arrived at a satisfactory knowledge of the muscular system in the Vertebrata, and that our existing nomenclature is far from perfect. It is to be hoped that the present work will aid in laying the foundations of a more satisfactory system, and will help to point out the gaps in our knowledge as regards the muscles in many important types of birds. The author does not entirely confine himself to a description of the raven: other types are referred to in special cases when certain muscles are not represented in the latter; and comparisons are given in many cases with other forms—mammals as well as birds.[1]

Though acknowledging the importance of the question of nerve-supply in helping to determine the homologies of muscles, the author by no means considers this to be an infallible guide; for the representative of the same muscle in various vertebrates is not always supplied by the same nerve. He also quotes some remarks of his own from a previous paper (referred to in the text as No. 124 in the Bibliography, instead of No. 121), with regard to the value of muscles in classification, which are of importance in helping to relegate the muscular system of birds to its proper subordinate position in taxonomy. While fully recognizing the value of Garrod's work in this field, it must be agreed that his myological formulæ only represent one set of characters, which must be taken in conjunction with all others if they are to be of any value in classification.

The various groups of muscles are treated in order, beginning with the dermal system, and a chapter is devoted to each main set. In each group, directions are given how to proceed with the dissection. The figures, of which there are 76, are on the whole excellent, and with few exceptions are original: they represent the different parts of the skeleton, showing the origins and insertions of the muscles, as well as dissections of the muscles themselves. A copious bibliography and index are given at the end of the book.

It is almost impossible to comment on the details of the work without having first made use of it practically. But we can congratulate Dr. Shufeldt on the production of an original and well-arranged text-book, the result of much patient labour in collecting and dissecting, and careful thought in arrangement. The volume will appeal especially to ornithologists, as well as to students of comparative anatomy.

OUR BOOK SHELF.

Chemical Arithmetic. Part I. By W. Dittmar, LL.D., F.R.SS. Lond. and Edin. (Glasgow: William Hodge and Co., 1890.)

THIS volume is not a mere collection of tables extracted from the numerous books which deal with this subject, but is a good and trustworthy piece of work, with no lack of originality about it, and contains the mathematical auxiliaries and both chemical and physical constants which a chemist needs "in the ordinary routine of his laboratory work." Some of the tables contain the results of the author's work, and others have received slight alterations, been put in more convenient forms, and brought up to date.

Indexed tables of three-, four-, and five-place logarithms are given, and following these will be found formula values, F, of a number of substances and radicals and their logarithms, analytical factors and their logarithms, gas volumetric determinations, metric and British systems of units, &c. Near the end there is represented in diagrammatic form a double thermometer scale, with Fahrenheit degrees on one side and Centigrade on the other, ranging from $-148°$ F. to $+392°$ F., by which a result in degrees Fahrenheit can be directly stated in terms of the Centigrade, or *vice versâ*. Throughout the book the author seems to have taken the utmost pains to eliminate all errors that are likely to be made in a work of this sort, and by this means he has placed before the scientific as well as before the technical chemist a useful and trustworthy reference book.

Dictionary of the Language of the Micmac Indians. By Rev. Silas Tertius Rand, D.D., LL.D. (Halifax, N.S.: Nova Scotia Printing Company, 1888.)

THE Micmac Indians are an aboriginal tribe of the Algonquin family, residing in Nova Scotia, New Brunswick, Prince Edward Island, Cape Breton, and Newfoundland. The late Dr. Rand, the compiler of the present work, laboured among them as a missionary for more than forty years. He was a man of great learning and ability, and soon not only mastered the language of the Micmacs, but devoted himself to the task of reducing it to writing. He also translated into it the New Testament and portions of the Old Testament, and claimed to have collected and arranged in alphabetical order no fewer than 40,000 Micmac words. His dictionary consists of a Micmac-English and an English-Micmac part. Greatly to their credit, the Dominion Government paid for the manuscript of both parts, and now they have issued the English-Micmac section of the work. The other section must necessarily be of more scientific interest, but this volume may be of greater practical value. Even from the scientific standpoint, it cannot fail to be of service to philologists. Most students will probably be surprised to find how highly developed a language the Micmac is. It was strongly admired by Dr. Rand, who went so far as to say that in various important particulars it would not suffer from "a comparison with any of the most learned and polished languages of the world."

Elementary Manual of Magnetism and Electricity. By Andrew Jamieson, M.Inst.C.E. (London: Charles Griffin and Co., 1890.)

THIS work is arranged in the form of a series of lectures, and covers the ground laid down in the syllabus for the elementary stage of the Science and Art Department examinations. The subject is divided into three distinct parts. Part I. treats of magnetism; Part II. of electromagnetism and current electricity, under the general heading voltaic electricity; while Part III. deals with frictional electricity. At the conclusion of each lecture a number of examples is given, and in many cases a test question, with the answer. By no means less important are the appendices to each part, in which students will find all the necessary information for making experimental apparatus and for conducting experiments with them. Throughout the book the author has made many references to the practical applications of these experimental facts, such as telegraphy, telephony, electric lighting, &c., and has given some very good illustrations and diagrams, in which the various points for which they were intended have been brought out effectively.

In the present manual, only an elementary knowledge of arithmetic is required in order to follow the expositions; but in the advanced text-book, which the author informs us is now in preparation, and to which the present volume will form an introduction, an elementary knowledge of mathematics will be assumed.

[1] Frequent references are made to Owen's description of the muscles of Apteryx. Quite recently T. J. Parker has shown that some eleven or twelve muscles are present in the vestigial wing of this form in addition to those described by Owen.

The Stereoscopic Manual. By W. I. Chadwick. (London: John Heywood, 1890.)

IN this little manual the author takes up the case of the once popular instrument the stereoscope, and shows how by means of recent improvements very good results may be obtained. The first two chapters are devoted to the subject of binocular vision and the theory of the stereoscope; then follow some practical points concerning the production of stereoscopic photographs. To enable the stereoscopic camera to be used as an ordinary camera, the author has devised a spring roller and curtain division that can be removed in an instant when single views are required, and replaced as quickly for stereoscopic work again. In the remaining part of the book are short chapters on the best lenses to use, transposing and mounting of slides, transparencies on glass, and combination printing. Throughout the manual the author has given enough information as regards the mode of procedure to enable one to obtain a finished picture, so that for amateurs and others who are thinking of taking up this branch of photography the book should prove a most useful and efficient guide.

Astronomy: Sun, Moon, and Stars, &c. By William Durham, F.R.S.E. (Edinburgh: A. and C. Black, 1890.)

IN this little work we have a clear and interesting account of the more important facts connected with the science of astronomy. It treats of the sun and moon, the earth, stars, nebulæ, planets, astronomical speculations—such as those relating to the formation of the heavenly bodies—concluding with chapters on the tides, light and spectrum analysis. Perhaps an improvement might be made in the order in which the chapters are arranged. The first two deal with some bodies in the solar system—namely, the sun, moon, and earth; the next one treats of the stars, nebulæ, &c.; while in the fourth we are suddenly brought back to the solar system in connection with the planets.

The information is well up to date, and is presented in simple and plain language. The author gives a very good idea of the present general knowledge on these subjects, without entering into details which would tend to puzzle the reader for whom works of this kind are written.

The Life Story of Our Earth. By N. D'Anvers. (London: George Philip and Son, 1890.)
The Story of Early Man. By N. D'Anvers. (Same Publishers.)

THESE volumes belong to the "Science Ladders" series, which consists of simple reading-books in elementary science for the young. The author has selected his facts with care, and presents them in a plain straightforward style, well adapted for the purpose for which the books have been prepared. There are 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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Great Mogul's Diamond and the Koh-i-nur.

IN the review of my edition of "Tavernier's Travels" which was published in NATURE (vol. xli., p. 313), the writer having commented, with approval, on the arguments advanced by me in favour of the hypothesis that the Koh-i-nur can be identified with the Mogul's diamond, proceeds to say:—

"But as regards the identity of the Koh-i-nur and the Mogul diamond, there remains one objection which, as it appears to us, Mr. Ball has hardly adequately disposed of. If Tavernier's figure, as reproduced by Mr. Ball, represents at all faithfully the general form and especially the height of the Mogul diamond, it is difficult to see how a comparatively flat stone like the Koh-i-nur could have been obtained from it without a much greater reduction of its weight than the 82 carats which are all that his data admit of. The lateral dimensions of the two stones accord fairly enough, so that any reduction of Tavernier's figured stone, to bring it down to the required size, could be effected only by diminishing its height; in which case it would hardly correspond to his description of its form as that of an egg cut in two. The question can only be fairly tested by the weighment of a model constructed, as nearly as possible, in accordance with Tavernier's figure, and of such lateral dimensions as to be capable of including the Koh-i-nur. It may be found that such a model, of the specific gravity of the diamond, would be found much to exceed Tavernier's reported weight of the stone, in which case the importance of his figure as an item of evidence would be invalidated."

This very point suggested itself to me also for investigation long ago; but hitherto from want of a suitable model I postponed undertaking it. Recently I have obtained a glass model, which coincides, exactly, with Tavernier's figure; another model, of which an example has been examined by me, is too small in diameter. These models are included in the well-known collections of glass models of famous diamonds; from them the often faulty figures given in works on precious stones have generally been drawn.

On submitting the first-mentioned model to weighment, it proves to represent a diamond not of 279·56 Florentine carats, *i.e.* 268·38 English carats, as it should; but it represents a diamond of 437·9 carats—in other words, it includes an excess of 169·52 carats. It is therefore far too large, and if we consider in what respect it is most likely to be defective it is surely in the height and general mass of the stone. Tavernier was not an artist—he admits as much himself; but he probably measured the diameter of the stone accurately, though his sketch or diagram was otherwise exaggerated. Out of a flatter stone with the same diameter, the Koh-i-nur might easily have been cleaved, with a loss of about 82 carats (268·38–186·6). And the original might, to some extent, have resembled, as he describes it, an egg cut in two, not across, perhaps, but longitudinally.

It may be asked, Can the true form of the Mogul's diamond be approximately reconstructed? I think it can. By taking an authentic model of the Koh-i-nur, as it was when brought to England, and by replacing symmetrically on the cleaved surfaces and upon the upper portion of the stone an amount of material equivalent altogether to 82 carats, the result would give, I venture to believe, a very close approximation to the form of the Mogul's diamond as it was when seen and described by Tavernier.

Many collateral facts which have come to my knowledge since I wrote on this subject, but which I cannot trespass so far on your space as to detail here, tend to confirm the hypothesis that the Koh-i-nur is identical with the Mogul's diamond—less by certain portions which were demonstrably removed by cleavage, after the original cutting.

It may be added that the models of some of the other famous stones in the collections above referred to also prove to be very inaccurate in their dimensions; none of them, however, are so outrageously exaggerated as that which professes to represent the Mogul's diamond.

Little wonder is it, therefore, that some writers should assert that such a stone no longer exists, for it never did exist in the form and of the size represented by these models, and by the figures prepared from them for works on precious stones.

V. BALL.

Science and Art Museum, Dublin, November 29.

Pectination.

IN looking over a collection of skins early in the present year, I was struck by the recurrence of the pectination of the claw of the third digit in many different orders of birds; such a structure occurring among the owls, nightjars, herons, and gannets. The fact is, of course, well known; but the popular explanation of it—that the serration is useful in enabling the bird to hold its prey (fish, &c.) more securely—cannot be correct: first, because the serration is small in extent, and only present in a single claw; and secondly, because it is at the side, and not on the under surface, of that claw. At the same time, it is too widespread and well-marked a characteristic not to be functional.

The pectination seems to attain its greatest development in the nightjars; and it has been said that these birds utilize it for the purpose of cleaning their mouth-bristles. But the bristles and the pectination do not by any means always occur together, even among the nightjars; while many other birds (*e.g.* barbets) have the former without the serration.

In order to investigate the point, I procured a young common heron (*Ardea cinerea* ♂?) in June last, and have had the bird under more or less close observation from that time until the middle of October. It fed freely for the most part, and continued healthy during the whole of this time. Its food, whether living or dead, and whether taken from water or from the ground, was never touched at all by the feet. The only use to which the serrated claw was put, that I could observe, was that of scratching the cheeks and throat. In this action—which occurred most frequently after a meal—the two other front toes were curved down, so as to leave the middle claw free.

It is, however, the case that other birds, in which the pectination is absent, use the middle claw alone in scratching. Nor would the presence of this peculiarity seem to give any great advantage to its possessor in this connection, owing to its lateral position. I fear, therefore, that I can claim to have done no more than disprove experimentally what hardly needed disproving—the popular theory alluded to above.

Inselstrasse 13, Leipzig, Nov. 25. E. B. Titchener.

The Common Sole.

The correction from Mr. Green, H.M. Inspector of Irish Fisheries, which is published in Nature of November 20 (p. 56), and which announces that the specimens he at first believed to be the young of the common sole proved to be really the young of *Pleuronectes cynoglossus*, is one more instance of the great importance of specific identification in the investigation of fishery problems. This was a mistake not merely as to the species, but as to the genus of the specimens in question. And yet it is by no means difficult, after a little experience, to distinguish the genus *Solea* at any stage of development from all other genera of *Pleuronectidæ*. It is much more difficult to distinguish from one another the various species of Gadus in their early stages. But in order to ascertain the life-history of the various species of sea-fish used as food, we must trace each with absolute certainty from one environment to another through the successive stages of its existence.

It is natural enough that the young fry of *Pleuronectes cynoglossus* should be found in deep water. The adult of this species has an exceedingly wide bathymetrical range. It is found in abundance on our ordinary trawling grounds in the North Sea at 15 to 30 fathoms, and I have taken it in numbers in the Firth of Clyde. In the same region which Mr. Green investigated, Mr. G. C. Bourne found it at 70 fathoms and at 200 fathoms. The Norwegian North Atlantic Expedition found it at 125 to 150 fathoms off the Lofoten Islands, while on the North American side of the Atlantic it has been taken at 120, 263, 395, 603, and 732 fathoms. On the other hand, *Solea vulgaris* has never been found at a much greater depth than 60 fathoms.

Seeing that I have found young soles immediately after metamorphosis between tide-marks, it would have been extremely surprising if the next stage only occurred beyond 50 fathoms. Most probably the fry of the sole will be found pretty uniformly distributed over the grounds where the adults live. After the stage I have mentioned, they are not found in shallow water. I have some evidence that the above suggestion is true, for specimens from 3 to 6 inches in length have been recorded for us as captured by the large beam-trawl in the North Sea in February and March. These specimens would be a year old: in order to find younger and smaller specimens between June and Christmas a small-meshed trawl would have to be worked on the ordinary trawling grounds. The Marine Biological Association has not at present sufficient funds to give me the means of doing this. I have had to obtain my results by going out in deep-sea trawlers, living with the men, and taking whatever opportunities occurred in the course of the regular fishing operations. J. T. Cunningham.

Marine Biological Laboratory, Plymouth, Nov. 21.

Weights Proceeding by Powers of 3.

The following rule gives the allocation of weights of 1, 3, 9, &c., necessary for making up any given weight.

Find the least value of $\frac{1}{2}(3^n - 1)$ which exceeds, or is equal to, the given weight. Add it to the given weight, and express the sum in the ternary scale. The digits will indicate the weights to be used, and the pan in which they are to be put, according to the following scheme:—

Digit 2 indicates the positive pan,
,, 0 ,, ,, negative ,,
,, 1 ,, that the weight is not to be used.

Example.—Let 500 be the given weight to be made up. The number to be added is $\frac{1}{2}(2187 - 1) = 1093$, and the work is as follows:—

			Positive pan.	Negative pan.
500				
1093				
3)1593				
3)531	remainder	0	— ...	1
3)177	,,	0	— ...	3
3)59	,,	0	— ...	9
3)19	,,	2	27 ...	—
3)6	,,	1	— ...	—
3)2	,,	0	— ...	243
0	,,	2	729 ...	—

756 − 256 = 500.

I should be glad to learn whether this rule has been previously published. J. D. Everett.

5 Princess Gardens, Belfast, November 18.

Measures of Lunar Radiation.

In your issue of the 13th inst. (p. 44) you have, under the above heading, a notice of a paper in vol. xxiv. of the Proceedings of the American Academy of Arts and Sciences, by Mr. C. C. Hutchins.

The so-called "new thermograph" is obviously, with very slight differences in constructive details, the apparatus used here for some years past. We have employed the thermo-couple in place of the ordinary thermopile in our determinations of lunar radiant heat from the year 1870, and the condensing mirror of focal length about equal to its aperture from the commencement of our heat-work in 1869 (Proc. Roy. Soc., No. 112, 1869, and Nos. 122 and 123, 1870); and about four years ago we replaced the brittle bismuth and antimony alloys by the more tractable metals iron and German silver, for the same reasons which weighed with Mr. Hutchins.

We cannot give an unqualified assent to Mr. Hutchins's conclusions as to absorption of lunar radiant heat in our atmosphere, but I will defer remarks upon this for the present; I may say, however, now, that I have looked over Mr. Hutchins's paper, and he appears to have entirely overlooked Dr. Copeland's extensive and careful work here on the lunar phase curve and the law of absorption in our atmosphere (Phil. Trans., vol. clxiii. p. 587), or he would not have made the sweeping assertion that, "previous to the invention of the bolometer, no instrument existed capable of dealing accurately with so small an amount of heat as the moon affords," a statement which, from what I know of Prof. Langley, I feel confident he would not fully endorse, notwithstanding his success in working with that instrument.

In conclusion, may I congratulate Mr. Hutchins on having taken up with so much apparent manipulative skill and energy so interesting an investigation, and one in which the workers have been so few, and wish him every success? Rosse.

Birr Castle, Parsonstown, November 29.

The Distances of the Stars.

It is quite a familiar illustration to represent the distances of the stars in terms of the *light-year*, but I am not aware that it has been noticed that the same figures which express in years the time light occupies in reaching the earth from a star, will also express in miles the distance of the star upon a scale of the radius vector of the terrestrial orbit to the inch. The illustration appears to me useful, as it gives, perhaps, a more distinct idea of the isolation of the solar system in space than can be otherwise obtained, and does not introduce the question of time into the measurement of distance. Thus if the annual parallax of 61

Cygni is assumed to be 0"·434, which is probably very nearly correct, it will take 7½ (7·464) years for its light to reach the earth, and 7½ (7·499) miles will represent the distance of this star on a scale that gives 1 inch to the distance of the sun from the earth.

JOHN I. PLUMMER.

8 Constitution Hill, Ipswich.

Great Waterfalls.

I SHALL be much obliged to any of your readers who can inform me where I can find descriptions of the waterfalls named below:—

Falls of the Rio Grande near Guadalajara, Mexico, referred to by Miss Kingsley in "South by West."

Falls in the Ala-tau Mountains, Central Asia, stated in the "Universal Geography" as consisting of falls of 600, 350, and 350 feet separated by rapids.

Lattin, in Swedish Lapland, stated in the "Universal Geography" as of the height of 400 feet. It is also mentioned in the "Popular Cyclopædia," but I could hear nothing of any waterfall of that name when travelling in Lapland some few years since.

Aguara-y, in Paraguay, also classed amongst the great waterfalls of the world in the table in the "Universal Geography," as of the height of 409 feet.

Falls of the Pykara, India.

Falls of the Ooma Oya and Badulla Oya, Ceylon.

Any particulars of any of these falls will be most useful.

ARTHUR G. GUILLEMARD.

Eltham, Kent, November 22.

AID TO ASTRONOMICAL RESEARCH.

A CIRCULAR was issued last summer announcing the gift by Miss Bruce of 6000 dollars for aiding astronomical research. No restrictions were made upon its expenditure which seemed likely to limit its usefulness, and astronomers of all countries were invited to make application for portions of it, and suggestions as to the best method of using it. Eighty-four replies have been received, and with the advice of the donor the entire sum has been divided so as to aid the following undertakings:—

(3) Prof. W. W. Payne, Director of the Carleton College Observatory. Illustrations of the *Sidereal Messenger*.

(6) Prof. Simon Newcomb, Superintendent of the *American Nautical Almanac*. Discussion of contact observations of Venus during its transits in 1874 and 1882.

(16) Dr. J. Plassmann, Warendorf. For printing observations of meteors and variable stars.

(23) Prof. H. Bruns, Treasurer of the Astronomische Gesellschaft. To the Astronomische Gesellschaft for the preparation of tables according to Gyldén's method for computing the elements of the asteroids.

(27) Prof. J. J. Astrand, Director of the Observatory, Bergen, Norway. Tables for solving Kepler's problem.

(29) Prof. J. C. Adams, Director of the Cambridge Observatory, England. Spectroscope for the 25-inch telescope of the Cambridge Observatory.

(36) Prof. A. Hirsch, Secretary of the International Geodetic Association. To send an expedition to the Sandwich Islands to study the annual variation, if any, in latitude.

(40) Mr. H. H. Turner, Assistant in Greenwich Observatory. Preparing tables for computing star corrections.

(45) Prof. Edward S. Holden, Director of the Lick Observatory. Reduction of meridian observations of Struve stars.

(46) Prof. Lewis Swift, Director of the Warner Observatory. Photographic apparatus for 15-inch telescope.

(54) Prof. Norman Pogson, Director of Madras Observatory. Publication of old observations of variable stars, planets, and asteroids.

(57) Dr. Ludwig Struve, Astronomer at Dorpat Observatory. Reduction of observations of occultations during the lunar eclipse of January 28, 1888, collected by the Pulkowa Observatory.

(60) Dr. David Gill, Director of the Observatory of the Cape of Good Hope. 1. Reduction of heliometer observations of asteroids. 2. Apparatus for engraving star charts of the Southern *Durchmusterung*.

(78) Prof. A. Safarik, Prague. Photometer for measuring variable stars.

(79) Prof. Henry A. Rowland, Johns Hopkins University. Identification of metals in the solar spectrum.

Of the remaining replies many describe wants no less urgent than those named above. Some relate to meteorology or physics rather than to astronomy, some to work already completed, and others were received too late to be included. Two important cases may be specially mentioned. In each of them an appropriation of a part of the sum required would have been made; but in one, in our own country, an active and honoured friend of the science undertakes the whole; and in the other, in France, the generous M. Bischoffsheim, already known as the founder of the great Observatory at Nice, ignoring political boundaries and the comparative selfishness of patriotism, came forward and gave the entire sum required. The same sky overarches us all. It is to be hoped that the above-named, and other foreign institutions, will obtain more important aid from neighbours when these become aware how highly the work of their men of science is appreciated in this country. The replies not enumerated above are confidential, and cannot be mentioned except by the permission of the writers. But they have placed me in possession of important information regarding the present needs of astronomers. In several cases a skilful astronomer is attached to a college which has no money for astronomical investigation. He has planned for years a research in the hope that some day he may be able to carry it out. A few hundred dollars would enable him to do this, and he offers to give his own time, taken from his hours of rest, if only he can carry out his cherished plans.

Such valuable results could be attained by the expenditure of a few thousand dollars, that no opportunity should be missed to secure this end. Fortunately, the number of persons in the United States able and willing to give liberally to aid astronomy is very large. It is hoped that some of them may be inclined to consider the case here presented. The income derived from a gift of one hundred thousand dollars would provide every year for several cases like those named above. A few thousand dollars would provide immediately for the most important of the cases now requiring aid. The results of such a gift would be very far-reaching, and would be attained without delay. Correspondence is invited with those wishing to aid any department of astronomy, either in large or small sums, by direct gift or by bequest.

EDWARD C. PICKERING.

Harvard College Observatory,
Cambridge, Mass., U.S.A., November 11, 1890.

NOTE ON THE DISAPPEARANCE OF THE MOA.[1]

MAJOR MAIR, in an interesting paper on the disappearance of the Moa in vol. xxii. of the Transactions of the New Zealand Institute, makes, on p. 71, the statement that he is a "supporter of the belief

[1] Read before the Philosophical Institute of Canterbury, October 2, 1890, by H. O. Forbes, Director of the Canterbury Museum, Christchurch, New Zealand.

that the Maoris," from the absence of all mention of the bird from their songs and traditions, "never had any personal knowledge of the Moa." Major Mair so intimately knows the history and literature of the Maoris, and their habits and modes of thought, that one—especially one like myself who has had time as yet to acquire only a small amount of experience of New Zealand things—can scarcely hope to contribute any suggestion on the subject of the history of the Moa which has not occurred to this specialist.

Still, the following observations, made last year during a very complete exploration of a recently discovered cave in the property of Mr. Monck, near Sumner, lead me to think that the Maoris must have been personally acquainted with the *Dinornis*. The exploration was conducted under my own direction by two very trustworthy workmen, and the more important finds in it have been described in a paper read before the Institute last session by the President. The cave, it is certainly known, has been closed since before the advent of Europeans to Canterbury, but for how long before it is impossible to determine. The condition of the cave on entry gave all the appearance of having been untouched since the last dwellers in it left it. Its entrance was covered over by a very extensive landslip, which evidently fell during the absence of its frequenters, as no human bones were discovered in it. Quarrying occupations have been carried on amid the material of this landslip for between twenty and thirty years. These operations, on reaching last year the live rock of the hills, disclosed an aperture through which a lad squeezed himself into the cave. On its floor were found implements in wood and in greenstone, half-burned pieces of timber and fire-making apparatus, so lying as to give the doubtless correct impression that its occupiers intended to return. The greenstone objects were beautifully made, some of them; while the implements of wood, such as the canoe bailer, the paddle, and the fragment of a paddle handle, exhibit ornamentation characteristic of the Maoris. On the floor of the cave were found also numerous largish fragments of Moa bones, partly burned and partly broken, scattered round the last fireplace, or lying on the surface of the ground in the inner caves. In the kitchen-midden in front of the cave were found many fish-hooks and barbed spear tips, made of bone from the same birds. On the surface I picked up several bones of more than one individual of a species of swan, which I described to the Institute last year under the name of *Chenopis sumnerensis*. Just below the surface of an untouched part of the midden, I myself picked out pieces of Moa egg-shell, each with its shell membrane perfectly preserved. The question, therefore, stands thus: The Moa egg-shells, being among the refuse of the feasts of the occupants of the cave, who used the carved bailer, are the remains, it is legitimate to argue, of the eggs they used for food. There is no purpose I can think of for which pieces of shells of rotten eggs could be used; for eggs exposed on the ground, or buried under the soil, with their contents, would soon burst or disintegrate into fragments. It may be inferred, consequently, that these eggs were found in a more or less fresh condition, and were brought into the cave for food purposes. If they were sufficiently fresh for food, it is obvious that the birds that laid them could have been then still living, and probably were so; and that the bones from which the frequenters of this cave made their implements were as likely to be obtained directly from birds which they killed or might have killed. It may be suggested that eggs of Moas might have been found sufficiently whole to be used for utensils. The fragments that I found could not have been so used, as demonstrated by the condition of the membrane lining the interior. In the Sumner caves explored by Sir Julius von Haast, and at many Maori encampments, the remains of Moa eggs have been found abundantly in the kitchen-middens, and in such positions among the *débris* of their meals as to suggest that they had been used for food.

The black swan (*Chenopis atrata*), the only undomesticated swan in the country, was introduced into New Zealand from Australia a number of years after the settlement of Canterbury. The bones of the swans found in the Sumner cave were consequently also brought there by the feasters who ate the Moa eggs, and they, too, must have been, therefore, contemporaries of the Moa.

The figure of a dog carved out in wood was also discovered in the cave. It probably formed the termination of the handle of a paddle. A good figure of it may be found in the President's paper to which I have already referred. The Maori dog must, therefore, also have been contemporaneous with the Moa, and with the now, in New Zealand, non-indigenous (if not altogether extinct) swan *Chenopis sumnerensis*.

The fishing family, or families, who fed on the Moa eggs, and who last occupied the Sumner cave, were, as far as the style of their ornamentation and handiwork can decide for us, as much Maoris as those who carved the woodwork of the typical Maori dwellings of the King Country, or executed the ornamentation of which our museums exhibit specimens labelled "Maori"; and they were Maori in contradistinction to an earlier more primitive people who have been named "Moa-hunters," as is testified by their highly-executed and polished greenstone work.

How long ago it is since the Maori and the Moa were living together no evidence has yet been afforded by the Sumner cave explorations. A very great deal still remains to be done in the determination of the extensive osteological and other material obtained. It is being slowly examined; and when this work has been accomplished, some more light may possibly be thrown on the question of which this note forms the subject.

GLACIAL STRIÆ AND MORAINIC GRAVEL IN NORWEGIAN LAPLAND FAR OLDER THAN "THE ICE AGE."

AROUND the inner part of the Varanger Fjord, the mountains are low, and consist chiefly of sandstone and conglomerate, the strata of which lie in a nearly horizontal position. Between the village of Nesseby and the farm, Mortensnes, a mass of unstratified conglomerate or breccia occurs at least 50m. thick. Its component stones, which have been mostly derived from Archæan rocks (gneiss, &c.) are not properly water-worn pebbles, but have only their edges rounded, while flat faces may often be observed among them. A few of them consist of dolomite. On some of these fragments very distinct striæ occur, while similar markings may occasionally be detected on other kinds of material among the included stones. As in recent moraines, it would seem that here also the depth and distinctness of the engraving have had some relation to the relative hardness of the material. Not far from this conglomerate a smaller layer of a similar rock lies in the sandstone. The conglomerate is here very friable, and by its weathering a part of the upper surface of the hard sandstone under it has been laid bare. On this surface some excellent glacial furrows have been preserved. I had the pleasure of laying before a meeting of my geological colleagues on October 27 my specimens and diagrams. They all agreed with me in believing that in this deposit we have evidence of glacier-action dating back to the time of the sandstone and conglomerate of Lapland. The geological age of these strata is not yet settled, as no fossils have been found in them. Dr. Dahll has referred the formation to the Permian period. I think it not improbable that it belongs to the Cambrian or Silurian series, which in other parts of Scandinavia play so large a part in our mountain system.

Further details will be published in the year-book (*Aarbog*) of the Geological Survey of Norway for 1890.

Kristiania, November 29. HANS REUSCH.

NOTES.

THE anniversary meeting of the Royal Society was held on Monday at Burlington House. Sir G. Stokes resigned the presidency in favour of Sir William Thomson. The new Council was elected, and Sir G. Stokes presented the medals, a list of which we have already published. The official report of the proceedings is not yet ready. The annual dinner was held at the Hôtel Métropole in the evening, and, as usual, it was very numerously attended. Many of the speeches were so long that it is impossible to reproduce them here, but it is fitting that Dr. Hopkinson's reply for the medallists should be put on record. He said :—"You have done me great honour in asking me to respond to this toast, but the honour carries its burden with it. I could have wished for your sakes that the duty had been in hands abler for its performance. In matters of science, so far as nationality exists at all, it must be held to depend not upon the accidents of government, but upon community of descent and community of language. If you were to ask Prof. Newcomb, I have no doubt he would tell you that he was of the same nation as Bacon and Newton. We are undoubtedly of that nation, it therefore follows that Prof. Newcomb is our fellow-countryman. But intellectually Newcomb is a descendant of Newton in a peculiar sense. Newton not only originated that greatest of all scientific generalizations, the law of universal gravitation, but followed out in some measure its effects in the perturbation of the bodies of the solar system. Newcomb has attained to the proud position he now holds by advancing the theory of gravitation in its application to the details of the motion of the moon and some other of the planetary bodies. When Prof. Newcomb visited Birmingham some 16 years ago, he did me the honour of being my guest. He had then attained a high reputation. I little thought that on the day on which he should receive the highest honour the Royal Society has to bestow, I should myself receive a Royal Medal. Of Prof. Hertz and his work, I could say much of its connection with our own Maxwell, and of its immediate and enthusiastic appreciation in this country; but Prof. Hertz is here to-day, and can answer for himself far better than I could have answered for him. It is one of the boasts of modern science that she can accomplish by inanimate means many results which it was supposed that Nature had reserved for the laboratory of the living body. The work of Prof. Fischer belongs in part to this class; amongst other work he has produced by synthesis from inorganic sources many definite sugars, and in so doing has greatly extended the territory of chemistry into the domain of physiology. Rather than speak of Mr. Wallace's work, I would set him forth as an example of that chivalrous feeling which one would wish men of science should always exhibit. You all know the story of how Wallace had worked out the theory of natural selection and was ready to publish; of how he learnt that Darwin had worked it out further and worked at it earlier, and how he postponed his own publication till Darwin had time to take the first place. When we read of the noble work which is being done in France and in Germany by Pasteur and by Koch, and others, we feel with much bitterness that we do not live in a free country. The work which Dr. Ferrier has done on the brain proves that we have men here as capable of physiological discovery by experiment as anywhere abroad, and that, if there is fear that we may fall behind, it is because our investigators are unnecessarily hampered in their work. My own case in physics is very analogous to Dr. Ferrier's in physiology—we are both of us professional men, we both of us desire to further the pure science of our subjects on lines suggested by our professional work. I say deliberately that if I had been obliged to obtain the sanction of a Government Department to make experiments or to make them in a licensed place it is very little experimental work I could have done; I should not have been in the proud position of one of your medallists to-day. Neither physics nor physiology can be efficiently advanced by mere observation: the more powerful method of experiment is necessary. If we must have laws restraining our best men, let us at least admit what it costs us. Speaking for myself, looking back, I have been fortunate in my surroundings. My father cultivated in me a taste for science from a time before I can remember, my mother gave me the first systematic instruction of which I have any recollections. If my father gave me my first taste for science, you may be sure that taste was encouraged at Owens College. Mathematics is the most essential weapon of the physicist, and nowhere can mathematics be learned as at Cambridge. I owe to Sir William Thomson the first impulse to experimental work in electricity and magnetism. He has been to me for many years the kindest of friends, always ready to encourage and to help. Looking at the present, I admit that your Council, in awarding me a Royal Medal, have raised me not a little in my own estimation. There is one point of view from which this cannot be regretted, if a man's past work obtains for him honour from the highest and most competent tribunal: surely that should make him feel that it is incumbent upon him to endeavour to do more and better work of the same kind. This, gentlemen, speaking for the other medallists and for myself, is the best thanks we can return you—to endeavour to justify you in the future in giving us these honours to-day."

LETTERS have been received from Mr. J. Graham Kerr, Naturalist to the Pilcomayo Expedition, dated from the s.s. *Bolivia* on that river, in lat. 24° 58′ S., long. 58° 40′ W., on October 4 last. The vessel had got so far up the Pilcomayo with great difficulty, owing to the extreme shallowness of the water, and had stuck exactly in the same spot since June 14. They had almost given up all hope of ever getting out, when on October 4 a relief party of twenty soldiers reached them and brought assistance. Some of the soldiers and Captain Page's son (Captain Page himself having died on August 2, whilst proceeding to Corrientes for medical advice) came back immediately by land, while Mr. Graham Kerr and the remainder of the party proposed to return down stream in the *Bolivia*. If that turned out to be impossible, it had been determined to burn the boat, and return to the La Plata overland. The Pilcomayo Expedition must therefore be considered to be at an end.

THE Scientific Committee of the Royal Horticultural Society have undertaken, under the direction of Dr. D. H. Scott and Dr. Francis Oliver, to investigate the effects of London fogs on cultivated plants. The Royal Society has granted £100 in aid of the experiments.

MANY horticulturists feel that there should be some permanent memorial of the services rendered by the late Mr. Hibberd to horticulture. A meeting, therefore, will be held to take the matter into consideration.

THE authorities of the Natural History Museum have placed in the Central Hall of that institution a small temporary exhibit, consisting of a set of highly magnified daawings of Bacteria. It includes such prominent forms as *Bacillus tuberculosis*, Koch, and the *Bacillus* of fowl cholera, and is the work of Dr. W. Migula.

THE Board of Agriculture proposes the appointment of a widely representative body under whose direction all examinations in dairy work shall be held.

The Photographic Society has now secured premises which are likely to be suitable for its work. At the meeting of the Society on November 11, Mr. W. S. Bird gave all necessary information on the subject. The new quarters are "in a fine building of considerable architectural pretension," exactly opposite the centre of the British Museum. They consist of one large room, capable of holding about sixty persons; a smaller room, suitable for a library; a good dark-room; and a smaller one for a similar purpose.

The workshop accommodation of the Physical Laboratory in the University College of Wales, Aberystwith, has recently been considerably extended, and arrangements are being made for a course of instruction in electrical engineering. The recent additions include a Crossley D. high-speed electric lighting engine, Crompton dynamo (shunt and compound-wound), and a Crompton D.D. arc lamp, &c.

The following lectures on scientific subjects will be delivered at the Royal Institution before Easter:—Prof. Dewar, six Christmas lectures to juveniles, on frost and fire; Prof. Victor Horsley, nine lectures on the structure and functions of the nervous system (part i. the spinal cord, and ganglia); Prof. C. Meymott Tidy, three lectures on modern chemistry in relation to sanitation; the Right Hon. Lord Rayleigh, six lectures on the forces of cohesion. The Friday evening meetings will begin on January 23, when a discourse will be given by the Right Hon. Lord Rayleigh on some applications of photography; succeeding discourses will probably be given by Lord Justice Fry, Prof. J. W. Judd, Prof. A. Schuster, Dr. E. E. Klein, Mr. Percy Fitzgerald, Dr. J. A. Fleming, Dr. Felix Semon, Prof. W. E. Ayrton, and other gentlemen.

The second series of lectures given by the Sunday Lecture Society begins on Sunday afternoon, December 7, in St. George's Hall, Langham Place, at 4 p.m., when Sir James Crichton Browne, F.R.S., will lecture on "Brain Stress." Lectures will subsequently be given by Mr. Whitworth Wallis, Mr. Edmund Gosse, Mr. Eric S. Bruce, Dr. Henry Hoole, Sir R. S. Ball, and Prof. G. S. Boulger.

Early on Monday morning the inhabitants of the valley of the Ness, in Inverness-shire, were alarmed by another shock of earthquake. The *Times* says that a slight tremor was felt shortly before midnight, and this was followed about ten minutes past 12 o'clock by a sharp shock, accompanied by a low rumbling noise. People were awakened by the shaking of their houses, but the shock was of short duration, and was not so serious as that experienced a fortnight ago.

The United States National Electric Light Association will hold its thirteenth convention at Providence on February 17, 18, and 19, 1891. The following subjects will be dealt with:—How can the National Electric Light Association best serve central station interests? by C. R. Huntley; distribution of steam from a central station, by F. H. Prentiss; distribution and care of alternating currents, by T. Carpenter Smith; municipal control of electric railroads, by M. W. Mead; the Ferranti system, by C. B. Haskins. *Science* says that the committee has not only secured the promise of these papers, but has named a person to open the discussion on each paper—a plan which can hardly fail to increase the value and interest of the proceedings.

In his message to Congress, presented on Monday, President Harrison said Congress would be asked to provide means for the acceptance of the invitation of Italy to take part in a Conference to consider the adoption of a universal prime meridian. He further suggested the passing of a general law giving the Executive discretionary authority to accept foreign invitations to Conferences having for their objects international reforms in science, sanitation, &c.

There is much complaint among American men of science with respect to the effect of the McKinley Bill on the cost of scientific instruments. The tax on microscopes has been raised to 60 per cent., so that a microscope which can be bought in Germany for 94 dollars costs in the United States 150.40 dollars. "Not content," says the New York *Nation*, "with the attack on the microscope, McKinley increased the duty on mathematical glass instruments from 45 to 60 per cent. *ad valorem*. Everybody knows the importance of glass tubes in laboratory work; well, the brave McKinley increased the tax on them from 40 to 60 per cent. Everyone knows the value of telescopes in astronomy and navigation. McKinley has increased the duty on them from 45 to 60 per cent. His keen protective eye also fell on 'raddle,' a substance used in polishing lenses for microscopes and telescopes, so he put the duty on that at 1.50 dollars per ton. What rate per cent. this is we are unable at this moment to say, but nobody but an ignorant barbarian would have taxed it at all. Everyone knows the value of spy-glasses on shipboard. McKinley has raised the tax on them from 45 to 60 per cent., and to prevent cheap navigation he has raised the tax on compasses from 35 to 45 per cent. Glass disks for optical instruments he has also raised from 45 to 60 per cent. He has done the same thing by glass tubes for thermometers."

The Fishery Board for Scotland has issued a memorandum on the present state of the shore and sea fisheries of Scotland, by Dr. T. Weymss Fulton, secretary for scientific investigations. His review of the subject brings out the following facts:—(1) That the shore fisheries, especially those for lobsters, crabs, oysters, and mussels are steadily declining. (2) That the number of fishermen, the number of fishing-boats, and the value of the boats and gear, are decreasing. (3) That the number of Scottish beam-trawlers is increasing. (4) That the number of English beam-trawlers which have forsaken the less productive fishing-grounds off the English coast and have resorted to those lying off the Scotch coast is increasing year by year.

The Pilot Chart of the North Atlantic Ocean, issued by the Hydrographer of the United States, shows that the month of October was very stormy north of a line from Bermuda to the British Isles. The month opened with a severe storm about 500 miles north-east from Newfoundland, and pressures below 28·61 inches. There was also at this time a storm in the North Sea, which appears to have originated off Cape Sable on September 27. A marked feature of the month was the existence of a great anticyclone extending from the Azores to near the British Isles, which caused the storms originating to the west of it to take a north-east instead of an east-north-east direction. Considerable fog was reported off the Banks and west of the Irish Channel, but the amount for the month was probably not greater than the average. More ice than usual for the time of year was reported off the Banks as far south as the 45th parallel.

Symons's Monthly Meteorological Magazine for November contains a climatological table for the British Empire for 1889. These interesting results of a certain number of well distributed stations, prepared from the summaries published in the *Magazine* each month, have been continued for many years, and, as the author points out, the extremes are monopolized year after year by the same stations. The highest temperature in the shade was 109°·0 at Adelaide on January 13; for the last five years Adelaide has recorded the highest temperature in the shade, reaching 112°·4 in 1886. It had also the highest temperature in the sun, 170°·7, and was the driest station during the year, having a mean humidity of 63 per cent. The lowest shade temperature was recorded at Winnipeg, on February 23, – 42°·6; only once does any other station come within 20° of it. It had also the greatest range in the year, the greatest mean daily range, 24°·5, the lowest mean temperature, and the least rainfall, 14·95 inches. The highest mean temperature was 80°·5, at Bombay, and the greatest rainfall 73·79 inches, at Trinidad. London was the most cloudy and the dampest station, the mean humidity being 81 per cent. The brightest station was Malta, which had little more than half the cloud of London.

SPEAKING a few days ago at Agra College, the Viceroy of India expressed his approval of the proposal to add scientific and technical training to the curriculum at Indian Colleges, and hoped that it would tend to stimulate and resuscitate some of those arts and industries for which India was famous in by-gone days. If this is really the result, it will be a striking instance of the importance of scientific and technical instruction.

A NOVEL whaling expedition is about to be undertaken by three American whalers—*Hume*, *Grampus*, and *Nicoline*—which have gone to the Arctic regions to winter at the mouth of the Mackenzie River. In order to be well supplied with food, they have taken what will last for two years, and they expect also to get food from the whalers in the summer. This is the highest point any whaler has reached, being 1000 miles from the North Pole. Directly the ice breaks after the winter, the whales come to the mouth of the river in great numbers to feed, and it is expected that a large number of them will be secured. If the experiment proves successful, it will cause a revolution in the whaling industry, as vessels at present have to spend several months on the voyage to good whaling-ground, and then have only a very short time for hunting.

THE new number of the Journal of the Marine Biological Association of the United Kingdom (new series, vol. i., No. 4) opens with a list of governors, founders, and members. In addition to the report of the Council for the year 1889–90, and the director's report, it contains the following papers:—Notes on recent experiments relating to the growth and rearing of food-fish at the Laboratory; report on the surface collections made by Mr. W. T. Grenfell in the North Sea and West of Scotland, by G. C. Bourne (with plate); notes on the herring, long-line, and pilchard fisheries of Plymouth during the winter 1889–90, by W. Roach; notes on the hydroids of Plymouth, by G. C. Bourne (with plate); a complete list of the Opisthobranchiate Mollusca found at Plymouth, by W. Garstang (with plates); notes and memoranda—(1) colour-changes in *Cottus bubalis*, by J. T. Cunningham, (2) note on the British Palæmonetes (*P. varians*), by W. F. R. Weldon, F.R.S.

HAZELL'S Annual for 1891 has been published. This is the sixth year of issue, and we need scarcely say that the volume contains a vast mass of useful information, clearly arranged. In the additions included in the present issue the editor has been careful to keep the work well up to date.

To the new number of the Natural History Transactions of Northumberland, Durham, and Newcastle-upon-Tyne (vol. x., Part 2), Mr. R. Howse contributes a note on the South Durham salt borings, with remarks on the fossils found in the magnesian limestone cores, and the geological position of the salt; a catalogue of the local fossils in the Museum of the Natural History Society; and a catalogue of the fishes of the rivers and coast of Northumberland and Durham and the adjacent sea. The number also contains the report of the Committee for 1888–89.

THE Geological Survey of New Zealand has issued its reports of exploration during 1888–89, with maps and sections. The Director, Sir James Hector, notes that the Department was very fully represented at the Melbourne Exhibition by collections and maps illustrating the geological structure and mineral wealth of the colony. He also mentions that, as a contribution to the work of the Australasian Association for the Advancement of Science, a complete census of the mineral species which have been found in New Zealand has been compiled, together with the localities where found, the names of the first discoverers, and the dates. From this census it appears that 237 species are now recorded.

MESSRS. DULAU AND CO. have published a catalogue of the botanical works which they offer for sale. It includes a large number of valuable books relating to Phanerogamia.

IN 1865, Mr. J. G. Lockhart, while serving as an officer of the Hudson Bay Company, wrote some interesting notes on the moose in the far north of British America. These notes have been preserved in the archives of the Smithsonian Institution, and now appear in the Proceedings of the National Museum. Referring to the hunting of the moose, Mr. Lockhart says the most usual way is to approach the animals on snow-shoes or on foot, as only a hunter knows how, and shoot them. The old men who are not able to walk much in deep snow make a kind of fence of three poles tied equidistant from each other, a little taller than a man, stretching, perhaps, for two days' march between lakes, or a lake and a river, or between two mountains, or in any particular place where the moose are accustomed to pass. Spaces are left vacant here and there in this fence, and in these snares are set. In autumn, during the rutting season, the hunter carries with him the clean, dried shoulder-blade of a moose, and when he hears the call of the male moose, which is audible at a distance of several miles, he rubs the shoulder-blade against a small, dry tree and imitates the call of the male. The moose, as soon as he hears the sound, imagines, no doubt, that it is another moose, and runs in the direction, till met by a shot. The male is very dangerous at that season, especially when wounded. Many years ago, before guns and ammunition found their way into the country, the Indians used to build snow embankments near favourite feeding places, and lie hid there for days until a moose should chance to pass near, when they would kill him with arrows.

THE U.S. Department of Agriculture has issued the third of a series of papers entitled "Contributions from the U.S. National Herbarium." It consists of a list of plants collected by Dr. Edward Palmer in 1890, in Lower California and Western Mexico. Great interest was felt in Dr. Palmer's trip to La Paz and vicinity, and his collection is said to have added much to what was previously known regarding the flora of that region.

AN interesting paper is communicated by Prof. von Hofmann to the current number of the *Berichte* upon the dissociation of carbon dioxide gas into carbon monoxide and oxygen by means of the electric spark. Dalton and Henry long ago showed that carbon dioxide, although formed by exploding a mixture of two volumes of carbon monoxide with one volume of oxygen by the passage of an electric spark, is again partially decomposed into carbon monoxide and oxygen by the continued passage of the spark. This dissociation, however, is very slow, and usually incomplete. Hofmann and Buff, in the course of their well-known work upon gaseous reactions, further showed that "the electric spark passes through carbon dioxide with a violet glow, producing at first a rapid increase in the volume, which, however, becomes less and less marked until at the expiration of about half an hour the separated carbon monoxide and oxygen recombine with a sudden explosion, the re-formed carbon dioxide at once commencing to be again dissociated." Deville and Berthelot afterwards investigated the same phenomena, and also found that the reaction was never complete, proceeding only until about 28 or 29 per cent. of the carbon dioxide was decomposed, but they never observed any explosive recombination as described by Hofmann and Buff. Prof. Hofmann has therefore determined the exact conditions under which the explosive recombination occurs. It certainly appears somewhat remarkable that the same spark can effect both dissociation and recombination, yet such, within the limits described in the memoir, is an actual fact. The first essential point to be observed is the length of path of the spark; the most suitable distance apart of the platinum terminals appears to be between two and a half and three millimetres, and Prof. Hofmann advises the use of adjustable terminals rather than the ordinary platinum wires fused into the side of the eudiometer. A Leyden jar in the circuit renders the occurrence of periodical explosions more certain. The spark should also pass at about a quarter the height of the gas column instead of, as usual, near the top. The current itself, moreover, should not be too strong; that from two Bunsen cells and only a moderate sized Ruhmkorff coil is quite sufficient, and yields the best results. It is also preferable to use a volume of carbon dioxide, previously dried over oil of vitriol, not exceeding ten cubic centimetres at a pressure of 650–700 mm.; eight c.c. give excellent results. Under these conditions the first explosion usually occurs in 15–20 minutes, and sometimes earlier. The flame commences in the neighbourhood of the spark, and then perceptibly spreads through the whole length of the gas column. It is coloured blue in the first explosion, and green in the succeeding ones owing to the volatilization of a little mercury vapour. The second and succeeding explosions occur after shorter intervals than the first. This experiment is certainly one of the most interesting in all the range of dissociation phenomena, and full details, with drawings of the apparatus, are given by Prof. Hofmann in his memoir.

The additions to the Zoological Society's Gardens during the past week include an Ocelot (*Felis pardalis*) from South America, presented by Mr. J. H. Bennett; a Common Fox (*Canis vulpes* ♀) from North Wales, presented by Mr. B. Myddelton Biddulph; two Cape Zorillas (*Ictonyx zorilla* ♀ ♀) from South Africa, presented by Mr. R. Southey; two Ring-necked Parakeets (*Palæornis torquatus*) from India, presented by Miss S. L. Hands; ten Thunder-fish (*Misgurnus fossilis*) from the Baltic Sea, five Golden Orfe (*Leuciscus orfus*), European fresh waters, purchased.

OUR ASTRONOMICAL COLUMN.

The Variations in Latitude.—The most important question discussed at the International Conference on Degree Measurement held at Freiburg on September 15 was that of the variability of terrestrial latitudes. The Central Bureau, represented by Drs. Helmert and Albrecht, communicated two notes, one by Dr. Albrecht, "Resultate der Beobachtungsreihen betreffend die Veränderlichkeit der Polhöhen," the other by Dr. Marcuse, "Resultate der fortgesetzten Berliner Beobachtungsreihen betreffend die Veränderlichkeit der Polhöhen." These papers, and a general account of the whole question of variation in latitude, first raised by Prof. Fergola at the Conference of Rome (1883), is given by M. Tisserand in the *Bulletin Astronomique* for September. The method adopted in the observations made at the different stations is that of Horrebow. It consists in forming nine groups, each containing eight or nine couples of stars, two groups at least being observed each evening. The two stars of each couple were of almost equal magnitude; their difference in right ascension was comprised between 3m. + 15m., and that of their meridian zenith distances between ± 12′, whilst the meridians were never separated more than 27°. By choosing such stars, and taking the arithmetical mean of the differences of zenith distances of the two components, the small error due to an imperfect knowledge of the movement of the micrometer wire is eliminated. At each station a number of couples of stars ranging from 1400 to 1700 have been observed during the whole of 1889 and the first three months of 1890. It is evident that if the same couple of stars could be observed throughout the year, during the day as well as the night, the variations in latitude could be obtained independently of the errors of the absolute declinations of the two stars, provided that we knew exactly the variations of these declinations during the whole interval of observation. These variations depend on the motion of the terrestrial axis (precession and nutation), on aberration, annual parallax, and proper motion. It is impossible to work in this way for two reasons, (1) stars cannot be observed in the day-time by means of the instruments employed; (2) only a very small number of observations could be made. The groups of stars referred to above have been formed in order to avoid these inconveniences, for the differences, from the mean declinations, of each of the couples may be used as if only a single couple had been observed. When the necessary corrections are applied to the calculations it is found that the latitudes of Berlin, Potsdam, and Prague, indicate clearly a diminution of about 0″·5 from August 1889 to February 1890. The observations made at Berlin from April 15 to August 30 in this year show an increase of 0″·4 in the latitude of this place during the interval considered, and point to a period of about a year. The fact that the latitude of a place is subject to an annual periodic variation is not entirely new, for Gaillot discussed 1077 observations of latitude made between 1856 and 1861. On arranging the observations according to the month in which they were made, the following differences from the mean value were obtained (*Annales de l'Observatoire de Paris*, p. 319):—

	Paris.	Potsdam.		Paris.	Potsdam.
	″	″		″	″
January	−0·23	−0·11	July	+0·25	+0·19
February	−0·06	−0·07	August	+0·16	+0·17
March	−0·03	−0·04	September	+0·13	+0·10
April	−0·03	0·00	October	−0·07	−0·03
May	+0·10	+0·05	November	−0·11	−0·14
June	+0·16	+0·14	December	−0·27	−0·26

These observations, therefore, indicate a difference of almost half a second between the observed latitudes in January and July. M. Nobile has discussed, from the same point of view, the observations made at Greenwich, Milan, Oxford, Pulkova, and Washington (*Bulletin Astronomique*, vol. v. p. 544). The Greenwich observations show a maximum of latitude in July and August, and a minimum in December and January, the amplitude of the variation being nearly 1″. At Washington a minimum also occurs at the end of the year, but at Milan it occurs in May. The Oxford observations show a maximum in the autumn, whilst those made at Pulkova do not appear to present any periodic variation with the seasons. But although these results are somewhat contradictory, those communicated at Freiburg clearly demonstrate the existence of a variation of 0″·5. It remains to be seen how the amplitude and the phase of the variation is affected by passing from the northern to the southern hemisphere, or when observations are made at two stations having appreciably the same latitude, but widely separated in longitude.

The cause of the variation has yet to be found. M. Tisserand has shown ("Traité de Mécanique céleste," vol. ii. p. 489) that the transportation of a mass of water, 0·10m. thick, from latitude + 45° to − 45°, and covering a tenth of the earth's surface, may cause the principal axis to move 0″·16. The weight of a column of water 0·10m. thick, is equal to that of a column of mercury 0·007m. thick, hence considerable changes in the barometric pressure may have an appreciable effect on the direction of the earth's axis. That the variations are connected with the succession of the seasons is manifest. This seems to indicate that temperature plays an important part, and that the variation is a result of the varying amount of heat received throughout the year by the atmosphere and the instruments. The theories of refraction require the atmosphere to have a regular constitution and a statical condition that is probably never realized. If the constitution varies with the season, the refractional effect must also change, and this may cause the difference in the observations of latitude.

Another probable cause was suggested by M. D. Lamey at the meeting of the Paris Academy on November 17. He showed that if the atmospheric tidal effects be calculated for each month in the year, the values obtained followed a curve very similar to that derived from the observed differences in latitude. That the sun and moon produce atmospheric tides must be incontestable. The physical consequence is that refraction phenomena must also vary with the tidal effect, and therefore the positions of stars may experience an annual periodic fluctuation comparable with that observed. The cause wherein lies the solution of the problem can only be definitely determined, however, after a more extended study than has yet been made.

NEW ASTEROIDS.—M. Charlois discovered the asteroid (298) on September 9. On October 3, what was supposed to be the same body was again observed by Charlois. Dr. Palisa also observed an asteroid near the computed position of (298) on October 11. It has since been found that the observations of October 3 and 11 must refer to another asteroid, and, as Dr. Palisa discovered (299) on October 11, the one thought to be identical with (298) will be reckoned as (300). M. Charlois rediscovered (298) on November 14. Dr. Palisa added (301) the list on November 16 (*Astronomische Nachrichten*, 3006).

The large number of asteroids now known renders it impossible for the Berlin Computing Bureau to furnish all the data for their observation. The editors of the *Berliner Jahrbuch* have therefore decided only to furnish predictions for the following asteroids:—

(1) Those that approach near the earth, and are therefore well adapted to the determinations of parallax.

(2) Those whose near approach to Jupiter renders them useful for determining his mass.

(3) Those remarkable for a period having a closely commensurable ratio to that of Jupiter; such orbits being of the highest importance in the theory of absolute perturbations.

(4) Those that are of value for photometric work on account of their considerable brightness.

ZONA'S COMET (*e* 1890).—Dr. Bidschof gives the following elements and ephemeris of this comet in *Astronomische Nachrichten*, 3006:—

T = 1890 July 25·051 Berlin mean time.

$$\left.\begin{array}{l}\omega = 321^\circ\ 58' \\ \Omega = 84\ 45 \\ \iota = 153\ 28\end{array}\right\}\ \text{Mean Eq. 1890.}$$

Perihelion distance = 1·8996 (earth's mean distance = 1).

Ephemeris for Berlin Midnight.

1890.	R.A. h. m.	Decl. ° ′	Distance in terms of the earth's mean distance.
December 4	3 47·0	+ 34 50	1·53
„ 8	3 26·8	+ 34 19	1·59
„ 12	3 8·7	+ 33 36	1·65

The comet is, therefore, in the constellation Perseus, which is in the south about 10 p.m. It passed perihelion about the end of July, and is increasing its distance from the earth. M. Bigourdan, of Paris Observatory, describes the comet as "très faible (grandeur 12·5–13), et se présentait sous l'aspect d'une petite tache blanche, ronde, de 1′ environ de diamètre, avec condensation central assez diffuse et d'aspect un peu grande."

THE SCIENTIFIC WORK OF JOULE.[1]

PROF. DEWAR commenced by remarking that the Royal Institution had been so closely identified with the great workers in physical science that it was impossible to allow the work of Joule, whose researches had produced as marked a revolution in physical science as Darwin's in biology, to pass without recognition in the present series of Friday evening discourses. Sir William Thomson, as Joule's friend and fellow-worker to the last, had been invited to undertake the duty, and had agreed to do so; but at the last moment had been compelled to decline by reason of important official duties in Scotland, and the task had consequently devolved upon him.

Having given a brief account of Joule's parentage, early life, and education, Prof. Dewar reviewed, as fully as time would permit, his scientific work, which extended over about forty years, and was represented by 115 original memoirs. The first period (1838–43) was distinguished as that in which Joule educated himself in experimental methods, chiefly in connection with electricity and electro-magnetic engines. This work led him in 1840 to his first great discovery, the true law governing the relation between electric energy and thermal evolution, which enabled him later on to account for the whole distribution of the current, not only in the battery in which it is produced, but in conductors exterior to it. Joule was thus led to take up the study of electrolysis. Faraday had already made the discovery that electrolytic bodies could be split up into equivalent proportions by the passage of the same electric current; Joule saw that there would be great difficulty in finding out the distribution of the current energy, and accounting for the whole of it. After a laborious research he succeeded in showing that during electrolytic action there was an absorption of heat equivalent to the heat evolved during the original combination of the constituents of the compound body. The prosecution of his electrical researches rapidly brought Joule on the road to his great discovery of the mechanical equivalent of heat, it being clear from a footnote to a paper dated February 18, 1843, that he already had well in hand the study of the strict relations between chemical, electrical, and mechanical effects.

In working out these laws, it was to be remarked that Joule—in common with most inventors and seekers after new scientific truths—chose perhaps the most difficult means that could have been selected; and in looking back at his work in the light of present knowledge, it seemed simply astounding that he should have succeeded so completely as he did. The original coil used by Joule for the mechanical determination of heat (kindly lent for the occasion by Prof. Rücker) was shown, and the course of the experiment explained. The vast difficulties which Joule had to overcome in order to prove that there was a definite, permanent, and persistent relation between the amount of mechanical energy expended and the heat produced were commented on; the thermal effects being produced not directly but through the medium of an electric current varying in intensity, and calculations having to be made not only for these fluctuations, but for the effects of radiation, the movement of the air, and other indirect complications. The very small increment of heat to be measured obliged Joule to use thermometers of great delicacy, and these he had to devise and construct himself. One of the thermometers so used was exhibited.

Working in this way, Joule was able, by the end of July 1843, to state definitely that the amount of heat capable of increasing the temperature of a pound of water by 1° F. was equal to, and might be converted into, a mechanical force capable of raising 838 lbs. to the height of 1 foot. Soon afterwards he attained almost identical results by a more direct method—the friction of water passing through small tubes—which gave him 770 foot-pounds per unit of heat.

It was impossible, said the lecturer, to thoroughly appreciate Joule's work without glancing at the early history of the subject; and when one did so it was amazing to find how near men of the stamp of Rumford, Davy, and Young had been to Joule's great discovery, and yet missed it. Count Rumford was the first to clearly define the relation between the constant production of heat and loss of movement by frictional motion. He proved that the amount of heat produced by friction was continuous, and apparently unlimited; but he did not think of measuring the relation between the mechanical energy expended and the amount of heat produced. Alluding to the results obtained from this apparatus, the lecturer said that Count Rumford might have shown that in his experiments the heat produced was proportional to the time of working, and so obtained a result capable of being expressed in horse-power. The value so deduced from Rumford's experiments is not far removed from Joule's first number.

The experiments commenced by Count Rumford were carried on by Davy, at that time working with Beddoes at Bristol; and led to one of the most remarkable essays on heat of that period, which disposed for ever of the theory of the separate existence of caloric. Taking two pieces of ice on a cold day, Davy mounted them so that they could be rotated against each other with frictional pressure, the effect being that the pieces of ice were melted, the water so produced having a much higher specific heat than the original ice. To guard against the possibility of heat being conveyed to the frictional apparatus by the surrounding air, Davy made an experiment *in vacuo*, isolating the apparatus by means of ice; and found that under such conditions sufficient heat could be produced to melt wax placed in the receiver. The lecturer here showed an experiment illustrating the production of water by the friction of two pieces of ice *in vacuo*, under conditions of temperature much much severe than those of Davy's experiment.

Following Davy, Young devoted a great deal of attention to the subject, and by 1812 he and Davy had quite changed their opinions, and had adopted the view that heat and motion were convertible effects.

Having by July 1843 assured himself of the principle of his discovery, Joule now devoted his attention to the elaboration of methods of working, modifying, and repeating experiments in

[1] Abstract of the Friday evening discourse delivered at the Royal Institution of Great Britain on January 24, 1890, by Prof. Dewar, F.R.S.

various ways, but always approaching nearer and nearer to exactness, as shown by the following table of results :—

Joule's Values of the Mechanical Equivalent of Heat.

		Kilogramme metres.
Magneto-electric currents	1843	460
Friction of water in tubes	,,	424·6
Diminution of heat produced in a battery current when the current produces work	,,	499
Compression of air	1845	443·8
Expansion of air	,,	437·8
Friction of water	,,	488·3
,, ,, ,,	1847	428·9
,, ,, ,,	1850	423·9
,, ,, mercury	,,	424·7
,, ,, iron	,,	425·2
Heat developed in Daniell's cell	,,	419·5
,, ,, in wire of known absolute resistance	1867	429·5
Friction of water in calorimeter	1878	423·9

Prof. Dewar here exhibited and explained the action of the original calorimeter used by Joule. It was seen to consist of a set of vanes which were made to revolve in water by the falling of known weights through a definite and known height, the heat produced being due (after making the necessary deduction for the friction due to the momentum of the weights) entirely to the friction of the fluid. It was found that, whatever fluid was employed, the same definite results were obtained—a production of heat in the liquid bearing a constant relation to the unit of mechanical energy expended. The extreme delicacy of Joule's apparatus, and the marvellous accuracy of his observations, were shown by the fact that working with weights of 29 pounds each, and repeating each observation 20 times, the total increase of temperature did not exceed half a degree Fahrenheit. In contrast to this the lecturer showed, by means of apparatus kindly lent by Prof. Ayrton, the method now employed for repeating Joule's work and arriving at substantially the same results by much simpler means.

While continuing to work intermittently at his great discovery, Joule employed himself in the following years in elaborate investigations bearing upon the point of maximum density of water, specific gravity, and atomic volumes. An illustration of his method of determining maximum density was given by means of two large cylinders filled with water and connected by a narrow channel in which was placed a floating indicator. It was shown that the slightest variation in density of the water of either cylinder—variations far beyond the scope of the most delicate thermometer—set up currents which were immediately detected by the movement of the indicator, and that by this means it was quite possible to ascertain the exact temperature at which water attained its maximum density.

Joule's determinations of atomic volumes were marvellous at the time they were made, and were still interesting. Illustrations of his work in this direction were given by means of a solution of sugar, which was seen to occupy practically the same space as was occupied by an amount of water exactly equivalent to that combined in the carbohydrate, the carbon hypothetically combined with the water to form the sugar appearing to make no sensible difference to the volume; and in contrast to this was seen the enormous difference in volume brought about by dissolving two equal portions of soda carbonate, one portion being ordinary hydrated crystals, and the other portion being anhydrous, in equal volumes of water.

Joule's last great research was carried out conjointly with Sir William Thomson, and occupied nearly ten years of laborious inquiry. Its chief object was to prove that in compressing a gas the amount of heat produced is equivalent to the work done, and independent of the mere fact of the approach of the particles. But Joule was desirous of amplifying the inquiry, and in fact the work might be divided into three sections: (1) the study of gases passing through narrow apertures; (2) the velocity attained by bodies passing through the air; and (3) the temperature ultimately attained by such moving bodies. With respect to (2) and (3), it was shown that a body rotating in the air at the rate of about 150 to 180 feet per second increased in temperature by nearly 1° F., and that this increase of temperature was definite for a given velocity, and independent of the size of the moving mass and the density of the gaseous medium. With regard to (1), the relation of gaseous pressure and volume to temperature, the researches of Regnault had already shown that the simple law of Marriotte and Boyle could not stand by itself; and Joule sought to modify it by the study of gases passing through very small tubes or porous bodies. The investigations were carried out at Manchester on a large scale, and were assisted by a Government grant. Steam engines were employed to maintain a current of gas at a constant temperature and pressure through long coils of pipe placed in water tanks. They proved that any difference of temperature in the gas brought about in its passage through the porous body must be due to work done by it, and that this difference of temperature varied for different gases, according to their constitution. Working under the same conditions, hydrogen was shown to be reduced a small amount in temperature, air somewhat more (about 0°·3), and carbonic acid a much greater amount. A repetition of Joule and Thomson's experiment was shown by means of a 100-feet coil of lead pipe, compressed hydrogen, air, and carbonic acid gas being employed, and the original results verified in each case. The effect of this research was to enable Joule and Thomson to formulate a great improvement on the gaseous laws; for, instead of the product of the volume and pressure being strictly proportional to the absolute temperature, as it had been hitherto believed, they found that a new term was involved, which is equivalent to a constant divided by the absolute temperature and multiplied by the volume.

In conclusion, Prof. Dewar read the following letter, which he had received from Sir Lyon Playfair in response to his request for some reminiscences of Joule :—

January 20, 1890.

DEAR DEWAR,—You ask for some of my memories of Joule from 1842 to 1845, when I was Professor of Chemistry at the Royal Institution in Manchester. The great Dalton died in the autumn of 1844, and had long been President of the Manchester Philosophical Society. He naturally gave impulse to the study of science in that town, where there was an active band of young workers in research.

Joule was, even then, foremost among these; and the names of Binney, Williamson, Schunck, Angus Smith, Young, and others show that the spirit of scientific inquiry was active. We were also stimulated by the fact that Baron Liebig and Bunsen came to pay me visits during that time; they were men to excite research.

Joule was a man of singular simplicity and earnestness. We used to meet at each other's houses at supper, to help the progress of our work by discussion. Joule was an earnest worker, and was then engaged on his experiments on the mechanical equivalent of heat. He took me to his small laboratory to show me his experiments, and I, of course, quickly recognized that my young friend the brewer was a great philosopher. We jointly worked upon questions of far less importance than his great central discovery, but he was equally interested. I was very anxious that he should devote his life to science, and persuaded him to become candidate for the Professorship of Natural Philosophy at St. Andrews. He was on the point of securing this, but his slight personal deformity was an objection in the eyes of one of the electors; and St. Andrews lost the glory of having one of the greatest discoverers of our age.

When Joule first sent an account of his experiments to the Royal Society, the paper was referred, among others, to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge, but the discovery did not then recommend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their consequences, if true, to science. But all my arguments were insufficient to convince my friend; and I fear that then the Royal Society did not appreciate and publish the researches. I write from memory only, for I know that, later, no Society or institution honoured Joule more than the Royal Society and its members.

Not for one moment, however, did Joule hesitate in the accuracy of his experiments or his conclusions. He once suggested to me that we might take a trip together to the Falls of Niagara, not to look at its beauties, but to ascertain the difference of temperature of the water at the top and bottom of the fall. Of course the change of motion into heat was a necessary consequence of his views.

No more pleasant memory of my life remains than the fact that, side by side, at my lectures in the Royal Institution, used to sit the illustrious Dalton, with his beautiful face, so like that of Newton, and the keenly intelligent Joule. I can give no other explanation than the fact of organic chemistry being then a new science that two philosophers of such eminence should come to the lectures of a mere tyro in science. I used to look upon them as two types of the highest progress in science. Newton had introduced law, order, and number into the movements of masses of matter in the universe; Dalton introduced the same into the minute masses which we call atoms; and Joule, with a keen insight into the operations and correlation of forces, connected them together, and showed their mutual equivalence.

I do not know whether these memories are of any use to you, but, such as they are, they are at your disposal for your lecture on the friend of my youth. Yours sincerely,

LYON PLAYFAIR.

WEIGHING BY A SERIES OF WEIGHTS.

THIS subject, now under discussion in NATURE, is by no means new. The following remarks on the general theory of such questions may prove interesting. The problems are divisible into two classes:—

(1) To assign a series of weights so as to be able to weigh any weight of an integral number of pounds from 1 to n inclusive, the weights being placed in only one scale-pan.

(2) The same problem when the weights may be placed in both scale-pans.

Two other conditions may be imposed, viz.:—

(*a*) That no other weighings are to be possible.

(*b*) That each weighing is to be possible in only one way, *i.e.* to be unique.

The question considered in NATURE is of the second class, and is further subject to the two conditions above stated. Moreover, the problem for the number of pounds 1, 4, 13, 40, &c...., as solved by the series of weights 1; 1, 3; 1, 3, 9; 1, 3, 9, 27; &c...., respectively gives in each case the series containing the least number of weights for which the solution is possible.

For a number of pounds equal to 40, the solution may be given by means of a single algebraic formula, constituting an identity, viz.

$$(x^{-1} + 1 + x)(x^{-3} + 1 + x^3)(x^{-9} + 1 + x^9)(x^{-27} + 1 + x^{27})$$
$$= x^{-40} + x^{-39} + \ldots\ldots + x^{-1} + 1 + x + \ldots\ldots + x^{39} + x^{40}.$$

This is easily verified. When the left-hand side is multiplied out, the resulting powers of x are obtained by addition of certain indices, some positive and some negative. The indices in question are 1, 3, 9, 27, and, from the form of the factors, may in any single term be taken *either* positively or negatively. Thus one term on the left is $x^{-3} \cdot x^9 \cdot x^{27} = x^{-3+9+27}$, giving the term x^{33} on the right-hand side. The identity shows that every number from 1 to 40 inclusive can be thus composed by the numbers 1, 3, 9, 27, taken positively or negatively. It shows, moreover, since the coefficients of the several powers on the right are each unity, that the composition is unique. The identity includes also negative integers from -1 to -40. This is necessitated by the very nature of the question.

The above is one out of eight distinct solutions of the problem before enunciated. Before giving the other seven, it will be as well to show the principles of solution in general.

Consider, first, the problem of the first class in which weights can only be placed in one scale-pan. The number of pounds to be weighed being n, we must discover the ways in which it is possible to break up the expression

$$1 + x + x^2 + x^3 + \ldots\ldots + x^n$$

into similar expressions, for, as will be evident, each such factorization corresponds to a definite solution of the problem. By similar expression is to be understood one in which the coefficients are unity and the indices are in arithmetical progression.

At the outset may be remarked the trivial solution consisting of n ones corresponding to the unfactorized expression. There is one solution of this kind in respect of every number n.

Putting the expression in the form

$$\frac{1 - x^{n+1}}{1 - x},$$

it will be clear that if $n + 1$ be a prime number, the expression cannot be factorized in the required form. This is a consequence of the properties of prime roots of unity.

Hence, when $n + 1$ is prime, the only solution is obtained by taking n one-pound weights.

If, however, $n + 1$ be not prime, the number of solutions depends upon the composite character of $n + 1$. If $n + 1 = st$ where s and t are primes, we may write

$$\frac{1 - x^{n+1}}{1 - x} = \frac{1 - x^{st}}{1 - x} = \frac{1 - x^s}{1 - x} \cdot \frac{1 - x^{st}}{1 - x}$$
$$= (1 + x + x^2 + \ldots + x^{s-1})(1 + x^s + x^{2s} + \ldots + x^{st-s}),$$

a factorization of the required form.

Multiplying x^p in the first factor into x^{qs} in the second, we find that the number $p + qs$ is composed by p numbers equal to 1, and q numbers equal to s. In general, every number from 1 to n inclusive can be composed by means of $s - 1$ ones, and $t - 1$ s's. That is to say, that all numbers of pounds from 1 to n (where n is of the form $st - 1$) can be weighed, and in a unique manner, by means of $s - 1$ weights of 1 pound each, and $t - 1$ weights of s pounds each.

Another solution is obtained from the identity

$$\frac{1 - x^{n+1}}{1 - x} = \frac{1 - x^{st}}{1 - x} = \frac{1 - x^t}{1 - x} \cdot \frac{1 - x^{st}}{1 - x_t}$$

obtained from the former by interchange of s and t. These two solutions, together with the trivial one, constitute the complete set of three solutions. For brevity, these may be represented in the form—

$$1^{st-1},$$
$$1^{s-1}\, s^{t-1},$$
$$1^{t-1}\, t^{s-1},$$

where s^{t-1} means $t - 1$ s's, and so on.

From the above simple example it will be sufficiently clear that the number of solutions in the case of a given number n is entirely dependent upon the form of $n + 1$, when regarded as a product of prime factors. The number of solutions is not in all cases easy to determine, but some of the simpler results are interesting. Suppose the number $n + 1$ to be a product of k unrepeated prime numbers, and, under these circumstances, represent the number of solutions in regard to the number n by

$$[1^k].$$

Then it is easy to prove the relation—

$$[1^k] = 1 + k[1^1] + \frac{k(k-1)}{2!}[1^2] + \frac{k(k-1)(k-2)}{3!}[1^3] + \ldots$$
to k terms,

and thence to calculate the values of $[1^1]$, $[1^2]$, $[1^3]$,... in succession. The series of numbers thus obtained commences 1, 3, 13, 75, 541,... where it will be noticed that the second number, 3, corresponds with the result obtained above for the case of $n + 1$, being of the form st, or the product of two unrepeated primes. It may be observed that this series of numbers is of great interest, and presents itself frequently in mathematics, notably in Prof. Cayley's "Theory of the Analytical Forms called Trees" (see vol. xiii. and subsequent volumes of the *Philosophical Magazine*, and "Collected Papers," No. 203).

A much simpler case to consider is that for which $n + 1$ is merely a power of a prime number. If $n + 1$ be the kth power of a prime, represent the number of solutions in regard to the number n by

$$[k].$$

A small amount of reflection shows the truth of the relation

$$[k] = 2[k - 1];$$

whence

$$[k] = 2^{k-1}.$$

Turning now to the second class of problems, in which it is permissible to place weights in either or both scale-pans, it can be shown that the theory is not essentially different from that belonging to those of the second class.

In order to weigh any number of pounds from 1 to n inclusive, we have to factorize the expression

$$x^{-n} + x^{(-n-1)} + x^{-(n-2)} + \dots + n^{-1} + 1 + x + \dots + x^{n-2} + x^{n-1} + x^n,$$

which may be thrown into the form

$$\frac{1 - x^{2n+1}}{x^n(1-x)}.$$

The solutions depend upon the composite character of the number $2n + 1$. There always exists the trivial solution consisting of n ones, and when $2n + 1$ is prime, this constitutes the only solution.

Supposing $2n + 1$ the product of two primes, viz.

$$2n + 1 = st,$$

we may write

$$\frac{1 - x^{2n+1}}{x^n(1-x)} = \frac{1 - x^{st}}{x^n(1-x)} = \frac{1}{x^n} \cdot \frac{1-x^s}{1-x} \cdot \frac{1-x^{st}}{1-x^s}$$

$$= \frac{1-x^s}{x^{\frac{s-1}{2}}(1-x)} \cdot \frac{1-x^{st}}{x^{\frac{s(t-1)}{2}}(1-x^s)}$$

$$= \left(x^{-\frac{s-1}{2}} + x^{-\frac{s-3}{2}} + \dots + x^{-1} + 1 + x + \dots + x^{\frac{s-3}{2}} + x^{\frac{s-1}{2}}\right)$$

$$\times \left(x^{-\frac{st-s}{2}} + x^{-\frac{st-3s}{2}} + \dots + x^{-s} + 1 + x^s + \dots + x^{\frac{st-3s}{2}} + x^{\frac{st-s}{2}}\right).$$

The factors appearing in this last expression are of the required form, and the factorization indicates that any number from 1 to n inclusive may be composed by means of $\frac{1}{2}(s-1)$ ones, and $\frac{1}{2}(t-1)$ s's, if all the ones that are taken be taken *either* positively or negatively, and all the s's also *either* positively or negatively.

The solution may be represented, according to the method before explained, by

$$1^{\frac{1}{2}(s-1)}\ s^{\frac{1}{2}(t-1)},$$

and the complete system of solutions will in the present case be denoted by

$$\begin{cases} 1^{\frac{1}{2}(st-1)}, \\ 1^{\frac{1}{2}(s-1)}\ s^{\frac{1}{2}(t-1)}, \\ 1^{\frac{1}{2}(t-1)}\ t^{\frac{1}{2}(s-1)}. \end{cases}$$

From the above, it is clear that there is a one-to-one correspondence between the solutions of the second class problem in regard to the number n and the solutions of the first class problem in regard to the number $2n$. The theory of the second class problems is thus included in that of the first class problems. If we take the case considered in this journal of $n = 40$ and both scale-pans, the number of solutions will be the same as that for $n = 80$ and one scale-pan; the number depends upon the composite character of the integer 81, which is 3^4; hence the number of solutions (see *ante*) is [4], which is 2^3 or 8.

Corresponding to the several identities—

$$\frac{1-x^{81}}{x^{40}(1-x)}$$
$$= x^{-40} + x^{-39} + \dots + x^{-1} + 1 + x + \dots + x^{39} + x^{40};$$

$$\frac{1-x^{81}}{x^{39}(1-x^3)} \cdot \frac{1-x^3}{x(1-x)}$$
$$= (x^{-39} + x^{-36} + \dots + x^{-3} + 1 + x^3 + \dots + x^{36} + x^{39})(x^{-1} + 1 + x);$$

$$\frac{1-x^{81}}{x^{36}(1-x^9)} \cdot \frac{1-x^9}{x^4(1-x)}$$
$$= (x^{-36} + x^{-27} + \dots + 1 + \dots + x^{27} + x^{36})$$
$$(x^{-4} + x^{-3} + \dots + 1 + \dots + x^3 + x^4);$$

$$\frac{1-x^{81}}{x^{36}(1-x^9)} \cdot \frac{1-x^9}{x^3(1-x^3)} \cdot \frac{1-x^3}{x(1-x)}$$
$$= (x^{-36} + x^{-27} + \dots + x^{36})(x^{-3} + 1 + x^3)(x^{-1} + 1 + x);$$

$$\frac{1-x^{81}}{x^{27}(1-x^{27})} \cdot \frac{1-x^{27}}{x^{13}(1-x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-13} + x^{-12} + \dots + x^{13});$$

$$\frac{1-x^{81}}{x^{27}(1-x^{27})} \cdot \frac{1-x^{27}}{x^{12}(1-x^3)} \cdot \frac{1-x^3}{x(1-x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-12} + x^{-9} + \dots + x^{12})(x^{-1} + 1 + x);$$

$$\frac{1-x^{81}}{x^{27}(1-x^{27})} \cdot \frac{1-x^{27}}{x^9(1-x^9)} \cdot \frac{1-x^9}{x^4(1-x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-9} + 1 + x^9)(x^{-4} + x^{-3} + \dots + x^4);$$

$$\frac{1-x^{81}}{x^{27}(1-x^{27})} \cdot \frac{1-x^{27}}{x^9(1-x^9)} \cdot \frac{1-x^9}{x^3(1-x^3)} \cdot \frac{1-x^3}{x(1-x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-9} + 1 + x^9)(x^{-3} + 1 + x^3)(x^{-1} + 1 + x).$$

These are the eight solutions represented by

1^{40}
$1 . 3^{13}$
$1^4 . 9^4$
$1 . 3 . 9^4$
$1^{13} . 27$
$1 . 3^4 . 27$
$1^4 . 9 . 27$
$1 . 3 . 9 . 27.$

The subject is more fully entered into in a paper by myself in the *Quarterly Journal of Pure and Applied Mathematics* for 1886.

P. A. MACMAHON.

THE SCIENTIFIC RESULTS OF THE OCCUPATION OF BRITISH NEW GUINEA.

A VOLUMINOUS and extremely interesting report on the first year of the administration of British New Guinea by Sir William MacGregor was issued some time ago by the Colonial Office. It deals with the period ending June 30, 1889. One of the sections of the report deals with "scientific results," which we are glad to notice have a place like "finance," "legislation," "trade and shipping," and the other usual divisions of these colonial reports. In sending the report home, Sir Henry Norman, the Governor of Queensland, observes that it is fortunate that the administrator is most anxious to obtain the best scientific results on his visits and tours, and that he is well able to judge for himself in such matters. The scientific collections, therefore, are made with judgment, and the various reports on collections are of interest and value. Sir William MacGregor himself, in summing up the scientific results, says that during the year some addition was made to our knowledge of the natural history of the country. Unfortunately, it is not possible to set out fully the progress made, as the report on specimens sent to England had not reached him at the moment of writing. It is his hope, however, that in future all specimens collected may be examined in Australia, so that the information gained can be kept together and be summarized in each annual Report.

Geology.—Thirty-one small bags of specimens were examined by Mr. Jack, Government Geologist of Queensland. All except two were from the Louisiade and D'Entrecasteaux groups. Mr. Jack's report, which will be found to be interesting and valuable, is given in an appendix. A set of specimens, covering the route from Manu-Manu to the summit of the Owen Stanley Range, was submitted to careful examination by Mr. Rands, Assistant Government Geologist of Queensland, who at the same time classified the specimens collected in the Rigo district. Mr. Rand's report will also be found in an appendix. Although not forming any part of the work of the year, there is added to the same appendix a report on certain geological specimens collected by Mr. C. S. Wilkinson. These three reports practically contain all that is really known of the geology of the country.

Ornithology.—The greater portion of the birds obtained were classified by Mr. de Vis. His report, prepared after much careful labour, is added as an appendix. From it may be inferred that the probability is that no great addition will be made now to the more beautiful and gorgeous birds of British New Guinea.

Reptiles.—The reptiles collected during the latter part of the year were classified by Mr. de Vis, Director of the Queensland Museum. They include ten species of snakes. Mr. de Vis has reported that of these ten species only two, a death-adder and a whip-snake, are to be dreaded. A small batrachian, mentioned Mr. de Vis, is interesting as having been brought from the top of Mount Victoria. A note furnished by Mr. W. H. Miskin, giving the results of his examination of a collection of Lepidoptera, and a description by Mr. C. Hedley of a new Rhylida from the highest summit of the Owen Stanley Range, are also given in an appendix.

Botany.—All botanical specimens were sent to Baron Mueller. He has found time to classify the specimens forwarded to him, and his report is also included in the "White-Book." In this branch much yet remains to be done, the collecting of plants being attended with great difficulties in New Guinea. It thus appears that the scientific work is contained in a series of appendices by specialists, in the shape of reports made on the various collections of Sir William MacGregor and his officers. Appendix B is a report by Prof. Liversidge on the hot springs of Ferguson Island in the D'Entrecasteau group; while D is a very long report from Sir William MacGregor himself on a tour made by him from Manu-Manu on the coast to the Owen Stanley Range in the interior. It gives *inter alia* a fascinating description of the mountain scenery in that great range. Appendix F contains the reports of Messrs. Jack and Rands on the geological collections. Mr. Jack observes that the specimens tell no connected tale such as would enable one to construct even a theory regarding the geology of the islands. They show, however, that palæozoic rocks, such as are the matrices of gold and other metallic deposits in Australia and elsewhere, are abundant, and that basaltic lavas are of common occurrence. The limestones may yield fossils which would be of great service in unravelling the structure of the islands. Mr. Rands, to whom were submitted the specimens collected by Sir William MacGregor on his expedition up the Vanapa river to the top of Mount Victoria, says that they enable one to form an idea of the geological character of the country drained by the Vanapa. The whole region consists almost entirely of schists, which become more highly metamorphosed as the loftier heights of the Musgrave Range and Mount Victoria are reached. On Mount Victoria the schists are very numerous, highly crystalline, and closely approaching to gneiss; on passing down the river, the country consists of clay, schists, and slates; while from near the mouth there are specimens of a but slightly altered sandstone. The report of Mr. de Vis, of the Brisbane Museum, on the birds is contained in Appendix G. The collection contained 161 specimens, representing 82 species, of which 13 appear to be hitherto unrecorded; "and of the apparent novelties one at least lays claim to generic rank." This is a very distinct kind of bower bird, obtained on Mount Knutsford, at an elevation of 11,000 feet, and rivalling the Regent Bird in beauty. "The name *Cnemophilus* (Mountain-slope Lover) has been appropriated to it, and the species I propose, with permission, to dedicate to yourself [Sir William MacGregor]. A second new bower bird, constituting a third species of the genus *Amblyornis*, and distinguished by a very ornate crest, will, if allowed, be honoured with the name of Lady MacGregor. It is well to note that the diversity in the structure of the bowers of this and of the other crested species of *Amblyornis* is far greater than the differences in their personal attributes. At your request the name of Mr. Belford, one of your party, has been associated with a capture in which he was concerned, a new honeyeater, of the genus *Melirrhophetes*. A similar compliment has been paid to another member of your collecting staff, Mr. C. Kowald, in connection with the beautiful genus of flycatchers, *Todopsis*. The number of species procured during the expedition to the Owen Stanley Range was 61, eight of them being apparently new to science. The expectations of ornithologists who have for some time been awaiting the exploration of that region will thus be in some measure fulfilled, notwithstanding that no new Birds of Paradise have been discovered. Perhaps, however, the greatest interest attaching to the ornithological results obtained arise from the fact that the decided change of climate observed at the altitude attained, over 13,000 feet, is not attended by a corresponding change in the types of bird life; it would seem that there is even here no infusion of forms characterizing temperate or cold latitudes. It is true that no birds were brought down from the highest points reached, but at 13,000 feet a flycatcher was procured which is essentially Australian in type. The presence of a blackbird, now first discovered in New Guinea, is not in this connection contradictory, since the genus *Merula* is represented in other of the Pacific Islands. Some interesting additions to our knowledge of the birds of the Louisiade Archipelago result from your visits to the islands within your jurisdiction. 21 species from East, Sudest, Ferguson, Rossel, and other isles have been determined; of these several cannot be identified with species previously known, as far as I am able to judge. As these birds were procured hurriedly, they doubtless represent but a very small proportion of the several faunas. If it were possible to station a collector on one of the larger islands, Sudest, for example, so that a fairly complete knowledge of its zoology could be obtained, science would be greatly benefited." The descriptive list of the birds which follow is very full and interesting. It is succeeded by a report by the same gentleman on the reptiles collected during the expedition; they consisted of two species of lizards, ten of snakes, and one frog. The snake-like lizard (*Lialis*) is common to Australia and New Guinea; the sleeping lizard (*Cyclodus*) is in Australia represented by other species; both are perfectly harmless. Of the snakes also, the greater majority are innocuous; the death-adder (*Acanthophis*) and the whip-snake (*Diemansia*) are, indeed, the only kinds to be dreaded. With the exception of the tree-snake (*Dendrophis*) the rest are the constrictors *Liasis*, *Chondropython*, *Aspidopython*, and *Enygrus*, and the colubine snakes *Lycodon*, *Mainophis*, and *Pappohis*. It is clear that deadly snakes are not to be added to the imaginary terrors of the Papuan climate. Four of the snakes represented are Australian species. The sole exampler of the batrachians is a little frog, which Mr. de Vis regards as a new species of its genus, *Michrohyla*. This is followed by Mr. Miskin's note on the collection of Lepidoptera. He says that a glance at the specimens, confirmed upon closer inspection, conveyed the impression that they represented only the fauna of the lower altitudes, although as a fact they were collected at some distance from the coast, proving, with but two or three exceptions, to be species already known to science. Sir William was unfortunately unable to give attention to this branch of zoology during his explorations of the higher altitudes, where new forms of the greatest interest might be anticipated to occur. He observes that the collection is interesting as showing the similarity of the New Guinea fauna with that of North East Australia, there being no less than 23 of the 53 species common to both regions; while of the 31 genera represented 25 are found in both countries.

Mr. Hedley in his note on the new Rhytida says that from the highest summit (13,000 feet) of the Owen Stanley Range two land shells were brought down by Sir W. MacGregor and committed for examination to the Queensland Museum. As these are the first traces of molluscan life collected in the New Guinea mountains, and as no form at all resembling them is described by Sig. Tapparone-Canefri, he feels some confidence in deciding them to be a hitherto unknown species. More globose than other of the genus, the glossy exterior, nacreous interior, characteristic colour and sculpture, stamp it as a Rhytida; and thus another genus is added to those common to Australia and New Guinea. Though well preserved both the specimens that furnish the following description are "dead shells"; one is slightly darker and less globose than its fellow:—*Rhytida Globosa*, n.s. Shell, depressed-globose, thin, translucent, perforate, very glossy; whorls $4\frac{1}{2}$, the earlier flattened, the latter rounded, rather rapidly increasing, the last a little expanded, not descending at the aperture; colour, reddish-chestnut above, lighter beneath, first three whorls bleached nearly white; sculpture almost effaced on the body whorl, where nearly obsolete spiral impressed lines cross the faint irregular growth lines; the earlier whorls exhibit fine close oblique striae cut by fine spiral grooves, a pitted (not striated) surface is offered by the first whorl and a half, which seem embryonic; suture impressed, slightly crenulated, bordered beneath by a narrow white band, which is in turn edged by a black line; aperture ovate, oblique; peristone simple above, slightly reflected below; interior bluish-white, probably iridescent when fresh; columella wall overlaid by a thin deposit; umbilicus narrow, partially hidden by the reflected peristone at its junction with the base; base a little inflated. Greater diam. 17 mm., lesser 14 mm., height 10 mm. A report from Baron von Mueller on the Papuan Highland plants collected brings the scientific part of the "White-Book" to a conclusion. This is somewhat lengthy to reproduce in full now, and it is not easy to summarize it adequately. Readers must therefore be referred to the volume itself, where the report appears at pp. 125–129.

THE DETERMINATION OF THE WORK DONE UPON THE CORES OF IRON IN ELECTRICAL APPARATUS SUBJECT TO ALTERNATING CURRENTS.[1]

When the case is one of a transformer, the problem may be solved by the employment of three dynamometers in the way I have already pointed out; but in that of an electro-magnet, where we have only one coil to deal with, the problem still admits of solution if we further employ a condenser of determined capacity, and possess a knowledge of the period by means of some speed indicator. The plan is as follows:—

Arrangement.—Having the machine and magnet in series, insert the three dynamometers in series immediately at one terminal of the electro-magnet, placing one pole of the condenser to the other terminal, and the second pole to that point of the middle dynamometer where its two coils join.

Observations.—Obtain good simultaneous readings of the three dynamometers, and if necessary of the speed indicator.

Elements of Calculation.—Let a_1, a_2, a_3 be the angles read upon the instrument (1) in the generating section, (2) in the electro-magnet section, (3) which has its coils divided.

Let the reducing formula for the three instruments be respectively,

$$(\text{Current})^2 = k_1 \theta,$$
$$\text{,,} \quad = k_2 \theta,$$
$$\text{,,} \quad = k_3 \theta.$$

Let C be the capacity of the condenser.
R ,, ,, resistance of the electro-magnet.
T ,, ,, semi-period.

Then the entire power at work beyond the terminals, *i.e.* the heating of the wire, the heating of the core by induced currents, and the heating of the core due to hysteresis is expressed by the simple formula—

$$\frac{T}{\pi C}\sqrt{k_1 k_2 a_1 a_2 - k_3^2 a_3^2}.$$

The expression itself is independent of the resistance, but if we desire to know the power heating the core, we must deduct from the above the power heating the wire, viz.:

$$k_2 a_2 R.$$

The difference between these two quantities also happens to be proportional to the tangent of the magnetic lag, another proof of the universal concurrency of lag and loss of power.

Royal Naval College, Greenwich, October.

[1] By T. H. Blakesley, M.A., M.Inst.C.E., Hon. Secretary of the Physical Society.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—Dr. A. Macalister has been elected Chairman of Examiners for the Natural Sciences Tripos. Dr. C. H. Ralfe has been appointed an additional examiner in Medicine. Prof. Hughes has been elected a member of the General Board of Studies, Mr. J. Prior and Mr. C. Geldard members of the Botanic Garden Syndicate, Dr. Cayley a member of the Library Syndicate, Prof. J. J. Thomson and Mr. H. F. Newall members of the Observatory Syndicate, Dr. Bradbury and Dr. Ingle members of the State Medicine Syndicate, Mr. L. Humphry a member of the Special Board for Medicine, Mr. W. N. Shaw of the Fire Prevention Syndicate, Dr. Besant of the Mathematical Board, Mr. Newall of the Physics and Chemistry Board, and Dr. Gaskell of the Biology Board. Mr. E. H. Griffiths, Lecturer at Sidney, has been approved as a Teacher of Physics with reference to the regulations for medical degrees.

SOCIETIES AND ACADEMIES.

London.

Royal Society, November 20.—"On the Specific Heats of Gases at Constant Volume. Part I. Air, Carbon Dioxide, and Hydrogen." By J. Joly, M.A., B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Prof. Fitzgerald, M.A., F.R.S., F.T.C.D.

In this first notice the specific heats, at constant volumes, of air, carbon dioxide, and hydrogen are treated over pressures ranging from 7 to 25 atmospheres. The range of temperature is not sensibly varied. It is found that the specific heats of these gases are not constant, but are variable with the density. In the case of air the departure from constancy is small and positive; that is, the specific heat increases with increase of the density. The experiments afford directly the mean value 0·1721 for the specific heat of air at the absolute density of 0·0205, corresponding to the pressure of 19·51 atmospheres. A formula based on the variation of the specific heat with density observed in the experiments ascribes the value 0·1715 for the specific heat at the pressure of one atmosphere. The formula assumes the specific heat to be a linear function of the density, which must as yet be regarded only as an approximation, the exact nature of the relation being concealed by variations among the experiments.

These results appear to be in harmony with the experiments of Wiedemann on the specific heat at constant pressure, and of Rowland on the mechanical equivalent of heat, from which the value 0·1712 is deduced for C_v at 760 mm.

The experiments on carbon dioxide reveal a more rapid variation of the specific heat with density, the variation in this case being again positive in sign. The formula

$$C_v = \rho \times 0{\cdot}2064 + 0{\cdot}16577$$

appears with considerable reliability to express the relation between specific heat and density.

The relation between specific heat and density in the case of hydrogen is of a negative character; that is, the specific heat diminishes with increase of density. The experiments are chiefly directed to elucidate this point, for, owing to the difficulty of preparing pure hydrogen, it was found that variations in the quantitative results of experiments on different samples of the gas were unavoidable. Accordingly the experiments were directed to a comparison of the specific heats of like samples of the gas at different densities. The variation with density is small, but (with one exception) all experiments on the purer hydrogen ascribe a negative character to it.

The nature of these variations of specific heat with change of density is, in the case of the three gases, in accord with their behaviour as regards Boyle's law, within the range of pressure.

The experiments were effected in the steam calorimeter, a differential method being used in which an empty or idle vessel is thermally compared with the vessel holding the gas at high pressure. The vessels possessing approximately the same calorific capacity, the result, theoretically, is as if the gas was dealt with isolated from any containing vessel. Although practically this is not attained, many sources of error are eliminated by the procedure adopted.

November 27.—"On the Homology between Genital Ducts and Nephridia in the Oligochæta." By Frank E. Beddard, M.A., Prosector of the Zoological Society. Communicated by Prof. E. Ray Lankester, M.A., LL.D., F.R.S.

It is usually stated in text-books that the genital ducts of the Oligochæta are homologous with nephridia; but nevertheless the question is one which has not yet been satisfactorily settled, for the total independence of the two structures in *Lumbricus* and those aquatic Oligochæta of which the development is known is a difficulty in the way of accepting this view. Claparède, who first clearly formulated the arguments in favour of regarding the genital ducts as slightly modified nephridia, made a mistake in stating that the genital segments of the aquatic Oligochæta contain no nephridia; this error was pointed out by Vejdovsky, who discovered that the genital segments are originally furnished with nephridia, which atrophy on the ripening of the sexual products and the appearance of their ducts. Prof. Lankester pointed out that in *Lumbricus* the genital ducts and the nephridia have a close relation to one or other of the two pairs of setæ with which each segment is provided. He suggested that the genital ducts might represent the only portion left of a ventrally opening series of nephridia. M. Perrier's memorable investigations into the structure of exotic earthworms tended at first to confirm this theory. He discovered that in one earthworm (*Plutellus*) the nephridia alternated in position from segment to segment, thus suggesting that the supposed original two sets of nephridia had both partly persisted and partly disappeared. In other forms the nephridia were found to be related to the ventral setæ, and the genital apertures to the dorsal setæ, the exact converse of the condition which occurs in *Lumbricus*. Later investigations, however, which resulted in the discovery that the genital apertures and nephridiopores may coincide at the same setæ, led M. Perrier to abandon the hypothesis. My own discovery, first published in the Proceedings of this Society, that in *Acanthodrilus multiporus* there are more than a single pair of nephridiopores to each segment, removed the difficulties urged by Perrier. And as this discovery has been extended by myself and by others to many species and genera of earthworms, there can be no longer any intrinsic improbability in the hypothesis. The whole subject has been lately reviewed by Eisig in his treatise upon the anatomy and physiology of the Capitellidæ, which forms one of the series of monographs issued by the Zoological Station at Naples. Dr. Eisig decides that the genital ducts are probably modified nephridia in the Oligochæta; in the Capitellidæ they certainly are; but, as the Capitellidæ do not appear to me to be so nearly related to the Oligochæta as Dr. Eisig considers, I should regard this argument as only having the force that an argument from analogy can have. Since the appearance of Dr. Eisig's work, an important paper by Dr. Stolc, dealing with the generative organs of *Æolosoma*, has come into my hands; it appears that in this Annelid there are no special sperm ducts, but that the function of such ducts is performed by several pairs of nephridia. This fact, however, interesting though it is, is not a proof of the homology between sperm ducts and nephridia in other types.

I have lately had the opportunity of studying the development of the New Zealand species *Acanthodrilus multiporus*. The sum of money which the Government Grant Committee of the Royal Society were good enough to place at my disposal has enabled me to defray the expenses of this investigation.

In the young embryos of this worm each segment is furnished with a pair of nephridia, each opening by a ciliated funnel into the segment in front of that which carries the dorsally placed external pore. In later stages the funnels degenerate, and that portion of the tube which immediately follows the funnel becomes solid, losing its lumen; at the same time the nephridium branches, and communicates with the exterior by numerous pores. At a comparatively early stage, four pairs of gonads are developed in segments X.–XIII.; each of these is situated on the posterior wall of its segment, as in *Acanthodrilus annectens*, and not on the anterior wall, as in the majority of earthworms. When the gonads first appear, the nephridial funnels, with which they are in close contact, are still ciliated, and their lumen is prolonged into the nephridium for a short distance. Later the cilia are lost, and the funnels increase greatly in size, while those of neighbouring segments—in fact, all the remaining funnels—remain stationary for a time, and then become more and more degenerate. The large funnels of the genital segments become the funnels of the vasa differentia and oviducts; it will be observed that the number of ovaries and oviducal funnels (*two* pairs) at first corresponds to that of the testes and sperm duct funnels; subsequently the gonads and commencing oviducts of segment XII. atrophy. Each of these large funnels is continued into a solid rod which passes back through the septum, and then becomes continuous with a coiled tuft of tubules, in which there is an evident lumen, and which is a part of the nephridium of its segment. In the segments in front of and behind the genital segments, the rudimentary funnels communicate in the same way with a solid rod of cells which runs straight for a short distance and then becomes coiled and twisted upon itself and provided with a distinct lumen. In fact, apart from the relative size of the funnels and the presence of the gonads, it would be impossible to state from which segment a given section through the terminal portion of a nephridium had been taken. In a later stage the large funnels of the genital segments become ciliated; but this ciliation takes place before there is any marked change in the tube which is connected with the funnel.

In the young worm which has just escaped from the cocoon the funnels are ciliated, and they are each of them connected by a short tube, in which a lumen has been developed, but which ends blindly in close proximity to a coil of nephridia. No trace of any nephridial tube other than the sperm duct or oviduct could be observed, whereas in the preceding and succeeding segments the rudimentary nephridial funnel, and a straight tube leading from it direct to the body wall, was perfectly plain. Dr. Bergh has figured, in his account of the development of the generative organs of *Lumbricus*, a nephridial funnel in close contact with the funnel of the genital duct. It may be suggested that a corresponding funnel has been overlooked in the embryo *Acanthodrilus*; the continuity of a structure, identical (at first) with the nephridia of the segments in front and behind, with the genital funnels, seems to show that a search for a small nephridial funnel would be fruitless.

I can only explain these facts by the supposition that *in Acanthodrilus multiporus the genital funnels and a portion at least of the ducts are formed out of nephridia*. This mode of development is a confirmation, to me unexpected, of Balfour's suggestion that in the Oligochæta the nephridium is broken up into a genital and an excretory portion.

In the comparison of the facts, briefly described here, with the apparently independent origin of the generative ducts in other Oligochæta, it must be borne in mind that in *Acanthodrilus* the segregation of the nephridium into several almost detached tracts communicating with the exterior by their own ducts precedes the formation of the genital ducts.

"The Patterns in Thumb and Finger Marks: on their Arrangement into naturally distinct Classes, the Permanence of the Papillary Ridges that make them, and the Resemblance of their Classes to ordinary Genera." By Francis Galton, F.R.S.

The memoir describes the results of a recent inquiry into the patterns formed by the papillary ridges upon the bulbs of the thumbs and fingers of different persons. The points especially dwelt upon in it are the natural classification of the patterns, their permanence throughout life, and the apt confirmation they afford of the opinion that the genera of plants and animals may be isolated from one another otherwise than through the influence of natural selection.

The origin of the patterns was shown to be due to the existence of the nail, which interfered with the horizontal course of the papillary ridges, and caused those near the tip to run in arches, leaving an interspace between them and the horizontal ridges below. This interspace was filled with various scrolls which formed the patterns. The points or point at which the ridges diverged to inclose the interspace were cardinal points in the classification. It was shown that there were in all only nine possible ways in which the main features of the inclosure of the interspace could be effected. In addition to the nine classes there was a primary form, occurring in about 3 per cent. of all the cases, in which the interspace was not clearly marked, and from this primary form all the other patterns were evolved. The forms of the patterns were easily traced in individual cases by following the two pair of divergent ridges, or the one pair if there was only one pair, to their terminations, pursuing the innermost branch whenever the ridge bifurcated, and continuing in an adjacent ridge whenever the one that was being followed happened to come to an end. Twenty-five of the principal patterns were submitted, and a few varieties of some of them, making a total of forty. They are by no means equally frequent.

The data as to the permanence of the patterns and of the ridges that compose them were supplied to the author by Sir W. J. Herschel, who, when in the Indian Civil Service, introduced in his district the practice of impressing finger marks as a check against personation. Impressions made by one or two fingers of four adults about thirty years ago, and of a boy nine years ago, are compared with their present impressions. There are eight pairs of impressions altogether, and it is shown that out of a total of 296 definite points of comparison which they afford, namely the places where ridges cease, not one failed to exist in both impressions of the same set. In making this comparison no regard was paid to the manner in which the several ridges appear to come to an end, whether abruptly or by junction with another ridge. The reason was partly because the neck where junction takes place is often low, and may fail to leave a mark in one of the impressions.

Lastly, the various patterns were shown to be central typical forms from which individual varieties departed to various degrees with a diminishing frequency in each more distant degree, whose rate was in fair accordance with the theoretical law of frequency of error. Consequently, wide departures were extremely rare, and the several patterns corresponded to the centres of isolated groups, whose isolation was not absolutely complete, nor was it due to any rounding off by defined boundaries, but to the great rarity of transitional cases. This condition was brought about by internal causes only, without the least help from natural selection, whether sexual or other. The distribution of individual varieties of the same patterns about their respective typical centres was precisely analogous in its form, say, to that of the shrimps about theirs, as described in a recent memoir by Mr. Weldon (Roy. Soc. Proc., No. 291, p. 445). It was argued from this, that natural selection has no monopoly of influence either in creating genera or in maintaining their purity.

"The Conditions of Chemical Change between Nitric Acid and certain Metals." By V. H. Veley, M.A.

This paper is in continuation of a preliminary communication on the same subject. The main points contained in it are as follows:—

(1) The metals copper, mercury, and bismuth do not dissolve in nitric acid of about 30 per cent. concentration (the acid commonly employed for the preparation of nitric oxide gas) and heated to a temperature of 30° C., provided that nitrous acid is neither present initially nor formed subsequently. To prevent this, it is necessary in the cases of copper and bismuth to add a small quantity of some oxidizing substance, such as hydrogen peroxide or potassium chlorate, or, as less efficacious, potassium permanganate, or to pass a current of air, or, lastly, such a substance as urea, which destroys the nitrous acid by its interaction.

(2) If the conditions are such that these metals dissolve, then the amount of metal dissolved and the amount of nitrous acid present are concomitant variables, provided that the nitric acid is in considerable excess. Change of conditions, such as concentration of acid and variation of temperature, which increase the former increase also the latter.

(3) If the conditions are such that these metals dissolve, it would appear that the metallic nitrite is at first formed, together with nitric oxide; the former is decomposed by the excess of nitric acid to liberate nitrous acid, while the latter reduces the nitric acid to form a further quantity of nitrous acid.

Eventually the net result is the product of two reverse chemical changes represented by the equations:—

$$\text{(i.)}\ 2NO + HNO_3 + H_2O = 3HNO_2,$$
$$\text{(ii.)}\ 3HNO_2 = 2NO + HNO_3 + H_2O.$$

The nitrous acid is thus destroyed as fast as it is generated.

(4) If the conditions are such that metals dissolve in nitric acid, then nitrous acid is invariably the initial product of reduction.

(5) The metals copper, mercury, and bismuth dissolve very readily in a 1 per cent. solution of nitrous acid; under these conditions nitric acid present in slight excess interferes with, rather than promotes, the chemical change. This result is probably due to the greater stability of nitrous acid in the presence of nitric acid.

(6) Hydrogen gas reduces nitric to nitrous acid in presence of cupric or lead nitrate; it also converts mercuric into mercurous nitrate, but does not produce any change in solutions of bismuth and zinc nitrates dissolved in nitric acid.

Physical Society, November 14.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were made:—On certain relations existing among the refractive indices of the chemical elements, by the Rev. T. Pelham Dale. The first part of the paper corroborates the results announced in a communication made in May 1889 on the same subject, and says that as far as experimental data are forthcoming the refraction $(\mu - 1)$ divided by the vapour density (d) is equal to a constant multiplied by some integer. Several metals whose refractions have since been determined conform to this law. On examining the relation between molecular weight (M) and refraction, similar conclusions are arrived at; for to a fair degree of approximation the ratio $M/(\mu - 1)$ is a constant, or a simple multiple of this constant. The question as to how far the relation $(\mu - 1)/d = c$ holds good for the same element in the three states of vapour, liquid, and solid, has been examined as far as data exist for this purpose. The resulting numbers are not identical, but some of the data themselves are doubtful. Another relation is between the molecular distances (h) (see Proc. Phys. Soc., vol. ix. p. 167) and the atomic weight (a) of the elements, h being nearly proportional to $\sqrt{a}$. In the case of selenium, sulphur, and phosphorus, the agreement is close, but for bromine, chlorine, and carbon, not so good. A fifth relation appears to exist between the upper limit of refraction and the line spectra of elements. For example, the upper limit of refraction for selenium occurs at wave-length 5295·7, whilst its spectrum exhibits a remarkable series of strong lines about this wave-length. A similar relation apparently holds with sulphur, phosphorus, and bromine. Gold also shows a series of strong lines about c, in the vicinity of which the metal has the greatest reflective power. The author finds that selenium polarizes and reflects nearly all the light that falls on it at a large angle, and suggests that it may be used in polariscopes. He has also endeavoured to connect together the phenomena of a limit of refraction and anomalous dispersion. In the case of fuchsin the dark space coincides with the limit of refraction, and the same is probably true of cyanin. If one of the anomalous indices be given the other can be found. He also believes that bodies of high molecular weight give anomalous dispersion, and thinks solutions of iodine will exhibit the phenomenon. The mathematical investigation of the whole subject involved difficulties arising from the want of trustworthy data, and the author hopes that some member will take up the necessary experimental determinations. Dr. Gladstone thought the author under-estimated the amount of work done and in progress on the subject, for the question whether $(\mu - 1)/d$ is constant or not is being investigated by many. The French physicists, he said, had found the quantity nearly constant, but Lorenz's expression $(\mu^2 - 1)/(\mu^2 + 2)$ is slightly better when applied to compounds in the liquid and gaseous states. Metals were difficult to deal with, especially as, according to the recent paper of Du Bois and Reubens, their refractions do not follow the law of sines. Mr. Dale here suggested that they might be related to hyperbolic sines. Dr. Gladstone, continuing, said that, by taking solutions of metals, it was found that their specific refractive energies were nearly inversely as the square roots of their combining weights, but at present the known cases were not sufficient to establish a law. Prof. Rücker said that of the two expressions, $(\mu - 1)/d$ and $(\mu^2 - 1)/(\mu^2 + 2)$, the latter seemed preferable, for it could be converted into electrical quantities by writing K for μ^2. The expression then becomes $(K - 1)/(K + 2)$, and if this can be shown to be constant by electrical work, this would be an argument in its favour. On the subject of anomalous dispersion he directed Mr. Dale's attention to Mr. Glazebrook's Report on Optical Theory made to the British Association. Mr. Dale, in reply, pointed out that, from the nature of the two formulæ, any inaccuracy or variation in μ would affect theirs more than Lorenz's. He also thought that $(\mu - 1)/d$ was a limit towards which the numbers tend.—Tables of spherical harmonics, with examples of their practical use, by Prof. J. Perry, F.R.S. The author defined a spherical harmonic as a homogeneous function of x, y, z, satisfying the equation—

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = 0,$$

stated the fundamental properties of such functions, and pointed out their importance in problems on heat, electricity, and hydrodynamics. Referring to zonal harmonics (homogeneous functions of $(x^2 + y^2)^{\frac{1}{2}}$ and z), he showed that these harmonics are symmetrical with respect to the axis of z, and might be expressed as functions of the angle (θ) which the line joining the point (x, y, z) to the origin makes with the axis of z, multiplied by r^i; where r is the radius-vector and i the degree of the homogeneous function. These functions of θ are called zonal surface harmonics, and are designated by $P_0, P_1, P_2, \ldots P_i$, according to the degree of the function, and it was the values of these quantities which form the tables brought before the Society. The tables comprise the values of P_1 to P_8, and are calculated to 4 places of decimals and for every 1° between 0° and 90°. As an example of the use of such tables, the case of a spherical surface covered with attracting matter whose density varied as the square of its distance from a diametral plane was taken. It was required to find the potential both outside and inside the sphere, and to determine the equipotential surfaces and lines of force. The potentials inside (A) and outside (B) were shown to be given by

$$\frac{A}{\pi} = 8 + \frac{16}{5} r^2 P_2 \quad \text{and} \quad \frac{B}{\pi} = \frac{8}{r} + \frac{16}{5} \frac{1}{r^3} P_2$$

respectively. By giving A and B definite values, and choosing values of r, the corresponding P_2's can be calculated, and the value of θ determined from the tables. Hence any equipotential surface can be easily determined, and lines drawn to cut these surfaces orthogonally are lines of force. Another problem which had been tried consisted in finding the directions of the lines of force near a circular coil of rectangular cross-section when an electric current circulates in the coil. This was treated approximately by first calculating the potential at 6 points along the axis in the neighbourhood of the coil, and then finding by Gauss's method the coefficients A_0, A_1, A_2, &c., of an expression in ascending powers of z which agreed with the calculated potentials at the points chosen. The formula

$$V = A_0 + A_1 r P_1 + A_2 r^2 P_2 + \&c.,$$

or its corresponding expression in inverse powers of r, was then assumed to give the potential at any point in the space considered. By giving V definite values, a series of equipotential surfaces were determined, and the lines of force drawn. On putting the calculations to the test of experiment, the approximate solution of this very difficult problem was found to be very nearly correct.

Linnean Society, November 20.—Prof. Stewart, President, in the chair.—Mr. G. Murray exhibited specimens of a fresh-water *Delesseria* previously unknown.—On behalf of Mr. Henry Hutton, of Cape Town, Mr. B. D. Jackson exhibited some follicles and seeds of a somewhat rare Asclepiad (*Dregia floribunda*); and showed also, on behalf of Mr. W. Matchwick, some ripened seeds ot *Ailanthus glandulosa* from a tree at Reigate said to be a hundred years old.—Prof. Bower exhibited several drawings from microscopic sections of Carboniferous nodules belonging to Prof. Williamson, and pointed out the peculiarities of structure. Microscopic details of such sporangia being very rare, he remarked that a comparison of the slides showed a peculiar uniformity of type. For comparison with these sporangia from the coal, he exhibited sections of the sporangia of *Todea barbara*, and while not going so far as to refer these Carboniferous sporangia (which are not attached to the plants which bore them) to any distinct genus, he thought the Osmundaceous affinity was unmistakable.—Mr. J. E. Harting exhibited some original MSS. and water-colour sketches of birds, fishes, and plants, found in Sussex by William Markwick, the friend and correspondent of Gilbert White, of Selborne, which had been presented by him to the Society in his lifetime, and had been lost sight of for many years. The drawings are sufficiently well executed to enable the correct determination of several species which the author had failed to identify.—A paper was read by Prof. T. Johnson, of Dublin, on *Punctaria*, a genus of brown seaweeds (*Phæophyceæ*). He described in detail the formation of the plantlets by trichothallic germination from the tufts of primary hairs and from the secondary hairs formed on epicortical filaments on the old thallus of *P. plantaginea* and *P. latifolia*. He pointed out that neither the nature of the dots nor the position of the sporangia is of specific value, and that sporangiferous and plant-forming hairs are to be found on the filamentous root-disk of *P. tenuissima*, Grev., which, he maintained, is the young state of one species, of which *plantaginea* and *latifolia* are the more mature growths.—Mr. Vaughan Jennings gave an abstract of a paper on a variety of sponge, *Alectona millari*, Carter, boring in the shell of *Lima excavata*, from the Norwegian coast. The sponge had endeavoured to grow inwards, dissolving the nacreous layer and encroaching on the mollusk, instead of restricting its wanderings to the thickness of the shell. The mollusk had retaliated by depositing fresh layers on the intruder, and the struggle had gone on until the chambers were several times the normal thickness of the shell, and were roofed over by a thin curved layer of secondary shell substance, while the points at which branches had been pushed further in were represented by thick conical papillæ.

CAMBRIDGE.

Philosophical Society, November 10.—Prof. Darwin, President, in the chair.—The following commuications were made:—Note on the principle upon which Fahrenheit constructed his thermometrical scale, by Arthur Gamgee, F.R.S., Emeritus Professor of Physiology in the Owens College (Victoria University). The author commenced by drawing attention to the fact that, although the Fahrenheit thermometer has been so generally used in England, no accurate information was to be found in our text-books concerning the principles upon which its scale had originally been constructed. He referred, however, to a view advanced by Prof. P. G. Tait in his elementary treatise on "Heat," and which had been accepted by many teachers, according to which Fahrenheit divided his scale between 32° and 212° into 180 degrees, in imitation of the division of a semicircle into 180 degrees of arc. This theory rested on the incorrect supposition that, before Fahrenheit's time, Newton had suggested, as a basis for a thermometric scale, the fixing of the freezing and boiling points of water, the space between these being divided into a number of equal degrees. The author pointed out that in his "Scala graduum caloris," Newton made no such suggestion as that attributed to him by Prof. Tait, and prior to him by Prof. Clerk Maxwell; and, indeed, that Fahrenheit had settled the basis of his scale and had constructed a large number of thermometers, which were used by scientific men throughout Europe, many years before the discovery by Amanton (which Fahrenheit confirmed and gave precision to) of the fact that under a constant pressure the boiling point of water is constant. The author stated that the thermometers which were first constructed by Fahrenheit were sealed alcoholic thermometers, provided with a scale in which two points had been fixed. The zero of the scale, representing the lowest attainable temperature, was found by plunging the bulb of the thermometer in a mixture of ice and salt, whilst the higher of the two points was fixed by placing the thermometers under the armpit or inside the mouth of a healthy man. The interval between these two points was, in the first instance, divided into 24 divisions, each of which corresponded to supposed well-characterized differences in temperature, and each being subdivided into four. In his later alcoholic and mercurial thermometers, the 24 principal divisions were suppressed in favour of a scale in which 96 degrees intervened between zero and the temperature of man; in these later thermometers the 32nd degree was fixed by plunging the bulb of the thermometer in melting ice. The author then pointed out that Fahrenheit was led to construct mercurial thermometers in order to be able to ascertain the boiling point of water; with this object the scale, constructed as has been stated, was continued upwards, in some cases so as to include 600 degrees. It was *as the result of experiment alone*, that the number 212 was obtained as the temperature at which water boils, at the mean, atmospheric pressure. The author in conclusion argued that Fahrenheit took as the basis of his thermometric scale the duodecimal scale, which he was constantly in the habit of employing.—On variations in the floral symmetry of certain plants with irregular corollas, by Mr. W. Bateson and Miss A. Bateson.—On the nature of the relation between the size of certain animals and the size and number of their sense-organs, by H. H. Brindley. In speculation as to the evolution of various forms, it is generally held as a principle that the conditions of the struggle for existence are such that variations in the direction of atrophy or diminution in bulk of a useless organ must necessarily be beneficial by reason of the saving of tissue and effort which is effected by this reduction. It has been assumed by many that this benefit must be so marked as to lead to the natural selection of the individuals thus varying. This principle has been invoked especially in the case of sense-organs, and, for example, it has been suggested that the blindness of cave-fauna may have come about by its operation. With the object of testing the truth of this assumption, it seemed desirable to obtain a knowledge of the normal variations in size and number of sense-organs occurring within the limits of a single species. The cases chosen were (1) the olfactory organ of fishes (eel, loach, *Pleuronectidæ*, &c.), and (2) the eyes of *Pecten opercularis*. In the first case tables were given showing that large individual fluctuations occur, but that on the whole the number of olfactory plates increases with the size of the body; and it was pointed out that a similar relation holds with regard to the eyes of fishes. In the case of *Pecten*, however, though the *size* of the eyes increases with the diameter of the animal, yet in specimens having a diameter of 3 cm. to 6 cm. the *number* of the eyes is not thus related (cp. Patten), but varies in a most surprising and, as it were, uncontrolled manner. Statistics were given showing that, in individuals of the same size, the number of eyes may vary between 70 and 100, and that no uniformity is to be found. It was pointed out that these eyes are large and complicated organs, having lens, retina, tapetum, &c., involving great cost in their production. These facts suggest that the "economy of growth" cannot be a principle of such precise and rigid character as to warrant its employment as a basis for speculation as to the mode of evolution of a species. The diverse results in the case of the two sets of organs examined further indicate that the problem is one of far greater complexity, and shows clearly that argument from analogy is inadmissible in these cases.—On the oviposition of *Agelena labyrinthica*, by C. Warburton. The oviposition and cocooning of *Agelena labyrinthica* is a striking case of the performance of a series of complicated operations in obedience to a blind instinct. The eggs are always laid at night, but the presence of artificial light is quite disregarded by the animal. For about 24 hours before laying, the spider is engaged in preparing a chamber for the purpose. Near its roof a small sheet is then formed, and the eggs are laid upwards against it and are covered with silk. A box is then constructed with this sheet as its roof, and is firmly attached by its angles to the roof and floor of the chamber. This box is constructed and jealously guarded even if the eggs are removed immediately on oviposition. The whole operation involves about 36 hours of almost incessant industry.—Supplementary list of spiders taken in the neighbourhood of Cambridge, by C. Warburton.

PARIS.

Academy of Sciences, November 24.—M. Hermite in the chair.—Experiments on the mechanical actions exerted on rocks by gas at a high pressure and in rapid motion, by M. Daubrée. The working of diamond mines in South Africa has revealed the existence of vertical canals or chimneys in the earth's crust. All these chimneys are circular or elliptical in section. Their diameter varies from 20 metres to 450 metres, and is generally comprised between 150 metres and 300 metres. Their depth is considerably greater. M. Daubrée finds that he can produce precisely similar formations by means of the gas evolved when dynamite or gun-cotton is exploded, and therefore believes that the chimneys referred to are produced by the action of gases at high pressures and velocities. That the effect may be considerable is evident from a consideration of the action of gases at high pressure upon the meteorites that traverse our atmosphere.—On some facts relative to the history of carbon, by MM. Paul and Léon Schutzenberger.—On the relation of the circumference to the diameter, by Prof. Sylvester. Lambert has shown that π cannot be the square root of a whole number. The author proves that π cannot be the root of a rational equation.—Observations of Zona's comet (November 15, 1890) made at Paris Observatory with the West Tower equatorial, by M. G. Bigourdan. Three observations for position were made on November 21.—Observation of the same comet made with the East Tower equatorial, by Mdlle. D. Klumpke. An observation for position was made on November 21.—Generalization of one of Abel's theorems, by M. Albert La Maestra.—Variations of conductivity under different electrical influences, by M. Ed. Branly. If a circuit be formed with a Daniell's cell, a high resistance galvanometer, and a very thin layer of copper deposited on a ground glass or ebonite plate, 7 cm. long and 2 cm. wide, only an insignificant current passes. An abrupt diminution of resistance is experienced, however, when one or more electric discharges, from a Wimshurst machine or a Ruhmkorff's coil, are produced in the neighbourhood of the circuit. The action diminishes as the distance of the sparking apparatus increases, but it may be very easily observed, without special precautions, at several metres. The author has examined the conditions necessary to produce the observed phenomena.—Periodic visibility of interference fringes, by M. Charles Fabry.—On the artificial production of a chromium blue, by M. Jules Garnier. By heating in a brasqued crucible a mixture of K_2CrO_4 48·62 grammes, fluor spar 65 grammes, and silica 157 grammes, the author obtained a blue glass, the colour of which he attributes to reducing action taking place at a high temperature in the chromium salt.—Researches on the application of the measure of rotatory power to the determination of combinations formed by aqueous solutions of malic acid with the double molybdate of potassium and sodium, and the acid molybdate of sodium, by M. D. Gernez.—On the employment of the potato in the agricultural distillery in France, by M. Aimé Girard. The French potato crop in 1890 was remarkable for quantity and quality. The author urges the application of these crops to the production of alcohol, and experimentally proves that such a use may be commercially successful.—On the spermatozoa of Locustides, by M. Armand Sabatier.—On *Cyclatella annelidicola* (van Beneden and Hesse), by M. Henri Prouho.—On the destruction of *Heterodera schachtii*, by M. Willot.—On an eruptive rock from Ariège, and on the transformation of felspar into wernerite, by M. A. Lacroix.—On a tornado observed at Fourchambault (Nièvre), by M. Doumet-Adanson.

GÖTTINGEN.

Royal Society of Sciences.—The *Nachrichten* from January to June 1890, contain the following papers of scientific interest:—

January.—B. Galitzine, on Dalton's law. The author reviews the different estimates taken of Dalton's law, and concludes from his experiments that it is not accurately true either as regards the resultant pressure of a mixture of gases, or as regards the pressure of vapours.

February.—Nernst, on a new principle in the determination of molecular weights. The author gives the results of experiments conducted to verify the familiar formula $a - a'/a' = n/100$.

March.—Felix Klein, on the theory of Lamé's functions. This is an account of lectures in continuation of previous courses, and is concerned with the extension of Lamé's equation, the replacing of his functions by algebraic forms, and the conformable representation of a quotient of two solutions. Hertz, on the fundamental equations of electrodynamics. This is an attempt to explain the method of proof and the significance of Maxwell's equations.

May.—Voigt, on the coincidence of two simple tones—being a reconciliation of the formulæ obtained by Helmholtz and R. König. Hartlaub, contribution to the knowledge of the *Comatulidæ* of the Indian Archipelago. Riecke, the pyro-electricity of tourmaline. The object is twofold—to examine the correctness of the formula $\epsilon = E(1 - e^{-at})$ for the electric charge during the cooling (where t denotes the time), and to find the dependence upon the difference of the initial and final temperatures of the electricity developed during the cooling.

June.—Riecke, on W. Gibb's theory of the changes of state of a system; geometrical development of the same. Fr. Brioschi, on a transformation of the differential equation of the even sigma-function of two variables; deduction of the development. Schoenflies, the mutual relations of the various theories of crystalline structure; a general account.

CONTENTS.

THURSDAY, DECEMBER 11, 1890.

A CURE FOR TETANUS AND DIPHTHERIA.

THE greatest interest has been aroused in scientific circles in Berlin by a paper in the current number of the *Deutsche medicinische Wochenschrift*[1] by Behring and Kitasato. These well-known bacteriologists, who for a long time past have been working in Dr. Koch's Hygienisches Institut, have not only succeeded in producing immunity against diphtheria and tetanus, but also in curing animals already infected by these diseases. Their results are to a great extent self-explanatory, and there is every reason to expect that the same method will be found to be applicable to other infectious diseases. The most remarkable part of their discovery is the fact that the blood of an animal that has been made immune against diphtheria possesses the extraordinary power of destroying the poison formed by the microbe of this disease. This power is also possessed by the serum of such an immune animal, which serum can therefore be used as a curative means on other animals that are suffering from this disease. The same statement holds good for tetanus.

Before describing in detail these interesting results, it will be well to give a short historical review of some recent bacteriological work which can be regarded as having led up to this discovery.

Towards the end of 1888 Nuttall,[2] working in Flugge's laboratory at Breslau, discovered that various bacteria are destroyed when mixed with fresh blood or blood-serum, and further that this destruction cannot be ascribed to the action of cellular elements, but rather to the fluid part of the blood. This discovery (which really arose from the German criticism of Metschnikoff's phagocyte theory) was soon followed by the work of Buchner[3] and Nissen[4] on the bacteria-killing power of the cell-free blood-serum. These authors considered that their work necessitated a limitation of the phagocyte theory, and suggested a new view of the nature of immunity, whether natural or acquired. In other words, they suggested that immunity was conditioned by the bacteria-killing power of the various body fluids rather than by that of any particular kind of cell. These opinions were rather severely criticised in a paper by Lubarsch[5] that was published towards the end of last year. Lubarsch emphasized the fact that while the serum of the rabbit—an animal extremely sensitive to anthrax, has a great power of destroying anthrax-bacilli, horses' serum has no such power, although this animal is comparatively refractory to the disease. Again, while on the one hand such eminently pathogenic microbes as the anthrax and cholera bacilli are capable of being destroyed by serum from various animals, several perfectly harmless microbes find blood-serum to be an excellent food-medium. Further, though the serum of the rabbit kills anthrax bacilli in a pre-eminent degree, the living blood-plasma of this animal can only do so to an infinitesimal extent. Such considerations suggested to Lubarsch that the bacteria-killing power of the blood-serum was a fact rather of the nature of an epi-phenomenon than an essential factor in the conflict between the organism and the microbe. In May of this year appeared my own work on "Defensive Proteids."[1] I gave this name to a new class of proteid bodies, which I found to possess a bacteria-killing power, and which I have obtained from the spleens and lymphatic glands of various animals. This work has a distinct bearing on the foregoing, in that it suggests that the bacteria-killing power of blood-serum is due to minute traces of these substances liberated from the breaking down of lymphatic cells. The absence of a bacteria-killing power from certain kinds of serum (*e.g.* horse) and from living blood-plasma (as has been shown for that of the rabbit in regard to anthrax), appears to be connected with the intactness of the leucocytes in these special cases. Further, the fact that I obtained these substances from cells which either are, or can become, phagocytes, may be taken as an additional proof of Metschnikoff's well-known theory. These substances appear to be absent from the normal blood-plasma, or, at any rate, only present in such small quantities that they cannot be separated from it. With blood of febrile animals, however, the case is different, and from such blood I have been able to isolate a bacteria-killing substance.[2] This fact appears to indicate that these substances are actually used by the organism in its reaction against the attack of pathogenic microbes.

During last summer, while I have been engaged in this work, various other papers have appeared, which tend to show still more clearly that the bacteria-killing power of the blood serum (or if my work be accepted, of defensive proteids) is of real importance in the production of immunity. Bouchard[3] was, I think, the first of many authors who have succeeded in showing that the bacteria-destroying power of blood-serum from immune animals, is greater than that of normal serum. Bouchard proved this in the case of bacillus pyocyaneus for rabbits. He made the animals immune by injections of sterilized culture fluids, and found that serum from such animals exerted a far greater "bactericidal" action on the microbe in question than serum from a normal animal. Behring and Nissen,[4] in a paper published in May of this year, went a step further. They showed that whereas blood serum from an animal made immune against anthrax exerted an increased bactericidal action on the anthrax bacillus, it showed no increased action on the bacillus pyocyaneus. Conversely blood-serum from an animal made immune against the latter microbe, had no increased action on the anthrax bacillus, though it exerted a powerful bacteria-killing action on pyocyaneus. The authors considered that they had proved the ex-

[1] No. 49, December 4, 1890, p. 1113. "Ueber das Zustandekommen der Diphtherie-Immunität und der Tetanus-Immunität bei Thieren."
[2] "Experimente über die bakterienfeindlichen Einflüsse des thierischen Körpers" (*Zeitschrift für Hygiene*, vol. iv. p. 353).
[3] "Ueber die bakterientödtende Wirkung des zellenfreien Blutserums" (*Centralblatt für Bakteriologie*, vol. v. p. 817, and vol. vi. p. 1).
[4] "Zur Kenntniss der bakterienvernichtenden Eigenschaft des Blutes" (*Zeitschrift für Hygiene*, vol. vi. p. 487).
[5] "Ueber die bakterienvernichtenden Eigenschaften des Blutes und ihre Beziehungen zur Immunität" (*Centralblatt für Bakteriologie*, vol. vi. p. 528).

[1] "A Bacteria-killing Globulin" (Proceedings of the Royal Society of London, vol. xlviii. p. 93), and "The Conflict between the Organism and the Microbe"; Part 2, "On Defensive Proteids" (*British Medical Journal*, July 12, 1890.)
[2] "Indications of a Method of Curing Infectious Diseases." Read at the Leeds Meeting of the British Association for the Advancement of Science, September 1890.
[3] "Sur l'effet des produits sécrétées par les microbes pathogènes" (Paris, 1890)
[4] "Ueber den bakterienfeindlichen Einfluss von verschiedenen Serumarten" (*Zeitschrift für Hygiene*, vol. viii. p. 412).

istence of two bodies, each having a specific action on one of the two microbes in question; and further that these substances are present in animals made immune against the above-named diseases. These remarkable conclusions acquire a still greater interest when received in the light of a research by Gamaleia published at the beginning of last year.[1] Gamaleia found that the aqueous humour of a sheep, about three days after inoculation with attenuated anthrax, acquires bactericidal properties for this microbe. This condition lasts for nearly a month, and then gradually vanishes, though, as is well known, the sheep remains immune for a far longer period. These researches, therefore, suggest, firstly, that when an animal has been made immune against a pathogenic microbe, its blood and other body fluids contain a substance capable of destroying the microbe in question; secondly, it follows that such protective substances can remain in the body undestroyed for a considerable time; and thirdly, that they can be present in such quantities as to be able to kill the microbes involved (even without the help of living cells) and yet produce no appreciable ill effect on the general health of the animal. If this is so, why should it not be possible to cure any infectious disease by injecting a "lymph" obtained from the blood or tissues of an animal previously made refractory to the disease in question?

Whether or not the above considerations stimulated the researches of Behring and Kitasato, their work affords a positive answer to this question, which promises to be of the greatest importance to humanity, and has led them to the most unexpected and interesting results from the scientific standpoint. The following is a summary of their paper, which is of the nature of a preliminary communication.[2] The method by which, in the first case, they produced immunity against tetanus and diphtheria is not described. Only so much of their results is communicated as is necessary to support the following propositions:—

"The immunity of rabbits and mice against tetanus depends on the power possessed by the fluid part of their blood of rendering harmless the poisonous substances produced by the tetanus bacilli."

This proposition involves a completely new theory of the nature of acquired immunity. Hitherto it has been thought that immunity must depend either on the voracious activity of phagocytes, or on the above-mentioned bacteria-killing power of the blood, or on an acquired tolerance against a poison; and, further, that by the method of residues, any one of these theories could be proved by showing the other two to be false.

Behring, however, was able to prove, by his work on diphtheria, that none of these theories would account for the natural immunity of rats or the artificially-produced immunity of guinea-pigs against this disease. After disproving many speculations on this subject, the above-given explanation was arrived at, but they only obtained a satisfactory proof of its correctness when they began to test it on the tetanus microbe.

Their experiments prove:—

(1) That the blood of rabbits which have been made immune against tetanus can destroy the tetanus poison.

[1] "Sur la Destruction des Microbes dans les Corps des Animaux Fébricitants" (*Annales de l'Institut Pasteur*, 1889, p. 229).

[2] A fuller account will shortly appear in the *Zeitschrift für Hygiene*.

(2) This character can be shown to be possessed by the blood both before and after it has left the vessels, and in the cell-free blood-serum obtained from it.

(3) This character is of so permanent a nature that it is still manifested by such serum after it has been injected into other animals. Consequently, by transfusion of such blood or serum, important therapeutic actions can be obtained.

(4) This power of destroying the tetanus poison is absent from the blood of such animals as are not immune against tetanus; and after such animals have been killed by the tetanus poison, it can be shown to be present in their blood and tissues.

In support of these assertions the following experimental results are brought forward.

A rabbit was made immune against tetanus by a method which will be described in a forthcoming paper by Kitasato in the *Zeitschrift für Hygiene*. To prove the completeness of its immunity, 10 c.c. of a virulent culture was injected into it. Half a cubic centimetre of the same culture was quite sufficient to produce tetanus in a normal rabbit. The treated rabbit, however, remained immune, and it not only showed immunity against the tetanus bacillus, but also against the poison produced by this microbe. For it remained unharmed by an injection of twenty times the quantity of tetanus poison which will kill with certainty a normal rabbit. Blood was taken from the carotid artery of this rabbit. Before clotting occurred 0·2 c.c. of this blood was injected into the body cavity of a mouse, and 0·5 c.c. into that of another. Twenty-four hours later, these mice, together with two control-mice, were inoculated with tetanus of such virulence that the latter showed the symptoms of tetanus after 20 hours, and were dead in 36 hours. Both of the treated mice, on the contrary, remained healthy.

The greater quantity of the blood of this rabbit was allowed to stand, and its serum collected.

Six mice each received 2 c.c. of this serum in the abdominal cavity, and all withstood a subsequent inoculation with tetanus. Control-mice died of tetanus within forty-eight hours.

With this serum the authors succeeded in curing animals that had been previously infected with tetanus. They have also been able to show that this serum possesses an intense power of destroying the tetanus-poison.

Of a ten-days old tetanus culture which had been sterilized by filtering, 0·00005 c.c. was enough to kill a mouse after four to six days, and 0·0001 would always produce the same result in less than two days.

Five c.c. of the serum of a tetanus-immune rabbit was mixed with 1 c.c. of this filtered-culture, and kept for twenty-four hours. Of this mixture four mice each received 0·2 c.c. (that is to say, 0·033 of the original culture, or more than 300 times the quantity which would otherwise be capable of killing a mouse). All these four mice remained in good health. Control-mice, on the contrary, which were at the same time inoculated with 0·0001 c.c. of the original culture, succumbed within thirty-six hours.

All the mice mentioned in each of the above series of experiments have been subjected to repeated injections with the tetanus bacilli, and have shown themselves to be permanently and completely immune.

This result is the more remarkable in that up till now, in spite of innumerable attempts, no one has ever succeeded in making any animal whatever immune against tetanus. A theory of the nature of acquired immunity which at once led to a method of treating the disease which is easy to understand, harmless to the animal, and certain in its effect, must surely possess some basis in fact.

Naturally every kind of control experiment with serum of normal rabbits has been carried out with uniformly negative results. Serum of cattle, horses, and sheep has also been found to have no action on the tetanus poison. The living blood and tissues of an animal which has not been made immune likewise show no power of destroying the tetanus poison, as appears from the following experiment, which has been many times repeated:—

Rabbits into which 0·5 c.c. of a germ-free tetanus culture is injected subcutaneously, succumb after showing typical tetanus symptoms; almost always a serous transudation is to be found in the thoracic cavity. Of this transudation, 0·3 c.c. is on the average enough to kill a mouse with typical tetanus symptoms. The same is true for the blood.

The authors close their paper by pointing out the possibility that their method of curing tetanus and diphtheria which they have used with such brilliant results on animals so highly susceptible to these diseases as mice and rabbits, may also be used for the far less susceptible hospital patient. They also note the possible influence of their work on the practice of blood-transfusion.

E. H. HANKIN.

THE STEAM-ENGINE.

The Steam-Engine considered as a Thermodynamic Engine. A Treatise on the Thermodynamic Efficiency of Steam-Engines, illustrated by Diagrams, Tables, and Examples from Practice. By James H. Cotterill, M.A., F.R.S., Professor of Applied Mechanics in the Royal Naval College. Second Edition, revised and enlarged. (London: E. and F. N. Spon, 1890.)

IN view of the general interest attaching to the recent improvements in steam-engines, and the consequent gain so remarkably illustrated by the increased speeds of ocean travel, an exposition from a purely scientific point of view of the causes of this progress comes at a very opportune time. The book before us is the second edition of a treatise which was originally published in the year 1877, and has since that time remained one of the chief authorities on the practical applications of thermodynamics.

It seems to be inherent in the industrial development of scientific laws that the course of invention should occasionally lead engineers away from the principles of pure science on which their work is based. The minds of inventors often become so much occupied with the intricate and perplexing problems presented by the mechanical realization of their ideas, that in the specifications of their successful arrangements, the naked principles which are the common and essential part of all practice are sunk far beneath the surface. It is of no ordinary importance that means should exist for co-ordinating the two independent growths of knowledge which are thus indicated. Probably no more weighty or influential calling exists for the industrial progress of the nation than that of the scientific engineers who are able to take into their view both sides of this field, and lead to the fertilization of each by the results of the other.

The progress of the science of heat forms one of the most interesting chapters in scientific history, and illustrates in a remarkable manner the way in which conceptions that at the time of their development appear of the most advanced and recondite character become the common elementary ideas of succeeding generations. The first great advance was no doubt the introduction of precision into the measurement of temperature by the invention of thermometers, and of the method of graduating them by first marking the fixed points on their scales. As Dr. Gamgee has recently pointed out, in a paper read before the Cambridge Philosophical Society, the fixed points by means of which Fahrenheit graduated his original alcohol thermometers were hardly physical at all; the higher one was the temperature of the armpit or mouth of a healthy man, which he had observed to be constant. It was after the Fahrenheit scale had been fixed by this temperature and that of the best available frigorific solution (snow and salt) that the construction for the first time of a mercury thermometer gave an opportunity of testing the boiling-point of water, which in this way was found to be constant, and under standard conditions to be at the point marked 212° on the scale. It was not until near our own time that the possession of methods of thermometry, combined with the researches of Black and others on the transfer and latency of heat, led, under the influence of Davy, Mayer, and Joule (and also Carnot), to the recognition of heat as a form of energy, and made possible an exact study of its transformations.

The final stage in the dynamical specification of the subject arrived when the very remarkable generalization of Carnot led, in Sir W. Thomson's hands, to a purely physical definition of a temperature scale, and the experiments of Joule and Thomson established the very close approximation of this scale to that of an ordinary thermometer, formed with one of the more permanent gases as expanding substance. This temperature scale enters into the general entropy equation employed by Clausius to express mathematically the results of Carnot's principle; so that in this form temperature has virtually a definition in analytical dynamics, and is an essential element in any dynamical theory which attempts to explain the phenomena of heat-transformation. These topics, treated in the simpler cases, now form a part of every course of physics.

The entropy formula expresses a relation between the initial and final states of the system, which is independent of the path of transition from the one to the other, provided this path be reversible. It is, therefore, to be classified with the fundamental dynamical investigations of Sir W. Rowan Hamilton, whose principal function is expressed in terms of the initial and final positions of the system, and the energy contained in it. It thus becomes a question of pure dynamics to inquire whether the entropy relation may be postulated as a general property of all motions, or whether its truth is constituted by the process of averaging the motions which is found essential to the development of the dynamical theory in the cases now in part unravelled, as, for instance, the kinetic theory of gases. In these discussions the chief difficulty is the dynamical realization and interpretation of temperature, as it is defined by its occurrence in the equation of entropy.

The nature of the difficulty is more fully realized when the fact is recognized that in a dynamical system, involving no dissipation of its energy to other systems, an exact reversal of the motions of all its molecular parts would cause it to retrace its original path, and would therefore reverse any degradation of energy in the thermodynamic sense that may have occurred in it. Thus, not merely is it possible for the thermodynamic order of Nature to be reversed by a continuous expenditure of intelligence without expenditure of energy, as in the case of the demons introduced by Clerk Maxwell into the exposition of the theory of gases; but a system may actually be put into such an initial state that it will go on reversing the laws of entropy of its own accord.

It is true that we are saved from the necessity of having to admit that such exceptions to the course of Nature may be actually existent; for all transfers of heat are inextricably mixed up with the phenomena of radiation, which involve transport of energy through the ether with a velocity the same as that of light, and which, therefore, require that if our system is to be absolutely conservative it must include the ether and extend beyond the farthest star. In any portion of a system thus constituted, however we may suppose degradation staved off and reversed by a complete reversal of its motions, the effect produced will be only temporary, and degradation will ultimately reassert itself.

Nevertheless, the problem remains for solution, how far the law of entropy, whose truth in a limited range is demonstrated by the fact that it is fundamental to the wide and firmly established science of thermodynamics, is to be considered as a principle of a purely dynamical character; and although the investigations of Clausius, von Helmholtz, Boltzmann, J. J. Thomson, and other mathematicians who have attacked the subject, have thrown much light on its affinities to known analytical laws of pure dynamics applied to systems in a steady state of motion, yet it is not too much to hope that there is a great deal more to be gained in this department of investigation.

In particular, a theoretical basis has yet to be supplied to the applications of the law of entropy to purely chemical actions. The results of its application to voltaic phenomena by Willard Gibbs and von Helmholtz have met with satisfactory experimental confirmation; while its extension to the theory of osmotic phenomena, dissociation, and the whole field of the chemical action of dilute solutions seems to be at any rate in qualitative accordance with facts, and even promises to fundamentally modify some of our notions of chemical action.

And there yet remains for answer the question which has long been put, whether there is any reason to suppose that the activities of living animal (or plant) tissue are limited by the principle in the same way as are those of dead matter.

The fundamental character of the views opened up by the development of Carnot's ideas on the efficiency of heat-engines adds a strong theoretical interest to the manner in which engineering practice has approximated to its ideal conclusions. Even though their full and complete application in the domain of heat-engines would not be questioned by anyone, yet it is clear that the investigations that have arisen out of their experimental testing and improvement have much value as illustrations and models for similar inquiries in more abstract departments of physics. Indeed, the subsidiary discussions in this treatise, relating to causes of waste of available energy, remind one, on a magnified scale, of the corresponding corrections necessary in thermo-chemical investigations, particularly those relating to entropy; and there seems to be a considerable field in which the one subject may profit from a study of the other. In this connection the admirable arrangement, discussion, and analysis contained in the chapters on the causes of loss of efficiency supply a store of information and general principles that would not otherwise be easily available for non-professional workers.

It is unfortunate that the science of thermodynamics suffers more than any other department of physics from the difficulty of exact comparison between theory and experiment. It may be a surprise to some people to learn that even the value of the mechanical equivalent of heat is hardly certain within 1 per cent.: it appears that recent experiments by Rowland give a result nearer to 782, that derived by Joule from the electrical method, than to 772, the value finally put forward by him from the results of several concordant series of direct experiments.

The treatise which has suggested these remarks contains a connected account of the different ways in which thermodynamic theory has been applied and realized, the subjects treated and co-ordinated ranging over air-engines, gas-engines, guns or powder-engines, reversed air-engines or refrigerators, and, in greatest detail of course, the different types of steam-engine. The reader will find in it a simplicity in the statement of physical results, and a freedom from the encumbrances of algebraic analysis in the discussion of general laws, which form one of the highest merits of a treatise on the principles of natural philosophy, and the most fitting preliminary to the examination by mathematical analysis of special problems. This is combined with a very interesting account, illustrated by actual examples derived from recent practice, of progress made in actual construction towards the ideal of perfection, which it would be beyond our province to refer to in detail. From the point of view of the student of physics, the book forms a most valuable supplement and corrective to the necessarily abstract discussions which form the substance of treatises on theoretical thermodynamics.

J. Larmor.

THE CLASSIFICATION OF ANIMALS.

Zoological Types and Classification. By W. E. Fothergill, M.A., B.Sc. (Edinburgh: James Thin, 1890.)

A Zoological Pocket-book or Synopsis of Animal Classification. By Emil Selenka and J. R. A. Davis, B.A. (London: C. Griffin and Co., 1890.)

THE author of the work bearing the first of the above titles has set himself a most formidable and ambitious task, viz. that of compressing into a small volume of 214 pages diagnoses of the organology of all classes and orders of living animals, together with brief descriptions of the structure of types of the latter, especially where represented by familiar creatures, and of adding thereto a list of most of the leading families, with short diagnoses of the same when occasion should demand. Such a book to be of real service should be as free of errors as possible; and, in view of the unprecedentedly rapid growth of zoological literature within the last two decades, the difficulties of the undertaking might be expected to be inversely proportionate to the smallness of the volume. The author sets out with the Protozoa, and advances, in ascending order, to the Primates. The plan of his work is a good one. Each fasciculus leads off (with few exceptions) with a recapitulation of the characters distinctive of the class with which it deals, and then follows a brief description of some central representative of each of its leading orders, and, in many instances, a tabular *résumé*. The book concludes with some notes upon the maturation and segmentation of the ovum. In dealing with the vertebrate vascular system, the author resorts to the construction of remarkable schemes which he believes may "aid the formation of a mental picture"; they appear to us to very efficiently confuse the mind, and we urgently recommend that their place be taken by diagrams which shall delineate the vessels themselves. The book, regarded as a piece of clerical work, is a good one, and the author has taken immense pains in compiling it. Its utility is, however, seriously marred by the constant recurrence of small errors, with which, as with insufficiently guarded assertions, it teems throughout. The mischief wrought by such, as distinguished from gross errors, which the student is tolerably certain to detect and rectify for himself, is, as the working of a subtle poison, slow but sure, and they cannot be too strenuously guarded against in an elementary treatise. Omissions, and occasional wrongly constructed sentences occur, and there is evident in places a want of uniformity of treatment—as, for example, the summary dismissal of the Echinodermata, and the non-recognition of the families of the important order Insectivora. Important groups, such as the Choano-flagellate Infusoria, and important characters, such as those of the central capsule of the Radiolaria and of the dentition of the Marmosets, pass unnoticed; while absolutely erroneous definitions are given of leading organs, such as the brain of the Craniata and the Arthropod eye—to say nothing of the relegation of the Arachnida and Prototracheata to the class Tracheata. There undoubtedly exists a demand for a book such as this which shall be up to date; and if the author will carefully revise his work this demand will have been met. The volume should serve as one of reference for the elementary student, and to this end it should be provided with an efficient index.

Under the second of the above titles there is in circulation a small volume of 238 pages, which is for the most part a translation of the third German edition of some notes printed for special use in Prof. Selenka's classes. They were originally intended "to serve for the reception of sketches and notes during lectures and practical work, and at the same time to facilitate a review of classification"; as reproduced in English, they are extensively interleaved, the blank paper being provided "to receive brief synopses from voluminous lecture notes" (*sic!*), "or, in some cases, definitions of families and smaller subdivisions." Numerous "additions and revisions" have been made by the translator, chief among them being a concise and well-compiled appendix of five to six pages, with a table, dealing with certain principles of distribution. This is, in some respects, the most satisfactory portion of the work, although in itself a literary essay such as might be produced by any intelligent student who had mastered the broad principles of his subject and could command scissors and paste. On turning to the body of the volume, we read, among other things, the following:—*Fierasfer* is parasitic upon Holothuria, *Exocoetus* is a Malacopteroid, *Diprotodon* was a springing Marsupial, *Glyptodon* was insectivorous. The *Rodentia* are diagnosed as deciduate Placentalia, with "$\frac{1}{1}$ chisel-shaped continuously growing incisors, and three to six back teeth with transverse folds of enamel"; while of the Apes it is said that the alisphenoids are "united with the parietals" (*Platyrrhini*) or are not united with them (*Catarrhini*), no mention being made of the malar. A novel version this of the discovery of Joseph and Forbes.

Upon the strength of his "additions and revisions" the translator practically claims a joint authorship. It will, we believe, be generally admitted that of all translations of the kind before us there are none which excel those into French. We have no wish to force comparisons, but we cannot refrain from contrasting the translator's work and assumed position towards Prof. Selenka's little volume, with, say, that of Carl Vogt towards Gegenbaur's "Grundzüge" or of Moquin-Tandon towards Claus's "Handbuch," the two finest translations of zoological treatises the world has yet seen. So efficiently has the translator of the last-named supplemented and extended the original that the French edition has become a new work; but, this notwithstanding, the translator is content to be regarded as such, and such alone. The endeavour, on the part of a translator or of an adapter, to pose as joint author of a work in connection with which he has performed a mere clerical labour is no new device; but it is one which cannot be too strongly denounced. It betokens, to say the least, an unfairness on the part of the junior which, it would not be difficult to show, has, in the past, amounted to an imposition upon the generosity of the senior, and to the infliction of a pang whose effects have been ineradicable. The translator of the work before us would have done well if, instead of having endeavoured to extend its scope, he had verified the accuracy of the statements which it contains. Why he should not have corrected errors such as those we have cited we are at a loss to understand; and he certainly should have avoided the addition of fresh defects. These seriously mar the pages of a book which, if revised anew, might be useful to the student.

G. B. H.

OUR BOOK SHELF.

The Hand-book of Folk-Lore. By G. L. Gomme. (London: Published for the Folk-Lore Society by D. Nutt, 1890.)

THE energetic Folk-Lore Society has just issued a handbook for the guidance of collectors and workers on folk-lore. The preparation of the work was intrusted to Mr. G. L. Gomme, the Director of the Society, who, it will be remembered, has recently published a very suggestive book entitled, "The Village Community," and is the author of numerous papers on folk-lore. Mr. Gomme has been assisted in the preparation of the book by several members of the Folk-Lore Society. At the outset Mr. Gomme defines "What folk-lore is," and after some judicious remarks he states: "From what has been advanced it may be conceded that the definition of the science of folk-lore, as the Society will in future study it, may be taken as follows, '*the comparison and identification of the survivals of archaic beliefs, customs, and traditions in modern ages*.'" The subject is divided into four sections, each of which is further subdivided. The principal divisions are as follows:—(1) Superstitious belief and practice: including superstitions connected with natural objects, goblindom, witchcraft, leechcraft, magic, beliefs relating to future life, &c. (2) Traditional customs: festival customs, ceremonial customs, games, local customs. (3) Traditional narratives: nursery tales, *Märchen*, fables, creation, deluge, &c., myths, ballads and songs, place legends. (4) Folk sayings: nursery rhymes, riddles, proverbs, nicknames, place rhymes. For each division there is a short account of the scope of that especial branch of folk-lore treated in a comparative manner, illustrations being drawn from the most varied sources; by this means the reader is enabled to gain a comprehensive view of the subject; then follow a number of questions for the guidance of collectors. The plan of the book is much the same as that of the well-known "Anthropological Notes and Queries," and it does for those interested in folk-lore what that estimable little work has done for the traveller—it instructs him what to do and how to do it. The author appeals not only to those who have the opportunity or the inclination to mix with "folk" (or "the less advanced classes in cultured nations"), but also to those who prefer to or possibly only can work in the library, and valuable hints are given to the latter, and two specimen tabulation forms are inserted. It is to be hoped that the publication of this carefully prepared hand-book will give definiteness of aim to the numerous people who take an interest in all "relics of an unrecorded past," and who would gladly collect information if they knew what was worth recording, how to arrange their facts, and where to send them to. Such information this book supplies, and the cost, being only half-a-crown, places it within reach of all.

Physikalische Krystallographie. By Dr. M. Liebisch, o.ö. Professor der Mineralogie an der Universität Göttingen. (Leipzig: Veit and Co., 1891.)

THIS book furnishes another example of the profound learning and patient industry of the German man of science. Dr. Liebisch, who is well known as one of the editors of the *Neues Jahrbuch für Mineralogie, Geologie, und Palaeontologie*, has recorded in the large octavo volume (of 614 pages) lying before us, a mass of investigations and abstruse mathematical proofs and deductions dealing with the physical properties of crystals. The subject is treated throughout in such a way as to bring out clearly the interesting relations existing between the physical phenomena and the geometrical symmetry of crystalline substances.

With this end in view, the author treats consecutively of homogeneous deformations, of the nature and orientation of isothermal planes, of thermo-electricity, of magnetic induction, of dielectrical polarization, and of pyro- and piëzo-electricity in crystals. The last part of the work deals with the elastic properties of crystals and with the remarkable applications of the theory of the elasticity of crystals on the changes in double refraction produced by pressure, concluding with an account of the investigations on the elastic deformation of dielectrical crystals in the electrical field and the electro-optic phenomena observable in piëzo-electrical crystals.

Not the least valuable part of the book is the series of beautiful plates at the end of the volume, illustrating pyro-electrical phenomena and the phenomena of interference in doubly refractive crystals, the figures being prepared from photographs made by the author. Worthy of note is the fact that Dr. Liebisch has succeeded in photographing interference phenomena displayed in the monochromatic light of the sodium flame.

Valuable as the book is in its completeness and accuracy of information, there are few, we venture to think, who possess sufficient mental digestive power to assimilate its learned contents. Even the professed crystallographer will no doubt find within its pages some rather heavy reading.

Anleitung zur Darstellung chemischer Präparate: ein Leitfaden für den praktischen Unterricht in der anorganischen Chemie. By Dr. Hugo Erdmann. (Frankfurt-a-M.: Verlag von H. Bechhold, 1891.)

A CONSIDERABLE evil of the present system of education is that the variety of subjects which a student has to pursue is increased, while the time at his disposal is, in many cases, less than it used to be. And as the examination is the only criterion of his success, both students and teachers are anxious to exclude all work that has not an immediate bearing upon this final test. In the study of chemistry, this state of affairs is leading to the exclusion of some of the most important branches of the subject, because practical examinations are invariably of an analytical character. When a student takes up organic chemistry, he is generally set to make a few preparations, and so accustom himself to work with larger quantities than a gram or two. But, although such practice is just as valuable and necessary to the student of inorganic chemistry, he very rarely enjoys a similar privilege. Dr. Erdmann has endeavoured to supply this deficiency so far as a guide-book is concerned, and though in many cases his directions are rather meagre, he has put together an excellent series of instructions which refer to compounds of all the commoner elements. The word inorganic is not too literally interpreted, for we find that oxalic acid crystallized and dry, ethyl bromide, hydrocyanic acid, urea, thiophen, Prussian blue, and some acetates are included. The volume of 71 pages concludes with a few useful notes concerning apparatus and processes, and gives directions for obtaining streams of several of the commoner gases without the aid of heat.

Analysis of a Simple Salt. By William Briggs, F.C.S., and R. W. Stewart, B.Sc. Lond. (London: B. W. Clive and Co.)

IN compiling a guide, it is a good thing to go to the best sources for information, but it is a graceful thing to acknowledge whence the information has been gathered. We do not for a moment suggest that every paragraph should be encumbered with a statement of the various authorities that support the facts expressed in it; but when the manner and order of treatment, and very often the actual verbiage, are taken from a volume that is still in current use, it is certainly due to all who have an interest in that volume, that the compilers shall acknowledge their indebtedness. If the compilers had followed more closely still the scheme for the "preliminary examination in the dry way" as detailed in Valentin's "Qualitative Analysis," we think they would have improved their method. And it may also be remarked that, to prepare always neutral solutions in searching for acids, leads to much waste of time, and especially so when the substance for analysis is a simple salt. A few specimen analyses are added at the end of the book. These, even when judged of by the tables given in the body of the work, are not always complete, and sometimes inaccurate. Overlooking the points already referred to, the volume is likely to prove a useful and trustworthy assistance to those for whom it is especially intended.

Magnetism and Electricity. By J. Spencer, B.Sc. (Lond.), F.C.S. (London: Percival and Co., 1890.)

THIS work will form a useful class-book when the elementary parts of these subjects are being taught. It is divided into three parts, the first dealing with magnetism, while the other two treat of electricity, subdivided into frictional and voltaic electricity. The idea throughout has been to arrange a series of easy experiments, which the teacher or student can with little difficulty perform. In each case good instructions are given, and, following these, are the conclusions which can be derived from them. The scope of the subjects under treatment is limited to the elementary stage of the Science and Art Department, and at the conclusion of each chapter are arranged exercises, among which are some that have been previously set by that Department. In the appendix will be found the syllabus, and questions that have formed the subject of examinations of the Department during the last two years, which should be found useful to both teachers and students.

The Elements of Euclid. Books I. and II. By Horace Deighton, M.A. New Edition, Revised. (Cambridge: Deighton, Bell, and Co., 1891.)

THE chief alterations made in this new edition consist in the introduction of symbols and abbreviations; and in order that beginners should not be confused with them at the commencement, the first fifteen propositions are proved without their use, with the exception of the symbols for therefore, because, and equal to. For several of the exercises the author has substituted others which he considers more suitable, and, in addition, some new ones have been inserted. Of the two books dealt with, Book I. is treated very fully, and each proposition is followed by a considerable number of easy examples, which, if worked out by the student, should give him a thorough knowledge of the propositions preceding them. A most useful collection of propositions is added, with which the reader is recommended to make himself familiar.

LETTERS TO THE EDITOR.

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

Mr. Wallace on Physiological Selection.

I WILL endeavour, as briefly as possible, to justify what I have said elsewhere touching "the peculiar position to which Mr. Wallace has eventually gravitated with reference to my views." For a fuller statement I must refer to the *Monist*, vol. i. pp. 1–20.

It is quite true, as he says, that "in his original paper, and in the summary of it published in NATURE, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection." And this is exactly what Mr. Wallace himself has done in his "alternative theory." Taking it for granted that these variations must always occur—as in my "original paper" I said they probably "most frequently" occur—in a whole race or strain, his theory seeks to explain, (1) the *causes* of such variations, and (2) their *effects* in furnishing an important condition to the origination of specific types, and this exactly in the manner that the theory of physiological selection had previously exhibited. Space forbids any long quotations, and therefore on the present occasion I will confine myself to transcribing two sentences from the paragraph in which he very correctly summarizes his theory, viz. the sentences which deal with the *effects* of these "variations in regard to fertility and sterility."

"The preceding argument, it will be seen, depends entirely upon the assumption that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species. . . . It is by no means necessary that all varieties should exhibit incipient infertility, but only some varieties; for we know that, of the innumerable varieties that occur, but few become developed into distinct species; and it may be that the absence of infertility, to obviate the effects of intercrossing, is one of the usual causes of their failure."

Here we have the whole essence of "physiological selection" in a nutshell. The first sentence conveys the "fundamental fact;" the second indicates its possibly important consequence in permitting the origination of species by preventing the effects of intercrossing. Why, then, does Mr. Wallace not perceive that this is the whole essence of physiological selection? As far as I can understand, the reason appears to be that he deems his variations in the direction of cross-infertility to differ from mine, in that while his may be associated with other conditions or causes of modification, mine, as he now repeats in NATURE, are supposed always to act "alone in an otherwise undifferentiated species." Now, not only did I expressly guard against this interpretation in my "original paper," but in all my answers to Mr. Wallace's criticisms which have since appeared I have over and over again corrected the mis-statement on his part; and I am the more surprised that he should again reproduce it as the sole basis of his present reply, inasmuch as some of the passages which he quotes for this purpose from the paper in question themselves furnish the needful correction in their own immediate context.[1] Moreover, not only have I thus from the first fully recognized the sundry other causes of specific change with which the physiological variations may be associated; but Mr. Gulick has gone into this side of our common theory much more fully, and elaborately calculated out the high ratio in which the differentiating agency of any of these other causes must be increased when assisted by—*i.e.* associated with—even a moderate degree of the selective fertility, and *vice versâ*. Therefore, it is simply impossible for Mr. Wallace to show that "our theory" differs from his in this respect. Yet it is the only respect in which his reply alleges any difference.

I do indeed myself believe that in many cases the physiological variation may arise alone, in an otherwise undifferentiated species; but from the first I have always maintained that it makes no essential difference to the theory in what proportional number of cases it has done so. And in a forthcoming treatise I shall be able very completely to dispose of Mr. Wallace's "two carefully considered cases," whereby he claims to have proved that the possibility of physiological selection ever working alone is "absolutely unfounded."[2] At present, however, the point is that, even if I am wrong in supposing that physiological selection can ever act alone, the principle of physiological selection, as I have stated it, is not thereby affected. And this principle is, as Mr. Wallace has re-stated it, "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species"—infertility, whose absence, "to obviate the effects of intercrossing, may be one of the

[1] For instance, to take only the first of his "few quotations," he reproduces by itself the following sentence:—"It becomes almost impossible to doubt that the primary specific distinction [meaning sterility] is, as a general rule, the primordial distinction." Now this, be it remembered, is quoted for the expressed purpose of "making it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, *was founded on the supposition of sterility alone, arising in an otherwise undifferentiated species*." Yet, if Mr. Wallace had but read the context, he would have seen that this statement is directly contradicted. For the very next sentence is as follows:—"I say as a general rule, because the next point which I wish to present is, that it constitutes no part of my argument to deny that in some—possibly in many—cases *the primary distinction may have been superinduced by the secondary distinctions*." By the "primary distinction" Mr. Wallace correctly understands me to mean cross-infertility between allied species, while by "secondary distinctions" it is explained in the same place that I mean specific characters of all other kinds. The passage then goes on, through a number of pages, to show "how natural selection, or any other cause [which is concerned in the differentiation of species], may have induced this particular kind of variation in the reproductive system by its operations on other parts of the organism," and the ultimate conclusion of this lengthy argument is: "Thus, we see, it really makes no essential difference to my theory whether it be supposed, in any given case, that the primary distinction was prior, or subsequent to the secondary distinctions." Comment appears needless.

[2] These cases consist in some exceedingly simple arithmetical computations, which, as I shall hereafter show, rest upon erroneous data. Mr. Fletcher Moulton, who has kindly gone into the matter in a really efficient manner from the mathematical point of view, reports, to use his own words, "an enormous difference from Mr. Wallace's results."

usual causes of their failure to become developed into distinct species."

This, I repeat, is the essence of physiological selection; and any "originality" which my views upon the subject present consists in recognizing the "fundamental fact" set forth in the first of the two sentences above quoted, together with its consequence as set forth in the second. Before Mr. Catchpool published the theory in these columns, no one—with the partial exception of Mr. Belt—had perceived this factor of organic evolution; and while, for about the sixth time, repudiating the grotesque "originality" which Mr. Wallace continues to ascribe, I may conclude by observing that—personalities apart—it is to me a matter of satisfaction that he has now himself begun to perceive the existence and the importance of the factor in question.[1] George J. Romanes.

Oxford, December 1.

The Tornado.

In Nature, vol. xlii. p. 612, there appears a notice of my book on "The Tornado," from "H. F. B." I must thank so high an authority for noticing the book; all I ask for is a full, free, and fair discussion of the facts presented. May I call attention to one or two points which may not be clearly understood?

(1) My object in writing the book was to bring together all the facts known regarding tornadoes, and to give a brief *résumé* of theories, as far as possible, showing the gradual development during the past fifty years.

(2) I have nowhere touched one of Prof. Ferrel's mathematical discussions of the problem. In some cases I have tried to show that there may be an interpretation of certain physical phenomena not exactly in accord with his own views.

(3) I have not denied a single thermodynamic principle. The quotations given by "H. F. B." are quite plain when the book is read as a whole. These do not refer to thermodynamic principles at all, but rather to the experiments made by Prof. Espy nearly fifty years ago, and whose results I have tried myself to check. I am sure that anyone who carefully peruses the book will be satisfied that there have been read into it statements or inferences which are not there.

(4) Above all, I have not advanced any visionary electric speculations regarding the generation of tornadoes. Rather I have tried to avoid that very thing. In the very quotation made by "H. F. B.," from p. 76, occur these words:—"It has been my purpose for many years to avoid, as much as possible, all speculations in considering air motions and the causes of atmospheric phenomena. This is especially pertinent when we consider electric action in the atmosphere. It is very difficult to believe that electricity has nothing to do with our thunderstorms."

It is a significant fact that "H. F. B." begins his quotation with this last clause, and from it tries to show that I have adopted an electric hypothesis. I cannot quite see why he did not begin where he should have done. I have not given my name to any hypothesis at all, but have advanced a few facts which I hope may ultimately help us to build up the true view of tornadic phenomena.

The familiar lecture experiment of forming a cloud in a receiver by a stroke of the air-pump is given by "H. F. B." as an illustration of dynamic cooling. It would be quite interesting if some one would compute the amount of work done by the air in this case, premising that the stroke of the pump is made quickly enough to form a vacuum into which the air from the receiver rushes. Tyndall says: "Mere rarefaction is not of itself sufficient to produce a lowering of the mean temperature of a mass of air." It is quite well known that the work done in this experiment is not that of driving out a piston, but is rather the very slight work needed to impart a motion to the molecules in the receiver—in other words, to drive out these particles from the receiver.

As to whether a dense and cold stratum of air in motion can overrun a warmer stratum, I have to say that this question has been negatively settled in this country. While such a condition might be possible in quiescent air, and has been observed in balloon voyages, yet in these cases there was no disturbance of the atmospheric equilibrium. In balloon voyages I have myself found a distribution of temperature in a vertical direction, which, according to theory, should have given rise to a violent tornado, but there was no marked disturbance. I have faith to believe that in the near future there is to be a marshalling of facts which shall establish true views of storm-generation; and even now there are many intelligent men who have grown restive under the present pure theories and mathematical analyses of atmospheric phenomena. To my mind, the Dr. Hann agitation has done a great deal to open the eyes of orthodox meteorologists, and even "H. F. B." seems to be in a little doubt as to the final outcome of his views in relation to orthodox meteorology. It seems to me all persons who are studying storm-generation and movements are realizing the absolute need of a solid ground-work of fact on which to base our views, and this is the great point that I have been contending for these many years.

Washington, D.C., November 18. H. A. Hazen.

Prof. Hazen can alone speak as to what views he intended to advocate; a reviewer can only take count of such as are expressed or implied in the work he reviews, and the present writer is unable to see in the above letter any evidence that the quotations given from Prof. Hazen's work were not fairly representative of his text, or that they fail to justify the comments upon them. That he is not alone in his inference that Prof. Hazen "appears to regard as inapplicable to the movements of the atmosphere, those laws of thermodynamics that are based on the results of Joule's labours," is shown in the following extract from Prof. J. Hann's paper in the September number of the *Meteorologische Zeitschrift*:—"Da Herr Hazen so ziemlich alle Grundlagen, auf welchen man die Meteorologie in neuerer Zeit mit Sicherheit weiter ausbauen zu können vermeint, leugnet oder in Zweifel zieht, darunter selbst allgemein anerkannte physikalische Gesetze, wie. z. B. die adiabatische Temperatur-Aenderungen in feuchter Luft, so scheint es zunächst allerdings überflüssig, sich mit ihm in eine Kontroverse einzulassen, da der Boden für eine Verständigung gänzlich fehlt." The passage referred to in this remark is one of those quoted in the present writer's review.

H. F. B.

Araucaria Cones.

I am drawn to add, with your permission, a few words to what has already been written on the subject of Araucaria cones, by noticing that all your correspondents speak of trees situated in the south of the British Isles, or, at least, not further north than Cambridge, whereas it may be of equal, if not greater, interest to the Duke of Argyll and others to know something of the behaviour of the Araucaria in the north of Scotland. It also seems to me unfortunate that many of the correspondents have omitted to mention the most interesting point concerning the fruiting of the Araucaria, viz. the monœcious or diœcious character of the trees they describe. Loudon, in his well-known book on the "Trees and Shrubs of Great Britain," pronounces the Araucaria to be diœcious. At that time knowledge could only be gained of it on its native hills. In the "Manual of Coniferæ," published by J. Veitch and Sons, is figured a branch from the monœcious tree at Bicton with both pollen and ovule-bearing catkins. It would be interesting to learn if many of the

[1] It is, perhaps, desirable to add, as already stated elsewhere, that I entertain no doubt at all touching the unconscious or unintentional nature of the "adoption." Nevertheless, I may further add, the adoption itself is so manifest, that several eminent men of science wrote to me on the subject when first his work on "Darwinism" appeared. Among the mildest of their comments are:—"Mr. Wallace has treated you very badly. After having set up a caricature of your theory, he adopts the theory itself, pure and simple." But of more importance is Mr. Gulick's opinion, seeing that he was the first to conceive, though the last to publish, the theory of physiological selection. As soon as he had read "Darwinism," he wrote me from Japan a long letter, the substance of which may be gathered from the first two sentences, as follows.—"Mr. Wallace has most certainly adopted the fundamental principles of our theory. He takes our principles, which in the previous chapter he has combated: but he makes such disjointed use of them that I am not willing to recognize his statement as an intelligible exposition of our theory." More recently he sent to the *American Journal of Science* a paper, which he summarizes thus:—"Mr. Wallace's criticism of the theory of physiological selection is unsatisfactory: (1) because he accepts the fundamental principles of that theory on pp. 173–79, in that he maintains that without the cross-infertility the incipient species there considered would be swamped; (2) because he assumes that physiological selection pertains simply to the infertility of first crosses, and has nothing to do with the infertility of mongrels or hybrids; (3) because he assumes that infertility between first crosses is of rare occurrence between species of the same genus, ignoring the fact that, in many species of plants, the pollen of the species is prepotent on the stigma of the same species when it has to compete with the pollen of other species of the same genus; (4) because he not only ignores Mr. Romanes' statement that cross-infertility often affects 'a whole race or strain,' but gratuitously assumes that the theory of physiological selection excludes this 'racial incompatibility' (which Mr. Romanes maintains is the 'more probable form'), and bases his computation on the assumption that the cross-infertility cannot be associated with any other form of segregation.

trees now under cultivation are monœcious, the tree perhaps adapting itself to the enforced solitude of its new abode in gardens. However, it appears at present to be both monœcious and diœcious, like several other Coniferæ.

In the grounds at Beaufort Castle, near Inverness, are several specimens of this Araucaria, producing only male or pollen-bearing catkins. One, a particularly fine tree, has borne such pendent cones for several years past. Last week I had the opportunity of inspecting a rather famous Araucaria at Conan, the seat of Sir Kenneth Mackenzie. This is said to be one of the first three specimens introduced into Scotland; the other two at Edinburgh were, I believe, killed by the severe winter of 1860. The tree at Conan has lost all its lower branches, and has grown but little of late years; it, however, yearly produces the erect ovule-bearing cones, and about twelve years ago, these contained fertile seed. Specimens grown from the seed of that year are still thriving at Gairloch, on the north-west coast of Scotland. No fertile seeds have been produced since; and as there was no other specimen certainly within several miles that could have produced the necessary pollen, we must conclude in the absence of direct evidence that this is a diœcious tree, and produced the more insignificant male catkins on the one occasion only. I therefore need hardly point out that unless the Araucaria at Inveraray is also provided with male flowers, or some other specimen similarly provided grows in the neighbourhood, the Duke of Argyll may assuredly expect his lawn to be strewn next year with only empty seed-vessels.

In answer to Mr. Gardiner, I may remark that I could detect no difference in habit or foliage between the diœcious male tree at Beaufort and the monœcious one at Conan; the latter, however, is so much damaged as to render comparison difficult.

ADRIAN WELD-BLUNDELL.

The Abbey, Fort Augustus, November 30.

YOUR correspondent, Mr. A. D. Webster, in your issue of November 20 (p. 57), states that the male catkins of the above tree are extremely rare as compared with the fruiting cones. If this is the case, though my own observations would have led me to the contrary opinion—the following instances may be of interest. I have observed the male catkins on a tree in the cemetery of this town for two or three successive years, in considerable quantities. In the grounds at The Elms, Houghton, Hunts., there is a tree which for several years past has borne large quantities of the male "amenta," giving to it, as your correspondent describes, a very striking appearance. Another tree in the same grounds has this year produced a single specimen of the same nature, while a third, of the opposite sex, is also developing a fruiting cone, which will doubtless, in the near proximity of the pollen-bearing ones, perfect its seeds.

Among specimens of the male catkins that I possess from the latter place is one which is "double," the floral axis being bifid. It has occurred to me that this may be some slight indication as to the much-vexed question of the morphology of the "amenta," whether each consists of a series of *monandrous* flowers or constitutes a single *polyandrous* one; the above monstrosity seeming to point towards the former, as the bifurcation of the axis of an *inflorescence* is a common phenomenon, that of a single *flower* being, on the contrary, much rarer.

I am not sure whether I should be right in generalizing from the comparatively few fruiting examples I have seen, but in the cases which have come under my observation the female trees have been more distantly branched than the male, where the ramification is considerably closer and more luxuriant.

Northampton, December 3. H. N. DIXON.

Dry-rot Fungus.

THE "beautiful growth of fungus covering the wall and floor (in a wine-cellar) to a depth of 4 inches, suggesting cotton-wool in form and colour," referred to by "M. H. M.," is the destructive dry-rot (*Merulius lacrymans*), and I would advise your correspondent to make war upon it without delay. The cotton-wool form is an early stage of the fungus. If neglected, it will in a few months develop a leathery sheet, sending out tough leathery cords a quarter of an inch thick, with spore-bearing folds of a rusty colour. These spores will scatter themselves all over the cellar, and will be difficult to eradicate. The mycelium of the fungus buries itself in any kind of wood, especially deal, runs rapidly down the longitudinal fibres, and, as it goes, destroys the "nature" of the wood, so that it snaps and crumbles under the slightest pressure. I have had to deal with this pest in a range of cellars with a timber roof, and have found the best remedy to be repeated applications of corrosive sublimate dissolved in methylated spirit freely painted on the timber, walls, or floor, wherever the "cotton-wool" makes its appearance. I had to cut away 8 feet in length of a 10-inch Memel beam which was permeated by the mycelium, and rotten to the core. Between the end of this beam and the back of the recess in the brick wall in which it rested was a vacant space filled with the mature fungus full of spores. This was two years ago. I have been fighting the fungus ever since with the corrosive sublimate, and have nearly exterminated it. The first appearance of the cotton-wool should be attacked without delay. F. T. MOTT.

Birstal Hill, Leicester.

The Effect of Fog on Plants.

As my name appears somewhat prominently in your note on the important inquiry into the effect of fog on plants, may I explain that the experimental investigation of the subject from a botanical point of view is entirely in the able hands of my friend, Dr. Oliver?

I am prepared, as stated in the Scientific Committee's circular, to examine any specimens of plants affected by fog which may be sent to me, but my share in the work does not go beyond this.

The inquiry is of very great interest, both to horticulturalists and botanists, and I am glad that it has been noticed in the columns of NATURE. D. H. SCOTT.

Royal College of Science, South Kensington,
London, S.W., December 6.

Great Waterfalls.

WOULD you allow me to supplement my inquiries published in last week's NATURE (p. 105) by asking for a description of the Pambam-arivy Falls in India, of which I have only the following brief note:—"In the Travancore Hills between Tinevelly and Travancore is situate the magnificent Pambam-arivy, or Snake Fall. It is a double fall, descending in the first plunge from the cliff edge 1200 feet, and it can be seen from a distance of forty miles." ARTHUR G. GUILLEMARD.

Eltham, Kent, December 9.

A Band of Light.

THE account of the so-called comet that was seen by Mr. Eddie at Grahamstown (see NATURE, November 27, p. 89) reminds me of the phenomenon seen some years ago in this country during an auroral display. A band of light, in shape somewhat resembling a comet, was seen to move across the sky, rising in the north-east and disappearing in the north-west; it moved, however, much faster than the comet-like body lately observed, being in sight, as far as I remember, only one or two minutes. C. C.

Trinity College, Cambridge.

Some Habits of the Spider.

THE following record of the habit of certain spiders, alluded to by "A. S. E.," is the only one known to me. "He saw great spiders with crowns and crosses marked on their backs, who sat in the middle of their webs, and when they saw you coming, shook them so fast that they became invisible" (Kingsley, "Water Babies," p. 40.) W. E. H.

BOTANICAL ENTERPRISE IN THE WEST INDIES.

DURING the last twelve years considerable effort has been made to enlarge the sphere of action of the botanical organizations in the West Indies. At the beginning of the period there were only two botanical establishments in this part of the world, one at Jamaica and the other at Trinidad. Since that time an important botanical garden has been successfully established at British Guiana, and lately a scheme of botanical stations has been put forth to suit the circumstances of the smaller islands, described in these columns (vol. xxxv. pp. 248-250). This scheme of botanical stations for the West Indies has been very carefully and assiduously fostered at Kew, and it appears now as if it were likely to be fully carried out. It was felt that the smaller islands under present circumstances could not support any considerable organization of their own, but, on the other hand, if they joined together and affiliated their stations to one or other of the larger gardens there were good grounds for believing that satisfactory results would be attained.

Stations have been already established at Grenada, St. Vincent, St Lucia, Barbados, and Antigua, while others are in course of being established at Dominica, and St. Kitts, and Nevis. The curators of these stations are carefully trained men (mostly from Kew), whose chief qualification is a thorough knowledge of horticultural methods as applied to tropical plants. They devote themselves to the maintenance of the stations as centres for the growth and distribution of economic plants, and they carry on experiments with the view of improving old or introducing new industries. It may readily be gathered that the objects of these stations are thus of a very simple and unassuming character. They necessarily work within narrow limits. They cannot give much attention to plants of a purely decorative character, or, again, to the maintenance of large areas under cultivation as pleasure grounds. The main object in view is to meet the special circumstances of the West Indies at the present time, and do all that is possible to encourage a diversified system of cultural industries so that they may not suffer so much as hitherto from fluctuations of prices in the chief staples.

It is believed that the Botanical station scheme will eventually meet the wants of each island in a simple and economical manner. It will become the basis of a federation for purely economic purposes likely to be generally beneficial both to the European planter and the Negro small proprietor. The stations will, it is hoped, have the occasional supervision of the heads of the Botanical Departments at Jamaica and Trinidad, and they will draw from thence such supplies of seeds and plants as may be required from time to time for their special wants.

In the organization of the scheme successive Secretaries of State for the Colonies have taken a warm interest, while the elaboration of the details has necessarily to a large extent fallen upon Kew. As the scheme took root, the discussion of these details involved a rather heavy burden of correspondence. It was eventually felt to be advisable both by the West Indian Governments immediately concerned and by the Colonial Office, that one of the Kew Staff should proceed to the West Indies in order to advise on the spot as to the various questions incidental to the successful start of the various stations. The mission was accordingly intrusted to the Assistant-Director, Mr. Morris, who has had a considerable experience of planting industries both in the Old World and in the New. Mr. Morris left by the mail of November 12, and will be absent about three months.

NOTES.

MR. W. F. R. WELDON, F.R.S., has been appointed by the Council of University College, London, to the Jodrell Professorship of Comparative Anatomy and Zoology, which was held for sixteen years by Prof. Ray Lankester. Mr. Weldon is a Fellow of St. John's College, Cambridge, and is a lecturer on Invertebrate Morphology to the University of Cambridge.

THE Council of the Royal Geographical Society has agreed to grant £200 to Mr. Theodore Bent to aid him in exploring the now famous ruins in Mashonaland.

ACCORDING to the *Colonies and India*, the important post of Superintendent-General of Education in the Cape Colony will fall vacant at the end of the present year, owing to the retirement, after 30 years' service, of Sir Langham Dale, K.C.M.G. This gentleman was in 1848 selected by Sir John Herschel to be Professor of Classics in the South African College, Cape Town, and in 1859 was appointed to the office which he now holds. He also became Vice-Chancellor of the University of the Cape of Good Hope in 1873. The value of the appointment is £1000 per annum.

THE Whitworth Trustees have added to their gifts to Manchester 12½ acres of land, known as the Stanley Grove Estate, purchased by them for £27,000, for a hospital under the direction of the Owens College. The site is alongside the Whitworth Park.

ACCORDING to the will of the late Sir Edwin Chadwick, a trust fund is to be applied as the trustees shall direct for the advancement of sanitary science. By a codicil to his will, the testator suggests that the trustees may offer a cup of the value of £20 to be given annually, for a term of years, to sanitary authorities at home or abroad showing the greatest reduction in the death-rate of the population in their district. He suggests also the offer of a silver medal to the school teacher showing the best mental results of the half-time principle, and a gold medal for district school managers obtaining the best results in subjects named.

LAST week we referred, in a leading article, to a Conference convened by the National Association for the Promotion of Technical and Secondary Education for the purpose of considering the best means of utilizing the new fund placed at the disposal of County Councils under the Local Taxation Act of this year for the promotion of education. The Conference met on Friday, December 5, and was largely attended. Lord Hartington, who occupied the chair, opened the proceedings with a short speech, in which he urged that the best way of securing the fund will be to see that it is used for the purpose for which it was originally granted, by stimulating existing institutions in the work they are now doing, by adding a scientific and practical side to schools, and providing new schools where such do not now exist. There are, he pointed out, many ways in which the grant can be applied, and it is clear that one cut-and-dried system cannot be applied throughout the country. The system in urban districts must be different from that established in agricultural districts. The secretaries presented a report on the working of the Technical Instruction Act and the Local Taxation Act; and afterwards there was a most careful discussion of all aspects of the question which the Conference had met to consider. No formal motions were adopted, as several members of the Conference felt that they could not commit their County Councils to definite resolutions. In the course of the discussion Sir W. Hart Dyke assured the Association of his willingness in every possible way to co-operate with them. Mr. Mundella said the best result of the meeting would be if it insured the continuance of the grant in the channel in which they wished it to go. If this money was not promptly utilized for education, it might revert to the Treasury. No doubt embarrassment might be felt in some localities, but he would recommend in such case that the money should be placed in a suspense account, which the Act enabled them to do. The total absence of provision for intermediate education had been a grave defect in our system. Wales had appropriated the whole fund, and stood higher in respect of intermediate education than any other part of the country.

THE Second International Ornithological Congress will be held at Buda-Pesth in May 1891. The Hungarian Committee invite all specialists and members of Ornithological Societies to attend the meeting. There will be excursions to various parts of Hungary of ornithological interest. An official programme will shortly be published.

AT a meeting of the State Commissioners on Niagara Falls, held at New York on December 8, a report was presented from the State Engineer upon the survey which has just been made. According to a telegram sent through Dalziel's Agency, this report gives particulars of the recession of the Falls since 1742, when the first survey was made. It shows that the total mean recession of the Horse-Shoe Falls since 1742 has been 104 feet 6 inches. The maximum recession at one point is 270 feet. The mean recession of the American Falls is 30 feet 6 inches. The length of the crest has increased from 2260 to 3010 feet by the washing away of the embankment. The total area of recession of the American Falls is 32,900 square feet, and that of the Horse-Shoe Falls 275,400 feet.

DR. BERGHAUS, the well-known geographer, died at Gotha on December 3.

THE death is announced at Warsaw of Herr Anton Waga, one of the most eminent Polish naturalists, at the age of 91.

MR. R. D. OLDHAM, of the Geological Survey of India, has been attached to the military expedition into the Zhob country. His principal duty will be to inquire into the reported oil-fields in the Shirani country. The last specimen examined by an expert has been declared of most excellent quality.

A DESPATCH from Mexico states that an earthquake, lasting some minutes, occurred there on the evening of December 2. The shock was the severest felt for years, and the inhabitants rushed into the streets in terror.

EXTENSIVE subsidences of cliff have taken place at Walton-on-Naze, Essex. In one spot a surface area of 100 square feet slid bodily down to the beach, carrying with it part of a road leading to Frinton. Opposite the Marine Hotel, Walton, where the cliff is lower, a chasm large enough to berth a Thames steamboat has opened in the sea-front. The *Times* says that landslips have resulted from the loosening of the soil of the cliff through the action of springs, rather than from the direct attack of the sea.

THE Decimal Association has issued a pamphlet containing a popular explanation of decimal coinage, weights, and measures, by Sir Guilford Molesworth and Mr. J. Emerson Dawson. The information is condensed as much as possible under different headings, all examples and evidence in support of the statements put forward being relegated to appendices.

AT the meeting of the American National Academy of Sciences at Boston on November 11, 12, and 13, the following papers were presented:—On the primary cleavage products formed in the digestion of the albuminoid, gelatine, by R. H. Chittenden; on the classification and distribution of stellar spectra, by Edward C. Pickering; on the relation of atmospheric electricity, magnetic storms and weather elements, to a case of traumatic neuralgia, by R. Catlin; on the growth of children studied by Galton's method of percentile grades, by Henry P. Bowditch; on electrical oscillations in air, together with spectroscopic study of the motions of molecules in electrical discharges, by John Trowbridge; some considerations regarding Helmholtz's theory of dissonance, by Charles R. Cross; a critical study of a combined metre and yard upon a surface of gold, the metre having subdivisions to two millimetres, and the yard to tenths of inches, by W. A. Rogers; on evaporation as a disturbing element in the determination of temperatures, by W. A. Rogers; on the use of the phonograph in the study of the languages of the American Indians, by J. Walter Fewkes; on the probable loss in the enumeration of the coloured people of the United States, at the census of 1870, by Francis A. Walker; on the capture of periodic comets by Jupiter, by H. A. Newton; on the proteids of the oat-kernel, by Thomas B. Osborne; on the present aspect of the problems concerning Lexell's comet, by S. C. Chandler; the Great Falls coal-field, Montana, its geological age and relations, by J. S Newberry; notes on the separation of the oxides in cerite, samarskite, and gadolinite, by Wolcott Gibbs; on the relationships of the Cyclopteroidea, by Theo. Gill; on the origin of electro-magnetic waves, by Amos E. Dolbear.

A PAPER by Mr. W. B. Mason in the Transactions of the Seismological Society of Japan deserves the attention of all who take special interest in seismology. It contains a list of earthquakes recorded at telegraph stations in central and northern Japan from August 11, 1888, to December 31, 1889. Mr. Mason, while allowing for various sources of uncertainty in the observations, thinks that some results may be deduced from what are still meagre statistics. Thus of the 151 earthquakes recorded in Tokio only 89 were felt at the other telegraph stations. Some of those which were felt at all the stations seem to have been felt at almost exactly the same instant. In other words, there was no indication of a progression of the earthquake from point to point.

THE Brooklyn Institute is making most praiseworthy efforts to raise the standard of geographical teaching in the United States. Through its Department of Geography it will open, about the new year, an exhibition of specimens of the best geographical text-books, maps, atlases, globes, reliefs, models, telluria, and other apparatus used in the various countries of Europe and America in their courses of gergraphical instruction. The collection will be exhibited first in the building of the Brooklyn Institute, then in New York, Philadelphia, Boston, Baltimore, Washington, Chicago, St. Louis, and other great centres of population. The entire collection, with the exception of specimens which have been lent, will afterwards be arranged as a permanent exhibition in the building of the Brooklyn Institute. *Science*, from which we learn these details, says that, in connection with the exhibition, the Brooklyn Institute is collecting material for a comprehensive report which it will publish regarding the position and methods of geographical instruction in America and Europe.

THE Chief Signal Officer of the United States has just issued his Report for the year ending June 30, 1890. It shows that the meteorological department is yearly increasing in extent and importance; the duties include the issue of forecasts and storm warnings, the gauging of rivers for navigation and flood-warnings, the reporting of temperature and rainfall conditions for the cotton interests, the display of frost-warnings in the interest of agriculture, and the notification of advancing cold waves for the benefit of the general public. The average percentage of successful weather forecasts amounted to 82·6. Long time forecasts are also issued at the discretion of the forecast official, with successful percentages of 81·6 for 48 hours, and 80·5 for 72 hours. The demands for daily weather charts have increased to a remarkable extent; the subscription for them is two cents. a copy. The co-operation with the Meteorological Office in Paris is continued; each night a cablegram is sent to the latter office summarizing the synchronous meteorological observations, gales, derelicts and dangerous ice of the western Atlantic for the previous five days, together with the current weather conditions of the United States. Part of this information is regularly published in the Paris *Bulletin International*. The Report states that a card index of the stations in the United States at which meteorological observations have ever been taken is being prepared; when finished, it will afford a comprehensive history of the climatic observations in that country. Various important scientific researches are being carried on, among them the accurate determination of the velocity of the wind, and more particularly its actual pressure during violent gusts, by Prof. Marvin; the relation of the dewpoint to the subsequent movement of the storm-centre, by Captain Allen; and the determination of the average destruction caused by tornadoes, by Prof. Hazen. The average number of persons killed by them is 102 yearly—which is considerably less than the number killed by lightning. These reports will be eventually printed as appendices to the General Report.

AT the meeting of the Linnean Society of New South Wales on October 29, Mr. J. J. Fletcher read a paper presenting contributions to a more exact knowledge of the geographical distribution of Australian Batrachia. While the broad facts relating to the geographical distribution of Australian Batrachia are fairly well known, much yet remains to be learnt respecting details, especially in regard to inland forms, since the species were originally described chiefly as they came to hand and without reference to the general batrachian fauna of the particular localities from which the types came, and with very few exceptions from coastal habitats. In Mr. Fletcher's paper, the first of what promises to be a useful series, three fairly complete collections are recorded from Dunoon, Richmond River (12 species); Guntawang, near Mudgee (13 species); and Dandaloo, Bogan River (10 species); and comparisons are instituted between the Batrachia of these localities and those of Port Jackson, the Blue Mountains, and Illawarra.

IN the report on the Chillingham cattle, read at the British Association in 1887, the following statement is made: "The cattle live on good terms with the red-deer, but they will not tolerate fallow-deer or sheep in the park." With regard to this statement, Mr. C. Oldham, of Ashton-on-Mersey, says in the December number of the *Zoologist* that on September 13 last, when at Chillingham, he watched a small party of fallow-deer for some time, as they fed on the hill-side with five of the white cattle; and the keeper, Michie, assured him that both red- and fallow-deer live in perfect harmony with them, and, if in any way alarmed or disturbed, generally seek safety in their company. The editor of the *Zoologist* adds that in Mr. Assheton Smith's park at Vaynol, near Bangor, where he has just spent some weeks, white cattle and red- and fallow-deer roam together, and no such hostility as that above referred to has ever been noticed.

THE collections of the Australian Museum, Sydney, are being steadily increased. In the report of the trustees for 1889, just received, it is stated that the principal purchases during the year were a collection of shells comprising some 15,000 species, and a collection of minerals, including specimens of gold from various parts of the world other than Australia. Several collecting expeditions were sent out with satisfactory results. The principal of these were:—(1) To Mount Kosciusko (Mr. R. Helms, collector), where an extensive collection of insects and other specimens from high altitudes, including many not previously represented in the Museum, was obtained. (2) To the Bellenden-Ker Ranges, north-eastern Queensland (Messrs. E. J. Cairn and R. Grant, collectors). This expedition obtained many rare mammals and birds of interest, including the remarkable tree kangaroo (*Dendrolagus lumholtzi*), and a new species of Petaurista, as well as a recently described new bower bird. (3) To Mount Sassafras, Shoalhaven District (Mr. R. Etheridge, palæontologist, and Mr. J. A. Thorpe, taxidermist, acting as collectors). This journey was undertaken with the view of obtaining some aboriginal remains, said to be concealed in a rocky recess at Mount Sassafras. The expedition was successful in obtaining the remains sought for, and in other ways. (4) To Blackhead, Illawarra District (Mr. A. J. North acting as collector). This was a short trip, to obtain certain fossils which were required to fill gaps in the collections. Abridgments of the reports of these various expeditions will be found in the first number of "The Records of the Australian Museum."

AT the request of Prof. Baird, Mr. William Palmer accompanied the United States Fish Commission schooner *Grampus* on her summer cruise in 1887 for the purpose of observing and collecting the fish-eating birds, together with their eggs and young. Mr. Palmer contributes to the Proceedings of the United States National Museum (vol. xiii., pp. 249–265) a record of his observations, and this has now been printed separately. He treats each species briefly. As all on board were interested in the matter, and frequently called his attention to birds seen by them, he believes his list contains all the species that came within a reasonable distance of the vessel. It might naturally be supposed that on a cruise of this character sea-birds would be found to be generally numerous, but such was not the case. With few exceptions, and these mainly on breeding islands, birds were very scarce, most of the many species having completed their migrations, and being in the far north or inland. As to the relative abundance of the species, Mr. Palmer places the most prominent in the following order: puffins, shearwaters, black hagdons, murres, and gannets. Good skins were made of the greater number of the species, and in many cases, also, eggs, embryos, and young in various degrees of plumage, were obtained. The localities visited were as follows: the Magdalen Islands and Bird Rocks, in the Gulf of St. Lawrence; St. John's; Funk Island; Seldom Come By; Cape Freels Penguin Islands; Toulinguet and Canada Bay, in Newfoundland; Black Bay and Mingan Islands; Southern Labrador, and Percé, Canada. The time covered was from July 8 to August 31.

MR. J. M. COODE records, in the new number of the Journal of the Bombay Natural History Society, the following instance of an exceptional method of hunting which the panther is occasionally forced to adopt. Mr. Coode was lately asked by the Patel of a village in the Amraoti district to accompany him one evening to a forest nursery of young bamboo shoots, to assist in killing a large boar which nightly visited the place and did immense damage. They waited for some time, when, just as is was getting dark, they heard the short guttural sound of a panther and heavy footfall of some running animal. The noises came nearer and nearer, until a nilghai and a panther could be distinctly seen against the sky-line, the former being chased by the latter. The nilghai kept moaning, and was evidently in an abject state of fear. The two ran round in a circle of about 160 yards diameter, within 30 yards of where the observers were standing, and passed them twice, both animals making their respective noises. They then disappeared, but Mr. Coode has reason to believe the nilghai got away.

THE following are the principal contents of the Journal of the Bombay Natural History Society (No. 3, vol. v.):—On new and little-known butterflies from the Indian region, with descriptions of three new genera of Hesperidæ, by Lionel de Nicéville (with plates); Bombay grasses (Part 2), by Dr. J. C. Lisboa; on new and little-known Hymenoptera from India, Burma, and Ceylon, by Major C. T. Bingham, Forest Departments, Burma (with plates); mules, by J. H. Steel; notes on the larvæ and pupæ of some of the butterflies of the Bombay Presidency, by J. Davidson and E. H. Aitken (with plates); the butterflies of the Central Provinces (Part 3), by J. A. Betham; notes on the economic botany of the Cucurbitaceæ of Western India, by Dr. W. Dymock; list of Chin-Lushai butterflies, by Lionel de Nicéville.

MESSRS. R. FRIEDLÄNDER AND SON, Berlin, have just issued a catalogue of books relating to Invertebrata. It includes a very large number of important works on the special branch of zoology to which it relates.

IN our fifth note, on November 27, p. 87, *for* Friana *read* Triana.

AN important paper is contributed by MM. Barbier and Roux to the current number of the *Bulletin de la Société Chimique*, in which are described the final results of an elaborate experimental investigation concerning the relations between the optical dispersive power of the alcohols, ethers, and fatty acids, and their molecular weight and constitution. The relations brought to light, as might perhaps have been expected, are of the most regularly systematic character, and capable of expression in a few simple generalizations. The experimental portion of the work consisted in determinations of the refractive indices of the liquid in question at known temperatures, and for two widely separated lines of the spectrum of wave-lengths λ_a and λ_b. A measure of the dispersive power is then afforded by the amount of the difference between the values of the two indices thus obtained. It has also been shown that if the well-known expression of Cauchy, $n = A + \frac{B}{\lambda^2} + \ldots$, for the refractive index n be adopted, and for all practical purposes the expression appears to be sufficiently approximate, the constant B is a true measure of the dispersion. This value B is termed by MM. Barbier and Roux the dispersive power. In order, however, to eliminate differences due to the unavoidable differences of temperature at which the various determinations are made, the conventional expression $\frac{B}{d}$, where d represents the density of the liquid at the temperature of observation, is employed in the comparisons, and is called the specific dispersive power. The value of B is readily calculated from the two determinations of refractive index by means of the formula—

$$B = \frac{n_a - n_b}{\frac{1}{\lambda_a^2} - \frac{1}{\lambda_b^2}}$$

The general conclusion arrived at from the experiments upon the alcohols is that "the dispersive powers are continuous functions of the molecular weight, and they increase with increasing molecular weight in the fatty series, and decrease with ascending molecular weight in the aromatic series." Similarly as the result of the investigation of the ethers, it was found that "the value of the dispersive power augments with the molecular condensation." It was further remarked that "the values of the dispersive powers of isomeric ethers are practically identical." In illustration of this latter point, which appears to show that the dispersion practically depends alone upon the molecular weight of the substance, it may be noted that ordinary ether, ethyl oxide, and the isomeric mixed ether, methyl propyl oxide, both possess the same dispersive power, as do also propyl ether, ethyl isobutyl ether, and methyl isoamyl ether. It was also curiously observed that in the mixed ethers of the series $\left.\begin{matrix}(C_nH_{2n-1})\\(C_nH_{2}+1)\end{matrix}\right\rangle O$, in which the unsaturated allyl radicals occur, the specific dispersive power is approximately the same for all the members of the series, while for the mixed ethers of the series $\left.\begin{matrix}(C_nH_{2n-7})\\(C_nH_{2n+1})\end{matrix}\right\rangle O$, in which the still further unsaturated aromatic radicles are introduced, the value actually diminishes as the molecular weight increases. The very important conclusion was also arrived at that the specific molecular dispersive power $\frac{B}{d}M$, where M represents molecular weight, of a compound is equal to the algebraic sum of the specific molecular dispersive powers of its constituents. Precisely analogous results were obtained from the experiments upon the liquid fatty acids, confirming the main generalization that dispersion is a true function of the molecular weight.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus* ♂) from North Africa, presented by Madame Ruoy; a White-fronted Capuchin (*Cebus albifrons* ♂) from South America, presented by Mrs. Akers-Douglas; a Pinche Monkey (*Midis œdipus* ♀) from New Granada, presented by Mr. J. Barry O'Callighan; a Himalayan Bear (*Ursus tibetanus* ♀) from Beloochistan, presented by Mr. B. T. Ffinch, C.M.Z.S.; a Common Raccoon (*Procyon lotor*) from North America, presented by Mr. C. E. Brewerton; a Greater White-crested Cockatoo (*Cacatua cristata*) from Moluccas, presented by Mr. C. J. Cassirer; a Blue and Yellow Macaw (*Ara ararauna*) from Brazil, presented by Mr. A. Cohen; a Pennant's Parrakeet (*Platycercus pennanti*) from Australia, presented by Mrs. Moon; a Water Rail (*Rallus aquaticus*), British, presented by Mr. T. E. Gunn; two Alligators (*Alligator mississippiensis*) from Florida, presented by Mr. Henry Birkbeck; two —— Snakes (*Pituophis melanoleucus*) from New Jersey, U.S.A., presented by Mr. Morton Middleton; two Snow Buntings (*Plectophanes nivalis*), European, purchased; a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE PHOTOGRAPHY OF SOLAR PROMINENCES.—Mr. G. E. Hale, of Kenwood Physical Observatory, Chicago, describes in *Astronomische Nachrichten*, No. 3006, the results of some attempts made last winter to photograph solar prominences. One of the methods employed is to alter the rate of the driving clock of the equatorial, so that a prominence may move slowly across the slit of a spectroscope adjusted radially on the sun's image. A prominence line, say C, is brought into the centre of the field of the observing spectroscope, and at the same time falls upon a photographic plate having a motion such that the radius of curvature of the sun's limb upon it is the same as that of the focus of the equatorial.

A second method proposed is to use a stationary solar image and photographic plate, with two slits in uniform motion—one the radial slit of a spectroscope, the other a slit moving directly in front of the plate.

The plates have been stained with cyanin, alizarin, and erythrosin for the photography of the prominence lines C and D_3, utilized in the experiments. The work is to be resumed shortly with a rotating cylinder, having upon its circumference a strip of dyed celluloid film moving across the focus of the observing telescope, instead of the plates previously used. If this be done, and a uniform motion given by a good clock or clepsydra, some definite results may be expected.

THE FREQUENCY OF METEORS.—MM. Terby and Van Lint made some observations of the relative frequency of meteors on August 9 and 10, 1890 (*Bulletin de l'Académie Royal des Sciences, Brussels*, No. 9, 1890). An inspection of the observations tabulated shows that before midnight an observer whose field of view embraced one fifth of the horizon, saw from three to five meteors in fifteen minutes. This number was increased after midnight, however, so that an observer, viewing one-sixth of the sky, saw from five to six meteors in the same interval. On August 10 a maximum appeared to be reached between 12h. 30m. and 13h. 11m., the average in that interval being one in two minutes. Only two meteors were observed whose briliancy was comparable with that of Jupiter. The trains were few, and of short duration.

THE ANNIVERSARY OF THE ROYAL SOCIETY. [1]

THE proceedings of every anniversary meeting remind us of the losses which the Society has sustained during the preceding year by the deaths of several of its Fellows. Of

[1] Presidential address delivered by Sir George Stokes at the anniversary meeting, December 1.

those whom we have lost during the past year, some were distinguished for their scientific labours, some were well known in the world at large. Of several, full obituary notices have appeared or will appear in the Proceedings, but it will, I think, be in accordance with the wishes of the Fellows that I should say a few words on the present occasion about some of those whom we have lost.

The Rev. Stephen J. Perry, S. J., whose name is well-known in connexion with his accurate magnetic observations, and his labours in the domain of solar physics, was last year elected a member of the Council. It was known that he could not attend the first meeting, for he would be on his way to the West Indies to observe the total solar eclipse of the 22nd of December, but we looked forward to having him at the meetings of our Council after that was over. In this we were doomed to disappointment, for a telegram which arrived shortly after the day of the eclipse brought the sad intelligence of his death. His illness began shortly before the day of the eclipse, but he did not suffer it to interrupt his preparations, but worked on to the end as far as his failing strength would allow. The observation of total solar eclipses was a branch of solar physics to which he had paid special attention, and, considering the circumstances of his death, we may regard him as a martyr to science, while at the same time his kindly disposition ensured attachment from all who knew him.

William Lewis Ferdinand Fischer, who was a friend of mine for more than forty years, was a German by birth, and was educated in that country. He came to Cambridge later in life than most men commence residence, entering at my own college, Pembroke. At the time when he entered he was only imperfectly acquainted with the English language. He took his degree in due course, having come out fourth wrangler. He obtained a fellowship at Clare College, and, after some time, was elected Professor of Natural Philosophy in the University of St. Andrews, at which town he continued to reside till his death, and his widow still resides. He was remarkably well acquainted with what had been done in physical science.

In the middle of January we lost, in Lord Napier of Magdala, a man of world-wide reputation, who had been a Fellow of the Society for more than twenty three years. A sketch of his life has already appeared in the Proceedings.

The eminent physician Sir William Gull, who for some considerable time had been in failing health, was taken from us at the beginning of this year. The Fellows have already in their hands a sketch of his life.

In February there died, at an advanced age, our Fellow Sir Robert Kane, who was eminent among what we may regard as the last generation of chemists. A biographical notice of him is already in the hands of the Fellows.

Robert William Mylne, who died in July, was twelfth in direct descent of a family of architects and engineers, his direct ancestor having had the erection of new buildings for Holyrood Palace in the reign of Charles the Second. He had still in his possession the correspondence relating to that work. His own work lay chiefly in hydraulic engineering and the geology of the south-east of England, especially the London area, and in advising respecting the construction of new buildings in the City.

General Sir John Henry Lefroy, who died last April, combined the duties of his profession and of his responsible offices as Governor of Bermuda, and, for a time, of Tasmania, with active scientific work in relation especially to terrestrial magnetism, with which he was connected by his post as director of magnetic observations at St. Helena and at Toronto. He is the author of a treatise on the subject, and has entered into some investigations bearing on a possible cause of local magnetic irregularities, which seem well deserving of consideration.

Sir Warington Wilkinson Smyth, who was so high an authority on all that relates to mining, and geology as bearing upon it, was one of our Fellows who repeatedly served on our Council and on various committees, and by his sound judgment aided us in our deliberations. Though his health had been failing for some time, his end came upon us with startling suddenness. It will be remembered by many of the Fellows that he was present at our *conversazione* on 18 June, and next morning he breathed his last. He was widely known as a man of science, and was honorary Fellow of various societies on the Continent. In him I have lost one who was formerly a colleague of my own as lecturer in the School of Mines.

William Kitchen Parker held a very high place among biologists in relation especially to the homologies of the vertebrate skeleton. Notwithstanding the laborious nature of his profession, he managed to find time for his scientific pursuits, and our Transactions contain a large number of papers which came from his pen, and are illustrated by elaborate drawings. So highly were his biological researches thought of, that for several years means were found, through an application of a portion of the Government Grant, for enabling him to dispense with the laborious work of his profession, and devote himself to science. His genial disposition and vivacity of manner, and, curiously enough, his personal appearance, reminded one of Faraday.

Dr. James Matthews Duncan, who died suddenly from heart disease, was eminent as an obstetric physician, and was a man of singular straightforwardness of character.

Our Fellow Charles Handfield Jones died on September 30. His chief scientific labours lay in the domain of pathological anatomy.

Alexander John Ellis, who died on October 28, devoted great attention to philology and to the theory of the perfection of musical sounds, and translated v. Helmholtz's work, "Tonempfindungen." He had, shortly before his death, received an honorary doctor's degree from the University of Cambridge.

During the last year we have lost two out of our three senior Fellows, Christopher Rice Mansel Talbot, a Fellow also of the Linnæan Society, who was elected our Fellow in 1831, and, still more recently, in fact only a few days ago, Sir John Francis Davis, who was elected as long ago as 1822, and died at the very advanced age of ninety-six.

The number of Fellows elected before 1847 is now reduced to eighteen, so that in any statistical calculations of the effect of the statutes of 1847 on the number of Fellows, the present condition of the Society may be taken as practically normal.

The Committee appointed in 1888 to consider the best mode of administrating the fund, which was inaugurated in 1882, for founding a memorial to our late eminent Fellow, Charles Darwin, have now presented their report, which has been adopted by the Council. It has been decided that the proceeds of the Darwin Fund be for the present applied biennially in reward of work of acknowledged distinction (especially in biology) in the field in which Mr. Darwin himself laboured; that the award consist of a medal in silver or bronze, accompanied by a grant of £100; that it be made either to a British subject or to a foreigner, and without distinction of sex; and that the award should be conferred at the same time as other medals at the anniversary.

It was further intended, in accordance with Mr. Darwin's known views, that, as a rule, the award should be made rather for the work of younger men in the early part of their career than as a reward to men whose scientific career is nearly finished.

The Committee appointed at a meeting of the Council held immediately before the last anniversary meeting of the Society, to set on foot a memorial of our late Fellow James Prescott Joule, have naturally not got quite so far in their work. They decided that the memorial should take an international character, and should have for its object the encouragement of research in physical science, and should also have in view the erection of some personal memorial in London. The subject was accordingly brought to the notice of a number of scientific men abroad, from many of whom favourable replies have been received. The Joule Committee have resolved, "That the balance of the fund, after providing a suitable personal memorial, be transferred to the President, Council, and Fellows of the Royal Society, and that the President and Council be requested to undertake the administration of the proceeds in such manner as may appear to them most suitable for the encouragement of research, both in England and abroad, especially among younger men, in those branches of physical science more immediately connected with Joule's work;" and, also, "That the treasurer be instructed to retain for the present a sum not exceeding £300 for the expenses of the medallion, and hand over the balance to the President, Council, and Fellows of the Royal Society."

This offer of the Joule Committee was accepted with thanks by the Council, but the further consideration of the steps to be taken has not yet been entered on. Meanwhile the treasurer of the fund has handed over to the treasurer of the Royal Society a sum of about £1,400.

In 1663, when the second charter was granted to the Society, a body of statutes was drawn up for regulating various matters not fixed by charter. Alterations have since been made from time to time, as provided for in the statutes themselves. The last considerable alteration was made in 1847, when the present system was introduced, according to which the Council select from the candidates, other than those who have a special privilege as to coming on for ballot, a definite number whom they recommend to the Society for election, and the election takes place on one definite day in the year. A few changes, of less importance, have been made since that time, and experience has pointed out the desirability of some changes of detail, chiefly as regards the mode of dealing with papers. A committee was appointed last session, and continued at the commencement of the present, to revise the whole body of statutes, with a view to bring them into stricter conformity with existing practice, and at the same time to propose further changes, should any such appear desirable. The Committee have now reported ; but as the session was near its end, and the subject was one requiring full consideration by the Council, the report has been merely received and entered on the minutes, and it has been left to the Council that is to be elected to-day to take such further steps as may appear to them desirable.

Some of the proposed alterations relate to the mode of dealing with papers which are communicated to the Society, which is a matter of practical business that may well be left to the judgment and experience of the executive body. But some points have been raised which it seems desirable to bring to the notice of the Fellows at large, in order that they may have an opportunity of considering them before a final decision is come to by the new Council.

The question has more than once been raised whether, considering the increase of population and the more general diffusion of scientific knowledge which has taken place within the last forty years, the number of candidates to be selected by the Council for recommendation to the Fellows for election might not now, with advantage, be made a little larger than fifteen, the number at which it was fixed in 1847. On this question there was considerable difference of opinion in the Committee, but the majority were in favour of keeping the number as it is at present.

Connected, to a slight extent, with this question is another, whether the Council should not have the power of recommending to the Fellows for election, in addition to the fifteen selected from among the candidates on the ground of scientific merit, a strictly limited number of men of very high eminence in other ways. The Committee recommend that such a power be entrusted to the Council, the number of Fellows who have been thus elected, existing at any time, being limited to a maximum of twenty-five, and the number elected in any year to a maximum of two.

The question was also discussed whether the maximum number of foreign members, which at present stands at fifty, should be increased, and was decided in the negative.

Another recommendation of the Committee which perhaps it may be as well to mention, is one enabling the Council, in any year, to regulate for the ensuing year the length of the Christmas and Easter holidays. At present the weekly meetings are resumed in the second week after Christmas week, and there is then no intermission till Passion week, though the earlier portion at least of this interval is a time during which papers intended for reading do not usually come in so frequently as towards the end of the session. According to the statute in force till 1888, three of the ordinary weekly meetings between Whit-Sunday and the last meeting in June, were cut out by the Whitsun holiday, Ascension Day, and the annual election of Fellows ; and as at that time of year papers commonly come in pretty frequently, there was a considerable congestion of papers towards the close of the session. This congestion was partially relieved by an alteration of the statutes, which came into force in 1888, enacting that an ordinary meeting should be held at the conclusion of the Annual Meeting for the election of Fellows ; but the fact that the proportion between the number of meetings held and the number of papers that come in varies a good deal with the season, seems to render it desirable that the regulation of the number of meetings should be rather more elastic, and should to some extent be left in the hands of the Council.

Since the last anniversary twenty-five memoirs have been published in the Philosophical Transactions, containing a total of 1068 pages and 72 plates. Of the Proceedings, eleven numbers have been issued, containing 1165 pages.

In the library, the work of making room for growing series, and of obtaining volumes or parts to complete series that were imperfect, has been continued. In the course of this work the Council have, upon the recommendation of the Library Committee, distributed some 1500 volumes, consisting partly of duplicates and partly of works of small scientific value, among various public institutions. The catalogue of the manuscripts, which I mentioned in my last year's address, as about to be commenced, has been completed during the past session. The maps and charts, the pictures and busts, have also been catalogued, and the collection of the manuscripts of memoirs in the Philosophical Transactions, the Proceedings, and the Archives, having been completed, the binding of them is now going forward.

With a view to increasing the circulation of the Society's publications on the Continent of Europe, Messrs. Friedländer and Son, of Berlin, have lately been appointed additional agents for their sale.

The Royal Society has always been ready to assist the Government of the day when requested so to do, by expressing its opinions or offering its advice on questions involving special scientific knowledge. Last year I received, as your President, a request from the President of the Board of Trade that I should, in conjunction with two Fellows of the Royal Society nominated by me in consultation with the Council, examine a report in two parts presented by the Corporation of the Trinity House to the Board of Trade, relative to lighthouse illuminants, and express our opinion whether the conclusions of the Trinity House, as set forth in their Reports, are justified by the records of the experiments contained therein. Lord Rayleigh and Sir William Thomson were asked, and consented to join me as referees, and our Report was some time ago sent in to the Board of Trade.

Another subject in which scientific principles are blended with practical application, is that of colour blindness in its relation to the correct perception of coloured signals used at sea and in the railway service. It is easy to understand what serious accidents might be occasioned, for instance, by confusing red with green ; and so well is the liability to such confusion, arising from a not very rare abnormal condition of colour perception, understood at the present day, that persons who propose to engage in service at sea or on the railways are now, as a matter of course, examined as to their perception of colour. But, glaring as the difference between red and green appears to persons whose vision is normal, the detection of those who are liable to confound them, and who, for the most part, are quite unconscious that they see colours differently from people in general, is by no means so easy as it might appear at first sight ; and there appeared reason to think that sometimes the tests applied are defective, and let pass persons who are afflicted with this peculiarity of vision, while, on the other hand, they may lead to rejection of persons whose vision is normal, perhaps, after they have engaged in their course for life in an employment for which normal vision is demanded. Mr. R. Brudenell Carter wrote a letter to us suggesting that we should appoint a committee to investigate the subject of colour blindness, and after discussion of this proposal I was requested to write a letter to the President of the Board of Trade informing him that, should the Government desire it, the Council will be prepared to appoint a committee to consider the whole question of colour blindness. A reply was received from the Board thanking us for the communication, and saying that they regarded with satisfaction the proposal of the Council to appoint such a committee. A committee has accordingly been appointed, and has held several meetings, and examined several witnesses ; but the subject is a wide one, and the committee have not yet brought their labours to a close.

The proceedings of to-day bring to an end my long tenure of office in the Royal Society, which has extended now over thirty-six years, during the last five of which I have held the honourable office of your President. I am deeply sensible of the kindness which I have always experienced from the Fellows, and of the indulgence with which they have overlooked my deficiencies, due, in part, to the pressure of other work. It cannot be without a strong feeling of regret that I come to the close of an official connection with the Society that has now extended over full half my life. But I feel that it is time that I should make way for others, and that I should not wait for those infirmities which advancing years so often bring in their train ; besides which, there are personal reasons which led me to request the members of the Council not to vote for my nomination for re-election as your President.

And now it only remains to me, as virtually my last official act as your President, to perform the pleasing duty of delivering the medals which the Society has to award to the respective recipients of those honours.

The Copley Medal has been awarded to our Foreign Member, Prof. Simon Newcomb, who has been engaged during the last thirty years in a series of important researches, which have contributed greatly to the progress of gravitational astronomy. Among his labours in this field may be mentioned his able discussion of the mutual relations of the orbits of the asteroids, with reference to Olbers' hypothesis, that they were formed by the breaking up of a ring of nebulous matter, his discussion of the orbits of Uranus and Neptune, and of the orbit of the moon. Recently he has turned his attention to Saturn's satellites, and has investigated the remarkable action of Titan on Hyperion. For many years back he has chiefly been engaged in perfecting the tables of the moon; and in his important work, "Researches on the Motion of the Moon," he has discussed observations of eclipses and occultations previous to 1750, with the important practical result that, by the removal of an empirical term of long period from Hansen's lunar tables, and by an empirical alteration of another term of long period, he is enabled to represent satisfactorily the observations of the moon from 1625 to the present time; and by thus indicating empirically the long period inequality required to represent the moon's motion, he has prepared the way for its theoretical investigation.

The Rumford Medal has been awarded to Prof. H. Hertz for his work on electro-magnetic radiation.

One of the most remarkable achievements of the late Prof. Clerk Maxwell was his electro-magnetic theory of light, in which it was shown that a certain velocity, determinable numerically by purely electrical experiments, and expressing theoretically the velocity of propagation of an electro-magnetic disturbance, agreed within the limits of error of experiment with the known velocity of propagation of light; and accordingly that we have strong reason for believing that light is an electro-magnetic phenomenon, whatever the appropriate physical idea may hereafter prove to be which we ought to attach to the propagation of an electro-magnetic disturbance. But as yet no means existed by which phenomena, such as those of interference, which are bound up with the propagation of undulations, could be exhibited by purely electrical means. Prof. Hertz was the first to detect electro-magnetic waves in free space by his invention of a suitable receiver, consisting of a resonating circuit, which gives visible sparks when immersed in a region of sufficiently intense electric radiation.

By reflection, refraction, and interference experiments, he has further verified the undulatory nature of the disturbance near a quick electric oscillator, such as had been suggested by Prof. Fitzgerald, on the basis of Clerk Maxwell's electro-magnetic theory of light, and Sir W. Thomson's theory of the oscillatory character of a Leyden jar discharge.

These important researches contribute powerfully to the inducements we have to refer the phenomena of light and electricity to a common cause, different as hitherto their manifestations have been; and by this means the theory of each may be advanced through what we know of the other.

One of the Royal medals has been awarded to our Fellow, Dr. David Ferrier, for his researches on the localization of cerebral functions.

We owe to his experiments, and the method of experimenting upon monkeys which he introduced, almost the whole of our knowledge of cerebral localization in man. From pathological observations, Broca located the centre for speech in the third left frontal convolution, but with this exception nothing was known of cerebral localization in man until Dr. Ferrier commenced his experiments in 1873.

Fritsch and Hitzig, in 1870, had observed that definite movements could be obtained by electrical irritation of the cerebral cortex in the dog, and this indicated the existence of localized motor areas in the brain. They did nothing, however, towards localizing sensory centres, and even in regard to the motor centres their observations were very limited. Their observations attracted hardly any attention; they had neither followed them up themselves, nor had any one else taken them up; and when Dr. Ferrier began his experiments he was ignorant of their observations, and discovered the method independently. By the happy device of experimenting upon monkeys, whose brains present a great similarity in the arrangement of the convolutions to those of man, he was able to map out the most important *motor* areas with great precision; but not content with the investigation of motor centres, he experimented on the localization of *sensory* centres in the brain, and not only showed conclusively the existence of such centres, but determined their position. In addition to his work on cerebral localization he has shown, in conjunction with Dr. Yeo, that the complex, and at first sight seemingly purposeless connections of nerve fibres in certain plexuses is really connected with the co-ordination of various muscles for definite purposive movements. As so often happens, these researches, purely scientific in the first instance, have been turned to practical account. Dr. Ferrier himself predicted the application of cerebral localization to cerebral surgery. This application he and others have already made, and his prediction is now being fulfilled with brilliant results.

The other Royal Medal has been awarded to our Fellow, Dr. John Hopkinson, for his researches in magnetism and electricity.

Dr. Hopkinson's researches in magnetism comprise investigations of the effect of temperature upon the magnetic properties of iron, nickel, and various alloys of these metals (Phil. Trans., 1889, A, p. 443). Before these investigations were published it was thought that increased temperature tended to diminish the magnetic susceptibility of iron. Dr. Hopkinson's experiments show, however, that, on the contrary, the magnetic susceptibility increases enormously as the temperature increases, until the temperature reaches about 660° C.; beyond this temperature iron entirely ceases to be magnetic.

Dr. Hopkinson's contributions to the theory of dynamo-electric machinery are most important. The method, now so extensively used, of solving problems relating to dynamos by the use of what M. Deprez has called the "characteristic curve," is due to him.

He has also made a series of determinations of the specific inductive capacities and refractive indices of a large number of transparent dielectrics, the results of which are of great importance in the theories of electricity and light.

The Davy Medal has been awarded to Prof. Emil Fischer for his discoveries in organic chemistry, and especially for his researches on the carbo-hydrates.

To him, in conjunction with Otto Fischer, we owe the determination of the constitution of rosaniline, a most valuable dye-stuff, and the typical member of a very large group of important dyes.

He is the discoverer of phenylhydrazine, one of the most important of the reagents placed at chemists' disposal within recent years, and he has most exhaustively studied the behaviour of this substance and its congeners. The hydrazines have also been employed by Fischer in preparing indole derivatives, among others, skatole, and the study of a class of substances of considerable physiological importance has thereby been rendered possible.

During the past seven years Fischer has devoted his attention to the study of the sugars, and has obtained most marvellous results, having succeeded in preparing, by purely artificial methods, the known sugars dextrose and levulose, as well as other isomeric sugars, and having established the relationship of the various members of the glucose group. He has, in addition, determined the constitution of milk-sugar and of starch-sugar—the isomer of cane-sugar formed on hydrolysing starch. He has also prepared "glucoses" containing seven, eight, and nine atoms of carbon, and has established the remarkable fact that only those which contain three, six, or nine atoms of carbon are fermentable by yeast. His researches are not only of the highest value to chemists, but also of extreme importance to physiologists, on account of the insight which they afford of the processes concerned in the natural formation of sugars.

The Darwin Medal has been awarded to Mr. Alfred Russel Wallace for his independent origination of the theory of the origin of species by natural selection.

It was natural that this, the first award of the Darwin Medal, should have been made to one who independently originated the theory, since named that of natural selection, which, in conjunction with his other numerous and important contributions in the domain of natural history, have made the name of Darwin so famous, and who made known a large series of important and novel observations in support of that theory, the result of many years work in the Malay Archipelago. These views Mr. Wallace has subsequently most ably advocated in various published works, among others his laborious volumes on the "Geographical Distribution of Animals," his brilliant "Island Life," and more recently his "Darwinism," which was published only last year.

WHO ARE THE AMERICAN INDIANS?

ATTENTION has once more been attracted to the American aborigines by the troubles of the United States Government with some of the Indians of Dakota and various neighbouring districts. Special interest therefore attaches to a lecture on the question "Who are the American Indians?" recently delivered by Mr. H. W. Henshaw, in the National Museum, Washington, in the "Saturday Course," under the auspices of the Anthropological, Biological, Chemical, National, Geographic, and Philosophical Societies. The following is the more important part of the lecture:—

When Columbus discovered America he discovered not only a new continent but a new people—the American Indians. From one end to the other of its broad expanse the continent was occupied by Indian tribes that had held the land from time immemorial—so far at least as their own traditions aver—knowing nothing of any country but their own. The commonly presented picture of the Indians as they appeared at the time of the discovery is that of a horde of wandering savages, half or wholly naked, living on roots and herbs, or existing by the capture of wild animals scarcely more savage than themselves, and the chief objects of whose existence was to enslave, to torture, and to kill each other. Those who hold such opinions have ever taken a hopeless view of the Indian's present and a still more hopeless view of his future. Such a picture conveys a totally false impression of the Indian and of the state of culture to which he had attained at the era of the discovery. Though still living in savagery, he was in the upper confines of that estate, and was fast pressing upon the second stage of progress, that of barbarism—that is to say, he had progressed far beyond and above the lowest states in which man is known to live, to say nothing of the still lower conditions from which he must have emerged, and had travelled many steps along the long and difficult road to civilization.

Already he had become skilful in the practice of many arts. Though the skins of beasts furnished a large part of his clothing, he had possessed himself of the weaver's art, and from the hair of many animals, from the down of birds, and from the fibres of many plants he knew how to spin, to weave, and to dye fabrics.

Basket-making he had carried to so high a degree of perfection that little further improvement was possible.

The potter's art also was his, and though his methods were crude and laborious, the results achieved, both as regards grace of form and ornamentation, may well excite admiration at the present day.

Copper had been discovered and was mined and roughly beaten into shape to serve for ornament and, to some slight extent, for mechanical use. In Mexico and Peru, gold, silver, and copper were worked, and many authors contend that the method of making bronze, an invention fraught with tremendous possibilities, had there been discovered.

In much of South and Central America, Mexico, and the eastern parts of the United States, so important an advance had been made in agriculture that it furnished a very large part of the food supply, and it should not be forgotten that the chief product of the Indian's tillage, maize or Indian corn, which to-day furnishes a large part of the world's food, was the gift of the Indian to civilization. A scarcely less important contribution to mankind is the potato, the cultivation of which also originated with the Indians. A third important agricultural product, though less beneficial, is tobacco, the use and cultivation of which had been discovered centuries before the advent of the European.

Architecture may seem like a large word to apply to the dwellings of the Indians. Nevertheless many of their houses were more substantial and comfortable than is generally supposed, while in the North-West many tribes reared dwellings of hewn planks, sometimes as large as 210 feet long by 30 feet wide, which were capable of accommodating several hundred individuals. More pretentious and durable were the communal houses of mud and stone reared by the Pueblo people of Arizona, New Mexico, and Mexico, while further south, in Central and South America, were edifices of hewn stone, which from their dimensions, the size of some of the blocks contained in them, and the extent and ornate character of the ornamentation, justly excite the wonder and admiration of the traveller and archæologist.

The advantages of a beast of burden had been perceived, and though the human back furnished by far the greater part of the transportation, yet in North America the dog had been trained into an effective ally, and in the Andes the llama performed a similar office. Insignificant as was the use of the dog as a carrier, its employment cannot well be over-estimated as a step in progress when it is remembered that the plain's tribes that most employed it lived in the midst of the buffalo, an animal which must have become of prime domestic importance in the never-to be-enacted future of the Indian.

The need of some method of recording events and communicating ideas had been felt, and had given rise, even among the ruder tribes, to picture-writing, which in Mexico and Central America had been so far developed into ideographs, popularly called hieroglyphics, as to hint strongly at the next stage, the invention of a true phonetic alphabet; nay more, the Mexicans and Mayas are believed to have reached a stage of true phonetic writing, where characters were made to represent not things, as true ideographic writing, but the names of things and even of abstract ideas; and this is a stage which may be said to be on the very threshold of one of the proudest achievements of civilization, that of a phonetic alphabet.

Instead of living in an unorganized state, where each man was a law unto himself in all things, the Indians lived under organized forms of government, rude enough indeed when compared with the highly organized system of civilized nations, but marking an essential advance on the conditions attained by savage peoples in other parts of the world. The chieftaincy was transmitted by well understood laws, or, as in some tribes, was more purely elective. Their social system was very ingenious and complex, and, being based largely upon kinship ties, was singularly well fitted for the state to which they had attained, of which indeed it was simply an expression and outgrowth. In many sections a considerable advance had been made in political confederation, and neighbouring tribes combined for defence and to wage war against a common enemy. They had invented many and singularly efficient laws to repress and punish lawlessness against the individual and the social body, and as a consequence they enjoyed almost entire immunity from theft and many other crimes.

The development of religious ideas among our Indians is a curious and instructive study. Though the Great Spirit and the Happy Hunting Ground which missionaries and theologians thought they had discovered among them are now known to have had no existence, the Indians had by no means reached the state of culture in which they were found without developing religions. Their gods or fetishes were innumerable, their priests endowed with immense influence, and their ceremonies of devotion and propitiation were as devout as they were elaborate. The precision of the beliefs of many tribes and the elaborateness of their rituals are simply astonishing. Thus their advance in the domain of religious thought equalled, if it did not surpass, their progress in some other directions.

If by medicine we mean the rational treatment of disease, the Indian can be said to have learned only the rudiments of the healing art. Medicine, in so far as it was a distinct profession, was almost wholly in the hands of the Medicine Man or Shaman, who filled the twofold office of priest and doctor. Neither the theory nor the practice of the Shaman had in it anything that was rational and very little that was efficacious, except through the influence exercised over the mind of the patient—in other words, except so far as the Shaman was a faith-curer. Whatever that is marvellous in the modern cases of faith-cure can be more than matched out of the practice and experience of the Shaman, who learned his trade long before the European came to these shores. He who would see the Indian Shaman need not seek the wilds of the Far West. He may find his counterpart on Pennsylvania Avenue. The whole medical practice of the Indian Shaman was based upon the idea that all disease was the effect of evil disease spirits that had obtained lodgment in the body, or that it was caused by witchcraft, and so long as practice was directed to the dislodgment of these spirits no rational treatment was possible. I am aware that the above idea of Indian medicine is contrary to popular belief, which to some extent at least is in harmony with the claims of alleged Indian doctors of white extraction who claim to have derived their skill and their herbs directly from the hands of Indian experts. Recent and carefully conducted investigations on this subject, however, fully substantiate the above statements. Though roots and herbs were employed in the treatment of nearly all diseases, they were chiefly used as adjuncts to the harms and sorceries of the medicine man. Often they were not given to the patient at all, but were taken by the medicine man to heighten his power over the disease spirits. Often they were applied by being rubbed on the body of the patient or by being blown in the shape of smoke on the afflicted part.

Among the Indians was found flourishing to a remarkable degree the so-called doctrine of seals or signatures. A few examples of the doctrine derived from the eastern Cheroki by Mr. James Mooney may prove of interest. Doubtless you are all familiar with the cone flower. The Cheroki call it deer eye, and from its fancied resemblance to the strong-sighted eye of the deer and its connection by name, for the Indian believes that there is a potent connection between the name of a thing and the thing itself, it is used as a wash for ailing eyes.

The common purslane (*Portulaca oleracea*) is used as a vermifuge, because the red stalk looks like a worm.

An infusion of the roots of the hoary pea (*Tephrosia virginiana*), called devil's shoe-strings in the South because of their toughness, is used by the Cheroki ball-players as a wash to strengthen their bodies, and by the women as a hair wash to strengthen it and keep it from falling.

Who of you has ever walked in our woods without getting on his clothing the common beggar's lice (*Desmodium*)? How tenaciously they stick you all know; so do the Cheroki, and because the burrs stick fast they use a tea made of them to strengthen the memory. The Cheroki at least can dispense with the services of a Loisette.

You whose ambition it is to be good singers have only to drink a tea of crickets, according to the Cheroki, for does not the cricket possess a fine voice and doth he not sing merrily?

The tendency of the human mind to speculate and to draw inferences—a tendency common alike to the savage and the civilized man—cannot be held in check forever, however strong the bonds; and just as knowledge and science escaped from priestly thrall within the history of civilized times, so a certain small amount of knowledge of the therapeutical use of drugs was gaining ground among the common folk of the Indians. It was fairly to be called old women's practice, as it was largely in their hands. It grew out of observation; infusions of certain herbs produced certain results, acted as emetics or purgatives, and hence these herbs came to be employed with something like an intelligent purpose. Many of the herbs used were absolutely inert; many were harmful, of course, since where there is practically no true diagnosis and no correct knowledge of the effect of drugs there can be no really intelligent selection of remedies; but in the case of certain simple diseases, herbs, the actual cautery, and above all the sweating process, were beginning to be recognized by the common folk as serviceable, and to be employed to some extent without recourse to the Shaman.

As the child must creep ere it can walk, in such theories and treatment, childish though they may seem, may be discerned the beginnings of the noble science of Medicine, which, having largely cast aside the superstitions that hampered her infant steps, now walks erect, and although of late she seems to have revived the beliefs of her childhood, her handmaiden, Science, bids her call the demon disease spirits ignorance and vicious habits, the diseases themselves bacilli or germs. The Indian believes that the white man carried the spirit of small-pox in bottles, and let it loose among them. Modern science actually does bottle the small-pox germs, and germinates them at will. So the Indian theory of disease reappears in a new form.

Such in briefest outline are some of the achievements of the Indian as he was found by civilized man. Whatever value may be placed upon them, whatever rank may be assigned them in the scale of human efforts, they were at least his own, and some of them compare favourably with the record of our Aryan ancestors before they split up into the numerous nations which have done so much to civilize the world. Many, I am aware, hold that the Indian had progressed as far towards civilization as his capacities admitted; others have held, and possibly some now hold, that he was already on the decline; they see in his crude ideas and rude inventions only the degradation of a higher estate; in other words, instead of a savage preparing to enter civilization through the necessary half-way state of barbarism, he is held a half-civilized man lapsing into savagery. Such views, it is needless to say, find no favour in the mind of the evolutionist. To him, the achievements of the Indian are only the milestones which have marked the progress of every civilized nation in its march from what it was to what it is; to him the chief value and significance of his studies of the mental state of the Indian, as expressed in his mythology, his medicine, his social and political organization, or in his more concrete arts, is the fact that in them he reads the records of his own past. If there be any truth whatever in the theory of evolution as applied to human progress, only one inference can be drawn from the history of the Indian race as it appears in historical pages and in the no less eloquent records interpreted by archæologists. This inference is that, starting in its career later than some other races, or being less favoured by circumstances or conditions of environment, or possibly being less endowed, the Indian, despite all, had progressed an immense distance towards civilization; that the race contained all the capabilities for a further advance, and for achieving a civilization of its own, differing, it may be, markedly from our own, as other civilizations differ, but still containing within itself all the essentials of that wonderfully complex thing called civilization. Such at least is the lesson evolution teaches.

Hardly had the new land been discovered when the question arose, Who are the Indians and where did they come from? Naturally enough the Indian had his own answers to these questions. As to who they are all tribes agree. "We are men," said the Illinois to the French; and the name of every tribe in America—the name by which they know themselves—signifies "true men," "men of men," "the only men," as evincing their superiority to all others. As to their origin, their ideas are as confused and perplexing as they are multifarious and conflicting. It may almost be said, as many tribes so many origins. A large number of tribes claim to have originated in the localities where they were first found by Europeans, where they emerged from the ground or came from the recesses of some neighbouring mountain. The Chocta, for instance, claim to have come from an artificial mound in Mississippi. The mound has a depression at its centre which is accounted for by the Creator stamping upon it to close the aperture when he saw that a sufficient number had emerged to form the Chocta tribe. One of the Shawni tribes was created from the ashes of the fire. The Yuchi, of Georgia, claim to be children of the sun, who is their mother, and the earth, their father, an exact reversal of the usual parentage. The Pomo, of California, claim that their ancestors, the Coyote men, were created directly from a knoll of red earth. The Klamath, of Oregon, were made from the service berry. The Yokut, of California, emerged from badger holes, as their name implies. Somewhat more poetical is the idea of the Aht, of Vancouver Island, who allege that animals were first created at Cape Flattery, and from the union of these with a star that fell from the skies resulted the first men, their ancestors.

The above are fair examples of the ideas entertained by the Indians respecting their own origin. Puerile they certainly are, yet who will maintain that they are more so than the theories of origin held by the Greeks and other classical peoples?

Who, then, are the American aborigines? For Columbus and his followers there was but one answer to the question. As he had reached the eastern shores of India the people must be Indians, and his error is perpetuated to-day in the name. Later, when the newly discovered country was found to be not an old but a new continent, the question of the origin and consanguinity of the Indians was renewed. So strongly tinged with religious thought was the philosophy of the day that Biblical sources were naturally first appealed to to solve the knotty problem. As mankind was supposed to have originated in Asia, and as all but the ten lost tribes were accounted for, they were rationally appealed to for the origin of the Indian. Perhaps the best exponent of the belief in the Jewish origin of the Indians was Adair, who published his celebrated essay in 1775. Thoroughly familiar with Indian beliefs and customs, he succeeded in bringing together a mass of evidence, derived from a comparison of religious rites, civil and martial customs, marriages, funeral ceremonies, languages, and traditions, as curious and contradictory as it is inconclusive.

The Jewish origin of the Indians secured a very strong hold on the minds of the writers and thinkers of the eighteenth century, and so firmly did the theory take root that it has never been wholly given up, but is held to-day by a greater or less number as the only rational belief.

Though the favourite, the Jewish hypothesis is by no means the only one. Men of science and laymen count their theories by the score. The Bible and ancient philosophy alike have been appealed to in support of pet hypotheses.

One believes America to have been colonized by Phœnician merchants; another, by Carthaginians. America was peopled by Carthaginians, says Venegas, and Anahuac is but another name for Anak. Besides, both nations practised picture-writing; both venerated fire and water, wore skins of animals, pierced the ears, ate dogs, drank to excess, telegraphed by means of fires on hills, wore all their finery on going to war, poisoned their arrows, beat drums and shouted in battle. Not an unfair example this of the scientific deductions of the day. Surely he must be unreasonable who refuses to be convinced by such testimony!

Says the pious Cotton Mather, the natives of the country now possessed by the New Englanders had been forlorn and wretched heathen ever since they first herded here, and though we know not when or how these Indians first became inhabitants of this mighty continent, yet we may guess that probably the devil decoyed those miserable savages hither, in hopes that the Gospel would never come here to disturb his absolute empire over them.

The evidences that the Indians came from Scandinavia are as convincingly put as those proving that they came from Ireland, or Iceland, or Greenland. Equally conclusive are the arguments for a passage by the Indian across Bering Strait from Asia, across the Northern Pacific from Japan or China in junks, or across the Southern Pacific in canoes from the Polynesian Islands, or Australia. Even Africa is not too far distant to send its contingent to the new land; and when the ocean has been deemed to be too broad to permit a passage from foreign shores the willing imagination of the writer has dropped an island into mid-ocean, and called it Atlantis, to facilitate alike the crossing of the Indian and the reception of a fanciful theory. Thus there is a theory of origin to suit the tastes of all. If you have a special bias or predilection, you have only to choose for yourself. If there be any among you who decline to find the ancestors of our Indians among the Jews, Phœnicians, Scandinavians, Irish, Welsh, Carthaginians, Egyptians, or Tartars, then you still have a choice among the Hindu, Malay, Polynesians, Chinese, or Japanese, or, indeed, amongst almost any other of the children of men.

Preposterous as may seem many of the theories above alluded to, nearly all of them rest upon a certain basis of fact and comparison. Many, at least, of the similarities of thought, custom, methods, arts, religions, and myths from which the theories are deduced indeed exist, though false analogies permeate them all. The thread of fact which sustains the theories is, moreover, far too slender to bear the weight put upon it. It is not that the theories contain so much that is erroneous but the proof offered is entirely insufficient. The science of yesterday reared its edifices upon foundations of fact the very slightest. The science of to-day demands broader foundations and more deeply laid upon which to base its conclusions. Erroneous hypotheses like the above have, however, been productive of great good in pointing out and emphasizing some of the most useful lessons which the student of anthropology of the present day must learn and ever keep in mind. Of these perhaps the most important is that the human mind is everywhere pratically the same; that in a similar state of culture man in groping his way along will ever seek the same or similar means to a desired end. That, granting the same conditions of environment, man acts upon them and is acted upon by them in the same way the world over. Hence, in large part, arise those similarities of customs, beliefs, religions, and arts which have been appealed to as evidences of genetic connection or of common origin, when, in fact, they are evidences of nothing but of a common humanity.

This leads us to speak briefly of some of the leading methods of classification by which men of science have sought to solve the problem of the origin and relation of races, and among other peoples of our own Indians.

The physical tests of race most approved by ethnologists are colour, viz. the colour of the skin, hair, and eyes; the structural differences of the hair; the size and shape of the skull as determined by capacity and measurements; and the test of language.

Few of the tests formerly relied upon in classifying mankind have proved less satisfactory to modern investigators than that of colour. The microscope appears to show that colour is not due to organic differences of race; not only are there great differences in the colour of individuals of the same tribe, but of the same family, and even in the same individual at different periods of life. Thus, in the case of our Indians, it is well known that the skin of the infant at an early age is very light-coloured, scarcely distinguishable, in fact, from a Caucasian child, and that it assumes a deeper shade only with advancing years. This, I believe, is true irrespective of tribe or habitat. The hair of the Indian child is brown instead of black. The colour of the adult Indian varies within very wide limits, but singularly enough he is never copper-coloured or red, as he has been called from the time of the discovery. All our Indians are brown, and while certain tribes, as the Tuscaroras and Mandans, are so light as to give rise to the theory that they are descendants of the Welsh, other tribes, as some of the Californians, are so dark as to closely approach the black Africans. I say black Africans, for it is to be remarked, in passing, that some African tribes are very light-coloured.

The division of mankind into four groups—white, black, copper, and olive—is, in a general way, consistent with facts. Moreover, these divisions are, to a certain extent, correlated with geographical range and climate, and thus correspond to the colour differences of birds and animals. That they are also and perhaps more strictly correlated with culture status is not to be doubted; for it may be said, in a general way, that all civilized peoples are light-coloured and nearly all barbaric and savage peoples are dark-coloured. So complete, however, is the intergradation of colour when all varieties of mankind are considered, and so intangible are the distinctions which must be relied upon to distinguish them in the case of individuals and even of tribes, that it appears that while colour affords a convenient off-hand means of classification, and while it may be made useful in connection with other criteria, it is quite insufficient in itself as a test of race.

The more obvious peculiarities of the hair, according as it is straight, crisped, or curly, early attracted the attention of ethnologists. The modern microscope has shown that these peculiarities are more or less intimately correlated with structural differences, and that the straight hair of the American Indian and the Mongolian is nearly cylindrical in section, while the frizzled hair of the Negro and Papuan shows an oval or flattened section. Between the two extremes, however, are too many shades of difference to permit the extensive use of this criterion as a race classifier, except in a subordinate way.

Much time and thought has been expended by craniologists in the effort to classify races by means of the skull. Notwithstanding the great ingenuity exercised in devising methods and instruments to secure constant results and trustworthy figures as a basis for comparison, the results thus far obtained have been disappointing. So faulty were the mechanical means adopted by the earlier craniologists that students of to-day are compelled to discard their data and resulting conclusions and begin almost *de novo*. There are many able men who are sanguine not only that the physical structure of man may yet be made to reveal secrets bearing upon the origin of races, if there be more than one, but that the science of craniology in particular is destined to have an important bearing upon these racial problems. Whatever the future of craniology or the other methods of classification by physical characteristics may have in store, the contradictory results thus far obtained offer little to satisfy us. Not only do the naturalists and ethnologists who have studied man's physical characteristics differ as to the number of races of mankind, and as to what constitutes a proper basis for classifying them, but thus far there has been little agreement as to the assignment of particular tribes or peoples. Perhaps more authorities are agreed that there is but one race and one origin of mankind than agree upon any greater specific number; but when it is remembered that there are authorities who place the number of distinct races at two, three, four, five, six, seven, eight, eleven, sixteen, and that one places the total as high as sixty-three, it will be agreed, I think, that it is better to suspend judgment and not to accept any present result as final.

We have already noted that the earlier theories of origin for the Indian, based as they largely were upon certain assumed analogies of customs, laws, religious observances, myths, &c., rested upon such slight foundations as to hardly entitle them to be classed as scientific hypotheses. We have also seen that up to the present time the attempts to classify mankind by his physical characters have produced discordant results, and that little dependence is to be placed upon the results themselves or upon the theories arising therefrom which relate to the more profound question of the origin of races. In turning to the test of language, if doubt and uncertainty were left behind and harmony and agreement took the place of discordant views we might count ourselves fortunate indeed. But such is not the case. We have indeed only to go back a short time to find that the generalizations drawn from the study of language are as crude, the hypotheses as baseless, the theories as wild as are those we have just glanced at. Nor is this strange. Like its sister sciences, linguistics had to pass through a period in which speculation and hypothesis, instead of going hand-in-hand with facts and induction, usurped their place. Until the inductive method was born no science of philology was possible. The science of comparative philology is indeed of recent origin, nothing worthy of the name existing before the present century. Within the last fifty years it has made a wonderful growth and achieved results little short of marvellous. Though still in its infancy as regards future possibilities, and while it needs and welcomes the aid of all the other sciences to solve the complex questions which come properly within its domain, it is unquestionably our best guide in problems relating to the origin and relationship of the races of mankind. . . .

No part of the known world affords a better opportunity for the study of the nature of language and its processes of growth than America. The Indian languages are by no means the most primitive at present spoken by man, and it may surprise some of my hearers to be told that in respect of some of their characteristics they compare favourably with Greek and other classic tongues, though the classic languages as a whole belong to a much higher stage of development. Instead of being mere jargons of words, disconnected with each other and capable of expressing only the simplest ideas, as I find many intelligent people believe, they are in some directions singularly highly developed, and not only are they capable of serving as the vehicle of every thought possible to their possessors, but their vocabularies are extensive, possess many synonyms, and furnish the means of discriminating the nicest shades of meaning.

There is not a principle or process in the most highly developed languages of which the germs at least are not discernible in Indian languages. The differences are not those of kind but of degree of culture.

Moreover inherent in them is the power of unlimited growth and expansion, and just as our own language grows, keeping step with advances in science and art, so Indian languages are capable of a development equal to the most exacting demands of civilization.

While thus in many respects highly developed, Indian languages are not to be compared as vehicles of thought with such languages as our own English, for instance. As a body they are still in that stage of development in which the various processes of language-making may be studied with comparative ease. Just as the various natural processes by which mountains are levelled and the earth's surface carved out and remodelled are more apparent and more readily studied by the geologist in the still primitive West, so Indian languages offer to the scrutiny of the linguistic student a similar unfinished condition highly favourable for analysis and study.

For the past fifteen years Major Powell and his assistants of the Bureau of Ethnology, with the aid of many collaborators in various parts of the country, have been accumulating vocabularies by means of which to classify Indian languages. The present provisional results of the study of the large amount of material accumulated appear before you on the Linguistic Map, which is coloured to show the areas occupied by the several linguistic families. Of these there appear no fewer than fifty-eight.

What interpretation are we to place upon the astonishing fact that in the territory north of Mexico there were at the time of the discovery fifty-eight distinct Indian linguistic families, containing some 300 or more languages and dialects?

So far as Language is a competent witness, she has exhausted all the evidence thus far accumulated when she has grouped the Indians in fifty-eight families. Back of this point she may not now go except as a theorist and in pure speculation. So far as she is entitled to speak authoritatively, these fifty-eight families are separate entities, which never had any connection with each other. But she recognizes her own limitations too well to dare to state positively that this is the interpretation that must be placed upon the results she has attained. When facts from which to draw deductions fail, men may and do resort to theories. Let us glance at the two broad hypotheses which have been based upon the development theory of language. The first is in effect that all the present languages of the earth are not so unlike that they may not have been developed from a single original parent language. By this view the original language is supposed to have changed and developed into all the various forms of speech that are now spoken or that have ever been spoken. According to this view the families of languages as at present classified have no other significance than as groups of related tongues, the once existing connection of which with other tongues cannot now be proved, because through the process of change the connecting links have been lost.

The second hypothesis assumes that there must have been at least as many original languages as there are now existing families; it assumes, in other words, that the families of speech are fundamentally distinct and therefore cannot have had a common origin. The first theory postulates that from original unity of language has come infinite diversity; the second that the tendency has ever been from original diversity towards unity.

Widely different as are these two theories of the origin of linguistic families, they agree in one essential particular. They both remove the origin so far back in time as to make it practically impossible to *prove* the truth or falsity of either theory.

Both of these hypotheses have able advocates; but for a variety of reasons, which time will not permit me to give, the second is deemed the more plausible. At all events, it best explains many difficulties.

There is abundance of archæologic evidence showing that man has resided on this continent for a very long period, and the character of the remains proves that the farther back in time we go the ruder being he was. Linguistic testimony is to the same effect, and there is no *a priori* reason why man may not have lived upon this continent ages before he learned to talk—no reason, for that matter, why America may not have peopled the earth, if the earth was peopled from a single centre, or why, if there have been several centres of origin for mankind, the Indians, as they themselves believe, may not have originated here where they were found.

It is the fashion, I hardly know why, unless it be the religious bias, for those who hold that language has had but one origin to assume that America is the younger continent, so far as her people are concerned, and to infer that it was peopled from Asia. If America was peopled from Asia in modern times, there should be some evidence of the fact in American languages. But there is no evidence of the sort. None of the American families of language are in any way related to the Asiatic tongues. Bering Strait furnishes, indeed, a perfectly practicable canoe route from Asia to America, but it appears to have been generally overlooked that the Strait furnishes an equally accessible route from America to Asia. The latter is demonstrated by the fact that the Eskimo of Alaska have in recent times sent an Eskimo speaking colony across Bering Strait to Siberia. In other words, so far as direct testimony goes, Asia is indebted to America for a small segment of its people, but America owes no similar debt to Asia. With reference to the origin of our Indian tribes, then, linguistic science is in a position to state this much, that if our Indians came to America, either from Asia or from any other foreign shore, it was at a period so remote as to permit such profound changes in the structure of the language brought here by the immigrants that no traces of genetic connection are now discernible.

If we reject the one origin theory of language, and assume that each linguistic family originated independently, there is obviously not the slightest use of turning to Asia or Europe for anything like a recent importation of the Indian; for have we not fifty-eight distinct origins to account for? Obviously the fifty-eight families are as likely to have originated here as anywhere else; for remember that every country has linguistic families of its own to account for. Is there, then, any possible theory which will meet the case? There is certainly one that is possible, if not probable. It is the theory that, whether born from the soil or an immigrant from other lands, our Indians spread over the entire continent before they acquired organized language, and that from not one but from fifty-eight centres sprang up the germs of speech which have resulted in the different families of language. This theory accords with the idea that there may have been but one origin of man, and that in any event all the Indians from the Arctic to Patagonia are of one race. It does not forbid the supposition that the Indian was an immigrant from other shores, though it permits the thought that the American Indian may have originated on American soil.

Though this theory seems more probable than the other, which assumes that the languages of our Indians were brought here from foreign shores, it must be frankly admitted that linguistic science is not now and possibly never will be competent to decide between them. If she is unable to decide fully as to the origin of the Indians' languages, how can she be expected to solve the infinitely more complex problem which concerns the ultimate origin of the peoples who spoke them? She certainly has no solution for this problem now. When she considers the number of linguistic families, and the vast length of time it must have taken to develop their languages and dialects, she finds herself confronted by a problem beyond her present powers. And yet the case is not hopeless. Linguistic science is still in its infancy, and its future may contain possibilities far exceeding the dream of the most sanguine. As science has revolutionized the world's processes, and has made the impossibilities of a hundred years ago the common-places of to-day, so like wonders may be achieved in the domain of thought, and the science of Language, with the assistance of her sister sciences, may yet answer the unanswerable questions of the present. . . .

When interrogated as to the origin of the Indian, all that she can now say is that, whether the Indian originated on this continent, where he was found, or elsewhere, it was in bygone ages—ages so far removed from our own time that the interval is to be reckoned, not by the years of chronology, but by the epochs of geologic time; with such problems she affirms that at present she cannot deal.

THE NEW OBSERVATORY IN CATANIA.

WE learn from the *Corriere di Palermo* the following details concerning the new Observatory at Catania, in which active work is about to begin. For many years astronomical studies were carried on with success in the Observatory at Palermo, but owing to the great drawbacks arising from the position of this institution (amongst which the most serious was the close proximity of a railway), it became exceedingly difficult to make precise observations. Some of the astronomers of the Observatory preferred to devote themselves to researches in astronomical physics, in which the inconveniences were less felt, while others who continued to occupy themselves with astronomy of position had to contend against ever-increasing difficulties. The Government, being informed of the state of affairs, nominated in 1888 a Commission to select a more suitable spot, and to make arrangements for a new Observatory; and the only place found suitable was the mountain Consuono, near Bagheria. As an Observatory built there would have the most perfect conditions of quiet and stability, it was decided that it should be devoted to the ancient and classical branch of astronomy of position.

In the meantime, several years ago, a new Observatory had been built on Etna (at a height of 3000 metres), through the activity of Prof. Tacchini, and the approval and help which the project found in Catania. Prof. Tacchini, recognizing the necessity that the Observatory at Etna should be joined with another, that it might work with greater regularity, succeeded in inducing the Government to build and organize the Observatory connected with the University in the town of Catania. This institution is destined principally for the study of astronomical physics, for celestial photography, for meteorology, and for seismology. Direct astronomical and spectroscopical observations will be made for the most part in the upper story of the Observatory with the great refractor, which has an objective of 35 centimetres in aperture and 5⅓ metres in focal length. It was made by the celebrated Merz at Munich, and the equatorial mounting was executed by the able mechanician Cavignato, of Padua, under the direction of the astronomers Lorenzoni and Abbeti, of that town. Other observations will be made with the equatorial of 15 centimetres in aperture, constructed by the renowned American, Clarke, to which an apparatus can be added for the photographing of stellar spectra. This instrument will be placed in the garden of the Observatory, liberally given by the Municipality of Catania.

In the same place, in a suitable pavilion, will be fixed the photographic telescope, with an objective of 33 centimetres in aperture. This was constructed by Steinheil, and the mounting was made under the direction of Prof. Tacchini in the premises of the clever mechanical engineer, Salmsiraghi, at Milan. With this instrument the Observatory at Catania will take part in the great international enterprise of preparing photographic charts of the stars. In the same garden the apparatus of Huggins will be set up, which serves for the photographing of the solar corona. Besides, at the Observatory on Etna, there is an equatorial, equal to that of the great refractor; and to this it will be possible to apply the same objective of Merz, and another which, for special observations, astronomical and physical, will be taken to the volcano.

Observations in meteorology will be made, some in the upper story of the building, where the apparatus of Mascart for the photographic registration of atmospherical electricity will be worked. Other meteorological observations will be made in the garden; and the Observatory on Etna will be furnished with a collection of meteorological and seismological instruments capable of automatic registration at times when the Observatory will be inaccessible. The seismological instruments will be by degrees removed from the University of Catania, where they are now collected, to a far more suitable place—to the vast subterranean rooms of the Observatory; thus, in the network of stations, this institution will take the first place for the seismological service of Sicily and the adjacent islands.

Prof. Riccò has been appointed regular Professor of Astronomical Physics in the University of Catania, and Director of the two Observatories. He is a member of the International Committee for photographing the stars.

NOTES FROM THE OTAGO UNIVERSITY MUSEUM.

X.—THREE SUGGESTIONS IN BIOLOGICAL TERMINOLOGY.

(1) MR. R. J. HARVEY GIBSON has recently criticized my proposal to use the terms *gamobium* and *blastobium* for the sexual and asexual generations respectively in both plants and animals, and has given some very good reasons for objecting to the latter term.[1] I therefore propose to replace it by *agamobium*, using the word simply as a monomial equivalent of "asexual generation," *gamobium* being similarly employed for the sexual generation. Both terms are equally applicable to plants and animals, and to the various kinds of alternation of generations.

(2) Mr. Gibson, while using the word *ovarium* for the female gonad or ovum-producing organ both in plants and animals, objects to my employment of the anglicized form of it (ovary), because of the confusion arising from the fact that the name is applied to something quite different in Angiosperms. My own impression is that reforms of this kind should be thorough, and that, if it is once admitted that terms of fundamental importance should not be used in one sense for animal and in a totally different sense for vegetable organisms, it would be the ruin of the whole scheme to be obliged to describe the ovarium of an Angiosperm as something contained within an ovule, which is itself enclosed in an ovary. We have already the accurate word megasporangium for the ovule, and I propose to speak of the so-called ovary, by a purely descriptive term, as the *venter* of the pistil.

(3) Every zoologist must acknowledge the services rendered by Haeckel and others in giving names to the more important embryonic stages of animals. It appears to me desirable that the equally important stages in plant development should be similarly emphasized.

In the paper already referred to I have proposed that the one-celled embryo, *i.e.* the immediate product of the conjugation of ovum and sperm should be universally called the *oosperm*, the objectionable name oospore being dropped.[2]

In mosses and vascular plants, as in animals, the next stage of importance after the oosperm is that in which the embryo consists of a mass of undifferentiated or but slightly differentiated cells. It is a matter of minor importance that in some cases a long axis is already obvious, or that in others an apical cell is formed: the stage may be conveniently distinguished by Lan-

[1] See Gibson, Proc. Biol. Soc. Liverpool, vol. iv. (1889) p. 24; *ibid.*, 1887; and Parker, Proc. Austr. Assoc. Adv. Sci., vol. i. (1888).

[2] May I venture to protest against the extended use of the word oosperm sanctioned by my friend Prof. Haddon, who, in his excellent "Embryology," uses it (p. 91) for an advanced mammalian embryo with membranes?

kester's name *polyplast*, which is to be preferred to Haeckel's *morula*.

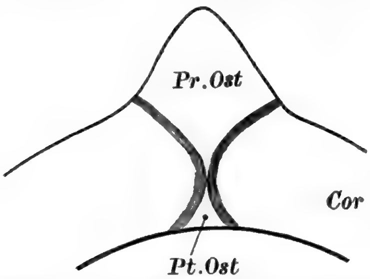
In mosses, the agamobium never gets beyond this stage, the fully formed sporogonium being nothing more than a highly differentiated polyplast. But as in animals the polyplast is succeeded by a gastrula, *i.e.* a stage in which the characteristic organ of animal nutrition has appeared, so in vascular plants it is succeeded by a stage in which the characteristic organs of plant nutrition—the leaf and root—have made their appearance. From ferns up to Angiosperms the first important differentiation after the polyplast is the formation of a cotyledon and of the primary root. This stage, the obvious correlative of the animal gastrula, I propose to call the *phyllula*.

XI.—ON THE PRESENCE OF A STERNUM IN *Notidanus indicus*.

As far as I am aware, nothing answering to a sternum has hitherto been found in fishes, the Urodela being the lowest group in which this constituent of the skeleton is known. In them it consists of a median plate of cartilage formed from paired chondrites (independent cartilaginous elements) developed in the inscriptiones tendineæ immediately caudad of the coracoids, and in close connection with them. In many Anuræ there is found in addition a median element cephalad of the procoracoids. The entire Amphibian sternum is conveniently called by Albrecht the *omosternum*, to distinguish it from the costal sternum of Amniota; the anterior division (omosternum, W. K. Parker) being called the *pre-omosternum*, the posterior division (xiphisternum, auct.) the *post-omosternum*. According to Wiedersheim ("Grundriss," 2te Aufl., p. 58), the phylogeny of the Amphibian sternum is still entirely unknown.

In Elasmobranchs the shoulder-girdle has the form of an inverted arch, usually formed of a single continuous cartilage, due to the union in the middle ventral line of paired elements, one connected with each pectoral fin. In Hexanchus (Notidanus) the right and left sides are described by Hubrecht ("Bronn's Thierreich," Fische, p. 77) as being united by fibrous tissues.

In a skeleton of *Notidanus indicus*, recently prepared for this Museum, the middle region of the shoulder-girdle has the structure shown in the figure. It is produced in front into a blunt



Median ventral region of the shoulder-girdle of *Notidanus indicus*, ventral aspect (natural size). *Cor.*, coracoid region: *Pr.Ost.*, pre-omosternum; *Pt.Ost.*, post-omosternum.

process, while it is evenly curved posteriorly. Two curved areas of fibrous tissue, with their convexities towards the median plane, extend from the anterior to the posterior border, touching one another in the centre, and thus bounding two distinct cartilaginous areas—an anterior (*Pr. Ost.*) of a rhomboid, and a posterior (*Pt. Ost.*) of a triangular form. The anatomical relations of these seem to show that the former is to be considered as a pre-omosternum, the latter as a post-omosternum.

The specimen in question appears to furnish a very good indication of the phylogeny of the sternum, and to show that it arose in the first instance by the segmentation of a mid-ventral element from the shoulder-girdle, in much the same way as the basi-hyal and basi-branchials are formed from the hyoid and branchial arches. T. JEFFERY PARKER.

Dunedin, N.Z., September.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Prof. M. J. M. Hill, of University College, London, has been approved for the degree of Doctor in Science.

Mr. H. W. Page, M.C., M.B., of Christ's College, has been appointed an additional Examiner in Surgery.

A. H. L. Newstead, B.A., of Christ's College, has been appointed by the Biology Board to occupy the University table in the Laboratory of the Zoological Station at Naples for six months from December 15, 1890.

Mr. J. Y. Buchanan, F.R.S., University Lecturer in Geography, announces a course of lectures in Physical Geography and Climatology for the ensuing Lent and Easter Terms.

The Clothworkers' Exhibition of fifty guineas a year in natural science for non-collegiate students will be awarded next July. Candidates are to apply to the Censor of Non-Collegiate Students.

The General Board of Studies recommend that Mr. R. T. Glazebrook, F.R.S., be appointed Assistant Director of the Cavendish Laboratory. Mr. Glazebrook resigns the Senior Demonstratorship which he has hitherto held.

A very interesting account is published in the *University Reporter* of December 9, 1890, of the course of study in natural science pursued by the University Extension students who were resident in Cambridge during the last Long Vacation. The course included invertebrate palæontology, practical chemistry, and physics. Courses were also given in Greek art, architecture, Egyptology, &c. The Lectures Committee recommend that the experiment be repeated on a larger scale next year.

Additional demonstrators in physiology and in experimental physics are about to be appointed, to be paid out of the laboratory fees of students.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for November 1890 contains:—On the structure of a new genus of Oligochæta (Deodrilus), and on the presence of anal nephridia in Acanthodrilus, by Frank E. Beddard (plates xxxii. and xxxiii.). This large worm, *Deodrilus jacksoni*, measuring thirteen inches in length by nearly half an inch in diameter at the broadest part, was collected by Prof. Moseley in Ceylon; the nephridia in a species believed to be *Acanthodrilus multiporus* are described as connected with the terminal region of the intestine.—Excretory tubules in *Amphioxus lanceolatus*, by F. Ernest Weiss (plates xxxiv. and xxxv.). At the Zoological Station in Naples, the author was able to have a constant and unlimited supply of Amphioxus, and he took the opportunity of experimenting with the view of determining whether the curious patches of modified epithelial cells on the ventral wall of the atrium of Amphioxus had any excretory function, as Johannes Muller had held to be probable; and whether, also, the atrio-cœlomic funnels, first described by Prof. Ray Lankester, had any such function; positive results were not obtained.—Studies in mammalian embryology; ii. The development of the germinal layers of *Sorex vulgaris*, by Prof. A. W. Hubrecht (plates xxxvi. to xlii.). Giving a brief but very lucid sketch of the various views held by recent writers concerning the gastrulation process of the Amniota and the formation of their mesoblast and notochord, placing what is agreed on on the one side, and indicating the points of difference on the other, without any polemical remarks, the author proceeds to an account of the early developmental stages of Sorex, the blastula and the didermic blastocyst, the development of the mesoblast; then follow some theoretical considerations on the gastrulation of the Mammalia, concluding with points of comparison in earlier investigations by other authors.—Terminations of nerves in the nuclei of the epithelial cells of tortoise-shell, by Dr. J. B. Haycraft (plate xliii.). The scutes of the land tortoise (*Testudo græca*), in spite of their hard, dense nature, form a very typical epidermic sensory covering for the animal. As in the soft skin of mammals, the nerves end in localized sensitive spots in the epidermis, and before penetrating this tissue they form a horizontal plexus in the upper part of the connective tissue; figures of the nerve-endings are given.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 27.—"The Variations of Electromotive Force of Cells consisting of certain Metals, Platinum, and Nitric Acid." By G. J. Burch and V. H. Veley.

The description of the apparatus, the capillary electrometer, and the method of working are given fully in the paper. The following conclusions are drawn from the results of the experiments:—

(1) When the metals copper, silver, bismuth, and mercury are introduced into purified nitric acid of different degrees of concentration, and a couple made of platinum, the electromotive force of such a cell increases considerably from an initial value until it reaches a constant and (in most cases) a maximum value. The rise of E.M.F. is attributed to the production of nitrous acid by the decomposition of the nitric acid, and the final value is considered to be due to the former acid only, while the initial value is due for the most part to the latter acid, though it is affected to a remarkable degree by the amount of impurity of nitrous acid, either initially present or produced by minute and unavoidable uncleanliness of the metallic strip and the containing vessel.

(2) If nitrous acid has been previously added to the nitric acid, then the maximum E.M.F. is reached *at once*.

(3) If the conditions—namely, increase of temperature, of impurity, and of concentration of acid—are such as would favour a more rapid solution of the metal, and consequently a more rapid production of nitrous acid, then the rise of E.M.F. is concomitantly more rapid.

(4) Conversely, if the conditions are unfavourable to the production of nitrous acid, the rise of E.M.F. is less rapid.

(5) If any substance, such as urea, be added which would tend to destroy the nitrous acid as fast as it may be formed, then the rise of E.M.F. is extremely slow, being dependent upon the number of molecular impacts of the nitrous acid upon the surface of the metal.

Thus the results obtained by the electrometer and by the chemical balance are in every way confirmatory the one of the other.

The authors propose to conduct further investigations on cells containing other acids, to determine whether the action of them upon metals is conditioned by the presence of their products of electrolysis.

Geological Society, November 26.—Dr. A. Geikie, F.R.S., President, in the chair.—The appointment of Mr. L. Belinfante as Assistant-Secretary was confirmed.—The following communications were read:—Account of an experimental investigation of the law that limits the action of flowing streams, by R. D. Oldham, Deputy Superintendent of the Geological Survey of India. A discussion followed, in which Dr. Blanford, Mr. Binnie, Mr. Topley, and the President took part.—On the rocks of North Devon, by Dr. Henry Hicks, F.R.S. During a recent visit to North Devon the author obtained evidence which has led him to believe that far too little importance has hitherto been assigned to the results of movements in the earth's crust as affecting the succession of the rocks in that area. The supposed continuous upward succession from the rocks on the shore of the Bristol Channel to those in the neighbourhood of Barnstaple, including, according to some authors, no less than ten groups, and classed into three divisions under the names Lower, Middle, and Upper Devonian, is, the author believes, an erroneous interpretation. The beds, he says, have been greatly plicated and faulted, and consequently several times repeated, and instead of being one continuous series, they occur folded in more or less broken troughs. In the Morte Slates, previously considered unfossiliferous, the author found a *Lingula*, and he believes that these slates are the oldest rocks in the area, and formed the floor upon which the Devonian rocks were deposited unconformably. As the result of movements in the earth's crust, the Morte Slates have been brought to the surface and thrust over much newer rocks, producing a deceptive appearance of overlying the latter conformably. The Morte Slates mark the dividing line between the two main troughs. On the north side in ascending order are the Hangman (or Lynton), Combe Martin Bay, and Ilfracombe Beds, and on the south side the Pickwell Down, Baggy Point, and Pilton Beds. Those on the south side of the Morte Slates are, the author believes, a repetition of the beds on the north side. The palæontological evidence is not antagonistic to this view, for an analysis of the Brachiopoda, the only group of fossils in the beds on the south side, which hitherto have been systematically examined, shows that of the twenty species mentioned by Mr. Davidson and others as occurring in the Pickwell Down, Baggy Point, and Pilton Beds (the so-called Upper Devonian rocks), no less than thirteen have already been found in the Middle or Lower Devonian rocks on the north side of the Morte Slates. Four others are recognized Middle Devonian species in other areas; and the three remaining are either doubtful species or ones which have a great vertical range. These facts show that the so-called Upper Devonian rocks in this area do not contain a distinguishing fauna of any importance; and the stratigraphical evidence is opposed to the view that they are a series of rocks distinct from those on the north side of the Morte Slates, which have been classed as Middle and Lower Devonian. In the discussion which followed the reading of this paper the speakers were Mr. T. Roberts, Mr. Marr, Prof. Seeley, Mr. H. B. Woodward, Rev. H. H. Winwood, Rev. G. F. Whidborne, Mr. Hudleston, Prof. Blake, the President, and the author. The President remarked that, in altering the recognized order of succession of these rocks, the author would have to reckon also with continental stratigraphers and palæontologists. Dr. Hicks had not made as clear as might be the evidence for such thrust-planes and faults as his views of the structure of the ground required. The plications and dislocations in South Devon and Cornwall, although abundant and often very complicated, appeared to the speaker to be on a comparatively small scale; and, although he did not wish to insist that this must be the case in North Devon, he felt that it might be so.—A special general meeting was held at 7.45 p.m., before the ordinary general meeting, at which Mr. J. W. Hulke, F.R.S., was elected Foreign Secretary, and Mr. J. J. H. Teall, F.R.S., was elected a Member of Council.

Zoological Society, December 2.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1890, and called special attention to the acquisition of a specimen of the Cryptoprocta (*Cryptoprocta ferox*) of Madagascar.—A letter was read from M. A. Milne-Edwards, containing an account of the mode in which the typical specimen of Grévy's Zebra had been mounted for the Gallery of the Museum, and pointing out that the mounted specimen has been carefully modelled after the living animal.—A letter was read from Dr. Emin Pasha, dated "Tabora, East Africa, August 16, 1890," containing an expression of his thanks for having been elected a Corresponding Member; and giving some remarks on the Striped Hyena of that district.—Mr. Richard Crawshay read a paper on the Antelopes of Nyassaland, treating especially of those to be met with west of the Lake. Lichtenstein's Hartebeest was stated to be very generally distributed, and seven other Antelopes to be plentiful. The Kudu, Sable Antelope, and Black-tailed Gnu were seldom met with; but exact localities were given where these Antelopes were to be found. In conclusion, the author added that there are at least two other species of small Antelopes found in the hills, which hitherto he had not been able to identify.—Prof. G. B. Howes read a paper on the peculiar mode of the suspension of the viscera in the Australian Batoid fish *Hypnos subnigrum*.—A second communication from Prof. Howes contained notes on the pectoral fin-skeleton of the Batoidea and of the extinct genus *Squaloraia*, which he maintained must be referred to the Chimæroid group.—Mr. G. A. Boulenger read a paper on the presence of pterygoid teeth in a tailless Batrachian (*Pelobates cultripes*), and added remarks on the localization of the teeth on the palate in the Batrachians and Reptiles.—Mr. H. Seebohm read a paper on the Fijian birds of the genus *Merula*, and gave a description of a new species from Viti-Levu, which he proposed to call *Merula layardi*.

EDINBURGH.

Royal Society, December 1.—Sir Douglas Maclagan, President, in the chair.—This was the first meeting of the Society for the present session.—After the delivery of the President's opening address, Prof. Crum Brown read an obituary notice of Prof. Kolbe.—Dr. John Gibson submitted the results of an analytical examination of manganese nodules, made with special reference to the occurrence of the rarer elements. The nodules which he examined were dredged by the *Challenger* in the North Atlantic. Dr. Gibson has detected the presence of twenty-eight elements. Of these, zinc, tellurium, molydenum, vanadium, and thallium have never previously been observed.—Mr. J. Y. Buchanan, F.R.S., read a paper on the occurrence of sulphur in marine muds and nodules, and its bearing on their modes of formation. The sand and mud at the bottom of the sea, at all depths, forms the food of a numerous and important ground-fauna. In the search after their food, the animals are continually passing large quantities of the mud through their bodies. Evidence of this is supplied by the coprolitic structure of all finer muds, which is rendered visible by careful levigation. The effect of trituration of silicates, under distilled water alone, has been shown to result in partial decomposition. When the milling apparatus is situated in the inside of an animal, and is therefore moistened by organic secretion as well as by sea-water, it is well-known that reaction, resulting in the reduction of the sulphates in the water, takes place. If the silicates, which are being triturated and partially decomposed, contain iron and manganese, the sulphides of iron and manganese are produced, and, being rejected by the animal, form a more or less bluish mud. When this mud lies on the surface of the sea bottom, and is exposed to the action of the sea-water, which always contains dissolved oxygen, the sulphides are oxidized, so that the reddish-brown surface layer, so frequently observed, is formed. When the sulphides come into contact with pre-existing higher oxides, such as Fe_2O_3, ferrous oxide is formed, and sulphur is separated. Reasoning on these grounds, Mr. Buchanan concluded that it must be possible to find free sulphur in all marine muds. In blue muds the sulphur is partly pre-existent, and is partly formed on drying in the air. In red clays, and in manganese nodules, it is wholly pre-existent. Examination of twenty-five muds, and of nodules from all regions, comfirmed this view, as they all gave up sulphur on exhaustion with chloroform. Mr. Buchanan made a number of experiments on the reaction of the sulphides of iron and of manganese on the ferric salts and oxide. He found that precipitated MnS acts on solutions of iron salts exactly in the same way as an alkaline sulphide does. It reduces ferric salts, and at once, and completely, precipitates ferrous salts as FeS. The reactions of MnS on ferric hydrates gave interesting indications regarding the probable nature of the "red cherty particles" so frequently observed in manganiferous bottoms. Mr. Buchanan concludes that the ochreous matter, hydrated oxides of iron and manganese, so abundant on the surface of the bed of the ocean, spring from silicates, and other combinations, by reduction to sulphides in a process of digestion and subsequent oxidation by the atmospheric oxygen of the sea-water. They may also be re-reduced and re-oxidized repeatedly.—Dr. John Murray communicated an anatomical description, by Mr. F. E. Beddard, of two new genera of aquatic Oligochætæ.—Mr. John Aitken exhibited and described a pocket form of his dust-counter, intended for meteorological observations. This form of the instrument is much reduced in size, and is greatly simplified in construction.

PARIS.

Academy of Sciences, December 1.—M. Duchartre in the chair.—On the Fourchambault tornado, by M. H. Faye. The author points out, among other things, that it will be extremely interesting to verify M. Dommet-Adamson's assertion that the gyrations took place in the same direction as the hands of a watch. This has also been noticed in some tornados in the United States, but is very rare.—On some new fossil plants observed at Portugal, and indicating the passage from the Jurassic to the Lower Cretaceous formation, by M. G. de Saporta.—Observations of Zona's new comet, made at Algiers Observatory, with the *coudé* equatorial of 0·318 metres aperture, by MM. Trépied, Rambaud, and Renaux. Observations for position were made on November 17, 18, and 20.—On a new method of displacement of a double cone, by M. A. Mannheim.—On the compressibility of mixtures of air and carbon dioxide, by M. Ulysse Lala. The author has experimented upon mixtures containing 11, 19·35, 26·29, 33·33, 40·08, 47·54, and 56·92 per cent. of carbon dioxide. Between the limits of his experimental pressures, viz. 100·38 cm. of mercury, with volume = 1, and 1613·96 with volume = 0·5, the compressibility of mixtures of dry air and carbon dioxide, when the quantity of the latter gas does not exceed about 22 per cent., is contained between those of the two gases used. It was observed that with mixtures richer in carbon dioxide the compressibility increased.—Reflection and refraction by bodies with abnormal dispersion, by M. R. Salvador Bloch.—On a new process for the differentiation of arsenic and antimony metallic mirrors produced on porcelain, by M. G. Denigès.—On an epithelial fibrillous tissue of Annelides, by M. Et. Jourdan.—Influence of acetic acid on gaseous respiratory changes, by M. Alfred Mallèvre.—On a new haemato-alkalimetric method, and on the comparative alkalinity of the blood of the vertebrata, by M. René Drouin. It is shown that if the alkalinity of the serum of the blood of different species of vertebrata be tabulated, the values may be divided into groups following precisely the same order as the zoological affinities, and that the order in which these classes succeed one another is the same as that of the increase of activity of respiration.—On the structure of nervous centres of *Limulus polyphemus*.—On the external differences presented by the *Nematobothrium, a propos* a new species (*N. Guernei*), by M. R. Moniez.—The enterocele nervous system of Echinodermata, by M. L. Cuénot.—Experimental researches on the locomotion of Arthropods, by M. Jean Demoor.—Comparative influence of light and weight on mosses, by M. Eugène Bastit.—On the presence of lactifers in Fumariaces, by M. L. J. Léger.—Principal indices of refraction of anorthite, by MM. A. Michel Lévy and A. Lacroix.—On the tempest of November 23 and 24, 1890, and the vertical movements of the atmosphere, by M. Alfred Angot.

STOCKHOLM.

Royal Academy of Sciences, November 12.—On the spectrum of absorption of brome, by Prof. Hasselberg.—Determinations of the longitude between the Observatories of Stockholm, Copenhagen, and Christiania, executed by C. F. Fearnley, F. C. Schjellerup, and D. G. Lindhagen.—On the recently executed expedition to Spitzbergen, conducted by G. Nordenskiöld, and its scientific results, by Baron Nordenskiöld.—On the results of the latest Swedish hydrographic expedition in Skagerack and Kattegat during the winter 1890, by Prof. O. Pettersson.—On floating pumices and slags stranded on the shores of North Europe, by Hr. H. Bäckström.—Mineralogical notes, iii., by Hr. G. Flink.—On the employment of infinite determinants in the theory of linear, homogeneous, differential equations, ii., by Hr. H. von Koch.—On a single case of permanent movement with rotation, by Hr. E. Phragmén.—On the waterspout at Wimmerby, July 4, 1890, by Hr. T. Wigert.—On the oxidation of phenyl-methyl-triazol-carbon acid, ii. Phenyl-triazol-dicarbon acid; *a*. The constitution of phenyl-triazol-carbon acid, by Dr. Bladin.—On the constitution of the cummenyl-propion acid, by Prof. Widman.—On the shifting from propyl to isopropyl in the cumin series of the latter, by Prof. Widman.—A review of the mosses of Småland, by Hr. R. Tolf.—Researches on the soft parts of the supernumerary radii of the hand and the foot, by Miss A. Carlsson.

CONTENTS.

THURSDAY, DECEMBER 18, 1890.

SHAKING THE FOUNDATIONS OF SCIENCE.

DURING the past twenty years there has been gathered together round the site of the 1862 International Exhibition a national collection of museums and schools for instructing the people in art, natural history, and in pure and applied science. For a time a senseless howl was raised that the whole thing was a mighty job to benefit a few, and the antagonism reached its climax when it was proposed to celebrate the year of the Queen's Jubilee by erecting the Imperial Institute on the "inaccessible" spot at which so many millions from all parts of London—nay, from all parts of Great Britain—had congregated to study fishes and food, microbes and music, inventions and instantaneous illuminations, the colonies and coloured fountains.

At length the more sensible of the critics began to realize that the supposed sinecurists at South Kensington were enthusiastic workers who, had they devoted their intellectual powers to the common occupation of enlarging their balance at the bankers instead of to the uncommon occupation of enlarging our knowledge of Nature, would have become capitalists whose vested interests would have been deemed more sacred than life or honour. As it is, the hosts of Mammon now threaten the domain of science in Exhibition Road, for it is actually proposed to make an underground railway, with trains running at frequent intervals, right under Prof. Rücker's laboratory, right under Prof. Norman Lockyer's observatory, and skirting the electrical research laboratories of the City and Guilds Technical College, called the Central Institution.

The English nation (for what concerns the development of our science concerns the nation at large) must abandon its oft-boasted claim of being a practical people. Germany will laugh us to scorn, France will hit us with an epigram, and Italy will view us with polite amazement. To the Royal College of Science come from all parts of our country science teachers to be taught; to the Central Institution come, in addition to English lads, graduates from colleges in Europe and America to learn how England teaches the application of science to industry. Shall they come to find a rumbling earthquake led from early morn to late at night close to the foundations of the very pillars that have been erected at considerable cost in order to secure for the instruments placed on them freedom from even the vibration caused by passing footsteps? On concrete foundations, some 13 feet below the level of the street, rest many pillars of 9 feet square, each quite separate from its neighbour and from the floor on which the students stand; and the upper portion of each pillar is stuffed with a thick cushion of wool, so that the instrument resting on it may give as unwavering a decision as does the Lord Chancellor on the woolsack.

And even this is not sufficient to secure all the quiet that Urania loves, for, just as earthquakes are foretold by the fluttering of the wings of birds, so the approach of a cart at the other end of Exhibition Road is foreshadowed, long before it can be heard, by the uneasy trembling of the very delicate galvanometer needles. Hence must Urania's attendants, the students engaged in research, worship at her shrine by night; and woe betide them if they happen to assemble when a "small and early" is held in Exhibition Road; for then must they cover their heads and mournfully depart, persecuted by the rolling brougham, heartbroken by the rumbling growler, and driven to despair by the rattling hansom. Even at the Cavendish Laboratory, in a quiet back street in the peaceful University town of Cambridge, Lord Rayleigh's most accurate work on the electric units had to be done at night; and Wheatstone's galvanometer magnetic needles at King's College followed the penny iron steamers during the day rather than the electric currents he was measuring.

Judge, then, what will be the effect of passing trains, even though they be drawn by no

> "Kittle of steäm,
> Huzzin' and maäzin' the blessed ground with the Divil's oän teäm,"

on the telescopes in Exhibition Road, which, besides other work, by long-exposure photographs have discovered for us new stars never yet seen in telescope by human eye; or on the galvanometers, whose ray of light aids all electrical industries by pointing out electric flaws in glass and porcelain which the microscope would mistakenly pronounce perfect.

England has just given the Rumford Medal to Prof. Hertz, of Bonn, for his splendid work on electro-magnetic radiation. Will England allow the short-sighted policy of Mammon—

> "Mammon the least erected spirit that fell from heaven"—

to prevent Prof. Boys continuing his work on the same subject at South Kensington? We honour Cavendish for weighing the earth: and now, when Boys has discovered how to hang bodies (not human) by quartz fibres, and so to weigh the earth with far greater accuracy than Cavendish could have done had his attracting balls been the size of houses, shall Boys be prevented from using his own apparatus? Shall the analysis of alternate-current waves at the Central Institution be interfered with by other waves set up by the new burrowing earthworm? We can hardly think so, when we remember how even a dear old soul has just received the news that an underground railway is to run close against our national science schools and laboratories. She, like Huxley's chum Gallio, cares for none of these things, but she had the sense to observe promptly, "Why, it will make all the things wriggle inside."

To study one set of vibrations in the laboratories in Exhibition Road, when another disturbing set is superimposed by the vibration of the building itself, will be like listening to a violin solo when a brass band is braying in the neighbourhood. A prince, the Duke of Edinburgh, renowned for his musical skill, and distinguished as a violin-player at the Albert Hall close by, might fitly take the lead in jealously preserving the possibility for scientific research, the importance of which his father would have been the first to recognize. Urania's voice is like Cordelia's, "ever soft, gentle, and low," and catch it the student never can if his beams of light when moving across his galvanometer and electrometer scales begin

> "To trip it as they go
> On the light fantastic toe."

Radio-micrometers, magnetometers, and every other meter the motion of whose index must be greatly magnified, will become simply museum specimens, illustrating the blindly commercial enterprise of Englishmen, who removed from Trafalgar Square the statue of Jenner, because he only showed us how to save countless lives in every country, and was not distinguished, like the people whose statues remain, by killing many to add to England's landed estate. The 3-foot telescope now being erected in Exhibition Road, will, of course, become useless when the earth-shaking train goes whirling underneath it. Why not leave it at once, like a broken monumental column, to mark the spot where lies buried many a noble hope?

But those who know, from past experience, how "great effects from little causes spring," already ask whether the scientific education of many, and the carrying on of scientific researches, such as have already been conducted in the laboratories in Exhibition Road, may not be worth more to the nation than any number of twopenny-ha'penny railway fares.

And others, whose only claim to be heard is the possession of that rarest of all senses—common sense—ask, Can it be possible that the nation, after having spent such vast sums on the erection and equipment of these laboratories, will now permit them to be rendered well nigh useless?

To move the laboratories of applied science is as undesirable as it is now impossible. Lord Carlingford, at their opening, congratulated the nation that a site had been selected for them close to the Museums, and stated that the French had even just moved the École Centrale of Paris, so that it might be placed in close proximity to the Conservatoire des Arts et Métiers. It is not likely that we shall now reverse our policy, divorce our Schools from our Museums, and, in securing such a result, waste all that has already been spent. If a railway is needed between South Kensington and Tyburnia, let it go under Queen's Gate, and not "Round by the Serpentine under the trees," as sung Lord Algernon. The Muse utters a cry of distress, let us join with her in shouting—

"Procul, oh! procul, este profani."

THE PALÆOZOIC FISHES OF NORTH AMERICA.

The Palæozoic Fishes of North America. By John Strong Newberry. (Monograph of the United States Geological Survey, No. XVI., dated 1889, issued August 1890.) Pp. 228, Pls. liii. (Washington: Government Printing Office.)

NEARLY all of importance that has hitherto been written concerning the Palæozoic fishes of the United States has proceeded from the pen of Prof. J. S. Newberry, of Columbia College, New York, whose numerous contributions during the last thirty years are to be found both in the American journals and periodicals, and in the beautifully illustrated volumes of a few of the State Surveys. Some of these contributions form extensive memoirs and are accompanied by numerous figures; but many are scattered notices without adequate illustration, relating to subjects worthy of much more elaborate treatment. The newly organized United States Geological Survey thus did good service to palæichthyology when it undertook, some time ago, to publish an extended monograph summarizing the whole of Dr. Newberry's researches and bringing them up to date. The result of the undertaking is the fine quarto volume now before us, which not only deals with forms already described, but also makes known a large amount of valuable new material, which the author has assiduously collected from various sources, and, for the most part, added to the unique collection in the Columbia College Museum.

The monograph occupies 228 pages, printed in the Survey's usual excellent style; and there are 53 plates, of which, unfortunately, little complimentary can be said. Eight of the latter are inferior process-reproductions of plates that have already appeared elsewhere, and many of the others are so carelessly produced by the same photographic method, that they give a very imperfect idea of the fossils they are supposed to represent.

Dr. Newberry treats the subject in stratigraphical order, and deals almost exclusively with the fishes of the Devonian and Carboniferous periods. Two pages only are devoted to the recent discovery of Upper Silurian fishes by Prof. Claypole; and no reference whatever is made to the few interesting Palæozoic types briefly described some years ago by Prof. Cope, from the Permian of Texas. The information concerning Devonian and Carboniferous genera, however, is most copious, accompanied by numerous references to literature; and we can only regret that the author did not continue his researches further into taxonomy, thus arranging the forms of each great period in some definite zoological order.

The limits of the Devonian age adopted by Dr. Newberry are somewhat different from those accepted by most American authors, and we doubt whether the inclusion of the Chemung and Catskill groups in the Carboniferous series will be generally accepted by either American or European geologists. The fish-fauna of each of these groups is certainly not Carboniferous, as ordinarily understood. Dr. Newberry considers that the Devonian series in the United States consists of the Oriskany (Lower), Corniferous (Middle), and Hamilton (Upper) groups; and the only fish-remains worthy of description are those from the two latter horizons. The account of the Corniferous fishes occupies 24 pages, and comprises little that is new, being almost exclusively a verbatim reprint from the Ohio Survey Reports of 1873 and 1875. A plate, illustrating several of the bones of the problematical Ganoid *Onychodus*, is the most important addition; and the remarks on the armoured fish *Macropetalichthys* constitute the most serious error that modern researches ought by this time to have eradicated. The fishes of the Hamilton group include more novelties, among which may be specially noted the triturating dental pavement apparently of an Elasmobranch (*Goniodus*), in addition to a new type of ribbed Ichthyodorulite (*Heteracanthus*), resembling in shape the well-known Carboniferous *Physonemus*.

The Chemung, Catskill, and Waverly groups constitute the Lower Carboniferous in Dr. Newberry's classification; and the series of limestones that contain the same fish-fauna as the lowest beds of the British Mountain Limestone are termed Middle Carboniferous. Indeed, looking at the subject from the point of view of physical geology, Dr. Newberry adopts a well-known principle, and also assigns the whole of the Carboniferous Limestone of Western Europe to the middle portion of the Carboniferous epoch, considering that this great division of time terminates with the upper limit of the Permian. The fishes of the Chemung and Catskill groups comprise, among others, the genera *Palædaphus* (*Heliodus*), *Phyllolepis*, *Bothriolepis*, *Onychodus*, *Holoptychius*, and *Glyptopomus*, all of which are essentially characteristic of the Old Red Sandstone and Devonian of Europe, and are never found in the lowest mechanical sediments of the Carboniferous system even in Scotland; while the remarkable new genus *Holonema* is more suggestive of a Devonian Coccostean than of any later type. The fishes of the Waverly group, on the other hand, agree in general character with those of the Calciferous Sandstone and Carboniferous Limestone series of Scotland, except in one particular—namely, the occurrence in certain American horizons of the remarkable gigantic "Placoderms," to which Dr. Newberry has given the names of *Dinichthys*, *Titanichthys*, *Trachosteus*, *Glyptaspis*, &c.

The detailed descriptions of these genera constitute the most important part of the present volume, and add most materially to existing knowledge of the subject. *Dinichthys* is essentially a huge *Coccosteus*, often with jaws two feet in length. *Titanichthys* is a still more gigantic fish, the head sometimes measuring four feet across, and the long, slender, toothless jaws were apparently provided during life with a horny sheath. Some of the remaining genera are also noteworthy for the elaborate ornamentation of their armour; while at least one other (*Mylostoma*) has loose dental plates very suggestive of those of certain Chimæroids and Dipnoi. The series of *Dinichthys* and its allies in the Museum of Columbia College is, indeed, one of the most remarkable exhibitions to be seen in the institutions of the United States, and deserves the closest attention of all interested in the earliest types of fish life.

A contemporaneous Elasmobranch, occurring in the same formation as *Dinichthys*, is also a most striking addition to the Palæozoic fauna; but it is not described, and only forms the subject of two unsatisfactory plates. The dentition is Cladodont, and the species is named *Cladodus fyleri*, while the anterior half of an allied fish is both described and figured under the name of *Cladodus kepleri*. This form of Elasmobranch has small pectoral fins, with an abbreviated basipterygium, and the cartilaginous rays all extending to the outer border; the tail is attenuated, diphycercal, with a pair of horizontal dermal expansions at its base; there are apparently no dorsal fin-spines; and the orbit is surrounded by a complete ring of four ornamented dermal plates. The fish is altogether different from any hitherto found in Europe provided with *Cladodus*-shaped teeth, and increases the already considerable difficulties surrounding the classification of the predaceous Palæozoic sharks.

The discovery of an undoubted *Rhizodus* in the Carboniferous Limestone of Illinois is another fact of some interest; though in this case, as in several others, the author unfortunately omits to mention that the fossil has already been described, without figure, in the publications of the New York Academy. Several fossil teeth and spines of Elasmobranchs are also discussed from the same formation; and the final section of the work is a brief summary of the fishes of the American Coal-measures, accompanied by a reprint of the author's memoir on *Edestus*, which appeared in the Annals of the New York Academy in 1888. The last-mentioned fossil is still problematical, but is considered to be most nearly paralleled by the group of spines met with upon the tail of the existing *Trygon*.

Dr. Newberry's researches have excited so much interest among geologists residing in the neighbourhood of Palæozoic formations in the United States, that Columbia College is the destination of nearly all new discoveries of Palæozoic fishes from every quarter. So much has thus accumulated since the completion of the manuscript for the present monograph, that we understand the author contemplates the issue of an extensive supplement. That such an additional contribution may soon appear will be the wish of all naturalists interested in this line of research.

A. S. W.

HAND-BOOK FOR MECHANICAL ENGINEERS.

Hand-book for Mechanical Engineers. By Prof. Henry Adams, M.Inst.C.E. (London and New York: E. and F. N. Spon, 1890.)

THIS useful book is practically a new and improved edition of Prof. Adams's previous work, "Notes in Mechanical Engineering" The older work has been recast, and much additional matter added to render it of more use to mechanical engineers and to raise it above the level of the ordinary text book. The author rightly observes that busy men must have facts and opinions put before them as briefly as possible, and this is his reason for condensing the information in the book to a compact yet clear form. The work is sure to be an acceptable one to mechanical engineers. The information has in most cases been taken from trustworthy sources, and these are duly acknowledged. It is a pity that the index has not been differently arranged, for there is nothing more annoying when picking up a book of this kind in order to obtain some particular formula or information than to find that it is necessary to settle in one's mind what section it is likely to be in, and then have to search down the columns for a likely paragraph. Had the index been an alphabetical one pure and simple, the handiness of the book would have been greatly enhanced. In some few cases it will be observed that data are quoted from amateur engineering papers. These may be perfectly correct; at the same time the "busy man" will probably doubt their value and trustworthiness.

The book covers an extensive field of mechanical engineering, most branches being well treated. Some of the information, however, is antiquated; for instance on p. 27, we are told that wrought iron is used for "rails." We presume railway rails are here meant. It must be very many years since the last iron rails were rolled for that purpose, and it is possible that there are no works left in this country where the old iron plant is in existence. On the same page it is stated that Yorkshire iron from Lowmoor, Bowling and other forges is used for tyres! This also is distinctly wrong. Who in the year 1890 would use iron tyres on railway rolling stock, when Yorkshire iron costs £20 or more a ton, and when steel tyres can be obtained for a fraction of the cost, without even considering the extra mileage obtained from the steel tyres? Prof. Adams tells us in paragraph 65 of the important process of casehardening wrought iron by heating it in contact with prussiate of potash, &c. In the opinion of many, ordinary bones used in the same way give far better results; and another way of obtaining the same result is to use wood charcoal, soda ash and a little lime. These give excellent results and are generally in use by manufacturers of locomotives and the like. Casehardening in many cases may extend to a depth of $\frac{3}{32}$ inch in important details of valve motions; pins and less important parts may do with a casing of $\frac{1}{16}$ inch in depth.

Paragraph 187 deals with cleaning castings, and here the author would put manufacturers "up to wrinkles," which they certainly are not ignorant of! He speaks of "Holes (we presume he means blow holes) stopped with black putty, cement, or lead." This is all very well in its way, but a casting requiring such treatment should be broken up and sent to the scrap heap. In paragraph 358 the author recommends that safety valves for boilers should have flat faces to prevent sticking. This is not the view taken by locomotive engineers. The most satisfactory valve is one with a conical seat fitted with three or four wings to keep it in position. The actual bearing of the valve on its seat should be about $\frac{1}{16}$ inch wide, or difficulty will be experienced in keeping it tight. In paragraph 369 new boilers are said to be tested to twice their working pressure in the best practice. The author must mean with hydraulic pressure, although he does not say so. This is certainly not the case with new locomotive boilers, nor is it to be recommended on any considerations. The bursting pressure required for any particular boiler can be approximately calculated near enough for all practical purposes, and with a suitable factor of safety allowed there is no necessity for this high test pressure on a boiler. To put a test pressure on a boiler much above its working pressure is to subject it to undue and unnecessary strains, and serves no useful purpose. The usual hydraulic test for a new locomotive boiler does not exceed 50 or 60 lbs. per square inch—about the working steam pressure. This is quite sufficient for the detection of bad workmanship, which is mainly the object of the test. The only satisfactory way to test a boiler is to have it periodically thoroughly inspected by a competent man. The mere testing a boiler with hydraulic pressure is no guarantee that the boiler is fit for work.

Paragraph 398, on the tractive force of a locomotive, is interesting. The reader is told that "the mean effective pressure on the piston is commonly assumed to be 85 per cent. of the boiler pressure." This wholly depends on circumstances. The speed of the engine and the cut-off have everything to do with the mean effective pressure. A heavy 6-coupled goods engine may occasionally exert a tractive force corresponding to a mean effective pressure of 85 per cent. of the working pressure, when starting a heavy train; but, as the speed increases, the cut-off is regulated by the driver and becomes earlier. With a working pressure of 150 pounds, with such an engine, and a cut-off of 30 per cent., the mean effective pressure becomes about 69 pounds per square inch, or about 46 per cent. of the working pressure. A mean effective pressure of 85 per cent. of the boiler pressure corresponds to a cut-off of approximately 70 per cent. of the stroke, or, in other words, the engine is in full gear. It is necessary to assume the highest possible mean effective pressure when the necessary weight on the driving or coupled wheels is being determined for adhesion, since the adhesive weight is in a fixed proportion to the maximum tractive force exerted; but for the purpose of determining the tractive force of a locomotive under varying conditions of speed and cut-off, the mean effective pressure of the steam in the cylinders will be found to be a varying quantity, as may be seen by studying the excellent paper on locomotives, read by the late Mr. William Stroudley before the Institution of Civil Engineers.

It is not necessary to say anything further on these points. The volume contains a mine of information. The theoretical portion is not unduly burdened with complicated formulæ. Those used are simple, and easily used by those not well up in the higher mathematics. Mechanical engineers have to thank Prof. Adams for putting within their reach a most useful book, clearly and concisely written, nicely printed and well bound, and one that certainly ought to be much used by mechanical engineers and those of the allied professions.

N. J. L.

FOSSIL FLORA OF AUSTRALIA.

On the Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania; with Special Reference to the Fossil Flora. By B. O. Feistmantel. Memoirs of the Geological Survey of New South Wales, 1890. (Sydney: Charles Potter, Government Printer.)

THIS work is a translation from the author's older work in the "Palæontographica," amended to 1887, edited by R. Etheridge, Jun., Government Palæontologist, and with notes by the Geological Surveyor-in-charge. The greatest development of the older beds occurs in New South Wales, where the coals, sandstones, and shales with plant impressions, are intercalated with porphyries to the thickness of 14,000 feet. Two remarkable facts give special interest to the beds: one is that under beds with Conularia, Spirifer, and Productus, believed to be Upper Carboniferous, certain plants of Mesozoic type appear; and the other, that just under these plant beds there are conglomerates regarded as having been deposited by the action of ice. The formations in New South Wales, Queensland, Victoria, and Tasmania, are described separately, but together they form a series; commencing with the Devonian Goonoo Goonoo, containing Lepidodendron; the Lower Carboniferous Lepidodendron beds; the Boulder beds, showing signs of glacial action, correlated with the similar Dwyka Conglomerate of Africa, and the Talchir Boulder bed of India; the lower marine beds of Upper Carboniferous age, followed by coal measures with Glossopteris, and an upper marine series; the Permian Newcastle beds with Glossopteris and heterocercal fish; the Hawkesbury Trias with

Labyrinthodonts; and lastly some beds of Jurassic age. Until the base of the Upper Carboniferous is reached there is nothing abnormal, but at this point occur the remarkable intercalations of glaciated conglomerates, of which we have some slight indications in our English Permian, and which stretch both to India and Africa. This climatic change killed off the Lepidodendrons, and introduced a new flora containing the wide-spread Glossopteris and its ally Gangamopteris, and Nöggerathiopsis, types which would in Europe appear more at home in the Rhætic than in the Carboniferous, and which followed the changing climate as it spread to other continents. Meanwhile, the Carboniferous fauna of the seas is unaffected, and heterocercal fish accompany plants of quite Jurassic character in the Permian. This mingling of newer land floras with older marine faunas seems so universal in extra-European geology, particularly in America, that it appears as if during period after period the development of marine life was especially forced or quickened in the area which is now Europe, while terrestrial plant life was retarded, especially in England. The genera Glossopteris, Gangamopteris, and Nöggerathiopsis do not extend up beyond the Permian in Australia. In India Glossopteris is rare in the Jurassic, but its recorded extension into the Cretaceous of Russia must be questionable, while its supposed occurrence in the Tertiary of Novale is certainly due to its having been confused with the common Tertiary, and still existing, *Chrysodium aureum*, identical with it in venation but quite different in fruiting. In addition to these, the most noteworthy species are the beautiful adiantoid Rhacopteris of the Lower Carboniferous, to which several plates are devoted, and the Osmunda-like Thinnfeldia of the Mesozoic. The flora of the latter is only remarkable for its very European facies, and is said to include a Jurassic Sequoia, Cunninghamites, Baiera, Walchia, Taxites, and several Cycadeaceæ; it also shares with the Permian the fine *Brachyphyllum australe*, Feistm. There are altogether about 129 species or varieties described, of which about 50 are illustrated, and the volume is certainly a valuable contribution to the history of plant distribution.

J. S. GARDNER.

OUR BOOK SHELF.

The Development of Africa. By Arthur Silva White. (London: George Philip and Son, 1890.)

THERE can be no doubt that Africa is destined to occupy a much more prominent place in the thoughts of Europeans than it has occupied hitherto. The recent marking-off of vast "spheres of influence" may not have very important immediate results, but sooner or later attempts will certainly be made on a great scale to find out in these regions new channels for industry and commerce. It is evident, therefore, that the conditions of the development of Africa ought to be carefully studied; and in the present work, Mr. White supplies all the materials, so far as they are now known, for an adequate comprehension of the subject. He begins with a bird's-eye view of the continent, showing its geological and physical structure, its oceanic and inland drainage-basins, and the coincidence of political settlement with oceanic drainage-areas. Next he deals with the geographical distribution of the chief mountain-systems, and the consequent development of the great river-systems, in relation to accessibility from the sea and internal communications. Under the heading of "climate and cognate phenomena" he treats of the distribution of temperature; actual temperatures; the distribution of atmospheric pressure, and prevailing winds; annual rainfall; distribution of soils; zones of vegetation; distribution of animals; classification of climates; and acclimatization. Then come chapters on the indigenous populations, Islam and Christianity, the traffic in slaves, the progress of exploration, commercial resources, the European domination, and political partition. In the concluding chapter the author presents the general principles underlying the development of Africa along natural lines, derived from an examination of the various aspects under which the continent is known to Europe at the present day. He displays a thorough mastery of the facts relating to the various questions he discusses, and his work will be of genuine service to all who may be for any reason, whether theoretical or practical, interested in the utilization of Africa's material resources. The volume is accompanied by a most valuable series of maps, specially designed by Mr. Ravenstein.

The Pinks of Central Europe. By F. N. Williams, F.L.S. 66 pages, with 2 plates. (London: West, Newman, and Co., 1890.)

THIS forms a third instalment of Mr. Williams's studies of the genus Dianthus. His first paper was devoted to an enumeration and classification of all the known species. In his second he described the pinks of Western Europe, and traced out in detail their synonymy and geographical distribution. In the present paper he deals in a similar manner with the species that inhabit Central Europe. For Central Europe as a whole he claims 76 species out of a total of 230, which are disposed through the different countries as follows: viz. Austria, 59; Roumania, 24; Servia, 22; North Italy, 17; Switzerland, 15; Germany, 11; Poland, 7; Denmark, 5; and South Sweden, 4. His descriptions are clear and concise, but he has followed the Continental authors in classing as species many types which most English writers would place as varieties. None of the species are new, but it is a great convenience to have them all brought together, and treated on a uniform plan. The book is dedicated to Cardinal Haynald.

Materials for a Flora of the Malayan Peninsula. By Dr. G. King, F.R.S. No. 2. 93 pages. (Calcutta, 1890.)

THIS is a second part of Dr. King's Flora of the Malay peninsula, reprinted from the Journal of the Asiatic Society of Bengal. It covers the orders Bixineæ, Pittosporeæ, Polygaleæ, Portulaceæ, Hypericineæ, Guttiferæ, and Ternströmiaceæ. These were dealt with in Hooker's "Flora of British India" in 1872-74. Since that time a large amount of new material has been accumulated, principally by Kunstler, Scortechini, and other collectors whom Dr. King has sent out. Out of the orders just enumerated the first and two last are well represented in the Malay peninsula. In Bixineæ, Erythrospermum, known before only in Mauritius and Ceylon, is now recorded from Perak, an endemic species. In the same order the two curious Malayan genera, Teraktogenes, of Hasskarl, and Ryparosa of Blume, are added to the Indian flora, and four species of the former, and six of the latter, all endemic, are here for the first time described. In Polygaleæ there are ten new species of Xanthophyllum. In Garcinia, which has lately been fully dealt with in Pierre's "Forest Flora of Cochin China," fifteen new species are described out of a total of thirty-six now known in the Malay peninsula. In Calophyllum there are six new species, in Kayea four, and in Ternströmiaceæ two new species of Adinandra, one of Ternströmia, one Eurya, one Actinidia, two of Pyrenaria, and three of Gordonia. The publication of Hooker's "Flora Indica" has given the Indian botanists a firm platform to work upon, and it is very gratifying to find that so much has been done within so short a time, and that the working out of the new material has fallen into such competent hands.

The Colonist's Medical Hand-book. By E. A. Barton. (London: Cassell and Co., 1890.)

THE author explains that this little volume has been "written expressly for the use of colonists and squatters, who are entirely out of reach of medical assistance." A more suitable book of the kind could not be at their disposal. They will readily understand his directions, and in recommending appliances for the treatment of emergencies he has taken care to refer only to such as are likely to be found in any squatter's hut.

LETTERS TO THE EDITOR.

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

Dr. Romanes on Physiological Selection.

AS Dr. Romanes now declares that the essence of his theory of physiological selection is "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species," and that the occurrence of infertility among the members of an undifferentiated species is secondary and comparatively unimportant, I ask leave to quote one or two more of his original statements, in addition to the four emphatic passages quoted in my communication of November 27.

(1) "When accidental variations of a non-useful kind occur in any of the other systems or parts of organisms, they are, as a rule, immediately extinguished by intercrossing. But whenever they happen to arise in the reproductive system in the way here suggested, they must inevitably tend to be preserved as new natural varieties, or incipient species. *At first the difference would only be in respect of the reproductive system; but eventually, on account of independent variation, other differences would supervene, and the new variety would take rank as a new species*" (NATURE, vol. xxxiv. p. 316).

The words I have italicized show clearly that variation in fertility only was what Dr. Romanes then claimed as essential to his theory. Again, after referring to variations in the season of flowering as a "well-known and frequently observed cause" of isolation, he adds:—

(2) "But it is on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection" (*l.c.*, p. 337).

The meaning of this is still further enforced by other passages. After discussing the supposed causes of infertility, he says:—

(3) "Why should we suppose that, unlike all other such variations, it can never be independent, but must always be superinduced as a secondary result of changes taking place elsewhere? It appears to me that the only reason why evolutionists suppose this is because the particular variation in question happens to have as its result the origination of species" (*l.c.*, p. 339).

And again:—

(4) "It appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that *the cases where it has been superinduced by the secondary distinctions are comparatively few in number*" (*l.c.*).

Notwithstanding the passages I have now quoted, emphasizing eight times over, in different ways, that the theory is essentially one of variations as regards fertility and sterility alone, Dr. Romanes now says that, even if all this is wrong, "the principle of physiological selection, as I have stated it, is not thereby affected." If this is not an absolute change of front, words have no meaning; and it is further shown to be so by the fact that Dr. Romanes acknowledged that Mr. Catchpool had "very clearly put forward the theory of physiological selection." But Mr. Catchpool clearly distinguished between the old theory that species arise *first* by variation in form and structure, and only *gradually* become mutually infertile, and the new theory that they arise "by spontaneous variations in the generative elements, and are in this case *originally* mutually infertile, but only *gradually* become otherwise divergent" (*l.c.*, vol. xxxi. p. 4).

That this was the essential and original "physiological selection," that was claimed as supplying the missing link required to make the origin of species by natural selection a reality, is yet further shown by the repeated statements that physiological "selection" is a powerful preservative agent. Besides the statement already quoted, that variations in fertility "cannot escape the preserving agency of physiological selection," we have the assertion, quoted above, that such variations "must inevitably tend to be preserved as new natural varieties or incipient species," and the following still more emphatic assertion:—"Neither are we concerned with the degrees of sterility which the variation in question may in any particular case supply. For whether the degree of sterility with the parent form be originally great or small, the result of it will in the long run be the same: the only difference will be that in the latter case a greater number of generations would be required in order to separate the varietal from the parent form."

Now my contention has always been, and still is, that there is no principle at work which can accumulate or even preserve the variations of infertility occurring in an otherwise undifferentiated species, and that the term physiological "selection" is therefore a misnomer, and altogether misleading. If Dr. Romanes will carefully work out numerically (as I have attempted to do) a few cases showing the preservative and accumulative agency of pure physiological selection within an otherwise undifferentiated species, he will do more for his theory than volumes of general disquisition or any number of assertions that it *does* possess this power.

My next contention is, that this is the only new part of his theory—as he himself shows by his reference to the ordinary view, of sterility following other changes, as that which "evolutionists suppose." All the rest is to be found more or less fully discussed in Darwin's works; and I myself claim only to have carefully studied Darwin's facts, and his brief but most suggestive discussion of them in his chapter on "Hybridism" (vol. ii. of "Animals and Plants under Domestication"), and by arranging them more systematically to have shown that they do really give a fairly consistent and sufficient solution of the problem. The only part of my work I claim as a distinct addition to the theory is the proof that, under certain conditions that appear to me probable, natural selection *is* capable of increasing incipient infertility between distinct races or varieties; and the same view was submitted to Darwin twenty years ago.

Lastly, I totally and emphatically deny that any portion of my facts or conclusions on the subject were derived from Dr. Romanes's writings on "physiological selection." The only two sentences he has quoted from my book to prove that I have done so merely express what he himself has declared to be the common opinion of evolutionists, and which is also the direct outcome of the facts collected by Darwin. If this is "the whole essence of physiological selection," then physiological selection is but a re-statement and amplification of Darwin's own views, since he certainly assumed that "some amount of infertility" characterized "some varieties" of animals and plants, and that this infertility, when it occurs, is of some use in preventing the swamping effects of intercrossing. I feel sure that if *this* had been stated, at the outset, to be what was termed "physiological selection," no discussion would have arisen as to the principle involved, but only as to its novelty and as to the appropriateness of the name given to it.

If now, notwithstanding his repeated and emphatic statements that variation as regards fertility in otherwise undifferentiated species was what constituted the basis of his theory of physiological selection, Dr. Romanes continues to assert that I have adopted that theory "purely and simply, without any modification whatever," it will show that our respective standards of scientific reasoning and literary consistency are so entirely different as to render any further discussion of the subject on my part unnecessary and useless.

ALFRED R. WALLACE.

A Large and Brilliant Fire-ball Meteor.

ON Sunday night, December 14, between 9h. 40m., and 9h. 45m. G.M.T., I had the good fortune to witness the display of a most magnificent fire-ball meteor. It rose rapidly with a bright blue trail from an altitude of 6° above the horizon, at a point 7° south of west, and in about 7 seconds of time attained a culminating altitude of 55° at a point 19° north of west. Two large trees interlaced prevented me from seeing the head of the meteor till shortly before its culmination, but the light given out by it soon after its first appearance was equal to that of the full moon, and at culmination it much surpassed the light of a full moon in a cloudless sky. The ball seemed to be of a most dazzling bluish-grey colour, and it had a diameter of at least three-quarters of a degree. The disk presented a nebulous appearance with radiations within it as from a centre, but was well defined, except on its lower edge. The glare was almost too much for eyesight, and although the night was very frosty, calm, and clear, all the stars in the west became invisible. I turned to look again very shortly after, and at 5 seconds from culmination found the meteor had become a small yellow ball only one-twelfth of a degree in diameter, and was dropping ruddy sparks. It then disappeared at an altitude of 23° towards a point about 51° north of west. My impression was that this meteor was at no great distance from this place in any part of its course (lat. 51° 20′ N., long. 3m. 0s. E.). I noted the positions relatively to trees and tall shrubs, and measured them exactly with a theodolite this morning. To-day I hear that the fire-ball was seen to fall by a man in Chestnut Street, a hamlet on the Maidstone road, and then appeared quite close to him. The direction of fall accords fairly well with my own observation, and would make it descend about two and one-third miles from me. At culmination I should say it seemed very much nearer to me, considering especially its great apparent size at that time. I heard not the slightest noise either of rushing or bursting.

A. FREEMAN.

Murston Rectory, Sittingbourne, Kent, December 15.

ON Sunday, the 14th inst., about 9h. 45m. p.m., I was entering my house by the back door, when the whole place was so brilliantly illuminated that I momentarily supposed there had been a flash of lightning. That erroneous impression was at once removed by the continuance of the light. Wheeling round, I saw a splendid meteor of the fire-ball type, descending obliquely through the sky. Though the Monday newspapers reported serious fog in London on the previous day, yet the night sky at Loughton was perfectly clear; and it was easy to see that the meteor in its descent was passing a little to the right of the constellation Gemini, in a direction nearly, but not quite, parallel to a line joining Castor and Pollux. The head, which was downwards, was a large oval mass of light. The tail was not a mere thread of silvery radiance, like those of November 1866; it seemed broad, irregular on the edges, and sending out sparks. The fire-ball had not descended far when it vanished among a shower of sparks, which also very speedily disappeared. I heard no rushing sound during its course, and no noise of an explosion when it came to its end.

As the time available for observation extended to only a few seconds, it is possible that there may be some error of detail in the foregoing statements. I shall not, therefore, call them observations, but mental impressions of what took place.

ROBERT HUNTER.

Forest Retreat, Staples Road, Loughton, Essex,
December 16.

Attractive Characters in Fungi.

IN the communications which have recently appeared in your journal on this subject, it has been taken for granted that, in the development of *spores* into *mycelium*, the former must necessarily pass through the body of an animal host. We have no scientific evidence of this. I am inclined to think that the theory is a remnant of the old superstition that toadstools are the result of the excrement of toads, and that we must seek for more natural processes of fertilization if we are to solve the mystery. Unbelief is sometimes the nearest road to a right faith. Scepticism is often the gate to truth. It is at least desirable that those who are investigating the subject should approach it unfettered by a theory which is yet destitute of proof, and should direct their researches to ground which is not littered with what may be only the fragments of an exploded superstition. My own observations tend to convince me that germination of the spore and development of the mycelium are alike dependent upon conditions of soil, modified by atmospheric influences, fertilizing agents, &c., &c. It may be added that the solution of the problem is rendered more difficult by the apparently inexplicable fact in Nature, so strikingly exemplified in the field of mycology,

"that of fifty seeds
She often brings but one to bear."

The countless millions of spores which never reproduce their kind must be transmuted into other conditions of being, to form part of the "living whole" of the universe, in which nothing can be lost.

J. S.

Glamis, December 5.

Some Habits of the Spider.

IT would be strange indeed, if, as your correspondents infer, there is no record of the gyrating habit of a species of geometric spider so common as even to be well known to Londoners who have a garden. It may be that Kingsley referred to this species, but his "Water Babies" is not found in scientific libraries. It occurred to me, last September, whilst amusing myself by making some of these spiders gyrate, by blowing on or gently touching them, that the instinct, in this kind, is in a decadent state; it does not appear well suited to a heavy-bodied, sharp-legged species like this one, and is certainly much less perfect than in other species possessing a similar habit.

Some years ago, when describing the habits of some Argentine spiders, I mentioned a species of *Pholcus*, abundant in La Plata, with legs of extraordinary length, in colour and general appearance something like a crane-fly, but double the size of that insect. When approached or disturbed in any way, it gathers its feet in the centre of the web, and swings itself round and round with the rapidity of a whirligig, so that it appears like a very slight mist on the web, and offers no point for an enemy to strike at. Here the correspondence between structure and habits is very perfect; the slimness and great length of the legs causing the creature, at the moment the swift revolutions begin, to seem to disappear from sight; and, owing to the string-like form of the legs, the fatigue experienced is probably very much less than the action would cause in a stout short-legged spider like the English species. At all events, it can revolve for fifteen or twenty seconds at a stretch; and, if the cause of alarm continues, it will perform the action no less than three times before quitting the web. The English spider exhausts itself in a few seconds.

Those of your readers who are interested in the habits of spiders will find the paper referred to in the *Gentleman's Magazine* for 1884. Some of my observations contained therein, I find, have been served up—after a fashion, and (of course) without acknowledgment—in a spider article in the current number of *Longmans' Magazine*.

W. H. HUDSON.

"Nowhere can Mathematics be learned as at Cambridge."

THESE words are ascribed to Dr. Hopkinson in his speech at the Royal Society on behalf of the medallists; and as they are calculated to sustain a belief which, Heaven knows, is already widely enough prevalent among even tolerably well-informed people in England—the belief, namely, that no one but a Cambridge Wrangler is worth thinking of as a teacher of mathematics—perhaps you will allow me to enter a protest.

In the department of applied mathematics, thanks to the work of Thomson, Tait, and Clerk Maxwell, Cambridge is supreme, and so far Dr. Hopkinson is right. The weakness of the Cambridge system has always been on the side of geometry; and I am sure that those who studied this subject under Hamilton, Salmon, and Townsend (to go no farther back), will agree with me in saying so. It is a weakness apparent in almost every work emanating from Cambridge. What would the subject of conic sections, for example, be, if Salmon had not shown how it should be treated? The answer is easily supplied by some of the Cambridge works on the subject which are still in extensive circulation in England. As a result of the belief which Dr. Hopkinson seeks to strengthen, the schools and Colleges throughout the whole country are becoming dominated by Cambridge methods exclusively, all appointments in them being virtually filled with Wranglers and no others; and hence we shall have, in time, a dead-level of sameness of method and thought which will partake of whatever shortcomings may characterize the Mathematical Tripos. Is this result desirable?

GEORGE M. MINCHIN.

R.I.E. College, Cooper's Hill, December 11.

THE DARKNESS OF LONDON AIR.

ONCE more the inhabitants of London are suffering from the inconvenience, that a darkened atmosphere naturally causes. This darkened atmosphere usually commences towards the end of October, and continues off and on sometimes to the end of February—that is, for a period of some sixteen weeks we partially and at times wholly deprive ourselves of the full benefit of the sun's rays, and live in a state of artificial darkness; and when we consider how very little daylight there is during the winter months (even under the best of conditions), it is surely very foolish to reduce the daylight, and create darkness in place of the daylight.

Is anything being done to remedy this state of affairs?

Apparently very little is being done to make matters better, whilst on the other hand a good deal is being done to make things worse; for instance, each year sees thousands of new buildings added to London, many of these buildings being factories and workshops.

We have now (within the Metropolitan area) some 765,000 tenements, and at present (with the exception of a few Acts dealing with factory smoke), the countless chimneys of these numerous buildings are permitted to belch forth volumes of sooty smoke, almost without any check whatever. As for the great majority of the buildings in London (that is, the dwelling-houses), we positively have no means of controlling their smoke.

Under these circumstances it is hardly very surprising, that the atmosphere over London in the winter is so unnaturally dark, and it is further not to be wondered at that we have to expend (or rather waste) vast sums of money in supplying ourselves with artificial light, to enable us to carry on our daily business. It is needless to point out that this outlay would be unnecessary, if we were really to adopt means to deal effectively with this smoke nuisance.

In NATURE, vol. xxxix. pp. 441 *et seq*, various results were given, which were intended to show, in an easily-understood manner, how dark the London air is during the winter-time, owing to smoke fog; it was also shown that other large towns suffer from the same cause.

It is now proposed to give some of the results of a few more observations, taken in London during the winter of 1889-90.

Table I. gives the results obtained in two districts in London, one being at the West End and the other at the East End, about 9 miles apart.

TABLE I.

District in London in which the chart was kept.	December 1889.	January 1890.	February 1890.	Total number of hours during which artificial light was used.
Hammersmith, W.	10	3½	3	16½ hours
Homerton, N.E.	68	38	9½	115½ hours

It will be seen that at Homerton the inhabitants lived for about 115½ hours (during the three months named) in a partial state of darkness during the day-time, and if we consider how short the average length of each day is at this time of year, we find that out of the 90 days concerned, nearly 14 days were practically turned into nights, causing both inconvenience and expense.

During the month of December 1887, and the months of January and February 1888, the number of hours when artificial light was used at Homerton reached the total of 67¼ hours.

TABLE II.—*Chart for Black or Dark Yellow Fog.*

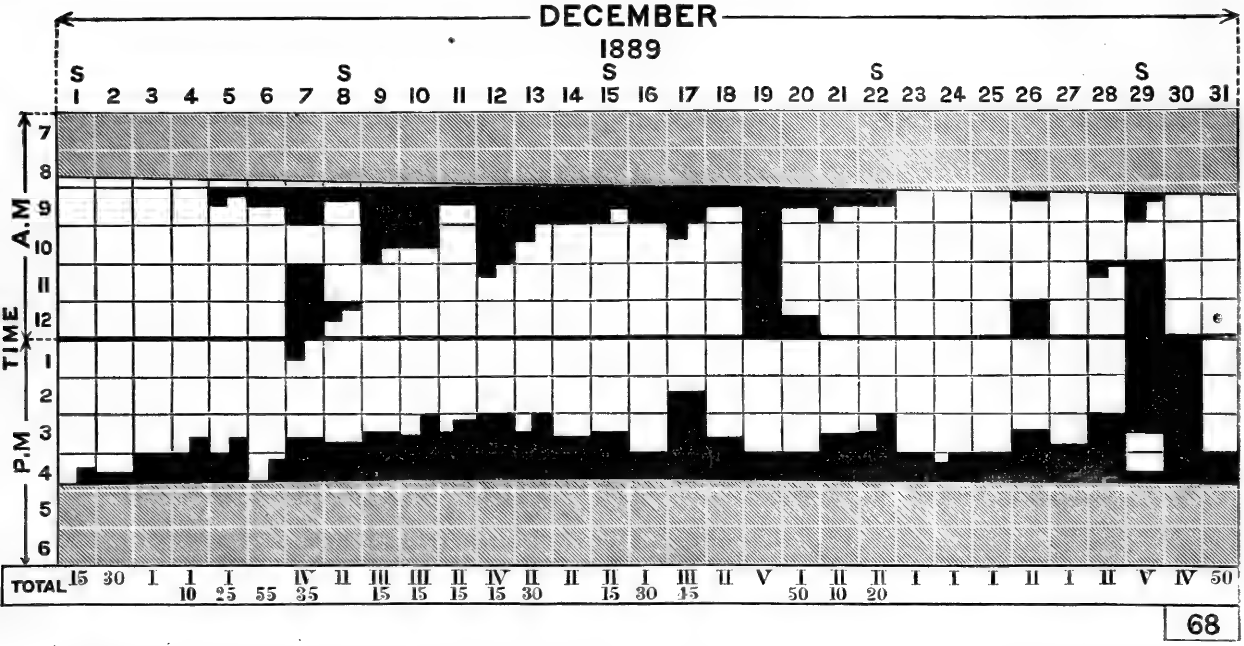


Table II. shows in detail, the kind of chart used to record the darkness caused by smoke fogs. I am indebted to a friend for partly suggesting the form of chart which is given in this table.

The original chart (only a portion being given here) was divided into 2160 squares, every 24 of these squares representing one day, and each single square one hour; these single squares are again subdivided into four parts of 15 minutes each.

The grey colour shows the night-time, the white the day-time, and the black denotes the time each day during which partial darkness prevailed owing to fog.

To show the serious way, in which this artificial darkness of the London air interferes with the numerous kinds of work, that depend upon a clear atmosphere and good light, various records were kept during the winter of 1889–90.

The results of two of these records are given below.

The photographer is, perhaps, one of the greatest sufferers from this artificial state of darkness during the winter-time.

There are various methods in use by which photographs are reproduced, and most of these methods depend on good natural light; Table III. deals with the "silver process."

TABLE III.—*Photography (Silver Process).*

	November 1889.	December 1889	January 1890.	February 1890.	March 1890.
Number of days on which observations were taken in each month	26	24	27	24	26
Number of days when no copy could be printed, owing to the darkness of atmosphere	7	10	7	3	5
Longest time taken to print a single copy in one day [1]	5h. 10m.	5h. 45m.	4h. 40m.	4h. 45m.	3h. 0m.
Shortest time taken to print a single copy in one day...	2h. 5m.	3h. 35m.	2h. 15m.	1h. 25m.	1h. 5m.
Average time taken to print one copy in so many days in each month	Days 19 — Time taken $3\frac{5}{19}$h.	Days 14 — Time taken $5\frac{3}{14}$h.	Days 20 — Time taken $3\frac{4}{5}$h.	Days 21 — Time taken $3\frac{1}{21}$h.	Days 21 — Time taken $1\frac{3}{7}$h.

[1] (*a*) In fine clear weather, it takes about 30 minutes to print a single copy. (*b*) Rain was the cause of no copy being printed on certain days; this remark applies to Table IV. as well.

These observations were taken at Ealing, London, W. It is hardly necessary to point out that Ealing is far away from the centre of London.

The record was kept daily (Sundays and Bank Holidays excepted) during the five months named in Table III.

It will be seen, that the shortest time taken to print a single copy on one of the 127 days in question, was about twice as long as it would have taken if the atmosphere had been clear, &c.; it will also be seen that, out of the 127 days on which observations were taken, on no less than 32 days no copy could be printed at all.

Table IV. deals with a method of copying drawings known as the ferro-prussiate process; this process is very extensively employed by architects, engineers, &c.; it absolutely depends upon a clear atmosphere.

TABLE IV.—*Ferro-Prussiate Process for Copying Drawings.*

	November 1889.	December 1889.	January 1890.	February 1890.	March 1890.
Number of days on which observations were taken	21	15	15	17	22
Number of days when no copy could be printed owing to the darkness of atmosphere	9	9	5	4	6
Longest time taken to print a single copy in one day [1]	7h. 30m.	4h. 15m.	4h. 0m.	1h. 30m.	0h. 35m.
Shortest time taken to print a single copy in one day	0h. 25m.	1h. 20m.	0h. 15m.	0h. 5m.	0h. 4m.
Average time taken to print one copy in so many days in each month	Days 12 — Time taken $2\frac{1}{6}$h.	Days 6 — Time taken 3h.	Days 10 — Time taken $1\frac{4}{5}$h.	Days 13 — Time taken $\frac{8}{13}$h.	Days 16 — Time taken $\frac{1}{4}$h.

[1] In fine clear weather it takes about 4 minutes to print a single copy.

The observations were taken at New Cavendish Street, Portland Place, W.

Out of the 91 days on which a record was taken during the five months named in Table IV., on 33 days it was found impossible to print a single copy of a drawing, and on one day only could a copy be printed in 4 minutes, and this was on March 14, 1890; about 4 minutes is the shortest time possible, that a clear copy can be printed in.

It is, perhaps, unnecessary to give any more results in this paper, to help to prove the existing state of artificial darkness caused by these smoke fogs; but would it be asking too much, if we were to request the London County Council to take steps at once to enforce rigorously the existing Acts of Parliament, which have been framed to deal with factory smoke, and to consider (in a comprehensive and thorough manner) the best means of coping effectively with the smoke nuisance generally?

W. HARGREAVES RAFFLES.

VIBRATION-RECORDERS.

AN interesting paper, by Prof. John Milne, F.R.S., and Mr. John Macdonald, was read before the Institution of Civil Engineers on November 25. The authors presented an account of certain instruments which had been designed to register the oscillations and vibrations of trains, so as to give an automatic record of the run, and information as to the condition of the track. These instruments were modified forms of seismographs, the ordinary earthquake instruments being affected so much by the suddenness of the jolts, and by changes in inclination, as to be unsuitable for this purpose.

The apparatus for recording vertical movements consisted of a clock-spring coiled upon an axle, and connected with a lever carrying a weight in such a way that when the spring was wound the weight was supported by it. The result of this combination was, that for up and down movements of the apparatus some point in the weight remained at rest, and the relative motion of the apparatus and the weight was recorded by means of a pointer attached to the lever.

Horizontal vibrations were registered by the movement of pointers attached to two pendulums, the planes of motion of which were at right angles to each other. The simplest form of pendulum was a metallic cylinder, free to swing on pivots placed on its upper edge. The oscillations of the pendulum might be controlled by giving a certain frictional resistance to the movements of the pointers, as, for instance, by increasing the pressure upon the writing point. Another method of making the vibrations of the pendulum dead-beat, by coupling together two pendulums with a sliding joint, was also shown.

The recording-surface might be a drum, covered with metallic or ordinary paper, and driven by clockwork, the pointers being strips of metal, pencils, or pens; and to obtain a record extending over a considerable period a long band of paper rolled on a drum, as in the Gray-Milne seismograph, was pulled over another drum and wound up on a third, driven by its own system of clockwork.

Reference was made to fifty-eight locomotives now in Japan, on which experiments were carried out. In England, diagrams had been taken in carriages running on the London and North-Western, the Caledonian, the Great Northern, the South-Eastern, and the Metropolitan Railways; and in America a diagram, the original of which was exhibited, was taken, showing the motion of a carriage between New York and San Francisco, a distance of about 3300 miles.

As the diagrams produced by the vibration-recorder were evidently connected with the balancing of the engines, details of the weights of reciprocating parts and the balance-weights were given, together with the equivalent balance-weight, which was determined experimentally in the following manner. A pair of wheels, with their balance-weights, and crank-pins, were placed on the rails; a large round steel ring was then hung from the crank-pin, and from this weights were suspended until a balance was obtained. This weight hung upon the crank-pin was called the equivalent balance-weight, and it was found that when this weight was nearly equal to that of the reciprocating parts—that was to say, when the horizontal component of the energy due to the reciprocating parts was balanced—the diagrams for longitudinal motion were small.

Most of the diagrams were taken on locomotives running on the line between Tokyo and Yokohama, a distance of 17¾ miles, and they showed that there was a relationship between the character of the line and the recorded movements. Soft ground could be distinguished from hard ground, and the effects of bridges and culverts were recorded, all diagrams on the same line showing the same characteristics.

As an illustration of the use of the instrument, two diagrams were shown, taken on different engines running over the line from Shinbashi to Yokohama, the paper moving at the rate of 1 inch to the minute. It was pointed out that, as Shinagawa was approached, the diagram became larger. This was explained by the fact that this portion of the track rested on the mud of Tokyo Bay, and was, therefore, soft and yielding. At the third minute after starting, a sudden movement of the pointer occurred as the engine passed one of the culverts, but the cause of this jerk had not been determined. The train was seen to have left Shinagawa two minutes late, and to have stopped at Omori a minute and a half, instead of one minute, the schedule time. Between Omori and Kawasaki there were irregularities indicating soft places, but the most important mark occurred in the second span of the 40-feet girders forming the waterway leading up to the large bridge at Kawasaki. This mark was found regularly in all the diagrams, and a staff of workmen who knew nothing of the experiments having been sent to examine the place, reported that a sleeper on the second span of the down track yielded, when a train passed, twice as much as any of the other sleepers. Between Tsurumi and Kanagawa the diagram was rather larger, indicating a soft place that required attention. Yokohama was reached four minutes late, the places where time was lost being easily determined upon a close examination of the diagram.

The record of longitudinal motion was found to show most clearly differences in balancing, the size of the diagrams upon engines which differed only in the balancing having been as 6 to 27. A striking feature in the diagram of longitudinal motion was that it indicated ascents and descents by deviating to the right and left of a median line. Curves were marked in a similar manner by the portion of the instrument recording transverse motion.

The observation that certain locomotives gave a larger longitudinal diagram than others suggested that such engines were not exerting their power in an economical manner. It was found that the engines which had used the least coal per mile were those in which the difference between the weight of the reciprocating parts and the equivalent balance was small; and, on the contrary, in those engines where the difference was large the consumption of coal and oil was considerably greater. It occurred to the authors that engines yielding different diagrams might show a difference in the wear of their tires. The data obtainable were meagre, but they appeared to indicate that those engines which gave a small diagram for longitudinal motion did not wear out their tires so quickly as others.

The conclusions arrived at by the authors were:—(1) The vibration-recorder might be of value to those who had to deal with the management of the traffic, inasmuch as it furnished details of the times of stoppage and the speed at any part of the run. (2) The instrument might be used by those who had to inspect lines. Variations due to carelessness on the part of the plate-layers were recorded. Curves, ascents and descents, and even slight variations in grading, were indicated; faults in sleepers, irregular yieldings on bridges, soft portions of the track, and other imperfections, were definitely marked; and changes in the permanent way could be at once detected if such diagrams were taken at intervals. (3) The vibration-recorder might furnish information of value with regard to the manner in which a locomotive should be balanced, the vibrations due to this cause being measured by the diagrams for longitudinal and vertical motion. For this purpose, diagrams might be taken on a surface running at the rate of 1 inch per second. In this case each vibration of the locomotive was recorded separately, and from these diagrams the maximum acceleration of each backward and forward motion had been calculated.

GLACIAL CLIMATE.[1]

EVERY fragment of evidence, which can serve to show us the character of the climatal conditions during the last glacial period, is so important that I venture to present certain facts which, so far as I am aware, have hitherto escaped attention. The evidence I mean to discuss is found in America and Europe in the regions immediately south of the glaciated areas of the two continents. It is a well-known fact that in the present condition of the climates of the earth, the decrease in temperature as we rise above the sea is about 3° F. for each 1000 feet of altitude. Local circumstances may considerably affect this variation, but the range is not great. If glaciers were by the refrigeration of the climate restored to the surface which they occupied during the last ice period, we should expect to find the line of perpetual snow rising as we went southward about 3030 feet for each degree of latitude.

If, on an inspection of the areas glaciated during the recent ice epoch, we should find that this principle in the distribution of the glacier did not hold, we should apparently be justified in the supposition that the glacial climate was not due to greater cold than that which exists at present. Any departure from the normal rate of ascent of the perpetual snow-line in the region south of the glacier would be likely to throw some light on the climatal conditions prevailing during the time when the continental ice-sheets were developed.

Beginning our inquiry with the Appalachian section of Eastern America, we find there a region in many ways well suited for the determination we seek to make. The principal front of the ice stretched across the continent on a line which is now well determined. It is unmistakably evident that it crossed the valley of the Ohio at Cincinnati, and extended a little distance south of that stream into Kentucky. I have recently re-examined the evidence which goes to show the presence of the ice at the above-named point, and have no doubt as to the goodness of the determination. At this point the surface of the country lies at a height at no point exceeding 900 feet above the level of the sea. From this position the level of the country gradually rises in a southerly direction until in the synclinal mountains near Cumberland Gap it attains a height probably exceeding 3500 feet. From this elevation the profile descends in the broad valley of the Upper Tennessee to about 1000 feet above the sea-level; thence it again rises until, in the mountains of North Carolina, we enter a field where many peaks rise to more than 6000 feet in height. From the front of the ice-sheet near Cincinnati to the central part of the North Carolina mountain district is about 200 miles. It is to be observed that the whole of this district is within the same great valley, and in a region where the isotherms at the present time follow each other with normal curves. We may therefore fairly conclude that, under the usual conditions of climate such as prevailed in North America, the ice-line should be found in the mountains of North Carolina at the height of 2000 feet above the base of the glacier at Cincinnati, or, say, at 3000 feet above the level of the sea. From that level to the top of the North Carolina mountains, or, say, for the height of 3500 feet, we should have indications of glacial conditions. A tolerably careful investigation of this country has shown me no evidence of ice action whatsoever, and all the other students of the subject who have visited this area have failed to find any facts which might afford even a supposition of glacial work in that field. I am therefore compelled to assume that the slope of the snow-line rose so rapidly from the ice-front at Cincinnati southward that it passed above the summits of these mountains.

If the elevation of western North Carolina was in the form of an isolated peak, we might have less confidence in this indication. But the district of land which should have lain much above the snow-level is some thousand square miles in area, a field sufficiently great to have developed very extensive glacial areas in case the peaks lay above the line of perpetual snow. The same considerations, though in a less accented way, are met when we examine the highlands of the Blue Ridge in Virginia, or the Alleghany Mountain district on the uplands of Virginia and West Virginia. A large part of the Blue Ridge in Virgina is high enough to have been the seat of glaciers, provided the snow-line were anywhere near the level of the glacial sheet where it crossed the existing Atlantic coast. The traces of glacial work in the Blue Ridge are extremely scanty. At the western extremity of Rock Fish Gap, immediately south of the Chesapeke and Ohio Railway, near its junction with the Shenandoah Valley Railway, there are accumulations which apparently are to be classed as glacial. This point is about 1600 feet above the level of the sea. If this accumulation be really of glacial origin, it apparently establishes the height of the ice-front in the Shenandoah, but as yet I must regard the indication as somewhat questionable. In the Alleghany Mountains west of Covington, Va., there are deposits which I am disposed to consider of a glacial nature. At this point the deposits lie about 2000 feet above the sea-level. These are the southernmost points at which I have found any satisfactory indications of glacial work, in the region south of the Potomac, and, until further investigated, both of these deposits must be regarded as of doubtful character.

In Europe, in the region south of the Alps, we find the facts similar in their character to those existing in North America. During the last glacial period the ice-sheet extended down on to the Italian plains, unquestionably attaining levels less than 1000 feet of altitude above the level of the sea, and probably occupying positions not more than 500 feet above that level. From my observations on the field I am disposed to think that the general mantle of the ice covered the southern face of the Alps down to within a few hundred feet above the sea. From 150 to 200 miles south of the Alps in the mountains of Tuscany, we have an extensive surface rising 4000 or 5000 feet above the sea. A careful search over much of this field showed me no evidence of occupation by ice. At the present rate of rise in the perpetual snow-line in Switzerland we should expect an ascent of that plane about 1500 feet in passing from the foot of the Alps to the Apennine Mountains north of Florence. We have thus a case similar to that we find in the North Carolina mountains, in which there are elevations just south of the continental glaciers of a sufficient height to have been covered by ice under normal circumstances, but where the evidence of such coating is conspicuously wanting.

I have endeavoured to apply the same considerations to the glacial phenomena of the Rocky Mountains, but the facts are as yet so imperfectly in hand that I have not been able to determine the relative altitude of the sheet in a satisfactory manner. This, however, may be said: the distinct glacial accumulations in Colorado probably do not extend below the level of 6000 feet. As this region is about on the parallel of the mountains of western North Carolina, it may perhaps indicate that the snow-line lay throughout the southern parts of the United States above the summits of the Carolina mountains. It seems to me, however, that in the existing state of our knowledge of the distribution of the glacial sheet in the Cordilleran section we cannot attach much importance to this evidence.

We have now to consider the possible explanation of the facts above adduced. Assuming that the relative height of the surface occupied by the glacier, when it crossed the Ohio River, and that of the region within two hundred miles south of it, even during the ice epoch,

[1] By Prof. N. S. Shaler. Reprinted from the Proceedings of the Boston Society of Natural History, vol. xxiv. parts 3 and 4, May 1889–April 1890.

were what they are at the present day, it at first sight seems necessary to suppose that there was a rapid change in the temperature in passing from the ice-front towards the Gulf of Mexico. Before we adopt this consideration, however, we must bear in mind the fact that the ice-sheet of the last glacial period probably advanced for a considerable distance south of the perpetual snow-line, in substantially the same way in which an Alpine glacier descends in many cases to a depth of 1000 feet or more below the fields of enduring snow by which it is fed. Accepting the elevation of the continents as they now exist, and allowing 3° of temperature for each 1000 feet of altitude, it seems likely that the snow-line just touched the summit of the Carolina mountains and came to the surface of the sea near the southern end of Hudson's Bay. In other words, the protrusion of the ice to the south of this glacial snow-line carried it at a distance of near 1000 miles south of the gathering ground. This supposition, however, is of little value, for the reason that the level of the continent was clearly much disturbed during the glacial period, the surface declining to the northward within the glacial envelope, and probably rising to the southward of the ice-front.

It seems to me most likely that during the occupation of the northern part of the continent by glaciers, the southern portion of the continent was considerably elevated. All the streams which discharge into the ocean south of the former ice-front between New York and the Rio Grande show in their lower parts only moderate accumulations of alluvium which has been deposited since the close of the glacial period. They generally enter bays which appear to be the lower parts of gorges which were formed during the period when the area was more elevated than it is at the present time. These facts make it probable that if the mountains of North Carolina varied in elevation from the present height, they were more elevated than at this day. All the facts are against the supposition that we can explain the absence of glaciers in their highlands by supposing that the summits were lower during the ice period than they now are.

It seems to me we are compelled to suppose that the climate in the mountains of North Carolina, and probably in the great portion of the Apennine section south of the Alps, had during the glacial period a temperature not much if any lower than they have at the present time. As far as it goes, the evidence is thus opposed to the supposition that the glacial period was brought about by a general refrigeration in climate of the continents occupied by the sheet.

Within the basin of the Ohio, especially in the valleys of the Upper Tennessee system of waters, we find certain phenomena which lead us to the conclusion that the rainfall in a recent period, probably contemporaneous with the glacial epoch, was more considerable than at the present day. In many valleys which I have observed in that section the *débris* built into the imperfect alluvial plains is of a much coarser nature than that now brought down by the rivers The channels bear the aspect of having recently been the seat of more voluminous streams than now occupy them. This evidence, gained from many points in the Southern Appalachians, leads me, independently of the hypothesis I am now suggesting, to the conclusion that during the last glacial epoch the rainfall of this country was much greater than it is at present. At Big Bone Lick in Kentucky, which lies within a few miles of the southern boundary of the ice-sheet, excavations made by me in 1868 show embedded in the deposits formed by the springs an abundant set of herbivorous mammals, including the mastodon and elephant, an extinct species of buffalo, and a musk-ox kindred to our Arctic species but of much larger size, a species of carabou, indistinguishable from our living American forms. The conditions of this deposit led me to suppose that these animals were probably not more ancient than the glacial period, and that they most likely occupied the surface during the time of abundant rainfall when the marshes were more extensive than at present, a period which if not exactly coincident with the extreme advance of the ice must fall within the glacial epoch.

The abundance of these large Herbivora, the relatively great size of the species, point also to the coincident occurrence of a rather abundant vegetation. If the period indicated by the massive gravels of the torrential streams and the Herbivora of Big Bone Lick be identical, and if this period coincides with the glacial period, as it appears to do, then we may fairly assume that the climatal conditions immediately to the south of the glacial sheet were not those of extreme cold. This evidence has nothing like the sure foundation obtained by the lack of glaciers in the mountains of North Carolina, but as far as it goes it confirms the results of those observations.

It is not my purpose, however, in the present writing to consider the perplexing question as to the cause of glacial climate. I desire only to call attention to the extent to which our glacial streams appear to have advanced, by what we may term forced marches, far to the south of the line of perpetual snow. Although the value of the evidence above noted cannot be determined until the matter has been more carefully brought together and abundantly discussed, the facts seem to me to militate against any hypothesis which seeks to account for the glacial period on the supposition that the climate in the glaciated regions was cooler than at present.

THE SUBJECT-MATTER OF EXACT THOUGHT.[1]

WHEN mathematicians, logicians, and other exact thinkers think and reason, they think and reason about something. What is that something? And wherein consists the infinite variety which it presents? Is it a mere assemblage of detached subjects of entirely different natures? Or is it an harmonious whole, admitting of a definition and treatment which, though perfectly general, will yet preserve the essential characteristics of its component parts?

To judge by the usual habit of thought on these questions, we ought apparently to conclude that the former is the correct hypothesis; but that such a conclusion would be wholly erroneous, there can, I think, be no doubt whatever. The prevailing view is due, I believe, to the want of a proper appreciation of the difference between that which is the essential or necessary matter of exact thought, and that which, so far as the processes of reasoning are concerned, is merely the dress in which that necessary matter is clothed. This dress has, of course, a real importance of its own, and the study of it is not to be undervalued. But when we are investigating the subject-matter of exact thought it is not with it that we are concerned: it is not with the case, but with the works of the clock that we have to do; and thus our anxiety should be to get rid of the environments, to treat them as the "disturbing agents" of the experiments of the physicist, as likely to mislead, and therefore to be eliminated with the most scrupulous care.

That such dress exists in the case of every subject that we investigate is obvious enough; much of it is recognized

[1] The object of this paper is to set forth in as simple and non-technical a manner as possible, the principles which were first formulated in my "Memoir on the Theory of Mathematical Form," published in the Philosophical Transactions of the Royal Society for 1886, vol. clxxvii. p. 1, and a "Note" thereon contained in the Proceedings of the Royal Society, vol. xlii. p 193. which corrects the Memoir in some important particulars. The special applications of the theory principally dwelt on here are considered in detail in a recent paper by me, "On the Relation between the Geometrical Theory of Points and the Logical Theory of Classes," contained in the Proceedings of the London Mathematical Society, vol. xxi. p. 147. The mode of treatment of the whole subject adopted in the present communication must however be regarded as entirely new.—A. B. K.

without an effort. Thus the geometrician must draw the lines of his diagrams of some colour, but never dreams of supposing that the particular colour affects the real matter he is investigating; and the logician, bidding us fix our attention on the relations of implication, contradiction, &c., of the passages he selects for analysis, properly rejects the poetic beauties or philosophic truths they may enshrine as matters with which he is not for the moment concerned. But though there is a great deal of this accidental environment which is readily seen to be such, and is therefore rejected without difficulty, there remains much which, unless attention is expressly directed to its discovery and rejection, is likely to be regarded as the kernel itself, and not as mere husk. In innumerable instances this separation of the essential from the non-essential has, I am satisfied, not yet been effected. That this is so is, to a very considerable extent, due to the practice, no doubt for some purposes a necessary one, of dividing the study of the subject-matter of exact thought into different sciences, such as logic and mathematics, each with its own literature and students, the latter generally misinformed as to the real nature of those subjects which are not their special study. By this divorce of studies the student of either has the dress in which his particular subject is clothed so invariably associated with its essential elements that he fails to regard it as a dress at all, and looks upon it as a part of the naked body itself. No wonder then that such attempts as have been made to define the nature of the subject-matter of certain sciences are in general vague and metaphysical, rather than exact and mathematical; or that there are those who are content to say that logic is the science of "quality," mathematics of "quantity," and therefore their domains are, and should be kept, distinct. Such persons would no doubt be surprised to learn that mathematics is no more the science of quantity only than physiology is the science of the arm or leg; and that the algebra which expresses the relations of quantity is but a drop in the ocean of algebras which the mathematician can point to; another drop, be it observed, being the algebra which expresses the logical relations of classes or propositions to each other.

The essential elements of the subject-matter of exact thought are in reality of an extremely simple character; and, though they exhibit infinite variety, that variety is due to simple and easily defined causes. There is nothing vague or metaphysical about them; but, even when mere figments of the brain, they are precise and definite, with parts and properties which can be analyzed and catalogued, just as much as if they were the elements of a chemical compound, the wheels of a watch, or the organs of a vital structure. Let me try to show that this is so.

I will begin by considering and comparing the essential matter of two "branches of science," which will, I think, be regarded by most persons as of quite different characters, and as very properly relegated to separate and distinct treatises. I refer to the geometrical theory of points, and the logical theory of statements. The investigation will, I hope, fully prepare the way for an acceptance of the general definition of the subject-matter of exact thought which will follow.

I take points first. The geometrician deals, of course, with many things besides points, viz. lines, planes, curves, surfaces, &c.; but points may be considered alone, and it conduces to much greater clearness of ideas to confine ourselves to one species of entity, instead of introducing several into our field of view. I deal therefore with points only. And first let us consider individual points. Euclid says that they have neither parts nor magnitude; but these facts, however true and interesting, are, so far as the exact thought of the geometrician is concerned, beside the mark, and introduce considerations which tend to divert our attention from that which is really essential. When these immaterial considerations are swept on one side, all that we can say about points as individual entities is that they are all exactly like each other, are undistinguished from each other; no remark can be made of one which is not equally true of every other. But there are innumerable other entities about which the same thing can be said; and yet these do not possess those properties which characterize points. To say that points have *positions* is no explanation: it merely leads us to consider positions, and these are really the same things as points. Thus far, then, we have not obtained much insight into the essential characteristics of a system of points.

Let us go a step further and consider *pairs* of points. But even this will help us but little, for pairs of points are geometrically speaking, undistinguished from each other, just as single points are. No remark can be made about any one pair of points which is not equally applicable to every other pair. It is true, no doubt, that in some pairs the points are further apart than in others; but in considering distances we are not considering properties of individual pairs of points, but the relations of certain pairs to each other; and when we consider the relation of one pair of points, a, b, to another pair, c, d, we do not deal with peculiarities of the individual pairs a, b, and c, d, but with a peculiarity of the tetrad of points, a, b, c, d. To put the matter in another way, the geometrical properties of a diagram do not depend upon its size; whether it be enlarged or diminished they remain the same.

We are still, then, as much in the dark as ever as to what there is about a system of points, which gives to it the properties which are discussed by the geometrician; and we must advance yet another step, and consider *triads* of points. And here at last light breaks in upon us. Unlike single points and pairs of points, triads of points are not exactly like each other, and remarks may be made of certain triads of points, which, though true of some others, are not true of all. In the first place, there is the great division of triads of points into *collinear* triads, *i.e.* triads consisting of three points through which a straight line could be drawn, and *non-collinear* triads through which no such line could pass. This division is of fundamental importance, and the consideration of it introduces us to the whole of those geometrical properties which, for reasons into which I need not now enter, are termed "projective." But besides this division into collinear and non-collinear triads, there are apparently subdivisions of the triads of each sort, arising from the differences which may exist in the relative distances of the three points in a triad from each other—in the *shape* of the triangle which they form. The geometrical properties which depend on such subdivisions are known as "metrical" properties. We need not, however, separately consider these subdivisions; for, thanks to the researches of Poncelet and Cayley, we now know that the consideration of "projective" properties comprehends the study of those which are "metrical"; and thus a complete discussion of the results which flow from the division of triads of points into collinear and non collinear triads, would include a full examination of those which arise from metrical subdivisions. In fact, when we consider the metrical properties of triads of points we are not considering triads at all, but collections of a larger number of points, arrived at by adding to the triads certain other points technically known as "the absolute." If we consider the triads alone and apart from "the absolute," *i.e.* if we consider their projective properties, we have only two sorts of triads to deal with, viz. collinear and non-collinear. All the triads of each sort are precisely alike in properties, nothing can be said of one of the triads of one sort which is not true of each of the others of that sort; but every collinear triad differs radically from every non-collinear triad.

All, therefore, that we need now concern ourselves with is the division of triads of points into collinear and non-collinear triads. Now, though the distinction between collinear and non-collinear triads is of fundamental importance, it is not the collinearity and non-collinearity in themselves which are the subjects of consideration by the geometrician. The fact that in a collinear triad the three points are characterized by a symmetry or straightness which is not found in triads of the other species is, so far as the geometrician is concerned, a matter of indifference; it is a piece of accidental clothing which is not an essential part of the subject-matter with which he is dealing. He is concerned with the fact that triads of the one species differ radically from those of the other; but not with the fact that they differ in respect of straightness. With what, then, is he concerned beyond the mere fact that they differ? The answer is, that it is with the way in which the triads of the two species are distributed through the whole system of points; and with the fact that this distribution is not a random one, but is regulated by definite laws, so that if we are told that certain triads are collinear or non-collinear it necessarily follows that certain others are also collinear and non-collinear respectively.

The laws regulating this distribution may be stated as follows:—

Law I.—If the two triads *a*, *b*, *p*, and *c*, *d*, *p* are collinear triads, there exists a point *q* such that the two triads *a*, *c*, *q* and *b*, *d*, *q* are collinear triads.

Law II.—If the two triads *a*, *b*, *c* and *b*, *c*, *d* are collinear triads, so also are the triads *a*, *c*, *d* and *a*, *b*, *d*.

Law III.—No point is absent from the system whose presence is consistent with the foregoing laws.

The distribution of the triads of the two species through the system in accordance with these laws completely determines it as one possessing all those properties which are usually studied by the geometrician. I insert the limitation "usually," because there are certain matters sometimes considered which require other circumstances than those previously referred to to be taken into account. To these I shall return later.

It necessarily, of course, follows from this definite distribution of the distinguished and undistinguished triads that there exists a similar regularity of distribution of distinguished and undistinguished collections of any number of points. But, in addition to this, there also exists a like regular distribution of what may be termed "aspects" of those collections, about which I must say something.

By an "aspect" of anything in the ordinary sense of the word, we mean the appearance which it presents under conditions. If the conditions are altered, we have different aspects of the thing. Thus, a thing presents different aspects when viewed from different standpoints, or when placed among different surrounding circumstances. The conditions may be purely mental. Thus, when we consider the relation of one of a collection of objects to the other objects of the collection, we mentally attach a different degree of prominence to the former object from that which we assign to the latter objects, and we have a particular aspect of the whole collection in view. An aspect of a collection of entities will also be obtained if we regard the entities of the collection as taken in a particular order. It is with aspects of this latter description that we are here concerned. It must not, however, be supposed that the *order*, in the sense of *succession*, is of any importance so far as we are here concerned. All that is material is the fact that the entities are conceived of as taken first, second, third, &c., and that to be taken first is different from being taken second, and so on, the nature of the difference being immaterial; so that an aspect of the sort considered would equally well be obtained by conceiving that each entity of the collection is marked with a different mark of any sort. It is, however, convenient for my purpose to consider the aspects obtained by supposing the entities of the collection to be taken in a particular order; among other reasons, because such aspects admit of being represented in an extremely simple manner; viz. if *a*, *b*, *c*, *d*... represent the entities of any collection of entities, we may represent the various aspects of that collection by arranging those letters in rows, thus *abcd*..., or *bdca*.... Aspects may be different, and yet be undistinguished from each other. Thus, by looking at a sphere from different points of view we obtain different aspects of it, but one of these may be exactly like another. So in the case of an aspect of a collection of any number of points; for example, to take the simplest case, of a pair of points, *a*, *b*. Here the two aspects *ab* and *ba* are different aspects of the pair, but they are undistinguishable from each other; nothing can be said of a pair of points taken in one order which is not true of the pair taken in the reverse order. In other words, a pair of points is *symmetrical*. On the other hand, if a collection of points is unsymmetrical, *e.g.* if it consists of a collinear triad, *a*, *b*, *c*, and one point, *d*, which is not collinear with *a*, *b*, *c*, then we can always find two aspects of the collection which are distinguished from each other, *e.g.* in the given case the aspects *abcd* and *abdc* are distinguished from each other.

The consideration of these aspects of collections of points is of much use in the discussion of the properties of a system of points. To take one simple example. Suppose we were told that the collection of four points, *p*, *q*, *r*, *s*, is undistinguished from the collection *a*, *b*, *c*, *d*, which we have just been considering. Then, though we know that three of the points *p*, *q*, *r*, *s* compose a collinear triad, we do not know which three; but if we are told that the aspects *abcd* and *prsq* are undistinguished from each other, then we know that, since *a*, *b*, *c* is a collinear triad, *p*, *r*, *s* is one also.

Every collection of points has, then, a number of aspects, arrived at by taking the points composing it in every possible order; and of these aspects of the various collections which compose a system of points, some are distinguished from each other, and some undistinguished; and these distinguished and undistinguished aspects are regularly distributed through the system, just as the distinguished and undistinguished collections are distributed. This regular distribution of the aspects is determined by the distribution of the collinear and non-collinear triads of points in accordance with the laws which I have given, and on that distribution alone.

Owing to this regular distribution of the distinguished and undistinguished collections, and aspects of collections, which compose a system of points, that system possesses a specific character or "form," as it may be termed. It is to the possession of this "form" that the system owes its various properties as studied by the geometrician, and it is this "form," and that only, which is the real subject-matter of his exact thought in the study of a system of points.

I do not propose to show here how the various properties of a system of points necessarily follow from the laws which I have given as defining its "form," or how it is that such a system contains sets of points having the characteristic properties of such as lie on straight lines, curves, surfaces, &c. These are all matters of detail involving no new questions of principle, and can be worked out without difficulty by anyone who has a knowledge of modern projective geometry. It is sufficient for my purpose to have indicated the position which "form" occupies in the geometrical theory of points.

Before, however, I pass away from the consideration of points, I must call attention to one or two matters with regard to them, which, as I have already indicated, may on occasion have to be considered by geometricians, and are not covered by the foregoing treatment.

I have hitherto assumed that the coincidence of points implies their identity, and in most geometrical investigations this is taken to be so. We may, however, regard coincidence as amounting merely to equivalence, *i.e.* we may regard coincident points as such that each bears precisely the same relation to all other points.[1] Where a system of points thus includes equivalent points, pairs of points are of two sorts, viz. we have equivalent pairs and non-equivalent pairs. Equivalent pairs are distributed through the whole system of points in accordance with the following law :—

If each of the pairs *a*, *b* and *a*, *c* is an equivalent pair, then the pair *b*, *c* is an equivalent pair.

Another matter to which reference should be made is this. The laws which I have given as defining the "form" of a system of points define that form by specifying the mode of distribution of the collinear and non-collinear triads, *i.e.* of the various *collections* of three points to be found in the system. Nothing is said about the *aspects* of those triads, and thus we do not know, in the case of any collinear triad, which is the mean point of the triad, and which are the extremes—which point lies between the other two. In general it is not necessary that we should know this; in a vast number of geometrical investigations it is wholly immaterial. In certain cases, however, the matter is of importance; and, as its consideration brings out a very interesting and important fact as to the relation between points and statements, I must not pass it over. Here the laws which define the distribution of the collinear triads must not only specify what triads are collinear, but also which is the mean point and which the extremes in each. It must, however, be noticed that the fact that in a collinear triad one point "lies between" the other two is not a material circumstance; all that is of importance is that the one point bears a different relation to the triad from that which is borne by the other two points. It is convenient to denote a collinear triad in which *b* is the mean point, and *a* and *c* the extremes, by the symbol *ac.b*. The laws of distribution of the collinear triads may then be stated thus :—

LAW I.—If we have the collinear triads *ap.b* and *cp.d*, a point *q* exists such that we have the collinear triads *ad.q* and *bc.q*.

LAW II.—If we have the collinear triads *ab.p* and *cp.d*, a point *q* exists such that we have the collinear triads *aq.d* and *bc.q*.

LAW III.—If we have the collinear triad *ab.c*, and *a* and *b* are equivalent points, all the three points, *a*, *b*, *c*, are equivalent.

LAW IV.—If *a* and *b* are equivalent, we have the collinear triads *ac.b* and *bc.a*, whatever point *c* may be.

LAW V.—If the triads *a*, *b*, *c* and *b*, *c*, *d* are both collinear triads, so also are both the triads *a*, *c*, *d* and *a*, *b*, *d*.

LAW VI.—No point is absent from the system whose presence is consistent with the foregoing laws.

I shall have occasion again to refer to these laws, but, for the present, I pass away from the consideration of the geometrical theory of points, and proceed to discuss the logical theory of statements.

In place of points, we have now to deal with statements. Most persons would, I think, say that our new subject-matter is something altogether different from that with which we have hitherto been dealing, and would demur to my observation that, as subjects of exact thought, statements are just the mere entities that points are. "Statements," they would say, "are complex structures; some very complex, consisting, in fact, of a number of other simpler statements combined together by the use of the conjunctions 'and' and 'or'; and, of the simpler statements, even the most simple comprise parts—'terms,' &c.—which cannot be overlooked. It would be absurd to say that all these are to be ignored, and a statement regarded as if it were a mere entity such as a point is."

In answer to such objections, I would point out, on the one hand, that a statement is not other than a mere entity because it happens to be expressed as a combination of other statements, any more than a number is other than a mere number when it is expressed as the sum or product of other numbers. Nor, on the other hand, do we, by regarding a so-called "complex" statement as a mere entity, ignore the other statements in terms of which it is expressed, any more than we ignore certain numbers when we regard the number which is their sum or product as a single number, and not as a sum or product of two or more numbers. Those other statements will not be ignored, but will be regarded and treated each as a distinct entity. Every statement when considered alone is regarded as a single entity. When it is taken in conjunction with others, we see that it bears to them certain relations which we call "inconsistency," "implication," &c.; and, by virtue of the existence of these relations, it may be expressed in terms of those other statements, just as one number may be expressed as the sum or product of other numbers to which it is related.

As regards the "terms," &c., which compose statements, I remark that we shall here be concerned only with the relations of statements to each other, and not with the relations that they bear to "terms" or other things. The relations which statements bear to each other may, of course, be considered and expressed by dealing with the terms, &c., which compose them; and equally the relations of points to each other might be expressed by reference to the straight lines, curves, surfaces, &c., which pass through them: but, just as points have relations to each other which may be considered without reference to other geometrical conceptions, so statements and their mutual relations may, and will here be, discussed without any regard to terms.

But a further objection may be raised to the notion that statements as the subjects of exact thought are mere entities, such as points are, which must be considered and dealt with; and that is, that it leaves no place for those conceptions of the truth and falsity of statements which seem to be of the essence of the logical theory: ideas of truth and falsehood can hardly be associated with mere entities. In order to dispose of this objection, let us consider what the logical theory of statements is, as it is usually understood. Statements, with certain special exceptions to be presently referred to, are conceived of by the logician as admitting of being regarded at will as either true or false. This liberty, which we have to regard individual statements as either true or false, does not extend to all pairs, triads, &c., of those statements; for, in general, if certain statements are regarded as true, there are others which must be regarded as true also, and others which must be regarded as false. Similarly, if certain statements are regarded as false, there are others the truth or falsity of which is thereby determined. Our liberty, then, in this respect is subject to certain restrictions. It is to these restrictions that statements owe those mutual relations which it is the object of the logician to investigate and define.

I have said that there are certain special exceptions to the rule that individual statements are conceived of as admitting of being regarded at will as either true or false. The restriction, in fact, which exists in the case of pairs, &c., of statements, extends equally to the case of individual statements, for there are some which cannot be regarded at will as either true or false, but must be regarded some as always true, and others as always false. These statements are called *truisms* and *falsisms* respectively. Logically speaking, all truisms are equivalent: each bears precisely the same logical relation to every other state-

[1] Observe here the difference between undistinguishableness and equivalence. In order that two entities may be undistinguished, it is sufficient that the relation which one bears to any collection of entities may be borne by the other to a collection which is undistinguished from the former collection. But, in order that two entities may be equivalent, it is necessary that the relation which one bears to any collection of entities should be borne by the other to the *same* collection, and not merely to one which is undistinguished from it.

ment or body of statements; and this is equally the case with falsisms. The introduction of truisms and falsisms into our field of view brings us to the root of the difficulty about the truth and falsehood of statements, and enables us to dispose of it. For to regard a statement as true is merely to ignore the difference between it and a truism—to regard it as equivalent to a truism; and the conception of the truth of a statement is thus simply one as to the *equivalence* of two statements one of which is a truism. In the same way the conception of the falsity of a statement is one as to the equivalence of two statements one of which is a falsism; and generally the logical relation between statements which is expressed by saying that if certain statements are true or false certain others are also some true and some false, is one which may equally well be expressed by saying that if certain statements, one of which is a truism, or a falsism, are equivalent, so also are certain others, one of which is a truism, or a falsism. The truth of a truism and the falsity of a falsism are not matters with which we are here concerned at all, any more than we are with the elegance or conciseness of the language in which they are couched; the question is one merely as to the equivalence of statements, and the relations expressed are such as may and do exist between statements no one of which is a truism or falsism. In fact, truisms and falsisms, as regards the logical relations which they bear to other statements, differ in no material respects from any other statements; and indeed all statements are, so far as their logical relations are concerned, undistinguished from each other; for, whatever relation a statement bears to any body of statements, that relation is also borne by every other statement to some other body of statements.

How comes it, then, it may be asked, that truisms and falsisms unquestionably do appear to bear exceptional relations to other statements? The reply is simple. We are accustomed to consider statements with reference to the relations which they bear to truth and falsehood, *i.e.* to truisms and falsisms, and the verbal shape which they assume in general involves such a reference. In fact, as we shall presently see, whenever the words "*and*" or "*or*" are used, there is such a reference involved. Statements which thus involve a reference, whether express or implied, to the relations which they bear to truisms or falsisms naturally seem to bear exceptional relations to the latter; though in fact, logically speaking, they bear no such exceptional relations.

Statements, then, as subjects of the exact thought of the logician, compose a system of entities which are undistinguished from each other. Let us proceed, as in the case of points, to consider pairs of these entities. At first sight it may seem that pairs of statements are of many different sorts; for two statements may be equivalent, inconsistent, contradictory, one of them may imply the other, and so on. But, as regards certain of these relations, a little examination in the light of the preceding observations will make it clear that they are not really relations between *two* statements at all, but between *three*, one of which is a truism or a falsism. Thus two "inconsistent" statements are such that they and a truism cannot all three be regarded as simultaneously equivalent; and similarly in other cases, which will be considered when we come to deal with triads of statements. There are, in fact, but three species of pairs, viz. *equivalent* pairs, *contradictory* pairs, and *simple* pairs.

An *equivalent* pair consists of two statements which are such that, whatever logical relation one of them bears to any body of statements, that same relation is borne by the other to the same body. Equivalent pairs are distributed among the whole body of statements in accordance with the following law:—

If each of the pairs a, b and a, c is an equivalent pair, then the pair b, c is also an equivalent pair.

A *contradictory* pair, as usually defined, consists of two statements which cannot both be regarded as true or both as false, and the relation considered would therefore seem to be one between *four* statements, and not *two*. This is not, however, really the case. The relation may no doubt be thus defined by reference to the two additional statements—a truism and a falsism; but it may also be fully defined without reference to any such additional statements: viz. two statements are contradictory if they cannot be regarded as equivalent without ignoring all logical relations. Since two contradictory statements cannot be equivalent to each other, it of course necessarily follows that they cannot both be equivalent to a third statement, whether it be a truism or a falsism, or any other statement. Two statements which are a contradictory pair may be said to be *obverses* of each other. A truism and a falsism are obverses of each other.

The following law holds with regard to contradictory and equivalent pairs, viz.:—If a, b and a, c are both contradictory pairs, the pair b, c is an equivalent pair. As however this law is a necessary consequence of certain laws which we shall have presently to consider, I do not include it among the fundamental laws which define the properties of a system of statements.

The third sort of pair, viz. a *simple* pair, is one which is neither an equivalent pair nor a contradictory pair: *i.e.* it consists of two statements which are neither necessarily equivalent nor necessarily non-equivalent, but may at will be regarded either as equivalent or not.

The division of pairs of statements into the three species, and the distribution of the pairs of the different species in accordance with the foregoing laws is not enough to determine the properties of a system of statements; and we must, as in the case of points, go on to consider triads of statements. These are of two sorts, which, for reasons that will presently appear, I term "linear" and "non-linear" respectively. There are other subdivisions of the triads into different sorts, but the division into linear and non-linear triads determines the other subdivisions. As in the case of a collinear triad of points, a linear triad of statements consists of two statements which may be called the "extremes," and one which may be called the "mean." It is such that if the two extreme statements are regarded as equivalent the mean must also be regarded as equivalent to them, and in this respect also it resembles a collinear triad of points. Any three statements which are thus related compose a linear triad, and any three which are not so related compose a non-linear triad. I shall employ the same symbol to denote a linear triad of statements that I employed in the case of a collinear triad of points; viz. a linear triad in which a, b are the extremes and c is the mean statement will be denoted by $ab.c$.

These linear triads are not scattered at random through the whole body of statements, but are distributed in accordance with the following laws:—

Law I.—If we have the linear triads $ap.b$ and $cp.d$, a statement q exists such that we have the linear triads $ad.q$ and $bc.q$.

Law II.—If we have the linear triads $ab.p$ and $cp.d$, a statement q exists such that we have the linear triads $aq.d$ and $bc.q$.

Law III.—If we have the linear triad $ab.c$, and a and b are equivalent statements, all the three statements a, b, c are equivalent.

Law IV.—If a and b are equivalent, we have the linear triads $ac.b$ and $bc.a$, whatever statement c may be.

Law V.—No statement is absent from the system whose presence is consistent with the foregoing laws.

The distribution through a system of statements of the triads of the two species in accordance with the foregoing laws completely defines the system as one possessing all those properties of statements which are really under consideration by the logician when studying the relations of statements to each other. The fact that the extreme statements of a linear triad cannot be regarded as equivalent without also regarding the mean statement as equivalent to them is a necessary consequence of Law III.; and thus all that is essential in a system of statements, so far as the exact thought of the formal logician is concerned, is that it is a system of entities, pairs and triads of which are of different sorts, and are distributed through the system in accordance with the specified laws.

This uniform distribution of pairs and triads of statements involves of course a similar regularity of distribution of distinguished and undistinguished collections of larger numbers of statements, and also of the aspects of those collections; and consequently a system of statements possesses "form," and owes its properties to the possession of this "form," in precisely the same way as a system of points does. Thus in the case of the logical theory of statements, as in that of the geometrical theory of points, it is "form" as here defined which is the real subject-matter of exact thought.

A remarkable circumstance connected with the laws defining the "form" of a system of points, and that of a system of statements, will no doubt have already been noticed. If we exclude Law V. of the former set of laws, the two sets of laws are the same; and it is thus on this Law V., and that only, that the differences between the properties of the two systems depend. This Law V. is that which expresses the fact that two straight lines can only cut once; so that, if in geometry this restriction were removed, the study of points would in all that is essential be the same thing as the study of statements. I cannot pursue this very interesting fact any further here; but it will now be understood why the expression "linear" has been used with reference to certain triads of statements.

As the views here put forward as to the true nature of the subject-matter of the exact thought of the logician in his consideration of the mutual relations of statements are somewhat novel, I cannot well pass on with the observation that the rest is mere matter of detail, but must briefly allude to one or two matters of importance. And first let us consider those relations which are usually considered as relations between *two* statements, but are, as I have already said, really relations between *three* statements, one of which is a truism or a falsism.

In a linear triad, let the mean statement be a falsism, then the extremes cannot both be regarded as true. For to regard them both as true is to regard them as both equivalent to a truism, *i.e.* as equivalent to each other. But if they are equivalent they must be equivalent to the mean statement, which is a falsism, *i.e.* they must be false and not true. The two extremes are here, therefore, *contrary* or *inconsistent*.

If we take the mean statement of a linear triad to be a truism, then the extremes cannot both be regarded as false, they are *subcontrary*.

If we take one of the extremes to be a truism, then, if the other extreme is regarded as true, the mean must also be so regarded. Here the two latter statements are *subalterns*, the extreme being the *subalternant*, and the mean the *subalternate*. In common parlance the former statement "implies" the latter.

Next let me point to one instance in which certain relations of statements to each other involve others. If we have the linear triads $ab.c$ and $bc.d$, it can be shown to be an immediate consequence of the laws which I have given as defining the "form" of a system of statements, that we have also $ab.d$ and $ad.c$. Taking, then, b to be a truism, this becomes:—If the statement a implies the statement c, and the statement c implies the statement d, then the statement a implies the statement d, and $ad.c$ is a linear triad. Observe here that the last part of the conclusion is not usually pointed out, because the fundamental character of the linear triad has not been noticed.

I proceed finally to consider the use of the words "and" and "or," and the logical relation of statements such as "a and b," "a or b," to the statements a, b. If a, b, c be any three statements whatever, there exists a statement x, which is such that the three triads

$$ab.x,\ bc.x,\ ca.x$$

are all linear triads. This statement x is uniquely related to the triad a, b, c; that is, it is a necessary consequence of the laws defining the system that, if any other statement, y, is such that the triads

$$ab.y,\ bc.y,\ ca.y$$

are all linear triads, then the statements x and y are equivalent. This statement, x, I term the *symmetrical resultant* of the triad a, b, c.

Now let t be a truism, and f a falsism. Then the symmetrical resultant of the triad a, b, f is the statement usually written

"a and b,"

and the symmetrical resultant of the triad a, b, t is the statement usually written

"a or b,"

where, however, the "or" would be more properly written "$\frac{\text{and}}{\text{or}}$," as in mercantile documents.

In the general case, in which we take any three statements, a, b, c, without taking one of them to be necessarily either a truism or a falsism, the symmetrical resultant will be

"a and b, or b and c, or c and a,"

or

"a or b, and b or c, and c or a,"

the two statements being equivalent. These statements have tacit reference to a truism and a falsism, as they are expressed in terms of the statements "a and b," "a or b," &c., which are, as we have seen, symmetrical resultants of the triads a, b, t, and a, b, f, &c. But no truism or falsism is really involved in arriving at the symmetrical resultant of a, b, c; it is a function of a, b, and c, only. As, however, there is no simpler verbal expression for the symmetrical resultant in vogue, we are obliged to have recourse to one which expresses it by tacit reference to statements which are not really involved; just as, in the algebraic treatment of geometry, the relations of points to each other are expressed by means of an algebra which has tacit reference to an "origin" and "axes," which are not really involved in the relations which are represented.

I now pass away from the special consideration o statements.

The principles which I have enunciated in the case of points and statements may readily be shown to extend to entities of other descriptions. Thus, straight lines, curves, surfaces, &c., as individuals are mere entities, just as points are. Some are undistinguished from each other, and some are distinguished; thus one plane is exactly like another, but is distinguished from a straight line. These various entities owe their properties to the fact that they bear various relations to each other, and to points. The relations which they bear to each other are defined when we know those which they bear to points; and the relations which they bear to points are due simply to the fact that some points lie on them, and others do not. That a point does or does not "lie on" a curve is not, however, of itself a material circumstance; it is accidental clothing. All that is material to the geometrician is, that the pair of entities consisting of a curve and a point which lies on it is distinguished from the pair which consists of that curve and a point which does not lie on it, and that the two sorts of pairs are distributed through the whole system of points and curves in a definite way. Though the relations of these other geometrical entities to each other may be arrived at by considering the relations which they bear to points, we may of course consider their relations without reference to points. Thus the system which consists wholly of straight lines is one composed of entities which are undistinguished from each other, and are such that pairs of those entities are of two sorts, viz. two straight lines cut or do not cut each other; and these two sorts of pairs are distributed through the system in accordance with definite laws.

Such considerations as these apply, not merely to straight lines, curves, surfaces, &c., but to all other entities which are dealt with by the geometrician. Vectors, quaternions, matrices, and even algebras, are as individuals all mere entities, and owe their properties to the fact that they bear certain relations to the entities previously referred to, and to each other; and all that is essential in these relations depends merely on the fact that certain individuals, pairs, triads, &c., and aspects of these, are distinguished from each other, and certain undistinguished, and have a specific distribution. Here therefore, also, it is "form" in the sense in which I have defined that word, and "form" only, which is under consideration.

In the same way terms, classes, syllogisms, and other logical conceptions are all mere entities; and all that is essential in their properties, so far as the exact thought of the formal logician is concerned, depends upon the fact that they compose systems possessing "form," *i.e.* a definite distribution of distinguished and undistinguished collections of entities, and of aspects of those collections.

A system of entities under consideration in any investigation will usually comprise entities of many sorts, some of which are taken into account merely for the purpose of facilitating the study of the properties of others. Algebras, and operations such as quaternions, are examples of entities thus added by the geometrician to a system of points, lines, curves, &c., in order to aid in arriving at the properties of the latter. By thus adding "auxiliary" entities to a system, we may also greatly simplify the definition of its "form"; so that a system which would otherwise be defined only by reference to the distribution of aspects, or of collections of a large number of entities, can be defined by definitions which refer merely to the distribution of collections of a small number of entities, and make no allusion to aspects. In fact, it may be shown that, by the addition of suitable entities, the "form" of any system of entities whatever may be defined by specifying merely that certain *individual* entities, and certain *pairs* of those entities, are like and certain unlike.

The light which is thrown by the foregoing investigations will, I hope, be sufficient to insure the appreciation, if not the acceptance, of the following general definition of the subject-matter of exact thought:—

Whatever may be the true nature of things, and of the conceptions which we have of them (into which points we are not here concerned to inquire), in the operations of exact thought they are dealt with as a number of separate entities.

Every entity is distinguished from certain entities, and (unless unique) is undistinguished from others. In like manner every collection of entities is distinguished from certain collections of entities, and (unless unique) is undistinguished from others; and every aspect of a collection of entities is distinguished from certain aspects of collections, and (unless unique) is undistinguished from others.

Every system of entities has a definite "form," due (1) to the number of its component entities, and (2) to the way in which the distinguished and undistinguished entities, collections of entities, and aspects of collections of entities, are distributed through the system.

The peculiarities and properties of a system of entities depend, so far as the processes of exact thought are concerned, upon the particular "form" it assumes, and are independent of anything else.

It may seem in some cases that other considerations are involved besides "form"; but it will be found on investigation that the introduction of such considerations involves also the introduction of fresh entities, and then we have merely to consider the "form" of the enlarged system.

If the definition of the subject-matter of exact thought which I have thus ventured to formulate be an accurate one, it will be obvious that there are divisions at present maintained between different branches of exact science which must be regarded as unnecessary and arbitrary; the only differences which should be treated as material being such as are due to differences of "form." These differences are certainly not such as to justify the isolation and separate study of individual systems, without any attempt to fix the position which each holds in the general body of possible systems, or to ascertain the exact points in which each resembles or differs from other systems of the whole body. The scientific study of each system must involve that of the properties common to all, of the general laws regulating the distribution of distinguished and undistinguished collections and aspects of collections in systems of any "form," and of the possibilities of variety in their "forms."

A. B. Kempe.

NOTES.

The retirement of Sir Gabriel Stokes from the Presidency of the Royal Society is to be taken as an occasion for the expression of the Fellows' high appreciation of his services. He has held office, either as one of the secretaries or as president, for thirty-six consecutive years; and it is unnecessary to say how much the Royal Society has benefited by his labours during that long period. It is thought that the most suitable way of marking the present occasion would be to obtain a good portrait of Sir Gabriel for the gallery of the Society; and an influential committee has been appointed to make the necessary arrangements.

At a meeting of the City side of the Gresham Committee at Mercers' Hall on Monday, Mr. Karl Pearson, Professor of Mechanics and Applied Mathematics in University College, was elected Gresham Lecturer in Geometry, in succession to the Dean of Exeter.

Dr. J. Jagor, the eminent ethnographical traveller, intends to make a scientific tour in British India. Remembering his researches forty years ago, many men of science in Berlin take much interest in his present plans.

The German Colonial Society have forwarded a number of books on tropical plants to Emin Pasha to further his scientific researches, as he lately complained (in a letter to Prof. Schweinfurth) of his want of works of reference. Prof. Noack has lately received a letter from the Pasha, dated Tabora, the middle of August. He then intended to leave for Uramba in four or five days, on his way to Lake Tanganyika.

At a meeting of the Royal Botanic Society on Saturday, the Secretary answered various questions as to the destructive action of fogs on plants. He said it was most felt by those tropical plants in the Society's houses of which the natural habitat was one exposed to sunshine. Plants growing in forests or under tree shade did not so directly feel the want of light; but then, again, a London or town fog not only shaded the plants, but contained smoke, sulphur, and other deleterious agents, which were perhaps as deadly to vegetable vitality as absence of light. Soft, tender-leaved plants, and aquatics, such as the *Victoria regia*, suffered more from fog than any class of plants he knew.

On Monday, Mr. T. G. Pinches read a paper before the Royal Asiatic Society, on the newly-discovered version of the story of the creation. He had had the good fortune, in the course of his investigations into the contents of the unregistered tablets in the British Museum, to find in one of them, brought home by Mr. Rassam in 1882, a still earlier version than that which the late Mr. George Smith had translated. It was a bilingual tablet, the text being Akkadian, and the gloss Assyrian; and while the date of the tablet itself was, like the rest of those in Assur-bani-pal's library, not older probably than 650 B.C., the Akkadian text was, in his opinion, an exact copy of an older document, which had, in all probability, been put into its present shape 3000 B.C., or even earlier. One side, the obverse, is devoted to the creation story. The other, the reverse, is simply an incantation form for the purification of the great temple tower E-zida, now so well known as the mound called Birs-Nimrud. The text might be roughly divided into three paragraphs or sections of about ten lines each. The first describes the time when nothing was, neither "the glorious house of the gods," nor plants, nor trees, nor cities, nor houses, no, not even the abyss (Hades) nor Eridu (regarded by the author as Paradise). The second section describes the making of Paradise with its temple tower E-Sagila, founded within the abyss. Then was Babylon made, and the gods, and the land, and the heavens, and mankind. The third section then proclaims the creation of animals, plants, and trees (in that order), of the Tigris and of the Euphrates. The fourth records the building of cities and houses. Of all except the last, Merodach, the god, seems to be the active creator, and he is also to be understood as the builder, through men, of the cities, &c. Mr. Pinches pointed out several interesting words and forms occurring in this oldest form of the creation account, which had subsequently assumed so many diverging shapes. A discussion followed, more especially on the word Adam, rendered by Mr. Pinches "foundations" (of earth), but by Dr. Zimmern "living things." This was probably the origin of the Hebrew word Adam.

According to the *Journal de la Chambre de Commerce de Constantinople*, the greatest electric project which has yet been suggested is being planned—the construction of a line from St. Petersburg to Archangel. The electric current would be supplied by a series of generating stations distributed along the line. It is estimated that the cost, including the rolling stock, would be 46,509 francs per kilometre.

The forty-first volume of the *Izvestia* of the Moscow Society of Friends of Natural Science contains a valuable review of the work done in zoology, anatomy, and embryology in connection with the Society during the last twenty-five years.

Natural History and Ethnographical Museums have been opened lately in several towns of East Siberia; and a like institution has now been established at Tobolsk. It contains most valuable ethnographical collections to illustrate the life of the Ostyaks and the Samoyedes, as also a very complete herbarium of the Tobolsk flora, and a collection of books, pamphlets, &c., on the region.

At a recent meeting of the Paris Academy of Medicine, M. Motais, of Angers, maintained that myopia, or short-sightedness, is one of the products of civilization. An unexpected proof of this view was found in the condition of the eyes of wild beasts, such as tigers, lions, &c. M. Motais, having examined their eyes by means of the ophthalmoscope, discovered that animals captured after the age of 6 or 8 months are, and remain, hypermetropic, while those who are captured earlier, or, better still, are born in captivity, are myopic. This short-sightedness is evidently induced by artificial conditions of life.

M. A. Berthoule has contributed to the *Revue des Sciences Naturelles Appliquées* a series of papers, well illustrated, on the lakes of the Auvergne region, and their fauna, natural or introduced. He considers that new species might be placed in many of these lakes, and, if properly attended to, would yield large profits to pisciculturists.

The curious idea of preserving dead bodies by a galvanoplastic method is not new, but we note that a Frenchman, Dr. Variot, has been lately giving his attention to it (*La Nature*). To facilitate adherence of the metallic deposit, he paints the skin with a concentrated solution of nitrate of silver, and reduces this with vapours of white phosphorus dissolved in sulphide of carbon, the skin being thus rendered dark and shiny. The body is then ready for the electric bath, which is served by a thermo-electric battery, giving a regular adherent deposit of copper if the current is properly regulated. With a layer of ¼ to ¾ mm. the envelope is solid enough to resist pressure or shock. Dr. Variot further incinerates the metallic mummy, leaving holes for the escape of gases. The corpse disappears, and a faithful image or statue remains.

Mr. Nathaniel Waterall, Waddon, Croydon, writes to us that, in one of the outhouses in the garden belonging to the house in which he resides, a robin's nest was found some time ago in a flat hand-basket, hanging on the side of the wall. In the nest there were four young ones, recently hatched. They were allowed to remain until their time came to fly away. The basket had been in the same position for a considerable time.

Prof. E. A. Kirkpatrick contributes to the *Evening Gazette* (Mass., U.S.), a paper in which he shows how parents —mothers especially—might do good service to psychology by recording "certain facts of child development." He suggests that they should devote particular attention to the growth of the power of speech. They ought, he thinks, to keep two lists of words—one containing all words articulated by the child, with indications as to how they are pronounced; the other, all words used "understandingly." The first list would indicate the common difficulties encountered in learning to articulate, and an examination of a sufficient number might make it possible to determine whether there really are "general laws of mispronunciation." The second list would show the intellectual progress of the child as it learns new words, and learns to use old ones with increasing accuracy, and to put them together into phrases and sentences. Words that are invented by the child, and those used in a sense different from the ordinary meaning, are especially interesting, and throw considerable light on the subject of how children classify and generalize. Prof. Kirkpatrick proposes that separate sheets shall be kept for each week, or perhaps for each month in the case of the articulating vocabulary. No confusion will then result, and on the back of the sheets may be given the peculiar meanings attached to words, the earlier attempts at putting words together, the later sentences of interest, especially those showing the characteristic grammatical errors, and other items. He is of opinion that the comparison of a number of such vocabularies would help to solve several interesting problems; and he expresses a hope that those who have begun, or think of beginning, the preparation of lists will send him the record for several months, before the middle of next May. Anything of scientific value that may be thus reached will be published, and along with it will be given "the names of those by whose patient observation it has been obtained."

Prof. Brückner, of Berne, has recently called attention to the existence of climatological periods of about 35 years for the whole globe (more marked in the interior of continents). The years 1700, 1740, 1780, 1815, 1850, and 1880 appear as centres of cold, wet periods; while the years 1720, 1760, 1795, 1830, and 1860 are centres of warm, dry periods. During the warm periods the passage of oceanic air to the continent has been hindered, and during the cold it has been favoured, increased rainfall occurring in the latter case.

IT is stated that another meteorological station has just been added to the list of those in telegraphic communication with the Observatory at Si-ka-wei near Shanghai—namely, at Tientsin. M. Chapsal, whose services in promoting and improving the present system of semaphore signalling and meteorological observations are deserving of great praise, requested M. Ristelhueber, the French Consul-General at Tientsin, to use his influence with the Director-General of Chinese Telegraphs, to procure the free transmission of the necessary telegrams, with the result that permission has been obtained. The observations at Tientsin, made by Mr. Bellingham, will probably be of much value.

WE have received a small pocket-book, published by Alfred Watkins, Hereford, on exposure notes, for use with the Watkins exposure meter. This meter seems to be of a very ingenious construction, and, according to the accounts we have read about it, should be found a very serviceable appendage to the photographic kit. The instrument is very neat and compact, being only $2\frac{1}{2}$ inches long and $1\frac{3}{4}$ inches in diameter, and consists of a combination of a bromide of silver actinometer, a chain pendulum for timing the exposure, and a set of calculating rings, each carrying a pointer. If each of these rings be set for the correct value of the factors indicated, a fifth pointer automatically indicates the correct time of exposure in seconds or in fractions of a second, as the case may be. In this note-book, full instructions are given as to the method of using it, followed by 60 pages in which notes on exposure, values of the factors, and other details may be inserted. In addition to the above, at the end are inserted various hints and jottings which should be found useful to the many photographers, both amateur and professional, who will use this note-book.

THE Belgian Consul-General at Singapore, in a report quoted in the new number of the *Board of Trade Journal*, says that rubies and sapphires abound in the Siamese provinces of Chantaboun and Battambang. Several mines have been worked since a remote period by the natives, but for a long time they produced for the most part only stones of little value. It was in 1874 that the first mine of sapphires of good quality was discovered by a native huntsman in the environs of Chantaboun. The place was very difficult of access, so that the news of the discovery spread slowly. Rangoon being still at that time the nearest market to Siam for the sale of precious stones, the Burmans were the first to know of the existence of the new mine by the stones which were offered for sale at Rangoon. Some went there, and the large sums which they brought on their return from the sale of their produce brought about a movement of very active emigration for the same destination during the years 1878 and 1879. The new-comers discovered several mines as rich as the first. But there, as at Bantaphan, fevers made such sad ravages in the ranks ef the workers, that in 1880 the number of arrivals decreased in considerable proportions, and at the present time the population of these mines, which once reached the figure of 10,000, consists of a few Pegu Toung-Thons, who can ward off better than other races the ills resulting from the terrible climate of the country. Rubies, onyx, and jades are also found in considerable quantities in the province of Chantaboun, but their quality leaves much to be desired. Battambang is as rich in precious stones as Chantaboun, and it is stated that recently diamonds have been found near the frontier of Cambodia; but the mines of this province are almost abandoned because of the insalubrity of the climate, and the want of protection for foreign workers.

THE canalization of the immense marshes of Pinsk, in West Russia, is rapidly going on. No less than 185 square miles of marshes have been drained on the banks of the Pripet, and more than 7,000,000 acres of meadow-land have been reclaimed in this way. Forests which formerly remained inaccessible and valueless, are now easy of access, and begin to be profitable.

A SERIES of experiments upon the synthetical production of cyanogen compounds by the mutual action of charcoal, gaseous nitrogen, and alkaline oxides or carbonates, at high temperatures and under great pressure, are described by Prof. Hempel in the new number of the *Berichte*. Bunsen and Playfair long ago showed that when charcoal and potassium carbonate are heated to redness in an atmosphere of nitrogen, a certain quantity of cyanide of potassium is formed. Since that time Margueritte and Sourdeval have further shown that barium carbonate may be used in place of the potash, and that the barium cyanide produced may be again decomposed by steam into ammonia and barium carbonate. These reactions afforded a theoretically continuous process for the conversion of atmospheric nitrogen into ammonia, a process which, if it could only be worked on the large scale, would doubtless be of immense value. Unfortunately, however, only small proportions of the substances appear to enter into the reaction at ordinary pressures, hence the yield is not sufficiently large to render the process economical. Prof. Hempel, however, by means of a simple pressure apparatus, has shown that the reaction is very much more complete, and when potash is used very energetic, under the pressure of sixty atmospheres. His apparatus consists of a strong cylinder closed at one end, and worked out of a single block of steel. The steel top screws tightly down, so as to form a closed chamber, and is pierced with two apertures—one for connection with the compressing-pumps, and a second to admit the passage of an insulated copper rod. Within the steel cylinder is placed a smaller cylinder of porcelain, in which the mixture of the alkaline oxide or carbonate and charcoal is placed. Through the centre of this mixture passes a rod of charcoal, which is connected above with the copper rod and below with the steel cylinder itself, in such a manner that when the wires from a strong battery or dynamo are connected with the projecting end of the copper rod and the exterior of the steel cylinder respectively, the rod of charcoal becomes heated to redness. The pumps are then caused to force in nitrogen gas until the desired pressure is registered on the gauge. Experimenting in this manner it was found that the amount of barium cyanide formed in fifteen minutes under a pressure of sixty atmospheres was nearly four times that formed at ordinary atmospheric pressure; while in case of potassium carbonate the reaction was so energetic that in a few seconds the heated carbon rod itself was dissolved. Hence it is evident that the formation of cyanides by heating together alkaline carbonates and charcoal in an atmosphere of nitrogen is greatly accelerated by largely increasing the pressure under which the reaction occurs.

THE additions to the Zoological Society's Gardens during the past week include a Cape Hyrax (*Hyrax capensis*), an Areolated Tortoise (*Homopus areolatus*), a Galeated Pentonyx (*Pelomedusa galeata*), two Rough-scaled Lizards (*Zonurus cordylus*), six Dwarf Chameleons (*Chamæleon pumilus*), two Rufescent Snakes (*Leptodira rufescens*), three Smooth-bellied Snakes (*Homolosoma lutrix*), a Rufous Snake (*Ablabes rufulus*), a Ring-hals Snake (*Sepedon hæmachates*), a Robben Island Snake (*Coronella phocarum*), from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Common Fox (*Canis vulpes* ♂), British, presented by Mr. C. T. Stanhope Bilbrough; a Demoiselle Crane (*Grus virgo*) from North Africa, presented by Mrs. Wright; two Cactus Conures (*Conurus cactorum*) from Brazil, presented by Mr. H. C. Martin; two Common Mynahs (*Acridotheres tristis*) from India, presented by Mr. G. W. Blathwayt; two Snow Buntings (*Plectrophanes nivalis*), three Bramblings (*Fringilla montifringilla*), British, presented by Mr. J. E. Baldwin; a Broad-fronted Crocodile (*Crocodilus frontatus*) from West Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

VARIATIONS OF CERTAIN STELLAR SPECTRA.—The November number of the *Monthly Notices of the Royal Astronomical Society* contains a note by the Rev. T. E. Espin, "On the Variation of the Spectra of R Coronæ and R Scuti, and on the Spectra of R Aurigæ and R Andromedæ." The following are the observations recorded:—

R Coronæ.

1890 March 26.—Star about 5·8 mag. Colour yellowish-white. Nothing certain seen; sometimes irregularities, either dark or bright lines suspected.

1890 April 10.—Continuous spectrum, but again suspected lines; one bright one strongly suspected near the place of F, but believed more refrangible.

1890 September 8.—A most wonderful change has taken place in this star's spectrum. Two large absorption bands have appeared, one in the bluish-green, and one in the bluish-violet. These bands are sharply defined on the least refrangible side. Bringing the spectrum to a line, bright patches were seen far away in the violet—these may be bright lines or bright spaces. The star is now pale yellow. The magnitude still about 6.

1890 September 14.—The spectrum is apparently of the IV. type [Group VI.] since the bands fade away on the more refrangible side, but are sharply defined on the less refrangible. The band in the bluish-green was thought to be occasionally resolved into fine lines; between the two bands a bright line was suspected. The star is of the same magnitude, and now pale orange.

1890 October 8.—The star is only dim, but the bands seem to have faded.

1890 October 10.—The star has now nearly returned to the continuous first type spectrum observed in the spring. The big band in the bluish-green has disappeared, but the band in the violet is probably there still, but faint. The bright line previously mentioned again suspected. The star is now yellowish-white, and the magnitude about the same.

R Scuti.

1890 August 21.—III. Type (= Group II.). Bands 1, 2, 3 of Dunér's nomenclature seen, and also 7 and 8, which are the strongest.

1890 August 23.—Estimated 7·2 mag.; pale orange-red; carefully examined. Type III., but peculiar. The usual bands 1, 2, 3, seen; the bands 4 and 5 faint; 7 and 8 are strong. Bringing the spectrum to a line, bright knots seen in the violet and ultra-violet, either lines or spaces.

1890 September 8.—Bands 4 and 5 much better seen, but the other bands remain the same. The star is now brighter.

1890 October 10.—The star is now about 6 mag. The III. type spectrum is no longer certainly seen. A remnant of band 7 still remains; the others have almost, if not quite, disappeared. Perhaps 8 is there.

1890 October 12.—The star about 6 mag. Type doubtful; possibly III., but very indistinct.

1890 October 15.—The star is about 6·5. The spectrum is again clearly III. type. Bands 7 and 8 best seen; also 1 to 4 visible. The bands are, however, faint and dim, but are larger now, and the spectrum is similar to that observed on August 21.

1890 November 1.—Normal III. type, and about 6·8 mag. The bands are well seen in all parts of the spectrum. Bands 7 and 8 are especially broad.

R Aurigæ.

1890 August 18.—Mag. 7·2; colour fine rose-red. Very fine III. type, and the spectrum appears to resemble that of Mira rather than that of R Andromedæ.

R Andromedæ.

1890 August 23.—Mag. 7·3 ±. Bands not deep except in the blue and violet. Bringing the spectrum to a line, several bright lines in the violet and ultra-violet suspected. F possibly bright.

1890 September 8.—The star has increased in light, and γ hydrogen and F certainly seen, but still faint.

1890 September 14.—Bands in the red well seen, the yellow bands faint. F very plain now.

1890 September 15.—The F line now a wonderful spectacle. The star is not so red, the bands are generally faint, except in the red. A bright space in the yellow looks like a mass of fine bright lines. A deep band in the violet—$H\gamma$ and D_3—possibly bright.

BRITISH ASTRONOMICAL ASSOCIATION.—The first number of the Journal of this Association has been recently issued. Miss A. M. Clerke has contributed a paper on the rotation periods of Mercury and Venus, in which she brings forward the evidence which led Schiaparelli to the conclusion that their rotation period is the same as their sidereal period of revolution around the sun. Another paper, by the editor, Mr. E. Walter Maunder, entitled "The Chief Nebular Line," deals with the character and position of the chief line seen in the spectrum of the nebulæ, and its probable origin. Beginning with Dr. Huggins's discovery, in 1864, of the character of nebular spectra, it is shown how he suggested that the brightest line was due to an unknown form of nitrogen. This view was widely taught until 1887, when Prof. Lockyer enunciated the principles of his meteoritic hypothesis, and testified to the coincidence of the line with the remnant of the brightest magnesium fluting at λ 5006·5—a statement combated by the later observations of Dr. and Mrs. Huggins. Mr. Keeler's observations of nebular spectra, made at the Lick Observatory with a Rowland grating having 14,438 lines to the inch, demonstrate that the nebular line may appear both *more* and *less* refrangible than the brightest edge of the magnesium fluting. This being so, Mr. Maunder concludes "that we do not know the position of the nebular line with sufficient accuracy to say positively that it does or does not accord with the magnesium fluting." The Journal is supplied free to members of the Association.

ELEMENTS AND EPHEMERIS OF ZONA'S COMET (*e* 1890).—A Royal Astronomical Society circular contains the following elements and ephemeris computed by Dr. Hind. The orbit depends upon an observation at Rome on November 16, one by Baron von Engelhardt on the 18th, and the Paris observations of the 21st.

Perihelion passage, 1890 August 8·43592 G.M.T.

		°	′	″	
Longitude of perihelion (π)	...	113	16	52·1	Appt.
,, ,, ascending node (☊)		85	25	2·7	Eq.
Inclination (*i*)	... ...	25	38	57·4	Nov. 20.

Perihelion distance, 2·0597 (Earth's mean distance = 1).
The motion of the comet is retrograde.

Ephemeris for Greenwich Midnight.

1890.		R.A. h. m. s.		Decl. ° ′		Distance in Terms of the Earth's Mean Distance from the Sun.
Dec. 18	...	2 42 0	...	+32 19·9	...	1·757
,, 20	...	2 35 4	...	31 53·6		
,, 22	...	2 28 38	...	31 27·4	...	1·844
,, 24	...	2 22 42	...	31 1·5		
,, 26	...	2 17 14	...	30 36·0	...	1·937

The comet is, therefore, still in Perseus, and moving towards Triangulum. The sidereal time at Greenwich at 10 p.m., on December 18 = 3h. 49m. 50s. The intensity of light on December 26 = 0·47, that on November 16, the date of discovery, being taken as unity.

CHEMICAL ACTION AND THE CONSERVATION OF ENERGY.

THE conservation of energy is accepted as a general principle without question, but its operation is often so disguised, especially in chemical changes, that it is not apparent to a superficial observer, and, as a consequence, it is too often treated as a dead letter.

This is largely due to the enunciations given by thermochemists, who attempt to draw an impossible distinction between chemical and physical changes, and imply that the latter are not subservient to the law of the conservation of energy. The accepted principles of dissociation, together with the recognition of the complex nature of liquid molecules, and of the existence of compounds in solution, gives us, however, the means of explaining *all* the thermal results of any action in accordance with the recognized principles of science (see Chem. Soc. Trans., 1889, 14).

Chemical affinity, or the potential energy possessed by atoms, becomes satisfied to a greater or less extent when these atoms combine together, and a corresponding amount of kinetic energy must be developed. In all ordinary calorimetric operations this kinetic energy takes the form of heat. An "endothermic" compound is, therefore, an impossibility, if the term be used in the sense of a "body formed from its constituent *atoms* with absorption of heat."

The same principle which governs atomic combinations must also govern those complex reactions with which we generally deal. They occur in order to satisfy affinity—in order to convert potential into kinetic energy—and heat *must*, therefore, be liberated during them. If any absorption of heat does occur, it can only be due to some secondary decomposition, which has followed the primary heat-evolving action, owing to the alteration in conditions occasioned by this action. Dissociation affords a simple explanation of the occurrence of such secondary decompositions.

Just as a certain temperature must be attained before any particular combination can occur, so there is a certain temperature above which any particular compound cannot exist; but, owing to the molecules in a mass of fluid being at different temperatures, dissociation begins when the average temperature of the mass is below this point, and is not complete till the average temperature is considerably above it. The stable condition of a fluid at any temperature between these limits is such that there are $\frac{1}{x}$th of the total molecules dissociated: if this condition be disturbed by the removal of any of the dissociation products, other molecules will have to dissociate in order to reproduce it.

The effect of dissociation on the thermal value of any change may be illustrated by the case of carbon dioxide and carbon, which react at 600° to produce carbon protoxide with an absorption of 39,000 cal. At 600° carbon dioxide is partially dissociated, and consists of $xCO_2 + (1 - x)CO + (1 + x)\frac{1}{2}O_2$. With the free oxygen in this mixture the carbon can combine, *evolving* 29,000 cal.; and more of the carbon dioxide must then dissociate to reproduce the stable condition; this dissociation absorbs 68,000, leaving 39,000 cal. as the algebraic sum of the combination and consequent decomposition.

Nearly every series of reactions in which heat is absorbed can, I believe, be fully explained by one of the reagents being to start with in a state of partial dissociation,[1] and in the remaining few a similar explanation is obtained in the dissociation of the product of the reaction.

An exothermic reaction cannot possibly be impeded by the fact that its occurrence will *subsequently* involve a greater absorption of heat. The carbon cannot be supposed to refrain from combining with the free oxygen by the fact that its doing so will disturb the equilibrium of the mass and necessitate the decomposition of other CO_2 molecules. To imagine that it would do so, would be to endow the atoms with intelligence.

The converse fallacy is often held—that an endothermic reaction may occur if it forms part of a cycle of which the final result is an evolution of heat. This is obviously impossible. Unless we endow the molecules with prescience, we can no more imagine that they will react at first so as to increase their stock of potential energy (absorb heat) in order that this stock may subsequently be diminished by the interaction of the bodies first formed, than we can imagine that a stone will roll of its own accord a short way up a hill in order to have a long roll down on the opposite side.

A considerable amount of misconception also exists as to the influence of heat in effecting endothermic reactions. No amount of heating can make an endothermic reaction possible so long as it remains endothermic; but it is, of course, quite possible that a reaction, which would be endothermic at one temperature, may, owing to the relative magnitude of the heat capacities of the reagents, become exothermic at another; or that heating the reagents may induce dissociation, and that the new reagents thus introduced may render an action possible which was impossible with the original reagents.

Another not uncommon error is to imagine that an endothermic reaction may be brought about by the simultaneous occurrence of some independent reaction evolving heat. If the second reaction is really independent of the first, it cannot possibly have any influence on it; the heat liberated during it will be no more capable of rendering the endothermic reaction possible than heat supplied from any other source. Cases in which the contrary appears to take place may be explained either by the formation of new reagents during the exothermic action, or by the previous combination of the body; which reacts exothermically with that which could by itself react endothermically only.

Endothermic changes in which one or more of the reagents is a gas either do not occur directly, or, in the one or two cases where they do so, the thermal value of the reaction at the temperature of its occurrence is not known, or else the results may be easily explained in the same way as in the case of the reaction between carbon and carbon dioxide. The reactions which present serious difficulties are those where one or more of the reagents is liquid, such as (1) the dissolution of a solid in a liquid; (2) the dilution of a strong solution; (3) double decomposition between two substances in solution.

The heat absorbed when a solid salt is dissolved in a solvent—water, for example—must be attributed, not merely to the fusion, but also to the volatilization of the salt, and would amount to 5000 to 15,000 cal. per gram-molecule. To bring this about, some affinity must be brought into play capable of developing more than this amount of heat. Now, the satisfaction of the affinity of a large proportion of water for the salt molecule present develops less—the heat of dissolution is a negative quantity; but the water consists of aggregates of the fundamental molecules in a state of partial dissociation; and, from the fact that water will give off its fundamental molecules (*i.e.* has a vapour tension) at ordinary temperatures, we must conclude, I think, that this dissociation extends so far that some of the fundamental molecules themselves are present. To convert a mass of water at ordinary temperatures into its fundamental molecules (into gas), requires an absorption of 10,000 cal. per H_2O, so that those fundamental molecules must possess an amount of potential energy equivalent to 10,000 cal. more than that possessed by the average aggregate: they would, consequently, be capable of combining with a salt molecule and evolving 10,000 cal. more per H_2O than the average aggregate would, and they would be capable of effecting a combination which the average aggregate could not. The combination of one, or at most two, such fundamental molecules with a salt molecule would evolve more heat than that absorbed in the separation of the salt molecule from its fellows: we have, therefore, the conditions necessary for a possible reaction—a primary action *evolving* heat. The removal of the fundamental water molecules would necessitate the dissociation of other aggregates to supply their place, and these, in their turn, would combine with the salt hydrate present till the highest hydrate capable of existing under the circumstances was formed. The heat absorbed in the decomposition of the water aggregates is, however, nearly entirely counterbalanced by the recombination of the water molecules with each other after they have combined with the salt, for there is independent evidence to show that the water molecules present in a very complex hydrate are as much combined with each other as they are in the aggregates of pure water: thus, the net result obtained, when dissolution is complete, is simply the algebraic sum of two quantities: (1) the heat evolved in the combination of the salt with the water aggregates, (2) the heat absorbed in volatilizing the salt; and, according as the former or latter of these is the greater, so will the heat of dissolution be positive or negative; but the motive power, if I may use such a term, which produces these results is the potential energy possessed by the free water molecules, an energy which enables them to overcome the affinity of the salt molecules for each other, and produce a primary heat-evolving reaction.

The second case in which heat is absorbed by the dilution of a strong solution occurs as a part of the process of the dissolution of a solid salt, and is comprised in the above explanation.

Cases of double decomposition in which heat is absorbed would require too long an explanation for insertion here, and reference must be made to my former communication on this subject: it will suffice to say that they can in all cases be explained in accordance with the principle of there being a primary exothermic reaction, by recognizing the presence of several hydrates of various degrees of complexity, the less complex of these bringing about this primary reaction, and its removal necessitating as a consequence the endothermic decomposition of the higher hydrates to supply its place.

If the three conditions necessary to render a reaction possible exist—(1) a certain proximity of the reagents, (2) a certain affinity, *i.e.* power of developing heat by their reaction,

[1] The amount of dissociation necessary is almost infinitesimal, and may often be too small to be recognized by other means.

(3) a temperature within certain fixed limits—it follows that the reaction in question must occur; for, if it did not, the atoms would be in a state of strain inconsistent with stability; it would be as if a stone did not fall to the earth when there was nothing to prevent its falling. As a consequence of this, it follows that, in any complex system of atoms, where two or more different arrangements are independently possible, and where the various products remain within the sphere of action, and are capable of further interaction, then, those products, the formation of which is attended with the greater evolution of heat, will be formed to the exclusion of the others; or, if the two actions develop the same amount of heat, they will both occur to an equal extent.

I showed that the division of a base between two acids takes place entirely in accordance with this principle; wherever the salt formed remains undissociated in the liquid, the base is divided equally between the two acids, because the heat of neutralization of all such acids is the same, whereas, in other cases, where one of the salts formed is stable, and the other is in a state of partial dissociation, the undissociated salt is formed to the exclusion of the dissociated one; such small divergencies from this rule as are observed being due to the solutions examined being considerably stronger than they should have been, the heat of neutralization in such cases not exhibiting absolute constancy, and the dissociation of the dissociated salts being incomplete. This simple principle does away with the cumbrous hypothesis that each acid and base possesses a certain "avidity" or "affinity" peculiar to itself—an hypothesis which is, as I then pointed out, at variance with many facts of the case. A very striking confirmation of my views has been afforded by finding that the heat of neutralization of sulphuric acid in very weak solution is normal, thus verifying a prediction which I made on the strength of the above considerations.

I formerly held that the dissociation which explains endothermic reactions cannot be that of the product, but only that of the reagents; this I still think is true in cases where the product would dissociate into the same substances as the reagent would (if this latter could dissociate)—*e.g.* the hydrate of a salt dissociating into water and the elements composing the salt—but it is not true in other cases; for instance, a hydrate formed by dissolving a salt may dissociate into acid and base, and cause thereby an absorption of heat.

As with two possible reactions, where the products remain within the sphere of action, that which develops the most heat will occur to the exclusion of the other, so in two possible decompositions where the products remain within the sphere of action, that which absorbs least heat will occur to the exclusion of the other; and the question consequently arises, Why does a hydrated salt dissociate into acid and base, instead of into the anhydrous salt and water? The probable explanation is that in many cases the latter dissociation would be the more endothermic of the two; for the salt molecules in various contiguous hydrates are so far removed from the sphere of each other's attraction, that their attraction for each other would be practically *nil*, and the hydrate could only dissociate to form water and *free* salt molecules, absorbing in so doing a quantity of heat exceeding the observed heat of dissolution by an amount corresponding to the heat of fusion and volatilization of the salt, and exceeding in many cases the heat of neutralization itself. The presence of excess of water would moreover practically prevent the dissociation of the hydrate into water and the anhydrous salt.

A class of endothermic changes which present considerable difficulties are those which occur without the absorption of external energy in living organisms. We are at present so utterly ignorant of the nature of the reagents and products that it is hopeless to attempt any explanation of the *modus operandi* in these cases, but a suggestion made by Mr. Warrington at the recent meeting of the British Association with regard to the nitrifying organisms indicates the direction in which such an explanation may be obtained on the same principles as those given in the cases here discussed. SPENCER U. PICKERING.

THE WORKING OF THE TECHNICAL INSTRUCTION ACT AND THE LOCAL TAXATION ACT.[1]

THE Secretaries of the National Association for the Promotion of Technical and Secondary Education, in reporting on the working of the Technical Instruction Act, have to congratulate the Association on the rapid progress which has been made during the last year in taking advantage of the benefits of the Act. This is partly to be ascribed to the grant of a small sum by the Science and Art Department to meet local effort, but chiefly to the allocation to County Councils of England and Wales, under the Local Taxation Act of this year, of a sum amounting in all to £743,000, with permission to use it for the benefit of education over and above any sum that may be raised by rate under the Technical Instruction Act.

Technical Instruction Act, 1889.—The number of local authorities in England and Wales which have taken advantage of the Technical Instruction Act has risen from *four* to *forty* since the date of the Manchester Conference a year ago. These districts are as follows:—Atherton, Aberystwith, Barnsley, Birmingham, Birkenhead, Blackburn, Burnley, Burslem, Bridgwater, Bingley, Bolton, Blaenau Festiniog, Cardiff, Coventry, Darwen, Guiseley, Keighley, Kidderminster, Manchester, Maidstone, New Mills, Macclesfield, Northampton, Nottingham, Oxford, Rochdale, Rotherham, Sheffield, Stockport, Salford, Shipley, Sherborne, Southport, Stalybridge, Worcester, Wakefield, Wrexham, Westmoreland, Widnes, and York. This list is probably not complete. In addition to the above, several of the Welsh County Councils have appointed their Intermediate Education Joint Committees to be Committees under the Technical Instruction Act.

Local Taxation Act, 1890.—In addition to these districts, a number of counties have taken action in the direction of utilizing the local taxation grant under the machinery of the Technical Instruction Act.

The following English counties have already resolved to set aside the whole or part of the money for education:—

Shropshire has set aside the whole of its share, amounting to £6543, for education, and the Technical Instruction Committee has presented a valuable report on the use of the money.

Somersetshire has set aside the whole of its share, about £11,000, for education, and is now engaged in considering applications.

The Technical Instruction Committee of Hertfordshire has recommended that the whole of the sum (£6429) be devoted to education.

Staffordshire has voted £7000 for education, and a scheme for its distribution is being framed.

Oxfordshire has set aside half of the grant, about £2000, for education, and Gloucestershire has also voted half the grant for the same purpose.

The North Riding of Yorkshire has voted £2000 for education.

Cheshire and Devonshire have determined to devote some part of the fund to education.

Leicestershire gives £300 to the Leicestershire Dairy Association, and Westmoreland gives £250 to existing schools, to be spent on apparatus.

The county borough of Croydon has voted the new fund to meet capital expenditure on technical instruction.

Besides these twelve districts (eleven counties and one county borough) which have definitely voted the whole or part of the new fund for education, the subject is now receiving careful consideration in a large number of districts, in many of which the new fund is practically certain to be applied, at least in part, to educational purposes.

Conspicuous among these districts is the county of Lancashire, which was the first to move in the matter, having appointed a Technical Instruction Committee in August last, which is just now reporting results of exhaustive inquiries as to the best means of assisting education from the new fund. Essex also has advertised for applications for a share of the money, and is now considering such applications with a view to assisting technical education. Committees have been appointed to consider the question in the following twenty counties:—Kent, Wiltshire, Cornwall, Berkshire, Northumberland, Peterborough, Southampton, Cumberland, Durham, East and West Sussex, Cambridgeshire, Worcestershire, Warwickshire, Dorset, West Riding, East Riding, Bedfordshire, and Herefordshire, and London. In London, however, the bulk of the grant for the present year has been used for the reduction of rates.

A large number of county boroughs are considering the desirability of using the new money for education. It is stated to be most probable that it will be so used in Nottingham, Salford, Blackburn, and Bradford. Worcester proposes to increase its grant under the Technical Instruction Act by £450, to be taken out of the new fund.

In addition to the above, the following county boroughs

[1] Secretaries' Report, read at the Conference on December 5.

among others, have the matter now under the consideration of a committee :—Barrow, Bootle, Burnley, Canterbury, Gateshead, Hull, Leeds, Norwich, Oxford, Southampton, and Wigan.

In Wales the County Councils, with at most one or two exceptions, are proposing to utilize all the new fund for the purposes of education. In most cases the money will be used under the Intermediate Education Act, but some Councils, as, for example, Cardiff, have resolved to divide the fund between intermediate and technical education.

The general results hitherto attained, so far as the information of the Association goes, which is probably incomplete, may be summarized as follows :—

English.

County Councils which have voted money to education under the new Act	11
County Councils which have appointed committees to consider the question	22
Total counties moving in the matter ...	33 out of 49

This result has been attained during the short period of four months since the passing of the new Act, during the first half of which time hardly any County Councils were sitting. The figures for county boroughs are as follows :—

English county boroughs working the Technical Instruction Act	17
County boroughs allotting the new fund for education, but not rating themselves ...	1
Other county boroughs considering the application of the new fund for education ...	9
Total county boroughs moving in the matter	27 out of 59
Other districts in England working the Technical Instruction Act	19

The general result gives a total of 47 local authorities as yet assisting technical education in England.

This is exclusive of the Welsh counties and county boroughs, which, broadly speaking, may be said to be devoting almost the whole of the new fund to the purposes of education.

The total number is likely to be very largely increased when it becomes generally known that there is every reason to believe that the grant, or at least so much as is applied to education, will be renewed in the future.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, December 8.—M. Duchartre in the chair.—Observations of minor planets made with the great meridian instrument of Paris Observatory, from Oct. 1, 1889, to March 31, 1890, communicated by Admiral Mouchez.—Experiments on the mechanical actions exercised on rocks by gases at high pressures and in very rapid movement, by M Daubrée. The author applies the results recently arrived at (*Comptes rendus*, December 1) to the creation of volcanic ducts, and shows that it is probable that they are produced by mechanical actions much superior to the volcanic actions of which eruptions are the effects.—On the membrane of the lymphatic sac of the œsophagus of the frog, by M. Ranvier.—Proof that π cannot be the root of an algebraic equation having entire coefficients, by Prof. Sylvester. It is remarked that this proof should be substituted for the note by the same author, which unfortunately appeared in *Comptes rendus* of November 24. The latter note, which only dealt with a restricted case of the theorem enunciated in the text, is affected by inaccuracies which render it of no value.—A new method of studying the compressibility and the expansion of liquids and gases; results obtained with oxygen, hydrogen, nitrogen, and air, by M. E. H. Amagat. The experiments have been made between 0° and 200° C., and with pressures from 100 to 1000 atmospheres. The results obtained indicate that the coefficient of expansion of hydrogen at constant pressure diminishes regularly with increase of pressure. In the cases of oxygen, nitrogen, and air, the coefficient is at a maximum at the commencement, and this maximum corresponds to the pressure at which the product pv has the least value. The results obtained when the gases were reduced to constant volume show that $\frac{dp}{dt}$ is always greater between 0° and 100° than between 100° and 200°. The coefficients between 0° and 100°, at a pressure of 500 atmospheres, are 3·698, 3·085, 2·971, and 1·895, for oxygen, air, nitrogen, and hydrogen respectively. The values obtained at other pressures and temperatures follow a similar sequence.—Observations of Zona's comet, made with the great equatorial of Bordeaux Observatory, by MM. L. Picart and Courty. Observations for position were made on November 29 and 30.—On the observations of the transits of satellites of Jupiter and on the occultations of stars, by M. Ch. André.—On a transformation of motion, by M. Dautheville.—On the fluoride of allyl, by M. H. Meslans. Some of the physical and chemical properties of this compound are given. This ester is a colourless gas, possessing an alliaceous odour; it is formed from the iodide according to the equation $C_3H_5I + AgF = C_3H_5F + AgI$.—On some endothermic and exothermic reactions of organic alkalies, by M. Albert Colson.—On some derivatives of dimethylaniline, by M. Charles Lauth. A method of preparing tetramethylbenzidine is given, by which a yield of 40 per cent. may be obtained. Tetramethylbenzidine in hydrochloric acid solution is acted on at 45° by ferric chloride; fine green crystals of an unstable dye are produced, of which the formula is shown by analysis to be $C_{16}H_{21}ClN_2O$.—Contributions to the study of the nucleus of Spongidæ, by M. Joannes Chatin.—On the new class of jumping Acarina (*Nanorchestes amphibius*) from the coast of the British Channel, by M. Topsent and Dr. Trouessart.—On the age of sands and clays of the south-east, by MM. Ch. Depéret and V. Leenhardt.—Observations on extracts of meat, by M. Balland.

CONTENTS.

THURSDAY, DECEMBER 25, 1890.

THE SYSTEM OF THE STARS.

The System of the Stars. By Agnes M. Clerke. (London: Longmans, Green, and Co., 1890.)

THE gifted author of this work became justly famous by the publication of her now classical "History of Astronomy during the Nineteenth Century." The mass of accurate information there brought together, and the delightful way in which it was related, won well-earned praise on every hand. Her success in that effort has now encouraged her to a more ambitious one. In addition to the simple relation of facts, she ventures to give her opinions on the various inferences which have been drawn from them by those who have spent many years in elaborate investigations. Still, "the statement of facts has been kept primarily in view, but the more important efforts to interpret them have been noticed, and the difficulties attending rival theories impartially pointed out."

We may look upon the book as consisting of two parts, one simply presenting a most valuable mass of information, while the other gives the author's views on contemporary work. As far as the first part is concerned, Miss Clerke has done her work admirably. We regret, however, for her own sake, that she has not confined herself entirely to this kind of work. A trustworthy book, bringing together all the latest discoveries and views of competent judges as to their true meaning would have been priceless, and no one has better opportunities for such an undertaking than Miss Clerke. As it is, we find a certain amount of information, selected rather than complete, intermingled with her own views. Certain facts have been omitted, not with the intention of misleading, but because their importance was apparently not recognized.

The first two chapters give general ideas of the problems of sidereal astronomy, and the methods of research which are adopted. The third chapter deals with "Sirian and Solar Stars," and here we get the first glimmerings of the author's special leaning to what may be called the "electrical theory." There is little difficulty connected with these groups of stars: it is generally agreed that the Sirian are the hottest, and that they have a definite evolutionary connection with the solar ones. Like Vogel, the author includes amongst the Sirian stars those stars of Orion (*e.g.* Rigel) which are characterized by very few visual and comparatively few photographic lines. Although their spectra show hydrogen absorption conspicuously, they differ very widely from the Sirian stars in other particulars. Their structure is not improbably of a very different character, due no doubt to their connection with the great nebula. Condensations taking place away from such a widely-diffused nebulosity probably follow a different path, and for the present the Orion stars should be classed apart from the more perfectly formed ones of the Sirian type. α Cygni, again, is included amongst the "solar stars," although its spectrum exhibits very wide divergences from that of the sun; the characteristic structure about the G region is entirely absent, while there are many well-marked non-solar lines in its spectrum. Altair, also, is said to be located between the Sirian and solar stars; but here again there is little resemblance to either, beyond the lines of hydrogen; the diagram on p. 43 makes this sufficiently evident. The more the "solar stars" are studied, the more it seems probable, as Mr. Lockyer has pointed out, that they must be divided into two groups—one of increasing temperature, including such stars as Aldebaran, α Cygni, and Altair; the other of decreasing temperature, including such stars as Procyon, Capella, Arcturus, and the sun.

The author advocates that the difference between the Sirian and solar stars can be accounted for by assuming the solar stars to be the more strongly electrified. It is argued that an increase of temperature in such a star as the sun would increase rather than diminish the complexity of its spectrum, by increasing the absorbing metallic vapours. It is not evident, however, how a diminution of the electrical repulsion would cause such an increase of the hydrogen absorption as that indicated in the spectra of the Sirian stars, the author simply asserting that the layers of hydrogen would be much more concentrated than at present. The increased thickness of the hydrogen lines is more probably due to some cause which increases the quantity of hydrogen in the atmospheres, and for the present, at all events, that may be regarded as the dissociation of the metallic vapours due to the increased temperature brought about by condensation.

Chapter iv. deals with the stars having banded spectra. These, it is well known, are of two types—one showing dark flutings most probably of metallic vapours, and the other dark flutings of carbon. They form Secchi's third and fourth types respectively. Flutings undoubtedly indicate that the vapours producing them are at a comparatively low temperature, and the presumption would be that the red stars are cooler than the yellow and white ones. On this point it is remarked that "the alteration obviously implies an augmented extent of absorbing atmosphere. For the bands must originate in a region of less heat—that is, at a greater distance from the stellar photospheres than the lines."

Let us consider first of all the stars of the third type, in which the hottest members show a fluted spectrum superposed upon a line spectrum, whilst there are practically no indications of the hydrogen lines. It has been suggested on various grounds that the stars of this type are not stars in the true sense of the word, but that they are of a cometary nature; that is, that they are composed of discrete masses, and not of a photosphere surrounded by glowing vapours. Perhaps the chief evidence in favour of this view lies in the alleged existence, in the spectra of these stars, of the bright carbon flutings which characterize the spectra of comets. The author does not consider the evidence on this point conclusive, but this appears to be partly due to the fact that all the evidence has not been brought together. In Nova Orionis, with a well-marked spectrum of this type, Dr. Copeland demonstrated the presence of two of the cometary flutings with almost absolute certainty (*Monthly Notices R.A.S.*, vol. xlvi. p. 110); Messrs. Lockyer and Fowler showed them to be present in α Herculis and Mira Ceti (Proc. Roy. Soc., vol. xlvii. p. 35); and, more recently, Mr. Maunder has stated that in the spectrum of α Herculis the brightest green fluting is coincident in position with the chief hydrocarbon band, and, moreover, presents exactly the same appearance ("Greenwich Spectroscopic Observations," 1888, p. 13). The brightening of this same fluting at the maximum of Mira tends to the same conclusion. Not a single one of these important observations, however, is referred to by Miss Clerke, although, if they are confirmed by future work, they must inevitably revolutionize the old idea that the red stars of the third type are "suns." The objection that the dark band adjacent to this, if an effect of contrast, would indicate that in the stars where it appears very dark there was no continuous spectrum at all does not hold good. A sun-spot appears intensely dark by contrast with the surrounding photosphere, and yet we know that it is brighter than the electric light. It is further objected that the other two cometary bands do not present themselves; but the blue band was included in both Copeland's and Fowler's observations, and it must be remembered that the citron band is the faintest in comets, and that in the stars in question it may be masked by the dark flutings which fall near it. It is stated (p. 56) that "there are grave objections to admitting the reality of the masking," but, as these are not formulated, it is impossible to discuss them. Whether the masking be admitted or not, the third band seen in comets often assumes the positions of two of the apparent bright bands in the spectra of stars of the third type, and with the additional evidence of the other two flutings, it is only reasonable to suppose that comets and these stars are bound together by close ties of relationship. Hence we cannot agree that "conclusive evidence seems to be provided that stellar spectra of the third type originate at various heat-levels in powerfully ignited vaporous envelopes." The mere existence of a continuous spectrum cannot be regarded as evidence of the existence of a photosphere. Comets, as a rule, give continuous spectra in addition to their fluted radiation, but few would venture to assert that this is produced by the radiation of a photosphere. The case of Mira at maximum, when the hydrogen lines appear bright, is quoted as evidence of an intensely high temperature close to the supposed photosphere; but this, it should be added, is abnormal, and only occurs when, through some cause or other, the star is some hundreds of times its normal brightness.

In support of the view that the various phenomena presented by stars of this type are produced by a photosphere underlying a highly distended atmosphere, the observations of the intermittently hazy aspect of some of them is referred to. It is only necessary to add here a remark omitted by the author—namely, that this equally supports the "cometary theory" of their structure, and is, in fact, one of its essential points. The electrical theory is again brought to the front in connection with these stars, it being suggested (p. 61) that the red stars are more strongly excited than the Sirian and solar stars, and that, in consequence, the atmospheres are very widely distended.

Coming now to the other type of red stars, in which we get the cometary spectrum reversed, Miss Clerke is again in error. It has hitherto been pretty generally accepted that these stars are almost on the verge of extinction, but our author says that "their powerful incandescence is undoubted" (p. 64). The evidence of such a temperature depends upon very doubtful facts. In addition to the chief bands of carbon, the spectra exhibit six secondary bands, two of which agree almost in position with the D lines of sodium and the E line of iron, and on the strength of this it is stated that, "through the obscurity of the carbon bands can be distinctly seen a 'Fraunhofer spectrum'—a spectrum, that is to say, composed, like that of the sun, of dark metallic lines thrown out upon a continuous background. Among the substances originating in them, sodium is certainly, iron probably, recognizable." If the existence of the Fraunhofer lines be admitted, there should be no hesitation in connecting these stars with the solar stars, for a slight but certain carbon absorption is exhibited by the sun, and there would be no need for the subsequent remark (p. 89) that "their spectroscopic isolation leaves us without the means of tracing their relationships." It is more likely that the secondary bands are identical with those in the solar spectrum which are produced by the absorption of our own atmosphere (see Proc. Roy. Soc., vol. xliv. p. 92), and the approach to a planetary condition is thus plainly foreshadowed. This piece of important evidence is not referred to by Miss Clerke, notwithstanding its suggestiveness.

"Gaseous Stars and Nebulæ" form the subject of chapter v., the idea being to continue the succession from Sirian and solar stars, through the red stars and bright-line stars to nebulæ. If the stars of the fourth type and those like Rigel be omitted, this may be taken as the probable sequence of events. The nebulæ are closely connected with the bright-line stars, these, again, with the stars of the third type—the bright hydrogen lines here showing similarity of constitution; stars of the third type merge insensibly into stars like Aldebaran and α Cygni, and, finally, into stars like Sirius.

So far so good; but the author is inconsistent. The Orion stars had previously been grouped with those of the first type, but on p. 72 we find it stated that "the brilliant stars of Orion may be said to mark the first stage on the road to nebulosity." We first find nebulæ placed at one end of the series and then at the other.

Opinions are still divided as to the interpretation of the spectra of "gaseous stars," but these bodies can certainly no longer be regarded as suns; they are similar in structure to the nebulæ, whatever that may be. There are some nebulæ which appear as little more than points of light, and we have Prof. Pickering's word that their photographic spectra strongly resemble those of the bright-line stars. Visually, their spectra are identical with those of such widely-diffused masses as the Great Nebula in Orion. A more perfect sequence of spectra from nebulæ to stars like Sirius could not be wished for.

"Sidereal Evolution" forms the subject of chapter vi. Nebulæ were formerly regarded as quite "distinct and of another order from the group of cosmical bodies to which our sun and the fixed stars belong"; but now we can agree with Miss Clerke in accepting the view that they gradually "merge into unmistakable suns" (p. 84).

"But when we come to the various classes of stars, the order of their succession is less easily determined. The earliest and most obvious idea on the subject was based on a false analogy between the colours of the stars and the colours of glowing terrestrial solids. Red stars, it was thought, should be regarded, because they had cooled from a condition of white heat, as older than white stars. The colours of stars, however, depend primarily upon the quality and extent of their absorbing atmospheres, and quite secondarily upon their stage of incandescence" (p. 85).

Surely the quality of the absorbing atmosphere must also depend upon the stage of incandescence, and colour may therefore still serve as a guide to the temperature and age of the stars. That the red stars of the third type are young may readily be granted from the evidence previously referred to; but, at the same time, it must be allowed that their temperature is comparatively low. It is, however, argued that the stars of the fourth type are also at an early stage of growth, but the evidence depending upon their alleged intermittently hazy aspect, indicating enormous atmospheres, is not conclusive on this point. In fact, the authorities quoted (Messrs. Pogson and Peek) have only observed it in *variable* stars of the *third* type. From the fact that there is carbon absorption in the sun, it seems reasonable to suppose that the fourth type stars are at a lower temperature than stars like the sun, and are probably the result of the cooling of such bodies.

The discussion of the sun's status amongst celestial bodies naturally comes here. That it was once a star like Betelgeuse, then like Aldebaran, is granted. But has it passed through the Sirian stage? This involves the question of whether the "solar stars" are divisible into two distinct groups, but our author is not quite clear upon this point. She remarks that "it is scarcely conceivable that a state abolished as an effect of condensation should be restored by its further progress" (p. 91). If there are two groups of "solar stars," one group will include bodies a little more condensed than the stars of the third type, in which perfect photospheres have not been formed, while the other will include bodies formed by the further condensation of such stars as Sirius. The two states would not be identical. We know that the sun has a photosphere, and hence the suggestion that it is a cooling body. Its relation to the stars of the fourth type strengthens the view that the sun has already passed through the Sirian stage and will eventually be a fourth type star. Miss Clerke, however, does not accept this view of the effects of further condensation upon a Sirian star, but suggests another which has not a single fact to support it. It is pointed out that the spectrum of the satellite of Sirius, if it could be observed, might give some clue to the spectrum of a waning body; and she is bold enough to predict that it will prove to be of "an undistinguished character, interrupted neither by bands nor conspicuous dark lines, and feeble, not through effects of absorption, but intrinsically. The same dull uniformity may be expected to belong to the spectra of all stars of impaired splendour" (p. 92). What, then, will be the transition stage, say between Sirius and such a body as that suggested? The broad hydrogen lines could not disappear suddenly, and we know that there are no stars showing fine hydrogen lines alone. In fact, it is especially emphasised (p. 42) that "the conspicuousness of rays due to absorption by ordinary metals in the spectra of white stars varies inversely with that of the hydrogen series." Some solar stars, at least, must therefore represent the stage that will be arrived at by the cooling of such a star as Sirius, and the evidence tends to show that the sun is one of them.

Further, if the spectrum of such a waning star were simply dimly continuous, the absence of an atmosphere would be indicated. We certainly know that the moon has no atmosphere, but it does not shine by light of its own. Even the earth has sufficient atmosphere to give a very definite spectrum of absorption, and this atmospheric absorption must have been much greater when the earth was hotter than at present. Cooling stars are not likely, therefore, to give such spectra as Miss Clerke supposes, for we know that a powerfully absorbing atmosphere remains after the photospheric luminosity has disappeared. The spectrum of such a cool atmosphere would no doubt be a fluted one.

Temporary and variable stars occupy the next two chapters, and some valuable information is brought together in an interesting way. Among the spectroscopic observations of new stars, probably the most important were those of Nova Cygni. The most striking thing here was the increased brilliancy of the chief nebula line as the star faded away. That is to say, as the star cooled, it became a nebula, thus affording very decided evidence that nebulæ are comparatively cool. The dimming of new stars takes place so suddenly that it is certain only small bodies can be in question, and the suggestion has been made that new stars were produced by collisions of meteor-swarms. For collisions of this kind, Miss Clerke substitutes "grazing encounters with nebulous masses revolving in hyperbolic orbits, and overthrowing, by their proximity to the attractive body, a thermal equilibrium already eminently unstable." In the case of Nova Cygni no star had previously been recorded in its place, and it is difficult to conceive that such a grazing of a dark body, supposing one existed, with a moving nebulous mass could produce such a brief "conflagration" as was observed.

The old theories of stellar variation, assuming the existence of immense masses of slags in the photospheres, or that one side of a variable star was brighter than the other, have long been discarded. No single explanation is good for all variables, but there is no difficulty with those of the Algol type. After brief references to the explanations which have been suggested for the red stars, Miss Clerke concludes that "the time has not come to formulate a theory of stellar variability" (p. 125). The only comprehensive one we have as yet—the collision theory—is not considered sufficient by Miss Clerke; but careful consideration will remove the objections made against it. This theory assumes as proved the cometary character of stars of the third type, and suggests that the increase of light at the maximum of a variable of this kind is produced by the collisions at the periastron passage of satellite comets or swarms of meteorites. It is objected (p. 124) that this state of things could not long subsist, as the satellite swarm must inevitably become extended into a ring, with complete effacement of variability. This is certainly true, but we have only to look at the "November swarm" in our own system to understand that the disruption of the satellite comet need not be very speedy. This swarm has been observed now for at least a thousand years, and yet the brilliancy of the showers is apparently not diminished. Mira Ceti is the only variable of this class which has been observed for a long period, and the observations in this case only extend over three hundred years. On p. 124 we read further:—

"The periodicity of variable stars is, besides, of far too disturbed a kind to be thus accounted for. Systematic stability would assuredly prove incompatible with the enormous irregularities it discloses. The abrupt acceleration or retardation, for example, by a month of the hypothetical attendant swarm of Mira, would be impossible without such a total change in the elements of its circulation as would unmistakably break the continuity of its returns. But there are other objects far more recalcitrant than Mira to this mode of explanation. Take the outbursts of U Geminorum. They are not wholly capricious. There is a certain disorderly order about them by which they are manifestly akin to the changes of the more strictly periodical stars. We cannot relegate them to a class apart and invent a fresh hypothesis to suit them; the collision theory, to be acceptable in the one case, must be capable of meeting the other. But we can scarcely conceive any construction of assumptions by which such an extension of its powers could be effected."

Irregularities such as are here referred to are almost bound to occur if the collision theory be true. A star is not limited to one short-period comet any more than is our sun. There may very well be swarms of various masses travelling round the star in regular orbits, and occasionally a swarm travelling in an open orbit may enter the system. These swarms would so react upon each other and upon the central swarm that irregularities would be the inevitable result. Again, the central swarm might vary locally, like the Andromeda nebula, so that the revolving swarm would not always encounter it under exactly the same conditions. That the intervals between successive maxima and the magnitudes at maxima are not constant does not prove that the actions are not periodic.

In NATURE, vol. xlii. p. 550, there are some interesting examples of the apparently irregular results which might be produced by the integration of two sources of regular light variation. Two or more swarms of regular periods with the occasional advent of one moving in a parabolic or hyperbolic orbit, can be made to explain all the facts relating to this class of variables. In the variables of the fourth type, the same explanation holds good, if we consider the direct luminosity of the cometary swarms to be added to that of a dim condensed central body. Miss Clerke, however, supposes that variability depends upon extensive atmospheres, these being disturbed periodically in some way or other, probably by the tidal action of a satellite, so as to produce the observed fluctuations of light. It is a question whether such an atmospheric disturbance could increase the apparent brightness of a star hundreds of times.

The succeeding eight chapters are mainly descriptive, dealing with colour phenomena, double stars, stellar orbits, star-clusters, and the forms of nebulæ. It is only necessary to say of these that they are admirable.

Chapter xix., "The Nature and Changes of Nebulæ," discusses the relations of nebulæ and comets, and nebular variability. Referring to comets and nebulæ, it is stated (p. 286) that "traces of a spectroscopic analogy can indeed be shown to exist; but they are met with only in the secondary elements of each spectrum. The resemblance seems only incidental; the dissimilarity essential." It has, however, previously been pointed out (p. 76) that two faint comets observed by Dr. Huggins in 1866–67, gave spectra identical with that of a faint planetary nebula. This may well be regarded as conclusive spectroscopic evidence of the similarity between comets and nebulæ, but although it is apparently only regarded as of secondary importance, "it does not detract from the closeness of a physical analogy." It seems unphilosophical to rely on telescopic similarity when such conclusive spectroscopic evidence is at hand. We might go further than Miss Clerke, and say that as a comet is usually regarded as a swarm of meteorites the same view must be accepted for nebulæ. The information relating to the views on the temperature of nebulæ is very meagre. Some argue that they are intensely hot, whilst others argue that they are cool bodies not unlike comets. Apparently the only statement on this important subject is that on p. 77, where it is stated they "are not greatly heated." Indeed, the evidence afforded by their cometary relationships and by Nova Cygni is conclusive. Yet, as late as 1889, Dr. Huggins stated his belief that the temperature of nebulæ is very high.

It will thus be seen that, although a good deal of Mr. Lockyer's recent important work is left out of consideration, two of his main propositions are accepted by Miss Clerke; viz. (1) "nebulæ merge into unmistakable suns," and (2) they "are not greatly heated."

The remaining chapters consider the distances of the stars, the motion of the solar system, proper motions, the Milky Way, the "status of the nebulæ," and finally the "construction of the heavens." The two latter really discuss the distribution of stars and nebulæ, and bring together a wonderful amount of information which has hitherto been much dispersed.

In justice to Miss Clerke, it is only fair to add that she has exercised a sound judgment on many problems, and has made many valuable suggestions as to the lines on which future investigations should be proceeded with.

The book is well illustrated throughout, among the illustrations being reproductions of some of Mr. Roberts's marvellous photographs. With the exception of that of the Andromeda nebula, these are highly satisfactory.

In conclusion, we would again express our unqualified admiration of a good deal that the book contains, but we cannot help feeling that its value as a contribution to astronomical literature would have been greater if the author had confined herself to simply giving a trustworthy account of contemporary astronomical researches, and of the views held by competent thinkers. A safe and impartial guide to current thought is still a desideratum.

F.

ACROSS GREENLAND.

The First Crossing of Greenland. By Fridtjof Nansen. Translated from the Norwegian by Hubert M. Gepp. With Maps and numerous Illustrations. Two Vols. (London: Longmans, Green, and Co., 1890.)

THE only serious fault to be found with this book is that it is much too long. Only a small proportion of it is devoted to the actual crossing of Greenland, the rest being occupied with matters which might have been, and ought to have been, dealt with concisely. So far as it relates directly to the author's experience, the work is one of the deepest interest. Dr. Nansen is not only an explorer of remarkable courage, resource, and foresight; he has also many of the qualities of a man of letters. He knows exactly on what parts of his story emphasis should be laid, and his skill as a writer is so great that even incidents which do not in themselves seem to be either very striking or very suggestive are made to add to the freshness and vividness of his tale. One of the chief elements of attraction in the book is the more or less unconscious revelation which the author gives of his own character. He has evidently an intense delight in adventure for its own sake, and he writes in such good spirits, with so unaffected a delight in the remembrance of all the difficulties he has overcome, that it is impossible not to follow his record with sympathy and admiration; and every reader will lay down the work with a feeling of sincere regard for a traveller who is at once so hardy, enterprising, and enthusiastic. He has been most fortunate in his translator, who has done his work so well that the narrative reads almost as if it had been originally written in English. The maps and illustrations, too, are unusually good.

In the summer of 1882, when still a very young man, Dr. Nansen spent some months in the northern seas on board the *Viking*, a Norwegian sealer. The ship was caught in the ice off the eastern coast of Greenland, and it was at this time that he first felt a strong desire to investigate the strange, desolate region at which, many times a day, he gazed from the maintop. In the following year the wish was renewed and strengthened when he read of Baron Nordenskiöld's return from the interior of Greenland; but many things came in the way to prevent the fulfilment of his scheme. In 1887 he attempted, through the Norwegian University, to obtain from the Government the means of organizing an expedition; but the Government declined to support the proposal, and one of the newspapers expressed the opinion that "there could be no conceivable reason why the Norwegian people should pay so large a sum as 5000 kr. in order to give a private individual a holiday trip." Herr Augustin Gamél, however, who was already known as an enlightened and generous promoter of Arctic research, offered to provide the necessary funds; and thus Dr. Nansen was enabled at once to make preparations for his contemplated journey. Three of his countrymen—Otto Sverdrup, Oluf Christian Dietrichsen, and Kristian Kristiansen Trana, all of whom were admirably fitted for the work—undertook to accompany him; and he also secured the services of two Lapps—Samuel Johannsen Balto and Ole Nielsen Ravna. The Lapps were of less use than might have been expected; and under a less genial leader they might have become extremely troublesome. Dr. Nansen knew how to appeal to them, and nothing in its way could be better than his description of the devices by which he contrived to inspire them with a little of his own energetic spirit. It may be noted to his credit that he has nothing to say of any of his companions that may not be read with pleasure. He acknowledges frankly and gratefully all that he owed to them in carrying out his bold and hazardous project.

On May 2 the party left Christiania for Leith, whence they started for Iceland; and about a month afterwards they joined the sealer *Jason* at Isafjord, with the owners of which he had come to an agreement that the captain should do his best to put them ashore on the east coast of Greenland. Dr. Nansen has much to say about the voyage to Iceland, about cruising in the ice, about the bladder-nose seal and its capture, and about life on the *Jason*; and some of the facts he records are not without interest. But they are out of place in a work of this kind, which stands in no need of "padding." Even the account of "Ski" and "Skilöbning," by which the chapters on these subjects are preceded, might with advantage have been shortened. Here, however, the author has some excuse for the minuteness of his descriptions, for without "ski" the journey could scarcely have been undertaken, and the subject is one of which he is able to write picturesquely and effectively.

They quitted the *Jason* on July 17, hoping that they might effect a landing without much delay. But for many days they drifted southwards, and sometimes, battling for life among the floes, they could not but feel that success was almost impossible. Dr. Nansen never lost heart, and no part of his story is more stirring than that in which he records the unexpected and perilous adventures encountered at this early stage of his journey. When at last they reached land, they were, he says, "just like children." "A bit of moss, a stalk of grass, to say nothing of a flower, drew out a whole rush of feelings. All was so fresh to us, and the transition was so sudden and complete. The Lapps ran straight up the mountain-side, and for a long while we saw nothing more of them." Dr. Nansen was so relieved and delighted that, when gnats settled on his hand, he "let them sit quietly biting, and took pleasure in their attack." On the same day they embarked again, and pushed on quickly, as it was necessary for them to start from a more northerly position. They landed at their last camping-place on the eastern coast on August 10, and on August 15, after various preliminary arrangements, they set off for the interior, intending that their goal should be Christianshaab. Afterwards Dr. Nansen found it expedient to alter the route, and to make for Godthaab.

Of the journey itself little need be said, as the main facts are already well known, and an adequate conception of the details can be obtained only from the author's fascinating narrative. For some time they had to mount steadily, until they reached a height of 8970 feet. Then they came to a comparatively flat region, beyond which the ice-sheet descended gradually towards the western coast. The difficulties were enormous, for they had to drag their provisions on sledges, and they were repeatedly caught in violent storms, during which they were obliged to remain for shelter in their small tent. The cold was sometimes terrible; their rations were anything but abundant; and they had to sleep in bags. But no obstacles were formidable enough to daunt the leader of the party, and his task was lightened by the fact that he succeeded in maintaining from first to last the most friendly relations with his comrades. Although few incidents of any importance broke the monotony of their progress, Dr. Nansen found much to interest him in such scientific observations as the conditions rendered possible; and even in the interior of Greenland elements of splendour and beauty were not wanting. The greater part of their travelling was done in the daytime, but at first they thought it best to proceed by night. Here are some of Dr. Nansen's reminiscences of his impressions during their night-marches:—

"But if we often suffered a good deal in the way of work, we had full compensation during these nights in the wonderful features of the sky, for even this tract of the earth has its own beauty. When the ever-changing northern lights filled the heavens to the south with their fairy-like display—a display, perhaps, more brilliant in these regions than elsewhere—our toils and pains were, I think, for the most part, forgotten. Or when the moon rose and set off upon her silent journey through the fields of stars, her rays glittering on the crest of every ridge of ice, and bathing the whole of the dead frozen desert in a flood of silver light, the spirit of peace reigned supreme, and life itself became beauty. I am convinced that these night-marches of ours over the 'inland ice' left a deep and ineffaceable impression upon the minds of all who took part in them."

As they advanced over the ice-sheet, they longed more and more for land; and one afternoon, just two months after they had left the *Jason*, they were enchanted by the sudden appearance of a snow-bunting, from which they knew that the western coast could not be very far off. On September 19, Balto caught sight of a black spot in the distance, and immediately afterwards all of them were able to make out, through the mist of snow, "a long, dark mountain ridge, and to the south of it a smaller peak." On September 24, Dr. Nansen found himself "on the brow of an ice-slope which overlooked a beautiful mountain tarn, the surface of which was covered with a sheet of ice"; and shortly afterwards they had the pleasure of walking on solid ground. He says:—

"Words cannot describe what it was for us only to have the earth and stones again beneath our feet, or the thrill that went through us as we felt the elastic heather on which we trod, and smelt the fragrant scent of grass and moss. Behind us lay the 'inland ice,' its cold, grey slope sinking slowly towards the lake; before us lay the genial land. Away down the valley we could see headland beyond headland, covering and overlapping each other as far as the eye could reach."

On October 3, Dr. Nansen and Sverdrup reached Godthaab, where they were soon joined by their companions. For a moment it was unpleasant for them to find that they would have to spend the winter at this remote station; but they met with so much kindness that their stay proved to be in every way more agreeable than they anticipated.

Of the scientific results of the expedition, the most important is the establishment of the fact that the ice-sheet stretches in unbroken continuity over that part of Greenland which the party traversed. How far the ice-sheet extends towards the north we do not yet know; but Dr. Nansen concludes that "its limit must lie beyond the 75th parallel, for so far along the west coast it sends huge glacier-arms into the sea. Among these is Upernivik Glacier, in lat. 73° N., which has a rate of advance of no less than 99 feet in the twenty-four hours. So rapid a movement must presuppose the existence of an enormous interior ice-sheet, which can supply material for consumption on this huge scale." An idea of the form and elevation of the ice-sheet is conveyed by a section or profile drawing, constructed by Prof. Mohn and Dr. Nansen from numerous astronomical and barometrical observations, supplemented by the notes of Dr. Nansen's diary. This section shows that "the ice-sheet rises comparatively abruptly from the sea on both sides, but more especially on the east coast, while its central portion is tolerably flat. On the whole, the gradient decreases the further one gets into the interior, and the mass thus presents the form of a shield with a surface corrugated by gentle, almost imperceptible, undulations lying more or less north and south, and with its highest point not placed symmetrically, but very decidedly nearer the east coast than the west." Dr. Nansen indulges in some speculations as to the action of "great ice-sheets," but here his work is less satisfactory than in those passages in which he simply sets down what he himself has seen. Speaking of the fact that glaciers are stated by geologists to "have carried huge moraines of boulders and grit upon their backs," he says that to his mind "this idea is nothing less than absurd"; which is surely an odd way of talking about a conclusion based on a wide range of careful and exact observations. Dr. Nansen serves science more effectually in presenting facts as to temperature, snow- and rain-fall, and atmospheric pressure, and in describing the daily life of the Eskimo, some of whom he met on the eastern as well as on the western coast.

Dr. Nansen is now making preparations for a Polar expedition, which he proposes to undertake in 1892. If we may judge from his achievements in Greenland, he possesses in no common degree the special talents which are necessary for any measure of success in this larger and more difficult enterprise. No one who reads his book will hesitate to acknowledge that he has shown himself one of the ablest and most daring explorers of the present generation.

OUR BOOK SHELF.

Les Microbes de la Bouche. Par le Dr. Th. David. (Paris: Felix Alcan, 1890.)

THIS book will be found of considerable value. Although Dr. David does not record original observations of his own, most facts stated being copied from other works, he nevertheless deserves credit for putting together in concise form all that is known of the micro-organisms occurring in the oral cavity, on and in the organs of the mouth in health and disease. The morphology, cultural and physiological characters of most of these microbes are described in detail, including those of diphtheria, tubercle, and actinomycosis. From p. 205 to p. 262 the author gives a valuable account of the bacteria present in diseased teeth, but unfortunately he adds—probably for purposes of the professional dentist—a number of prescriptions on cosmetics, tooth powders, &c., which ought not to find a place in a scientific work of the character of the present book. The appendix on influenza might well have been omitted.

Notes on the Products of Western Afghanistan and North-Eastern Persia. By J. E. T. Aitchison, C.I.E., M.D., F.R.S. Pp. 228. (Edinburgh: Neill and Co., 1890.)

A REPRINT from the Transactions of the Botanical Society of Edinburgh, containing an alphabetical synopsis of the animal, vegetable, and mineral products of Western Afghanistan and Eastern Persia, together with their native names, with their English and Latin equivalents, and their applications. Dr. Aitchison's contributions to the botany and zoology of Afghanistan and the adjoining countries are well known to specialists, and the present work is an amplification of the economic branch of his investigations. Persia has been specially noted from time immemorial for its drugs, dyes, perfumes, and other vegetable productions, but much yet remains to be done in the elucidation of the plants which yield them. Dr. Aitchison was attached to the last military expedition to Afghanistan, and he was also naturalist to the "Delimitation Commission," and made very extensive collections of botanical and zoological objects, especially of the former. During the latter expedition he specially investigated the origin of the drugs, such as the asafœtida, obtained from umbelliferous plants, and the main results are given in a handsomely illustrated memoir that appeared in the Transactions of the Linnean Society.

The "Notes" now offered to the public afford another proof of the immense energy and perseverance of the author in collecting materials and information, often under great difficulties; and they will be welcomed by all interested in the subject. As the author informs us in some prefatory remarks, he has brought these notes together as a guide to future workers, having himself greatly felt the want of some such aid.

We believe that Dr. Aitchison contemplates another journey to Persia in order to extend his researches into the products of the country. W. B. H.

Metal-Turning. By a Foreman Pattern Maker. (London: Whittaker and Co., 1890.)

THIS book is written to explain and illustrate the practice of plain hand turning, and slide-rest turning as performed in engineers' workshops. The ornamental section of the craft is therefore entirely excluded. No attempt is made to describe all the numerous types of heavy lathes which are to be found in large workshops. The author tells us this in the preface, his object evidently being to treat the subject thoroughly from the works point of view, and not from that of the amateur manufacturer of ivory boxes and the like. The section on tools and tool angles is excellent, and will repay careful reading by turners; many, who are in all other respects good workmen, often make a fearful hash when grinding their tools.

When the much-abused "practical man" is induced to give his experience to the world in the form of a book, there is generally something worth reading and remembering to be found. This is the case in the work before us, which can be well recommended to all interested in its subject-matter. N. J. L.

The Century Arithmetic (complete.) (London: Blackie and Son, Limited, 1890.)

IN glancing through the pages of this volume, we are struck by the immense number of examples that are inserted; in fact, the work consists of practically nothing more than a series of exercises, commencing with simple addition and concluding with stocks. They have been arranged in an easy and progressive manner, and at the beginning of each new series of examples a typical case is worked out fully, and at the conclusion are mental tests which are given by inspectors. Intended as they are for Board schools and the like, the exercises should be found most instructive, for the subject can be grasped best by the constant working out of examples such as those that are here so copiously brought together.

LETTERS TO THE EDITOR.

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

Biological Terminology.

IN a recent number of NATURE (December 11, p. 141), Prof. T. J. Parker gives three suggestions in biological terminology, on which you will perhaps permit me to make one or two remarks, as his notes are *à propos* of some criticisms of mine on his paper on that subject (Proc. Austr. Assoc. Adv. Sc., vol. i., 1888).

(1) The term *agamobium* for the asexual generation in plants and animals, as suggested by Prof. Parker, certainly escapes the objection I raised to the term *blastobium*. I am not sure, however, that botanists, even though they might agree with Prof. Parker's general views on terminology, would be inclined to designate as an *agamobium* both the asexual state of a *Vaucheria* (for example) and the asexual generation of a fern. Prof. Bower in a recent paper (*Ann. Bot.*, vol. iv. No. 15) has drawn attention to the distinction made by Celakovsky between homologous and antithetic alternation of generations. The important differences between those two types of alternation would, I think, be in danger of being once more lost sight of if the asexual stages both of the homologous and of the antithetic type were known by the same name.

(2) My objection to Prof. Parker's use of the anglicized form of *ovarium* (ovary) would disappear, if "ovary," in its ordinary acceptation in botany, could be got rid of altogether, "ovule" of course following in its train. Megasporangium (for "ovule") is now generally in use, and it is possible that in time "ovary" may give place to some more suitable term, which will express the true morphological value of the structure known as the "ovary." For uniformity's sake one might wish to see either classical names or their English equivalents used throughout, and since we have already adopted *megasporangium*, *microsporangium*, *ovum*, *sperm* (or *spermatozoon*), &c., and may adopt *gamobium* and *agamobium* (for which there are no English equivalents), the balance seems to turn distinctly in favour of the classical as opposed to the English names.

(3) Prof. Parker advocates the use of special terms for certain "important stages in plant development." All botanists will, I think, accept—at least in time—the term *oosperm* in place of "oospore." That this term ought to be applied to the *unicellular* embryo only there can be no question. Prof. Parker objects to the use of *oosperm* (in Haddon's "Embryology") to indicate the advanced mammalian embryo. I unite with him in his protest, but might add that *oosperm* as applied to an advanced embryo is far preferable to the term *ovum* for the same structure (McKendrick, "Text-book of Physiology," vol. ii. p. 746). In Quain's "Anatomy," for instance, such phrases as "in an ovum of from five to six weeks" (vol. ii. p. 765, 9th ed.) are of frequent occurrence. Whether names *are* required for successive stages in plant development is perhaps an open question. At all events, I cannot see that Prof. Parker is justified in applying the term *polyplast* to the fully formed sporogonium of a moss. A body with the histological differentiation observable in the sporogonium of *Polytrichum* or even of *Jungermannia*, cannot surely be designated by the same term as that applied to the body to which Haeckel gave the name of *morula*. If "highly differentiated," surely it has ceased to be a *polyplast*. Personally, I would feel inclined to use the general term *embryo* for all stages of development after segmentation has begun.

R. J. HARVEY GIBSON.

Botanical Laboratory, University College, Liverpool.

Streamers of White Vapour.

I STAND this morning in my private room in the University building, looking out over the harbour, and away to the open expanse of Lake Ontario. The shore is to the south of me, and runs east and west. The wind is blowing from the north, and hence from the land over the water. The temperature of the air is about 3° or 4° F., and the temperature of the water is about 45° F.

The whole surface of the water, as far out as the eye can distinguish, is covered with a perfectly white mist like a low-lying fog, so that the appearance of steamers on the bay is that of vessels drifting through a cloud bank, but with the higher upper works and the smoke-stacks mostly elevated above the cloud.

The most peculiar phenomena, however, are the streamers of white vapour which rise in straight or spiral columns, very limited in breadth, but reaching to a great height. These may be seen by hundreds, some rising to a greater height, some to a less, although from the large extent of surface under view, the more prominent ones cannot be very near together. I have watched these streamers from year to year with great interest, for they always present themselves at the first appearance of very cold weather at the coming on of winter, and before the surface of such an immense body of water has had time to cool down after the summer heats.

You will see one of these streamers start up from its apparent bed of cloud, and, with a sort of wriggling, twisting motion, make its way upwards to a great height in a very few seconds, and drift slowly along with the wind, like a spindling but giant column of steam. They, in the meantime, undergo a slow but incessant change, and sometimes they gradually vanish at the top while being renewed from below, while at others they detach themselves at the base and float heavenward until they vanish in mid air or lose themselves in the overhanging and distant cloud.

The whole phenomenon reminds me very forcibly of the pictures of the solar streamers which are to be seen in various illustrated works on the sun, and the two are probably brought about by somewhat similar causes, although the difference in the degree of action must be almost infinite.

I have no means of determining the heights of these streamers except approximately. There is, however, a wooded island about one half a mile wide, and the distant shore of which is about five miles from the city. It is a common thing, under favourable circumstances, to see the streamers rise from the further side of that island to a height of from ten to twelve times the height of the tallest trees on the island, which would give a height of at least about 500 feet; and as the base of these streamers may be at some distance beyond the island, their height may occasionally be considerably above this estimate.

The continual shooting upwards of these, and their continual motion and change, offers a phenomenon at once very interesting and very beautiful. I have never seen the appearance at its best to last above five or six hours, and any second appearance is always inferior to the first. I may add that the same scene presents occasionally, in the spring, some of the finest mirages to be seen anywhere. N. F. Dupuis.

Queen's College, Kingston, Canada, December 2.

On the Affinities of Hesperornis.

Among some very useful and important papers recently issued by the Museum of Zoology of the University College, Dundee, appears one by Prof. D'Arcy Thompson, "On the Systematic Position of *Hesperornis*." In this excellent scientific brochure, Prof. Thompson has critically compared in detail the skeleton of a diver (*Colymbus septentrionalis*) with the skeleton of *Hesperornis*, as presented us by Marsh; and the outcome of this investigation has fully convinced the author of the work in question of the kinship, which undoubtedly exists, between the extinct *Hesperornis* and the existing pogopodine birds, as the divers, loons, and grebes. We have long been satisfied of these affinities, and firmly believed that the *Colymbidæ* were the descendants, perhaps direct descendants, of the toothed birds of the genus *Hesperornis*. It required but such comparison as has been so ably instituted by Prof. Thompson to make it quite clear to any thoughtful anatomist; and, as he hints, a similar comparison will probably go to show the fact that another extinct toothed bird-form, *Ichthyornis*, lies in the line of descent of the terns and their allies.

It is said by those who are opposed to these opinions that the agreement in skeletal characters between two such forms as *Hesperornis* and *Colymbus* are but superficial, and due to the fact that the birds did have in the case of the first, and now have in the case of the divers, similar habits.

The advocacy and adoption of such a view as this could but tend to mask their real affinities, impede the solution of the natural taxonomy of extinct avian forms, and be dangerous to the proper use of osteological characters in the premises. Hardly would "similarity of habits" produce morphological likenesses in such bones as the vomer, the pterygoids, the occipitals and the condyle they form, and a number of others which practically agree in *Hesperornis* and the *Colymbidæ*, and are very different in the *Ratitæ*.

So far as such a bone as the sternum is concerned, the fact whether it be "keeled" or "not keeled" must be used with no little caution when we come to decide upon the affinities of bird-forms, be they extinct or otherwise. And were the fossil remains of birds, which formerly existed upon the earth, in our possession in sufficient number and variety, I am quite sure that true avian kinships could not be established upon any such hard and fast lines as to whether or no their sterna were "carinated" or "non-carinated." We would undoubtedly meet with ostrich-types that could fly, and so have keeled sterna; and also ratite-types that enjoyed not such volant powers, and consequently possessed the "raft-sternum," as did *Hesperornis*, our great extinct ancient diver, which, as we know, was flightless, raft-sternumed, but withal, in the remainder of its skeleton, presenting all the fundamental characters of the now-existing Pygopodes, especially in so far as they are represented by the grebes and loons. R. W. Shufeldt.

Takoma, D.C., December 5.

A Swallow's Terrace.

In your issue of November 27, Mr. Warde Fowler gives a description of an unusually straggling nest of the swallow. An example to the contrary, of extreme neatness, which came under my notice a year or two ago, and which I still preserve, is the following:—My brother, on entering an old cottage in Somersetshire which had been empty for a long time, found in one of the rooms a lath, broken at one end, depending from the ceiling at an angle of about 30°. The lath was about 18 inches long, and on the free end was a swallow's nest containing four very handsome eggs, heavily marked with large blotches of purple-brown. The nest was perfectly circular and shallow, like a tea-saucer, its external dimensions being about 5 inches diameter by 2 inches deep. It was built of the usual materials, the exterior being of mud, with which it was secured to the lath, and the lining of hay with an inner lining of feathers. Close by were other swallows' nests, just inside the top of a chimney and quite open to the sky, so that a covered site does not seem indispensable if the nest be sufficiently sheltered. In view of this and the preceding description of nest with its fragile support, it would not appear surprising to hear of a swallow building on the branch of a tree provided it were in a well sheltered situation. Robert H. Read.

Cathcart, Glasgow, December 17.

Nests of the Red-backed Shrike.

Whilst writing on the subject of nests, I would like to remark that I examined three nests of the red-backed shrike this last summer, and that the colour of the linings appear to bear out the remarks of a correspondent which appeared in Nature some months ago. Two of the nests contained eggs of a pale pink ground-colour, whilst the eggs in the third were of a creamy-white ground-colour. In the third nest the lining was of roots, a few black horsehairs, red and white cowhair and a little wool. In the first two nests there was no white hair or wool, the lining consisting chiefly of roots and red cowhair. Although this seems to corroborate the experience of your correspondent, yet the difference in ground-colour of the three sets of eggs was so comparatively slight that I would not like to infer from these three nests alone that the colour of the nest-lining had any significance from a protective point of view.

Cathcart, Glasgow, December 17. Robert H. Read.

"Fire-ball" Meteor of December 14.

It will probably be of interest to many besides your two correspondents—whose letters are respectively dated from Sittingbourne in Kent, and Loughton in Essex—to learn that the remarkable meteor they describe was also observed about the time mentioned by them (9.45) in this neighbourhood.

Dr. Dixey, of Finchley, described it to me on the following day (Monday), remarking that "it would be sure to be in the papers"; and, moreover, the staircase of this house was brightly lit up, through the skylight above it, also at 9.45, I observing to my wife, who came to tell me of it, that the light probably came from a large meteor. My friend Dr. Dixey told me that he did not notice any trail. James Turle.

North Finchley, Middlesex, December 18.

THE FOSSIL MAMMALS OF NORTH AMERICA.[1]

THIS important contribution to our knowledge of the extinct mammals of the United States is the joint production of Profs. W. B. Scott and H. F. Osborn, of the Geological Museum at Princeton College, to whom we are already indebted for much valuable work on the subject. The present memoir is of more than ordinary importance, since the authors have endeavoured to complete our knowledge of forms already more or less fully described, rather than to add fresh burdens to the memory by recording a host of so-called new species and genera founded upon specimens which are not sufficiently characteristic to prove their distinctness from forms already described. Indeed, they have taken the opposite course, and endeavoured to show how the number of such species and genera may be reduced; not shrinking, as the manner of some is, from relegating when necessary some of the terms proposed by themselves to the rank of synonyms. This line of work, we are assured, is the one now urgently called for, as it is almost certain that the number of names which have been already proposed must, if properly defined and correlated, really include by far the greater proportion of the animals of the better known formations.

The work is divided into four parts; the first and second being by Prof. Scott, the third and fourth by Prof. Osborn. Part I. treats of the geological and faunal relationships of the Uinta beds; Part II. includes those mammals referable to the groups known as Creodonts, Rodents, and Artiodactyle Ungulates; Part III. is devoted to the Perissodactyle Ungulates; while the concluding Part is an endeavour to trace the gradual modification of the foot-structure of the Ungulates from the generalized older forms to the specialized types found at the present day.

In the first part we are told that in the Upper Green River valley in Colorado there are three well-marked groups of Tertiary strata overlying the Upper Cretaceous Laramie beds, and named, in ascending order, the Wasatch, Green River, and Bridger Eocene groups; the earliest Puerco Eocene being apparently missing between the Laramie and the Wasatch beds. The Bridger, or Middle Eocene, which is further divisible into three minor groups, is characterized as a whole by the presence of the now well-known Dinocerata, so fully described by Profs. Marsh and Cope. The geology of the Uinta beds and their relation to the Bridger group appear to be somewhat obscure; but it seems that while part of these beds may be contemporaneous with the Bridger, the greater portion is decidedly newer, and consequently that the entire group should be classed as Upper Eocene, and regarded as forming the transition to the Miocene beds of the White River. These Uinta beds are readily distinguished from the Bridger group by the absence of the remains of Dinocerata; and their fauna of Perissodactyle Ungulates is described as being intermediate between that of the Bridger Eocene and the White River Miocene. The genera of mammals which the authors record from these beds include (1) *Mesonyx* and (2) *Miacis* among the Carnivorous types, (3) the Rodent *Plesiarctomys*, (4) the Lemuroid *Hyopsodus*, in the Artiodactyle Ungulates (5) *Protoreodon*, and (6) *Leptotragulus*, and in the Perissodactyles (7) *Diplacodon*, (8) *Isectolophus*, (9) *Triplopus*, (10) *Pachynolophus*,[2] and (11) *Amynodon*. Of these genera, Nos. 5, 6, and 7 are peculiar to the Uinta beds, while all the others are represented in the underlying Bridger group.

Of the forms described in Parts II. and III., we shall merely notice a few of those which are of more especial interest. The first of these is the Rodent genus *Plesiarctomys*, first described by Bravard upon the evidence of very fragmentary remains from the European Tertiaries, but now fully known through the specimens described in this memoir. Dr. Scott regards this form as one of, if not actually the oldest of the known Rodents, and as therefore entitled to especial interest from an evolutionary point of view. He finds that the molar teeth (Fig. 1) are of the tritubercular type so characteristic of the earlier

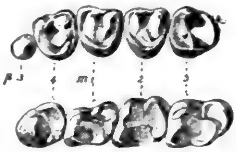


FIG. 1.—The left upper and lower cheek-teeth of *Plesiarctomys sciuroides*.

Eocene mammals of all orders (see NATURE, vol. xli. p. 465), and therefore concludes that the Rodents were probably derived from the same generalized group of mammals which has given origin to the existing Carnivora and Ungulata. *Plesiarctomys* itself should apparently be placed among the existing Sciuromorpha (squirrels and marmots), although in certain generalized features of the skull it shows signs of affinity with the Hystricomorpha (porcupines).

Another form of considerable interest is the genus *Leptotragulus*, a small Ungulate at first regarded as allied to the existing chevrotains (Tragulina), but which proves to be the earliest definitely known ancestral type of the camels and llamas (Tylopoda). This genus is indeed now regarded as the direct ancestor of *Poëbrotherium* of the overlying White River Miocene, the latter being an early cameloid type not larger than a fox; and thus affords another example of the rule that all groups of mammals increase in the size of their representatives with the advance of time. Other observations induce the author to suggest that these early Cameloid types may themselves have originally branched off from the little *Dichobunus* of the Eocene of Europe and probably also of America—a small chevrotain-like Ungulate, with bunodont molars carrying five cusps on their crowns. If this full phylogeny be substantiated by later researches it will be of extreme interest.

The Artiodactyle Ungulate described as *Protoreodon* is another annectant genus of more than ordinary interest. Thus while it conforms to the Miocene Oreodonts in the structure of the feet, and in the peculiar feature that the first lower premolar assumes the form and functions of a canine, its upper molar teeth differ in that they have five instead of four cusps on the crown, and thus accord with those of the generalized hog-like Ungulates known as *Anthracotherium* and *Hyopotamus*. This is a very important fact pointing very strongly to the derivation of the Oreodonts from an early stock more or less closely allied to the known *Anthracotheriidæ*.

With the Perissodactyla we come to the work of Prof. Osborn, and some important observations are made, in the introductory portion of Part III., regarding the synonymy of some of the earlier forms of the ancestors of the horse. It is there stated that Prof. Marsh's genus *Eohippus* is identical with Owen's *Hyracotherium* of the London Clay, from which *Pliolophus* appears to be likewise inseparable. The distinctive feature of this genus is that the fourth premolar in both jaws is unlike the first molar, the fourth upper premolar having but a single inner cusp. *Orohippus*, however, which has been hitherto identified with *Hyracotherium*, is shown to be distinct, the fourth premolar being as complex as the true molars; this genus is, however, identical with the European *Pachynolophus*. *Epihippus*, in which both the third and fourth premolars become like the molars, forms the next step in the ancestry of the horse, leading on to the well-known European genera

[1] "The Mammalia of the Uinta Formation." By W. B. Scott and H. F. Osborn. Transactions of the American Philosophical Society, Ser. 2, Vol. XVI., Part III. Pp. 111, plates 5. and woodcuts. (Philadelphia, 1889.)
[2] Incorrectly *Orotherium* in the text.

Anchilophus and *Anchitherium*. An interesting section is devoted to the rhinoceros-like animals described under the names of *Amynodon* and *Metamynodon*, the latter occurring in the White River Miocene. These genera are regarded as representing a distinct family, distinguished from the living rhinoceroses, among other features, by the resemblance of the last upper molar to the two preceding teeth. We are scarcely disposed to regard this and the other features mentioned as of sufficient importance to justify the formation of a distinct family, but this is purely a matter of opinion. *Amynodon* has been generally regarded as the ancestor of the modern rhinoceroses, but Prof. Osborn points out several objections to this view, and also shows that *Metamynodon* is clearly a separate branch from *Amynodon*, departing still more widely from the modern rhinoceroses. It is suggested, however, that the real ancestor of the latter will prove to be more or less closely allied to *Amynodon*.

Of the other Perissodactyles described in the third part, it will suffice to mention that *Isectolophus* is regarded

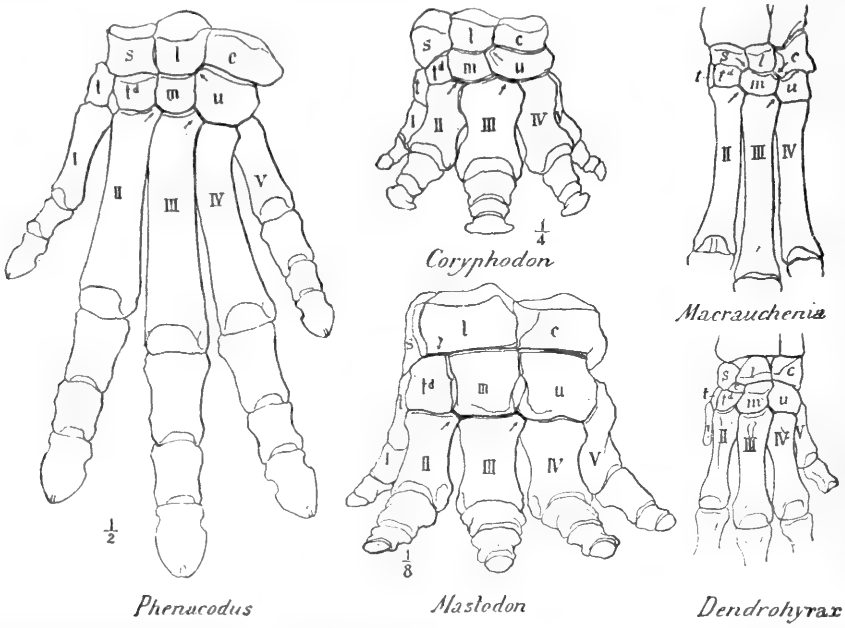


FIG. 2.—The left fore-foot of various Ungulates, to show the more generalized condition, in which the scaphoid (*s*), lunar (*l*), and cuneiform (*c*), are respectively placed directly over the trapezoid (t^d), magnum (*m*), and unciform (*u*). In *Mastodon* the lunar has extended over the trapezoid. *t* = trapezium.

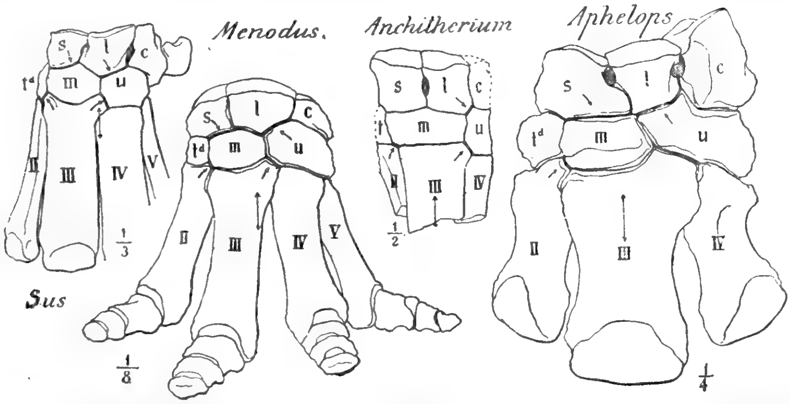


FIG. 3.—The left fore-foot of more specialized Ungulates, showing the displacement and mutual interlocking of the carpal bones. Letters as in Fig. 2. The vertical arrows indicate the median line of the foot, and the oblique ones the direction of displacement.

as an ancestral type of the tapirs, in which the fourth, and probably the third, upper premolar approximated to the type of the molars. The author considers that an imperfectly known tapiroid from the White River Miocene will prove to have three upper premolars of a molariform type, and would thus lead on closely to the true tapirs, in which all the premolars are molariform.

These observations show that in each of the three existing families of Perissodactyles there is a gradual advance in the complexity of the premolars, till in all the living types they become as complex as the molars.

In the concluding part, relating to the advance from a plantigrade and pentadactylate type of foot in the Ungulate to the digitigrade type with a reduced number of digits, Prof. Osborn lays great stress on the effects of strain and impact as leading to the gradual displacement and thence abortion of the lateral elements in the wrist, ankle, hand, and foot, supporting his conclusions with several diagrammatic figures, of which we reproduce two. The author observes that the feet of the modern Ungulates are connected with the simple type of foot found in the Ungulates of the Puerco Eocene by the genus *Phenacodus*, and that without this annectant form it would have been almost impossible to say that the Puerco mammals were Ungulates at all, since their feet are more like those of the plantigrade Carnivores. The details of how the foot of the generalized primitive type has become gradually modified into the numerous modifications exhibited by the Ungulates of the present day, are far too technical and complicated to be even touched upon in these pages, and we must therefore refer the reader desirous of entering upon this difficult branch of study to the memoir itself. We may, however, mention that the primitive type of foot (Fig. 2) is characterized by the component bones of the two horizontal rows of the wrist and ankle being placed vertically one above another, over the axes of the digits they respectively support; while in the specialized types (Fig. 3) these bones mutually overlap and interlock, so as to totally obliterate the original vertical lines of division coinciding with the intervals between the individual digits. The nearest approach to the primitive type now remaining, is found in the elephant and the hyrax; but the elephant and its ally the mastodon (Fig. 2) are peculiar in that the lunar bone of the wrist has become extended towards the outer side so as to rest upon the trapezoid.

In conclusion, we may observe that a memoir like the present marks a distinct advance in our knowledge, not only of the mammals of the Upper Eocene of North America, but also in respect to several points in connection with the phylogeny of the Ungulates, and of the relationship of the extinct Old World representatives of that order to those of the new. We offer our congratulations to the authors, and look forward to seeing equally good work on the mammals of other horizons of the Tertiaries of the United States. R. L.

EARTHWORMS.

THE Colonial Office can hardly render a greater assistance to science, than by publishing such reports from our distant possessions as that contained in the October number of the *Kew Bulletin*, from Mr. Alvan Millson, Assistant Colonial Secretary at Lagos, who has also acted as a Special Commissioner to the interior, and who has made good use of his opportunities in observing Nature in those remote parts.

The report in question is contained in a despatch from Sir Alfred Maloney to Lord Knutsford, dated June 11, 1890, and we propose to give a short account of its contents.

In Yoruba Land, after passing the fringe of forest, which skirts the lagoons to the eastward of Lagos for some 50 miles inland, a vast tract of open country is reached, extending as far as the valley of the Niger and the Houssa States beyond. The only difference between these grass lands and the forest fringing the lagoons appears to be that "for many generations the farmers of Yoruba have with axe and fire destroyed the growing trees, and robbed the soil of its original covering, leaving nothing but a rank growth of tall and tangled grass to take its place." This is apparently unfavourable to the growth of deep-rooting trees, but shows a truly surprising surface-fertility, when subjected to cultivation; and as the Yoruba people are cut off from the coast by the tribes inhabiting the forest belt, and are entirely dependent on the soil for food and clothing, it has to support a considerable population.

The soil appears to consist of a sandy loam derived from the igneous rocks, ironstone or quartz conglomerate, which form the bed rock, the soil increasing in fertility where the harder strata give place to more friable micaceous rocks, but even where the soil is not over a foot in depth, the fertility is truly astonishing. Crops in Yoruba are not only of unusual excellence, but the surface soil shows a marvellous recuperative power, even when compared with that of favoured lands in other portions of the tropics.

In this district, after a very simple tillage with the hoe only, the following rotation of crops is raised. In the first year a crop of yams and Indian corn is planted, and a second crop of maize and beans in the autumn; the same in the second year; while in the third year both crops are maize and beans. No manure of any kind is used, nor any tool more powerful than the hoe; and then for two or three years the land lies fallow, after which it is ready for a similar rotation, and so on. A crop of Guinea corn, cotton, indigo, tobacco, and sweet potatoes is also in some places gathered.

In spite of the exhaustive system of cultivation pursued, the crops show no sign of falling off, and such is the inexhaustible fertility of this belt, that maize sells for 4½*d.* and sweet potatoes at 1*d.* per 70 pound load, and other products in proportion, even in large towns like Ibadau, said to have 150,000 inhabitants.

Mr. Millson considers that the fertility cannot be caused by termites, as described by Drummond in "Tropical Africa," as the ant-hills of the Yoruba land are exceptionally small and widely scattered; and visiting the country only in the wet season, it would appear impossible to account for the unusual fertility. In the dry season the mystery is at once solved, in a very simple and, Mr. Millson considers, in a most unexpected manner; although, taking into account the universal presence of the earthworm, both in temperate and tropical climates, we think there is little reason for surprise; but we do not remember to have heard of a more marked instance of the importance of small agencies in effecting great results.

Mr. Millson continues:—"The whole surface of the ground, among the grass, is seen to be covered by serried ranks of cylindrical worm-casts, varying from a quarter of an inch to 3 inches, and existing in astonishing numbers. For scores of miles they cover the land, closely packed, upright, and burnt by the sun into rigid rolls of hardened clay, which stand until the rain breaks them down into a fine powder. . . . On digging down, the soil is found to be drilled in all directions by countless multitudes of worm-drills; while from 13 inches to 2 feet in depth the worms are found in great numbers in the moist subsoil."

Mr. Millson estimates that the worm-casts, in one season, average over 5 pounds per square foot of soil; and at this estimate, which he considers very moderate, the annual result of the work done by the earthworms of Yoruba gives a total of not less than 62,233 tons of subsoil brought to the surface in each square mile of cultivated land every year. "This work goes on unceasingly year after year, and to the untiring labours of its earthworms this part of West Africa owes the livelihood of its people. Where the worms do not work, the Yoruba knows that it is useless to make his farm."

Mr. Millson estimates that every particle of earth in each ton of soil, to the depth of 2 feet, is brought to the surface once in twenty-seven years, which gives an average of 0·88 inch per annum, or four times as much as Darwin estimated ("Earthworms," p. 130) to be the case in the experiments tried at Maer Hall; but on the Nilgiries, worm-casts are found to average 3 ounces each, the largest weighing 4¾ ounces (*ibid.*, p. 129). We have, therefore, little doubt that Mr. Millson's estimate of the movement of soil is quite probable.

Mr. Millson considers that, most probably, the comparative freedom of this part of West Africa from dangerous malarial fevers is due, in part at least, to the work of the earthworms in ventilating and constantly bringing to the surface the soil in which the malarial germs live and breed. Darwin (*ibid.*, p. 239) remarked that the disappearance of organic matter from mould was probably much aided by its being brought again and again to the surface in the castings of worms, and this would account for the absence of malarial fevers.

The worms sent home by Mr. Millson have been submitted to Mr. F. E. Beddard, who reports that they belong to a probably new species of the genus *Siphonogaster*.

We trust that Mr. Millson will continue to send home further accounts of his valuable observations, and that the example he has set will be followed by others stationed in our colonies. S. N. C.

MUSEUMS FOR PUBLIC SCHOOLS.

ALL teachers, I have no doubt, have experienced the refreshing interest that boys and girls take in objects of natural history. And not in natural history merely, for whenever the lesson, no matter what it is about, admits of being illustrated by specimens, then the presence of these specimens gives a point to the description and produces an effect that otherwise would be wanting. Take your young audience in imagination into the country or to the seaside, and show the nest and bird, insect, plant, seaweed, or crab you are describing, and you know you can sustain their attention throughout. It is equally certain, too, that, in describing the characteristics of any particular place, it is desirable to be able to indicate by something more than the bare statement what is meant by "solan-geese and other sea-fowl," and such expressions. How vague often are the ideas produced by the words ruby, marble, granite, isinglass, and other "articles of produce," even though well described, when the actual article itself is bound to implant the nature of the object in the pupil's mind, and the description at the same time. How much could be taught in a very agreeable way, moreover, with a series of specimens exhibiting the manufacture of certain important fabrics from the raw material, and especially those that are made in the immediate district of the school.

It will be generally admitted, I think, that schools should be provided with the means of giving such instruction: the difficulty is, how can it be done? Suggestions have been made on several occasions that the specimens in our public museums should be made available for the purpose. Dr. James Colville, in a paper on "Public Museums as Aids in Teaching,"[1] has pointed out that he is allowed to draw upon the Glasgow Museum for specimens. In Liverpool, alone, however, as far as I know, is the circulation of museum objects carried out in a satisfactory and systematic manner. A good account of the method is given in a pamphlet on a "Proposed Circulating Museum for Schools and other Educational Purposes," by the Rev. Henry H. Higgins, published in 1884; and in the Report which was issued by Mr. Thomas J. Moore, the Curator, later in the same year. The Museum distributes a series of boxes of specimens, and these are removed from school to school at stated intervals.

In many districts, however, and especially where the public Museum is more or less remote, the system so generously inaugurated by the Liverpool Museum will not be found available. In some of our large provincial towns, indeed, where there is a Museum, such a scheme could not be carried out without a great extension of the work of the Museum, for which there is at present no provision. Where this is so, and alas, it is quite general, an attempt is made in many of our schools to gather together what must necessarily be a desultory and very limited collection. I am afraid that few schools, indeed, can show such a typical series of natural history specimens as that sketched by Prof. Flower in NATURE (vol. xli. p. 177).

Here, then, is a good work for the secondary schools which are now and again springing up all over the country. In each of these academies and high schools there is a science department, which, besides its chemical apparatus and the like, can boast, no doubt, of a number of objects gathered at random. And though these may be far more numerous than the specimens to be seen in the ordinary school in the next street, still, how far is the collection from being systematic and typical! The specimens are usually kept, moreover, in drawers, and at any rate with little attempt to display, arrange, or name them. But were the facilities for gathering specimens increased; were a room set apart and furnished with cases for their reception; were, in fact, the secondary school encouraged to get together a double series of objects—one for circulation in the surrounding district, and one for its own use—the benefits now being enjoyed in Liverpool might be extended to other towns without waiting for the public Museum to take the matter up. For each town, in this way, one or two reasonably typical collections, including actual specimens, models, and diagrams, would be provided; and the amount expended at present in individual endeavours would go far towards having this done with considerable completeness.

A good outline of what should be aimed at in such a museum is given in the article by Prof. W. H. Flower, cited above. He has also contributed a series of papers on "Methods," which will be found of the utmost service (NATURE, vol. xv. pp. 144, 184, 204); and it will be seen from these latter that it is quite possible to prepare a great portion of the museum in the school. Still, no better model could be copied than that provided at Liverpool, both as regards the choice of the specimens, and the method of circulating them. The pamphlets referred to above contain information on both points. I should only take the opportunity of recommending that which I have incidentally mentioned in the opening sentences, viz. the addition of important articles of commerce and manufacture.

With such a museum at the disposal of our schools, and provision made for its growth, and the display of a duplicate series of specimens in the secondary school itself, the results of our teaching would be far more real and lasting, and we might now and then touch a sympathetic chord, and awaken an interest having a lifelong influence. ALEXR. MEEK.

University College, Dundee.

[1] Read before the Philosophical Society of Glasgow in 1888.

JAMES CROLL, F.R.S.

BY the death of this well-known writer geological literature loses one of its most voluminous and able contributors Though not in the proper sense of the word a geologist, he had made himself well acquainted with many geological problems, and first attracted notice more than five-and-twenty years ago by the brilliance and suggestiveness of his attempts to solve them. He was born in 1821 at Little Whitefield, in Perthshire, and after the usual brief schooling of a peasant's son he was apprenticed as a millwright in his native village. The employment allowed him leisure for reading, and he devoted himself with ardour to the study of philosophy and of physical science. At the age of twenty-four, however, the effects of an accident which he had met with in boyhood compelled him to seek a less laborious vocation, and eventually he became agent for an insurance company. These early years gave but little promise of the particular bent of his genius by which he would attain distinction. Eventually his general acquirements and the zeal with which he was known to devote his spare time to philosophical reading attracted the interest of the governing body of the Andersonian University and Museum in Glasgow, and in 1859 he was appointed keeper at that establishment. He had already found his way into print by publishing anonymously a volume on the "Philosophy of Theism." But his new position in an institution devoted largely to the teaching of science led him to throw himself more fully into the study of physics. In 1861 he published, in the *Philosophical Magazine*, his first contribution to scientific literature—a paper on an electrical experiment of Ampère's.

About that time the Geological Society of Glasgow was founded, and became the centre of an active company of geologists who specially took up the study of the traces of the Glacial period, so striking and abundant in the west of Scotland. Croll was drawn into the prevalent enthusiasm, and soon with characteristic ardour and acumen began an investigation of some of the physical difficulties which had arisen in the course of geological inquiry. In 1864 he published his remarkable essay, "On the Physical Cause of the Change of Climate during the Glacial Epoch." This paper speedily attracted the notice of men of science. In it the author endeavoured to find a true cause for the extension of snow and ice during the Ice Age far beyond their present limits. For this purpose he invoked the aid of astronomical and terrestrial physics, and he provided an explanation which captivated geologists by its simplicity as well as by the wide range of phenomena which it helped to elucidate.

It was this paper which laid the foundation of his scientific reputation. It was likewise the means of opening up for him a new and more congenial employment, for it led to his being selected by the present Director-General of the Geological Survey to take charge of the maps and correspondence of the Survey in Edinburgh. He was appointed to this office in 1867, and found himself able to prosecute with more vigour than ever the researches in physical geology which had now so great a charm for him. The question of the origin of climate led him into a far wider field of investigation than he had at first contemplated. It brought him face to face with many theoretical problems which geologists had been unable to solve. These he attacked with characteristic energy. He enforced his arguments with a single eye to the discovery and establishment of truth, and exposed without reserve views which seemed to him erroneous. With no intention of rousing controversy, he soon found himself in collision with other writers who disputed his arguments. One of the most interesting and vigorous of these disputations was with the late Dr. W. B. Carpenter, regarding the theory of Oceanic Circulation. Croll maintained with great force and with general approbation the position for which he contended, that the prime motors in the circulation of the ocean were the winds. After publishing many papers on this and cognate subjects, he collected, condensed, and partly re-wrote these, adding fresh materials to them, and issuing the whole as his well-known work on "Climate and Time in their Geological Relations," which appeared in 1875. Though much division of opinion was aroused as to the real value of some of his views in relation to the establishment of sound geological theory, there was a general recognition of the originality and acuteness of his mode of dealing with accepted facts and principles, and of the value of his writings as stimulating and directing inquiry. He was accordingly elected a Fellow of the Royal Society in 1876, and the University of St. Andrews conferred on him the degree of LL.D.

By degrees, however, Dr. Croll's health began to fail. He suffered so intensely from pains in the head that he was compelled, in 1881, to resign his appointment in the Geological Survey, and retire on the miserably small pension to which, by the rigid rules of the Civil Service, his length of service only entitled him. By exercising the greatest care he was still able at intervals to resume his studies in geological physics, and to publish occasional papers, partly in reply to his critics, who were now increasing in number and pertinacity. In 1885 he published a smaller volume embracing some of these papers, and much new material under the title of "Discussions in Climate and Cosmology."

Dr. Croll's investigations into the geological history of terrestrial climate had led him to consider the question of the origin of the sun's heat, and thence to reflect on the probable condition and development of nebulæ and stars. The later chapters of the volume just mentioned were devoted to these subjects, which he would fain have discussed more at length, had not the increasing failure of his bodily powers warned him that, if he wished still to return to that philosophy which was his first love, he must husband his remaining strength. Nevertheless, the attraction of these astronomical problems proved insuperable. He continued to work at them, gradually enlarging the scope of the investigation until it embraced not the earth and the sun merely, but the origin and development of the whole material universe. At last he followed his usual method, gathered together his various contributions to the subject, trimmed, enlarged, and modified them, and published them in a separate volume, entitled "Stellar Evolution and its Relation to Geological Time."

The publication of that work marks the close of his labours in more definitely scientific inquiry. He was now free, with such remaining strength as he could command, to re-enter the field of philosophical speculation in which he had spent his earliest years of mental exertion, and which for nearly thirty years, through all the engrossing attractions of geological inquiry, had never lost its fascination for him. Accordingly he betook himself once more to the study of such subjects as Force, Matter, Causation, Determinism, Evolution, and proceeded to apply the facts and principles with which he had in the interval been dealing so actively to the problems in philosophy that had aroused his thoughts in the early years of his life. In spite of his increasing infirmity, he persevered in committing to writing the ideas which he had now matured, and this year he sent to press his last work, published only a few weeks ago, "The Philosophical Basis of Evolution."

Of all recent writers who have contributed so much to current scientific literature, probably no one was personally so little known as Dr. Croll. His retiring nature kept him for the most part in the privacy of his own home. But he endeared himself to those who were privileged with his friendship by his gentleness and courtesy, his readiness to help, and the quiet enthusiasm with which he would talk about the topics which absorbed his thoughts. After quitting the Geological Survey of Scotland he tried residence at different places in hopes of finding one where his failing bodily health would least impede the powers of his mind, which he retained with singular freshness up to the close. He settled at last in the town of Perth, where he spent the few remaining years of his life. Struggling on in spite of several ominous warnings, he finished his last book just before the final stroke which carried him off on Monday, the 15th inst.

A. G.

NOTES.

THE President of the Board of Trade has appointed a Committee, to consider whether any, and if so what, steps should be taken for the provision of electrical standards. The following are the members of the Committee:—Lord Rayleigh and Sir William Thomson (representing the Royal Society), Prof. G. Carey Foster and Mr. R. T. Glazebrook (representing the British Association for the Advancement of Science), Dr. John Hopkinson and Prof. W. E. Ayrton (representing the Institution of Electrical Engineers), Mr. E. Graves and Mr. W. H. Preece (representing the General Post Office), Mr. Courtenay Boyle and Major P. Cardew, R.E. (representing the Board of Trade).

The first meeting of the Committee will be held at the Board of Trade on Thursday, January 15. Sir Thomas Blomefield, of the Board of Trade, will act as secretary to the Committee.

There ought to be no doubt as to the response to the appeal which has been addressed to Lord Salisbury with respect to Egyptian monuments. As everyone knows, they are among the most interesting monuments in the world, and it is scandalous that they should be exposed to wanton injury. The signatures to the memorial include many eminent names, and we may hope that a suitable inspector will speedily be appointed.

The Medical Faculty of University College, Dundee, St. Andrews University, in conjunction with the Royal Infirmary of that city, have appointed Prof. Percy Frankland and Dr. Stalker as delegates to visit Berlin in order to study Dr. Koch's new method for the treatment of tuberculosis.

The committee appointed by the Royal Horticultural Society to consider the best way of doing honour to the memory of the late Mr. Shirley Hibberd met on December 19. They unanimously decided that a portrait of Mr. Hibberd should be secured, and placed in the hands of the trustees of the Lindley Library, on behalf of the Royal Horticultural Society. Any surplus that may remain after the payment of expenses connected with the preparation of the portrait is to be invested for the benefit of Mr. Hibberd's orphan daughter. Dr. Masters is chairman of the committee appointed to carry out the scheme.

The Lord President of the Council has appointed Dr. C. Le Neve Foster, Inspector of Mines under the Home Office, Professor of Mining in the Royal College of Science, London.

The Board of Agriculture has approved of the scheme for establishing a Chair of Agriculture at the Yorkshire College, and it has been decided that a Professor shall be appointed at a salary of £300, with emoluments in addition.

In an interesting pamphlet on agricultural education, Mr. P. McConnell, Lecturer on Agricultural Science at Balliol College, Oxford, refers to the supposed want of teachers competent to teach scientific farming. In his own experience in the matter—and it has not been little—he has found that there is no such want. On the contrary, he says, the great want is the want of students to be taught by such teachers as we have. He contends that agricultural science can be taught at any College where there is already a science curriculum, and that, in fact, "it is only there that we can hope to have the matter developed on a system cheap enough to have it brought within the reach of ordinary farmers."

The following are the probable arrangements for scientific lectures at the Friday evening meetings at the Royal Institution before Easter 1891:—January 23, Lord Rayleigh, F.R.S., some applications of photography; January 30, Lord Justice Fry, British mosses; February 6, Prof. J. W. Judd, F.R.S., the rejuvenescence of crystals; February 13, Prof. A. Schüster, F.R.S., some results of recent eclipse expeditions; February 20, Edward Emanuel Klein, F.R.S., infectious diseases, their nature, cause, and mode of spread; March 6, Prof. Dr. J. A. Fleming, electro-magnetic repulsion; March 20, Prof. W. E. Ayrton, F.R.S., electric meters, motors, and money matters.

Prof. Dewar will deliver at the Royal Institution a course of six lectures (adapted to a juvenile auditory) on frost and fire, on the following days, at three o'clock:—Saturday, December 27, Tuesday, December 30, Thursday, January 1, Saturday, January 3, Tuesday, January 6, Thursday, January 8.

Messrs. Newton and Co. are making for the Royal Institution a new electric lantern, which will be first used in Prof. Dewar's Christmas lectures. The arc light is fixed on an adjusting table, and the body of the lantern revolves so as to bring any one of the fronts opposite the fixed light.

Mr. C. Callaway, D.Sc. (Lond.), has been directed by the Shropshire County Council to inquire into the needs of the county with regard to scientific instruction, and to present to the Technical Education Committee of the Council a report on the subject.

In the *Kew Bulletin* for December there is a letter from Mr. G. T. Carter, C.M.G., on the attempts which have been made to develop the agricultural industries at the Gambia. It affords, as the *Bulletin* observes, striking evidence of the almost insuperable physical difficulties which stand in the way of the permanent success of such efforts. The *Bulletin* notes that by the West African agreement between Great Britain and France of August 10, 1887, a frontier line between the English and French possessions was established. In accordance with its provisions a Special Commission of Delimitation was appointed to trace upon the spot the line of demarcation between the English and French territory. The partition of tropical Africa amongst European nations has made it more than ever a matter of importance to procure materials for, at any rate, a rough survey of its botanical resources. It was desirable to use the opportunity presented by the Gambia Delimitation Commission for the purpose. The Colonial Office was, however, unable to find the funds for attaching a botanist to the staff. But they were willing to allow a medical officer to be selected who would do what was possible in the way of botanical exploration. The appointment was accepted by Mr. J. Brown-Lester, M.B., C.M., of the University of Edinburgh. He was supplied with the necessary botanical outfit from Kew, and left Liverpool with the expedition in the s.s. *Congo* on November 15.

In addition to the section on cultural industries at the Gambia, the *Kew Bulletin* for December contains much valuable information on the production of prunes in the south of France, the cultivation of perfumery plants in the colonies, banana disease in Fiji, and fibre productions in the Caicos.

We are glad to see that the City Commission of Sewers are devoting serious attention to the smoke nuisance. At a recent meeting the Sanitary Committee brought up a report relating to frequent complaints on the subject in the City. They stated that for years past Bills had been introduced into Parliament giving local authorities greater powers to deal with such cases, but none of these measures had become law. They recommended that in the next suitable instance a prosecution should be directed against the offenders. Dr. Sedgwick Saunders, medical officer of health, said he had seen the experiments on the Embankment which were being conducted by Mr. Elliott, of Newbury, and the results were eminently satisfactory, especially so far as they related to smoke from furnaces and shafts. They were not so satisfactory in regard to domestic fires. The report was carried, and the Sanitary Committee was directed to investigate and consider the best means of abating the smoke nuisance, and to report thereon to the Commission.

According to a telegram from San Francisco, there has recently been an eruption of the volcano at Macao.

The Report of the Meteorological Council for the year ending March 31, 1890, shows that the work included under the three departments—ocean meteorology, weather telegraphy, and meteorology of the British Isles—is actively carried on. Owing to the want of information in the Pacific, which was felt on the occasion of the Samoa cyclone in March 1889, the Council have decided to establish stations on some of the islands there, with the view of obtaining further observations. The information received by telegraph as to the state of the weather on the British coasts is now conspicuously displayed outside the office twice daily for the use of the public. Among the various reports issued, the Weekly Weather Report merits special mention, as it supplies a very complete and instructive view of the weather changes day by day over the greater part of Europe. Among the various experiments which have been carried out may be mentioned those for determining the pressure of wind on plates of various sizes and inclined at different angles. These experiments have been made with great skill by Mr. W. H. Dines, and the results have been partially published. The determination of solar radiation is a matter upon which considerable uncertainty exists, as no two instruments can be depended upon for giving similar results under similar circumstances. Experiments with two sets of Violle's actinometer apparatus, consisting of *boules conjuguées*, or two hollow copper spheres, one coated with lamp-black and the other gilt, surrounding the bulbs of two thermometers precisely similar in all respects, have been under trial at Kew Observatory, with the view of determining whether their relative values are constant. The result of the experiment is not yet published. The Report tates that there are about 11,000 volumes and pamphlets in the library, containing meteorological data from all parts of the world. The books and other documents are accessible to scientific men for reference at the Meteorological Office; and to facilitate inquiry, reference catalogues are prepared both under authors and subjects.

ONE of the decisions of the last Congress of Russian Naturalists was to the effect that a *Meteorological Review* should be published in Russian, under the direction of the Academy of Sciences. The first volume has been issued, and contains several valuable papers, namely:—On the comparison of the normal barometers of the West European Observatories, by A. Shenrock; on the average temperatures which can be deduced from observations of the maximum and minimum thermometers, by E. Leist; on magnetic observations in Caucasia, by E. Assafrey; and on the Lena, by E. Stelling; on the thunderstorms in Russia in 1886, by E. Berg (9544 observations at 549 different places having been taken into consideration); and on the accumulations of snow on Russian railways, by B. Sreznevski.

THE Board of Editors at the Columbia College, New York, have decided to publish in January the first number of a monthly review, which will be devoted to "the scientific study and discussion of education." It will be known as the *Educational Review*, and will include within its scope education of every grade, higher, secondary, and primary. Each number will contain signed articles on topics of current interest, critical notes and discussions on educational subjects, "an account of important movements in educational thought throughout the world, and results of contemporary psychological research so far as the same have an educational significance."

AN important work on Russian ethnography is being published by A. Pypin. The first volume contains the history of ethnographical research in Russia, and deals with the ethnography of the great Russian stem.

STUDENTS of ethnography will be sorry to hear that Adrian Jacobsen, who has done so much good service by the ethnographical collections he has brought together, finds his scientific labours so unprofitable from a material point of view that he is compelled to abandon them. The attention of Dr. Bastian was attracted to Jacobsen by a most valuable collection he had formed in Greenland, Lapland, Labrador, and elsewhere in 1876–80. The Ethnological Committee founded by Dr. Bastian commissioned Jacobsen to collect objects for the Berlin "Museum für Völkerkunde"; and from 1881 to 1883 he devoted himself to the accomplishment of his task, bringing back to Berlin from the north-western regions of America no fewer than 8000 specimens. In the same service he travelled in 1884 and 1885 among the peoples of Eastern Russia and in many parts of Asia, returning with 3000 objects of great interest; and in the course of a third journey, in 1887 and 1888, he secured 5000 specimens in Singapore, Java, Celebes, and other regions. There are about 90,000 objects in the Berlin Museum, and more than one-sixth of them have been obtained by this skilful and enthusiastic collector, whose devotion to his chosen mission has often placed his life in danger. *Globus*, by which these facts are recorded, finds it hard to believe that "for such a man Germany has no further work and no further gratitude."

La Nature records an interesting archæological discovery which has lately been made near Apt, in Vaucluse, in the valley of the Caulon. M. Rousset, a retired inspector of forests, while superintending the digging of a ditch, was lucky enough to find, on a bed of pebbles, at a depth of 2·50 metres, the remains of what seems to have been a prehistoric workshop. The flint implements had such sharp edges, and were generally in so excellent a state of preservation, that they had evidently never been disturbed from the time when they had been chipped into shape. Among the objects were three nuclei, and students who have been examining them have succeeded in reuniting with each nucleus the fragments broken from it. Thus it is possible to note exactly the procedure of the prehistoric workmen.

M. EDOUARD MARBEAU contributes to the current number of the *Revue Française de l'Étranger et des Colonies* an instructive paper on what he calls "the depopulation of France." From Prof. Léon Le Fort he quotes the following comparison between France and other European countries. For every group of 1000 inhabitants there are born in Hungary 42 children; in Germany, 39; in England, 35; in France, 25. In 1778 the number in France was 38·4. At the present rate of increase, the population would be doubled in Saxony in 45 years; in England in 52 years; in Prussia, in 54 years; in France, in 198 years. If the period of 1886–89 were taken, the time for the doubling of the French population would be 349 years. The slow rate of increase excites much anxiety among thoughtful Frenchmen. It compels them, as M. Marbeau says, to regard "the foreign invasion" as a benefit. At the last census, there were in France 1,137,037 foreigners, three times as many as were to be found in England and Germany combined. These immigrants come chiefly from Belgium, Germany, Italy, and Spain. Recent inquiries show that only 408,000 persons of French birth are living out of France. Of these, 50,000 are in Switzerland; 26,000 in England; 17,000 in Spain; 10,000 in Italy; 55,000 in Belgium; 100,000 in the United States; 60,000 in La Plata.

THE eighteenth fasciculus of M. Fabre's "Traité Encyclopédique de Photographie" (Gauthier-Villars) gives a very useful and interesting account of the methods employed in astronomical photography of every description, and the decisions of the International Star-charting Congress.

DR. G. B. LONGSTAFF'S "Studies in Statistics, Social, Political, and Medical," will be issued by Mr. Edward Stanford on January 12.

THE following science lectures will be given at the Royal Victoria Hall during January:—January 6, the moon, by Dr. Fleming; January 13, glaciers, by Prof. A. H. Green; January 20, our bodies, by Dr. P. H. Carpenter; January 27, all about spectacles, by Dr. Collins.

AT a recent meeting of the Asiatic Society of Japan, held at Yokohama, Admiral Belknap, of the United States Navy, read a paper on the depths of the Pacific Ocean. Admiral Belknap was in Japan in 1874, in command of the U.S.S. *Tuscarora*, engaged in surveying the proposed route for a Pacific submarine cable. The greatest depth found on the voyage was 3287 fathoms, and H.M.S. *Challenger*, then on her exploring voyage, had not discovered anything in the Pacific of as great a depth. But upon leaving Yokohama the *Tuscarora* made very deep soundings. Only 100 miles from the coast 3427 fathoms were found, and a little further on 4643 fathoms went out without bottom being touched. Still keeping on the great circle track a number of soundings of over 4000 fathoms were made, the deepest being 4655. After re-coaling at Hakodate a fresh departure was made, and the Kuriles were skirted, and here again very deep water was found, except in one place, where there is a ridge of land on which there was only 1777 fathoms, whilst there was 3754 on the western side of it, and 4037 on the eastern side, only eighty miles from land. Admiral Belknap observed that there is, therefore, evidently a deep submarine valley running parallel with the coast of Japan, and some 250 miles in width. Whether the Kuro Siwo, or Japan current, corresponding in a sense to the Gulf Stream, has anything to do with this is a matter for surmise. Since the *Tuscarora* first showed us these great depths others have been discovered. The *Challenger* after leaving Japan found 3750 fathoms 200 miles east of Cape King, and nearly the same depth another 200 miles farther, after which the water shoaled. She also got 4475 fathoms only 150 miles from Guam in the Caroline Islands. The U.S.S. *Albatross* found 3820 fathoms off the coast of the Aleutians, and the U.S. Coast Survey steamer *Blake* got 4561 fathoms 70 miles north of Porto Rico, whilst H.M.S. *Egeria*, surveying in the South Pacific, has succeeded in getting depths of 4428, 4295, and 4530 fathoms. Subsequent researches have proved that the deepest portions of both the Atlantic and Pacific Oceans are close to their western shores. The Admiral then gave some description of the apparatus employed, and after paying tribute to the English and American Navies for their researches, he concluded a most interesting lecture by suggesting that the Japanese Naval Service should take up the work, more particularly on their own coasts and along the course of the Kuro Siwo.

THE additions to the Zoological Society's Gardens during the past week include a Wild Cat (*Felis catus* ♂) from Scotland, presented by Mr. Osgood H. Mackenzie; an African Cat (*Viverra civetta* ♀) from West Africa, presented by Mr. John J. Pitcairn, M.R.C.S., F.Z.S.; two Weka Rails (*Ocydromus australis*) from New Zealand, presented by Mr. Edward T. Dixon; a Common Tequexin (*Tupinambis tequexin*) from Rio de Janeiro, presented by Mr. Edward Sloane; a Himalayan Bear (*Ursus tibetanus*) from Tibet, deposited; a Molucca Deer (*Cervus moluccensis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

STARS WITH PECULIAR SPECTRA.—A communication from Prof. E. C. Pickering to *Astronomische Nachrichten*, No. 3008, announces the discovery of the following stars having peculiar spectra, and of some new variables in the constellations Triangulum and Hydra:—

Designation of star.	R.A. 1900.	Decl. 1900.	Mag.	Date of photograph.	Description of spectrum.
	h. m.	° ′			
D.M. + 33°·470	2 31·0	+33 51	9·2	1890 Oct. 13	III. Type. Bright hydrogen lines.
Cord. G.C. 7192	5 59·4	− 6 42	5·8	1888 Feb. 15	F line bright.
,, ,, 17717	12 56·3	−70 56	6·6	1890 May 14	F line bright.
,, ,, 18770	13 43·4	−27 44	7·0	,, May 15	III. Type. Bright hydrogen lines.
,, ,, 18947	13 51·6	−55 51	8·0	,, May 25	IV. Type.
,, ,, 20554	15 48	−69 42	6·2	,, May 26	IV. Type.

It is remarked that "the stars D.M. + 33°·470 and Cord. G.C. 18770 in the above list have a spectrum in which the lines due to hydrogen are bright, as in that of *o* Ceti and other variables of long period. They were therefore suspected of variability." An examination of the Harvard College Observatory photographic charts and spectra indicates that the former star varies between 7·1 mag. and 9·2 mag., and the latter between 7·0 mag. and 10·4 mag. Three observations of Cord. G.C. 18770 by Stone gave a magnitude 6. It is interesting to note that the Rev. T. E. Espin announced the probable variability of the star D.M. + 33°·470 in *Walsingham Observatory Circular*, No. 27 (NATURE, November 13, p. 45).

A photograph of the spectrum of T Ceti shows that it belongs to the third type. A photograph of the spectrum of the variable of the Algol type, viz. S Antilæ (R.A. 9h. 27·9m., Decl. −28° 12′), discovered by Paul in 1889, shows the hydrogen lines at λ 410 and λ 434 (*h* and G) narrow, while the lines at λ 394 and λ 397 are very broad. This would place it in "an intermediate class between stars of the first and second types."

THE ROTATION OF JUPITER.—A report in the *Revue Générale des Sciences*, of the meeting of the St. Petersburg Academy of Sciences held on November 18, contains an account of a paper by M. Biolopolski, on the rotation of the planet Jupiter. Cassini appears to have been the first to point to the analogy between the rotation of Jupiter and the sun, by demonstrating that the velocity at the equator is greater than on the rest of the surface of these two bodies. It is known that on the sun the angular velocities are functions of the heliographic latitudes. By utilizing observations and drawings made by Cassini, Herschel, Schröter, and many other observers, M. Biolopolski has been able to determine more than a hundred angular velocities at various "Jovigraphic" latitudes. Among these velocities two predominate, one of 9 hours 51 minutes, and another of 9 hours 55·5 minutes (approximately). The first is found almost exclusively in the zone between 0° and 5°, in both hemispheres; the second is obtained from the remainder of the surface, except between 5° and 10°, both N. and S., where the two velocities appear to occur with equal frequency. These conclusions are confirmed by Mr. Keeler's drawings of Jupiter, made at the Lick Observatory during this year.

EDDIE'S COMET (?).—A telegram from Cape Town Observatory to the editor of the *Astronomische Nachrichten*, with regard to the supposed remarkable comet seen by Mr. Eddie on October 27 at Grahamstown, reads: "It has been cloudy here, October 27; has not been seen by anyone else."

NAMES FOR ASTEROIDS.—The following names have been given to recently-discovered asteroids:—

(283) Emma.	(289) Nenetta.
(284) Amelia.	(293) Brasilia.
(285) Regina.	(294) Felicia.

PERIOD OF χ CYGNI.—Mr. Chandler represents the inequalities in the period of the variable χ Cygni by the formula

$$406{\cdot}02\ E\ \text{days} + 0{\cdot}0075\ E^2\ \text{days} + 25^\circ{\cdot}0\ \sin\,(5^\circ\ E + 272^\circ),$$

where E is the normal epoch of a maximum. This formula, containing quadratic and periodic terms, gives the epochs of maxima much nearer than those derived from Schönfeld's uniform period of 406·5 days.

BIOLOGICAL NOTES.

ILLUSION OF WOODPECKERS AND BEARS.—Mr. J. D. Pasteur, Inspector of the Post and Telegraphic Service at Java, communicated to Dr. F. A. Jentink, in July last, the following very curious and interesting facts about woodpeckers, who, under the illusion that the buzzing sound so apparent on applying the ear to telegraph poles is caused by the vigorous efforts of gnawing or boring insects, make large holes in the timber, on a hopeless chase after such. He incloses a piece of a telegraph pole made of teakwood, with two woodpeckers (*Picus analis*), from the Kediri Residency, Java. The wood, which is of iron hardness, is perforated with rather large holes near the place where the insulators had been attached. Although Inspector Pasteur passes thousands of telegraph poles under view each year, only in a very few cases has he found any damage done to them by woodpeckers, and, until now, the damage done has always been on the living kapok trees (*Eriodendron anfractuosum*), which are used in Java for this purpose. The piece of telegraph pole now sent is the only instance known to him of damage being done to the sound and very hard poles of the teak (*Tectona grandis*). Besides the above-mentioned woodpecker, from time to time the rare little *Picus moluccensis* was seen also among the others at work. Mr. Pasteur remarks on the great rarity of such a phenomenon: in the Paris Electrical Exhibition of 1881 there was exhibited, as a great curiosity, a telegraph pole sent from Norway, which was perforated by a hole of 7 centimetres in diameter. The Norwegian Administration was for a long time uncertain to what cause to ascribe this damage done to poles, which were otherwise quite sound, till a mere chance at last revealed woodpeckers at work. In Norway, too, another equally remarkable case of damage had been noted as done to telegraph poles by the large stones, which are heaped round their base to insure their stability in the ground, being removed and scattered, apparently without any reason. This, which was for a length of time inexplicable, was at last found to be the work of bears, who apparently mistook the sound in the timber for the buzzing of a swarm of bees. It is too much to expect of either bears or woodpeckers that they should be versed in the ways of modern science. (*Notes from the Leyden Museum*, October 1890, p. 209.)

ALGÆ LIVING IN THE SHELLS OF MOLLUSKS.—For a long time back zoologists have, in a more or less desultory and unsatisfactory manner, made known to us the occurrence, in the hard parts of both living and extinct animals, of sundry ramifying canals, which have been described as the work of vegetable parasitic forms. These forms have been found in shells, corals, sponges, in fish-scales, and, indeed, the literature of the subject is quite a large one. Until the other day, however, there was but little progress made in our knowledge of the life-history of these parasitic plants, but the researches of Reinsch (*Bot. Zeit.*, 1879), and especially of Lagerheim (*Oefver. af K. Vet. Akad. Forhand.*, 1885), marked a new era in their investigation. A memoir, recently published in the Report of the Botanical Congress held in Paris last year, from the pens of MM. Ed. Bornet and Ch. Flahault, reduces the whole subject to order, describes the method of investigation, and a number of new genera and species. As most of these minute plants grow within the calcareous shells of Mollusca, it is not possible to study their morphology, unless by decalcifying these structures; this is effected by placing them in a sufficient quantity of Perenyi's fluid, consisting of four volumes of a 10 per cent. solution of nitric acid, three of alcohol, and three of a 5 per cent. solution of chromic acid, which fluid, while it dissolves the carbonate of lime, fixes the protoplasm, and leaves it and the other cell-contents (after a good washing) in a fit state for the use of reagents or staining. The same general features seem to attend the gradual perforating of the shell-structure by these parasites. At first they penetrate just under the outer epidermis of the shell, then speedily they make further advances, until in time the greater quantity of the hard structures disappear. This will account for the gradual extinction of dead shells in tranquil bays, where they are not subject to being ground into powder by the force of the waves. In addition to marine shells, instances of the occurrence of these forms in fresh-water shells are given, and they would also seem occasionally to lead an independent existence on calcareous rocks. Of the Chlorosporeæ, the most important is *Gomontia polyrhiza*, briefly described by Bornet and Flahault in Morot's *Journal of Botany* for May 1888, but here fully described and illustrated. This plant has a minute thallus, consisting of ramifying filaments, certain cells of which become altered into sporanges, and become detached; from these issue biciliated zoospores and immobile spores (the aplanospores of Wille). The germination of these spores seems to differ; the aplanospores increase in volume, emit one or more rhizoids, and soon present an appearance resembling that of a sporangium, but smaller. Whether the zoospores conjugate or not has not yet been observed—the difficulties in the way of observation are great—but they soon give origin to filamentous growth of a bright green colour. The other species described are *Siphonocladus voluticola*, *Zygomitus reticulatus*, and *Ostreobium queketti*. Among the Phycochromaceæ are enumerated *Mastigocoleus testarum* (Lagerheim), *Plectonema terebrans*, *Phormidium incrustatum*, and *Hyella cæspitosa*—this last a very interesting species; its thallus has a great resemblance to that of the Sirosiphoniaceæ, but in reality it belongs to the Chamæsiphoniaceæ, and it has two modes of propagation—one by the ordinary dissociation of vegetative cells, and the other by the formation of spore-like bodies resulting from the division of the contents of certain cells into a great number of minute cellular spheroids, after the manner in Dermocarpa, these cells being either terminal or intercalary. Two fungoid forms are described and figured: *Ostracoblabe implexa*, for the form regarded as *Achlya ferax* by Duncan, and *Lithopythium gangliforme*. This memoir seems to open a new field for research. Without doubt many new facts, as well as new species, await the patient labours of our botanists among these shell-frequenting species, and the way is made plain for them in this memoir. (*Bull. Soc. Bot. de France*, tome xxxvi., 1890.)

THE AUSTRALIAN ABORIGINES.

IN the latest number of the Journal and Proceedings of the Royal Society of New South Wales (vol. xxiii. part 2) there are two remarkably interesting articles on the Australian aborigines. One of them is by the Rev. John Mathew, Coburg, Victoria; the other by Mr. Edward Stephens, Bangor, Tasmania. It may be worth while to give a general idea of the contents of both papers.

Of the two, that by Mr. Mathew is by far the more elaborate. It represents the results of observation and study which have evidently been carried on for many years. A considerable part of it is devoted to the question, Who are the Australian aborigines? and about this he has much to say that must command the attention of ethnologists. He holds that the continent was first occupied by a homogeneous people, a branch of the Papuan family, and closely related to the Negroes. These settlers came from the north, but whether from New Guinea or any other island of the Eastern Archipelago we have no means of knowing, because at the time of their arrival the islands to the north were probably all inhabited by people of the same blood. Mr. Mathew thinks that these first-comers, the veritable Australian aborigines, occupied all the continent; and, having spread right across to the southern shores, crossed what is now Bass's Strait, which may at that distant date have been dry land, their migration terminating in Tasmania. According to this view, the now extinct Tasmanians were a survival of the primitive Australian race; and Mr. Mathew is at great pains to mass the evidence in favour of this theory.

One of the strongest arguments against the hypothesis is that the Tasmanians differed in appearance from their neighbours across the Strait. By a careful comparison of various accounts of both races, one of which is well known to him, he has convinced himself that the physiological differences narrow down to these: that, as compared with the natives of the continent, the islanders were on the average of shorter stature, of much darker complexion, and had hair of very different quality. These differences, he thinks, are to be explained by the fact that the Australian aborigines were greatly modified by fresh settlers, whereas the Tasmanians, being separated from other races, retained their primitive characteristics. There are now various physical types in Australia, but the Papuan type has not been effaced. Mr. Mathew cites various authorities to show that there is a decided Papuan fringe on the south-eastern and western coasts, with a departure from it landwards and in the north. The hooked nose is a feature common to Papuans, Australians, and Tasmanians.

Having discussed the argument from mythology and tradition, Mr. Mathew goes on to consider the argument from implements. The Tasmanians, as warriors, were much less skilfully equipped than most Australian tribes. They possessed, for example, neither the shield nor the boomerang, nor were their weapons ornamented. But in Australia there is a people at an equally low stage; and, with regard to those who are more advanced, Mr. Mathew suggests that the arts they possess may have reached Australia after the departure of the tribes who peopled Tasmania. His conclusion, after a survey of all accessible facts, is that the arms of the Tasmanians were of the same kind as those of the lowest of the Australians; and it is, he maintains, anything but illogical to infer that the "autochthonous" Australians once used exactly the same weapons and implements as those of the islanders. By circumstances which affected only the continent, the arms and implements there were almost universally improved. The author further strengthens his case by linguistic evidence, and by reference to customs which were common to Tasmania and the whole or part of Australia.

Having thus set forth his ideas as to the earliest inhabitants of Australia, he proceeds to disentangle and identify the other elements which go to constitute the Australian race as it is now. Upon the original Papuan stock of Australia there must have been grafted a very strong scion from another and in some respects very different stem, and the union must have been effected in the remote past, the stock from which the graft came having since then altered by progressive development almost beyond identification. The new element Mr. Mathew believes to have been the Dravidian. "Although," he says, "the Australians are still in a state of savagery, and the Dravidians of India have been for many ages a people civilized in a great measure and possessed of a literature, the two peoples are affiliated by deeply-marked characteristics in their social system and by sure affinities in language." A reference to the boomerang, which is found in India as well as in Australia, is one of the means by which he endeavours to establish this connection. "We search the Malay and Papuan armouries in vain for any trace of it, and are therefore obliged to credit some other race with its introduction to Australia, unless we unnecessarily assume that it was invented here independently. The boomerang is used in Africa about the upper course of the Nile, but we need not travel so far for it across barriers that might be termed impassable, when it is obtainable so much nearer, and in a plaee from which a highway has led thither almost to Australia's shores. If the framework of society, and those terms which are almost as close to a man as his own name, have both been introduced from India or its neighbourhood, it requires no stretch of imagination to suppose that the boomerang came along with them."

Universal and strong as the Dravidian influence is, it is not sufficient, Mr. Mathew thinks, to account for the great divergence of the Australians from the pure Papuans in physical features and language. He maintains, however, that the phenomena can be explained by the introduction of a Malay element. Mr. Curr is of opinion that the visits of the Malays are only of recent date, and quotes the statement of a Malay, who, in 1803, professed to have been one of the first of his countrymen to visit Australia. According to Mr. Mathew, the evidence of this Malay, whose historical knowledge must have been very limited, is overborne by the physique of the people, in the north especially, but elsewhere as well; by the naturalization of a number of important Malay words, such as the term for "teeth," a change which mere visitors could not effect; and, further, by faces of the Malay type and pure Malay words appearing in localities far removed from casual intercourse, as, for instance, in the extreme east and west. The admixture of Malay blood is supposed to account for the difficulty "that a race such as the Australian, with a Papuan basis, should have the hair straight or wavy, and not woolly. One even cross of a woolly-haired with a straight-haired race would hardly have transmitted such straightness of hair to posterity; but if after a first cross, the fresh invasions, though only in scant filtrations, were of straight-haired people, the effect of the mingling of two straight-haired races with one the hair of which was woolly, would surely be to make the spirals uncurl."

In so far as the succession and distribution or commingling of different races are concerned, Mr. Mathew suggests that the peopling of Australia may be compared with that of Great Britain. Taking the word "Celtic" in the widest sense, he says:—"The Celtic element in Britain is represented by the Papuan in Australia, the Saxon by the Dravidian, the Norman by the Malay. In both cases, population has poured in mainly on one side, the earliest settlers gradually retiring to the further shore. The second race takes entire possession of the centre, shedding the indigenes to either side. Wales and Cornwall might correspond to Victoria, the Isle of Man to Tasmania, not in relative position to the mainland, but in isolation and racial purity, and the Highlands of Scotland would represent Western Australia. In each case, from the first two races the bulk of the people is sprung, and the vocabulary and grammar are inherited; while the third race, sprinkled here and there over the land, has left the slightest lingual traces of its presence."

Dealing with the physical appearance of the natives, Mr. Mathew shows that it is subject to considerable variation, not only in different localities, but even in the same community, and this as regards stature, muscular development, cast of feature, and other particulars. In his opinion the Australians exhibit powers of mind "anything but despicable." In schools it has often been observed that aboriginal children learn quite as easily and rapidly as children of European parents. For three consecutive years the aboriginal school at Ramahyuck, in Victoria, stood the highest of all the State schools of the colony in examination results, obtaining 100 per cent. of marks. But the limit of the native's range of mental development is soon reached; and he has "an inherent aversion to application." He has a strong propensity to mimicry, and "it is astonishing how easily and completely young blacks, not cut off from intercourse with their relatives, but living and working constantly among the whites, fall into European modes of thought." They are not wantonly untruthful, or deficient in courage, or excessively selfish, and they are "by no means lacking in natural affection." But they have no stability, so that their moral qualities are prone to operate capriciously. An almost universal feature of the aboriginal character is "gaiety of heart"; and Mr. Mathew believes this to be a Papuan inheritance. The black is a very vain man, and it is "perhaps as much owing to his vanity or his fondness for praise as to any other motive that he has been got to work at all."

The paper includes instructive paragraphs on dwellings, clothing, implements, food; government, laws, institutions; marriage, man-making, mutilations, burial customs; art, corroborees, sorceries, superstitions; and the Australian languages. Speaking of the magicians called "doctors," corresponding to the medicine-men and rain-makers of other barbarous peoples, Mr. Mathew says:—"The power of the doctor is only circumscribed by the range of his fancy. He communes with spirits, takes aërial flights at pleasure, kills or cures, is invulnerable and invisible at will, and controls the elements. I remember a little black boy, when angry, threatening me with getting his father to set the thunder and lightning agoing. The same boy told me seriously that on the occasion of a raid being made upon the blacks' camp by the native police, one of his fathers—a doctor—lifted him and pitched him a mile or two into the scrub, and vanishing underground himself, reappeared at the spot where the boy alighted." The Australians have "an apprehension of ghosts rather than a belief in them." In the tribe with which Mr. Mathew is best acquainted, they had a term for ghosts, and thought that departed spirits were sometimes to be seen among the foliage; but "individual men would tell you upon inquiry that they believed that death was the last of them." A ghost was called a "shadow," and the conception of its existence was "shadowy" like itself. "The Kabi tribe deified the rainbow—a superstition apparently confined to this people. He lived in unfathomable water-holes in the mountains, and when visible was in the act of passing from one haunt to another. He was accredited with exchanging children, after the fashion of the European fays."

Of Mr. Stephens's paper we have not space to say much. It is of special interest, because it is nearly half a century since he landed, a lad with his parents, in the infant colony of South Australia; and since that time he has had many opportunities of observing the natives. He does not at all agree with those who think that the Australians are "a naturally degraded race." Those who talk in this way, he says, either do not speak from experience, or judge the aborigines by what "they have become when the abuse of intoxicants and contact with the most wicked of the white race have begun their deadly work." "As a rule, to which there are no exceptions, if a tribe of blacks is found away from the white settlement, the more vicious of the white men are most anxious to make the acquaintance of the natives, and that, too, solely for purposes of immorality. The native women have hearts to break, and the native men outraged honour to vindicate, and these have ever been the chief factors in the so-called atrocities of the aborigines of Australia. I saw the natives, and was much with them, before these dreadful immoralities were well known. I saw them, and was often with them, when the old died off, and the race no longer propagated itself; and I say fearlessly that nearly all their evils they owed to the white man's immorality and to the white man's drink."

Mr. Stephens has a very favourable opinion of the capacity of the Australians for mental improvement. A full-blooded native, called Cottrel, whom he knew, was "an agreeable companion, interesting in conversation, full of anecdote and adventure." He married a white woman, and settled comfortably, near the

Bremer, on land given to him by the South Australian Government. "To see him track, or smell the tracks of his own bullocks that had gone astray with a number of others, was a sight long to be remembered"; and he never failed to find them.

An odd and significant incident which occurred long ago is described by Mr. Stephens. Some men of the eastern tribes called at his father's house on their way inland. "There being only my mother and myself at home, and the nearest neighbour some distance away, the men became very bold and bodily entered the house. We had a nice lively little magpie at the time, which we had tamed and taught to say a few words, and to whistle 'There's nae luck about the house,' &c. The magpie hid himself under the sofa, and, singularly incredible as it may appear, it, in a rich, full, clear tone, whistled the tune "There's nae luck,' &c. The natives were strangely silent in a moment. In less time than it takes to pen the words, little mag was out from his hiding-place, biting the naked toes of the savages here, there, and everywhere, and talking at a tremendous rate. They all looked like a lot of scared demons, and madly rushed for the door, as if the old general himself were after them. The door was instantly closed and bolted. The blackfellows never returned, and never knew but that the words came from an avenging spirit, and that they had had a very narrow escape."

GARDEN SCHOLARSHIPS.

IN accordance with the intention of its honoured founder, the Trustees of the Missouri Botanical Garden propose to provide adequate theoretical and practical instruction for young men desirous of becoming gardeners. It is not intended at present that many persons shall be trained at the same time, nor that the instruction so planned shall duplicate the excellent courses in agriculture now offered by the numerous State Colleges of the country, but that it shall be quite distinct and limited to what is thought to be necessary for training practical gardeners.

To this end, the following resolution was adopted by the trustees, at a meeting held on November 19, 1889:—

"*Resolved*, That there be established the number of six scholarships for garden pupils of the Missouri Botanical Garden, to be available on and after April 1, 1890, such scholarships to be awarded by the Director of the Garden on the results of competitive examination, except as hereinafter provided, to young men between the ages of 14 and 20 years, of good character, and possessing at least a good elementary English education; each scholarship to grant such privileges and be subject to such conditions as are provided below or may subsequently be provided by the Trustees of the Garden.

"Until otherwise ordered, two such scholarships shall be reserved for candidates to be named by the State Horticultural Society of Missouri, and the Florists' Club of St. Louis, respectively; provided, that such candidates shall be given scholarships only after passing satisfactory preliminary examinations, and shall be subject after appointment to all tests and regulations prescribed for other candidates and pupils, and that if the names of such candidates are not presented by the societies designated, within sixty days after such action is requested by the Director, the vacancies may be filled by him on competitive examination, as in other cases.

"Each scholarship so conferred may be held by the original recipient for a period not exceeding six years, subject to the following conditions:—

"Each garden pupil shall be required to lead a strictly upright and moral life, and shall be courteous and willing in the performance of all duties prescribed for him. He shall devote his entire time and energy to the labour and studies prescribed for him, except that from time to time he may be granted leave of absence to visit his home or for other good reason, at the discretion of the Director, provided that the aggregate of such absences in any calendar year shall not exceed thirty days. He shall also show such ability in his work and studies as to satisfy the Director that it is advantageous for the scholarship to be held by him; and from time to time he may be subject to both theoretical and practical examinations, or may be given special tasks calculated to test his knowledge or resources. Failure to meet the requirements in any one of these respects, making due allowance for extenuating circumstances, shall forfeit all claim on any scholarship, which may then be awarded to another person in the prescribed manner.

"Garden pupils, appointed as above indicated, shall be regarded as apprentices in the Botanical Garden, and as such shall be required to work in it under the direction of the head gardener, performing the duties of garden hands. They shall be successively advanced from simpler to more responsible tasks; and in such order as may seem best shall be transferred from one department of the Garden to another, until they shall have become thoroughly familiar with the work of all.

"To the end that Garden pupils shall be repaid for their services to the Garden, and that the absence of pecuniary means need not deter any young man from obtaining such training as is contemplated, each regularly appointed garden pupil holding a scholarship shall be entitled to the following wages, payable in equal instalments at the end of each fortnight: for the first year 200 dollars; for the second year, 250 dollars; and for each year after the second, 300 dollars; together with plain but comfortable lodgings convenient to the Garden.

"In order that they may have opportunity to become instructed in the theoretical part of their profession, and in subjects connected therewith, such pupils shall not be required to do manual work in the Garden for more than five hours per day after the first year, devoting the remainder of their time to the study of horticulture, forestry, botany, and entomology, under the direction of the Director of the Garden; and they shall for this purpose be granted free tuition in the School of Botany of Washington University. They shall also receive practical instruction in surveying and book-keeping, so far as a knowledge of these subjects is held to be necessary for a practical gardener charged with the management of an estate of moderate proportions.

"At the expiration of six years, the holder of a scholarship, who is recommended as practically proficient, shall be entitled to examination by the Garden Committee, in the subjects prescribed for study, and on passing such examination to the satisfaction of the Committee and Director, he shall receive a certificate of proficiency in the theory and practice of gardening signed by the Chairman of the Garden Committee and the Director of the Garden. In exceptional cases, candidates may be admitted to examination at the end of the fifth year, when this shall be deemed advisable by the Garden Committee, and on passing such examination satisfactorily, shall be entitled to a statement to that effect from the Director, and to the regular certificate on the subsequent completion of a year's work to the satisfaction of their employers."

All applicants for scholarships, whether named by the societies indicated above or not, will be examined in the following subjects so far as they are taught in the upper classes of grammar schools: English grammar, reading, writing, and spelling; arithmetic; and geography. If the number of candidates for scholarships exceeds the number of scholarships to be awarded at any time, all candidates except those named by the societies indicated will be required to pass a further competitive examination, which will cover history of the United States, English literature, algebra, German, the elements of botany, zoology, and physiology, and such other subjects as may from time to time be prescribed. It is not intended to make the passing of examinations in these last-named branches a requirement for the award of scholarships, but merely in this way to obtain a means of selecting the most deserving and able candidates when it is necessary to reject some. Hence, the Director will always use his discretion as to the importance to be attached to greater or less proficiency in any of the subjects covered by competitive examinations, as well as to the other qualifications of candidates admitted to such examinations.

Under the above provisions, the following announcement is made:—

Two scholarships will be awarded by the Director of the Garden, prior to April 1 next. In case both are not then awarded, the remainder will not be awarded until the corresponding period of the following year, and vacancies which may subsequently arise will be filled annually, after published announcement.

Applications for scholarships, to receive consideration, must be in the hands of the Director not later than March 1. The preliminary examination for all candidates will be held on Tuesday, March 3, at the Shaw School of Botany, 1724 Washington Avenue, St. Louis, between 10 a.m. and 5 p.m. If the number of applicants exceeds two, competitive examinations, based on the subjects indicated above, will be held at the same place on Friday and Saturday, March 6 and 7.

Candidates who live at places remote from St. Louis, and who wish to be spared the expense of coming to the city for examination, may send, with their application, the name and address of the principal of the nearest high school, or of some approved private school, in case he is willing to take charge of such examination for them; but all applications of this character must be in the hands of the Director not later than the middle of February. If the examiner is approved, papers will be sent to him before the date set for the examination, and on the payment of a fee of 2 dollars to him, the candidate may write on them in his presence. If competitive examinations are also required, the same examiner will receive the papers for them in time to submit them to the candidate on the date set for similar examinations in St. Louis, on receipt of an additional fee of 3 dollars as a partial payment for his time in conducting the examination. The papers written on such examinations will be forwarded by the examiner to the Director, who will read them in connection with those written in St. Louis, before making any awards.

Successful candidates will be started in their duties as garden pupils on Tuesday, March 31, at the Botanical Garden. They will be lodged in comfortable rooms in a spacious dwelling adjoining the Garden under the charge of the head gardener, or some other competent person. It is not the intention of the Trustees to furnish table board, but good board can be obtained in the lodging-house or elsewhere at the usual cost. The lodging-house includes a reading-room supplied with the more valuable horticultural and agricultural papers, and also with a small but standard collection of books on the same subjects, which the pupils have free use of. So far as possible, the surroundings of pupils are made home-like, and, without assuming any responsibility for their behaviour, an effort is made to subject them to influences calculated to insure for them gentlemanly manners and habits of industry and investigation.

During the first year of their scholarship, garden pupils will work at the practical duties of the Garden nine or ten hours daily, according to the season, the same as regular *employés* of the Garden, and will also be expected to read the notes and articles referring to the subject of their work in one or more good journals.

In the second year, in addition to five hours' daily work of the same sort, they will be given instruction and will be required to do thorough reading in vegetable gardening, flower gardening, small-fruit culture, and orchard culture, besides keeping the run of the current papers.

In the third year, in addition to five hours of daily labour, they will be instructed and given reading in forestry, elementary botany, landscape gardening, and the rudiments of surveying and draining, and will be required to take charge of clipping or indexing some department of the current gardening papers for the benefit of all.

In the fourth year, besides the customary work, they will study the botany of weeds, garden vegetables, and fruits, in addition to assisting in the necessary indexing or clipping of papers, &c., and will be taught simple book-keeping, and the legal forms for leases, deeds, &c.

The course for the fifth year, in addition to the customary work, will include the study of vegetable physiology, economic entomology, and fungi, especially those which cause diseases of cultivated plants; and each pupil will be expected to keep a simple set of accounts pertaining to some department of the Garden.

In the sixth year, in addition to the manual work, pupils will study the botany of garden and green-house plants, of ferns, and of trees in their winter condition, besides the theoretical part of special gardening, connected with some branch of the work that they are charged with in the Garden.

From time to time changes in this course will be made, as hey shall appear to be desirable, and the effort will be made to give the best theoretical instruction possible in the various subjects prescribed; but it is not intended to make botanists or other scientific specialists of garden pupils, but, on the contrary, practical gardeners.

Applications for scholarships, and any inquiries regarding them, are to be addressed as below, on or before the dates mentioned above. If requested, blanks will be mailed to persons who contemplate making application.

WILLIAM TRELEASE,
Director of the Missouri Botanical Garden,
St. Louis, Mo.

WASHINGTON OBSERVATIONS, APPENDIX I.

THIS volume consists of reports, to the Superintendent, of the Transactions of the International Astrophotographic Congress, and of "A Visit to certain European Observatories and other Institutions," by Albert G. Winterhalter.

In the first report will be found a good detailed account of the most important points that have been discussed in order to obtain the best and most accurate methods by which a chart of the heavens may be made. Among the resolutions that were passed at this Congress, the following may be mentioned:—Refractors shall be used exclusively, the object-glasses of which shall have apertures of 33 centimetres and focal lengths of about 3·43 metres. The aplanatism and achromatism of these glasses shall be calculated for the wave-lengths near the Fraunhofer line G, by which means use may be made of the maximum of sensitiveness of the photographic plates. All the plates will be prepared from a single formula, and as regards their sensitiveness a permanent control will be instituted. In order to eliminate defects in the plates, two series of photographs of the whole sky will be made, and so arranged that stars on the edges of one will be more or less central on the other. In addition to the above-mentioned series of plates, another will be taken with shorter exposures, so as to obtain greater precision in the micrometric measurement of the fundamental stars, and to make possible the formation of a catalogue. The author, in his general conclusions at the end of this report, points out the necessity for a mutual dependence and relation of all workers in astronomy, and he adds, "The necessity becomes urgent in the light of the late development and the multiplying branches of that science. Had there been no practical results of the international reunion at Paris, there would still have remained much good derived from personal acquaintance and discussion."

The second part of the work forms a most valuable record of the instruments that are employed in most of the chief Observatories of to-day. The author made the visits in obedience to orders of the Department and of the Superintendent that were issued to him, all of which he has appended in the latter end of Part IV. To give an account of the various Observatories described in this volume, or even to summarize the information recorded, would occupy too much space, so that we will restrict ourselves to the visit paid to England, and make some brief extracts relating to the Observatories mentioned.

Of course, the first Observatory visited was that of Greenwich, and the author does not give such a detailed account of it as he does of many others, as "no observatory is more intimately known to Americans. This is largely due to the thoroughness with which the parts have been described from time to time, and to the frequency with which the establishment has been referred to as a model in one or the other particular." At any rate, he gives a good description of most of the chief instruments employed, with details of their construction; he also paid great attention to the various styles of domes employed, and the means adopted for rotating them. The photoheliograph is described, also the magnetical and meteorological instruments, and a brief account is given of the chronometer, time signals, clocks, and chronographs.

The next Observatories visited were those of Kew, Huggins's, and Common's; during the author's stay at the last-named Observatory the 5-foot mirror was in progress of rough polish. He also briefly states the means that were going to be adopted for floating the axis, the tube of which would measure 8 feet in diameter, so that its ends would then only have to be confined to the extremities of the tank by pins.

The visits of the author not being strictly confined to Observatories, he went over the workshops of Troughton and Simms, that are situated at Charlton, in which he saw instruments of all kinds in course of construction. As a typical example of the work turned out he takes the transit-circle with an object-glass of 8 inches and a focal length of 9 feet, an illustration of which is given.

The last English Observatories visited were those of Oxford and Cambridge, and after some short historical notes, the various instruments in each are described in detail.

Part III. consists of reports on sundry astronomical and nautical constructions and processes, and amongst the subjects treated may be mentioned the following: the construction of the great dome at Nice, M. Bigourdan's apparatus for determining the personal equation in double-star measurements, various forms of artificial horizons, a new level-trier, public time-service, and the equatorial coudé.

In Part IV. the author relates a few very general conclusions that he has arrived at after his series of visits, some of which we will state quite briefly. Each prominent instrument should have its own building; dwellings for observers should be on the premises; good workshops should be provided, so that repairs may be done on the spot; and electricity should be used throughout. Copies of the orders from the Navy Department follow next in order; and the volume terminates with a list of books, photographs, &c., that were bought at the various places visited.

The work as a whole supplies the reader with an excellent and trustworthy description of many of the chief Observatories of the present day; it may be stated, however, that the author has deemed it necessary to omit a large amount of information on methods, instruments, &c., his reason for so doing being that it will be found in other publications.

The illustrations throughout add greatly to the value of the book, and the American Government ought to be congratulated on the result of the work.

FIELD EXPERIMENTS AT ROTHAMSTED.[1]

THESE memoranda are issued yearly, in order to bring the results of the field experiments up to the present time. The matter is closely condensed, most of the information being in the form of schedules and tables. The results include observations and experiments upon rainfall, drainage, composition of drainage water, disposal of rainfall through percolation and evaporation, as well as upon the effects of artificial manures on various crops, most of which have been grown upon the same land, under strictly regulated conditions, for forty years. To those who are accustomed to follow the results obtained at Rothamsted, there is nothing novel in those portions of the memoranda, as they merely contain the account of a work with which the agricultural public is familiar. No one not already aware of the elaborate nature of the Rothamsted experiments can take up these memoranda without being struck with the vast amount of labour, patience, expenditure, and ability disclosed. Agriculturists will probably be disposed to confine their attention to the effects of fertilizers upon certain crops, but after perusing this interesting document we come to the conclusion that the Rothamsted observations reach much further in their significance than the limits of agricultural practice. They must assist in the solution of physiological questions involving both animals and vegetables, and physical questions relating to the gradual changes wrought upon the surface of the earth through rainfall and atmospheric action. The Rothamsted results, carefully tabulated year by year, contain a mass of data from which important conclusions may be drawn. Among these data we note—(1) Observations upon the determination of nitrogen as ammonia, as nitric acid, and as organic nitrogen, and also some other constituents, in many samples both of the rain and of the various drainage waters collected at Rothamsted. It is only by such observations that any idea can be formed as to the sources of the nitrogen of growing plants. We have here a means for estimating both the amount of nitrogen brought down in rain and wasted through drainage—two factors which must be kept in view when the relations of plants to combined nitrogen at present existing in the soil are to be investigated. (2) Observations on the difference in the character and amount of the constituents assimilated by plants of different botanical relationships, under equal external conditions, or by the same description of plants under varying conditions. (3) Observations on the character and range of the roots of different plants, and on their relative development of stem, leaf, &c. (4) Observations on the composition of the entire plant, and various parts of the same plants, at different stages of growth.

One of the most important subjects, which has acquired fresh interest at the present time, is that of the assimilation of free nitrogen by growing plants. In treating of this deeply interesting question Sir John Lawes says:—"In recent years this question has assumed quite a new aspect. It now is, whether the free nitrogen of the atmosphere is brought into combination under the influence of micro-organisms or other low forms, either within the soil or in symbiosis with a higher plant, thus serving indirectly as a source of nitrogen to plants of a higher order. Considering that the results of Hellriegel and Wilfarth on this point were, if confirmed, of great significance and importance, . . . a preliminary series (of experiments) was undertaken in 1888, a more extended one was conducted in 1889, and the subject is being further investigated in the present year. The results so far obtained show that, when a soil growing leguminous plants is infected with appropriate organisms, there is a development of the so-called leguminous nodules on the roots of the plants, and, coincidently, increased growth and gain of nitrogen. These results were obtained after adding to a sterilized sandy soil growing leguminous plants a small quantity of the watery extract of a soil containing the appropriate organisms. There is no evidence that the leguminous plant itself assimilates free nitrogen; the supposition is rather that the gain is due to the fixation of nitrogen in the growth of the lower organisms in the root nodules, the nitrogenous compounds so produced being taken up and utilized by the leguminous plants. It would seem, therefore, that in the growth of leguminous plants, such as clover, vetches, peas, beans, lucerne, &c., at any rate some of the large amount of nitrogen which they contain may be due to atmospheric nitrogen brought into combination by the agency of lower organisms." No hint is given as to the identity of the "lower organism" so frequently mentioned, but there is reason to think that it is a fungus belonging to the class *Ustilaginæ*, which exists in and is the cause of the tubercles, or root nodules, found upon the root fibres of leguminous plants.

The bulk of the memorandum is occupied with the continuation and re-editing up to date of the results obtained at Rothamsted by the application of fertilizers. The matter is important, but as it takes the form of an annual report in which the latest results are added to and incorporated with those of previous years, already noticed by us in due course, we must refrain at the present time from more lengthy notice. Sir John Lawes has always been a stout advocate of the view that the source of nitrogen in plants was to be looked for *in the soil* and not in the air. It is, therefore, an interesting fact, as between him and the believers in the assimilation of atmospheric free nitrogen, that the micro-organisms in question seem to offer a *modus vivendi*, or means of reconciliation, between two hitherto conflicting doctrines.

[1] "Memoranda of Field Experiments at Rothamsted, Herts, conducted by Sir John Lawes, Bart." 1890.

SCIENTIFIC SERIALS.

IN the *American Meteorological Journal* for November, Prof. W. Ferrel replies to the attacks made by Prof. Hazen on Espy's experiments on storm generation. He points out that this theory does not rest upon the experiments of Espy alone, but on those of Regnault, Clausius, Sir W. Thomson, and others, all of which were finally embraced in a convenient formula by Dr. Hann, and adopted by himself (*Meteor. Zeitsch.*, 1874, and Smithsonian Report, 1877). M. H. Faye continues his articles on the "Accessory Phenomena of Cyclones." This number also contains a summary of a report by Mr. W. Ogilvie on his exploration of the Canadian Yukon, and the region between it and the Mackenzie, in 1887 and 1888, embracing some portions of country never before visited by a white man. He wintered in 1887 in lat. 64° 61′, long. 140° 54′. The maximum pressure in January reached 30·3 inches or 0·3 inch higher than the most trustworthy maximum isobars for that region. The mean minimum temperature in December was $-33°·6$, and the absolute minimum in the same month $-55°·1$. The mean temperature is not given. Drifting ice in the river commenced on October 21, the ice set on November 15, and on February 3 the thickness amounted to 48 inches. From the station on the Yukon to one on the Porcupine, lat. 65° 43′ long., 139° 43′, the following meteorological notes are given: lowest temperature in April, $-37°$. The last time a minus reading was recorded was May 5, $-1°·8$. Highest temperature in April, 40° on 30th; the highest in May, 55° on 7th. The report contains a series of notes on the opening and closing of the Mackenzie, on the duration of sunlight, and on the climate and capabilities of that region. Green clouds, a phenomenon apparently rare, were observed on the mornings of February 19 and 29 just before sunrise; at the same time there was a slight fall of minute ice-crystals.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 11.—"On Ellipsoidal Harmonics." By W. D. Niven, F.R.S.

In this paper some of the properties of these functions are obtained from a discussion of their expressions in Cartesian co-ordinates.

If Θ_r denote

$$\frac{x^2}{a^2+\theta_r}+\frac{y^2}{b^2+\theta_r}+\frac{z^2}{c^2+\theta_r}-1,$$

the general expression for an ellipsoidal harmonic is given by

$$G = (1, x, y, z, yz, zx, xy, xyz)\Theta_1 \ldots \Theta_n,$$

where any of the quantities inside the brackets is the multiplier of the product of Θ's outside, provided n equations of the form

$$\frac{p}{a^2+\theta_1}+\frac{q}{b^2+\theta_1}+\frac{r}{c^2+\theta_1}+\frac{4}{\theta_1-\theta_2}+\ldots+\frac{4}{\theta_1-\theta_n}=0$$

are satisfied, p, q, r being respectively 3 or 1, according as G does or does not contain x, y, z as factors.

For the same values of θ, another function satisfies Laplace's equation, given by

$$H = (1, x, y, z, yz, zx, xy, xyz)K_1 \ldots K_n,$$

where

$$K_r - 1 = \Theta_r.$$

The functions (H) are spherical harmonics, and several properties of the functions (G) depend upon those of (H).

The functions (G) are applicable to the interior of the ellipsoid. Those pertaining to the exterior, as is well known, are of the form GI, where I is a certain integral depending on the quantities θ pertaining to G.

If G and H are corresponding functions of the nth degree according to the above definitions, then

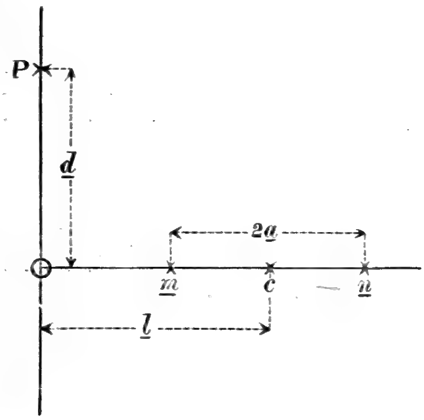
$$GI = (-1)^n \frac{1}{2^n n!} H\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}\right)V_n,$$

where V_n is the potential at xyz due to an ellipsoid whose density at any internal point f, g, h is

$$n\left(1-\frac{f^2}{a^2}-\frac{g^2}{b^2}-\frac{h^2}{c^2}\right)^{n-1}.$$

The remainder of the paper is occupied with the reduction and application of these results to ellipsoids of revolution.

Physical Society, November 28.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were made:—Additional notes on secondary batteries, by Dr. J. H. Gladstone, F.R.S., and Mr. W. Hibbert. After referring to the debatable points as to what compounds are formed and decomposed in the working of such batteries, the authors give the results of their examination of the red substance formed by the action of dilute sulphuric acid on minium, and which Dr. Frankland believes to be a compound having the formula $Pb_3S_2O_{10}$. The ultimate analysis showed 72 per cent. of lead. A portion of the substance was treated with a 3 per cent. solution of ammonium acetate to dissolve out any normal sulphate that might be present; this left a residue much darker in colour than the original substance, and containing 82 per cent. of lead. PbO_2 contains 86·6 per cent. of lead. The colourless solution yielded a ratio of Pb to SO_4, varying from 2·0 to 2·15; pure $PbSO_4$ requires a ratio of 2·16, and Frankland's compound 3·23. From these results the authors conclude that the portion dissolved was not a basic sulphate, and that the evidence tells against the original substance being a chemical compound. The authors have also continued their comparative experiments on the action of spongy lead on dilute sulphuric acid, either pure, or containing a small quantity of sulphate of soda. After the experiments had gone on for five months, the residues were analyzed; that from the pure acid showed 82 per cent. of lead sulphate and 18 per cent. of metallic lead, whilst that mixed with sodium sulphate gave 89 per cent. of lead sulphate and 11 per cent. of lead. They therefore conclude that although the action of acid on lead is initially diminished by the presence of sodium sulphate, the final result is rather the other way. Mr. G. H. Robertson, who had used ammonium acetate to analyze plugs from storage cells, said he had arrived at results much the same as those stated by the authors. Mr. Swinburne said Dr. Frankland was absent, but, without agreeing with him, he would suggest that Dr. Frankland might say that ammonium acetate decomposed the subsalt $Pb_3S_2O_{10}$, and then dissolved the sulphate $PbSO_4$. The question might be attacked by acting on equal quantities of the substance and the mixture PbO_2, $2PbSO_4$, in a calorimeter with ammonium acetate, to see if the same heat is produced; this would show whether the substance is a mixture or a compound. Dr. S. P. Thompson was glad that the authors allowed a possibility of basic sulphate being formed, for it was well known that an almost irreducible sulphate resulted from leaving a cell nearly discharged; this, he thought, would point to a possible formation of a basic sulphate from PbO and $PbSO_4$. Mr. Swinburne did not see where the PbO came from, except in newly pasted negatives, and he knew of no evidence of an intermediate stage of oxide on the plates. They appear to change directly to sulphate. Dr. Thompson said that a rapid discharge was known to produce basic salts. This, Mr. Swinburne thought due to deficiency of acid near the plates. Peroxide, he said, could not be formed on the negative, without the acid was heterogeneous and gave rise to local currents. Mr. W. Hibbert, referring to Mr. Swinburne's statement, said he had put one plate of spongy lead into strong acid and another into weak, and from this arrangement obtained a fairly large current. As regards the basic sulphate spoken of by Dr. Thompson, he did not think there was much probability of its being formed. Time, he said, had an important influence on a partially discharged cell, and he would not expect to easily reduce the $PbSO_4$ formed by the long-continued action of lead on sulphuric acid. The President inquired whether Mr. Hibbert's argument would apply to a fully charged cell. Mr. Hibbert, in reply, said that in this case the time required to produce sufficient sulphate to be irreducible would be very much longer, for in a partially discharged cell much sulphate was already formed. Dr. Gladstone said he had anticipated that Dr. Frankland would raise the objection referred to by Mr. Swinburne. As far as he was aware, there was no direct evidence either way, but he thought that the suggested decomposition was improbable. If he acted on a mixture of PbO_2 and $PbSO_4$, he would expect to get the results actually obtained. Mixtures, however, were difficult to deal with, and the results not conclusive, for the physical condition of the mixture was not the same as that of the actual products. Referring to Dr. Thompson's remarks, he understood that it was the basic sulphate which he (Dr. Thompson) considered irreducible. Dr. Frankland, however, believed this sulphate more easily reduced than $PbSO_4$. The President remarked that he thought Dr. Frankland had two reasons for his belief in the existence of the basic sulphates. One of these was the difficulty in reducing normal sulphate, whilst the other was based on the rapid fall of E.M.F. at a certain part of the discharge. It was at this point that Dr. Frankland thought the new sulphate formed, and to meet this argument it was necessary to find some other explanation of the rapid fall. In this connection he (the President) inquired whether there was sufficient peroxide formed on the negative plate to account for the drop. On this point Dr. Gladstone could not speak decisively.—An illustration of Ewing's theory of magnetism, by Prof. S. P. Thompson. A number of small "charm" compasses were placed together on a glass plate of an ordinary vertical attachment to a lantern. A large magnet at a distance served to neutralize the earth's field, and a coil enabled a magnetizing force to be applied in the plane of the needles. By this apparatus all the various phenomena exhibited by Ewing's model were beautifully shown on a screen. In the course of his experiments Dr. Thompson had found that when small magnets placed at moderate distances apart were used, it was much more necessary to neutralize the earth's field, in order that they might set themselves according to their mutual attractions, than when larger magnets were employed. A weak field directed the small openly spaced magnets, whereas with larger ones, their mutual actions were much more powerful. This fact may, he thought, throw some light on the molecular groupings in magnetite (loadstone). This substance exists in two forms, viz. one crystalline and the other of a heterogeneous structure. The former variety exhibits no magnetic retentiveness, whilst the latter is decidedly magnetic. As far as he was aware, no sufficient explanation had been given of the non-retentiveness of the crystalline variety. A difference in the molecular distances or groupings might account for the peculiarity. Mr. Boys said it was rather curious that Prof. Rücker had just devised a somewhat similar illustration of Ewing's theory, and he exhibited it at the meeting. It consists of little magnets made of long U-shaped pieces of watch-spring pivoted by glass caps on needle-points; the needle-points are fixed in little disks of lead stuck on a sheet of glass which forms the base of a glass box. An open helix surrounding the box serves to apply magnetic force. Mr. Swinburne called attention to two theoretical points. First as regards susceptibility (which he regarded as a mere ratio and not a property), he said that if particles of iron at a high temperature rotate, as has been supposed, the susceptibility should be negative, and Prof. Ewing had some reason to think that this was the case. The next point concerned the loss of energy when an armature rotates in a strong magnetic field; this, he said, was known to be considerable, whereas if Ewing's theory is correct he would expect little or no loss, for all the little magnets would always put themselves in the direction of the field, and would never pass through positions of unstable equilibrium. The President said he had discussed the question of negative susceptibility some years ago with Dr. Lodge, with reference to the drop in the characteristics of dynamos, but he was not aware that any direct evidence had been obtained. Prof. Perry thought negative susceptibility might be possible in strong fields but not in weak ones. Mr. Swinburne, on the contrary, considered its existence would be more marked in weak fields. Mr. H. Tomlinson said he had tried experimentally whether the susceptibility of nickel, when heated above its critical temperature, was negative, but he had not been able to detect it, although his apparatus was very sensitive.—The solution of a geometrical problem in magnetism, by Thomas H. Blakesley. The problem referred to was the following: Given the two poles of a magnet and a straight line intersecting at right angles, its axis produced, to determine at what point this line is parallel to the field. The question is of scientific interest, because, if the point be found experimentally, the distance between the virtual poles of the magnet can be determined, whilst it is important practically from its bearing on the deviation of ships' compasses in certain cases. The instances in which it would apply are pointed out in the paper. Assuming



the points m and n (see diagram) to be the positions of the virtual poles, and P the required point, it is shown that

$$\frac{m}{(d^2 + m^2)^{\frac{3}{2}}} = \frac{n}{(d^2 + n^2)^{\frac{3}{2}}},$$

where

$$Om = m, \quad On = n, \quad \text{and } OP = d.$$

From this the expression

$$\left(\frac{d^2}{2mn}\right)^3 - \frac{3}{4}\frac{d^2}{2mn} - \frac{1}{4}\frac{m^2 + n^2}{2mn} = 0,$$

is deduced. Now, in hyperbolic trigonometry, we have a formula

$$\cosh^3\theta - \tfrac{3}{4}\cosh\theta - \tfrac{1}{4}\cosh 3\theta = 0,$$

hence making

$$\frac{m^2 + n^2}{2mn} = \cosh 3\theta;$$

we have also

$$\frac{d^2}{2mn} = \cosh\theta.$$

The value of θ can be then found by aid of the tables of hyperbolic sines and cosines compiled by the author, and published recently by the Society. The distance d can thus be determined in terms of m and n. A method of finding the point experimentally is then described, and the distance between the poles ($2a$) shown to be given by the expression

$$\frac{a}{l} = \tanh\frac{3\theta}{2},$$

where

$$\frac{l}{d} = \sqrt{\frac{\cosh 3\theta + 1}{4\cosh\theta}},$$

l being the distance Oc. The latter function can be deduced from the tables already referred to. Experiment shows that the distance between the virtual poles soon approaches the length of the magnet, as d increases. The strength of the field at P is given by the expression

$$\frac{4M}{d^3}\,\frac{\cosh^2\theta}{4\cosh^2\theta - 1}$$

where M is the moment of the magnet. This can be simplified by arranging d and l so that

$$\cosh^2\theta = \frac{5}{4},$$

and then becomes

$$\frac{5}{8}\frac{M}{d^3}.$$

Under these conditions

$$\frac{l}{d} = 0{\cdot}85065,$$

and therefore the angle

$$OCP = 40^\circ\ 23'\ 10''.$$

Entomological Society, December 3.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—Dr. D. Sharp, F.R.S., exhibited specimens of *Papilio polites*, *P. erithonius*, and *Euplœa asela*, received from Mr. J. J. Lister, who had caught them on board ship when near Colombo, in November 1888. Dr. Sharp read a letter from Mr. Lister, in which it was stated that from the ship hundreds of these butterflies were seen flying out to sea against a slight breeze. Many of them, apparently exhausted by a long flight, alighted on the deck of the ship, and large numbers perished in the sea.—Lord Walsingham exhibited a coloured drawing of a variety of *Acherontia atropos*, which had been sent to him by Mons. Henri de la Cuisine, of Dijon. He also exhibited specimens of an entomogenous fungus, apparently belonging to the genus *Torrubia*, growing on pupæ (received from Sir Charles Forbes), which had been collected in Mexico by Mr. H. B. James. Mr. McLachlan, F.R.S., expressed an opinion, in which Mr. C. O. Waterhouse and Mr. G. C. Champion concurred, that the pupæ were those of a species of *Cicada*. Mr. F. D. Godman, F.R.S., said that at the meeting of the Society on October 3, 1888, he had exhibited a larva of a *Cicada* with a similar fungoid growth. —Mr. R. Adkin exhibited male specimens of *Spilosoma mendica*, Clk., bred from ova obtained from a female of the Irish form which had been impregnated by a male of the English form. These specimens were of a dusky white colour, and were intermediate between the English and Irish forms.—Mr. R. W. Lloyd exhibited specimens of *Anisotoma Triepkei*, Schmidt, and *Megacronus inclinans*, Er., collected last August at Loch Alvie by Aviemore.—Mr. Merrifield read a paper entitled, "On the conspicuous changes in the markings and colouring of Lepidoptera caused by subjecting the pupæ to different temperature conditions," in which it was stated that the results of many experiments made on *Selenia illustraria* and *Ennomos autumnaria* tended to prove that both the markings and colouring of the moth were materially affected by the temperature to which the pupa was exposed: the markings, by long-continued exposure before the last active changes; the colouring, chiefly by exposure during these last changes, but before the colouring of the perfect insect began to be visible, a moderately low temperature during this period causing darkness, a high one producing the opposite effect, and two or three days at the right time appearing in some cases sufficient. Dryness or moisture applied during the whole pupal period had little or no effect on either markings or colouring. Mr. Merrifield said he had obtained from summer pupæ of *illustraria* some moths with summer colouring and spring markings, some with spring markings and spring colouring, and some with summer markings, but an approach to spring colouring. These specimens, with enlarged and coloured photographs of them were exhibited. Mr. C. Fenn, who said he did not agree with Mr. Merrifield's conclusions, exhibited a very long and varied series of *Ennomos autumnaria*, all of which, he stated, had been bred at the same temperature. He expressed an opinion that the presence or absence of moisture, rather than differences of temperature, was one of the principal causes of variation. The discussion was continued by Lord Walsingham, Colonel Swinhoe, Mr. Waterhouse, Mr. Jenner-Weir, Captain Elwes, Mr. McLachlan, Mr. Porritt, Dr. Mason, Mr. Goss, and Mr. Barrett.—Mr. G. T. Baker read a paper entitled "Notes on the Lepidoptera collected in Madeira by the late T. Vernon-Wollaston." The paper was illustrated by a number of figures drawn and coloured some years ago by Prof. Westwood.—Mr. Hamilton H. Druce exhibited several very beautiful species of butterflies, belonging to the genus *Hypochrysops* from the Solomon Islands and Australia, and read a paper on them, entitled "A monograph of the Lycænoid genus *Hypochrysops*, with descriptions of new species."—Mr. C. J. Gahan read a paper entitled "Notes on some species of *Diabrotica*."

Linnean Society, December 4.—Prof. Stewart, President, in the chair.—The President exhibited some eggs of the shell slug, *Testacella haliotidea*, and briefly described the habits and mode of feeding of this mollusk. He also delineated and described the feeding tract of the snail.—Mr. F. J. George exhibited an autumnal flowering form of *Mercurialis perennis*, with stems four feet in length, which he had found at Preston, Lancashire.—Mr. R. A. Rolfe exhibited and made some remarks on a coloured drawing of *Cycnoches Rossianum*, showing both male and female inflorescences on the same pseudo bulb.—Mr. J. E. Harting exhibited an immature example of Bonaparte's gull, *Larus philadelphia*, Ord, of North America, which had been shot on the Cornish coast at Newlyn on October 24 last.—Mr. T. Christy exhibited and made remarks on some coca-leaves which had been forwarded from an East Indian plantation, and which were found to be superior to any received from South America.—On behalf of Mr. H. N. Ridley, of the Botanic Gardens at Singapore, Mr. B. D. Jackson read a paper on orchids, genus *Bromheadia*, on which some critical remarks were offered by Mr. R. A. Rolfe.—The next paper was one by Messrs. J. H. Lace and W. B. Hemsley on the vegetation of British Beluchistan, illustrated by a route-map showing the district in which Mr. Lace had been collecting. Seven hundred species were catalogued, amongst which were eleven new to science. The paper was ably criticized by Mr. C. B. Clarke, and Mr. J. G. Baker made some interesting observations on the peculiar prickly character of the vegetation which predominates in the hot and dry district explored.

Anthropological Institute, December 9.—Francis Galton, F.R.S., Vice-President, in the chair.—A paper on an apparent paradox in mental evolution, by the Hon. Lady Welby, was read.—Mr. Francis Galton, F.R.S., exhibited a large number of impressions of the bulbs of the thumb and fingers of human hands, showing the curves of the capillary ridges on the skin. These impressions are an unfailing mark of the identity of a person, since they do not vary from youth to age, and are different in different individuals. There is a statement that the Chinese—who seem to be credited with every new discovery—had used thumb impressions as proofs of identity for a long time; but Mr. Galton pronounced it to be an egregious error. Impressions of the thumb formed, indeed, a kind of oath or signature among the Chinese, but nothing more. Sir W. J. Herschell, however, when in the Civil Service of India, introduced the practice of imprinting finger-marks as a check on personation. Mr. Galton's impressions were taken from over 2000 persons by spreading a thin film of printers' ink on a plate of glass, then pressing the thumb or finger carefully on the plate to ink the papillary ridges, and afterwards printing the latter on a sheet of white paper. Typical forms can be discerned and traced, of which the individual forms are mere varieties. Wide departures from the typical form are very rare.

Mathematical Society, December 11.—Prof. Greenhill, F.R.S., President, in the chair.—The following communications were made:—On the reversion of partial differential expressions with two independent and two dependent variables, by E. B. Elliott.—Newton's classification of cubic curves, by W. W. R. Ball.—On the stability of a plane plate under thrusts in its own plane, with applications to the "buckling" of the sides of a ship, by G. H. Bryan (communicated by A. E. H. Love).—On the *q*-series derived from the elliptic and zeta functions of $\frac{1}{3}$K and $\frac{1}{4}$K, by Dr. Glaisher, F.R.S.—On the extension to matrices of any order of the quaternion symbols S and V, by Dr. Taber.—Steiner's poristic systems of spheres, by Prof. G. B. Mathews.

PARIS.

Academy of Sciences, Dec. 15.—M. Hermite in the chair.—The general relation of the state and increase of population, by M. Emile Levasseur. The relation is stated between the number of births, marriages, and deaths in France at different periods during this century. It appears that the proportion of suicidal deaths is increasing, although it is but 0·75 per cent. The illegitimate births amount to 7·5 per cent. in the period 1871–88, this proportion being about the mean illegitimacy of European nations. The number of male children born is known to be in excess of the number of female children; they become a minority, however, in the proportion of 57 to 61, because of the fact that the mean life of females is longer than that of men. All the statistics are graphically represented.—On the anomalous propagation of waves of sound, by M. Gouy.—On a modification of the electric gyroscope for the rectification of marine compasses, by M. G. Trouvé.—On the presence of very small quantities of aluminium in cast-iron and steel, by M. Adolphe Carnot.—On the increase of the number of red corpuscles in the blood of the inhabitants of the high plateaus of South America, by M. F. Viault.—On the development of the Copepodian parasite of Ascidiæ, by M. Eugène Canu.—On the localization of the active principle in the grain of Cruciferæ, by M. Léon Guignard.—On the structure of Peronosporæ, by M. L. Mangin.—Old observations of the vaccine tubercle of Leguminosæ, by M. Prillieux.—Synthesis of kainite and tachydrite, by M. A. de Schulten.—The depths of the Black Sea, by M. Venukof.

CONTENTS.

THURSDAY, JANUARY 1, 1891.

THE COMMON SOLE.

A Treatise on the Common Sole (Solea vulgaris), considered both as an Organism and as a Commodity. Prepared for the Marine Biological Association of the United Kingdom by J. T. Cunningham, M.A., F.R.S.E., late Fellow of University College, Oxford; Naturalist to the Association. (Plymouth: Published by the Marine Biological Association, 1890.)

THIS handsome monograph is (passing over the "Journal") the first-fruits of the Plymouth Laboratory. From the very outset, the Marine Biological Association has given a prominent place in its programme to economic matters. In the speeches at the foundation meeting, held on March 31, 1884, in the rooms of the Royal Society, and again on the occasion of the opening of the Laboratory at Plymouth, on June 30, 1888, the investigation of the habits and life-histories of important food-fishes, and other similar problems having a practical bearing upon fishing industries, was put forward as a primary function of the Association; and, in fact, the objects of the Association were officially stated to be "to promote accurate researches leading to the improvement of zoological and botanical science, and to an increase of our knowledge as regards the food, life-conditions, and habits of British food-fishes and molluscs." In response to this declared intention of dealing with the practical applications, the Fishmongers' Company have given most valued support to the Association from the beginning, and have subscribed largely to the funds, while H.M. Treasury has added £500 for five years to the annual income, on certain conditions.

In order to carry out efficiently this economic side of their work, and to fulfil their pledge with the Government and the public, the Council of the Association determined, in the summer of 1887, to appoint a skilled naturalist (in addition to the Resident Superintendent, now Director), who would carry on investigations into the natural history of British marine food-fishes, under the direction of the Council. To this post, Mr. J. T. Cunningham, who had been engaged for some years at similar work in connection with the Scottish Marine Station at Granton, was duly appointed, with special instructions from the Council to investigate the life-history of the common sole, with a view to practical results, and with the object of preparing an illustrated monograph on this fish. We now have the results of Mr. Cunningham's investigations before us.

It is now pretty generally recognized that results of economic value in connection with the fisheries can only be obtained, or looked for, as the result of properly conducted scientific investigations and carefully collected statistics. This first monograph issued by the Association is an excellent combination of material from these two sources of knowledge. It is a union of pure science and economics, made with the view of applying the results of scientific study to the extremely practical question of increasing the supply of soles to the market; and we find that it discusses that most important food-fish from almost every point of view. In determining the scope of the investigations and the general plan of the book, the guiding hand of Prof. Ray Lankester (to whose enthusiasm and energy the Association owes its existence and its success, and who has now succeeded Prof. Huxley in the presidential chair) is acknowledged by the author in the preface.

The work is divided into four parts, viz. Taxonomical, Morphological, Bionomical, and Economical. The first part deals with the classification and the characteristics of flat-fishes in general, and of the sole in particular. Throughout the book there is evidence that it is written, not for the professional biologist alone, but for the general public. The descriptions are not always in purely technical language, and at the beginning of each section, or when a fresh subject is entered upon, such as embryology (p. 84), we find a certain amount of elementary explanation which cannot be intended for the specialist, but may perhaps enable an intelligent non-professional reader to gain a general idea of the nature, objects, and results of each investigation, although he is not able to appreciate all the details. With the help of the hen's egg, the protoplasm and the food-yolk and the more important changes which take place in the young embryo are explained in a manner for which many may be grateful who are not aware of the significance of Kupffer's vesicle, and have no particular views on the nature of the periblast.

When, however, such explanations were being given, it would surely have been well to supplement the account of how to draw up the specific description of a flat-fish by an actual numerical example. The table on p. 15, or a part of it, or even a reference to it, might with advantage have been given at the foot of p. 10. There is no bibliography, beyond what naturally occurs in the taxonomy, and one is struck, in some parts of the book, by the absence of any reference to the work of previous investigators, so that it is difficult to know precisely how much is put forward as original; for example, under embryology there is no mention of the observations made by Prof. McIntosh and others.

In the morphological section there is a good account of the anatomy of the chief systems of the body, in which the most interesting passages are naturally those dealing with the modifications of structure caused by the remarkable "asymmetry of the Pleuronectidæ," which was first ably elucidated by Dr. Traquair more than twenty years ago. Mr. Cunningham, from his detailed study of the eye-muscles, supports the view that the twisting of the facial region of the skull, and the migration of the left eye on to the right (or upper) side in the adult is not the result of selection alone, but has been aided by the inheritance of characters or modifications acquired during the life of the individual. But this does not seem to follow as a necessary consequence of his observations and argument. For it is perfectly conceivable that anything which can be produced by the inheritance of acquired characters can also be brought about by the selection of congenital variations. It is true that "a man cannot lift himself up in a basket," and that (p. 53) "the eye-muscles could not by physiological effects have removed themselves bodily by their own action to a new position," but if there were enough men in baskets they might occur piled up ("congenitally") in such positions that some of them would form a series extending from the lower level to a higher; and, similarly, a series of selected congenital variations in muscles may form a gradation from the original position to a new one. I am not, however, objecting to a certain amount of inheritance of acquired characters coming in occasionally as a secondary factor—it is what I have always argued for of late years—but it does not seem to me that the present case yields any fresh evidence.

It is scarcely correct to say (p. 51) that evolutionists are divided into two schools at present, those who would interpret all modifications as the result of natural selection alone, and those who believe that acquired characters are inherited and that the modification "would take place without selection, while selection could not produce adaptations without the inheritance of acquired characters" (p. 52). These are rather the two extremes, and many biologists—judging at least by recently published opinions, and by the discussions in Section D at the last few meetings of the British Association—hold views of an intermediate nature. Probably all grades of opinion between the two extremes discussed by Mr. Cunningham will be found represented amongst leading evolutionists.

The remarks in regard to the origin of the follicle cells of the ovum are of interest in view of the controversy which has long waged as to the origin of the follicular epithelium and the "testa-zellen" in the Tunicata. Mr. Cunningham appears to incline to the view that the follicular epithelium is derived in bony fishes, not from the germinal epithelium, but from amœboid lymph-cells in the connective tissue. In Ascidians the most recent authorities agree in regarding the follicle cells as homologous with ova, and as being derived from germinal epithelium. Another interesting point is the modification of the right dorsal branch of the fifth nerve, and its explanation (p. 70).

One would like to know on what principles Mr. Cunningham has used his italics. It seems impossible to discover any from the book. This is a minor point, but not altogether unimportant. It is confusing to the reader to have italics used inconsistently, and besides it renders the difference of type meaningless. We find italics used indifferently for family titles, generic and specific names (for which they might with advantage have been retained), popular or local names, ordinary words requiring emphasis, and names of bones and other structures. But the type is not even used consistently for all of these. Why (on p. 50) should the "*recti*" muscles be in italics and not the "oblique"? Why (top of p. 67) should "*infundibulum*" be in italics when "cerebellum" and "medulla oblongata" are not, and why should "*hypophysis cerebri*" be favoured rather than "lobi inferiores" and "crura cerebri"? Then, again, we find (p. 68) "recti muscles" sometimes in italics and sometimes not.

At the end of the section on morphology there is a description of *Phyllonella soleæ*, a Trematode parasite which frequently occurs adhering to the skin of the fish (as shown in plate v.). By the way, why is there no explanation of the absence of Figs. 6 and 6*a*, which ought to illustrate this parasite, from plate xiv.? The woodcuts on p. 93 are probably the missing figures, but the matter as it stands, without any note of explanation, is rather mysterious. Plate xiv. and its description do not tally, and the figure labelled 6 is evidently 7.

Under the head of Part III., "Bionomical," are interesting articles on the geographical distribution, the habits, food, parasites, and enemies, the colours, and the breeding, development, and growth of the sole. Mr. Cunningham has determined the spawning season of the sole in his district to be from the middle of February to the end of April, but McIntosh and Prince state (Trans. R.S. Edin., vol. xxxv., p. 848) that off the eastern shores of Scotland the period extends to August. The observations on the colour, both in its detailed markings and its general effect, the variations under diverse circumstances, and the relation to environment, are particularly interesting, and the range, as shown in plates i. to iv., is certainly very wide. Mr. Cunningham finds that the change of colour which takes place when the fish is removed from one background to a differently coloured one is very rapid; a sole placed in a white porcelain dish begins to get lighter almost immediately.

The very beautifully coloured plates (plates i. to ix.) which illustrate especially this section on colour, are reproductions of water-colour drawings by Miss Annie Willis, and no doubt the original sketches are even more artistic representations of the actual objects. If one who has not seen the specimen drawn may criticize—and it is the very excellence that invites the critical spirit—one notices an absence of sufficient light and shade, and especially of high lights, in plate i. It is all too much at a dead level, as if a steam roller had been passed over fish and gravel, and had reduced them to a mosaic; but this is a fault common to many chromo-lithographs. In the present instance the effect of the plate is greatly improved by placing it at a considerable distance from the eye. If it is laid on the ground in a good light at 3 or 4 yards distance and looked down on, it looks as real and natural as can be expected from any representation which is produced by a mechanical process and therefore wants the subtle vivacity of the artist's actual hand-touch.

The fourth and last part, "Economical," is divided into sections on artificial propagation, the sole fishery, and practical measures. Looked at from the economic point of view, this most estimable fish is of interest to everyone. It is said that many Americans come over to England every year for the express purpose of indulging in fresh fried soles for breakfast. Is it with a view of affecting the demand that Mr. Cunningham tells us that our sole may have been swallowed by a *Lophius* a day or two before we meet it at table, and then disgorged on the deck of the trawler? It would be kinder of him to reduce the market price (which appears to be rising steadily, and has increased fourfold since 1856) by increasing the supply of soles by some such process of artificial fertilization on a large scale as that proposed on p. 147. A considerable practical difficulty will, however, probably be encountered in the remarkably unenlarged condition of the testes in the male sole, and the difficulty of distinguishing the milt from mucus and sea-water, especially if the stripping of the fish is to be conducted by the trawlers themselves. Everyone, both on the scientific and on the economic side, will look with deep interest for the results of the further observations and experiments to be made by the staff of the Plymouth Laboratory on these practical questions.

A few misprints (none of great moment) have been noticed in reading over the book, viz. "ectetmoid" (p. 53), "an" for on (p. 61), "Kupher's" vesicle (p. 120), and Fig. 3 for Fig. 5 (p. 123, foot); while a slip occurs near the foot of p. 50 in the phrase "those of the dorsal or right eye," since the dorsal eye in the sole is the left.

The book, as a whole, is good, and forms an interesting and important contribution to our slender list of monographs of British animals, and on account of its economic side will, no doubt, interest a wider circle of readers than is usually the case with zoological works. The Marine Biological Association are to be congratulated on the appearance of their first memoir, and it is hoped that others of the series will speedily follow.

W. A. HERDMAN.

WOOD-WORKING.

Exercises in Wood-working, with a Short Treatise on Wood, written for Manual Training Classes in Schools and Colleges. By Ivin Sickels, M.S., M.D. (New York: D. Appleton and Company. London: W. Allen and Co., 1890.)

THIS is, in some respects, a more interesting work than its title would seem to indicate. Of late years, since it has been recognized that manual or practically industrial training of some kind should form a part of the education of every child, books of this kind have greatly improved in every respect, because it has become more necessary to make them thorough in details, and at the same time present them in such clear and succinct language that they may be perfectly intelligible to youthful minds. And as the interest in the subject of technical education is rapidly increasing, and with it the demand for good manuals, it is not without pleasure that we welcome a work which fulfils admirably what is requisite for its purpose. The author, in an introduction which we could wish had been longer, remarks that the tendency of modern systems of education is towards a proper distribution of practical with theoretical training—that is, manual with "literary" education—and that the *mind* is to be aided in its development by the action of the eye and hand. "The prime object of all manual training is to aid mental development." This is a very great and little-known truth, which was first fully set forth in a work entitled "Practical Education," by C. G. Leland, in which it was shown that, out of 110,000 children in the public schools of Philadelphia, the 200 who attended industrial art classes, and who were chosen at random for them, were the first in *all* studies, such as arithmetic or geography; that is to say, it has been proved by years of careful experiment on a very large scale, that if we take two children of equal capacity, following the same studies in the same school, and let one of these have at the same time from two to four hours' weekly training in design, modelling, wood-carving, and carpenters' work, &c., the latter will invariably take precedence in all the ordinary school studies, so much is the mind impressed by industrial art culture. The recognition of this principle by a practical teacher like Dr. Sickels indicates that his work is written in accordance with the most advanced ideas on the subject of technical education.

The author begins by giving in Part I. a sufficiently detailed description of the structure of wood of different kinds, its composition and manner of growth, the season for cutting trees, drying of wood, and warping, its properties, measure, and value, with an account of the twenty-five kinds most generally used. In describing wood-working trades, he declares there are only two, carpentry and joinery, carving and turning being only "adjuncts" to joinery; a degradation of wood-carving with which we should suppose few would agree. It is difficult for us, with a magnificent fourteenth century carved wood image of the Virgin and Child before us as we write, to comprehend what it has in common with "joinery," while, as regards turnery, the great and admirable work of John J. Holtzapfel, certainly establishes its claim to be an independent art. "But these be trifles." The principal part of the work, or about one hundred pages, which is copiously illustrated with pictures which are all intelligible at a glance (not an *invariable* thing in such works), is, on the whole, without errors. It treats of carpenters' tools (those for wood-carving are not included, but should have been) and their use in detail, this portion being admirably executed; the construction of joints, which is, on the whole, the most difficult and interesting part of the wood-worker's art, being very well written. The student who wishes, however, to be perfect in this, should also consult "Forty Lessons in Carpentry Practice," by C. F. Mitchell (London: Cassell and Co.), a little book of great value. The mysteries of the mitre joint, stretchers, dowels, and dovetailing, have, however, never been treated more cleverly or clearly than by Dr. Sickels Among remaining topics are drawers, framing—of which we have by no means enough, though "good, what there is of it"—laying floors, trimming, and the construction of all the minor details, such as doors, stairs, sashes, and hand-rails. This portion of the work would be of great practical value to settlers in the wilderness, who would often like to build for themselves houses, yet know not how. We believe that the world, however, still lacks a work teaching men all the art of making shelters and homes, from building wigwams or adjusting stones and boughs for a night's lodging, up to log huts, Pictish "bee-hives," box cottages, "and so wider." As regards typographical details, paper, and binding, this work is all that could be expected.

BACTERIA.

Les Bactéries et leur rôle dans l'Étiologie, l'Anatomie, et l'Histologie pathologiques des Maladies Infectieuses. Par A. V. Cornil et V. Babes. Third Edition, in Two Volumes. (Paris: Félix Alcan, 1890.)

THIS enlarged edition treats of the whole range of pathogenic and to a certain extent also of non-pathogenic bacteria in a fairly exhaustive manner. The classification, the chemical nature, the biological characters, the methods used in their study, both in cultures and in staining them in animal tissues, are described in a lucid though rather a dogmatic fashion. The *rôle* and the nature of the *rôle* the pathogenic bacteria play in relation to infectious diseases are, as the title indicates, the principal subjects of the book, and we have no hesitation in saying that the authors have produced a very creditable work. But there are a number of diseases of the lower animals described here, in which the demonstration of bacteria in microscopic sections through one or the other of the diseased organs is sufficient for the authors to assign to those bacteria a specific character; more than this, every granule which appears stained in those sections is regarded by the authors as a microbe. The exact proof, and even attempt at exact proof, that this is so is omitted in many of these cases.

The illustrations accompanying the text are very numerous, many of them in colours: amongst these latter some are very excellent and true to nature, *e.g.* those illustrating the chapter on tuberculosis; many others are decidedly bad and erroneous, *e.g.* most of the illustrations of cultivations, and some of the sections stained in one or two colours, while many others are badly printed. We fail to see the use of such a confusing and diagrammatic crowd of illustrations as form plate i. The authors give numerous references to other workers, but there is, we think, a rather large dose of reference to their own works, such reference occurring in remarkable sameness. In several instances they do not seem to have read the original statements of their references. Thus, for instance, Cornil and Chantemesse in 1887 described and illustrated the pathology of the disease known in England as swine fever; they describe it under the name of "pneumo-entérite des porcs," both the name and the pathology of which disease have been minutely described and copiously illustrated in the Reports of the Medical Office of the Local Government Board for 1878-79, under the name of "infectious pneumo-enteritis of the pig." If they had really read the text in that English Blue-book, and inspected the illustrations of the pathology of our infectious pneumo-enteritis of the pig, they could not have failed to see that they are using the same name for the same disease, viz. they would have recognized the identity of the English and French disease; as a matter of fact, they give us erroneously to understand that our infectious pneumo-enteritis is the French "rouget de porcs," a disease utterly different from it.

There are various assumptions as regards the biology of certain specific bacteria, for which not even an attempt at proof is undertaken. Thus the authors ascribe to the bacillus of diphtheria and the bacillus of typhoid fever the power to form spores, and this deduction they make from certain differentiated granules in the bacilli brought out by staining; but every bacteriologist knows very well that such proof is valueless, unless the alleged spores have been observed to be able to germinate; and unless certain experimental evidence (drying, heating, action of antiseptics) is forthcoming, no one would be justified in concluding that those granules in bacilli are spores.

Though the work, as has been remarked, is not all that could be wished, it nevertheless deserves to take a high place amongst the text-books on bacteriology. It is written in a very lucid style, and abounds in valuable and original observations.

OUR BOOK SHELF.

Verslag omtrent den Staat van 'S Lands Plantentuin te Buitenzorg en de daarbij behoorende Inrichtingen over het Jaar 1889 ("Report on the Condition of the State Gardens at Buitenzorg"). (Batavia: Government Printing Office, 1890.)

UNDER the able direction of Dr. Treub, the author of the Report before us, the botanic garden at Buitenzorg in Java has developed into one of the most important establishments of the kind in existence, and has become an active centre of both scientific and practical botany. The present Report renders an account of the staff, publications, library, herbarium, museum, botanical laboratory, chemical-pharmacological laboratory, botanical garden, experimental garden for trials of trees producing gutta-percha, together with copious lists of plants and seeds distributed and received. As usual during the past few years, a number of foreigners, chiefly Germans, have occupied tables in the laboratory, having travelled from Europe expressly for the purpose of availing themselves of the facilities there afforded for original researches. This Report is recommended to botanists contemplating work in the tropics. W. B. H.

The Electric Light. Fifth Edition. By A. Bromley Holmes, M.Inst.C.E. (London: Bemrose and Sons, 1890.)

QUITE recently we had occasion to refer to the rapid growth that has taken place during the last few years in the development and application of electricity, and at the same time we gave an illustration of one of the latest forms of gas-engines for its production. This work places before the reader (who is assumed to be quite ignorant of electrical science) a popular and intelligent account "of the means used for producing electric light." The author, at the commencement, conscious of the difficulty of employing technical terms, explains each in simple language, so that their meanings may be fairly grasped by the reader.

After showing how electricity can be produced by batteries, he explains how it may be produced by mechanical means, leading up to the latest forms of dynamos. A very meagre description is given of these dynamos, descriptions of which, if they had been more fully treated, would have added great interest to this part of the work.

Conversion of electricity into light, and storage of electricity, are the subjects of the next two chapters, in the former of which are described the various lamps employed in lighting, with an account of the many self-adjusting arrangements for the purpose of keeping the carbon poles at a constant distance from each other. The remaining chapters deal with the distribution and measurement of electricity, testing and necessary precautions, and selection of light, in which the selection of lamp and the arrangements for any particular purposes are discussed. The author also treats of motive power and cost, concerning the latter of which he gives some interesting statistics relating to the comparison of gas and electricity.

On the whole, the author has produced an interesting book, which explains in a simple manner the elements of electric lighting.

Maps and Map-Drawing. By William A. Elderton. "Macmillan's Geographical Series." (London: Macmillan and Co., 1890.)

THIS little book will be found to form a most useful supplement to the works on geography published in the same series. In it an excellent brief account is given concerning the history of maps from the records of the Egyptians down to the present day. Then follow various methods of making surveys, including descriptions of the various instruments employed, such as the prismatic compass theodolite, sextant, &c. In the section on the globes, a short summary is given of ancient and curious globes, succeeding which are descriptions showing how latitudes and longitudes, day and night, &c., are measured, also the principles of great circle sailing. Part iv. deals with map-drawing, in which a brief but plain description is given of the various methods of projection, such as orthographic, gnomonic, stereographic, conical, &c.; reference also is made to the different symbols used in map-drawing. The last two parts treat of map copying and memory-maps, of which the latter will be found of great importance, for, by using the method adopted, and carrying out the suggestions, the learner may remember much that might otherwise be forgotten.

Krystallographisch-chemische Tabellen. Von Dr. A. Fock. (Leipzig: Wilhelm Engelmann, 1890.)

THIS little work of ninety pages supplies a much-needed want in chemical crystallography. The increase of late years in the number of original memoirs, describing the crystallograpical characters of newly-discovered chemical compounds, and pointing out relations between many of the longer known ones, has been so great that the text-books now in use, such as that of Rammelsberg, are very much out of date. Dr. Fock presents the data acquired up to the present in a very condensed and easily referable form; a form, moreover, which at once exhibits such relationships as have been noticed between chemical composition and crystalline form. The tables will be found to include brief descriptions of all the more recent measurements contributed to the *Zeitschrift für Krystallographie*, as far as regards the crystalline system and the elements of the crystals measured. The arrangement adopted is somewhat similar to that in Groth's "Tabellarischer Uebersicht des Mineralien," but including in addition almost all the known chemical salts, and all the measurements of organic compounds yet made. The information furnished concerning each of the compounds mentioned consists of (1) its symmetry, (2) the ratio of its axes, (3) the axial angles of monoclinic and triclinic compounds, and (4) the observer by whom the measurements were made. In many places, where important relations have been noticed, additional information of a character extremely useful for lecture purposes is given. It is quite evident from the whole character of the work that great care and a large expenditure of time have been involved in its preparation, and those who are interested in the subject must feel greatly indebted to Dr. Fock for collecting such a valuable store of information in so handy a form.

A. E. TUTTON.

LETTERS TO THE EDITOR.

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

Mr. Wallace on Physiological Selection.

BY his second letter Mr. Wallace leaves no possibility of doubt touching (1) the manifest agreement, and (2) the alleged difference, between his recent theory of cross-infertility in relation to the origin of species, and the preceding theory on the same subject, as published by Mr. Catchpool, Mr. Gulick, and myself.

(1) The manifest agreement consists in supposing, as he says, that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species; and that such "incipient infertility" is of so much importance in this "process of differentiation" that its absence may be regarded as one of the *usual* causes of the failure of varieties to become developed into distinct species.

(2) The only point of difference alleged is, that while Mr. Wallace says this incipient infertility can *never* arise alone, or except in association with some other and preceding varietal difference, we (according to him) have represented that it must *always* arise "alone, in an otherwise undifferentiated species," and therefore always constitute the *initial* change in the way of varietal divergence.

Such being the only point of difference alleged, it is obvious in the first place that the allegation, even if valid, has reference to a point of but secondary importance. For if we are all agreed that the "incipient infertility," whenever it does arise, is a factor of such high importance in the origination of species as Mr. Wallace now admits, surely the question whether it can ever arise before (or can only arise after) incipient varietal characters of any other kind becomes a question of comparatively little consequence. But now, in the second place, the allegation is not valid, being, in fact, the very opposite of the truth. Taking first my own presentation of the theory, both in "the original paper and in the summary of it published in NATURE," I not only expressly stated, but carefully argued, that the incipient infertility may arise *either before or after* variations of any other kind; and, in order to emphasize this distinction, I devoted one part of the paper to the first class of cases, while relegating to another part my consideration of the second class. Therefore it is merely by an eclectic method of quotation that Mr. Wallace now represents that I began by setting forth only one side of "physiological selection," or the cases where incipient infertility is the prior change. Why he should persistently ignore all the other part of the same paper, or the cases where I show that incipient infertility need not be the prior change, I do not care to inquire. But at least the omission cannot be due to any want of clearness on my part, inasmuch as in his first criticism of the paper, which he published several years ago, he displayed a complete understanding of what I had said upon this point.[1]

After this much explanation it seems almost needless to say that I stand by every one of the "eight quotations" which Mr. Wallace has given. For "it [still] appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that the cases where it has been superinduced by the secondary distinctions are comparatively few in number;" "it is [still] on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection;" I still continue to ask, "Why should we suppose that, unlike all other variations, it [*i.e.* the physiological variation] can never be independent?" And so on through all the eight selected sentences, provided that any regard at all be paid to their context and relation to other parts of the paper. For no one of these sentences in the smallest degree affects the position which from the first I have consistently and persistently held—viz. that it makes no difference to the theory in what proportional number of cases the physiological change has been the prior change. Indeed, the *immediate* context of the first of the above quotations sets forth that it would make no difference to the theory even if we were to suppose that in *no* case can the physiological change have been the prior change.[2] In other words, it is expressly stated that, even if we were to adopt the identical opinion on which alone Mr. Wallace now relies as constituting any difference at all between his theory and my own, still the latter, in its "principle" or "essence," would be in nowise affected. Yet Mr. Wallace now accuses me of "an absolute change of front" on the sole ground that I repeat these statements![3]

So much for my own paper. Mr. Catchpool's enunciation of the theory was much too brief to admit of any fair criticism of the kind which Mr. Wallace now passes upon it. But Mr. Gulick's elaborate essays—which he abstains from mentioning—are quite another matter; and, as stated in my last letter, they considered much more fully than mine had done the subordinate

[1] *E.g.*, "Mr. Romanes then goes on to argue that, as a rule, these physiological variations are those which occur first, and form the starting-point of new species. He admits that in some ['possibly in many'] cases sterility may be a secondary character, due perhaps to the constitutional change indicated by the external variation; but even in that case physiological selection plays an equally important part, because if it [*i.e.* the incipient infertility] does not arise, either coincidently with the ordinary external variation, or as a consequence of it, then that variation will not be preserved, but will rapidly be extinguished by intercrossing with the parent form" (*Fortnightly Review*, 1886, p. 302). This brief extract is enough to show how widely Mr. Wallace's first representation of my "original paper" differs from his last, as regards the only point now in question.

[2] See NATURE abstract, vol. xxxiv. p. 339.

[3] There are several other distortions of my views in Mr. Wallace's letter, but space prevents me from dealing with them.

question at present before us. In the result Mr. Gulick completely agreed with me, that it cannot signify *how* or *when* the physiological variation of initial cross-infertility arises; for to whatever *causes* it may be due, and at whatever *time* in the process of varietal divergence it may first occur, it must alike furnish as highly important a condition to the origination of species as Mr. Wallace has eventually himself assigned to it.

I say "eventually," because Mr. Wallace has never before expressed himself to the effect that, in his opinion, cross-infertility is a factor of such prime importance in the origination of species. Why has he never done so? Surely the matter is one of sufficient magnitude to have justified some mention in one or other of the many valuable "contributions" which he has made to the theory of evolution. Or, not to go further than his past criticisms of my own paper on the subject, what pages of controversy he might have saved in this journal and elsewhere by stating, at any time within the last four years, that he had no disagreement with me touching the probable occurrence, and the important consequence, of some degree of infertility characterizing varieties which afterwards, and on this account, develop into species; but merely doubted whether any degree of infertility could ever arise before differentiation of some other kind had begun to take place. Such criticism would have been mild indeed. But hitherto the crown and front of opposition to the theory of physiological selection has been that, in representing cross-infertility as a factor of any great importance in the origination of species, the theory is not only untrue in itself, but tends to "shrivel up natural selection to very small dimensions." Now, however, criticism "changes front." It is no longer denied, but actually upheld, that "selective fertility" is as highly important a "co-operative cause in the origination of species" as I have ever claimed; and the new attack is directed only to a very subordinate point—a point, moreover, which both Mr. Gulick and myself had expressly anticipated, fully discussed, and shown not to belong to "the essence of the theory."[1]

Oxford, December 22, 1890. GEORGE J. ROMANES.

[1] P.S.—Mr. Wallace alludes to my "standards of scientific reasoning and literary consistency." As regards the former, I am satisfied with a full and independent corroboration by a consistent and a logical mind. As regards the latter, it is enough to quote the concluding words of my reply to Mr Wallace's first criticism of four years ago:—"The *main* feature of the theory is what my paper states it to be—viz. that sterility with parent forms is one of the *conditions*, and not *always* one of the *results*, of specific differentiation. But, if so, is it not evident that all causes which induce sterility are comprised by the theory, whether these causes happen to affect a few individuals sporadically, a number of individuals simultaneously, or even the majority of an entire species?" (*Nineteenth Century*, January 1887). And is it not equally evident, as elsewhere stated, that it does not signify whether the sterility arises before or after the "differentiation" has begun; seeing that, in either case, without the sterility the differentiation (as Mr Wallace now says) will usually fail to proceed to the formation of distinct species? I have no space to discuss Mr. Darwin's views on this subject; but assuredly they are far from those which are expressed either here or in "Darwinism."

Molecular Dispersion.

IN the notes that appeared in your journal of December 11 (p. 133), you gave a very full account of some papers lately published in the *Bulletin de la Société Chimique de Paris*, on optical dispersion, by Messrs. Barbier and Roux.

This investigation is a remarkable instance of how laborious and intelligent work may be almost wholly thrown away for want of the knowledge of what has been previously done in the same direction. The authors commence their first paper with the astounding statement: "Dispersion has never been studied from the point of view of the relations which connect this physical property of bodies with their composition, their molecular weight, and their chemical constitution." This, however, was attempted by Sir John Herschel more than half a century ago, though with little success: and most of the scientific men who have best elucidated the subject of molecular refraction, such as Mascart, Brühl, and Nasini, have paid some attention also to dispersion. Mr. Dale and I gave numerical values for dispersion equivalents, analogous to Landolt's "refraction equivalents," for CH_2, Cl, &c., as far back as 1866. The dispersion of isomeric bodies was treated by me in 1881; and within the last few years I have communicated papers on the subject of molecular dispersion to learned Societies in England, France, and Switzerland. Some of the substances worked on by Messrs. Barbier and Roux have not, I think, been optically examined before; but the value of their careful observations is much diminished by their having measured two lines of the tin spectrum, instead of the A and H of the solar spectrum, or the α and γ of that of hydrogen; thus their results cannot easily be compared with the hundreds of measurements of dispersion already published by others. They have unfortunately employed Cauchy's formula, and have taken as the "specific dispersive power," $\frac{B}{d}$; that is, Cauchy's B divided by the density of the substance, instead of $\frac{n_a - n_b}{d}$, the difference between the refraction of two lines divided by the density. Brühl commenced to work in the same way, but shows in a paper in *Liebig's Annalen* for August 1886 that the method is unsatisfactory. The generalizations of our present authors are definite, and apparently correct, but they could have been mostly foreseen and explained if they had more clearly grasped the idea of molecular dispersion.

In the early summer I sent Messrs. Barbier and Roux copies of my papers on the subject; on August 1 they acknowledged receipt, and promised to refer in a forthcoming paper to previous work. As the paper reviewed in NATURE appeared in a preliminary abstract in the *Comptes rendus* of July, it is evident that they have yet something to bring before the public.

J. H. GLADSTONE.

17 Pembridge Square, December 27, 1890.

Weighing with a Ternary Series of Weights.

IN a former communication (NATURE, November 13, p. 30) I omitted, in order to avoid prolixity, to show how readily any given number may be expressed in the notation therein proposed. To effect this, it is only necessary to express the given number in the ternary scale, and then, beginning on the right, to substitute for 2, wherever it occurs, − 1, whilst increasing the previous figure by 1. When 3 occurs in the application of this rule, it must of course be replaced by 0, the previous figure being again increased by 1.

Examples:—

41 is in ternary notation			1112, in new notation			**11111.**
500	,,	,,	,, 200112,	,,	,,	**1101111.**
71	,,	,,	,, 2122,	,,	,,	**10101.**

Or (still more briefly) in dividing by 3, to express the number in the ternary scale, we may substitute for the remainder 2, wherever it occurs, − 1, and increase the quotient by 1.

Examples:—

425			474			500		
142	remainder	− 1	158	remainder	0	167	remainder	− 1
47	,,	1	53	,,	− 1	56	,,	− 1
16	,,	− 1	18	,,	− 1	19	,,	− 1
5	,,	1	6	,,	0	6	,,	1
2	,,	− 1	2	,,	0	2	,,	0
1	,,	− 1	1	,,	− 1	1	,,	− 1
0	,,	1	0	,,	1	0	,,	1
$3+27+729$ $-(1+9+81+243)$			$729-(3+9+243)$			$27+729$ $-(1+3+9+243)$		

Bradford, December 13, 1890. J. WILLIS.

PROF. EVERETT's rule (NATURE, December 4, p. 104) is needlessly complicated. All that is necessary is to express the given weight in the ternary scale with digits that are either 0, + 1, or − 1. His example is thus solved:—

3)500
3)167 − 1
3)56 − 1
3)19 − 1
3)6 + 1
3)2 + 0
1 − 1 or $500 = 1\bar{1}01\bar{1}\bar{1}\bar{1}$ ternary

$$= 729 - 243 + 27 - 9 - 3 - 1.$$

We thus see generally that, to express weights in units and powers of n, we must be provided with $\frac{1}{2}n$ or $\frac{1}{2}(n-1)$ balance-weights of each sort, according as n is even or odd; and that then any given weight can be expressed in a single way only if n is odd. R. E. B.

I AM glad to have elicited a simple and direct rule for the required distribution, to obviate the necessity of a tedious tentative process, or of the reference to tables suggested in Mr. Willis's first letter. My own rule effected the desired object, but the device of admitting negative as well as positive remainders in the successive divisions by 3 is a decided improvement. R. E. B.'s method is identical with Mr. Willis's second method, and is undoubtedly the best. Its relation to my method is seen by noting that $\frac{1}{2}(2^7 - 1)$ is ternary 1111111, which, being added to $1\bar{1}01\bar{1}\bar{1}\bar{1}$, converts it into 2012000. My rule might have been generalized by adding (instead of *the least* value) *any* value of $\frac{1}{2}(2^n - 1)$ that exceeds the given weight.

In connection with Mr. Willis's suggestion (in his first letter) of tables for finding what number a person has thought of, I may mention that I published through Simpkin and Marshall, nearly forty years ago, a set of 4 cards for this purpose under the name of "Sibylline Leaves," of which a few specimens are still in my possession. Taking advantage of the fact that weights of 1, 3, 9, 27 will make up any integer from -40 to $+40$, that is 81 different integers when 0 is included, the numbers on the cards ran from 1 to 81. The computation consisted in taking 41 to start with, and adding or subtracting 1, 3, 9, or 27, two kinds of type being employed to distinguish between addition and subtraction. J. D. EVERETT.

The Composition of Sea-Water.

COULD any reader of NATURE who may have given attention to the subject offer some explanation of the fact that the water of the sea contains such a very large excess of sodium salts relatively to salts of potassium?

Many of us have thought about the question, and heard it discussed, but I am not aware that any satisfactory conclusion has been arrived at.

I think it is usually assumed that the salts in the ocean have been mainly derived from the solutions carried in by rivers; solutions formed during the waste of rocks at the surface, or brought up by springs. Alkaline salts in such solutions will be principally due to the decay of felspars; and if we consider the rocks of the earth's crust, we find that the potash-felspars very much exceed in quantity the soda-felspars. It used to be considered that in earlier geological periods the "acid," mainly potash-bearing rocks, so enormously exceeded the "basic" rocks, in which the soda-felspars occur principally, that these latter were relatively quite insignificant in amount. Later petrographical work tends to show that this preponderance may not have been so large as was once supposed, but still there is no question that the excess of the potash-bearing rocks was, and is, very great. We might therefore look for more potash than soda in the drainage waters. We find, however, that in the sea, and in the rivers, the reverse is the case. Instances are, indeed, quoted (Roth, "Chemische Geologie") of rivers with more potash than soda in solution, but only as exceptions, and at points where only granite and gneiss had been drained.

This excess of soda in river-waters may be explained by the fact that though more potash-rocks are exposed than soda-rocks, yet the more rapid decay of the soda-lime felspars causes the proportions of the dissolved salts to be such as we find them: though this would hardly suffice to explain the great difference we find in the ocean.

Some people, again, assume that the composition of sea-water, though it may have been modified by the river-waters, is due to the constituents which were contained in the original first ocean as it condensed on the surface of the cooling earth—the "Urmeer" of the Germans; and Roth points out, in discussing analyses of river-waters and sea-water, that the composition of the former is such that they never could be the cause of the present composition of the latter.

This going back to the original ocean may be, as indeed it seems to be, the only explanation open to us, but it is not without its weak points. If potash-salts so greatly exceed soda-salts in the earth's crust now, we may assume that the same relative proportions existed, more or less, when the whole mass was still gaseous; and it is not easy to see any reason why, when cooling and condensation had allowed of the formation of a mass of molten silicates, the sodium-salts should have remained in great excess in the still uncondensed heated atmosphere out of which the "Urmeer" would eventually be deposited on the cooling crust. Mere difference of volatility of the respective salts would not suffice to account for this; and if we are to take the hypothesis at all, we must perhaps assume that when a low enough temperature had been reached to allow of the combination of the respective elements to molten silicates, the potassium, as the stronger base, would be taken more largely into these combinations by preference to the sodium, which would partly remain in the still intensely heated atmosphere, to be condensed with other vapours at a later period. M.

Birds' Nests.

IN addition to your "curious places for birds' nests," I give you my own experience between 1842 and 1882 at Highfield House, Nottinghamshire.

Redbreast for four consecutive years on a shelf.

Redbreast in a fern (*Platycerium alcicorne*) for four consecutive years.

Redbreast in a *Strelitzia regina* plant.

Hedge Warbler in a tall Fuchsia in a greenhouse.

Chiff Chaff in a fern (two years).

Pied Wagtail on a shelf in vinery (two years).

Flycatcher on hinge of door (ten consecutive years).

Flycatcher on ledge of thermometer stand (three years).

Wren in a Daniels's hygrometer stand.

County Club, Chepstow. E. J. LOWE.

Butterflies Bathing.

IN answer to the inquiry of Mr. G. A. Freeman (NATURE, vol. xlii. p. 545) as to the food and habits of *Papilio macleayanus*, the butterfly which has been observed to visit water apparently for the purpose of performing its ablutions, I may inform him that the species is commonly found about Sydney, where it feeds in its larval condition on the camphor laurel (*Laurus camphora*), and the tender shoots and leaves of the orange. It certainly is not aquatic during any part of its life, nor do the plants upon which it feeds grow near water; the insect simply follows the example of its brothers, depositing its eggs singly, and undergoing the transformations on the food-plant as any reasonable butterfly should. Mr. G. Lyell's note as to the bathing habits of *P. macleayanus* is most interesting, and as far as I am aware the observation is entirely new, although many butterflies of the family Lycænidæ frequent pools on very hot days, settling on the mud at their margins, probably in search of a little moisture. Only recently at Toowoomba, in Queensland, I noticed a number of *Holochila absimilis* settled about puddles formed on the roads by a passing shower.

A. SIDNEY OLLIFF.

Department of Agriculture, Macquarie Street, Sydney, November 11, 1890.

THE RESEARCHES OF DR. R. KŒNIG ON THE PHYSICAL BASIS OF MUSICAL SOUNDS.[1]

I.

NOT often does it fall to the lot of a scientific man to become the mouthpiece of another whose researches have lasted over a quarter of a century; yet this is the enviable position in which I find myself on this occasion as the spokesman of Dr. Rudolph Kœnig, who is known not only as the constructor of the finest acoustical instruments in the world, but as an investigator of great originality and distinction, and author of numerous memoirs on acoustics. Dr. Kœnig, who has of late made very important contributions to our knowledge of the physical basis of music, using apparatus immeasurably superior to any hitherto employed in experimental investigations of this subject, has on various occasions, when I have visited him in Paris, shown me these instruments, and repeated to me the results of his researches. Important as these

[1] By Prof. Silvanus P. Thompson. (Communicated by the author, having been read to the Physical Society of London, May 16, 1890.)

are, they are all too little known in this country, even by the professors of physics. It was, therefore, with no little satisfaction that the Council of the Physical Society learned that Dr. Kœnig was willing to send over to London for exhibition on this occasion the instruments and apparatus used in these researches. And their satisfaction to-day is heightened by the fact that Dr. Kœnig has himself very kindly come over to demonstrate his own researches, and has given us the opportunity to welcome him personally amongst us.

The splendid apparatus around me belongs to Dr. Kœnig, and forms but a very small part of the collection which adorns his *atélier* on the Quai d'Anjou. He lives and works in seclusion, surrounded by his instruments, even as our own Faraday lived and worked amongst his electric and magnetic apparatus. His great tonometer, now nearly completed, comprises a set of standard tuning-forks, adjusted each one by his own hands, ranging from 20 vibrations per second up to nearly 40,000, with perfect continuity, many of the forks being furnished with sliding adjustments, so as to give by actual marks upon them any desired number of vibrations within their own limits. Beside this colossal masterpiece, Dr. Kœnig's collection includes several large wave-sirens, and innumerable pieces of apparatus in which his ingenious manometric flames are adapted to acoustical investigation. There also stands his tonometric clock, a timepiece governed, not by a pendulum, but by a standard tuning-fork, the rate of vibration of which it accurately records.

It is not surprising that one who lives amongst the instruments of his own creation, and who is familiar with their every detail, should discover amongst their properties things which others whose acquaintance with them is less intimate have either overlooked or only imperfectly discerned. If he has in his researches advanced propositions which contradict, or seem to contradict, the accepted doctrines of the professors of natural philosophy, it is not that he deems himself one whit more able than they to offer mathematical or philosophical explanations of them: it is because, with his unique opportunities of ascertaining the facts by daily observation and usage, he is impelled to state what those facts are, and to propound generalized statements of them, even though those facts and generalized statements differ from those at present commonly received and supposed to be true.

At the very foundations of the physical theory of music stand three questions of vital importance:—

(1) Why is it that the ear is pleased by a succession of sounds belonging to a certain particular set called a scale?

(2) Why is it that when two (or more) musical sounds are simultaneously sounded, the ear finds some combinations agreeable and others disagreeable?

(3) Why is it that a note sounded on a musical instrument of one sort is different from, and is distinguishable from, the same note sounded with equal loudness upon an instrument of another sort?

These three queries involve the origin of *melody*, the cause of *harmony*, and the reason of *timbre*.

The theories which have been framed to account for each of these three features of music are based on a double foundation—partly physical, partly physiological. With the physiological aspect of this foundation we have to-night nothing to do, being concerned only with the physical aspect. What, then, are the physical foundations of melody, of harmony, and of timbre? Demonstrable by experiment they must be, in common with all other physical facts, otherwise they cannot be accepted as proven. What are the facts, and how can they be demonstrated?

We are not here, however, to fight over again the battle of the temperaments, nor do I purpose to enter upon a discussion of the origin of melody, which, indeed, I believe to be associative rather than physical. I shall confine myself to two matters only, with which the recent researches of Dr. Kœnig are concerned—the *cause of harmony* and the *nature of timbres*. Returning, then, to the ratios of the vibration numbers of the major scale, we may note that two of these, namely, the ratios 9 : 8 and 15 : 8, which correspond to the intervals called the major whole tone and the seventh, are dissonant—or, at least, are usually so regarded. It will also be noticed that these particular fractions are more complex than those that represent the consonant intervals. This naturally raises the question: *Why is it that the consonant intervals should be represented by ratios made up of the numbers* 1 : 6, *and by no others?*

To this problem the only answer for long was the entirely evasive and metaphysical one that the mind instinctively delights in order and number. The true answer, or rather the first approximation to a true answer, was only given about forty years ago, when von Helmholtz, as the result of his ever-memorable researches on the sensations of tone, returned the reply: *Because only by fulfilling numerical relations which are at once exact and simple can the "beats" be avoided which are the cause of dissonance.* The phenomenon of beats is so well known that I may assume the term to be familiar. An excellent mode of making beats audible to a large audience is to place upon a wind-chest two organ-pipes tuned to $ut_2 = 128$, and then flatten one of them slightly by holding a finger in front of its mouth. Von Helmholtz's theory of dissonance may be briefly summarized by saying that any two notes are discordant if their vibration numbers are such that they produce beats, maximum discordance occurring when the beats occur at about 33 per second; beats if either fewer than these, or more numerous, being less disagreeable than beats at this frequency. It is an immediate consequence that the degree of dissonance of any given interval will depend on its position on the scale. For example, the interval of the major whole tone, represented by the ratio 9 : 8, produces four beats per second at the bottom of the pianoforte keyboard, 32 beats per second at the middle of the keyboard, and 256 beats per second at the top. Such an interval ought to be discordant, therefore, in the middle octaves of the scale only.

To this view of von Helmholtz it was at first objected that, if that were all, all intervals should be equally harmonious provided one got far enough away from being in a bad unison: fifths, augmented fifths, and sixths minor and major, ought all to be equally harmonious. This no musician will allow. To account for this von Helmholtz makes the further supposition that the beats occur, not simply between the fundamental or prime tones, but also between the upper partials which usually accompany prime tones. This leads me to say a word about *upper partial tones* and *harmonics*. I believe many musicians use these two terms as synonymous; but they ought to be carefully distinguished. The term harmonics ought to be rigidly reserved to denote higher tones which stand in definite harmonic relations to the fundamental tone. The great mathematician Fourier first showed that any truly periodic function, however complex, could be analyzed out and expressed as the sum of a certain series of periodic functions having frequencies related to that of the fundamental or first member of the series as the simple numbers 2, 3, 4, 5, &c. Thirty years later, G. S. Ohm suggested that the human ear actually performs such an analysis, by virtue of its mechanical structures, upon every complex sound of a periodic character, resolving it into a fundamental tone, the octave of that tone, the twelfth, the double octave, &c. Von Helmholtz, arming himself with a series of tuned resonators, sought to pick up and recognize as members of a Fourier series, the higher harmonics of the tones of various instruments. In his researches he goes over the ground previously traversed by Rameau, Smith, and Young, who had all observed the co-existence, in the tones of musical instruments, of higher partial tones. These higher tones correspond to higher modes of vibration in which the vibratile organ—string, reed, or air-column—subdivides into two, three, four, or more parts. Such parts naturally possess greater frequency of vibration, and their higher tones, when they co-exist along with the lower or fundamental tone are denominated *upper partial tones*, thereby signifying that they are higher in the scale, and that they correspond to vibrations *in parts*. It is to be regretted that Prof. Tyndall, in his lectures on sound, rendered von Helmholtz's *Oberpartialtöne* by the term *overtones*, omitting the most significant half of the word. To avoid all confusion in the use of such a term I shall rather follow Dr. Kœnig in speaking of these as *sounds of subdivision*. And I must protest emphatically against calling these sounds harmonics, for the simple reason that in many cases they are very inharmonious. It is a matter to which I shall recur presently.

Returning to the subject of beats, the question arises, What becomes of the beats when they occur so rapidly that they cease to produce a discontinuous sensation upon the ear? The view which I have to put before you in the name of Dr. Kœnig is that they blend to make a tone of their own. Earlier acousticians have propounded, in accordance with this view, that the *grave harmonic* of Tartini (a sound which corresponds to a frequency of vibration, that is the difference between those of the two tones producing it) is due to this cause. Von Helmholtz has taken a different view, denying that the beats can blend to form a sound, giving reasons presently to be examined. Von Helmholtz considered that he had discovered a new species of combinational tone—namely, one corresponding in frequency to the *sum* of the frequencies of the two tones, whereas that discovered by Tartini (and before him by Sorge) corresponded to their *difference*. Accordingly, he includes under the term of combinational tones the differential tone of Tartini and the summational tone which he considered himself to have discovered. To the existence of such combinational tones he ascribed a very important part in determining the character, harmonious or otherwise, of chords; and to them also he attributes the ability of the ear to discriminate between the degrees of harmoniousness possessed by such intervals (fifths, sixths, &c.) as consist of two tones too widely apart on the scale to give beats of a discontinuous character. He also considers that such combinational tones are chiefly effective in producing beats, the summational tones of the primaries beating with their upper partial tones; and that this is the way in which they make an interval more or less harmonious.

The whole fabric of the theory of harmony, as laid down by von Helmholtz, is thus seen to repose upon the presence or absence of beats; and the beats themselves are in turn made to depend, not upon the mere interval between two notes, but upon the timbres also of those notes, as to what upper partials they contain, and whether those partials can beat with the summational tone of the primaries. It becomes, then, of the utmost importance to ascertain the precise facts about the beats and about the supposed combinational tones. What the numbers of beats are in any given case: whether they do or do not correspond to the alleged differential and summational tones: these are vital to the theory of harmony. Equally vital is it to know what the timbres of sounds are, and whether they can be accurately or adequately represented by the sum of a set of pure harmonics corresponding to the terms of a Fourier series.

In investigating beats and combinational tones, Dr. Kœnig deemed it of the highest importance to work with instruments producing the purest tones; not with harmonium reeds or with polyphonic sirens, the tones of which are avowedly complex in timbre, but with massive steel tuning-forks, the pendular movements of which are of the simplest possible character. Massive tuning-forks properly excited by bowing with a violoncello bow, or, in the case of those of high pitch, by striking them with an ivory mallet, emit tones remarkably free from all sounds of subdivision, and of so truly pendular a character (unless over-excited) that none of the harmonics corresponding to the members of a Fourier series can be detected. No living soul has had a tithe of the experience of Dr. Kœnig in the handling of tuning-forks. Tens of thousands of them have passed through his hands. He is accustomed to tune them himself, making use of the phenomenon of beats to test their accuracy. He has traced out the phenomena of beats through every possible degree of pitch, even beyond the ordinary limits of audibility, with a thoroughness utterly impossible to surpass or to equal. Hence, when he states the results of his experience, it is idle to contest the facts gathered on such a unique basis. The results of Dr. Kœnig's observations on beats are easily stated. He has observed primary beats, as well as beats of secondary and higher orders, from the interference of two simple tones simultaneously sounded.

When two simple tones interfere, the primary beats always belong to one or other of two sets, called an *inferior* and a *superior* set, corresponding respectively in number to the two remainders, positive and negative, to be found by dividing the frequency of the higher tone by that of the lower.

This mode of stating the facts is a little strange to those trained in English modes of expressing arithmetical calculations; but an example or two will make it plain. Let there be as the two primary sounds two low tones having the respective frequencies of 40 vibrations and 74 vibrations. What are the two remainders, positive and negative, which result from dividing the higher number, 74, by the lower number 40? Our English way of stating it is to say that 40 goes into 74 once, and leaves a (positive) remainder of 34 over. But it is equally correct to say that 40 goes into 74 twice all but 6: or that there is a negative remainder of 6. Well, Dr. Kœnig finds that, when these two tuning-forks are tried, the ear can distinguish two sets of beats, one rapid, at 34 per second, and one slow, at 6 per second.

Again, if the forks chosen are of frequencies 100 and 512, we may calculate thus: 100 goes into 512 five times, plus 12; or 100 goes into 512 six times, minus 88. In this actual case the 12 beats belonging to the inferior set would be well heard: the 88 beats belonging to the superior set would probably be almost indistinguishable. As a rule, the inferior beat is heard best when its number is *less* than half the frequency of the lower primary, whilst, when its number is *greater*, the superior beat is then better heard. Dr. Kœnig has never been able to hear any primary beat which did not fall within this rule.

Dr. Kœnig will now illustrate to you the beats, inferior and superior, as produced by these two massive tuning-forks,[1] each weighing about 50 pounds, and each provided with a large resonating cavity consisting of a metal cylinder, about 4 feet long, fitted with an adjustable piston. One of them is tuned to the note $ut_1 = 64$. The other also sounds ut_1; but by sliding down its prongs the adjustable weights of gun-metal, and screwing in the piston of the resonator, its pitch can be raised a whole tone to $re_1 = 72$. Dr. Kœnig excites them with the 'cello bow, first separately that you may hear their individual tones, then together. At once you hear an intolerable beating—the beats coming 8 per second. This

[1] These splendid forks, with their resonators, along with other important pieces of Dr. Kœnig's apparatus, have since been acquired by the Science and Art Department for the Science Collection at South Kensington.

is the inferior beat, corresponding to the positive remainder; the superior beat you cannot hear. Dr. Kœnig will raise the note of the second fork from re_1 to $mi_1 = 80$; and the beats quicken to 16 per second. Raising it to $fa_1 = 85\frac{1}{3}$, and then to $sol_1 = 96$, while the first fork is still kept at ut_1, the beats increase in rapidity, but are fainter in distinctness. If Dr. Kœnig now substitutes for the second fork one tuned to $la_1 = 106\frac{2}{3}$, you may be able to hear two beats, the inferior one rapid and faint at $42\frac{2}{3}$ per second, and the superior one slower, but also faint, at $21\frac{1}{3}$ per second. Still raising the pitch to the true seventh tone $= 112$, the rapid inferior beat has died out, but now you hear the superior strongly at 16 per second. If it is raised once more to $si_1 = 120$ (the seventh of the ordinary scale), and the beats are still stronger and slower at 8 per second. Finally, when we bring the pitch up to the octave $ut_2 = 128$, we find that all beats have disappeared: there is a perfectly smooth consonance. The facts so observed are tabulated for you as follows:—

Table I.

Primary Beats.

Primary Tones.		Ratio.	Inferior Beats.	Superior Beats.
ut_1 64	re_1 72	8 : 9	8	—
ut_1 64	mi_1 80	4 : 5	16	—
ut_1 64	fa_1 $85\frac{1}{3}$	3 : 4	$21\frac{1}{3}$	—
ut_1 64	sol_1 96	2 : 3	32	32
ut_1 64	la_1 $106\frac{2}{3}$	3 : 5	$42\frac{2}{3}$	$21\frac{1}{3}$
ut_1 64	(7) 112	4 : 7	—	16
ut_1 64	si_1 120	8 : 15	—	8
ut_1 64	ut_2 128	1 : 2	—	0

Suppose now, keeping the lower fork unaltered, we raise the pitch of the higher note (taking a new fork that starts at the octave) from ut_2 to sol_2 by gradual steps, we shall find that there begins a new set of primary beats—an inferior set, which are at first slow, then get more rapid and become undistinguishable, but succeeded by another rapid and indistinct, which grow stronger and slower, until, as the pitch rises to sol_2, the frequency of which is exactly three times that of ut_1, all beats again vanish. This range between the octave and the twelfth tone may be called the second "period," to distinguish it from the period from unison to the first octave, which was our first period. Similarly, the range from the twelfth tone to the second octave is the third period, and from thence to the major third above is the fourth period, and so forth. In each period, up to the sixth or seventh of such periods, a set of inferior and a set of superior beats may be observed; and in every case the frequency of the beats corresponds, as I have said, to one or other of the two remainders of the frequencies of the two tones. No beat has ever been observed corresponding to the sum of the frequencies, even when using the slowest forks. None has ever been observed corresponding to the difference of the frequencies, save in the first period; where, of course, the positive remainder is simply the difference of the two numbers.

That you may hear for yourselves the beats belonging to one of the higher periods, Dr. Kœnig will take a pair of forks which will give us some of the superior beats in the fourth period. One of the forks is the great $ut_1 = 64$, as previously used. The other is $mi_3 = 320$; their ratio being 1 : 5. Sounded together they give a pure consonance, but if the smaller one is loaded with small pellets of wax to lower its pitch slightly, and then bow it, at once you hear beats. It was in studying the beats of these higher periods that Dr. Kœnig made the observation that whereas the beats of an imperfect unison are heard as alternate silences and sounds, the beats of the (imperfect) higher periods—twelfth tone, double octave, &c.—consist mainly in variations in the loudness of the lower of the two primary tones; an observation which was independently made by Mr. Bosanquet, of Oxford.

Passing from the beats themselves, I approach the question, What becomes of the beats when they occur too rapidly to produce on the ear a discontinuous sensation? On this matter there have been several conflicting opinions: some holding, with Lagrange and Young, that they blend into a separate tone; others, with von Helmholtz, maintaining that the combinational tones cannot be so explained, and arise from a different cause. Let it be observed that, even if beat-tones exist, it is quite possible for beats and beat-tones to be simultaneously heard. A similar co-existence of a continuous and a discontinuous sensation is afforded by the familiar experiment of producing a tone by pressing a card against the periphery of a rapidly-rotating toothed wheel. There is a certain speed at which the individual impulses begin to blend into a continuous low tone, while yet there are distinguishable the discontinuous impulses; the degree of distinctness of the two co-existing sounds being dependent on the manner in which the card is pressed against the wheel—that is to say, on the nature of the individual impulses themselves. The opponents of the view that beats blend into a tone state plainly enough that, in their opinion, a mere succession of alternate sounds and silences cannot blend into a tone different from that of the beating tone. Having said that the beats cannot blend, they then add that they do not blend; for, say they, the combinational tones are a purely subjective phenomenon. Lastly, they say that even if the beats blend they will not so explain the existence of combinational tones, because the combinational tones have frequencies which do not correspond to the number of the beats.

In the teeth of all these views and opinions, Dr. Kœnig—without dogmatizing as to how or why it is—emphatically affirms that beats do produce *beat-tones*; and he has pursued the matter down to a point that leaves no room for doubting the general truth of the fact. The alleged discrepancy between the frequency of the observed combinational tones and that of the beats disappears when closely scrutinized. Those who count the beats by merely taking the difference between the frequencies of the two primary tones, instead of calculating the two remainders, will assuredly find that their numbers do not agree in pitch with the actual sounds heard. But that is the fault of their miscalculation. Those who use harmonium reeds or polyphonic sirens instead of tuning-forks to produce their primary tones must not expect from such impure sources to reproduce the effects to be obtained from pure tones. And those who say that the beats calculated truly from the two remainders will not account for the summational tones have unfortunately something to unlearn—namely, that, when pure tones are used, under no circumstances is a tone ever heard the frequency of which is the sum of the frequencies of the two primary tones.

The apparatus which Dr. Kœnig has brought over enables him to demonstrate, in a manner audible, I trust, to the whole assembly in this theatre, the existence of the beat-tones. His first illustrations relate to tones of primary beats, some belonging to the inferior, others to the superior set, in the first period.

He takes here the fork $ut_6 = 2048$, five octaves higher than the great ut_1. To excite it, he may either bow it or strike it with an ivory mallet. With it he will take the fork one note higher, $re_6 = 2304$. When he took the same interval with ut_1 and re_1, the number of beats was 8.

The *ut* and *re* of the next octave higher would have given us 16 beats, that of the next 32, that of the next 64, of the fourth octave 128, and that of the fifth 256. But 256 per second is a rapidity far too great for the ear to hear as separate sounds. If there were 256 separate impulses, they would blend to give us the note $ut_3 = 256$. They are not *impulses*, but *beats:* nevertheless, they blend. Dr. Kœnig strikes the ut_6, then the re_6, both shrill sounds when you hear them separately; but when he strikes them in quick succession one after the other, at the moment when the mallet strikes the second fork you hear this clear ut_3 sounding out. I am not going to waste your time in a disputation as to whether the sound you hear is objective or subjective. It is enough that you hear it, pure and unmistakable in pitch. It is the grave harmonic; and the number 256, which is its frequency, corresponds to the positive remainder when you divide 2304 by 2048.

Now let me give you a beat-tone belonging to the superior set: it also will be a grave harmonic, if you so please to call it; but its frequency will correspond neither to the difference nor to the sum of the frequencies of the two primary tones. Dr. Kœnig takes $ut_6 = 2048$ as previously, and with it $si_8 = 3840$. Let us calculate what the superior beats ought to be. 2048 goes into 3840 twice, less 256. Then, 256 being the negative remainder, we ought to hear from these two forks the beat-tone of 256 vibrations, which is ut_3, the same note as in our last experiment. He strikes the forks, and you hear the result. The beat-tone, which is neither a differential tone nor a summational tone, corresponds to the calculated number of beats.

If I take $ut_6 = 2048$ and $sol_6 = 3072$, the two remainders both come out at 1024, which is ut_5. Dr. Kœnig will first sound ut_5 itself, separately, on an ut_5 fork, that you may know what sound to listen for. Its sound has died away; and now he strikes ut_6 and sol_6, when at once you hear ut_5 ringing out. That sound which you all heard corresponds to the calculated number of beats. That is enough for my present purpose.

The next illustration is a little more complex. I select a case in which the beat-tones corresponding to the inferior and the superior beats will both be present. We shall have four tones altogether—two primary tones and two beat-tones. The forks I select are $ut_6 = 2048$, as before, and a fork which is tuned to vibrate exactly 11 times as rapidly as ut_3—it is the 11th harmonic of that note, but does not correspond precisely to any note of the diatonic scale. It has 2816 vibrations, and is related to ut_6 as 11 : 8. The two remainders will now be 768 and 1280, which are the respective frequencies of sol_4 and mi_5. Dr. Kœnig will first sound those notes on two other forks, that you may know beforehand what to listen for. Now, on striking the two shrill forks in rapid succession, the *two* beat-tones are heard.

If I select, instead of the 11th harmonic, the 13th harmonic of ut_3, vibrating 3328 times in the second, to be sounded along with ut_6, the same two beat-tones will be produced as in the preceding case; but $mi_5 = 1280$ is now the inferior one, corresponding to the positive remainder, whilst $sol_4 = 768$ is the superior tone, corresponding to the negative remainder. It is certainly a striking corroboration of Dr. Kœnig's view that the beat-tones actually heard in these last two experiments should come out precisely alike, though on the old view, that the combinational tones were simply the summational and differential tones, one would have been led to expect the sounds in the two experiments to be quite different.

One other example I will give you of a beat-tone belonging to the second period. The two primary notes are given by the forks $ut_5 = 1024$ and $re_6 = 2304$. The beat-tone which you hear is $ut_3 = 256$, which corresponds to the positive remainder.

It will be convenient to draw up in tabular form the results just obtained. These may be considered as abbreviations of the much more extended tables drawn up by Dr. Kœnig, which hang upon the walls, and which are to be found in his book, "Quelques Expériences d'Acoustique."

TABLE II.

Sounds of Primary Beats.

Primary Tones.		Ratio.	Inferior Beat-tone.	Superior Beat-tone.
ut_6 2048	re_6 2304 ...	8 : 9	1 { ut_3 256	—
ut_6 2048	si_8 3840 ...	8 : 15	—	1 { ut_3 256
ut_6 2048	sol_6 3072 ...	8 : 12	4 { ut_5 1024	4 { ut_5 1024
ut_6 2048	(11th) 2816 ...	8 : 11	3 { sol_4 768	5 { mi_5 1280
ut_6 2048	(13th) 3328 ..	8 : 13	5 { mi_5 1280	3 { sol_4 768
ut_5 1024	re_6 2304 ...	4 : 9	1 { ut_3 256	—

(*To be continued.*)

THE ORIGIN OF THE GREAT LAKES OF NORTH AMERICA.

AT one time glaciers—perhaps in the co-operative society of an ice-sheet—were gravely suspected of having excavated even the great lakes of North America. This, however, is hardly probable. The *a priori* difficulties in the hypothesis are great. Apart from objections which have often been pointed out, the work done would be on so gigantic a scale that a longer period must be assigned to the glacial occupation of the region than seems probable from other considerations. Further, the direct evidence which will presently be noticed seems conclusive against the hypothesis; but it may be affirmed with better reason that ice has indirectly aided in the process, though to what extent we can, as yet, hardly venture to say.

During the last few years numerous observations have been made, both in Canada and in the United States, upon the configuration of the lake beds, and the elevation of their ancient margins. To some of these Dr. Wright refers in his volume on "The Ice Age in North America," and Prof. J. W. Spencer (who has been engaged on this subject for several years) brings them into a focus in a paper recently published in the Quarterly Journal of the Geological Society of London.[1]

At first sight the great lakes, from Superior to Ontario, are suggestive of glacial excavation. They seem to occupy true rock basins. Superior discharges into Huron over the ledges—once a "portage"—of Sault Ste. Marie. Huron as it were, leaks into Erie, the fall between the two sheets of water being only nine feet. Erie flows towards Ontario over the rocky rapids and the final precipice of Niagara; and the St. Lawrence, after leaving Ontario, gives frequent evidence of a rocky bed, the level of which is considerably above that of the bottom of the lake, for the depth of this near its eastern end is more than seven hundred feet. But more careful investigation of the lakes has shown that in these apparently perfect basins (as is sometimes discovered in household affairs) hidden cracks exist, which, under different physical conditions, would have let the water run out. This indeed is not the whole story; another agency must be presently mentioned; but that these apparent basins once had

[1] Vol. xlvi. p. 523 (read April 16, 1890).

outlets, by which they would have been drained, at any rate partially, seems beyond question.

The following is a brief statement of the results of sounding in the water and boring on the land. The surface of Lake Superior is 630 feet above sea-level, the deepest part of its bed 375 feet below that datum plane. The fall from the shore line is generally rather rapid; a large part of the basin is more than 300 feet deep, and a considerable area is below 600 feet. The original outlet, according to Dr. Wright, was on the southern side; and by this, in pre-glacial times, the drainage was discharged towards the Mississippi. But, apparently, the information in regard to the ancient valley-system of the Lake Superior area is less complete than in the case of the other lakes.

The Huron-Michigan basin at once arrests attention by its extraordinary outline. Michigan is a gigantic backwater without inclosing hills at its southern or upper end. Huron proper is almost divided from Georgian Bay by the Indian Peninsula and a chain of rocky islands, of which Manitoulin Island is the chief. All this is far more suggestive of submergence than of any other mode of formation. Closer study has confirmed first impressions. Michigan really consists of two basins, divided by a plateau submerged at a maximum depth of 342 feet; the northern and larger basin sinks to a depth of 864 feet, the southern one only descends to 576 feet. Hence, if the level of the water were lowered by 350 feet, Michigan would be divided into two lakes.

Of these, the former must have drained into the northwest end of Lake Huron. It is true that the deepest soundings at the present outlet do not exceed 252 feet, but near this a fjord-like channel has been traced in the shallower part, trending northward, with a depth of 612 feet, and there are indications of other buried channels. Thus there can be little doubt that in pre-glacial times the northern basin of Michigan communicated, as the lake still does, with that of Lake Huron. But as to the outlet of the southern basin there is some dispute. Prof. Spencer, however, states that a buried channel has now been traced along the valley of the Grand River, across the peninsula of Michigan to Saginaw Bay on Lake Huron. Its exact depth has not been ascertained, but it has been pierced in several places to depths of from 100 to 200 feet below the level of the lake, and in one case the drift was found to extend 500 feet below the surface of the ground, and 350 below that of Lake Huron.

Next as to the drainage of this lake. Submerged channels resembling river valleys have been traced along its bed. One is a prolongation, in a north-easterly direction, of that which has just been mentioned. Another runs to join it from the south—that is, in the opposite direction to that of the present flow of the water; and a third is a continuation of the channel which drained the northern basin of Michigan. These three ultimately come together, and the united valley rounds Cabot's Head, and makes for the southern end of Georgian Bay, keeping near its south-western side. Here also an ancient outlet has been found. Across the low flat land separating the waters of Georgian Bay from Lake Simcoe a buried channel has been struck in borings, at various depths—in one case 280 feet—below the surface of the latter. Between this and Lake Ontario, well-borings indicate that the drift is very deep, and that it conceals an ancient channel, which entered Lake Ontario some thirty miles west of Toronto. Lake Erie, which is generally less than 84 feet deep, also exhibits a buried system of ramifying valleys, and the line of discharge into Ontario was not over the lip of Niagara but by a deep valley, now choked with drift, which can be traced several miles to the west of the present course of the St. Lawrence. In Ontario, also, a channel has been found, the greatest depth of which is over 700 feet below the surface of the lake. This runs near the southern shore, and receives other valleys from this direction.

The conclusion to which these investigations point is that in pre-glacial times the great lakes did not yet exist, but their site formed part of a system of river valleys, which ultimately coalesced in one main channel, now concealed beneath the waters of the eastern part of Ontario. Of these valleys, the one was cut off from the united system of the other tributaries at Detroit, and the head waters of these were parted by the plateau now buried beneath Lake Michigan. Some, indeed, have contended that the water of these rivers passed from the Ontario region towards the Hudson, but Prof. Spencer considers that they were even then tributary to the St. Lawrence.

But it would not suffice to block these channels with glacial drift. Parts of Lake Superior, the southern basin of Michigan, a little of Huron, and the eastern end of Ontario are beneath the sea-level: the last as much as 491 feet below it. We must assume in addition a considerable downward movement of the whole area, otherwise these valleys could never have emptied themselves into the sea. To drain the valley occupied by Ontario would require, at the least, an elevation of more than 700 feet; Southern Michigan, of not much less, perhaps of more. This hypothesis, however, presents no real difficulty, for it can be proved that many regions have been affected by movements, both upwards and downwards, in glacial or post-glacial times. The coast of Norway and many parts of northern America have been affected by a great downward movement—amounting not seldom to at least a thousand feet, and sometimes even as much as a thousand yards. This, again, after the ice had melted away and the main physical features of the district were sculptured, was followed by one in the contrary direction, which may be occasionally measured by some hundreds of feet; as, for example, at the beaches of Novaya Zembla, the terraces of the Varanger Fjord, and of many another inlet in Norway. Of this movement also there is proof on the Fraser and other rivers in America.

But to convert Lake Ontario into a river valley it would not be enough to give a general uplift and clear away the dams of glacial drift. Differential movements of the earth's crust are required. That these have sometimes occurred has been long since proved, in the case of Norway. Now, careful observations, by Prof. Spencer and others, show the reasonableness of the hypothesis in the district of the great lakes. Around their shores are old terraces, which extend in some cases to a height of 1700 feet above the present water-level, and are indicative, in Prof. Spencer's opinion, of a depression to that amount. A series of careful measurements undertaken on different terraces and around more than one American lake prove that these terraces do not correspond with the existing contour lines, but have been affected by a differential uplift, amounting in one case to as much as 4 feet per mile.

Hence, it follows that the great North American lakes are of comparatively modern date, and are nothing more than a great system of river valleys, which have been converted into a chain of huge lakes, partly by the blockage of old channels, partly by differential movements of the earth's crust. If this view be established, and the evidence in its favour (which finds much support from other regions) appears very strong, it will help in elucidating several important questions, bearing on not only the history of the Glacial epoch and the exact mode of the accumulation of the *débris*, but also on the cause of the movements of a crust which is asserted by physicists to be rigid. But into these questions, fascinating as they are, want of space precludes us from inquiring on the present occasion.

T. G. BONNEY.

THE ISOLATION OF HYDRAZINE, $\begin{array}{c}NH_2\\ |\\ NH_2\end{array}$.

IN the June of 1887, Prof. Curtius, of the University of Erlangen, announced, in the *Berichte* of the Berlin Chemical Society, the important fact that he had succeeded in preparing the hydride of nitrogen, $\begin{array}{c}NH_2\\ |\\ NH_2\end{array}$, di-amidogen or hydrazine, and an account of his preliminary experiments was given in NATURE (vol. xxxvi. p. 185). Since that time several further contributions to the history of the base and its salts have been published by Drs. Curtius and Jay (see NATURE, vol. xxxix. p. 377, and vol. xli. p. 547), and the work is now completed by the publication, in the current number of the *Journal für praktische Chemie*, by Drs. Curtius and Schulz, of full details of the perfected methods of preparation of the base and its most important salts, together with the results of determinations of its vapour density and of its molecular weight when dissolved in water.

It will doubtless be remembered that free hydrazine was supposed by Prof. Curtius to be a gas, possessing such an exceedingly powerful affinity for water that it was found almost impossible to isolate it from its hydrate. This difficulty has since to a large extent been overcome, and the nature of the gas itself more certainly ascertained. The hydrate is a very definite compound of the composition $N_2H_4 . H_2O$, and it serves as the best starting-point for the preparation both of the free gaseous hydrazine itself and of the salts of the base.

Preparation of Hydrazine Hydrate.

Hydrazine hydrate is best prepared by distilling the sulphate, $N_2H_4 . H_2SO_4$, with alkalies. Hydrazine sulphate is a beautifully crystalline salt consisting of colourless rhombic tables, the macropinacoid being the most fully developed face. It is obtained most readily by the method of Curtius and Jay from triazoacetic acid.

The perfected method of preparing hydrazine hydrate from these crystals of the sulphate is as follows:—About one hundred grams of pure caustic potash are dissolved in 250 grams of water. After cooling, a hundred grams of the finely powdered crystals of the sulphate are added, and the mixture subjected to distillation. Owing to the tremendously active properties of hydrazine, the distillation apparatus requires to be constructed throughout of pure silver. The form employed by Prof. Curtius consists of a silver flask of about a litre capacity, which can be screwed in a gas-tight manner to the silver condenser. The connecting-tube between the flask and the condenser is bent into a double U shape, so that particles cannot possibly be projected from the flask into the condenser; a thermometer is inserted in the limb of the U nearest the condenser. The flask is protected from the flame by an asbestos cushion. Cork and india-rubber connections must be rigorously excluded, as hydrazine most energetically attacks them. When the powdered sulphate is added to the strong potash solution in the flask, considerable heat is generated and the thermometer rapidly rises. Heat is then carefully applied, and liquid begins to distil. As soon as the thermometer indicates 119° C., the boiling-point of hydrazine hydrate, the receiver is changed. It is then found that the temperature remains constant at 119° until almost the last drop, the liquid distilling at this temperature being almost pure $N_2H_4 . H_2O$. The distillate collected before the thermometer reached 119°, generally amounting to about 250 c.c., should then be fractioned. It is a most curious fact that, although the boiling-points of water and hydrazine hydrate are so close together, only very small quantities of the latter are carried over by steam. More than two-thirds of the 250 c.c. pass over on redistillation before the mercury rises above 100°. Between 100° and 106° a distillate is obtained which does not contain more than 1 per cent. of hydrazine. The 106°–117° fraction contains still only about 11 per cent., while that passing over between 117° and 119° contains 60–62·5 per cent. of hydrazine. The liquid obtained in the first distillation, while the thermometer indicated 119°, contains 64 per cent. of the free base, the theoretical quantity for the formula $N_2H_4 . H_2O$. From one hundred grams of hydrazine sulphate 36 grams of the pure hydrate are obtained in a careful experiment.

As regards the analysis of the hydrate, the amount of base, N_2H_4, may be determined by means of standard sulphuric acid, all the usual indicators except phenol phthalein being available. The nitrogen and hydrogen may be estimated by combustion with copper oxide, and the nitrogen also by reduction of platinum chloride, the whole of the nitrogen being evolved in the free state in accordance with the equation

$$N_2H_4 . H_2O + 2PtCl_4 = 2PtCl_2 + 4HCl + H_2O + N_2.$$

Molecular Composition of Hydrazine Hydrate.

The vapour density of the compound has been determined by Hofmann's method in the Torricellian vacuum, using a jacket of steam. The molecular weight corresponding to the formula $N_2H_4 . H_2O$ is 50. The numbers obtained from three such density determinations were 48·79, 51·67, and 49·48, showing that the simple formula $N_2H_4 . H_2O$ expresses in all probability the molecular condition. When vapour density determinations at ordinary pressure by Victor Meyer's method are carried out, some rather surprising results are obtained. At the temperature of boiling aniline (183°) the numbers 28·25 and 23·52 were obtained, about half those at 100° *in vacuo*; it appears probable, therefore, that at this temperature and under ordinary pressure complete dissociation into N_2H_4 and H_2O occurs, the molecules of the hydrazine existing side by side with those of water vapour without combination. At higher temperatures the molecular weight appears to increase again, a phenomenon which requires further investigation.

The molecular weight of the hydrate dissolved in water has also been determined by Raoult's method, by noticing the lowering of the freezing-point of water brought about by dissolving a small quantity of the hydrate in it. The numbers obtained from three experiments were 69·73, 70·71, and 71·25, corresponding almost exactly to a hydrate of the formula $N_2H_4 . 2H_2O$.

It appears, therefore, from the above experimental results, that the vaporized hydrate posesses the composition $N_2H_4 . H_2O$. At 183° these molecules appear to dissociate into free hydrazine and water vapour. When the hydrate dissolves in water, it takes up another molecule of water, becoming $N_2H_4 . 2H_2O$.

Properties of Hydrazine Hydrate.

Hydrazine hydrate is a colourless, highly-refractive, but not very mobile, liquid, which fumes when brought into the air. In closed vessels it may be preserved unaltered for any length of time. Its odour is quite different from that of ammonia, and is indeed comparatively weak compared with that of free gaseous hydrazine. It tastes like alkalies, and leaves on the tongue a burning sensation. It possesses strongly corrosive properties, at once destroying cork or caoutchouc. The boiling liquid rapidly attacks glass. It is hygroscopic, and also extracts carbon dioxide from the atmosphere. It mixes with water and alcohol in all proportions, but not with ether, chloroform, or benzene. It solidifies when surrounded by a mixture of solid carbon dioxide and ether to a mass of leaf-like crystals, which again liquefy from some reason or other below −40°. Although hygroscopic, when a drop of the liquid is allowed to fall into a cylinder of water, it remains for a long time at the bottom without mixing.

The specific gravity of the pure liquid of boiling-point 118°·5 (corr.) is 1·0305 at 21°.

A peculiar phenomenon is noticed during the distillation of hydrazine hydrate mixed with water. When a certain stage of concentration is reached, the drops falling from the end of the condenser into the glass receiver before dissolving take the form of extended filaments, often fine threads, which do not adhere to the walls of the receiver. The liquid hydrate reacts very strongly alkaline to vegetable colouring matters. Its reducing properties, as mentioned in the earlier notes in NATURE before referred to, are extraordinarily marked, most of the ordinary salt solutions of silver, gold, platinum, ferric iron, and cupric copper being reduced, and in the case of silver beautiful metallic mirrors deposited. Mercuric oxide reacts with it so energetically as to bring about an immediate explosion.

Free Gaseous Hydrazine.

Owing to the extreme affinity of hydrazine for water to form the hydrate, the free base is only liberated from the latter compound with the greatest difficulty. When the liquid hydrate is dropped upon barium oxide in a fractionating flask, the mass becomes very hot; but on distilling, almost the total quantity of hydrazine hydrate distils over unchanged. By repeated distillation over recently ignited baryta, a portion of the water in the hydrate is certainly retained, however, and the distillate fumes more strongly in the air. When the mixture of hydrate and baryta is heated in a sealed tube to 100° C. the reaction goes much further; and when the temperature is increased to 170°, at which temperature under ordinary pressure hydrazine hydrate is dissociated into N_2H_4 and H_2O, the decomposition is complete, the tube containing barium hydrate and gaseous hydrazine at high pressure. When the end of the tube is softened at the blowpipe, a tremendous rush of the free gas occurs, forming as it escapes a long rod-like white cloud with the moisture of the air, and rendering the atmosphere almost unbearable by its fearfully penetrating odour, compared with which that of the liquid hydrate is extremely weak. Owing to the difficulty of collecting and preserving such an active gas, which reminds one irresistibly of fluorine, Prof. Curtius has not been able to experiment further with it.

Action of Halogens and Halogen Acids upon Hydrazine Hydrate.

The halogen salts of hydrazine have been examined in great detail, and present some most interesting phenomena, throwing considerable light upon the constitution of the base.

Two kinds of salts are found capable of existing—mono-salts of the type $N_2H_4 . HR$, where R represents a halogen element, and di-salts of the type $N_2H_4 . 2HR$.

When the hydrate diluted with water is mixed with hydrofluoric acid, the dihydrofluoride, $N_2H_4 . 2HF$, is obtained in crystals belonging to the regular system melting at 105°. This difluoride appears to sublime unchanged. It may also be obtained by adding hydrofluoric acid to the alcoholic solution of the hydrate, and then precipitating with ether.

The dihydrochloride, $N_2H_4 . 2HCl$, is similarly obtained when hydrazine hydrate is evaporated with hydrochloric acid, or by action of hydrochloric acid upon the alcoholic solution of the hydrate. It is also formed when gaseous chlorine is led into a flask containing hydrazine hydrate; after a few minutes' passage of the gas, the separation of beautiful octahedrons begins, and bubbles of nitrogen escape. The reaction is as follows:—

$$3(N_2H_4 . H_2O) + 2Cl_2 = 2(N_2H_4 . 2HCl) + 3H_2O + N_2.$$

It is interesting to note that this same chloride was obtained by Curtius and Jay by boiling a compound which they prepared, termed benzalazine, $N_2(CH . C_6H_5)_2$, with hydrochloric acid—

$$N_2(CH . C_6H_5)_2 + 2H_2O + 2HCl = N_2H_4 . 2HCl + 2C_6H_5CHO.$$

The monohydrochloride, $N_2H_4 . HCl$, is obtained in long needles by heating the dihydrochloride to 140°. Prof Curtius recommends heating the dihydrochloride in an apparatus heated by vapour of xylene, until no further loss of weight occurs.

The dihydrobromide, $N_2H_4 . 2HBr$, may be prepared by direct evaporation of a mixture of hydrazine hydrate and hydrobromic acid; but if an alcoholic solution of the hydrate is employed, curiously enough the monohydrobromide separates on the addition of ether. If the pure hydrate is added to excess of liquid bromine, the substance decomposes completely with liberation of nitrogen and hydrobromic acid gases, nothing but the excess of bromine remaining. But if the bromine is allowed to act upon the hydrate suspended in chloroform, the monohydrobromide separates as a white mass of crystals, nitrogen being also evolved.

$$5(N_2H_4 . H_2O) + 2Br_2 = 4(N_2H_4 . HBr) + 5H_2O + N_2.$$

Recrystallized from alcohol, the mono-salt may be obtained in large prisms.

The dihydriodide, $N_2H_4 . 2HI$, is only obtainable by one method, the decomposition of the benzalazine above mentioned with fuming hydriodic acid. It is singular that none of the methods applicable to the fluoride, chloride, and bromide yield it. When tincture of iodine is added to a dilute alcoholic solution of hydrazine hydrate, the colour disappears completely, in accordance with the quantitative reaction symbolized by the following equation:—

$$5(N_2H_4 . H_2O) + 2I_2 = 4(N_2H_4 . HI) + 5H_2O + N_2.$$

Nitrogen is evolved, and if the addition of iodine is continued until a permanent coloration is afforded, colourless prismatic crystals of the monohydriodide are obtained on evaporation. These crystals melt at 127°, and in doing so detonate violently. The reaction of hydrazine with iodine may be used volumetrically to estimate the amount of the base in a solution.

In addition to the above iodides, a third, of the composition $N_6H_{12} . 2HI$, or $3N_2H_4 . 2HI$, is obtained in well-defined crystals when an insufficient amount of iodine to form the monohydriodide is used.

Finally, a series of molecular weight determinations of these halogen salts by Raoult's method, using water as the solvent, have been made, with most interesting results. The mono-salts all give numbers agreeing with the molecular weight $\frac{N_2H_4 . HR}{2}$, showing that in solution they are dissociated into hydrazine N_2H_4, and the acid HR. The tri-iodide yields numbers corresponding to $\frac{3N_2H_4 . 2HI}{5}$, a fact which appears to indicate a dissociation into three molecules of N_2H_4, and two molecules of HI. Similarly the sulphate gives values corresponding to $\frac{N_2H_4 . H_2SO_4}{2}$, pointing to its dissociation into separate molecules of N_2H_4 and H_2SO_4. The di-salts afford results indicating a molecular weight $\frac{N_2H_4 . 2HR}{4}$, as if there were a dissociation of the kind $\begin{matrix} NH_3 \mid I \\ NH_3 \mid I \end{matrix}$. The fluoride appears to behave abnormally, yielding numbers corresponding to $\frac{N_2H_4 . 2HF}{2}$, as if the hydrofluoric acid liberated in the dissociation possessed the constitution H_2F_2.

A. E. TUTTON.

THE ALPINE FLORA: WITH A SUGGESTION AS TO THE ORIGIN OF BLUE IN FLOWERS.

WHEN in Colorado, I made notes on the plants of the Rocky Mountains, and collected some memoranda from various sources bearing upon alpine floras. Recently, when going over my records to ascertain as far as possible the definite results of environment on high alpine vegetation, I arrived at some conclusions which seemed to throw light on what had puzzled me before. These are at present more or less hypothetical, but I hope to work out the subject in more detail later on, so as to ascertain more clearly whether they will satisfy all the necessities of the case.

High alpine plants differ from those of the lowlands in one or more ways. They may be dwarfed, the leaves more divided or more thickly clothed with hairs, and the flowers are more often blue or pink. For the present, the dwarfing and the colour of the flowers need only be considered. Dwarfing may be—doubtless often is—the direct result of environment, as lack of nourishment. The dwarfed trees grown by the Japanese artificially in small pots are well known.

If this were the only cause of dwarfing, the alpine flora would present clear evidence for the transmission of acquired characters, as the character has undoubtedly become a *specific* one in several mountain plants. Thus, oaks are normally trees: the Rocky Mountain oak, *Quercus undulata*, is usually a shrub. The genus *Phlox* contains fine herbaceous plants, but *P. cæspitosa*, which I have gathered at about 12,000 feet on the Sangre de Cristo Range in Colorado, is so dwarfed as to be called (with other dwarfs growing beside it) "flowering moss." It appears, however, that natural selection may come into play, the low stature of these plants benefiting them in three ways :—

(1) They may escape the violence of the high winds which prevail at those altitudes; taller plants being broken off before the seed matures.

(2) They may obtain some additional warmth from their close proximity to the ground and partial shelter.

(3) The short summer of the mountain-tops necessitates very rapid development, and requires every energy to be thrown into the essential function of producing flowers and seed, leaving nothing to spare for the production of branched stems and diffuse foliage. In short, the plants are obliged to develop as *katabolically* as possible, and those which fail in the race are ruthlessly cut off by the autumn storms.

The evidence at hand tends to show the reality of this rapid development and the necessity for it. Every alpine traveller knows how rapid is the bursting into bloom of the plants but lately covered by snow and ice. Their impatience, if one may so call it, is astonishing, some plants will begin to develop under the snow. Then the flowers that are produced are very brilliant, lasting but for a short season (like some alpine Lepidoptera), and especially is it to be noted that they are nearly always *large in proportion to the size of the plant*. That is, the plants have dwarfed, but the flowers *have not become dwarfed at the same time*. This were strange, if bad nourishment were the only factor: in the light of the facts considered above it is exactly what one might expect.

The intense blue of some high alpine flowers is most noticeable, and the large number of blue flowers is equally remarkable. I never saw anything equalling in *blueness* the *Omphalodes nana* var. *aretioides* of the Colorado mountains above timber-line. The brilliancy of these flowers is almost dazzling. The large and beautiful blue flowers of species of *Gentiana*, *Polemonium*, &c., are conspicuous on high mountains. Next to the blues come the pinks. I have shown in the *Bulletin of the Torrey Botanical Club* how species of *Castilleia*, which are scarlet or yellow at lower altitudes, become crimson-bracted as they ascend. Many other examples could be given.

I have found no one to dispute the reality of this change of colour, at least in many groups of plants. The cause of it seemed much more obscure. It is generally agreed that, developmentally, reds come after yellows, crimson follows red, and blue crimson. Further, it is supposed that this series of colours is the result of different degrees of pigmental complexity, perhaps of the nature of, or similar to, various degrees of oxidation. Katabolism, strong metabolism, produces the higher colours—the crimsons, the blues. Moisture, slow development, great growth, with an expansion of the parts—these are favourable to the yellows, and, in a less degree, to the reds; of course the green, the primitive chlorophyll, especially.

May not all this, therefore, be correlated with the dwarfing? Strong metabolism is necessary; every energy must be thrown into the inflorescence; and, as a side-result, we get the crimsons and blues of mountain flowers.

It may be, even, that all, or nearly all, blue flowers were produced in this way. Very few of those groups of plants which are never alpine have blue flowers. There will be some exceptions, of course—for instance, I do not know how to account for the blue water-lily—and there are some groups in which a blue flower, however alpine the species, seems to be unproduceable. But even some of the very refractory genera, as *Erysimum* and *Troximon*, do go so far as to produce an occasional purple flower at high altitudes.

If I am right in my suggestions, the need for the selection by bees, made so much of by various authors, no longer exists. My own observations make me very doubtful whether bees do really prefer blue flowers at all, as a general thing—and I can hardly bring myself to believe, though the present hypothesis fall to the ground, that blue in flowers is a result of insect-selection.

T. D. A. COCKERELL.

NOTES.

AN International Scientific Congress will be held in Moscow in 1892. It will be divided into two sections, one of which will deal with zoology, the other with anthropology and ethnography. It is expected that the meetings will be largely attended. According to the *Revue Scientifique*, there is some talk of excursions to the Caucasus and to Turkestan.

WE learn from *Science* that the United States Coast and Geodetic Survey Office lately received from its sub-office in San Francisco a telegram announcing that an agent of the Alaska Commercial Company had arrived from St. Paul, Alaska, bringing letters from the Coast and Geodetic Survey parties who have been engaged in making explorations and surveys on and near the 141st meridian of longitude (the boundary between Alaska and the British possessions). These two parties were commanded by Messrs. J. E. McGrath and J. H. Turner, assistants of the Coast and Geodetic Survey. The party under Mr. McGrath ascended the Yukon River to the boundary-line, and there made its head-quarters, while that headed by Mr. Turner went up the Porcupine River to the Rampart House (the Hudson Bay Company's trading-post in the vicinity of the boundary), and there camped for the further prosecution of their work. Both parties were at their posts early in the autumn of 1889. The records made by Mr. McGrath's party comprise a set of magnetic and meteorological observations for a year; a set of specimens of sediments obtained from filtering certain measured quantities of the water of the Yukon River, made at regular intervals; certain botanical specimens; and a series of photographs. Mr. McGrath also gathered considerable information from some of the most intelligent of the Indians whom he encountered at Forty-Mile Trading-Post Owing to the stormy weather, Mr. McGrath was unable to obtain a sufficient number of astronomical observations to justify him in returning last autumn; and his party will therefore remain until next spring, and then descend the river, doing what work they can in the cause of science on their way down. Mr. Turner's party were much more favoured by the weather than the other party. They completed the necessary astronomical observations for the determination of the geographical position of their station on the Porcupine River at the boundary-line, also a set of magnetic and meteorological observations, and made a topographic map (on a scale of 1 : 5000) of the river in the vicinity of their camp, and a survey (on a scale of 1 : 200,000) from the boundary to Fort Yukon, a distance of about 100 miles. A small scheme of triangulation was undertaken to "locate" three monuments placed to mark the boundary-line. An exploring expedition was sent during the months of March and April to explore the line northward to the Arctic Ocean. The party visited Herschel Island. During May another trip was made about forty miles to the southward, as far as Salmon Trout River. Mr. Turner reached St. Michael's on August 30, 1890, with his party, too late to catch the steamer going south. The party will winter there, and in the spring carry the triangulation toward the mouth of the Yukon River, until relieved by orders from the Coast and Geodetic Survey Office.

THE next Deutsche Geographentag will be held in Vienna on April 1, 2, and 3.

THE other day, on the occasion of the sixtieth anniversary of the first doctorate of M. de Quatrefages, the anthropologist and zoologist, a small group of his most intimate friends and pupils met at his house and presented him with a very fine copy of an etching of himself, prepared without his having heard anything about the matter. M. de Quatrefages is over eighty years of age; he is in excellent health, and works hard. A few weeks ago he was elected President of the French Geographical Society.

IN St. Petersburg, an Institute for Experimental Medicine has been founded by the liberality of Prince Alexander Petrowitch of Oldenburg. It is intended chiefly for the study of the causes of infectious disease, and methods of prevention and cure. The Prince proposes to invite eminent men of science to make experiments in bacteriology, physiology, chemistry, biology, and the veterinary art. The building, which is being elaborately fitted up, stands in the Lapukins Kaja Street, on the banks of the Neva. Like the Pasteur Institute in Paris, it will have a department for clinical observation.

AT the meeting of the Photographic Society of Great Britain, on December 9, 1890, Mr. W. S. Bird spoke of the work done by the Committee which had been deputed to consider various questions relating to the proposed Photographic Institute. The labours of the Committee had resulted in the drawing up of a large scheme and of a smaller scheme. The larger scheme was a project of the future, but the smaller one was of a more practicable nature, and the Committee had had the advantage of conferring with the Lord Mayor on the subject, who thought that it might be possible to collect a sum which had for its limit £10,000, provided that the project was shown to be of public utility. If this was so, he thought that the photographic community generally should take some steps towards defining practical methods of carrying the scheme forward, and then they could approach the Lord Mayor with this project, and with some promises of financial support. In foreign countries the Government provided the means by which such institutions were founded. In England this was not the case, and he (Mr. Bird) felt that if this movement was to be a success, it must begin at home, and the Photographic Society ought to use what influence it possesses to assist in the work. After some discussion the following resolution was adopted :—"That the project be submitted, in the form of a circular letter, to the different provincial and metropolitan Societies, to elicit the opinion of the photographic public generally upon the scheme now brought forward."

AT the Annual Conference of Principals of University Colleges, held at Bangor, on December 23, the following subjects were discussed :—(1) The University of London: (*a*) the proposed scheme for reconstituting the University; (*b*) local centres at provincial Colleges for the honours examinations of the University. (2) Day Training Colleges: (*a*) relations of the University Colleges in this connection to the Education Department and the Science and Art Department; (*b*) organization necessary to make these relations operative. (3) County Councils: (*a*) relation of County Councils to educational institutions outside the strict county area; (*b*) devotion of local taxation money to purposes of technical instruction. An invitation from University College, London, to meet in London in 1891 was accepted.

AT the meeting of the French Academy on December 8, M. Mascart presented a work by General A. de Tillo on the distribution of atmospheric pressure in the Russian Empire and Asia, from 1836 to 1885. The work consists of an atlas of 69 charts, and a discussion of the monthly and annual values, as well as of the variability of pressure, and the relations existing between the variations of pressure and those of temperature at 136 stations. The highest pressure quoted is 31·63 inches (reduced to sea-level) in December 1877 at Barnaoul, and this is stated to be the highest reading on record. But in the Quarterly Journal of the Royal Meteorological Society for July 1887, Mr. C. Harding quoted, on the authority of Prof. Loomis, a reading of 31·72 inches on December 16, 1877, at Semipatalinsk. In NATURE, vol. xxxv. p. 344, Mr. Blanford quoted the lowest reading on record at any land station, viz. 27·12 (reduced to English standards), which occurred on September 22, 1885, on the coast of Orissa. These readings give a difference of 4·6 inches, probably the maximum range of the barometer ever observed at the earth's surface.

THE weather review accompanying the Pilot Chart issued by the U.S. Hydrographic Office states that very heavy weather prevailed in the North Atlantic during the month of November; westerly gales accompanied by heavy rain, hail, or snow, with tremendous seas, having raged over almost the entire region between Newfoundland and the British Isles, with the exception of a few days. Eight storms reached the ocean from the continent of North America, two of which crossed the British Isles. Six severe storms also originated in mid-ocean, most of which more or less affected the weather in these islands. Very little fog was reported during the month, and only one iceberg. It is proposed to publish with the next Pilot Chart a supplement devoted to the subject of ice in the North Atlantic during the season of 1889–90, which is perhaps the most notable ice season on record.

AT the recent Congress of Americanists at Paris, Dr. Seler showed that the name Anahuac had been applied by mistake to the plateau of Mexico. "Anauac" means "on or near water," and by all ancient writers was used in the sense of coast-land. Anauac Ayotlan was the seaboard of the Pacific; Anauac Xicalanco that of the Atlantic. Motolinia alone used the word differently. He did not, however, apply it to the plateau, but to the whole of New Spain. According to Dr. Seler, this also was a blunder, and was due to the phrase "cem anahuăc," which is used for "the whole world." The original meaning was "the entire land down to the sea-shore."

A CIRCULAR has been issued by the Department of Science and Art, calling the attention of secretaries of science classes and schools, to alterations to be adopted in the "staging" of science drawings, which are to be sent up to South Kensington for examination in April next. This "staging" has a distinct meaning different from that of "elementary," "advanced," or "honours" stages as used in respect of the subjects of science examinations held in May. The secretaries are therefore requested to avoid using the terms "elementary," "advanced," and "honours," in connection with the labelling of the drawings from copies, from measurement, and original designs which may be submitted next April. Such drawings are to be labelled according to stages about which precise instructions are given.

THE utility of the microphone for observation of earth-tremors and noises was soon recognized, and in Italy especially attention has been given to adapting the instrument for this use. We learn from the *Rivista Scientifico-Industriale*, that Signor Baratta, finding some defects in a method of mechanical registration of the motions of a seismo-microphone, which he had devised, has substituted a photographic system with advantage. The device is briefly this:—The telephone wire is connected with a subterranean microphone. Before the telephone-diaphragm (vertical), and connected with its centre by a fine aluminium wire, is a short slip of the same metal, fixed below, and having a curved piece at the top, which rests against a small mirror, movable about a horizontal axis. This mirror reflects the light from a lamp and lens to photographic paper on a rotated drum. The light is momentarily shut off every quarter of an hour, by a shutter arrangement, worked electro-magnetically by the clock-work which moves the drum.

THE French Department of Trade and Industry is issuing, in two large octavo volumes, the popular lectures given in the Trocadéro Palace during the Exhibition, for the benefit of visitors. The first volume has just appeared. The subjects are most varied, and many of the lectures are valuable.

A RECENT microscopical study, by Herr Schultz, of the skin of toads and salamanders, has yielded some interesting results (*Archiv für mikroscop. Anat.*). There are two kinds of glands, mucus and poison glands. The former are numerous over the whole body; while the latter are on the back of body and limbs, and there are groups in the ear-region behind the eye; and, in the salamander, at the angle of the jaw. The mucus glands are spherical, have a clear glassy appearance, and contain mucus cells and mucus; the poison glands, which are in regular strips on the salamander, are oval, much larger, and have a dark granular look, from strongly refractive drops of poison, a good reagent for which is copper-hæmatoxylin. The poisonous elements are from epithelial cells lining the glands. The mucus glands are for moistening the skin, and the liquid has no special smell, nor a bitter or acid taste. The poison glands are of course protective, and the corrosive juice is discharged differently in toads and salamanders, on stimulating electrically; in the latter it is spirted out in a fine jet, sometimes more than a foot in length, whereas in the toad, after longer action of the current, it exudes sparingly in drops. The physiological action of the poison has lately been studied by some Frenchmen. There is no reason, according to Herr Schultz, for supposing that the mucus glands sometimes become poisonous.

THE preliminary copy of the new map of the Chin-Lushai country, prepared for the surveys made during the recent military expeditions, shows, according to an Indian paper, what remarkable progress has been made in filling up the blank spaces which disfigured the old survey sheets. In the expedition of 1871–72 the country for some considerable distance to the south of Manipur was surveyed, and now the operations from the Chittagong side and from Upper Burmah have enabled the British officers to map in the hill tracts between Demagiri and Kan. Fort White, to the north-east, has also served as a base for valuable observations, while on the south-east the Chinbok country has also been explored. During the present winter several survey parties will be sent out, and a year hence the whole mountainous region between Manipur and Arakan will probably have been mapped.

THE wren is generally supposed to be a gentle little bird; yet on occasion it seems capable of displaying anything but an amiable temper. In the current number of the Selborne Society's magazine, Mr. Aubrey Edwards gives from his note-book the following account of what he calls "a disgraceful scene" between two male wrens:—"April 15, 1889.—I have just been watching two golden-crested wrens fighting. They first attracted my attention by getting up from the ground almost under my feet, and engaging again and falling to the ground. Then rising again one chased the other into a yew-tree near, where I had a good close view of them as they challenged each other, ruffling their feathers, shaking their bodies, singing and dancing about with crests erected, the sun shining on the orange-coloured crests—such a pretty sight. After they had been talking big at each other for some minutes, the hen arrived on the scene, and a desperate fight ensued, the two cocks falling to the ground in fierce embrace, rolling over each other occasionally, but for the most part lying still on the ground with their claws buried in each other's feathers for about a minute. The hen was close by them on the ground, moving about and looking very much concerned at the affray. Her pale yellow crest contrasted notably with the rich orange of the males. After getting up, renewing the combat in a currant bush, falling again and struggling on the ground, they rose and had a chase round the yew-trees, the hen following to see the fun, and presently went off and were lost to view."

DURING the past year the question of technical education in Burmah was taken up under the orders of the Government of India. A new set of examination standards was considered; and at the same time a survey was made of the industries practised in Lower Burmah. A provincial Board is at present being constituted to consider the results of the survey, and to advise the Government generally in all matters connected with technical and industrial instruction. A sum of Rs. 10,000 has been allotted in the current year's budget, and it is hoped that the cause of technical education has secured a real and substantial foundation for future progress. Four engineering pupils at the Sibpur College were some time ago in receipt of Burmah scholarships, but the grant of these scholarships has now ceased, and no more engineering students will be sent to Calcutta.

THE Government of Bombay, in concluding a recent resolution on education in the Presidency, remarks that the degree to which private enterprise has expanded under the revised Grant-in-aid Code has exceeded all anticipations. In 1884–85 the expenditure on institutions maintained by the Department was Rs. 8,47,260 out of a total expenditure of Rs. 41,42,734. It is now little more than 8 lakhs out of $56\frac{1}{2}$ lakhs of expenditure. The whole growth of ways and means has thus been left to other agencies than Government. In the same year aided institutions under private management received from provincial revenues under 2 lakhs, and they now receive from that source alone nearly $4\frac{1}{2}$ lakhs. Private enterprise is becoming the chief power in educating the people. Another marked change is the growth of technical education and the introduction of various types of instruction. In 1884–85 the greater part of the expenditure by Government on its own institutions, which aggregated. Rs. 8,47,260, was assigned as follows: nearly 6 lakhs to colegiate and secondary education, and 1¾ lakhs to special education. In the current year Rs. 2,39,479 out of the 8 lakhs spent on Government institutions were devoted to special education. Private enterprise manages special institutions at a cost of Rs. 1,46,261, whereas the expenditure on this head by aided schools in 1884-85 was only Rs. 39,319. There is thus a greater diversity introduced into educational activity as well as into educational agency. Collegiate and secondary education of the ordinary type receive more expenditure than they did, but there is still greater expansion in other directions. The vastly increased employment of Municipal and Local Boards in the management of primary education is another marked feature of recent years. The tendency of education to range itself under diverse forms of instruction, and under a great multiplicity of controlling agencies, renders the task of administration and of apportionment of public funds as grants-in-aid more difficult. It is remarked with satisfaction that complaints against the Grant-in-aid Code are never heard, and that the record of each succeeding year shows increasing attendance at schools and the enlistment of larger private resources in the work of education. "The Governor in Council is satisfied that the Report of public instruction for the year ending March 31, 1890, will be generally regarded as affording proof of the sagacity with which the Department was administered, and new impulses given to it during the administration of His Excellency the late Governor" (Lord Reay).

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mr. P. Boulton; a Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Captain Worster; two Common Marmosets (*Hapale jacchus*) from South-east Brazil, deposited.

OUR ASTRONOMICAL COLUMN.

GREENWICH SPECTROSCOPIC OBSERVATIONS.—The results of the spectroscopic and photographic observations made at the Royal Observatory, Greenwich, in the year 1889 have recently been issued. Mr. Maunder examined the spectrum of χ Cygni on June 3, and compared it with the spectrum of hydrogen as given by a vacuum tube, and that of carbon as given by a Bunsen flame. C and F were not present as bright lines. There was some uncertainty about the G hydrogen line, a bright spot being suspected at or near its position in the violet region. D_3 was not seen as a bright line, but there was a brighter part about λ 5823, which may have been "a mere effect of contrast, or a local brightening of the continuous spectrum." It is also recorded that "the close correspondence as to position of the green and blue bands of the hydrocarbon spectrum with two of the bright zones or interspaces of the stellar spectrum was very apparent. No such correspondence was evident in the case of the yellow band." The mean wave-lengths obtained from two sets of measures of the more refrangible edges of Dunér's bands VII. and IX. are 5170 and 4766. On June 17 an observation was made of the spectrum of Uranus. The spectrum was traced from about λ 6200 to about λ 4600. Measures of the positions of two "dark bands," and an "ill-defined bright region which seemed to consist of two diffused bright bands," gave the following mean wave-lengths :—

Object measured.	Mean wave-length.
Darkest line	5419
Bright band	5336
Bright band	5276
Dark line	4866

"Other irregularities in the brightness of the continuous spectrum were suspected on either side of the principal dark line."

Measures of two of the bright lines in the spectrum of R Andromedæ were obtained on October 19. The brightest line was found to have a wave-length 4872, and was probably F, whilst the position of a feeble bright line was determined as λ 5136.

The spectrum of Davidson's comet (*e* 1889) was observed on August 29. It is recorded that "one bright band in the green could be distinctly made out when the slit was opened very widely. It coincided nearly, if not exactly, with the green band of the hydrocarbon spectrum." No measures were made of the wave-length of the bright band.

The other observations contained in this publication refer to the motion of stars, the sun, moon, and planets in the line of sight, and measures of photographs of the sun.

The volume of "Greenwich Observations" for 1888 has also been issued, and is of the usual character.

COMETS ZONA AND SPITALER.—The following elements have been computed for Spitaler's comet :—

T = 1890 October 26·50833 Berlin mean time.

$$\left.\begin{array}{l} \pi = 58^\circ\ 24'\ 28''\!\cdot\!2 \\ \Omega = 45\ \ 7\ 51\cdot 2 \\ \iota = 12\ 51\ 49\cdot 0 \end{array}\right\} \text{Mean Eq. } 1890\cdot 0.$$

$$\phi = 28\ 11\ 26\cdot 6$$

$$\log a = 0\cdot 537532$$

$$\mu = 554''\cdot 2$$

Period = 6·4 years.

Ephemerides for Berlin Midnight.

ZONA'S COMET.

1891.	R.A. h. m. s.	Decl. ° ′	Brightness.
Jan. 2 ...	2 1 42 ...	+ 29 14·5 ...	0·37
,, 4 ...	1 57 59 ...	28 53·1	
,, 6 ...	1 54 36 ...	28 32·8 ...	0·33
,, 8 ...	1 51 31 ...	28 13·6	
,, 10 ...	1 48 42 ...	27 55·6 ...	0·29

SPITALER'S COMET.

1891.	R.A. h. m. s.	Decl. ° ′	Brightness.
Jan. 5 ...	4 52 24 ...	+ 40 22·5 ...	0·71
,, 9 ...	4 51 52 ...	40 25·5	
,, 13 ...	4 51 58 ...	40 25·5	
,, 17 ...	4 52 49 ...	40 23·5	
,, 21 ...	4 54 24 ...	40 19·5	

Brightness at discovery = 1.

Zona's comet is therefore still near Triangulum. Spitaler's comet is in Auriga, and not far from Capella.

The sidereal time at 10 p.m. on January 1 is 4h. 45m. 2·5s., and on January 10 is 5h. 20m. 30·1s.

THE LEONID METEORS.—The current number of *Comptes rendus* (December 22) contains a note from Prof. Denza, the Director of the Vatican Observatory, on the observations made in November 1890 under the direction of the Italian Association for the Observation of Luminous Meteors. The results of the observations made at seven stations on November 13-14, 14-15, and 15-16 are tabulated. They show that the number of meteors seen was greater on November 15-16 than on November 14-15, and much greater on these evenings than on November 13-14. In previous years the maximum occurred on November 13 or 14.

The whole of the observations of this year indicate that the Leonids were more frequent than in previous years, hence it is probable that this augmentation will go on until the next shower in 1899.

SPECTROSCOPIC NOTE (D_3).—The Rev. A. L. Cortie, S. J., has brought forward the question, "Has the line D_3 a coincident dark line in the solar spectrum?" From a discussion of available observations, and an examination of some photographs taken by Mr. Higgs with a Rowland grating, it is concluded—

(1) That D_3 has been seen dark in a sun-spot.

(2) That there is a faint dark line in the solar spectrum, at least within two-tenths of a tenth metre from the bright line.

(3) That this line is due to our atmosphere.

(4) That observers are disagreed as to whether the dark and bright lines coincide in position. (*Monthly Notices*, November 1890.)

AMERICAN BLAST-FURNACE WORK.

THE history of the gradual development of blast-furnace working in America, culminating in the production of 2500 tons of pig-iron in the short space of one week, with an expenditure of only 16·8 cwt. of coke per ton of iron, is interesting, especially if compared with ours, where the largest make has not exceeded 1000 tons, with an expenditure of about 1 ton of coke. It is difficult to find satisfactory reasons for this great difference, and the members of the Iron and Steel Institute may well have been astonished at the results laid before them.

It is stated that the first good results were obtained by breaking away from the traditional practice of regulating the quantity of blast or air by the pressure-gauge, and noting instead the number of revolutions made by the blowing engine; probably it would have been better still if some means had been devised of registering the flow of waste gas issuing from the furnace-mouth.

The new era in the manufacture began in 1880, and is divided into three periods:—(1) Old practice—air supply limited, slow driving. (2) Air in excess, excessive driving, attended with a greater output of iron, but increased consumption of coke. (3) Moderate supply of air, rapid driving, great output of iron with decreased consumption of coke. The best results were obtained with air at a pressure of 9 pounds heated to 1100°, blown in at the rate of 25,000 cubic feet per minute, furnace 18,200 cubic feet capacity; a greater quantity, 30,000 cubic feet, proving too much, whilst with less the make of iron diminished. This furnace produced 10,035 tons of iron in one month, with an expenditure of 1834 pounds of coke; the average ratio of CO to CO_2 being 40 per cent. as against about 48 to 50 in this country. Altogether, the volume of air was increased from 16,000 cubic feet "old practice," to 25,000 per minute with improved results. Ultimately it is probable that 300,000 tons of iron may be produced in three years without repairs.

Some of our most prominent metallurgists took part in the discussion on Mr. Gayley's report, and it soon became evident that some long-cherished ideas and formulæ were open to question. One eminent authority was of opinion, however, that on the whole the utilization of fuel, as shown by analysis, was in favour of English practice; although the actual quantity of fuel burnt to CO_2 was 5·97 as against 4·99 cwt. in Pittsburg, accounting for the difference by pointing out the variations in the appropriation of the heat-units or calories under the various items. It was also remarked that the Pittsburg ore was much richer in iron than the English, the latter fusing 28 cwt. of slag as against 10·71 in the former. The loss of heat due to radiation, &c., and in the waste gases, was also less, and these combined causes might be said to account for the remarkable saving.

English metallurgists and engineers were not unanimous in their approval of the new system; such methods might not after all be better than the older and slower processes practised in this country; an expensive array of air-heating stoves, blast engines, boilers, &c., were required, entailing considerable outlay, possibly exceeding the margin of profit. On the other hand, it was contended that the greater output of iron and the saving in fuel, amounting to £24,000 per annum, would more than cover additional expense, and would be sufficient for relining and repairs of the furnace and accessories three or four times over. Fewer furnaces would suffice, and it would be easier and less expensive to manage, say, two furnaces, than four or more. A well-known authority declared in favour of large makes, coupled with rapid driving, but confessed that after all it was not clear where the advantages lay as regards the plant, working, and final results.

The most suitable form of furnace was discussed, but nothing very definite seems to have been said, beyond the general admission that the form facilitating the unchecked free descent of the ores, &c., was most suitable; a large hearth or crucible was also desirable. The equal distribution of the ascending reducing hot gases was not thoroughly discussed, although it is obvious that unequal distribution of the hot gases must correspond to a like varying reduction and heating of the ores, &c., charged. According to Dr. Percy, the equal distribution of the gases, &c., is of the utmost importance, and in his work a furnace is figured, giving the form of furnace most suitable for general uses; he states that, unless the interior parts, from the mouth downwards, are not properly proportioned, the regular production of iron of uniform quality cannot be insured.

It is submitted that the able discussion which has just been summarized, although accounting for a good deal, is still not so satisfactory as might be. The scientific researches on the *rationale* of the chemical reactions occurring in the blast-furnace, may now be termed complete. We can now determine exactly the chemical reactions of the gases passing through, as also the relative distribution of the calories which a given weight of carbon will produce when burnt to carbonic oxide in the furnace. So far, this seems satisfactory; but one pound of carbon, when burnt in the calorimeter, always gives the same number of calories, *i.e.* number of pounds of water heated 1°, "this is all." It is, however, well known that its combustion may be so regulated as to barely suffice for the fusion of a metal like lead; and with the same fuel a temperature may be attained equalling the fusion-point of the softest steel.

Temperature or heat-intensity may play an important part in blast-furnace work. Iron cannot be properly melted below a temperature well above its fusion point, and unless the fuel is burnt under well-understood conditions, *i.e.* very rapidly, it often occurs in practice that fuel is so slowly burnt as to barely suffice for fusion. Fusion is unduly prolonged, and obviously fuel is wasted, for the simple reason that heat-intensity is proportionate to the rate at which it is consumed.

Economical work depends entirely on the difference in temperature betwixt the heating medium and the heated body; and it follows, as the temperature of the furnace flame and gases approximates to that of the metal heated, considerably less heat is absorbed; finally, when the temperature of the metal approximates closely to that of the flame, the heat absorption becomes nearly inappreciable; thus fusion may be unduly prolonged. This final stage may be termed the critical point, sharply dividing the economical use of fuel from the reverse.[1]

The importance of maintaining the temperature at the end of the process of fusion cannot well be over-estimated: a few hundred degrees more or less makes all the difference.[2] Apparently sufficient attention has not been paid to this in blast-furnace work, although it is plain that as regards fusion of the iron and slag, the same law is applicable as when simply fusing iron in ordinary melting-furnaces.

The delivery of large quantities of air at a high pressure through many large openings (technically the *tuyères*), as practised in America, and the consequent rapid consumption of coke, must necessarily raise the heat-intensity (temperature) much above that of furnaces supplied with only a limited quantity of air, and it follows that, in the latter, iron, &c., is fused slowly, thus entailing a greater consumption of coke.

Concurrently with rapid melting, it is self-evident that the furnace drives faster, for it is obvious that the rate of descent of the materials charged is governed by the more or less rapid fusion of iron and the accompanying consumption of fuel. It is pretty generally admitted that rapid driving should be avoided, on the reasonable assumption that time is required for the complete reduction of the ore by the ascending gases. Yet in America only 16·8 cwt. of coke per ton of iron has been consumed in conjunction with rapid driving; but possibly temperature plays an important part throughout the whole smelting process. The law of heat exchanges previously discussed cannot be evaded. We will assume that the temperature at the *tuyères* in one furnace is 500° higher than in another; it follows that the comparatively cool descending materials absorb in a similar ratio, or more plainly a greater percentage of the total heat is utilized in the former than in the latter instance. It is, therefore, not so surprising that the waste gases leave the high temperature furnace at the comparatively low heat of 340°.

It would appear that, on the whole, sufficient attention has not been directed here to the absolute weight or volume of air used per unit of time with its consequent effects, and it may be ques-

[1] "When iron has attained a temperature of 2500°, it will, when exposed to a heat-intensity of 3200°, absorb less than 1/11 of this, and when iron is receiving the last 100 requisite to bring it up to the welding-heat of 3200°, the temperature of the furnace being 3300°, only 1/24 will enter the iron, the remaining 23/24 being wasted. By raising the temperature to 3700°, 1/6 instead of 1/24 will be utilized, thus exhibiting the fact that an addition of temperature of only 1/8 increases the efficiency of the furnace fourfold."—*Prideau.*

[2] Cause of the remarkable saving of fuel effected by hot blast (Percy, "Metallurgy," p. 425). "It is therefore plain that the mere quantity of heat, *i.e.* the number of units of heat evolved can have little to do with the matter. This being admitted, the inevitable conclusion is that calorific intensity must be concerned, and that the temperature of what may be designated the most active part of the furnace must be higher in the case of hot blast than in the case of cold blast."—*Percy.*

tioned whether as a general rule in this country the area of the air or blast-delivery pipes has been correspondingly enlarged in due proportion to the increased air temperature, often over 1000°, now not uncommon since the introduction of the modern brick-heating stoves. Surely with air expanded to nearly four times its normal volume at 60°, the ordinary blast pipes should be correspondingly enlarged, coupled also with a higher blast pressure. Thus it may be, when using highly heated air, the absolute weight of air (oxygen) supplied may not be properly proportioned to the weight of coke charged, greater or less; and if this be true, it is not surprising that many blast-furnace managers have condemned the use of superheated air; it seems just possible that coke may be charged in excess of the air supply, or *vice versâ*. In America they appear, "by dint of sheer practice," to have somehow realized this; they have even gone further, and ascertained that it is quite possible to deliver too much air. It seems, on the whole, that their superior practice may partly, at least, be explained on the probability that heat-intensity—in other words, temperature expressed in C. or F. degrees—plays its part; and that calculations based on heat-units alone, undoubtedly useful and necessary as they are, have only a limited application.

THE ZOOLOGY AND BOTANY OF THE WEST INDIES.[1]

THIS Committee was appointed in 1887, and reappointed in 1888 and 1889.

During the past year chief attention has been directed to the exploration of the island of St. Vincent, and two collectors have been maintained in that island at the expense of Mr. F. Du Cane Godman, who has kindly assisted the Committee in this manner in order that the funds at its disposal may be chiefly applied to the remuneration of contributors, to whom would be referred the large collections in zoology, already amounting in Insecta alone to about 3000 species. The plants have been determined at the Herbarium of the Royal Gardens, Kew, and are nearly completed to date. A separate report on the collections in zoology and botany is given below.

It is proposed by the Committee to accept the services of Mr. R. V. Sherring, F.L.S., to make collections in botany in the island of Grenada during the coming winter. Mr. Sherring is well acquainted with the West Indies, and has already made collections there, and added several new species of ferns to the flora of Jamaica.

Zoology.

Since the last Report of the Committee three collections have been received from Mr. H. H. Smith, the collector sent by Mr. Godman to the island of St. Vincent. These collections include a complete set of the birds already known to inhabit the island, and a few additional species; a small number of reptiles and crustaceans; a large series of spiders; and a great many Insecta; these last amounting, it is thought, to about 3000 species.

In 1889, Colonel Feilden paid a visit to the island of Dominica for the purpose of ascertaining whether the Diablotin (*Œstrelata hæsitata*) has become extinct there, as has been reported by Ober. The account of his expedition that Colonel Feilden has published leaves little doubt that this is the case.

Although Mr. Smith has now been occupied about a year and a half in the exploration of the island of St. Vincent, Mr. Godman has decided, with the concurrence of the Committee, that he shall still continue there, as it is not yet clear that the more inaccessible portions of the island have been sufficiently examined.

Mr. Godman has agreed to give a first set of the zoological specimens obtained by his collector to the National Collection contained in the British Museum, and the Committee is at present endeavouring to find competent zoologists to work out the extensive series of insects and spiders that has been obtained.

Commander Markham, R.N., contributed some specimens in zoology collected by him in the Leeward and Windward Islands of the West Indies, and Captain Hellard, R.E., local secretary to the Committee at St. Lucia, has recently forwarded four boxes of Lepidoptera collected by him in that island.

Botany.

A small collection of plants, numbering 143 specimens, was received from Mr. J. J. Walsh, R.N. This collection included plants from Dominica, St. Martin's, St. Eustatius, St. Kitts, St. Lucia, and Grenada. Most of the plants consisted of common West Indian species, presumably such as would be met with in the more accessible spots in the various places visited.

The remainder of the plants collected by Mr. Ramage at St. Lucia have been determined. Of 84 species sent, 62 have been fully determined. The others include several that are apparently new. They are wholly woody or forest plants, and comprise *Sloanea* sp., *Picramnia* sp., *Xanthoxylum* sp., *Bursera* sp., *Miconia* sp., *Cybianthus* sp., *Lucuma* sp., *Siparuna* sp., *Helosis* sp., *Gymnanthes* sp., and *Cyclanthus* sp. In one or two cases the material is hardly sufficient for satisfactory determination. Two of the above undetermined species have also been collected in Dominica and one in Martinique by earlier collectors.

Three collections have been received from St. Vincent through Mr. Godman, viz. in September 1889, and March and August 1890. The first collection has been determined at Kew by Mr. Rolfe as far as the end of the *Polypetalæ*. Of the 252 numbers (to this point) 47 were duplicates; thus 205 species were represented. All but about 9 of these were fully determined, the great bulk consisting of widely diffused West Indian plants; 128, or more than half, appear to have been recorded from the island before.

The undetermined specimens are *Trattinickia* sp., *Stigmaphyllon* sp., *Trichilia* sp., *Meliosma* sp., *Lysiloma* sp., *Moquilea* sp., a species of *Eugenia* obtained by Hahn in Martinique, and two species, probably of *Pithecolobium*, of which the material was somewhat inadequate. Several of these appear to be new, the first-named being specially interesting, because the genus was hitherto only known from Guiana and Brazil. In addition to this may be mentioned that several species of somewhat restricted distribution in the West Indies, more especially from Martinique and St. Lucia, have also been found in St. Vincent.

The second collection from St. Vincent consisted for the most part of ferns. Mr. J. G. Baker has fully worked out these. They include 133 species and well-marked varieties, three of which are new. The specimens are in excellent state of preservation, and it is probable that we have amongst them nearly all the fern flora of the island, both of the mountains and the lowlands.

As our knowledge of the fern flora of St. Vincent may be now regarded as practically exhaustive, it seems probable that some species hitherto attributed to the island, on the authority of specimens collected by the Rev. Lansdowne Guilding, really belong to other islands. This error has arisen from want of precision in exactly localizing the specimens, a practice the importance of which was hardly recognized at the time they were collected.

The collections received in August last contain three additional species of ferns, making the total number collected by Messrs. Smith 136. The added species are *Dicksonia cicutaria*, Sw., *Davallia aculeata*, Sw., *Cheilanthes radiata*, R. Br. In addition there are 389 numbers of flowering plants, and 3 palms. These will be determined later.

The Committee would again draw particular attention to the botanical and zoological bibliography of the Lesser Antilles prepared under its direction, and published as an appendix to the Report for 1888. This bibliography has been widely distributed in the West Indies and in Europe, and has proved of considerable service in carrying out the objects for which the Committee was appointed.

The Committee recommend their reappointment, and that a grant of £100 be placed at their disposal.

THE HILL ARRIANS OF INDIA.

AT a recent meeting of the Anthropological Society of Bombay, a paper was read by the Rev. A. F. Painter on the Hill Arrians, who live along the slopes of the Western Ghats in the Native State of Travancore, between Quilon in

[1] Third Report of the B.A. Committee, consisting of Prof. Flower (Chairman), Mr. D. Morris (Secretary), Mr. Carruthers, Dr. Sclater, Mr. Thiselton-Dyer, Dr. Sharp, Mr. F. Du Cane Godman, Prof. Newton, Dr Günther, and Colonel Feilden, appointed for the purpose of reporting on the present state of our knowledge of the Zoology and Botany of the West India Islands, and taking steps to investigate ascertained deficiencies in the Fauna and Flora.

the south and the Travancore-Cochin boundary line in the north. They differ considerably from the ordinary hill tribes of India, and Mr. Painter considers them as Dravidian rather than Kolarian. In colour many of them are remarkably fair. The men average 5 feet 6 inches in height. Their features are, as a rule, well formed. The lips are thin and the nose frequently aquiline. Their villages are situated at a height between 2000 and 500 feet above sea-level. The houses are generally built of split bamboo and mud with grass thatching, but wooden houses such as those used by the inhabitants of the plains are not uncommon. They cultivate the surrounding lands with rice and vegetables.

Their religion and social customs differ considerably from other Malayalam-speaking people, more markedly so in places where they have not come under the power of those living in the plains. Thus in Malabar the law of inheritance through the sister's children prevails. Even the Musnud, or throne, is inherited in this way. But among the Arrians, except where they have been brought under the power of the Hindus, inheritance through the father's children prevails. Even where the former law has been forced upon them they evade the consequences by marrying cousins, so that the property remains in the family. They are divided into *illams*, or clans. Members of the same *illam* may not intermarry; men of a superior *illam* may marry a woman of an inferior *illam*, but the reverse may not be done. There appears to be no difficulty about eating together, but only about intermarriage.

Women occupy a much better position among the Arrians than among the Hindus. They are regarded as equals, move about unrestrictedly, and eat with their husbands, especially at feasts. The fact that a woman eats out of a man's plantain leaf is a sign that she is his wife. The marriage tie is considered sacred, and seldom broken. Polygamy is almost unknown. A man married two sisters, and was considered to have disgraced himself, and was shut out from all feasts, &c. Adultery is considered a great crime. Infant marriage is unknown amongst them, but a curious ceremony prevails, copied from the customs of Nairs and Chogans, though differing in several particulars. As soon as a woman attains maturity, relatives and friends are summoned to a feast. The propitious hour having been fixed, the girl is brought in and made to stand on a plank of jackwood (a tree considered sacred by the Arrians); the father's sister then ties the *tali* or thread round the neck, the feast is then partaken of, and the ceremony is considered complete.

The actual marriage ceremony among the Arrians takes place when the woman is seventeen or eighteen years of age. The horoscopes of the different parties are examined, and the day fixed by the astrologer. Invitations are issued, and a *pandal* erected and the bride placed seated inside. The bridegroom is then brought up by his friends, who demand to know who is inside. The reply is such and such Illakar, as the case may be. If the reply is satisfactory they advance inside, and the bride is brought and placed in the centre. The conductor of the ceremonies on the bride's behalf then proclaims in a loud voice, "I am about to give a woman of such an *illam* to a man of such an *illam*." On the bridegroom's behalf a similar announcement is made. And a set of new clothes is presented by the bridegroom to the bride, and afterwards the happy pair eat out of the same vessel or leaf. This is the crowning part of the ceremony. After a feast the bride is conducted by the wedding party in state to the bridegroom's house, where another feast is spread. The wife lives with her husband in his house.

The Arrians bury their dead. Ancestral worship being practised among them, the ceremonies connected with death are the most elaborate and important. Death brings defilement with it, and none in the house may eat until after the funeral. The body having been washed and betel-nut placed in the mouth, a member of the same clan is appointed, who undertakes to act as master of the ceremonies. He first carefully bathes, then takes a new cloth, and from it tears a narrow strip which he fastens upon himself after the fashion of the Brahminical thread. Going to the place selected for the grave he calls upon the earth to give up 6 feet. He then advances backwards and digs with a hoe, removing three hoesful of the earth. Afterwards he may dig facing the grave. This completed, the body is brought forth and laid in it, the head always lying towards the south. The earth having been thrown, he again advances backwards and draws with a knife three lines round the grave, which are supposed to protect it from evil spirits. A cocoa-nut is broken and some paddy is strewn on the top. In addition to this in some hills a light is placed at the head, another at the foot of the grave. The master of the ceremonies again bathes and returns to the house; two sticks tied crossways are taken and rags soaked with oil tied in the ends and lighted. Taking them in his hands he walks in procession round the house three times, followed by the relations of the deceased. The sticks are then placed, one at the head, the other at the foot of the grave.

After the ceremonies at the grave are over, all concerned in them bathe, a clean new cloth is placed in an inner room of the house, and on it the dead man's property, knife, betel-box, topee, &c., are placed. A feast is prepared, plantain leaves are cut into narrow strips, rice, boiled fowl, plantain, fish, toddy, arrack, and parched rice are placed upon the leaves, lights are lighted, the master of the ceremonies then does obeisance to the spirit which is now supposed to be in the house. The door is closed and the spirit is left to feast. After half an hour it is opened and the things taken forth. At the conclusion of the ceremony the whole assembly partake of a feast consisting of flesh, fish, rice, and arrack. As soon as possible an image of the deceased is prepared, which is brought into the house. Twice a year similar offerings are presented; and in times of drought, ravages by wild beasts, or sickness, vows are made, and prayers such as, "O Ancestor, be not angry with us," are offered.

Female ancestors receive equal honours with males. Of what happens to a soul after death they have no certain belief. The doctrine of transmigration is unknown amongst them. Spirits of men and women for whom no offerings have been made are said to wander about working mischief. If a man dies from accident, such as the fall of a tree, or is killed by a man or wild beast, no ceremonies may be performed for him, nor in the event of a woman dying in child-birth. The spirit is said to wander about working mischief. Ancestral worship is the essential part of their religious system. It consists of a yearly feast and offering to the spirit of ancestors similar to that described as made on the eleventh or fifteenth day after death.

Besides worship of ancestors, there is also the worship of evil spirits or demons, which appears to consist in paying "black-mail" to avoid injury, or bribes to inflict injury on others. The chief demon worshipped by them is the goddess of small-pox, cholera, &c., and it is noticeable that in all their religious festivals and ceremonies strong drink plays a very large part.

ANCIENT MOUNDS AT FLOYD, IOWA.[1]

ON the west side of the Cedar River, one half mile east from Floyd, Iowa, are located a group of three ancient mounds. These mounds, instead of being located on the highest eminence in the region, as is most usually the case, are arranged in a slightly curved line, on a high but level space, fifty feet above, and two hundred and twenty yards back from the stream, and midway between two points (from fifty to sixty rods from each) which face the river, and rise from twenty-five to fifty feet above this level space. The ground, between the mounds and the Cedar, has a rather gently sloping surface. At this point the stream makes a bend to the east, and the mounds thus occupy a position on the south side. The north side of the stream is occupied by a steep, and somewhat broken, wooded bank, which affords a limited though beautiful bit of scenery to this place.

This area, as well as the surface of the mounds themselves, was originally possessed by a heavy growth of timber, but which was cleared away more than twenty years ago, and the soil kept under the plough ever since. These mounds are low and circular, and twenty feet distant from each other. The east, or largest mound, is thirty feet in diameter, and was originally two feet high (so reported by Mr. Sharkey, who first cleared, and still owns the tract), although owing to degradation by the plough it now rises only one and a half feet above the surface of the ground surrounding the mound. The two remaining mounds are smaller and lower than the first one. The third mound—there may be some slight doubt expressed regarding its origin, for the reason that in the south portion of it there is embedded a drift boulder, weighing some seven or eight hundred pounds. This, however, may have been placed here by human hands in the long ago, or the mound may have been an intrusion upon the stone. A partial exploration of the two smaller mounds was made, but without discovering anything.

[1] Reprinted from *The American Naturalist*.

In making a thorough exploration of the larger mound, however, the remains of five human bodies were found, the bones, even those of the fingers, toes, &c., being, for the most part, in a good state of preservation. First, a saucer or bowl-shaped excavation has been made, extending down three and three-fourths feet below the surface of the ground around the mound, and the bottom of this macadamized with gravel and fragments of limestone. In the centre of this floor, five bodies were placed in a sitting posture, with the feet drawn under them, and apparently facing the north. First above the bodies was a thin layer of earth; next above this was nine inches of earth and ashes, among which were found two or three small pieces of fine-grained charcoal. Nearly all the remaining four feet of earth had been changed to a red colour by the long continued action of fire.

All the material of the mound, above and around the bodies, had been made so hard that it was with great difficulty that an excavation could be made even with the best of tools. The soil around the bodies had been deeply stained by the decomposition of the flesh. The first (west) body was that of an average-sized woman in middle life. Six inches to the east of this was the skeleton of a babe. To the north, and in close proximity to the babe, were the remains of a large, aged individual, apparently that of a man. To the east and south of the babe were the bodies of two young though adult persons. The bones of the wonan, in their detail of structure, indicated a person of low grade, the evidence of unusual muscular development being strongly marked. The skull of this personage was a wonder to behold, it equalling, if not rivalling in some respects, in inferiority of grade, the famous "Neanderthal skull." The forehead (if forehead it could be called) is very low, lower and more animal-like than in the "Neanderthal" specimen.

This skull is quite small for an adult individual. The inner portions of the brow ridges are slightly prominent.

The distance from the lower portion of the nasal bone to the upper margin of the eye cavities is only four centimetres. A slight portion of this bone has, however, apparently been broken away.

The distance between the eye sockets at a point midway between the upper margin of the eye cavities and the lower portion of the nasal bone is two and three-fourths centimetres. One of the jaws, containing well-preserved teeth, was found. This was rather strong, but the teeth only moderately so. We were at first inclined to consider the strange form of this skull as due to artificial pressure while living, but a critical examination of it revealed the fact that it was normal, *i.e.* not having been artificially deformed. The teeth of the babe were very small, and the skull thick, even for an adult person.

The next skeleton was that of a man nearly six feet in height. The crowns of all the teeth had been very much worn down, some of them even down to the bone of the jaw.

As before stated, the remaining bodies were those of young adult persons, the skull of one of which was small for a full-grown individual. No relics of any description were found with the human remains in this mound. Their burial appeared to be a very ancient one, the limestone fragments in the floor of the excavation being nearly if not all decomposed.

In other mounds opened on the same stream, at Charles City, six miles below, fragments of the same limestone were not infrequently found, but in no case was decomposition visible, except as a thin outer crust, although the human bones, which were usually more or less abundant, were in no case very well preserved, but, on the contrary, often nearly or entirely decomposed. The fine preservation of the remains in the mound at Floyd was due to the method of burial. This being evidenced by the fact that over a small portion of one of the bodies the earth had not been so thoroughly packed, and as a consequence the bones were almost entirely decomposed away, while the other portion of the body over which the soil had been very firmly packed was well preserved. Judging from all the facts gathered, it seems not improbable to suppose that this represented a family burial.

The question has been raised, "How was it that these five persons were all buried here at the same time, their bodies being still in the flesh?" As we have no reason to suppose that these ancient people possessed any means for preserving, for any length of time, in the flesh, the bodies of their dead, it seems plausible to suppose that these individuals were all swept off at about the same time by some pestilence; or else, upon the death of some dignitary of the tribe or people (perhaps represented by the remains of the old man) the other members of the family were sacrificed, similar to the custom which has prevailed among some ancient tribes or races of historic times.

On the same stream, a short distance below this mound, several other mounds occur which promise to yield interesting results, and which we purpose to explore as opportunity offers.

Charles City, Iowa. CLEMENT L. WEBSTER.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 11, 1890.—"Determinations of the Heat Capacity and Heat of Fusion of some Substances to test the validity of Person's Absolute Zero." By S. U. Pickering.

Person made determinations with eight substances to show that the temperature at which their heat of fusion became nil, $t - \frac{l}{C - c}$ (t = temperature of fusion, l = heat of fusion at $t°$, C = heat capacity of liquid, c = heat capacity of solid), was $-160°$ in all cases. This he called the absolute zero. His conclusion may for several reasons be questioned, the chief reason being that he determined C and c at any temperature which happened to be most convenient, and the value of these is largely dependent on temperature: they should both refer to the same temperature, and this is, necessarily, $t°$. The author deduces his values for C and c at $t°$ from determinations made at a series of different temperatures. The substances examined were, sulphuric acid and its monohydrate, hydrated calcium nitrate, and naphthalene, and their temperatures of recrystallization were found to be $-369°$, $-177°$, $-234°$, and $-214°$ respectively, thus refuting Person's conclusion. Benzene was also examined, but the heat capacity of the solid was found to be greater than that of the liquid; this is probably due to an incipient fusion occurring below the temperature of true fusion.

December 18, 1890.—"On the Generic Identity of *Sceparnodon* and *Phascolonus*." By R. Lydekker, B.A. Communicated by Prof. W. H. Flower, C.B., F.R.S.

In the year 1872, Sir R. Owen described two imperfect lower jaws of a large extinct Wombat, from the Pleistocene of Queensland, under the name of *Phascolomys* (*Phascolonus*) *gigas*. Subsequently he described certain imperfect upper incisors, from Queensland and South Australia, characterized by their peculiarly flattened and chisel-like shape, under the new generic name *Sceparnodon*.

In cataloguing the fossil Mammalia in the collection of the British Museum, the author was struck by the circumstance that, while the upper incisors of the so-called *Phascolomys gigas* were unknown, there were no cheek-teeth which could be referred to *Sceparnodon*, and it was accordingly concluded that the teeth described as *Sceparnodon* were probably the upper incisors of *Phascolomys gigas*, and on this supposition it was considered that the latter was generically distinct from existing Wombats, and it was accordingly entered as *Phascolonus gigas* in the Museum Catalogue.

The author now described incisors of *Sceparnodon* and lower jaws of *Phascolonus gigas* obtained at Bingera, New South Wales, from the evidence of which it was concluded that we are now justified in definitely regarding the so-called genus *Sceparnodon* as based upon the upper incisors of the gigantic extinct Wombat known as *Phascolonus*.

Geological Society, December 10.—Dr. A. Geikie, F.R.S. President, in the chair.—The following communications were read:—On some water-worn and pebble-worn stones taken from the apron of the Severn Commissioners' weir erected across the river at Holt Fleet about eight miles above Worcester, by Henry John Marten, Engineer to the Severn Commissioners. The weir referred to in the paper was built in 1844 of soft red sandstone, and some of the stones composing the apron of the weir showing signs of decay were removed in 1887. The average quantity of water passing over each square foot of the stones composing the apron has been estimated at about 2000 gallons per minute. A large proportion of the stones had been drilled through and through by the action of the current upon small pebbles lodged in hollows or between the joints of the stone; and the author estimates that, as a result of 43 years of erosion, six of the stones of the apron, which may be taken as a sample, had lost the following amounts respectively: 45, 60, 48, 50, 37, and 58 per cent. In answer to various questions, the author said he believed the action was principally abrasive, as there was only a small proportion of lime in the stone which would be the subject of chemical action. The weir was placed diagonally across the river, and the stones referred to, which were average samples, were taken from the apron at the upper end of the diagonal, where the abrasive effect appeared to be greatest. The pebbles were principally of quartzose description. The rock from which the stones were taken was of a homogeneous character. In the case of stone No. 1, had the action been uniform, the abrasion would represent a loss of nearly one foot three inches from the surface of the stone as originally placed in the apron, and the others in proportion.—On the physical geology of Tennessee and adjoining districts in the United States of America, by Prof. Edward Hull, F.R.S., late Director of the Geological Survey of Ireland. The area described in the paper is occupied by the Unaka or Blue Ridge, which may be regarded as one of the parallel ridges of the Alleghanies, and the prolongation of Prof. J. D. Dana's "Archæan Protaxis." It runs in a general south-westerly direction, and attains an elevation of 6760 feet. At its base, and to the north-west of it, is the Valley of East Tennessee, about 40 miles wide, and furrowed by north-east and south-west ridges and depressions, parallel to the strike of the Cambrian and Silurian beds. Through this runs the Tennessee River, which, instead of running south to the Gulf of Mexico, turns to the north-west, some distance below Chattanooga, and cuts through the Cumberland table-land, a prolongation of the Appalachian Mountains, and flows into the Ohio River. The Cumberland table-land has an average height of 2000 feet above the sea, and 1350 feet above the Tennessee River at Chattanooga. It consists of a synclinal of Carboniferous rocks resting conformably upon the Devonian beds, and is bounded along the East Tennessee Valley by a curved escarpment; a similar though more indented escarpment forming its north-western margin, and separating it from the Silurian plain of Nashville. The table-land is about 40 miles wide, and is intersected by the Valley of the Sequachee River, running in a north-easterly direction along a subsidiary anticline from near Jasper for a distance of 60 miles. From the base of the Cambrian beds, the whole Lower and Upper Palæozoic formations succeed each other in apparently conformable sequence, except at the junction of the Upper and Lower Silurian series, where a probable discordance occurs. The prolonged period of subsidence and deposition at length gave way to elevation; acting with the greatest effect along the Alleghanies. Under these circumstances, denudation proceeded most rapidly along the tract bordering the Protaxis, whilst the synclines were protected from erosion to a greater degree; and as the elevatory movement was more rapid along the Unaka range, the flow of the streams was generally westward. At a later period the Cumberland plateau began to be formed by backward erosion of the strata in the direction of the dip; so that it owes its development to the erosion of the Tennessee and Clinch Rivers on the one hand, and to the Cumberland River on the other. Where the Tennessee River flows in a north-westerly direction through the Cumberland plateau, the divide between it and the Gulf of Mexico is only 280 feet above the river-bed, whilst the table-land is 1400–1500 feet above. The author infers, therefore, that when the river began to erode its channel the plateau was relatively lower than the tract to the south of the present course of the stream, but that by denudation the relations have been reversed, whilst the river has never left its originally selected course. The author compares the state of things with that which must have occurred in the case of the northerly rivers running from the centre of the Wealden axis; but mentions that Prof. Safford and Mr. J. Leslie account for the Cumberland plateau by faulting, though he thinks that the well-defined escarpment along the Valley of East Tennessee seems to show that this cause is insufficient. In conclusion, he believes that the denudation was accelerated during the pluvial or "Champlain" period, and calls attention to the "Columbia formation" of the east side of the Alleghanies, and to the deposit of red loam by which the surface of the country of the valleys of the Tennessee and Sequachee is overspread, and which is probably referable to a similar stage. After the reading of the paper there was a discussion, in which Mr. Topley, Prof. Hughes, Mr. Wills, Dr. Hyland, and the President took part. The President found difficulties here, as elsewhere, in realizing the form of the ground when the rivers began to flow, and in discovering whether there were subterranean movements which affected the denudation. He felt that the explanation of the topography might not be so simple as Prof. Hull made out, and would like to have more details as to the structure of the ground. The author, in reply, concurred with the remarks of the President as to the complex character of the subject.—On certain Ornithosaurian and Dinosaurian remains, by R. Lydekker.

Royal Meteorological Society, December 17, 1890.—H. F. Blanford, F.R.S., Vice-President, in the chair.—The following papers were read:—Note on a lightning stroke presenting some features of interest, by R. H. Scott, F.R.S. On January 5, a house near Ballyglass, Co. Mayo, was struck by lightning, and some amount of damage done. A peculiar occurrence happened to a basket of eggs lying on the floor of one of the rooms. The shells were shattered, so that they fell off when the eggs were put into boiling water, but the inner membrane was not broken. The eggs tasted quite sound. The owner's account is that he boiled a few eggs from the top of the basket, the rest were "made into a mummy," "the lower ones all flattened, but not broken."—Note on the effect of lightning on a dwelling-house, by A. Brewin. This is an account of the damage done to the author's house at Twickenham on September 23.—Wind systems and trade routes between the Cape of Good Hope and Australia, by Captain M. W. C. Hepworth. The author is of opinion that the best parallel on which commanders of vessels navigating the South Indian Ocean between the Cape of Good Hope and the Australian colonies should run down the longitude is between the 41st and 42nd parallels during the winter months, and between the 45th and 46th parallels during the summer months.—Report on the phenological observations for 1890, by E. Mawley. Taking the year ending August, the weather of the autumn, winter, and spring, and of the first summer month could scarcely have been more favourable for vegetation, while that of July and August proved altogether as unpropitious.—The climate of Hong Kong, by Dr. W. Doberck. This is a discussion of the meteorological results at the Hong Kong Observatory, and at the Victoria Peak, during the five years 1884–88.

CAMBRIDGE.

Philosophical Society, November 24, 1890.—Prof. G. H. Darwin, President, in the chair.—The following communications were made:—On the beats in the vibrations of a revolving cylinder or bell, and their bearing on the theory of thin elastic shells, by Mr. G. H. Bryan. It is well known that if a vibrating rod of circular section is rotated upon its axis the plane of vibration remains fixed in space instead of turning round with the rod—an experiment frequently used to illustrate the corresponding property of polarized light. If, on the other hand, a tuning fork is rotated, beats will be heard which indicate that the planes of vibration turn with the fork. In this paper it is shown that when a bell or other body symmetrical about an axis is vibrating, and at the same time revolving about that axis, an intermediate effect will in general be observed. The nodal meridians will rotate, but with a smaller angular velocity than the body. This is in the first place proved from general considerations for the case of a rotating ring or cylinder, which, as in the investigations of Hoppe and Lord Rayleigh, is supposed inextensible to the first order of small quantities. In the mathematical investigations which follow, the purely statical effects of centrifugal force are separated more easily by supposing the ring to be also acted on by an attraction to the centre proportional to the distance. Taking the type of vibration which has $2n$ nodes, the author finds that these nodes rotate about the axis with angular velocity

$$\frac{n^2 - 1}{n^2 + 1}\,\omega,$$

where ω is the angular velocity of the ring. The number of beats heard per revolution of the ring will therefore be

$$2n\,\frac{n^2 - 1}{n^2 + 1},$$

instead of $2n$, which would be the number if the nodes were to rotate with the ring. Putting $n = 2, 3$, &c., we find the numbers of beats per revolution corresponding to the successive tones to be 2·4, 4·8, 7·059, 9·231, 11·351, &c., approximately. The results of experiment were found to agree fairly closely with theory.—On liquid jets, by Mr. H. J. Sharpe.—Notes on the application of quaternions to the discussion of Laplace's equation, by Mr. J. Brill.—On a simple model to illustrate certain facts in astronomy, with a view to navigation, by Dr. A. Sheridan Lea.—Note on a paper in the Society's Proceedings, by Mr. W. Burnside.

PHILADELPHIA.

The Franklin Institute, November 18, 1890.—Chemical Section.—Alloys of sodium and lead, by Wm. H. Greene and Wm. H. Wahl. Recently, in the course of certain investigations, in which we had occasion to make use of alloys of lead and sodium, we found that the properties of such alloys did not correspond with what we had anticipated from previous publications on the subject, and we were led to an examination of the properties of lead-sodium alloys of definite composition. These alloys may easily be made by direct combination, and the products are then sensibly constant in composition, which is not the case when they are prepared by reducing lead oxide by carbon in presence of soda, or by heating litharge with sodium tartrate, as described by Vanquelin and Serullas. The required quantity of sodium was added to lead melted in a covered crucible, and the alloy was roughly analyzed by determining lead only. Our alloys contained from 3 to 31 per cent. sodium. They are all brittle and crystalline; all decompose water, that containing the least sodium producing a hardly perceptible evolution of gas, while that containing 31 per cent. reacts with violence. The brittleness and oxidability increase with the percentage of sodium. The richest alloy is greenish in colour, and instantly blackens on exposure to air. We made special examinations of the alloys corresponding in composition to Na_2Pb_2, Na_2Pb, and Na_4Pb: the first of these contained 10 per cent. sodium, the second 19·5 per cent., rather more than would be indicated by the formula, while the last contained 31·7 per cent. The densities were determined in aniline, and found to be considerably higher than would be the densities of mixtures of the same composition. Thus the 10 per cent. alloy has a density of 6·91, the 19·5 per cent. a density of 4·61, and the 31·7 per cent. alloy a density of 3·81. The densities of corresponding mixtures would be 5·6, 3·7, and 2·7 respectively. The theoretical and calculated percentages of Na in alloys of above assumed composition compare as follows:—

	Na_2Pb_2.	Na_2Pb.	Na_4Pb.
P.C. Na by theory	10	18·18	30·8
,, found	10	19·5	31·7

PARIS.

Academy of Sciences, December 22, 1890.—M. Hermite in the chair.—On the history of the hydrostatic balance, and some other pieces of apparatus and scientific processes, by M. Berthelot.—On the ultra-violet limit of the solar spectrum, from photographs obtained by Dr. O. Simony at the summit of the Peak of Teneriffe, by M. A. Cornu. The author discusses some photographs of the solar spectrum obtained at Teneriffe, in relation to others taken at Courtenay, for the purpose of determining the influence of the atmosphere on the ultra-violet region. The following is a comparison of results:—

Photographs obtained at	Altitude. Metres.	Wave-length: of the last trace of the spectrum.	Wave-length: of the beginning of the end.
Teneriffe ...	3700	292·2	293·7
Courtenay ...	170	284·8 (U)	298·0

—Contribution to the natural history of the truffle, by M. Ad. Chatin.—On minima surfaces, by Prof. A. Cayley.—Singular case of germination of the grains of a Cactacea in their pericarp, by M. D. Clos.—Improvement of the culture of the potato, for industrial and fodder purposes, in France, by M. Aimé Girard.—Meteoric period of November 1890, by M. P. F. Denza. (See Our Astronomical Column.)—On the normals to quadrics, by M. Georges Humbert.—Electro-magnetic resolution of equations, by M. Félix Lucas.—Researches on refraction and dispersion in an isomorphous series of crystals having two axes, by M. Fr. L. Perrot. The substances investigated are the double salts formed by the combinations of sulphate of zinc with sulphates of the metals K, Rb, Cs, Tl, and the group NH_4. It is shown that, with the exception of the ammonium salt, the index of refraction increases with the molecular weight.—On a new series of ammoniacal compounds of ruthenium, derived from $RuNOCl_3$, by M. A. Joly.—On the combinations of ammonia with the chlorides and bromides of phosphorus, by M. A. Besson.—A method for obtaining pure phosphoric acid, in solution or in the vitreous state, by M. M. Nicolas.—Colour reactions of aromatic amines, by M. Ch. Lauth.—New process for the detection of fraud in olive oils, by M. R. Brullé.—Experimental researches on cow vaccine, by MM. Straus, Chambon, and Ménard.—Physiological action of morphine on the cat, by M. L. Guinard.—On the action of motor nerves during the desiccation of muscles, by M. N. Wedensky.—On the dimorphism of males of Crustacea amphipoda, by M. Jules Bonnier.—On the reproduction of autolyte, by M. A. Malaquin.—On the apidological fauna of South-West France, by M. J. Pérez.—The relations between the actual deformation of the earth's crust, and the mean densities of rocks and of the waters of seas, by M. A. Romieux. The relation discussed is that between the volume and density of the waters on the earth and that of the land surface.—On the geological history of the Sahara, by M. Georges Rolland.—On the soundings of the lake of Annecy, by MM. A. Delebecque and L. Legay.—On millerite from Morro-Velho, in the Minas Geraes province, Brazil, by Dom Pedro Augusto de Saxe-Cobourg-Gotha.—On offretite, a new mineral, by M. Ferdinand Gonnard.—On the rocks inclosed in trachyte from Menet (Cantal), and on their modifications and origin, by M. A. Lacroix.—On the distinction of two ages in the formation of dunes at Gascogne, by M. E. Durègne.—The tornado of August 18, 1890, in Brittany, by M. G. Jeannel.

CONTENTS.

THURSDAY, JANUARY 8, 1891.

THE SOUTH KENSINGTON AND PADDINGTON SUBWAY RAILWAY.

WE print elsewhere a letter from General Webber, in which he begins by stating that the land at South Kensington which is under the control of the Exhibition Commissioners, was originally, to use the words of the late Prince Consort,

"At no distant date to form the inner court of a vast quadrangle of public buildings, rendered easily accessible by the broad roads which surround them, buildings where science and art may find space for development, with the air and light which are elsewhere well nigh banished from this overgrown Metropolis."

If General Webber had been merely a company-promoter he would never have supplied us with so apt a quotation, telling so dead as it does against his underground railway; but the fact is, his appreciation for science has unconsciously led him to give the scientific rather than the Stock Exchange view of the matter. By all means, we urge, let the destiny foreshadowed by the late Prince Consort be fulfilled, especially as regards the "buildings where science may find space for development."

Again, General Webber's sincerity compels him to be quite silent about the disastrous disturbance that his railway, if ever constructed along the route now proposed, would cause to the pursuit of scientific investigation in the various present laboratories in Exhibition Road. The only remedy he can suggest is that "laboratories and observatories for original research used for the instruction of a very few experts may have to find a home elsewhere." Now, even this remedy he would never have advanced had he realized that his "very few experts" are in actual fact some hundreds of students, all of whom are now daily using the very apparatus, and making the very experiments, that his railway would render impossible. And since many of these students are being trained to become teachers, the education now given to hundreds daily in Exhibition Road is indirectly the teaching of thousands throughout Great Britain.

But we will go farther and say that, even if the interests of large numbers of present and future students were not at stake, and even if General Webber's contention were correct, that the work of only a few experts would be stopped by his railway, that alone should compel the railway to seek another route. For the work of these few experts at South Kensington, some of whom are rapidly advancing pure science, and others of whom are as rapidly advancing applied science, will produce a far wider effect on the future of this country than the utilization or non-utilization of the existing subway by the South Kensington and Paddington Railway Syndicate.

Towards the end of his letter General Webber feels bound, however reluctantly, to act the true "company-promoter," and so he refers to "the steady annual decline in the records of the numbers of visitors to the permanent institutions," as an argument to show the necessity of a new mode of locomotion. Probably this reference to "the steady annual decline" is only a quotation from the prospectus of the "South Kensington and Paddington Subway Company," and so ought to be taken with that liberal seasoning of salt appropriate to prospectuses of new companies. For, as a matter of fact, "the steady annual decline in the records of the numbers of visitors" is disposed of at once on examining the records themselves, which give the following figures:—

Number of visitors to the South Kensington Museum, the Natural History Museum, the Indian Museum, and the Museum of Scientific Apparatus.

During	1886	1,206,741
,,	1887	1,146,590
,,	1888	1,270,027
,,	1889	1,235,854
,,	1890	1,187,142

from which we see that the numbers during the last two years together were actually larger than the numbers during the first two together.

General Webber says that by his proposed railway "easy and rapid communication will also be afforded to hundreds of thousands of persons living to the north of Hyde Park to visit and use all the great institutions collected at South Kensington." Would not the communication be just as easy and just as rapid if the railway went down Queen's Gate, and connected the Gloucester Road Station on the District Railway with Paddington? For the main entrances to the Albert Hall, the Natural History Museum, and the Imperial Institute, are just as near Queen's Gate as Exhibition Road. The South Kensington Museum is no doubt on the Exhibition Road side of the space, but to balance this the Exhibition of Machinery and Inventions is on the Queen's Gate side of the area in question.

The public will be equally well served along whichever of the two alternative sides of the Albert Hall the railway goes. But, while an untimely selection of the eastern, or Exhibition Road, route would mean a disastrous stoppage of the scientific work now being carried on, the trains might run along the western, or Queen's Gate, route without causing practically any magnetic or mechanical disturbance to the delicate instruments in use at the existing laboratories.

We shall probably be answered that, unless the present subway (the length of which, as a matter of fact, is but one-tenth of the whole length of the proposed railway) be utilized, the District Railway and General Webber's syndicate will not make quite so much money out of the scheme. Possibly not; but is the London home of scientific research to be destroyed merely to enable General Webber's syndicate to pay extra dividends?

"All attempts hitherto made to complete the work" (that is, the subway) "as far as the Albert Hall have failed," says General Webber. May his present scheme, the ill-omened child of many failures, say we, walk in the footsteps of its fathers.

ARE THE EFFECTS OF USE AND DISUSE INHERITED?

Are the Effects of Use and Disuse Inherited? An Examination of the View held by Spencer and Darwin. By William Platt Ball. (London: Macmillan and Co., 1890.)

THE question which constitutes the title of this essay still continues—and is likely for some time to continue—the most important question in the field of

Darwinian thought. On the one side we have the school of Weismann, which answers the question with an unequivocal negative. On the other side we have the writings of Darwin himself, which entertain the so-called Lamarckian factors as subsidiary to natural selection, or as lending considerable aid to natural selection in carrying out the work of adaptive evolution. Again, we have the writings of Herbert Spencer, which attribute a still higher proportional value to the Lamarckian factors; while, lastly, we have the self-styled neo-Lamarckians, who regard these factors as of even more importance than natural selection. Amid so great a conflict of opinions on a matter of such extreme importance, any survey of the actual evidences in favour of the Lamarckian factors cannot fail to be opportune, even though the value of such an attempt must depend upon the care and the judgment with which it is undertaken. Now, we are glad to say that the essay before us is, in all respects, as admirable as it is opportune. Scientific in spirit, and logical in execution, it deals with its subject in a manner at once concise and exhaustive. Restricting his ground for the most part to the domain of fact, Mr. Ball has made a full inventory of the cases, or classes of cases, which have hitherto been adduced in evidence of the transmission of acquired characters, and briefly weighs the value of the evidence in each. In the result he concludes that there is no real evidence for any of the cases; and as his work is throughout performed in a thoughtful and painstaking manner, we deem it the most instructive contribution which has hitherto appeared upon the subject of which it treats. Of course the writings of Galton and Weismann present the greater merit of having been the first publicly to challenge the doctrines of Lamarck; but this they did on grounds of general reasoning, and by viewing the evidences of those doctrines, as it were, *en masse*. The merit of Mr. Ball's work, on the other hand, consists in its detailed analysis of each of the facts and arguments which have ever been brought forward in support of what he conveniently calls "use-inheritance."[1] He thus restricts himself to the one question of fact, whether or not there is any good evidence of the transmission of acquired characters, without embarking upon any general theory of heredity. And, as already remarked, he has done this purely analytical work in an exceedingly able manner. So much, indeed, is this the case, that we can find but little to say in the way of criticism; and that little must take the form of pointing out the particular cases where it seems to us that his examination is not quite so thorough as it usually is.

He begins by taking *seriatim* "Spencer's examples and arguments," and the first of these is "diminution of the jaws in civilized races." Here he shows that "cessation of the process by which natural selection favoured strong thick bones during ages of brutal violence might bring about a change in this direction;" and he points to the simultaneous thinning of the skull, &c., as virtual proof that such is the true explanation. Nevertheless, in the next section, which treats of "diminished biting muscles of lap-dogs," he invokes the principle of artificial selection to explain the facts, thus: "The conscious or unconscious selection of lap-dogs with the least tendency to bite, would easily bring about a general enfeeblement of the whole biting apparatus—weakness of the parts concerned favouring harmlessness" (pp. 12, 13). But surely, if the cessation of selection is sufficient to account for the diminution of the biting apparatus "in civilized races of mankind," it is no less capable of explaining a similar diminution in the case of "lap-dogs." If artificial selection has taken any part in the matter at all, it must have done so in the direction of "favouring harmlessness" by acting on instincts or dispositions rather than on jaws and muscles (see NATURE, vol. xxxvi. p. 405).

In opposing Spencer's argument drawn from the facts of co-adaptation, Mr. Ball appears somewhat unduly to assume the character of a special pleader. The argument is, that the more the utility of any co-ordinated set of parts depends upon their co-ordination, the more difficult does it become to see how natural selection alone could have produced the mechanism. For if the mechanism be such that its utility depends on all its parts simultaneously co-operating, no utility can arise unless all the parts are developed simultaneously in the same individuals. Now, Spencer represents that the chances must be enormously against an accidental occurrence of many concomitant changes in any single individual, where such changes are thus without benefit to the individual save when they do occur in combination; while, of course, the inherited effects of use and of disuse of all the parts concerned would explain their simultaneous evolution. Mr. Darwin answered this argument by showing how natural selection might be held to operate in the production of these effects; but in doing so he continued to attribute a probably large share of the work to the Lamarckian (*i.e.* Spencerian) factors. Now, Mr. Ball, in quoting Darwin's opinion, ignores this latter point. For instance, after giving it as Darwin's view "that natural selection alone 'would have sufficed for the production of this remarkable quadruped'" (*i.e.* the giraffe), he omits the conclusion of Darwin's sentence, viz. "but the prolonged use of all the parts together with inheritance will have aided in an important manner in their co-ordination." Again, while referring to what Darwin has said touching another of Spencer's examples—the elk—Mr. Ball omits to notice the sentence: "Although natural selection would thus tend to give to the male elk its present structure, yet it is probable that the inherited effects of use, and of the mutual action of part on part, have been equally or more important."

In bringing together all the cases adduced by Darwin to support the theory of use-inheritance, Mr. Ball begins by observing that

"he [Darwin] appears to have acquired the belief in early life, without first questioning and rigorously testing it, as he would have done had it originated with himself. In later life it appeared to assist his theory of evolution in minor points, and in particular it appeared absolutely indispensable to him as the *only* explanation of the diminution of disused parts in cases where, as in domesticated animals, economy of growth seemed to be practically powerless. He failed to adequately notice the effect of panmixia, or the withdrawal of selection, in causing or allowing degeneracy and dwindling under disuse."

[1] This term is coined by him as equivalent to Darwin's "inherited effects of use and disuse," to Spencer's "inheritance of functionally produced modifications," to Weismann's "inheritance of somatogenetic characters," &c.

These remarks are very judicious, as likewise are those where, further on, he alludes to the important distinction between selection as merely withdrawn (panmixia) and as actively "reversed" (through economy of growth, &c.). For it is shown that, if Darwin had not thus failed adequately to notice the former principle without reference to the latter, he could not have continued to regard our domesticated animals as furnishing any conclusive proof of use-inheritance; or, indeed, any better evidence than is furnished by animals in a state of nature. Having satisfied himself that there can be no economy of growth in our highly-fed domesticated animals, seeing that parts may reappear in them which are obsolete in their parent or undomesticated types, he concluded that, where other parts presented diminution, the fact could not be attributed to a reversal of selection. Therefore he attributed it to the inherited effects of disuse as to the only imaginable alternative. But, as Mr. Ball—following Weismann and others—plainly shows, the mere cessation of selection, without any reversal of selection, is in all such cases bound to produce the effects observed; with the consequence that the former principle "invalidates Darwin's strongest evidence for use-inheritance."

Passing from such general remarks on Darwin's attitude of mind with respect to the question of use-inheritance, Mr. Ball deals separately and exhaustively with all the particular instances that Darwin has given. The criticism here is uniformly good, and as regards some of the cases—*e.g.* "wings and legs of ducks and fowls"——remarkably so. But as we have no space to consider these numerous instances in detail, it must be enough to say, in general terms, that Mr. Ball has certainly reduced their evidential value to very small dimensions. Even the results of Brown-Séquard's experiments on the apparent transmission of injuries are shown to be of a more doubtful character in relation to the question of use-inheritance than anybody else has hitherto indicated.

There are, however, two or three remarks which seem worth making on Mr. Ball's treatment of some of Darwin's examples. Thus, in considering the "larger hands of labourers' infants" as compared with those of infants belonging to the upper classes, he attributes the phenomena to "sexual selection in the gentry." And in many other cases, both in man and the lower animals, he shows how the apparent effects of use-inheritance may be due to this cause. Such, of course, is a very reasonable position for Mr. Ball to adopt; but what is to be said about all such cases by the school of Wallace, which rejects the theory of sexual selection as well as that of use-inheritance? These cases, of course, are cases where the theory of natural selection cannot be applied; and therefore it would seem that the school of Wallace must either confess them inexplicable, or else devise some additional theory for the purpose of explaining them.

This allusion to the views of Mr. Wallace leads to a consideration of an important argument recently published in his "Darwinism"—namely, that, even if use-inheritance be physiologically possible, it can never be allowed to act, inasmuch as natural selection will always effect the required alterations more rapidly than they could be effected by use-inheritance. The only answer to this argument appears to be, as Mr. Ball puts it, "that slight changes in each generation need not necessarily be matters of life and death to the individual, although their cumulative development by use-inheritance might eventually become of much service." This answer he disposes of by adding: "But selection would favour spontaneous variations of a similarly serviceable character." This, however, does not appear to meet the requirements of the case. For the whole point of the answer to Mr. Wallace's argument lies in the consideration that "slight changes in each generation need not necessarily be matters of life and death to the individual," *i.e.* that the amount of change due to use-inheritance may be so small *in each successive generation* as not to make any appreciable difference in the struggle for life. Hence this amount of change need not be in any degree "serviceable," although, by an accumulation of such changes in the same line of change, a high degree of serviceability may be attained without the necessary aid of selection. Of course, this answer to Mr. Wallace's argument can only go upon the supposition on which the argument itself is founded—namely, that use-inheritance is physiologically possible; and therefore both the argument and the answer are irrelevant to the question of such possibility as raised by Weismann.

Once more in the same section of this essay—namely, that which is concerned with Darwin's examples of use-inheritance—Mr. Ball repeatedly adduces the case of neuter insects as demonstrative evidence against use-inheritance. But he does not consider the possibility of the instincts of neuters being survivals from a time when all the female insects of a hive were both fertile and industrious. He quotes, indeed, an absurd passage from Büchner upon this subject, but does not allude to what Perrier has said. We are far from maintaining that Perrier has made out his case; but we think that the question raised by him ought to have been gone into by Mr. Ball. Moreover, in the event of there being a second edition of his essay, it is to be hoped that Mr. Ball will consider the ingenious suggestion with regard to these instincts which has just been published by Prof. Lloyd Morgan in his admirable treatise on "Animal Life and Intelligence," pp. 440–42.

The concluding part of the treatise is devoted to "Miscellaneous Considerations." These all appear to us both apposite and cogent, except the sections which argue that use-inheritance, even if it were physiologically possible, would prove of more harm than good in the matter of adaptive evolution. In the first place, there was no necessity for Mr. Ball to propound such a question—his aim elsewhere being to show that, as a matter of fact, there is no evidence of use-inheritance having ever been in operation. In the next place, this foreign and superfluous argument is not well sustained; for although Mr. Ball shows that in many instances use-inheritance would be an "evil," he not only disregards the vastly greater number of cases in which it would helpfully co-operate with natural selection, but he also disregards the important consideration that in all cases the "minor factor" would require to be under control of the "major factor"—with the result that, where harmful, its effects would not be allowed to develop.

Although we have thus devoted considerable space to a review of this little book, we regret our inability to devote more. For, having restricted ourselves to points where criticism seems possible, we must have failed to give a sufficient idea of all the rest of the essay. In conclusion, then, we must add that Mr. Ball's analysis as a whole appears to us to stagger the theory of use-inheritance more seriously than ever it has been staggered before; and, therefore, that no one who henceforth writes upon the subject can afford to disregard his treatment of the question, "Are the Effects of Use and Disuse Inherited?" George J. Romanes.

TECHNICAL EDUCATION.

Manual Training in Education. By C. M. Woodward, A.B., Ph.D. (W.U.), &c. With Illustrations. (London: Walter Scott, 1890.)

THIS is in some respects a valuable work, and therefore it is the more to be regretted that its title is in a double sense misleading. A very little reflection would have taught the author that "manual," as from *manus*, the hand, means anything which hands are capable of effecting. He, however, defines "manual training" as limited to teaching and learning the use of tools and working materials; these, according to his system, being limited in turn to wood and metal work, such as turning, carpentry, and smithing. The immense range of work of which children are capable is not included, therefore, in "Manual Training." Again, we find in the book that the education in question not only does not include that of girls, but actually takes no note of young children of either sex. "Starting with boys in their teens," Prof. Woodward shows, what has always been well known, that such boys can be taught the rudiments of certain "trades." But as the very great majority of children leave school early in their teens to go into active life, what parents or the public chiefly wish to know is, what manual training can be imparted to all children while they are yet at school? Extensive experiment has perfectly shown that they can be taught to draw, model, and execute much useful art work even from six years of age, and what is of more importance is that, as one hour of sleep before midnight is worth two after it, so those who learn to draw in early childhood acquire a certain dexterity and skill such as is rarely, if ever, attained after thirteen years of age. "The proper mental maturity," says Prof. Woodward, "rarely comes before the fourteenth year. I think of the class as about fifteen years old." "The minimum rate of admission" (to my school) "is fourteen years"—meaning, we suppose, that no younger pupils are received. If this means anything, it is that, according to the author, manual training in education should not begin till "the proper mental maturity" is attained. But what we expect from education is that pupils shall be trained *before* their minds are matured.

There are many persons who will buy this work under the impression that it will teach all the details of manual training. But, in fact, of its 310 pages only 77 are devoted to practical instruction in drawing, wood and iron work. These are truly excellent of their kind, so much so that we cheerfully wish *si sic omnes*—"would that all were like it"—since in that case we should have had a work of practical use, although there is at present no lack of admirable if not better hand-books for such training in England. In the remaining part of the book Prof. Woodward advances theories which no man of culture and intelligence can admit, and which imperatively call for refutation, since at present the British public in its bewilderment as to "technical education" is being extensively deluded by them.

It has been said by some journalist that the aim of the Socialists seems to be to make of all society a well-organized poor-house. It is not in the least an exaggeration to say that Prof. Woodward's idea of education is that every male in a community shall be, first of all, a mechanic. He admits, it is true, a moderate amount of culture in literature and other branches, but exacts that *three hours a day* shall be given to drawing and manual labour, while with boys above fourteen it may be more. That is to say, three hours in school, with two hours of home study, are to be given to mathematics and book-keeping, science (*i.e.* geography, zoology, botany, chemistry, physics, physiology) and literature (which is to include "some choice specimens of modern prose and poetry"), and *one* foreign language—French, German, or Latin. But it is very evident that Prof. Woodward considers that in all cases, with every pupil, the manual labour should be the most thoroughly taught, and that carpentry and metal work are of paramount importance. The main force or tendency of all education should be to form a mechanic, and give to every youthful mind the habit of regarding all things from a mechanic's point of view.

Now, while this is a commendable education for a blacksmith, and while we may admit that it amuses and pleases youth, it is evident "on the face of it" that the literary course prescribed is utterly inadequate to *properly* prepare pupils for any career above that of the workshop. And this tone pervades the whole book: "there's nothing like leather" appears on its every page. The author quite forgets, what is also left out of sight by most of our own reformers in education, that, absolutely necessary as it may be to educate the majority to become mechanics, the world requires a very respectable number of professional, literary, and really scientific men, who could not be properly trained for such pursuits on one language, even with a knowledge of "some choice specimens of *modern* prose and poetry."

It is true that the author admits that, when "a lack of mechanical interest or power" manifests itself, the lad should unquestionably be sent to his grammar and dictionary rather than to the laboratory and drafting-room." That is to say, when the bit of wood is fit for nothing else we may make a god of it. And a master mechanic is to decide as to who shall thus take the back seats in education, and occupy the inferior positions of men of science and literature!

There is a class of boys, according to Prof. Woodward, "who are so constituted that their controlling interests are not in the study of words, the forms of speech, or the boundless mass of information which is given in books." These appear to be his favourites. "The claims of this class of boys," he asserts, "have been set forth by no one so eloquently as by General Francis A. Walker." It may interest the reader to see what is regarded as surpassing eloquence by one who prescribes the limits of all literary education.

"It not infrequently happens that the boy who is regarded as dull because he cannot master an artificial system of grammatical analysis, who isn't worth a cent for giving a list of the kings of England, who doesn't know and doesn't care what are the principal productions of Borneo—has a better pair of eyes, a better pair of hands, a better judgment, and even by the standards of the merchant, the manufacturer, and the railroad president, a better head than his master."

The reader has possibly heard such brilliant eloquence at a certain class of popular meetings, where the assertion that the ignorant and book-hating man is the cleverest in the community is always received with cheers. Prof. Woodward himself asks if there "may not be something mischievous in the power of attention to certain book-learning, as shown by the tendency of bookish people to dislike manual labour, and sometimes to become bad citizens?" It is evident enough that "bookish people" do not stand high in his graces. He assures us that his manual education is carried on in the interest of rational intellectual training and culture, but we confess that we fail to see it. These extracts indicate exactly what his standard of "culture" must be. Indeed, he declares that his system, far from being too narrow, is rather too broad.

We cordially believe that Prof. Woodward is an accomplished, skilful, very earnest and successful teacher of wood and metal work to boys over fourteen or fifteen years of age, but we have rarely met with a writer who needed more the caution not to go beyond his last. His work is to all intents and purposes inspired with the belief that all schools should be like his own, and all education for all classes be based on the very limited mechanical training with which he is familiar. That it is admirable to a certain degree, but most inadequate to all the demands of a really good education, is apparent on every page. As he handles all his adversaries without gloves, sparing no one who does not believe his method of education to be perfection, we have the less scruple in setting forth the truth regarding it in plain words. Singular as it may seem, not only to him, but to a great number of reformers, the education of the future will require a much higher stimulant or a far better basis than mere mechanical drawing, hammering, and filing.

OUR BOOK SHELF.

Monographie der baltischen Bernsteinbäume: Vergleichende Untersuchungen über die Vegetationsorgane und Blüthen, sowie über das Harz und die Krankheiten der baltischen Bernsteinbäume. Von H. Conwentz. Pp. 151, mit achtzehn lithographirten Tafeln. (Danzig. London: Williams and Norgate, 1890.)

DR. CONWENTZ was long associated with Goeppert and Menge in the investigation of the "Amber Flora," and indeed wrote the whole, or nearly the whole, of the second volume of the "Flora des Bernsteins," comprising the angiospermous fossils found in Baltic amber. Engaged upon a third volume of this work, devoted to cryptogamous or spore-bearing plants, Dr. Conwentz became impressed with the necessity of first working out the relationships of the trees which yielded the resin now found in a fossil state and known as amber, and those who know the admirably executed plates and exhaustive text of the two volumes issued of "Die Flora des Bernsteins" will not be disappointed with this companion volume.

We cannot attempt a critical review of this work, and must therefore be content with giving the result of Dr Conwentz's investigations, as set forth by himself. During the Tertiary period the division or distribution of land and water, especially in Europe, was very different from what it is at the present time; and in Eocene times, the beginning of this period, the Scandinavian continent extended southward nearly to the Samland district of northern West Prussia and Mecklenburg, and supported a vegetation whose principal types now characterize the flora of the southern parts of the temperate zone and the northern subtropical zone. Evergreen oaks and beeches flourished, associated with palms, laurels, magnolias, and camellias; also the amber trees and various kinds of cypress.

Chief among these amber trees were four species of *Pinus*, not one of which was very closely related to *Pinus sylvestris*, the characteristic fir or pine-tree of the region of the present period. One of the species, with leaves or needles in pairs, recalls some of the North American species belonging to the section *Parrya;* a second resembles the Japanese *Pinus Thunbergii;* and a third, having the leaves in fascicles of five, is near *P. Cembra* and the Japanese *P. parviflora.* In addition there was a kind of spruce more nearly allied to *Picea ajanensis*, of the extreme east of Asia, than to the silver spruce of Germany.

It is probable, Dr. Conwentz believes, that all these different trees and shrubs did not form a mixed forest, but were peculiar to separate regions, and then the amber trees formed dense forests interspersed here and there only with other trees. Dr. Conwentz pictures the conditions under which he imagines these amber trees existed and discharged the resin now so universally employed. The trees he assumes were exposed to the ravages of almost numberless enemies, animal and vegetable, insomuch that there was scarcely a sound tree in the forests. The trees thus enfeebled would be swept down or mutilated by violent storms, causing an abnormal exudation of resin. It seems, however, hardly necessary to insist on the general unhealthiness of the trees to account for the large deposits of resin. Clouded amber is said to be due to the presence of cell-sap in the resin. The majority of the plates illustrate the destruction of the tissues by various fungus parasites. W. B. H.

The Birth and Growth of Worlds. By A. H. Green, M.A., F.R.S. (London: Society for Promoting Christian Knowledge, 1890.)

THIS little work is the extension of a lecture delivered by Prof. Green. It contains an account of various cosmical theories from 1684, when Thomas Burnet, D.D., a learned divine, expounded his "Sacred Theory of the Earth," to recent times. The researches of Prof. Lockyer on the constitution of the heavenly bodies are well described; as are also the hypotheses of Kant and Laplace. A list is given of the chief works bearing on the subject of the lecture. This, in conjunction with the descriptive text, and a few well-chosen illustrations, renders the book extremely useful as a popular short exponent of the many attempts that have been made to fathom the origin of celestial species.

Chambers's Encyclopædia. New Edition. Vol. VI. (London and Edinburgh: W. and R. Chambers, Ltd., 1890.)

THE present volume of the new edition of Chambers's well-known "Encyclopædia" takes in words ranging from "Humber" to "Malta." In every respect it is up to the level of the preceding volumes; and, as usual, scientific subjects have been entrusted to thoroughly competent writers. Under "Hydrophobia" M. Pasteur sketches his discoveries and practices in regard to rabies, while Mr. J. Arthur Thomson contributes "a brief unargumentative review of current adverse criticism." There is an excellent article on insanity by Dr. T. S. Clouston. Prof. James Geikie writes on igneous rocks and other subjects; Prof. C. G. Knott on hydrodynamics and terrestrial magnetism; Dr. J. Anderson on lake dwellings; Dr. Alfred Daniel on light, lenses, and magnetism; Mr. R. T. Omond on lightning; and Mr. F. E. Beddard on the lion and the leopard. Iceland is described by M. Hjaltalín; India by Sir Richard Temple; the Indian Ocean by Dr. John Murray; the geography of Italy by Mr. W. D. Walker; and Madagascar by the Rev. J. Sibree.

LETTERS TO THE EDITOR.

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

Shaking the Foundations of Science.

MY attention has been drawn to the article in NATURE under the above title, and, as it presents to your readers the proposed South Kensington and Paddington Subway in an aspect which the writer erroneously describes as "the hosts of Mammon threatening the domains of science," I feel sanguine that a scientific journal such as yours will be even more than ordinarily desirous to have the correct view set before them.

The writer appears to ignore the fact that the rectangular area bounded by Kensington, Cromwell, Exhibition, and Prince Albert (now Queen's Gate) Roads forms part of the estate purchased out of the surplus funds of the Great Exhibition of 1851, and vested in a body of Commissioners by Royal Charter; and that this land was originally destined, to use the words of the late Prince Consort, "At no distant day to form the inner court of a vast quadrangle of public buildings, rendered easily accessible by the broad roads which surround them, buildings where science and art may find space for development, with the air and light which are elsewhere well nigh banished from this overgrown Metropolis."

The site of the Exhibition of 1862, and of the buildings of the South Kensington and Science and Art Departments, originally formed part of that estate.

When the Royal Albert Hall was first projected, a pneumatic railway from it to the South Kensington Station *viâ* Exhibition Road formed part of the original scheme, which was promoted by the Commissioners who own the estate, and the paramount necessity of such a means of communication has never been lost sight of.

When the existing subway was constructed in 1885, its continuance as far as the Albert Hall was a recognized part of the project, and the route alongside of the Eastern Arcades was then decided upon by the Commissioners. When the District Railway Company obtained their Act of Parliament for the subway, its use with traction was sanctioned.

The proposal now before Parliament is for absolutely no more and no less, so far as the estate of the Commissioners of 1851 is concerned, than the completion of that subway, with nearly its present form and dimensions, and with almost the same powers to use it in the same way.

All attempts hitherto made to complete the work as far as the Albert Hall have failed, and the existing incomplete portion of the subway is practically closed, and is a dead loss financially.

At last a practical solution of the difficulty has been found, with the additional advantage that, besides the means of covered access being afforded from South Kensington Station through the length of the estate, easy and rapid communication will also be afforded to hundreds of thousands of persons living to the north of Hyde Park to visit and use all the great institutions collected at South Kensington, to which covered access will thus further be secured from all parts of the metropolis.

In his eagerness to prove that the present means of access are all that can be desired, your contributor refers to the congregations of visitors attracted by former Exhibitions. The steady annual decline in the records of the numbers of visitors to the permanent institutions is not the only evidence which entirely refutes such a misleading line of argument.

I do not dispute for one moment that Urania must have her quiet retreats secluded from the movements and throng of men and vehicles, and it is hardly surprising that, as your contributor states, the repose she loves cannot even now be found at South Kensington.

But it is pure affectation to contend that the interests of the highest scientific education for the many will suffer, because laboratories and observatories for original research used for the instruction of a very few experts may have to find a home elsewhere.

If the vibration caused by omnibuses by day and market carts by night, passing in the Kensington and Cromwell Roads at each end of Exhibition Road, to say nothing of "small and earlies," are found already to affect the ultra-sensitive nerves of Urania, the question may be asked why the Professors continue to countenance the expenditure of the "vast sums" mentioned by your contributor, and also, if it is right that any more public money should be allowed to be spent in a way which experience has already shown to be injudicious.

It appears, therefore, evident that the degree to which the traffic in the proposed subway might augment this inconvenience does not affect the true merits of the case as set up by your contributor. C. E. WEBBER.

17 Egerton Gardens.

The Darkness of London Air.

THE interesting article on the above subject which appeared in your issue of December 18 (p. 152) is one of those periodical reminders that Englishmen—as if not content with the innumerable climatic ills to which they are unhappily heirs—are yearly endeavouring their "level best" to make city life more and more polluted and noxious. It is apparently in vain that they are constantly shown how injurious to human comfort in every way, how fatal to plant life, and how destructive to architecture, is this accumulation of unburnt fuel in the air; neither, strange to say, are they more heedful of the enormous annual waste, sheer waste, of fuel: to all arguments and expostulations they oppose stolid apathy. Seeing how hopeless it is to induce any active interest in this question among the inert mass of citizens, your contributor very sensibly suggests an appeal to the County Council to take up the matter. My object in writing is to offer a further suggestion on the *modus operandi*. Why not invite the Councils of all our learned and scientific Societies and institutions to combine in a memorial to either the County Council or Parliament direct, asking for legislation on this smoke question, and for simple machinery to enforce such legislation? I cannot but think that such a memorial, signed by, *e.g.*, the Royal Society, the Chemical, Linnean, Astronomical, and the various other Societies, by the College of Surgeons, the Royal Academy, and in fact by the representatives of all such corporations would carry very great weight. F. H. P. C.

A Remarkable Flight of Birds.

I HAVE not noticed any reference to the extraordinary flight of birds that was observed in many parts of Devon on the morning of December 21, after the first heavy fall of snow took place at the beginning of the present severe weather. At eight o'clock on Sunday morning I was astonished at a continuous stream of skylarks flying overhead in a westerly direction. The flight continued for more than an hour after that in the most astonishing numbers. Over 500 were counted in three minutes, and the cloud of birds seemed endless in every direction. An old farmer here said that he had seen a similar thing about ten years ago. The birds then were found on the estuaries, and by the sea-coast of Cornwall, where they died by thousands. Several letters have appeared in the local papers announcing a similar migration on the same morning, so that there must have been millions of birds on the wing. One correspondent mentions other birds, thrushes and blackbirds, &c., as well, but here I saw only skylarks. I have seen no record of their destination. It would be interesting to know if any of your readers could tell us where the birds went. They were all flying towards Cornwall. I observed also large detached flocks of plover, flying towards Dartmoor, on the edge of which I live, in a southerly direction. The appearance of these birds all hasting away in perfect silence was almost weird in the dead stillness, all the ground and every twig and bush being covered with deep snow, and not a breath of wind stirring.

The event has certainly justified their instincts, for until to-day, January 1, it has been almost impossible for the birds to obtain any food, except from the berries, which this year are exceptionally plentiful.

Large flocks of fieldfares have taken possession of my garden, where there are a great many hollies, and at any noise they rush out of the bushes like a swarm of flies. It is curious to watch them from the windows in the morning, some ten or a dozen sitting in the snow under the bushes, mere dejected heaps of feathers, occasionally pecking at the berries that their busy comrades have knocked off. The thrushes are in the wildest excitement. They sit above the hollies, quivering and chattering, and occasionally darting upon a luckless fieldfare, whose unwonted presence they resent most strongly. I do not know how these birds discover the berries. It cannot be by their colour, for there are two large hollies within ten yards of each other; one of them was for days full of birds constantly flying past the other, which was almost a mass of brilliant red berries. One tree was almost stripped bare, and the birds all went to an adjoining field for three days. Then one morning I found them in the remaining bush, which they speedily stripped as bare as the rest.

E. C. SPICER.

Throwleigh Rectory, Devon, January 1.

On the Flight of Oceanic Birds.

THE oceanic soaring birds undoubtedly take advantage of the air-currents as sailing-vessels do, trimming their wings so as to be acted on by the wind to the best advantage. I have frequently observed their flight in high southern latitudes, and have seen the sooty albatross sail round, and up and down, for ten minutes and more with never a flap of the wings; but with a pair of binoculars the tail, head, and portions of the wings could be seen to move slightly with each change of direction or elevation.

There are two well-marked ways of flying, as follows: (*a*) when the bird is flying with the wind blowing sideways on to it, as represented in Fig. 1, where it will be seen the whole of the

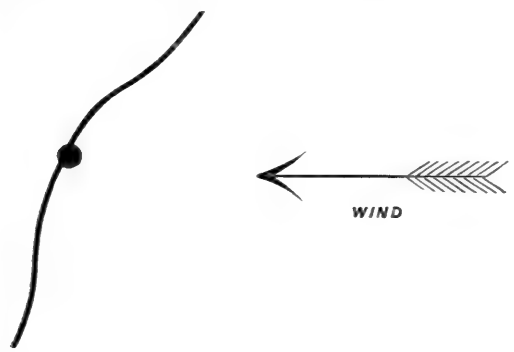


FIG. 1.—"On a wind."

under surface is exposed to the wind; and (*b*) when the bird is flying either directly towards or from the direction of the wind, as in Fig. 2, in this position a slight movement of the tail sending the bird up or down, and of the wings (not a flap) altering the direction.

Roughly speaking, the area of wing surface exposed to the wind is about 3 square feet, and the weight of the bird 7 pounds (sooty albatross). In all these true oceanic birds the wings are long and narrow, and the birds appear to have great power over the movements of the different joints.

FIG. 2.—Before or against the wind.

In calm weather the birds constantly settle on the water, and when flying flap their wings a good deal. As the wind makes and increases, the flaps become less in number, until the birds sweep round and under the stern of the ship in immense circles at the rate of 20 to 30 miles per hour, in most cases as represented in Fig. 3, at the part marked (*a*) close to the sea surface, and at (*b*) high in the air.

It is a common thing in high winds to see them apparently motionless for some time over the mast-heads, the ship at the same time going through the water at a speed of 10 or 11 knots per hour.

I would not pretend in any way to explain their marvellous flight, but one thing should be remembered, that the velocity of the air certainly increases from the surface of the sea to the altitude to which they attain, viz. about 200 feet; and that

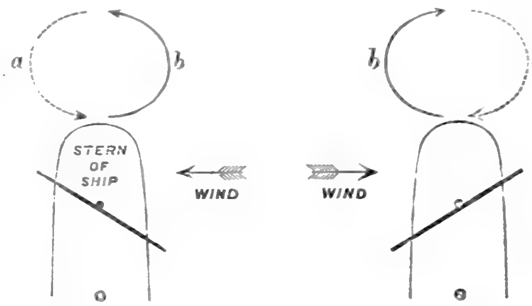


FIG. 3.

when the wind is strong the sea is thrown into considerable waves, which may affect the horizontal movement of the wind in immediate contact with them. There certainly seems to be some connection between the way the wings are spread to the wind's direction, the sliding up and down in their sweeping circle (Fig. 3), and their method of flight.

DAVID WILSON-BARKER.

The Locomotion of Arthropods.

I HAVE been making some observations on the locomotion of various insects, and find that in the case of those which move quickly the best method for observation is instantaneous photography. Instantaneous photographs of moving flies show that they move the front and hind leg of one side almost simultaneously with the middle leg of the other, while they stand on the other three. When the tripod which is moving has come to the ground, the other tripod is raised, and so on. The photographs show, however, that while no leg of one tripod ever moves simultaneously with any leg of the other, yet there is a succession in the movements of the legs of each tripod. The hind leg on one side is first moved, then the middle on the other, and when the hind leg has been moved forward and almost reached the ground, the front leg of that side is raised. The middle leg and the front leg of the opposite sides come to the ground almost simultaneously. It is usually just when the hind leg is reaching the ground, and the front leg is being raised, that the tripod on which the fly is resting thrusts the body forward. After the movement of each tripod there appears to be a short pause, during which all six legs are on the ground together.

I have observed this "tripodic" walk in earwigs, water scorpions, aphides, and some beetles. In the case of some slowly moving beetles and aphides which can be observed without photographic means, quite irregular movements have been observed. By cooling aphides, they can be made to move very slowly. In this condition one was observed to move its legs in slow succession in the following order: (1) right hind, (2) right middle, (3) right front, (4) left hind, (5) left middle, (6) left front. This walk was continued for some time, occasionally interrupted by the following order, or some other quite irregular walk: (1) right hind, (2) right middle, (3) left hind, (4) left middle, (5) left front, (6) right front.

In caterpillars the legs forming a pair seem to move simultaneously; the motion begins at the posterior end of the body, and proceeds regularly forward till the most anterior pair of legs are moved.

The above few observations, which were made in the Physical Laboratory of Trinity College last spring, formed the subject of a recent communication to the Dublin University Experimental Association. I hope to be able to extend this application of photography to the other groups of Arthropoda.

H. H. DIXON.

Physical Laboratory, Trinity College, Dublin.

P.S.—In taking the photographs a small camera with magnifying lens and fast shutter were used, and the analyses of the motions arrived at by the comparison of a large number of "snap" pictures. The insect was in each case placed on a white ground within a shallow box covered with glass and illuminated by oblique sunlight. In this way the disposition of the shadows became of great service in determining the positions of the legs.

Attractive Characters in Fungi.

No doubt the characters of the fungi mentioned by Mr. C. R. Straton on p. 9 are attractive, but it does not follow that they are useful. Mr. Straton's letter contains many incorrect statements: the importance of colours and odours to flowering plants cannot be correctly said to be "perfectly understood"; fungi are not "insusceptible of fertilization"; the common mushroom has no "parasitic period," and the first stage of its existence is not "passed in the body of an animal host."

The common mushroom never grows on the droppings of the horse, or upon the droppings of any other animal, and it cannot even be made to grow artificially upon fresh dung. An admixture of earth with decayed dung is required before mushrooms will grow. The mushroom is not a dung-borne fungus at all. It grows in pastures in common with a vast number of other pasture-fungi, and if we allow that the spores of the mushroom must needs first germinate in an animal stomach, it would be but reasonable to assume that the spores of other pasture-fungi would have a similar habit. The mycelium so commonly seen on horse dung is not the mycelium of the mushroom; it belongs to various dung-borne *Coprini*, and it is certainly not necessary for the spores of these *Coprini* to pass through a horse's intestinal canal, as they will readily germinate, not only on and in dung and its juices, but on wet blotting-paper, and on almost any other non-corrosive damp and warm material.

Mr. Straton seems to have a remarkable idea of the nature of a "parasite"; because an animal swallows spores, therefore the spores on being swallowed become "parasites." He might as reasonably describe the seeds eaten by graminivorous animals as "parasites," or the seeds of the strawberry and raspberry, when eaten with the pulp by man, as "parasites" in the human system.

As Dr. Cooke, in his interesting letter on p. 57, has not mentioned one special character of certain fungi which has greatly interested me, I may venture to advert to it here. I refer to *gluten*. Many fungi, especially Agarics, are highly glutinous, and if this gluten is not "attractive," it is, I think, useful.

Agaricus radicatus is one of our commonest stump-fungi; it has, when young, a *perfectly dry* pileus. As the fungus reaches maturity, the pileus becomes *glutinous*. The change reminds one of the change in the condition of the stigma in flowering plants. If sections are made through the pileus of *A. radicatus* in different stages of growth, it will be found that as maturity is gradually reached the centre of the pileus softens and swells, and through the now soft mass an enormous number of cystidia protrude themselves. The cystidia exactly resemble the cystidia of the gills. The cystidia open at the apex, and gluten exudes through the open mouths on to the cap of the fungus. If the sticky material from a mature example is microscopically examined, it will be found more or less full of germinating spores, which have been wafted on to the sticky pileus by the wind. The gluten possibly aids germination, if it does not—as I believe it does—cause fertilization.

Agaricus mucidus has a highly glutinous pileus, and it grows in such a fasciculose and closely imbricated manner on beech trunks, that all the spores from the upper examples fall of necessity on to the thick gluten of the examples immediately underneath. If the gluten is microscopically examined, it will be found full of the germinating spores of *A. mucidus*. The uses of the gluten here seem obvious. The simple spores of an Agaric might have but a poor chance of effectually germinating and growing upon a beech trunk, but if the spores had previously germinated and formed a mycelium in a highly glutinous material, they would have a fairly good chance. The sticky pileus would at length be blown from the beech trunk by the wind, and its mucidous character would cause it to stick to the first trunk or branch it might be blown against, and so the gluten and its living mycelium would become attached to a fresh host. *A. mucidus* is peculiar to beech woods.

I could extend these notes to other gluten-bearing fungi, as *A. adiposus*, &c., as well as to Agarics with glutinous stems; but the two examples above given may serve as a hint to other observers.

How have truffles acquired their subterranean habit? They are always eagerly eaten by pigs, squirrels, rats, and other animals when they grow near the surface, and the deeper placed examples alone survive. Would the constant destruction of tubers near the surface at length bring about a subterranean habit? WORTHINGTON G. SMITH.

Dunstable.

UNDOUBTEDLY there are numerous glutinous fungi, and the coating of gluten has a useful purpose. Whether Mr. Worthington Smith has quite apprehended the nature of that purpose may be an open question. The majority of the species in the genus *Hygrophorus* are glutinous, and this genus, as a whole, is about the latest in its time of appearance in the autumn. It is very suggestive to observe them apparently unharmed by frost, whilst the Agarics have collapsed, and are in rapid decay. That the glutinous coating is in this instance a protection from frost can scarcely be denied. Dr. Quelet, the French mycologist, has stated that in the Vosges some species of *Hygrophorus* do not appear until the early frosts have commenced, and he has borne testimony to the fact that they flourish in frosty weather without apparent injury. Both the species of Agaric to which Mr. Smith alludes, *Agaricus mucidus* and *Agaricus radicatus*, may be found late in the season, apparently indifferent to the frost, which affords a suspicion that the glutinous coating is a protection from frost. *Agaricus carbonarius* is viscid, but much more so as cold increases, and for two consecutive years we have watched it growing uninjured far into January, when no other Agaric could be seen. We do not contend that the "useful purpose" is in *all* cases a protection from frost, because in some early species we imagine it serves primarily as a protection against evaporation. Presuming that Agarics may contain more than 80 per cent. of water, such a protection would be of service to species growing in exposed situations. If Mr. Smith's suggestion as to *Agaricus mucidus* is accepted, it can only apply to that species, and *Agaricus adiposus*, and one or two others, as the majority of tufted species growing on trunks are not glutinous. Another explanation must be found for the whole *Myxacium* section of *Cortinarius*, for *Gomphidius*, for many of the *Boleti*, and for such Agarics as *Agaricus æruginosus*, *Agaricus semiglobatus*, *Agaricus lentus*, *Agaricus glutinosus*, *Agaricus roridus*, and many others. The idea that gluten "causes fertilization" is a novel one, and may be held as a private opinion, but cannot be accepted generally without evidence much stronger than individual belief; hence, although put forward by so experienced a mycologist as Mr. Worthington Smith, it must *not* be accepted as an admitted fact, but only as an individual opinion. M. C. COOKE.

THE RESEARCHES OF DR. R. KŒNIG ON THE PHYSICAL BASIS OF MUSICAL SOUNDS.[1]

II.

SO far we have been dealing with primary beats and beat-tones; but there are also secondary beats and secondary beat-tones, which are produced by the interference of primary beat-tones. An example of a secondary beat is afforded by the following experiment. Recurring to the preceding table of experiments, it may be observed that when the two shrill notes, ut_6, sol_6, giving the interval of the fifth, are sounded together, the inferior and superior beat-tones are both present, and of the same pitch. If, now, one of the two forks is lightly loaded with pellets of wax to put it out of adjustment, we shall get beats, not between the primary tones, but between the beat-tones. Suppose we add enough wax to reduce the vibration of sol_6 from 3072 to 3070. Then the positive remainder is 1022, and the negative remainder is 1026; the former being ut_5 flattened two vibrations, the latter the same note sharpened to an equal amount. As a result there will be heard four

[1] By Prof. Silvanus P. Thompson. (Communicated by the author, having been read to the Physical Society of London, May 16, 1890.) Continued from p. 203.

beats per second—secondary beats. Similarly, the intervals 2 : 5, 2 : 7, if slightly mistuned, will, like the fifth, yield secondary beats. Or, to put it in another way, there may be secondary beats from the (mistuned) beat-tones that are related (as in our experiment) in the ratio 1 : 1, or in the ratios 3 : 4, 3 : 5, &c., and even by those of 1 : 2, 4 : 5, 4 : 7, and so forth.

I have given you an example of secondary beats: now for an example of a secondary beat-tone. This is afforded by one of the previous experiments, in which were sounded ut_6 and the 11th harmonic of ut_3. In this experiment, as in that which followed with the 13th harmonic, two (primary) beat-tones were produced, of 768 and 1280 vibrations respectively. These are related to one another by the interval 3 : 5. If we treat these as tones that can themselves interfere, they will give us for their positive remainder the number 256, which is the frequency of ut_4. As a matter of fact, if you listen carefully, you may, now that your attention has been drawn to it, hear that note, in addition to the two primary tones and the two beat-tones to which you listened previously.

In von Helmholtz's "Tonempfindungen," he expresses the opinion that the distinctness with which beats are heard depends upon the narrowness of the interval between the primary tones, saying that they must be nearer together than a minor third. But, as we have seen, using bass sounds of a sufficient degree of intensity and purity, as is the case with those of the massive forks, beats can be heard with every interval from the mistuned unison up to the mistuned octave. Even the interval of the fifth, ut_1 to sol_1, gave strongly-marked beats of 32 per second. When this number is attained or exceeded, the ear usually begins to receive also the effect of a very low continuous tone, the beats and the beat-tone being simultaneously perceptible up to about 60 or 70 beats, or as a roughness up to 128 per second. If, using forks of higher pitches but of narrower interval, one produces the same number of beats, the beat-tone is usually more distinct. Doubtless this arises from the greater true intensity of the sounds of higher pitch. With the object of pursuing this matter still more closely, Dr. Kœnig constructed a series of 12 forks of extremely high pitch, all within the range of half a tone, the lowest giving si_6 and the highest ut_7. The frequencies, and the beats and beat-tones given by seven of them, are recorded in Table III.

TABLE III.

Frequencies of Forks.	Ratio.	Beats (Calcd.)	Resulting Sound.
ut_7 4096 and si_6 3840 ...	16 : 15	256	ut_3
,, 3968 ...	32 : 31	128	ut_2
,, 4032 ...	64 : 63	64	ut_1
,, 4048 ...	256 : 253	48	sol_{-1}
,, 4056 ...	512 : 507	40	mi_{-1}
,, 4064 ...	128 : 127	32	ut_{-1}
,, 4070 ...	158 : 157	26	—

The first of these intervals is a diatonic semitone; the second of them is a quarter-tone; the third is an eighth of a tone; nevertheless, a sensitive ear will readily detect a difference of pitch between the two separate sounds. The last of the intervals is about half a comma.

These forks are excited by striking them with a steel hammer. Some of the resulting beat-tones will be heard all over the theatre; but, in the case of the very low tones of 40 and 32 vibrations, only those who are close at hand will hear them. The case in which there are 26 beats is curious. Most hearers are doubtful whether they perceive a tone or not. There is a curious *fluttering* effect, as though a tone were there, but not continuously.

We have seen, then, that the beat-tones correspond in pitch to the number of the beats; that they can themselves interfere, and give secondary beats; and that the same number of beats will always give the same beat-tone irrespectively of the interval between the two primary tones. What better proofs could one desire to support the view that the beat-tones are caused, as Dr. Young supposed, by the same cause as the beats, and not, as von Helmholtz maintains, by some other cause? Yet there are some further points in evidence which are of significance, and lend additional weight to the proofs already adduced.

Beats behave like primary impulses in the following respect, that when they come with a frequency between 32 and 128 per second, they may be heard, according to circumstances, either discontinuously or blending into a continuous sensation.

It has been objected that, whereas beats imply interference between two separate modes of vibration arising in two separate organs, combination-tones, whether summational, or differential, or any other, must take their origin from some one organ or portion of vibratile matter vibrating in a single but more complex mode. To this objection an experimental answer has been returned by Dr. Kœnig in the following way. He takes a prismatic bar of steel, about 9 inches in length, and files it to a rectangular section, so as to give, when it is struck at the middle of a face to evoke transversal vibrations, a sound of some well-defined pitch. By carefully adjusting the sides of the rectangular section in proper proportions, the same steel bar can be made to give two different notes when struck in the two directions respectively parallel to the long and short sides of the rectangle. A set of such tuned steel bars are here before you. Taking one tuned to the note $ut_6 = 2048$, with $re_6 = 2304$, Dr. Kœnig will give you the notes separately by striking the bar with a small steel hammer when it is lying on two little bridges of wood, first on one face, then on the other face. If, now, he strikes it on the corner, so as to evoke both notes at once, you immediately hear the strong boom of $ut_3 = 256$, the inferior beat-tone. If Dr. Kœnig takes a second bar tuned to ut_6, and $si_6 = 3840$, you hear also ut_3, this time the superior beat-tone. If he takes a bar tuned to ut_6 and the 11th harmonic of ut_3 (in the ratio 8 : 11), you hear the two beat-tones sol_4 and mi_5 (in ratios of 3 and 5 respectively) precisely as you did when two separate forks were used instead of one tuned bar.

Dr. Kœnig goes beyond the mere statement that beats blend to a tone, and lays down the wider proposition that any series of maxima and minima of sounds of any pitch, if isochronous and similar, will always produce a tone the pitch of which corresponds simply to the frequency of such maxima and minima. A series of beats may be regarded as such maxima and minima of sound; but there are other ways of producing the effect than by beats. Dr. Kœnig will now illustrate some of these to you.

If a shrill note, produced by a small organ-pipe or reed, be conveyed along a tube, the end of which terminates behind a rotating disk pierced with large, equidistant apertures, the sound will be periodically stopped and transmitted, giving rise, if the intermittences are slow enough, to effects closely resembling beats, but which, if the rotation is sufficiently rapid, blend to a tone of definite pitch. Dr. Kœnig uses a large zinc disk with 16 holes, each about 1 inch in diameter. In one set of experiments this disk was driven at 8 revolutions per second, giving rise to 128 intermittences. The forks used were of all different pitches from $ut_3 = 256$ to $ut_7 = 4096$. In all cases there was heard the low note ut_2 corresponding to 128 vibrations per second. In another series of experiments, using forks ut_2 and ut_3, the number of intermittences was varied from 128 to 256 by increasing the speed, when the low note rose also from ut_2 to ut_3.

From these experiments it is but a step to the next, in which the intensity of a tone is caused to vary in a periodic manner. For this purpose Dr. Kœnig has constructed a siren-disk (Fig. 1), pierced with holes arranged at equal distances around seven concentric circles; but the sizes of the holes are made to vary periodically from small to large. In each circle are 192 equidistant holes, and the number of maxima in the respective circles was 12, 16, 24, 32, 48, 64, and 96. On rotating this disk, and blowing from behind through a small tube opposite the outermost circle, there are heard, if the rotation is slow, a note corresponding to the number of holes passing per second and a beat corresponding to the number of maxima per second. With more rapid rotation two notes are heard—a shrill one, and another 4 octaves lower in pitch, the latter being the beat-tone. On moving the pipe so that wind is blown successively through each ring of apertures, there is heard a shrill note, which is the same in each case, and a second note (corresponding to the successive beat-tones) which rises by intervals of fourths and fifths from circle to circle.

These attempts to produce artificially the mechanism

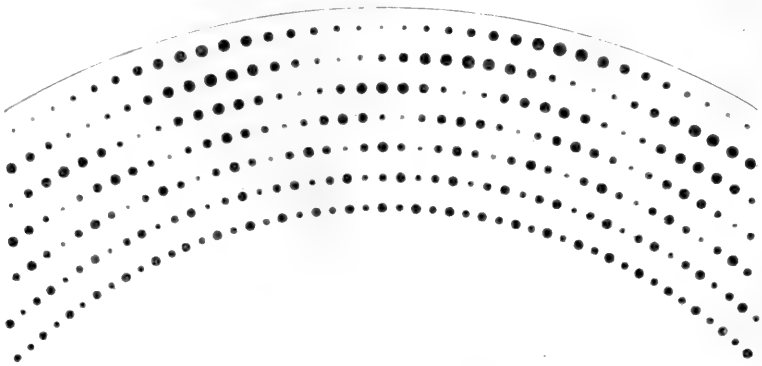


FIG. 1.

of beats were, however, open to criticism; for in them the phase of the individual vibrations during one maximum is the same as that of the individual vibrations in the next succeeding maximum; whereas in the actual beats produced by the interference of two tones the phases of the individual vibrations in two successive maxima differ by half a vibration; as may be seen by simple inspection of the curves corresponding to a series of beats. When this difference was pointed out to Dr. Kœnig, he constructed a new siren-disk (Fig. 2), having a similar series of holes of varying size, but spaced out so as to correspond to a difference of half a wave between the sets. With this disk, beats are distinctly produced with slow rotation, and a beat-tone when the rotation is more rapid.

Finding this result from the spacing out of apertures to correspond in position and magnitude to the individual wavelets of a complex train of waves, it occurred to Dr. Kœnig that the phenomena of beats and of beat-tones might be still more fully reproduced if the edge of the disk were cut away into a wave-form corresponding precisely to the case of the resultant wave produced by

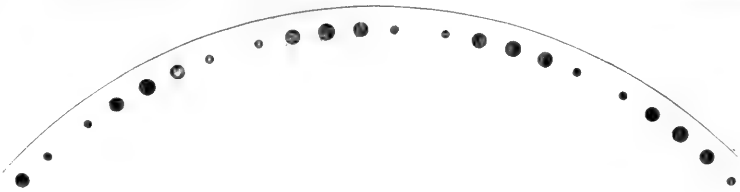


FIG. 2.

the composition of two interfering waves. Accordingly, he calculated the wave-forms for the cases of several intervals, and, having set out these curves around the periphery of a brass plate, cut away the edge of the plate to the form of the desired wave. Two such wave-disks, looking rather like circular saws with irregular teeth, are depicted in Figs. 3 and 4. These correspond to the respective intervals 8 : 15 and 8 : 23. A number of such

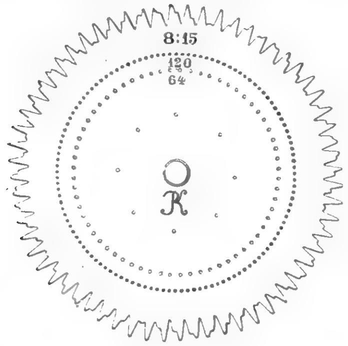


FIG. 3.

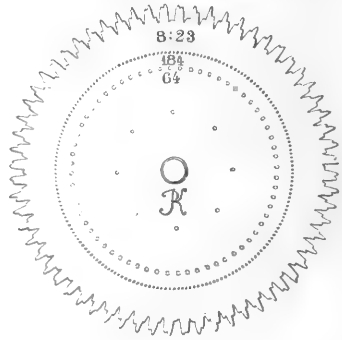


FIG. 4.

wave-disks corresponding to other intervals lie upon the table; these two will, however, suffice. In the first of these the curve is that which would be obtained by setting out around the periphery a series of 120 simple sinusoidal waves, and a second set of 64 waves, and then compounding them into one resultant wave. In order to permit of a comparison being made with the simple component sounds, two concentric rings of holes have been also pierced with 120 and 64 holes respectively. Regarding these two numbers as the frequency of two primary tones, there ought to result beats of frequency 8 (being the negative remainder corresponding to the superior beat). An interior set of 8 holes is also pierced, to enable a comparison to be made. To experiment with such wave-disks they are mounted upon a smoothly running whirling-table, and wind from a suitable wind-chest is blown against the waved edge from behind, through a narrow slit set radially. In this way the air-pressures in front of the wave-edge are varied by the rush of air between the teeth. It is a question not yet decided how far these pressures correspond to the values of the ordinates of the curves. This question, which involves the validity of the entire principle of the wave-siren cannot here be considered in detail. Suffice it to say that for present purposes the results are amply convincing.

The wave-disk (Fig. 3) has been clamped upon the whirling-table, which an assistant sets into rotation at a moderate speed. Dr. Kœnig blows first through a small pipe through one of the rows of holes, then through the other. The two low notes sound out separately, just a major tone apart. Then he blows through the pipe with a slotted mouth-piece against the waved edge; at once you hear the two low notes interfering, and making beats. On increasing the speed of rotation the two notes become shrill, and the beats blend into a beat-tone. Notice the pitch of that beat-tone: it is precisely the same as that which he now produces by blowing through the small pipe against the ring of 8 holes. With the other wave-disk, having 184 and 64 holes in the two primary circles, giving a wave form corresponding to the interval 8 : 23, the effects are of the same kind, and when driven at the same speed gives the same beat-tone as the former wave-disk. It will be noted that in each of these two cases the frequency of the beat-tone is neither the difference nor the sum of the frequencies of the two primary tones.

To be continued.)

DR. HENRY SCHLIEMANN.

THE death of Dr. Schliemann comes on his friends not only as a sorrow but as a surprise, for though he had tried his constitution, his strength was so great that it seemed equal to many more labours. He was essentially a self-made man, not merely as the architect of his own fortunes, but as having at an early age deliberately adopted certain purposes in life, and having acomplished them with astonishing success. These deliberate purposes made his career brilliant, and his character manly and sturdy.

In the preface to "Ilios" (1880) Schliemann gives a sketch of his early life and his excavations which reads like a romance. Before he was ten years old, he had made up his mind that the mighty walls of Troy could not have entirely disappeared, but must have been only buried by the dust of ages, and that he would himself some day bring them to light. But a romantic boyhood ended in a bitter struggle with poverty, until at one time he had to sell his last coat, and after shipwreck arrived at Amsterdam a penniless outcast. Obtaining there a post worth £32 a year, he spent the half of that sum on living, and the other half on self-education, his dinner costing him twopence, and a fire being an unknown luxury. By sheer business talent he rose from this poverty to great wealth; but this has been done by hundreds of uninteresting men: the interesting thing about Schliemann is that he never looked on wealth save as a means for accomplishing his darling purposes. While never ceasing to pray that some day he might have the happiness of learning Greek, he began on modern languages, which he mastered one after another in an incredibly short space of time, learning not merely to read, but to write and speak fluently, Swedish, Polish, Russian, Arabic, and many other tongues.

In 1871, he began his career as an explorer by an attack on the hill of Hissarlik, where he expected to find the remains of the Troy of his early dreams. In the art of excavation he seems to have had no teacher, and there can be no doubt that he has opened a new era in that art through his scientific genius. Literary genius may be erratic and incalculable, but genius works in science through clear discernment of means and by infinite pains. Schliemann's method was simple—to remove all the earth of a site and pass it through a sieve, taking care that, though hundreds of hands were at work, every one worked as an immediate organ of his own intelligence and design. The quality of insight came in principally in the choice of sites.

Splendid as were the results of Schliemann's excavations at Hissarlik, at Mycenæ, and at Tiryns, both as regards the recovery of antiquities, and as regards the advancement of knowledge, scholars recognize that he was not to be followed in his interpretation of his own discoveries. His individuality was too strongly marked, his imagination too fervent, to allow him to walk safely in the narrow ways of archæological science. And as a man who was born a fighter, and thoroughly carried out the old rule as to loving one's friends and hating one's enemies, he could not be impersonal in the choice of theories. To the judgment of his allies he often gave way with a delightful simplicity and modesty, but to attack he was impervious. One of his most interesting appearances in London was when he came in hot haste from Athens, accompanied by Dr. Dörpfeld, at the invitation of the Hellenic Society, to meet in open debate the objections, which he characteristically called "calumnies," which some archæologists had brought forward against his theory in regard to the prehistoric palace at Tiryns. Though Schliemann was a decided believer in Providence, there was in him an immense force of tough old Teutonic heathenism.

Yet he was a man of the world, and cosmopolitan in a sense in which few can claim to be so, for in almost any country he could have made himself at home as an active citizen. His ideal was Greece, and he succeeded in a very difficult task by finding in modern Greece the materials for a splendid ideal, when worked up with traditions of ancient glory. In this respect he was a greater poet than Byron, who spoke of some modern Greeks as "craven crouching slaves." Athens has indeed good reason for the gratitude which has assigned him a public grave at Colonus. At Athens he will leave a great gap. His palace, reflecting in every corner his career and his enthusiasms, was a place where the most open hospitality was accorded to men of all nations. In the living-rooms were Homeric texts; the servants bore high-sounding Greek names; the basement rooms were full of the spoils of Troy; while on the summit stood replicas of celebrated Greek statues. The host poured forth the simplicity of his heart and the overflowings of his enthusiasm, talking with an unconventional plainness sometimes disconcerting to Western ladies. Not even Madame Schliemann, long as she has shared her husband's labours and pursuits, can fill the blank which he has left. All archæologists must regret that his purposes of further excavation at Hissarlik, in Crete, and elsewhere, have dropped, or at any rate must be carried out by others who have not his marvellous power of finding valuable remains which others have failed to find.

THE METEORITE OF OSCHANSK.

A METEORITE which has lately been added to the collections of the Natural History Museum of Paris is described in *La Nature* by M. Stanislas Meunier; and by the courtesy of the editor of that journal we are enabled to reproduce the figures by which the article is illustrated. The meteor, of which this is a fragment, fell in Russia, on August 18 (30), 1887. Before it fell it was seen by various persons in the south-western part of the Government of Perm, and in the Government of Viatka—principally in the districts of Perm, Oschansk, Kungur, Osoo, and Sarapul. Between Perm and Oschansk, according to an inhabitant, it appeared about 12.30 p.m. in a clear sky, leaving behind it an almost horizontal train of great brilliancy. Detonations were heard, resembling a discharge of musketry rather than thunder. A little afterwards it fell in a shower of incandescent stones, which buried themselves more or less deeply in the earth. They were very numerous, and weighed from one to 330 kilogrammes. Fig. 1 represents the meteor as it was seen by M. Selivanof, a professor of the seminary of Perm. This observer writes:—

"On August 18, a little before one o'clock p.m., I returned to the seminary. The weather was calm, and the sky covered with small fleecy clouds. Just as I was about to cross the threshold, I happened to look towards the south, and saw a brilliant body like a shooting-star, or, rather, like a piece of iron glowing at the forge, gliding from east to west in a direction almost horizontal or slightly inclined towards the earth. The meteor made scarcely more noise than a rocket, and at first I believed it was one. Its course was sufficiently rapid, and during two or three seconds I followed the bolide over the space of a small number of degrees. It left a luminous train, which was very rapidly extinguished. Perhaps this train resulted simply from the persistence of the luminous impression upon the retina. The case, however, was otherwise with a pale nebulous band, which persisted about five minutes."

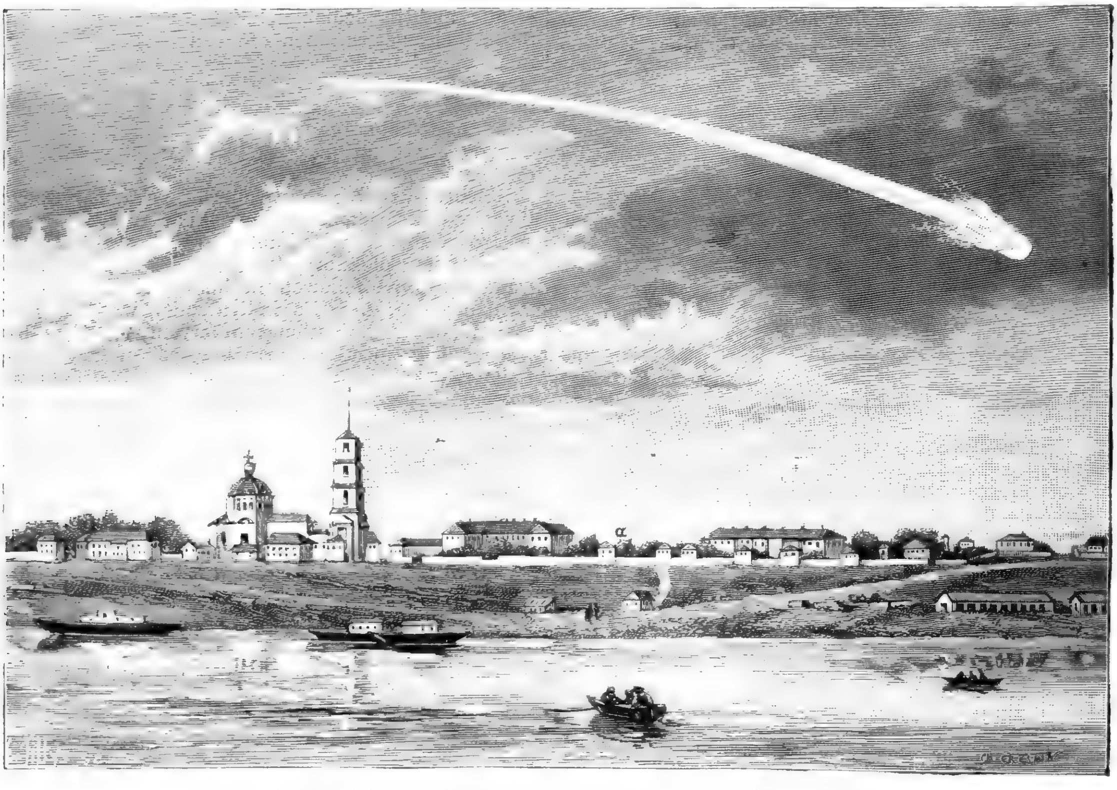


Fig. 1.—Passage of the meteorite of Oschansk, above Perm *a*, the point of observation.

The majority of the meteorites brought by this fine meteor have certainly been lost. Only six of them have been found—five at Oschansk, one at Tabor. At the moment of the fall, M. Nagibine was in a street of Oschansk, and heard the noise which announced it. About half a minute after the cessation of this noise, he observed a blackish stone which hissed through the air as a cannon-ball might have done. Several workmen ran and found the meteorite at the bottom of a hole—about 50 centimetres deep—which it had hollowed in the ground. It was as large as a child's head, and was still hot; it weighed 1·790 kg.

At Tabor the phenomenon was noticed by two peasants who were working in a field. Surprised by the detonations and the rumbling sound, they looked up, and saw the bolide, of a dark red colour, followed by a white smoke, which the wind agitated; and sending forth an odour of sulphur (Fig. 2). The mass seemed to be at a height of about 200 metres, and by the shock of its fall raised a column of dust. One of the peasants, who was mounted on a corn-rick, was thrown to the ground by the atmospheric disturbance. At the place where the stone fell they found a hole 4·20 metres deep, 2·10 metres wide (Fig. 3). The stone was too hot to be removed immediately. Next day it was taken out in fragments, one of which weighed 98 kilogrammes, the others from 100 grammes to 11 kilogrammes. There were about 82 kilogrammes of *débris*. The entire weight of this single stone is estimated at 328 kilogrammes.

From information furnished by the observers it is concluded that the bolide moved from 10° to 15° from

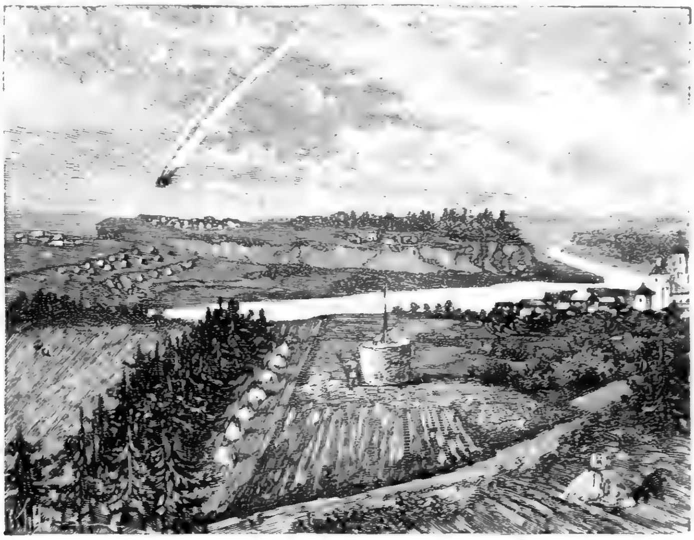


FIG. 2.—Passage of the meteorite at Tabor.

the east towards the south, and that, from the point of observation in the field, the angle of the fall appeared to be about 55° above the horizon.

Another stone, which has not been found, fell into the River Kama, at Tabor. A forester who was near the spot, says that the banks of the river trembled, and that the water, after having been thrown up in a high column, continued to bubble for a long time. The air was so violently agitated that a troop of 50 or 60 horses, which were drinking at the river, were thrown to the ground.

When the fragments found at Tabor were pieced together, the stone presented the form of a polyhedron with

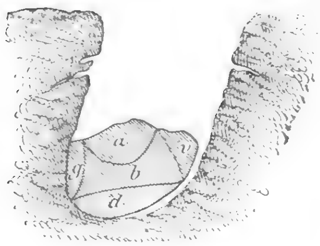


FIG. 3.—Meteorite of Tabor, after its fall. *a*, its largest fragment (98 kg.); *b*, part taken away by the public; *v*, grey part; *g*, fragment weighing two kilogrammes; *d*, small fragments.

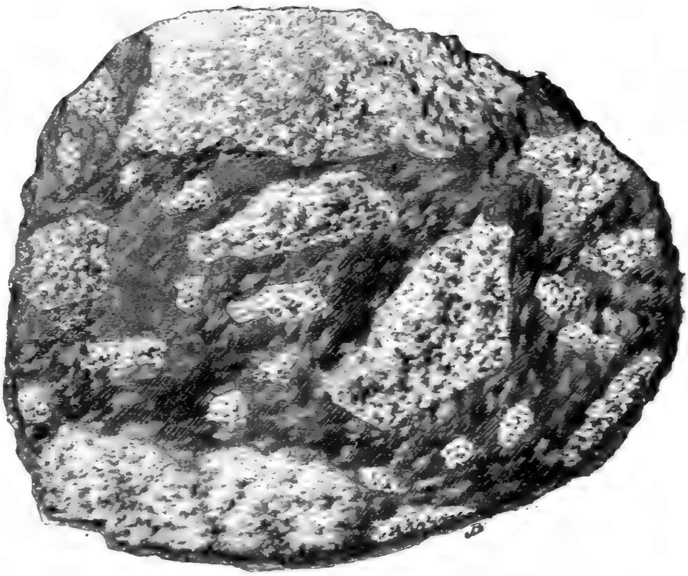


FIG. 4.—Fragment of the meteorite of Oschansk (in the Museum of Natural History, Paris).

angles very much blunted. It was enveloped in the usual black crust, but this was exceptional in presenting flaws as big as a pea, or even larger.

Fig. 4 represents the appearance of the specimen in the Paris Museum. M. Stanislas Meunier has made it a subject of chemical and microscopical study, which has led him to the conclusion that the meteorite of Oschansk belongs to a lithological brechiform type, which he described twenty years ago under the name of "Canellite." To the same type belonged, among others, the meteorites of La Baffe (Vosges), September 13, 1842; of Assam (India), 1846; of Canellas (Spain), May 14, 1861; of

Feid-Chair (Algeria), August 16, 1876. The stone consists of fragments of two very oolithic rocks, one called "Limerickite," of a dark violet colour, the other, called by M. Meunier "Montréjite," quite white. From the juxtaposition of different rocks in this as in some other meteorites, M. Meunier argues that, in the *milieu* where these stones are formed, the general geological conditions must be analogous to those which exist on the earth.

NOTES.

THE general meeting of the Association for the Improvement of Geometrical Teaching is to be held at University College, Gower Street, on Saturday, January 17. At the morning sitting (10.30 a.m.), the reports of the Council and Committees will be read, and the new officers will be elected. On the conclusion of the elections Miss Wood will read a paper "On the use of the term 'Abstract' in Arithmetic." After an adjournment for luncheon at 1 p.m., members will re-assemble for the afternoon sitting (2 p.m.). Papers will be read by Prof. Minchin, on "Another Voyage to Laputa"; by Mr. E. T. Dixon, on "The Foundations of Geometry"; and by Mr. E. M. Langley, on "Some Notes on 'Statics and Geometry.'"

PROF. RUDOLF VIRCHOW will reach his seventieth birthday on October 13. It has been decided that the occasion shall be commemorated by the striking of a large gold medal in his honour.

SEVERAL men of science were included in the list of New Year honours. A baronetcy was conferred on Dr. Richard Quain, and Prof. George Humphry, Cambridge. Prof. Ball Director of the Museum of Science and Art, Dublin, was made a C.B.; Dr. Theodore Cook, Principal of the College of Science, Poona, became a Companion of the Most Eminent Order of the Indian Empire; and Mr. Frederick McCoy, C.M.G., Professor of Natural Science in the University of Melbourne, was promoted to be an ordinary member of the second class, or Knight Commander of the Most Distinguished Order of St. Michael and St. George.

THE memorial concerning the ancient monuments of Egypt, which was lately presented to Lord Salisbury, has been so far successful. It is to be forwarded to H.M.'s Agent and Consul-General at Cairo, for presentation to the Egyptian Government; and Sir E. Baring will be instructed to state that if an official inspector is appointed the question of his nationality will not be considered important, the only desire of the British Government being that adequate steps shall be taken "to preserve the monuments from further destruction or mutilation."

WE regret to have to record the death of Mr. John Marshall, F.R.S., President of the General Medical Council, and Professor of Anatomy to the Royal Academy. He died on January 1, at the age of seventy-two. Mr. Edward Bellamy, whose studies were akin to those of Mr. Marshall, died on the 4th inst. after an illness of only three days. He had been for many year lecturer on artistic anatomy at the South Kensington School.

PROF. CASEY, F.R.S., Fellow of the Royal University of Ireland, died on January 3, at the age of seventy. He was an eminent mathematician, and much regret at his death has been expressed in Ireland.

A CIRCULAR letter from the Societa Italiana di Scienze Naturali of Milan, dated January 2, announces the death of their President, the Cav. Abate Antonio Stoppani, Professor of Geology in the R. Istituto Tecnico Superiore. The President died on New Year's Day, at the age of sixty-six.

PROF. W. J. STEPHENS, President of the Linnean Society of New South Wales, died on November 22, 1890. At the meeting of the Society on November 26, resolutions were passed, expressing high appreciation of his services and sympathy with his family.

MR. WILLIAM LANT CARPENTER, who died on December 23 last, was the eldest son of the late Dr. W. B. Carpenter. He was born in 1841, and educated at University College School and University College. At the age of eighteen he went to Bristol as chemist to Messrs. C. J. Thomas and Brothers, soap and candle manufacturers, in which firm he became a partner. Afterwards he gave up business, and settled in London, eventually becoming one of the managers of the School of Electrical Engineering in Hanover Square. For some time past he has been widely known, more especially in the north of England, as one of the most successful lecturers in connection with the Gilchrist Educational Trust and the University Extension movement. In 1885 he brought out "A Treatise on the Manufacture of Soap and Candles, Lubricants, and Glycerin"; and he contributed a few papers on kindred subjects to the Journal of the Chemical Society, and the Proceedings of the British Association. Under the title "Energy in Nature" he published in 1883 a popular exposition of the doctrine of the conservation of energy, being the substance of six lectures delivered for the Gilchrist Educational Trust two years previously; and in conjunction with the late Prof. Balfour Stewart he contributed some papers to the Proceedings of the Royal Society on the periodic changes of sun-spots, and their connection with terrestrial and magnetic phenomena.

ON January 2 the Institution of Civil Engineers reached its seventy-third anniversary. The numbers of the various classes now on the books comprise 1700 members, 2880 associate members, 427 associates, 19 honorary members, and 927 students—together 5953.

THE Calendar of the Department of Science and Art for the year 1891 has been issued.

A SPECIAL Commission has been for some years established at Trieste by the Austrian Government for the oceanographic investigation of the Ionian and Adriatic Seas. During the past year the transport *Pola* has been set apart for the study of the sea-bottom, currents, temperature, &c., of the Adriatic, and has obtained important results. At a depth of 2000 metres abundance of a green Alga, *Halosphæria viridis*, was found.

THE marine laboratory of the Johns Hopkins University is to be opened next spring.

A PHOTOMICROGRAPHIC laboratory has been opened at Rimini by Count R. Sernagiotto, for the preparation and publication of photographic reproductions. Among the microphotographs already issued, *Notarisia* mentions, as of remarkable clearness and beauty, one of *Pleurosigma angulatum*, 5000 diams., of *Bacillus radiciformis*, 1000 diams., and a tangential section of the stem of the vine, 75 diams.

THE American National Educational Association will hold its next meeting at Toronto in July. Many teachers of the Dominion have become members, and *Science* says that "they will meet in Toronto in full force, and will prepare an exhibit giving a complete view of Canadian systems of education."

UNDER the auspices of the Penzance Natural History and Antiquarian Society and the Committee of the Penzance Library, a fund is being raised for the purpose of erecting a suitable monument or tombstone in Penzance Cemetery, as a memorial of the late Mr. John Ralfs, who died July 14, 1890. Subscriptions should be sent to Mr. W. Bolitho, Jun., Penzance, Treasurer of the Ralfs Fund.

DR. JULIUS WORTMANN, one of the editors of the *Botanische Zeitung*, has been appointed Director of the physiological experimental station of the Royal Institution for instruction in fruit- and vine culture at Geisenheim on the Rhine.

THE Trustees of Columbia College, U.S.A., have appointed as Curator of their Herbarium Dr. Thos. Moring, who has just returned from a botanical exploration of southern South America with very large and valuable collections.

THE Horace Mann School, Boston, Mass., recently celebrated its twenty-first anniversary. The Hon. Gardiner G. Hubbard delivered an interesting address on the occasion, sketching the history of the articulating system for the deaf in America. In 1860, Mr. Hubbard's daughter, who was then four years old, lost her hearing through a severe illness. Mr. Horace Mann and Dr. Howe had been studying the various methods of educating the deaf in Europe, and had reported in favour of the oral system which had for some time been practised in Germany. Mr. Hubbard therefore consulted Dr. Howe, who recommended that the child's relatives should talk to her, and teach her to recognize the spoken words of their lips. He especially urged that they should not have recourse to signs, and that they should persistently refuse to understand them. The plan was tried, and the result was so successful that a little school for the deaf, with Miss Rogers as teacher, was opened at Chelmsford in 1866. By and by a charter was granted, and the school was transferred to Northampton. The system attracted the attention of Mr. Dexter S. King, and through his efforts a school was started in Boston in 1869. This institution was afterwards called the Horace Mann School, and its success was to a large extent due to the intelligence and enthusiasm of Miss Fuller and Miss Bond, to whose work Mr. Hubbard, in his address, did ample justice. Now the advantages of the oral system are widely recognized, and, as Mr. Hubbard pointed out, the influence of the Horace Mann School has been felt in England as well as in many different parts of the United States.

IN connection with a meeting of the American Electric Light Association, which is to be held at Providence in February, there will be an exhibition of electrical apparatus and appliances, especially such as are used in the furnishing of light and power. This meeting will practically mark the close of the first decade of "electric lighting commercially." It has been suggested, therefore, as we learn from *Science*, that efforts should be made to show the progress in the art by the exhibition of the earlier forms of apparatus and appliances, along with those embodying the latest improvements.

AT the meeting of the French Cremation Society on December 13, an able address on the subject of cremation was delivered by M. Fréderic Passy, member of the Institute. Dealing with the common objection that incineration would facilitate poisoning, he urged that the traces of vegetable poisons vanish rapidly, and that if mineral poisons were used most of them could be detected in the ashes. Moreover, he insisted that there are poisons the presence of which in a human body does not necessarily prove that a crime has been committed; and that if cremation were generally adopted greater care would be taken to determine the precise causes of death. That there is in many minds a strong prejudice against cremation he admitted; but this he attributed to the influence of ancient custom. Irresistible evidence, he maintained, showed that the existing system cannot but be injurious to public health.

THE Administration of the schools of Caucasia is continuing the publication of the excellent linguistic works of Baron Uslar. The fourth volume, which is now out, contains his study of the Lakh language, as well as (in an appendix) his letters to Prof. Schiepner and a Kazy-kumukh alphabet.

THE British Museum (Natural History) has issued the third edition of a Catalogue containing a list of all the subjects of which moulds and store-casts have been prepared for the purpose of supplying museums, and the public generally, with copies of the most striking objects in the collection that will admit of being moulded without injury. The casts are chiefly reproductions of specimens of fossils in the Department of Geology. The prices (revised to date) have been approved by the Trustees.

AN excellent number completes the third volume of the *Internationales Archiv für Ethnographie.* Dr. P. Schellhas, of Berlin, contributes (in German) a most careful paper embodying the results of comparative studies in the field of Maya antiquities. These antiquities he divides into three classes—architectural remains; Maya MSS.; and smaller objects. The latter class has been greatly enriched by the Yucatan collection now in the Berlin Museum für Völkerkunde. Dr. J. H. Spitzly, military surgeon in the Dutch West Indian army, brings together some instructive notes (in English) on three stone adzes from Surinam (Dutch Guiana), and on eight stone implements from the islands of St. Vincent and St. Lucia. A paper (in German) on old Mexican and South American throwing-sticks is contributed by Dr. H. Stolpe, of Stockholm; and Dr. J. D. E. Schmeltz, Director of the Leyden Ethnographical Museum, offers (in German) various facts and suggestions relating to the ethnography of Borneo. The illustrations, as usual, are remarkably good.

WE have received the volume of Records of the meeting of Russian Naturalists and Physicians which was held at the beginning of this year at St. Petersburg. It is a well printed octavo volume of about 1000 pages, with several engravings and maps, and it is sold at the extremely moderate price of five roubles. It contains the proceedings of all the meetings, and abstracts of the chief papers read, or the papers themselves; thus giving a series of most valuable contributions and notes in mathematics and astronomy, physics, chemistry, botany, zoology, geology, geography and anthropology, scientific agriculture, and scientific medicine. Some of the papers are very elaborate.

AN earthquake in Northern California, on January 2, is reported by Prof. Holden, of the Lick Observatory, to have been the most severe experienced in that district since 1868. The ceilings of the Observatory were cracked, the plaster falling to the floor. The large equatorial telescope is, however, believed to be uninjured.

LIEUT. J. P. FINLEY has published his long-promised storm-track, fog, and ice charts of the North Atlantic Ocean, in a quarto atlas. Part i. consists of track-charts for each month; these form a very tangled skein, but the *general* drift of the storms can be clearly seen: the majority of them pass to the north-west of the British Isles. Part ii. consists of 13 charts representing the storm frequency by shaded areas. A careful study of these charts will materially assist shipmasters to determine the general locality and frequency of storms in different months. If the main object were safety instead of speed, a more southerly route than the "lanes" generally followed would be preferable. In voyages *from* America vessels sail more or less with the prevailing direction of the wind, and *vice versâ.* The fog and ice charts show the average probable and extreme eastern and southern limits at which these may be encountered in each month. Icebergs are most frequently met with from July to September inclusive; from November to June the drift of the bergs southwards is impeded by ocean ice in the friths.

THE *Bulletin Mensuel* of the Zi-ka-Wei Observatory (near Shanghai) contains a discussion of the monsoons of the Yang-tse-Kiang from the observations at the stations of the maritime customs established from the entrance of that river as far as Hankow. The results are given in the form of wind-stars, which show that at the mouth of the river the influence of the two monsoons is sharply marked: the winter monsoon blows from between north-west and north-east, the north-west winds being most prevalent, while the summer monsoon blows from between south and south-east. After leaving the mouth the distinction is not so plainly marked; the prevalent winds tend more to the north-east in winter, and in summer more to the east and even north-east.

THE new Russian *Meteorologitcheskiy Sbornik*, published by the Russian Academy of Sciences, to the first volume of which we referred the other day (p. 183), contains Russian translations and Russian originals of papers published in German and French in vol. xiii. of the *Repertorium für Meteorologie*. The two publications will be parallel, under the Director of the Central Observatory, H. Wild.

IN a report to the Foreign Office, recently published, Colonel Stewart, the British Consul-General at Tabreez, calls attention to the curious system of lakes in that region, situated at a great elevation above the sea-level. These are the lake of Urumia, situated 4100 feet above the sea, Lake Van, and the Guektcha lake. Lake Van is in Turkish territory and the Guektcha lake in Russian territory, though both are near the bottom of the Persian province of Azarbaijan, in which is situated the lake of Urumia, the largest and most important. It is 84 miles long and 24 miles broad, and is probably the saltest piece of water on earth, being much salter than the Dead Sea. The water contains nearly 22 per cent. of salt. Its northern coasts are encrusted with a border of salt glittering white in the sun. It is said that no living thing can survive in it, but a very small species of jelly-fish does exist in its waters. Many streams pour down from the Kurdish mountains which border Turkey, and render the country between them and the lake of Urumia very green and fertile. This part of the country looks more like India than Persia, but the climate is severe in winter. The whole country being situated from 4000 feet to 5000 feet above ocean level, the snowfall in winter is great. At night in winter the thermometer falls frequently below zero of Fahrenheit, but in the day-time it rises considerably, generally reaching 28° or 30°, and this with a bright sun overhead. Many people are frozen to death on the roads in winter while crossing the various passes. The winter climate may be compared to that of Canada, but the summer approaches that of Northern India.

THE area under the administration of the Bengal Forest Department, according to its last Report, during 1889–90 consisted of 5195 square miles of reserved forest, 2239 square miles of protected forests, and 4034 square miles of unclassed State forest and waste lands, aggregating 11,468 square miles, which is 5½ per cent. of the total area of the province, viz. 193,198 square miles. The forests are, however, confined to the districts bordering on the sea, the sub-Himalayan tracts and the plateau of Central India, so far as it stretches into Chota Nagpore and Orissa. An area of 207 square miles was added to the reserves during the past year, and 25 square miles of protected forests in the Sunderbunds were farmed out for reclamation. The title of Government to existing reserves is being completed by a compliance with the requirements of the Act, and the inquiries incidental to these proceedings will also secure the record and protection of private easements. The special measures taken for the protection of forests from fires have been increasingly successful, 95 per cent. of the areas thus dealt with having escaped, in spite of the dryness of the season, against 72·9 per cent. in the previous year. The financial results of the past year show a net surplus of Rs. 3,78,454, against Rs. 3,08,738 in 1888–89.

TOADS have been observed by some persons to feed willingly on bees and even wasps; and M. Hiron-Royer, who has noticed the fact, says that *Hyla versicolor* is positively frantic about wasps. He has seen one prefer them to every other kind of food, and devour them eagerly, although the sting does sometimes bring the creature to temporary grief.

ONE of the last volumes of the *Mémoires* of the Kazan Naturalists (vol. xxii. 5) contains a most valuable work by P. Krotoff and A. Netchaieff, upon the geology of the Kama region, being the first part of a general description of that region which the Society intends to publish. The oldest geological formation of the region is that succession of red and grey sandstones and limestones, as well as of "variegated marls," which is considered as Permian by the Kazan geologists, and Triassic by other Russian explorers. The chief interest of the Lower Kama region is, however, in its Post-Pliocene deposits. They cover nearly the whole of the territory, and belong to two different series: the yellow loess-like sandy clays—containing the usual terrestrial shells (*Helix*, *Pupa*, *Succinea*, *Limnæus*, *Pisidium*, &c.), together with bones of Mammoth and Rhinoceros—and the Caspian deposits. The latter attain a quite unexpected development, as they are found in all the valleys, thus bearing unmistakable evidence of the former extension of gulfs of the Caspian Sea up the valley of the Kama (almost as far as its junction with the Vyatka) and its tributaries. Deposits, containing the undoubtedly Caspian species of *Adacna plicata*, *Cardium edule*, *Dreissena polymorpha*, and *Valvata piscinalis*, as well as teeth of fishes, reach the heights of from 530 to 540 feet above the sea-level; and their presence has been noticed on the Byelaya river as far as Angasyak, as also in the north of the Kama at the town of Laishev, and in the lower parts of the Mesha river. It is thus certain that a gulf of the Caspian Sea penetrated up the valley of the Volga and its tributaries as far as 55° 23′ of northern latitude, and that the upper parts of this gulf contained a water less salt than that of the Caspian Sea. It is very difficult to ascertain whether the deposits just mentioned belong to the later parts of the Pliocene period, or to the earlier parts of the Post-Pliocene, and Russian geologists are divided upon this point. But the Kazan authors point out the absolute identity of the fossil species of *Adacna*, *Cardium*, *Dreissena*, *Didacna*, and *Hydrobia*, with those now living in the Caspian Sea; while the discovery in a fossil state of a species of *Corbicula*, and Prof. Sintsoff's *Hydrobia novorossica*, which are not met with now in the Caspian Sea, is not considered as sufficiently proving the greater antiquity of the Kama deposits. Part of these deposits have been denuded by the rivers and mixed with the above-mentioned loess.

OUR ASTRONOMICAL COLUMN.

PERIHELIA OF COMETS.—In *Astronomische Nachrichten*, No. 3005, Dr. Holetschek discusses the apparent connection between the heliocentric longitudes of comet perihelia and the heliocentric longitude of the earth at the times of their perihelion passages. It is evident that any one comet appears brightest when its *perihelion* passage occurs at the same time as that of *perigee*. The possibility of discovering and observing comets decreases, therefore, with the increase of the arc contained between the heliocentric longitudes (l) of comet perihelia and the heliocentric longitude (L) of the earth during the perihelia.

Dr. Holetschek has tabulated the value of (l – L ± 180°) for every known comet, and the values obtained are as follows:—

(l – L ± 180°)	Number of elliptic comets.	Number of parabolic comets.	Total number with perihelion distances greater than 0·3.
0 to 30	16	90	106
30 ,, 60	10	45	55
60 ,, 90	3	46	49
90 ,, 120	2	26	28
120 ,, 150	—	21	21
150 ,, 180	—	22	22
Total ...	31	250	281

It will be seen that the majority of all the known comets have the longitude of their perihelion at a small angular distance from that of the earth. This grouping is especially well marked in the case of elliptic comets of short period. The following table exhibits the results found for comets with perihelion distances less than 0·3 times the mean distance of the earth from the sun:—

l – L	Number of parabolic comets.
0 to 60	19
60 ,, 120	10
120 ,, 180	7
Total ...	36

Another interesting result brought out by Dr. Holetschek's investigations is that the positive sign occurs with very nearly the same frequency as the negative sign in the angle l – L ± 180°. This indicates that the perihelion points, when reduced to the ecliptic, lie, with equal frequency, in front of and behind the earth.

Mr. W. E. Plummer points out, in this month's *Observatory*, that the cause of the abnormal distribution exhibited by comets whose orbits are greatly inclined to the elliptic has still to be explained.

GASEOUS ILLUMINANTS.

I.

THE autumn course of Cantor Lectures at the Society of Arts was delivered by Prof. Lewes, who, after discussing the nature of various gases capable of burning with luminosity, gave a review of the theories which have been advanced to explain the light-giving power of certain flames.

Sir Humphry Davy first propounded his theory that the cause of luminosity in ordinary flames was the incandescence of nascent carbon—a theory which was accepted as the true one until the researches of Dr. Frankland in 1868, on the effect of pressure on non-luminous flames, showed that, under certain circumstances never likely to arise in a gas-flame, luminosity might be due to other causes. Later observers have shown that luminosity in a flame is to a great extent affected by temperature. These factors, though of the greatest importance, do not affect the truth of the original theory.

In the Philosophical Transactions for 1817, Sir Humphry Davy says, while alluding to a paper published in one of the early numbers of the *Journal of Science and Arts*: "I have given an account of some new results on flame, which show that the intensity of the light of flames depends principally upon the production and ignition of solid matter in combustion."

This definition, however, has been gradually altered until it is more often stated that "the presence of solid particles suspended in the flame (or in immediate contact with the burning gas) is essential to its luminosity"—an idea which Davy never had, as is shown by him later in the paper defining flame as follows: "Flame is gaseous matter heated so highly as to be luminous;" and again: "When, in flames, pure gaseous matter is burnt, the light is extremely feeble." Moreover, he alludes to "common flames"—evidently meaning the flames of candles, lamps, or gas; in all of which cases I think it can be proved beyond a doubt that his theory, as expounded by himself, was perfectly correct.

On June 11, 1868, Prof. E. Frankland read a communication before the Royal Society, in which he described experiments which led him to doubt Sir Humphry Davy's theory. He points out that the deposit of soot formed when a cold surface is held in a gas or candle flame is not pure carbon, but contains hydrogen, which can only be got rid of by prolonged heating in an atmosphere of chlorine. Also that many flames possessing a high degree of luminosity cannot possibly contain solid particles. Arsenic burnt in oxygen gives a bright white light; yet as arsenic volatilizes at 180° C., and the arsenic trioxide forms at 218° C., it is evident that at the temperature of incandescence (which is at least 500° C.) there can be no solids, but simply vapours present in the flame; and for the same reason, the intense light resulting from the burning of phosphorus in oxygen cannot be explained by the solid particle theory. From these results, Dr. Frankland considers that "incandescent particles of carbon are not the source of light in gas and candle flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapours;" and he further shows that non-luminous flames, such as that produced by carbon monoxide and hydrogen, can, when burning in an atmosphere of oxygen, be rendered luminous if the ordinary atmospheric pressure is increased to 10 atmospheres, so as to prevent or retard as far as possible expansion during combustion. From Dr. Frankland's experiments, there is no doubt that the luminosity of a flame is increased by pressing around it the atmosphere in which it is burning, and also that rarefaction has the opposite effect—a point also worked at by Davy; but his experiments do not show that incandescent particles of carbon are not the principal source of luminosity in a gas-flame. He also shows that, the higher the density of the vapours present in a flame, the more likely is it to be luminous.

In 1874, Soret attempted to demonstrate the existence of solid particles in a luminous hydrocarbon flame, by focussing the sun's rays on the flame, and examining the reflected light by means of a Nicol prism; but neither his research nor that of Burch, who repeated his experiments, using the spectroscope instead of the prism, showed more than that solid particles are present. Herr W. Stein, in considering Dr. Frankland's objections to Davy's theory, pointed out that the soot which is deposited from a candle or gas flame, and which Frankland looked upon as a condensed hydrocarbon, contains 99·1 per cent. of carbon and only 0·9 per cent. of hydrogen, which is about the quantity of hydrogen one would expect to be occluded by carbon formed under these conditions, and he also pointed out that if the soot were a heavy hydrocarbon condensed by a cold surface, cooling the vapour present in the flame, it ought to again become volatile at a high temperature, which it does not. The next steps in the controversy were the attempts made by Hilgard, Landolt, and Blochman, to trace the actions taking place in various flames by withdrawing the gases from different parts of the flame and determining their composition.

The experiments so made show that of the ordinary constituents of the gas the hydrogen is the first to burn, as one would expect from its relatively low igniting point and great rapidity of combustion. The burning of the carbon monoxide cannot be traced in the same way, as it is formed more rapidly (by the incomplete combustion of the marsh gas) than it burns, so that a steady increase in the proportion present takes place while the marsh gas steadily burns away, until a height of 1½ inches is attained, when its combustion becomes very rapid. Practically the illuminants do not undergo any change at first—indeed, they slightly increase in quantity from the decomposition by heat of some of the marsh gas into acetylene. They only begin to decompose at a height of 1½ inches above the orifice of the burner; and then burn rapidly in the highest part of the flame. Moreover, a most important fact to be noted is that at the height of 1½ inches there is a sudden rise in the quantity of carbon monoxide at the moment that the illuminating olefines begin to disappear—a result undoubtedly due to the action of the nascent ignited carbon on carbon dioxide.

The illuminants in the flame consist of various hydrocarbon gases and vapours which in the lower part of the flame are reduced by the heat to simpler hydrocarbons, and finally in the luminous zone become decomposed to methane and carbon; and it is the carbon in excessively minute particles which at the moment of liberation is heated to incandescence, and "principally" gives the light of the flame—the marsh gas originally present, and also that formed from the heavier hydrocarbons, adding its quota to the luminosity by still further decomposition during combustion, and finally becoming carbon dioxide and water. In 1876, Dr. Karl Heumann made a most important contribution to the theory of luminous flames in some papers published in *Liebig's Annalen*, in which he carefully went over the work of previous observers, and, by a large number of original experiments, proved that Davy's theory was correct, but that other causes also affected the degree of luminosity in a gas or candle flame.

In the ordinary atmospheric burner in which a mixture of coal gas and air burn with a non-luminous flame, it was supposed that the admixture of air, by supplying oxygen to the inner portion of the flame, caused immediate and complete oxidation of the hydrocarbons, without giving time for the liberation of carbon in the flame, and consequently luminosity. More modern researches, however, have proved this to be utterly wrong. The loss of luminosity is due to two causes—first, to the diluting action of the air introduced; secondly, to the fact that when a gas is so diluted, it requires a far higher temperature to break up the hydrocarbons present than when the gas is undiluted, and therefore the temperature which serves to liberate carbon and render the undiluted gas-flame luminous, is totally insufficient to do so in the diluted gas. Consequently the hydrocarbon burns to carbon dioxide and water without any such liberation, and hence with a non-luminous flame. The truth of this theory can be easily proved by the fact that diluting the gas with nitrogen, carbon dioxide, or even steam, serves to render it non-luminous, and therefore more rapid oxidation has very little or nothing to do with it, while the non-luminous flame can again be rendered luminous either by heating the mixture of air and gas just before combustion, or by heating the air with which the gas is diluted. This being so, it is evident that in the non-luminous flame we have the same hydrocarbon present as in the luminous flame; and anything that will tend to break them up, and liberate the carbon before the hydrocarbons are consumed, should again make the flame luminous.

That heat will do this has been already shown; but it can be demonstrated in a still more striking way. It is well known that chlorine gas and bromine vapour will both support the combustion of a gas containing much hydrogen, but that the combustion is very different from that of the same gas burning in air, as the chlorine or bromine, having no affinity for the carbon, combines with the hydrogen only, and deposits the carbon in clouds of soot; in other words, at the temperature of flame, chlorine will break up the hydrocarbons and liberate solid carbon. If now a small quantity of chlorine is led into the non-luminous Bunsen flame, it at once becomes luminous, proving conclusively that luminosity is due to solid particles of carbon liberated in the flame. Again, Heumann points out that a small rod held in the luminous flame becomes rapidly covered on its lower side with a deposit of soot; that is to say, the soot is present in particles in the flame, and the uprush of the gas drives it against the rod and deposits it there. If the soot were present in the flame, as Frankland supposed, in the state of vapour, and the rod merely acted by cooling and condensing it, the soot should be deposited on all sides of the rod; while a still further proof is, that if the soot existed as vapour in the flame, then, if the rod were heated to a high temperature, no soot should be deposited on it, whereas the soot deposits on a heated surface just as well as on a cool one.

It has been objected to the "solid particle" theory that, if it were true, solid carbon particles introduced into a non-luminous flame should render it luminous and make it look like an ordinary gas flame, whereas it simply gives rise to a cloud of sparks. But it must be remembered that the "nascent" carbon, as it is liberated from the decomposing hydrocarbons, is in the molecular condition, and has a very different degree of coarse-grainedness to any preparation of charcoal or lampblack we can make; and that, although our finest particle is a mass which takes so long to burn that it leaves the flames only partly consumed, and is projected into the air as a spark, the molecular particles of carbon are consumed as soon as they are rendered incandescent, and a steady luminosity, free from sparks, results. It is possible, however, to make the particles in a luminous flame roll themselves together, when they can be either deposited in a very coarse kind of soot, or be seen as glowing sparks and particles in the mantle of the flame. This can be done when two luminous flames are allowed to rush against each other or against a heated surface. Heumann also shows that the luminous mantle of a flame is not altogether transparent, and that the thicker the flame-layer, and the greater the number of solid particles contained in it, the less transparent does it become. If a non-luminous flame—say, hydrogen—is charged with the vapour of chromyl dichloride (CrO_2Cl_2), chromic oxide is produced; and this flame, which undoubtedly contains solid particles, is quite as transparent as the hydrocarbon flame. Finally, those flames which undoubtedly owe their luminosity to the presence of finely-divided solid matter produce characteristic shadows when viewed in sunlight; the only luminous flames which do not throw shadows being those which consist of glowing vapours and gases. Luminous gas flames, oil-lamp flames, and candle flames produce strongly-marked shadows in sunlight, and therefore contain finely-divided solid matter; and that this can be nothing but carbon is evident from the fact that all other substances capable of remaining solid at the temperature of the flames are absent.

From these considerations it is evident that Sir Humphry Davy's theory as propounded by himself is perfectly true as regards the ordinary illuminating flames.

In the second lecture, Prof. Lewes pointed out that, in the various analyses of illuminating gases, the heavy hydrocarbons are, as a rule, expressed as "illuminants," and were formerly considered to consist mainly of ethylene. This is an idea which the researches of the last few years have shown to be totally erroneous, as, besides ethylene, there is undoubtedly present benzene, propylene, butylene, and acetylene, and probably such members of the paraffin series as ethane, propane, and butane, while under certain circumstances, crotonylene, terene, allylene, and others, are present. The determination of the illuminants is therefore by no means the simple process one would imagine it to be from the directions given in most text-books on gas analysis.

The illuminants present in any given sample of coal gas depend upon (1) the kind of coal used, (2) the temperature at which it is distilled, and (3) the length of time the gas is in contact with the heated sides of the retort, as well as with the liquid products of the distillation.

Having dealt with this part of the subject at considerable length, Prof. Lewes went on to criticize the various methods of gas analysis, showing that all analyses of coal-gas have hitherto been founded on the idea that the "illuminants"—*i.e.* the heavy hydrocarbons responsible for the illuminating power—could be absorbed by fuming sulphuric acid, chlorine, or bromine. This, however, he said, is undoubtedly not the case. Mr. Wright has shown that, when coal gas has been treated with this acid, the residual gas still retains from 32 to 55 per cent. of its original luminosity; and although this may, to a certain extent, be owing to the methane, which at high temperature becomes slightly luminous, it is certain that a considerable percentage is due to the higher members of the paraffin series which are not absorbed by the acid, and which the methods of analysis usually employed utterly fail to detect. Indeed, given a gas containing any member of the paraffin series other than methane, the analytical results are not only incorrect, but misleading, as the percentages of hydrogen and methane present will be absolutely nullified by a very small quantity of the higher hydrocarbons.

All researches on the composition of coal gas point to the presence of ethane, and probably of higher members of the marsh gas series; while in carburetted gases they are undoubtedly present to a far higher extent. Ethane, propane, and butane have all been shown to be present in small quantities; and as ethane gives double, propane three times, and butane four times its own volume of carbon dioxide, it is evident that exploding with oxygen and taking the volume of carbon dioxide as representing marsh gas will undoubtedly give too high results with ordinary coal gas, while with a carburetted gas it will render the whole analysis useless. Moreover, the free oxygen is next absorbed, and the remainder taken as nitrogen; and the volume of gas after absorption by Nordhausen acid, less the marsh gas and nitrogen, is taken as representing the hydrogen in the gas. The result is that the hydrogen is always far too low, not only because the volume of marsh gas is too high, but because the residual nitrogen, having to bear the brunt of all the errors of analysis throughout some seven or eight absorptions, is also nearly always too high. These palpable errors in the quantity of marsh gas and hydrogen also render worthless the calculations of the carbon and hydrogen density of the gas, on which great stress has been laid by previous observers. On the whole, therefore, it is not to be wondered at that no relation has been discovered between the carbon and hydrogen density and the illuminating value of the coal gas.

The method of analysis which Prof. Lewes finds best is as follows:—

Two of Stead's apparatus are taken and placed with the entrance tubes end to end, and filled—one with distilled water saturated with air, and the other with clean pure mercury. The gas to be tested is collected in one of the Stead absorbing tubes, over water, so as to be saturated; it is then transferred over mercury in the eudiometer tube of the second apparatus, and measured and passed into sodic hydrate, in order to absorb the small trace of carbon dioxide to be found in the highly-purified London gas. When present in only small traces, the amount of carbon dioxide lost by water saturation cannot be detected. After the absorption of the carbon dioxide, the gas is run into the second apparatus, and the oxygen estimated by absorption with alkaline pyrogallate, which must be strong and fresh, containing about 25 grammes of pyrogallic acid dissolved in 50 grammes of sodic hydrate in 200 c.c. of water. It is absolutely essential that the solution should be fresh, as after some time it will evolve a considerable amount of carbon monoxide. The heavy hydrocarbons have now to be estimated; and inasmuch as benzene is one of the most valuable illuminants in the coal gas, it would be of great value if any absorbent could be found that would separate the benzene and ethylene series. Unfortunately this does not exist as far as is known; the usual absorbents having the following drawbacks:—(1) Nordhausen sulphuric acid, in which sulphur trioxide has been dissolved until it will solidify on cooling, absorbs both ethylene and benzene, and therefore cannot be used to separate them. (2) Fuming nitric acid is a good absorbent for both series. (3) Bromine water acts far more rapidly on ethylene than on benzene, but undoubtedly does absorb a considerable quantity of the latter if left long in contact with a mixture of the two. (4) None of the foregoing affect methane in diffused daylight. The nearest approximate result is obtained by treating the gas first with strong bromine water, but not leaving it too long in contact with it, and then removing bromine vapour over sodic hydrate—the absorption being taken as the ethylene series; while the benzene is absorbed by fuming nitric acid or saturated Nordhausen acid—acid fumes being removed in the sodic hydrate tube before measurement over water. After absorption with nitric acid gas, is run back into the eudiometer, and measured over water. It is then passed into an absorption tube filled with a fresh solution of ammoniacal cuprous chloride. This must not be used for more than six determinations of an ordinary coal gas containing (say) 3 to 6 per cent. of carbon monoxide, or 3 of a carburetted water gas, as, after much carbon monoxide has been absorbed, the solution has a tendency to again give up small quantities of the gas. The gas is now returned to the mercury eudiometer tube; and, after measurement, it is passed into an absorption tube containing ordinary paraffin oil (previously heated until everything that will distil at 100° C has gone off), which absorbs ethane, propane, butane, and a good deal of the methane. The residue is then washed and mixed with oxygen, which has itself been analysed, so that the percentage of nitrogen and foreign gases in it is known, and the mixture exploded over mercury. The carbon dioxide formed is estimated; and its volume *plus* the volume of gas absorbed by the paraffin gives the volume of gases in the methane series. A fresh portion of gas is now taken over mercury, and is exploded with excess of analyzed oxygen. The carbon dioxide is absorbed by sodic hydrate, and the oxygen by pyrogallate; and the residue will be the nitrogen—the hydrogen being determined by difference. In this way an analysis of South Metropolitan gas shows—

	Hydrogen		47·9	
Illuminants	Ethylene series	approx.	3·5	Total Hydrocarbons 45·6 per cent.
	Benzene series	approx.	0·9	
	Methane series	by paraffin	7·9	
		by explosion	33·3	
	Carbon monoxide		6·0	
	Carbon dioxide		0·0	
	Oxygen		0·5	
	Nitrogen		0·0	
			100·0	

In such an analysis, the lecturer remarked, no pretence was made that the exact percentage of each illuminant was given, but the total of the hydrocarbons is accurate; and their rough subdivision gave a far clearer insight into the characters of the gas than the more pretentious and more faulty analysis upon which it has been customary to argue. He said it must be clearly borne in mind that he only put forward this scheme of analysis to meet the need now rapidly arising for a method which would show whether ordinary coal gas enriched by cannel, coal gas carburetted with either gasoline or oil gas, or coal gas enriched by highly carburetted water gas, was being dealt with. In the first case, the ethylene and benzene series would be found well represented, while the carbon monoxide was low; in the second, the amount of hydrocarbons in the methane series would have become greater, and if oil gas had been used, a small increase in carbon monoxide might also be noticed; while the presence of carburetted water gas at once brought up the quantity of carbon monoxide, and the methane series became more important illuminants.

Prof. Lewes went on to show that the light-giving value of the hydrocarbons present in coal gas varies very greatly, the illuminating power increasing very rapidly with the number of carbon atoms in the molecule; and concluded by fully discussing the effect which the various diluents present in coal gas had upon its illuminating value.

(*To be continued.*)

ON THE ORBIT OF α VIRGINIS.

PROF. H. C. VOGEL has contributed a further discussion o the orbit of α Virginis (Spica) to the *Astronomische Nachrichten*, No. 2995. The following is a translation of the greater part of his paper:—

"After the periodical approach and retreat of this star, which was suspected from last year's observations, had been proved by some spectrographic observations made this year, an examination of the spectrum has been made at every favourable opportunity. So far, it has been possible to observe Spica on twenty-four evenings, thus affording material which allows its period to be determined with a greater degree of accuracy. Before communicating the results of the measurements of the photographs it will be necessary to preface a few remarks relative to the accuracy attainable.

"In No. 2896 of the *Astronomische Nachrichten* I detailed the first results which were obtained by means of the new spectrograph. The measurements were then made on some stars with spectra of the second class, and the increased accuracy given to these observations by the fact that the measurements were not made on the Hγ line only, but on some exceedingly sharp lines in the neighbourhood, has been fully confirmed by the now completed measurements of all the stars of the second and third class accessible to the Potsdam instruments.

"In the case of stars of class I.*a*, however, in which the Hγ line is more or less broad and fuzzy at the edges, and there are no other lines near it, greater difficulties than I at first expected opposed themselves to the satisfactorily exact measurement of their spectra. At one time I had the intention of making the measurements, not on the hydrogen line, but on the better defined lines of another metal, such as magnesium, which possesses a strong line at 448 μμ. I abandoned this intention, however, because by adopting this means a number of stars, in whose spectra the said line of magnesium is weak or invisible, must have been excluded.

"The absorption lines of hydrogen in star spectra show by their appearance the following four types:—

"(*a*) Dark lines, with somewhat undefined edges, *e.g.* Capella.

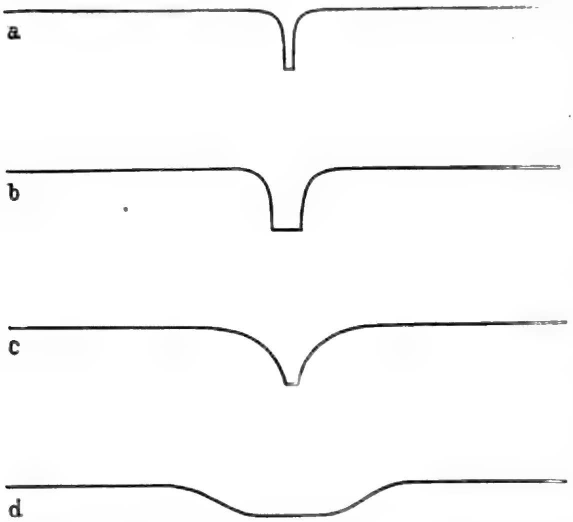
"(*b*) Broad dark lines, with somewhat undefined edges, *e.g.* Rigel.

"(*c*) Broad bands, gradually fading away, but with a more or less broad and well-defined maximum of intensity in the centre, *e.g.* Sirius.

"(*d*) Broad bands, undefined at the edges, without any remarkable maximum of intensity in the centre (Spica).

"The four types are graphically represented by the curves in the accompanying figure."

Prof. Vogel notes that when the maximum of intensity of the form (*c*) lies outside the line of comparison, the measurement of the distance between the two is attended with no special difficulties. When, however, the comparison line overlaps the line in the star spectrum under examination, or in the case of spectra of the form (*d*), special devices have to be used to determine the distance from the centre of the absorption line to the line of comparison, due to the star's motion in the line of sight. These methods



Diagrammatic representation of the appearance of hydrogen absorption-lines in four types of stellar spectra.

are described in the paper. The values obtained from measurements of photographs of the spectrum of Spica are shown in the following table :—

No.	Potsdam Mean Time. 1889.	d.	h.	Observed Motion (Mean).	Reduction to the Sun.	Star minus Sun.
1.	April	21	9.25	−11·9	−0·6	−12·5
2.	,,	29	11.17	−12·7	−1·1	−13·8
3.	May	1	10.97	+10·3	−1·3	+ 9·0
	1890.					
4.	April	4	11.50	− 4·0	+0·6	− 3·4
5.	,,	9	10.50	−14·2	+0·2	−14·0
6.	,,	10	11.50	− 0·9	+0·2	− 0·7
7.	,,	11	10.83	+ 8·9	+0·1	+ 9·0
8.	,,	13	10.83	−14·4	−0·1	−14·5
9.	,,	15	11.00	+11·8	−0·2	+11·6
10.	May	1	10.25	+10·8	−1·2	+ 9·6
11.	,,	4	10.00	− 1·4	−1·4	− 2·8
12.	,,	7	9.42	−13·7	−1·6	−15·3
13.	,,	8	10.43	− 0·4	−1·7	− 2·1
14.	,,	9	10.08	+13·4	−1·7	+11·7
15.	,,	17	10.57	+13·6	−2·2	+11·4
16.	,,	18	10.08	+ 1·5	−2·2	− 0·7
17.	,,	23	10.50	−13·7	−2·5	−16·2
18.	,,	24	10.67	+ 0·3	−2·6	− 2·3
19.	,,	25	10.12	+12·1	−2·6	+ 9·5
20.	,,	26	10.72	+ 1·8	−2·7	− 0·9
21.	,,	27	10.18	−10·7	−2·7	−13·4
22.	,,	28	10.11	− 2·0	−2·8	− 4·8
23.	,,	31	10.29	−11·3	−2·9	−14·2
24.	June	4	10.27	− 9·7	−3·1	−12·8

(+ = Recession ; − = Approach.)

With regard to these results, Prof. Vogel remarks :—

"If an early circular orbit be accepted, the observations here described allow of its possessing the form and period given by me in my first communication on α Virginis (*Sitzungsber. d. Akad. d. Wissensch. zu Berlin*, April 24, 1890), and it is shown that the period of revolution of 4·011 days is a safe one for a year's time, for the observations of this year are not in accordance with the epochs computed from the periods 3·967 and 4·055 days. Only a slight increase of the first period, 4·011 days, is indicated by the better reduction of last year's observations. The following elements have been deduced from the collected material :—

"Epoch t_0 (when the total orbital motion of the two components in the line of sight = 0) = 1890 May 4d. 10.50h. Potsdam mean time.

"Period p = 4·0134 days.

"Motion of the star system = − 2·0 geographical miles.

"Velocity in the line of sight, when the translation of the system has been deducted, = $12{\cdot}3 \sin\left(\frac{t-t_0}{p} 360°\right)$ geographical miles.

"The difference of four miles between the mean maximum positive and negative velocities might be explained by the hypothesis that the orbit of the star deviates considerably from a circle, and that the major axis of the ellipse is almost perpendicular to the line of sight. The observations, however, are not sufficient to decide this point, and as the motion of the system of − 2·0 miles (obtained by taking a circular orbit) corresponds to the average movement arrived at from the Potsdam observations, no foundation is afforded for any other hypothesis. . . . In consequence of my first communication on the orbit of α Virginis, Prof. Bakhuyzen has examined whether the Greenwich observations, notwithstanding their very slight accuracy, would afford data for a somewhat more exact determination of the period, as they extend over several years; and he has arrived at a period of 4 days 0·357 hours. From further communications I have learned from Prof. Bakhuyzen that his determination only rested on the comparison of some observations made at Greenwich in the years 1883 and 1886 with the Potsdam observations for the same years, but that no comparison of all the observations had been made. A somewhat more exact repetition of the calculation with respect to the times of observation, has, however, shown that, in consequence of the unaccustomed reckoning of time from midnight to midnight, which has been in use at Greenwich since 1885, Prof. Bakhuyzen had counted an observation as having been made on May 4, 1886, whereas according to astronomical reckoning it was made on May 3. When this oversight is corrected, the period obtained was 4 days 0·324 hours. I mention this, because Mr. Christie, in his annual report for 1890, has referred to the above-named period of 4 days 0·357 hours.

"I have examined the Greenwich observations somewhat carefully, and have come to the conclusion that they cannot be looked upon as sufficient to contradict the results of the Potsdam observations, and that no correction to the discovered period can be deduced from them with any certainty."

After a statement of the results of twenty-six observations of α Virginis made at Greenwich between 1876 and 1889, for motion in the line of sight, Prof. Vogel remarks :—

"If, taking twenty-six observations of α Virginis, the average result be counted, and no orbit introduced, it amounts to ±5·8 geographical miles. In the case of α Leonis it is ±4·4, of α Ophiuchi ± 6·7, and of α Aquilæ ± 6·5 (putting on one side the observations of 1874, 1875, and 1876, which give the value of ± 4·5 miles). Hence it is seen that, according to the Greenwich observations, there is no more cause for suspecting a periodical change of motion for α Virginis than for the other stars examined.

"If values are computed back to 1886 from the elements of the orbit previously given, a minimum of motion in the line of sight is obtained for 1886 April 30, 13·12 hours, and a maximum of negative motion on May 3, 13·36 hours, Potsdam time. These results agree very well with the Greenwich observations of those days. From the interval of time between the two minima in 1886 and in 1890, periods of 3·970 days and 4·058 days were obtained. With these two periods, and that best represented by the Potsdam observations, viz. 4·0134 days, I have computed some of the Greenwich observations according to the formula $12{\cdot}3 \sin \frac{t - t_0}{p} 360°$, after the translation of the system deduced from the Potsdam observations had been subtracted. It did not seem allowable to carry the reckoning any further back, as even the period of 4·0134 days may be doubtful in the third decimal place."

A comparison of nine observations made between April 1886 and May 1889 with calculated motions based on the assumed period of 3·970 days, gave a residual of ± 4·4 ; with the period 4·0134 days it amounts to ± 6·6 ; and with the period 4·058 days reaches ± 8·5. If the star be considered to have no orbital motion, the residual error is ± 7·2 miles. It appears, therefore, that the Greenwich observations of 1886 to 1889 are best represented by the period 3·970 days. Prof. Vogel then goes on to say :—

"I repeat, with the new values, the calculation of the extent of the orbit and the mass of the stars, which I gave in my first communication on this subject. Upon the supposition of a circular orbit, the resulting period is 4·0134 days, while the two components have equal masses and a velocity of 12·3 miles per second. The mass of the system is 2·6 that of the sun, and the distance of each component from the common centre of gravity is 679,000 geographical miles. With a parallax of 0″·2 the maximum apparent distance amounts to 0″·014, so that the satellite cannot be seen with the strongest instrument.

"Finally, I must not omit to say that, by means of repeated examinations of the photographic plates and measurements of the spectrum, it appears more and more certain that the satellite of α Virginis has made itself apparent on the impressions. On several plates, taken at the time of maximum movement, one edge of the Hγ line seems to be rather more undefined and more gradually to diminish in intensity than the other edge; also on some other plates, taken at the time of minimum motion, the Hγ line seems to be rather narrower. These plates also show other lines in the spectrum more plainly on account of their being less faint. These appearances go to show that the satellite has a similar spectrum to the primary star, and that the Hγ line is also broad and faint, but so faint in comparison to the line in the spectrum of the primary star that its presence is only suspected after most careful examinations of the photographs. The phenomenon observed has no influence on the measurements. If, however, the spectrum of the satellite were stronger, it might exercise an influence on the measurements of the maximum motion by causing the differences to measure somewhat less than they really are.

"The satellite, if we grant that it really exists, is probably of about the third magnitude, and it may be possible that powerful reflectors may demonstrate its presence by showing more distinctly the slight periodic changes in the composite spectrum."

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 11, 1890.—"Photometric Observations of the Sun and Sky." By William Brennand. Communicated by C. B. Clarke, F.R.S.

The paper begins with a short account of the various papers communicated by Sir H. Roscoe, and published in the Transactions of the Royal Society.

My observations were made at Dacca, East Bengal, in 1861–66, repeated at Milverton, in Somersetshire, during the last two years. My first experiments were directed to ascertaining the action of the sun on sensitized paper exposed at right angles to the solar rays for different altitudes of the sun, and largely to ascertaining the laws of distribution of the actinic power in the sky.

I take no observations except when the sky is quite clear.

The method of measurement I adopted is the darkening produced in sensitized paper. I cut strips from one uniform sheet of ordinary photographic paper. My observations being relative, I obtain the same results (ratios) with any paper. I compare ultimately the effects of the sun and of a candle on this same paper.

I assume that, in burning a stearine candle, the chemical action is proportional to the material consumed; I have taken as my unit (i) of measure of chemical action the darkening produced at a distance of 1 inch from the wick of the candle when 100 grains were consumed, which in the candle I used in India occupied about forty-seven minutes. My observations, being almost entirely relative, are independent of these assumptions, which affect hardly any of my results except comparisons with the absolute unit measures of Sir H. Roscoe.

The water-motion actinometer, with which observations of the action of sun and sky were made, is the instrument depicted in the photograph (see figure).

[The water-motion actinometer was shown and explained. By it, a shutter is removed with uniform velocity from a strip of sensitized paper, which is thus, by any light to which it is exposed, tinged with a gradually intenser depth from 0 to 16 seconds (the paper being suddenly covered at the end of 16 seconds). If, for example, at a particular altitude of the sun, the action of the sky alone in 6 seconds produced the same depth of tint that the action of the sun alone produced in 10 seconds (in a similar strip cut from the same paper), then I estimate the chemical effect of the sun to have been $\frac{6}{10}$ that of the sky, *i.e.* it would be 0·6 if the effect of the sky was taken as the unit of measurement. In the same unit, the effect of the sun and sky together would be 1·6; that is, it would require $3\frac{3}{4}$ seconds' exposure to sun and sky together to produce the unit tint of measurement in the same paper, which calculated value could be immediately verified by another strip cut from the same paper. I have repeatedly made such verifications, which give me a test of the degree of accuracy with which tints can be matched.

By comparing a strip thus gradually shaded in proportion to the time, with another strip shaded as the inverse square of the distance from a near source of light, it is readily shown that the effect of a light acting at the distance unity for four seconds is equal to that of the same light acting at the distance $\frac{1}{2}$ for one second.

Observations of these ratios can be made with any *uniform* piece of paper from which the strips are cut. In the early experiments at Dacca I prepared sheets of sensitized paper with great care. I have found, in later experiments at Dacca and in Somersetshire, that any sheet of fairly good photographic paper gives satisfactory results; *i.e.* the error introduced by

Water-motion actinometer.

want of uniformity in one sheet of paper is inappreciable in comparison with the margin of possible error in the "reading," *i.e.* the matching of tints.

I originally took as my unit of absolute measurement a particular tint produced in a piece of particular paper by a Dacca candle. I found that at an altitude α of the sun this tint was produced (by the effect of the sun alone) in the same paper in (say) 16 seconds, *i.e.* the effect of the sun alone at altitude α was represented by the number $\frac{1}{16} = 0·0625$. By the system of ratios, I found the corresponding numbers for all altitudes 10° to 45°. These ratios were the means of very numerous observations extending over several cold seasons at Dacca where I had a perfect sky for months. If at any time I wish to recover the Dacca unit of measurement *in any particular paper*, I take a sun strip (at any altitude of the sun, γ) in the water-motion actinometer; I see in my table (B) that for the altitude γ the effect of the sun alone is (say) 0·12; I make a mark on my sun strip at the point where it has been exposed $8\frac{1}{3}$ seconds; the effect at that point in that paper is my Dacca unit.

My English observations have been taken in this manner without any reference to a candle. The observations of Sir H

Roscoe, apart from my own very numerous observations, show the chemical effect of the sun to be always the same (in a perfectly clear sky) at the same altitude.]

For obtaining the effects of the sun and sky, I have always experimented mainly by exposing the paper at right angles to the sun's rays. Sir H. Roscoe, on the other hand, exposes his paper on a horizontal plane. Theoretic considerations have led me to another method of observation (with the "octant" actinometer below) which gives directly the measure of the effect really desired.

A table is given of the first observations I made, which afterwards led to the formation of Table B.

TABLE B.—*Chemical Action of the Sun and Sky.*

Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.	Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.
1	·001	·003	·004	31	·110	·064	·174
2	·002	·005	·006	32	·113	·064	·178
3	·003	·007	·010	33	·116	·065	·181
4	·004	·010	·014	34	·118	·065	·184
5	·006	·012	·019	35	·121	·066	·188
6	·009	·016	·025	36	·124	·066	·190
7	·012	·019	·031	37	·126	·067	·193
8	·016	·022	·038	38	·129	·067	·196
9	·020	·026	·045	39	·131	·068	·199
10	·024	·029	·052	40	·133	·068	·201
11	·028	·032	·060	41	·135	·069	·204
12	·033	·035	·068	42	·137	·069	·206
13	·038	·038	·075	43	·138	·069	·208
14	·043	·040	·082	44	·141	·069	·210
15	·047	·042	·090	45	·143	·070	·213
16	·052	·045	·097				
17	·057	·048	·105				
18	·061	·049	·110				
19	·066	·050	·116				
20	·070	·052	·122	50	·150	·071	·221
21	·075	·053	·128	55	·157	·072	·229
22	·079	·054	·134	60	·162	·073	·235
23	·083	·056	·139	65	·166	·073	·239
24	·086	·057	·144	70	·170	·073	·243
25	·091	·058	·149	75	·172	·074	·246
26	·094	·059	·153	80	·173	·074	·248
27	·097	·060	·158	85	·175	·074	·249
28	·101	·061	·162	90	·175	·074	·249
29	·104	·062	·166				
30	·107	·063	·170				

N.B.—For sun altitudes 50° to 90°, the figures are not the result of direct observations; for sun altitudes 1° to 10°, the figures are less certain by reason of thin haze often present.

The numbers of the table were obtained, by taking the inverse of the times required at each altitude for producing the darkening of the candle unit.

I found the chemical action of the sun, as far as my experiments went, the same at all hours of the day and at all seasons of the year. And in Somersetshire I got exactly the same chemical action of the sun as at Dacca. Observations near the horizon cannot be depended upon.

Various observations had led me to suspect that the chemical action of the sky at the same moment was different in different parts of it. To investigate this suspicion, I designed an instrument which I call the mitrailleuse actinometer. I mount a number of similar cylindrical tubes in one plane in a semicircle, to the centre of which the axis of each tube is directed: one extremity of each tube lies in the circumference of the circle; the other extremities lie on a concentric circle of about one-half the radius. In the circumference of this smaller circle is a semicircular series of holes, against which a semicircular block carrying the sensitized paper is pressed by a screw. Each cylinder cuts out of the sky a circle of 8° 28′ angular diameter. One of the tubes near its top carries a small plate of wood on which stands a stile parallel to the tube, by means of which this particular tube can be brought in a line with the sun. By another motion the plane of the tubes can be adjusted to the plane of symmetry (or elsewhere).

[A vertical plane through the sun at any time divides the visible sky into two exactly similar portions. I call this the plane of symmetry.]

The observations (Table C) were taken December 23, 1864, at Dacca (among other similar observations taken in the same cold weather) in the plane of symmetry. The barrels of the mitrailleuse were fixed 10° apart, the altitude of the sun being 42° 28′.

I give the table as an early observation that shows well that there is a point of minimum sky intensity at 90° from the sun. It also appears that if i_α be the intensity for the altitude α of the sun (= 0·12), then the intensity of the sky at a point $\theta°$ from the sun is given (roughly only according to this table) by the formula

$$i_\alpha \operatorname{cosec} \theta.$$

This observation was made in the plane of symmetry: it turns out that the value, $i_\alpha \operatorname{cosec} \theta$, gives the intensity very accurately, for any point, in any other *great* circle, whose distance from the sun is $\theta°$ measured on that circle.

For any altitude of the sun (α), the chemical action of the sky is a minimum at all points in a great circle the plane of which is at right angles to the line joining its centre to the sun.

[This plane I call the plane of minimum intensity (i_α).]

As the whole of the mathematical developments of this paper are founded upon the law that at any point of the sky whose distance is $\theta°$ from the sun

$$\text{the intensity} = i_\alpha \operatorname{cosec} \theta,$$

I have been careful to verify it by numerous observations both at Dacca and in Somersetshire, and also to vary the observations in every way I could devise. Thus the mitrailleuse has been placed in the plane of minimum intensity. In this case all the barrels give the same reading for points not too near the horizon.

Next the mitrailleuse was placed in planes of great circles through the sun at various angles with the plane of symmetry; by turning it round the line joining one of its tubes with the sun the observed chemical actions agree well with

$$i_\alpha \operatorname{cosec} \theta.$$

Next by means of stops I made the aperture of each barrel of the mitrailleuse to be

$$c \sin \theta,$$

where $\theta°$ is the distance of the axis of the barrel from the sun; this mitrailleuse being exposed, the barrel $c \sin 0$ being directed to the sun, the circular darkened spots were found to be very accurately of the same depth.

Further, I calculated the *times* of exposure for a (particular) mitrailleuse with barrels of uniform aperture, which ought, on the law $i_\alpha \operatorname{cosec} \theta$, to give a uniform tint. I exposed this mitrailleuse for these calculated times, first in the plane of symmetry, afterwards in a plane inclined to it at 62°; the results agreed closely with my anticipation, and show $i_\alpha \operatorname{cosec} \theta$ to be a very good approximation.

I have therefore made full use of the expression $i_\alpha \operatorname{cosec} \theta$ for the chemical action of the light of the sky in a circle θ from the sun (whose altitude is α).

I calculate (having given me i_α, the chemical action in the circle of minimum intensity) the total chemical action of the sky, first, on a plane exposed at right angles to the sun; second, on the horizontal plane. The first is an elliptic integral, the second is $2i_\alpha(\pi \sin \alpha + 2 \cos \alpha)$. By these values I am able to compare some of Sir H. Roscoe's observed values with my own.

The mathematical processes in reducing these integrals suggested to me (within the last few months) the construction of the octant actinometer, a new instrument which I tried in Somersetshire in October last. I am fairly satisfied with the results considering the imperfect sky of England. This instrument measures the value i_α directly; and it possesses, moreover, the great advantage of not taking in the low band of sky near the horizon, and thus avoids a principal element of uncertainty in other observations.

Geological Society, December 17, 1890.—Mr. W. H. Hudleston, F.R.S., Vice-President, in the chair.—The following communications were read:—On nepheline rocks in Brazil: ii. the Tingua mass, by Mr. O. A. Derby. In a former paper the general distribution of the nepheline rocks, so far as known, was given, with a particular description of a single one, the Serra de Pocos de Caldas. The present paper treats of a second mass, the Serra de Tingua, a high peak of the Serra do Mar, some forty miles from Rio de Janeiro. This peak is essentially a mass of foyaite rising to an elevation of 1600 metres, on the crest and close to the extremity of a narrow gneiss ridge of a very uniform elevation of about 800 metres. As seen from a distance, the conical outline and a crater-like valley on one side are very suggestive of volcanic topography. In the structure of the mass both massive and fragmental eruptives are found, the former greatly predominating. The predominant rock is a coarse-grained foyaite which is found everywhere in loose blocks about the margins of the mass, but not extending beyond it. In the numerous cuttings in the immediate vicinity, dykes of phonolite and basic eruptives (augitite) are exceedingly abundant, foyaite never appearing in a dyke form. There is, however, abundant evidence that foyaite and phonolite are but different phases of the same magma. Aside from the dyke phonolites, true effusive phonolites associated with fragmental eruptives (tuffa) were found high up in the crater-like valley, proving that the mass was a volcanic centre in the most restricted sense of the word. This conclusion affords an explanation of some of the peculiarities of the foyaite, which has many characteristics of effusive eruptives mingled with those of the deep-seated ones (*Teifengesteine*). These have, aside from the porphyritic structure, a *schlieren* structure revealed by a peculiar fluted weathering (illustrated by a photograph) and the presence of pseudo-crystals in the form of leucite. Stratigraphically the Tingua foyaites lie in sheet-like masses like lava-flows, extending from the higher to the lower portions of the mountain, the underlying gneiss being revealed at nearly all levels, wherever the mass has been scored by streams. The general fragmentary character of the rock seems to be due to the undermining of these sheets. Specimens and photographs illustrating the peculiar pseudo-crystals in the form of leucite that occur in both the foyaites and phonolites of Tingua (although no leucite has been detected in the rock) were exhibited and discussed. After the reading of this paper there was a discussion, in which the Chairman, Mr. Bauerman, Mr. Hulke, Prof. Green, and the author took part.—The variolitic diabase of the Fichtelgebirge, by Mr. J. Walter Gregory.

EDINBURGH.

Royal Society, December 15, 1890.—Prof. Crum Brown in the chair.—Prof. Tait communicated a paper, by Dr. E. Sang, on the extension of Brouncker's method to the comparison of several magnitudes. The method employed is essentially an application of continued fractions.—Prof. Tait also communicated a paper by Mr. A. M'Aulay, on proposed extensions of quaternion powers of differentiation. In this paper, Mr. M'Aulay discusses a proposed modification of quaternion notation which leads directly to remarkable extensions in the mathematical treatment of the theories of elasticity and of electricity. His object in communicating this paper to the Society is to obtain information as to the likelihood of his modified notation—which is entirely opposed to ordinary conventions—being accepted by scientific men.—Prof. Ewing exhibited a model illustrating a molecular theory of magnetism. The model consists of a number of pivoted magnets which are arranged in parallel rows. These magnets are placed in the interior of a rectangular coil of copper wire, around which an electric current can be made to pass. So long as no current passes around the wire, the magnets arrange themselves in positions of stable equilibrium under their mutual forces, some of them pointing in one direction, some in another. This illustrates the condition of non-magnetized steel. If a feeble current be now passed round the coil, each magnet is slightly turned from its first position, which, however, it reassumes when the current is stopped. This illustrates the first stage of the process of magnetization. A somewhat stronger current causes instability among the originally less stable groups of the magnets, so that the magnets composing these groups swing round into a new stable position. As the strength of the current increases still further, more and more groups break up, until all have taken the new position of equilibrium under their own mutual forces and the external directive force. This illustrates the second stage of magnetization, in which the ratio of magnetization to magnetizing force increases with great rapidity. The third stage, in which the above ratio is practically constant, is exemplified by the fact that an infinite force is now needed to make the magnets point exactly in the direction of the external lines of force. If the current be now stopped, a considerable proportion of the magnets retain their final position of equilibrium —in other words, magnetic retentiveness is exhibited. The model may be made to exhibit the effects of strain on the magnetic properties. For this purpose the magnets are placed on a sheet of india-rubber. If the india-rubber is stretched, the magnets are separated out from each other in one direction, and are brought nearer to each other in a direction at right angles to the former. The magnetic susceptibility is increased, or diminished, according as the stability of the magnets is diminished, or increased, by the alteration of relative position. Similarly, the increase of the susceptibility of iron with rise of temperature is explained by the diminution of mutual magnetic influence which results from increased distance. Prof. Ewing suggests that the total loss of magnetization which occurs at a high temperature is due to continuous whirling motion of the magnetic molecules. The dissipation of energy which occurs when hysteresis is exhibited is due to the induced currents which are caused by angular motions of the magnets.—Dr. Gulland read a paper on the development of adenoid tissue. He believes that ingrowth of the epithelium (for example, in the development of the tonsils) compresses the connective tissue, and renders difficult the passage of leucocysts. The leucocysts consequently increase in numbers at the part, and eat their way through the condensed tissue.

SYDNEY.

Royal Society of New South Wales, October 1, 1890.—Dr. Leibius, President, in the chair.—A discussion took place upon the paper read at the September meeting by Prof. Warren, on some applications of the results of testing Australian timbers to the design and construction of timber structures.

November 5.—Dr. Leibius, President, in the chair.—The following papers were read:—Geological notes on the Barrier Ranges silver-field, by C. W. Marsh.—Record of hitherto undescribed plants from Arnheim's Land, by Baron Ferd. von Mueller, K.C.M.G., F.R.S.—Some folk songs and myths from Samoa, translated by Rev. T. Powell and Rev. G. Pratt, with an introduction and notes by Dr. John Fraser.—Mr. H. C. Russell, C.M.G., F.R.S., exhibited and described some of the surprising star photographs recently taken at Sydney Observatory.

PARIS.

Academy of Sciences, December 29, 1890.—M. Hermite in the chair.—List of prizes awarded to successful competitors in 1890:—*Geometry*: Grand Prix des Sciences Mathématiques, M. Paul Painlevé; honourable mention, M. Léon Autonne; Prix Bordin (not awarded); Prix Francœur, M. Maximilien Marie; Prix Poncelet, M. le général Ibañez. *Mechanics*: Extraordinary Prize of 6000 francs—this has been divided between M. Madamet, MM. Ledieu and Cadiat, and M. Louis Favé; Prix Montyon, M. le colonel Locher; Prix Plumey, M. Jules-Ernest Boulogne. *Astronomy*: Prix Lalande, M. J. V. Schiaparelli; Prix Damoiseau (not awarded); Prix Valz, Prof. S. de Glasenapp; Prix Janssen, Prof. C. A. Young. *Statistics*: Prix Montyon, Dr. Paul Topinard; honourable mention, M. Dislère. *Chemistry*: Prix Jecker, divided between the late M. Isambert and M. Maurice Hanriot. *Geology*: Prix Vaillant, M. Marcel Bertrand; Prix Fontannes, M. Ch. Depéret. *Physical Geography*: Prix Gay, M. Franz Schrader. *Botany*: Prix Desmazières, M. Maurice Gomont; Prix Montagne, divided between M. Paul Hariot and Dr. Albert Billet. *Anatomy and Zoology*: Prix Bordin (not awarded); Prix Savigny, divided between Dr. Jousseaume and M. R. P. Camboué; Prix Thore (not awarded); Prix Serres, M. Camille Dareste. *Medicine and Surgery*: Prix Montyon, divided between M. Félix Guyon, M. Auguste Ollivier and M. Paul Richer; mentions were accorded to M. Ch. Fiessinger, MM. J. Chauvel and H. Nimier, and M. Ch. Mauriac; Prix Bréant, divided between M. G. Colin and M. A. Layet; Prix Godard, M. Samuel Pozzi; honourable mention, MM. Ch. Monod and O. Terrillon; Prix Barbier, M. Claude Martin; honourable mention, M. Gaston

Lyon and M. B. Dupuy; Prix Lallemand, divided between Mme. Déjerine-Klumpke and M. G. Guinon; Prix Dusgate (not awarded); Prix Bellion (not awarded); Prix Mège, M. Nicaise. *Physiology:* Prix Montyon, divided equally between M. E. Gley and M. E. Wertheimer; honourable mention, M. E. A. Alix and MM. G. Arthaud and L. Butte; Prix Pourat (not awarded). *General Prizes:* Prix Montyon (Unhealthy Industries), M. Casimir Tollet; Prix Jérome Ponti, M. R. P. Colin; Prix Trémont, M. Beau de Rochas; Prix Gegner, M. Paul Serret; Prix Delalande-Guérineau, Dr. Verneau; Prix de la fondation Leconte, M. Prosper de Lafitte; Prix Laplace, M. Bailly.—The following prizes were proposed for the year 1891:—Prix Francœur: For discoveries or useful works tending to further the progress of pure and applied mathematics. Prix Poncelet: For the author of any work tending most to further the progress of pure and applied mathematics. Extraordinary Prize of 6000 francs: Any improvements tending to increase the efficiency of the French naval forces. Prix Montyon: Mechanics. Prix Plumey: Improvement of steam-engines or any other invention contributing most to the progress of steam navigation. Prix Dalmont: For the engineer who shall present to the Academy the best work on bridges or on highways. Prix Fourneyron: Improvements in the theory of steam-engines which take most account of the exchanges of heat between the water and the cylinders and tubes. Prix Lalande: Astronomy. Prix Damoiseau: Improvements of the lunar theory which consider inequalities of long period caused by planets. Prix Valz: Astronomy. Prix Janssen: Astronomical physics. Prix Montyon: Statistics. Prix L. La Caze: The best work on physics, chemistry, and physiology. Prix Jecker: Inorganic chemistry. Prix Delesse: The author of the best work on geological science, or, in default of such, mineralogical science. Prix Bordin: The study of the phenomena of the fecundation of Phanerogamic plants, with particular reference to the division and transport of the cellular nucleus; also the study of the connections which exist between these phenomena and those observed in the animal kingdom. Prix Bordin: Comparative study of the auditory nerves of warm-blooded Vertebrata; Mammiferæ and Birds. Prix Desmazières: The best work on the whole or any part of Cryptogamic flora. Prix Montagne: The author of important works on the anatomy, physiology, development, or description of the lower Cryptogamic plants. Prix Thore: Works on the cellular Cryptogams of Europe, and on the habits or anatomy of any species of European insect, alternately. Grand Prix des Sciences Physiques: On the organs of sense of Invertebrata, from an anatomical and physiological point of view. The prize may be awarded for a complete work on one of the organs of sense in a group of Invertebrata. Prix Savigny: For young zoological travellers. Prix da Gama Machado: On the coloured parts of the tegumentary system of animals, or on the genital matter of living beings. Prix Montyon: Medicine and surgery. Prix Bréant: The discovery of a cure for Asiatic cholera. Prix Godard: On the anatomy, physiology, and pathology of genito-urinary organs. Prix Chaussier: Important works in legal or practical medicine. Prix Barbier: The most important discovery in surgery, medicine, pharmacy, and botany, having reference to the healing art. Prix Lallemand: Researches on the nervous system in the widest sense of the term. Prix Bellion: Works or discoveries serviceable to the health or to the improvement of the human species. Prix Mège: The author of a continuation and completion of Dr. Mège's essay on the causes that have retarded or favoured the advancement of medicine. Prix Montyon: Experimental physiology. Prix Pourat: Functions of thyroid bodies. Prix Martin-Damourette: Therapeutic physiology. Prix Gay: Newly formed lakes and how they become stocked. Prix Montyon: Unhealthy industries. Prix Cuvier: The most remarkable work on the animal creation, or on geology. Prix Trémont: For any naturalist, artist, or mechanic, needing help for carrying out any project useful and glorious for France. Prix Gegner: In aid of any man of science distinguished for his works towards the advancement of the positive sciences. Prix Jean Reynaud: For the most meritorious work produced in a period of five years. Prix Petit D'Ormoy: Pure and applied mathematics, or applied and natural science. Prix Laplace: For the best student leaving the École Polytechnique.

BRUSSELS.

Academy of Sciences, November 8.—M. Stas in the chair.—On the variations in latitude observed at Berlin, Potsdam, and Prague, by M. F. Folie. The periodical changes in the declination of stars, due to aberration, is adduced as a possible cause of latitude variation.—On the Belgium graptolites, by Prof. C. Malaise.—On the points of variation in cubes, by M. Cl. Servais.—On dypnone, $\begin{matrix} C_6H_5 \\ CH_3 \end{matrix}\!\!>\!C = CH . CO . C_6H_5$, by M. Maurice Delacre.—On the new species *Posadæa*, belonging to the family of Cucurbitaceæ, by M. Alfred Cogniaux.—The reduction of nitrates to nitrites by seeds and tubercles, by M. Émile Laurent. The author makes known the results of his new researches on the reduction of nitrates. He has experimented on Indian corn, barley, peas, white lupines, broad beans and kidney beans, previously sterilized, but afterwards allowed to germinate. The crops obtained are immersed in a 1 per cent. solution of potassium nitrate, and give rise to the production of potassium nitrite. With regard to this action it is concluded that "La réduction des nitrates en nitrites par les végétaux est, comme la fermentation alcoolique, une conséquence de la vie qui se continue dans un milieu privé d'oxygène libre." This reduction of nitrates by the tubercles of the Jerusalem artichoke, radish, turnip, &c., by their petioles, and by the peduncles and fruits of different plants, has also been observed.

STOCKHOLM.

Royal Academy of Sciences, December 10, 1890.—The third and fourth part of "Erythrææ exsiccatæ" exhibited and commented on by Prof. Wittrock.—On the genera Kurutas, Nidularium, and Regelia, of the family of the Bromeliaceæ, by Dr. C. Lindman.—Researches on the structure of the central nervous system of the Evertebrates and especially of the Crustacea, and in general on the connection between the nervous cells and nervous filaments in the nervous central organs, by Prof. G. Retzius.—Contributions to the question of secular perturbations, by Dr. K. Bohlin.—The influence of the temperature on the capillary constants of some fluids, by Herr Tinnberg.—On the reaction of the iodohydric acid on 1–6 nitro-naphthalin-sulphon-acid-amid, by Herr A. Ekbom.—"Sur la notion de l'énergie libre," by Herr A. Rosén.

CONTENTS. PAGE

THURSDAY, JANUARY 15, 1891.

THE INTERNATIONAL CONGRESS OF HYGIENE AND DEMOGRAPHY.

INTERNATIONAL Congresses on Hygiene have been held at about two years' interval in various capitals of Europe since the year 1877. The first was held at Brussels under the especial auspices of the King of the Belgians, and was accompanied by an Exhibition of Sanitary Appliances. After the second of these Congresses, it was decided to associate with the Hygienic Congress one on the cognate subject of demography, which may be defined as the science of statistics applied to the social well-being of the people. The Congress which will assemble in London in August next will be the seventh on hygiene, and the fifth on demography. The last Congress was held at Vienna under the auspices of the Crown Prince Rudolf in 1887. It was then settled that the next Congress was to be held in London, and the year 1891 was selected; because the organizers of the French Exhibition had already announced Congresses on cognate subjects to be held in 1889 in Paris.

The object of these Congresses is to promote the interchange of views between those persons in various countries who have studied the subjects of hygiene and demography, especially with respect to their bearing upon the welfare of the people, as well as upon the intercourse between nations. So far as regards hygiene, in these days of rapid transit, the sanitary condition of any one nation is more than ever a matter of concern to its neighbours, and assemblies of delegates of different nationalities for the discussion of hygienic problems, and the comparison of hygienic methods, are of great importance. For instance, among the most prominent of the subjects which have been treated at these Congresses, has been that of quarantine. England has for some time maintained the view that the efficient sanitation of a country, and especially of seaport towns, is a better safeguard against the importation of cholera than any measures of quarantine. No doubt England has been somewhat at a disadvantage in maintaining its argument, because, whilst it has attended carefully to the sanitation of this country, it has allowed the sanitation of India, which is the birthplace of cholera, to remain in a disgraceful condition. Yet for all that, whilst at the earlier Congresses the views of England upon this important question were entirely set aside, at the last Congress the discussions which had arisen at previous Congresses had so permeated the minds of students of hygiene on the Continent, that it was generally conceded at the Vienna Congress that the enforcement of the laws of health amongst the population of a country was a far more effective measure for preventing the spread of cholera in the country than any measures of quarantine. We may therefore hope that the discussions which will take place at the Congress in London will still further awaken Continental nations to the advantages of sanitation in contradistinction to the absurdities of quarantine; and will also be a means of compelling our Indian authorities to bestir themselves to remove the stigma which now attaches to India of affording a prominent instance of defective sanitation.

In addition to this, numerous social questions intermixed with hygiene now press for a solution, which can be arrived at only after discussion. The term demography is new in this country. Dr. Mouat, the eminent President of the Statistical Society, has, in his late very able address to the Society, given an interesting explanation of the origin of the term. The following extract may advantageously be given here:—

"The term 'demography' is not to be found in any of our dictionaries, even of tolerably recent date, and in France is only contained in the great work of Littré, who denominates it 'a didactic expression descriptive of peoples as regards the population in relation to ages, professions, dwellings, &c.,' and he also adopts the definition of Quetelet, that it is 'the natural history of society.' The appellation appears to have been first employed by M. Galliard in his 'Elements of Human Statistics, or Demography,' published in 1855. The first President of the Society, the late Dr. Bertillon, treated it as dealing with the inner life of the social bodies which form a people (births, marriages, deaths, migrations, &c.), but only in their collective influence, of which it measures the powers of the parts or of the whole, without meddling with biological proceedings, which distinguish it from physiology."

Dr. Bertillon regarded England as the real home of demography, as it surpassed all other nations in the incomparable richness of its demographic inquiries, and in the unbroken continuity of its published returns.

The subjects which will be open to discussion in the Congress may be classed under the following general heads:—(1) The prevention of communicable diseases, as, for instance, (*a*) whether sanitation or quarantine is most efficient against cholera; (*b*) how the spread of disease from milk and from water can be checked; (*c*) the relation which tuberculosis and other diseases in animals bear to mankind; (*d*) vaccination, the prevention of leprosy, rabies, and such like contagious diseases; (*e*) the effect of soil on communicable diseases; (*f*) disinfection and disinfectants. (2) The science of bacteriology in relation to communicable diseases. In connection with this subject an exhibition of microscopic and cultivation specimens would be arranged. (3) Industrial questions, as, for instance, the regulation of industrial occupations from a health point of view, including the length of hours of labour in different occupations, the influence of dwellings upon labour, and the effect of large cities on the health of the population; the influence of the health condition of the people, and the effect of different sorts of food and of wages upon the efficiency of labour. (4) The hygiene of infancy and childhood, as, for instance, protection and insurance of infant life; school hygiene, including length of hours of study, nature of studies, and the effect of physical training; school buildings and their accessories, and other educational questions bearing on health. (5) The hygiene of houses and towns, including questions of width of streets, height of buildings, air space round houses, house construction, water supply, river pollution, drainage, treatment of refuse, disposal of the dead. (6) State hygiene, or the duty of the Government towards the nation in regard to health, and the machinery necessary for exercising that duty; the duties of communities towards each other in regard to questions of health, and towards the individuals of which they are composed; the laws for notification and isolation of disease; the status and education of medical officers of health and of sanitary inspectors.

In consequence of the wide scope covered by these subjects, it is proposed that the work of the Congress shall be divided between ten Sections. The order in which they will stand is not finally settled, but they may be briefly summarized as follows:—(1) Demography, which necessarily involves questions of industrial health; (2) communicable diseases; (3) bacteriology; (4) diseases of animals in relation to man; (5) hygiene of infancy and childhood; (6) engineering in relation to hygiene; (7) architecture in relation to hygiene; (8) chemistry in relation to hygiene; (9) military and naval hygiene; and last, but not least important, (10) the functions of the State in relation to hygiene.

The Prince of Wales, who has so deep a personal interest in all matters relating to the well-being of the community, at once acceded to the request made to him to accept the post of President of the Congress; and the British Presidents and other officers of Sections will be among the most eminent men in their several departments of knowledge. Upon them will lie the duty of organizing the work of the several Sections before the Congress meets. They will be supplemented at the meeting of the Congress by foreign or colonial and Indian representatives, distinguished in the several branches of science included in the programme. It may be mentioned that above 2000 members, exclusive of representatives from Great Britain, attended the Vienna Congress; and it is anticipated that the Congress in London will attract at least an equal number of persons from foreign countries, because Great Britain affords so many examples of important sanitary works, as well as of institutions having the preservation or restoration of health as their main object.

The expenses of correspondence and other matters connected with the organization of the Congress are so large that appeal is made to those interested in the subjects to be discussed to aid the work by moderate contributions.

GEOLOGICAL TEXT-BOOKS.

Class-Book of Geology. By Archibald Geikie, F.R.S. Second Edition. (London: Macmillan and Co., 1890.)

Elementary Geology. By Charles Bird, B.A., F.G.S. (London: Longmans, Green, and Co., 1890.)

ALL who care for geology, teachers and students especially, will hail with delight the appearance of a second edition of Dr. A. Geikie's "Class-book." All the more that it may now be had at less than one-half the cost of the first edition. Such a substantial reduction in the price of a scientific book is a piece of liberality and far-seeing policy which it could be wished were more common. Too often, when a rapid sale of such a work has proved that its value is appreciated and its success assured, there seems to be a feeling that its established reputation may be relied upon to ensure a demand for it in the future equal to that of the past, and that it will be quite safe to keep up its price. But no such purely mercantile views have prevailed here, and we may feel sure that both publishers and author have been actuated first of all, in the bold step they have taken, by a wish to promote the extension of knowledge by placing within the reach of as many as possible a work so well got up, so admirably illustrated, and so full of matter presented in an eminently readable form. And there can be little doubt that this disinterested conduct will reap the reward it deserves. Many a teacher will bear me out that the book has been a universal favourite. Students who would not put up with the dry compendium of the normal text-book have been won over by its attractive style; and even those earnest workers, who despise or excuse the absence of literary elegance, have found their labours lightened and have felt an added pleasure when their first introduction to geology has been through the medium of this class-book. One thing only prevented its being adopted far and wide as a text-book in the school and lecture-room. Now that obstacle is removed, it is sure to come into more and more extensive use; and it may be confidently predicted that a rapidly increasing sale will soon recoup those concerned for y temporary loss that their generous treatment of the public may at first occasion.

It is the conviction that it will not be long before a second opportunity for revision presents itself, and a wish to give, if it can be done, some help towards making so excellent a book more perfect still, that embolden me to be critical, and to point out a few minor points which seem capable of emendation; and I take this course with the less hesitation, and with no fear of being misunderstood, because I have endeavoured, in a previous notice of the first edition, to do justice to the many excellences of the work and to explain its scope.

I cannot say that the account of the formation of "flood plains," on p. 39, quite commends itself to my mind. The author postulates the existence of an alluvial flat, over which sand and silt are deposited during floods, but he does not explain how this flat is produced. Possibly the required explanation may be found in his "Physical Geography," which he recommends should be read in connection with the present work. If this be so, a reference to the passage required would be useful: indeed, wherever the one book is required for the elucidation of the other, it might be well to call attention to the fact in a note. The impartial and judicious blending of Darwin's and Murray's views on the growth of coral reefs is much to be commended. If we take into account the vast number of cases in which minerals occur without any external crystallized shape, it seems hardly safe to say that they have "in most cases a certain geometrical form" (p. 123). But the mineralogical sketch seems capable of improvement in more than one direction. The using "sides" and "faces" as if they were convertible terms, in the description of rock-crystal on p. 124, is apt to engender confusion of thought. In the account of felspars there is no mention of their cleavages, a property so often of practical value in distinguishing them from quartz, and a property far more easily recognized by the beginner than their monoclinic and triclinic crystallization. In some cases—mica, for instance—there is no mention of hardness, where that is a character useful in identification. It would not be fair to blame a geologist for the definition of the systems of crystallization given in the book before us, as long as it still survives in treatises on mineralogy. But it is much to be desired that the number and position of certain ideal axes should not be the starting-point for classification. If the grouping were based on the degree of symmetry possessed by each system, and it were afterwards, if necessary, pointed out that ideal axes had their uses, the point of real importance would have its due place, and things of subsidiary value would find their proper level. But, really, the amount of crystallography that there is room for in an elementary treatise on geology is so small, and it is so hopeless to attempt in this space to convey any idea of what crystallography means, that the subject in its general form had better be passed over altogether; it would be quite enough to explain what is meant by crystalline cleavage, and to call attention to the shape of crystals only in those cases, such as fluor spar and calcite, where they are common and easy to recognize. Many a student, who would simply be driven wild by an attempt at a systematic teaching of crystallography, has an eye good enough to enable him to recognize dog-tooth spar when he sees it, and to form a very fair idea whether an hexagonal pyramid is dumpy enough to belong to quartz. In teaching, it is quite enough, as a rule, to let the small amount of crystallography that a geologist must know drop in incidentally as occasion requires, taking special care that each fact is illustrated by a concrete example, so that the knowledge required may come in instalments and be assimilated bit by bit.

To continue in the censorious mood. The statement that "perlitic structure is one of the accompaniments of devitrification" (p. 145) is fraught with danger. Many readers, I fear, would take the words to imply that the structure is a result of devitrification, which of course is not their meaning. Why not say, "is often found in devitrified rocks, and is useful as indicating that they were once glassy"? I take decided objection to the statement, on p. 150, that grains of sand are usually rounded; as a rule, the sand-grains of a sub-aqueous sandstone are most markedly chips, and it is only when they have been exposed to æolian wear that they become even approximately rounded. The author is wisely guarded when he treats of the origin of flint; but the doctrine of the replacement of calcium carbonate by silica has so much in its favour, that it might, without much risk and with manifest advantage, have been mentioned. That quartzite is "an indurated siliceous sand" we shall all admit: it might have been usefully added that in many cases the induration has been caused by the deposition of secondary silica between its grains. In discussing the methods by which sedimentary deposits are consolidated, no mention is made of the tangential pressure exerted during great earth movements; surely this is one of the most, if not the most efficient agent in the work.

The stratigraphical part of the book is necessarily brief, but it is all to the point, and the reader ought to carry away from it clear notions as to the main features in the life and physical geography of each geological period. It is here that the distinctive excellence of the work, which places it so far above the level of the average dismal text-book, comes out most strongly. In the place of comparative tables of strata and lists of characteristic fossils, which an examinee commits to memory and forgets, we have graphic pictures of what England, and Europe in many cases, were like, and an account of what went on during geological periods—history, in fact, and written as an historian, and not as a compiler, writes. The illustrations are excellent: the author's well-known artistic skill adds a charm to the usefulness of many; and the figures of minerals, rock structures, and fossils are such faithful pictures, that, where a teacher is quite unable to procure specimens, they will to a large extent supply the want. The book closes with an appendix giving the classification of the vegetable and animal kingdoms, in which those points which are of special importance in palæontology have been judiciously picked out and made prominent.

In the preface to Mr. Bird's "Elementary Geology," we are told that "the following lessons were given to a class of thirty boys, and were very successful in arousing an interest in geology and in sending a number of town boys on long walks into the country;" and that they had also the comparatively unimportant result of enabling the class "to pass the South Kensington examination in the elementary stage of the subject." I have the best possible reason for feeling sure that the class-teaching of the author would deserve and secure the good results which he tells us followed from it. But I have grave doubts whether a beginner, who had the book alone to rely upon, would be won over to feel any great love for its subject. This, however, is no fault of the author's. As long as the main end of study is to enable the student to pass examinations, books must be written whose chief object is to help him to this end; they must be cheap, and therefore small; so they must be thickly packed. The second chapter of the work before us is an instance of how this must be done. It consists really of a definition of nearly all the terms which are to be used in the course of the work, and the reader who commits it carefully to memory will be armed with answers to a large number of the questions that may be put to him in examinations. But I am perfectly certain that Mr. Bird never gave a lesson in class, or a series of class-lessons, which is fairly represented by this chapter. No human being could survive the tedium which must be the result of thrusting upon him at one fell swoop such a crowd of new words and new conceptions. The success which crowned Mr. Bird's labours is in itself a proof that he never taught in this fashion; enthusiasm such as he kindled is not evoked by overwhelming the learner with a flood of dry statements at the outset of his career. That is just the way to create a distaste for a subject which no amount of subsequent amplification suffices to remove. Definitions we must have, but one who can teach like Mr. Bird knows how to bring them in one by one, at intervals, as they are wanted. Why his book is so unlike what must have been the style of his teaching has been explained already, and it is only giving him his due to confess that, considering how he has been trammelled, he has executed his task well, and that the book is above the average level of its class. He will, I am sure, allow me to point out, in no unkindly spirit, portions which seem capable of improvement.

There is the same crowding in chapters iii. and iv. as in chapter ii. The descriptions of the minerals in chapter iii. might enable a reader to answer some questions often set in examinations, but in many cases they would do little towards helping a student to know the mineral when he saw it, and this after all is what the real student wants. To learn how to worry out for himself that a mineral is probably a felspar is infinitely more valuable as a bit of educational discipline than to be able to write down in an examination the chemical composition of felspars. If only a few of the commoner rock-forming minerals had been treated of, it would have been quite possible in the space available to have given descriptions of the way in which a student must go to work in order to recognize them; but then some minerals must have been omitted that are liable to be "set" in examinations.

There are in the book many little slips, not very serious, which might usefully be corrected when opportunity offers. On p. 28 it should be stated that clay is hydrated. There is hardly reason to say that quartzite has been "almost melted" (p. 42); it is certain that in many cases its hardness is due to the deposition of secondary silica. It would have been as well to give some account of the physical characters of serpentine (p. 43), and greater definiteness of statement as to the minerals by the alteration of which it has been produced. I really hardly know what grounds there are for the statement, on p. 78, that the action of glaciers has had any thing to do with the production of immense perpendicular cliffs of limestone. One would have liked to see a section somewhat nearer the actual thing in the place of our old time-honoured friend the section across the Jura on p. 98. I am not sure, but I fear, that the dissolution of actual chalk in carbonated water would go on slowly (p. 45); freshly precipitated calcium carbonate dissolves fast enough, and is safer for lecture-room experiment. It is not carbonate of iron that gives a bluish-grey tint to rocks, as stated on p. 47. In chapters vii. to xi. it would seem that we have a much closer representation of the author's actual teaching than in the earlier part of the book; there is much life and brightness about them; and, while they are full of matter, it is presented in an attractive form. It would have been well to give distinctly the two main points of difference between Conchifers and Brachiopods on p. 113. The only character mentioned is that the latter are equilateral. The Trilobite on Fig. 110 is not a Paradoxides. The section on Fig. 139 is misleading; the Permian scarp nowhere towers above the Penine ridge in the way represented. It would certainly be desirable to add a word about the Alpine Trias on p. 169. The position of the siphuncle in Goniatites is incorrectly stated on p. 181. By a curious slip the Portland Beds get the credit of Mammalian remains on p. 193. In the figure of Inoceramus, on p. 213, the cartilage-pits of the hinge are described as teeth. In the notice of the Forest Bed, on p. 226, room might be found for a word about the Arctic Freshwater Bed.

After all this fault-finding, it is a pleasure to be able to call attention to the excellent character of the illustrations: they are numerous, well chosen, and admirably executed; the figures of fossils deserve special praise. The geological map of the British Isles will be most useful. It is not crowded, and the clear transparency of the colours is such as till lately was unknown in English chromolithography. The book belongs to a class that I have no great love for, but it is distinctly good of its kind; and it will prove useful in the hands of a teacher who knows how to dilute and season the condensed food which it offers, and whose aim, like the author's, is not merely to get his students through an examination, but to educate them and imbue them with the scholar's disinterested love for learning. A. H. GREEN.

ELECTRO-METALLURGY.

The Art of Electrolytic Separation of Metals, &c. (*Theoretical and Practical*). By G. Gore, LL.D., F.R.S. *Electrician* Series. (London: The *Electrician* Publishing Co., 1890.)

A Treatise on Electro-Metallurgy. By W. G. McMillan, F.I.C., F.C.S. Griffin's Scientific Text-books. (London: Charles Griffin and Co., 1890.)

ANY metallurgical process which reduces the cost of production as compared with older processes for effecting similar results must of necessity prove of great commercial importance. This is especially the case in places where labour is dear and unskilled, and fuel and refractory materials both scarce and costly—perhaps, even, absolutely unobtainable. If, too, the new process enables a metal to be produced, as in the case of copper, which possesses superior quality to the metal produced by the older process, such a metal will command a higher market price, and the use of the new process is consequently attended with important commercial advantages.

Such an improvement over ordinary metallurgical processes has been effected in recent years by the gradual introduction of electrolytic methods. They have not, however, made the rapid progress which was at one time anticipated, and at present are practically only of importance in the metallurgy of copper. There are many reasons for this. The tendency to-day is to endeavour to increase to its maximum the daily output of existing works, provided the product already attainable is of fair quality, rather than to lay down new plant, which, although it might produce a metal of greater purity, would be severely handicapped by the slowness of the work and smallness of the output as a return for the capital invested. It is in this respect that electrolytic methods for the preparation of the ordinary commercial metals fail to give satisfactory results. It is true that in the cases of gold and silver this objection does not to so large an extent apply, but, unfortunately, other circumstances have also to be considered. The metallurgy of the precious metals is for the most part effected in the immediate neighbourhood of the mines, and these, in turn, are usually situated in out-of-the-way districts. Miners, too, and mill-men are generally but slightly acquainted with a knowledge of matters pertaining to electrolytic methods, and comparatively little attention has therefore been directed in the past to the treatment of the ores of the precious metals by such methods. There can, however, be but little doubt that in the near future this will cease to be the case, and that by the improvement of known methods, and by devising new ones, electrolytic methods will attain a degree of commercial importance which at present can hardly be foreshadowed. Any sound contribution to the literature of the subject which is likely to assist in this development of metallurgical processes thus becomes of much importance. We record, therefore, with pleasure, the appearance of a manual by Dr. Gore, devoted to electro-metallurgy proper, which is likely to prove of much use in the spread of the theoretical knowledge so requisite for industrial success. The author gives much attention to this portion of the subject, but in view of the claim "that this book is written to supply a want," it is to be regretted that fuller details are not forthcoming as to the practical working of the processes, though such information as is given is well and clearly put. The greater part of it, however, has already appeared elsewhere. In the case of copper, for instance, if the working arrangements are faulty, the mud liberated on the solution of the impure copper may be deposited again on the cathodes; the tank solution, too, may vary in density, and the working become irregular with a consequent irregularity in the character of the metal produced. Here, again, excessive rapidity, fatal to the purity of the product, is very common, the result being that in the majority of instances it is necessary to melt the deposited copper, thus introducing sources of error which it is one of the main objects of the process to avoid. The incidental collection of the gold and silver in copper submitted to such a refining process may occasionally cease to be an incident and become the main object of the process, ores of gold and silver being intentionally added in the previous smelting operations.

The difficulty of obtaining admission to works employing electrolytic refining methods, which the author himself laments, is so great, that it is with all the more regret that we have to call attention to this want of fuller details as to the more recent modifications of working adopted in this country and elsewhere. The text-book is otherwise an excellent one.

The term "electro-metallurgy" has been generally applied in the United Kingdom to all operations in which a metallic deposit is produced by means of electrolysis, however small the scale of production may be. Such a designation of the art is much too broad, and it would be better to limit the use of the term "electro-metallurgy" to those processes which are employed on a large scale for the purpose of extracting or refining metals, as distinguished, that is, from ordinary galvanoplastic methods.

This criticism applies to the "Treatise on Electro-Metallurgy," by Mr. W. G. McMillan, the greater part of which relates to galvanoplastic methods proper. Metallurgical processes, however, are also considered, though but little space is given to them; and a chapter, which might well have been a longer one, is devoted to electrolytic methods of assay.

Dr. Gore, after giving a brief historical sketch of the subject, passes to a consideration of the theory of electrolysis, which is considered at some length, useful tables being also given. The mode of establishing an electrolytic refinery, together with the plant required for this purpose, is next considered, and in addition to an account of the various types of dynamo-electric machines in use for electrolytic purposes, brief descriptions are also given of the various electro-metallurgical processes which have been from time to time proposed.

In Mr. McMillan's treatise, an historical introduction to the subject of electro-metallurgy is given, accompanied by a theoretical consideration of the question. A chapter is devoted to the "sources of current," and in a series of other chapters the art of electro-plating is described. Another chapter refers to electro-typing, and others relate to the electro-deposition of the various metals and some alloys. Electro-metallurgical processes proper are also considered, together with methods of assay, and a glossary is added of substances commonly employed in electro-metallurgy. Forty-three useful tables are given as addenda, and the printing is clear and distinct.

We can recommend both these manuals, not only to students, but also to those who are interested in the practical application of electrolytic processes.

OUR BOOK SHELF.

Leçons sur l'Électricité professées à l'Institut Électro-Technique Montefiore annexé à l'Université de Liége. Par Eric Gerard. Tome II. (Paris: Gauthier-Villars et Fils, 1890.)

THIS volume completes the work the first part of which has been already noticed in NATURE (vol. xlii. p. 219). In the first volume, the general principles of electricity and magnetism and the theory of dynamo-electric machinery were explained; the volume now before us contains a very clear and full account of the most important industrial applications of electricity. The principal subjects discussed in this volume are: methods of distribution in electric lighting; transformers, and meters; the insulation of electric light cables; electric motors, both with constant and alternating currents; the transmission of power; electric railways and tramways; descriptions of the various kinds of incandescent and arc lamps; photometry; electro-metallurgy, including the deposition of metals from solutions and fused salts; electric welding. All these subjects are clearly explained, and generally illustrated by instructive diagrams and figures; the book is well up to date, and should prove of great service to students of electric technology. The only fault we have to find with it is that it contains no references which would enable the student to consult for himself papers in which the various processes are described more fully than is possible in a text-book of moderate size. This omission, though exceedingly common in French treatises, is one which we think is greatly to be deplored: in the first place, it leaves the student at the mercy of the author, for, if he does not understand the explanations in the text-book, he does not know where to turn for another with which he might have a better fate; and secondly, we hold that the habit of consulting papers in the Transactions of learned Societies and scientific journals is a most valuable one for the student to acquire, and that it has no chance of developing unless text-books contain references to such papers. J. J. T.

Fathers of Biology. By Charles McRae, M.A., F.L.S. (London: Percival and Co., 1890.)

STUDENTS of anatomy and physiology, as the author of this little book points out, are apt to suppose that the facts with which they are now being made familiar have all been established by recent observation and experiment. There could not be a greater mistake. Biology is a science of "venerable antiquity," and the way was prepared for modern discoveries by the labours of many patient and far-seeing investigators. In the present volume Mr. McRae has sought to illustrate this by sketching the biological work of Hippocrates, Aristotle, Galen, Vesalius, and Harvey. He could not have selected five inquirers better suited for his purpose; and within the limits he has allotted to himself he has succeeded admirably in indicating the nature and value of the contribution which each of them made to biological science. He is especially happy in his treatment of the three representatives of ancient research; but the essays on Vesalius and Harvey are also clear, well-arranged, and suggestive. Mr. McRae is not content with second-hand information. He has evidently studied the original sources with care; and the result is that his method of exposition is invariably fresh and interesting. He knows, too, how to connect the results attained in former times with those at which later anatomists and physiologists have arrived. He does not, of course, claim to have exhausted the interest of his subject. But the work he has done, so far as it goes, is sound, and should be of service to many of his readers in helping them to understand the various stages in the development of the scientific conceptions with which he deals.

Through Magic Mirrors. By Arabella B. Buckley. (London: Edward Stanford, 1890.)

THIS volume is intended to form a sequel to the "Fairy-Land of Science," and is written with the clearness and brightness which make that book so attractive. The power that Miss Buckley has of interesting young people in the more popular parts of the various sciences cannot be doubted, and is well shown in the present book. A magician is supposed to teach young lads; and the author is thus enabled to bring in different parts of the sciences, and to preserve a continuity throughout.

There are ten chapters, and the following are some of the headings: "The Moon," "Life-History of Lichens and Mosses," "History of a Lava Stream," "An Hour with the Sun," "An Evening with the Stars," "Little Beings from a Miniature Ocean, &c." In the chapters with these headings the uses of the telescope, spectroscope, camera, microscope, &c., are all mentioned and well explained, and their principles clearly brought out.

The information throughout is up to date, and is taken from the best sources; and the illustrations form a most important addition to the text. The frontispiece is a reproduction of Mr. Isaac Roberts's most exquisite photograph of the great nebula of Orion, taken on February 4, 1889.

LETTERS TO THE EDITOR.

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

The Proposed South Kensington and Paddington Subway.

ALTHOUGH there can be no question but that any facilities of access to the group of buildings on the ground of the Commissioners of the Exhibition of 1851 will be greatly to the advantage of the public, the objections to the proposed railway raised in your recent article on "Shaking the Foundations of Science" are worthy of most serious consideration, if they are not altogether fatal to the scheme. The alternative route for the line which you suggest, along Queen's Gate, would meet with equal difficulties, much greater expense, and certain opposition in several quarters. There is, however, another solution of the question free from most of the objections to both the others.

Let the existing subway be continued as originally projected as a walking road to a station at the Albert Hall; and let the tramway or railway line be brought across Kensington Gardens from Paddington to meet it there. This will have the desired effect of giving easy access to the Albert Hall and neighbourhood from the north-west of London, as well as a convenient dry covered approach from the South Kensington Station. The cost of this scheme to the promoters of the railway will be far less than if they have to enlarge the present subway so as to make it available for carriages. The convenience to the public will be nearly as great. In the proposed scheme anyone coming by the District Railway must change carriages at the South Kensington Station; and there are few who, having once alighted, would not as soon take a short walk to the Museums or the Albert Hall through the subway (as was originally designed when this was made) as mount into another carriage. It will be a totally different thing from having (as now) to emerge to the upper surface, and either take a cab or trudge along a wet, dirty, and cold road. It would, in fact, be scarcely a longer walk than is often necessitated along the platforms of some of our existing railway stations.

The walking subway, instead of opposition, may well receive the cordial support of all interested in the "foundations of science," as it will lessen the number of wheels which rattle along the streets above. The marvellous improvement that it made in the state of the streets during its brief period of usefulness, the summer of the Indian and Colonial Exhibition, was apparent to all dwellers in the neighbourhood.

W. H. FLOWER.

Natural History Museum, January 12.

WOULD it not be desirable to prepare a petition to the House of Commons, to be signed exclusively by scientific men, against the proposed South Kensington and Paddington Subway Railway? A copy might lie for signature at the rooms of each of the learned Societies. ALFRED W. BENNETT.

Chemical Action and the Conservation of Energy.

IN NATURE of December 18, 1890 (p. 165), there appears a paper by Mr. Pickering under the above heading, in which some of the errors of thermo-chemists are exposed. As, on account of the well-known experimental skill of Mr. Pickering in thermo-chemistry, there may be some risk that all the positive statements in this paper may be accepted by students as facts, it is, I think, worth pointing out that some of these statements, although given positively as if they were obvious physical laws, are, to say the least, matters of controversy, whilst others are absolutely erroneous. It would seem as though prolonged calorimetrical studies lead the experimenter to regard heat changes as the only factors to be considered in cases of chemical equilibrium, since the same erroneous view of the subject has been taken by Berthelot in his "Law of Maximum Work," by J. Thomsen in a similar "Law," and now by Mr. Pickering in this paper. He concludes:—

"As a consequence of this, it follows that, in any complex system of atoms, where two or more different arrangements are independently possible, and where the various products remain within the sphere of action, and are capable of further interaction, then those products, the formation of which is attended with the greater evolution of heat, will be formed to the exclusion of the others."

It is somewhat curious that by way of clearing the ground for his own views Mr. Pickering demolishes one of the chief arguments of the older school—namely, that an endothermic reaction may occur if it forms part of a cycle of which the final result is an evolution of heat. His illustration of the impossibility of this, by comparison with a stone rolling a short way up a hill by itself in order to have a long roll down on the opposite side, is exceedingly apt. But immediately following this is a statement which must be regarded as incorrect: "No amount of heating can make an endothermic reaction possible so long as it remains endothermic."

I have characterized two statements as erroneous; perhaps I may be allowed to mention the evidence usually accepted as proving this, premising that, since it partially depends on the second law of thermodynamics, and this in its turn depends upon experiment (see J. J. Thomson, "Application of Dynamics to Physics and Chemistry," pp. 4 and 5), no argument against it is valid which deals only with single molecules. The researches of J. W. Gibbs (Trans. Conn. Acad., iii. 108, 343), of Lord Rayleigh (Proc. Roy. Inst., vii. p. 386), of Massieu (*Journ. de Phys.*, vi. p. 216, and of Helmholtz (*Sitz. Akad. Berl.*, February 2, 1882, July 27, 1883, and *Monats. der Berl. Akad.*, May 31, 1883, p. 647; also, Physical Society, Translated Memoirs, vol. i., Part 1), prove conclusively the general principle that the evolution of heat alone is not conclusive as to the possibility of a chemical change taking place in a given direction. The magnitude which does condition this change having been arrived at by different methods, in some cases independently, has received different names; thus it is the "free energy" of Helmholtz, the "characteristic function" of Massieu, the "force function for constant temperature" of Gibbs, and the "thermodynamic potential" of Duhem. All these names represent the same function, F (or − F), the integral energy (U) less the product of the absolute temperature (T) into the entropy (S); or in symbols the free energy $F = U - TS$. In the above-mentioned researches it is shown that a system will be in stable equilibrium if the free energy is a minimum. Hence, to restate Mr. Pickering's conclusion, it follows that, in any complex system of atoms or molecules, where two or more arrangements are independently possible, then the final arrangement will be such that its formation is attended with the greatest loss of "free energy." Although this work has now been published nearly fifteen years, so far as I am aware no serious attempt has been made to prove it to be inaccurate.

Let the energy, entropy, and temperature before the reaction be U, S, and T; after the equilibrium is established, U′, S′, and T′, then $(F - F') = (U - U') - (TS - T'S')$. But $(F - F')$ must be positive, *i.e.* $(F - F') > 0$. If the temperature be so low that the second term is small compared to the first, then there will be a loss of energy—an evolution of heat. But this is not the only possible case: the reaction may take place with an absorption of heat if $T'S' > TS$, and also $(TS - T'S') > (F - F')$. Here $(U - U')$ is essentially negative; in other words, the reaction will take place of itself and with absorption of heat. This will account for many apparent anomalies in high temperature reactions.

Further, as regards the statement that "no amount of heating can make an endothermic reaction possible so long as it remains endothermic," we see from the equation $F = U - TS$ that the importance of the second term increases with the temperature, and thus F may decrease by the increase in TS as well as by the more generally recognized decrease in U. U may, in fact, increase in the course of the reaction, provided that the corresponding increase in TS is larger.

Exception might also be taken to the definition given of chemical affinity as "the potential energy possessed by atoms." It is rather difficult to imagine potential energy becoming "satisfied" when the atoms combine together. As, however, there are now several works giving a complete account of the other views on this subject, it is unnecessary to discuss this point.

In conclusion, it may be as well to point out that the above remarks contain nothing new, having been known for years; but as they have not yet found their way into the text-books of thermal chemistry, and are apparently ignored by our leading thermo-chemists, it seems worth while to bring them to the notice of students. GEORGE N. HUNTLY.

Richmond, Surrey, December 22, 1890.

Forestry in North America.

THE timber-cutters, graziers, and settlers, who are rapidly destroying the forests of North America have recently obtained a valuable ally in Major J. W. Powell, Director of the United States Geological Survey; who, in his enthusiasm for the construction of dams, appears to care little from whence his countrymen are to get their future supplies of timber, whilst his proposed means for securing to them a perpetual water-supply for irrigating the farm lands below the Rocky Mountains, when confronted with the experience acquired in Europe, are likely to prove worse than useless. Most students of physical geography consider hill forests as efficient aids in storing water, but not so Major Powell; for in the April number of the *Century Magazine* in a paper on the "Non-irrigable Lands of the West," and in another paper published in the August number of the *North American Review*, entitled "The Lesson of Conemaugh," he advocates the denudation of the higher slopes of the Rocky Mountains, in order to allow snow-drifts to accumulate in the folds of the hills, and afford a perennial supply of water for irrigating the drier lands below the forest belt. Major Powell contends that, as long as the higher hill slopes remain forest-clad, the snow falling on them will always be uniformly distributed, and cannot be drifted together by the wind. It therefore melts away gradually, without affording a steady supply of water to the hill streams. He also asserts that in California the hill streams have increased wherever the hills have been denuded; and expresses his opinion that, although forests may be useful in districts with a heavy rainfall, as they evaporate excessive moisture rapidly from the soil, yet in drier regions this rapid evaporation is prejudicial, and therefore forests should be cleared in order to preserve the requisite amount of moisture in the soil. This appears to be a complete inversion of what actually occurs, as I hope to show further on.

Major Powell gives a most striking picture of the ease with which forest denudation may be effected in the dry coniferous forests of the Rocky Mountains. When camping out twenty years ago in this magnificent forest tract, where the growth was then so dense that not a blade of grass was to be found under the trees, but the soil was completely covered with dead needles; the Major allowed his camp fire to ignite a splendid pine tree, and watched the "welcome flame" rising till the whole tree was in a blaze. The fire soon spread to the surrounding forests, and before nightfall the country for miles around was in flames, more timber being destroyed in a few days than "has been used by the people of Colorado for the last ten years."

Major Powell, however, as we have stated, would preserve forests in the damper regions, and admitting the danger of forest fires to their existence, he proposes as a remedy a great extension of sheep-farming; so that the undergrowth of the forests may be nibbled off, and thus a great source of danger from fire may be removed. What the sheep are to find to nibble in forests where the soil is covered with dead needles, it is not easy to say; and if all undergrowth be bitten off by the sheep, the future re-growth of the forest could evidently not be assured by natural seedlings, but the older portions must be gradually fenced and planted up: if this be done on a large scale it is to be feared that even the vast resources of the States would prove inadequate. An answer has, however, already appeared in an American periodical, *Alta California*, to the Major's suggestions regretting the progress of forest arson, and anticipating that thousands of acres of the noblest timber in the world will be destroyed before the first rains next autumn. The writer states that these forest fires are chiefly due to shepherds, who burn the pine needles, and consequently the entire forest, in order to get young grass to spring up for their flocks.

So much for the real results of extending sheep-runs to preserve the forest from fire, which reminds one of a suggestion in Prof. Wallace's "Indian Agriculture"; except that this author goes further than Major Powell, and wishes to induce the Government of India to put an end to the efforts of the Indian Forest Department to protect their forests from fire, considering that Indian forest trees have grown up under a *régime* of annual fires till they have become quite inured to them, and would suffer if the fires were stopped. The Professor also urges that the Indian flocks and herds would miss the fresh young grass always springing up in burned forest, but if admitted to graze in the annually burned area, would keep down the undergrowth, the occasional burning of which in a forest protected from fire, according to him, does more damage than annual fires in well-grazed forests. It is to be feared that both the Edinburgh Professor of Agriculture and the Director of the United States Geological Survey have more sympathy for the shepherds than for the forests.

Besides the paper controverting the Major's advocacy of sheep as friends to the forest, I was glad to see two very sensible articles, in *Garden and Forest* of June 18 and September 4, entirely opposed to his theories, whilst Mr. Abbot Kenney has written a paper in the August number of the *Century* showing that the statement about the increase in volume of the hill streams in California being due to forest denudation, is at variance with the facts of the case.

There can be no doubt that directors of geological surveys should study the action of the forest as a great natural factor influencing soil and climate. In Germany, the elements of forestry are taught in primary schools as a necessary part of the general education of the country people; but the German peasants have owned forests, and participated in their benefits, for centuries, and a forest fire in France or Germany is considered a great calamity by the whole country-side: the Anglo-Saxon race on the contrary has been chiefly engaged during the last 300 years in clearing away the virgin forests of the new worlds in the western and southern hemispheres; and, as we see, the benefits of forests are still questioned by certain influential people both here and in America.

Darwin and A. R. Wallace admit over and over again the inestimable value of the forest as Nature's great conservative force; and in America, Major Powell's heresies were long ago disposed of in Marsh's "Man and Nature." In Wallace'

"Tropical Nature," we read as follows:—"A systematic planting of all hill-tops, elevated ridges, and higher slopes, would probably cure the bad effects of the intermittent rainfall of Central India; whilst the action of forests in checking evaporation from the soil, and in causing perennial springs to flow which may be collected in reservoirs, would serve to fertilize a great extent of country."

Major Powell evidently disagrees with the results of Wallace's observations, but the facts are quite opposed to his theories. Forests do not evaporate moisture nearly so fast as bare ground; and although denuded hill-sides may favour the accumulation of snow-drifts, yet they allow the rainfall to drain off rapidly, and cause dangerous floods; loosening of the soil on hill-sides; avalanches; silting up of river-beds; and frequently give rise to the complete devastation of cultivable lands at the foot of the hills, as the material washed down from above is spread over them by the floods in the form of silt, gravel, and boulders.

In Dr. Schlich's "Manual of Forestry," at pp. 43 *et seq.*, we read that experience in Germany shows evaporation from forests to be only two-fifths of that in the open country, and that the balance of water retained in forest soil increases rapidly with the altitude, so that evaporation in mountain forests may be reduced to about 10 per cent. of the rainfall. We also know that, in France and Germany, mountain forests have long been looked upon as preservers of moisture and feeders of springs. In 1889, the French Government spent 3,192,800 francs in *reboisement* works in the Alps, Pyrenees, and Auvergne; and this almost entirely for the indirect benefits resulting from forests to the mountains, as the plantations and embankments which form the *reboisement* works are too costly ever to yield a direct revenue commensurate with the heavy expenditure incurred in such remote and inaccessible places. If, however, Major Powell's proposed denudation of the Rocky Mountains were to be effected, besides its disastrous indirect results, America would suffer from a greatly curtailed supply of timber to meet the ever-increasing demands of a vast continent, which cannot depend on any adequate supply from abroad. We see that in the McKinley Act the Government of the United States already acknowledges its own short supply by withdrawing all import duties from Canadian timber; and it is for Canada to assure its own future prosperity by establishing a State forest service to prevent the exhaustion of the Canadian forests, now that they are likely to be fully utilized.

Up to the present time, the Forest Department of the United States has been chiefly occupied in collecting forest statistics and encouraging private planting, but what is really required is to induce each State in the Union to establish a practical control of its own still existing forests.

The Americans have recently refused to join in a postal confederation of English-speaking countries, on the ground that they are now to a large extent German-speaking as well; it is a pity, therefore, that they do not listen to the warnings of the German forester, Dr. Mayer, who has studied the forests of the Rocky Mountains and has given the last word of German scientific opinion on the utter absence of a State forest policy in the United States, in his recently published work on the forest trees of North America. W. R. FISHER.

Cooper's Hill College.

Throwing-Sticks and Canoes in New Guinea.

I HAVE just received here my copy of the February number of vol. xix. of the Journal of the Anthropological Institute, in which I have read, with the greatest interest and appreciation, the long and valuable account of the western tribe of Torres Strait, by Prof. Haddon. With regard to the throwing-sticks, of which, on p. 332, he says, "the heavy spears of South-east New Guinea are hurled by a throwing-stick which differs from any Australian implement," I think some error must have been made by his informant. I never saw a throwing-stick in existence, or in use, during my three years' residence in the country, either in the interior, along the south-eastern peninsula, in the Louisiade Archipelago, or on the northern coast as far as Mitre Rock. If these implements do exist on the southern side, they must be very rare. The first spear-thrower from New Guinea brought to England, as far as I am aware, was, nevertheless, the one brought home by me in 1888, which is now in the British Museum. It came, however, from the German possessions on the north-east coast, either from Finch-haven, or from the Augusta River, if I recollect correctly, and was given to me in Cooktown.

In the same paper, on p. 384, occurs this passage with regard to canoes:—"I was much puzzled when I first went to Torres Straits by occasionally seeing a canoe with a single outrigger. I afterwards found that it belonged to a Kanaker from Ware [? Mare], one of the New Hebrides, residing at Mabuiag, and that he had outrigged a native canoe according to the fashion of his own people. When I was at Mabuiag, some natives of that island were fitting up a canoe in imitation of this one, and with a single outrigger. Here a foreign custom is being imitated." The bulk of the large canoes seen on the south-eastern coast at Motu-Motu, Port Moresby, Kerepunu, and in Milne Gulf, have no outriggers at all; while along the coast in small canoes, and in both large and small in the Louisiade Archipelago, the single outrigger is the prevailing form. It is the canoe indigenous to the region, and is undoubtedly not an introduced or imitated custom. The single outrigger in Torres Strait may be an imitation, but it is also a true New Guinea model.

HENRY O. FORBES.

Canterbury Museum, Christchurch, New Zealand, October 29, 1890.

Pectination.

I HAVE been somewhat disappointed to find that no one can suggest a better explanation of the pectination of birds' claws than that which I gave in NATURE of December 4, 1890 (p. 103). As this is the case, however, perhaps I may be permitted to add a remark to what I then said. It has been pointed out to me by a friend that the lateral position of the serration is not so disadvantageous for scratching purposes as I had imagined. While gladly admitting this—which removes a difficulty from the explanation—I still think that my observations must not be taken as conclusive.

It would be most useful and interesting if an observer could be found to give time and attention to representatives of the different orders of birds which possess this peculiarity.

E. B. TITCHENER.

Inselstrasse 13, Leipzig, January 7.

The Flight of Larks.

THE extraordinary flight of larks to which the Rev. E. C. Spicer refers was observed at Bournemouth. The birds appeared to come across the Channel in thousands, and in a few days had entirely disappeared. There were certainly some fieldfares among them. ALFRED W. BENNETT.

PROFESSOR VIRCHOW ON THE CONSUMPTION CURE.[1]

THE important communication made by the renowned German pathologist at the last meeting of the Berlin Medical Society is a severe shock to the opinions of those who expect that Koch's mysterious lymph will prove applicable in every case of consumption. Prof. Virchow gives the result of his observations on twenty-one cases that have died, after treatment with the lymph, up to the end of December. Since then, six or seven other cases have come under his notice, but have not yet been completely examined. Of the twenty-one cases, sixteen were phthisical. The remaining five included a case of joint tuberculosis; a case in which lung tuberculosis was accompanied with carcinoma of the pancreas; another had empyema; the next had pernicious anæmia, slight changes in the lungs, and tuberculous pleuritis; and, lastly, comes a case of tubercular inflammation of the arachnoid.

It appears, from an examination of these cases, that the lymph has an action on tuberculosis of internal organs similar to that which it has already been seen to exert on external portions of the body similarly affected. The signs of an intense irritation, such as redness and swelling, are very generally to be met with. An excellent example of this action is afforded by the above-mentioned

[1] Reported in the *Berliner klinische Wochenschrift*, January 12, 1890.

case of inflammation of the arachnoid. Death occurred after the fourth injection, and Prof. Virchow has never seen so intense a hyperæmia of the pia mater and brain as this case presented. After careful examination no regressive changes could be found in the tubercular tissues.

The inflammatory changes met with in the various cases were not confined to a simple hyperæmia, which might possibly be regarded as of a transitory nature, but tissue changes which promised to be of a more lasting nature were also to be met with. The lymph glands near the affected parts, for instance, were found to be greatly enlarged. The increase in size seems due to a rapid multiplication of the cells in the medullary part of the gland—a change which is characteristic of acute irritations. This is probably connected with the increase in the number of white blood-corpuscles that has frequently been found to follow lymph injections, and this, again, with the frequent infiltration by leucocytes of affected parts and their surrounding tissues.

The changes produced in the lungs themselves belong to two widely different categories. Firstly, comes "caseous hepatization." That this can be actually caused by the injections is rendered highly probable by a very striking case, in which infiltration only commenced after the treatment had ceased, and led to a caseous hepatization of almost unique extent. Six injections had been made on this patient, of which the last was made four weeks before his death. Secondly, a "catarrhal pneumonia" is met with, sometimes alone, sometimes accompanied by the first-mentioned change. This form of pneumonia differs from ordinary catarrhal pneumonia in that it seems to lead to a rapid destruction of the lung-parenchyma, and a sort of cavity formation.

The most important conclusion that Prof. Virchow puts forward is that the formation of new tubercles which has been met with in many of these fatal cases must be ascribed with great probability to the action of the lymph itself.

The appearance of new tubercles has already been observed in lupus and tuberculosis of the larynx. Hitherto it has been asserted that the changes in question were merely due to the action of the lymph on tubercular material latent in the apparently healthy tissues. This view appears to be no longer tenable, at any rate as a general explanation. On serous membranes, which Virchow has always regarded as being best fitted for the observation of the early stages of tuberculosis, perfectly new submiliary tubercles have been found, under conditions which make it scarcely probable that they dated from an earlier stage of the disease. All these tubercles were perfectly intact, even in cases in which the injections had been made several weeks before. There was nothing to support the suggestion that these tubercles had been in any way affected or harmed by the action of the lymph.

How can this outbreak of new tubercles be explained?

In a phthisical case which terminated fatally, four small tubercles, surrounded by a zone of well-marked hyperæmia, were found situated on a part of the pericardium that could in no way come into contact with the lungs. In this case a direct infection was impossible, and we must suppose that, owing to the action of the lymph in breaking down the tubercular masses in the lungs, tubercle bacilli were thrown into the circulation, and thus reached the pericardium, where they succeeded in producing a metastatic infection.

In consequence of these and other similar observations, Prof. Virchow comes to the conclusion that the lymph should not be employed in cases in which one would expect some difficulty in excreting the tubercular matter set free by the treatment.

In forming an opinion as to the clinical bearing of these *post-mortem* appearances, it must be borne in mind that most, if not all, of these fatal cases were already in an advanced stage before they came under treatment; and, from the practical point of view, Virchow's work merely adds to the evidence that is gradually accumulating in the Berlin clinics, of the unsuitability of Koch's lymph for advanced cases, at all events with the present methods of administration.

For other considerations arise besides that of the stage of the disease. Thus I have good reason for believing that Koch's treatment of consumption has been less successful at the Charité (in which hospital Virchow's twenty-one cases have occurred) than in the other hospitals and clinics of Berlin. It would be interesting to determine whether any difference in the details of the treatment at the various hospitals could help to explain the difference in the results. The quantities of lymph employed and the frequency of the injections are by no means uniform in the various hospitals. For example, the Charité and the Friederichshain appear to stand at the opposite ends of the scale in these respects. For the following details respecting these hospitals I am indebted to a friend, who during the last month has been studying the results in the various clinics. At the Charité the injections appear to be administered more frequently and with a more rapid increase in the size of the doses than is the custom in the Friedrichshain Hospital. Further, the largest dose administered to a patient at Friedrichshain seems almost always below that given at the Charité, whilst the average dose given at the latter hospital also seems generally larger. Naturally, it would be unwise to draw definite conclusions until the details of such a comparison have been thoroughly investigated, but whatever the explanation may be, I have good reasons for asserting that the results obtained at Friedrichshain have been far more favourable than those obtained at the Charité. Not only do the milder cases seem to have made better progress at the former hospital, but the severer cases have less often had a fatal termination.

It would thus appear as if the dosage alone has a considerable influence upon the results obtained, even in the advanced cases which alone are the subject of Prof. Virchow's animadversion.

E. H. HANKIN.

THE RESEARCHES OF DR. R. KŒNIG ON THE PHYSICAL BASIS OF MUSICAL SOUNDS.[1]

III.

A FINAL proof, if such were needed, is afforded by an experiment, which, though of a striking character, will not necessarily be heard by all persons present, being only well heard by those who sit in certain positions. If a shrill tuning-fork is excited by a blow of the steel mallet, and held opposite a flat wall, part of the waves which it emits strike on the surface, and are reflected. This reflected system of waves, as it passes out into the room, interferes with the direct system. As a result, if the fork, held in the hand be moved toward the wall or from it, a series of maxima and minima of sound will successively reach an ear situated in space at any point near the line of motion, and will be heard as a series of beats; the rapidity with which they succeed one another being proportional to the velocity of the movement of the fork. The fork Dr. Kœnig is using is ut_6, which gives well-marked beats, slow when he moves his arm slowly, quick when he moves it quickly. There are limits to the speed at which the human arm can be moved, and the quickest speed that he can give to his fails to make the beats blend to a tone. But if he will take sol_6, vibrating $1\frac{1}{2}$ times as fast, and strike it, and move it away from the wall with the fastest speed that his arm will permit, the

[1] By Prof. Silvanus P. Thompson. (Communicated by the author having been read to the Physical Society of London, May 16, 1890.) Continued from p. 227.

beats blend into a short low growl, a non-uniform tone of low pitch, but still having true continuity.

This first portion of my account of Dr. Kœnig's researches may then be summarized by saying that in all circumstances where beats, either natural or artificial, can be produced with sufficient rapidity, they blend to form a beat-tone of a pitch corresponding to their frequency.

I now pass to the further part of the researches of Dr. Kœnig which relates to the timbre of sounds. Prior to the researches of Dr. Kœnig, it had been supposed that in the reception by the ear of sounds of complex timbre the ear took no account of, and indeed was incapable of perceiving, any differences in phase in the constituent partial tones. For example, in the case of a note and its octave sounded together, it was supposed and believed that the sensation in the ear, when the difference in phase of the two components was equivalent to one-half of the more rapid wave, was the same as when that difference of phase was one-quarter, or three-quarters, or zero. I had myself, in the year 1876, when studying some of the phenomena of binaural audition, shown reason for holding that the ear does nevertheless take cognizance of such differences of phase. Moreover, the peculiar rolling or revolving effect to be noticed in slow beats is a proof that the ear perceives some difference due to difference of phase. Dr. Kœnig is, however, the first to put this matter on a distinct basis of observations. That such differences of phase occur in the tones of musical instruments is certain: they arise inevitably in every case where the sounds of subdivision are such that they do not agree rigidly with the theoretical harmonics. Fig. 5 depicts a graphic record taken by Dr. Kœnig from a vibrating steel wire, in which a note and its octave had been simultaneously excited. The two sounds were scarcely perceptibly different from their true interval, but the higher note was just sufficiently sharper than the true harmonic octave to gain about one wave in 180. The graphic trace has in Fig. 5 been split up into 5 pieces to facilitate insertion in the text. It will be seen that as the phase gradually changes the form of the waves undergoes a slow change from wave to wave. Now, it is usually assumed that in the vibrations of symmetrical systems, such as stretched cords and open columns of air, the sounds of subdivision agree with the theoretical harmonics. For example, it is assumed that when a stretched string breaks up into a nodal vibration of four parts, each of a quarter its length, the vibration is precisely four times as rapid as the fundamental vibration of the string as a whole. This would be true if the string were absolutely uniform, homogeneous, and devoid of rigidity. Strings never are so; and even if uniform and homogeneous, seeing that the rigidity of a string has the effect of making a short piece stiffer in proportion than a long piece, cannot emit true harmonics as the sounds of subdivision. In horns and open organ-pipes the width of the column

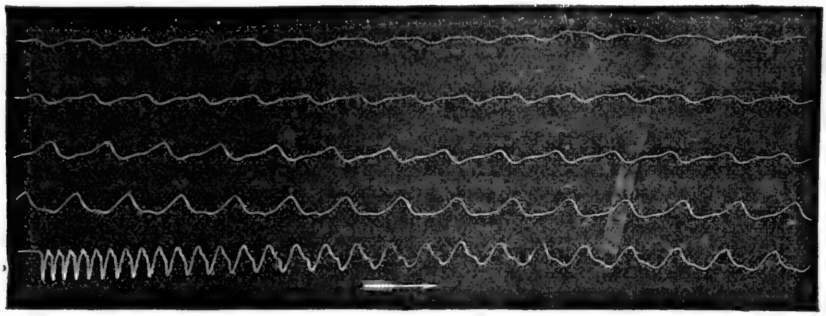


Fig. 5.

(which is usually neglected in simple calculations) affects the frequency of the nodal modes of vibration. Wertheim found the partial tones of pipes higher than the supposed harmonics.

These things being so, it is manifestly insufficient to assume, as von Helmholtz does in his great work, that all timbres possess a purely periodic character; with the necessary corollary that all timbres consist merely in the presence, with greater or less intensity, of one or more members of a series of higher tones corresponding to the terms of a Fourier series of harmonics. When, therefore, following ideas based on this assumption, von Helmholtz constructs a series of resonators, accurately tuned to correspond to the terms of a Fourier series (the first being tuned to some fundamental tone, the second to one of a frequency exactly twice as great, the third to a frequency exactly three times, and so forth), and applies such resonators to analyze the timbres of various musical and vocal sounds, he is trying to make his resonators pick up things which in many cases do not exist—upper partial tones which are exact harmonics. If they are not exact harmonics, even though they exist, his tuned resonator does not hear them, or only hears them imperfectly, and he is thereby led into an erroneous appreciation of the sound under examination.

Further, when in pursuance of this dominant idea he constructs a system of electro-magnetic tuning-forks, accurately tuned to give forth the true mathematical harmonics of a fixed series, thinking therewith to reproduce artificially the timbres not only of the various musical instruments but even of the vowel sounds, he fails to reproduce the supposed effects. The failure is inherent in the instrument; for it cannot reproduce those natural timbres which do not fall within the circumscribed limits of its imposed mathematical principle.

Nothing is more certain than that in the tones of instruments, particularly in those of such instruments as the harp and the pianoforte, in which, the impulse, once given, is not sustained, the relations between the component partial tones are continually changing, both in relative intensity and in phase. The wavelets, as they follow one another, are ever changing their forms: in other words, the motions are not truly periodic—their main forms may recur, but with modifications ever changing.

To estimate the part played in such phenomena by mere differences of phase—to evaluate, in fact, the influence of phase of the constituents upon the integral effect of a compound sound—Dr. Kœnig had recourse to the *wave-siren*, an earlier invention of his own, and of which the wave-disks which have already been shown are examples.

In the first place, Dr. Kœnig proceeded synthetically to construct the wave-forms for tones consisting of the resultant of a set of pure harmonics of gradually decreasing intensity. The curves of these, up to the tenth member of the series, were carefully compounded graphically: first with zero difference of phase, then with all the upper members shifted on one quarter, then with a difference of a half-wave, then with a difference of three-quarters. The results are shown in the top line of curves in Fig. 6, wherein it will be noticed that the curve for difference of phase = $\frac{1}{2}$ is like that for zero difference, but reversed, left for right; and that the curve for difference of phase = $\frac{3}{4}$ is like that for difference = $\frac{1}{4}$, but inverted. Now, according to von Helmholtz, the sounds of all these four curves should be precisely alike, in spite of their differences of form and position. To test the matter, these carefully-plotted curves were set out upon the circumference of a cylindrical band of thin metal, the edge being then cut away, leaving the unshaded portion, the curve being repeated half a dozen times, and meeting itself after passing round the circumference. For convenience, the four curves to be compared are set out upon the separate rims of two such metallic cylindrical hoops, which are mounted upon one axis, to which a rapid motion of rotation can be imparted, as shown in Fig. 7. Against the dentellated edges of these rims, wind can be blown through narrow slits connected to the wind-chamber of an organ-table. In the apparatus (Fig. 7) the four curves in question are the four lowest of the set of six. It will be obvious that, as these curves pass in front of the slits from which wind issues, the maximum displacement of air will result when the slit is least covered, or when the point of greatest depression of the curve crosses the front of the slit. The negative ordinates of the curve correspond, therefore, approximately to condensations. Air is now being supplied to the slits; and when I open one or other of the valves which control the air-passages, you hear one or other of the sounds. It must be audible to everyone present that the sound is louder and more forcible with a difference of phase of $\frac{1}{4}$ than in any other case, that produced with $\frac{3}{4}$ difference being gentle and soft in tone, whilst the curves of phase 0 and $\frac{1}{2}$ yield tones of intermediate quality. Dr. Kœnig found that, if he merely combined together in various phases a note and its octave (which was indeed the instance examined by me binaurally in 1876), the loudest resultant sound is given when the phase-difference of the combination is $\frac{1}{4}$, and the mildest when it is $\frac{3}{4}$.

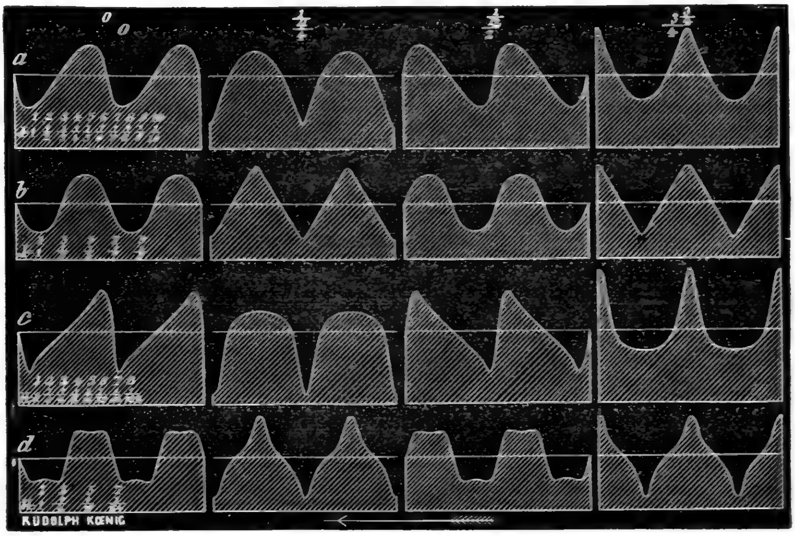


FIG. 6.

FIG. 7.

Returning to Fig. 6, in the second line are shown the curves which result from the superposition of the odd members only of a harmonic series of decreasing amplitude. On comparing together the curves of the four separate phases, it is seen that the form is identical for phases 0 and $\frac{1}{2}$, which show rounded waves, whilst for phases $\frac{1}{4}$ and $\frac{3}{4}$ the forms are also identical, but with sharply angular outline. These two varieties of curve are set out on the two edges of the highest metallic circumference in the apparatus depicted in Fig. 7. The angular waves are found to yield a louder and more strident tone than the rounded waves, though, according to von Helmholtz, their tones should be alike.

A much more elaborate form of compound wave-siren was constructed by Dr. Kœnig for the synthetic study of these phase-relations. Upon a single axis, one behind the other is mounted a series of 16 brass disks, cut at their edges into sinusoidal wave-forms. These represent a harmonic series of 16 members of decreasing amplitude, there being just 16 times as many small sinuosities on the edge of the largest disk as there are of large sinuosities on that of the smallest disk. A photograph of the apparatus is now thrown upon the screen. It is described fully by Dr. Kœnig in his volume "Quelques Expériences," and was figured and described in NATURE, vol. xxvi. p. 277. Against the edge of each of the 16 wave-disks wind can be separately blown through a slit. This instrument therefore furnishes a fundamental sound with its first fifteen pure harmonics. It is clear that any desired combination can be obtained by opening the appropriate stops on the wind-chest; and there are ingenious arrangements to vary the phases of any of the separate tones by shifting the positions of the slits. The following are the chief results obtained with this instrument. If we first take simply the fundamental tone and its octave together, the total resultant sound has the greatest intensity when the difference of phase $\delta = \frac{1}{4}$ (*i.e.* when the maximum displacement of air occurs at the same instant for both waves); and at the same time the whole character of the sound becomes somewhat graver, as if the fundamental tone predominated more than in other phases. The intensity is least when $\delta = \frac{3}{4}$. If, however, attention is concentrated on the octave note while the phase is changed, its intensity seems about the same for $\delta = \frac{1}{4}$ as for $\delta = \frac{3}{4}$, but weaker in all other positions. The compound tones formed only of odd members of the series have always more power and brilliancy of tone for phase differences of $\frac{1}{4}$ and $\frac{3}{4}$, than for 0 and $\frac{1}{2}$; but the quality for $\frac{1}{4}$ is always the same as for $\frac{3}{4}$, and the quality for 0 is always the same as for $\frac{1}{2}$. This corresponds to the peculiarity of the corresponding wave-form, of which the fourth line of curves in Fig. 6 is an example. For compound tones corresponding to the whole series, odd and even, there is, in every case, minimum intensity, brilliancy, and stridence with $\delta = \frac{3}{4}$, and maximum with $\delta = \frac{1}{4}$. Inspection of the first and third lines of curves in Fig. 6 shows that in these wave-forms that phase which is the most forcible is that in which the maximum displacement, and resulting condensation, is sudden and brief.

Observing that wave-forms in which the waves are asymmetrical—steeper on one side than on the other—are produced as the resultant of a whole series of compounded partial tones, it occurred to Dr. Kœnig to produce from a perfect and symmetrical sinusoidal wave-curve a complex sound by the very simple device of turning into an oblique position the slit through which the wind was blown against it. In Fig. 8 is drawn a simple sym-

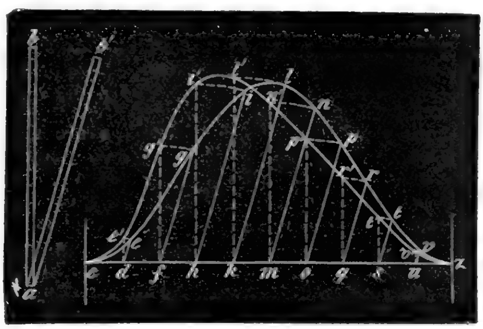


FIG. 8.

metrical wave-form, *eglnprtv*. If a series of such wave-forms is passed in front of a vertical slit, such as *ab*, a perfectly simple tone, devoid of upper partials, is heard. But by inclining the slit, as at *ab'*, the same effect is produced as if the wave-form had been changed to the oblique outline *e'g'l'n'p'r't'v'*, the slit all the while remaining upright. But this oblique form is precisely like that obtained as resultant of a decreasing series of partial tones (Fig. 6, *a*). If the slit be inclined in the same direction as the forward movement of the waves, the quality produced is the same as if all the partial tones coincided at their origin, or with $\delta = 0$; while if inclined in the opposite direction the quality is that corresponding to $\delta = \frac{1}{2}$. It is easy to examine whether the change of phase produces any effect on the sound. Before you is rotating a simple wave-disk, and air is being blown across its edge through a slit. Dr. Kœnig will now tilt the slit alternately backward and forward. On tilting the slit forward to give $\delta = 0$, you hear a purer and more perfect sound; and on tilting it back, giving $\delta = \frac{1}{2}$, a sound that is more nasal and forcible.

All the preceding experiments agree then in showing that differences of phase do produce a distinct effect upon the quality of compound tones: what then must we say as to the effect on the timbre of the presence of upper partial tones or sounds of subdivision that do not agree with any of the true harmonics? A mistuned harmonic—if the term is permissible—may be looked upon as a harmonic which is undergoing continual change of phase. The mistuned octave which yielded the graphic curve in Fig. 5 is a case in point. The wavelets are continually changing their form. It is certain that in a very large number of musical sounds, instrumental and vocal, such is the case.

It was whilst experimenting with his large compound wave-siren that Dr. Kœnig was struck by the circumstance that under no conditions, and by no combination of pure harmonics in any proportion of intensity or phase, could he reproduce any really strident timbres of sound, like those of harmonium reeds, trumpets, and the like; nor could he produce satisfactory vowel qualities of tone. Still less can these be produced satisfactorily by von Helmholtz's apparatus with electro-magnetic tuning-forks, in which there is no control over the phases of the components. The question was therefore ripe for investigation whether for the production of that which the ear can recognize as a *timbre*, a definite unitary quality of tone, it was necessary to suppose that all the successive wavelets should be of similar form. Or, if the forms of the successive wavelets are continually changing, is it possible for the ear still to grasp the result as a unitary sensation?

If the ear could always separate impure harmonic or absolutely inharmonic partials from their fundamental tone, or if it always heard pure harmonics as an indistinguishable part of the unity of the timbre of a fundamental, then we might draw a hard and fast line between mere mixtures of sound and timbres, even as the chemist distinguishes between mere mixtures and true chemical compounds. But this is not so: sometimes the ear cannot unravel from the integral sensation the inharmonious partial; on the other hand, it can often distinguish the presence of truly harmonious ones. Naturally, something will depend on the training of the ear; as is the case with the conductor of an orchestra, who will pick out single tones from a mixture of sounds which to less perfectly trained ears may blend into a unitary sensation.

Dr. Kœnig accordingly determined to make at least an attempt to determine synthetically how far the ear can so act, by building up specific combinations of perturbed harmonics or inharmonic partials, giving rise to waves that are multiform, as distinguished from the uniform waves of a true periodic motion. The wave-siren presented a means of carrying this attempt to a result. On the table before me lie a number of wave-disks constructed with this aim. This will be successively placed upon the whirling table, and sounded: but I must warn you that the proper effects will only be perceived by those who are near the apparatus, and in front of it.

Upon the edge of the first of the series there has been cut a curve graphically compounded of 24 waves as a fundamental, together with a set of four perturbed harmonics of equal intensity. The first harmonic consists of 49 waves (2 × 24 plus 1); the second of 75 waves

(3 × 24 plus 3); the third of 101 (4 × 24 plus 5); the fourth of 127 (5 × 24 plus 7). The resulting curve possesses 24 waves, no two of them alike in form, and some highly irregular in contour. The effect of blowing air through a slit against this disk is to produce a disagreeable sound, quite lacking in unitary character, and indeed suggesting intermittence.

The second wave-disk is constructed with the same perturbed harmonics, but with their amplitudes diminishing in order. This disk produces similar effects, but with more approach to a unitary character.

In the third disk there are also 24 fundamental waves, but there are no harmonics of the lower terms, the superposed ripples being perturbed harmonics of the fifth, sixth, and seventh orders. Their numbers are 6 × 24 plus 6; 7 × 24 plus 7; and 8 × 24 plus 8; being, in fact, three harmonics of a fundamental 25. This disk gives a distinctly dual sort of sound; for the ear hears the fundamental quite separate from the higher tones, which seem in themselves to blend to a unitary effect. There is also an intermittence corresponding to each revolution of the disk, like a beat.

The fourth disk resembles the preceding; but the gap between the fundamental and the three perturbed harmonics has been filled by the addition of three true harmonics. This disk is the first in this research which gives a real timbre, though it is a peculiar one: it preserves, however, a unitary character, even when the slit is tilted in either direction. The 24 waves in this disk all rake forward like the teeth of a circular saw, but with multiform ripples upon them. The quality of tone becomes more crisp when the slit is tilted so as to slope across the teeth, and more smooth when in the reverse direction.

The fifth disk, which is larger, has 40 waves at its edge; these are cut with curves of all sorts, taken hap-hazard from various combinations of pure harmonics in all sorts of proportions and varieties, no two being alike, there maxima and minima of the separate waves being neith isochronous nor of equal amplitude. This disk gives an entirely unmusical effect, amid which a fundamental tone is heard, accompanied by a sort of rattling sound made up of intermittent and barely recognizable tones.

The sixth disk is derived from the preceding by selecting eight only of the waves, and repeatiug them five times around the periphery. In this case each set of eight acts as a single long curve, giving beats, with a slow rotation, and a low tone (accompanied always by the rattling mixture of higher tones) when the speed is increased.

The seventh disk was constructed by taking 24 waves of perfect sinusoidal form, and superposing upon them a series of small ripples of miscellaneous shapes and irregular sizes, but without essentially departing from the main outline. This disk gives a timbre in which nothing can be separated from the fundamental tone, either with vertical or tilted slit.

The eighth and last disk consists of another set of 24 perfect waves, from the sides of which irregular ripples have been carved away by hand, with the file, leaving, however, the summits and the deepest parts of the hollows untouched, so that the maxima and minima are isochronous and of equal amplitude. This disk gives also a definite timbre of its own, a little raucous in quality, but still distinctly having a musical unity about it.

We have every reason, therefore, to conclude that the ear will recognize as possessing true musical quality, as a timbre, combinations in which the constituents of the sound vary in their relative intensity and phase from wave to wave.

What, then, is *a timbre?* Dr. Kœnig would be the first to recognize that these last experiments, though of deepest interest, do not afford a final answer to the question. We may not yet be in a position to frame a new definition as to what constitutes a timbre, but we may at least conclude that, whenever that definition can be framed, it will at least include several varieties, including the non-periodic kinds with multiform waves, as well as those that are truly periodic with uniform waves. We must not on that account, however, rush to the conclusion that the theory of von Helmholtz as to the nature of timbre has been overthrown. The corrections introduced into lunar theory by Hansen and Newcomb have not overturned the splendid generalizations of Newton. What we can and must confess is that we now know that the acoustic theory of von Helmholtz is, like the lunar theory of Newton, correct only as a first approximation. It has been the distinctive merit of Dr. Kœnig to indicate to us the magnitude of the correcting terms, and to supply us not only with a rich store of experimental facts but with the means of prosecuting the research synthetically beyond the point to which he himself has attained.

In thanking Dr. Kœnig for the courtesy which he has shown to this Society in bringing over his apparatus and in demonstrating its use to us, we must join in congratulating him on the patience, perspicacity, and skill with which he has carried out his researches. We know that his exceptional abilities as experimentalist and constructor have done more than those of any other investigator to make the science of experimental acoustics what it is to-day; and we must unite in wishing him long life and prosperity to complete the great work on which already he has advanced so far.

THE GEOLOGY OF ROUND ISLAND.

BOTANISTS have been a little perplexed as to the exact geological age of some of the islands in the Indian Ocean, and the current statements on the subject are by no means accordant. Any exact information on the subject seems, therefore, worth placing on record.

Round Island is a minute island, about two miles in diameter, which lies north-east of Mauritius, and about 13 miles distant. It was visited last November by Mr. William Scott, Assistant to the Director of Forests and Botanical Gardens, Mauritius, accompanied by Surgeon H. H. Johnston.

There is only one point where landing can be effected in calm weather; the end of November was therefore chosen for the visit as being the best season for landing. Unfortunately, the season at this time was very dry, so that the whole of the vegetation was in a dormant state, and little could be done in the way of procuring good specimens.

"The only trees," Mr. Scott writes, "of any size to be found on the island, are the Palms:—*Latania Loddigesii*, *Hyophorbe amaricaulis*, *Dictyosperma alba*, var. *aurea*, and the screw-pine *Pandanus Vandermeeschii*. These trees look stunted, and grow in clumps mixed up together where any root-hold is to be found. There were only at that time two or three grasses to be found, and these were in a very dry state. One or two Passifloras [probably introduced] and *Asclepiadaceæ* were also found. On these two latter, the wild goats which inhabit the island in hundreds, appear to exist."

Mr. Scott made a very careful collection of the rocks composing the island. His specimens were sent to Kew and were submitted to Prof. Judd, F.R.S., who very kindly examined them. He furnished me with the following report, which he has kindly allowed me to publish. W. T. Thiselton Dyer.

Royal Gardens, Kew, December 4.

Science Schools, South Kensington, S.W., September 29.

My dear Dyer,—Immediately upon my return to town I have, as I promised you, examined the specimens from Round Island, with the notes on the same by Mr. Scott. My conclusions are as follows:—

The rocks of the island appear to be entirely of organic or volcanic origin.

The lower part of the island consists of a limestone made up of shell and coral (?) detritus, cemented into a hard and crystalline rock (see specimens 1, 3, 15). Masses of this rock of greater hardness than usual seem to have resisted denudation, and stand up as pinnacles among the overlying stratified tuffs (specimen 14). Fissures in the various rocks are filled with a stalagmitic deposit of calcic carbonate (specimen 8). But the great mass of the island would appear to be made up of stratified *palagonite tuffs*, some coarse-grained, others very fine-grained. These palagonite tuffs, which closely resemble the similar rocks in Sicily and Iceland, are very beautiful and interesting varieties; and, besides the hydrous basic glass known as palagonite, contain various beautiful zeolites and other minerals (specimens 4, 5, 6, 7, 9, 10, 11, 12).

The lavas associated with these tuffs appear to occur both as ejected blocks and in lava-currents, and consist of a highly scoriaceous basalt rich in olivine (specimens 2 and 13).

These are the general facts concerning the structure of the island which are revealed by a general examination of the specimens with Mr. Scott's interesting notes.

I return these last with this letter. The specimens I retain for the present, as some among them will, I think, repay a minuter study than I have yet been able to afford time for. JOHN W. JUDD.

NOTES.

THE Medals and Funds to be given at the anniversary meeting of the Geological Society of London on February 20 have been awarded by the Council as follows: the Wollaston Medal to Prof. J. W. Judd, F.R.S.; the Murchison Medal to Prof. W. C. Brögger, of Christiania; the Lyell Medal to Prof. T. McKenny Hughes, F.R.S.; and the Bigsby Medal to Dr. G. M. Dawson; the balance of the Wollaston Fund to Mr. R. Lydekker; that of the Murchison Fund to Mr. R. Baron; and portions of the Lyell Fund to Messrs. C. J. Forsyth Major and G. W. Lamplugh.

AT the meeting of the Institution of Civil Engineers on Tuesday, M. Carnot, President of the French Republic, was elected an honorary member.

THE forty-fourth annual general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, January 29, and Friday evening, the 30th, at 25 Great George Street, Westminster. The chair will be taken by the President, Mr. Joseph Tomlinson, at half-past seven p.m. on each evening. After the presentation of the annual report, and the transaction of other business, the following papers will be read and discussed, as far as time permits:—On some different forms of gas furnaces, by Mr. Bernard Dawson, of Malvern; on the mechanical treatment of moulding sand, by Mr. Walter Bagshaw, of Batley; fourth report of the research committee on friction: experiments on the friction of a pivot bearing.

THE Royal Microscopical Society will hold its annual meeting on Wednesday evening, the 21st inst., at 8 o'clock. The President will deliver an address.

THE annual general meeting of the Royal Meteorological Society will be held at 25 Great George Street, Westminster, on Wednesday, the 21st inst., at 8.15 p.m. It will be preceded by an ordinary meeting, beginning at 7 p.m., at which the following papers will be read:—Note on a peculiar development of cirrus cloud observed in Southern Switzerland, by Mr. Robert H. Scott, F.R.S.; and some remarks on dew, by Colonel W. F. Badgley, F.R.Met.Soc.

THE Council of the Royal Meteorological Society have arranged to hold at 25 Great George Street, Westminster, from March 17 to 20, an Exhibition of rain gauges, evaporation gauges, percolation gauges, and kindred instruments. The Exhibition Committee invite co-operation in this undertaking. They will also be glad to show any new meteorological instruments or apparatus invented, or first constructed, since last March; as well as photographs and drawings possessing meteorological interest. Anyone willing to co-operate in the proposed Exhibition should furnish Mr. William Marriott, the Secretary, not later than February 10, with a list of the articles he will be able to contribute, and an estimate of the space they will require.

THE Sanitary Institute has made arrangements for a course of sixteen lectures for the special instruction of those who may wish to obtain knowledge of the duties of sanitary officers. These lectures, which will be a continuation of previous courses, will be delivered at the Parkes Museum, and will be illustrated with diagrams, drawings, and models. The first lecture will be given on January 30 by Sir Douglas Galton, who will deal with ventilation, warming, and lighting. Among the other lecturers will be Mr. H. Law, Dr. Louis Parkes, and Prof. W. H. Corfield.

DR. SYMES THOMPSON will deliver at Gresham College, on January 20 and the three following days, a course of four lectures on the preservation of health. He will deal with questions relating to healthy homes, healthy schools, and the early detection and prevention of disease.

MR. A. SIDNEY OLLIFF, late assistant in the Museum, Sydney, has been appointed to the newly-instituted office of Government Entomologist in the Department of Agriculture, New South Wales. His duties will be chiefly the study of insects affecting fruits and crops, whether injurious or beneficial, and publishing reports on the results for the information of farmers and horticulturists. According to the latest news as to the new insect pest, Mr. Olliff will not lack employment.

M. PATOUILLARD has been elected President, and MM. Prillieux and de Seynes, Vice-Presidents, of the Société Mycologique de France.

THE death is announced of M. Clavaud, Professor of Botany at Bordeaux, author of the "Flora de la Gironde," of which two parts only have appeared, embracing the *Thalamifloræ* and the *Calycifloræ*. The work is characterized by its beautiful plates, and by the attempts to place on a scientific basis the genetic relationship of the various species with one another.

MR. GEORGE W. ORMEROD, of Teignmouth, died on January 6, at the age of eighty. He had been a member of the Geological Society of London for fifty-eight years, and geologists owe much to him for his classified index to the *Quarterly Journal* and other publications.

ACCORDING to a telegram from Rusk, Texas, two sharp earthquake shocks were felt there on January 8. Chimneys were thrown to the ground, and people awakened by the violence of the oscillations.

AT the meeting of the French Meteorological Society on December 2, M. Angot communicated the results of comparisons of the pressure and temperature observations at the Eiffel Tower during 1889 with the low-level stations. The night maxima and minima of pressure are less marked on the Tower than at the lower levels, as is also the case with mountain observations, and inversely for the day maxima and minima. For individual readings small variable differences were found between the actual pressure at the summit and the pressure calculated from observations at the base. Generally, during rapid falls of the barometer, the true pressure at the summit is lower than the calculated value, and higher when the barometer is rising. The temperature observations will be referred to subsequently. At the meeting of December 16 a discussion took place upon the trustworthiness of aneroid barometers, owing to their not always immediately returning to their original condition after a sudden rise or fall. The balance of opinion was in their favour. M. Doumet-Adanson communicated an account of a tornado at Fourchambault on October 1, 1890. Its sudden appearance there is supposed to be due to one of a series of bounds and rebounds which these meteors are sometimes reported to make. M. Angot was elected President of the Society for the ensuing year.

La Nature of January 3 describes a useful recording barometer brought out by MM. Redier and Meyer. In the upper part of a case with glass front is an aneroid, the elastic cover of which is connected with a metallic slip projecting over rotated paper below. At the end of this slip is a conical ink-holder with perforated point. Three times in the hour this descends to make a dot on the paper, and the dots form a curve of the variations of pressure. The instrument goes eight days without needing attention. For observers who do not want to keep a long record, but merely to compare the variations of the day with those of a few days previous, a recording cylinder with smooth white surface is supplied, on which the record can be wiped out. This number of *La Nature* has also an account of a new steam carriage for ordinary roads, which appears to work satisfactorily.

IN his presidential address to the Queensland Royal Society, delivered on November 22, 1890, Mr. W. Saville-Kent spoke strongly in favour of the establishment of a well-appointed zoological station and marine biological laboratory in Queensland. The best site, he said, would be Thursday Island. Situated in Torres Straits, at a distance of twenty miles only from Cape York Peninsula, with a climate far more temperate than that of the mainland, and serving as a weekly or bi-weekly port of call to various lines of mail-steamers, this island is "a perfect paradise for the naturalist." In it is the central depot of the Torres Straits pearl and pearl-shell and the *bêche-de-mer* fisheries. Mr. Saville-Kent is of opinion that the acquirement of accurate knowledge concerning these products, with the application of approved methods of scientific culture, would very soon immensely increase their export value.

IN the current number of the *Revue Scientifique*, M. Hermann Fol has an interesting paper on what he calls the resemblances between husband and wife. The fact that such resemblances are not uncommon has often been noted, but the usual idea is that when they occur they are to be explained by the influence which husband and wife exert on one another in the course of years. M. Fol, however, maintains that in a large number of cases there is a more or less striking likeness from the beginning; and he draws the conclusion that in such instances the mutual attraction which leads to marriage is due to the qualities the lovers have in common, not to those in which they differ. His attention was first directed to the subject in Nice, which is visited by many newly-married people. He was so struck by the resemblances which he observed, or thought he observed, that he obtained the photographs of 251 couples who were not personally known to him, and in each case carefully noted the appearance of husband and wife. The general results he presents as follows:—

Couples.	Resemblances.		Non-resemblances.		Total.
		Per cent.		Per cent.	
Young ...	132 :	about 66·66 ...	66 :	about 33·33 ...	198
Old ...	38 :	,, 71·70 ...	15 :	,, 28·30 ...	53

THE Russian painter Krilof has for some time been engaged in painting the portraits of typical representatives of the various races included in the Russian Empire. In carrying out his purpose, he has undertaken many long journeys; and he has now a small gallery which ought to be of considerable value from an anthropological as well as from an artistic point of view. In the current number of *Globus* there is a good reproduction of one of these portraits. It represents with much force and insight a Kasak-Kirghiz.

THE director of the central dispensary at Bagdad has sent to *La Nature* a specimen of an edible substance which fell during an abundant shower in the neighbourhood of Merdin and Diarbékir (Turkey in Asia) in August 1890. The rain which accompanied the substance fell over a surface of about ten kilometres in circumference. The inhabitants collected the "manna," and made it into bread, which is said to have been very good and to have been easily digested. The specimen sent to *La Nature* is composed of small *sphérules*; yellowish on the outside, it is white within. Botanists who have examined it say that it belongs to the family of lichens known as *Lecanora esculenta*. According to Decaisne, this lichen, which has been found in Algeria, is most frequently met with on the most arid mountains of Tartary, where it lies among pebbles from which it can be distinguished only by experienced observers. It is also found in the desert of the Kirghizes. The traveller Parrot brought to Europe specimens of a quantity which had fallen in several districts of Persia at the beginning of 1828. He was assured that the ground was covered with the substance to the height of two decimetres, that animals ate it eagerly, and that it was collected by the people. In such cases it is supposed to have been caught up by a waterspout, and carried along by the wind.

MR. E. W. CARLIER writes in the current number of the *Entomologist* that one of the most remarkable features of the entomological record of 1890 was the extraordinary abundance of autumn larvæ. In a garden in Norwich, where he was staying during the month of September, everything was infested with larvæ, even to the ferns, which were in many cases almost entirely stripped of their green parts. The many-coloured larvæ of *Orgyia antiqua* (the common vapourer) were by far the most abundant, proving a perfect nuisance by their curious habit of constantly flinging themselves to the ground, from a wisteria arbour which spans the path. What could induce the insects to act in this manner he was unable to ascertain, as there was nothing but an iron garden-seat and the pebbles of the path to tempt them. This falling was not produced by wind or birds, or any other obvious cause, for, during perfect stillness of all their surroundings, they would fall by dozens upon the unfortunate occupants of the garden-chair. Nor was the habit confined to any particular age or period of the larva's existence, for among those that fell were small, large, and intermediate-sized individuals. Moreover, this habit caused a great mortality among them, for they were no sooner fairly down than they began to make for a white-washed wall which forms one boundary of the path, and attempt to climb up again to the arbour from which they fell. It so happened that many small garden spiders had elected to weave their webs from this wall to the iron framework of the arbour, and as the larvæ came to this part of their journey they often became entangled in the webs, were captured, and preyed upon by the small spiders.

THE *Zoologist* for January contains an interesting paper, by Messrs. W. E. Clarke and E. H. Barrett-Hamilton, on the Irish rat, which is "a melanistic form of *Mus decumanus*." The authors point out that this creature has a peculiar geographical distribution. In Ireland it is widely distributed, and not rare. The only known British localities in which it occurs are in the Outer Hebrides, where it has long been known to the inhabitants, and whence the authors have examined three specimens. It would appear to be quite unknown on the mainland of Britain, where all their endeavours to procure specimens have failed, though they would not be surprised to hear of the occurrence of a melanism of so common a creature as the brown rat. On the continent of Europe the only instance of the occurrence of black varieties of *Mus decumanus* known to the authors is the one recorded by A. Milne-Edwards (*Ann. Sci. Nat.*, 1871, xv. art. 7) for Paris, where, in 1871, it had been known for twenty years in the menagerie of the Museum, and is described as abundant and increasing in numbers.

PROF. J. MARK BALDWIN, writing in *Science* on "infant psychology," points out that this branch of inquiry has the great advantage of offering opportunities for the use of the experimental method. In experimenting on adults, psychologists are confronted with the difficulty that reactions are broken at the centre, and closed again by a conscious voluntary act. The subject hears a sound, identifies it, and presses a button. What goes on between the advent of the incoming nerve process and the discharge of the outgoing nerve process? Something, at any rate, which represents a brain process of great complexity. Anything that fixes this sensori-motor connection, or simplifies the central process, in so far gives greater certainty to the results. For this reason, experiments on reflex reactions are valuable and decisive where similar experiments on voluntary reactions are uncertain and of doubtful value. The fact that the child consciousness is relatively simple, and so offers a field for more fruitful experiment, is seen in the mechanical reactions of an infant to strong stimuli, such as bright colours. Of course, this is the point where originality may be exercised in the devising and executing of experiments. After the subject is a little better developed, new experimentation will be as difficult here as in the other sciences; but at present the simplest phenomena of child life and activity are open to the investigator.

THE "Year-book of Pharmacy" has just been published. It contains abstracts of papers relating to pharmacy, materia medica, and chemistry, contributed to British and foreign journals from July 1, 1889, to June 30, 1890; with the transactions of the British Pharmaceutical Conference at the twenty-seventh annual meeting, held at Leeds in September 1890. The volume has been carefully compiled, and presents a great mass of scientific information. Its utility is greatly increased by a full index.

MESSRS. IMRAY AND SON have issued, for the use of candidates preparing for the Board of Trade examinations, a syllabus of examination in the laws of compass deviation and in the means of compensating it, with explanatory notes and answers, by W. H. Rosser. The paper was written as an appendix to the author's "Deviation of the Compass, considered practically."

MESSRS. DULAU AND CO. have issued a catalogue of zoological and palæontological works which they are offering for sale. The list includes many valuable books.

THE cadmium and magnesium analogues of zinc methide and ethide have been prepared and investigated by Dr. Löhr in the laboratory of Prof. Lothar Meyer at the University of Tübingen, and an account of the work is published in the latest number of *Liebig's Annalen*. The alkyl compounds of cadmium and magnesium have formed the subject of several previous researches, but the results hitherto obtained have been mainly negative, and nothing was known with certainty concerning them. The methide and ethide of cadmium are liquid bodies, spontaneously inflammable when gently warmed. The similar compounds of magnesium are, contrary to expectation, solid substances, which possess the almost unique property of being spontaneously inflammable in carbon dioxide gas, as well as in air, being capable of actually extracting oxygen from its most stable combination with carbon. Cadmium methide was obtained by the following process:—Metallic cadmium, either in filings or pieces of sticks, was placed in a tube closed at one end together with a thin glass bulb containing methyl iodide. The tube was exhausted by the Sprengel pump and sealed. It was then heated to 110° C. for 24 hours. At the end of this time all the methyl iodide had disappeared, and the tube contained a crystalline mixture of cadmium iodide and cadmium methyl iodide, $Cd(CH_3)I$, and gaseous ethane, C_2H_6, at high pressure. After opening the tube so as to permit of the escape of the ethane, the crystalline residue was subjected to dry distillation; when the temperature of the paraffin bath in which the tube was heated reached 260° a small quantity of a liquid distilled over, and was caught in a receiver filled with carbon dioxide. Ten similar experiments were made, and the total product of this liquid was fractionally distilled in a small apparatus also filled with carbon dioxide. By this means the greater part of the admixed methyl iodide was removed, and a heavy residual liquid obtained which was found to consist mainly of cadmium methide, $Cd(CH_3)_2$. Cadmium methide is a clear heavy liquid of most unpleasant odour, violently affecting the respiratory organs, and producing a most persistent nauseous metallic taste in the mouth. It boils at about 105° C. On gently warming it spontaneously inflames, burning with a brilliant sooty flame producing thick clouds, which, according to the amount burnt, take a dark green or a reddish-brown tint. It oxidizes very rapidly to a white mass of ethylate, $Cd(OCH_3)_2$. It reacts most violently with water, producing great heat, and evolving marsh gas. Dilute hydrochloric acid acts similarly, methane being evolved and cadmium chloride formed:

$$Cd(CH_3)_2 + 2H_2O = Cd(OH)_2 + 2CH_4;$$
$$Cd(CH_3)_2 + 2HCl = CdCl_2 + 2CH_4.$$

The liquid solidifies to a white crystalline mass when the vessel containing it is immersed in a freezing mixture. Cadmium ethide is prepared with greater difficulty than the methide. It is a liquid much resembling the methide in properties. Magnesium methide is a solid obtained, mixed with magnesium iodide, when magnesium filings or ribbon are heated in a sealed tube with methyl iodide and acetic ether, methyl iodide alone having no action. It is also obtained mixed with globules of mercury when magnesium is heated with mercury methide in a sealed tube. Its most remarkable properties are its intense action with water, incandescence usually occurring with ignition of the evolved gas; and its spontaneous inflammability in carbon dioxide, the combustion being accompanied by beautiful scintillations. The ethide is a very similar substance.

THE additions to the Zoological Society's Gardens during the past week include two Toque Monkeys (*Macacus pileatus*), a Starred Tortoise (*Testudo stellata*) from Ceylon, presented by Mr. W. J. Bosworth; a Peregrine Falcon (*Falco peregrinus*), British, presented by Mr. A. C. Ionides; a Humboldt's Lagothrix (*Lagothrix humboldti*) from the Upper Amazons, purchased.

OUR ASTRONOMICAL COLUMN.

SPECTROSCOPIC OBSERVATIONS OF SUN-SPOTS.—The *Monthly Notices of the Royal Astronomical Society* for December 1890 contains the main conclusions deduced by the Rev. A. L. Cortie, S.J., from the sun-spot observations made at the Stonyhurst College Observatory in the years 1882–89. The region of the spectrum in which observations have been made is between the lines B and D. The widening of the lines in the sun-spot spectra observed has generally been reckoned in tenths of their normal breadth, and are classified as "widened," "more widened," and "most widened," for comparison among themselves. At South Kensington only the six most widened lines in the spectrum of a sun-spot are recorded, hence the two sets of observations are not easily comparable. The interval 1882–89 has been divided into two periods in the discussion, viz. the disturbed period of solar activity extending from 1882–86, and the quiet period 1886–89.

In the disturbed period only one of the fifty-three iron lines in the region B—D was observed to have a mean widening. During the quiet period, however, many more iron lines appeared among the more widened. In this particular, therefore, Father Cortie's results confirm the conclusion arrived at by Prof. Lockyer in 1880 from a similar discussion. With respect to other substances, the observations show that seven out of the eleven titanium lines in the region studied were very much affected in the spot spectra at both periods, the lines most persistently widened being among the faintest Fraunhofer lines, and among the brighter of the metallic lines. The mean widening of calcium lines increased slightly during the minimum period. Sodium lines were much affected in the maximum epoch, especially in the large spots. Several lines given by Ångström as "telluric" have been seen widened. The C line has often appeared less dark over sun-spots, but when bright the reversal was generally due to faculæ between the spots. From a total of 2088 individual observations of other lines it is concluded that (1) About the maximum period a great number of faint lines not in Ångström are to be seen in sun-spots. (2) Such lines are not seen exclusively in maximum spots, but reappear in minimum spots when they are large. (3) Some faint lines which have been persistently watched are to be seen greatly widened in every sun-spot, large or small, whether in the disturbed or quiet period. (4) The mean widening of all the five bright chromospheric lines coincident with unknown lines in this region has been low. A Browning automatic spectroscope with a dispersion of twelve prisms of 60° was used for the observations.

TURIN OBSERVATORY.—Some publications of interest have recently been issued from this Observatory. Signor Porro gives the results of observations of the magnitude of the star U (Nova) Orionis throughout a whole period of variation. On November 21, 1889, the star was 8·81 mag.; on April 28, 1890, it was 8·80; and a maximum magnitude = 5·80 was observed on January 21, 1890. Mr. Chandler has given the period of this variable as 371 days, with a maximum on December 7, 1885. Signor Porro finds that the observations made in 1885, in conjunction with those now given, indicate a period of 378·5 days from the epoch December 7.

A large number of determinations of the latitude of Turin has also been made. The mean of 120 observations results in the value $\phi = 45° 4' 7''·942 \pm 0''·029$. The observations do not exhibit the periodic variation observed in the latitude observations made at Berlin, Potsdam, and Prague.

Convenient ephemerides for the sun and moon in 1891 have been calculated for the meridian of Turin by Signor Aschieri. The meteorological observations made in 1889 have been tabulated by Dr. G. B. Rizzo.

THE DUPLICITY OF α LYRÆ.—The duplication of the K line in some photographs of the spectrum of Vega taken by Mr. A. Fowler, and from which he inferred that the star was a spectroscopic double of the β Aurigæ type, has not been confirmed by photographs taken by Prof. Pickering, Prof. Vogel, and MM. Henry. Some other explanation must therefore be found to account for the phenomenon.

GASEOUS ILLUMINANTS.[1]

II.

ORDINARY coal gas of an illuminating power of 14 to 16 candles can be produced at a fairly low rate, but if a higher quality is required considerable additional expense has to be incurred in order to enrich it. Up to now, the material almost universally employed for this purpose has been cannel; but as this article is rapidly rising in price, and the best qualities are not easily obtainable, attention is being seriously directed to other means of bringing up the illuminating power of gas. This question of enrichment has been the study of inventors from the earliest days of the gas industry. The methods employed for this purpose may be classified as:—(1) The carburetting of low-power gas by impregnating it with the vapour of volatile hydrocarbons. (2) Enriching the gas by vapours and permanent gases obtained by the decomposition of the tar formed at the same time as the gas. (3) Mixing with the coal gas, oil gas obtained by decomposing crude oils by heat. (4) Mixing with the coal gas, water gas which has been highly carburetted by passing it, with the vapours of various hydrocarbons, through superheaters, in order to give permanency to the hydrocarbon gases.

In the first method, many points have to be taken into consideration, as the hydrocarbons which have from time to time been used for this purpose, vary so greatly in composition; a very volatile naphtha, although it evaporates quickly, and larger quantities of its vapour are taken up by the gas, often giving a less increase of luminosity than a heavier hydrocarbon of which but little is vaporized.

The great trouble which presented itself in the older carburetting systems was that all the commercial samples of naphtha are mixtures of various hydrocarbons, each having its own boiling-point, and that therefore, when used in any of the old forms of carburetters, they gave up their more volatile constituents very freely at the beginning of the experiment, while the amount rapidly diminished as the boiling-point of the residue became higher; so that when 2113 cubic feet of poor coal gas were passed through a naphtha having a specific gravity of 0·869 and a boiling-point of 103° C., the temperature during the experiment being 22° C., the first 80 cubic feet of gas took up 23·2 grains of the naphtha, while the last 450 cubic feet only took up 7·3 grains. Another difficulty found was the increase of evaporation with the rise in the temperature of the gas; as with an ordinary form of carburetter, exposed to atmospheric changes, the enrichment of the gas, which reached 54·4 per cent. in summer with an average temperature of 22° C., fell in winter to only 22 per cent. with an average temperature of 3° C. Of course, in these carburetters a good deal depended upon the form of apparatus; and it was found, on trying different shapes with the same naphtha, that when the gas merely flowed through a box containing a layer of it, only about 3·2 grains were taken up; while with a carburetter in which the naphtha was sucked up by cotton fibre, so as to expose a large surface to the gas, as much as 22 to 23 grains were absorbed. One of the most important points noticed during these experiments was, that it was only a poor gas which could be enriched in this manner, and that if a rich cannel gas was passed through the naphtha, it became robbed of some of its illuminating power.

It must be clearly borne in mind, in approaching this subject, that the evaporation of a hydrocarbon into a permanent gas—*i.e.*, a gas which does not liquefy within the ordinary range of temperature—is a question neither of specific gravity nor of boiling-point, although the latter has more to do with it than the former. It is purely a question of vapour tension. Most liquids, when left to themselves in contact with the atmosphere gradually pass into the state of vapour, and disappear; and those which evaporate most quickly are said to be most volatile. If ether, for example, is dropped upon an exposed surface, it at once disappears, and causes, by its evaporation, considerable cold; and the lightest forms of naphthas do the very same thing. But although this evaporation takes place with rapidity with liquids of low boiling-point, it must not be forgotten that even many solids have the same property—naphthalene, camphor, and iodine being cases in point. It must also be remembered that evaporation occurs over a very wide range of temperature; but that for each substance there is a limit below which evaporation does not seem to take place. So that, when considering the suitability of a liquid for carburetting in this way, it is far more important to determine its vapour tension than its specific gravity or its boiling-point.

So far all systems for carburetting gas with liquid hydrocarbons at the burners have proved failures, but in the albocarbon light the vapour of naphthalene is caused to mingle with the gas just before combustion, the volatilization being effected by a spur of metal heated by the flame itself, which conducts the heat back into a chamber containing solid naphthalene, through which the gas passes, and this process has proved very successful.

Any system to be generally adopted must be applied to the gas in bulk before distribution. In doing this, there are two factors to be considered: the vapour added must be in such proportion to the gases which have to carry them that no fear need exist of their being deposited by any sudden cooling of the gas;

[1] Continued from p. 235.

and care must be taken that the vapour added is not in sufficient quantity to throw out of suspension the volatile hydrocarbons in the gas. The carrying power of a gas depends upon its constituents; for in the same way that liquids vary in their power of dissolving and carrying—*i.e.*, keeping in solution—solids, so do gases vary in their power of bearing away the more volatile hydrocarbons. If the carrying power of air is taken as unity, then the power of ordinary coal gas is about 1·5, while hydrogen would be nearly 3·5; and it is manifest that attention must be paid to the ratio of the constituents present, if gases of varying composition are to be carburetted to the same degree.

During the past few months the idea of the possibility of carburetting coal gas in bulk has again been revived by the construction of an extremely ingenious apparatus, the outcome of the combined engineering skill and practical experience of Messrs. Maxim and Clark, which obviates, to a very great extent, the difficulties which arose with the older forms of carburetter. It has been shown that, when carburetting a gas with a gasoline or light naphtha spirit, the more volatile portions enrich the gas to an undue extent at first, and that, as the process continues, the amount taken up becomes gradually less. This would not so much matter in carburetting the gas in bulk before it went into the holder, as it would become to a great extent mixed by diffusion, and a gas of fairly even illuminating power would result; but the Maxim-Clark apparatus is intended not only to do this, but also to carburet the gas used in large establishments and works.

This apparatus is of such a form that in small installations the whole of the gas to be used can be passed through, and each portion supplied with its own share of hydrocarbon, whilst when carburetting gas in bulk a certain portion can be withdrawn from the main, carburetted and again return it to the main, where, mingling with the steady flow of gas, the whole becomes of uniform composition.

In the earliest days of the gas industry, attempts were made to utilize tar for the production and enrichment of gas; and the patent literature of the century contains many hundred such schemes, most of them being still-born, while a few spent a short and sickly existence, but none achieved success. The reason of this is not difficult to understand. In order to make gas from tar, two methods may be adopted: either to condense the tar in the ordinary way, and afterwards use the whole or portions of it for cracking into a permanent gas; or to crack the tar vapours before condensation by passing the gas and vapours through superheaters. If the first method is adopted, the trouble which at once presents itself, and in a few hours brings the apparatus to grief, is that tar contains 60 per cent. of pitch, which rapidly chokes and clogs up all the pipes; while, if an attempt is made to use a temperature at which the pitch itself is decomposed, it is found that a non-luminous or very poorly luminous gas is the result, and that a heavy deposit of carbon remains in the superheater and retort, and even at high temperatures easily condensable vapours escape, to afterwards create trouble in the pipes.

The most successful attempt to utilize certain portions of the liquid products of the distillation of coal is undoubtedly the Dinsmore process, in which the coal gas and the vapours which, if allowed to cool, would form tar, are made to pass through a heated chamber, and a certain proportion of otherwise condensable hydrocarbons are thus converted into permanent gases. Using a poor class of coal, it is claimed that 9800 cubic feet of 20 to 21 candle gas can be made by this process; while by the ordinary system 9000 cubic feet of 15-candle gas would have been produced.

In distilling the coal in the ordinary way, the yield of tar is 11 gallons per ton; but by the Dinsmore process only 7 gallons. On examining the analysis of the ordinary and Dinsmore tar, it is at once evident that the 4 gallons which have disappeared are the chief portions of the light oils and creosote oils; and these are the factors which have given the increase of illuminating power to the gas.

In enriching a poor coal gas by injecting paraffin oil into the retort during distillation, it must be borne in mind that, as the coal is undergoing distillation, in the earlier stages a rich gas is given off, while towards the end of the operation the gas is very poor in illuminants and rich in hydrogen; the methane disappearing with the other hydrocarbons, and the increase in hydrogen being very marked. Mr. Lewis T. Wright employed a coal requiring six hours for its distillation, and took samples of the gas at different periods of the time. On analysis, these yielded the following results:—

Time after commencement of distillation.	10 min.	1h. 30m.	3h. 25m.	5h. 35m.
Sulphuretted hydrogen...	1·30	1·42	0·49	0·11
Carbon dioxide	2·21	2·09	1·49	1·50
Hydrogen	20·10	38·33	52·68	67·12
Carbon monoxide	6·19	5·68	6·21	6·12
Marsh gas	57·38	44·03	33·54	22·58
Illuminants	10·62	5·98	3·04	1·79
Nitrogen...	2·20	2·47	2·55	0·78

This may be regarded as a fair example of the changes which take place in the quality of the gas during the distillation of the coal. In carburetting such a gas by injecting paraffin into the retort, it would be a great waste to do so for the first two hours, as a rich gas is being given off which has not the power of carrying a very much larger quantity of hydrocarbons—being practically saturated with them. Consequently, to make it take along with it, in a condition not easily deposited, any further quantity, the paraffin would have to be broken down to a great extent; and the temperature necessary to do this would seriously affect the quality of the gas being given off by the coal. When, however, the distillation had gone on for three hours, the rich portions of the coal gas would all have distilled off, and the temperature of the retort would have reached its highest point; and this would be the time to feed in the oil, as its cracking being an exothermic action, the temperature in the retort would be increased, and the gas rich in hydrogen which was being evolved would carry with it the oil gas, and prevent any re-deposition.

When carbon is acted upon at high temperatures by steam, the first action that takes place is the decomposition of the water vapour; the hydrogen being liberated, while the oxygen unites with the carbon to form carbon dioxide, thus—

$$\underset{\text{Carbon.}}{C} + \underset{\text{Water.}}{2(H_2O)} = \underset{\text{Carbon dioxide.}}{CO_2} + \underset{\text{Hydrogen.}}{2H_2}$$

The carbon dioxide so produced interacts with more red-hot carbon, forming the lower oxide, carbon monoxide, thus—

$$CO_2 + C = 2CO.$$

So that the completed reaction may be looked upon as yielding a mixture of equal volumes of hydrogen and carbon monoxide—both of them inflammable, but with non-luminous flames. This decomposition, however, is rarely completed, and a certain proportion of carbon dioxide is invariably to be found in the water gas, which, in practice, generally consists of a mixture of about the following composition:—

Hydrogen	48·31
Carbon monoxide	35·93
Carbon dioxide	4·25
Nitrogen	8·75
Methane	1·05
Sulphuretted hydrogen	1·20
Oxygen	0·51
	100·00

The above is an analysis of water gas made from gas coke in a Van Steenbergh apparatus. The ratio of carbon monoxide and carbon dioxide present depends entirely upon the temperature of the generator and the kind of carbonaceous matter employed. With a hard dense anthracite coal, for instance, it is quite possible to attain a temperature at which there is practically no carbon dioxide produced; while with an ordinary form of generator, and a loose fuel like coke, a large proportion is generally to be found. The sulphuretted hydrogen in the analysis quoted is, of course, due to the high amount of sulphur to be found in the gas coke, and is practically absent from water gas made with anthracite. The nitrogen is due to the method of manufacture; the coke being, in the first instance, raised to incandescence by an air-blast, which leaves the generator and pipes full of a mixture of nitrogen and carbon monoxide (producer gas), which is carried over by the first portions of water gas into the holder. The gas so made has no photometric value—its constituents being perfectly non-luminous; and attempts to use it as an illuminant have all taken the form of incandescent burners, in which thin "mantles" or "combs" of highly refractory metallic oxides are heated up to incandescence. In the case of carburetted water gas, the gas is only used as a carrier of illuminating hydrocarbon gases made by decomposing various grades of hydrocarbon oils into permanent gases by heat.

Water gas generators can be divided into two classes:—(1) Continuous processes, in which the heat necessary to bring about the interaction of the carbon and the steam is obtained by performing the operation in retorts externally heated in a furnace. (2) Intermittent processes, in which the carbon is first heated to incandescence by an air-blast, and the air-blast being cut off, superheated steam is blown in until the temperature is reduced to a point at which the carbon begins to fail in its action, when the air is again admitted to bring the fuel up to the required temperature; the process consisting of the alternate formation of producer gas with rise of temperature, and of water gas with lowering of temperature.

Of the first class of generator, none, so far, have as yet been practically successful in England.

Of the intermittent processes, the one most in use in America is the Lowe, in which the coke or anthracite is heated to incandescence by an air-blast in a generator lined with fire-brick; the heated products of combustion, as they leave the generator and enter the superheaters, being supplied with more air, which causes the combustion of the carbon monoxide present in the producer gas, and heats up the fire-brick baffles with which the superheaters are filled. When the necessary temperature of fuel and superheater has been reached, the air-blasts are cut off, and steam is blown through the generator, forming water gas, which meets the enriching oil at the top of the first superheater, called the "carburetter," and carries the vapours with it through the main superheater, where the firing of the hydrocarbons takes place. The chief advantage of this apparatus is that the enormous superheating space enables a lower temperature to be used for the fixing, which does away to a certain extent with the too great breaking down of the hydrocarbon, and consequent deposition of carbon.

The Springer apparatus differs from the Lowe only in construction. In the former the superheater is directly above the generator; and there is only one superheating chamber instead of two. The air-blast is admitted at the bottom, and the producer gases heat the superheater in the usual way; and when the required temperature is reached, the steam is blown in at the top of the generator, and is made to pass down through the incandescent fuel. The water gas is led from the bottom of the apparatus to the top, where it enters at the summit of the superheater, meets the oil, and passes down with it through the chamber, the finished gas escaping at the middle of the apparatus. This idea of making the air-blast pass up through the fuel, while in the subsequent operation the steam passes down through it, is also to be found in the Loomis plant, and is a distinct advantage—the fuel being at its hottest where the blast has entered, and, in order to keep down the percentage of carbon dioxide, it is important that the fuel through which the water gas last passes should be as hot as possible, to insure its reduction to carbon monoxide.

The Flannery apparatus is also only a slight modification of the Lowe plant, the chief difference being that, as the water gas leaves the generator, the oil is fed into it, and with the gas passes through a D-shaped retort tube, arranged round three sides of the top of the generator. In this tube the oil is volatilized, and passes with the gas to the bottom of the superheater, in which the vapours are converted into permanent gases.

The Van Steenbergh plant stands apart from all other forms of carburetted water gas plant, in that the upper layer of the fuel itself forms the superheater, and that no second part of any kind is needed for the fixation of the hydrocarbons. This arrangement reduces the apparatus to the simplest form, and leaves no part of it which can choke or get out of order—an advantage which will not be underrated by anyone who has had experience of these plants. While, however, an enormous advantage is gained, there is also the drawback that the apparatus is not at all fitted for use with crude oils of heavy specific gravity, such as can be dealt with in the big external superheaters of the Lowe class of water gas plant, but requires to have the lighter oils used in it for carburetting purposes. This, which appears at first sight to be a disadvantage, is not altogether one, as, in the first place, the lighter grade of oils, if judged by the amount of carburetting property they possess, are cheaper per candle-power added to the gas than the crude oils, while their use entirely does away with the formation of pitch and carbon in the pipes and purifying apparatus—a factor of the greatest importance to the gas manufacturers. The fact that light oils give a higher carburation per gallon than heavy crude oils is due to the fact that the crude oils have to be heated to a higher temperature to convert them into permanent gases; and this causes an over-cracking of the most valuable illuminating constituents. This trouble cannot be avoided, as, if a lower temperature is employed, the result is the formation of non-permanent vapours, which, by their condensation in the pipes, give rise to endless trouble. The simplicity of the apparatus is a factor which is a considerable saving of time and expense, as it reduces to a minimum the risk of stoppages for repairs, while the initial cost of the apparatus is necessarily low, and the expense of keeping it in order practically *nil*.

In such an apparatus 1000 cubic feet of carburetted water gas, having an illuminating value of 22 candles, can be made with the consumption of about 30 pounds of coke or anthracite and 2·5 gallons of light naphtha.

The great objection to the use of carburetted water gas is undoubtedly the poisonous nature of the carbon monoxide, which acts by diffusing itself through the air-cells of the lungs, and forming with the colouring of the blood corpuscles a definite compound, which prevents them carrying off their normal function of taking up oxygen and distributing it throughout the body, and at once stops life. All researches on this subject point to the fact that something less than 1 per cent. only of carbon monoxide in air renders it fatal to animal life; and this at first sight seems to be an insuperable objection to the use of water gas. It has, indeed, influenced the authorities in several towns—notably Paris—to forbid the introduction of water gas for domestic consumption. It would be well, however, to carefully examine the subject, and see, by the aid of actual figures, what the risk amounts to, compared with the risks of ordinary coal gas. Many experiments have been made with the view of determining the percentage of carbon monoxide in air which is fatal to human or rather to animal life, the most trustworthy as well as the latest results being those obtained by Dr. Stevenson, of Guy's Hospital, after an investigation instituted in consequence of two deaths which took place at the Leeds Forge, from inhaling uncarburetted water gas containing 40 per cent. of carbon monoxide. Dr. Stevenson found that 1 per cent. visibly affected a mouse in 1½ minutes, and killed it in an hour and three-quarters, while 0·1 per cent. was highly injurious. Taking, for the sake of argument, the last figure as being a fatal quantity, so as to be well within the mark, it is found that in ordinary carburetted water gas, as supplied by the superheater processes, such as the Lowe, Springer, and others, the usual amount of carbon monoxide is 26 per cent.; but in the Van Steenbergh gas, for certain chemical reasons to be discussed later on, it is generally about 18 per cent., and rarely rises to 20 per cent. An ordinary bedroom is 12 feet by 15 feet and 10 feet high; and therefore it will contain 1800 cubic feet of air. Such a room would be lighted by a single ordinary batswing burner, consuming not more than 4 cubic feet of gas per hour. And if this were left full on, in one hour the 1800 cubic feet of air would be mixed with four-fifths of a cubic foot of carbon monoxide (the carburetted water gas being supposed to contain 20 per cent.) or 0·04 per cent. In such a room, however, if the doors and windows were absolutely air-tight, and there were no fire-place, diffusion through the walls would change the entire air once in an hour. Therefore the percentage would not rise above 0·04; while in any ordinary room, imperfect workmanship and an open chimney would change it four times in the hour, and reduce the percentage to 0·01—a quantity which the most inveterate enemy of water gas could not claim would do more than produce a bad headache. The point under consideration, however, was the use of carburetted water gas as an enricher of coal gas, and not as an illuminant to be consumed *per se*; and it might be calculated that it would be probably used to enrich a 16-candle coal gas up to 17·5-candle power. To do this, 25 per cent. of 12-candle power carburetted water gas would have to be mixed with it. Taking the quantity of carbon monoxide in London gas at 5 per cent. (a very fair average figure), and 18 per cent. as the amount present in the Van Steenbergh gas, we have 8·25 per cent. of carbon monoxide in the gas as sent out—a percentage hardly exceeding that which is found in the rich cannel gas supplied to such places as Glasgow, where it is not found that an unusual number of deaths occur from carbon monoxide poisoning. Moreover, carburetted water gas has quite as strong a smell as coal gas, and can be quite as easily detected by the nose.

The cost of most of these methods of enriching coal gas can be calculated, and give the following figures as the cost of enriching a 16-candle gas up to 17·5-candle power per 1000 cubic feet:—

By cannel coal	4*d.*
By the Maxim-Clark process	$2\frac{6}{10}$*d.*
By the Lowe or Springer water gas	$1\frac{1}{2}$*d.*
By the Van Steenbergh water gas	$\frac{3}{4}$*d.*

In adopting any new method, the mind of the gas manager must, to a great extent, be influenced by the circumstances of the times; and the enormous importance of the labour question is a main factor at the present moment. With masters and men living in a strained condition, which may at any moment break into open warfare, the adoption of such water gas processes would relieve the manager of a burden which is growing almost too heavy to be borne. Combining, as such processes do, the maximum rate of production with the minimum amount of labour, they practically solve the labour question.

The cost of paraffin oil of lighter grades, and the fear that the supply might be hampered by the formation of a huge monopoly, has been a great drawback, but we have materials which can be equally well used in this country of which an almost unlimited supply can be obtained.

At three or four of the Scotch iron-works, the Furnace Gases Company are paying a yearly rental for the right of collecting the smoke and gases from the blast-furnaces. These are passed through several miles of wrought-iron tubing, gradually diminishing in size from 6 feet to about 18 inches; and as the gases cool there is deposited a considerable yield of oil. At Messrs. Dixon's, in Glasgow, which is the smallest of these installations, they pump and collect about 60,000,000 feet of furnace gas per day, and recover, on an average, 25,000 gallons of furnace oils per week; using the residual gases, consisting chiefly of carbon monoxide, as fuel for distilling and other purposes, while a considerable yield of sulphate of ammonia is also obtained. In the same way a small percentage of the coke-ovens are fitted with condensing gear, and produce a considerable yield of oil, for which, however, there is but a very limited market; the chief use being for the Lucigen light, and other lamps of the same description, and also for pickling timber for railway sleepers, &c. The result is that four years ago the oil could be obtained in any quantity at $\frac{1}{2}$*d.* per gallon; though it has since been as high as $2\frac{1}{2}$*d.* per gallon. It is now about 2*d.* per gallon, and shows a falling tendency. Make a market for this product, and the supply will be practically unlimited, as every blast-furnace and coke-oven in the kingdom will put up plant for the recovery of the oil. As, with the limited plant now at work, it would be perfectly easy to obtain 4,000,000 or 5,000,000 gallons per annum, an extension of the recovery process would mean a supply sufficiently large to meet all demands.

Many gas managers have from time to time tried if they could not use some of their creosote oil for producing gas; but, on heating it in retorts, &c., they have found that the result has generally been a copious deposit of carbon, and a gas which has possessed little or no illuminating value. Now the furnace and coke-oven oils are in composition somewhat akin to the creosote oil; so that, at first sight, it does not seem a hopeful field for search after a good carburetter. But the furnace oils have several points in which they differ from the coal-tar products. In the first place, they contain a certain percentage of paraffin oil; and, in the next, do not contain much naphthalene, in which the coal-tar oil is especially rich, and which would be a distinct drawback to their use. The furnace oil, as condensed, contains about 30 to 50 per cent. of water; and, in any case, this has to be removed by distilling. Mr. Staveley has patented a process by which the distillation is continued after the water has gone off, and by condensing in a fractionating column of special construction, he is able to remove all the paraffin oil, a considerable quantity of cresol, a small quantity of phenol, and about 10 per cent. of pyridine bases, leaving the remainder of the oil in a better condition, and more valuable for pickling timber, its chief use.

If the mixed oil so obtained, which we may call "phenoloid oil," is cracked by itself, no very striking result is obtained; the 40 per cent. of paraffin present cracking in the usual way, and yielding a certain amount of illuminants. But if the oil is cracked in the presence of carbon, and is made to pass over and through a body of carbon heated to a dull red heat, it is converted largely into benzene. As this is the most valuable of the illuminants in coal gas, and also the one to which it owes the largest proportion of its light-giving power, it is manifestly the right one to use in order to enrich it. On cracking the phenoloid oil, the paraffin yields ethane, propane, and marsh gas, &c., in the usual way; while the phenol interacts with the carbon to form benzene:—

$$\underset{\text{Phenol.}}{C_6H_5HO} + C = \underset{\text{Benzene.}}{C_6H_6} + CO.$$

And in the same way the cresol first breaks down to toluene in the presence of the carbon; and this in turn is broken down by the heat to benzene. A great advantage this oil has is that the flashing-point is 110° C., and so is well above the limit; this doing away with the dangers and troubles inseparable from the storage of light naphthas in bulk.

In using this oil as an enricher, it must be cracked in the presence of carbon; and it is of the greatest importance that the temperature should not be too high, as the benzene is easily broken down to simpler hydrocarbons of far lower illuminating value.

(To be continued.)

SCIENTIFIC SERIALS.

American Journal of Science, December 1890.—Long Island Sound in the Quaternary era, with observations on the submarine Hudson River channel, by James D. Dana. The discussion of a chart containing some new soundings recently made under the direction of the U.S. Coast and Geodetic Survey leads Prof. Dana to conclude that during the Glacial period "Long Island Sound, instead of being, as it is now, an arm of the ocean twenty miles wide, was for the greater part of its length a narrow channel serving as a common trunk for the many Connecticut and some small Long Island streams, and that the southern Sound river reached the ocean through Peconic Bay. Under these circumstances the supply of fresh water for the Sound river would have been so great that salt water would have barely passed the entrance of the Sound." He attributes the origin of the channel over the submerged Atlantic border to the flow of the Hudson River during a time of emergence.—The preservation and accumulation of cross-infertility, by John T. Gulick. The author discusses some of the conclusions arrived at by Mr. Wallace in his work on "Darwinism."—The deformation of Iroquois Beach and birth of Lake Ontario, by J. W. Spencer. The author believes that the great Iroquois Beach was constructed approximately at sea-level and that its upheaval was the means that gave birth to Lake Ontario. This episode commenced almost synchronously with the creation of the Niagara Falls.—Experiments upon the constitution of the natural silicates, by F. W. Clarke and E. A. Schneider.—Eudialyte and eucolite, from Magnet Cove, Arkansas, by J. Francis Williams.—Prediction of cold waves from Signal Service weather maps, by T. Russell. In addition to the regular fall of temperature that takes place from day to night, irregular falls occur from time to time. When the fall in the latter case exceeds 20°, and covers an area greater than 50,000 square miles, and the temperature in any part of the area falls as low as 36°, it is called a cold wave. The author has investigated the shapes and relative positions of the various high and low areas of pressure preceding cold waves, and proposes a method for the prediction of them.—On a peculiar method of sand transportation by rivers, by James C. Graham. Numerous blotches of sand, some about six inches square, have been observed floating on the surface of the Connecticut River. This indicates that, by surface tension, it is possible for coarse sand to be floated away on a current having less velocity than would otherwise be required, and affords a possible explanation of the coarser particles of sand usually found in otherwise very fine deposits.—Note on the Cretaceous rocks of Northern California, by J. S. Diller.—Magnetic and gravity observations on the west coast of Africa and at some islands in the North and South Atlantic, by E. D.

Preston.—On the Fowlerite variety of rhodonite from Franklin and Stirling, N.J., by L. V. Pirsson.—Some observations on the beryllium minerals from Mount Antero, Colorado, by S. L. Penfield.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 11, 1890.—"On Stokes's Current Function." By R. A. Sampson, Fellow of St. John's College, Cambridge. Communicated by Prof. Greenhill, F.R.S.

In a liquid any irrotational motion which is symmetrical about an axis may be regarded as due to the juxtaposition at the origin and upon the axis of symmetry of sinks and sources.

Let us consider the system formed by a line source and a line sink, of equal strengths, extending along the axis from an arbitrary origin to infinity in opposite directions. Such a system I shall call an *extended doublet*, of strength m, where m is the strength per unit length of that part which lies on the positive side of the origin.

By the superposition of two extended doublets, of equal but opposite strengths, we can produce a sink or a source upon the axis. Hence, in a liquid, any irrotational motion which is symmetrical with respect to an axis may be produced by superposition of extended doublets, whose origins depart but little from an arbitrary point on the axis of symmetry.

Now, for an extended doublet of strength m, we find Stokes's current function ψ, for any point distant r from the origin $= -2mr$. Whence, if $r\sin\theta = \varpi$, $r\cos\theta = z - \zeta$, $\mu = \cos\theta$

$$\frac{d^2\psi}{d\varpi^2} + \frac{d^2\psi}{dz^2} - \frac{1}{\varpi}\frac{d\psi}{d\varpi} = 0 = \frac{d^2\psi}{dr^2} + \frac{1-\mu^2}{r^2}\frac{d^2\psi}{d\mu^2}.$$

Thus it will be seen that the direct distance of any point from a point on the axis of symmetry plays the same part in the theory of Stokes's current function that is played by its reciprocal in the theory of the potential function belonging to symmetrical distributions of matter.

And if r_0, 0, r, θ, be the co-ordinates of a point upon the axis, and of any other point, the distance between these points, $\sqrt{(r_0^2 - 2r_0r\cos\theta + r^2)}$, may be developed in a convergent series, say

$$\sum_{n=0}^{n=\infty} - \frac{r^n}{r_0^{n-1}} I_n(\cos\theta),$$

or

$$\sum_{n=0}^{n=\infty} - \frac{r_0^n}{r^{n-1}} I_n(\cos\theta),$$

according as r_0 is greater or less than r, $I_n(\cos\theta)$ being a certain function of θ, satisfying

$$(1-\mu^2)\frac{d^2 I_n(\mu)}{d\mu^2} + n(n-1)I_n(\mu) = 0.$$

It is evident from the analogue of zonal harmonics that it is proper to discuss the function $I_n(\cos\theta)$, and other solutions of this equation, before considering the applications of Stokes's current function to the motion of liquids. As might be expected, the theory closely resembles that of spherical harmonics.

The applications to hydrodynamics which I here give are chiefly in connection with the motion of viscous liquids. In *Crelle-Borchardt*, vol. lxxxi., 1876, Oberbeck has given the velocities produced in an infinite viscous liquid by the steady motion of an ellipsoid through it, in the direction of one of its axes, and from these Mr. Herman (*Quart. Journ. Math.*, 1889, No. 92) has found the equation of a family of surfaces containing the stream lines relative to the ellipsoid. I obtain Stokes's current function by a direct process for the flux of a viscous liquid past a spheroid, and it is shown that the result differs only by a constant multiple from the particular case of Mr. Herman's integral.

Some minor applications are also given—namely, the solutions are obtained for flux past an approximate sphere, and past an approximate spheroid. The solution is also obtained for flux through a hyperboloid of one sheet, where it appears that the stream surfaces are hyperboloids of the confocal system. A particular case is that of flux through a circular hole in a wall, and this is interesting because we see that, by supposing internal friction to take place in the liquid, we find an expression which gives zero velocity at the sharp edge, and thus avoids the difficulty which is always present in the solution of such problems on the supposition that the liquid is perfect.

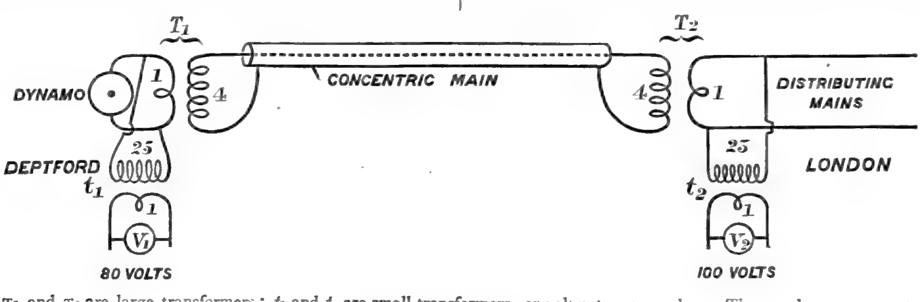
The paper concludes with an attempt to discuss the flux past a spheroid, or through a hyperboloid at whose boundary there may be slipping. The current function is not obtained, all that appears being that it probably differs from the parallel case of the sphere in being far more complicated than when there is no slipping. From this we except the case of the flux through a circular hole in a plane wall, when the solution for no slipping satisfies the new conditions.

Chemical Society, November 6, 1890.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—The magnetic rotation of saline solutions, by Dr. W. H. Perkin, F.R.S. The remarkable results given by solutions of the halhydrides and their compounds with ammonia and organic bases when examined as to their magnetic rotatory power (Chem. Soc. Trans., 1889, 740) made it important to study the solutions of metallic salts in a similar manner. The substances which have been examined up to the present are chiefly chlorides, bromides, iodides, nitrates, a nitrite, sulphates, and phosphates, also hydroxides of alkali metals. For haloid metallic salts in aqueous solution, the rotations were found to be practically 2·2 times greater than the calculated values for the dry substances, and greater therefore than those of the analogous ammonium compounds. A similar remarkable increase of rotation was observed with the hydroxides of the alkali metals, but with sulphates and phosphates numbers agreeing much more closely with the calculated values were obtained. In the discussion which followed the reading of the paper, Dr. Gladstone, F.R.S., said that similar excessive values were obtained on determining the refractive powers of solutions of metallic chlorides, &c., although the differences between the calculated and observed values were much smaller than in the case of Dr. Perkin's measurements. It was all-important to determine the difference in behaviour to light of a substance in its solid state and when in solution, but this was difficult as few solids were uniaxial; as an example of the difference he mentioned that in the case of sodium chloride the solid has a refraction of 14·4, while that of the dissolved substance is 15·3.—Note on normal and isopropylparatoluidine, by Mr. E. Hori and Dr. H. F. Morley.—The action of light on ether in the presence of oxygen and water, by Dr. A. Richardson. In a recent paper by Dunstan and Dymond (Chem. Soc. Trans., 1890, 574) it is stated that hydrogen peroxide is not formed when carefully purified ether is exposed at a low temperature in contact with air and water to the electric light or diffused daylight. Employing ether which had been purified by some of the methods of Dunstan and Dymond, the author found that hydrogen peroxide is formed in the liquid in every case after exposure to sunlight in contact with moist air or oxygen, but not in the dark at the ordinary temperature.—Action of ammonia and methylamine on the oxylepidens, by Dr. F. Klingemann and Mr. W. F. Laycock.—Condensation of acetone-phenanthraquinone, by Mr. G. H. Wadsworth.—Action of phosphorus pentachloride on mucic acid, by Dr. S. Ruhemann and Mr. S. F. Dufton.—Halogens and the asymmetrical carbon atom, by Mr. F. H. Easterfield. The author has endeavoured to prepare optically active haloid derivatives similar in constitution to Le Bel's optically active secondary amyl iodide, which at present stands alone as the only active compound in which a halogen is united to the asymmetric carbon atom. The results obtained with optically active mandelic acid were negative.

November 20.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—A new method of determining the specific volumes of liquids and of their saturated vapours, by Prof. S. Young. When a tube closed at both ends and partly filled with a liquid is raised in temperature, the liquid expands, but the apparent expansion is less than the real, for a certain amount of the substance separates and occupies the space above the liquid in the form of saturated vapour. If the density of the vapour were known, it would be possible to apply the necessary correction; but at high temperatures and pressures this is not the case. If, on the other hand, the upper part of the tube (enclosing the vapour and a portion of the liquid) be heated to a high temperature, the lower part being kept at a constant low temperature, and if subsequently a greater length of the tube be heated to the high temperature, there will again be expansion, but in this case the observed expansion will be greater than the real, for in consequence of the diminution in volume of the saturated vapour, a portion of it must have condensed. In both cases there are the same two unknown values, the true volume of the liquid and the specific volume of the vapour, and from the two equations involving the experimental data it is, therefore, possible to calculate both values. The experimental method based on these principles possesses the following advantages: it is applicable to substances such as nitrogen peroxide or bromine which attack mercury; it is available for a very wide range of temperature and pressure, even to the critical point of many substances; the data obtained serve to determine not only the specific volume of the liquid, but also that of its saturated vapour.—The molecular condition of metals when alloyed with each other, by Messrs. C. T. Heycock and F. H. Neville. The authors in their earlier experiments (Chem. Soc. Journ., 1890, 376) showed that one atomic proportion of a metal when dissolved in tin produces a fall in the freezing-point that on the theory of osmotic pressure should be produced by one molecular proportion, and therefore concluded that when metals are dissolved in tin their molecules are monatomic. Further experiments with other metals as solvents have led to the following results:—Of fourteen metals dissolved in bismuth, seven (viz. lead, thallium, mercury, tin, palladium, platinum, and cadmium) have monatomic molecules; of fifteen metals dissolved in cadmium, seven (viz. antimony, platinum, bismuth, tin, sodium, lead, and thallium) have monatomic molecules; and of fourteen metals dissolved in lead, five (viz. gold, palladium, silver, platinum, and copper) have monatomic, and three (viz. mercury, bismuth, and cadmium) have diatomic molecules.—The estimation of cane sugar, by Messrs. C. O'Sullivan, F.R.S., and F. W. Tompson.—The spectra of blue and yellow chlorophyll, with some observations on leaf-green, by Prof. W. N. Hartley, F.R.S. The author draws the following conclusions from the results of his investigation of the different colouring-matters described under the name chlorophyll:—(1) Living tissues which are fresh and young, and which therefore contain the leaf-green unaltered, exhibit no trace of a band close to D, such as is usually attributed to chlorophyll, and there is no indication of one in the green. (2) Yellow chlorophyll has a distinct absorption-band in the red differing from that of blue chlorophyll. It has likewise a distinct fluorescence. (3) When light is concentrated on living tissues the absorption spectrum of the green colouring-matter is soon altered. (4) Blue chlorophyll may be extracted from minced leaves by cold absolute alcohol, and may be precipitated by addition of baryta. Yellow chlorophyll is not so precipitated, or not precipitated so readily. A warm solution of boracic acid in glycerine, mixed with a little alcohol, liberates the unchanged blue chlorophyll from the dried barium compound. (5) Blue chlorophyll exhibits two absorption-bands in the red, close together; in the less refrangible region of rays one overlies B and the other overlies C. There is a feebler band near D. (6) Concentrated solutions of yellow chlorophyll in benzene are brownish in colour, and exhibit a magnificent red fluorescence. (7) When blue and yellow chlorophyll are separately treated with formic acid and ether, there are produced two new substances showing absorption-bands in the green. It is believed that when these bands have been observed, either in preparations of chlorophyll or in living tissues, the chlorophyll has been altered by oxidation of formic aldehyde in the plant. This oxidation could be caused in living tissues by an excessive degree of illumination, which causes the destruction of the tissues, and otherwise by exposure of the contents of the plant-cells to air or oxygen. An excessive illumination causes an exceedingly great activity in decomposing carbonic acid, and probably oxygen cannot be respired sufficiently rapidly; hence there may be a reverse action, or an oxidation of formic aldehyde to formic acid. (8) The leading characteristics of unaltered leaf-green are those of blue chlorophyll—namely, an intense absorption in the red, stronger even than in the violet or ultra-violet.—Note on dibenzanilide, by Dr. J. B. Cohen.

December 4.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—The action of heat on ethylic β-amidocrotonate, by Dr. J. N. Collie.—The action of heat on nitrosyl chloride, by Messrs. J. J. Sudborough and G. H. Miller. An account is given of a series of determinations of the vapour-density of nitrosyl chloride at various temperatures. At temperatures from 15° to 693°, the values obtained so nearly coincide with the theoretical value 32·67 that it is to be supposed that no dissociation takes place below 700°. At higher temperatures the compound is no longer stable. The results show that in comparison with nitrogen dioxide, which is completely decomposed below 620°, nitrosyl chloride is a highly stable compound.—The volumetric estimation of tellurium, by Dr. B. Brauner.

Physical Society, December 12, 1890.—Prof. W. E. Ayrton, President, in the chair.—Mr. Shelford Bidwell, F.R.S., showed some experiments with selenium cells. The crystalline variety of selenium was, he said, most interesting to physicists, owing to its electrical resistance being greatly diminished by light. This property was shown experimentally with different forms of cells, the construction of which was explained. The form recommended was that in which two copper wires are wound near each other round a slip of mica, and the spaces between the wires filled with selenium. The wires form the terminals of the so-called "cell," which, before being used, is annealed for several hours at a temperature above 200° C. Many such cells were made in 1880, 1881, and their sensitiveness to light remained unimpaired during 1882. In 1885, however, several were found less sensitive, and others totally useless; only one out of thirteen retained its sensibility till September 1890. The loss of sensitiveness Mr. Bidwell believes due to an excessive amount of selenide of copper being formed, for, although some selenide is essential to the satisfactory working of the cell, too much is fatal to its action. The selenide of one defective cell was electrolyzed, red tufts of amorphous selenium appearing on the anodes. A white substance resembling moist calcium chloride was also present; this he believed to be oxide or hydroxide of selenium. Small polarization currents had been obtained from selenium cells. A lecture apparatus illustrating the properties of selenium cells was exhibited. It consisted of a cell connected in series with a relay and a battery. The relay was arranged so that it might either ring a bell, or light an incandescent lamp. When the bell was joined up, it remained silent so long as the selenium cell was illuminated, but on screening the cell, the bell rang. By using various coloured glasses as screens, the effect was shown to be due to the red and yellow rays. A similar experiment with the glow lamp was very striking, for on turning down the gas-lamp illuminating the cell, the electric lamp lighted, and was extinguished on turning up the gas. This demonstrated the possibility of an automatic lamp-lighter, which would light or put out lamps according as they are required or superfluous. Amongst the other practical applications suggested were, announcing the accidental extinction of railway-signal lamps or ships' lights, and the protection of safes and strong rooms. Prof. Minchin said he had lately constructed cells of a different kind from those shown by Mr. Bidwell, and found that they gave an E.M.F. when exposed to light. For his purposes the long annealings, &c., were quite unnecessary, and a complete cell could be made in ten minutes. One of his cells gave an E.M.F. of over 0·25 volt as measured by an electrometer, by the light of a fog. Their promptness of action falls off in a day or two, but if they are kept on open circuit a week has no effect on the final E.M.F. On closed circuit, however, they deteriorate. Prof. S. U. Pickering said both oxides of selenium were deliquescent, and the author's conclusion as to the white substance formed by electrolysis was probably correct. Prof. S. P. Thompson believed Prof. Graham Bell had tried platinum instead of copper, and found that the selenium cracked off in annealing. He also found that it was only necessary to carry on the annealing until the characteristic slate colour appeared. Mr. Bidwell's experiments, he said, showed the possibility of seeing at a distance, and had also suggested to him that the effect of screening might be utilized for driving a completely detached pendulum electrically. Prof. Forbes said that silver sulphide when electrolyzed presented appearances resembling those noticed by Mr. Bidwell in copper selenide. In reply to questions from the President and Prof. Perry, as to whether the low resistance and unsensitiveness of old cells was due to moisture, Mr. Bidwell said drying them had no effect, but baking restored the resistance but not their sensitiveness. Speaking of the effect of annealing cells, he said this reduced their resistance considerably. Prof. Graham Bell, he believed, gave up using platinum because the resistances of such cells were very high.—Mr. James Swinburne read a paper on Alternate Current Condensers. It is, he said, generally assumed that there is no difficulty in making commercial condensers for high-pressure alternating currents. The first difficulty is insulation, for the dielectric must be very thin, else the volume of the condenser is too great. Some dielectrics 0·2 mm. thick can be made to stand up to 8000 volts when in small pieces, but in complete condensers a much greater margin must be allowed. Another difficulty arises from absorption, and whenever this occurs the apparent capacity is greater than the calculated. Supposing the fibres of paper in a paper condenser to be conductors embedded in insulating hydrocarbon, then every time the condenser is charged the fibres have their ends at different potentials, so a current passes to equalize them and energy is lost. This current increases the capacity. One condenser made of paper boiled in ozokerite took an abnormally large current and heated rapidly. At a high temperature it gave off water, and the power wasted and current taken gradually decreased. When a thin plate of mica is put between tin foils, it heats excessively; and the fall of potential over the air films separating the mica and foil is great enough to cause disruptive discharge to the surface of the mica. There appears to be a luminous layer of minute sparks under the foils, and there is a strong smell of ozone. In a dielectric which heats, there may be three kinds of conduction: viz. metallic, when an ordinary conductor is embedded in an insulator; disruptive, as probably occurs in the case of mica; and electrolytic, which might occur in glass. In a transparent dielectric the conduction must be either electrolytic or disruptive, otherwise light vibrations would be damped. The dielectric loss in a cable may be serious. Calculating from the waste in a condenser made of paper soaked in hot ozokerite, the loss in one of the Deptford mains came out 7000 watts. Another effect observed at Deptford is a rise of pressure in the mains. There is as yet no authoritative statement as to exactly what happens, and it is generally assumed that the effect depends on the relation of capacity to self-induction, and is a sort of resonator action. This would need a large self-induction, and a small change of speed would stop the effect. The following explanation is suggested. When a condenser is put on a dynamo, the condenser current leads relatively to the electromotive force, and therefore strengthens the field magnets and increases the pressure. In order to test this, the following experiment was made for the author by Mr. W. F. Bourne. A Gramme alternator was coupled to the low-pressure coil of a transformer, and a hot-wire voltmeter put across the primary circuit. On putting a condenser on the high-pressure circuit, the voltmeter wire fused. The possibility of making an alternator excite itself like a series machine, by putting a condenser on it, was pointed out. Prof. Perry said it would seem possible to obtain energy from an alternator without exciting the magnets independently, the field being altogether due to the armature currents. Mr. Swinburne remarked that this could be done by making the rotating magnets a star-shaped mass of iron. Sir W. Thomson thought Mr. Swinburne's estimate of the loss in the Deptford mains was rather high. He himself had calculated the power spent in charging them, and found it to be about 16 horse-power, and although a considerable fraction might be lost, it would not amount to nine-sixteenths. He was surprised to hear that glass condensers heated, and inquired whether this heating was due to flashes passing between the foil and the glass. Mr. A. P. Trotter said Mr. Ferranti informed him that the capacity of his mains was about $\frac{1}{3}$ microfarad per mile, thus making $2\frac{1}{3}$ microfarads for the seven miles. The heaping up of the potential only took place when transformers were used, and not when the dynamos were connected direct. In the former case the increase of volts was proportional to the length of main used, and 8500 at Deptford gave 10,000 at London. Mr. Blakesley described a simple method of determining the loss of power in a condenser by the use of three electro-dynamometers, one of which has its coils separate. Of these coils, one is put in the condenser circuit, and the other in series with a non-inductive resistance r, shunting the condenser. If α_2 be the reading of a dynamometer in the shunt circuit, and α_3 that of the divided dynamometer, the power lost is given by $r\,(C\alpha_3 - B\alpha_2)$ where B and C are the constants of the instruments on which α_2 and α_3 are the respective readings. Prof. S. P. Thompson asked if Mr. Swinburne had found any dielectric which had no absorption. So far as he was aware, pure quartz crystal was the only substance. Prof. Forbes said Dr. Hopkinson had found a glass which showed none. Sir W. Thomson, referring to the same subject, said that many years ago he made some tests on glass bottles, which showed no appreciable absorption. Sulphuric acid was used for the coatings, and he found them to be completely discharged by an instantaneous contact of two balls. The duration of contact would, according to some remarkable mathematical work done by Hertz in 1882, be about 0·0004 second, and even this short time sufficed to discharge them completely. On the other hand, Leyden jars with tin-foil coatings, showed considerable absorption, and this he thought due to want of close contact between the foil and the glass. To test this he suggested that mercury coatings be tried. Mr. Kapp considered the loss of power in condensers due to two causes: first, that due to the charge soaking in; and second, to imperfect elasticity of the dielectric. Speaking of the extraordinary rise of pressure on the Deptford mains, he said he had observed similar effects with other cables. In his experiments the sparking distance of a 14,000-volt transformer was increased from $\frac{3}{16}$ of an inch to 1 inch by connecting the cables to its terminals. No difference was detected between the sparking distances at the two ends of the cable, nor was any rise of pressure observed when the cables were joined direct on the dynamo. In his opinion the rise was due to some kind of resonance, and would be a maximum for some particular frequency. Mr. Mordey mentioned a peculiar phenomenon observed in the manufacture of his alternators. Each coil, he said, was tested to double the pressure of the completed dynamo, but when they were all fitted together their insulation broke down at the same volts. The difficulty had been overcome by making the separate coils to stand much higher pressures. Prof. Rücker called attention to the fact that dielectrics alter in volume under electric stress, and said that if the material was imperfectly elastic some loss would result. The President said that, as some doubt existed as to what Mr. Ferranti had actually observed, he would illustrate the arrangements by a diagram. Speaking of condensers he said he had recently tried lead plates



T_1 and T_2 are large transformers; t_1 and t_2 are small transformers or voltmeters V_1 and V_2. The numbers 1, 4, 1, 25, represent their conversion ratios.

in water to get large capacities, but so far had not been successful. Mr. Swinburne, in replying, said he had not made a perfect condenser yet, for, although he had some which did not heat much, they made a great noise. He did not see how the rise of pressure observed by Mr. Ferranti and Mr. Kapp could be due to resonance. Mr. Kapp's experiment was not conclusive, for the length of spark is not an accurate measure of electromotive force. As regards Mr. Mordey's observation, he thought the action explicable on the theory of the leading condenser current acting on the field magnets. The same explanation is also applicable to the Deptford case, for when the dynamo is direct on, the condenser current is about 10 amperes, and this exerts only a small influence on the strongly magnetized magnets. When transformers are used the field magnets are weak, whilst the condenser current rises to 40 amperes. Mr. Blakesley's method of determining losses was, he said, inapplicable except where the currents were sine functions of the time; and consequently could not be used to determine loss due to hysteresis in iron, or in a transparent dielectric.—Mr. Swinburne's note on electrolysis was postponed till the next meeting.

Linnean Society, December 18, 1890.—Prof. Stewart, President, in the chair.—Prof. T. Johnson exhibited and made remarks on the male and female plants of *Stenogramme interrupta*.—Mr. Clement Reid exhibited specimens of *Helix obvoluta* from new localities in Sussex, and by aid of a specially prepared map traced the present very local distribution of this mollusc in England.—Mr. E. M. Holmes exhibited some examples of galls formed on *Styrax benzoin* by an Aphis (*Ætegopteris styracophila*). He also exhibited and described some new British Algæ, *Mesoglœa lanosa* and *Myriocladia tomentosa*.—A paper was then read by Prof. R. J. Harvey Gibson on the structure and development of the cystocarps in *Catanella opuntia*, and critical remarks were offered by Messrs. D. H. Scott, E. M. Holmes, and others.—Mr. G. F. Scott Elliot then read an interesting paper on the effect of exposure on the relative length and breadth of leaves, upon which a discussion followed.

Mathematical Society, January 8.—Prof. Greenhill, F.R.S., President, in the chair.—Mr. H. Perigal, in a communication on geometrical metamorphoses by partition and transformation, exhibited a great number of interesting dissections, starting from his now classical dissection of Euc. i. 47, which dates from the year 1835.—Major Macmahon, R.A., F.R.S., then gave an account of a theory of perfect partitions and the compositions of multipartite numbers.—Mr. Tucker read a paper, by Prof. G. B. Mathews, on a certain class of plane quartics.

PARIS.

Academy of Sciences, January 5.—M. Duchartre in the chair.—M. d'Abbadie was elected Vice-President for the year 1891.—On the waves caused by explosions, the characteristics of detonations, and the velocity of propagation in solid and liquid bodies, and especially in methyl nitrate, by M. Berthelot. Methyl nitrate, CH_3NO_3, may give by explosion $CO_2 + CO + N_2 + 3H_2O$, or $2CO_2 + N_2 + H_2 + 2H_2O$. In both cases the volume of the gas generated is the same, viz. 1028 litres for 1 kilogramme, the heat of decomposition being 1451 calories. These numbers are very nearly the same as those furnished by nitro-glycerine and gun-cotton. The pressure developed when 1 kilogramme of methyl nitrate is exploded in a vessel of 1 litre capacity, is no less than 11,000 kilogrammes per square centimetre. The author has attempted to measure the velocity of propagation of the waves, but the vessels employed were always broken by the shock. A calculation shows that the resistance offered by the vessels only increases with the thickness up to a certain limiting pressure. The pressure developed above this limit has infinite force, hence nothing can resist it.—On a class of modulary equations, by M. Brioschi.—On some linear differential equations capable of transformation among themselves by a change of a function and a variable, by M. Paul Appell.—On the value of the magnetic elements in absolute measure on January 1, 1891, by M. Th. Moureaux. The values are given for the Observatories of Parc Saint-Maur and Perpignan, together with the secular variation obtained by a comparison with those found on January 1, 1890.—On the absorption spectra of solutions of iodine, by M. H. Rigollot. The author has studied the absorption spectra of various iodine solutions with reference to the displacement of the absorption bands and the quantity of light transmitted. The general result arrived at is, that for similar substances, or for compounds of the same radicle used as solvents for iodine, and submitted to experiment, with an increase of molecular weight (1) the absorption band is shifted slightly towards the violet end of the spectrum; (2) the minimum amount of light received diminishes in value.—The influence of tempering on the electrical resistance of steel, by M. H. Le Chatelier. The experiments show that the measure of electrical resistance may be used to determine the state of carbon in iron, and also to find the proportion transformed in tempered steel. This method will be adopted in further researches on the mechanical properties of steel.—Influence of the *covolume* of gases on the velocity of propagation of explosive phenomena, by M. Vielle. (See M. Berthelot's paper above.)—On the conductivities of isomeric organic acids and their salts, by M. Daniel Berthelot. From the observation at 17° C. of the conductivities of dilute solutions of oxybenzoic, of amidobenzoic, of maleic and fumaric, of itaconic, metaconic, and citraconic, and of racemic, dextro-rotatory tartaric, and inactive tartaric acids in solutions containing the free acids alone and with varying quantities of potash, the author concludes that the rule given by M. Arrhenius for the calculation of the conductivities of isohydric solutions is rigorously true in the case of monobasic acids.—On trithienyl, by M. Adolphe Renard. The analyses of this body indicate that its formula is $C_4H_3S—C_4H_2S—C_4H_3S$. Its vapour density is 8·6 by Meyer's method; theory requires it to be 8·68.—Action of sodium benzylate upon camphor cyanide, by M. J. Minguin.—On a general method of analysis applicable to the spirits and alcohols of commerce, by M. Ed. Mohler.—The method described is asserted to permit of the determination of not only the alcohol, extract, acidity, and furfurol, but, in addition, the ethers, aldehydes, higher alcohols, and nitrogenous bodies.—On the urinary function of acephalian mollusks—viz. the Bojanus organ and the Keber and Grobben glands, by M. Augustin Letellier.—On the development of the chromatophores of octopodian cephalopods, by M. L. Joubin.—On *Atlantonema rigida*, v. Siebold, the parasite of various Coleoptera, by M. R. Moniez.—On the position of the chalk of Touraine, by M. A. de Grossouvre.—Contributions to the geological knowledge of the Alpine chains between Moutiers (Savoy) and Barcelonnette (Lower Alps): formations prior to the Jurassic, by M. W. Kilian.—Soundings of Lake Leman, by M. A. Delebecque. This lake is composed of two parts—the great lake, between Nernier and Villeneuve; and the little lake, between Nernier and Geneva. The great lake has a mean depth of 310 metres. At the junction with the Geneva lake the transverse barrier of an old moraine rises, and the depth is only about 70 metres. The mean depth of the whole lake appears to be about 153 metres.

CONTENTS.

THURSDAY, JANUARY 22, 1891.

NATURE OF KOCH'S REMEDY.

SINCE Koch announced at the meeting of the International Medical Congress in Berlin, more than five months ago, that he had discovered a remedy for tuberculosis, a lively curiosity has been felt as to the nature of this remedy. This curiosity has now been gratified, and on another page we reproduce the paper in which Koch has explained both the nature of the remedy and the experiments which led him to employ it.

The remedy is a glycerine extract of pure cultivations of tubercle bacilli. It does not appear from Koch's paper what the media were in which the tubercle bacilli were cultivated, and this is a matter of some importance, because it is quite possible that bacilli cultivated in gelatine, in meat broth free from albumen, and in albuminous solutions may yield different products. Thus, Hoffa extracted ptomaïnes from cultivations of anthrax bacillus grown upon meat, but did not obtain them from the same bacillus when grown upon broth; and Brunton and Macfadyen have found that bacteria appear to have the power of adapting themselves to the soil upon which they grow, by forming such unorganized ferments or enzymes as will decompose it and render it soluble so as to be suitable for their nutrition. Thus certain bacilli when grown upon a starchy soil form an enzyme which will convert the starch into sugar, while the same bacilli grown upon albuminous soil form an enzyme which converts the albumen into peptone.

The quantity of active material produced by the tubercle bacilli is, according to Koch, very small, and he estimates the amount of it in the glycerine extract he employs at fractions of 1 per cent. In its nature it appears to be allied to enzymes and peptones, for, while closely related to albuminous bodies, it does not belong to the group of so-called tox-albumens. Like unorganized ferments or enzymes, and like peptones, it is precipitated by alcohol, but it differs from the former and resembles the latter in its power of rapid diffusion. In addition to this soluble substance, the tubercle bacilli seem to produce another body which adheres closely to them, is not readily removed by solvents, and tends to produce local suppuration when injected under the skin of an animal, while the soluble material which forms the active part of the curative lymph has no such action. Koch has already shown that the tubercle bacillus, unlike the anthrax bacillus, is of a very slow growth, so that when cultivated on a glass covered with coagulated serum ten days elapse from the time of inoculating the slide before the growth of the bacillus becomes at all abundant. A similar condition occurs when the bacillus is inoculated subcutaneously in a healthy guinea-pig. After inoculation the wound generally closes up, and appears to heal entirely within a day or two. In ten to fourteen days afterwards, when the tubercle bacillus has begun to grow, a hard nodule appears, which soon opens and an ulcer forms, lasting until the death of the animal. At the same time as the ulceration begins, the lymphatic glands swell up, the animal becomes emaciated, and death occurs from the lungs and other organs being invaded by the bacilli which are carried to them by the blood from the point of inoculation. When a similar injection is made in an animal which has been already rendered tuberculous by previous inoculation, instead of no local symptoms appearing at the point of inoculation for ten days, as in the healthy animal, the place where the needle has been introduced appears on the first or second day hard and dark, and this condition spreads a short distance around. The dark colour indicates that the cells of the tissue round the spot of inoculation have become dead, and they are thrown off, leaving an ulcer which heals quickly and completely, and does not infect the neighbouring glands. Koch's further experiments showed that the local ulceration produced in the way just described was not due to living tubercle bacilli, for a similar result was obtained when they had been killed by boiling or by the action of disinfectants. It was therefore clear that the effect was produced by chemical substances, either entering into the composition of the bacilli, or closely associated with them. When pure cultivations of dead bacilli were diluted in water, they produced nothing more than local suppuration in healthy guinea-pigs; but in guinea-pigs already rendered tubercular by previous inoculation a very small quantity was sufficient to produce death, while a still smaller quantity, too small to kill the animal, was sufficient to produce widespread necrosis round the point of inoculation. When still further diluted, the injection of the fluid, so deadly in large doses, becomes salutary; the animals improve in condition, local ulceration diminishes and finally heals up, the swollen glands become smaller, the disease is arrested, and, if not too far gone, the animal recovers. The objection to using diluted cultures of dead tubercle bacilli is that the bodies of the bacilli are not readily absorbed, and give rise to suppuration. The glycerine extract, on the contrary, gives rise to no suppuration, and produces all the general conditions just described as occurring after the injection of the dead bacilli. In Koch's first paper (*vide* NATURE, November 20, 1890, p. 68), he was careful to point out that his remedy would not be of universal application, and said:—"I would earnestly warn people against conventional and indiscriminating application of the remedy in all cases of tuberculosis." He insisted on the fact that his remedy did not kill the tubercle bacillus, but only the tissues in which it was present, and pointed out that in cases where the necrosed tissue could not be removed his remedy was not likely to be of use. But Koch's warnings have been to some extent neglected, and his remedy has been used in unsuitable cases, with the result, as might have been expected, that harm, and in some cases death, has been produced. For example, it has been used in tubercular disease of the membranes of the brain, with the worst possible results.

The remarks of Prof. Virchow, summarized in NATURE of January 15, are probably only the beginning of a flood of unfavourable criticism which will be made upon Koch's remedy during the next few months. During the last month or two unwarranted expectations have been entertained by very many regarding the curative powers of Koch's lymph, and when these hopes are dashed they are likely to be succeeded by equally unwarranted abuse of the remedy.

As we pointed out in our issue of November 20, 1890, although analogy pointed to cultivations of the tubercle bacillus as being likely to prove preventive or curative in tuberculosis, and although Koch's present paper shows that they are what he has actually employed, still a consideration of the nature of phthisis would lead one to doubt whether these were actually the best adapted for the purpose of curing consumption, and whether we might not yet find cultivations of other disease germs more likely to cure this disease than cultivations of the tubercle bacillus itself. Most of the results which have hitherto been obtained confirm those put forward by Koch in his original paper, and they also show very clearly indeed the necessity for the closest attention to the caution in the use of the remedy which he earnestly enjoined. As a means of diagnosing phthisis in its earliest stages, Koch's lymph is certain to prove a most valuable if not an absolutely infallible means of diagnosis, and will thus ensure proper care in those cases where at present the slightness of the symptoms leads to doubt on the part of the physician, and sometimes to indiscretion on that of the patient. In such cases, as well as in lupus, it is likely to prove a potent curative agent, and to fulfil to a great extent the hopes expressed by Koch himself in the careful and moderate manner which is characteristic of the man. As its failure to effect everything that the public expected becomes generally known, we may expect to hear it even more abused than it has been praised; but it will nevertheless remain a great addition to our power of recognizing and treating consumption, as well as an earnest of yet better things to come.

INDIAN BIRDS.

The Fauna of British India, including Ceylon and Burma. Published under the authority of the Secretary of State for India in Council. Edited by W. T. Blanford. "Birds," Vol. II. By Eugene W. Oates. 8vo, pp. i.–x., 1–407. (London: Taylor and Francis, 1890.)

MR. OATES has, for the present, finished his work on Indian Birds, with the present instalment. It is satisfactory to learn, on the one hand, that the Indian Government so highly appreciate his administrative abilities in Burma, that they could not grant him the extra furlough necessary to complete his scientific work, and he was thus forced to terminate his duties in England, to return to his post in the Public Works Department at Tounghoo. It may be India's gain thus to sever him from the work which he so dearly loved and which he has executed with such conspicuous ability, but it will prove a loss to science, and it will be very hard to find anyone capable of continuing the description of the Birds of India in the same complete way that Mr. Oates has done. As the work has been done almost entirely in the writer's private room at the Natural History Museum, he is able to speak with some authority on the subject, and he wishes thus publicly to acknowledge the earnestness with which Mr. Oates wrote his book, the consideration which he showed for the officers of the Zoological Department, and the care which he took of the specimens, numbering many thousands, which passed through his hands. Curators of Museums will understand what we mean, for there is no part of their duty more irksome than the constant vigilance which is required to supervise the treatment of specimens by students, who seem to be often animated with the sole idea that when they have seen a specimen for their own purposes, it matters little whether the future investigator finds it with its head or wings off, or not. We only wish that every student of birds were endued with the reverent love for a well-prepared specimen which animates Mr. Oates and a few other naturalists we could mention. This by the way.

With the first portion of the second volume of the "Birds," Mr. Oates completes his account of the Passeres or Perching Birds of the British Asian Empire. Following out his ideas of classification, he first describes the Flycatchers and Thrushes, and follows them with the Dippers and Accentors. Then come the Weaver Birds and Finches, Swallows, Wagtails, Larks, ending with the Sun-birds, Flower-peckers, and, of course, finally with the Ant-thrushes or Pittidæ. No one will find fault with the position of the latter; but we greatly question the natural sequence of the other families. No one can doubt that Mr. Oates, in his classification of the Passeres, the most difficult of all ornithological problems, has advanced our knowledge of the characters of differentiation, but we must demur to some of his conclusions. However, here is a genuine piece of work, with chapter and verse for every one of the author's opinions, and we will therefore append a succinct account of the new facts brought forward by the author, and give a practical aspect to the present review.

Fam. MUSCICAPIDÆ.

Muscicapa parva is a *Siphia*. Oates, *t.c.*, p. 9. [This is an innovation, to be accepted with caution, for it introduces *Siphia*, hitherto an Indian genus, into the Palæarctic Region.]

Muscicapa albicilla is a *Siphia*. Oates, *t.c.*, p. 10. [This follows as a matter of course, as the species is the Eastern representative of *M. parva*.]

Muscicapa hyperythra is a *Siphia*. Oates, *t.c.*, p. 10. [So Cabanis was right, according to Mr. Oates, in describing this bird as a *Siphia*.]

Cyornis should be separated from *Siphia*, and not united to it, as has been done by Sharpe, as there is blue in the plumage. *Ergo*, *Muscitrea cyanea* is a *Cyornis*. Oates, *t.c.*, p. 13. [This is an aggregation of species, which we do not think will be ratified.]

Poliomyias hodgsoni (Verr.) apud Sharpe, is a *Cyornis*. Oates, *t.c.*, p. 14.

Muscicapula hyperythra (Blyth) apud Sharpe, is a *Cyornis*. Oates, *t.c.*, p. 15.

Digenea leucomelanura (Hodgs.) apud Sharpe, is a *Cyornis*, Oates, *t.c.* p. 16.

Muscicapula superciliaris (Jerd.), apud Sharpe. is a *Cyornis*. Oates, *t.c.*, p. 17.

M. melanoleuca, Blyth (*M. maculata*, Tickell, apud Sharpe) is a *Cyornis*. Oates, *t.c.*, p. 18.

M. astigma (Hodgs.) and *M. sapphira* (Tick.) apud Sharpe, belong to *Cyornis*. Oates, *t.c.*, pp. 19, 20.

Niltava oatesi (Salvad.), is a *Cyornis*. Oates, *t.c.*, p. 21.

Siphia pallidipes (Jerd.) and *S. unicolor* (Blyth) apud Sharpe, belong to *Cyornis*. Oates, *t.c.*, pp. 22, 23.

Muscitrea grisola (Blyth) is a Flycatcher, not a Shrike. Oates, *t.c.*, p. 31.

Cyornis poliogenys, Brooks, and *C. olivacea*, Hume, belong to the genus *Anthipes*. Oates, *t.c.*, pp. 33, 34.

Alseonax mandellii (Hume) = *A. muttui*, Layard. Oates, *t.c.*, p. 36.
Terpsiphone nicobarica, sp.n. Oates, *t.c.*, p. 45.
Hypothymis tytleri, from the Andamans, distinct from *H. azarea*. Oates, *t.c.*, p. 50.

Family TURDIDÆ.

Pratincola robusta, Tristram, said to be from Bangalore, is a Madagascar bird. Oates, *t.c.*, p. 58.
Saxicola barnesi, sp.n., from Afghanistan. Oates, *t.c.*, p. 75.
Cochoa is allied to *Geocichla*. Oates, *t.c.*, p. 158.

Family MOTACILLIDÆ.

Anthus cockburniæ, sp.n., from the Nilghiris. (*A. sordidus*, Sharpe (*nec* Rüpp.). Oates, *t.c.*, p. 305.

Family NECTARINIIDÆ.

Æthopyga anderssoni, sp.n., from Upper Burma. Oates, *t.c.*, p. 349.
Chalcoparia is not a Sun-bird, but is allied to *Zooterops*. Oates, *t.c*, p. 373.

Family DIEÆIDÆ.

Acmonorhynchus, gen. n. Type *A. vincens* (Scl.). Oates, *t.c.*, p. 382.

Besides these new features in Mr. Oates's book, there are many valuable criticisms on less important matters. One further point should be mentioned, as it was missed by ourselves in the British Museum "Catalogue," and Mr. Oates has unfortunately followed our lead. Erythrospiza of Bonaparte is quoted as published in 1831, but we quote from a letter of Count Salvadori: "You will find it in the 'Osservazioni el Regno animale del Barone Cuvier' (p. 80), and it is equivalent to *Carpodacus* of Kaup. So the genus *Bucanetes* must be used."

In a comprehensive work like this one of Mr. Oates, it is unlikely that all his conclusions, many of them novel and unexpected, will commend themselves at once to ornithologists. Our own opinion is that he has gone a little too far in promoting his theory of the value of the style of plumage in the young birds; but no one will deny that, for conciseness and painstaking labour, Mr. Oates's volumes are a model of what an advanced "hand-book" should be, and he has set such a high standard of work, that Mr. Blanford, who announces his intention of completing the ornithological portion of the "Fauna," will find it no easy task to follow in Mr. Oates's footsteps. As the latter gentleman is prevented by his superior official duties from continuing his work, it is at least fortunate that such a conscientious naturalist as Mr. Blanford has undertaken the task of completing the work which Mr. Oates has so well begun. We may add that the woodcuts by Mr. Peter Smit are as good as those which he drew for the first volumes of the "Birds," and are excellent in every way.

R. BOWDLER SHARPE.

A MANUAL OF PUBLIC HEALTH.

A Manual of Public Health. By A. Wynter Blyth, M.R.C.S., L.S.A. (London: Macmillan and Co., 1890.)

EARNEST efforts are being made to insist that candidates for the appointment of a medical officer of health shall have an adequate knowledge of sanitary science. The issue of this volume is therefore opportune. All the subjects of which a knowledge is required in examinations in hygiene and sanitary science are dealt with. Necessarily the ground travelled over is extensive, and some of the sections have not received that comprehensive treatment which they appear to us to need.

The matter is discussed under twelve heads. The first section is occupied with a brief account of vital statistics, but only in so far as they affect the duties of a health officer. As far as the subject is taken up it is clearly and lucidly treated, the description of the construction of life tables being particularly good. The section on air, ventilation, and warming is written in a thoroughly practical manner, but in describing the mechanical appliances for ventilation the author gives no illustrations, nor does he touch upon the ventilation of ships. The chapter concludes with an elaborate account of the methods of calculating cubic space, which, although useful for reference, is probably seldom required by the medical officer of health in his daily duties. In the description of hygrometers Daniels's does not find a place.

The first part of section iv., dealing with the sources of water-supply, is disappointing, for the subject has not received the share of attention which it decidedly merits. Cisterns obtain mention only as a necessary evil in an intermittent system of water-supply, there being no description of their varieties, and the dangers associated with the use of objectionable kinds. The water-supply of the metropolis is minutely detailed, full particulars being given of the exact area and districts supplied by the companies, the amount of water daily drawn from the Thames, the filtering appliances, and the average composition of the water distributed. The analysis of water is exhaustively discussed, and amongst the apparatus employed is a description of the useful pipette invented by the author. In section v., treating of drains, the varieties of drain-traps might perhaps have been more fully discussed, and the man-hole or disconnecting chamber dwelt on at greater length.

Like the water-supply, the sewage of the metropolis receives the most careful consideration, and the chapter on it contains an excellent explanation of the plan of the London drainage, together with a map of the same. In the treatment of sewage, precipitation processes receive a very brief notice. The subject of nuisances, so important for medical officers of health, is very fully entered into, the chapter embodying all the researches of Dr. Ballard on effluvium nuisances, and his recommendations for their removal.

Section vii., on disinfection, leaves nothing to be desired. Microparasitic diseases receive a greater share of attention than any other subject in the book, the bacteriology of each of the zymotic diseases being comprehensively treated. We do not of course underrate the value of such knowledge, but it appears to us that much of the detail which has been introduced would have been more suitable to a text-book on pathology than to one on public health. The remaining sections of the volume are devoted to isolation, hospitals, food, and the duties of sanitary officers.

On the whole, Mr. Wynter Blyth may be congratulated on the excellent text-book he has produced, based as it is upon the practical experience of many years of sanitary work, obtained in one of the largest metropolitan districts. If we have pointed out a few shortcomings, they have been those of omission, and common to most authors. Of the high value of the work as a text-book of public health there can be no question; and we hope that Mr. Blyth's manual will be in the hands, not only of students, but of all those whose calling is sanitary science.

J. H. E. BROCK.

OUR BOOK SHELF.

Lehrbuch der Zoologie für Studirende und Lehrer. Von Dr. J. E. V. Boas. Mit 378 Abbildungen. (Jena: Gustav Fischer, 1890.)

THIS newly published manual of zoology is a translation of the author's work, which was published in Danish in 1888. It is written from the modern standpoint, dwelling rather on the embryological and structural details of the forms of animal life, and using the scheme of classification as a subject of secondary importance. While the present volume is based on the author's previous work, it is no mere translation; not only is there a quite new chapter added under the heading of "Biology," in which the distribution of animals on land, sea, and fresh water, parasitism, non-locomotory animals, and such like subjects are briefly discussed, but changes have been made in the species of animal forms selected for illustration when those previously selected would not have been easily attainable by the German student. New figures have been introduced, and the work has generally been revised. The author warmly thanks Prof. Spengel, of Giessen, for much help rendered in the revision of the translation, German not being Dr. Boas's mother tongue. The first portion of this manual treats of the cells and tissues, the various organs or systems, development, and phylogeny, and includes the chapter above-mentioned on biology, and on the distribution of animals in space and time. The special portion treats of the classes of animals, from the Protozoa to Mammalia. Certain groups, the position of which is uncertain, are treated as "appendages" to the larger ones, such as the Sponges to the Cœlenterates, the Tunicates to the Vertebrates, &c. Possibly, from the student point of view, this is going too far afield. Another point that struck us in a perusal of this volume was the absence of all references to the work of others in this field of zoology. We are very far from suggesting that it would be desirable to refer, in a necessarily compressed statement of facts, to the first discoverer of, or recorder of, the same; but there have been some epoch-making discoveries, such as have revolutionized our ideas of development, structure, and classification, and we think it a good plan to let the student know the names of the authors of these, as we fancy that, by doing so, the facts are all the more impressed upon his mind. In some few cases we would even go further, and, by telling the student where to look for further details, try and interest him in bibliography. It may be as well to add that in an indirect way this reference to the labours of others is, in a few instances, made in this volume, for some of the illustrations are inscribed as "after Allmann, Huxley, Weismann, Sars," &c.

The great majority of the figures are well selected, and the volume of nearly six hundred pages is published in a style worthy of the firm which introduced Balfour's "Comparative Embryology" to the German student, and that has introduced to us the works of the Hertwigs, Kölliker, Lang, Weismann, and others.

A Pocket-book of Electrical Rules and Tables. Seventh Edition. By John Munro, C.E., and Andrew Jamieson, M.Inst.C.E., F.R.S.E. (London: Charles Griffin and Co., 1891.)

THE rapid progress made in the application of electricity for various purposes makes it necessary for every engineer to carry about with him some book to which he may refer. The present work has for some time been a boon to many, and its value has been increased by the improvements in the new edition. Among several additions by which the book is enriched is an article on telephony, by J. D. Miller. Not the least important item is the admirable and well-arranged index, which in a work of this kind is so essential.

LETTERS TO THE EDITOR.

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

"Modern Views of Electricity"—Volta's so-called Contact Force.

DR. LODGE's treatment of this subject at pp. 107–114 of his book presents at first certain difficulties. It is in the hope that he or some of his numerous readers may give a fuller explanation that I communicate them to NATURE.

We are told (p. 112) that a piece of isolated zinc has potential 1·8 volts below that of the surrounding air. This, it is said, is owing to the affinity of zinc for oxygen, and to the fact that atoms of oxygen combining with the zinc bring with them negative electricity. But (p. 110) the zinc cannot thus combine with many atoms, receiving their charges, without becoming so negatively charged as to repel oxygen atoms electrically as much as it attracts them chemically. This, indeed, may be considered as the state of equilibrium which is instantaneously attained.

In this passage Dr. Lodge does not explain how the oxygen atoms come by their negative charge. We can understand how they come to have it in electrolysis, to which we are told to compare the Volta phenomena. In case of electrolysis, oxygen atoms, seeking to combine with zinc, have first to dissolve partnership with atoms of hydrogen, and the condition of that dissolution of partnership is that oxygen goes away with a negative, and hydrogen with the corresponding positive charge. But in air atoms of oxygen exist only in combination with other like atoms, forming molecules of oxygen. And unless these molecules are negatively charged, it is difficult to see why on their dissolution the atoms combining with zinc are charged negatively.

We are told further (p. 110) that when metallic contact is made between zinc and copper, a rush occurs of positive electricity from copper to zinc, and of negative from zinc to copper, bringing both metals to a common potential 1·3 volts below the surrounding air. If that be so, the equilibrium which we said was attained in the case of the zinc is destroyed. The zinc, having by this rush been deprived of part of its negative charge, it can no longer be true that it "repels oxygen atoms electrically as much as it attracts them chemically." We should expect that a further combination of oxygen with zinc would take place, renewing the negative charge on the zinc, and causing a further rush of positive electricity from the copper. In fact, the conditions of equilibrium, when copper and zinc are in contact, seem to be unexplained.

Again, to explain the Thomson experiment with the aluminium needle (p. 111), Dr. Lodge says that the air near a couple of zinc and copper plates in contact is in a state of electrostatic strain, being at higher potential near the zinc than near the copper. But why should this be so if the two metals are at a common potential? And is it not inconsistent with the statement that the two metals are at a common potential 1·3 volts below the surrounding air?

S. H. BURBURY.

WITH much pleasure do I reply to Mr. Burbury's questions concerning the Volta effect, but must refer him to my memoir on the subject, "Seat of E.M.F. in Voltaic Pile," published by Messrs. Taylor and Francis, for the complete statement and argument, of which only a brief and picturesque summary is given in "Modern Views."

(1) The difficulty which Mr. Burbury mentions concerning the electric charge of gas atoms is a very real one, but it is not a difficulty peculiar to Voltaic doctrine; and, however it is to be accounted for, the fact that gas atoms are charged seems well

established by recent researches on gaseous conveyance of electricity and vacuum-tube phenomena. This, however, is a large subject, and cannot yet be regarded as by any means satisfactorily understood; though everything points to the fact that gases transmit currents by atomic convection, *i.e.* electrolytically.

(2) When copper touches zinc, the previous state of equilibrium *is* disturbed, and a fresh equilibrium is set up, into which dielectric strain in the surrounding insulator enters as a prominent component. Attack of the zinc, and continuous progression of electricity, are precisely what then tend to occur, being only prevented by the insulating character of the medium. Permit it to conduct, and the whole at once becomes a Voltaic cell on closed circuit.

(3) If the potential of a metal is defined as Sir W. Thomson defines it, viz. as the potential energy of a unit charge in the air close to the metal, the statement quoted from p. 111 must of course be modified; but if, as I venture to hold, it is more convenient to define the potential of a metal as the potential energy of a small unit charge in or on the metal itself, the statement involves no difficulty, and is, I believe, true. An intrinsic step of potential exists between each metal and the air in contact with it, which step is constant for each metal and calculable from thermo-chemical data; if therefore by metallic contact two metals be forced to the same potential, it at once follows that a slope of potential is set up in the air from one to the other. This is the very thing observed in all static Volta experiments, and has been cursorily stated as if it were a difference of potential between the metals themselves.

I think Mr. Burbury will find this quite clear if he does me the honour to read the complete argument; but if he still perceives a difficulty, I shall be much interested in hearing from him further.

OLIVER J. LODGE.

Attractive Characters in Fungi.

IT is to be hoped that the interesting discussion on the colours and attractive characters of fungi may induce someone, with the requisite time and patience, to undertake a study in this rich field of investigation, which has scarcely been entered. In a paper published in the *Annals of Botany* (vol. iii., No. 10, May 1889) it is shown that among the Phalloidei the coloration, odour, and contrivances for the attraction of insects for the dispersion of the spores are as remarkable as those possessed by many Phanerogams for cross-fertilization. Among 1288 species of fungi, other than Phalloids, tabulated from Bulliard's "Champignons de la France," Tulasne's "Fungi Hypogæi," and Cooke's "Agarici," the proportion of those with inconspicuous colours is about 73 per cent., while among the Phalloids the proportion is under 2 per cent.; 90 per cent. of the latter being either red or white. According to Köhler and Schubler, as quoted by Balfour, the proportion of inconspicuously coloured flowers, among 4197 species tabulated, is about 4 per cent., the proportion of red and white being slightly over 50 per cent. Seventy-six per cent. of Phalloids have functionally attractive odour, and only 9·9 per cent. of flowers; and 18·6 per cent. of these fungi have rayed or stellate forms, so common among flowers—a shape which I have shown by measurement and experiment to be that which gives the maximum conspicuousness *at moderate distances* (*i.e.* within the range of insects' vision) with the minimum expenditure of material. In *Coprinus*, where the spores become immersed in black and frequently very fœtid fluid, some species appear to resemble certain composite flowers which are visited by large numbers of flies, and Dr. Haas has found glucose in the hymenial fluid. There are reasons to suppose that the fœtor developed by *Phallus* may be due to the secondary action of putrefactive bacteria.

From analogy it is probable that the colours and many of the characters in other groups are not adventitious, but have been selected to aid in the preservation of the species; *e.g.* the *Pezizæ* are even more brilliantly coloured than the Phalloidei, and have the hymenial surface and spores freely exposed, and many small forms (*Amanita*, *Mycena*) are beautifully coloured, and grow in places where insects abound. In other cases the colours are no doubt protective by inducing resemblance, or by conspicuousness, as in many brightly-coloured poisonous forms (procryptic and aposematic colours of Poulton). I would suggest that in some cases the glutinous character referred to by Mr. Worthington Smith and Dr. Cooke may be protective against the attacks of animals, as insects and slugs. Of hundreds of specimens of *Phallus impudicus* which I have examined, I never found the gelatinous layer eaten through by slugs, although the spongy stem after emergence from the volva is frequently so eaten, and numbers of Agarici and other forms not so protected are attacked by insects and slugs. It is known that the mucoid secretion of slugs tends to protect them from the attacks of birds and ants, and other enemies.

T. WEMYSS FULTON.

20 Royal Crescent, Edinburgh, January 10.

The Morphology of the Sternum.

MY friend Prof. T. J. Parker has in these pages (Dec. 11, 1890, p. 142) lately recorded the existence of a sternum in the shark *Notidanus indicus*. The anterior of the two cartilages which he figures has been already described by Haswell (Proc. Linn. Soc. N.S.W., vol. ix., part 1); and, in view of Parker's conclusions, it is interesting to note that he speaks of it (p. 23) as "temptingly like the presternal," but that "the presence of such an element in the skeleton of any group nearer than the Amphibia seems to preclude this explanation." That the Amphibian sternum is for the most part, if not wholly, a derivative of the shoulder-girdle, there can no longer be a question; and, although the researches of Goette leave us in doubt concerning the hypo (post-omo) sternum, they show that that can be no derivative of the costal apparatus. Working anatomists will realize in Parker's application of Albrecht's terminology the expression of a fundamental difference between the sternal skeleton of the Ichthyopsida and Amniota. The researches of Goette, Hoffmann, Ruge, and others, show the sternum of the higher Amniota to consist of a greater costal portion and of lesser ones, chief among the latter being the episternum or interclavicle. They suggest (especially if Hoffmann's assertion that the precoracoid or clavicular bar is, in Mammals, primarily continuous with the spine of the scapula) that the interclavicle may be, throughout, the vanishing vestige of the coracoidal sternum of the Ichthyopsida. The latter would appear, therefore, to have been replaced in time by the more familiar costal sternum, derivative of the hæmal arches (ribs); and, this being so, might we not boldly, and with advantage, go a step further than Parker has done, and distinguish between a coracoidal *archisternum* of the Ichthyopsida, and a hæmo-coracoidal *neosternum* of the Amniota? If this be conceded, the characters referred to must be incorporated in our diagnoses of the two great types named.

G. B. HOWES.

South Kensington, January 12.

Stereoscopic Astronomy.

THE following exquisite test of the delicacy to which astronomical photography has attained may be interesting. In Admiral Mouchez's "Photographie Astronomique" (1887)—a small book, and cheap—are eight photographs of Jupiter, by the MM. Henry, taken on April 21, 1886. Several are at intervals of only three minutes in time. What with the large red spots, the irregularities of the two belts, and white spots on the upper belt, there are quite details enough to enable the eye to perceive the solidity of the planet, in a stereoscope, if the earlier picture is submitted to the right, and the later to the left eye. Reversing the order of the pictures gives a puzzling effect, which, with a little practice, is seen to be hollowness instead of solidity. But the mind resents this true result, and so gets puzzled.

To satisfy myself that I was not, on the other hand, misled by the wish to see solid, I put the matter to the proof by asking a friend to shuffle the photographs, and submit any two to me in the stereoscope without either of us knowing which they were, or in which order they were placed. After recording my judgment, "solid" or "hollow," on each pair, the times and order of place were ascertained and recorded. I found that I was able, after twenty trials, not only to say whether two images taken three minutes apart were rightly or wrongly placed in the stereoscope, but I could guess in any case with some accuracy what the interval was before either of us knew it. This, of course, was only possible by familiarity with these particular images.

W. J. H.

Lawn-Upton, Littlemore, January 17.

Mock Sun.

LAST evening, about five minutes after five o'clock, I observed that a cloud in the south-west was strongly illuminated from below. As the sun had set more than half an hour, and considerably more to the south, I was surprised by the degree of illumination. Observing more closely, I saw about 5° above the horizon, and about 12°–15° north of Hartland Point, the appearance of the sun in a fog, but only about one-third the apparent diameter when in the same place. I watched it for about five minutes, when it was gradually obscured by the rising mist. T. MANN JONES.

Northam, Devon, January 17.

Our Latest Glacial Period.

I AM informed that near the Wash, and I suppose at other parts of the coast, the sea at low water is frozen into masses which with the rising tide become floes, and are urged backwards and forwards on the beach. This is, I believe, not a frequent occurrence on our shores, and it would be interesting if any observers could note whether the shingle or the stones embedded beneath the floes, when such are found, have become polished or scratched as by glacial action.

W. ATKINSON.

17 Trafalgar Square, Chelsea, S.W., January 5.

P.S.—My anticipation has proved correct as far as the small bergs in the Thames are concerned, for after a little search I have found in Chelsea Reach chalk blocks with grooves and striations that would be no discredit to a boulder clay specimen. I should be glad to hear of any similar markings on flint, chert, or other hard rocks, or even on limestone or sandstone, and also to learn whether there are, as I think there must be, other recorded instances of the formation of glaciated rocks in the British Isles or the coasts of Europe since Pleistocene times.

January 17.

THE GREAT FROST OF THE WINTER OF 1890–91.

TO find a parallel to this frost for intensity and endurance, we must go back, as regards London and the south of England generally, to the severe winter of 1814, when the great fair was held on the Thames, which for long presented from bank to bank a uniform stretch of hummocky ice and snow. In that year the severity of the winter was more equably felt over the whole of Great Britain than during the present winter. Thus in 1814, the mean temperature of Gordon Castle, near the Moray Firth, for January was 27°·0, whereas during last December it was 36°·5; and, so far as records go, all parts of the United Kingdom suffered nearly alike during that memorable winter.

But during this winter of 1890–91, the contrasts of temperature in the different parts of the country from Shetland to the Channel are altogether unprecedented. In Shetland and Orkney, the mean temperature of December was about half a degree above the mean of the month for the past thirty-five years. In Caithness it was about the average, but on advancing southward the cold was the more intense, till its maximum intensity was unquestionably at Oxford, where the mean of the month was 11° below the mean of the past 35 years. The following short scheme shows generally the geographical distribution of this great frost, the first column giving the depression below the mean at places on the west coast; the second, at places in the interior of the island; and the third, at places on the east coast:—

West Coast.	°	Inland.	°	East Coast.	°
Barrahead ...	−0·9	Inverness	−1·4	Fraserburgh	−0·3
Skye	−1·2	Braemar ...	−1·8	Aberdeen ...	−0·6
Islay	−2·0	Glasgow ...	−3·8	St. Abbs ...	−2·8
Douglas (Isle of Man)	−4·4	York ...	−5·6	Spurn Head	−4·7
		Loughboro'	−8·8	Yarmouth ...	−5·9
Holyhead ...	−6·0	Oxford ...	−11·0	——	
Pembroke ...	−6·7	Southampton	−8·8	Dungeness...	−8·1
Scilly... ...	−4·4				

As occurs in all low winter temperatures, the intensity of the cold is most pronounced in situations farthest removed from the ocean. Thus, from Oxford, the intensity of the frost was in all directions less felt. In Ireland, the intensity was pretty evenly distributed, ranging below the average from −2°·5 at Dublin to 4°·6 at Foynes and Killarney.

A very cursory examination of the weather maps of the Meteorological Office shows at once the cause of this singular difference in the degree to which different parts of Great Britain have been subjected to this frost. During the whole of this period atmospheric pressure to the east and north-east of the British Islands, notably over Russia and Scandinavia, has been unusually and persistently high, rising on occasions above 31·000 inches; thus, so to speak, stopping the way to the usual easterly course of the cyclones from the Atlantic over NorthWestern Europe. Thus, in the extreme north of the British Islands, pressure has been lowered below what prevailed to the south, and consequently the preponderance of south-westerly winds has been greater. On the other hand, farther south, barometers have been almost constantly higher than they have been away still farther to southward; and be it particularly noted, low-pressure areas, or cyclones, have been almost constantly present over the Mediterranean, or even on occasions farther south, either formed over this region or drafted in from the Atlantic, with the inevitable result that the whole of Western Europe has been overspread with polar winds from north, north-east, and east, bringing with them a degree of cold which the newspaper press has been chronicling for us at our breakfast-tables day by day.

INDIAN ETHNOGRAPHY.

OUR Indian dependencies form a vast field for ethnological inquiry which we have not as yet sufficiently cultivated; in fact, its importance is realized by but very few. What is really required is a systematic study of the various races of India, carried out according to a definite plan. Independent observers may do, and many have done, much; but by co-ordination more and better work can be accomplished. The Bureau of Ethnology in Washington has for its especial object the investigation and recording of all that relates to the North American Indians, and the splendid series of Reports issued by that Bureau form an invaluable mine of information on American anthropology. Is it too much to ask from our Government that we should have an analogous Bureau of Indian Ethnography? It would not suffice merely to have a department for researches on Indian ethnology, and for the publication of the results; something more than this is wanted. It would be necessary to have a library of works relating to Southern Asia, and to have an elaborately classified catalogue of books, memoirs, articles, and so forth, on every branch of Indian anthropology. Were this done, anyone who wished for information about a particular district would be able to find references to all that was known about the people, their customs, arts, and crafts. The catalogue should be a systematic bibliography, irrespective of the actual contents of the library of the institution, though every endeavour should be made to make this as complete as possible.

Such a Bureau, if properly directed, would serve as a great stimulus to those who are interested in the native races of India, but who require encouragement and direction. There can be little doubt that an immense number of isolated observations are lost for the lack of a suitable depository, the recorders of such observations being fully aware that these are too casual to be of much value; when accumulated, however, the case is very different. Were it known that a record of any obscure or rarely observed custom would be duly filed and so classified as to be readily available to anyone who was studying Indian folk-lore, the probability is that many memoranda would find their way to the Bureau which otherwise would be lost.

It cannot be too often or too strongly insisted upon that now is the time for the collection of all anthropological data in every department of that far-reaching science. To many, results are alone interesting, and there is too frequently a danger to generalize from imperfect data. Unfortunately in no department of science is it more easy to theorize than in this, and those who have not sufficiently studied the subject are often the most given to framing hypotheses which are as easy to refute as they are to make, and it is this which has brought discredit upon anthropology. Posterity will have plenty of time in which to generalize and theorize, but it will have scarcely any opportunity for recording new facts. This century has been one of most rapid transition. The apathy of our predecessors has lost to us an immense amount of information: let not this reproach be applied to us by our descendants.

The change which is everywhere noticeable is from individuality to uniformity. Religious beliefs are less varied than formerly, there are fewer local customs, there is greater uniformity in dress and personal ornament, the tools and weapons of the white man are now cosmopolitan. It is unnecessary to multiply instances: every book of travel directly or indirectly witnesses to these facts. The vulgarization of Oriental fabrics, the degeneration of Japanese art products, also testify to a levelling down, which together with a levelling up is characteristic of our modern civilization.

Every effort should be welcomed which endeavours to place on permanent record local peculiarities of any sort, and it is with pleasure we notice the too short paper[1] in which Herr L. H. Fischer gives the results of his personal investigations on the jewellery of the people of India and on the manner in which it is worn. As the author points out, the Hindoos are very fond of ornament: the ears, nose, neck, upper and lower arms, fingers, ankles, and toes are adorned; but not the lips, as in some African and American tribes.

The culture and history of a people are intimately

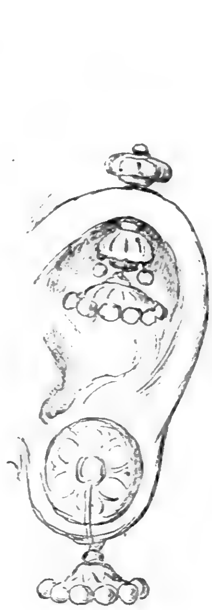


FIG. 1.—Ear ornaments

FIG. 2.—Sinhalese ear ornament; this is very similar to an ornament common in the Solomon Islands.

interwoven, and Indian history is so complicated that India at the present time appears at first sight to be a

FIG. 3.—Silversmith.

conglomeration of races, religions, languages, and States which can scarcely be unravelled; and now this is further complicated by the introduction of European culture.

At first it seems almost impossible to discriminate the typical ornaments of the separate race-stems, but in time it is discovered that the lower classes keep to traditional forms. The village smith transmits his art from father to son and grandson, always with the same archaic moulds, the same simple tools, the same designs; and it

[1] "Indischer Volksschmuck und die Art ihn zu Tragen," L. H. Fischer, 30 pp., 51 woodcuts and 6 plates, *Annalen des k.k. Naturhistorischen Hofmuseums*, Bd. v. Nr. 3 (Wien, 1890).

is only the present luxury which induces fashions. The author chiefly turned his attention to the jewellery which the main mass of the people wear, and not to that of the rich, for this appears to be frequently imitated from European articles.

The material which in India is employed for jewellery is mainly silver, pure or in mixture with tin, zinc, and lead; of these, there are many alloys which constitute a gold-like metal. As a rule, yellow metal obtains in the south and white metal in the north-west, silver always predominating. In Peshawur, for example, there is hardly anything but silver. Gold is rare in India.

India possesses all known kinds of precious and less precious stones, but the polishing is as a rule very primitive. Particular provinces appear to have a predilection for stones of a certain colour; thus, in the Madras Presidency especially, green stones are almost invariably worn in the men's earrings. In Jeypore, ornaments of Indian garnets can be bought in great abundance, and the turquoise is characteristic of the Himalaya district. Naturally all kinds of stones are imitated in glass: there are glass arm-rings in South India which are principally made in Poona, Taragalla, and Surat, and are much worn. Ivory, coral, pearls, shells, and other materials are also pressed into the service of personal adornment. Bracelets made from the Changu (*Turbinella rapa*) occur in varied form in the Dacca district. The author only occasionally saw mother-of-pearl fabricated into amulets and in Ceylon into rings.

The author then goes on to describe the costume and types of ornaments characteristic of various parts of India. Numerous sketches of all kinds of jewellery illustrate the paper. There are ten representations of women from different districts scattered in the text, one of which, a Tamil from Trichinopoly, we reproduce as a specimen of the illustrations to the paper. There are also six plates of full-length portraits of women in typical costumes, three of which are in colours.

Specimens illustrative of this paper and collected by the author are to be found in the Vienna Museum. There is also in the Berlin Königliche Museum für Völkerkunde a fine collection illustrating Indian ethnography, which is arranged in a most instructive manner. Maps, photographs, and models are liberally interspersed, and the labelling is exceptionally good. Jewellery is dealt with ethnographically, and not merely as a branch of æsthetics, the use of the trinkets being illustrated by photographs and models. One thing is certain—that is, that Germans need not go further than Berlin if they desire to have an intelligent and comprehensive presentment of Indian ethnology. So firm is the conviction of Dr. Bastian, the energetic Director of the Museum, of the present necessity for gathering up the dying-away remnants of more or less barbaric and savage peoples, that he is once more on a collecting tour—this time in India—and is continually sending to Berlin cases of specimens, regardless alike of cost and space for exhibition. He feels that it is now his duty to collect, and this spirit is manifest in other departments of the Museum, notably also in one illustrative of another of our British colonies. Capt. Jacobsen is one of the best of collectors, and he has brought together an invaluable collection from North America, especially from British Columbia, the long series of grotesque dance-masks being of particular interest.

It is convenient for European ethnologists that these objects are in such an accessible Museum as that in Berlin; but we, as Englishmen, would like to see the ethnography of all our British colonies as fully represented in our own National Museum. It is true there does not at present exist any machinery for making special collections, nor was there in Berlin until enthusiasts like Dr. Bastian and others created it. There are difficulties with regard to funds and storage-room; perhaps Dr. Bastian's plan of ignoring these problems and of securing the specimens is not so very bad after all.

FIG. 4.—A Tamil woman from Trichinopoly.

It may be urged that we already have an Indian Museum. This is true, but that collection is little more than an assemblage of specimens.

A museum has at the present day quite a different object from what it had in the past. The distinction can be put succinctly by an analogy; most of the older museums bear the same relation to modern museums that dictionaries do to text-books. Most people will admit that the perusal of lexicons is somewhat monotonous and dull, and similarly the arrangement of the old class of museums was such as to give the least amount of instruction beyond the bare fact of the existence of given objects.

Large national collections should be exhaustive, and this necessitates a multiplicity of objects, but that should not preclude a scheme of arrangement which would make the specimens yield the maximum amount of information they are capable of giving. The Indian Museum affords an example of the worst style of museum arrangement.

The public has a right to expect that national specimens shall be arranged in the best possible manner, and the Government should appreciate the fact that museums, if properly conducted, afford the most interesting and vivid means for conveying instruction.

ALFRED C. HADDON.

THE APPLICATIONS OF GEOMETRY TO PRACTICAL LIFE.[1]

THERE is scarcely any branch of modern science which has of recent years made such progress as geometry: there is certainly no branch over the purport of which there is so much obscurity or has been so much discussion. On the one hand, geometry, like most sciences, was born of a practical need. The Egyptians,[2] an eminently practical people, were not interested like the Greeks in the properties of the circle for the circle's own sake, but they wanted an art to measure the capacity of their barns and the size of their haystacks, and to plan out their pyramids and great buildings. But above all they were landowners, and to sell property they required to measure land—to measure it in square feet, and not by the time that a yoke of oxen would take to plough it, which was not always an exact or convenient test. So the Egyptians invented land-measuring or surveying, and termed it *geometry*, and the geometricians they called *rope-stretchers*. Thus in the doggrel of an old text-book:—

> To teach weak mortals property to scan
> Down came geometry and formed a plan.

The origin and the early applications of geometry were thus essentially due to the needs of *practical* life.

On the other hand, the Egyptians, having satisfied their immediate wants, left geometry uncultivated, and by not pursuing it on purely theoretical grounds, failed to convert it into that great instrument of investigation which in the end was to master the mystery of the heavens, guide the mariner across the trackless sea, or help the engineer to span the St. Lawrence or Douro.

The next stage in the development of geometry was left to the Greeks, for whom to apply geometry to practical purposes would have been to debase it. They studied geometry for its own sake, much as some of our friends to-day study metaphysics, only, it seems to me, they did it to more purpose. They recognized in geometry a great instrument for sharpening the intellect, and they made it the basis for a sound education. A proposition was to them a delight in itself, and to deduce a new one a distinct intellectual advance. Thus they had the proverb, "A figure and a stride: not a figure and sixpence gained."

I cannot emphasize this purely theoretical tendency of Greek geometry better than by a tale which is told of Euclid by Stobæus:—A youth, who had begun to read geometry with Euclid, when he had learnt the first proposition inquired, "What do I get by learning these things?" So Euclid called his slave and said, "Give him threepence, since he must gain out of what he learns."

I have said enough perhaps to indicate how the two tendencies of modern geometry, and indeed of the whole of modern science, date back to the very beginnings of scientific activity—to the practical Egyptians, whose horizon was bounded by the immediate needs of life, and to the dreamy metaphysics-loving Greeks, who despised practical applications. There are few teachers of geometry who will not have felt at times the burden of these two tendencies. The great mass of material in the form of published papers on higher geometry, many of which can only be understood by the initiated few, and some of which have probably never been read except by their writers—this weighs at times upon the mind and makes one, without despairing of science, cry, "*Cui bono?* For whose good? How can this help the progress of mankind?" On the other hand, how the listless student, bent on struggling through life with the least expenditure of intellectual energy—how he calls up the spirit of the Greek, when he languidly asks his teacher after lecture, "What is the *use* of this? I've got the result in 'The Engineer's Pocket-book.'" For *him* the insight to be gained by seeing the *how* and *why* of a process is of no importance, and the fingers tingle to hand him threepence that he may at least gain something by attending our lectures.

It is not my purpose now to trace these practical and theoretical tendencies through the history of geometry down to the present day. Neither do I intend to emphasize one tendency at the expense of the other. But of this fact I feel clearly and absolutely certain, that a divorce between the two—such as has existed in some of our great mathematical schools—is wholly unnatural, and tends sadly to retard the efficiency of both. What we are slowly but surely learning in this country, owing to the pressure of foreign competition, is that education and theory are needed in all branches of practical life, if we are to maintain our industrial position. But it must be education and theory which is sympathetic to practise, can indeed be wedded to it, and takes upon itself no cynical and superior airs. When we compare on the one side the vast amount of mathematical talent out of touch with all human needs, and on the other the amount of practice which limps along for want of theoretical support, we cannot but be grateful for any institution or foundation which tends to promote a better fellowship between the two. This union of theory and practice, with its offspring the applied sciences, has nowhere in recent times met with more cordial support than in the City of London. Within the last twenty years the science of engineering has been revolutionized; from an empirical and mechanical craft engineering has been raised to the rank of a learned profession. The introduction of theory into engineering practice has been largely due to the progress of modern geometry and the geometrical methods of calculation.

Problems, which when clothed in mathematical symbols only served to appal the practical man, became intelligible to him when hieroglyphics were replaced by curves upon the drawing-board. The success of this particular union of practice and theory is largely, I believe, due to the choice of a geometrical method, to the recognition that form and figure are more easily realizable by the average mind than symbol and numeric quantity.

I have referred to the union of theory and practice which has been so largely realized of late years in engineering instruction because it offers us a striking example, not only of the success of theory as applied to practice, but also of the manner in which that theory, in order to be successful, must be applied. The theory does not need to be superficial, but it must be of a kind which the practical man can grasp; the calculations must be made

[1] A thirty minutes' Probationary Lecture, delivered at Gresham College, on Friday, December 12, 1890, by Prof. Karl Pearson.

[2] The historical facts of this lecture are chiefly drawn from two excellent books—Gow's "History of Greek Geometry," and Ward's "Lives of the Professors of Gresham College."

in a form which appeals to his imagination, and in the particular sciences preliminary to the engineering profession this has been largely done by the aid of geometrical and graphical methods. Twenty years ago these methods were scarcely discovered, or the few known were neglected or scouted. To-day the most scientific Government in Europe permits the calculations and plans of the largest engineering structures which are submitted for its approval to be made by purely graphical processes. Here, then, we have an instance of theory placing at the disposal of practice one of the most efficient instruments of modern calculation and investigation, and this, indeed, is peculiarly the light in which, owing to early tradition and present needs, geometry ought, I think, to be dealt with at Gresham College. I do not mean by this that the sympathies of the City should be entirely with what we may term the Egyptian as contrasted with the Greek view of science, but solely that the City has already entered upon the labour of reconciling theory and practice, and that for a long time to come more efficient work might probably be done in this College by spreading and utilizing existing knowledge than by extending the boundaries of pure theory. The Gresham lecturer will, I fully believe, best supply existing needs, if he deals rather with the applications of geometry to practical life, than if he discourses on the more complex aspects of his subject.

I have said that this seems to me consonant with the early traditions of the College. When Sir Thomas Gresham founded this College, the old mediæval conceptions of education were dying, and modern science and modern thought were in their birth-throes. The Renascence with its revival of learning had resuscitated the knowledge of the Greek geometry. But the minds of men were not content with pure theory; they were anxious to understand the laws of the physical universe—astrology was being replaced by astronomy, chemistry was deposing alchemy. The old forms remained, but they were filled with a new life. Sir Thomas Gresham, indeed, when he founded his College established his seven professorships on the lines of an old mediæval University, in which all knowledge was forced into one of the seven divisions—divinity, astronomy, geometry, music, law, physic, and rhetoric. But what a very different view the early science professors—those of astronomy and geometry—took of their subjects to what would have been possible a hundred years earlier! Geometry for them meant the application of mathematical knowledge to all the branches of physical science. It was not for them the pure theory of lines and circles and curves, but a process of calculating and investigating the facts of Nature. Thus the revival of geometry in the sixteenth and seventeenth centuries was on Egyptian rather than Greek lines. Newton, with astounding ingenuity, used geometry as his main instrument for investigating the motions of the moon and planets. The early occupants of the Gresham chairs of both geometry and astronomy were amongst the most distinguished scientific men of their time, especially interested in the application of mathematics to the problems of Nature and to the practical sides of life. Those were the days when England was building up a greater empire for itself on the other side of the world, and if you were to ask me what beyond their indomitable pluck carried our sailors and colonists over the Atlantic and Pacific in their frail and diminutive craft, I should reply, The labours of the Gresham professors of geometry and astronomy. It was they who published the first tables and manuals for English seamen, explained and improved the compass, the sextant, and the construction of ships. Briggs, the first occupant of the chair of geometry, wrote a work entitled, "The North-west Passage to the South Sea through the Continent of Virginia," and another entitled, "Tables for the Improvement of Navigation." It was Briggs who was mainly instrumental in introducing the use of logarithms, that most wonderful feature of modern calculation, the use of which is imperative on every seaman and astronomer of to-day. His colleague in the chair of astronomy, Gunter (1619–26) drew up a table of logarithmic sines and tangents for the first time—a table familiar now to every navigator and land-surveyor. He was also the first discoverer of the slide-rule, now found in every architect and engineer's office, while for long his sun-dials at Whitehall remained standard time-keepers. Gellibrand, his successor (1626–36) wrote a treatise on the variation of the magnetic needle, and an "Epitome of Navigation" for seamen. No less active in this direction was Samuel Foster, who held the astronomy chair from 1641 to 1652. He explained the use of the quadrant for finding position at sea, and wrote more than one work bringing home the results of theory to the seventeenth century seamen. Lawrence Rooke, who successively held the chairs of astronomy and geometry, published "Directions for Seamen going to the East or West Indies to keep a Journal." To Sir Christopher Wren, who was Gresham professor from 1657–60, there is no need to make any reference in the City. His practical applications of theory are well known; that he published books on navigation and the structure of ships, that he first gave a theory of the pendulum, and improved the telescope, is perhaps less generally remembered. In his days there was a scientific enthusiasm at Gresham College which we can hardly realize anywhere now. Wren, we hear, had special charge of the planet Saturn, and his colleague Rooke of Jupiter, and their observations and lectures turned on the great discoveries then being made with regard to these peerless chiefs of the solar system.

But perhaps the most brilliant of the Gresham professors was Robert Hooke, who held the chair of geometry from 1665 to 1703. He also published "Directions for Seamen"; he delivered and afterwards published "Lectures for improving Navigation and Astronomy." But more than all he invented the watch, with the declared object of measuring time at sea, where no pendulum clock could be of service. The first account of the construction of the watch was given by the Gresham professor of geometry in his lectures at the College on "Several new Kinds of Watches for the Pocket wherein the Motion is regulated by Springs." Hooke improved also the reflecting telescope; he invented a marine barometer, and several new kinds of lamps. He wrote a treatise on the sails of windmills. He laid the foundation of the modern science of elasticity, and made the earliest researches of scientific value on the strength of materials. After the Great Fire of London, Hooke, like his former colleague Sir Christopher Wren, presented a model for the rebuilding of the City. Indeed, it is no exaggeration to say that in the seventeenth century it was to the Gresham professors that practical men seeking help from theoretical science naturally turned.

I might, had I the time at my disposal, bring still further evidence to show that the earliest of Sir Thomas Gresham's lecturers were essentially occupied with the applications of science to practical life, and that this tradition lasted so long as the post of Gresham lecturer meant in itself one of the highest distinctions in the land. But I can only now refer to one fact from which in itself a true idea of the original activity of Gresham College might be formed. Gresham College was the cradle of the Royal Society. It was within its walls, and notably within the rooms of the professor of geometry, Lawrence Rooke, that the makers of England's earliest scientific reputation, men like John Wallis, Robert Boyle, and Lord Brouncker, together with the Gresham professors, Christopher Wren, Robert Hooke, and Sir William Petty, used to meet to discuss experiments, and it was at Gresham College that they received their charter of incorporation as the Royal Society in 1662.

A French traveller, who visited England in the year

1663, and whose diary has recently been republished, gives us an account of several visits to the Royal Society's meetings at Gresham College :—

"On May 23," he writes, "I was at the Academy of Gresham, where every Wednesday an assembly is held to make a variety of experiments upon matters not yet fully understood, but which are described according to each one's knowledge, while an account of them is written out by the secretary. The President, who is always a person of quality, is seated at the top of a great square table, and the secretary at one side. The Academicians are seated on benches running round the hall. The President is Lord Brouncker, and the secretary is Mr. Oldenburg. The President has a little wooden hammer in his hand, with which he strikes the table to call to silence those who want to speak when another is speaking; thus there is no confusion or clamour.

"It was reported that salt of tartar put upon toads, vipers, or other venomous beasts caused them to die; some one said that quicksilver had the same effect; that these animals could not live in Ireland, as they could not bear the soil, and that experiments had been made by putting them on soil brought from England along with the animals; when they thought to escape, and approached the soil of the country, they always had to turn back, and did this until they died. Further, that a branch of holly placed in a certain lake in Ireland, in such wise that a part was in the earth, a part in the water, and a part in the air, after some time—a year or thereabouts—changed its nature; the part in air remained indeed wood, but that in the water became petrified, and that in the earth metallic in character. . . . In order to procure in ponds fish of all sorts which are difficult of transport, it is only necessary to carry the eggs of the fish one requires, and these will afterwards hatch out; this a lord from Ireland said he had put into practice. Further, it was noted that the germination of insects does not arise from decay; for the intestines of an animal and other parts which easily corrupt, having been placed in a glass closed with cotton-wool, so that no fly or other animal could enter, but only the air could penetrate, they had been preserved for six weeks without maggot or other thing being observed. . . . Bodies weighed in the air had been afterwards weighed in a very deep pit, and had been found to weigh one-sixteenth less. That bodies which sunk in water came up again when one put more water into the vessel, which proved the compression of water by water. . . . Sir Robert Moray told me that the President wished to give to the public a new science of the movement of bodies in water, and so to improve the art of navigation; with this end in view he was experimenting on the ease with which bodies of diverse shapes moved through water. . . . That a method of learning the difference of weight of various liquids was to weigh in them a body attached by a fine thread of silver or other metal, and the difference of the weights of this body enabled one to estimate the weights of the liquids.

"The meeting concluded with the exhibition of a number of experiments made with an air pump invented by Robert Boyle."

Some of these experiments may sound strange to modern ears trained to a more scientific view of natural phenomena; but their general drift is in the right direction, and their bearing on the needs of every-day life sufficiently obvious to warrant us in asserting that it was in Gresham College and around its professors that in the seventeenth century those interested in the practical and experimental sides of science collected. I believe that the dignity and importance of the College in its early days were largely due to its being closely in touch with the wants of practical life. I have no wish to minimize the educational value of purely theoretical science. I recognize how great a factor it has been and is in the intellectual and spiritual growth of the nation. Investigations like those of Darwin and Maxwell, which appear at first sight to have no practical applications, may profoundly alter our whole view of human life, or of the physical world which surrounds it, and in doing this may modify indefinitely our practical conduct or our command of the forces of Nature.

Even geometry in its more abstruse speculations, when it transcends the space in which we live and theorizes of another, of which ours is as poorly representative as a landscape painted on flat canvas is poorly representative of the wealth of form and distance in the scene it depicts —even this abstruse geometry may some day react on practical life, by the modifications it is capable of producing in the current ideas of space and force. I recognize to the full this educational value in geometry, and in all forms of pure science; but I believe that there are other institutions—notably the great Universities—which sufficiently emphasize this side of learning. On the other hand, I think that there is a gap which Gresham College is well suited to fill, and I believe that to fill it would not be out of accordance with its early traditions. By this gap I understand the want of an institution which, while recognizing the educational value of science, would mainly devote itself to pointing out, in a popular manner, the bearing of the conclusions of modern science on practice and the applications which can be made of them to ordinary life.

In particular, it seems to me that the lectures on geometry can be made especially serviceable in this direction, if geometry be interpreted in the wide sense current in the seventeenth century, and which it retains to this day in France. The modern development of graphical and geometrical methods has placed a powerful instrument of calculation and investigation in the hands of those who have neither the time nor opportunity of learning to handle the abstruse tools of analytical mathematics. Wherever quantity of any sort has to be measured and reasoned upon, there these geometrical methods find their applications. Their applications are indeed so manifold that it is difficult to enumerate them: to questions of force and motion, to problems in the strength of materials, in the structure of bridges and roof-trusses, of machinery in motion, of cutting and embanking—they have been long applied, and form the basis of much of modern engineering practice. But there are other fields which would constitute more suitable topics for a Gresham lecturer. The graphical representation of statistics at once suggests itself. Mortality, trade, goods and personal traffic, furnish statistics which if dealt with in a graphical manner very often suggest conclusions which are of the greatest interest to those dealing with problems of insurance and commerce—conclusions more readily deducible from the geometrical than from the numerical representation of statistics. What may be achieved in this direction is admirably illustrated by the graphical album of trade returns published annually by the French Government. The like geometrical methods have in recent years been applied to the principles of political economy, till the theory of prices has become almost a branch of applied geometry.

But it is not alone in these very specialized subjects that we may reason geometrically. The whole field of physical science is occupied with the investigation, representation, and reasoning upon *quantity*, and therefore is essentially a field for the application of geometrical methods, but the bearings of physical science on practical life are too wide and too well known to be enlarged upon now. I had intended originally to take to-night some single point in this field, and explain how geometry might be used to elucidate it; but on second thoughts it seemed to me probable that the geometrical preliminaries would have absorbed all the time at my disposal, and that accordingly I might with more advantage lay general stress on the importance of the practical applications of geometry. In doing this, I have possibly had the future of Gresham College more in view than my own candidature for the lectureship in geometry.

But I believe that, quite apart from the present election, the College has a future worthy of its earliest days, and that, not improbably, this future, if in another field, will still lie within the same broad lines that the City has already laid down for itself in the matter of technical education, the motto of which I take to be: Practice enlightened by theory, theory guided by practical needs. Work on such lines as these, accompanied by the expansion due to modern scientific requirements, would, I fully believe, restore the College to something like its old position among the teaching bodies of London, and reverse the judgment of that Cambridge historian of mathematics who has recently remarked that, "with the beginning of the eighteenth century, an appointment at Gresham College ceased to be a mark of scientific distinction."

THE PHOTOGRAPHIC CHART OF THE HEAVENS.[1]

THE publication of the fifth fasciculus brings us within reasonable distance of the actual commencement of the celestial chart, and the centre of interest is shifted from the theoretical speculations which have characterized the earlier publications to the more practical details suggested by the employment of the photographic instruments in those Observatories which are now equipped for the undertaking.

After three years of anxious organization, Admiral Mouchez sees the goal for which he has laboured so strenuously well in view. We may offer him our congratulations on what may be regarded as the completion of the first, but not the least arduous, portion of the task he has undertaken. He has succeeded in binding together, with a common aim and with unity of purpose, the astronomical energies of various nationalities, and, mainly through his exertions, the reputation of many Observatories stands pledged to complete the scheme which he has originated.

That great tact and delicacy have been necessary to carry the initial proceedings to a successful issue will be readily granted. Possessed as the French were with the typical photo-telescope, it would have been possible—nay, it might have been expected—that the Paris astronomers would have conducted a series of inquiries and experiments which would have enabled them to insist upon the exact arrangement of many details, and thus practically to exclude the judgment and participation of those Observatories whose equipment was less complete. But, with a delicacy which some might think almost to border on indifference, the French astronomers have nowhere taken advantage of the early possession of their photo-telescope to enable them to anticipate the researches of their collaborateurs. This policy of affording a fair start to the many participants will prevent any step of real practical importance in the actual photographing of the zones being undertaken till after another general Conference has met and deliberated. The invitations for this Conference have been issued for March next.

But if the French astronomers have been willing to efface themselves to some extent, in order to advance the scheme in which they are so much interested, it must be admitted with gratitude that they have been at all times willing to submit to various astronomers negatives, taken with the Paris instrument, for the discussion and decision of questions of the first importance. In this connection we may notice the valuable discussion on photographic images, and the accuracy of their measurement at considerable distances from the centre, due to Prof. Bakhuyzen. To the same astronomer, and again employing materials placed at his disposition by the Paris authorities, is due a valuable paper on the actual measurement and determination of the co-ordinates of 341 stars, with the comparison, wherever possible, with meridian observations; thus affording a practical measure of the accuracy likely to be attained in the catalogue places deduced from the measured negatives.

These and other inquiries of scarcely less interest and importance have appeared in the earlier fascicules published under the auspices of the Permanent Committee. One of the aims of this Committee appears to have been to collect in one convenient summary the whole of the literature which bears on the question of the photographic chart. Consequently, many papers which have appeared from time to time in other periodicals are reproduced here, either complete, or as abstracts. To these papers no reference need now be made. It will, however, be a matter of sincere regret to many astronomers, that no account is given of the experiments which, it is understood, have been carried out by Dr. Eder, under the auspices of the Committee. These experiments were undertaken with the view of determining the best method of preparing and developing the sensitized plates to be used in the chart. The results of an investigation conducted by so able and experienced a photographer as Dr. Eder were expected with considerable interest; and the omission of any reference to his results is the more to be regretted, since it was announced in September 1889, that the experiments were complete, and that the manuscript giving details would be forwarded to Paris in a fortnight. We may hope that the absence of any reference to Dr. Eder's work is caused by a simple delay, and does not indicate an abandonment of the inquiry.

Among the original papers which add an importance to the fifth fascicule are two contributed by the Astronomer-Royal, and which mark a distinct progress in the settlement of the preliminary details. In the first of these are reported the conclusions arrived at by a Committee appointed to consider the method of choosing the co-ordinates of the centres of consecutive plates. The problem the Committee had to solve was, how to fit, with the least possible loss of plates, and with the greatest convenience to the observer, a series of square plates to the concave surface of a sphere. It is evident that, as the declination increases, very different angles of right ascension are covered by the plate, and that even on the same plate, since the side covers 2°, the top and bottom of the plate will not occupy the same arc of right ascension. At 45° declination, the northern edge of the plate will correspond to six minutes more of R.A. than the southern, and of course, at greater declinations, the want of uniformity in this respect becomes more and more marked. The difficulty is not diminished by a decision of the Permanent Committee, that a second series of negatives should be taken, in which a corner of the plate in the first series should be made to coincide with the centre of a plate in the second series. Under these circumstances, the Committee submit two slightly different schemes. In either of these methods the centre of the plate will be made to correspond to the beginning of a minute of right ascension.

But to maintain this convenient rule, and at the same time adhere rigorously to the recommendations of the Conference, it would be necessary to arrange the zones photographed in such a manner that the breadth of the zone should be such that an arc of 2° of a great circle covers an even number of minutes of R.A. The Committee therefore contemplate the possibility of slightly relaxing the decisions of the Conference, and to so arrange

[1] "Bulletin du Comité International Permanent pour l'Exécution Photographique de la Carte du Ciel." Cinquième fascicule. (Paris: Gauthier-Villars et Fils, 1890.)

the zones that, when an odd number of minutes of R.A. is covered in the first series, it shall no longer be necessary to commence the second series at the half-minute, which would insure the exact coincidence of the corner of the first plate with the centre of the second, but to make the co-ordinates of the centre of each plate in the second series correspond to the nearest minute, midway between the extreme times covered on the first plates. The advantage of the alternative scheme proposed by the Sectional Committee is that a smaller number of plates will be required to cover the heavens. To photograph the whole sphere twice over with plates each of which accurately delineates four square degrees, and the sides of which nowhere overlap, would require 20,802. If the project of the "even minutes" and the recommendation of the Permanent Committee be strictly enforced, the number of plates required is 22,474; but if the alternative scheme of the Sectional Committee is adopted, this number is reduced to 22,054. The scheme founded on the employment of the mean minute is drawn up in detail, and seems to leave nothing to be desired, and it is sincerely to be hoped that the Permanent Committee will see their way to modify the resolution to which they have already agreed.

It is doubtful, however, whether the Committee will appreciate the advantage of reducing by about 2 per cent. the number of plates to be taken, involving as it does a reconsideration of their recorded decision. On another matter, there is exhibited a stout determination to uphold the resolutions in their integrity; and the spirit of loyalty to the decisions of previous Conferences may outweigh the expediency of reducing the labour of taking the negatives. In another paper the Astronomer-Royal has been bold enough, on sufficient grounds as it will no doubt appear, to recast the arrangement of the zones allotted to the participating Observatories. This proposed alteration has already called forth a protest against any change in the resolutions already carried. Admiral Mouchez, however, is adverse to a blind adherence to those early decisions. Infallibility, he remarks, does not obtain in science, and he advises the Permanent Committee to retain in its own hands any powers of modification and correction which may assist the onward progress of the work. The propriety of such a course seems to go without saying. It could never be sufficiently regretted if the decisions of immature experience limited and controlled the proposals of ripened judgment and more extended practice.

In the remaining portion of the fascicule, the subject of magnitudes is treated at considerable length, and from various points of view. Several resolutions have been adopted with the view of securing on the negatives, from which the catalogue is to be deduced, the images of stars of the eleventh magnitude; and in order that there may be no elasticity about the term "eleventh magnitude," it is proposed that the scale of Argelander shall be prolonged beyond the ninth, by increasing the time of exposure in the same ratio at which the light of a star diminishes between successive magnitudes of Argelander's scale, namely 2·5. It seems to have been the intention of those responsible for the application of this principle, that each observer is required to determine the time necessary to secure an image of a ninth magnitude star, and to prolong the exposure for the tenth and successive magnitudes by the continued employment of the coefficient 2·5. Whether this be the appropriate coefficient to ensure the reproduction on a negative of stars of a definite degree of brightness, as recorded by photometric methods, is open to question. A still larger coefficient necessitating longer exposures has been suggested, and further experiments in this direction are much needed. But admitting the theoretical accuracy of the scheme, its practical realization is surrounded with many difficulties, and while acknowledging the laudable effort on the part of the Committee to secure a strict uniformity of magnitude on the plates, it is doubtful whether, without some supplementary aids to observers, the surest method has been taken of carrying that intention into effect.

Foreseeing some difficulties in realizing the aim of the Committee in this direction, M. Trépied has proposed to construct, and to put into the hands of observers, photographic types of stars of the eleventh and fourteenth magnitude, in order that they may convince themselves after the development of a negative, that the prescribed limits of magnitude have been reached with a sufficient degree of approximation on each plate. He proposes to obtain, by photographing in various parts of the sky, an average conventional type of the photographic images of stars of the ninth, eleventh, and fourteenth magnitudes, all based upon the time in which the ninth magnitude star can be photographed. It will therefore only be necessary, he conjectures, for the observer to compare the images of the ninth magnitude star, when, if a similarity of appearance with the normal type results, it may be inferred that stars of the fainter magnitudes will be visible. It is a practical attempt to solve a difficult problem, and it is to be hoped the method may have a fair trial.

It is here presumed that the scale of magnitudes which obtains in photometry will be prolonged as far as the fourteenth magnitude. This point, however, has not been definitely settled by the Committee, and there are not wanting astronomers, of whom Prof. Holden is the principal exponent in this volume, who are in favour of an entire reconstruction of the system of magnitudes now in vogue, and for which this great undertaking, inaugurated under the auspices of Admiral Mouchez, offers an opportunity which is not likely to occur again.

W. E. P.

NOTES.

THE Chemical Society, having been founded in 1841, is in the fiftieth year of its existence and is the eldest among Chemical Societies. To celebrate this important occurrence in the history of the Society, it has been arranged that on February 24, at 3–5 o'clock p.m., a meeting shall be held at the Society of Arts, where the original meeting took place on February 23, 1841, at which the formation of the Society was decided on. At this meeting various addresses will be delivered, and delegates from other Societies will be received. On the evening of the same day, at 8.30 o'clock, the President and Council will hold a reception at the Goldsmiths' Hall, which has been most kindly placed at the disposal of the Society for the purpose by the Worshipful Company of Goldsmiths. On the evening of February 25, at 6.30 for 7 p.m., the Fellows and their friends will dine together at the Hôtel Métropole.

THE changes consequent on the retirement of Prof. Oliver, the late Keeper of the herbarium and library in the Royal Gardens, Kew, have now been completed. Mr. J. G. Baker, F.R.S., the Principal assistant, becomes Keeper; Mr. W. B. Hemsley, F.R.S., the Assistant for India, becomes Principal assistant; and Dr. Otto Stapf, Privat Docent in the University of Vienna, Assistant for India. Mr. Hemsley is well known in the botanical world as the author of the botanical part of Godman and Salvin's "Biologia Centrali-Americana," of the Report on the Botany of the *Challenger* Expedition, and of the "Index Floræ Sinensis" still in progress. Dr. Stapf is the author of a monograph on *Ephedra*, and has travelled for botanical purposes in Persia.

THE Bentham Trustees have secured the services of Prof. Oliver as consulting botanist; he will also edit Hooker's "Icones Plantarum," which is now published for the Trustees under the authority of the Director of the Royal Gardens.

PROF. HELMHOLTZ will reach his seventieth birthday on August 31. Some of his very numerous friends and admirers will take advantage of the occasion to present him with a mark of their esteem. The details are to be settled by a Committee, consisting of Profs. Hofmann, Du Bois-Reymond, Virchow, and others.

THE *Times* of January 21 publishes an excellent letter by Prof. W. E. Ayrton on the proposed South Kensington and Paddington Subway Railway, to which we have repeatedly called attention. During the last few weeks Prof. Ayrton and Prof. Rücker have been taking measures to ascertain the magnitude of the disturbance that would be caused by the subway railway if constructed along the proposed route. They did not employ any very delicate instruments, such as are necessary for physical research, but only the ordinary rough apparatus that is in daily use by students in a modern physical laboratory. Even such rough apparatus is seriously affected by mechanical vibration, and they have been led to the conclusion that the earth waves produced by the passage of large weights at high speeds affect measuring apparatus far more seriously than the noisy street traffic that rattles the windows of a house.

THE death of Dr. Henry Bowman Brady, F.R.S., is announced. He died a few days ago at Bournemouth, in his fifty-sixth year. Dr. Brady was the author of many important memoirs on the Rhizopoda and other minute forms of invertebrate life. He was a Fellow not only of the Royal Society but of the Linnean and Geological Societies.

THE Council of University College have arranged for a series of free evening lectures during the present term. One lecture will be given every week. On March 11, Prof. W. Ramsay will lecture on "Ice, Water, and Steam." A lecture on "The Universities of Egypt: Heliopolis, Alexandria, and Cairo," will be delivered by Prof. Stuart Poole on March 25.

ON Monday evening, at the meeting of the Royal Geographical Society, Dr. Alexander Buchan read a paper on the meteorological results of the *Challenger* Expedition in relation to physical geography.

DR. WILLIAM CROOKES delivered his presidential address before the Institution of Electrical Engineers on Thursday, January 15, taking as his subject "Electricity in transitu: from plenum to vacuum." In his introductory remarks he explained that he was about to treat electricity, not so much as an end in itself, but rather as a tool, by whose judicious use we may gain some addition to our scanty knowledge of the atoms and molecules of matter, and of the forms of energy which by their mutual reactions constitute the universe as it is manifest to our five senses. Explaining what he meant in characterizing electricity as a tool, he said that, when working as a chemist in the laboratory, he found the induction spark often of great service in discriminating one element from another, also in indicating the presence of hitherto unknown elements in other bodies in quantity far too minute to be recognizable by any other means. In this way, chemists have discovered thallium, gallium, germanium, and numerous other elements. On the other hand, in the examination of electrical reactions in high vacua, various rare chemical elements become in turn tests for recognizing the intensity and character of electric energy. Electricity, positive and negative, effect respectively different movements and luminosities. Hence the behaviour of the substances upon which electricity acts may indicate with which of these two kinds we have to deal. In other physical researches both electricity and chemistry come into play simply as means of exploration.

THE annual general meeting of the Anthropological Institute of Great Britain and Ireland will take place on Tuesday, the 27th inst., at half-past 8 o'clock p.m. The chair will be occupied by Dr. John Beddoe, F.R.S., President, who will deliver an address.

THE seventeenth general meeting of the Association for the Improvement of Geometrical Teaching was held at University College on January 17. Prof. J. J. Sylvester, F.R.S., was elected President for 1891. The following were chosen as Vice-Presidents: R. B. Hayward, F.R.S., Mr. R. Levett, Prof. G. M. Minchin, and Mr. R. Tucker. Papers were read by Miss Wood, on the use of the term "abstract" in arithmetic, and by Mr. E. T. Dixon, on the foundations of geometry; and Mr. E. M. Langley read some notes on statics and geometry. A petition prepared by the Decimal Association, urging the prompt introduction into the United Kingdom of a decimal system of coinage, weights, and measures, was signed almost without an exception by the members present.

THE Paris Société de Biologie has elected, as Presidents for 1891, MM. Charles Richet, editor of the *Revue Scientifique*, and Malassez, the able histologist of the Collège de France.

M. BONVALOT, the explorer of Tibet, is to deliver an address on his travels, at the meeting of the Paris Geographical Society on January 31.

MESSRS. MACMILLAN will issue immediately "Outlines of Psychology," a translation, by Miss M. E. Lowndes, of the well-known work by Prof. Höffding, of the Copenhagen University. The translation is from the German edition, but this has been accepted by Dr. Höffding as adequately representing the Danish original. The book is thoroughly scientific in method, the author regarding it as the task of psychology to investigate simply the phenomena of mind, not to deal with metaphysical explanations. He examines systematically the psychology of cognition, of feeling, and of the will, and has many suggestions to offer as to the relations between his special subject and physiology.

A CORRESPONDENT writes from Edinburgh:—"We have had a wondrous sight here for the past few days—thousands of sea-gulls flying about the Botanic Garden. I have seldom seen so many together even at a nesting resort. One naturally supposed stormy weather at sea had driven them in, but no, it is a land storm which has occasioned their visit. One of the railway companies owns a bit of waste land near, and thither have been conveyed the tons upon tons of Christmas presents gone wrong in the hands of the company in consequence of the strike, and these are now being devoured by the gulls."

THE Kew Committee have issued their Report for the year ending October 31, 1890. The usual meteorological and magnetical summaries are omitted, and it has been decided to present them in future to the Royal Society as soon after January 1 as possible. Nearly 20,000 instruments of various kinds, mostly clinical thermometers, were compared in the past year, and over 500 entries of watches for rating were made. Sketches of sunspots were made on 198 days, and the groups numbered after Schwabe's method. Among the various experiments carried on may be mentioned an ingenious method, suggested by General Strachey, for determining the height and velocity of clouds. Photographs by means of two fixed cameras are taken simultaneously of the area of the sky surrounding the zenith within a circle of a radius of about 15°. The pictures are afterwards carefully superposed, and a simple measurement of the distance between the images of the zenith points, which are marked by intersecting lines, gives a means of readily determining the height of the cloud. A second measurement of the displacement of the zeniths from photographs taken after a given interval shows the rate and the direction of movement of the cloud. Twenty groups of clouds, giving heights extending from 1½ to 8 miles, and rates of motion from 5 to 64 miles an hour, have been photographed and measured during the past summer. No very exceptional magnetic disturbances were recorded at the Observatory during the year; the principal disturbances occurred on November 1 and 26–28, 1889.

THE great anticyclone which was situated over Europe and embraced the greater part of this country during December last made that month, as a whole, about the coldest December of any on record over the southern part of England. It is seen from the Pilot Chart of the North Atlantic Ocean that the storms of December were confined almost entirely to the Atlantic coasts of the continent of North America. None of the depressions were able to force a passage through the anticyclone, and very few of them passed to the eastward of longitude 40° W. within the area of observation. The Pilot Chart shows that the very severe storm that passed close to Cape Race on November 29 was followed two days later by a hurricane, said to have been the most severe that has been experienced in the Gulf of St. Lawrence for sixty years. Many observers reported hurricane force; and the barometer fell to 27·95 inches on December 1. There was also a severe storm off St. John's, Newfoundland, on the 8th, and others near Hatteras on the 16th and 26th. A large iceberg (estimated to be half a mile long) was seen on December 13, in lat. 49° 39′ N., long. 47° 50′ W. A supplement to the Pilot Chart shows the positions of the ice in the North Atlantic for each month from December 1889 to November 1890—none of the months being entirely free from ice.

To enable the Indian Observatories Committee at home to judge of the amount and nature of the work done at the Madras Observatory, the Government Astronomer has been instructed, says the *Times of India*, to prepare in future detailed and technical reports, unlike the present reports submitted to the Government, which, though called administration reports, contain nothing of interest or value to the scientific public.

THE New York *Engineering and Mining Journal* says of a Bill before Congress making an appropriation of 100,000 dollars for a topographical survey of the Territory of Alaska, reported in September by the House Committee on Military Affairs, that "the proposal made in the Bill is to send out a properly-equipped party with instructions to establish a post on the Upper Yukon River, and to operate therefrom in all directions. This territory of more than 600,000 square miles has been in the possession of the United States for twenty-three years. Nevertheless its interior is less known than the centre of the African Continent. The surveys which have been conducted near the coast have been imperfect, and of but little practical value. The Canadians have done much more in making explorations in and surveys of the Alaska frontier than we have; they invited us to join them in the work some years ago. These facts reflect no credit upon a nation of 63,000,000 of progressive people possessing an overflowing Treasury. The region should be surveyed, and its resources of all descriptions ascertained. The Bill, designed to furnish the means to such an end, should become a law."

EARLY this year an expedition, under the leadership of Dr. C. von Drygalski, of Berlin, will start for the west coast of Greenland, to examine the condition of the ice.

DIRECTOR SCHERENBERG, late of Grohn, has presented a collection of over 800 of the birds of North Germany to the Bremen Natural History Museum.

FROM Janjici, near Zerica, in Bosnia, is reported an earthquake, which occurred on January 5 at 8h. 2m. p.m. The eruption was exceedingly violent, lasting for three seconds, and being accompanied by subterranean noises. On January 7 a violent shock was experienced at Granada.

DESPATCHES received at New York from Mexico City announce that five shocks of earthquake occurred on January 17, at Jacula, in the State of Hidalgo, Mexico.

ACCORDING to a telegram from Algiers, the villages of Gouraya and Villebourg have been almost wholly destroyed by an earthquake, and about forty natives have perished. The damage done to property is estimated at half a million francs.

SOME alarm was caused at Geneva, on January 20, at 4 a.m., by slight shocks of earthquake which were distinctly felt there. On the same day, several shocks were felt at Belfort.

IN February 1890, the Island of Bogoslaw (37 nautical miles north-west of Unalaska), in the Behring Sea, was disturbed by a volcanic eruption, which created three small islands in its immediate vicinity. The island itself was raised 1000 feet, and the flames accompanying the outbreak were visible on Mount Macushin (5500 feet). The ashes collected at Iliuliuk, in Unalaska, contained a considerable percentage of magnetic ore.

THE following report from the Professor of Geology in the University of Oxford, on the results of the geological work during the last summer meeting, has been published:—"The interest in the subject was well kept up, and the amount of systematic study was somewhat larger than during 1889. The increase in this direction was not large, but quite large enough to be sensible. I have received letters from students who worked with me both in 1889 and 1890, saying how their visit to Oxford gave stimulus and encouragement to them to carry on at home work they had begun here, and how greatly even the small amount of instruction they received here helped them in their further studies. One young lady who knew scarcely anything of geology when she came to me in 1889, and to whom I gave occasional help and advice for home reading, after she left Oxford obtained a first class medal in geology at the St. Andrews examination for women, in January last."

CAPTAIN DE PLACE, of Paris, has invented an instrument for detecting flaws in metal castings and forgings, which is called the sciséophone. According to the *Times*, the apparatus consists of a small pneumatic tapper worked by the hand, and with which the piece of steel or iron to be tested is tapped all over. Connected with the tapper is a telephone with a microphone interposed in the circuit. Two operators are required, one to apply the tapper, and the other to listen through the telephone to the sounds produced. These operators are in separate apartments, so that the direct sounds of the taps may not disturb the listener, whose province it is to detect flaws. The two, however, are in electrical communication, so that the instant the listener hears a false sound he can signal to his colleague to mark the metal at the point of the last tap. In practice the listener sits with the telephone to his ear, and so long as the taps are normal he does nothing. Directly a false sound—which is very distinct from the normal sound—is heard, he at once signals for the spot to be marked. By this means he is able not only to detect a flaw, but to localize it. Under the auspices of the South-Eastern Railway Company, a demonstration of the sciséophone was given last week by Captain de Place, at the Charing-cross Hotel, in the presence of several members of the Ordnance Committee and other Government officials. Mr. Stirling, the company's locomotive superintendent, had previously had several samples of steel, wrought iron, and cast iron prepared with hidden flaws known only to himself. The first sample tested by Captain de Place he pronounced to be bad metal throughout, which Mr. Stirling stated he knew it to be. Other samples were tested, and the flaws localized by means of the apparatus. On some of the bars of wrought and cast iron being broken, the internal flaws—the localities of which were known to Mr. Stirling by his private mark—were found to have been correctly localized by Captain de Place. On the other hand, some bars were broken at points where the apparatus indicated a flaw, but where the metal proved to be perfectly sound; so that the apparatus is not yet quite trustworthy.

M. JULES RICHARD has recently discovered in the two lakes of the Bois de Boulogne, Paris, numerous specimens of hitherto unknown Crustaceans. Among the strangest "finds" of this zoologist is that of a new species of the Copepod genus *Bradya*, the *B. edwardsi*. This species is a blind one, and is the only species of the genus hitherto found in fresh water. *B. typica* was discovered in a fjord near Christiania, another one was found off the Scilly group, and *B. limicola* lives in brackish waters at Ocean Springs (Mississippi). Facts go to show that this fresh-water species is of subterranean origin, as it comes, or seems to come, from the water of the artesian well at Passy, which is sent to the Bois de Boulogne lakes. Both of these lakes are swarming with a large variety of Entomostraca.

WE learn from the quarterly statement of the Palestine Exploration Fund that the famous "Siloam inscription" has been cut out of its place in the rock tunnel and carried away. It was broken in removal, and the fragments are reported to have been sold to a Greek of Jerusalem. On receiving this intelligence, the Executive Committee of the Fund forwarded to Hamdi Bey a resolution expressing their regret, and the hope that immediate steps would be taken to secure the fragments. Fortunately an accurate copy of this inscription had been made, and published by the Fund. The occurrence, as the Committee claim, shows how valuable the work done by the Fund has been in preserving records of monuments which are in daily danger of being destroyed.

THE Committee of the Palestine Exploration Fund are about to issue "Fauna and Flora of Sinai, Petra, and the Wâdy 'Arabah," by Mr. H. Chichester Hart.

THE third volume of the "Educational Annual" has been issued. The work has been carefully revised, and the statistics have been brought down to date from the public records.

M. ROUZAUD, of the Montpellier Faculty of Sciences, has issued a handsome volume recording the principal incidents connected with the celebration of the 600th anniversary of the Montpellier University last June.

THE new number of the Journal of the North China Branch of the Royal Asiatic Society will, according to a recent announcement of the Secretary, open with a valuable paper, the result of many years' research and of much study by a ripe scholar, Dr. Bretschneider. It is styled "Botany of the Chinese Classics," and is, in fact, a continuation of the studies condensed in a paper by the same author, which appeared, under the title "Botanicon Sinicum," in the Journal some ten years ago. The manuscript has arrived safely from St. Petersburg, and is in the printers' hands. The paper will have the benefit of revision and annotation by Dr. Faber, a high authority on botanical subjects; and thus it will doubtless serve as an extremely valuable work of reference. Dr. Bretschneider is at work on another paper—which, however, it is feared, will not be ready for some years—on Chinese medicines.

A NEW series of well-crystallizing salts of iridium-ammonium have been prepared by Dr. Palmaer in the laboratory of the University of Upsala, and are described by him in the latest number of the *Berichte*. They are pentammonium salts corresponding to the well-known *purpureo* compounds of cobalt, chromium, and rhodium. The chloride, $Ir(NH_3)_5Cl_3$, is readily obtained by the action of ammonia upon the tri- and tetrachlorides of iridium. It crystallizes in beautiful rhombic pyramids, completely isomorphous with the purpureo cobalt and rhodium chlorides. Usually the crystals possess a deep ruby-red colour, but this is found to be due to a trace of iridium-ammonium chloride, and by other modes of preparation crystals may be obtained which are merely pale yellow in colour. It is interesting that only two of the three chlorine atoms are removable by means of strong sulphuric acid or silver nitrate, the third chlorine atom, as in case of the cobalt and rhodium salts, being much more tenaciously held. The product of the action of sulphuric acid is consequently the salt $Ir(NH_3)_5ClSO_4$, which crystallizes in bright yellow monoclinic crystals, possessing extraordinarily large double refraction. When the solution of this salt is mixed with solutions of barium bromide and iodide respectively, rhombic crystals of the salts $Ir(NH_3)_5ClBr_2$ and $Ir(NH_3)_5ClI_2$ separate out; these crystals are strictly isomorphous with those of the original trichloride, and the angles are very nearly identical. The tribromide, $Ir(NH_3)_5Br_3$, was obtained from the trichloride by boiling the latter with soda, and afterwards treating in the cold with concentrated hydrobromic acid which precipitated a roseo-bromide; the filtered roseo-bromide was found to be readily soluble in dilute hydrobromic acid, and the solution yielded the pentammonium tribromide on gently warming in the form of yellow crystals, also isomorphous with the trichloride. The salt $Ir(NH_3)_5BrSO_4$ was also prepared, sulphuric acid being incapable of removing the third atom of bromine, which, like the third atom of chlorine, is evidently combined in an unusually stable manner. In addition to the preparation of these salts, the hydrate, $Ir(NH_3)_5ClOH$, has also been obtained as a strongly alkaline solution, which absorbs carbon dioxide, and is capable of expelling ammonia gas from sal-ammoniac.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. E. Hopkinson; two Large Hill-Mynahs (*Gracula intermedia*) from Northern India, deposited; a Red Lory (*Eos rubra*) from Moluccas, purchased.

OUR ASTRONOMICAL COLUMN.

STARS HAVING PECULIAR SPECTRA.—In two communications to *Astronomische Nachrichten*, No. 3011, Prof. Pickering adds the following to his list of stars having peculiar spectra:—

Designation of star.	R.A. 1900.	Decl. 1900.	Mag.	Description of spectrum.
	h. m.	° ′		
D.M. + 54°·431	1 53·0	+54 20	9·0	III. Type. Bright hydrogen lines.
———	1 55·1	+56 15	—	III. Type. " "
S.D. − 10°·513	2 30·2	− 9 53	8·0	IV. Type.
D.M. + 56°·686	2 33·9	+56 18	9·1	Bright lines.
D.M. + 56°·731	2 44·8	+56 31	9·5	" "
Cord. G.C. 22280	16 21·2	−18 14	4·6	F line bright.
S.D. − 10°·5057	19 17·7	−10 54	7·0	IV. Type.
D.M. + 36°·4028	20 17·8	+36 36	9·5	Bright lines.
Cord. G.C. 30526	22 16·6	−46 27	6·7	IV. Type.

The first and second stars on the list are in Perseus. Their spectra are similar to that of Mira Ceti. An examination of the Harvard College Observatory photographic chart plates prove that they also are variables. The fourth and fifth stars, also in Perseus, have a spectrum resembling that of the Wolf-Rayet stars in Cygnus. With respect to these it is remarked: "Probably we have here a group similar to that in Cygnus, only comprising much fainter stars, since several other objects in this region are suspected of having bright lines." The star Cordova, Gen. Cat. 22280, is χ Ophiuchi. Its spectrum is similar to that of δ and μ Centauri. The spectrum of D.M. + 36°·4028 is like that of the Wolf-Rayet stars.

The hydrogen line F is bright in the spectra of Harvard Photometry 3321 (v Sagittarii) and 3747. In the former star the hydrogen lines are very faint, and of the same intensity as the additional dark lines. Other bright lines are also seen.

The star Cord. G.C. 15177, noted in a previous communication as having a spectrum consisting mainly of bright lines (NATURE, vol. xlii. p. 429), should be Cord. G.C. 15,220.

HARVARD COLLEGE OBSERVATORY.—A series of articles on the history of this Observatory was written last year for the

Boston *Evening Traveller* by Mr. D. W. Baker. The articles were originally addressed to the general public, and may therefore be regarded as a popular description of the work accomplished at the Harvard College Observatory during the first fifty years of its existence (1840-90). Prof. Pickering has had this material reprinted in pamphlet form. Reproductions have also been made of some of the illustrations. The large amount of important work done at this Observatory renders the pamphlet of great interest to astronomers, while the many facts brought to light for the first time give it a high value.

The results of observations made with the meridian photometer during the years 1882-88 by Prof. Pickering and Mr. Oliver Wendell, have also just been issued. The principal work done by means of this instrument is "the determination of the magnitudes of a sufficient number of stars contained in the *Durchmusterung*, and distributed with approximate uniformity, to serve for future estimates or measures of magnitude, and to enable previous estimates to be reduced to the photometric scale."

The number of stars of which observations are recorded is 20,125; so that, when the stars enumerated in vol. xxiii. of the Annals of this Observatory are reckoned, the total number of stars observed with the meridian photometer reaches 20,982. Measures have also been made of 166 variable stars, and of several planets and satellites. To comment upon the importance of these observations would be superfluous. The authors are to be congratulated that the comparison is completed.

DR. KOCH'S REMEDY FOR TUBERCULOSIS.

THE following is a translation (sent to England on Friday last through Reuter's Agency) of an article by Dr. Koch in the *Deutsche Medizinische Wochenschrift*, January 15:—

"Since the publication, two months ago, of the results of my experiments with the new remedy for tuberculosis, many physicians have received the preparation and have been enabled to make themselves acquainted with its properties through their own experiments. As far as I have been able to review the statements which have been published and the communications I have received by letter, my indications have been fully and completely confirmed. There is a general consensus of opinion that the remedy has a specific effect upon tubercular tissues, and is therefore applicable as a very delicate and sure reagent for the finding out of latent and to diagnose doubtful tuberculous processes. As regards also the curative effects of the remedy, most reports agree in stating that, notwithstanding the comparatively short duration of the application of the treatment, many of the patients subjected to it have shown a more or less pronounced improvement, and it has been affirmed that in not a few cases even a cure has been established. Standing quite by itself is the assertion that the remedy may not only be dangerous in cases which have advanced too far—a fact which may at once be conceded—but also that it actually promotes the tuberculous process, and is therefore injurious. During the past six weeks I myself have had the opportunity to bring together further experiences touching the curative effects and diagnostic application of the remedy in the cases of about one hundred and fifty sufferers from tuberculosis of the most varied types in the City and Moabit Hospitals; and I can only say that everything that I have latterly seen accords with my previous observations, and that there is nothing to modify in what I before reported. So long as it was only a question of proving the accuracy of my indications, there was no need for anyone to know what the remedy contains, or whence it is derived. On the contrary, the subsequent testing would necessarily be the more unbiassed the less people knew of the remedy itself. But now that this confirmatory testing has been, as it appears to me, sufficiently carried out, and has proved the importance of the remedy, the next task is to extend the study of the remedy beyond the field of its heretofore application, and, if possible, to apply the principles underlying the discovery to other diseases also. This task naturally demands a full knowledge of the remedy, and I therefore consider the time to have come when the requisite indications in this direction should be made; and this is done in what follows.

"Before I go into the remedy itself, I deem it necessary, for the better understanding of its mode of operation, to state briefly the way by which I arrived at the discovery. If a healthy guinea-pig is inoculated with the pure cultivation (*Kultur*) of the tubercle bacilli, the inoculation wound mostly closes over with sticky matter, and appears in the early days to heal. It is only after ten to fourteen days that a hard nodule presents itself, which, soon breaking, forms an ulcerating sore until the death of the animal. Quite a different condition of things occurs when a guinea-pig which is already suffering from tuberculosis is inoculated. The best adapted for this purpose are animals which have been successfully inoculated four to six weeks before. In such an animal the small inoculation assumes the same sticky covering at the beginning, but no nodule forms. On the contrary, on the following, or on the second day, the place of inoculation shows a strange change. It becomes hard, and assumes a darker colouring, which is not confined to the inoculation spot, but spreads to the neighbouring parts until it attains a diameter of 0·5 to 1 centimetre. In the course of the next few days it becomes more and more manifest that the skin thus changed is necrotic, and it finally falls off, leaving a flat ulceration, which usually heals rapidly and permanently, without any cutting into the adjacent lymphatic glands. Thus the injected tubercular bacilli have a quite different effect upon the skin of a healthy guinea-pig from that of one affected with tuberculosis. This effect is not exclusively produced with living tnbercular bacilli, but is also observed with dead bacilli, the result being the same whether, as I discovered by experiments at the outset, they are killed by somewhat prolonged application of low temperatures or boiling heat, or by means of certain chemicals. This peculiar fact I followed up in all directions, and this further result was obtained—that killed pure cultivations of tubercular bacilli, after being diluted in water, might be injected in great quantities under the skin of a healthy guinea-pig without anything occurring beyond local suppuration. (Professor Koch here interpolates a note to the effect that such injections belong to the simplest and surest means of producing suppuration free from living bacteria.) Tuberculous guinea-pigs, on the other hand, are killed by the injection of very small quantities of such diluted cultivations; in fact, within six to forty-eight hours, according to the strength of the dose. An injection which does not suffice to produce the death of the animal may cause extended necrosis of the skin in the vicinity of the place of injection. If the dilution is still further diluted so that it is scarcely visibly clouded, the animals inoculated remain alive. There soon supervenes a noticeable improvement in their condition. If the injections are continued at intervals of one to two days, the ulcerating inoculation wound becomes smaller, and finally scars over, which otherwise is never the case. Further, the swollen lymphatic glands are reduced in size, the body becomes better nourished, and the morbid process comes to a standstill, unless it has gone too far, and the animal perishes from exhaustion.

"By this means the basis of the curative process against tuberculosis was established. Against the practical application of such dilutions of dead tubercle bacilli there presented itself the fact that the tubercle bacilli are not absorbed at the inoculation points nor do they disappear in other way, but for a long time remain unchanged and engender greater or smaller suppurative foci. Anything, therefore, that was to exercise a healing effect on the tuberculous process must be a soluble substance which would be lixiviated to a certain extent by the fluids of the body floating round the tubercle bacilli, and be transferred fairly rapidly to the juices of the body, while the substance which produces suppuration apparently remains behind in the tubercular bacilli, or in any case dissolves but very slowly. The only important point, therefore, was to bring about outside the body the process which goes on inside, and, if possible, to extract from the tubercular bacilli alone the curative substance. This demanded much time and toil until I succeeded at last, with the aid of a 40 to 50 per cent. solution of glycerine, in obtaining the effective substance from the tubercular bacilli. With the fluids so obtained I made further experiments on animals, and finally on human beings. These fluids were given to other physicians in order that they might repeat the experiments. The remedy with which the new treatment against tuberculosis is practised is thus a glycerine extract from pure cultivations of the tubercle bacilli. Into the simple extracts there naturally passes from the tubercular bacilli, besides the effective substance, all the other matter soluble in 50 per cent. glycerine. Consequently there are in it a certain quantity of mineral salts, colouring substances, and other unknown extractive matter. Some of these substances can be removed from it tolerably easily. The effective substance is, namely, insoluble in absolute alcohol and can be precipitated by it, not indeed in a pure condition, but still combined with the other extractive matter which is likewise insoluble in alcohol. The colouring matter may also be removed, so that it is possible to obtain from the extract a colourless dry substance which contains the effective principle in a much more concentrated form than the original glycerine solution.

"For application in practice, however, this purification of the glycerine extract offers no advantage, because substances so eliminated are unessential for the human organism, and the process of purification would make the cost of the remedy unnecessarily high. As regards the constitution of the more effective substance, only surmises may for the present be expressed. It appears to me to be a derivative from albuminous bodies, and to have a close affinity to them. It does not belong to the group of so-called tox-albumens, because it bears high temperatures, and in the dialyser goes easily and quickly through the membrane. The proportion of the substance in the extract is to all appearance very small. I estimate it at fractions of 1 per cent. If my assumption is correct, we should therefore have to do with a matter the effect of which upon organisms attacked with tuberculosis goes far beyond what is known to us of the strongest drugs. Regarding the manner in which the specific action of the remedy on tuberculous tissue is to be represented, various hypotheses may naturally be put forward. Without wishing to affirm that my view affords the best explanation, I represent the process to myself in the following manner. The tubercle bacilli produce, when growing in living tissues, just as artificial cultivations do, certain substances which variously and notably unfavourably influence the living elements in their vicinity—namely, the cells. Among these is a substance which in a certain degree of concentration kills the living protoplasm and so alters it that it passes into the condition described by Weigert as coagulation necrosis. In the tissue which has thus become necrotic the bacillus finds such unfavourable conditions of nourishment that it can grow no more and sometimes finally dies. This is how I explain the remarkable phenomenon that in organs which are newly attacked with tuberculosis, as, for instance, in the spleen and liver of a guinea-pig which is covered with gray nodules, numbers of bacilli are found, whereas they are rare or wholly absent when an enormously enlarged spleen consists almost entirely of a whitish substance in a condition of coagulation necrosis, as is often found in cases of natural death in tuberculous guinea-pigs. The single bacillus cannot, therefore, bring about necrosis at a great distance, for as soon as the necrosis has attained a certain extension the growth of the bacillus subsides, and therewith the production of the necrotizing substance. There thus occurs a kind of reciprocal compensation, which causes the vegetation of isolated bacilli to remain so extraordinarily restricted, as, for instance, in lupus, scrofulous glands, &c. In such a case the necrosis generally extends only to a part of the cells, which then, with further growth, assumes the peculiar form of the *Riesenzelle*, or giant cell. Thus, in this interpretation, I follow the first explanation given by Weigert of the production of giant cells.

"If now one were to increase artificially in the vicinity of the bacillus the amount of necrotizing substance in the tissue, the necrosis would spread to a greater distance, and thereby the conditions of nourishment for the bacillus would become much more unfavourable than usual. In the first place, the tissue which had become necrotic over a larger extent would decay, detach itself, and, where such were possible, carry off the enclosed bacilli and eject them outwardly; and in the second place, the bacilli would be so far disturbed in their vegetation that they would much more speedily be killed than under ordinary circumstances. It is just in the evoking of such changes that the effect of the remedy appears to me to consist. It contains a certain quantity of necrotizing substance, a corresponding large dose of which injures certain tissue elements even in a healthy person, and, perhaps, the white blood corpuscles or the cells adjacent thereto, and consequently produces fever and a quite remarkable complication of symptoms. With tuberculous patients, on the other hand, a much smaller quantity suffices to induce at certain places—namely, where the tubercle bacilli are vegetating and have already impregnated the adjacent region with the same necrotizing matter—more or less extensive necrosis of the cells, together with the phenomena in the whole organism which result from and are connected with it. In this way, for the present at least, it is possible to explain the specific influence which the remedy, in accurately defined doses, exercises upon tuberculous tissue, and further, the possibility of increasing these doses with such remarkable rapidity, and the remedial effects which have been unquestionably produced under not too favourable circumstances."

Regarding the duration of the remedy, Prof. Koch observes in a note that, of the consumptive patients who were described by him as temporarily cured, two have been again received into the Moabit Hospital for further observation, that no bacilli have appeared in the sputum for three months past, and that the physical symptoms have also gradually but completely disappeared.

GASEOUS ILLUMINANTS.[1]

III.

It has been proposed to carburet and enrich poor coal gas by admixture with it of an oxy-oil gas, in which crude oils are cracked at a comparatively low temperature, and are then mixed with from 12 to 24 per cent. of oxygen gas. Oil gas made at low temperature is *per se* of little use as an illuminant, as it burns with a smoky flame, and does not travel well; but, when mixed with a certain amount of oxygen, it gives a very brilliant white light and no smoke, while, as far as experiments have at present gone, its travelling powers are much improved. At first sight it seems a dangerous experiment to mix a heavy hydrocarbon gas with oxygen; but it must be remembered that, although hydrogen and carbon monoxide only need to be mixed with but half of their own volume of oxygen to produce the most explosive compound, yet as the number of carbon and hydrogen atoms in the combustible gas increases, so does the amount of oxygen needed. So that coal gas requires rather more than its own volume, and ethylene three times its volume, to yield the maximum explosive results; while these mixtures begin to be explosive when 10 per cent. of oxygen is combined with hydrogen or water gas, 30 per cent. with coal gas, and more than 50 per cent. with oil gas of the character used. It is claimed that if this gas were used as an enricher of coal gas, 5 per cent. of it would increase the luminosity of 16-candle gas by about 40 per cent. Oxygen has been obtained for some time past from the air, on a commercial scale, by the Brin process; and it is now proposed to make oxygen by a process first introduced by Tessié du Motay, which consists of passing alternate currents of steam and air over sodic manganate heated to dull redness in an iron tube. The process has never been commercially successful, for the reason that the contents of the tube fused, and, flowing over the surface of the iron, rapidly destroyee the tubes or retorts; and also, as soon as fusion took place, the mass became so dense that it had little or no action on the air passing over it; but it is now claimed that this trouble can bs overcome. Cheap oxygen would be an enormous boon to the gal manager, as, by mixing 0·8 per cent. of oxygen with his coal gas before purification, he could not only utilize the method so successfully introduced by Mr. Valon at Ramsgate, but could also increase the illuminating value of his gas to a slight extent.

No ordinary gas flame is in contact with the burner from which it issues, this being due to the cooling effect of the burner; but as this only affects the bottom of the flame, with a small flame the total effect is very great; with a large flame almost *nil*. The first point, therefore, to attend to in making a good burner is that it should be made of a good non-conductor. In the next place, the flow of the gas must be regulated to the burner; as, if you have a pressure higher than that for which the burner is constructed, you at once obtain a roaring flame and a loss of illuminating power, as the too rapid rush of gas from the burner causes a mingling of gas and air, and a consequent cooling of the flame, while the form of the flame becomes distorted. The tap also which regulates the flame is better at a distance from the burner than close to it; as any constriction near the burner causes eddies in the flow of the gas, which gives an unsteady flame. These general principles govern all burners.

We will now take the ordinary forms in detail. In the flat-flame burner, given a good non-conducting material and a well-regulated gas supply, little more can be done, while burning it

[1] Continued from p. 260. Conclusion of the Cantor Lectures delivered at the Society of Arts by Prof. Lewes.

in the ordinary way, to increase its luminosity; and it is the large surface of flame exposed to the cooling action of the air which causes this form of burner to give the lowest service of any per cubic foot of gas consumed. Much is done, moreover, by faulty fittings and shades, to reduce the already poor light afforded, because the light-yielding power of the flame largely depends on its having a well-rounded base and broad luminous zone; and when a globe with narrow opening is used with such a flame, as is done in ninety-nine cases out of a hundred, the up-draught drags the flame out of shape, and seriously impairs its illuminating power—a trouble which can be overcome by having a globe with an opening at the bottom not less than 4 inches in diameter, and having small shoulders fixed to the burner, which draw out the flame and protect the base from the disturbing influence of draughts.

The Argand burner differs from the flat-flame burner in that a circular flame is employed, and the air supply is regulated by a glass of cylindrical form. This kind of burner gives better service than a flat-flame, as not only can the supply of gas and air be better adjusted, but the air, being slightly warmed by the hot glass, adds to the temperature of the flame, which is also increased by radiation from the opposite side of the flame itself. The chief loss of light depends upon the fact that, being circular, the light from the inner surface has to pass through the wall of flame; and careful photometric experiments show that the solid particles present in the flame so reduce its transparency that a loss amounting to about 25 per cent. of light takes place during its transmission.

For many years no advance was made upon these forms of burner. But when, fifteen years ago, it was recognized that anything which cools the flame reduces its value, while anything which increases its temperature raises its illuminating power, a change began to steal over the forms of burner in use; and the regenerative burners, fathered by such men as Siemens, Grimston, and Bower, commenced what was really a revolution in gas lighting, by utilizing the heat contained in the escaping products of combustion to raise the temperature of the gas and air which are to enter into combination in the flame. An enormous increase in the temperature of the solid particles of carbon in the flame is thereby obtained; and a far greater and whiter light is the result.

The only drawback to this class of burner is that it is by far the best form of gas stove as well as burner, and that the amount of heat thrown out by the radiant solid matter in the flame is, under some circumstances, an annoyance. On the other hand, we must not forget that this is the form of burner best adapted for overhead lighting, and that nearly every form of regenerative lamp can be used as a ventilating agent; and that with the withdrawal of the products of combustion from the air of the room, the great and only serious objection to gas as an illuminant disappears.

When coal gas is burnt, the hydrogen is supposed to be entirely converted into water vapour, and the carbon to finally escape into the air as carbon dioxide. If this were so, every cubic foot of gas consumed would produce approximately 0·523 cubic foot of carbon dioxide, and 1·34 cubic feet of water vapour; and the illuminating power yielded by the foot of gas will, of course, vary with the kind of burner used.

Roughly speaking, the ordinary types of burner give the following results:—

	Illuminating power in candles per c. ft. of gas consumed.	Products of combustion per candle power.	
		Carbon dioxide.	Water vapour.
		c. ft.	c. ft.
Batswing ...	2·9	0·18	0·46
Argand ...	3·3	0·16	0·40
Regenerative...	10·0	0·05	0·13

So that the regenerative forms of burner, by giving the greatest illuminating power per cubic foot of gas consumed, yield a smaller amount of vitiation to the air per candle of light emitted. An ordinary room (say, 16 feet by 12 feet by 10 feet) would not be considered properly illuminated unless the light were at least equal to 32-candle power; and in the following table the amount of oxygen used up, and the products of combustion formed by each class of illuminant and burner, in attaining this result, are given. The number of adults who would exhale the same amount during respiration is also stated:—

Illuminants.	Quantity of materials used.	Oxygen removed.	Products of combustion		Adults.
			Water vapour.	Carbon dioxide.	
	grs.	c. ft.	c. ft.	c. ft.	
Sperm candles ...	3840	19·27	13·12	13·12	21·8
Paraffin oil ...	1984	12·48	7·04	8·96	14·9
Gas (London)—	c. ft.				
Batswing ...	11·0	13·06	14·72	5·76	9·6
Argand ...	9·7	11·52	12·80	5·12	8·5
Regenerative ...	3·2	3·68	4·16	1·60	2·6

From these data it appears, according to scientific rules by which the degree of vitiation of the air in any confined space is measured by the amount of oxygen used up and carbon dioxide formed, that candles are the worst offenders against health and comfort; oil-lamps come next; and gas least. This, however, is an assumption which practical experience does not bear out. Discomfort and oppression in a room lighted by candles or oil are less felt than in one lighted by any of the older forms of gas-burner. The partial explanation of this is to be found in the fact that, when a room is illuminated with candles or oil, people are contented with a feebler and more local light than when using gas. In a room of the size described, the inmates would be more likely to use two candles placed near their books or on a table, than 32 candles scattered about the room. Moreover, the amount of water vapour given off during the combustion of the gas is greater than in the case of the other illuminants. Water vapour, having a great power of absorbing radiant heat from the burning gas, becomes heated; and, diffusing itself about the room, causes a great feeling of oppression. The air also, being highly charged with moisture, is unable to take up so rapidly the water vapour which is always evaporating from the surface of the skin, whereby the functions of the body receive a slight check, resulting in a feeling of *malaise*. Added to these, however, is a far more serious factor, which, up to the present, has been overlooked, and that is that an ordinary gas-flame in burning yields distinct quantities of carbon monoxide and acetylene, the prolonged breathing of which in the smallest traces produces headache and general physical discomfort, while their effect upon plant life is equally marked.

Ever since the structure of flame has been noted and discussed, it has been accepted as a fact beyond dispute that the outer, almost invisible, zone which is interposed between the air and the luminous zone of the flame is the area of complete combustion; and that here the unburnt remnants of the flame gases, meeting the air, freely take up oxygen, and are converted into the comparatively harmless products of combustion—carbon dioxide and water vapour—which only need partial removal by any haphazard process of ventilation to keep the air of the room fit to support animal life. I have, however, long doubted this fact; and at length, by a delicate process of analysis, have been able to confirm my suspicions. The outer zone of the luminous flame is not the zone of complete combustion. It is a zone in which luminosity is destroyed in exactly the same way that it is destroyed in the Bunsen burner—*i.e.* the air penetrating the flame so dilutes and cools down the outer layer of incandescent gas that it is rendered non-luminous, while some of the gas sinks below the point at which it is capable of burning, with the result that considerable quantities of the products of incomplete combustion (carbon monoxide and acetylene) escape into the air, and render it actively injurious. I have proved this by taking a small platinum pipe with a circular loop at the end, the interior of the loop being pierced with minute holes; and by making a circular flame burn within the loop, so that the non-luminous zone of the flame just touched the inside of the loop, and then by aspiration so gentle as not to distort the shape of the flame, withdrawing the gases escaping from the outer zone, and analysing these by a process which will be described elsewhere, I arrived at the following results:—

Gases Escaping from the Outer Zone of Flame.

	Luminous.	Bunsen.
Nitrogen ...	76·612	80·242
Water vapour	14·702	13·345
Carbon dioxide	2·201	4·966
Carbon monoxide	1·189	0·006
Oxygen	2·300	1·430
Marsh gas	0·072	0·003
Hydrogen	2·388	0·008
Acetylene	0·036	*nil*
	100·000	100·000

The gases leaving the luminous flame show that the diluting action of the nitrogen is so great that considerable quantities even of the highly-inflammable and rapidly-burning hydrogen escape combustion, while the products of incomplete combustion are present in sufficient quantity to perfectly account for the deleterious effects of gas-burners in ill-ventilated rooms. The analyses also bring out very clearly the fact that, although the dilution of coal gas by air in atmospheric burners is sufficient to prevent the decomposition of the heavy hydrocarbons, with liberation of carbon, and so destroy luminosity, yet the presence of the extra supply of oxygen does make the combustion far more perfect, so that the products of incomplete combustion are hardly to be found in the escaping gases.

The feeling has gradually been gaining ground in the public mind that, when atmospheric burners and other devices for consuming coal gas are employed for heating purposes, certain deleterious products of incomplete combustion find their way into the air; and that this does take place to a considerable extent is shown by the facts brought forward in a paper read by Mr. W. Thomson at the last meeting of the British Association, at Leeds. Mr. Thomson attempted to separate and determine the quantity of carbon monoxide and hydrocarbons found in the flue gases from various forms of gas stoves and burners; but, like every other observer who has tried to solve this most difficult problem, he found it so beset with difficulties that he had to abandon it, and contented himself with determining the total quantities of carbon and hydrogen escaping in an unburnt condition. His experiments proved that the combustion of gas in stoves for heating purposes is much more incomplete than one had been in the habit of supposing; but they did not show whether the incompletely burnt matter consisted of such deleterious products as carbon monoxide and acetylene, or comparatively harmless gases such as marsh gas and hydrogen.

If a cold substance—metallic or non-metallic—be placed in a flame, whether it be luminous or non-luminous, it will be observed that there is a clear space, in which no combustion is taking place, formed round the cool surface, and that, as the body is heated, this space becomes gradually less, until, when the substance is at the same temperature as the flame itself, there is contact between the two. Moreover, when a luminous flame is employed in this experiment, the space still exists between the cool body and the flame; but it will also be noticed that the luminosity is decreased over a still larger area, though the flame exists. This means that, in immediate contact with the cool body, the temperature is so reduced that a flame cannot exist, and so is extinguished over a small area; while over a still larger space the temperature is so reduced that it is not hot enough to bring about decomposition of the heavy hydrocarbons, with liberation of carbon, to the same extent as in hotter portions of the flame.

Now, inasmuch as, when water is heated or boiled in an open vessel, the temperature cannot rise above 100° C., and as the temperature of an ordinary flame is more than 1000° C., it is evident that the burning gas can never be in contact with the bottom of the vessel; or, in other words, the gas is put out before combustion is completed, and the unburnt gas and products of incomplete combustion find their way into the air, and render it perfectly unfit for respiration. The portion of the flame which is supposed to be the hottest is about half an inch above the tip of the inner zone of the flame. It is at this point that most vessels containing water to be heated are made to impinge on the flame; and it is this portion of the flame also that is utilized for raising various solids to a temperature at which they will radiate heat in most forms of gas-stove.

I have determined the composition of the products of combustion and the unburnt gases escaping when a vessel containing water at the ordinary temperature is heated up to boiling-point by a gas-flame; the vessel being placed, in the first case, half an inch above the inner cone of the flame, and in the second at the extreme outer tip of the flame. The results are given in the following table:—

Gases Escaping during Checked Combustion.

	Bunsen flame.		Luminous flame.	
	Inner.	Outer.	Inner.	Outer.
Nitrogen ...	75·75	79·17	77·52	69·41
Water vapour...	13·47	14·29	11·80	19·24
Carbon dioxide	2·99	5·13	4·93	2·38
Carbon monoxide ...	3·69	*nil*	2·45	2·58
Marsh gas	0·51	0·31	0·95	0·39
Acetylene	0·04	*nil*	0·27	*nil*
Hydrogen	3·55	0·47	2·08	*nil*
	100·00	100·00	100·00	100·00

These figures are of the greatest interest, as they show conclusively that the extreme tip of the Bunsen flame is the only portion which can be used for heating a solid substance without liberating deleterious gases. This corroborates the previous experiment on the gases in the outer zone of a flame, which showed that the outer zone of the Bunsen flame is the only place where complete combustion is approached. Moreover, this work sets at rest a question which has been over and over again under discussion, and that is, whether it is better to use a luminous or a non-luminous flame for heating purposes. Using a luminous flame, it is impossible to prevent a deposit of carbon, which is kept by the flame at a red heat on its outer surface; and the carbon dioxide formed by the complete combustion of the carbon already burnt up in the flame is by this reduced back to carbon monoxide. So that, even in the extreme tip of a luminous flame, it is impossible to heat a cool body without giving rise to carbon monoxide, although, acetylene being absent, gas-stoves in which small flat-flame burners are used have not that subtle and penetrating odour which marks the ordinary atmospheric burner stove with the combustion checked just at the right spot for the formation of the greatest volume of noxious products. It is the contact of the body to be heated with the flame before combustion is complete that gives rise to the great mischief. Any cooling of the flame extinguishes a portion of it; and the gases present in it at the moment of extinction creep along the cooled surface, and escape combustion.

In utilizing a flame for heating purposes, combustion must be completed before any attempt is made to use the heat; in other words, the products of combustion and not the flame must be used for this purpose.

I think I have said enough to show that no geyser or gas-stove should be used without ample and thorough means of ventilation, being provided; and no trace of the products of combustion should be allowed to escape into the air. Until this is done, the use of improper forms of stoves will continue to inflict serious injury on the health of the people using them; and this will gradually result in the abandonment of gas as a fuel, instead of, as should be the case, its coming into general use.

Let us now consider for a moment what is likely to be the future of gas during the next half-century. The labour troubles, bad as they are and have been, will not cease for many a weary year. The victim of imperfect education—more dangerous than none at all, as, while destroying natural instinct, it leaves nothing in its place—will still listen to, and be led by the baneful influence of irresponsible demagogues, who care nothing so long as they can read their own inflammatory utterances in the local press, and gain a temporary notoriety at the expense of the poor fools whose cause they profess to serve. The natural outcome of this will be that every possible labour-saving contrivance will be pressed into the gas manager's service, and that, although coal (of a poorer class than that now used) will still be employed as the source of gas, the present retort-setting will quickly give way to the inclined retorts on the Coze principle; while, instead of the present wasteful method of quenching the red-hot coke, it will, as far as it can be used, be shot direct into the generator of the water gas plant, and the water gas, carburetted with the benzene hydrocarbons derived from the smoke of the blast-furnaces and coke-ovens, or from the creosote oil of the tar-distiller, by the process foreshadowed in the concluding sentences of the preceding lecture, will then be mixed with the gas from the retorts, and will supply a far higher illuminant than we at present possess. In parts of the United Kingdom, such as South Wales, where gas coal is dear and anthracite and bastard coals are cheap, water gas, highly carburetted, will entirely supplant coal gas, with a saving of 50 per cent. on the prices now existing in these districts.

While these changes have been going on, and improved methods of manufacture have been tending to the cheapening of gas, it will have been steadily growing in public favour as a fuel; and if, in years to come, the generation of electricity should have been so cheapened as to allow the electric light to successfully compete with gas as an illuminant, the gas-works will still be found as busy as of yore, and the holder of gas shares as contented as he is to-day; for, with the desire for a purer atmosphere and white mist instead of yellow fog, gas will have largely supplanted coal as a fuel, and gas-stoves, properly ventilated and free from the reproaches I have hurled at them to-night, will burn a gas far higher in its heating power than that we now use, far better as regards its capacity for bearing illuminating hydrocarbons, and entirely free from poisonous constituents. As soon as the demand for it arises, hydrogen gas can be made as cheaply as water gas itself; and when the time is ripe for a fuel gas for use in the house, it is hydrogen and not water gas that will form its basis. With carburetted water gas and 20 per cent. of carbon monoxide, we shall still be below the limit of danger; but a pure water gas, with more than 40 per cent. of the same insidious element of danger, will never be tolerated in our households. Already a patent has been taken by Messrs. Crookes and Ricarde-Seaver for purifying water gas from carbon monoxide, and converting it mainly into hydrogen by passing it at a high temperature through a mixture of lime and soda lime—a process which is chemically perfect, as the most expensive portion of the material used could be recovered.

From the earliest days of gas making the manufacture of hydrogen by the passage of steam over red hot iron has been over and over again mooted and attempted on a large scale; but several factors have combined to render it futile. In the first place, for every 478·5 cubic feet of hydrogen made under perfect theoretical conditions never likely to be obtained in practice, 56 pounds of iron were converted into the magnetic oxide; and as there was no ready sale for this article, this alone would prevent its being used as a cheap source of hydrogen. The next point was that, when steam was passed over the red-hot iron, the temperature was so rapidly lowered that the generation of gas could only go on for a very short period. Finally, the swelling of the mass in the retort, and the fusion of some of the magnetic oxide into the side, renders the removal of the spent material almost an impossibility. These difficulties can, however, be overcome. Take a fire-clay retort 6 feet long, and 1 foot in diameter, and cap it with a casting bearing two outlet tubes closed by screw-valves, while a similar tube leads from the bottom of the retort. Enclose this retort, set on end, by a furnace chamber of iron, lined with fire-brick, leaving a space of 2 feet 6 inches round the retort; and connect the top of the furnace chamber with one opening at the top of the upright retort, while an air-blast is led into the bottom of the furnace chamber below rocking fire-bars, which start at the bottom of the retort, and slope upwards to leave room for ash-holes closed by gas-tight covers. The retort is filled with iron or steel borings—alone if pure hydrogen is required, or cast into balls with pitch if a little carbon monoxide is not a drawback, as in foundry work. The furnace chamber is filled with coke, fed in through man-holes or hoppers in the top, and the fuel being ignited, the blast is turned on, and the mixture of nitrogen and carbon monoxide formed passes over the iron, heating it to a red heat, while the incandescent coke in contact with the retort does the same thing. When the fuel and retort full of iron are at a cherry-red heat, the air-blast is cut off, and the pipe connecting the furnace and retort, together with the pipe in connection with the bottom of the retort, is closed. Steam, superheated by passing through a pipe led round the retort or interior wall of the furnace, is injected at the bottom of the red-hot mass of iron, which decomposes it, forming magnetic oxide of iron and hydrogen, which escapes by the second tube at the top of the retort, and is led away—to a carburetting chamber if required for illumination, or else direct to the gas-holder if wanted as a fuel: the mass of incandescent fuel in the furnace chamber surrounding the retort keeping up the temperature of the retort and iron sufficiently long to enable the decomposition to be completed. The hydrogen and steam valves are now closed, and the air-blast turned on; and the hot carbon monoxide, passing over the hot magnetic oxide, quickly reduces it down again to metallic iron, which, being in a spongy condition, acts more freely on the steam during later makes than it did at first, and, being infusible at the temperature employed, may be used for a practically unlimited period. What more simple method than this could be desired? Here we have the formation of the most valuable of all fuel gases at the cost of the coke and steam used—a gas also which has double the carrying power for hydrocarbon vapours possessed by coal gas, while its combustion gives rise to nothing but water vapour.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Candidates for the newly founded Clerk-Maxwell Scholarship in Experimental Physics are requested to send their names to Prof. Thomson, 6 Scroope Terrace, Cambridge, before February 21. Each candidate is requested to forward a statement of the original work which, in accordance with the conditions of the tenure of the Scholarship, he would undertake if elected.

The University Lecturer in Geography (J. Y. Buchanan, F.R.S.) announces a course of lectures in Physical Geography and Climatology, to be given on Mondays and Wednesdays, at 10 a.m., beginning January 26.

The degree of M.A. *honoris causâ* is to be conferred on James Alfred Ewing, F.R.S., Professor of Mechanism and Applied Mechanics, who gave his inaugural lecture on Tuesday, January 20. His subject was "The University Training of Engineers."

On Monday, January 26, the following communications will be made to the Philosophical Society:—Prof. J. J. Thomson, on the electric discharge through rarefied gases without electrodes; Mr. J. Larmor, St. John's College, on diffraction at caustic surfaces.

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for December 1890 contains an account, by H. J. Cox, of a waterspout which occurred at Newhaven, Connecticut, on October 19 last, between two thunderstorms about five miles apart. A funnel-shaped cloud rapidly descended, while the water below it rose upward, first about 3 feet, and, when the spout was complete, above 30 feet. The spout was about 300 feet high, and 25 feet in diameter. It moved about two miles in ten minutes, and when it met the thunderstorm it moved back in the opposite direction about a mile.—A summary of Dr. Hann's paper on temperature in anticyclones and cyclones, the subject of which has already been noticed in NATURE.—Observations and studies on Mount Washington, by Prof. Hazen, to determine, by means of the sling hygrometer, the temperature and humidity at each mile by walking down the mountain and up again. The results of sixteen journeys show that in the cases with partly dry air the decrease of temperature with elevation did not differ widely from the theoretical value, but with moist air the theoretical difference per 100 feet was much less than the observed difference.—Cyclones and tornadoes in North America, by J. Brucker. The object of the paper is to show that tornadoes or local air-whirls are analogous to water-whirls, and the subject is illustrated by diagrams.—The cooling of dry and moist air by expansion, by Prof. Marvin. The author refutes Prof. Hazen's objections to the principle that moist or saturated air is warmed by the latent heat set free from that portion of the vapour that is condensed by expansion. Prof. Marvin states, *inter alia*, that Prof. Hazen's calculations are not made by the proper thermodynamic equations, and are incorrect. Prof. Hazen, on the other hand, offers a prize of 100 dollars for the proof of the proposition, that Espy's experiments, when properly interpreted, prove his theory.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 18, 1890.—"On Certain Conditions that Modify the Virulence of the Bacillus of Tubercle." By Arthur Ransome, M.D., F.R.S.

In order to test the influence of light, air, and dry soils upon the virulence of the bacillus of tubercle, the following series of experiments were devised.

It was decided to expose tuberculous sputum—

(*a*) In a locality (Bowdon) where the soil was dry and sandy (about 100 feet in thickness), and where very few cases of phthisis were known to have originated. It was to be placed in full daylight or sunlight, and exposed to abundant streams of fresh country air.

(*b*) A portion of the same sputum would be exposed under similar conditions, in the same place, with the exception that it would be put into a darkened chamber.

(*c*) A third portion would be taken to a small four-roomed tenement in Manchester, on a clay soil, without cellarage, and badly ventilated, but it would be placed on the window-ledge, with as much light as could there be obtained.

(*d*) A portion would be placed in the same cottage, but in a dark corner of a sleeping-room in which it was known that three deaths from phthisis had occurred within the space of six or seven years.

(*e*) Finally, a portion would be exposed to used air coming from a ward in a Consumption Hospital in Bowdon, in darkness.

Two collections of sputum were obtained—

(*a*) From a woman dying of phthisis, collected on April 25. This specimen contained comparatively few bacilli.

(*b*) Also from a woman in an advanced stage of phthisis. This sputum contained abundance of bacilli.

These collections of sputum were divided into portions, and placed in watch-glasses marked A.1, A.2, A.3, B.1, . . . B.10. Some of these watch-glasses were exposed without further arrangement, but others, where there might be a possibility of infection, were inclosed in cages, so arranged that air could reach them through a thin layer of cotton-wool.

These watch-glasses were then exposed for five weeks under the conditions already noted, commencing on April 29, 1890, with the exception of B.9 and 10, which were started on May 2. Most of the specimens were withdrawn on June 3; but one, B.10, was divided on May 12, and a portion—B.10.(*b*)—was introduced into a glass bulb and exposed for several minutes each day to a current of ozonized oxygen.

All the specimens were then inclosed in a box, and forwarded to the Pathological Laboratory, Owens College, where Dr. Dreschfeld, the Professor of Pathology, had kindly undertaken to carry out the necessary inoculations. The animals used were rabbits, kept under favourable hygienic conditions. The dried sputum was mixed with sterilized water, to form a pasty mass, and this was inserted into the subcutaneous tissue of the back. All the instruments used were made thoroughly aseptic.

None of the four specimens of sputum exposed to fresh air and light on a dry soil conveyed the disease, but one of the three portions exposed under similar conditions in darkness produced tubercle.

Of the two exposed in the cottage in Ancoats, in the light, one produced tubercle; and of the two specimens exposed in the same place, in comparative darkness, one caused tubercle, the other did not.

Lastly, the specimen placed in the ventilating shaft from a ward in the Consumption Hospital, Bowdon, on a dry soil, conveyed the disease; and the portion removed from it after ten days, and exposed to the action of ozonized oxygen, did not produce tubercle.

These experiments are too few in number to justify the statement of positive conclusions, but, so far as they extend, they go to prove that fresh air and light and a dry sandy soil have a distinct influence in arresting the virulence of the tubercle bacillus; that darkness somewhat interferes with this disinfectant action; but that the mere exposure to light in otherwise bad sanitary conditions does not destroy the virus. There are some indications that the cotton-wool envelope interferes with the operation of the external conditions, whether for good or evil.

January 8.—"On the Minute Structure of Striped Muscle, with Special Reference to a New Method of Investigation by means of 'Impressions' stamped in Collodion." By John Berry Haycraft, M.D., D.Sc., F.R.S.E. Communicated by Dr. Klein, F.R.S. (From the Physiological Laboratory, University of Edinburgh.)

The author has held since 1880 that the cross striping seen on examining a muscular fibre by the aid of a microscope is due to the fact that the fibrils of which the muscle is composed are varicose in form, presenting alternating swellings and contractions. The striping, according to him, is the optical effect of their form, and not of their internal structure as is almost universally believed. Recently he has discovered very convincing proof of the truth of his opinions. It occurred to him to endeavour to "stamp" some soft material with muscular tissue, and to examine the "impression" or "intaglio" under the microscope: if this intaglio showed the cross striping, it of course would follow that this could be accounted for by the form of the muscle used as the "stamp." After experimenting for some months, he at last succeeded in his purpose, having found a suitable medium in a moist film of collodion. The properties of this film he found were very remarkable, for with it it is possible to take impressions of details too small to be recognized by any but the higher powers of the microscope. Such a film, pressed for a second or so against the back of the hand and then withdrawn, not only shows impressions of the tiny hairs covering the hand, but when examined under the microscope the minutest details of the imbricated scales of which they are composed come out far more clearly than when the somewhat opaque hair is itself examined. When the film is gently pressed upon some fresh or preserved muscle, the intaglio shows in every detail the striping so characteristic of the muscle, and not only so, but every change in the striping which is known to occur when the muscle contracts can be stamped as well. The intaglio in fact gives the details of the striping in whatever state of contraction or relaxation the fibre, used as a stamp, may happen to be, and it follows of course that these changes in the striping are due to changes in the form of the fibrils. In this case the most current views of contraction have to be discarded, for these explain contraction as being due to osmotic reactions between the substances which were supposed to constitute the cross stripes (these are generally held to mark the position of alternating bands of semi-fluid and solid substances). The author advances a new hypothesis relating to the stripes, which may be described as follows:—As a matter of fact, we find that, in a study of comparative histology, cross striping is found where rapidity of contraction is required; in other words, the fibrils of an unstriped fibre lose their cylindrical character, and become segmented up into tiny particles, each little particle shortening and thickening during contraction, and causing recurrent bulgings which produce the stripes. The reason of this segmentation may not unnaturally be ascribed to the fact that the smaller a contracting particle is the sooner it will reach its maximum of shortening, just as, in the case of the gross muscles, the hare can nearly keep pace with the horse, because its leaps, although shorter, are much quicker. While offering the above explanations, the author does not consider we are yet in a position to explain the phenomenon of contraction itself; and concludes by saying that, if ever we are in a position to express muscular contraction in terms of the inorganic world, it will result from a study of the lower and simpler types of contractile tissue, rather than from the highly evolved tissue of striped muscle.

"On the Reflection and Refraction of Light at the Surface of a Magnetized Medium." By A. B. Basset, M.A., F.R.S.

The object of the present paper is to endeavour to ascertain how far the electromagnetic theory of light, as at present developed, is capable of giving a theoretical explanation of Dr. Kerr's experiments (*Phil. Mag.*, May 1877, and March 1878) on the effect of magnetism on light.

In these experiments, polarized light was reflected from the polished surface of soft iron, and it was found that, when the reflector was magnetized, the reflected light exhibited certain peculiarities, which disappeared when the magnetizing current was off. It was also found that the effects of magnetization were, in most cases, reversed when the direction of the magnetizing current was reversed; that is to say, if the intensity of the reflected light was strengthened by a right-handed current, it was weakened by a left-handed one.

Since a metallic reflector was employed, the results were complicated by the influence of metallic reflection, and it therefore seems hopeless to attempt to give a complete theoretical explanation of these experiments until a satisfactory electromagnetic theory of metallic reflection has been discovered; and this, I believe, has not yet been done.

There are, however, several non-metallic substances (such as strong solutions of certain chemical compounds of iron) which are capable, when magnetized, of producing an effect upon light; and the theoretical explanation of the magnetic action of such substances upon light is accordingly free from the difficulties surrounding metallic reflection. I have accordingly, in the present paper, attempted to develop a theory which is applicable to such media.

The theory, which is due to Prof. Rowland, is founded upon the following considerations:—

It was proved by Hall (*ibid.*, March 1880) that, when a current passes through a conductor which is placed in a strong magnetic field, an electromotive force is produced, whose intensity is proportional to the product of the current and the magnetic force, and whose direction is at right angles to the plane containing the current and the magnetic force. Prof. Rowland (*ibid.*, April 1881) has assumed that this result holds good in a dielectric which is under the action of a strong magnetic force; accordingly, the general equations of electromotive force become

$$P_1 = -\frac{dF}{dt} - C(\gamma\dot{g} - \beta\dot{h}) - \frac{d\psi}{dx} . \quad . \quad . \quad (1)$$

where α, β, γ are the components of the external magnetic force, and C is a constant which depends upon the physical constitution of the medium.

Writing $p_1 = C\alpha$, &c., it follows that, if the medium is isotropic, the equations of electric displacement are of the form

$$\frac{d^2f}{dt^2} = \frac{1}{\mu K}\nabla^2 f + \frac{1}{4\pi\mu}\left(p_1\frac{d}{dx} + p_2\frac{d}{dy} + p_3\frac{d}{dz}\right)\left(\frac{d\dot{g}}{dz} - \frac{d\dot{h}}{dy}\right) . \quad (2)$$

The results of the paper agree with Dr. Kerr's experiments in the following particulars:—

(i.) The reflected light is elliptically polarized.

(ii.) When the magnetization is parallel to the reflecting surface, no effect is produced when the incidence is normal, or when the plane of incidence is perpendicular to the direction of magnetization.

(iii.) When the plane of incidence is parallel to the direction of magnetization, and the light is polarized *in* the plane of incidence, the magnetic term increases from grazing incidence to a maximum value, and then decreases to normal incidence.

The principal point of disagreement is, that in all cases the intensity of the reflected light is unchanged when the direction of the magnetizing current is reversed.

I do not think that the results of the theory can be considered altogether unsatisfactory, since they certainly explain some of Dr. Kerr's experimental results; and I am disposed to think that the disagreement is due to the disturbing influence of metallic reflection. At the same time, the question is one which can only be decided by experiment, and it is therefore most desirable that experiments on magnetized solutions should be made.

EDINBURGH.

Royal Society, January 5.—Prof. Chrystal, Vice-President, in the chair.—After the reading of some obituary notices, Prof. Tait communicated a paper on the soaring of birds, being a continuation of a letter from the late Mr. W. Froude to Sir W. Thomson. In the previous part of this letter, Mr. Froude had expressed the view that, when a bird soars or skims without moving its wings, an effective upward current of air must exist, of which the bird takes advantage. In one case, in which a bird seemed to soar in a glassy calm, he found that it was really soaring in the upward currents in the front of an advancing sea-breeze. He explained the skimming of albatrosses along the surface of the sea in a practical calm as due to the upward displacement of the air which necessarily occurs in the front of an advancing ocean-swell. In the continuation of the letter, now communicated, he adduces observational evidence that the birds actually do skim over this portion of the wave, and that they commence to flap whenever they pass away from it, or when the wave passes underneath them and leaves them behind. In the front of a wave, 500 feet in length and 10 feet high, advancing with a speed of 50 feet per second, the maximum speed of upward speed of the air is about 3 feet per second. In the present portion of his letter, Mr. Froude also dealt with the soaring of birds in a gale. He believed this to be due to the same cause as that which is effective in the carrying up of drops of spray to heights of 40 or 50 feet, so thickly as to resemble a dense shower of rain. Vortices are produced in the air over the surface, and the ascending currents in these vortices move more quickly than the descending currents move. Of course, the ascending portion has proportionately less cross-area; but, on the other hand, the resistance is proportional to the square of the speed, so that the upward momentum which is communicated to a drop of water while crossing the ascending portion is greater than the downward momentum which is communicated to it while crossing the descending part. Mr. Froude believed that this fact would explain the soaring of birds in a gale. Sir W. Thomson, however, thinks that this cause, though sufficient probably to account for the raising of the water-drops, will only produce effects of the second order in the suspension of birds. He believes that Lord Rayleigh's explanation—which does not seem to have occurred to Froude—that the bird takes advantage of the greater speed of the wind at higher levels, and its less speed at lower levels, is the true one.—Prof. Tait read a note on impact, in continuation of previous notes on the same subject. He shows that solid bodies may be divided into two large classes according to the effect of impact upon them. In one of these classes the time of impact remains constant, whatever be the distortion, up to a certain limit. When this limit is exceeded, the time of impact becomes shorter as the distortion is increased. This means that Hooke's law is obeyed up to the given limit, beyond which the force of restitution increases at a greater rate than does the distortion. In the other class of substances the time of impact first increases, then remains constant, and finally diminishes, as the distortion is continuously increased. Therefore, in the first stages, the force of restitution does not increase so rapidly as the distortion increases. Cork is a typical example of the latter class; vulcanized india-rubber of the former.

January 9.—A special meeting was held, Prof. Crystal, Vice-President, in the chair.—Dr. John Murray read a paper on the form, structure, and distribution of manganese nodules in the deep sea. He exhibited a number of specimens of the nodules. Fragments of pumice-stone, which have become water-logged and have sunk to the bottom, frequently form the nuclei. In other cases the nuclei are pieces of rock, sharks' teeth, ear-bones of whales, &c. Dr. Murray believes that the manganese is deposited from solution by way of the carbonates. While nodules are of comparatively rare occurrence in the shore deposits—blue muds—where organic life is greatest, they are found in great abundance in deep waters, where life is at a minimum.—Mr. Robert Irvine and Dr. John Gibson read a paper on the occurrence of manganese deposits in marine muds. The authors have found by experiment that manganous sulphide is dissolved and decomposed by sea-water which contains carbonic acid in solution.—Mr. J. Y. Buchanan read a paper on the composition of oceanic and littoral manganese nodules. His paper contained an analysis of nodules from the North Pacific, from the ocean south of Australia, and from the deep part of Loch Fyne. The localities, and attending circumstances, were fully described, as also were the physical characteristics of the different types of nodules. The principal object of the analysis was to determine the state of oxidation of the manganese. It was found that, in the oceanic nodules, the formula of the oxide varied from $MnO_{1\cdot945}$ to $MnO_{1\cdot979}$, so that it consisted of almost pure MnO_2. A slight difference was found in the oxidation of the outside shell from that of the kernel, the outside portions having the formula $MnO_{1\cdot951}$, while the formula of the inner portions was $MnO_{1\cdot974}$. The formula of the oxide in the Loch Fyne nodules varied from $MnO_{1\cdot394}$ to $MnO_{1\cdot562}$, so that these nodules approximated in composition to the sesquioxide, Mn_2O_3. The kernels were much richer in oxygen than were the exterior portions, the formula of one being $MnO_{1\cdot75}$. A number of determinations were made with regard to the moisture, the loss on ignition, and the density of the nodules in the moist, the dry, and the ignited states, from which the apparent density of the volatile products was calculated.—Mr. Buchanan also laid on the table a number of analytical results regarding the composition of some deep-sea deposits from the Mediterranean.—Messrs. Robert Irvine and W. S. Anderson communicated a paper on the action of metallic salts on carbonate of lime, specimens being exhibited.—The reading of these papers was followed by a short discussion on the bearing of some of the above results on the conclusions arrived at in Mr. Buchanan's paper (read on December 1) regarding the part played by sulphides in the formation of the ochreous deposits of the ocean. Mr. Irvine and Dr. Gibson hold that the results which they have obtained show that the manganese could never have been found in the circumstances described in Mr. Buchanan's paper. Mr. Buchanan recognized the importance of the observation, but he suggested that, although very alterable in sea-water, as it is also in fresh water, the MnS might be formed locally; and he also stated that, in his own previous paper, a transient existence alone was claimed for it. The results do not affect his views as to the formation of the ferric hydrates and of red and blue clays. Mr. Buchanan held that we are still in the dark regarding the formation of *nodules*.

PARIS.

Academy of Sciences, January 12.—M. Duchartre in the chair.—On the hypothesis of the spheroidal shape of the earth, and on the formation of its crust, by M. H. Faye. The most recent and precise geodetic measures are brought forward to confirm Laplace's opinion that the earth is an ellipsoid of revolution. Against the objection that all the measures have been made relative to continental surfaces, and most of them in the northern hemisphere, it is urged that contemporaneous measures with the pendulum give values from which the same conclusion must be drawn; and that these have been executed in both hemispheres and on both land and water surfaces. Geological considerations are also adduced in favour of this view.—Note on fly-wheels, by M. Léauté. The dimensions and weights of fly-wheels suitable for electric lighting machinery are discussed.—On a claim of priority in favour of M. Chancourtois relative to the numerical relations between the atomic weights of elements, by MM. Lecoq de Boisbaudran and A. de Lapparent. The authors bring forward a paper presented at the Academy in 1862, and having the title "Vis tellurique; classement naturel des corps simples ou radicaux obtenu au moyen d'un système de classification hélicoidal et numérique," as evidence of M. Chancourtois's discovery of the periodic law.—On the oscillations of a system submitted to periodic disturbances, by M. E. Vicaire.—Remarks on the theorem of the continuity of the gaseous and liquid states, by M. E. Mathias. The object of this note is the verification by experimental results, of Van der Waal's law expressing the relation between the volume, the pressure, and the absolute temperature of a fluid.—Practical solution of the problem of the emergent liquid column of a thermometer by the employment of a correcting stem, by M. C. E. Guillaume. The scale of an ordinary mercurial thermometer is only true on the supposition that the instrument is exposed at least up to the top of the liquid column, to the temperature which it is desired to measure. In order to obtain the necessary correction practically, M. Guillaume has used the stem of a thermometer having mercury in it, and graduated in the ordinary manner. Let a thermometer and a "correcting stem" be placed side by side in a bath, and have the same amount of mercury above the liquid. The thermometer will indicate approximately the temperature of the bath, and the correction will be given by the difference of the thermometer reading and the reading of the "correcting stem" expressed in terms of the former.—Variations of conductivity of insulating substances, by M. E. Branly. It is shown that many dielectrics and metallic powders increase in conductivity when subjected to the action of electric discharges.—Physical properties and molecular constitution of simple metallic bodies, by M. P. Jacobin. The following are among the conclusions arrived at: (1) for the diamagnetic metals the conductivity is sensibly proportional to the sixth power of the number of molecules; (2) for magnetic metals the conductivity is nearly inversely proportional to the sixth power of the distance between the molecules. Similarly, all the physical properties of metals of the same group are stated to depend exclusively on their molecular distance.—On the intensity of telephonic effects, by M. E. Mercadier. The conditions to be fulfilled in order to obtain the maximum effect in a telephone, are: (1) the movements of the lines of force of the field should be facilitated; (2) the greatest possible number of the bobbin wires should cut the lines of force perpendicular to their direction; (3) the thickness of the diaphragm should be diminished until it is just sufficient to absorb the greatest number of lines of force in its neighbourhood; (4) the relation between the volume of the diaphragm induced to the total volume should be as large as possible.—An apparatus of luminous projection, applicable to chemical balances, for obtaining rapid weighings, by M. A. Collot *fils*.—On some derivatives from phenol and naphthol, by M. J. Minquin.—On the production of higher alcohols during alcoholic fermentation, by M. L. Lindet.—New method for the detection of olive oil and linseed oil, also applicable to butters and margarines, by M. Raoul Brullé.—Note on poisoning by mussels, by M. S. Jourdain.—Contributions to the physiology of the root, by M. Pierre Lesage.—Influence of light on the production of the prickles of plants, by M. A. Lothelier.—On the diamondiferous sands collected by M. Charles Rabot in Lapland, by M. Ch. Vélain.

AMSTERDAM.

Royal Academy of Sciences, December 27, 1890.—Prof. van de Sande Bakhuyzen in the chair.—M. Beyerinck spoke of the life-history of a pigment bacterium. This organism (*Bacillus cyaneo-fuscus*, n.s.) is the cause of a much feared, local colouring process in Dutch cheese, called "blue illness"; and of "black glue," a calamity observed in a factory of animal bone gelatine. The natural habitat is ditch water and ground water. *Bacillus cyaneo-fuscus* is a *Pepton-bacterium*, *i.e.* it can be fed with albuminous matter alone. Thence, a solution of ½ per cent. pepton in common water is sufficient nutriment; gelatine or glue, egg albumen, fibrine, and caseine, are also, each alone, sufficient, but they are peptonized, before absorption, by the secretion of a powerful enzyme. This pigment is twofold: (1) deep blue spherites; (2) a dark brown diffusible colouring matter. Thereby a pepton solution becomes fully black. Coming from the wild state, the vegetation power is very active, but, by the culture at the *optimum-temperature* of 15° C. to 20° C., this power weakens. In a first state of deterioration the weakened form cannot be cultivated on solid matter, such as pure gelatine, whereas it will grow still in liquid food with all its ordinary characters. In a further state the weakening process is characterized as well by the loss of the power just mentioned, as by that of the pigment production. A last step leaves the organism almost fully incapable of growth and reproduction. A long exposure of weakened cultures, in excellent but diluted food (¼ per cent. pepton siccum in common water), to temperatures between 2° and 5° C., tends to restore the vegetative activity. These observations relate also to many other bacteria, and they are, no doubt, of the same order as the alterations in the virulence of contagious matter, caused by the influence of temperature.

CONTENTS.

THURSDAY, JANUARY 29, 1891.

UNIVERSITY COLLEGE AND ITS APPEAL FOR FUNDS.

UNFORTUNATELY it is an economic fact, that, under existing conditions, the highest education cannot be self-supporting. The more advanced the teaching, the smaller the class, and as a result the amount of fees received. The pursuit of knowledge in the highest sense, as has often been said, is one of the nineteenth century forms of "a vow of poverty." Research must be endowed; if not, it must be alimented by other pursuits, that is, by temporary desertion. But with certain branches of knowledge the teacher is not the sole cause of expense. In pure mathematics a blackboard, which is not a costly piece of apparatus, is almost the limit of his reasonable requirements. In classics and literature generally some maps and photographs content all but the most exacting; a few casts and models in addition place him in a state of pedagogic luxury. But in science, as the word is commonly understood, especially in the physical and natural departments, the teacher requires laboratories and museums, apparatus and specimens, which often are costly. Science, in short, as unfriendly critics in other departments are wont to say, is like a daughter of the horse-leech, always crying "give, give."

At the present time the two University Colleges in London are suffering from the pinch of poverty, and fearing that atrophy of the purse will result in diminution of efficacy. University College and King's College are appealing to the public simultaneously and with mutual goodwill, for a capital sum to enable them to provide for several pressing wants, more especially for additional laboratories and appliances required by students in an age of scientific progress. Of these two colleges, the former may be said to appeal *urbi et orbi*. It was founded some sixty years ago, at a time when the older universities were closed to all who could not subscribe to certain theological tests, practically to all but members of the Established Church of England. This position it has ever maintained. Its chairs and its lecture-rooms are open to all without respect of creed.

A committee is now being formed in order to make an appeal to the public, and especially to the citizens of London, on the basis of a statement issued by the Council of University College. It asks for a sum of not less than fifty thousand pounds, to be devoted mainly to the increase of buildings and appliances for teaching. The most pressing need is a new physical laboratory. That which at present exists was opened in the session of 1867–68. It was not built for the purpose, being merely one of the College lecture-rooms, but it was the first laboratory of its kind opened in London. Since then other rooms have been added, but all the arrangements are of a makeshift character. Additional space is absolutely necessary. At present the students are too much crowded; it is impossible to keep the more important instruments in fixed places, so that much time is lost and risk of injury incurred by moving them about, and advanced work or original investigation has become almost impossible. Grave difficulties also arise from want of steadiness in the floors, and from unfavourable magnetic conditions caused by local circumstances, such as the arrangements for warming the building. Hardly less pressing is the need for improving the means of teaching electrical engineering, in which instruction has now been given for about five years with yet more inadequate appliances. At the present moment hardly any branch of technical physics is of greater practical importance, and it cannot be better taught than in a college where a sound knowledge of pure and applied mathematics, of physics and of mechanical engineering, can be obtained.

The last-named of these subjects also calls for funds. The engineering laboratory dates from 1878, and for some years was the only one in this country. The system of laboratory instruction, initiated by the late professor, Mr. A. B. W. Kennedy, has been adopted in all the leading engineering schools in Great Britain. This has led to a very remarkable development in the methods of engineering education, so that additional space and additional machines are required to enable University College to retain the position which it has obtained as an engineering school.

A Professorship of Architecture was founded fifty years ago at University College, and was for long the only Professorship of the kind in England. It is very desirable to form a collection of architectural models, and a school of architectural drawing. The College appears to be a place exceptionally suited for a complete school of architecture, because of the existence of the Slade School of Fine Art, as well as an engineering school.

Further, the Society for the Extension of University Teaching in London finds that the lectures on chemistry and physics, given under its auspices, require to be supplemented by evening classes for laboratory instruction. A scheme is being drawn up, the main outlines of which have been already approved by the Council of University College, to provide for this want; but as the existing laboratories are already fully occupied, and the apparatus in use by more advanced students during the day cannot be cleared away to make room for an evening class, additional rooms will be required.

Lastly, many of the professors are insufficiently remunerated. For instance, with the single exception of English, no language-chair has, as yet, been adequately endowed. The Council is anxious to extend the teaching of European languages and to co-operate with the Imperial Institute in the development of its school of modern Oriental studies, in which work King's College also is giving effective aid. But this work cannot be self-supporting at present, if, indeed, it ever become so; and how are good teachers to be secured without some assured stipend?

It is, then, to be hoped that the citizens of London will not prove less liberal in the cause of education than those of Manchester, Liverpool, Birmingham, and other great towns. It may be said that in these the University College is the sole place of higher education, but that in London other institutions now exist in which such training may be obtained. Possibly there may be some truth in this remark, but if so, it would be an ungenerous answer to the present appeal. These two Colleges, and University College especially, have borne the burden and heat of the day. For many years it met a great and crying want. It cannot be charged with having failed in its duty. It may point in honest pride to the teachers who have occupied its chairs, to the many men of eminence in science, in literature, and in various professions, especially those of Medicine and Law, who have been trained within its walls. It is a standing monument of the munificence of a past generation; it has, indeed, received, within the last few years, more than one liberal bequest, but these are not applicable to the present, most urgent, need; so that, unless further aid be given, its usefulness must be diminished.

WESTERN CHINA.

Three Years in Western China: a Narrative of Three Journeys in Szechuan, Kweichow, and Yunnan. By Alexander Hosie, M.A., F.R.G.S., H.B.M.'s Consular Service, China. With an Introduction by Mr. Archibald Little. (London: George Philip and Son, 1890.)

In the year 1876 certain serious disputes between Great Britain and China were brought to an end by an agreement known, from the place at which it was negotiated and signed, as the Chefoo Convention. One of the provisions of this famous instrument was that the British Government might send officers to reside at Chungking, on the Yangtsze, in Szechuan province, in order to study the conditions of British trade in that region. Beginning with 1877, a succession of junior officers of the British Consular Service in China have been stationed at Chungking, and from this great commercial mart as a centre have travelled far and wide in parts of China hitherto wholly, or almost wholly, unknown to the West. The late Mr. Colborne Baber was the first of these, and his travels are recorded in the special volume of the Royal Geographical Society, "Travels and Researches in Western China," which the late Colonel Yule described as "that admirable and delightful narrative which the periodical press has allowed to pass almost absolutely unnoticed—taking it, I suppose, for a Blue-book, because it is blue!" The reports of these officers have from time to time been laid before Parliament, and are amongst the most interesting publications ever issued in this way. Not long ago a report from Mr. Bourne, Mr. Hosie's successor, recorded a journey from Chungking across Yunnan to the frontier of Tonquin, along that frontier towards the sea, and then back again across Kweichow province to Chungking; but this, though full of interesting and important information relating to the country, the people, and especially to the trade between Tonquin and Southern China, remains more or less inaccessible in its present shape. Mr. Hosie has followed Mr. Baber's example, and has prepared for popular perusal an account of his journeyings. He is fortunate in the time and circumstances of the publication, for, by a recent agreement between the British and Chinese Governments, the town of Chungking is to be opened, under certain conditions, to the trade and residence of British merchants. The book contains an introduction by Mr. Archibald Little, whose name has for some years been associated with efforts to navigate the upper waters of the Yangtsze, and to throw Chungking open to foreign trade.

Mr. Hosie's journeys carried him in various directions, and had various objects in view; but, naturally, he avoided as far as possible the footsteps of his predecessors. His first journey, which lasted for nearly ten weeks, took him southward from Chungking into the mountainous province of Kweichow, where he came across the aboriginal Miao-tsze, now almost extinct, to Kwei-yang-fu, the capital, whence he turned westward to Yunnan-fu, the capital of the province of the same name, and so by the mountains of North-Eastern Yunnan home to Chungking. On this journey, besides the Miao-tsze he first saw the white-wax insect, about which he has much to say later on. The second journey lasted nearly six months, and carried the traveller through a more interesting and less-known region. From Chunking he travelled to the north-west across the great plain of Cheng-tu, noted for its extraordinary fertility, the density of its population, and the wealth of its inhabitants. From another point of view, also, it is notable as the only large area of level ground in all Western China; it is a plateau about 7000 feet above the level of the sea, at the foot of the mountains of Tibet, and has an area of about 2400 miles. Richthofen says of it that there are few regions in China, equal areas compared, which can rival it in wealth and prosperity, density of population and productive power, fertility of climate and perfection of natural irrigation; and there is no other where, at the present time, refinement and civilization are so generally diffused among the population. From the city of Cheng-tu, the administrative capital of Szechuan, Mr. Hosie turned to the south-west through the districts inhabited by the Lolos, an aboriginal tribe, discovered practically by the French Expedition under De Lagrée and Garnier, and further investigated by Mr. Baber and Prof. de Lacouperie. Thence he passed into the famous valley of the Chien-chang, and to the city of the same name, the Caindu of Marco Polo, which is the centre of the insect white-wax trade, and following the track of the great Venetian arrived at Tali-fu in Yunnan, the Carajan of Marco. The brine industry which the old traveller found flourishing—it was here that cakes of salt were used as currency—still exists. From this city, Mr. Hosie, passing through Yunnan-fu and by the valley of the Chi-hsing River, reached Chungking.

Mr. Hosie's third journey was in one sense the most interesting of all, for, although the country traversed was not all new, the object was to study carefully, for the authorities at Kew, the Chinese insect wax industry. He decided for this purpose to visit the centre of the wax culture of the province, to ascend by the way the sacred Mount Omei, from whose summit the glory of Buddha is said to be visible, and then to strike the Yangtsze at its highest navigable point. The mountain, which is about 11,000 feet high, has been devoted to the worship of Buddha almost since the beginning of our era, and is sufficiently remarkable from a physical point of view. Mr. Hosie devotes a special chapter to the white-wax insect, and the industry associated with it, in which he traces the career of the *Coccus pe-la* of Westwood from its cradle, through its busy and interesting life, to its dishonoured grave. It is only in quite recent years, and mainly through Mr. Hosie's journey, that the mystery surrounding the industry has been cleared up. The subject was frequently referred to in older works in China, and the chief object of Mr. Hosie's journey was to procure for Sir Joseph Hooker specimens of the foliage, of the flowers, and trees, on which the insects are propagated, specimens of the living incrusted white-wax, samples of the latter as it appears in commerce, and Chinese candles made from it. The Chien-chang valley already alluded to, which is about 5000 feet above the level of the sea, is the great breeding-ground of the white-wax insect. One very prominent tree there is known to the Chinese as the insect tree. It is an evergreen with the leaves springing in pairs from the branches, very thick, dark green, glossy, ovate, and pointed. In May and June the tree bears clusters of white flowers, which are succeeded by fruit of a dark purple colour. The Kew authorities have come to the conclusion that it is *Ligustrum lucidum*, or large-leaved privet. In March, when Mr. Hosie saw the trees, he found attached to the bark of the boughs and twigs numerous brown pea-shaped excrescences. The larger of these were readily detachable, and, when opened, presented either a whity-brown pulpy mass, or a crowd of minute animals like flour, whose movements were just perceptible to the naked eye. From two to three months later these had developed in each case into a swarm of brown creatures each provided with six legs and a pair of *antennæ;* each of these was a white-wax insect. Many of the excrescences also contained either a small white bag or cocoon covering a pupa, or a perfect imago in the shape of a small black beetle. This beetle is a species of *Brachytarsus.* If left undisturbed, the beetle, which is called by the Chinese the "buffalo," will, heedless of the *Cocci*, continue to burrow in the inner lining of the scale which seems to be its food; the beetle is, in fact, parasitic on the *Coccus.* When a scale is plucked from the tree the *Cocci* escape by the orifice which is made. Two hundred miles to the north-east of the Chien-chang valley, and separated from it by a series of mountain ranges, is the town of Chia-ting, in which insect white-wax as an article of commerce is produced. The scales are gathered in the Chien-chang valley, and are made up into paper packets each weighing about 16 ounces. Sixty of these packets make a load, and are conveyed by porters from Chien-chang to Chia-ting (in former years there are said to have been as many as ten thousand of these porters). They travel only during the night, in order to avoid the high temperature of the day, which would tend to the rapid development of the insects and their escape from the scales. At the stopping-places the packets are opened out in cool places, but in spite of this each packet is found to have lost on an average an ounce in transit. A pound of scales laid down in Chia-ting costs, in years of plenty, about half-a-crown; in bad years the price is doubled. In favourable years a pound of scales will produce four to five pounds of wax. In the plain around Chia-ting the plots of ground are thickly edged with stumps varying from 3 or 4 to 12 feet high, with numerous sprouts rising from their gnarled heads, and resembling at a distance our own pollard willows. The leaves spring in pairs from the branches, and are light green, ovate, pointed, serrated, and deciduous. The tree is said in all probability to be the *Fraxinus chinensis*, a species of ash. On the arrival of the scales from Chien-chang about the beginning of May, they are made up in small packets of from twenty to thirty scales, which are inclosed in a leaf of the wood-oil tree. The edges of the leaf are tied together with a rice straw, by which the packet is suspended close under the branches of this ash, or white-wax tree as the Chinese call it. A few rough holes are drilled in the leaf with a blunt needle, so that the insects may find their way through them to the branches. On emerging from the scales, the insects creep rapidly up to the leaves, among which they nestle for a period of thirteen days. They then descend to the branches and twigs, on which they take up their position, the females doubtless to provide for a continuation of the race by developing scales in which to deposit their eggs, and the males to excrete the substance known as white wax. This first appears as an undercoating on the sides of the boughs and twigs, and resembles sulphate of quinine, or a covering of snow. It gradually spreads over the whole branch, and attains, after three months, a thickness of about a quarter of an inch. After the lapse of a hundred days the deposit is complete, the branches are lopped off, and as much of the wax as possible is removed by hand. This is placed in an iron pot of boiling water, and the wax, on rising to the surface, is skimmed off and placed in a round mould, whence it emerges as the white-wax of commerce. Where it is found impossible to remove the wax by hand, the twigs and branches are thrown into the pot, so that this wax is darker and inferior. The insects, which have sunk to the bottom of the pot, are placed in a bag and squeezed of the last drop of the wax, and are then thrown to the pigs. The wax is used for coating the exterior of animal and vegetable tallow candles, and to give greater consistency to the tallow. It is also said to be used as a sizing for paper and cotton goods, for imparting a gloss to silk, and as a furniture polish.

Such is a brief summary of the account given by Mr. Hosie of this extraordinary industrial product. Those who wish to know more of the subject will find it in chapter xi. of the present volume, and in certain Parliamentary Reports therein referred to. In his last chapter Mr. Hosie refers to the non-Chinese races of Western and South-Western China, a subject about which very little is known, but which is of great interest to the ethnologist. In an appendix he gives exercises on the language of the Phö tribes. There is also an excellent sketch-map of South-West China to illustrate the journeys. It will thus be seen that, although the main object of Mr. Hosie's explorations was trade, the volume contains much matter of scientific interest. It is written in a simple and entertaining manner, and takes a very high rank indeed in the records of recent travel in that interesting region. Members of Her Majesty's service possess the great initial advantage of being thoroughly acquainted with the Chinese language and the Chinese people. How far this has aided Mr. Hosie in his journeys, and especially in his book, will be evident to all readers. As a sting in the critical tail, however, we venture to take exception to the transliteration of Chinese names which Mr. Hosie has adopted, and which appears to us to be growing too common amongst Chinese scholars. We fail to see the object of those eccentric combinations of letters and points, of which "Ssŭ-ch'uan," as applied to the name of the great province in which Mr. Hosie's journeys mainly lay, affords an example. If it be admitted—and we think it must be—that any combination of our letters cannot be more than an approximation to the Chinese sounds, which, by the way, vary almost indefinitely throughout the country, it would seem more reasonable to adopt a simple combination than a very elaborate and difficult one. We have always thought that, in this matter of transliteration, it is better, in order to avoid confusion and complication, to "stand on the ancient ways," however defective they may be, than to change them for others, of which the most that can be said is, that they are less defective.

THE GENUS STELLETTA.

Die Gattung Stelletta. Unter Mitwirkung von F. E. Schulze. Bearbeitet von R. von Lendenfeld. Mit 10 Tafeln. (Berlin: Georg Reimer, 1890.)

OSCAR SCHMIDT, in the year 1862, in his well-known work on the sponges of the Adriatic Sea, established the genus Stelletta for a group of tuber-like corticate sponges. Schmidt described several species from the Adriatic, and others still, in after years, from the coast of Algiers and from the Gulf of Mexico. After Schmidt, Carter took up the task of describing new forms, and he added many to the list between the years 1880 and 1886. Sollas, also, in his great work on the Tetractinellida of the *Challenger* Expedition, with a large mass of material before him, took the genus as the type of a family, and grouped beneath it a number of new genera and species.

The work that had been done, therefore, during these past eight-and-twenty years has been very considerable, and we opened this monograph on the genus Stelletta, by Dr. R. von Lendenfeld, with great expectations, thinking to find therein a masterly account of the whole group. The first and second pages give a neat though condensed account of the origin of the genus, which in the first lines is described according to the "diagnosis" of Dr. Lendenfeld, followed no doubt closely by the original "diagnosis" of Schmidt; then comes, on the next page, a statement which at once arrested our attention, to the effect that Sollas's family Stellettidæ, with its four sub-families and their genera, were all "in our sense" but species of Stelletta.

Now in a large volume lately published, under the auspices of the Royal Society of London, on the "Horny Sponges," by Dr. Lendenfeld (1889), he gives us a classification of the phylum, in which he adopts the arrangement of Sollas, as "embodying the latest results." No doubt, we reasoned, Lendenfeld's researches on the forms of this genus, the investigation of a mass of material far exceeding that which the *Challenger* brought together, and the assistance of that without any doubt most able investigator, F. E. Schulze, with whose assistance this memoir was written, had brought all this change in his views to pass. We called to mind that Dr. Lendenfeld's views of genera and species were somewhat peculiar: has he not himself written, "it is impossible to give a definition of what I mean by a genus, species, or variety" ("Horny Sponges," p. 835)? and then again, in referring to the labours of Polejaeff, he tells us he does not establish any new genera; he is of opinion that there are virtually only three genera of these horny sponges to be distinguished, and he is further of opinion that these sponges should be considered as one family. "Considering the very limited material at the disposal of Polejaeff, we (Lendenfeld) cannot be surprised that he should have arrived at an opinion so very different from that of all other writers on the subject." So that, however peculiar Lendenfeld's views might be, yet he was sure to base them on a fair quantity of material. Thus reasoning, we resumed our reading of his memoir on the genus Stelletta.

We read through a list of species "which, with greater or less certainty, we place in this genus, taking, however, no account of synonyms." This list extends over four pages, and contains "sixty-nine" forms. Immediately after the list we read: "The material which was at our service was in part collected by ourselves in the Gulf of Trieste and near Lesina, in part preserved and sent to us by Dr. Græffe. Five species, *Stelletta grubei*, *S. dorsigera*, *S. boglicii*, *S. pumex*, and *S. hispida*, were with great exactness examined."

We turned over the next page, and it was at once evident that the memoir was no monograph of the genus, but a no doubt very exact account of these five species, of which four were described long ago by Schmidt, and one, *S. hispida*, quite recently by Buccich and Marenzeller; and even of these five, though all are figured in the ten plates which accompany this memoir, yet of one, *S. pumex*, O. Schmidt, there is only a brief diagnosis given. Of the four species mentioned, most careful descriptions are given, and these are accompanied by good figures of the mega- and microsclere, and the histology of each species. The sections are very pretty, but distinctly schematic.

The memoir certainly is a useful contribution to our knowledge of the Adriatic species of the genus Stelletta, though it cannot be said to add much to that knowledge; and it is, moreover, interesting as shadowing out in some measure the lines on which our author purposes, no doubt, to write his monograph of the corticate sponges, in which he will justify his assertions as to the genera and species of Sollas. He gently reproves Marenzeller for referring, in his recent memoir on the Adriatic species of this genus, *S. grubei*, *S. boglicii*, *S. dorsigera*, and *S. anceps* to a single form, to be known as *S. grubei*, and he states that he cannot agree with him (p. 6); but seeing what he hints as to the future of the numerous species described by Sollas in the *Challenger* Report (p. 14), it would not surprise us if in the next volume on sponges, brought out, let us hope, under the auspices of the Royal Society, Dr. Lendenfeld returns to the early views of Polejaeff, and perhaps even to those of Marenzeller. There is little difficulty in seeing how this could be done—a new character added to a genus (see p. 57), and so it can be made to hug within its embraces a whole new lot of forms; repeat the process, and you likewise increase the progeny that will lay claim to be included. It does seem impossible to give a definition of what Dr. Lendenfeld means by a genus, but in time he may be able to do so himself.

CORAL ISLANDS AND REEFS.

Die Theorieen über die Entstehung der Koralleninseln und Korallenriffe und ihre Bedeutung für geophysische Fragen. Von Dr. R. Langenbeck. (Leipzig: Wilhelm Engelmann, 1890.)

IN this work, an octavo volume of 190 pages, rather closely printed, Dr. Langenbeck gives a critical history and discussion of the controversy on the origin of coral reefs, commencing with the publication of the first edition of Darwin's well-known book. It consists of a preface, which gives a brief historical summary of the literature of the subject, and six sections. The first one deals with the coral reefs in regions which either are stationary or moving in an upward direction. This deals with the Florida coast, Bahamas, Cuba, Philippines, Solomon Islands, &c. Dr. Langenbeck, in summing up, points out that barrier-reefs, like atolls, can be separated into two classes: the one—such as that off New Caledonia—following the contour of the neighbouring coast line, but separated from it by a deep channel, and rising steeply on the outer side, sometimes from a great depth; the other, like those of Florida—more irregular in disposition, and in close relation with banks of sediment and marine currents.

In the second section Dr. Langenbeck points out that the hypotheses of Murray and Guppy fail to explain the structure of many atolls and barrier-reefs. His argument follows the lines usually adopted by the opponents of these hypotheses, and brings together in a comparatively short compass a large number of important facts. The third section treats of the occurrence of the three types of reefs—fringing-reefs, barrier-reefs, and atolls—in the same neighbourhood, and the evidence of transition from movements in one direction to those in the opposite. The fourth is devoted to a description of the fossil coral reefs in the different geological formations, dwelling especially on their thickness—a matter to which reference has often been made in recent controversies. This, perhaps, is the most valuable portion of the book, for after briefly referring to the difficulties in obtaining the evidence, Dr. Langenbeck gives a summarized account of the coral reefs which hitherto have been observed. Reefs, sometimes of considerable thickness, occur in the Silurian, but they are developed on a much larger scale in the Devonian. In the Asturias they attain a thickness of 100 metres, but this is much exceeded in Western Carinthia, where the maximum thickness amounts even to 700 metres. In the Carboniferous and Permian, reefs, as a rule, are poorly developed, but they are found on a grand scale in the Alpine Trias. These are described, together with a summary of the discussion relating to them since von Richthofen first asserted the dolomite masses of the South-East Tyrol to be ancient reefs. The evidence supplied by the remainder of the Mesozoic and the Tertiary period is reviewed, and that afforded by the hill El Yunque, in Cuba, regarded as an elevated coral reef, which is fully 300 metres thick, is not forgotten. The present distribution of coral reefs is described in the fifth section; and in the last they are discussed in relation to various theories of a more general nature, among them that of Suess in regard to an accumulation of the ocean in equatorial and Polar regions.

Dr. Langenbeck's conclusion as to the main question is nearly the same as that expressed by the editor of the third edition of Darwin's "Coral Reefs" (which he does not appear to have seen), viz. that, while it is not applicable in every instance, yet it is the only one which can explain the peculiarities of very many coral islands and reefs in all three oceans—that is to say, he considers that Darwin's "subsidence" hypothesis holds its own as a general explanation. But whether his conclusion be accepted or not, the value of his book can hardly be questioned as a very full, laborious, and conscientious summary of the observations which have been made since the publication of Darwin's classic work. As a book of reference it will be most useful to all who are interested in the history of coral reefs.

OUR BOOK SHELF.

Fauna der Gaskohle und der Kalksteine der Permformation Böhmens. By Dr. Anton Fritsch. Vol. II. Part 4 (pp. 93–114, pls. 80*b*–90); Vol. III. Part 1 (pp. 1–48, pls. 91–102). 4to. (Prag: Fr. Řivnáč, 1889–90.)

DR. ANTON FRITSCH still continues to issue annual instalments of his great work on the Permian Vertebrata of Bohemia, and the two latest parts are specially devoted to an account of the remarkable Palæozoic group of Elasmobranch fishes, of which *Pleuracanthus* is the earliest described type. No less than twenty-two plates and upwards of sixty figures in the text illustrate the skeletal anatomy of these fishes in a manner that has not hitherto been attempted; and there is a supplemental plate in the first of the two parts representing the finest known example of the Dipnoan genus *Ctenodus*. The Bohemian material at the disposal of Dr. Fritsch is, indeed, so much finer than any previously studied and scientifically described by an ichthyologist that the memoir now before us marks an important advance in our knowledge both of the primitive Elasmobranchii, and to a certain extent also of the Dipnoi.

Ichthyologists of the modern school will scarcely assent to the inclusion of the autostylic Holocephali and the hyostylic Plagiostomi in the same order; but Dr. Fritsch's arrangement of the Pleuracanth fishes and the Acanthodii as two special subdivisions of the last-mentioned group will probably be accepted by all who have followed the most recent researches. In the systematic account of the Pleuracanthidæ the three genera *Orthacanthus*, *Pleuracanthus*, and *Xenacanthus*, are recognized, and described in detail, the two latter also forming the subject of beautiful restored figures. There is a concluding chapter summarizing the general results of the investigation; and this is accompanied by some theoretical remarks on the origin of the so-called archipterygium of Gegenbaur, with a series of diagrams to illustrate the principal stages of its supposed evolution and subsequent specialization. Each of the pectoral fins in the Pleuracanthidæ is a biserial "archipterygium," with a tendency towards becoming uniserial; and, according to Dr. Fritsch, there is no pelvic cartilage, the hitherto reputed pelvic element being truly the basipterygium. The dorsal fin is also primitive in the fact that it is much extended, while its endoskeletal supports are in direct connection and numerical relation with the neural arches of the axial skeleton. Otherwise, many striking resemblances between the Pleuracanthidæ and the modern Notidanidæ are observed, and Dr. Fritsch points out for the first time that these two families agree in the possession of more than five pairs of branchial arches.

A future opportunity may be afforded for a discussion of the principal details of the memoir, and it will suffice on the present occasion merely to direct the attention of the pure morphologist, as well as the palæontologist, to one of the most important treatises on fundamental facts that has appeared in the recent literature of the Vertebrata. A. S. W.

The Life of Ferdinand Magellan. By F. H. H. Guillemard. (London: George Philip and Son, 1890.)

THIS volume belongs to the well-known series, "The World's Great Explorers," and is in every way worthy of the volumes by which it has been preceded. It is curious, as Dr. Guillemard points out, that, while the world is year by year presented with biographies of persons for whom a tithe of Magellan's renown cannot be claimed, no life of the great circumnavigator has yet been published in English. Dr. Guillemard, having resolved to write the present book, set to work in earnest to provide an adequate biography. He consulted many old Spanish documents relating to the subject, and did his best to present in a bright and attractive form the materials thus acquired. A good deal of his work is conjectural, but intelligent readers will have no difficulty in discriminating between the established facts of the story and those parts of it which are based merely on probabilities. After an introductory chapter, in which the way is prepared for the right apprehension of the geographical problems of Magellan's age, the author describes the explorer's early life, his Indian service, and his service with Albuquerque and in Morocco. The most important part of the book, of course, is that which relates to Magellan's last voyage, a most careful and spirited account of which in all its stages, so far as they are known, is given by Dr. Guillemard. This portion of the narrative includes, besides what is told about the discovery of the Strait, chapters on South-east America and the mutiny in Fort St. Julien, the Ladrones and the Philippine Islands, the battle of Mactan and the death of Magellan. We are also told how the survivors of the battle of Mactan arrived at the Moluccas, and how they returned to Spain. The volume is well supplied with maps and good illustrations.

Key to Arithmetic in Theory and Practice. By the late J. Brooksmith, M.A., LL.B. (London: Macmillan and Co., 1890.)

THIS is a most voluminous work, comprising as many as 789 pages. Each example is completely worked out, and the methods adopted are straightforward and clear. The book will be found useful by teachers, and it will be a valuable aid to beginners, whom it will enable to follow each step in detail. The late author evidently devoted a great deal of patient labour to the preparation of the volume: for one fully worked out cube root sum takes no fewer than 1269 figures, while others of the same nature employ as many as seven or eight hundred in their solution.

LETTERS TO THE EDITOR.

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

The Cold of 1890–91.

ON a number of days during the present severe frost, the temperature at 4 feet was colder than on the grass, causing the upper portion of trees to become loaded with *hoar*, whilst none was deposited on the lower branches. This was a marked feature here in January 1888, during a long prevalence of dense fog.

On December 11, 1890, at 3 p.m., the temperature was 34°·5 on grass, 33°·0 at 4 feet, and 30°·0 at 20 feet; and on the 26th, at 9 a.m., it was 34°·4 on grass, 33°·5 at 4 feet, and 31°·2 at 20 feet.

From the observations of Mr. G. Fellows at Beeston Fields, near Nottingham, the same increase of cold in the air is shown, and also at the same time. These two reports have been tabulated, using the + sign where the upper temperature was lower than that on the grass. It seems desirable to record this phenomenon. Beeston Fields is 206 feet above the sea, whilst Shirenewton Hall is 530 feet, and situated 5 miles from the Bristol Channel. On one occasion this increase of cold in the lower air caused cirri clouds to form over the water of the Bristol Channel.

The excessive cold of more inland localities is not felt here; our greatest cold has been 14° of frost, whilst at Beeston Fields 27° of frost has been registered.

1890. Nov.	Shirenewton Hall. Greatest Cold. 4 Feet.	Shirenewton Hall. Greatest Cold. On Grass.	Shirenewton Hall. Difference.	Beeston Fields. Greatest Cold. 4 Feet.	Beeston Fields. Greatest Cold. On Grass.	Beeston Fields. Difference.
	°	°	°	°	°	°
25	30.0	30.0	0.0	28.8	21.8	− 7.0
26	30.0	29.0	− 1.0	28.2	23.0	− 5.2
27	22.0	20.0	− 2.0	23.8	21.0	− 2.8
28	20.0	20.0	0.0	25.3	21.7	− 3.6
29	21.0	21.0	0.0	32.0	25.2	− 6.8
30	25.5	24.5	− 1.0	23.7	10.7	− 13.0
Dec.						
1	31.0	31.0	0.0	26.8	26.0	− 0.8
2	31.0	31.7	+ 0.7	31.9	32.5	+ 0.6
3	31.4	30.0	− 1.4	33.4	31.7	− 1.7
4	34.0	34.0	0.0	34.7	34.0	− 0.7
5	34.0	33.0	− 1.0	36.7	36.2	− 0.5
6	26.0	25.0	− 1.0	35.6	34.7	− 0.9
7	30.0	29.0	− 1.0	29.8	23.0	− 6.8
8	30.0	30.0	0.0	32.2	30.0	− 2.2
9	23.0	22.0	− 1.0	28.4	29.0	+ 0.6
10	29.0	30.0	+ 1.0	28.4	30.7	+ 2.3
11	29.1	30.2	+ 1.1	29.3	30.0	+ 0.7
12	24.0	23.0	− 1.0	27.3	28.0	+ 0.7
13	24.9	25.0	+ 0.1	27.4	23.0	− 4.4
14	19.2	19.0	− 0.2	13.4	10.2	− 3.2
15	19.5	16.9	− 2.6	15.8	11.8	− 4.0
16	24.0	19.5	− 4.5	27.5	23.3	− 4.2
17	26.0	26.0	0.0	30.8	27.7	− 3.1
18	26.0	24.0	− 2.0	28.3	27.4	− 0.9
19	26.5	28.0	+ 1.5	24.0	28.0	+ 4.0
20	18.0	16.9	− 1.1	19.4	22.8	+ 3.4
21	23.8	23.6	− 0.2	12.9	8.2	− 4.7
22	20.9	19.8	− 1.1	4.9	4.7	− 0.2
23	24.0	24.1	+ 0.1	11.0	10.7	− 0.3
24	29.5	28.0	− 1.5	25.5	24.8	− 0.7
25	23.0	24.5	+ 1.5	22.9	23.0	+ 0.1
26	25.4	28.2	+ 2.8	23.7	24.8	+ 1.1
27	31.0	29.0	− 2.0	28.0	27.3	− 0.7
28	29.0	27.0	− 2.0	27.8	28.0	+ 0.2
29	28.0	27.0	− 1.0	28.0	27.8	− 0.2
30	20.9	20.7	− 0.2	22.8	19.8	− 3.0
31	17.9	17.7	− 0.2	21.0	21.6	+ 0.6
1891. Jan.						
1	20.7	19.7	− 1.0	24.9	24.4	− 0.5
2	26.8	27.8	+ 1.0	30.7	28.4	− 2.3
3	27.6	27.6	0.0	23.4	24.5	+ 1.1
4	33.1	26.7	− 6.4	28.5	29.0	+ 0.5
5	25.1	22.7	− 2.4	28.7	26.1	− 2.6
6	24.0	21.7	− 2.3	24.8	21.7	− 3.1
7	19.5	17.9	− 1.6	20.5	15.8	− 4.7
8	22.3	14.9	− 7.4	21.0	17.6	− 3.4
9	27.3	27.3	0.0	21.2	20.0	− 1.2
10	26.9	26.9	0.0	24.0	19.3	− 4.7
11	22.7	15.7	− 7.0	18.8	14.6	− 4.2
Min.	17.9	14.9		4.9	4.7	

Shirenewton Hall, Chepstow. E. J. LOWE.

Destruction of Fish in the Late Frost.

IN passing across the small suspension bridge over the canal on the north side of the Regent's Park yesterday morning, I observed a number of white flakes on or in the floating ice. On looking more closely, I saw they were dead fish, which apparently were frozen into the ice. The canal was nearly covered with ice, and the fish were scattered in the latter for as far as I could see. I think I may safely say there were on the average three fish for every two square yards; not seldom I saw three or four lying within one square yard. They were roach, or one of the fish resembling it; more commonly 3 or 4 inches long, occasionally larger or smaller. Very likely small fry and minnows were present, but these I could not distinguish from where I stood. Of course it is well known that fish are killed during a long severe frost, but I never saw such wholesale destruction, and it led me to wonder whether in any case such a cause may have acted in the geological history of the globe. Perhaps I am asking a question which only displays my own ignorance, but can anyone tell me how it is with the fish in countries like Siberia? Do they desert those parts of the rivers which are frozen over, or are the currents more rapid, so as to transfer air beneath the ice from unfrozen parts, or, as in some glacier-streams, are they altogether absent?

T. G. BONNEY.

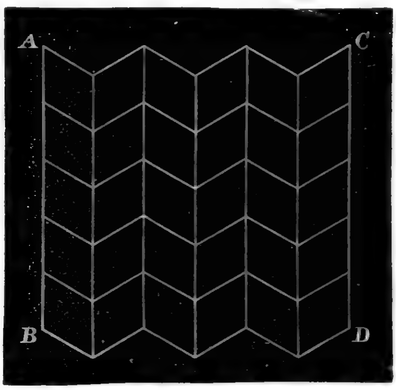
23 Denning Road, N.W., January 26.

Bees' Cells.

IN writing a paper upon the cells of hive-bees for the *Nineteenth Century* some months ago, a property of these cells occurred to me, which seems to be sufficiently interesting to be worth noting down.

The property is this. Typical bee cells may be manufactured entirely out of bee rhombs; that is, out of rhombs such as those by which the terminations of the cells are formed. Moreover, for the manufacture these rhombs will be required in dozens or half-dozens.

Suppose, for instance, that I have three dozen of such rhombs. Take thirty of them, and lay them upon a flat surface, in contact with each other, as in the figure. Conceive them to adhere into one sheet, A B C D, or (which comes to the same thing) let a piece of paper, A B C D, be shaped as in the figure. Now



let the figure A B C D be bent round a hexagon, so as to form a hexagonal prism, the edges A B and C D being thus made to coincide. The prism will have open ends, and we have six rhombs left with which to close them, three for each end. Now bisect the prism by a plane perpendicular to its axis, and we shall have two typical bee cells.

The same thing will be true of any number of dozens or half-dozens.

This geometrical construction has, of course, nothing to do with the question, How does the bee build her cells?, but it is curious, and (so far as I know) has not been noticed previously.

Rose Castle, January. H: CARLIOL:

The Crowing of the Jungle Cock.

IN the Proceedings of the Zoological Society, 1890, p. 48, Mr. Bartlett makes the following statement on the subject of the crowing of the jungle fowl: "There can be no doubt that the origin of our domestic fowls must be attributed to the wild jungle fowls of Asia, but none of the known wild species are ever heard to utter the fine loud crow of our domestic cock." I can recall very distinctly an exception to this statement. When living in Timor, at my hut on the Fatunaba hills, I heard—more than once—the crow of the jungle fowls which used to frequent a bit of very dense scrub not far from our camp. I was first led early one morning to the knowledge of the presence of these birds in my vicinity, by hearing (with more than ordinary satisfaction) a call which was the counterpart of the well-known cadences of the barn-door cock; but it was, if I may so represent it, considerably thinner in volume, more wiry, and higher pitched than his. I hastened after this first chanticleer, and succeeded in getting a perfect sight of and a shot at him, but without securing my victim, deeply to my disappointment, as I can well remember, for it would have been just then a most welcome accession to an empty larder.

HENRY O. FORBES.

Canterbury Museum, Christchurch, New Zealand, October 29, 1890.

Throwing-Sticks and Canoes in New Guinea.

IN reply to my friend Mr. H. O. Forbes's letter to NATURE of January 15 (p. 248), I would like to say that I admit that my statement regarding the occurrence of the throwing-stick in South-east New Guinea is misleading. When I wrote the paper from which Mr. Forbes quotes, I was unaware that the Papuan throwing-stick was confined to a portion only of Kaiser Wilhelm's Land, and that its use was unknown in the British Protectorate. It is almost impossible to find out the exact geographical distribution of Papuan objects, either from the accounts of travellers or from museum specimens.

With regard to the canoes, in the paragraph preceding that quoted by Mr. Forbes I refer to the fact that down the south-east coast of New Guinea "the canoes have only a single outrigger," and thereby admit that it is indigenous to New Guinea. My point was, and still is, that the single outrigger has been introduced into Torres Straits by South Sea men, and that as far as the western tribe is concerned it was first introduced by my friend Ned Ware (Uea, Loyalty Islands). I believe it can be shown that the particular form of outrigger in question differs in minor details from the "New Guinea model."

May I take this occasion to express the hope that Mr. Forbes will publish the anthropological notes which he must have accumulated during his three years' residence in the country? As he has travelled up and down the coast, he must be in a position to give us some of that precise information as to the special characters and manufactures of the various tribes which is now lacking.

A. C. HADDON.

Royal College of Science, Dublin.

THE SUPPOSED OCCURRENCE OF WIDE-SPREAD METEORITIC SHOWERS.

IN a recent paper[1] it was shown that the prevalent belief in widespread meteoritic showers, whether true or untrue in general, was, as regards the Desert of Atacama, on the western coast of South America, based on insufficient evidence: that in one case the wide-spreading of a shower was undoubtedly caused by a mere interchange of labels; in another by misinterpretations of the statements relative to a locality; that while the places were widely separated from which other fragments, belonging to a single type, had been brought, they were on definite and dangerous lines of traffic along which similar fragments are known to have been previously carried on the backs of capricious mules; further, that the statement that "meteorites were found at every step in the Desert" had been made at a time when almost the whole of the Desert was untrodden and unexplored; finally, that the latest explorations did not suggest the existence of meteoritic masses at small distances from each other over any large area of that part of South America.

[1] *Mineralogical Magazine*, vol. viii. p. 223; NATURE, vol. xli. p. 108.

Attention was thus directed to the following facts: (1) only nine falls of meteoric iron are known to have been observed during the last 140 years; (2) not more than two masses of iron have been seen to fall simultaneously; (3) the largest (Nejed) weighed only 131 pounds; (4) falls of stones, sometimes thousands in number, have been often observed; (5) the largest authenticated separation for the individuals of a single fall, whether of stone or iron, has been sixteen miles; (6) in some regions meteoric iron will endure for ages before rusting completely away; (7) the discovery of numerous masses of iron in certain districts may be due to the circumstance that the ground has been unexplored, or at least uncultivated, during many centuries.

It has already been pointed out by Prof. Daubrée that the small dispersion of a meteoritic fall is suggestive of the entry into the terrestrial atmosphere of only a single mass, afterwards fractured by the enormous resistance of the air; for the individuals of a swarm of meteoritic masses, various in form and size, would experience resistances so different in magnitude, that the residual masses would probably be scattered over areas of the earth's surface much larger than those which have characterized any of the observed falls.

The converse is not necessarily true, for a wide separation of the individuals of a meteoritic fall might conceivably be due to successive fractures of a single primitive mass. And it may be worthy of remark that, but for the differential action of the atmospheric resistance, the dispersion consequent on the breakage of the primitive mass would be very small, however numerous the so-called explosions, since each fragment would retain the enormous velocity belonging to it as part of the original moving body: that by simple division a single mass, or a swarm, could succeed in dropping fragments at distant points of the line of flight is mechanically impossible.

Remembering the close similarity in structure and material of the single representatives of meteoritic falls to those which have been picked up in hundreds, or even thousands, and the identity in character of the luminous and also of the detonatory phenomena in the different cases, it is difficult to grant that the enormous disparity in the numbers of the individual masses, which have been found after different meteoritic falls, is satisfactorily explained by any possible diversity in structure or velocity of singly entrant blocks.

The evidence for and against the natural occurrence, over a large area, of meteoritic masses belonging to a single and well-defined type, is thus not without scientific interest: firstly, as throwing light on the possible occurrence, within comparatively recent times, of large meteoritic showers, such as are not known to have been actually observed; and secondly, as bearing on the true relationship of meteorites and shooting-stars.

The occurrence of widespread meteoritic showers has been regarded as established by the distribution of meteoritic masses, not only in the Desert of Atacama, but also in Africa and Mexico. It is true that masses of meteoric iron, rarely more than one or two hundred pounds in weight, have also been found dispersed in considerable numbers over the extensive territory of the United States of North America; but it has been as yet impossible, by investigation and comparison of the mineralogical characters of those masses, to obtain any trustworthy evidence that distant individuals have ever belonged to one meteor.

The evidence relative to a wide distribution in Africa of masses belonging to a single type is extremely slight: when examined, it appears to be based solely on the brief statement made by Captain Alexander, "that there were abundant masses of iron scattered over the surface of a considerable tract of country." It is practically certain that Alexander never saw the masses, and that the above information was given to him by a native; in any case, it is not suggestive of a distribution over an extraordinary area.

But the evidence relative to Mexico is of a much more voluminous character, and is deserving of the closest attention. Mexico is remarkable beyond any other part of the earth's surface for the number and magnitude of the masses of meteoric iron found within its borders: it has been generally assumed that widespread showers are necessary to the explanation of their occurrence. Prof. Lawrence Smith came to the conclusion that masses belonging to a single meteor were distributed over hundreds of miles of country in Northern Mexico, and his conclusion has been generally accepted. Prof. Whitney, and also Señor Urquidi, regarded it as possible that a whole series of iron masses, for a distance of more than a thousand miles through Mexico and the United States, were the result of a single fall: Huntington states that the fact of certain masses having been "found in places so remote from each other does not seem to preclude their having belonged to one individual, since the Rochester meteorite was seen to pass over the States of Kansas, Missouri, Illinois, Indiana, Ohio, and is supposed to have passed over Pennsylvania and New York, and thence out to sea, dropping fragments in its course. It therefore is possible that at some remote period an enormous iron meteorite may have passed over the entire breadth of the United States, the main mass reaching Mexico, but large fragments breaking off and falling during its passage across the country." Prof. Barcena is further of opinion that "the peculiar property, difficult of explanation, which the Mexican soil has in attracting the meteoric irons is even noticed at present" (in the attraction of shooting-stars). We may remark that only a single fragment of the above-mentioned Rochester meteorite could ever be found.

The following questions thus present themselves for consideration:—

(1) What meteoritic falls have been actually observed in Mexico?

(2) In what localities are meteoritic masses said to have been met with?

(3) Is our knowledge of the distribution in these localities at all precise?

(4) Is the climate favourable to the preservation of meteoritic masses?

(5) Are any falls of remote date?

(6) Have any masses been transported from their place of fall?

(7) Had the ancient Mexicans any skill in the transport of heavy blocks?

(8) Had they any respect for meteorites as bodies fallen from the sky?

(9) Is there any evidence of the wide dispersion of masses belonging to a single type?

(10) If so, is the dispersion celestial in origin or due to the action of man?

1. Only seven meteoritic falls are known to have been actually observed in Mexico, and in no case were more than three fragments found. During the same interval of time, larger showers and more numerous falls have been observed within the British Isles, which are comparatively small in area.

2. Meteorites of unobserved fall have been found only in the following States:—I. Coahuila and Nuevo Leon; II. Chihuahua; III. Sinaloa; IV. Durango; V. San Luis Potosi; VI. Zacatecas; VII. Mexico and Morelos; VIII. Oaxaca; IX. Guerrero.

3. It is the supposed distribution of meteoric irons in the Bolson de Mapimi, Northern Mexico, that has been largely relied upon as illustrating the occurrence of widespread showers. It will be found, however, that, until many years after the publication of the papers of Shepard and Lawrence Smith, the Bolson was in the possession

of the dreaded Comanches and Apaches, and no trustworthy information relative to the greater part of that district can have been available.

4. Mexico is a lofty and extensive table-land—the capital being 7600 feet, Durango 6630 feet, Chihuahua 4600 feet, and El Paso del Norte 3800 feet, above the sea-level. Hence the air is exceedingly dry, and the climate unusually favourable to the preservation of meteoric iron.

In the discussion of the meteorites of Atacama, it was proved that, even in the still drier atmosphere of that region, meteoric stones could only escape disintegration for a very limited interval of time.

5. The history of some of the Mexican masses goes back to very distant times. One was found in an old tomb in the ruins of Casas Grandes: it was wrapped in the same kind of cloth as envelops the bodies found in the adjacent tombs, and must have been buried there before Mexico was conquered by the Spaniards. In the cleft of another mass is to be seen an ancient chisel of "copper," the material used by the Aztecs for their arms, axes, and tools in general. Two small worked specimens, belonging to the Aztec period and made of meteoric iron, are exhibited in the Museum of Mexico. At least one of the masses in the south of Chihuahua was known long before the latter part of the sixteenth century.

It is quite possible that, with the exception of the small piece which fell in 1885, not a single one of the Mexican iron masses has fallen since the Spaniards obtained possession of the country. The masses thus seem to be nearly permanent in their material, and may have been lying for any number of centuries in the region where they have been found.

6. Some of the masses have certainly been transported from their place of fall. The one found in the tomb at Casas Grandes was clearly not *in situ;* and yet it was so large that twenty-six yoke of oxen are said to have been used to haul it from the tomb to the village. The masses met with at Saltillo, Potosi, and Cerralvo, were discovered in forges in use as anvils; the large masses of San Gregorio, Concepcion, Descubridora, Charcas, Zacatecas, and Yanhuitlan, are all known to have been moved by the Spaniards, some of them for considerable distances. The possibility of similar removals prior to the conquest of Mexico cannot be entirely disregarded.

7. The Aztecs were capable of moving immense blocks of material when they wished. The remarkable carved Calendar-Stone or Sun-Stone, preserved in Mexico, weighs twenty-four tons; when extracted from the quarry it must have weighed forty tons: yet it was transported by the Aztecs many leagues across a broken country intersected by watercourses and canals.

8. The inhabitants of both the Old and New World have regarded meteorites as objects for reverence and worship. In the Aztec creed the creation of mankind was associated with the fall of a stone from the sky. The companions of Cortes are said to have seen a stone at Cholula which had fallen, enveloped in a cloud of fire, upon the pyramid: Lumbier refers to it as placed on the summit of the pyramid for the purpose of worship. The respect of the ancient Mexicans for native iron, whether known to be of meteoric origin or not, is illustrated in the careful envelopment and burial of the large mass of iron at Casas Grandes. One of the large Chihuahua masses was believed by the Mexicans of the sixteenth century to have been deposited by the Deity for their use as a landmark. Hence it would have been far from surprising if a religious and skilful people, like the Aztecs, had transported meteoritic masses from their original sites for the purpose of worship: the large blocks of stone on Salisbury Plain furnish a good example of the transportation of heavy blocks of stone for great distances by a still more ancient race.

9–10. In very few cases is there any evidence at all of wide distribution of masses belonging to a single type. The large masses of San Gregorio, Concepcion, and Chupaderos, all in Chihuahua, are very similar in their external characters, and probably belong to a single fall: the extreme separation is about sixty-six miles, but the two former have undoubtedly been transported from some distance to their present sites. Masses belonging to a single type, and probably the results of a single fall, have been brought from widely-separated places in Coahuila and its neighbourhood; masses belonging to another type have been brought from places in the Valley of Toluca, and also at considerable distances therefrom: there is very strong evidence, however, that in these two cases the dispersion has been artificial. In the remaining cases there is nothing suggestive of widespread showers.

The following is the result of an investigation relative to the discoveries of meteoric irons in the various States:—

I. *Coahuila and Nuevo Leon.*—The known meteoric masses of Coahuila are:—(1) The masses which Shepard designated by the name Bonanza. (2) Those collected by Dr. Butcher. (3) Those brought from Santa Rosa, but of which the previous history is matter of inference. (4) The so-called Sancha Estate mass. With the above must be considered (5) the Fort Duncan mass (from the Texas side of the Rio Grande); and conveniently also (6) the Potosi mass, and (7) the Cerralvo mass, both from Nuevo Leon.

After a minute comparison of various statements, it appears certain that the localities designated by Shepard and Butcher are really identical, and that all the masses brought from Santa Rosa, and the one from the so-called Sancha Estate, had been previously transported from this locality to Santa Rosa, the nearest town. Until recent years iron anvils were extremely costly articles in Northern Mexico, whence it arose that the mass of meteoric iron found about 1837 were distributed for use in forges. The Fort Duncan mass, though found 140 miles away in 1882, is of identical characters, and is probably part of the same meteor. Much larger masses were long ago dispersed from Santa Rosa to greater distances than Fort Duncan, though in the opposite direction. Santa Rosa was a town through which much traffic passed from the eastern side of the Mexican table-land to Texas; and Fort Duncan is the place where the river was crossed. The Potosi and Cerralvo masses were both found in forges, and have neither of them been compared with those of Coahuila: they had obviously been transported, possibly either from Santa Rosa or Catorce. Their carriage from either place would present no practical difficulty.

II. *Chihuahua.*—The meteoric irons of Chihuahua, mentioned in literature, belong to one or other of the following:—(1) the Casas Grandes mass; (2) a mass exhibited in 1876 at the United States International Exhibition; (3) the group between Presidio del Principe and Cuchillo Parado; (4) the group near Huejuquilla or Jimenez.

The first two masses will probably prove to be identical, for while the former has been lost since 1873, and the history of the latter previous to the appearance at the Exhibition of 1876 is not yet traced, the descriptions of the two masses are not inconsistent with each other. The Casas Grandes mass was estimated to weigh 5000 pounds, and was found in an old tomb. The scientific examination of the Exhibition-mass is not yet made, so that no comparison with the other Chihuahua masses is practicable.

No particulars of the group said to be between Presidio del Principe and Cuchillo Parado have yet been published.

The remaining Chihuahua masses, termed the Huejuquilla group, comprise the following:—

A mass now at San Gregorio, estimated to weigh 11 tons.

A mass now at Concepcion, estimated to weigh 3 tons.

A large mass said to have been seen many years since at Rio Florido.

Two masses now at Chupaderos, estimated to weigh 15 and 9 tons respectively.

A small fragment brought from Sierra Blanca.

A small fragment brought from Tule.

The first of these, the San Gregorio mass, is mentioned in works published in the years 1619 and 1629: it was already regarded as a great curiosity, and everyone passing along the road to New Mexico went to look at it. There was at that time a tradition among the Indians that it had accompanied their forefathers on their migrations from the north to people Old Mexico, and was to serve as a memorial of that great event. It is doubtless identical with the large Durango mass mentioned by Humboldt, who, in misapprehension, stated that it was located near Durango City. Such a mass has been unsuccessfully searched for again and again in that neighbourhood since Humboldt's time. The mistake arose in a very simple way: at that date the Province of Durango included the district in which the San Gregorio mass is lying: since the independence of Mexico has been established, San Gregorio has been assigned to the State of Chihuahua, which was made distinct from that of Durango. Hence it came about that the mass which had once been so well known that it was called the Durango iron (not, as Humboldt supposed, because it was near the city of that name, but from the locality being in the province of Durango) could later on not be recognized by its old name. The mass was moved to San Gregorio itself, according to one account, from El Morito, about 2 leagues distant.

The second large mass, now at Concepcion, was moved to that place in 1780 from Sierra de las Adargas, many leagues away.

There is very strong evidence that the Rio Florido mass is really identical with that of Concepcion.

The two enormous masses, still at Chupaderos, are probably where they fell; they seem to have been known many years before they were mentioned by Bartlett in 1852.

Only one small fragment of one of a number of large masses found before 1784 in the Sierra Blanca has been preserved to our day.

The small fragment brought from Tule is without history or description, so that no connection with the above masses can be asserted.

III. *Sinaloa.*—Only one mass of iron is known to exist in this State, but it is said to be of enormous size, 12 feet long, 6 feet broad, and 4 feet high: no comparison with the other Mexican meteorites has yet been made.

IV. *Durango.*—Four areas of Durango are represented by meteoritic masses—namely, La Plata, Guadalupe, Mezquital, and Bella Roca. The mass found at the first place was destroyed at the beginning of the century, and was never investigated; those of the three remaining areas belong to types which are widely different from each other. Strong evidence is brought forward that the Durango mass of Karawinsky was obtained from Guadalupe: the Cacaria mass belongs to the same area.

V. *San Luis Potosi.*—Only two areas are represented in San Luis Potosi, and one of these, Charcas, only by a single mass. It is almost certain that the Charcas mass was transported to that place from the other area—namely, that of Catorce. The Venagas mass of Lawrence Smith is proved to be identical with the Descubridora mass found near Catorce.

VI. *Zacatecas.*—Only one mass has been found in this State, and it has distinctive characters.

VII. *Mexico and Morelos.*—Most of the masses found in the former State have been got from the neighbourhood of Xiquipilco in the Valley of Toluca: they were carried to various towns as articles of merchandise, and were largely used for agricultural implements; hence their wide-spreading is almost entirely artificial. As regards the masses of Cuernavaca, Ameca-Ameca, and Los Amates, mentioned in 1889, there is no evidence that they have been examined and found identical in character with those of Xiquipilco, which were known before 1776; and even if they prove to be identical in character, there is as yet no evidence that they may not have been transported from that locality. The localities Tejupilco and Sizipilec are due to incorrect labels.

VIII. *Oaxaca.*—Only one mass, that of Yanhuitlan, has been found in this State. Another locality, Cholula, is merely a mistake for Tepos-colula, which is close to Yanhuitlan. A further locality, Chalco, is itself a mistake for Cholula. The fragments known as Misteca are merely pieces of the Yanhuitlan mass.

IX. *Guerrero.*—Only one mass, a very small one, is said to have been found in this State, and of this there are grave doubts as to its meteoric origin.

We may summarize the above as follows:—

In each of the States of Zacatecas, Oaxaca, and Guerrero, at most a single mass of meteoric iron has been found, and there is absolutely nothing to suggest that they do not represent independent falls.

In Sinaloa, likewise, only a single mass has been met with, and its characters have not been determined: a suggestion of a relationship with another group would rest on the slight fact that the site of this extremely large mass is in a straight line with two other sites where large masses are now lying.

In San Luis Potosi two localities are recognized, but there is a strong probability that the Charcas mass, which has undoubtedly been transported to that town from a distance, was brought from the other locality, Catorce.

In Durango four or five distinct localities are known, but the characters of the only masses which have been examined point unmistakably to the falls of distant masses having been independent of each other.

In Mexico there has undoubtedly been a large shower of limited dispersion in the Valley of Toluca: the three remaining masses from Mexico and Morelos have not been examined, and are very small and portable: even if they have not been transported, they may be found on examination to present characters which will differentiate them from the masses of the Toluca shower.

From Coahuila many masses have been got, but it is extremely probable that all of them were brought from a single district of very small area: the two Nuevo Leon masses had obviously been transported, possibly from Coahuila or San Luis Potosi.

In Chihuahua three or four areas are represented; but only those of the Huejuquilla group have been examined, and that in a very incomplete way: the recognition of the singleness of fall of that group depends almost entirely on the general similarity of appearance of the large masses. If the masses really belong to a single fall, as the available information makes most probable, the maximum dispersion would have been 66 miles if the masses had fallen where they now lie; but one of the terminal masses is known to have been transported on one occasion by the Spaniards for a distance of about two leagues, and, according to an old tradition, it had previously been brought from the north by the Indians.

There appears to be no satisfactory evidence of the occurrence of such widespread meteoritic showers as have been assumed by Smith, Whitney, Urquidi, and Huntington.

Full information relative to this subject, with three maps of the region, will be found in the last number (42) of the *Mineralogical Magazine.*

L. FLETCHER.

HENRY BOWMAN BRADY.

HENRY BOWMAN BRADY was born on February 23, 1835, at Gateshead. His father, an esteemed medical practitioner of that place, belonged to the Society of Friends, and retained to the end the dress and manner of conversation of that body. The father's house, for many years the home of the son, was one of those charming Quaker abodes where strength and quietude sit side by side, and where homely plenty and orderly preciseness hide for a moment from the stranger the intellectual activity which is filling the place. Though the son, when I knew him, had abandoned the characteristic dress and speech of the Society, without, however, withdrawing from the body, the influences of his surroundings moulded his character, making him singularly straightforward and free from manner of guile.

After an ordinary school career spent in Yorkshire and Lancashire, and an apprenticeship under the late Mr. T. Harvey, of Leeds, and some further study at Newcastle in the laboratory of Dr. T. Richardson, which may be considered as the forerunner of the present Newcastle College of Science, he started at business in that city as a pharmaceutical chemist in 1855, while yet a minor. That business he conducted with such ability that in 1876 he felt able to resign it to Mr. N. H. Martin, and to devote the whole of his time to scientific work.

He contributed to science in two ways—one direct, the other indirect. Of the many scientific movements of the last thirty years or so, one not of the least remarkable has been the scientific development of the pharmaceutical chemist. Into that movement Brady threw himself with great vigour, especially in his earlier years. He was for many years on the Council of the Pharmaceutical Society, and the progress of that body was greatly helped by his wide knowledge of science and of scientific men and things, as well as by his calm and unprejudiced judgment.

His more direct contributions to science were in the form of researches in natural history, more especially on the Foraminifera. His first publication seems to have been a contribution in 1863 to the British Association as a report on the dredging of the Northumberland coast and Dogger Bank; his last was a paper which appeared only a short time ago. Between these two he published a large number of researches, including a monograph on Carboniferous and Permian Foraminifera, an exhaustive report on the Foraminifera of the *Challenger* Expedition, as well as monographs on Parkeria and Loftusia, and on Polymorphina, in which he was joint author.

By these works he not only established a position, both in this country and abroad, as one of the highest authorities on the subject, but, what is of more importance, largely advanced our knowledge. Every one of his papers is characterized by the most conscientious accuracy and justice; and though his attention was largely directed to classification and to the morphological points therein involved, his mind, as several of his papers indicate, was also occupied with the wider problems of morphological and biological interest which the study of these lowly forms suggests. I have myself often profited by his wide knowledge and power of accurate observation in discussing with him questions of this kind arising out of his studies, and learning from him views and opinions which, to his critical mind, were not as yet ripe enough for publication.

The leisure of the last fifteen years gave him opportunity for travel, and he visited various parts of the world, utilizing many of his journeys—notably one to the Pacific Ocean—in the collection and study of Foraminifera. Some of these travels were undertaken on the score of health, to avoid the evils of an English winter, for he was during many years subject to chronic pulmonary mischief.

During his last journey for this purpose—one to the Nile in the winter of 1889-90—he met with difficulties, and failed to receive the benefit from the change which he had secured on former occasions. During the last two or three years, and especially during the last year, his condition gave increasing anxiety to his friends; the malady against which he had so long struggled seemed to be beating him at last; and we heard with sorrow rather than with surprise that the fierceness of the recent cold had conquered him. Settled for the winter at Bournemouth, and full of cheerful hopes for the coming summer, he succumbed to a sudden attack of inflammation of the lungs, and died on January 10, 1891.

Science has lost a steady and fruitful worker, and many men of science have lost a friend and a helpmate whose place they feel no one else can fill. His wide knowledge of many branches of scientific inquiry, and his large acquaintance with scientific men, made the hours spent with him always profitable; his sympathy with art and literature, and that special knowledge of men and things which belongs only to the travelled man, made him welcome also where science was unknown; while the brave patience with which he bore the many troubles of enfeebled health, his unselfish thoughtfulness for interests other than his own, and a sense of humour which, when needed, led him to desert his usual staid demeanour for the merriment of the moment, endeared him to all his friends.

M. FOSTER.

NOTES.

PROF. VICTOR HORSLEY AND MR. FRANCIS GOTCH have been appointed Croonian Lecturers to the Royal Society for the present year. They have chosen as their subject, "The Mammalian Nervous System; its Functions and their Localization as determined by an Electrical Method." Thursday, February 26, is the date fixed for the delivery of the lecture.

A VERY valuable addition has just been made to the Kew Herbarium by the purchase of an extensive collection of dried plants from West Sze-chuen and the Tibetan frontier, at elevations of 9000 to 13,500 feet, lately brought home by Mr. A. E. Pratt, who travelled and collected ornithological and other specimens of natural history at the expense of Mr. J. H. Leech. The botanical specimens are excellent, and promise many novelties of Himalayan affinities.

A CIVIL LIST pension of £150 has been granted to Lady Burton, the widow of the eminent traveller and Oriental scholar. The movement for this recognition of Sir Richard Burton's brilliant achievements originated with the Council of the Royal Geographical Society, who were supported by the Royal Asiatic Society, the British Association, and the Anthropological Institute.

A LETTER from the Palais des Académies, at Brussels, announces the death of Lieut.-General Liagre, the Permanent Secretary of the Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. He was born at Tournai, in 1815, and died at Ixelles, on the 13th of this month.

WE are glad to see that the *Speaker* has taken up the subject on which we have had so much to say lately—that of the proposed South Kensington and Paddington Subway Railway. In the current number it has an able and most interesting article, directing attention to the fact that the introduction of electricity as a motive power for trains has created a new peril for institutions devoted to the teaching of physical science. It points out that in the neighbourhood of an electric railway, such as that now running in South London, a moderately advanced course in experimental physics could not be given, and that even comparatively rough apparatus would be affected. The course marked out for the proposed line runs under one of the physical laboratories of the Royal College of Science, and is separated by the breadth of the road only from the other; and it passes within a few yards of the City and Guilds Central Institute for Technical Education. "That the line might be in some respects convenient," says the *Speaker*, "is quite possible; that if the powers to employ electricity are granted and used as at Stockwell, existing physical laboratories will be rendered useless, is certain. The South London line is surrounded by an iron tube which acts as a 'magnetic screen,' and serves to diminish its magnetic effects on external objects. No such precaution is, as far as we are aware, to be adopted at Kensington, and in this respect, at all events, the proposed line is likely to be the more injurious of the two. To the laboratories of the Royal College of Science teachers are brought by Government funds from all parts of the country. Every summer two hundred come from far and near to hear lectures and to go through a course of laboratory work. We believe that one of the main objects of the College is to raise the standard of knowledge among those on whom many a small town has to depend for scientific instruction. It is improbable that the laboratories could be removed from South Kensington, for their connection with the great collection of scientific apparatus in the Museums is essential. The City and Guilds Central Institute has been built at a cost of £100,000. After some years of contest with our English lethargy it has reached success. The electrical department is full. Is it reasonable that a commercial company should be allowed to acquire powers to ruin the physical laboratories of two such institutions by the employment of electric traction?"

WITH respect to our report of the meeting of the Royal Society of Edinburgh of January 5 (p. 287), Sir William Thomson writes to us that a statement attributed to him is obviously incorrect, and that six lines and three words after the words "Sir William Thomson, however," near the end of the report, should be deleted. "It is obvious," he says, "that any upward current of air which could carry drops of water 40 or 50 feet up from the sea is more than amply sufficient to supply what a bird needs for soaring."

AT the meeting of the International Congress of Hygiene and Demography there will be a special section for the discussion of questions connected with the relations of the diseases of the lower animals to those of man. The section proposes to consider, amongst other subjects, the infectious, contagious, parasitic, and other diseases communicable from animals to man, and *vice versâ*; the methods of the propagation of diseases affecting mankind by means of animals and animal products; the infection of meat, milk, and other comestibles; and the restrictions to be placed upon the sale of infected food and the movement of infected animals. On each of these questions papers will be obtained from prominent British and Continental authorities as the basis of debates. The President of the section will be Sir Nigel Kingscote, Chairman of the Board of Governors of the Royal Veterinary College. Prof. G. T. Brown and Dr. E. Klein, F.R.S., will act as Vice-Presidents.

THE *Kew Bulletin* for January opens with an interesting section on West African bass fibre, the prospects of which in the English market seem to be remarkably good. Writing on a sample sent to them from the Royal Gardens, Kew, Messrs. Ide and Christie, on October 10, 1890, expressed the opinion that, if properly selected and cleaned, the fibre might sell at £25 per ton in London, a figure which, they thought, would leave a handsome profit to the producer. Writing again, on October 24, Messrs. Ide and Christie stated that a few bales had been sold, and had fetched the extreme price of £42. They added, "We scarcely expect this price would be maintained for substantial quantities, but for fibre of equal merit, the immediate outlook would seem to indicate that £35 or £40 might be the range of value."

THE same number of the *Kew Bulletin* contains a paper on Chinese ginger, of which it is said in China, where it grows, that it never flowers. Dr. H. A. Alford Nicholls, writing from Dominica, West Indies, on July 5, 1890, and Mr. Charles Ford, writing from Hong Kong, on July 10, 1890, both state that the plant has flowered under their care. Mr. W. T. Thiselton Dyer contributes a paper in which he gives a full and most lucid account of the production of seed and seminal variation in the sugar-cane. Referring to the work done by Messrs. Harrison and Bovell in the West Indies in connection with this subject, Mr. Dyer points out that it was undoubtedly anticipated in Java. He does ample justice, however, to the independent labours of these gentlemen. Noting that their discovery has been termed "accidental," he says:—"Even if true, that is no demerit. Most discoveries in some sort are accidental. They often lie, so to speak, under our eyes, and only reveal their significance to those who are ready to appreciate it. This Messrs. Harrison and Bovell did, and the greatest credit is due to them for the fact. All that Kew has done in the matter was to put it on record, and give it a scientific verification. For my own part, I have no doubt, looking at the whole history of the improvement of cultivated plants, that the discovery, for so I think it, of Messrs. Harrison and Bovell, has been the starting-point of a new era in the cultivation of the sugar-cane."

MR. CLEMENT WRAGGE, the Government Astronomer of Queensland, has sailed from Brisbane for the New Hebrides, where he is to superintend the erection of some Observatories on the islands. They will be maintained at the expense of the Queensland Government.

AT the next International Geographical Congress, which will be held at Berne from August 10 to 15, the following will be the principal subjects considered:—(1) Technical geography (mathematical geography, geodesy, topography, cartography, photography, &c.). Under this head will be discussed the first meridian and the spelling of geographical names. (2) Physical geography (hypsometry, hydrography, meteorology, variation of climate, the boundary-line of the ice, terrestrial magnetism, botanical, zoological, and geological geography, volcanoes, earthquakes, ethnography, anthropology, and archæological geography). (3) Commercial geography. (4) Travels and discoveries. (5) The extension of geographical knowledge. Those who propose to contribute papers should write to Dr. Gobat, Berne, not later than March 1.

MR. R. BOWDLER SHARPE has received from America an interesting testimonial, congratulating him on the completion of the thirteenth volume of the British Museum "Catalogue of Birds." It is signed by members of the American Ornithologists' Union, and other American naturalists. They express the warmest appreciation of Mr. Sharpe's labours as an ornithologist, especially of his work in connection with the classification and the nomenclature of the Passeres. Among the signatures we note those of G. N. Lawrence, J. A. Allen, Elliott Coues, R. W. Shufeldt, C. B. Cory, Robert Ridgway, and G. Brown Goode.

DR. HUGH ROBERT MILL is delivering in Edinburgh a course of twelve lectures on "The Earth and Man: a Study in Advanced Geography." The lectures are being given under the auspices of the Scottish Geographical Society, and are intended to illustrate a memorial recently presented by the Society to the Universities Commission, asking that lectureships on geography should be founded in the Scottish Universities. The attendance is most encouraging, 104 tickets having been taken.

SOME time ago M. Berthelot, judging from a text of the eleventh century, formed the opinion that the word "bronze" was derived from "Brundusium," or Brindisi. According to *La Nature*, this view has been confirmed by the discovery of a passage in a document of the time of Charlemagne, where reference is made to the "composition of Brundusium":—copper, two parts; lead, one part; tin, one part. It would appear that at Brundusium bronze was in ancient times manufactured on a great scale.

THE Report of the Honorary Committee for the Management of the Calcutta Zoological Gardens says that, in accordance with a suggestion in the resolution of the Government of Bengal on the Committee's Report for the year 1888–89, a hand-book embodying the experiences gained in the management of animals in the Zoological Gardens, Calcutta, is in course of preparation.

A NATIONAL Exhibition will be held at Palermo from November 1891 to May 1892. It will include an international section for engines and machinery relating to industries such as are carried on in small workshops or dwellings.

THE collection of birds made by Mr. F. J. Jackson during his recent successful expedition to Uganda has been placed in the hands of Mr. Bowdler Sharpe, who, in the current number of the *Ibis*, describes fourteen new species, chiefly from Mount Elgon and the Kikuyu country. In the same periodical Mr. Ogilvie Grant describes four new species of Francolin and a new Hornbill. In the April number of the *Ibis* Mr. Sharpe will continue his account of Mr. Jackson's collection, and we understand that the list of novelties is by no means exhausted. The birds of the Teita district and those obtained in Ukambani seem to be the same as those of Shoa; but on Mount Elgon Mr. Jackson appears to have met with a peculiar fauna, which, to judge by the birds alone, has relations with the Cameroons, one or two species being closely allied to forms only known from the high mountains of this part of Western Africa.

AT the recent meeting of the American Ornithologists' Union, Mr. D. G. Elliot was elected President. Mr. Elliot is well known for his sumptuous zoological works, of which the monographs of the *Felidæ* and the *Phasianidæ* are the most important, the illustrations having been drawn by Mr. Wolf. Dr. Elliott Coues has retired from the Vice-Presidency, but remains on the Council of the Union. The active members number fifty-two, the honorary members twenty-eight, the corresponding members seventy-one, and the associate members 368. In the roll we notice several ladies' names. In the January number of the *Auk* appears a plate of *Icterus northropi*, drawn by Mr. J. L. Ridgway, and far exceeding in execution anything that the journal in question has hitherto produced. Our own ornithological journal, the *Ibis*, will have to look to its laurels, for recently, though the drawing of Mr. Keulemans has been as splendid as ever, the colouring of the plates has been anything but accurate.

AN American Morphological Society has lately been formed. A well-attended meeting was held for its "inauguration" in the Massachusetts Institute of Technology, Boston, on December 29 and 30, 1890. Prof. E. B. Wilson occupied the chair, and Prof. C. O. Whitman was appointed President for the current year. After the details of the organization had been completed, the following papers were read and discussed:—On the development of the Scyphomedusæ, by I. Playfair McMurrich; on the intercalation of vertebræ, by G. Baur; the heliotropism of Hydra, a study in natural selection, by E. B. Wilson; the early stages of the development of the lobster, by H. C. Bumpus; some characteristics of the primitive vertebrate brain, by H. F. Osborn; the development of Nereis and the mesoblast question, by E. B. Wilson; the præ-oral organ of Xiphidium, by W. M. Wheeler; a review of the Cretaceous Mammalia, by H. F. Osborn; spermatophores as a means of indirect impregnation, by C. O. Whitman; the phylogeny of the Actinozoa, by I. Playfair McMurrich.

THE National Association for the Promotion of Technical and Secondary Education has issued a report of the proceedings of the Conference between the executive committee and representatives of County Councils and others, held at the Society of Arts on December 5, for the discussion of the working of the education clauses of the Local Taxation Act, 1890. The Association also publishes selected reports of committees of County Councils, and other schemes and proposals for the utilization of the new fund for educational purposes. These reports have been edited by the secretaries of the Association.

THE *Annuaire de l'Académie Royale de Belgique* for 1891 has recently been issued. It mainly contains an historical account of the organization and work of the various sections of the Academy, and notices of deceased members.

THE *Annals of the Astronomical Observatory of Harvard College*, vol. xxx., Part 1, contains the observations made at the Blue Hill Meteorological Observatory, Massachusetts, U.S.A., in the year 1889, and a statement of the local weather predictions, by Mr. A. Lawrence Rotch. Special attention has been paid to observations of the distribution, density, and amount of cloud throughout the year. The thermometric and barometric readings, wind movements, and rainfall have been tabulated and summarized in a very concise manner.

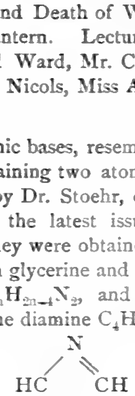
A PAMPHLET has been received from Mr. John Romanes, in which several questions of cosmical physics are discussed. The author believes that the planets, and probably their satellites, are ejects from the sun. He attempts to explain the motions and the forms of the orbits of these bodies on this theory, extends the results to double and multiple stellar systems, and propounds a new hypothesis as to the origin of celestial species.

SIGNOR GUGLIELMO has sent us a paper, presented to the Accademia dei Lincei in September 1890, in which he describes a method for multiplying the dispersive power of a spectroscope, by means of mirrors so arranged that they cause the rays of light to traverse the prisms several times.

ON February 7 and the three following Saturday afternoons, at 4.15 p.m., Prof. H. G. Seeley, F.R.S., will deliver a course of four lectures, at the Gresham College, Basinghall Street, on "The Gravel-beds of the Thames and its Tributaries in relation to Ancient and Modern Civilization." The lectures will be given in connection with the London Geological Field Class.

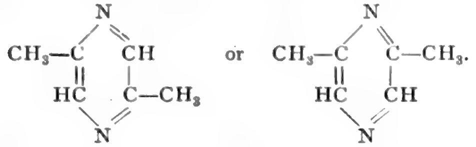
THE third series of lectures provided by the Sunday Lecture Society begins on Sunday afternoon, February 1, in St. George's Hall, Langham Place, at 4 p.m., when Mrs. S. D. Proctor will lecture on "The Life and Death of Worlds," with illustrations by the oxyhydrogen lantern. Lectures will subsequently be given by Prof. Marshall Ward, Mr. Charles Cassal, Mr. J. M. Robertson, Mr. Arthur Nicols, Miss A. B. Edwards, and Prof. Percy Frankland.

A NEW class of organic bases, resembling the pyridine series in constitution but containing two atoms of nitrogen, have been synthetically prepared by Dr. Stoehr, of the University of Kiel, and are described in the latest issue of the *Journal für praktische Chemie*. They were obtained as secondary products in the reactions between glycerine and ammonium salts. Their general formula is $C_nH_{2n-4}N_2$, and they appear to be the higher homologues of the diamine $C_4H_4N_2$,



which bears the same relation to pyridine as the latter bears to benzene. They are rendered doubly interesting by the fact that

Dr. Stoehr has also discovered their presence in the products of distillation of the alkaloid brucine. The best investigated member of the series, $C_6H_8N_2$, is a clear colourless liquid just slightly heavier than water, its sp. gr. at 0° being 1·0079. It boils constantly without decomposition at 153°·5–154° C. (corr.), and determinations of its vapour density at the temperature of naphthalene vapour by Victor Meyer's method yield numbers pointing to the above molecular composition. From its reactions it appears to be the dimethyl derivative of the primary diamine, and may consequently possess the constitution



It exhibits almost all the properties of the pyridine bases. Its odour is very similar to that of the higher members of that series, but reminds one, at the same time, of the narcotic bases, particularly nicotine. It is soluble in water, dissolving with such considerable rise of temperature as to indicate the formation of a hydrate; it is precipitated from its aqueous solution on the addition of potash. It is, curiously, nearly as soluble in cold water as in hot—a phenomenon which is familiar to us in the case of common salt. Its salts are most remarkably like those of the pyridines, and the peculiarities exhibited by the latter are strongly accentuated in them. Thus the hydrochloride, $C_6H_8N_2.HCl$, is deliquescent and sublimes below 100°. The platinochloride, $C_6H_8N_2.2HCl.PtCl_4 + 3H_2O$, which forms fine crystals of the colour of bichromate of potash, possesses in a marked manner the property so characteristic of the platinochlorides of the pyridines, known as "Anderson's reaction," of parting with hydrochloric acid on warming in solution, and depositing condensed salts. On merely warming the solution, bright yellow crystals of the salt $(C_6H_8N_2)_2PtCl_4$ commence to deposit. The double salt with gold chloride, $C_6H_8N_2.HCl.AuCl_3$, forms magnificent acicular crystals several inches long, while the mercuric chloride salt forms large rhombohedrons. The second member of the series investigated proved to have the molecular composition $C_8H_{12}N_2$, and to be an ethyl derivative of the primary diamine. It was likewise a liquid, boiled at 178°·5 (corr.), and possessed properties analogous to the methyl compound just described.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Count Povoleri, F.Z.S.; a Ring-tailed Coati (*Nasua rufa* ♂) from Guatemala, presented by Mr. Cyril Smith; two Hawfinches (*Coccothraustes vulgaris*), British, presented by Mr. J. Newton Hayley; two Blood-breasted Pigeons (*Phlœgnas cruentata*) from the Philippine Islands, a Chinese Turtle Dove (*Turtur chinensis*) from India, presented by Mr. Wilfred G. Marshall; a Rhesus Monkey (*Macacus rhesus* ♀) from India, a Yellow-crowned Penguin (*Eudyptes antipodum*) from New Zealand, deposited.

OUR ASTRONOMICAL COLUMN.

DARK TRANSITS OF JUPITER'S SATELLITES.—The Publications of the Astronomical Society of the Pacific, vol. ii. No. 11, contains several communications on dark transits of the satellites of Jupiter. Mr. Keeler sums up the phenomena as follows:—

(1) In ordinary transits, the satellite is bright when projected upon the surface of Jupiter near the limb, and is usually lost sight of when it reaches the central parts of the disk.

(2) Occasionally the satellite appears darker than the surface of Jupiter when in transit, even when projected on the brightest parts of the disk, and the depth of shade may be very considerable, as a satellite has often been mistaken for its shadow. On leaving the disk, the satellite nevertheless appears quite bright when projected against the sky.

(3) Dark transits of satellites increase in frequency with the order of distance from the primary, being more common for the outer satellites than for the inner ones.

(4) The phenomena are irregular in occurrence, and therefore not predictable.

In order to account for these facts a variety of theories have been propounded. Mr. Keeler advances the idea that the satellites are surrounded by atmospheres containing large quantities of aqueous vapour. A circulation of clouds may thus be set up by means of the intrinsic heat of the central planet. The cloud surfaces of the primary and its satellites being similar, the albedo of the two may be equal. If, however, any accidental disturbance should be set up which would cause a precipitation on the side of the satellite furthest from the source of heat, the albedo would be diminished, and if, at such a time, the satellite was passing across the disk of Jupiter, we should have the phenomena of a dark transit. On this supposition the unstable condition of the atmospheres of the outer satellites sufficiently explains their frequent dark transits.

A simpler and therefore more probable explanation is supported by Prof. Holden. It is that the phenomena of both bright and dark transits depend upon the contrast between the brightness of a satellite and that of the part of the planet upon which it happens to be projected. He finds that much of Jupiter's surface is only about three or four times as bright as the limbs—that is, has an albedo three or four times 0·07. If this be so, then on a background of 0·21 or 0·28, the first satellite, having an albedo of 0·22, or the second, with an albedo of 0·27, would usually be lost. Careful observations of the phenomena attending these transits will considerably elucidate the matter.

SOLAR ACTIVITY (JULY–DECEMBER 1890).—In *Comptes rendus* for January 19, Prof. Tacchini gives the following *résumé* of the solar observations made at the Royal Observatory of the Roman College during the second half of 1890:—

1890.	Relative frequency of spots.	Relative frequency of days without spots.	Relative magnitude of spots.	Relative magnitude of faculæ.	Number of groups per day.
July	3·80	0·40	8·23	12·83	0·97
August	3·42	0·52	15·29	11·77	0·68
September	5·83	0·18	23·68	22·32	1·46
October	3·17	0·58	17·33	10·83	0·75
November	2·45	0·50	7·95	22·75	0·55
December	3·38	0·38	9·25	17·75	0·81

The observations extend over 149 days. The following are the results obtained for solar prominences:—

1890.	Number of days of observation.	Prominences: Mean number.	Mean height. (″)	Mean extension. (°)
July	30	2·07	33·8	1·4
August	31	2·65	27·5	1·1
September	24	2·88	35·8	1·2
October	22	8·05	40·6	1·5
November	16	2·13	28·0	1·5
December	12	3·42	40·4	1·6

M. Marchand, in the same number, gives the result of sunspot observations made at Lyons Observatory last year. The following are the mean total areas of the groups measured expressed in millionths of the sun's visible hemisphere:—

January	22·5	July	49·7
February	20·0	August	51·0
March	51·0	September	154·1
April	34·6	October	160·5
May	20·5	November	137·2
June	8·3	December	41·0

M. Marchand remarks that in 1890 the proportion of days without spots was 0·456, whilst it was 0·555 in 1889. On the other hand, in 1889, only 29 groups of spots were observed, having a total surface area of 1890 millionths of the sun's visible hemisphere. In 1890, 43 groups had a total surface area of 3760 millionths. It is evident, therefore, that there was a sensible augmentation of solar activity last year.

PLANET OR NEW STAR?—The current number of *Comptes rendus* contains an announcement which astronomers would regard with much interest if it were substantiated. Dr. Lescarbault, the astronomer on whose statement the existence of the intra-Mercurial planet Vulcan mainly relies, observed a bright body in Leo on January 11, and, being unable to find it mapped in any atlas in his possession, he estimated its position as R.A. 11h. 4m., Decl. 6°, and concluded that the body was a new star, or one suddenly increased in brilliancy. There is little doubt, however, but that Dr. Lescarbault is mistaken in his conclusions, and that the body observed was the planet Saturn, whose position on the date named was R.A. 11h. 15m., Decl. 6° 59′.

THE FUTURE OF GEOLOGY.[1]

I PURPOSE, in this my retiring address, to make some observations and offer some suggestions as to the future of geology. Not, indeed, that I can claim the *rôle* of a prophet. But there are indications in the tone and manner of recent discussions and research which point clearly to the probable course of geological investigations in the immediate future. Geology has lately become too speculative. For at least a second time in its history we need to pause in order to gather up the records of the past, and to think seriously about the best method of progress in the future. A century ago, the conflict between the Wernerian and Huttonian theories was at its height. Speculation upon imperfect data ruled all. The practical work of William Smith and other English geologists, and the common sense of Englishmen, stayed this mad theorizing; and in the year 1807 the Geological Society of London was established for the express purpose of observing facts and recording observations. The results of this transference of energy from fierce controversy and dispute to patient investigation and labour of detail have been magnificent. And now again the old lines of controversy are reopened. The extraordinary revelations of recent microscopical research, and other improved modern methods, have necessarily reacted on physical geology; and there is once more great danger lest patient labour and accurate induction from proved data should give place to wild theorizing and acrimonious controversy.

The future of geology depends primarily on its practical uses. The method of its study, also, including the possible discovery of new methods of research, must be a potent factor in its development. Not less important is the consideration of the problems, whether physical, stratigraphical, or biological, which at present demand solution. And, as in all sciences and in all life, surrounding influences and environment have much to do with growth and progress. On each of these four points I wish to make a few remarks.

(1) The practical use of geology has received a striking illustration during the past year. Thirty-five years ago Mr. Godwin-Austen maintained the probability of Coal-measures beneath the newer strata of the south-east of England; and such Coal-measures have now been found. To the civil engineer, to the miner, to the stone-mason, builder, and architect, to the well-sinker and searcher for water, a knowledge of geology has become essential; and even so humble a person as the modern farmer and gardener might learn something from the coprolite and manure beds of the geologic series, and from the disposition of rocks and soils. During the past year nineteen committees appointed by the British Association have been concerned with geological subjects. Some of these are speculative and theoretic, but the majority are of practical use. The rate of increase of underground temperature has a direct bearing on mining operations; the circulation of underground waters is of great importance to the water-supply of towns and cities; the manure gravels of Wexford may revive the husbandry of Ireland; the flora of the Carboniferous rocks may throw light on the origin of coal and the probabilities of its occurrence in new localities; and even such apparently theoretic themes as the fossil Phyllopoda of the Palæozoic rocks, the Higher Eocene beds of the Isle of Wight, the erratic blocks sacred to our friends Dr. Crosskey and Mr. Martin, or the collection and identification of meteoric dust, may prove of importance in a practical direction. But not the least valuable are those researches which deal with foreign localities; and the Atlas Range of Morocco, the earthquakes of Japan, and volcanic phenomena of Vesuvius, may vie in value with the Bridlington sea-beach, and the action of waves and currents on the beds and foreshores of estuaries. So long as British enterprise is forcing its way to the centre of Africa, and exploring the Australian continent and the larger islands of the East, a fuller knowledge of foreign geology becomes imperative.

But apart from these matters of general interest or world-wide importance, the practical use of geology is exemplified in the researches of individual observers, and of societies like our own. The investigations of Dr. Callaway in the Uriconian rocks of Shropshire; the discovery by Dr. Lapworth of the Pre-Cambrian rocks at the Lickey and Nuneaton; and the more recent discovery by Mr. Landon of the Lower Bunter Sandstone at Barr Beacon, considerably to the east of the South Staffordshire coal-field, which had hitherto been thought to be its limit, are all illustrations of what may still be done by patient and zealous work. The crown of future success rests on the brow of toil and thought. Even the things of theory and speculation ofttimes become exalted into practical service by the growing developments of advancing knowledge, and the varying demands of human progress.

(2) I advance now to the method in which geology should be studied, and the possible discovery of new means of research. The day has long gone by when geology could be viewed "as a fashionable toy that everyone who has been to school is supposed capable of handling." No one now dares to touch its problems without some knowledge of physics, mathematics, biology, and chemistry. When Dr. Buckland led his tribe of random riders amongst the Oolitic strata of Oxford, or when Sir Roderick Murchison discoursed in sapient language on the rocks of Siluria, geology might have been a "fashionable toy." But not so now. The stern requirements of modern days have made it more accurate, and rendered it more sure. And with its enlargement has come an attention to minute detail, an observance of processes and of the results of processes which in the olden days had no place. To be a geologist now, at all events in the special phases of the science, a man must be either a mathematician, a physicist, a master of biology or of chemistry. Happy the man who can combine the whole!

Prof. A. H. Green, of Oxford, in his recent address as President of the Geological Section of the British Association, at Leeds, discoursed on the value of geology as an educational instrument. He began by admitting candidly that geologists were in danger continually of becoming loose reasoners, because the data from which they reasoned were necessarily scrappy and the geological record imperfect, or the facts were capable of interpretation in more than one way, or the determinations were shrouded in mist and obscurity. Notwithstanding this, he urged that the study of geology would be useful educationally by teaching wariness when the pupil comes to handle the complex problems of morals, politics, and religion. Further:—"There are immense advantages," he continues, "which the science may claim as an educational instrument. In its power of cultivating keenness of eye it is unrivalled, for it demands both microscopic accuracy and comprehensive vision. Its calls upon the chastened imagination are no less urgent, for imagination alone is competent to devise a scheme which shall link together the mass of isolated observations which field work supplies; and if, as often happens, the fertile brain devises several possible schemes, it is only when the imaginative faculty has been kept in check by logic that the one scheme that best fits each case will be selected for final adoption. But, above all, geology has its home, not in the laboratory or study, but *sub Jove*, beneath the open sky, and its pursuit is inseparably bound up with a love of Nature, and the healthy tone which that love brings alike to body and mind" (*Times* report, September 5; NATURE, vol. xlii. p. 455). Prof. Green proceeds to argue that geology should be taught in schools for more prosaic reasons; the two chief of which are that geography, and especially physical geography, cannot be taught without constant reference to certain branches of geology, and that there are many points of contact between the history of nations, the distribution and migrations of peoples, and the geological structures of the lands which they have dwelt in or marched over. And he concludes by sundry good illustrations of such school teaching, into which I need not now follow him.

Many may be disposed to think that this able and admirable address of Prof. Green is overstrained and overstated. But it must be remembered that, in order to convince the British public, it is necessary to state things strongly, and to draw things

[1] Presidential Address by the Rev. G. Deane, D.Sc., delivered before the Birmingham Philosophical Society on October 8, 1890.

clearly and deeply. I do not myself believe that the average school boy and school girl need to be taught "wariness" in matters of morals, politics, and religion. They are generally wary enough about these matters as it is; and, what is more, soon get weary of them. And, possibly, too much stress is laid upon the relation of geological structure to the history of nations and the migrations of peoples. An enthusiastic geologist might feel inclined to generalize that, inasmuch as the two great battles of the Franco-German war of 1870—Gravelotte and Sedan—were fought upon Jurassic strata, therefore such strata facilitate military operations, and all the great battles of history were fought upon Jurassic strata.

But, allowing that there may be some overstatement and special pleading, there is a great amount of truth in the address from which I have quoted. Many people go through life with their eyes shut. They do not really see what they think they see; and what they think they see is not what they ought to see—not what exists and presents itself to them. Whatever, then, trains to accurate observation of facts and phenomena is of value as an educational instrument. The great end of education is to call out and train the powers we possess, whether of mind or body. Whatever, therefore, develops and strengthens a faculty will further this end; and that scheme of training which best fosters *all* the powers of mind will be the most satisfactory. The mathematician is apt to think lightly of the probable and moral reasoning of the philosopher, and the classic sometimes prone to scorn the pretensions of science. For the highest kind of education, a severe, long-continued, and isolated application to one special branch of study is requisite; though there is then great danger that the mind will become warped and one-sided. But, for the initial stages of education, the pursuit of a definite science will tend to supplement and complete the discipline of other studies, and to render the juvenile mind *totus, teres, atque rotundus.*

Prof. Green has done good service in urging the claims of geology in this direction. It can never take the place of physics or chemistry; but it is at least the equal of either biology or physiology for training the mind to accurate and complete observation of facts and phenomena. The word has come from the chair of authority not a day too soon; and it is to be hoped that ensuing years may witness a great extension of geological teaching in our schools for both boys and girls.

The British Islands form the natural home of geological science. Though limited in area when compared with other countries, they contain in this small space a very great variety of different strata. Indeed, with one or two exceptions of no very great importance, the whole succession of geological rocks is represented in our land. Though we have not the immense development of one particular formation which occurs in some other regions, we have an immense variety of formations. The surface of our country is a miniature picture of the whole geological series; and England, Wales, Scotland, and Ireland thus become the key to the world.

Accordingly, it is not surprising that, in Britain, geology has been an honoured science, and that its leading votaries have attained a world-wide reputation. Other lands have nobly borne their share in discovery and speculation; but I think we may justly claim for Britain the crown of geological progress. As a science, geology began to emerge in Italy in the sixteenth and seventeenth centuries. Pythagoras, Strabo, and others of the ancients had, indeed, speculated on cosmogonies; but in Italy, at the time mentioned, the fossils of the sub-Apennine hills led to genuine geological controversy. A century later, Werner in Saxony, and Hutton in Edinburgh, were the great teachers of the science; and at the beginning of the present century the formation of the Geological Society of London based the study of geology upon a rigid induction of facts, and tended to discountenance crude speculation and hasty theory. Since then the nations of Europe and America have striven in friendly rivalry to further the knowledge of the science they love so well.

I am well aware that in two branches of study, as I shall explain further on, Britain until recently has lagged behind other countries. But this notwithstanding, Britain may well claim to be amongst the foremost in geological research. And we may to-night remember that Birmingham is the centre and apex of England, and that within a comparatively short distance from this room nearly all the geological formations can be observed and studied. Few places are more favourably situated for extensive and varied research than Birmingham. Within a distance of little more than fifty miles from this spot you may study the representatives of all the systems of rocks except the upper part of the Oolitic, the Cretaceous, and the Tertiary groups. And what are fifty miles in these days of railways? At slight expense and slight fatigue, a day's fresh air and bracing work full well repay the geologist. And within almost walking distance abundant materials for geologic study and pleasant recreation may be found. The Clent Hills, the Bromsgrove Lickey and the Severn Valley, the Clee Hills, the rocks of Ludlow, Wenlock, and South Derbyshire, the hills of Dudley and Rowley Regis, of Barr Beacon and Sedgley, of Coalbrookdale and the Wrekin, of Church Stretton, Charnwood Forest, Malvern, and the Cotswolds, are all within easy reach. And these together represent almost the whole of the series of known rocks, whether igneous, sedimentary, or metamorphic.

I have claimed for Great Britain pre-eminence in geological research. I might also claim for Birmingham pre-eminence in one, if not in both, of the branches of geological study which Great Britain has until recently neglected. In the chemical constitution of rocks, and their microscopical structure, the Continental geologists have in years gone by done better work than the English. But we are in recent years striving to wipe away this reproach. Forty years ago Mr. Sorby, of Sheffield, commenced his microscopical researches; and, in Birmingham, Mr. Samuel Allport has been conspicuous for success in this line of investigation. Mr. Teall, of the Geological Survey, Prof. Bonney, and many others, have amply redeemed, in this respect, the credit of the country. At the present time Dr. Callaway, Mr. T. H. Waller, and others, are making large use of this method of inquiry.

The chemical relations of geological questions do not even now command in England the attention they deserve; as there is a wide field of research for a qualified chemist in geological problems.

The study of a complex science like geology, then, includes almost all other sciences. In order to understand the real bearings of the manifold questions that emerge, the geologist needs to be almost omniscient. He must know something of almost every physical science—chemistry, mineralogy, and crystallography; what is now known by the name of physiography, including meteorology and natural philosophy; biology, physiology, and anatomy are all requisite. Microscopical research is essential. And if the higher parts of mathematics can be added, the observer will be still better equipped.

The past year has seen a new direction of enterprise in regard to the registration of geologic facts. The photographic art has been called into organized recognition. At the meeting of the British Association held at Newcastle-on-Tyne in September 1889, a committee was formed to arrange for the collection, preservation, and systematic registration of photographs of geological interest in the United Kingdom. In some counties, as in our own, this impetus has taken a rather wider sweep, and aims at a general photographic survey of the county. As is known to many here present, an influential county council has been appointed for this purpose in Warwickshire, with Mr. J. B. Stone as president, and Mr. W. J. Harrison as secretary; and two members of this Society have been delegated to act thereon. Unquestionably many instructive geological sections have been lost to science through want of correct drawing, or photographing, at the time of their exposure; and it is hoped that this committee may aid in recording and retaining such facts. The result of the first year's work has been satisfactory; and the report to the Association at the Leeds meeting shows that 196 geological photographs have been sent in. It is to be hoped that in years to come this method of rendering permanent transient sections may be productive of good.

The possible discovery of new methods of research opens up a wide field of speculation, into which time forbids me to enter at large. Improvements made in modern optical instruments have enabled the geologist to see through a brick wall, or force his way through a prickly problem; and the day may not be far distant when the very centre of the earth will be as clearly seen and understood as its surface now is. One of the most promising new methods of research is the separation of minerals by means of heavy solutions, and other methods are sure to come. It remains to be seen how far the recent marvellous advances in electrical science may throw light on the problems of rock metamorphism. The transitions from one kind of rock into another are startling and puzzling; and it may possibly be found that terrestrial magnetism and electrical force are potent factors in the results which field geologists have observed and recorded.

The genius of discovery is not yet exhausted; and the triumphs of the last fifty years augur an enlargement and expansion in the immediate future which will place the science of geology in the forefront of human progress.

(3) Passing now to the problems, whether physical, stratigraphical, or biological, which at present demand solution, I may remark that an ex-President of this Society—Mr. William Mathews—in the year 1883, made this topic, with great lucidity and power of reasoning, the theme of his address. Mr. Mathews dealt with four subjects: the doctrine of uniformity, the origin of mountains, the supposed cause of glacial epochs, and the eroding power of ice. As I have myself, in various papers read either before this Society or before the Natural History and Microscopical Society, discussed more or less fully the whole of these subjects, I do not propose touching them to-night. In fact, the last seven years have brought other questions to the front, on which I may venture to dilate.

Prof. Huxley, in his address as President of the Geological Society of London, in the year 1869, traced three phases of geological speculation, viz. the catastrophism of Murchison and the older geologists, the uniformitarianism of Hutton, Playfair, and Lyell, and what he styled evolutionism, which adopts all that is sound and good in the other two, and is destined to swallow them up. He argued that both catastrophism and uniformitarianism had kept alive the tradition of precious truths: catastrophism in insisting upon the existence of a practically unlimited bank of force, on which the theorist might draw; and uniformitarianism in insisting on a practically unlimited bank of time, ready to discount any quantity of hypothetical paper. And he maintained that there is no sort of theoretical antagonism between the two, as it is very conceivable that catastrophes may be part and parcel of uniformity; and still less is there any necessary antagonism between either of these doctrines and that of evolution, which embraces all that is sound in both, and applies the same method to the living and not-living world.

I consider the position thus taken up by Prof. Huxley to be absolutely impregnable. It is well to bear in mind that geology knows at present no finality. Time-honoured views have been shattered and pulverized by the recently-published papers on the Highlands of Scotland, and by both Continental and English geologists on the structure of the Alps. And no one could have attended the London meeting of the Congrès Géologique International without seeing that grave changes in geological conceptions, and in geological interpretations, are impending. The school of evolution will indubitably swallow up those of catastrophism and uniformitarianism.

And if evolution thus bids fair to dominate physical and stratigraphical geology, its influence can also be traced in the succession of living forms. Prof. Huxley himself has done as much as any living man in this direction. In his well-known lecture before the Royal Institution on February 7, 1868, he conclusively showed that bird fossils are reptilian in their character, and the reptile fossils of the Secondary rocks are bird-like in character. And further, in his lectures at the Royal School of Mines, in 1876, he gave a long and exhaustive series of transitional links, from the Ceratodus of the Trias, which affords a connecting link between fish and Amphibia through the reptilian types of the Secondary rocks, to the protohippus, hipparion, anchitherium, palæotherium, and orohippus of the Tertiary rocks, which are links of transition from the modern horse to the rhinoceros, the pigs, and the ruminants.

The recapitulation theory of development, as expounded in the writings of Fritz Müller, von Baer, Balfour, and Haeckel, supports the theory of evolution; and has been explained and illustrated at great length and with much power by Prof. A. Milnes Marshall in his recent brilliant address to the Biology Section of the British Association at Leeds. He writes:—

"The doctrine of descent, or of evolution, teaches us that as individual animals arise, not spontaneously, but by direct descent from pre-existing animals, so also is it with species, with families, and with larger groups of animals, and so also has it been for all time; that as animals of succeeding generations are related together, so also are those of successive geologic periods; that all animals living or that have lived are united together by blood relationship of varying nearness or remoteness; and that every animal now in existence has a pedigree stretching back, not merely for ten or a hundred generations, but through all geologic time since the dawn of life on this globe.

"The study of development, in its turn, has revealed to us that each animal bears the mark of its ancestry, and is compelled to discover its parentage in its own development; that the phases through which an animal passes in its progress from the egg to the adult are no accidental freaks, no mere matters of developmental convenience, but represent more or less closely, in more or less modified manner, the successive ancestral stages through which the present condition has been acquired.

"Evolution tells us that each animal has had a pedigree in the past. Embryology reveals to us this ancestry, because every animal in its own development repeats this history, climbs up its own genealogical tree" (NATURE, vol. xlii. p. 468).

The theory of evolution, then, in some one or other of its forms, must be accepted as the basis of the geology of the future. The physical problems which in past years have been examined and discussed by Sir William Thomson, M. Delauney, the Rev. Osmond Fisher, and others, have in recent years been still further elucidated; conspicuously so by our excellent secretary, Mr. Davison, and Mr. T. Mellard Reade. The stratigraphical problems have called in the aid of thrust planes, reversed faults, dynamo-metamorphism, and catastrophes to alter the dead level of uniformity. And the biological problems are explicable only on some theory of evolution. In Huxley's words, evolution is destined to swallow up the other two theories.

Perhaps the most striking development of modern geology is the rise and growth of the Congrès Géologique International; and the questions discussed thereat are, of course, the prominent questions of the present time. Beginning its existence at Paris in 1878, it has since met at Bologna in 1881, Berlin 1885, London 1888, and the next meeting is fixed for Philadelphia in 1891. Its growing importance is indicated by the numbers of foreign members in attendance. These were:—Paris 110, Bologna 75, Berlin 92, London 151, and our American cousins next year, as is their wont, will probably "whip creation." As I had the pleasure of attending the London meetings, I read a paper thereon before the Geological Section of this Society, and allude to the subject now only because the topics of the Congress suggest the matters which are under immediate discussion. These were three—the map of Europe, nomenclature and classification, and the nature and origin of the crystalline schists.

The geological map of Europe is under the care of an influential committee, meeting in Berlin, on which Germany, France, Great Britain, Austria-Hungary, Italy, Russia, and Switzerland, are all represented. The scale adopted is 1 : 1,500,000; that is, 1 inch to 23·673 miles; and the map will consist of forty-nine sheets. Some parts of Central Europe are on the eve of publication; and, although the colours are somewhat different from those we are accustomed to in England, it will be a great advantage to have uniformity of colouring for all European countries.

On nomenclature and classification, the Congress, having at the previous meetings dealt with the unification of geological terms, gave attention to the classification of the Quaternary and the Cambrian and Silurian strata. It was generally felt that, notwithstanding the insignificant thickness of the Quaternary strata, the advent of man and the existing mammals was sufficient to render this epoch absolutely distinct from the Tertiary. But on the great Cambro-Silurian question a battle royal ensued. As I have treated this fully in my previous paper, I must not take up time to-night upon it. This controversy still rages, both at the Geological Society of London, and in the pages of the *Geological Magazine*. Prof. Blake is mad on his Monian system; Dr. Hicks is naturally jealous for his Dimetian, Arvonian, and Pebidian of the St. David's promontory; Dr. Callaway is equally sensitive as to his Uriconian rocks of Shropshire; Dr. Lapworth, the inventor of the term Ordovician for the Upper Cambrian of Sedgwick and its equivalent the Lower Silurian of Murchison, may yet have something more to say before the controversy closes; and some of the Continental and American geologists seem to think the whole thing a storm in a teapot, and appear disposed to adhere to the ancient lines.

Closely connected with this controversy as to the base of the sedimentary rocks, comes the discussion concerning the nature and origin of the crystalline schists, which occupied two morning sittings at the Congress. Here, again, modern researches tend to subvert the older theories. Dynamic metamorphism, accompanied by recrystallization on freshly induced planes, curves, and surfaces, is now held to explain the most extraordinary transitions from one kind of rock into another. Both chemical analysis, aided by new methods, and microscopical investigation, with improved instruments, establish this conclusion; and it would seem likely that the immediate future would realize the reasoning of Hutton and Playfair that the sedimentary rocks give no indication of a beginning and no prospect of an end. Certain it is that the indications of bedding and sedimentary origin are encroaching fast upon what was only a short time ago considered part of the primæval crust of the earth. Some cases of supposed bedding, as for example in the Malvern crystalline axis and in some districts around the Wrekin, have been shown to be connected with igneous rock probably rearranged under great pressure. But as to the origin of the crystalline schists, Profs. Heim, of Zurich, Lehman, of Kiel, Drs. Lapworth and Callaway among ourselves, Dr. Hicks, and many other most able investigators, are firmly convinced that mechanical pressures and deformations are in reality the cause of the most sensational changes from both sedimentary and igneous rocks into crystalline schists. No doubt the old conflict will come on again, and it will be many years before these views will be universally accepted. But that they are destined to dominate the immediate future is as clear to me as the shining of the sun on a bright day at noon. The molecular changes induced by vast pressure and its accompanying natural forces are quite sufficient to change the structure and nature of crystals and rocks. Investigations in the field and in the laboratory will soon set these points at rest, though for some years to come the conflict of opinion may be strong and fierce. The chief difficulty at present is the apparent elimination of alumina and magnesia; but I have little doubt our chemists of the future will solve this problem, and their researches will throw light upon the nature of these widely extended though little-known substances. When practical geologists speak of the crystalline schists and associated strata as "a jumble of rocks," it is time someone arose to reduce the "jumble" to order; and there is every reason to believe that chemical and microscopical research will speedily bring him.

(4) The last element in the future of geology which I propose to speak of may be expressed as the external influences which bear upon the development of the science; so to speak, its environment.

Looking around at present upon geological activities in Britain, we find the Geological Society of London, the organization represented by the Geological Survey, a number of Societies scattered through the Kingdom which are devoted, either solely or partially, to the furtherance of geological research, and a large number of earnest individual workers in almost every nook and corner of the land. To these must be added the Royal Society, the Professors of the Science Colleges, with the great influence they spread, the British Association for the Advancement of Science, and the influence of the Universities in geological directions. With this vast army of workers geology *must* advance. Bnt, as in the past so in the future, it will be an advance amongst difficulties, and in the face of opposition and obstruction.

The spirit of the age has a mighty power on all things; and it might be thought, at first sight, that the spirit of the age would urge the science of geology forwards at almost headlong speed. But I am not at all so sure of this: it may urge general scientific inquiry forwards, but the popular directions do not run on geological lines so much as on some others. To put the matter in a nutshell, geology does not pay; and it must be made to pay, before competent and trained men will be able to give time and toil to its pursuit. Very few competent persons can afford to give up their leisure, and *also* their money, from a mere love of the science.

The last twenty years have witnessed a great expansion in scientific matters. Science Colleges have been established in many of the great centres of population, and to most of these a Professor of Geology is attached; in the Board Schools of most large towns and cities, science is taught, and with it Geology to a greater or less extent; more often less, and sometimes meagre. Private schools and organizations, likewise, sometimes favour, generally tolerate, the study. But still we are not happy. These things are not as they should be. Geology may reasonably claim a prominence it has not yet received.

In the days of Sedgwick, Buckland, Chalmers, Hugh Miller, Lyell, and Murchison, the leading geologists might be counted on your fingers; now they may be counted by scores, and it may confidently be expected that, notwithstanding pecuniary disability and in defiance of difficulties, the numbers will still increase. But at present the superior advantages of other lines of scientific thought and effort draw many away from the geologic path. Those who remain are attracted more by love of truth than hope of pay. If the amount of money sunk and lost through want of correct and accurate geological knowledge could be fully estimated, its total would be astounding. Some well-laid schemes, under good geological direction, doubtless have failed; but such are very few. The successes have greatly exceeded the failures. Here, close to Birmingham, the Sandwell Park Colliery may be pointed to; and the "Search for Coal" Committee in the south-east of England, will, in all probability, render a good account of their labours. With such examples before us, the public zeal for geology ought to be greatly stimulated.

In estimating the spirit of the age with regard to geology, one element ought to be noticed, which I rather shrink from introducing here—I mean the past theological hostility to the science. I will, however, deal with it generally, without introducing controversial matters. This hostility is scotched, but not killed, as it ought to be by this time. In centres of intelligence it has now but little or no power, but it lingers in the dark places of the land. A perverted theological bias has never yet succeeded in preventing the ultimate advance of a correct and accurate science; but it may hinder and obstruct. Lukewarm friends are little better than open enemies; and unconscious influence, from the cause referred to, may, and probably does, hold back many from hearty and earnest support of geological work. What is wanted for genuine and full growth and progress is the earnest and sympathetic aid of all classes and conditions. If this be withheld, the growth will languish and the progress will lag. Compared with the past, indeed, we have reason for immense thankfulness; but the evil still lurks, and has yet to be faced and finally destroyed.

And now, to apply this spirit of the age to concrete existence, and attempt to read the future in the light of the present, that future will depend upon at least two main supports, viz. (1) the union and the rivalry of effort, and (2) the devotion of either public or private money to geological objects, and to the determination of crucial points.

(1) The union of effort is represented by the many Societies already referred to scattered over the face of the country; by the central bonds of the Geological Society of London and Section C of the British Association; and by the newly developing Congrès Géologique International, which promises good service in the common cause. Some of these Societies—Field Clubs especially—are more of the picnic and social character than is likely to conduce to effective progress of scientific research; and even the meetings of the British Association are open to some criticism on this score. In our Midland district, in 1876, a new departure was started in the Midland Union of Natural History Societies; and this, after some vicissitudes, is still living a vigorous life; held a highly successful annual meeting at Oxford last year; and this year, at Leicester, a most pleasant meeting has been held, accompanied by two well-planned excursions—one botanical, and the other geological—through Charnwood Forest.

For one, I am not disposed to value lightly the influence of even mere social gatherings connected nominally with science. They tend to give a tone both to the neighbourhood where they are held, and to those who attend them; and also put people on the alert for possible discoveries. The experts in geology are few, but the watchers and labourers are many; and these last, scattered as they are throughout the country, may hear of or find out facts and points of interest which the experts may subsequently be called upon to examine and explain. Many an interesting geological fact, or even crucial section, has been lost simply because no one who understood the matter was at hand to decipher and preserve it, or report it to those who could do so. By all means let us increase our army of observers; what they hear of and discover experts can explain. A striking illustration of this kind of labour has recently occurred in this district. Mr. Sherwood, of Sutton Coldfield, found a freshly opened section near Barr Beacon, which exposed rocks that were new to him. He reported the fact to Mr. Landon, of Saltley College, who discovered an eroded surface of the Lower Bunter Sandstone, in a locality where it had previously been believed to be absent. Mr. Landon has since discovered quartzite implements in a river gravel at Saltley. I have had, in my own experience, similar illustrations, one of which is worth recording. When residing in the valley of the Ouse, in Bedfordshire, during an occasional absence from home, a well was sunk near my house. The workmen came upon the lower jaw of a hippopotamus, and of course proceeded to demolish it with their pickaxes. A friend of mine happened to pass, and he succeeded in saving for me some fragments of teeth and jaw. When I returned, the bulk of the remains had been used as stuffing to the back of the well. But my friend had saved sufficient to prove the existence of *Hippopotamus major* in that locality.

This union of effort necessarily involves some amount of friendly rivalry. It seems to be a law of humanity that two vigorous persons, jogging side by side along the same road, stimulate each other to increased pace. And so, in each society, the blending of effort is a stimulus to each individual worker. In a union of societies, the same power should be felt; each will vie with the others, not simply for pre-eminence of course, but for progress. And the result comes unconsciously in the advancement of the object they have in common. Geologists in the nations of Europe and America, organized in various societies, and surrounded by different influences, have one common object, and mutually stimulate each other towards the attainment of full and complete geological knowledge. It may be, sometimes, that this rivalry will lead to strenuous conflict; but conflict of opinion and thought, so long as personal rancour and strife are excluded, will always lead onwards in the path of truth.

(2) In approaching the matter of money—whether public or private—devoted to geological objects, I touch a subject of some magnitude, complexity, and difficulty. When the British purse is appealed to, buttons are often in requisition, not in lieu of coins, but to close the exit of coins. But it is perfectly clear that geological investigation is expensive, and the pecuniary resources of most competent geological observers are limited. Geologists have to rely, for the most part, upon natural sections and exposures, or upon those artificial sections and borings which commercial enterprise opens up. A judicious expenditure of money to make artificial sections and borings, in order to determine crucial points, would often be amply repaid.

In the allocation of public money to geological objects, we have conspicuously before us the Geological Survey of the Kingdom, the maintenance of the Royal School of Mines, and the Natural History Museum at Kensington. Then, during the last year, a grant in aid of provincial Science Colleges has, after much agitation, been wrung from the Treasury; though how much of this will find its way to geological objects is very problematic. Perhaps the most significant "sign of the times" in this direction is the Report of the Committee, appointed by the Commissioners of the 1851 Exhibition, as to the establishment of Science Scholarships in provincial and colonial Universities and Colleges. The second item of the Committee's recommendation runs thus: "That the scholarships be limited to those branches of science (such as physics, mechanics, and chemistry) the extension of which is specially important for our national industries" (NATURE, vol. xlii. p. 431). Of course this is a case of complete powerlessness on the part of geologists. The Commissioners are acting within their rights, and after due deliberation. But, with all deference to the illustrious men of science who have drawn up this Report, I humbly think that geology ought not to be excluded from the subjects that are specially important for our national industries.

Various scientific Societies allocate money in aid of geological research. The grants from the Royal Society and the British Association have been of great service, not only in rousing activity, but also in rewarding, or rather recompensing, worthy work. The Geological Society of London has at its disposal several most honourable awards for well-spent labour. The medals come first—the Wollaston, Murchison, Bigsby, and Lyell Medals being the highest geological honours of the country. But the surpluses of these funds, as also the "Barlow-Jameson Fund," are allocated from time to time to repay in some measure the expenses of those who have rendered distinguished service and wrought good work. With us in the Midlands the Darwin Medal of the Midland Union, and the grants made from our "Endowment of Research Fund," are a humble reflex of such awards. There is ample scope for extension and enlargement in awards of this kind.

But perhaps the development of future British geology may come from other sources. The enterprise of the South-Eastern Railway directors has opened up the possibility of coal-fields where thirty-five years ago geologists said they might exist; and public companies can do much to aid further research. Private enterprise also may do much. This old land of ours is not yet used up, and one need not despair of discovering still, in its soil and rocks, fresh elements of permanence and power.

I have thus endeavoured to trace what seem to me to be the possibilities of future geology. There may be regions yet unsearched which will yield up their treasures to the diligent. The current controversies on theoretical points afford scope for the acutest intellect to unravel and explain. New methods of research give promise of coming discoveries. The spirit of the age and the surroundings of the science are favourable on the whole to progress. If money be forthcoming to meet needful expenses, and observers are careful and accurate, the past triumphs of geology will appear small compared to the triumphs that are yet to come.

THE SOUTH AFRICAN DOCTRINE OF SOULS.

IN the second of two interesting papers on the manners, customs, superstitions, and religions of South African tribes (Journal of the Anthropological Institute, vol. xix. No. 3, and vol. xx. No. 2), the Rev. James Macdonald, who has had ample opportunities of studying the subject, has a good deal to say about the doctrine of souls which prevails among the aborigines of South Africa. It is extremely difficult, he explains, to discover what the people really believe about the spirit world, so many and varied are the traditions relating to it. There are, however, certain outstanding facts common to all; and of these Mr. Macdonald gives a clear and instructive account.

All human beings are supposed to have souls, but their souls are not believed to be entirely confined to the body. A man's soul may, it is thought, occupy the roof of his hut, and, if he changes his residence, his soul does so at the same time. Mr. Macdonald takes this to be a loose and indefinite way of expressing "the belief that a man's spirit may have influence at a distance from the place where he is himself at any time." The people often use the word "zitunzela"—from "izitunzi," shadows—to express their ideas of human spirits and the unseen world generally; and this is "the nearest description that can be obtained." A man is constantly attended by the shadows or spirits of his ancestors as well as his own, but the spirit of one who dies without speaking to his children shortly before death never visits his descendants except for purposes of evil. In such cases magicians or priests offer costly sacrifices to prevent misfortune and death.

Great importance is attached to dreams or visions, which are supposed to be due to spirit influence. When the same dream comes more than once, the dreamer consults the magicians, who profess to receive revelations through dreams. If the dreamer has seen "a departed relative," the magician says, "He is hungry." Then a beast is killed; the blood is collected, and placed in a vessel at the side of the hut farthest from the door; the liver is hung up in the hut, and must not be eaten until all the flesh of the animal has been used. The "essence" of the food is "withdrawn" by the spirit during the night, and after a specified time all may be eaten except the portions which the magician orders to be burned.

Ancestor-worship is not only professed by the South African tribes, but "they actually regulate their conduct by it." Says Mr. Macdonald:—

"If a man has a narrow escape from accident and death, he says, 'My father's soul saved me,' and he offers a sacrifice of thanksgiving accordingly. In cases of sickness, propitiatory sacrifices are offered to remove the displeasure of the ancestors, and secure a return of their favour. Should anyone neglect a national custom in the conduct of his affairs, he must offer sacrifice to avert calamity as the consequence of his neglect. When offering propitiatory sacrifices, the form of prayer used by the priest is: 'Ye who are above, accept our offering and remove our trouble.' In freewill offerings, as in escape from danger, or at the ripening of crops, the prayer takes the following form: 'Ye who are above, accept the food we have provided for you; smell our offering now burning, and grant us prosperity and peace.'"

Animals are not supposed to have souls; neither are inanimate objects. But spirits may reside in inanimate objects, and their presence has an influence on many customs and habits. A striking example of such influence was afforded during the rebellion of 1879, when Umhlonhlo, after the murder of the British Resident, was one day marching in a leisurely manner across country with his whole army. The forenoon was hot, and not a cloud was to be seen. Presently, the magicians noticed on the horizon a peculiarly shaped cloud :—"It rose rapidly in one mass and 'rolled upon itself.' Its movements were intently watched till it approached the zenith and passed over the sun. This was an evil omen. For some unknown cause the spirits were mortally offended, and had come over the army in shadow at noonday. In grief and sorrow their backs were turned upon their children, and the result of this would be certain defeat and disaster. There was, however, no immediate danger. That morning's scouts had reported that there were no troops within many miles of their line of march, and they could repair to some sacred place to offer sacrifices and make atonement. While they were discussing which place to repair to for this purpose, the van of a small column of cavalry appeared unexpectedly over a rising ground. Dismay struck into every heart. The war minister urged his men to form into order of battle. No one answered his summons. He did his best to organize an orderly retreat, but in vain ; not a blow was struck, and every man took to his heels, making for the nearest hiding-place in mountain or forest. That army never reassembled. Black-hearted fear utterly demoralized it."

Water or river spirits play a great part in South African mythology. They inhabit deep pools where there are strong eddies and under-currents. They are dwarfs, and are of a malignant disposition, which they display by greedily seizing on anyone who comes within their reach. They are, of course, greatly feared ; and the popular dread of them is shown in a way which has been known in many different parts of the world. Mr. Macdonald gives the following example :—

"Some years ago a number of Gcaleka girls were, on a fine summer day, bathing in the Bashee. One of them got beyond her depth, and began to struggle in the water and cry for help. Her companions promptly raised the alarm, and two men working close by ran down to the water's edge. She was still struggling feebly, but to the onlookers it was a clear case of being 'called' by the river, and they made no attempt to save her. The body was recovered by the magicians the same day, when it was found she had been drowned in less than 5 feet of water. All this came to the ears of C. G. H. Bell, Esq., the English Resident, and he cited the parties, magicians and all, to appear before him in court. The two men not only admitted that they could have waded to the spot where they saw her struggling, but also said the water would not be 'more than breast deep.' They had made no effort to save her, as it would be 'improper and dangerous to interfere when one is called by the river.' Mr. Bell tried to argue them out of such absurd notions, but to little purpose, and finally came to the conclusion that 'six months hard' might be more effectual in eradicating superstition than all his philosophy, and six months hard it accordingly was."

Mr. Macdonald says there is no periodical process of purging or driving away spirits. Without the presence and aid of magicians, ordinary people dare not interfere with these mysterious powers, however malignant and destructive they may become. Although a man is guarded by the spirits of his ancestors, they do not protect him from demons or from wizards and witches. A certain measure of protection can, however, it is supposed, be obtained by the use of charms provided by magicians. On one occasion, when war was being carried on with England, the magicians gave the soldiers a charm against English bullets. It was the blue flower of a species of rhododendron. "Those who carried this talisman rushed forward against columns of infantry without a shadow of fear or hesitation, and only when men began to bite the dust in all directions did the nature of the delusion break upon the army, and panic ensue."

THE ACTION OF LIME ON CLAY SOILS.

THAT lime promotes the fertility of heavy clay soils is a fact that has for many generations been well known to all agriculturists ; but the scientific reason for the beneficial action arising from its application has not, to the best of my belief, been at any time at all satisfactorily explained. The question, however, remains one of first importance in the science of soils, and I therefore make no apology for offering an explanation, or rather theory, which, to my doubtless somewhat partial mind, seems to go a considerable way towards the elucidation of the problem.

I take it for granted that all interested know that a clay soil is not by any means a pure clay (hydrated silicate of alumina), but a mixture of *impure* with pure clay (much more of the former than of the latter), plus sand, iron oxides, organic matter, &c. The clays which form its bulk have been derived from the natural decay or weathering of mineral silicates, containing, besides, aluminium, alkali, or alkaline-earth metals, and they occur in it in all stages of impurity or further decomposition. As an invariable rule, other things being equal, the greater the normal impurity of the clays the greater the fertility of the soil. A *pure* piece of clay is like pure quartz-sand—so much dead, inert matter ; a plant can make nothing of it, can take nothing from it. In no fertile clay soil, however, even of the heaviest description, does there occur at any time more than about 10 per cent. of *absolutely* pure clay. Well, then, what is the composition of the average clay particle? That depends on the mineral from which it was derived. If from the felspars, its most common origin, it will be a hydrated silicate of alumina plus silicate of potash, or, instead of the potash, soda and lime. I will suppose, for brevity's sake, that my clay particles have the former composition, and the explanation which I will offer with regard to their behaviour can be applied with very little difficulty to any of the other cases. Clay particles of the above composition, when subjected to the action of water containing carbonic acid gas, lose potash. That I have repeatedly proved by experiment. If the carbonic acid is in fair excess, it comes away altogether as carbonate of potash ; but if there is not a sufficiency of this anhydride, it is liberated partly as soluble silicate of potash (soluble glass). Should lime, however, in this latter case be present, the practically insoluble calcium silicate will be constituted, and the potash freed to form a soluble salt with any other acid that may be present and available, such as carbonic, sulphuric, nitric, &c. A grass plant growing in clay soil does not, it is evident, send sufficient carbonic acid through its root-hairs into the soil, as many other plants do, to completely convert the liberated potash into carbonate, and the consequence is that the soluble silicate of potash which is permitted to form is drawn into the vegetable, as well as the carbonate of that alkali. Now silica and silicates are decidedly injurious, to all vegetables doubtless, but particularly to agricultural plants. I say injurious ; the day has gone by for considering silica an essential, or useful, or even a merely innoxious accessory. A little examination of the plant-physiology shows that it is injurious. The organism tries to get rid of it as speedily as possible—that is, at least to get it out of the way of its general circulation ; it unfortunately has no means of casting it off altogether. Here, I need scarcely refer to the well-known experiments which have, over and over again, conclusively shown that the grass plant does not require silica as a supporting or strengthening material. Now we come to see the use of lime in the clay soil, especially in the case of the cultivation of cereals and pastural grasses. The lime added and mixed up with the soil acts on the soluble silicate of potash as it is formed, and combines with the silica, constituting, as I have already remarked, the practically insoluble silicate of lime, which, of course, being normally indissolvable in the soil, cannot pass into the body of the plant. Therefore, the organism profits by its exclusion, and as a consequence so does the farmer. The energies of the plant are not spent in getting rid of silica if there is no silica to get rid of, and its ordinary processes of nutrition can progress uninterruptedly.

The breaking up of the soluble silicate[1] could be as well accomplished by the perfect aëration of the soil so that every particle could be constantly exposed to fresh portions of aërial carbonic acid and oxygen ; and this is one great reason why fine deep tillage, where it is possible, so improves clayey soils, but of course a tillage that will bring about the perfect aëration of heavy clay, is all but impossible ; therefore the advantage of judiciously using lime.

The soluble alkaline silicate which, when undecomposed, passes into the plant in the water-stream through the roots, is evidently very soon split up by the vegetable, and the silica combined with some substance, such as an aldehyde, and carried on in solution in this state to the peripheries of the stem, &c., where, by the process of practically unrestrained evaporation, the compound is again split up, the aldehyde going off into the air, and the solid silica remaining stranded in the cuticle and the other walls, or occasionally even in the cavities of the epidermal layer.

[1] Only the alkali metals, of which potassium and sodium are the only two that normally occur in soils, form silicates soluble in water.

The silicate of lime formed in the above reaction itself ultimately undergoes decomposition by natural water containing carbonic acid, but the decomposition is always complete; it devolves entirely into carbonate of lime and free insoluble silica.

ALEXANDER JOHNSTONE.

Edinburgh University.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Prof. Sylvester is lecturing this term on the theory of numbers, and its application to the division of the circle into equal parts. Prof. Clifton and Mr. Baynes lecture on electricity, and Mr. Walker on physical optics. Prof. Odling's course is on ureas and uric acid, and Mr. Veley is lecturing on physical chemistry.

Prof. Ray Lankester is giving a general course on morphology, and Mr. Minchin a special lecture on Echinoderms. Dr. W. Benham has been appointed Demonstrator in the Morphological Laboratory in succession to Mr. P. C. Mitchell, who has been selected by the University Extension authorities as one of the lecturers who are to carry out the scheme of scientific education adopted by the County Council of Devonshire.

An examination for a Radcliffe Travelling Fellowship will take place before the end of Term. The Senior Mathematical Scholarship has been awarded to Mr. A. L. Dixon, Worcester College.

CAMBRIDGE.—The General Board of Studies announce the vacancy of the Lectureship in Advanced Morphology of Invertebrates, caused by the appointment of Mr. Weldon, F.R.S., to the Chair of Zoology in University College, London. The appointment is for five years, and the stipend £50 a year. Candidates are to apply to Prof. Sidgwick, Hillside, Cambridge, by February 3.

Prof. Macalister announces an introductory lecture on Thursday, January 29, at noon, by way of inaugurating the new anatomical lecture-theatre. His subject is "The History of Anatomical Study in Cambridge."

Prof. Roy announces six courses of lectures for the Lent and Easter terms in pathology and bacteriology, to be given by himself in collaboration with Mr. J. G. Adami, John Lucas Walker Student; Mr. E. H. Hankin, Fellow of St. John's; Dr. A. Gamgee, late Professor of Physiology at Owens College; and Mr. E. Lloyd Jones, Demonstrator of Pathology.

Prof. Ewing will lecture for the present in Sir George Stokes's lecture-room at the new Museum. He announces courses in strength of materials, theory of structures, mechanics, and geometrical and mechanical drawing.

The Botanic Garden Syndicate report that, while the new range of plant-houses, erected at a cost of £3000, has proved very satisfactory, it has been discovered that the remaining old houses are in a hopeless state of decay, and cannot with advantage be longer maintained. They propose that they be reconstructed at a cost of £2200.

The Special Board for Biology report that, during the five years ending Michaelmas 1890, the University's table in the Zoological Station at Naples, for which an annual grant of £100 has been made to Dr. Dohrn, has been occupied on eight occasions by Cambridge workers. In view of the importance of the opportunities there offered, and of the recent extensions and improvements that have been made, the Board propose that the grant be renewed for a further period of five years.

Dr. J. Griffiths, Assistant to the Professor of Surgery, is nominated an additional member of the Board for Medicine; Mr. W. N. Shaw, of the Board for Physics and Chemistry; Mr. S. F. Harner, of the Board for Biology.

Mr. T. Roberts and Mr. Acton, of St. John's, and Mr. Wilberforce, of Trinity, are appointed Examiners in Natural Science, in connection with the University Extension Scheme.

SCIENTIFIC SERIALS.

American Journal of Science, January.—On the alternating electric arc between a ball and point, by Edward L. Nichols. Let two wires, forming the terminals of the secondary coil of an alternate current transformer, be brought nearly into contact, one of them being armed with a point, the other with a ball. When the distance is such as to admit of a discharge between the two, a galvanometer in shunt around the ball and point will be found to indicate a considerable flow of continuous current, the direction being that which would result from a current flowing from the former to the latter. This phenomenon has recently been subjected to experiment by Messrs. Archbold and Teeple, who have determined the changes of electromotive force and current as the length of the arc formed between the ball and point is increased. Mr. Nichols also describes some observations made by Mr. F. C. Caldwell, from which it appears (1) that the discharge from the ball (positive) leaves the latter in a direction normal to the surface, but that it enters the other terminal at some distance from the apex; (2) that the discharge from the point (positive) leaves the apex of the latter, but is deflected into a course nearly 45° from the axis, and reaches the ball obliquely at some point on its side.—Deformation of the Algonquin Beach, and birth of Lake Huron, by J. W. Spencer.—The decimal system of measures of the seventeenth century, by Prof. J. Howard Gore. Evidence is adduced (1) that a priest, named Gabriel Mouton, of Lyons, proposed a system of measurement, based upon the scale of tens, about 1665; (2) that he derived his unit from the length of a minute of the terrestrial arc; (3) that he showed how this unit could be expressed in terms of the seconds pendulum.—On the Clinton Oolitic iron ores, by A. F. Foerste.—Effects of pressure on ice, by R. W. Wood, Jun. A solid steel piston was turned to work air-tight in a cylindrical cavity drilled in a block of cast-iron. A small hole, like the vent of a cannon, communicated with the bottom of the cavity. Ice was placed in the cavity, and the piston inserted. At a pressure of $4\frac{1}{2}$ tons to the inch, small cylindrical pieces of clear ice spurted from the orifice. To test at what pressure ice at the melting-point would become fluid as a mass, a similar iron block, without a vent, was filled with ice, in which were embedded several small bullets. The pressures were carried up to twenty tons per square inch without the ice being reduced to a liquid state. When this point was reached, fine jets of spray spurted in all directions from the block of iron; the ice was afterwards found, however, to have had sufficient viscosity to support the weight of the shot. It is possible, however, that the piston got jammed in the cylindrical cavity, and thus the ice might not be under the pressure indicated by the gauge. The experiments lead Mr. Wood to the conviction that any theory is questionable that accounts for peculiar motions of glacial ice by supposing the existence of a layer of pressure-molten water beneath the mass. —A review of the Quaternary era, with special reference to the deposits of flooded rivers, by Warren Upham. The Quaternary era, according to many authorities, began with the change from the mild Pliocene climate to that of the Glacial period, has continued to the present time, and must extend far into the future. Mr. Upham traces, in a very concise manner, the succession of events, glacial and fluvial deposits, and the changes in altitude and climate, from the beginning of this era to the present time.—On the illuminating power of flat petroleum flames in various azimuths, by Alfred M. Mayer. The flat flame of a Hitchcock lamp, in which combustion is maintained by a blast of air driven against the flame by a fan moved by clock-work, and the flame of an ordinary petroleum lamp were experimented upon. The latter flame was surrounded by a chimney, the former was not so inclosed. The following is a comparison of the candle-power of each flame at three of the azimuths at which photometric measures were made. The angles were measured from the plane of the flat flame.

Azimuth.	Hitchcok flame.	Ordinary flame.
0°	9·8	6·6
50	15·8	10·25
90	15·6	10·6

It therefore appears that the edges of the Hitchcock flame and the ordinary flame give, respectively, about 37 and 38 per cent. less light than the flat surface.—On the physical properties of hard-rubber or vulcanite, by the same author. The mean of twelve determinations of the coefficient of linear expansion of vulcanite, obtained by means of a specially devised piece of apparatus, gave the value 0·0000636, between the temperature at which the experiments were made, viz. 0° and 18° C. The cubical expansion of the substance is closely represented by the formula—

$$v_t = v_0 + 0{\cdot}000182t + 0{\cdot}00000025t^2.$$

The specific heat = 0·33125. The angle of maximum polarization of a polished surface of vulcanite was found to be 57° 29′. Hence the index of refraction = 1·568. The diathermancy has also been determined.—On some remarkably developed calcite crystals, by Louis V. Pirsson.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 15.—"Note on the Present State of the Theory of Thin Elastic Shells." By A. E. H. Love, M.A., St. John's College, Cambridge. Communicated by Lord Rayleigh, Sec.R.S.

This is a note in correction of the author's paper in Phil. Trans., A., 1888. In that paper it was shown that, when no surface tractions are applied to the surfaces of the shell, the potential energy per unit area can be expressed in the form $Ah^3W_1 + BhW_2$, where $2h$ is the thickness of the shell, and A and B are elastic constants. The function W_2 depends on quantities expressing the extension of the middle surface, and W_1 depends on quantities defining the changes of curvature; and it was shown that it is impossible to satisfy the boundary conditions which hold at a free edge, except by taking account of the term in W_2. A theory of the vibrations of bells was therefore proposed in which the term in W_2 was retained, and the term in W_1 rejected, *i.e.* the vibrations were supposed to depend mainly on the extension of the middle surface. This theory was in opposition to Lord Rayleigh's theory in Proc. Lond. Math. Soc., 1882, which was founded on the supposition that the middle surface remains unstretched. In December 1888 Lord Rayleigh proved from general principles that the mode of deformation corresponding to the gravest tone cannot be included among the extensional modes; but it has not yet been shown how, in any particular case, the boundary conditions can be satisfied by a mode of oscillation depending mainly on bending. The solution of the difficulty has, however, been recently pointed out by Mr. Basset and Prof. Lamb. Each of these writers has proved that, in particular statical problems relating to cylinders, the quantities expressing the extension can be very small everywhere except in the neighbourhood of an edge, and *there* they may increase with such rapidity as to secure the satisfaction of the boundary conditions, the total potential energy due to extension, which varies as the surface integral of hW_2 over the middle surface, being, nevertheless, negligible in comparison with that due to bending, which varies as the surface integral of h^3W_1. There is little doubt that, as Mr. Basset and Prof. Lamb suggest, all this will hold equally in the case of a vibrating shell under ordinary conditions. Some detailed corrections of the author's previous paper were given

Zoological Society, January 6.—Prof. Alfred Newton, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of December 1890.—Mr. Sclater exhibited some sketches made by Lieut. W. E. Stairs, R.E., of the horns of a large antelope, apparently new to science, which had been met with by the Emin Pasha Relief Expedition in the forest-district of the Aruwimi River.—Mr. G. A. Boulenger read the description of a new lizard of the genus *Ctenoblepharis* obtained in the Province of Tarapacà, Chili, by Mr. A. A. Lane, which he proposed to describe as *Ctenoblepharis jamesi*.—A second paper by Mr. Boulenger contained an account of some specimens of extinct and fossil Chelonians preserved in the Museum of the Royal College of Surgeons.—Mr. F. E. Beddard gave an account of certain portions of the anatomy of the Kagu (*Rhinochetus jubatus*) as observed in specimens lately living in the Society's Gardens.—Lieut.-Col. H. H. Godwin-Austen, F.R.S., read a paper on the land-shells collected in Borneo by Mr. A. Everett, Mr. Whitehead, and others. In this communication (the second of the series) the author gave a list of the species of the families Zonitidæ and Helicidæ as known from Borneo up to the present time. He described the anatomy of several species and defined two new genera (*Diukia* and *Everettia*), pointing out how they differ from previously known genera founded on anatomical characters.

Geological Society, January 7.—A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On the north-west region of Charnwood Forest, with other notes, by the Rev. E. Hill and Prof. T. G. Bonney, F.R.S. The paper contains the results of a re-examination of the north-west region, when the authors had the advantage of using the six inch Ordnance-map, published since the completion of their former work. In this they had expressed the opinion that the rock of Peldar Tor and that of High Sharpley were somewhat altered pyroclastics, being much influenced by the non-igneous origin asserted for the "porphyroids" of the Ardennes. But in 1882 one of them had visited this region, and was then convinced that the porphyroids, which closely resembled the rock of Sharpley, were felstones which had been rendered schistose by subsequent pressure. The result of their subsequent work in Charnwood has convinced the authors that the rocks of Sharpley and of Peldar Tor are in the main of a like origin and history. The mass of Bardon Hill, where the quarries have been much enlarged, has also been studied, and some details in the section formerly published have been corrected. The schistose bands, on which the authors relied as marking horizons for stratigraphical purposes, prove to be zones of exceptional crush. The occurrence of a rock exactly resembling that of Peldar Tor is fully established. It is extremely difficult to decide upon the true nature of the rocks which are chiefly worked in the pit, but the authors remain of opinion that for most of them a pyroclastic origin is the more probable. Some notes are added upon the relations of the holocrystalline igneous and the sedimentary rocks of the Forest, upon the Blackbrook group, and upon the fragments and pebbles in certain of the coarser ashy deposits. Some remarks are made upon the glacial phenomena exhibited in the Forest region; these indicate that this cannot have been overridden by a great northern ice-sheet, and it does not afford the usual signs of the action of local glaciers. At the same time it has been a centre of dispersion for erratics, especially towards the south and southwest, these being found sometimes more than twenty miles away. Hence, in the opinion of the authors, the erratics have been distributed by floating ice during an epoch of general submergence. Some minor "corrigenda" in the earlier papers are noted, with certain changes in the names of localities, bringing them into harmony with the six-inch map.—Note on a contact-structure in the syenite of Bradgate Park, by Prof. T. G. Bonney. The author described a specimen, obtained at Bradgate Park, showing a junction of the syenite and slaty rock of Charnwood. The latter rock is very slightly altered; the former exhibits a number of grains of felspar and quartz set in a matrix which has now a "trachytic," now a devitrified structure. He traced the former into the "micrographic" structure observed generally in these syenites, and discussed its significance. His study of these structures in this and many other instances led him to infer that they generally indicated that the rock, at a late stage, had consisted of a mixture of previously formed crystalline grains and a viscous magma, that the temperature of the mass had been comparatively low, that it had cooled rather gradually, and that the condition of the magma—*i.e.* one of very imperfect fluidity—had not permitted of free molecular movements among its constituents. Thus this structure, together with certain others mentioned, might be regarded as indicative of "crystallization under constraint." The reading of these two papers was followed by a discussion, in which Prof. Blake, Dr. Callaway, Mr. Gregory, General MacMahon, the President, and Prof. Bonney took part.—On the unconformities between the rock-systems underlying the Cambrian quartzite in Shropshire, by Dr. C. Callaway. In the course of a discussion on this paper, some remarks were made by Prof. Blake, Prof. Bonney, Dr. Hicks, and the author.

Anthropological Institute, January 13.—Mr. E. W. Brabrook, Vice-President, in the chair.—Mr. Lewis exhibited a specimen of the stone used by Admiral Tremlett to cut marks on the granite of which the Breton dolmens are composed.—Mr. R. B. Martin exhibited a fire-syringe from Borneo.—Mr. C. H. Read exhibited some specimens of worked jade from British Columbia, and a bored stone from San Juan, Teotihuacan.—Mr. J. Edge Partington and Mr. C. Heape exhibited an ethnographical album of the Pacific Islands.—Mr. F. W. Rudler read a paper on the source of the jade used for ancient implements in Europe and America. Its object was to call the attention of anthropologists to certain mineralogical discoveries which have been made within the last few years, and which tend to overthrow the well-known theory which suggested early intercourse with the East as the source of the jade objects found in the lake-dwellings of Switzerland, the prehistoric burial-places of France and Germany, and the ancient Indian graves on the north-western coast of America. Herr Traube, of Breslau, first recorded the occurrence of jade *in situ* at Jordansmühl, in Silesia, and afterwards discovered it at the arsenical-pyrites workings at Reichenstein. Rough pebbles have also been found in the valleys of the Sann and the Mur, in Styria. Dr. G. M. Dawson has described the occurrence of boulders of jade, partly sawn through, at Lytton and Yale, on the Fraser River; and Lieutenant Stoney has actually found the mineral *in situ* at the Jade Mountains, north of the Kowak River, in Alaska. These discoveries prove that, contrary to general belief, jade does occur in the rocks of Europe and of North America, thus supporting the view so long held by Dr. A. B. Meyer, of the Royal Zoological Museum in Dresden, and accepted in America not only by Dr. Dawson, but by Prof. F. W. Clarke and Mr. Merrill, Mr. Kunz, and others. In England, most anthropologists have hitherto inclined to the exotic rather than to the indigenous origin of the prehistoric jades.

Linnean Society, January 15.—Prof. Stewart, President, in the chair.—The President exhibited a bunch of holly berries which were remarkable for being perfectly black instead of red, but which in no other respect looked abnormal. The peculiarity was attributed to the effect of a fungus.—Mr. J. E. Harting exhibited a male specimen of the wigeon (*Anas penelope*) which had been shot in Ireland, and which had a tassel of feathers about an inch in length depending from the under side of the neck. The explanation suggested was that it was the result of a former shot wound, when the pellet, as often happens, plugged the wound with feathers, and the skin had grown round and below the obstruction.—A paper was then read by Dr. P. H. Carpenter, on certain points in the morphology of the *Cystideæ*, which were admirably demonstrated with the aid of diagrams. A discussion followed, in which Mr. H. Bury and Mr. Bather took part.—On behalf of Mr. Thomas Kirk, of Wellington, New Zealand, the Secretary read an interesting report of a botanical visit to the Auckland Islands.

Royal Meteorological Society, Jan. 21.—Annual Meeting.—Mr. W. H. Dines, Vice-President, in the chair.—Dr. Tripe read the Report of the Council, which stated that the progress of the Society during the past year had been of a satisfactory character. Among the investigations carried on by the Society are the following: the organization of a large number of meteorological stations, the observations from which are examined and reduced by the staff, and printed in the *Meteorological Record*; the regular inspection of these stations by the assistant secretary; the collection and discussion of phenological observations; and an inquiry into the thunderstorms of 1888 and 1889. An exhibition of instruments is held annually in March. During the year a complete catalogue of the library, extending to 222 pages, has been compiled and published. The library has so much overgrown the present limited accommodation, that the Council have been obliged to consider the question of obtaining more commodious rooms, and have consequently inaugurated a "New Premises Fund," which is being well supported by the Fellows.—After the adoption of the Report, the Officers and Council for the ensuing year were elected.——At the ordinary meeting the following papers were read:—Note on a peculiar development of cirrus cloud observed in Southern Switzerland, by Mr. R. H. Scott, F.R.S.—Some remarks on dew, by Colonel W. F. Badgley. These are notes on observations which were made to discover whether all dew is deposited from the air, or if some also comes from the earth and plants, and also what quantity is formed during the year. The conclusions which the author deduces from his observations are: (1) that the earth always exhales water vapour by night, and probably a greater quantity by day; (2) that the quantity of water vapour given off by the earth is always considerable, and that any variation in the quantity is mainly due to the season of the year; (3) that the greater part of the dew comes from the earth vapour; and (4) that plants exhale water vapour, and do not exude moisture. The total quantity of dew collected on the author's grass plates in the year was 1·6147 ins.

Entomological Society, January 21.—Fifty-eighth Annual Meeting.—Lord Walsingham, F.R.S., President, in the chair.—An abstract of the Treasurer's accounts having been read by Mr. H. Druce, one of the auditors, the Secretary, Mr. H. Goss, read the Report of the Council.—It was then announced that the following gentlemen had been elected as officers and Council for 1891:—President, Mr. Frederick DuCane Godman, F.R.S.; Treasurer, Mr. Robert McLachlan, F.R.S.; Secretaries, Mr. Herbert Goss and the Rev. Canon Fowler; Librarian, Mr. Ferdinand Grut; and as other members of the Council, Prof. R. Meldola, F.R.S., Mr. Edward Saunders, Dr. D. Sharp, F.R.S., Mr. Richard South, Mr. Henry T. Stainton, F.R.S., Colonel Charles Swinhoe, Mr. George H. Verrall, and the Right Hon. Lord Walsingham, F.R.S. It was also announced that the new President had appointed Lord Walsingham, Prof. Meldola, and Dr. Sharp, Vice-Presidents for the session 1891–92.—Lord Walsingham, the retiring President, then delivered an address. After alluding to some of the more important entomological publications of the past year, and making special mention of those of Edwards and Scudder in America, of Romanhoff in Russia, of the Oberthürs in France, and of Godman and Salvin in England, the President referred to Mr. Moore's courageous undertaking in commencing his "Lepidoptera Indica," on the lines adopted in his "Lepidoptera of Ceylon." Attention was then called to the unusual development during the past year of the study of those problems which have been the object of the researches of Darwin, Wallace, Weismann, Meldola, Poulton, and others, and to the special and increasing literature of the subject. In this connection allusion was made to Mr. Tutt's "Entomologist's Record and Journal of Variation," to Mr. Poulton's valuable book "On the Meaning and Use of the Colours of Animals," and to the interesting and important papers and experiments of Mr. F. Merrifield on the subject of the variation in Lepidoptera caused by differences of temperature. After alluding to the International Zoological Congress held at Paris during the past year, and to the rules of nomenclature which had been once more reviewed and revised, the President concluded by referring to the losses by death during the year of several Fellows of the Society and other entomologists, special mention being made of Dr. J. S. Baly, M. l'Abbé de Marseul, Mr. Owen Wilson, M. Lucien Buquet, M. Eugène Desmarest, Prof. Heinrich Frey, Dr. R. C. R. Jordan, Mr. W. S. Dallas, Dr. L. W. Schaufuss, Dr. Hermann Dewitz, M. Louis Reiche, and Herr Peter Maassen.—A vote of thanks to the President and other officers of the Society having been passed, Lord Walsingham, Mr. Goss, and Mr. Grut replied, and the proceedings terminated.

PARIS.

Academy of Sciences, January 19.—M. Duchartre in the chair.—On the estimation of mineral matters contained in vegetable moulds and on their *rôle* in agriculture, by MM. Berthelot and G. André. The authors have previously described methods for accurately estimating phosphorus, sulphur, carbon, silica, aluminium, iron, soda, potassium, and other substances, in soils, vegetable moulds, and plants. They now devote special attention to the determination of alkalies and oxides, after eliminating silicates by treatment with hydrofluoric acid.—On the presence and *rôle* of sulphur in vegetables, by MM. Berthelot and G. André. The results of a series of experiments indicate, among other things, that the proportion of sulphur increases to the time of efflorescence, when it attains a maximum, and then decreases.—Experiments on the mechanical actions exercised on rocks by gases at high temperatures and pressures and in rapid motion, by M. Daubrée. The author has continued his researches on the effects produced upon rocks in contact with gases suddenly developed by means of such explosives as gun-cotton and dynamite. Temperatures of 2500°, and pressures of 1100 atmospheres, thus obtained, have been sufficient to fuse and pulverize the rocks experimented upon in a very marked manner. The results lead M. Daubrée to believe that the perforated pipes or *diatrèmes*, diamantiferous, volcanic, or otherwise, and much of the subaërial dust and oceanic deposits are formed by such actions as he has obtained in the laboratory. He also shows that rocks may acquire an apparent plasticity under the influence of pressure.—Contribution to the botanical history of the truffle; second note, *Terfas*, or the truffles of Africa and Arabia, of the genera *Terfezia* and *Tirmania*, by M. A. Chatin.—Description and employment of the Eucalyptus, by M. Ch. Naudin. About eighty species of Eucalyptus have been cultivated at Thuret. Of these fifty-six are described in a memoir presented by M. Naudin. The memoir will be of great service to applied botany.—Influence of dissolvents on the rotatory power of camphols and isocamphols, by M. A. Haller. Of eleven dissolvents used, only methyl alcohol exercised an influence on the rotatory power of left-handed α-camphol. The action exercised by different liquids on left-handed isocamphol appears to vary with their constitution, but is the same for each homologous series.—On the destruction of sugar in the blood *in vitro*, by MM. R. Lépine and Barral.—Memoir on the constitution of albuminoids, by Dr. H. Arnaud.—Observations of a star having a brightness comparable to that of Regulus, and situated in the same constellation, by M. E. Lescarbault. (See our Astronomical Column.)—*Résumé* of solar observations made at the Royal Observatory of the Roman College during the last six months of 1890, by M. P. Tacchini. (See our Astronomical Column.)—Observations of sun-spots made in 1890 with the Brunner equatorial (0·18 metre aperture) of Lyons Observatory, by M. E. Marchand.—A new gyratory apparatus, called the alternating gyroscope, by M. G. Sire.—On the telephonic reproduction of speech, by M. E. Mercadier.—Researches on a sulphur derivative of olein called *l'huile pour rouge*, by M. Scheurer-Kestner.—On the experimental production of exophthalmia, by M. H. Stilling.—On the variations of the pelvis of Cachalots, by MM. G. Pouchet and H. Beauregard.—On the character of the conchological fauna of the Sahara, by Dr. P. Fischer. M. J. Dybowski explored the environs of El Goleah last year, and made a collection of land and water mollusks in the sub-fossil state. These animals testify to the existence, in relatively recent times, of ponds and watercourses in a region which may be taken as a type of many others. The inhabitants of the neighbourhood have a tradition that at some past age El Goleah was in the middle of a fertile and well-watered region, and this opinion seems now to be confirmed.—On the development of larvas of Ascidiæ, by M. A. Pizon.—On two new zoospores, parasites of the muscles of fishes, by M. P. Thélohan.—On the presence of native nickel in the sands of the Elvo torrent, near Biella (Piémont), by M. Alfonso Sella. An examination of auriferous sand from the Elvo torrent showed to M. Sella small metallic grains consisting of native nickel associated with iron. The mineral is analogous to certain meteoric irons, such as the Ovifak, but appears to be of terrestrial origin.

DIARY OF SOCIETIES.

London.

THURSDAY, January 29.

Royal Society, at 4.30.—The Bakerian Lecture, "On Tidal Prediction: Prof. G. H. Darwin, F.R.S.

Institution of Mechanical Engineers, at 7.30.—Annual General Meeting.—On some Different Forms of Gas Furnaces: Bernard Dawson.—On the Mechanical Treatment of Moulding Sand: Walter Bagshaw.—Fourth Report of the Research Committee on Friction; Experiments on the Friction of a Pivot Bearing.

Royal Institution, at 3.—The Little Manx Nation: Hall Caine.

FRIDAY, January 30.

Institution of Mechanical Engineers, at 7.30.—Annual General Meeting.—On some Different Forms of Gas Furnaces: Bernard Dawson.—On the Mechanical Treatment of Moulding Sand: Walter Bagshaw.—Fourth Report of the Research Committee on Friction; Experiments on the Friction of a Pivot Bearing.

Institution of Civil Engineers, at 7.30.—The Counterbalancing of Locomotive Engines: Edmund L. Hill.

Royal Institution, at 9.—On the Rejuvenescence of Crystals: Prof. J. W. Judd, F.R.S.

SATURDAY, January 31.

Royal Institution, at 3.—Pre-Greek Schools of Art: Martin Conway.

SUNDAY, February 1.

Sunday Lecture Society, at 4.—The Life and Death of Worlds: Mrs. S. D. Proctor.

MONDAY, February 2.

Society of Arts, at 8.—The Construction and Capabilities of Musical Instruments: A. J. Hipkins.

Royal Institution, at 5.—General Monthly Meeting.

TUESDAY, February 3.

Zoological Society, at 8.30.—On the Question of Saurognathism of the Pici, and other Osteological Notes upon that Group: Dr. R. W. Shufeldt, C.M.Z.S.—On Two New Species of Parrots of the Genus Platycercus: Count T. Salvadori.—On a Collection of Birds from Tarapacà, Northern Chili: P. L. Sclater, F.R.S.

Institution of Civil Engineers, at 8.—Ballot for Members.—Electric Mining Machinery: Llewelyn B. and Claude W. Atkinson.—At 9.—Reception by the President and Council.

Royal Institution, at 3.—The Structure and Functions of the Nervous System; Part I. The Spinal Cord and Ganglia: Prof. Victor Horsley, F.R.S.

WEDNESDAY, February 4.

Geological Society, at 8.—The Geology of Barbados and the West Indies; Part I. the Coral Rocks: A. J. Jukes-Browne and Prof. J. B. Harrison.—On the Shap Granite: A. Harker and J. E. Marr.

Entomological Society, at 7.—The Life-history of the Hessian Fly: Frederick Enock.—On some Recent Additions to the List of South African Butterflies: Roland Trimen, F.R.S.—Additions to the Carabideous Fauna of Mexico, with Remarks on Species previously recorded: Henry W. Bates, F.R.S.—Notes on the Genus Xanthospilopteryx, Wallgr.: William F. Kirby.—On the Rhyncophorous Coleoptera of Japan: Dr. David Sharp, F.R.S.—On Mimetic Resemblances between Species of the Coleopterous Genera Lema and Diabrotica: Charles J. Gahan.

Society of Arts, at 8.—Decimal Coinage, Weights, and Measures: J. Emerson Dowson.

THURSDAY, February 5.

Royal Society, at 4.30.

Linnean Society, at 8.—On the Tree Ferns of Sikkim: J. Gammie, Jun.—Life-history of Two Species of Puccinia: A. Barclay.

Chemical Society, at 8.

Royal Institution, at 3.—The Little Manx Nation: Hall Caine.

FRIDAY, February 6.

Geologists' Association, at 7.30.—Annual Meeting.—Further Notes on the Geological Record: The President.

Royal Institution, at 9.—Some Applications of Photography: Right Hon. Lord Rayleigh.

SATURDAY, February 7.

Geological Field Class, at 4.15.—The Gravel Beds of the Thames and its Tributaries in Relation to Ancient and Modern Civilization: Prof. H. G. Seeley, F.R.S.

Royal Institution, at 3.—Pre-Greek Schools of Art: W. Martin Conway.

CONTENTS.

THURSDAY, FEBRUARY 5, 1891.

THE BIRDS OF NORFOLK.

The Birds of Norfolk, with Remarks on their Habits, Migration, and Local Distribution. By Henry Stevenson, F.L.S. Continued by Thomas Southwell, F.Z.S. In 3 Vols. (Norwich and London: Gurney and Jackson, 1866-90.)

PRIMACY among all works of its kind has everywhere been accorded to the late Mr. Stevenson's "Birds of Norfolk," from the appearance of its first volume so long ago as 1866. The second, which came out in 1870, surpassed its predecessor in subjects of interest and happy treatment, so that great dismay naturally fell upon ornithologists when they came to learn about 1877 that the author's health had given way, and that, though considerable advance had been made in the third volume, the prospect of the work being completed was imperilled. So matters remained until 1888, when his eyes were closed in death. Thanks to Mr. Southwell, the anticipated danger has been avoided. To him Mr. Stevenson's papers were intrusted, and so admirably have they been used, that but for the narrow "leads" of the continuation, and the constant reference to Mr. Stevenson's own notes, one could hardly detect the change of hand, for that of the continuator is just like that of the original, "only more so"—by which Americanism we mean to imply that the good qualities which especially distinguished the two earlier volumes of "The Birds of Norfolk" from other county ornithologies are intensified in that which ends the work.

What these good qualities are may need some setting forth; but first is the command of his subject shown by each of these gentlemen. Too often it has been our lot, on taking up books of this kind, to find that whenever they depart from the mere catalogue the writer has a tendency to "slop over"—which tendency he generally obeys, and we are indulged consequently in matters which had far better be left to works which have not so limited a scope. It would be easy to name a local ornithology in which the author thought it expedient to include a somewhat elaborate description of each species mentioned—the descriptions being compiled from the standard authority on British birds; while others profess to give a list of the whole British avifauna, leaving the species not found in the particular district to which the book relates without comment, and their less educated readers in bewilderment. Or, again, an author may fill his book with figures that rather caricature than represent the subjects of which he is writing. Mr. Stevenson and his follower have been far wiser. They take for granted that most of their readers will know or be able to find out what the species treated of are like, and they occupy the space thus saved with information much more to the point, and therefore of greater value. It may be said, and truly enough, that Norfolk is an exceptional county. It is not only proud of its birds and of its ornithologists, but it has two Museums that do it credit, and to journey either to Norwich or King's Lynn is no great pilgrimage for any man or woman within its bounds to perform once a year or oftener. As regards local treasures, the Norwich Museum stands probably first in Europe, certainly we have none like it elsewhere in Britain, for they consist in great part not merely of adventitious strangers from afar that have had the ill luck to be immolated on arrival in this inhospitable land (though of these there is good store), but of native examples of species which in old times inhabited the county, though long since driven hence by agricultural operations and the like. It is local specimens of this kind which are of real and not fanciful worth, though the ordinary "collector" almost always overlooks the difference. In the present work it is made very plain to those who know how to read and can appreciate the carefully-written histories given by Mr. Stevenson and Mr. Southwell of species after species which once dwelt in Norfolk, or even yet occur there though in greatly reduced numbers. These histories are often fascinating, and always have an interest which no information subsequently collected is likely to lessen, for in the first place such information is chiefly traditional, so that some portion of it is lost with the death of every ancient inhabitant; and, though much has been saved by the inquiries and exertions of Mr. Stevenson and his friends, there is no doubt that much more would have been obtained had similar efforts been set on foot earlier. The extinction, as regards Norfolk, of several species which has taken place within the lifetime of men who yet survive is still very imperfectly understood, though there is a notable exception in the case of the Bustard; but this bird from its grandeur and certain other qualities attracted unusual attention, at least in East Anglia, though not enough in other parts of England to enable anyone to give the details of its extirpation in such a way as they are given by the authors of this work. One would fain hope that the process of extermination has ceased, but that we believe cannot be, though it is plain, from much that is stated in the third and concluding volume of "The Birds of Norfolk," which we, in our present remarks, have in especial view, that it has been salutarily checked. In the excellent account of the Great Crested Grebe—or Loon, to use its common Norfolk name—which Mr. Stevenson wrote some thirty years ago, the extinction of that fine species was shown to be almost imminent, and certainly there was sufficient ground for entertaining the gloomy anticipations which therein will be found. But a few years after that time, the first of the several Bird Preservation Acts, which the Legislature was induced to pass under the influence brought to bear by the Close Time Committee of the British Association, came into operation; and this species, the ornament of so many of the meres and broad waters of Norfolk, was saved, as Mr. Southwell is able joyfully to record in his picturesque continuation of Mr. Stevenson's mournful history. The Loon has even reappeared, he says, in some of the localities from which it had long ago been shot down—to make ladies' muffs, be it remembered. But this is not the only success of the Acts which he recounts. He does not hesitate to ascribe to recent legislation the establishment or re-establishment as regular and numerous breeders in the county of no fewer than four species of Duck, three of them at least being among those which are most highly esteemed for the table. Yet something is still wanting. All frequenters of the wilder and more desolate parts of our coasts in summer-time know the rapture inspired by the sight of a company of Sea-Swallows or Terns as they pass along the shore with their graceful, dancing flight. Not so very long ago this delightful spectacle might be witnessed in numerous places, and there were many spots to which the observer might be led where he could, if so minded, be entertained for hours by watching their aërial evolutions, for these were the chosen homes of these beautiful and wholly harmless creatures. There, on secluded sand-hills, among the campion or the marram, could be found their nests, with the variously tinted and spotted eggs or the mottled down-clad young; while on the bare shingle itself might be discovered by the trained eye the slight hollow containing the progeny of the smallest and most delicate species of the genus. In days anterior to the Close Time Acts these spots were commonly the scene of useless and brutal outrage on the part of the gun-bearing miscreant; and even now it is evident that their sequestered position tempts the person who miscalls himself a naturalist, but is generally a trafficker in "plumes," to deeds of blood which he would be ashamed to own in decent society. There is at present scarcely any kind of native bird that needs protection more strongly than the confiding Tern and all his kind. Its home is on the shore, which is free to all men—be they honest or murderers—and it enjoys none of the protection which the law of trespass affords to birds which nest on land belonging to some definite proprietor. It is plain, from the caution Mr. Southwell shows in not naming localities, that he believes, and we think with reason, the prolonged existence of Terns on the Norfolk coast to be perilous in the extreme. For ourselves, we cannot see why it should be in the power of any man—be he a blackguard or only a blockhead—to deprive his fellow-men of one of the most delicious sights of the sea-coast, simply because, by the common law of England, the foreshores are the property of the Crown, and he chooses to desecrate them for his own selfish purpose.

Those who study this volume will, however, find in it much more than has been indicated by the preceding remarks. The portions relating to Swans will prove good reading to persons of many tastes. The Swans of Norfolk, if not so widely known as its Turkeys, deserve as much celebrity, as those will admit who have eaten a fatted Cygnet from the St. Helen's Swan-pit—the only Swan-pit, we believe, in England now in working order. Here we have a good description of it, and, more than that, an account of Swan-upping (*i.e.* the taking up of the young Swans) on the Yare—a very much larger affair than that which attracts Londoners to the yearly Swan-upping on the Thames; while the curious subject of Swan-marks, which has so many charms for the antiquary, is duly if not exhaustively treated. Of more strictly ornithological interest, there is a valuable dissertation, by Mr. Stevenson, on that mysterious entity, the so-called "Polish Swan"—a subject on which Mr. Southwell was already a chief authority, as he was on that of decoys. The treatment of this last topic falling to his lot, he naturally refers his readers to his own papers—in the second edition of Lubbock's "Fauna of Norfolk," and in the second volume of the Transactions of the Norfolk and Norwich Nâturalists' Society—for the many particulars which it would have been useless here to repeat

An opinion has obtained among writers of local faunas that it is advantageous to draw up what they are pleased to designate a "strong list"—meaning thereby to enrol as many species in their work as by any excuse, and a liberal extension of the rules of evidence, they could possibly include. Our present authors have gone upon the very opposite plan, and assuredly it is the right one. Instead of welcoming on the slightest testimony every report of the appearance of a misguided straggler, these naturalists of Norfolk have not only required proof positive of the fact, but that its occurrence should be free from the taint of human complicity. Consequently, the list of so-called Norfolk birds has again and again been weeded of all weak members, and whatever be its number, which we have been no more careful to count than has Mr. Southwell, it is obviously to be accepted without hesitation; even stragglers from the Alps or the Antilles, like the Wall-Creeper and the Capped Petrel—the latter being of more general than special interest—since, after the researches of Mr. Ober and Colonel Feilden, at its ancient homes in Dominica and Guadeloupe, there cannot be much doubt that it must belong to the category of extinct species like the Great Auk, the Labrador Duck, the Phillip-Island Nestor, and many others—while the former will always be remembered for the share which Gilbert White, of happy memory, took in certifying its first occurrence in England nearly one hundred years ago, and a *fac-simile* of the drawing of two of its feathers sent to him by Robert Marsham is here given.

We must not conclude our review without mentioning that this third volume of Mr. Stevenson's work contains three beautiful plates by that unrivalled zoological artist, Mr. Wolf, whose labours have now, alas! practically ceased, and these plates are enough to give the book an especial value. That representing the "Gullery" at Scoulton is a picture on which lovers of Nature will for ever fondly dwell. Clever sketches of similar scenes have before appeared; but nothing of the kind approaching the accuracy, or what is called "feeling," has hitherto been published.

The reviewer's grumble, after all this praise, must be finally stated. This work has three faults. It wants a map, showing the localities mentioned and indicating the natural districts of the county, so well described in Mr. Stevenson's introduction, and an index of the local names of the birds; but worse than all, and absolutely destructive to its existence, the third volume is bound with *wire*—wherefore let all buyers of it take warning, and get it out of its case as speedily as possible to save pages so well worth preserving from the effects of corrosion.

SCIENCE WITHOUT TEARS.

The Threshold of Science. By C. R. Alder Wright, D.Sc., F.R.S. (London: C. Griffin and Co., 1890.)

IT was hardly to be expected that a chemist of such scientific distinction as Dr. Alder Wright would undertake to produce a new edition of a well-known popular work entitled "The Magic of Science," which was intended to amuse rather than to instruct its readers. It s therefore not surprising to find that, instead of preparing a new edition of this work, Dr. Wright has compiled the present volume. It represents a compromise between an ordinary text-book written to instruct and a play-book written to amuse. Like most compromises of the kind it is unsatisfactory. It attempts an impossible combination, which fails to satisfy perfectly either those who wish to learn or those who want to play. Dr. Wright refers in his preface to the change in the school curriculum that has occurred during the last thirty years through the introduction of physical science, and he is in sympathy with those who are anxious to arrange a course of instruction suitable for the mental education of boys and girls, most of whom may not become physicists or chemists, or indeed follow any profession in which physical science has a special application. Dr. Wright quotes with approval a part of the second Report of the British Association Committee on Teaching Chemistry, in which stress is laid on the importance of teaching children the science of common things instead of trying to make them learn systematic chemistry and physics. He thinks that this book, which he has named "The Threshold of Science," will to some extent furnish such a course of instruction, while at the same time it will serve to amuse its readers.

"The object is to provide a kind of 'play-book,' which, in addition to affording the means of amusement, shall also to some extent tend in the direction of that course of mental education advocated by the British Association Committee; so that, whilst the juvenile philosopher finds pastime and entertainment in constructing simple apparatus and preparing elementary experiments, he may at the same time be led to observe correctly what happens, to draw inferences, and make deductions therefrom."

While there is no doubt some truth in this statement, as we shall have occasion to point out later, yet it must be clearly understood that this book is not suitable for use in schools. In certain cases it may serve in leisure time as a useful supplement to the systematic instruction of the school, particularly by arousing interest in experimental work, but it can never in any case form a substitute for it. The fatal objection to the book from the educational stand-point is the want of method in its arrangement. Dr. Wright tells us that "not being written with a view to aid in 'preparing for examinations,' nor in accordance with any particular syllabus of requirements, the subject-matter is not arranged in any specially methodical order." The experiments often have no logical connection with each other, and the explanations given of them are frequently so meagre that their educational value is extremely small. The book (of 390 pages) is divided into nine sections, which nominally relate to the "States of Matter," "Physical Changes of State due to Heat and Pressure and not accompanied by Chemical Action," "Changes of State due to Solution not accompanied by Chemical Action," "Solution and Separation from Solution by Actions involving Chemical Changes," "Chemical Actions producing Change of State without the Employment of Solvents," "Physical Adhesion and Allied Phenomena of Surface Action," "Effects of Heat upon Bodies, other than Change of State and Production of Chemical Action," "Radiant Action—Visible Light," "Radiant Action—Invisible Light." Under these sections are described a variety of experiments, many of them quite popular in the sense that while they are easy to make if the directions given are closely followed, it is impossible for a boy who knows no chemistry or physics to fully comprehend the nature of the changes that have taken place. For example, Experiment 13 (in all 404 experiments are described) enables a boy "to produce a gas smelling like rotten eggs by double decomposition." Iron sulphide is decomposed by hydrochloric acid. Anyone will be able to produce the gas, but if he is ignorant of chemistry he will not be much wiser for the information "that double decomposition will take place, resulting in the evolution of the gas hydrogen sulphide whilst the complementary product iron chloride will remain in solution." If physical science is to serve the purpose of an educational instrument, the principal object of making experiments should be to discover facts and verify suppositions. It is only when the learners are put, as far as possible, in the attitude of discoverers that the scientific method of reasoning is acquired. In schools, at all events, there should be no indiscriminate and haphazard experimenting.

The unmethodical character of the book, which unfits it for scholastic purposes, might seem to recommend it as a play-book. But its style is far too didactic for such a purpose, and some of the experiments are too difficult, others too costly, or for various reasons unsuitable. It would be decidedly unsafe to allow boys, unfamiliar with chemistry, to perform many of the experiments described in this book except under competent supervision, the necessity for which is, however, not alluded to. Experiments with hydrogen sulphide, chlorine, bromine, cyanogen, and other poisonous substances, should be excluded from a book which may be used by irresponsible boys who have previously received no chemical instruction and who are bent on amusement or mischief. The production of luminous writing on a wall by means of a stick of phosphorus "held by a towel or pair of tongs" is an operation by no means to be generally recommended, any more than are other experiments with phosphorus—the smearing of the face and hands with a solution of phosphorus in oil, the manufacture of "Fenian fire," a name given to its solution in carbon disulphide—all of which are attended with considerable danger to the inexperienced. The highly ingenious device for setting a "pond or bucket of water on fire," by means of potassium and ether contained in a jar which is plunged into the water, is another operation not free from danger in unskilled hands. The preparation of dry ammonia gas, its collection over mercury, and the demonstration of its absorptibility by ice, are operations far too difficult for a beginner to perform by himself, even if the cost of the mercury were not a hindrance. The same may be said of many other experiments.

So much by way of criticism. It seemed necessary in the interests of science to insist that the book (for which a less ambiguous title might have been found) is unsuitable for schools, and in the interest of those who may use it as a play-book that some supervision and previous chemical knowledge are desirable.

For a boy who is receiving systematic instruction in physical science at school, and whose experimental performances at home can be to some extent supervised, the book is admirably adapted, and such a boy will find it a fascinating as well as an instructive holiday companion.

The volume, on which Dr. Wright has evidently bestowed great pains, contains an astonishing amount of useful and accurate information about common things, and the extreme ingenuity of many of the experiments deserves the highest praise. Teachers of elementary science will find in it much that is suggestive; and many of the experiments, sometimes slightly modified, might be profitably incorporated in a school course.

A work on the "Threshold of Science" in the educational sense of the phrase has yet to be written. The task is a difficult one, and very few men have the originality, knowledge, and scholastic experience which are alike necessary for its accomplishment. Owing to several circumstances, the value of instruction in the methods of acquiring "natural knowledge" has not been fully recognized in the school curriculum. It cannot be expected that much general progress will be made until teachers are provided with a book of this kind adapted for use in public schools. WYNDHAM R. DUNSTAN.

THE ZOOLOGICAL RECORD FOR 1889.

The Zoological Record for 1889. Edited by Frank E. Beddard. (London: Gurney and Jackson, 1890.)

THIS, the twenty-sixth volume of the "Record of Zoological Literature," is published by the Zoological Society of London, and we have to congratulate the Secretary and Editor on the volume having been published well within the year 1890, the literature of which it treats belonging to the year 1889. Possibly this welcome acceleration in publication may be in some measure due to the freshness of the recorders, to whom we would, however, venture to suggest that work of this nature, the value of which depends so greatly upon its accuracy, may be too hastily done. We are too thankful for what we have received to be in a fault-finding mood; yet a very superficial glance over this volume reveals some evidence of a want of care which it would be right, and we think easy, to avoid.

Some changes have taken place in the recorders, the work done last year by Mr. W. E. Hoyle being divided, Mr. P. C. Mitchell taking the Mollusca, Mr. O. H. Latter taking the Brachiopoda and Polyzoa, and Mr. S. J. Hickson the Cœlenterata.

The record of "General Subjects," compiled by Mr. J. A. Thomson, is a feature added to the programme, as carried out under Dr. Günther and the Zoological Record Association, and to a certain extent it supplies a deficiency, nor could it be in better hands than those of the present recorder; but, in order that there shall be as little as possible of duplicate references in the volume, the Editor-in-chief should keep a watchful eye that works that are of right treated of under the special subjects are not unnecessarily referred to here. Thus we think, for an example, that Ecker's "Anatomy of the Frog" need not have been quoted twice, under text-books in the "General Subjects," and under Ecaudata in the record of Reptilia and Batrachia; nor need Mr. A. G. Butler's paper on "Insects supposed to be distasteful to Birds" have been quoted in full three times (General Subjects, Aves, and Insecta), and there are very numerous instances like this. Such records serve little other purpose than to test the powers of the recorders. The "Record" is not a subject catalogue.

A more serious objection may be taken to the departure from uniformity in the arrangement of the memoirs in the record of the Reptiles and Batrachians, and of the Fishes, where, instead of the authors' names being arranged alphabetically, they are printed as they turned up in the recorders' notes; perhaps, as a very trifling blemish, may be mentioned the occurrence of the footnote indicating that the recorder had not seen the work indicated by an asterisk, which notice is not always attached to each record, and in some cases it is attached when not needed. The record of Insecta is a wonderful piece of work, with its 849 references in chief. Here, too, the most useful method of numbering all the papers is adopted, so that a reference is found with a minimum of trouble.

The index to genera recorded as new in this volume is most unfortunately incomplete; by some oversight the whole of the new genera to be met with among the Cœlenterata have been omitted. It may be also noted that the new forms described among the Alcyonaria in the "Narrative of the Cruise of the *Challenger*" have never as yet been alluded to. There was not a column for "Travels and Faunistic" in the volume for 1885, the year in which the "Narrative" was published, and probably the fact that new genera and species of Alcyonaria were described in it escaped the notice of the then recorder of the group; but they were also unnoticed in the "Record" for 1887; and in the volume now under notice they are again passed by, so that no trace of such remarkable genera as Callozostron, Strophogorgia, &c., will be found in the whole series of records.

We have called attention to these defects, as they may be easily rectified in future volumes, and because we think they are of such a nature that the avoidance of them for the future lies in the hands of the Editor. As suggestions, we might add that it is very desirable that, when possible, short but characteristic diagnoses of all the new genera should be given; of course, when these are founded for well-known species, the mere indication of this would suffice, but it would save many a weary search in out-of-the-way journals if such diagnoses of new genera —as, for example, we find in Mr. Hickson's record of the Cœlenterata—were always given, and this used to be the general practice.

OUR BOOK SHELF.

Reports on the Mining Industry in New Zealand. Pp. 209. (Wellington: George Didsbury, Government Printer, 1890.)

THIS volume of reports from the Minister of Mines on the production and condition of mining enterprise in 1889, to both Houses of the General Assembly in New Zealand, is a continuation of similar reports which we have noticed in previous years. As regards the principal items of production there appears to have been a slight increase in gold—203,211 ounces valued at £808,549, against 201,219 ounces and £801,066 in 1888; but the coal raised has diminished by nearly 10 per cent., or from 613,895 tons to 586,445 tons; Kauri gum, which is next in value to gold among the totals, has also diminished from 8482 tons and £380,933 value to 7519 tons worth £329,590. The final result is that the value of minerals raised was £1,493,167 in 1889, or a little less than in 1888, when it was £1,531,614. In addition to the native supply, 128,000 tons of coal are imported, mainly from New South Wales; but about 80,000 tons are exported, 47,115 tons of which are returned as going to the United Kingdom. It is not, however, shown what the latter quantity is, whether it is a special gas coal or merely coal taken by steamers for English ports.

The detailed reports from the different districts contain some matters of general interest, among which may be noticed the use of dredgers for raising auriferous drift from the beds of rivers, which has been successfully carried out by the Waipapa Company on an ocean beach by a Welman centrifugal-pump dredger, about £300 worth of gold per week being saved from sand and gravel, yielding about 3 grains or sixpence in value per ton. This dredger is not suitable for lifting coarse material, and where the stuff contains stones above 3 inches across, the ordinary bucket and ladder system is found to be more advantageous. Among the reducing processes noticed, that of Messrs. Macarthur and Forest, for dissolving out gold and silver by a weak solution of an alkaline cyanide and precipitating by filtering the liquors through granulated zinc, is the greatest novelty. This is described in several places, both under its own name and as the Cassel process, as it has been taken up by the Cassel Company of Glasgow in place of the original, but defunct process of Cassel for chlorinating gold ores by the electrolysis of common salt.

Astronomical Lessons. By John Ellard Gore. (London: Sutton, Drowley, and Co., 1890.)

WE greet with pleasure all works that have for their object the exposition of the principles and facts of astronomy, for this is the science which, perhaps above all others, is pursued for its own sake, and the end of the study of which is knowledge, pure and simple. It is especially satisfactory to meet with works of this character when written by practical men, hence we view with gratification the little book before us. All astronomers are acquainted with Mr. Gore's astronomical labours, and to them the productions of his pen need no recommendation. As might be supposed, the best chapters are on "Double and Binary Stars" and "Variable Stars." Other chapters deal with the figure and motions of the earth, the sun, and planets, and the measurement of time. We note that Mr. Gore, while asserting that the nebulæ are probably masses of glowing gas, does not put forward the now exploded notion that nitrogen is one of the main constituents.

The book may well be said to be an easy introduction to larger and more advanced works on astronomy, and a useful guide to beginners in the study of this fascinating science.

Soap-Bubbles. By C. V. Boys, F.R.S. "Romance of Science Series." (London: Society for Promoting Christian Knowledge, 1890.)

EVERYONE who has attended lectures delivered by Mr. Boys must have been struck by his ingenuity and zeal in designing and performing experiments illustrative of the subject he has under consideration. The present little work consists of a course of three lectures he delivered to a juvenile audience at the London Institution, and the subject dealt with is one in connection with which a great number of experiments can be performed. The author's idea throughout has been, not to display a large number of hard experiments, but to perform, by means of the simplest apparatus (in many cases only a few pieces of glass, india-rubber tubing, and other simple things readily obtained), interesting and instructive experiments that may be easily repeated by young people with a little care and patience. In order to facilitate the repetition of most of them, he has added some practical hints at the end of the volume, by the careful perusal of which the reader will be fully rewarded. It is unnecessary to allude to the special points touched on in the course of these lectures: suffice it to say that the book is written in simple and clear language, and illustrated with a set of excellent woodcuts. We may note that the figure placed at the end can be easily converted into a thaumatrope for showing the formation and oscillations of drops. If the disk, made to rotate, be held up to a looking-glass, the drop will first appear to form; it then gradually increases in bulk, changing its form all the time; at a particular stage a waist is formed, which grows narrower and narrower until the drop falls. The trouble of mounting the illustration in the specified way will be amply repaid by the interesting phenomena mentioned above.

The Canary Islands. By John Whitford, F.R.G.S. (London: Edward Stanford, 1890.)

IN this little book Mr. Whitford records the impressions produced upon him during a tour in the Canary Islands. He has chapters on Grand Canary, Tenerife, La Palma, Gomera, Hierro, Lanzarote, and Fuerteventura; and as he writes clearly, and displays a considerable power of observation, the volume should be of interest even to readers who do not themselves propose to undertake a like journey. In his travels the author always had his camera with him, and some of the results are seen in the pleasant woodcuts with which his descriptions are illustrated.

LETTERS TO THE EDITOR.

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

A Remarkable Ice-Storm.

IN March 1884 I wrote to you in regard to a heavy ice-storm which we had experienced on the 7th and 8th days of that month, giving some statistics as to the thickness of the ice deposits on trees and as to the weight which the extremities of branches were carrying. I can now report to you a more remarkable storm of the same kind, the effects of which still continue more than sixty hours after the storm has ceased; and there seems to be little hope that the morrow will give us an unclouded sun to loosen the casing under which the trees are labouring.

Before sunrise on the 17th instant, a light snow began, with the wind blowing gently from the north; the storm soon changed to a fine hail, which prevailed throughout the day, and by sunset had covered the ground to the depth of some two inches; then followed a rather heavy rain, the thermometer standing not far from the freezing-point. The rain ceased for a while about eight o'clock, and I found that the amount of precipitation reduced to water was 1·25 inches. Before midnight the rain began to fall again, and the ice formed rapidly on trees, fences, wires, and everything that could hold a drop of water for an instant. The amount of rain in the night, augmented by a little drizzle and a light snow on the following day, was 0·67 inch, making the total precipitation during the entire storm 1·92 inches. In the morning the appearance of everything out of doors needed but bright sunlight to make it beautiful in the extreme, except for the feeling that many trees were maimed or ruined, and that the wires on which the community depends for light and for telegraphic and telephonic communication were in a state of dire confusion. But I can best convey an idea of the effects of the storm by giving the results of some measurements which I made on our College campus.

The fence at the north side of the campus was decorated with icicles about eight inches long; the trees were covered with ice, coating every twig and hanging in icicles, which were formed of course nearly vertically, but which, as the trees bent under the increasing weight of the ice, stood out like fringes in every direction. One English elm, fifteen feet high, touched the ground with its topmost branches; its stem, 1·6 inches in diameter, carried 0·65 of an inch of ice on its most exposed side; the stem of another tree, having a diameter of 1·4 inches, carried 0·60 inch of ice. Twigs at the extremity of the branches of more spreading elms carried enormous loads of ice. One twig having a diameter of 0·07 inch was increased by the ice to 1·12 inches; another of the same diameter was increased to 1·26 inches; and one of 0·09 inch diameter was made 1·32 inches; that is to say, these twigs were carrying respectively fifteen, seventeen, and fourteen times their thickness of ice. Two connected twigs, taken from one of these trees, about sixteen inches long and weighing by themselves less than half an ounce, carried a little over a pound of ice; that is, they supported about thirty-five times their own weight. A few frail stems of weeds standing up from the ground carried relatively still larger amounts of ice. One which had a diameter of 0·05 inch had become increased to 1 inch, or by nineteen times its size; and one of 0·07 inch measured 1·23 inches, showing a gain of about seventeen times its size; and a bush-like weed, which weighed about a quarter of an ounce, was supporting eleven and a half ounces of ice.

When the ice had been on the trees for more than thirty hours, and there may have been a little melting, I made two additional observations as to the weight of the ice. On one twig of an elm-tree, itself weighing a decided fraction less than three-quarters of an ounce, there was found to be ice weighing over twenty-eight ounces; and on another, having a weight of three-eighths of an ounce, there was ice weighing more than nineteen ounces and a half; that is to say, in the one case the extremity of a branch was carrying at least forty times its own weight, and in the other case fifty-two times its own weight of frozen water.

The most beautiful piece of ice-work on the College grounds was shown by a wire net running east and west, and placed for a guard at the end of a tennis court on the lower campus. The wire, of about 0·04 inch in diameter, was woven into hexagonal meshes, of which the horizontal sides were about an inch long, those inclined to them being longer. The wire was covered with ice to a thickness of about 1·06 inches; and from each of the horizontal sides hung an icicle curving slightly to the south, owing to the influence of the wind, and so standing free of the wire below, and attaining a length of 2½ or 3 inches. It was a wonderful specimen of the beauty of Nature's work.

The figures given above, compared with those recorded in March 1884, show that this has been a heavier storm than the former. In that case the largest proportional measurement of ice upon the twig of a tree gave an increase of but nine times the original diameter, and the largest amount of ice which I found on an upright weed-stalk was about twenty times its own diameter, while the weight of ice attached to ten ounces of twigs from a tree was less than seventy ounces. This storm, while it has recalled the lesser though remarkable ice-storm of March 1884, has also revived the memory of a storm of the same kind at Christmastide in 1855.

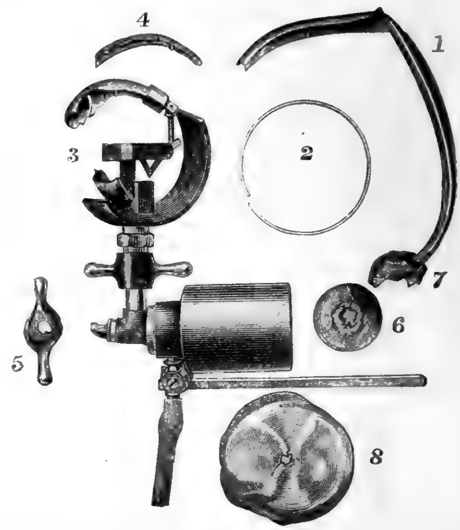
Perhaps I should add that, as the ice did not freeze on twigs, or stalks, or wires so that the cross-sections would be circular, the measurements made were those of the largest diameters in the several instances. SAMUEL HART.

Trinity College, Hartford, Connecticut, U.S.A., January 20.

The Bursting of a Pressure-Gauge.

AN accident caused by the bursting of a pressure-gauge attached to a compressed oxygen cylinder has recently come under my notice. As there are several points connected with it which may be of interest to those who use the oxyhydrogen light in their scientific work, I venture to send you a photograph of the remains of the apparatus, and a short description of the different parts.

The accident appears to have been brought about by the inside of the curved Bourdon tube giving way. The fusion of the metal being caused by the impact of the gas, about 35 cubic feet of oxygen escaped; the initial pressure was 115 atmospheres. The screw valve which formed part of the cylinder was fully open, so that the whole contents must have been discharged very quickly. The particles of metal which were shot out from the apparatus were found to be nearly quite spherical in shape; fragments of glass and metal fused together were also found. The metal seems to have been heated to fusion, in the same way as meteors are, by air-friction. For some time previous to the accident the gauge had been exposed to a far greater pressure than that at which it gave way. The stopcock and rubber tube leading to the jet were uninjured; from this it may be inferred that the hydrogen used in the jet, which was a blow-through one, had nothing to do with the bursting of the gauge. The injury done by the accident was, fortunately, not of a very



1, a part of the brass case, originally circular in shape. 2, the ring which held the dial in its place. 3, the pressure-gauge. The arc-shaped piece at the top is a part of the Bourdon tube; the inner curved surface is blown outwards; the outer surface is but little injured. 4, part of the cast-iron case of the instrument; this was driven edgewise into a pine plank to the depth of about a quarter of an inch. 5, the union nut for connecting the gauge and regulator to the cylinder. This is deeply cut away by fusion. 6, the disk of the regulator, partly fused. 7, a mass of fused brass found attached to the disk. 8, the dial. The gun-metal nose of the cylinder was fused away to the depth of three-eighths of an inch.

serious character, as the larger fragments missed the assistant. Some pieces of the case penetrated a plank to a considerable depth. A similar accident has recently taken place in the north of England, attended with very serious injury to the person using the apparatus. FREDERICK J. SMITH.

Trinity College, Oxford, January 15.

The Darkness of London Air.

LONDONERS need not be surprised to find black fogs when it is a fact that tons of soot float in the atmosphere every day. Hoping to get some *fact* on the subject, I collected a patch of snow, equal one square link, that had lain from November 27 to December 27 last, and from which I obtained two grains of soot. Now, supposing London to cover 110 square miles, it would produce 1000 tons of soot. Imagine a month's allowance being drawn off in a line by 1000 horses! The line would extend to about four miles in length. GEORGE WHITE.

7 Mildmay Grove, Canonbury, N.

A Swallow's Terrace.

AN explanation of the curious construction which I described in NATURE of November 27 (p. 80), has been suggested to me by Mr. Vernon Harcourt, in whose boat-house it was built. It is as singular as the construction itself, and, whether right or not, is probably worth the attention of naturalists.

Let me say beforehand that the "terrace" extends for nearly two feet along the beam at one end of which the nest is placed; that it consists of a layer of mud covered with dry grass; and that it is perfectly neat and finished throughout. No one who has seen it could possibly call it, as Mr. R. H. Read does in your issue of December 25 (p. 176), "an unusually straggling nest of the swallow." I entirely agree with Mr. Harcourt in believing it to have been built for some special and definite purpose.

Mr. Harcourt tells me that last summer the birds used to perch on two tie-beams which cross the boat-house just over his boat, and that their droppings made the boat in such a mess that he had the surface of both beams smeared with tar. Later on, the "terrace" was observed, occupying the surface of the beam alongside of the nest; but it did not strike Mr. Harcourt at the time that there could be any relation between this and the tarring.

The inference, however, is, that the definite object of the "terrace" was either to anticipate the tarring of that particular beam on which the young birds must necessarily perch as soon as they were able to leave the nest; or else to obliterate any traces of tar which they themselves may have left there after perching inadvertently on the tarred beams. In either case, if there is really any relation between the "terrace" and the tarring, it seems likely that the birds, having to meet an emergency, displayed a very unusual degree of reasoning power, and adapted means to end in a very ingenious way.

Oxford, January 29. W. Warde Fowler.

The Crowing of the Jungle Cock.

In the letter in your last issue (p. 295), in which Mr. Forbes endeavours to controvert my statement with reference to the crowing of jungle fowl, his account is extremely vague and indefinite. He admits he did not secure the bird, and omits to name the species; he cannot say it was a pure-bred jungle fowl, and according to his account the voice was "considerably thinner in volume, more wiry, and higher pitched." I have no doubt that there are in Timor plenty of common fowls, and knowing how readily they will cross with the jungle fowl, I think it highly probable that Mr. Forbes may have heard the crow of a hybrid: hybrids are, in some places, as common and wild as the pure breed. Wild caught hybrids are frequently brought here and offered as pure-bred birds.

I have had from time to time in my keeping all the known species of the genus Gallus, and have bred from most, if not all of them, and have had ample opportunities of listening to their various calls and crowing frequently uttered during their breeding season, and I must say none of them can fairly be compared with the loud, fine, clear crow of our common barn-door cock.

A. D. Bartlett.

Zoological Society's Gardens, Regent's Park, London, N.W., February 2.

On the Flight of Oceanic Birds.

The very interesting letter of Mr. David Wilson-Barker under the above title in Nature of January 8 (p. 223) leads me to say that during a recent voyage to Ivigtut, Greenland, I very often observed in the flight of the Arctic tern the same use of wings and tail which Mr. Wilson-Barker saw in the flight of the sooty albatross. I further noticed that the terns very frequently made use of their feet in steering a course through the air.

It was a common thing for a tern to poise itself on a windy day directly above the taffrail, and hold that position, regardless of the speed of the vessel, for from eight to ten minutes, examining, the while, everything abaft the house, apparently with a critical eye. When satisfied with the inspection, it would with a quick motion lower one of its black-webbed feet down with the web across the line of flight. The effect of this was exactly like that of a ship's rudder. When the left foot was drooped, the bird turned to port; when the right, to starboard. If the foot were lowered but a trifle, as sometimes was done, the bird turned but slightly; when lowered straight down and spread wide out, the bird turned almost as if on a pivot.

When the bird was sailing beside the ship, a foot was sometimes used to correct the course, which had been altered apparently by a flaw or eddy in the wind caused by the sails. Of course, the wings and the tail were very often used in conjunction with the foot, but I never saw the foot used when the bird was flying by flapping its wings continually.

New York, January 21. John R. Spears.

A Rare Visitor.

Just before the recent thaw I observed a water-rail searching for food in my garden. It approached within a few yards of the house, and showed very little timidity. It reappeared on two subsequent days, and was also seen in adjacent gardens; but it finally left when the frost was completely gone.

It is so very unusual to be visited by so shy a bird that I have thought the incident worth recording as an indication of the severity of the recent frost.

My house is in the suburbs of Reading.

Craven Road, Reading, January 30. O. A. Shrubsole.

The Erosive Action of Frost.

A somewhat striking illustration of the erosive action of frost was to be seen on a reservoir (North Wales Paper Company's) near here a few days ago. The reservoir—a fairly large one—is supplied by a stream entering at its upper end, and during the late severe frost was covered with ice to a thickness of about 8 inches, the ice being firmly attached to the mud and soil of the banks, especially in the narrower parts. With the thaw, about ten days ago, came a very heavy fall of rain, which resulted in the depth of water in the reservoir being raised some 2 feet. The sheet of ice then floating on this increased surface area tore away for long distances the adjacent mud and soil to which it was attached, and to such an extent that the contour of the banks at the narrower portion of the reservoir was completely reproduced by a band of soil from one to two feet wide fringing the sheet of ice, and upon which were growing many plants, grasses, &c. As the ice melted, this material would seriously assist the silting up of the reservoir, and no doubt similar action has taken place in other cases. H. T. M.

Flint, North Wales, January 31.

Skeleton of Brachycephalic Celt.

Last November, whilst some excavations were going on in the back premises of a house in Albion Road, Dunstable, a human skeleton was lighted on, resting on the right side, with the right hand to the face, and the knees drawn up in a crouching manner.

It fortunately happened that the tenant of the house had, some time before, seen two contracted skeletons of Celts in my possession, and he saw that the skeleton newly uncovered was probably of the same class with mine. He therefore called upon me at once, and as I happened to be at Dunstable, I returned with him to the spot and superintended the recovery of the bones.

The grave was about 5 feet deep, dug into the chalk rock; the head of the skeleton was to the north-east; the grave was filled in with clean, white, small, chalk rubble containing a considerable number of non-human broken bones and a few teeth.

Close to the femora of the man, on the inferior side, were two horn-cores of *Bos longifrons*, each attached to part of the skull, and taken from different individuals. Near the horn-cores were two whitish oval pebbles about the size of a bantam's egg, and a small piece of Romano-British pottery. No flint implements or flakes were in the grave.

As the interment was in chalk rock, with the dy covered with fragments of clean chalk, the bones when found were perfect in form to a remarkable degree. Of course, however, the remains were quite flat, with the skull crushed in and the lower jaw and most of the larger bones broken. On touching the maxillary bones, all the teeth dropped out. All the pieces of bone were brittle, and somewhat soft. After careful cleaning, drying, soaking in thin hot gelatine, and conjoining with shellac dissolved in spirit, it became possible for me to repair and replace nearly every bone—including all the vertebræ—in position.

Virtually the skeleton is perfect; no bones are missing, except one clavicle, a few of the small bones of the hands and feet, and the small terminal bones of the sacrum. Only one or two teeth are deficient. The skeleton represents a man of early middle life, and of considerable muscular strength; the height of the man when alive, as deduced from the femora, was 5 feet $7\frac{3}{4}$ inches.

Mr. A. Smith Woodward, of the British Museum, South Kensington, has kindly looked over and compared some of the small pieces of non-human bone and the teeth found in the grave covering; many of these he has determined as belonging to *Bos longifrons*, others to Equus, &c.

The two other Celtic skeletons mentioned at the beginning of this note were dug up by me in 1887, by permission of a farmer, from two ruined, round tumuli in a field on Dunstable Downs.

One is the crouching skeleton of a girl from 18 to 25 years of age; height when alive, 4 feet 11½ inches; interred on right side; in front of the girl were the fragmentary bones of a child under 5 years of age. Arranged round the body of the girl—one of the Bronze Age dolichocephali—were 158 chalky fossil Echini (86 of these were perfect, the remainder were broken examples), together with a stone muller, several scrapers, a few chevron-marked fragments of a British pot, and other articles.

The other skeleton is that of a boy or girl—one of the brachycephali—interred on the left side with knees drawn towards chin; age about 14 years; height when alive, 3 feet 7½ inches. A few flint flakes, and many fragments of British pottery were in the grave. Near the right hand was a nodule of iron pyrites.

Numerous bone fragments belonging to *Bos longifrons* were in the covering material of both the latter graves.

I have a femur, humerus, and a few other bones of a fourth Celt from the floor of a British hut on Blow's Downs, Dunstable; several flakes and a scraper were with the skeleton, together with a block of pyrites and a metatarsal bone of a horse. The human femur shows the height of the living individual to have been 5 feet 10 inches. I was not in time to secure all this skeleton; more than one-half, including the skull, was shovelled with chalk into a lime-kiln.

Towards the end of last month (January), whilst opening four presumed Saxon (English) graves on a hill at Chalton, near here, I noticed that one of the extended skeletons had but one leg; the left leg had been entirely removed at the hip during life, or before burial. I could see no difference in the condition of the two acetabula. I took a small corroded iron knife from the front of two of the skeletons just where a strap would be bound round the waist. Three bodies were interred with the head to the west, the fourth with the head to the north. I have preserved the femora, humeri, and skull of the last mentioned; the skull exhibits every tooth sound and perfect in both jaws.

Dunstable. WORTHINGTON G. SMITH.

TIDAL PREDICTION.[1]

AT most places in the North Atlantic the prediction of high and low water is fairly easy, because there is hardly any diurnal tide. This abnormality makes it sufficient to have a table of the mean fortnightly inequality in the height and interval after lunar transit, supplemented by tables of corrections for the declinations and parallaxes of the disturbing bodies. But when there is a large diurnal inequality, as is commonly the case in other seas, the heights and intervals, after the upper and lower lunar transits, are widely different; the two halves of each lunation differ much in their characters, and the season of the year has great influence. Thus simple tables, such as are applicable in the absence of diurnal tide, are of no avail.

The tidal information supplied by the Admiralty for such places consists of rough means of the rise and interval at spring and neap, modified by the important warning that the tide is affected by diurnal inequality. Information of this kind affords scarcely any indication of the time and height of high and low water on any given day, and must be almost useless.

This is the present state of affairs at many ports of some importance, but at others a specially constructed tide-table for each day of each year is published in advance. A special tide-table is clearly the best sort of information for the sailor, but the heavy expense of prediction and publication is rarely incurred except at ports of first-rate commercial importance.

There is not any arithmetical method in use of computing a special tide-table which does not involve much work and expense. The admirable tide-predicting instrument of the Indian Government renders the prediction comparatively cheap; yet the instrument can hardly be deemed available for the whole world, and the cost of publication is so considerable that the instrument cannot, or at least will not, be used for many ports at remote places. It is not impossible, too, that national pride may deter the naval authorities of other nations from sending to London for their predictions, although the instrument may be used on the payment of certain fees.

The object, then, of the paper was to show how a general tide-table, applicable for all time, may be given in such a form that anyone with an elementary knowledge of the *Nautical Almanac* may, in a few minutes, compute two or three tides for the days on which they are required. The tables are also to be such that a special tide-table for any year may be computed with comparatively little trouble.

Any tide-table necessarily depends on the tidal constants of the particular port for which it is designed, and it is supposed in the paper that the constants are given in the harmonic system, and are derived from the reduction of tidal observations.

The complete expression for the height of water at any time consists of a number of terms, each of which involves some or all of the mean longitudes of moon, sun, lunar and solar perigees; there are also certain corrections, depending on the longitude of the moon's node. The variability of the height of water depends principally on the mean longitudes of the moon and the sun, and to a subordinate degree on the longitude of lunar perigee and node, for the solar perigee is sensibly fixed. There are, therefore, two principal variables, and two subordinate ones. This statement suggests the construction of a table of double entry for the variability of tide due to the principal variables, and of correctional tables for the subordinate ones; and this is the plan developed in the paper.

The tide-table finally consists of the interval after moon's transit, and height of high and low water, together with corrections depending on the longitude of the moon's node and on her parallax, computed for every 20m. of moon's transit, and for about every ten days in the year. Each table serves for the two times of year at which the sun's longitude differs by 180°, and they may be used without interpolation.

It was hoped that less elaborate tables might have sufficed, but it appeared that, at a station with very large diurnal inequality, the changes during the lunation, and with the time of year, in the interval and height are so abrupt and so great, that short tables would give very inaccurate results, unless used with elaborate interpolations. It is out of the question to suppose that a ship's captain would or could carry out these interpolations, and it is therefore proposed to throw the whole of that work on to the computer of the table.

Such a paper as this is only complete when an example has been worked out to test the accuracy of the tidal prediction, and when rules for the arithmetical processes have been drawn up, forming a complete code of instructions to the computer.

The port of Aden was chosen for the example because its tides are more complex and apparently irregular than those of any other place which has been thoroughly treated.

The arithmetic of the example was long, and was rearranged many times. An ordinary computer is said to work best when he is ignorant of the meaning of his work, but in this kind of tentative work a satisfactory arrangement cannot be attained without a full comprehension of the reason of the method. The author was therefore fortunate in securing the enthusiastic assistance of Mr. J. W. F. Allnutt, and expressed his warm thanks for the laborious computations carried out. After computing fully half the original table, Mr. Allnutt made a comparison for the whole of 1889 of predictions made by the new tables with those of the Indian Government. Without going into the details of this comparison, it may be mentioned that the probable error of the discrepancy between the two tables was 9m. in time, and 1·2 inches

[1] Abstract of the Bakerian Lecture, delivered on January 29, by G. H. Darwin, F.R.S., Plumian Professor and Fellow of Trinity College, Cambridge.

in the height of high water; that there were reasons to expect some systematic difference between the two calculations, and that all the considerable errors of time fall on those very small rises of water which are of frequent occurrence at Aden.

Two other comparisons were also made, one with the Indian predictions of 1887, and the other with actuality of 1884. In the latter case, when a few very small tides were omitted, the probable error was 7m. in the time, and 1·4 inches in height. It is concluded from these comparisons that, with good values for the tidal constants, the tables lead to excellent predictions, even better than are required for nautical purposes.

It is probable that this method may be applied to ports of second-rate importance, where there are not sufficient data for very accurate determination of the tidal constants. Suggestions are made for very large abridgment of the tables in such cases, accompanied, of course, by loss of accuracy.

The question of how far to go in each case must depend on a variety of circumstances. The most important consideration is likely to be the amount of money which can be expended on computation and printing; and after this will come the trustworthiness of the tidal constants, and the degree of desirability of an accurate tide-table. The aim of the paper was to give the tables in a simple form, and if, as seems certain, the mathematical capacity of an ordinary ship's captain will suffice for the use of the tables, whether in full or abridged, the principal object in view has been attained.

THEORY OF FUNCTIONS.[1]

I.

THE papers in Herr Schwarz's "Gesammelte Abhandlungen" range over a wide field. But varied as the subject-matter of the papers is, most of them have an internal connection which lies in profound study of the theory of functions.

Herr Schwarz is a pupil of Weierstrass, but he has been greatly influenced by the writings of Riemann. Both these eminent mathematicians have developed a theory of functions. Whilst Weierstrass starts in all his investigations with analytical expressions and operations, Riemann, following methods borrowed from the theory of potential, bases his theory on certain partial differential equations which express fundamental properties of functions, and by developing general properties of all functions tries to replace calculation by reasoning. Both applied their theories to the Abelian functions, and here Riemann's investigations proved to be more general. His speculations are, however, of such generality, that objections were raised by Kronecker and Weierstrass as to the validity of his reasoning, and thus it became doubtful whether his most important theorems were actually proved.

In consequence, various mathematicians have attempted to place Riemann's theorems on a more satisfactory basis, to graft Riemann's far-reaching speculations on the more strongly-rooted methods of Weierstrass. These attempts were made, by Neumann and others, in connection with the theory of potential, whilst other mathematicians used purely analytical investigations. Among the latter, Schwarz stands out most prominently, and those of his papers which relate to this subject seem to me to deserve most attention.

As the subject in its great generality and abstractness has received little attention in England, it seems desirable to give an outline of Riemann's theory in order to be the better able to appreciate Schwarz's papers. The notion of a function, originally equivalent to that of a power, was gradually extended till Bernoulli gave a first definition, which was made more concise by Leibnitz. According to them, y is called a function of x if there exists an equation between these variables which makes it possible to calculate y for any given values of x for all (real) values of x from $-\infty$ to $+\infty$. This last condition excludes series which are convergent only for certain values of x. The functions here included will be essentially those which are given by an equation involving a finite number of algebraical, trigonometrical, and logarithmic or exponential terms. Such a function will, as a rule, be continuous—practically the only exceptions being infinite values of y—and it will have a derived function. In fact, these two properties, continuity and existence of a derived function, were considered as identical. Such a function is geometrically represented by a curve, x and y being taken as Cartesian co-ordinates of a point. But not every curve can be taken as representing a function.

The next step forward is chiefly due to the work of Fourier. Physical problems required the study of functions with new characteristics. The initial temperature of a rod, lying in the axis of x, may be supposed arbitrarily given, not for all values of x, but only for those which lie within the interval covered by the rod, and in the trigonometrical series means were found for expressing this arbitrarily given temperature in terms of x. This led Dirichlet to the new definition: y is called a function of x if the values of y are *arbitrarily given*, say by a curve, for all values of *x within a given interval*. This involves a deviation from Bernoulli's definition in two directions: the function is originally not given by a mathematical expression, and not for all values of x; in fact, it exists only as far as it is given. The *expression*, by Fourier's series, is not wanted for the definition of the function; it is needed in order to make the latter amenable to mathematical treatment.

The study of functions thus defined leads to new ideas of possible discontinuities. The curve representing the function may have sharp bends, may even have sudden breaks, the value of y suddenly changing from one value to another, different by a finite quantity. At such points we cannot any longer speak of a derived function, though the expression by Fourier's series is still existing. Further investigations in this direction have clearly established the fact that the existence of a derived function is not at all a necessary consequence of the continuity of the function itself. In connection with this it may be mentioned that Riemann has given a first example of a function which is defined by a process of calculating y from x, such that for every value of x there exists a value of y which has within a finite interval an infinite number of discontinuities, and which therefore has no derived function, though it has an integral. Schwarz gives, in one of his papers, an example of a function which, though continuous, has no derived function.

A much greater revolution in the ideas about function was produced by the introduction of imaginary values of the variables. Cauchy defines, in conformity with the second of the above definitions, that $w = u + iv$ is a function of $z = x + iy$ if for each set of values xy the corresponding values of u and v are given. Here a geometrical representation of the variables becomes very useful, if not absolutely necessary, viz. the variable z is represented by that point z in a plane which has the rectangular co-ordinates x, y; and w by that point in another plane which has the rectangular co-ordinates u, v. This enables us to make use of geometrical conceptions, and thus to become more concise in our language. We shall speak of the value z and of the point z representing this value as equivalent, the one completely determining the other.

If we start with a given value z_0 of z, and vary the latter, then the point z will move from the position z_0,

[1] "Gesammelte Abhandlungen." Von H. A. Schwarz. Two Volumes. Berlin: Julius Springer, 1890.)

describing any curve. To this value z_0 will correspond a value w_0 of w. If z is varied, w will also vary. Let us give z a small change dz; then the point z will move to a point z infinitely near to z_0. If now the corresponding change dw of w is infinitely small of the same order as dz, then the function w will be continuous. To fix the change dz we need not only know its magnitude, but also its direction. A change in either will alter dw, and therefore also the ratio dw/dz. The limiting value of this fraction, obtained by making the magnitude of dz disappear, will thus depend on the direction of dz, and not on the value z_0 only. In other words dw/dz will not be a function of z in Cauchy's sense; w has, in general, no derived function.

If w shall have a derived function, the definition of a function must be narrowed. Geometrically, it is seen at once what the required condition will be: dw/dz shall have the same value for all infinitely small values of dz; hence the magnitude of dw must be proportional to that of dz and independent of the direction of dz. Hence three positions of z near z_0 and the corresponding positions of w must form two similar triangles. Or: If the point z describes any figure, then the point w will describe a figure which in its smallest parts is similar to it.

If this condition is satisfied, then the function is said to be monogen by Cauchy. Weierstrass calls it an analytical function, whilst Riemann and others confine the word function to this case alone. We shall follow Schwarz in using Weierstrass's expression. We may then say that an analytical function establishes a *conform* (*i.e.* similar in the smallest parts) representation in the w-plane of the points in the z-plane; and conversely, if such conform representation between the points in the two planes is given, then also is w determined as an analytical function of z.

It has to be observed that such representation need not extend over the whole planes of w or z. It is also clear that a function as defined by Bernoulli is analytical. The analytical condition for the existence of a derived function is that $\frac{dw}{dy} = i\frac{dw}{dx}$; or, breaking it up in two real equations, that—

$$(1) \qquad \frac{dv}{dx} = -\frac{du}{dy}, \quad \frac{dv}{dy} = \frac{du}{dx}.$$

From these we obtain, by eliminating v,

$$(2) \qquad \frac{du}{dx^2} + \frac{du}{dy^2} = 0, \text{ or } \Delta u = 0.$$

A function u of x and y can therefore be the real part of an analytical function of z only in case that it is a solution of the partial differential equation $\Delta u = 0$. If u is given satisfying this condition, then $\frac{dv}{dx}$ and $\frac{dv}{dy}$ are given by (1), and therefore v itself is known up to a constant. For instance, $u = e^x \cos y$ satisfies (1), and then (2) gives $v = e\ \sin y + C$, and $w = e^z + iC$.

We thus see that w is determined as an analytical function of z if its real part, u, is given as a solution of a certain partial differential equation, and if besides for one value of z the value of v is given in order to determine the remaining arbitrary constant.

Our problem of determining w is therefore reduced to finding solutions of $\Delta u = 0$. The question is, What are necessary and sufficient conditions to determine any special u?

The equation $\Delta u = 0$ is only a special case of Laplace's equation—

$$\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dx^2} = 0,$$

which, on account of its fundamental importance in the theories of potential and of heat, has received a great deal of attention.

If we suppose that u is independent of z, we obtain our equation $\Delta u = 0$. There is thus a close connection between the theory of analytical functions and that of the potential in a plane. The latter has, on account of its concrete meaning, greatly helped the former. In fact, we have here one of the most interesting examples of the reaction of applied on pure mathematics.

Any results obtained for the potential in space can thus by specializing them so as to relate to a plane be applied to the theory of functions. Of such results there is especially one that has to be mentioned. Green has shown that in space the potential due to attracting masses is uniquely determined for all points within a closed surface which incloses no attracting mass if it is known for all points on the surface. His proof, however, is not a mathematical one, but based on the physical meaning of the potential. An analytical proof of the same theorem in a more general form was given by Sir Wm. Thomson. The same theorem, in more or less different form, is found by other authors, and Dirichlet gave it in his lectures on the theory of potential. Riemann, a pupil of Dirichlet's, therefore calls it "Dirichlet's principle," and by this name it is known in Germany, whilst Maxwell has called it Thomson's theorem.

When specified for a plane, the theorem becomes:—There is always one and only one function of x and y which satisfies the equation $\Delta u = 0$, and which is for all values xy within a given area finite and continuous, and which has for points on the boundary of the area arbitrarily given values.

This, in connection with what has been said about the determination of an analytical function, gives the following theorem:—

An analytical function $w = u + iv$ of $z = x + iy$ is uniquely determined for all points within a closed curve if u is arbitrarily given for all points on the curve, whilst v is given for one point within the curve.

The idea of making the partial differential equation $\Delta u = 0$ the foundation of a theory of analytical functions is due to Riemann (in his Inaugural Dissertation, 1851). In his theory of Abelian functions (1857), he gave a fuller account of some special points, and showed in a brilliant example the usefulness of his conceptions.

In this theory the leading theorem is the one just stated. But I have given it in the simplest form possible, leaving out the conditions about continuity and the conditions which must hold if the function w has more values for one value of z. To make it generally applicable, Riemann had first to invent the surfaces known by his name, consisting of a number of sheets spread over the plane of z, which are connected at single points—the branch points. Besides, he had to discuss the connection of that part of the surface at whose boundary u shall have given values, and this led him to what Maxwell has called cyclosis.

The essential part of Riemann's theory is to find criteria which will determine an analytical function by aid of its discontinuities and boundary conditions, and thus uniquely to define a function independent of a mathematical expression. It enables us, for instance, to decide whether a number of conditions which a function has to satisfy, are independent, sufficient, or inconsistent. If a complete set of conditions is given, the theory makes it possible to decide whether a given expression represents the function, or whether two different expressions are identical. According to the old methods, this identity can only be proved by the actual transformation of one expression into the other, and this requires calculations which are generally long and tedious.

Riemann gives in his dissertation only one application, and that a geometrical one, to which we have also to refer. We have already seen that every analytical function w of z determines a conform representation of the points on the w-plane on the z-plane. If we have two plane surfaces, each bounded by a single curve, then the question arises: Is it possible to find a conform representation of the one on the other? and, if so, what further conditions can be imposed? Riemann gives the theorem that this is always possible and in one way only, in such a manner that a pair of given points of the one figure, one on the boundary and one in the interior, correspond to similarly given points in the other figure. He considers the case when the one figure is a circle, and proves the theorem:—Any simply connected plane figure T can be conformly represented on a circle in such a manner that a given point on the boundary of T corresponds to a given point on the circumference of the circle, and a given interior point of T to the centre of the circle, and this can be done in one way only.

This special case includes the general one, for if two surfaces, T and S, have both been represented on the circle, then also is a representation of one on the other determined.

These and similar important theorems about the general theory of analytical functions were proved by Riemann by aid of "Dirichlet's principle" (Thomson's theorem) already mentioned. This is proved by aid of the calculus of variation. It maintains that a certain definite integral must have necessarily one, and only one, minimum value; its proof, therefore, depends on the calculus of variation.

Whilst the notion and definition of a function were gradually extended, more has also become known about possible discontinuities; and here, again, the theory of potential has greatly helped. In the latter, discontinuities occur which are due to the essentially discontinuous distribution of matter, and which extend over surfaces and lines. Accordingly, we see that an analytical function of z may have discontinuities which extend over lines. Here it must be borne in mind that we know nothing about the possible discontinuities of functions, excepting what we have learned from known functions. Riemann's theory requires that the discontinuities are given, but does not teach us which are possible. Other speculations have shown that the existence of a derived function is not a consequence of continuity; that a function may be integrable without being differentiable, and so on. In fact, of an analytical function in its generality we know almost nothing, and, above all, we do not know how far the methods of the infinitesimal calculus and of the calculus of variations can safely be applied to an unknown analytical function in all its generality. Hence, if, as in the proof of Dirichlet's principle, these methods are used, the functions are endowed with properties which themselves require to be proved. Thus the validity of that principle becomes doubtful, and with it the whole of Riemann's theory. Objections of this kind, first raised by Kronecker and Weierstrass in their lectures, have since been repeated in more specific terms by various mathematicians, and it has long been generally accepted that Riemann's theorems cannot be considered as proved by him.

O. HENRICI.

(To be continued.)

HUET'S ANEMOMETER.

THIS instrument is not a new invention. We claim for it rather the honour of being the first of its kind invented. In the article on "Anemometers" in the "Encyclopædia Britannica" mention is made of the efforts of several scientific men in this direction, but neither in this nor in any other such publication can we find any notice of M. Huet's invention. In the said article these instruments are divided into various classes according to the principle upon which they are based, the class to which M. Huet's anemometer would be assigned being described as instruments which "measure the wind force by the difference of level it is capable of producing in an inverted syphon, or U tube, containing water or some other liquid. Lind's anemometer, invented in 1775, is the best known of this type, and is still in common use." Turning to the Philosophical Transactions of the Royal Society for 1775, we find on p. 353 a "'Description and Use of a Portable Wind Gage by Dr. James Lind, Physician, at Edinburgh.' Redde May 11, 1775." Dr. Lind's description of his instrument may be briefly stated as follows:—"This simple instrument consists of two glass tubes . . . connected together like a siphon by a small bent glass tube. . . . The whole instrument is easily turned round upon the spindle by the wind, so as always to present the mouth of the tube towards it. The force or momentum of the wind may be ascertained by the assistance of this instrument by filling the tubes half full of water . . . and observe how much the water is depressed by it in the one leg and how much it is raised in the other."

Now we maintain on behalf of M. Huet's anemometer

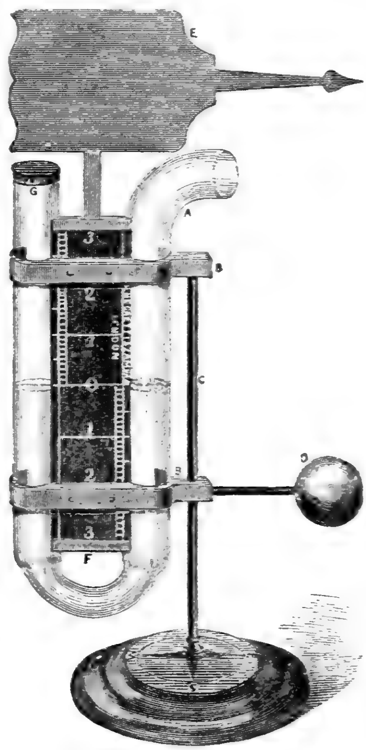


FIG. 1.—Lind's Anemometer.

that it exactly answers this description, and, moreover, that his discovery was given to the world half a century earlier than that of Dr. Lind. It is true that Dr. Lind's description and diagram are much more scientific and business-like than those of M. Huet, nevertheless the principle of the instrument is precisely the same.

Translated, M. Huet's description reads as follows:—

"We have worked with success of late to know exactly the quality of the air, its heat, its humidity and its weight by means of the thermometer, the hygrometer, and the barometer, which is a balance of air. But although we have endeavoured to weigh the air, we have not thought of weighing the wind! I made a suggestion about it to Hubin, an excellent English maker of this kind of instrument. He laughed at it, as of a thing easy to think about, but impossible to execute. I gave him the description of an instrument which I had imagined proper to this effect, and he was so satisfied with it that he left me with the design of making it as soon as possible, but his death frustrated his intention. Here it is in few words.

"It consists of a funnel of white iron, ABC, like a monk's hood. This funnel bends and becomes narrower up to C, where a tube begins and descends to D, where it bends round in DIE, and reascends to K, where it terminates. We fill the tube with quicksilver from CDE up to F. Above F up to G we pour some lye water, of which the rising and falling is perceived by some little dots which are marked on the tube from F to G. The wind entering by the funnel AB strikes the quicksilver at C, and presses more or less according to its force. The quicksilver pressed goes down in proportion, and going down from this side of the funnel it rises in the other branch of the machine above F, and raises the lye water which it supports; and this elevation is noticed and calculated by the dots marked on the tube.

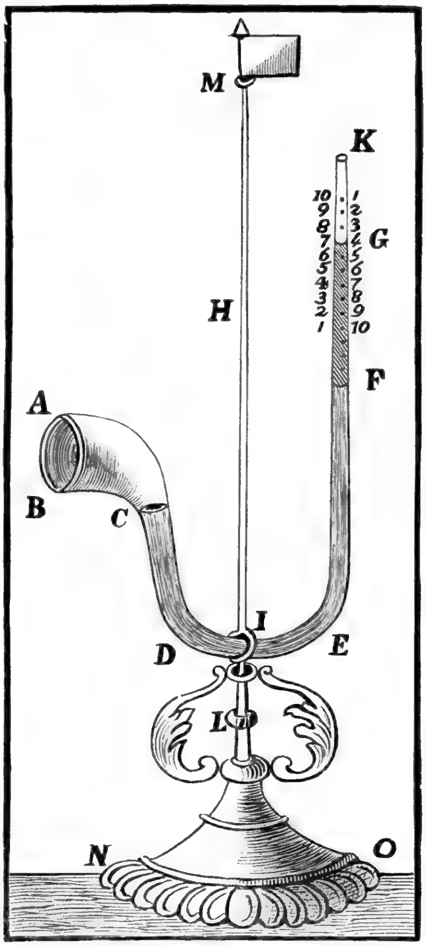


FIG. 2.—Huet's Anemometer.

"And because the instrument may not act if the funnel is not turned towards the wind, it is necessary to adapt a vane M supported by the iron rod MHI. This rod forms a ring at the point I, which encircles and holds firmly the tube. The iron rod below the ring enters a ferrule L, poised on the pedestal LNO, where it turns to the right and left according to the wind which turns the vane, and in turning thus it turns at the same time the whole machine, and holds the funnel always towards the wind."

The instrument, as sketched by the inventor, is extremely toy-like in appearance, and the inevitable result of its first exposure would be its destruction by the very force it was intended to measure. The tube, although encircled by the ring at I, is insufficiently supported, and the vane, M, is not large enough effectually to turn the machine. Lind's anemometer is an improvement on Huet's chiefly in these two particulars—the tubes being brought closely and bound firmly together, and the instrument itself acts as a vane by being set in a spindle. It must, however, be remembered that Huet's invention was never given a trial—in fact, not even made, or these faults would have been easily seen and rectified.

Pierre Daniel Huet, the inventor of this anemometer, was born at Caen on February 8, 1630. He was the author of many works, tutor to the Dauphin (1670), and Bishop of Avranches (1692). He died at Paris on January 26, 1721. The work from which the above description and accompanying sketch are taken is entitled "Huetiana; ou Pensées diverses de M. Huet, Évêque d'Avranches" (Amsterdam, 1723).

W. J. LEWIS.

VIRCHOW TESTIMONIAL FUND.

ON October 13, 1891, Prof. Rudolph Virchow celebrates his seventieth birthday. His pupils and admirers intend to commemorate the occasion by presenting him with a testimonial in recognition of his splendid services to medical science. A large and representative Committee has been formed in Germany with the view of collecting the necessary contributions, but it has been felt that this ought essentially to be an international movement, inasmuch as Prof. Virchow's followers are not of one nation, but of all.

In accordance with this view the undersigned have formed themselves into a Committee, in order to give Prof. Virchow's British admirers the opportunity of testifying to the gratitude which every member of the profession feels towards the man whose work in cellular pathology has so vastly contributed towards the advance of modern medical science, and may fairly be said to have made every member of the profession his pupil.

The form in which the universal feeling of gratitude is to find expression has been decided upon by the original German Committee. A large gold portrait medal is to be presented to Prof. Virchow himself, and bronze replicas of the same to members of his family and to some scientific institutions. The surplus—which, no doubt, will be large—is to be handed over to Prof. Virchow for the furtherance, subject to his decision, of scientific work.

To carry out this project the undersigned cordially invite the co-operation of the profession in the United Kingdom. Subscriptions, which are not to exceed two guineas, may be sent to the Hon. Treasurer, Dr. Lauder Brunton, 10 Stratford Place, London, W., and will be duly acknowledged in the medical journals. Cheques to be made payable to "Virchow Testimonial Fund," and to be crossed.

(Signed) JAMES PAGET, Chairman.
LAUDER BRUNTON, Hon. Treasurer.
FELIX SEMON, }
VICTOR HORSLEY, } Hon. Secs.

List of Committee of Virchow Testimonial Fund.

Henry W. Acland (Oxford), Th. Clifford Allbutt, John Banks (Dublin), W. Mitchell Banks (Liverpool), H. G. Barling (Birmingham), A. Barron, M.B. (Liverpool), J. S. Bristowe, M.D., W. H. Broadbent, Th. Lauder Brunton (Treasurer), Th. Bryant, H. T. Butlin, J. Chiene (Edinburgh), Andrew Clark, J. Coats (Glasgow), S. Coupland, J. Dreschfeld (Manchester), Dyce Duckworth, John Evans, Joseph Fayrer, D. Ferrier, W. H. Flower, M. Foster (Cambridge), W. T. Gairdner (Glasgow), Alfred Garrod, W. S. Greenfield (Edinburgh), F. de Havilland Hall, D. S. Hamilton (Aberdeen), T. Holmes, G. M. Humphry (Cambridge), J. Hutchinson, J. Hughlings Jackson, William Jenner, George Johnson, Joseph Lister, William MacCormac, Th. Oliver (Newcastle-on-Tyne), W. M. Ord, Richard Quain, George Paget (Cambridge), James Paget (Chairman), F. W. Pavy, George Porter (Dublin), R. Douglas Powell, J. Russell Reynolds, William Roberts, Ch. S. Roy (Cambridge), T. Burdon Sanderson (Oxford), E. A. Schäfer, S. G. Shattock, John Simon, A. R. Simpson (Edinburgh), E. M. Skerritt (Clifton, Bristol), Th. Grainger Stewart (Edinburgh), William Stokes (Dublin), Octavius Sturges, Th. Pridgin Teale (Leeds), William Turner (Edinburgh), Hermann Weber, Spencer Wells, C. S. Wheelhouse (Leeds), Samuel Wilks, A. H. Young (Manchester).

NOTES.

THE annual general meeting of the Geological Society will be held on Friday, February 20, at 3 p.m., and the Fellows and their friends will dine together at the Hôtel Métropole, Whitehall Place, at 7.30 p.m.

ON Tuesday Mr. C. Parker called attention in the House of Commons to the question of secondary education in Scotland, and moved, "That, for the better organization in Scotland of higher education, it is desirable that there should be grants in aid of it, not only, as at present, to the Universities and to primary schools, but also (as recommended by a Departmental Committee in 1888) to the public secondary schools, on condition of their general efficiency, and of free places being reserved in them for competition among children from the grant-aided primary schools." The motion was seconded by Mr. Parker Smith, and supported by Mr. D. Crawford, Colonel Campbell, Mr. S. Buxton, Mr. Bryce, and others. The Lord Advocate said that there was no conflict of opinion on the subject amongst the various sections of the House. On the question of financial aid, he might say that there was an immediate prospect of further money being available for education in Scotland. When the proper time came the Government would be prepared with a well-considered scheme of secondary education. The House would see that under these circumstances it was undesirable to pledge the Government to the terms of the resolution. This satisfactory assurance led of course to the withdrawal of the motion.

ON Sunday, February 8, the R. Università degli Studi at Naples is to hold a *festa* in honour of Prof. Arcangelo Scacchi, the eminent mineralogist, on the occasion of the fiftieth anniversary of his professoriate.

THE Essex Field Club, the foundation of which was noted in our columns eleven years ago (vol. xxi. pp. 215, 286, and 474), is about to enter on a new phase of its career. Since 1880, the Club has been carrying on most useful work in the county of Essex, and has deservedly taken rank as one of the most successful field clubs in the country. The publications now comprise some eight volumes of Transactions and Proceedings, together with the *Essex Naturalist*, the present form of the Club's journal. In addition to these publications, which are full of papers of local interest, the Club has also issued two volumes of "Special Memoirs"—the "Report on the East Anglian Earthquake," by Messrs. Meldola and White, and the "Birds of Essex," by Mr. R. M. Christy. Both of these volumes were reviewed in NATURE at the time of their publication. Although the Club has thus been carrying out its original programme for a period of eleven years, supported by a most energetic staff of officers, with Messrs. Meldola, Boulger, Holmes, and Fitch as successive Presidents, and Mr. William Cole as Honorary Secretary throughout the whole period of its existence, there is one branch of the Club's work which has been languishing, and which it is now proposed to take up and push forward with renewed vigour. It has always been present in the minds of the executive that one of the Club's functions was the establishment of a museum illustrating more especially the *local* natural history, geology, and prehistoric archæology of the county, but having also an educational side. Although there are already museums at Colchester, Chelmsford, and Saffron Walden, there is no establishment in the county which can be considered a local museum in the sense which the Club insists upon. To remedy this defect, and to set an example which it is hoped may be followed by other counties, an arrangement has been made between the Essex Field Club and the subscribers to the Essex and Chelmsford Museum, which was submitted to the Club at the eleventh annual general meeting held on Saturday, January 31, in the Museum at Chelmsford, under the presidency of Mr. E. A. Fitch. The formal resolution proposing the amalgamation of the two Societies was put to the meeting by Prof. Meldola, and seconded by Dr. Thresh, the Medical Officer of Health for the county, and carried unanimously. The two Societies will retain the name of the Essex Field Club, and the collections belonging to both will, it is hoped, be sooner or later combined, so as to form the nucleus of a museum in every sense worthy of the reputation of the Club and of the county. The draft scheme, which was submitted for final acceptation on Saturday, was drawn up by Mr. Cole, and had previously been adopted by the Committee of the Chelmsford Museum. An appeal will shortly be made on behalf of the amalgamated Societies for subscriptions from the public, and especially from those interested in the welfare of the county either as residents, as agriculturists, or as being concerned in its maritime industries. The funds thus raised are to be devoted to the erection and equipment of a museum building in Chelmsford, which is regarded as the most suitable locality for a county institution. Another scheme for extending the work of the Essex Field Club, in the direction of technical education, in accordance with the Local Taxation Act of 1890, was submitted formally to the Essex County Council on Monday last.

WE regret to announce the death of Dr. Karl Weihrauch, Director of the Meteorological Observatory of Dorpat, which occurred on January 19, in the fiftieth year of his age. As long ago as 1871 he was associated with Dr. Arthur von Oettingen in the management of that establishment, of which since 1876 he has had sole charge. The papers he has published have mainly had reference to the mathematical treatment of meteorological data.

M. ÉMILE REYNIER, well-known as an authority on electrical science, died of pneumonia on January 20, at the age of thirty-nine.

PARTICULARS have reached us of the death on October 31, 1890, of Mr. Cosmo Innes Burton, F.R.S.E., the first Professor of Chemistry in the Technical Institute recently founded by the English merchants in Shanghai. Mr. Burton, who was only in his twenty-eighth year, was well known in this country as an enthusiastic student of chemistry, well skilled in organic research, and exceptionally successful in University Extension lecturing. He was the youngest son of the late Dr. Hill Burton, Historiographer-Royal for Scotland, and inherited much of the versatility, originality, and conscientious laboriousness which distinguished his father. After studying at the Universities of Edinburgh, Munich, and Paris, he acted for a time as Research Assistant to Prof. Japp, at South Kensington, the record of his work being published in several papers. Returning to Edinburgh, he pursued private research and Extension lecturing until the appointment in China was offered to him last summer. He proceeded to Shanghai with his newly-married wife, and had just completed arrangements for commencing a course of lectures when his promising career of usefulness to science and to the world was ended by an attack of malignant small-pox.

THE London Chamber of Commerce, Botolph House, Eastcheap, are now displaying a most interesting collection of fibres of various kinds which have been supplied from the Kew Gardens Museum. The collection has been put together chiefly for the inspection and consideration of merchants and manufacturers.

THE expedition which is to be sent in the spring to the west coast of Greenland, by the committee of the Karl Ritter Endowment, is likely to be one of considerable importance. The chief of the expedition will be Dr. E. von Drygalski. Dr. O. Baschin will accompany it, defraying his own charges; and there will be a third scientific expert, who has not yet been selected. Dr. von Drygalski proposes to establish a station near the Umanackfjord, in about 70° 30′ north latitude, where Dr. Baschin will carry out a continuous series of meteorological observations, and from which he can make long or short excursions inland to study the interior ice. It is expected that the party will remain in Greenland about a year.

WE have received from Mr. Clement L. Wragge the following statement, written at Nouméa, New Caledonia, December 22, 1890:—"I have much pleasure in stating that I have just established a first-order meteorological station at Nouméa, under the auspices of the Queensland Government. The instruments are placed on the premises of the Australasian United Steam Navigation Company's agency, and consist of a fine Kew barometer by Adie; dry, wet, maximum, and minimum thermometers exposed in the 'Stevenson' screen (enlarged pattern); a Richard barograph and thermograph; capacious rain-gauge; earth-thermometers; and electric or 'turnover' thermometers of Negretti and Zambra's manufacture. The Governor of New Caledonia, M. Pardon, received me most kindly, and promised any assistance in his power. Early next month I hope to have placed a set of instruments at Aneiteum or Havannah, in the New Hebrides, and we also contemplate establishing a station at Tahiti, and other places in the South Pacific. Our observer at Nouméa is Mr. Samuel Johnston, a permanent resident, who takes a warm interest in the matter. Mr. Johnston's brother also assists, and every precaution has been taken to insure permanency and continuous records."

IN the first quarter of the year 1890, many places in Queensland suffered so severely from floods caused by excessive rainfall, that the Governor of that colony called for a special report upon the subject. This has been drawn up by Mr. Wragge, and contains numerous tables of rainfall for the months of January to March, 1888, 1889, and 1890, arranged for comparison. The report also contains data respecting the flood levels of certain rivers and creeks, and interesting reports of the condition of stock and state of country from the owners of cattle stations, referring both to the drought of 1888–89 and the floods of 1890. Mr. Wragge attributes the abnormal state of the weather which produced the floods to the unusual extension of the north-west monsoonal system, the isobars of which at times enveloped nearly the whole of Australia, thus changing the character of the usually arid conditions of the interior.

THE committee of the Lancashire County Council have recommended that the whole of the grant in aid made to the county of Lancaster under the Local Taxation (Customs and Excise) Act, 1890, should be appropriated for purposes of technical education, including agricultural and commercial instruction. The amount of the grant at the disposal of the county is estimated at £39,000. The committee have received 101 applications for assistance towards the expenses of carrying on existing schools and institutions for the purposes of technical instruction. They recommend that the special grant be placed to a separate account, to be applied in such manner as the Council may from time to time determine. It is recommended that grants be made to local authorities, or to institutions, schools, or classes under public management, the grants to be conditional upon suitable premises, appliances, and apparatus being provided. It is proposed to grant £500 for migratory lectures upon agricultural instruction, and to grant £500 for the purpose of assisting the formation of migratory dairy schools in such agricultural centres as would aid in their establishment by providing premises, appliances, and other facilities for the work.

THE *Oxford University Extension Gazette* reports that the County Council of Devonshire has voted £3000 for scientific lectures during the present year, and it is proposed to engage four lecturers to deliver twelve full courses on elementary mechanics and chemistry. Two of these lecturers are probably to be from Oxford, and two from Cambridge.

A MEMORIAL from the Dundee Chamber of Commerce, and numerous merchants, manufacturers, and others of that town, in favour of a decimal system of coinage, weights, and measures, was sent the other day to the Chancellor of the Exchequer. Mr. Goschen, in acknowledging the memorial, says:—"I must own frankly for myself that, though I am sensible that powerful arguments can be put forward in support of the decimal system, I cannot undertake to recommend its adoption to the country."

DR. GOEBEL has for some time been carrying on botanical researches in British Guiana. The Demerara *Argosy*, which speaks of his visit with much satisfaction, says that one of his chief objects is to study the morphology and life-history of a peculiar order of plants, of obscure affinities, known in systematic botany as the *Podostemaceæ*. "These plants" says the *Argosy*, "which grow on the submerged rocks in the falls and rocky river-beds of most or all of our main rivers, are known now to many of our gold diggers and others travelling up and down the rivers by the beautiful clusters or sheets of pink bloom which they present when the rivers subside and they become exposed in the dry seasons. They comprise many species and several genera. They adhere to the rocks, as seaweeds do, by the disk-like base of the stem, holding with such extreme tenacity in the stress and strain of the rushing waters that in removing them by manual force a portion of the rock is often broken away attached to the plant. The greater part of the year they are deeply submerged, floating as seaweed floats at the bottom of the sea, but as the waters subside in the dry seasons they take the opportunity while exposed to flower and fruit." There are numerous unsettled points as to their life-history, morphology, histology, &c., and, according to the *Argosy*, Dr. Goebel, on his return to Europe, will publish many results which are new to science and very interesting.

FROM Dorsetshire a singular instance of starlings being eaten by rooks is reported. It seems that, during the very severe weather we had lately, a flock of starlings was observed on a farm at West Stafford, near Dorchester, followed by a number of rooks in hot pursuit. The larger birds soon came up with their prey, and quickly despatched them, and, after stripping them of their feathers, devoured them then and there. When the scene of the occurrence was inspected, just afterwards, the ground was found to be strewn with their feathers, but beyond these not a vestige of the starlings could be discovered. It seems that the rooks, from sheer hunger, must have been driven to this extremity, owing to the scarcity of other kinds of food.

THE "Photographer's Diary and Desk-book" for the present year, which is issued by the proprietors of the *Camera*, should be in the possession of every photographer. It contains a good condensed account of dark-room procedure, including the latest discoveries that have been made in photographic printing. All the various formulæ, both for development and printing, which are recommended by each individual maker, have been revised, and are printed in a type that can be easily read in a dark-room. A few notes have been collected relating to some of the important and useful novelties in photographic apparatus, and in many cases illustrated. The diary part of the book consists of good smooth paper interleaved with blotting-paper, and ample room is allowed for the insertion of diary notes, with a few pages for memoranda. A useful addition might be made by carrying the diary a few days into the January of the following year; this might prove a source of convenience to many photographers.

THE report on the collection of birds made by the late Dr. Ferdinand Stoliczka, the naturalist of the Yarkand Mission of 1874, will shortly appear, when the record of Forsyth's expedition will be complete. The work was originally written by Mr. A. O. Hume, C.B., but the manuscript, on the eve of publication, was destroyed by a native servant, along with the other valuable manuscripts of this well-known naturalist. The collection of Yarkand birds was brought to England with the rest of the Hume collection by Mr. Bowdler Sharpe, who was entrusted by Mr. Hume with the task of completing the memoir. We understand that Mr. Sharpe has incorporated in his paper the chief results obtained by Dr. Henderson, Dr. Scully, and the other observers who accompanied the various expeditions to Eastern Turkestan, and will be a monographic report on the work done by all the English naturalists who have visited Central Asia.

WE learn from the *Times* that the Windsor and Eton Angling Preservation Association is about to place 1000 of the famous Loch Leven trout from Sir Gibson Maitland's Howietown fishery in the Thames waters in the Windsor district. The fish will be conveyed from Stirling in ten tanks. The artificial stocking of the river with trout has been carried out by the society with marked success, the captures during the last two years in the Windsor waters having been nearly double those of 1888.

IN 1868, Mr. A. S. Merry delivered a lecture before the Royal Institution of Swansea, under the title of "Heat." He has just published, in pamphlet form, the propositions then propounded. In this it is asserted that "interstellar space is occupied by two elements, which may be named electrine and thermine, which unite together and neutralize each other, forming the interstellar ether." All physical phenomena are explained on the supposition that this ether permeates all matter, and, under various conditions, is decomposed into its constituents. Absolute zero is produced by the perfect neutralization of thermine by electrine. Hence, on separating electrine from the ether, thermine is liberated, and the development of heat is the result.

THE Government of British North Borneo has accepted the sum of £100 from the Royal Geographical Society of Australasia, for Baron de Lissa to carry out a survey of Mount Kinabalu, which will be undertaken jointly with an agricultural survey by the Government of British North Borneo.

MESSRS. F. WARNE AND CO. will shortly issue the English edition of Major Casati's work, "Ten Years in Equatoria, and the Return with Emin Pasha." It will contain nearly two hundred original illustrations, and several maps.

IN NATURE of January 8 (p. 231) reference is made to Dr. J. D. E. Schmeltz as director of the Leyden Ethnographical Museum. The director is Dr. L. Serrurier. Dr. Schmeltz is "conservator" or assistant-keeper.

IN Mr. Howes's letter on "the morphology of the sternum," printed in NATURE of January 15 (p. 269), there is a mistake due to a slip of the pen. The word "presence" (line 7) ought to be "absence."

THE following are the arrangements for science lectures at the Royal Victoria Hall during February:—3rd, "From Dry Bones to Useful Material," by Mr. Ward Coldridge; 10th, "Plants and Animals of the Coal," by Prof. H. G. Seeley; 17th, "Our Chalk Hills," by Mr. F. W. Rudler; 24th, "A Trip to New Zealand," by Prof. Hall Griffin.

A THEORY attempting to explain the nature of the relationship between the optical activity of many substances in solution, and the hemihedrism of their crystalline forms, is advanced by Dr. Fock, the author of the new work on chemical crystallography, in the current number of the *Berichte*. It is certainly a most significant fact that all those substances whose solutions are capable of rotating the plane of polarization of light, and whose crystalline forms have been thoroughly investigated, are found to form hemihedral crystals—that is to say, crystals some of whose faces have been suppressed, and whose two ends are therefore differently developed. Moreover, in those cases where both the right rotatory and left rotatory varieties of the same chemical compound have been isolated and examined, as in the case of dextro- and lævo-tartaric acid, the hemihedral crystals are found to be complementary to each other, the faces undeveloped upon the one being present upon the other, so that the one is generally as the mirror-image of the other. Several ingenious attempts to account for the wonderful geometrical arrangement of the molecules in a crystal have been made of recent years by Bravais, Mallard, and others, who developed the "Raumgitter" theory, and by Sohncke, who showed that all possible crystallographical forms could be referred to systems of points; yet it has been found necessary by these crystallographers to assume a polarity of the molecule itself in order to fully explain the phenomenon of hemihedrism. This conclusion is, moreover, borne out by the more recent work of Lehmann upon his so called "liquid crystals." It is, indeed, evident that hemihedral crystals owe their hemihedrism to a differentiation of the various parts of the molecules themselves in space. Dr. Fock assumes, for the purpose of connecting this fact with the optical rotation of the dissolved crystals, the tetrahedral form for the element carbon, in the most recent conventional sense employed by Wislicenus, Van't Hoff, Victor Meyer, and other exponents of the new "stereo-chemistry." The axis of polarity of a molecule containing an asymmetric carbon atom, will, of course, be determined by its centre of gravity and the heaviest "corner" of the tetrahedron, and Dr. Fock shows that rotation of the molecule will be most easy round this axis, and in the direction, right or left, determined by the relative weights of the atoms or groups disposed at the other three "corners." He further shows that, if we consider any direction of vision through the solution, we must practically consider two positions of the molecules, in both of which the axis of rotation is parallel with our line of sight, and in one of which the apex of the tetrahedron is turned towards us, and in the other is directed away from us and the other three corners presented to us. As the molecules are, of course, in rapid motion, we must consider all other positions as balancing each other, and being resolved eventually into these two directions. It is then easy to see, as it is now accepted from Fizeau's work that the movement of molecules is capable of influencing the direction of light-waves, that there must be two oppositely moving circularly polarized rays produced. Now, it is generally supposed that the rotation of liquids is really due to the division of the light into two circularly and oppositely polarized rays, one of which, however, is stronger than the other, and determines the apparent optical activity. Dr. Fock completes his theory by showing the probability that there would be just this difference in the amount of rotation of the light in the two cases of the differently disposed molecules, those with their "apices" turned towards the direction of incidence of the light affecting it to a different extent from those whose "bases" were the first to receive it. The theory is well worth following out in the original memoir, many confirmations of it being adduced from other properties of hemihedral crystals.

The additions to the Zoological Society's Gardens during the past week include a Red-necked Grebe (*Podiceps griseigena*), presented by Mr. Thos. Hardcastle; two Japanese Pheasants (*Phasianus versicolor* ♂ ♀) from Japan, presented by Mr. E. Wormald, F.Z.S.; two Passerine Owls (*Glaucidium passerinum*), European, presented by Mr. St. John Northcote; forty River Lampreys (*Petromyzon fluviatilis*) from British fresh waters, presented by Mr. Thos. F. Burrows; a Black-headed Lemur (*Lemur brunneus* ♂) from Madagascar, a Triton Cockatoo (*Cacatua triton*) from New Guinea, deposited; a Milne-Edwards Porphyrio (*Porphyrio edwardsi*), a Grey-headed Porphyrio (*Porphyrio poliocephala*) from Siam, purchased; a Variegated Sheldrake (*Tadorna variegata*) from New Zealand, an Indian Python (*Python molurus*) from India, received in exchange; an American Bison (*Bison americanus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Transits of Venus in 1761 and 1769.—The Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. ii. Part 5, contain a discussion of observations of the transits of Venus in 1761 and 1769, by Prof. Simon Newcomb. The discussion was undertaken because of the mutations of opinion as to the value of the observations. For more than a century these observations were looked upon as affording the best data for the determination of the solar parallax. Now, however, opinion leans considerably to the opposite direction.

The general theory on which all the previous discussions were founded is said by Prof. Newcomb to be (1) that the formation of the thread of light at ingress and its breaking at egress mark the true time of tangency of the dark limb of Venus with the bright limb of the sun; (2) that a definite interval intervenes between this formation or breaking of the thread of light and the moment known as apparent or geometric contact of the limbs, which interval arises from and depends upon irradiation. A little consideration, however, will show that the time of formation of the thread of light, and therefore the interval between the two phases of contact, must vary with the observer, the quality of the telescope employed, and the conditions of vision. The clearer and steadier the air, and the sharper the vision, the more nearly must the two phases approach each other, until, under the best conditions, they come together. The indefinite and varying character of the phenomena, therefore, favours the division of the observations into the following classes: (1) those in which the formation or extinction of the thread of light is stated to have been observed; (2) cases in which some phenomenon of contact was noted earlier than the formation of the thread of light at ingress or later than its disappearance at egress; (3) observations in which no statement is made from which it can be inferred that one phase rather than the other was observed. These different kinds of observation have been satisfactorily treated by introducing corrections into the equations of condition.

The collected observations take up a large amount of space, and are of prime importance. Their classification and discussion, and the treatment of residuals in the formation of the equations of condition, is also in keeping with Prof. Newcomb's previous work. With respect to the values of the coefficients expressing the effect of imperfections of vision upon the visibility of the contacts, it is concluded that—(1) under the worst ordinary conditions of vision, Venus, at visible external contact, impinged heliocentrically $0''\cdot 7$ further upon the sun than it did under the best conditions, telescopic and atmospheric; (2) in the case of second contact the thread of light, when first seen, was heliocentrically $0''\cdot 16$ thicker under the worst than under the best conditions; (3) in the case of third contact no difference is shown in the thickness of the thread of light under different conditions when it vanished from sight.

When the weights of the observations of contacts III. and IV. in 1761, and contacts I., II., III., IV. in 1769, are considered, the value found for the equatorial horizontal parallax of the sun is

$$\pi = 8''\cdot 79 \pm 0''\cdot 051 \text{ or } \pm 0''\cdot 034,$$

where $0''\cdot 051$ represents the mean error, and $0''\cdot 034$ the probable one.

The mean probable result found by Encke from a discussion of the same observations was $8''\cdot 571$, with an estimated probable error of $0''\cdot 037$, which error was afterwards found to be too small. A variety of causes are put forward to account for the wide difference in the results, and it is hoped that some other astronomer will be able to make a thorough comparison of the two determinations.

Prof. Newcomb also provisionally concludes that the correction to Leverrier's latitude of Venus at the descending node for the mean of 1761 and 1769 is $+1''\cdot 915$, while the correction to Leverrier's longitude of the node (1765·5) appears to be $+32''\cdot 4$.

Leyden Observatory.—The fifth and sixth volumes of the *Annalen der Sternwarte in Leiden* have recently been issued by the Director, Dr. H. G. Van de Sande Bakhuyzen. The observations of stars contained in the former volume have been made with the usual care. The latter deals with the reduction of the zenith distances of several stars.

THE PRINCIPLE OF LAMARCK AND THE INHERITANCE OF SOMATIC MODIFICATIONS.[1]

"To reject the influence which use or disuse of a part may have on the individual or on its descendants is to look at an object with one eye."—Sir William Turner.

In last year's lectures we divided the factors of organic evolution into two great classes—firstly, primary factors; and secondly, secondary factors.

The primary factors are those which act directly on the individuals of a given generation; or indirectly on the individuals of the succeeding generation in consequence of a modification of the reproductive elements. Such are light, heat, climate in a general way, nutriment, the nature of the water for aquatic organisms, &c. Further primary factors are the ethological reactions of animals or plants against the inorganic and the living surroundings—what Lamarck called their needs and habits, what Darwin named the struggle for existence, sexual competition, migrations, &c.

We have seen that the action of these primary factors alone and of heredity could give rise to new races and consequently to new species, according to the law of Delbœuf. For this it is enough that the factors act continuously or even periodically, and that the changes produced are not disadvantageous to the modified organism; because, in this last case, natural selection comes into play and rapidly produces an elimination of the less favourable. But the most constant of primary factors of evolution are strongly aided by the secondary ones. These preserve and rapidly augment the results brought about by the primary factors, and determine the adaptation to any given environment of the forms whose variability has been acting. In the case of fully differentiated organisms, *i.e.* all whose parts are specially and rigorously adapted to definite conditions of existence, when a primary factor begins to modify one of these conditions, a return to biological equilibrium is henceforth impossible, and the organism vanishes. This explains the disappearance in olden times of highly differentiated forms (Trilobites, Ammonites, &c.) as a result of apparently unimportant changes in the state of the surroundings. Thus we also can understand how very slight ethological modifications would promptly bring about the annihilation of so highly specialized types as Peripatus, Ornithorhynchus, &c.

But on organisms still possessing a certain plasticity, still having a certain number of elements of unfixed value to dispose of, the action of primary factors causes only a momentary rup-

[1] Prof. Giard's opening lecture, in the Sorbonne, of his course on "Organic Evolution" (chair founded by the City of Paris).

ture of ethological equilibrium, and consequently more or less extensive variations. Then the secondary factors come in, and eliminate some and fix others of these variations, forming, from a dynamic aspect, new states of equilibrium; and from a morphological aspect, new species. This is the *rôle* of natural selection, of sexual selection, of segregation, and the other secondary factors we shall study this session.

The study of the primary factors of evolution is sometimes called Lamarckism. For Lamarck believed that it was possible to explain the evolution of all organisms by the action of these factors, to which he added heredity alone.

On the other hand, Darwin, while not denying the *rôle* of the primary factors in the production of new species, strove to show that the most important part should be given to natural selection and a small number of other secondary factors. Hence the name of Darwinism given to the study of these factors by many biologists.

Of late, attempts have been made to oppose Darwinism to Lamarckism, or at least to give almost exclusive weight to the one or to the other. Although we have protested several times already against these exaggerations, too often inspired by a deplorable chauvinism, the discussion has become so important of late that it is well to give it a few passing words. Among the disciples of Darwin, some, such as Romanes, have tried to show that natural selection alone cannot explain all the details of organic evolution; and that, along with it, other agents must be considered. Romanes has especially studied a new secondary factor, which he calls *physiological selection*, whose value we shall discuss later in the course. Moreover, Romanes does not reject the influence of primary factors any more than Darwin.

But other naturalists, more Darwinian than Darwin, refuse to admit any other cause of evolution than natural selection. They excommunicate at the same time Romanes, Delbœuf, and the new adherents to the views of Lamarck, revived and placed in touch with modern knowledge. At the head of the ultra-Darwinists is Weismann, who, in his numerous essays, strives to show that the explanations of Lamarck may be replaced by others drawn solely from the mechanism of selection. These essays, partly translated into English, have been received with great favour by the majority of British biologists, and have brought powerful help to the eminent Alfred Russel Wallace, who shares with Darwin in the honour of the discovery of natural selection, and has never ceased to give to this factor an altogether preponderating influence in the formation of species.

In an address delivered at Cologne on September 20, 1888, before the Association of German Naturalists, Weismann went the length of saying:—"I believe I am able to affirm to-day that the material existence of a transmission of acquired characters cannot be proved, and that there exist no direct proofs of the principle of Lamarck."

But what is the principle of Lamarck? That illustrious biologist formulated two fundamental laws. In his own words:—

(1) In every animal which has not got beyond the period of developing, the frequent and sustained use of any organ gradually strengthens it, develops it, enlarges it, and gives it a power proportionate to the duration of the using of it; while the continued disuse of this or that organ imperceptibly weakens it, and it deteriorates, loses its power by degrees, and finally disappears.

This is the law of *ethological reaction* or the law of *adaptation*.

(2) All that Nature has allowed individuals to gain or lose by the influence of the circumstances to which their race has been exposed for a long time, and, consequently, by the effect of predominant use of this organ or continued disuse of that, is conserved in the new individuals who spring from them, and who therefore find themselves better adapted than their ancestors, if the conditions of existence have not changed.

This is the *principle of heredity*, and it is to this law that Weismann alluded when he spoke of the principle of Lamarck.

If this principle of Lamarck is inaccurate and not demonstrable, one sees that the *rôle* of the primary factors is markedly lessened. The transmission of characters specially determined by these factors is no longer a scientific fact; their action is bound to set in motion in a vague way the variability of the germs, without which it is impossible to show a precise nexus of causes, a rigorous connection between the primary factor which acts and the variation it produces.

Before we begin our study of secondary factors we must examine one question—How far must we admit the restrictions to the primary factors brought forward by Weismann, and, before all, what have we to think of the absolute denial of Lamarck's principle?

If we follow Weismann in the very special criticism he has made of this principle, we find, at the outset, that he restricts considerably the limits in which Lamarck applied the law of inheritance of acquired characters:—

"We must not cite," says Weismann, "as facts capable of direct observation, transmissions of acquired characters in cases of injury or of mutilation: the observation of the transmission of functional hypertrophy or of atrophy has never been made, and one scarcely can hope to find it in the future, because it is a territory hardly accessible to observation."

Moreover, Weismann affirms that organs rudimentary through want of usage may be explained perfectly without the intervention of the principle of Lamarck.[1]

He reduces what one commonly calls acquired characters to a very narrow category of modifications, which in no way correspond to what Lamarck intended by the same expression.

In reality, among the many modifications of organisms, often in a vague manner called acquired characters, Weismann distinguishes those which affect the elements of the body—*somatogenic* modifications; and those which influence the reproductive elements—*blastogenic* modifications.

If, for example, a man has a finger amputated, his tetradactylity is a *somatogenic*[2] property; if, on the contrary, a child be born with six fingers, his hexadactylity arises from a special state of the germ, and is a *blastogenic* peculiarity. With this definition and limiting of somatogenic modifications to mutilation and to wounds, with which Weismann is content, then it is certain that the *majority* of somatogenic modifications are not transmitted by heredity.

"It is evident *a priori*," as Duval justly remarks ("Le Darwinisme," p. 309), "that only those variations can be inherited which have their source in an influence affecting the whole organism in such a fashion as to bring about profound transformations, of which the variation in question is a local manifestation. And indeed, if that modification be simply a local manifestation of a general tendency of the organism, it is, however, true that descent may transmit merely the tendency, which only shows itself later in the subsequent products of the variation in question. It is this that is presented to us in the case of atavism, in which the variation jumps over one generation.

"But a sudden accident, such as a blow which destroys part of the organism, does not result in a modification of the whole organism, and hence does not represent any general tendency nor any chance of forming an inheritable mutilation. The gardener, in slowly modifying plant or shrub by special conditions of culture, brings out variations which he may hope to see reproduced in the descendants; but when he capriciously prunes the branches of a shrub, he knows well that neither by slippings nor by seedlings can he get produced from that shrub, deformed by the sharp instrument, new plants which will bud forth similar deformations."

Thus Weismann seems to have given himself much trouble with meagre result in his discourse "On the Hypothesis of a Hereditary Transmission of Mutilations." In such a subject each case must be studied separately, and if, after cutting the caudal appendices from five successive generations of white mice, Weismann observed no modification with the descendants of these animals, that only proves that the sectioning of a mouse's tail carries with it no profound modification of the organism of these animals.

Similarly, all the discussion about the tailless cats of the Isle of Man and of Japan seems to us very ably and logically conducted, but the conclusions drawn therefrom do not go beyond the compass of that particular case. Among the feline species, at least in a domestic state, the existence of a caudal appendage more or less developed is of very secondary significance, and the artificial selection by man, guided by caprice or prejudice, may lead to the disappearance of that appendage in certain localities, particularly in islands.

[1] The demonstration of this assertion which Weismann has tried to give does not seem to me sufficient. I do not believe it is any more justifiable to make the assertion that the principle of Lamarck will be inapplicable to many instincts, in particular to those which appear only once in the life of an animal. But this discussion will be more *à propos* when we are studying the laws of heredity.

[2] We keep Weismann's terminology, though "somatic" seems a better word.

There is a whole series of facts which Weismann might have cited in support of his manner of view, but which do not furnish any argument more demonstrative against the inheritance of somatogenic modifications, if one gives to the word a meaning wider than that of simple mutilation. I speak of phenomena so curious as voluntary mutilation or autotomy, of which I recently pointed out the frequency and variety.[1] Innumerable generations of lizards have voluntarily broken off their tails to escape from various enemies, without that appendage ceasing to reappear among their descendants. At the most, one may say that selection has rendered this mutilation more easy and more frequent with certain species of saurians, as with certain echinoderms, certain molluscs, &c. The organism has acquired the power of parting easily with this or that part, while the part, sometimes seemingly without any use, does not fail to reappear in each new generation, because its suppression produces no reaction in the other organs.

But this is not always the case. Mutilations and wounds, which at first seem of little importance, nevertheless call forth somatogenic modifications as often inherited, because they give to the organism affected a disturbing action, which probably extends to the reproductive elements.

Weismann has not even made allusion to cases of this sort, of which a certain number were noted by Prof. Brown-Séquard long ago.[2]

Here are the leading varieties of the inheritance of the effects of accidental injuries, as given by that investigator:—

(1) Epilepsy in the descendants of guinea-pigs, male or female, in whom it was originally produced by cutting the sciatic nerve or the spinal cord.

(2) A singular change in the shape of the ear, and a partial shutting of the eyelids in the descendants of individuals (guinea-pigs) which had these as the result of cutting the cervical sympathetic nerve.

(3) Exophthalmia in the descendants of guinea-pigs which had this protrusion of the eye after an injury to the fourth ventricle.

(4) In the descendants of certain individuals in which a lesion of the restiform body had been produced, there occurred ecchymosis, followed by dry gangrene, besides other changes of the blood-supply to the ears.

(5) Absence of phalanges or whole toes of the hind paws in descendants of guinea-pigs which had accidentally lost their toes after cutting the sciatic nerve.

(6) Morbid state of the sciatic nerve in the descendants of individuals which had had that nerve divided, and the successive appearance of the phenomena, described by Brown-Séquard as characteristic of the periods of development and of abatement of epilepsy, and especially the appearance of an epileptic area in a part of the skin of the head and neck, and of the disappearance of hair around that area the moment the disease showed itself.

(7) Muscular atrophy of the thigh and leg of guinea-pigs, offspring of individuals with muscular atrophy following resection of the sciatic nerve.

(8) Defect in one or even both eyes of guinea-pigs whose parents had an eye deteriorate after the cutting of the restiform body.

Prof. Brown-Séquard has shown that the inheritance of the morbid conditions mentioned above may manifest itself in one side only, while both sides were affected in the parent. The inverse may also exist. Further, if the parent and descendant both have the morbid state in only one side, it sometimes happens that the side is not the same in both cases. The inheritance of these conditions may be wanting in one generation, and appear in the succeeding one. The female is better able to transmit morbid states than the male. As to the frequency of these transmissions, Prof. Brown-Séquard affirms that with more than two-thirds of the animals born of parents which have had several of these morbid conditions resulting from accidents, such modifications have shown themselves. The transmission by heredity of several of these pathological states may happen for generations, and has been proved to the fifth and even to the sixth generation.

These interesting facts have been confirmed since by Mr. E. Dupuy, who, further, has tried to explain them by an alteration of nutrition. One is astonished that none of the naturalists who have taken part in the discussion about the transmission of acquired characters so long carried on in NATURE, has thought to verify or even to discuss these facts.

After the preceding it seems to me that the partisans of Weismann's ideas have not paid sufficient attention to the marked reactions which certain somatogenic lesions may have on the modified organism, and in turn on the descendants.

Recently the botanists have given still more curious examples of the transmission of acquired characters. Certain somatogenic modifications produced by the slow action of parasites or symbions on various plants are capable of being transmitted by heredity. Scarcely four years ago Duval could write: "The oak and other trees have certainly borne galls since most primæval times, and yet nobody expects to see them produce inherited excrescences without the intervention of insects whose puncture is the origin of the galls." To-day that cannot be said of all galls and gall-like productions. Since then I have shown that, in a large number of cases, these productions profoundly modify the organism affected, and give rise to phenomena so singular that I have described them under the name of *parasitic castration* (*castration parasitaire*).

According to the excellent researches of A. N. Lundstroem, the deformations (*trichomes*), produced on the leaves of the lime and several other trees and shrubs by the prick of the arachnidans, are perfectly inheritable, even when those plants are grown protected from the parasites which have caused the modifications in the ancestors.

According to the researches of Treub and other botanists, the same is true for the singular transformations (*myrmecocecidies*), resulting from the action of the ants on several tropical plants.

Even whilst holding to the action of the most common primary factors, we believe we can establish in a stable fashion the transmission by heredity of somatic modifications. A certain number of acquired characters, which are specially revealed by somatogenic peculiarities, are moreover accompanied by correlative blastogenic modifications (and not merely consecutive as in the preceding cases) of such a nature that it becomes impossible to make the distinction proposed by Weismann, and these characters are justly considered inheritable by a majority of naturalists.

As in these examples the primary factors have modified at the same time the individual and the future product, the application of the principle of Lamarck cannot be disputed in the least degree. We invoke here the testimony of a naturalist, little suspected of partiality for the idea of evolution in general, and for Lamarck in particular. Godron, in his book "Sur l'Espèce et les Races chez les êtres organisés" (vol. ii. pp. 7, 8), tells how, according to Bishop Heber, dogs and horses taken from India into the Cashmere mountains become covered with wool; and how domestic animals in the tropics have their hair shortened and stiffened (sheep in Peru, Guinea, &c., merinos in South Sea Islands), or disappearing altogether (dog of Guinea, cattle of South America). The inverse action is not shown in our domestic animals brought from the tropics, which, says Godron, "proves that the action of climate is not always immediate or absolute."

Do not these last cases show that the modification produced is not due simply to the action of primary factors on the individuals, but that the blastogenic properties have been equally affected, and it follows that the principle of Lamarck finds its application?

Finally, if for certain plants modified, be it by their habitat among the mountains, or by their habitat on the sea-shore, the return to the normal type may take place the first generation; one knows that there are others with whom this return cannot be effected until after a long series of cultures. What cultivator does not know that there is a chance to keep up any race by taking for progenitors only the individuals showing most markedly the characters of that race? While in most cases the domestic races have been produced simply in view of the modifying of certain somatogenic characters, the breeder at the same time has produced unconsciously the correlative blastogenic modifications which insure the transmission of these somatogenic peculiarities.

But while there is acting the primary factor of the ethological reaction, that which Lamarck specially had in view, we may prove also the transmission of acquired character, or, if one prefers to employ Weismann's terminology, the concomitance in the parent of somatogenic modifications and of blastogenic modifications, destined to cause the reappearance in the off-

[1] Giard, "L'Autotomie dans la série animale" (*Revue Scientifique*, 1887, vol. xxxix. p. 629).

[2] See especially Brown-Séquard, "Faits nouveaux établissant l'extrême fréquence de la transmission par hérédité d'états organiques morbides, produits accidentellement chez des ascendants" (*Comptes rendus de l'Académie des Sciences*, March 13, 1882).

spring of somatogenic modifications of the same nature, even when the causative factor has ceased to act on the offspring.

Godron (*loc. cit.*, ii. 24), shows how the close relationship existing between muscles and skeleton makes any great change in the former affect the latter, and therefore the external appearance of the animal. The confinement of Brahmaputra and Cochin-China fowls prevents the use of the pectoral muscles, which become smaller, the wings also shorten, and the birds lose the power of flight, while the law of balancing of organs demands a development of the lower members. He also notes the change of shape of the horse according as it is used for riding or haulage; and the effect of continuous milking on secretions and development of mammifery organs of cows, and the diminishing of the udder and stoppage of milk-giving as the calf takes to cropping the grass, when continuous milking has ceased for several generations (*e.g.* on a return to wild conditions).

Among contemporary physiologists, Prof. Marey has insisted on the casual connection which exists between the animal mechanics and comparative morphology. While recognizing the importance of the facts already known, he has not ceased to demand new experiments for the purpose of knowing if the modifications one can produce on an animal by an exaggerated exercise of certain muscles cannot possibly be transmitted to its descendants. "One cannot yet affirm," he says, "but it is very probable, that the evolution theory will receive this last confirmation."

In this matter, as in many others, if evolutionists must content themselves in most cases with experiments unconsciously carried on in Nature, or those of breeders, instead of applying themselves to verifications made with all the rigour of modern scientific precision, is it not because of the deplorable insufficiency of our laboratories? One is astonished that in no country, not even where science is held in greatest honour, does there yet exist an *institut transformiste* consecrated to the long and costly experiments now indispensable for the progress of evolutionary biology.

The partisans of Weismann's ideas raise the objection that, in all the cases mentioned above, it is not the somatogenic character, but a blastogenic property, in virtue of which the descendant is susceptible of being impressed by the primary characters which determine that somatogenic character to the same and even to a superior degree to its parents.

This likeness, or rather harmony, between blastogenic modification and correlative somatogenic modification, is quite inexplicable if one will see nothing but a simple coincidence, accidental in origin, and fixed only later by selection. In reality, all takes place as if the somatogenic character were itself inherited, and putting aside all theoretical bias, it appears much simpler and more exact to explain the matter in this fashion. What else is it but heredity, this reappearance in the offspring at a given moment of physico-chemical or mechanical conditions, identical with those which have determined in the parent a morphological and physiological condition, resembling that which showed itself at a like moment in the progeny. Unless you attribute to the phrase *blastogenic modification* a mysterious and extra-scientific meaning, to speak of inherited blastogenic properties is simply to say that the order of the mechanical states which will be realized later in the development of a living being is already contained in the germ in a potential condition. Consequently, to say that at a given moment an animal inherits the possibility of losing its hair under the influence of heat, is equivalent to saying that it inherited the loss of hair which happened to its ancestors under the same conditions. When one goes to the root of the matter, the discussion becomes a simple dispute about words.

Besides, as Sir William Turner noted in a remarkable lecture on heredity, there are other facts which show that the separation of the reproductive and the somatic cells is not so absolute as Weismann and his followers seem to admit. If, in some animals, *Moina* for instance, the separation of the genital cells happens so precociously that they can already be distinguished in the first stages of the segmentation, one can affirm that in the majority of cases these cells come from somatic cells, and their plasma has passed through innumerable cellular generations before becoming the specialized sexual individuals of the colony.

In certain organisms, and particularly in plants, it even seems as if any somatic cell whatever is capable, in certain definite cases, of behaving like a parthenogenetic genital cell, and of completely reproducing the organism. Sachs has shown this for certain cells of the roots, of the leaves, and of the buds of several mosses.

One also knows when leaves of *Begonia* are cut, and the pieces grown in a hot-bed, one can get new stems which will bear flowers and fruits.

Undoubtedly it would be the same with some animals whose regenerating powers are highly developed (*e.g.* Turbellarians and certain Oligochætes) if one could nourish sufficiently the artificially separated bits. Theoretically one may say that each cell of a Planarian possesses in itself all that is needed for the reproduction of a new individual.

How is one to admit that a modification of somatic cells will not be followed by a correlative transformation of the product and of its blastogenic cells?

Variations produced by budding give us similar arguments, and show clearly the influence which the modification of certain somatic cells may have on other somatic cells and on the reproductive cells.

More interesting still, from the same point of view, are certain observations on the influence which the grafted subject may have, not only on the somatic elements, but even on the fruits of the graft. Darwin says:—

"Several North American varieties of the plum and peach are well known to reproduce themselves truly by seed; but Downing asserts, 'that when a graft is taken from one of these trees and placed upon another stock, this grafted tree is found to lose its singular property of producing the same variety by seed, and becomes like all other worked trees;'—that is, its seedlings become highly variable. Another case is worth giving: the Lalande variety of the walnut-tree leafs between April 20 and May 15, and its seedlings invariably inherit the same habit; while several other varieties of the walnut leaf in June. Now, if seedlings are raised from the May-leafing Lalande variety, grafted on another May-leaving variety, though both stock and graft have the same early habit of leafing, yet the seedlings leaf at various times, even as late as June 5" ("Animals and Plants under Domestication," 2nd ed., 1875, vol. ii. p. 247).

Inversely, the graft may communicate to the subject grafted certain somatic modifications with which it itself is affected. For instance, one knows that when one grafts the variegated variety of jasmine on the ordinary form, the latter sometimes bears buds with variegated leaves. The same is true of the laurel and of the ash. One has been able to produce even a half-breed of the graft, and perhaps the most curious are those obtained by Prof. Hildebrand at the request of Mr. Darwin. After having raised all the eyes of a smooth-skinned potato, and also those of a rough-skinned red potato, Hildebrand inserted them the one in the other reciprocally, and succeeded in raising two plants. Among the tubers produced by those two plants he found two red and scaly at one of their extremities, one white and smooth at the other, the intermediate portion being white and marked with red stripes.

These last examples bring us to cite facts of another nature, still insufficiently known to-day, but which seem to prove in an unexceptionable manner the influence of the somatic on the blastogenic cells. I am speaking of what Darwin called the direct action of the male element on the maternal form and even on the ulterior products.

"Even as long ago as 1729 it was observed that the white and blue varieties of the pea, when planted near each other, mutually crossed, and in the autumn blue and white peas were formed within the same pods." But this modification of the colour of the fruit may extend even to the husk, *i.e.* to the somatic cells of the maternal organism. "Mr. Laxton has cultivated the tall sugar-pea, which bears very thin green pods, becoming brownish-white when dry, with pollen of the purple-podded pea, which, as its name expresses, has dark-purple pods with very thick skin, becoming pale reddish-purple when dry. Mr. Laxton has cultivated the tall sugar-pea during twenty years, and has never seen or heard of its producing a purple pod; nevertheless, a flower fertilized by pollen from the purple pod yielded a pod clouded with purplish-red" (Darwin, *loc. cit.*, i. p. 428).

Numerous analogous examples of the action of the pollen of certain plants on the ovary of neighbouring varieties, have been collected by Gallesio, Naudin, Anderson, &c. Only recall the famous apple-tree of Saint-Valery, so carefully studied by Tillett of Clermont-Tonnerre. This tree did not produce pollen in consequence of the abortion of its stamens, and had to be artificially fertilized each year. The operation was done by the young girls of the district, by means of pollen taken from different varieties. Fruits of various varieties and sizes, colour and flavour, resulted, resembling the sorts which had furnished the pollen.

As the ovary of plants perishes after the production of the fruit, and presents transitory connections with the plant itself, it is not probable that the somatic modifications produced by the pollen extend to the cells of the branch or of the stem. For the same reason these modifications cannot have a reaction on the subsequent products.

But with animals, and especially with the Mammalia, where the fœtus is for a long time in intimate connection with the mother, one may suppose that the action of the male element will have an influence on the maternal organism first, and later on the subsequent descendant.

"In the case often quoted from Lord Morton, a nearly purely-bred Arabian chestnut mare bore a hybrid to a quagga; she was subsequently sent to Sir Gore Ouseley, and produced two colts by a black Arabian horse. These colts were partially dun-coloured, and were striped on the legs more plainly than the real hybrid, or even than the quagga. One of the two colts had its neck and some other parts of its body plainly marked with stripes. Stripes on the body, not to mention those on the legs, are extremely rare . . . with horses of all kinds in Europe, and are almost unknown in the case of Arabians. But what makes the case still more striking is that in these colts the hair of the mane resembled that of the quagga, being short, stiff, and upright. Hence there can be no doubt that the quagga affected the character of the offspring subsequently begot by the black Arabian horse" (Darwin, *loc. cit.*, i. p. 435).

Turner, who recalls Darwin's example, found the assumption that attributes the presence of the stripes to a reversion towards an ancestor common to horse and quagga too complex and hypothetical. He believes that the mother, while she had the hybrid in her womb, acquired from it the faculty to transmit the characters of the quagga, owing to the necessary nutritive changes during the development of the fœtus. The germinative plasma of the mother, belonging to the ovules not yet ripe, will have been modified in the ovary itself; and this acquired variation will have its reaction on the later-born individuals of the same mother.

The same explanation has been admitted by other physiologists for similar facts often proved by breeders and by hunters, for different domestic animals, and especially for dogs. Indeed, one knows that when a bitch has been covered for the first time by a dog of another breed, its subsequent offspring will show one or more little peculiarities belonging to that other breed, although she has been covered afterwards by dogs of her own race.

The accuracy of this hypothesis will be strongly restricted if, as certain observers, such as Mr. Chapuis, affirm, the influence of the first male also shows itself in the case of birds (pigeons) where the relations between the mother and the little ones are much less intimate than among the Mammalia. But, however it be with this explanation, the fact itself, outside of all theory, sufficiently shows the close connection which exists among the reproductive and the somatic elements.

In order not to leave the domain of facts scientifically established, or hypotheses susceptible of a more or less easy proof, I shall set aside the influences which impressions produced on the senses and nervous system of the mother may have on the offspring. The popular belief in these influences is very ancient, for we read in Genesis that Jacob placed rods whose bark was marked in various ways before his father-in-law's sheep, in order to get certain markings on the lambs which the ewes bore. But the antiquity of a belief is not always a proof of its accuracy, and I admit, with Weismann, that the cases quoted to prove the transmission of physical characters are not convincing, even in a case as curious as that of the mare of Baer.

Nevertheless, it seems to me very difficult to admit that the emotions and psychical impressions, which act so energetically and so evidently on all our secretions, have no influence on the products of the genital glands. Perhaps, outside the conditions of the surroundings and of education, which should be put in the first rank, one may attribute to an action of this sort the fact that all of one generation accept with the greatest ease the ideas which had been warmly combated and repelled by the preceding generation. It seems to me impossible that the intellectual movement caused by men of genius in one or more branches of human knowledge—a movement propagated and disseminated by men of letters and artists—has no reaction on the blastogenic elements of the contemporary generation, and consequently on the generation following, which thus will be prepared by hereditary transmission for an altogether new order of psychic modalities.

Finally, a last consideration leads us again to combat the opinion of those who maintain that acquired somatogenic characters cannot be transmitted from parents to infants. If, as already quoted from Turner, we push this theory to its final consequences, we are led to suppose that the ancestors of actual living beings, and the primordial protoplasm itself, possessed in themselves all the variations which have appeared since; and as, according to this hypothesis, the primary factors have acted only on the individual and not on the blastogenic elements (which alone can be transmitted), we must conclude that these possessed from the beginning, *i.e.* from the appearance of living matter, an evolutionary potency in a certain measure indefinite. We shall thus be led to the idea of creative forces, regulated, it is true, by selection. Thus again the door will be open to directing agents, immanent or outside of matter and we shall require to renounce the grand mechanical conception of the universe, seen by Descartes, and followed up by the inquirers of the eighteenth century.

If, on the other hand, we admit the transmission of somatogenic characters in the measure proved by the facts cited above, the transformation of living beings will become much quicker, because it will not depend entirely on the chances of internal variation, but will be determined by the action of primary factors.

The *rôle* of selection and of secondary factors will remain most important, accelerating and regulating the transformation.

But before passing to the examination of secondary factors, we shall first study a biological phenomenon which we find whenever new organisms are produced—hereditary transmission. In explaining the production of these forms, whether we make use of the principle of Lamarck, the law of Delbœuf, or the selection of secondary factors, we have seen we always must admit the action of heredity.

Properly speaking, heredity is neither a primary nor a secondary factor. It is the integral, the sum of indefinitely small variations, produced on each anterior generation by the primary factors. The laws of heredity, hardly studied yet experimentally, offer a vast field for the sagacity of biologists. Several of these laws, and especially the law of homochronic heredity (*l'hérédité homochrone*), give good arguments for the principle of Lamarck. The most recent embryological researches begin to afford us a little insight into the mechanical process of hereditary transmission, and the deeper phenomena of reproduction.

It is only after carefully examining all the information acquired on these delicate matters that we can begin to study the secondary factors of evolution with profit.

THE INSTITUTION OF MECHANICAL ENGINEERS.

On Thursday and Friday of last week, the 29th and 30th ult., the forty-fourth annual general meeting of the Institution of Mechanical Engineers was held in the theatre of the Institution of Civil Engineers, the latter society extending their usual hospitality to the sister Institution.

There were but two papers on the agenda, viz.:—On some different forms of gas furnaces, by Mr. Bernard Dawson; and on the mechanical treatment of moulding sand, by Mr. Walter Bagshaw, of Batley.

The fourth report of the Research Committee on Friction, which deals with experiments on the friction of a pivot bearing, was also down for reading, but time did not permit of it being taken. This was chiefly due to the fact that Mr. Macfarlane Gray proposed an alteration in the bye-laws, in virtue of which any full member who had paid thirty annual subscriptions—£3 each—should become a life member without further payment. This proposal received influential support in the course of a long discussion, but was nevertheless negatived on a division by a considerable majority.

The President, Mr. Joseph Tomlinson, occupied the chair on both evenings.

Mr. Dawson's contribution was one of considerable interest, and gave rise to a long discussion. The paper was illustrated by a large number of wall diagrams which covered the whole side of the theatre. The author treated his subject in a comprehensive manner, going into the question historically, and dealing with gas furnaces of almost all kinds; many of the types described being indeed only interesting from a historical standpoint, as they had not proved valuable from an economic point of view. As Mr. Dawson said, however, one often learns as much from failures as from successes; and it is necessary at times to know what to avoid in order to know what to imitate.

From what we have said it will be evident that we cannot hope to follow the author into his description of the many types of gas furnaces he deals with, both from limitations of space, and also from lack of the pictorial aids he had called in, by means of the diagrams, in describing the arrangements of the furnaces. We must therefore confine ourselves to the broad features of the paper, and refer those of our readers who wish to follow up this most interesting subject to the Transactions of the Institution.

The author commenced by stating that the greater number of gas furnaces in which crude heating gas has been successfully applied, have been of the reversing regenerative type. There are many processes, however, requiring temperatures below that maintained by the use of regenerators, and in these gas furnaces have also been used with success. It is also often an advantage to be able to concentrate in one spot the manipulation of all the fuel required in scattered furnaces. For these reasons, amongst others, it is often desirable to employ gas fuel when the cost of saving in fuel may be a secondary consideration. The annealing of steel castings, heating plates and angle bars, &c., are cases in point. On the other hand, there are cases in which a higher temperature is required, such as cannot be attained by combustion of gas with cold air, and in these continuous regeneration—as opposed to reversing regeneration—may be applied; the regeneration having the effect of recovering the heat from the waste gases. In either case the escaping gases must retain sufficient heat to secure the necessary draught; in fact regeneration may be carried too far. The author gives a useful word of warning on this point, some designers being of opinion that they cannot have too much of the good thing, regeneration. There have been many failures due to a want of appreciation of this point.

The author divides gas furnaces into four classes: (*a*) with reversing regeneration; (*b*) with continuous regeneration; (*c*) non-regenerative; and (*d*) with blowpipe or forced blast.

Furnaces with reversing regeneration (Class *a*) are of several different kinds. (1) The ordinary Siemens furnace, the arrangement of which is well known. (2) The Batho or Hilton furnace, in which the regenerators are above ground. (3) Furnaces in which the air only is regenerated, the gas being admitted direct. (4) Furnaces in which a portion of the waste heat is taken back to the producer.[1] (5) The regenerative blast-furnace stoves of the Cooper and Whitwell types.

In furnaces with continuous regeneration (Class *b*), the air is heated in flues by radiation or conduction from the bottom of the furnace, and through thin walls which separate the air-flues from the flues that carry the spent gases to the chimney.

In non-regenerative furnaces (Class *c*), the air is admitted to the furnace at atmospheric temperature.

The blowpipe or forced blast furnaces (Class *d*) are of two kinds: firstly, those in which air is supplied at atmospheric temperature by a fan; and secondly, those in which the air is heated by the spent gases by being passed either through coils or stacks of pipes, or else through brick tubes or flues.

For reasons already stated, we cannot follow the author into the details of the various types here broadly sketched. The classification is, however, valuable, and supplies a standard which doubtless will be followed by others when dealing with this subject of daily growing importance.

Before closing our brief abstract of this paper, we will repeat a quotation the author makes from an address of the late Mr. Holley, delivered sixteen years ago, as it does justice to the foresight of that great American engineer and metallurgist, to whom not only his own countrymen, but European engineers also, owe so much. In his Presidential address to the American Institute of Mining Engineers, Mr. Holley said: "Regenerative furnaces will gradually but inevitably take the place of the ordinary heating, puddling, and melting furnaces, thus preventing the application of unspent furnace heat to steam generation." It should be remembered that in those days the generation of steam was looked on, in the general metallurgical trades, as the proper and legitimate means of recovering heat from waste furnace gases. How ill the device served this end those who know the difficulties and dangers of furnace gas fired boilers will recognize.

[1] This is the new Siemens furnace which was fully described in a paper read before the Iron and Steel Institute, by Mr. John Head, in 1889.

The discussion on this paper was opened by Mr. Aspinall, the Superintendent of Mechanical Engineering to the Lancashire and Yorkshire Railway, who bore testimony to the successful working of gas furnaces in engineering practice at the company's works at Horwich.

Mr. John Head, who is connected with Mr. Frederick Siemens, also spoke at some length, in the course of his speech dealing with the new Siemens furnace, and giving instances of its successful working.

Mr. Smith-Casson, of Lord Dudley's Round Oak Iron Works, also gave interesting particulars of a furnace he had designed and erected. This furnace has overhead regenerators, a type which is now attracting a good deal of attention. It is interesting to note that Mr. Smith-Casson does not advocate overhead regeneration in all circumstances. It is a subject, he said, upon which he has still an open mind. As another speaker pointed out, there is this objection to an elevated regenerator, that the heated air naturally rises to the highest point, and therefore the circulation may not be as efficient as in cases where the regenerators are placed below the hearth.

Mr. A. Slater described a device in an ordinary boiler furnace in which iron retorts are placed at the back of the furnace bridge, and in these steam is dissociated and returns to the furnace for combustion of the gases. As Mr. Macfarlane Gray pointed out, this appears nearly akin to the perpetual motion theories; but a useful effect may be obtained by transferring heat from a place where it is not wanted to a place where it is. Mr. Slater said the application of this device gave a saving of 38 per cent. of fuel burnt, which only proves that Mr. Slater's boilers must have been of extremely bad proportions originally.

A good deal of the discussion turned on the burning of gaseous fuel in steam-boilers. In connection with this subject we cannot do better than quote a remark made by Prof. Alexander Kennedy. As to the saving of burning gas with regenerative furnaces in metallurgical operations, there can be but little divergence of opinion; but in the case of generation of steam, quite a different set of conditions will arise. In steel-making, for instance, it is necessary that there should be intense local heat, and the gases must leave the furnace at an enormously high temperature. In boiler furnaces intense local heat is to be avoided, and the products of combustion pass to the chimney comparatively cool. Thus a steam-boiler may show an evaporative efficiency or fuel economy so high that little more heat is left in the spent gases than is necessary to supply chimney draught, and in such a case regenerators would be useless. In metallurgical operations the efficiency of the furnace would be something absurdly low without regenerators, perhaps not more than 5 per cent. We commend these remarks to those visionaries who think that so much better results can be obtained by complicated gas-generating devices in steam-boilers, than by burning coal simply logically on a grate.

The paper of Mr. Bagshaw was of a nature which confined its interest chiefly to practical founders, and does not require extended notice at our hands.

The summer meeting of the Institution will be held at Liverpool during the last four days of July.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for January contains:—Studies on the comparative anatomy of sponges, by Arthur Dendy, M.Sc.: (3) On the anatomy of *Grantia labyrinthica*, Carter, and the so-called family Teichonidæ (plates i. to iv.). In the introduction to this paper the author refers to the large collection of sponges made by Mr. Bracebridge Wilson in the neighbourhood of Melbourne, during the last few years, numbering about 2000 specimens. This series, as well as the collection in the National Museum at Melbourne which has been placed at his disposal by Sir F. McCoy, he hopes to name and describe in time; but as such a work must extend over some years, he purposes from time to time to note new or important forms. The present paper describes a large and beautiful calci-sponge, originally described by Mr. Carter as *Trichonella labyrinthica*, but better located in the genus Grantia as defined by Vosmaer. It is very large for a calcareous sponge, well-grown specimens being about three inches in height and a little more in breadth. In adult forms there is a stout cylindrical stem surmounted by a greatly convoluted cup-shaped mass. Into the detailed description it would be impossible to enter; but in regard to the recent controversy about the existence of "Sollas's membrane" it may be mentioned that in this species this membrane remains visible even when the collars of the cells are retracted, thus indicating that it is probably a more or less permanent structure, and no mere temporary fusion of the margins of adjacent collars. As to the family Teichonidæ, it must be abandoned, and this branch of Lendenfeld's genealogical tree has to be cut off. (4) On the flagellated chambers and ova of *Halichondria panicea* (plate v.). In this study the minute structure of the above portions of this common British sponge are described from specimens killed with osmic acid. In this sponge the collared cells, when the chamber is seen in section, stand some little distance apart from each other. They have short nucleated bodies, surmounted by delicate funnel-shaped collars; these are all connected at their margins by a very distinct membrane, and the flagella of the collared cells were seen plainly projecting through the collars and into the cavity of the chamber; thus the question of the co-existence of Sollas's membrane and the flagella is settled. The ovum possesses a nucleus with a nuclear membrane and nucleolus, and it is suspended by a pedunculated envelope in the lacuna.—On *Megascolex cæruleus*, Templeton, from Ceylon; together with a theory of the course of the blood in earthworms, by Dr. A. G. Bourne (plates vi. to ix.). During a short visit in 1889 to Ceylon the author obtained thirty-eight species of earthworms; only seven of these have been found in India, and about twenty-nine Indian species have not yet been found in Ceylon. The author summarizes his theory of the circulation as follows: the vascular system consists of a portion in the cephalized region, and of other portions metamerically repeated in all succeeding segments. The cephalized portion differs only from that occurring in any other segment in having undergone a synthesis, and also in the presence of contractile hearts. Throughout the body, blood is forced from the contractile vessels into the peripheral networks; thence it is conveyed by a system of intestino-tegumentary vessels to intestinal capillaries, and from these it returns to the contractile vessels.—On a little-known sense-organ in Salpa, by A. B. Lee (plate x.). This organ, mentioned and figured by Ussow in 1876, seems not to have been since alluded to; Ussow's figures are very imperfect. The organ appears to be either a taste bulb, or as the author inclines to think, "a sensory areometer or hydrometric apparatus."—Immunity against Microbes (Part 1*a*), by M. Armand Ruffer.

Bulletin de la Société des Naturalistes de Moscou, 1890, No. 1. —Ornithological fauna of the Amu-daria region, from Tchardjui to Kelif, by M. Zarudnoi. The monograph mentions 159 species of birds, of which 138 nest within the region itself (116 in its cultivated part, 37 in the neighbouring desert, and 25 in towns). Hardly ten more species can be found which nest in the region. Many of the enumerated species are very scarcely represented. The deserts in the north of the Amu have almost the same bird-fauna as the Transcaspian region, although they differ as to their amphibians and reptiles; while further north, in Khiva, M. Bogdanoff found, as known, a much richer bird-fauna, probably due to the proximity of Lake Aral. The enumeration of species is accompanied by short notes relative to their habitats and feathering; some measurements are also given.—The system of chemical elements, by B. Tchitchérine (in French). This most valuable work, the importance of which was pointed out by Prof. Mendeleeff in his Faraday Lecture, is summed up for the *Bulletin*. The researches of the author into the numerical laws of the system bring him to a most interesting hypothesis relative to the structure of simple bodies; it evidently cannot be dealt with in a short note.—Studies as to the development of Amphipods, Part 4, by Mme. Marie Rossiiskaya-Koschewnikowa, being the history of development of the *Sunamphitoë valida*, Czerniawski, and the *Amphitoë picta*, Rathke (in French, with plates).—The Cladoceræ of the neighbourhood of Moscow, by Paul Matile (in German, with plates). The species *Daphnia dentata*, *Ceriodaphnia setosa*, and *Macrothrix borysthenica* are new.—Note on the Spongillidæ of the neighbourhood of Moscow, by W. Zykoff.

No. 2.—The Neocomian deposits of the Vorobievo Hills (near Moscow) by A. Pavloff (in French). The ferruginous sandstones and sands lodged between the Volgian (intermediate between Cretaceous and Jurassic) deposits and the ground moraine of the great ice-sheet are supposed to belong to the Cretaceous age, Wealdian, and Neocomian. The following fossils testify the Cretaceous age of the brown sandstone: *Olcostephanus* (*Ammonites*) *discofalcatus*, Lah.; *O. progrediens*, Lah.; *O. Decheni*, Röm. (not Weerth); and *Crioceras Matheroni*, D'Orbigny.—On the Turgaisk meteorite, by E. D. Kislakovsky. It was discovered by Kirghizes in 1888, when tilling the soil at Bish-tube, in the Nikolaevsk district of the province of Turgai, and consisted of two pieces—one about 70 pounds and the other about 36 pounds. The bigger one was bought by M. Nazaroff, who discovered also a third fragment (205 grammes) which was found some 3 feet north of the preceding. It has been given to the Moscow Society, and M. Kislakovsky's paper (illustrated by one plate) contains the results of its chemical analysis.—A catalogue of Lepidoptera of the Kazan province; Part 1, Rhopalocera, by A. Krulikovski (in Russian). The Lepidoptera of Kazan are very interesting, no fewer than 144 species of diurnal butterflies being already described by the author.—The *Tomicus Judeichii*, Kirsch., by Th. Teplouchoff (in German, with a plate). Its supposed identity with *T. duplicatus*, Sahlb., is discussed. Though formerly considered as a Uralian species, it has now been found all over middle Russia, as far as Courland.—On the history of development of *Hydrodictyon utriculatum*, Roth., by A. Artari (in German, with a plate).—Zoological researches in the Transcaspian region (continued), by N. Zarudnoi. Thirty-six species of reptiles and three amphibians are mentioned, and the catalogue of Mammalia and birds is completed according to observations made in 1889.

SOCIETIES AND ACADEMIES.

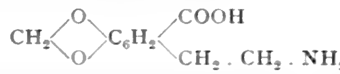
London

Physical Society, January 16.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The thanks of the meeting were unanimously accorded to the Maxwell Memorial Committee for presenting to the Society a copy of Maxwell's scientific papers. —Prof. G. M. Minchin read a paper on Photo-electricity. His experiments on the subject were commenced in 1877, in attempting to produce a photographic image of a distant object. The result sought for has not hitherto been obtained, but the experiments have led to the discovery of many interesting phenomena. Some of these were shown to the meeting. Electric currents are produced by the action of light on silver plates coated with collodion or gelatine emulsions of bromide, chloride, iodide, or other silver salts, or with eosine, fluorescine, and other aniline dyes, when the plates are immersed in a suitable liquid and one plate illuminated whilst the other is screened. The directions of the currents depend on the materials employed, and the blue end of the spectrum is most effective. Currents have a photographic effect on the plates, and this action is strictly confined to the parts through which the current has passed. Comparatively strong currents were obtained from plates coated with eosine and gelatine. A curious reversal was observed with some cells, the exposed plate first being positive to the screened one, and almost immediately afterwards became negative. On shutting off the light, a transient increase of current resulted, and afterwards disappeared gradually. M. Becquerel, who has studied the action of silver plates coated with bromide, &c., concludes that the nature of the exposed plate (whether positive or negative) depends on the thickness of the surface layer. The electromotive force of such cells rarely exceeds $\frac{1}{20}$ of a volt. Uncleaned tinfoil plates in common tap water give a current when one is exposed to light and the other screened. Cleaning the plates destroys the effect. In nearly every case the exposed plate was first positive, and after a time became negative; and, by exposing various parts of a plate (some portions of which had been previously exposed), the currents could be varied in direction at will. These peculiarities may explain the known divergence of tin from Volta's law. The phenomena have also been studied by aid of the electrometer. Tinfoil obtained from tobacco packages was found to be very sensitive to light. One side of such foil is dull and the other bright, and, by pasting slips of it on opposite sides of a glass plate, so that dissimilar sides were outermost, and immersing them in alcohol, a cell giving an E.M.F. of $\frac{1}{18}$ volt when the dull side was exposed to light was obtained. The addition of any salt to the liquid, with a view to diminishing the resistance, invariably reduced the E.M.F. Experiment showed that the alcohols were by far the best liquids to use with tin plates. A process for producing very sensitive tin plates was described, which apparently results in the formation of white oxide of tin on the surface of the foil. With one such plate and another unsensitized plate the best results have been obtained by immersing them in methyl alcohol prepared from oil of wintergreen. From experiments on the variation of E.M.F. with the intensity of light, it was found that the square of the E.M.F. is proportional to the intensity. Some tin cells behave in a very peculiar manner, for it often happens that a good cell will exhibit no E.M.F. after being kept a few days. A slight impulse or tap given to the cell or its support restores the sensitiveness; another impulse makes the cell unsensitive, and these effects can be repeated indefinitely. Such cells the author calls "impulsion cells," and some were exhibited at the meeting. The sensitive plate of one of these cells had different properties at its two ends, for impulses had no effect on the nature of the lower end, but changed the upper end from positive to negative and *vice versâ*. Electro-magnetic impulses such as produced by sparks are capable of altering cells from the unsensitive to the sensitive state, but fail to produce the reverse effect; a Hertz oscillator restored the sensitive state in a cell placed at a distance of 81 feet away. The "impulsion effects" can be got rid of by renewing the alcohol on several successive days. During last year the author made some selenium cells by spreading the melted substance on metal plates and immersing them in liquids together with an uncoated plate. Of the various metals and liquids tried, aluminium and acetone gave the best effects. The process of forming these "seleno-aluminium cells" was described. One of their peculiarities is that they are nearly equally sensitive to rays of all colours; and when exposed to strong light may give an E.M.F. of $\frac{1}{2}$ to $\frac{3}{4}$ of a volt, the sensitized plate being negative. An arrangement of 50 cells in series with an electrometer was exhibited, whereby the E.M.F. generated by light falling on the cells could be caused to ring a bell, light or extinguish electric lamps, &c. In conclusion, the author pointed out the possible applications of photo-electricity to photometry, telephotography, and the utilization of solar energy. At the request of the President, Prof. Minchin promised to show the experiments on February 13, on which date the discussion on the paper is to take place.—Prof. F. R. Barrell exhibited and described a lecture-room apparatus for determining the acceleration due to gravity. A number of iron balls are allowed to fall through a certain height in such a way that one starts off when its predecessor arrives at its destination. From the time occupied by all the balls, the time for one can be found, and, knowing the distance traversed, g can be determined. The apparatus consists of two electro-magnets joined in series with a battery and key, together with a ball-feeding device. One of the magnets is vertically over the key and serves to catch the balls as they emerge from the feeding tube, whilst the other magnet actuates a kind of slide for supplying successive balls. When one ball falls on the key, it breaks circuit, and thus causes the first-mentioned magnet to let go its ball. The key then springs up again, thus making circuit; the feeding magnet then supplies another ball, which is caught by the holding magnet until the falling ball reaches the key, and the operations are repeated. Fairly accurate results can be obtained by the apparatus.—Sir John Conroy's paper on the change in the absorption spectrum of cobalt glass by heat was postponed.

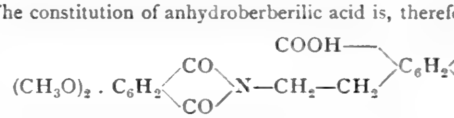
Anthropological Institute, January 27.—Anniversary Meeting.—Dr. John Beddoe, F.R.S., President, in the chair.—The following gentlemen were elected officers and Council for the ensuing year:—President: Dr. E. B. Tylor, F.R.S. Vice-Presidents: E. W. Brabrook, Hyde Clarke, and F. W. Rudler. Secretary: C. Peek. Treasurer: A. L. Lewis. Council: G. M. Atkinson, H. Balfour, C. H. E. Carmichael, Rev. Dr. R. H. Codrington, J. F. Collingwood, Dr. J. G. Garson, H. Gosselin, Sir Lepel Griffin, K.C.S.I., T. V. Holmes, H. H. Howorth, M.P., R. Biddulph Martin, Rt. Hon. the Earl of Northesk, F. G. H. Price, Charles H. Read, I. Spielman, Oldfield Thomas, Coutts Trotter, Sir W. Turner, F.R.S., M. J. Walhouse, General Sir C. P. Beauchamp Walker, K.C.B.

EDINBURGH.

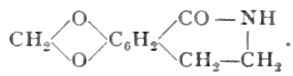
Royal Society, January 19.—The Hon. Lord Maclaren, Vice President, in the chair.—Prof. W. H. Perkin, Jun., read a paper on berberine. The alkaloid berberine, $C_{20}H_{17}NO_4$, yields, on oxidation with potassium permanganate, a number of substances, of which the more important are: oxyberberine, $C_{20}H_{17}NO_5$; dioxyberberine, $C_{20}H_{17}NO_6$; berberal, $C_{20}H_{17}NO_7$; anhydroberberilic acid, $C_{20}H_{17}NO_8$; and berberilic acid, $C_{20}H_{19}NO_9$. The study of these substances has given many results which afford a very clear insight into the constitution of the alkaloid. Anhydroberberilic acid dissolves in alkalies, forming a salt of berberilic acid; and this latter substance, when boiled with dilute sulphuric acid, is decomposed into hemipinic acid $(CH_3O)_2C_6H_2(COOH)_2$, and a new base, $C_{10}H_{11}NO_4$, which was shown to have the constitution



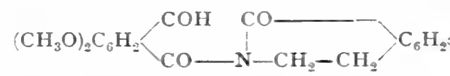
The constitution of anhydroberberilic acid is, therefore,



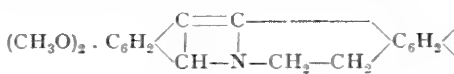
Berberal is decomposed by boiling with dilute sulphuric acid into pseudopianic acid, $(CH_3O)_2C_6H_2\langle^{COH}_{COOH}$, and a substance, $C_{10}H_9NO_3$, which is the internal anhydride of the base, $C_{10}H_{11}NO_4$, described above, and which, therefore, has the constitution



The constitution of berberal is represented by the formula—



and the constitution of berberine is, probably,



A detailed account of these experiments has appeared in the Journal of the Chemical Society (1889, p. 63, and 1890, p. 992).—Dr. Thomas Muir read a paper on some hitherto unproved theorems in determinants. Symbols such as

$$\begin{matrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ b_1 & b_2 & b_3 & b_4 & b_5 & b_6 \\ c_1 & c_2 & c_3 & c_4 & c_5 & c_6 \\ d_1 & d_2 & d_3 & d_4 & d_5 & d_6 \end{matrix} = 0$$

indicate the vanishing of *all* the expressions whose vanishing is indicated by the component determinants

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \\ d_1 & d_2 & d_3 & d_4 \end{vmatrix} = 0, \quad \begin{vmatrix} a_1 & a_2 & a_3 & a_5 \\ b_1 & b_2 & b_3 & b_5 \\ c_1 & c_2 & c_3 & c_5 \\ d_1 & d_2 & d_3 & d_5 \end{vmatrix} = 0, \quad \&c.$$

Dr. Muir has proved directly that the law of multiplication of such symbols is the same as that of the multiplication of determinants, and he has applied this result to obtain the proof of various theorems in determinants which have been hitherto unproved, though known to be true.—Dr. Muir also discussed a problem of elimination connected with glissettes of the ellipse and hyperbola. Prof. Tait has proved that the glissettes of an ellipse, which slides on rectangular axes, can be obtained as glissettes of an hyperbola sliding on axes inclined to the former. The equation of the glissette, referred to the guides as axes, is obtained by the elimination of a variable quantity between two equations. In carrying out the elimination, Prof. Tait obtained an equation of the tenth degree. But Prof. Cayley had shown that the equation should be of the eighth degree; and therefore Prof. Tait's equation contained an extraneous factor. Dr. Muir has succeeded in determining this factor.—The Hon. Lord Maclaren read a paper on the equation of the glissette of the curve $\frac{x^n}{a^n} + \frac{y^n}{b^n} = 1$. He obtains two equations by elimination between which the equation of the glissette can be found.

PARIS.

Academy of Sciences, January 26.—M. Duchartre in the chair.—An isochronous pendulum, by the late Prof. Phillips. This was one of the last memoirs prepared by Prof. Phillips before his death. It contains a description of a method of rendering the oscillations of a pendulum perfectly isochronous by means of a small steel spring. M. Wolf has conducted a series of experiments with a view of testing the efficiency of the method, and has obtained very satisfactory results.—On the approximate representation of functions, by M. Emile Picard.—On a recent experiment in which the direction of vibration in polarized light is determined, by M. A. Cornu. A paper by Herr Wiener, contained in *Wiedemann's Annalen*, vol. xl. p. 203, 1890, is said to give the experimental solution of this problem. The method consists in letting a wide beam of polarized light fall upon a reflecting surface at an angle of 45°. As the beam is wide, there is a zone where incident and reflected rays cut one another at right angles; and if interference phenomena are produced in this zone, the direction of vibration of the polarized light must be normal to the plane of polarization and perpendicular to the direction of propagation. In order to find the nodes and ventral segments, M. Wiener has used an extremely thin photographic pellicle, so transparent that it will allow a free passage to the two waves which cross at its surface, and yet sensitive enough to receive impressions. By means of this exploring pellicle the existence of interference fringes has been made manifest.—Some facts relating to the history of the principal nitrogenous compounds contained in vegetable mould, by MM. Berthelot and G. André.—New observations on the volatile nitrogen compounds given off by vegetable mould, by M. Berthelot. An interesting fact brought out by the experiments is that the nitrogen contained in the volatile organic compounds given off under certain conditions by argillaceous sand is always much greater in amount than the nitrogen given off in the form of ammonia. The vegetable mould employed was twenty times richer in nitrogen than the argillaceous sand, but gave off the two classes of compounds in equal proportions.—Essay on the synthesis of proteid matters (peptones), by M. P. Schutzenberger.—On the influence of the recent cold period on some of the animals in the menagerie of the Muséum d'Histoire Naturelle, by M. A. Milne-Edwards. The author remarks on the acclimatization of various animals which are generally supposed not to be able to live through the cold experienced this past winter, and on the death from cold of indigenous animals living under the same conditions.—Observations of comets Zona and Brooks (II. 1890) made with the great equatorial of Bordeaux Observatory, by MM. G. Rayet and Picard. Observations for position were made on December 8 and 28, and on January 6, in the case of Zona's comet. Brooks's comet was observed on January 7, 9, 11, 14, and 15.—On the personal equation in transit observations, by M. F. Gonnessiat. Two instruments have been utilized in the observations, having apertures of 57 mm. and 135 mm. respectively. The "eye and ear" and the "electrical" method have been studied concurrently; and various objects, from nebulæ to the sun, have been observed. In the former method the greatest plus value of the personal equation was 0·13s. for planets from 90″ to 20″ in diameter (second edge), and the greatest minus value was 0·02s. for stars of about the ninth magnitude. For faint nebulæ observed in a dark field having faintly luminous cross wires, the value reached +0·30s.—Arithmetical theorems, by M. H. Minkowski.—A purely algebraic demonstration of the fundamental theorem of the theory of equations, by M. E. Amigues. The theorem is that every complete algebraic equation with real or imaginary coefficients admits of at least a real or imaginary root.—On the movements of a double cone rolling on two guides, by M. A. de Saint-Germain.—On the resistance of the air to the movement of a pendulum, by M. G. Defforges. Commandant Defforges has determined the law of variation of the time and amplitude of oscillations executed by pendulums employed in geodetic operations, in terms of the pressure of the surrounding fluid.—On Huygens's principle, by M. A. Potier.—A theorem having reference to the calculation of certain electrical resistances, by M. C. E. Guillaume. The author considers the effect of the plugs of a resistance-box upon the values of resistances derived from it.—Researches on the application of the value of rotatory power to the determination of the combinations formed by aqueous solutions of malic acid with white alkaline phospho-molybdates, by M. D. Gernez.—On the conductivities of isomeric organic acids and their salts, by M. Ostwald.—Reply to M. Ostwald's note, by M. Daniel Berthelot.—Electro-metallurgy of aluminium, by M. A. Minet. An ingot of aluminium was exhibited which had been obtained by the electrolysis of aluminium fluoride, with an electromotive force of about four volts.—Employment of the calorimetric bomb for the determination of the heat of combustion of coal, by M. Scheurer-Kestner.—The mordants employed in dyeing, and their relation to Mendeleeff's periodic law, by M. Prud'homme. The author establishes a relation between the shades of colour obtained by fixing dyes with different metallic oxides, and the atomic weight of the substance employed. The shades, therefore, vary in a manner which follows the periodic series of the elements.—Experimental researches on tetanus, by MM. Vaillard and H. Vincent.—Chemical theory of the coagulation of blood, by MM. M. Arthus and C. Pagès.—Note *à propos* of diabetes, by M. H. Arnaud.—On the development of muscular fibres, by M. Louis Roule.—The vision of Gastropods, by M. Victor Willem.—Influence of some internal causes on the presence of starch in leaves, by M. Emile Mer.—Contribution to the study of green Bacteria, by M. P. A. Dangeard.—Conclusions relative to the inclosures in the trachyte from Mont Dore, by M. A. Lacroix.—Influence of the nature of soil on the conduction of heat, by MM. Ch. André and J. Raulin.—On the barometric pressure at Naples at different altitudes, by M. Eugène Semmola.—A magnetic perturbation exactly coincident with the earthquake observed at Algiers on January 15, by M. Moureaux.—On the method of correction of temperature for the emergent stem of a mercury thermometer proposed by M. Guillaume, by M. Renou.

CONTENTS.

THURSDAY, FEBRUARY 12, 1891.

ESSEX AND THE TECHNICAL INSTRUCTION ACT.

THE distribution of the funds placed at the disposal of the various County Councils, in accordance with the Technical Instruction Act of 1889 and the Local Taxation Act of 1890, is now engaging attention throughout the country, and it is widely recognized, by those who are responsible for the proper administration of these funds, that the money cannot be better spent than in furthering the cause of technical education with special reference to the needs of their own districts. It is, in fact, an open secret that the Councils are expected to apply their funds in accordance with the Acts of Parliament, and the voices of public opinion and of the framers and supporters of those Acts have frequently been heard to this effect. The facts and figures which we have from time to time published in the columns of NATURE have helped to keep our readers alive as to the present state of affairs in the different counties. The unanimity which prevails, so far as concerns the general principle of applying the funds to the purposes of technical instruction, is certainly a most encouraging indication of the direction in which public opinion is moving. The main difficulties in the way of apportioning the grants are likely to arise, however, when the various claims come to be considered by the Councils to whom they are submitted. This particularly applies to counties like Essex, where no great manufacturing centres exist, and where the occupations of the rural population are agricultural or maritime. It may be difficult at first sight to see clearly how the grants can be applied in such cases, so as to satisfy the wants of a non-urban community, and at the same time to convey assurance to the Council that the money has been well spent in accordance with the spirit of the Acts. It may be pointed out, however, that agriculture clearly comes within the definition, and is, in fact, recognized as a branch of technical science, and no County Councillor who has the maritime interests of his district at heart would grudge the extension of a similar recognition to the claims of applied marine zoology, of navigation, boat-building, or any of the other industries carried on along our coasts.

Essex may be taken as a typical example of a county which is both maritime and agricultural, and the action taken by its Council will no doubt be eagerly watched by the Councils of other counties similarly constituted. As being one of the home counties, moreover, its case presents particular interest. The total amount at the disposal of the county is about £21,000, of which about £4000 goes [to West Ham as the county borough, leaving £17,000 for the urban centres and rural districts of the remainder of the county. Numerous claims for grants have been sent in, and will receive attention in due course. Many grants for the carrying on of scientific and technical instruction in institutions already in existence in the larger towns will no doubt be justifiably made. But the means by which the ultra-urban districts can be provided for have yet to be developed, and the scheme put forward by the Essex Field Club, to which we have already briefly alluded in these columns, certainly seems to be sufficiently comprehensive to meet the wants of the case. Of the qualifications of the Club to carry on the work effectively it is not within our province to speak. It will suffice to say that the deputation from the Club, which was received by the County Council on February 2, comprised Sir Henry Roscoe, Profs. W. H. Flower, R. Meldola, and G. F. Boulger, Mr. F. W. Rudler, and others interested in the scheme. Lord Rayleigh, although unable to attend personally, had consented to allow his name to be added to the Committee.

The members of the deputation, whose scientific strength is unquestionable, expressed their approbation of the scheme, and spoke in the highest terms of the work hitherto done by the Club; if their expression of opinion is allowed that weight which it undoubtedly carries, there can be no doubt that Essex possesses in the Field Club an organization which the County Council would do well to avail themselves of. The scheme itself, which was formally submitted by the deputation, will be found in abstract in another portion of our columns. How nearly it falls in with the views of those most competent to speak authoritatively on the question will be gathered from the speech made by Sir W. Hart-Dyke at the recent Conference of the National Association for the Promotion of Technical and Secondary Education, in the course of which he said:—"To my mind the only practical way in which to carry on agricultural teaching is to have a central system. You must, I think, group together different villages and different schools, and have peripatetic teachers. If you do this you will find that the extra cost to school managers on the one hand, and to the ratepayers on the other, will be very small indeed, and yet you will be able to carry out an excellent system of agricultural education." An institution such as the Essex Field Club proposes to establish in connection with their Museum at Chelmsford would meet these views exactly.

MODERN BIOLOGY AND PSYCHOLOGY.

Animal Life and Intelligence. By C. Lloyd Morgan, F.G.S., Author of "Animal Biology," "The Springs of Conduct," &c. (London: Edward Arnold, 1890-91.)

THIS very interesting volume is nearly equally divided between the two subjects indicated by its title. In the earlier chapters we have excellent accounts of the nature of animal life and its relation to the environment; of the processes of life; of reproduction and development; of variation and natural selection; of heredity and the origin of variations; and of organic evolution. The later chapters deal with the senses and sense-organs of animals; the nature of mental processes in man, serving as a basis for our judgment as to the nature and amount of animal intelligence; the mental processes of animals are then very fully and carefully discussed in three long and very suggestive chapters; and this brings us to a final and very metaphysical chapter on mental evolution. It will be impossible here to do more than notice a very few of the interesting subjects which the author discusses with a fullness of knowledge and a judicial impartiality worthy of all praise.

In his chapter on "Variation and Natural Selection," Mr. Morgan deals with the question whether isolation, with no change in the conditions of existence, can lead to divergence of character, and thus to the formation of distinct species. He decides in the affirmative; and as he approaches the question apparently without any bias, it will be useful to see how far his arguments are well founded. He says:—

"Let us suppose that an island is divided into two equal halves by the submersion of a stretch of lowland running across it. Then the only possible cause of divergence would lie in the organisms themselves thus divided into two equal groups."

Before going further, it is well to note the utter impossibility of any such equality of conditions as is here supposed. Probably there is no island in the world but presents considerable climatal differences in its northern and southern, or in its eastern and western halves, even if its contour and geological structure were such as to admit of equal division. Neither is it conceivable that any division could separate every species upon the island into two equal portions, and without this the organic conditions would differ considerably. But, as we are considering a purely hypothetical case, we will allow the possibility of a sufficiently equal division in all respects. Mr. Morgan thus proceeds:—

"We have seen that variations may be advantageous, disadvantageous, or neutral. The neutral form a fluctuating, unfixed, indefinite body. But they afford the material with which Nature may make, through intercrossing, endless experiments in new combinations, some of which may be profitable. Such profitable variations would escape elimination, and, if not bred out by intercrossing, would be preserved. In any case the variety would tend to advance through elimination as previously indicated. But in the two equal groups we are supposing to have become geographically isolated, the chances are many to one against the same successful experiments in combination occurring in each of the two groups. Hence it follows that the progress or advance of the two groups, though analogous, would not be identical, and divergence would thus be possible under practically similar conditions of life."

Now this passage seems to me to be founded on a misconception of the true nature of the process of species production through variation and selection. The evidence we possess as to variation—and Mr. Morgan has given us some additional facts in this volume on the variation of bats—clearly shows that all the organs and parts of animals vary, in the two directions of greater or less development, about a mean value, which mean represents the typical or perfect character of the species for the time being. This typical character has been reached ages ago by all species in countries whose conditions are tolerably stable, and the remarkable fixity of character proved to exist, not only by the mummied animals of Egypt, but by those much older forms which were the contemporaries of Neolithic man, indicates that for such species, so long as conditions remain approximately unchanged, there are no "profitable variations" possible. They have long ago been brought into almost complete harmony with their environment; all possible combinations have long ago been tried; and the fact that though in every successive generation there are variations of every organ and every character in both *plus* and *minus* directions, these are not taken advantage of, but the mean assemblage of characters constituting the species remains unchanged, proves that changes in any direction without change of environment would be disadvantageous. There can therefore for such a species be no "profitable variations" so long as conditions remain, as they do *ex hypothesi*, absolutely unchanged; no "progress or advance" is possible without such change; and to suppose that there can be further divergence and specialization under the conditions assumed is to maintain that, what natural selection, acting on the innumerable variations yearly occurring in the whole species, has been unable to do in the past, will be done in the future by the same causes acting on the two halves of the species separately.

It cannot of course be denied that, however many combinations and variations have occurred in any species during the last ten thousand years, some few superior variations may occur in the next ten thousand, but there will probably be a corresponding number of cases in which the best variations of the year are below the usual standard, and there is no reason to believe that the one will do more than balance the other. The proviso which Mr. Morgan duly makes—"if not bred out by intercrossing"—would certainly require to be taken account of, since the idea that single favourable sports have had any part in the formation of new species is now rarely adopted by evolutionists. We must also always remember Darwin's maxim, generally admitted to be a sound one, that "Nature does not produce absolute perfection but only relative perfection,"—which again implies that when each species has reached an equilibrium with its environment there is for it no further perfection possible under the circumstances, no "profitable" variations tending to modify its mean specific characters of which natural selection can take account. For these various reasons it seems to me that any permanent modification of a species by mere isolation of a portion of it, and without some adequate change in the environment, is almost inconceivable.

Connected with this question is that of the existence of useless specific characters, which are not and never have been correlated with useful characters. Mr. Morgan here very properly suggests that the difficulty is as to what is to give such useless characters any fixity, and without fixity they will not be classed as specific. In a later chapter, on "Heredity and the Origin of Variations," he himself suggests a possible escape from this difficulty in the supposition that the intercrossing of individuals differing somewhat in character does not result in mere "hereditary mixture" but in "organic combination,"—meaning, I presume, that by such intercrossing new characters or the rudiments of new organs may be produced which were not present in any of the ancestral forms. He supposes that such combinations may initiate definite lines of variation, and that we may thus obviate the difficulty as to the origination of organs or structures whose first rudiments cannot be conceived to have been useful to their possessors. It seems to me probable that, however originated, there *are* such "lines of variation," and that some of the unknown laws of variation *do* lead to the initiation of the structures or organs which have been essential to the development of the varied types of the organic world; but I nevertheless maintain that this does not necessitate the acceptance of the doctrine of useless "specific" characters, or that of the formation of new species by isolation in an unchanged environment. For, by the assumption, these lines of variation and these nascent structures are produced by favourable combinations within the limits of a species. They appear more or less sporadically; they are at first of no utility; there is therefore nothing to give them fixity or to lead to their general and uniform development in all the individuals composing the species. Thus they must remain, sometimes dying out, sometimes advancing, till under some changed conditions of the environment they become of use in the struggle for existence. From that moment they become subject to the law of natural selection. All individuals not possessing these characters, or possessing them in too small a degree, are eliminated, leading at once to the steady increase of the character and its constant presence in all individuals of the species. It has now become a "specific" character, but only because it has become useful. The definite "line of variation" is now followed because it is a useful line. But, the moment it reaches a maximum of utility, elimination prevents any further development in that direction although the tendency may still exist, and variations which are now injurious may still continue to appear though they cease to be preserved. This is the view I have already expressed in regard to Prof. Geddes's theory of variation in plants ("Darwinism," p. 428), admitting the tendency of vegetative development in the various ways he suggests to be highly probable, but denying that such causes can produce definite fixed characters without the eliminating agency of natural selection.

It has been objected that this view is inconsistent with the theory that the ornamental appendages and colours of male birds and insects have been produced by exactly such a tendency to development along certain lines of variation; but it is forgotten that in this case such development is strictly correlated with the superabundant energy and vital activity of male animals, characters which are the subject of both sexual and natural selection. They are therefore increased within the limits of hurtfulness, while their utility as recognition marks, and as indications of sexual maturity, keeps them tolerably constant.

In discussing Weismann's theory of the continuity of the germ-plasm, Mr. Morgan gives reasons for believing that it is in some respects a retrograde step, and that the earlier view, of the continuity of reproductive cells, is the more probable; and after discussing the various opposing theories—Darwin's pangenesis, Haeckel's perigenesis, Spencer's physiological units, and some others—he arrives at the conclusion that there is a continuity of reproductive cells; that hereditary similarity is due to the fact that parents and offspring are derived eventually from the same germinal cells; that there is no convincing evidence that in the Metazoa special modifications of the body so influence the germ as to become hereditary; but nevertheless, he concludes, there is no reason why such influence should not be assumed as a provisional hypothesis.

The chapter on "Organic Evolution" is a most interesting one, discussing, as it does, in the author's suggestive manner, the various problems arising out of the theory of variation and natural selection. The greater part of this discussion is clear and convincing, but a few cases must be noticed in which the essential point of the argument appears to have been overlooked or insufficiently appreciated.

In considering the agency of natural selection it is urged that it can only act where there is a direct advantage to some individuals or disadvantage to others; that the advantage must be immediate and present; that the advantage must be sufficient to decide the question of elimination or non-elimination; and that we must distinguish between mere indiscriminate destruction and selective elimination. Now, throughout this discussion, and especially in the last portion of it, Mr. Morgan fails to give due weight to the enormous scale on which Nature works, both as regards the number of individuals, the space over which they are distributed, and the time during which the process is going on. If we take all these factors fully into consideration, we shall, I think, see that there is really no importance in what seems to us fortuitous destruction, and that, sooner or later, every beneficial or injurious variation, such as we know are abundantly produced every year, must produce a corresponding effect. He says:—

"A hundred are born, and but two survive. It is a mistake to say that of the hundred born the two survivors are necessarily the very best of the lot. It is quite possible that indiscriminate destruction got rid of ninety of all sorts, and left only ten subject to the action of a true elimination."

Now, this would be quite true, and a valid argument, if a species usually consisted of a few hundred individuals, and the question of modification by natural selection had to be decided within ten or fifty years. But when the hundred individuals are multiplied by perhaps a million spread over a large area, and when the operations of accidental destruction and elimination go on during thousands and even millions of generations, we feel sure that, on the average of the whole, the worst will be strictly eliminated, the best as strictly preserved. A passage is quoted from Prof. Weismann about the destruction of eggs by weasels, cats, crows, &c., of the helpless young birds by the same enemies, of others by cold and hunger in winter, and yet others by the dangers of migration, and it is said: "There is here, first, a certain amount of fortuitous destruction; secondly, some selection applied to the eggs, &c." But surely, as regards the whole species, and on an average over long periods, "fortuitous destruction"—that is, destruction which overtakes the very best as well as the very worst—must be totally insignificant, as compared with true selective elimination. The capacity of the parents to conceal the nest and eggs, and to protect the young birds, will have been constantly increased by selection, as well as every other faculty and character that is of value to the species, till a condition is reached by which the standard population of the species can be permanently maintained. In different years different qualities ensure survival, and thus some may often survive for a few years which are not so well fitted on the whole as some that have succumbed. But in considerable periods, including years of the severest trial, all these comparatively imperfect individuals will be destroyed, and only the very best be left to continue the race.

In discussing the origin of the beautiful forms and colours of birds, insects, and flowers, it again seems to me that there is some want of perception of the exact points at issue. Mr. Morgan argues, as I think very justly, that even the higher animals have no sense of beauty, and that "the word æsthetic should be resolutely excluded in any discussion on sexual selection." He urges, as I had myself done (NATURE, vol. xlii. p. 291), that a considerable portion of the beauty of flowers, as well as that of birds and insects, is due to symmetry, elegance of outline, surface texture, and other causes. It is, he says, the nectar, not the beauty of the flower, that attracts the bee; and in birds, "the mate selected has been that which has excited the strongest sexual appetence; his beauty has probably not, as such, been distinctly present to consciousness." All this seems to me to be excellent, but in another part of his work Mr. Morgan imputes to me opinions which seem to me erroneous, and which I am not aware of having expressed. Thus, at p. 206, speaking of flowers, he says:—

"And when we ask in this case, as we asked in the case of the beautiful colours and forms of animals, what has guided their evolution along lines which lead to such rare beauty, we are given by Mr. Wallace himself the answer: 'The preferential choice of insects.' If these insects have been able to produce, through preferential selection, all this wealth of floral beauty (not, indeed, for the sake of beauty, but incidentally in the practical business of life) there would seem to be no *a priori* reason why the same class, and birds and mammals, should not have been able to produce, through preferential selection, all the wealth of animal beauty."

I do not remember ever having used the term "preferential choice" as applied to insects and the special colours or markings of flowers. I have always held that these are merely signs of the presence of products which the insect needs and enjoys, and that there is probably no more preference for those particular colours and marks on the part of the insect than there is preference by us for a particular *number* because it indicates our friend's house, or for a particular *colour* because it is that of the seal of a favourite wine. Both number and colour may be in themselves either indifferent or even disagreeable to us, but they none the less serve their purpose of recognition marks. I see no more difficulty in the beauty of flowers and birds and insects being all incidental to the general laws of growth and development, subject always to the law of elimination, than in the beauty of landscape, of foliage, of crystals, of corals diatoms and shelly mollusks, of the exquisite forms and motions of the gazelle, the horse, or the kitten, which have all been produced without any question of preferential selection.

Again, after quoting my statement of certain facts showing that isolation is produced by the likes and dislikes of animals, Mr. Morgan says:—

"Mr. Wallace thus allows, nay, he lays no little stress upon, preferential mating, and his name is associated with the hypothesis of recognition marks. But he denies that preferential mating, acting on recognition marks, has any effect in furthering a differentiation of form or colour."

Now the passage Mr. Morgan quotes referred almost exclusively to preferential association, not to preferential mating, which I consider to be a result of the former. And this preferential association must certainly lead by elimination to a furthering of the differentiation of form or colour exactly so far as that differentiation was useful, and it even might be continued farther, as I believe it sometimes has been, till checked by absolute hurtfulness, if correlated with the extreme vigour and activity of male animals.

Mr. Morgan discusses at considerable length the question of whether the effects of use and disuse are hereditary. He admits the very imperfect character of the evidence in favour of the proposition that they are so, and he adduces, as in his opinion one of the best cases, "the instinctive avoidance" of nauseous and stinging insects by most birds. As neither the nauseous taste nor the stings are usually fatal, the avoidance of them is not of eliminating value, and cannot, therefore, have been produced by natural selection. Hence he thinks the inheritance of individual experience probable. But the "instinctive avoidance" is here assumed, whereas there is now good reason to believe that in the case of nauseous insects, and probably also of stinging insects, the avoidance is the result of individual experience or observation. Some of the most curious phenomena of mimicry can only be explained on this hypothesis.

To many readers, the latter portion of the volume, dealing with the senses and intelligence of animals, will be the most attractive. The chapter on the senses of animals is an admirable summary of the most recent observations and researches on this subject, and the explanation of the probable mode of vision of insects by means of their compound eyes is especially clear and very instructive. Then follow chapters on the mental processes of man and of animals, characterized by clear definitions and acute analysis. It is impossible to summarize these chapters, but some of the author's conclusions may be quoted. On the question of the psychology and intelligence of ants, bees, and other insects, after pointing out their widely different structure and sense-organs, he says:—

"Remember their compound eyes with mosaic vision, coarser by far than our retinal vision, and their ocelli of problematical value, and the complete absence of muscular adjustments in either one or the other. Can we conceive that, with organs so different, anything like a similar perceptual world can be elaborated in the insect mind? I for one cannot. Admitting, therefore, that their perceptions may be fairly surmised to be analogous, that their world is the result of construction, I do not see how we can for one moment suppose that the perceptual world they construct can in any accurate sense be said to resemble ours."

The following passage in like manner gives the author's conclusions as to the difference between the mental nature of man and the higher animals:—

"Furthermore, it seems to me that this capacity of analysis, isolation, and abstraction constitutes in the possessor a new mental departure, which we may describe as constituting, not merely a specific, but a generic difference from lower mental activities. I am not prepared, however, to say that there is a difference in kind between the mind of man and the mind of the dog. This would imply a difference in origin or a difference in the essential nature of its being. There is a great and a marked difference in kind between the material processes which we call physiological, and the mental processes we call psychical. They belong to wholly different orders of being. I see no reason for believing that mental processes in man differ thus in kind from mental processes in animals. But I do think that we have, in the introduction of the analytic faculty, so definite and marked a new departure that we should emphasize it by saying that the faculty of perception, in its various specific grades, differs generically from the faculty of conception. And believing, as I do, that conception is beyond the power of my favourite and clever dog, I am forced to believe that his mind differs generically from my own."

This seems a very fair statement of the case, and one to which, so far as it goes, I have no objection to make. But in the concluding chapter, on mental evolution, we have a serious attempt to overcome the difficulty of the relation between the physiological and psychical processes here stated to belong to "two wholly different orders of being." This is supposed to be done by the adoption of the *monistic hypothesis*, which assumes that these "wholly different orders" of things are really identical—that *neurosis is psychosis.* "The neurosis is the outer or objective aspect, the psychosis is the inner or subjective aspect." Then the subject is attempted to be made clearer by the adoption of other terms—"kinesis" for physical manifestations of energy, "metakinesis" for all manifestations of the mental or conscious order; and we have the following statement:—

"According to the monistic hypothesis, *every mode of kinesis has its concomitant mode of metakinesis, and when the kinetic manifestations assume the form of the molecular processes in the human brain, the metakinetic manifestations assume the form of human consciousness.*"

If this means anything, it means, what has been stated in simpler but equally exact and more intelligible language, that all force is will-power. But it goes further, and implies that there can be no mind like that of man, or superior to it, without a brain formed of similar materials and similarly organized with the brain of man. This necessary connection, and even identity, of the two is, however, what is not proved, and not even, in my opinion, shown to be probable.

The last few pages of the volume are devoted to a discussion of the causes which have led to the development of the higher intellectual faculties in civilized man, and a difficulty is found in explaining this development, except on the ground that the increase of intellectuality acquired by use of the intellectual faculties is inherited. The objection may be made that there is no proof of any increase of average intellect in Europe during the last two or the last twenty centuries; and, on the other hand, it seems probable that, although the unintellectual may generate more rapidly, a smaller proportion of the offspring survive. It is suggested that the development of the social habit, the mutual aid and protection thus afforded, may well have left a balance of the life-energy previously employed for individual self-preservation, available for the increase of pure intellect. The exceedingly sporadic character of exceptional intellectual power favours this view, which is analogous to that which I have suggested as having led to the development of the accessory plumes of male birds. Whether the very existence of such faculties can be adequately explained by increased brain-development alone, is another matter.

The present notice, necessarily confined to a few of the more salient features of the book, gives no fair idea of the great variety of topics treated, nor of the originality and clearness which are its great characteristics. The numerous woodcuts and diagrams are all illustrative of the text, and are fully explained, and the author is particularly happy in his use of diagrams and formulæ to illustrate the more obscure or difficult conceptions. The diagram, at p. 141, to explain Weismann's theory, as illustrated by the question, "Does the egg produce the hen, or does the hen produce the egg?" is one of these, and will render the problem intelligible to many who would otherwise have a difficulty in understanding it. On the whole, the work will prove a boon to all who desire to obtain a general knowledge of the more interesting problems of modern biology and psychology by the perusal of a single compact, luminous, and very readable volume.

ALFRED R. WALLACE.

THE LAKE-DWELLINGS OF EUROPE.

The Lake-Dwellings of Europe; being the Rhind Lectures in Archæology for 1888. By Robert Munro, M.A., M.D. (London: Cassell and Co., 1890.)

IN this work on the lake-dwellings of Europe, Dr. Munro has carried out on a wider field the inquiry begun in 1882 in his book on those of Scotland. He has brought to his task qualities of a high order. He had a large share in the exploration of the lake-dwellings in his own country, and has recorded the results in a business-like fashion. He has prepared himself for dealing with the lake-dwellers of the Continent by a painstaking examination of the evidence on the spot, and by visiting all the principal collections. He further has read the voluminous literature bearing on the subject scattered through various journals and periodicals, as well as that which lies ready to hand in separate books. The result of all this labour—and how great it has been only those can know who, like the writer, have gone over the ground—is this work "smelling of the oil" in every page, well illustrated with numerous cuts and with good indexes, and a systematic list of references. It is, indeed, to be looked upon as an encyclopædia of matters relating to lake-dwellings, rather than as an ordinary book. It is little less than a miracle that the vast array of facts brought together should have been compressed into the narrow limits of six lectures.

The lake-dwellings of Switzerland and of the surrounding parts of the Continent were laid before the archæological public in 1866 by Dr. Keller and his English translator and editor, the late Mr. Lee, to whom, some twelve years later, we owe a second and enlarged edition. Since that time discovery has rapidly followed discovery in various parts of Europe. These have been carefully recorded by Dr. Munro in the work before us. The first three hundred pages, comprising the first three lectures and part of the fourth, deal with the lake-dwellings in Switzerland, France, and Italy. The rest of the fourth lecture is devoted to the discoveries in North Germany made principally by Prof. Virchow, and to the curious mounds which stud the marshes of Holland, and which occur also in those at the mouth of the Elbe.

The *terpen*, as they are called, of Holland, deserve more than a passing notice. They rise to a point above high-water mark, and break the monotony of the dead level which, before the construction of the sea-dykes in the twelfth century, must have been covered by every tide. Then they must have been islands, now they are mounds of various sizes, and distributed at sufficiently convenient intervals to afford sites for modern churches and villages, and even towns such as Leeuwarden and Leyden. A few years ago it was discovered that they were largely composed of domestic refuse, so rich in animal matter as to be of great value for manuring the land. The excavations for that purpose have shown that they are artificial islands formed of clay and of timbers, both upright and horizontal, which have been inhabited long enough to allow of great accumulations of refuse. Among the fragments of pottery found in them may be noted the Samian ware; and among the articles made of bone, combs identical with those found in association with Roman remains in lake-dwellings in Scotland. There are also glass beads, bronze dishes and tripods, and Roman statuettes and fibulæ, together with iron bridle-bits, shears, and hammers. The associated coins of the Roman emperors—Byzantine, Anglo-Saxon, and Frankish—fix the date of their occupation. They were inhabited from the time of the Roman attack on the Rhine as late at least as the reign of Lothair (A.D. 840–855), whose empire extended from Italy northwards as far as the Meuse and the Rhine. It is a significant fact that the evidence of the occupation of these mounds ceases with the break-up of the empire of Charles the Great, and the struggles between his degenerate descendants. They probably lay desolate towards the end of the ninth century, and became so completely forgotten that their story has only been made out by the recent diggings.

These mounds, however, apart from the light which they throw on the darkness of the written records of the first eight centuries after Christ, have a further claim to our interest in the fact that they were well known to the students of history in Rome as early as the first century after Christ. They are described by Pliny the Elder, in a passage worthy of being quoted:—

"We saw," he writes, "in the north the Chauci, who are called the Greater and the Less. In their country there is a great tract over which the ocean rolls in great volume twice each day, and raises for us the never-ending discussion as to whether it should be called land or sea. Here a miserable tribe lives on lofty mounds or artificial eminences raised to a height which they know by experience to be above the highest tide. These they occupy with their cabins. When the tide is up, they look like sailors in ships with water on every side; when it is down, like mariners who have been stranded. Their food is fish, caught around their cabins in the ebbing waters. They have no cattle, and do not use milk as their neighbours do, nor does it fall to their lot to contend with wild beasts, since there are no shrubs for cover. They twist cords of sedges and marsh rushes, and make them into fishing-nets. They dig turves, too, and, after drying them more by the wind than the heat of the sun, cook their food with them, and so warm their stomachs, frozen by the northern blasts. Rain-water is their sole drink, stored in ponds at the entrance of their houses. Even these tribes, were they conquered to-day by the Roman people, would say that they were slaves" (Pliny, "Nat. Hist., xvi. 1).

At a later time they possessed oxen and sheep and horses, and from the occurrence of Roman coins and of Samian ware—the Sèvres of the period—it may be concluded that they were in touch with the Roman civilization. They may have served in the Roman army, and they most probably shared in the attacks repeatedly made by the Germanic tribes on the Roman Empire. It would, indeed, be strange if none of the spoils of the legions of Varus, and if no traces of the conquest of their country by Germanicus, followed by his disastrous retreat, were met with in these ancient dwellings in the marshes of Holland.

The fifth chapter is devoted to the lake-dwellings of Britain and Ireland. The Irish crannogs date as far back as the Bronze Age, if not as far back as the Neolithic, and were inhabited as late as the time of Charles II., while those of Scotland begin with the Iron Age, and range as far down as the seventh and eighth centuries after Christ. Those on the south-west—such, for example, as Loch Lee, near Dumbarton—contain the same mixture of fragments of Samian ware and other articles of Roman civilization, with barbaric implements implying a rude manner of life, as we find in the *terpen* of Holland. The region between the Roman Wall and the Highlands was the scene of continual fighting between the Picts and Scots on the one hand and the Roman legions on the other, from the days of Agricola down to the time of the retreat of the Romans from Britain, and afterwards between the Celtic tribes and the English. With the conquest of the kingdom of Strathclyde by the Northumbrian Angles their history ceases.

Dr. Munro ascribes all the lake-dwellings in the British Isles to a Celtic source, and he believes that the Celts of Britain got the idea of protecting themselves by making artificial islands in lakes or morasses from "contact with the inhabitants of the pile villages in Central Europe." These conclusions seem to me not to be proved. It may be allowed that most, if not all, of our British lake-dwellings were built and inhabited by Celtic peoples, but it does not follow that they got the art of building them from Switzerland. Lake-dwellings would be naturally invented by any race living in a state of warfare in a land of lakes and morasses. In other regions the Celts constructed for the same purposes fortified camps where there were no lakes and morasses. The lake-dwellings, therefore, are most probably a mere local development depending on the geographical conditions. Seeing that pile-dwellings are now used by widely different races, ranging over an area from Central Africa to New Guinea, it is improbable that they should have been built by one race only in prehistoric times. In Switzerland itself they were used by the various invaders from the Stone Age down to the time when their Gallic inhabitants were conquered by the Romans.

Dr. Munro sums up with great care in his concluding chapter the principal features of the civilization of the European lake-dwellers. He accepts the Age of Stone, and rejects that of copper, pointing out that copper implements are probably the result of the local scarcity of the tin necessary for the manufacture of bronze. In treating of the Bronze Age, he considers the crescent-shaped earthenware articles rising from a flat base, found in several of the villages, to be "suggestive of religious ideas" instead of being head rests. The latter and more popular view seems to me to be most probable. Again, our author says that "the lake-dwellings of the Bronze Age are built in deeper water, and consequently further from the shore, than those of the Stone Age" (p. 538). Surely they were built further from the shore for purposes of defence against the better weapons, and consequently in deeper water. These, however, are minor points of criticism in a work which will be of great service to archæologists. Dr. Munro is to be congratulated on his success in completing a most difficult task.

W. BOYD DAWKINS.

OUR BOOK SHELF.

Colour in Woven Design. By Roberts Beaumont, Professor and Director of the Textile Department in the Yorkshire College. Pp. xxiv. and 440. (London: Whittaker and Co., 1890.)

AMONGST the merits which this book may possess (and we do not deny that they are considerable), elegance and accuracy of diction cannot be reckoned. This criticism is justified by the occurrence of countless phrases, such as these—"Non-luminous bodies are incompetent of emitting undulations that convey any coloured appearance to the mind"; "linear and curvilinear lines"; and "in the rose there is displayed in perfection all the various modifications in tint and shade to which this important colour (red) is susceptive." And we cannot endorse in all particulars the exposition of the theories of colour given by Prof. Beaumont. For instance, he contrasts what he calls the "light theory of colours" with the "pigment theory," and then, speaking of the latter, says: "Scientifically, it is no more a correct scheme than the light theory is applicable to the industries or to the mixing of paints." But surely the theory of Young, Maxwell, and Helmholtz is as applicable to the results obtained by mixing pigments or coloured fibres, as it is to the results of mingling coloured lights. Yet, while the author writes, on p. 20, "many of the mixtures obtained by this system (that adopted by Chevreul and Brewster) are diametrically opposed to the laws of physics," he proceeds to explain the chromatic phenomena of textiles by its aid. It is needless to urge how deeply Prof. Beaumont's acceptance of the red-yellow-blue triad of primaries vitiates his reasoning as to the effects of contrast, as to the question of the existence of tertiary hues, and as to the true complementaries.

When, however, we turn to the practical or technical sections of this hand-book, we find much information of sterling value. Here Prof. Beaumont is evidently at home. The numerous diagrams and photographs of checks, stripes, weaves, yarns, twists, twills, and diagonals, illustrate the descriptions in the text most satisfactorily. The analysis and synthesis of the various "weaves" are particularly well carried out, and constitute the largest and most important part of the volume before us. A scientific journal is, however, not the place for the discussion of such details of manufacture.

A few of the coloured plates are satisfactory; in others the garish hues and harsh associations may, we hope, be attributed to the failure of the chromolithographs to realize the intentions of the author. But some of the coloured figures are deplorably poor, or even thoroughly debased, in design; note particularly Plate xviii.; Plate xxviii., Fig. 2; and Plate xxxi., Fig. 2. A. H. C.

Constance Naden: a Memoir. By William R. Hughes. (London: Bickers and Son. Birmingham: Cornish Brothers. 1890.)

MISS NADEN was a writer of considerable freshness and ability, and all who knew her agree that she was also a woman of great charm of character. She did not, however, live long enough to produce anything of first-rate importance, and it was hardly advisable to make her the subject of a special memoir. Mr. Hughes appreciates thoroughly all that was most characteristic of his friend's intellectual and moral nature, but he does not possess the secret of presenting brightly and vividly facts in which he himself happens to be interested. Consequently, he does not succeed in conveying any adequate conception even of qualities which he is never tired of praising. The volume contains, besides Mr. Hughes's sketch, an introduction by Prof. Lapworth, and "additions" by Prof. Tilden and Dr. Lewins. The latter gentleman, who delights in the use of an extraordinary philosophical jargon, thinks it would be impossible to be satisfied with any memoir of Miss Naden "which should ignore the scientific hylo-ideal, or automorphic principle, or synthesis underlying and suffusing her whole intellectual and ethical architectonic." He proceeds to supply the necessary exposition, his chief difficulty being "the elementary *naïveté* and simplicity of the concept, or ideal, involved."

Euclid's Elements of Geometry. Arranged by A. G. Layng. (London: Blackie and Son, Ltd., 1890.)

IN this work, Euclid's Books I.-IV., VI., and portions of V. and XI., are dealt with. The enunciations and axioms are the same as those in Simson's edition, but the propositions have received many minor alterations. Only the more common symbols are employed, and some of the propositions have been considerably shortened by the adoption of other proofs based on Euclid's methods. Each proposition is accompanied with examples and in many cases with notes.

An excellent plan adopted throughout is that by which the student can see at a glance the enunciations, propositions, and figures, without the necessity of turning over a page. The appendix contains some simple theorems of modern geometry, a few alternative proofs of the propositions based on other methods than those of Euclid, and a collection of miscellaneous examples and examination papers. Beginners will find the book rather troublesome at first, owing to the use of the symbols; but after these are understood little difficulty ought to be experienced.

LETTERS TO THE EDITOR.

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Hermaphroditism of the Apodidæ.

THE reproduction of *Apus cancriformis* has been a much discussed subject. Although the animal has been well known since the middle of last century, it was not till 1833 that a male was reported to have been found, and not till 1856 that the occasional presence of males in small numbers was certainly established by Kozubowski. On the other hand, the fact that several generations of "females" could be produced without the presence of a male, was established as long ago as 1755 by Schaeffer, who concluded that the animals were hermaphrodite. Since that time authors have been divided in opinion between hermaphroditism and parthenogenesis (not to mention v. Siebold's theory of Thelytoky); the latter view has lately prevailed.[1]

[1] For the history of this subject see Bronn's "Classen und Ordnungen des Thierreichs," vol. v. On p. 810 the following words occur:—"Untersuchungen über die Gattungen Apus und Daphnia welche offenbar in dem bis zu voller Evidenz geführten Nachweis der Parthenogenetischen Fortpflanzung beider gipfeln." See also Lang's "Lehrbuch der Vergleichende Anatomie," p. 393.

The animals, however, prove after all to be hermaphrodites. Since the last careful study of *Apus cancriformis*, as a whole, by Zaddach in 1841 (the works of Ray Lankester and others deal only with special points), new methods of research have been introduced into our laboratories which reveal details not easily discoverable by the older methods. Zaddach's figures of the ovaries and testes of Apus are thus naturally somewhat deficient—as deficient, indeed, as the best work we can do to-day will, we hope, be found to be fifty years hence.

As already announced in a preliminary note,[1] published in the current number of the *Jenaische Zeitschrift für Naturwissenschaft* (Band xxv., N.F. xviii.), a small species of Apus kindly handed me by Prof. Kükenthal, and presumably *Lepidurus glacialis*, Kroyer, proved on examination to be hermaphrodite. The specimens were found in East Spitzbergen during the expedition sent thither by the Bremen Geographical Society in 1889, under the conduct of Prof. Kükenthal. The species seemed to be new, as it did not agree with any of the descriptions of *Lepidurus glacialis*; not only was the whole animal much smaller, but its caudal plate was much smaller and not notched at the tip, and, most important of all, it possessed well-developed second antennæ, which have till now never been found in *Lepidurus glacialis* (Huxley's "Anatomy of the Invertebrata," p. 243). The new species, however, proved to be identical with the *L.* "*glacialis*" brought back from West Spitzbergen by Prof. Nathorst, specimens of which were kindly sent me by Prof. Leché, of Stockholm. It thus at first seemed likely that there were two species of Lepidurus in the Arctic regions—a *Lepidurus glacialis* and a *Lepidurus spitzbergensis*. I am now, however, inclined to look upon *L. spitzbergensis* as a stunted variety of *L. glacialis*, or, rather, as a precociously ripe young stage. My reasons for considering it merely a variety of *L. glacialis* are as follows:—

(1) I have succeeded in finding very distinct second antennæ in a large specimen of *L. glacialis*, from Greenland, kindly sent me by the Rev. Canon Norman, so that this supposed difference does not exist. (2) On examination of the genital tube, the sperm-forming centres are found in identically the same place in the two species, viz. at the posterior end of the genital tube, both, in this respect, differing from the other Apodidæ I have as yet had at my disposal to examine. (3) The other two differences—the small size of *L. spitzbergensis* and the undeveloped state of its caudal plate—are, on this supposition, easily accounted for.

We thus have, instead of two Arctic species of Lepidurus, a fully-developed *L. glacialis*, Kroyer, presumably in the warmer regions, and a small, precociously developed variety from the colder and more northerly regions—*L. glacialis* var. *spitzbergensis*.

Whether the variety is permanent or not, I have no means of deciding. It is interesting to find that Packard's measurements for *L. glacialis* make it smaller than *L. spitzbergensis* ("Monograph of the North American Phyllopoda," 1883), which shows that *L. glacialis* may be dwarfed by the unfavourable environment. As Packard's drawings are (judging from the development of the caudal plate) of the fully-developed animal, this leads one to think that perhaps, in rather longer summers, the Spitzbergen variety may develop into the typical *L. glacialis* without any great increase of size.

In my preliminary notice announcing the hermaphroditism of *L. spitzbergensis*, knowing how much the reproduction of the Apodidæ had been discussed, I ventured to assert that in all probability the other species of the genus would also prove on closer examination to be hermaphrodite. As above stated, I found the sperm-forming centres in *L. glacialis* in identically the same position as in the Spitzbergen variety. By the kindness of Prof. Möbius, the Director of the new Berlin Museum, and of the Rev. Canon Norman, I have also been able to examine *Apus cancriformis* and *Lepidurus productus*. In both these the sperm-forming centres were found scattered here and there among the rich branchings of the segmental diverticula of the genital tube. They occur either at the tips of such branches, where the eggs ordinarily develop, or as slight lateral bulgings of the same. In all cases the spermatogenesis is the same, the epithelium breaking up into sperm cells; these escape into the lumen of the tube, and are found in considerable numbers near the genital aperture, where the epithelial lining of the tube is hardly demonstrable, the walls of the tube consisting of a fibrous membrane, in the folds of which the sperm-cells lurk. The eggs are then fertilized as they stretch this membrane in passing out into the egg pouch. The whole richly-branched reproductive organ, with the eggs developing at the tips of the branches, and with here and there a testis, strongly reminds one of a monœcious plant, self-contained, and able to dispense with pollen from without.

[1] In this note, by an oversight, I stated that Schaeffer concluded the animals to be parthenogenetic.

I reserve the drawings and the more detailed description of the reproductive organs of the different species for a short comparative study of the Apodidæ which I hope soon to have ready for the press. By way of caution, however, I should here add that small yellowish sacs filled with minute cells occur here and there among the developing eggs. These must not be mistaken for the testes. They are the loci of discharged eggs, and the minute cells are the epithelium cells dislodged by the shrinking of the membrane of the genital tube, which is stretched some 100-fold by the ripening eggs.

The origin of this secondary hermaphroditism is not far to seek; it is clearly a protection against isolation, as in the case of the Cirripedia and certain parasitic Isopoda. The manner of life of all these animals is such that they are always in danger of being cut off from their kind; they would thus die out unless able to reproduce either parthenogenetically or by means of self-fertilization.

Some species of Cirripedia, as is well known, have dwarf males, the last remains of the original separation of the sexes. As already mentioned, small males of *Apus cancriformis* are sometimes found. Twelve finds of *A. cancriformis* and *L. productus*, recorded by Gerstaecker, give 4458 "females" (*i.e.* hermaphrodites) to 378 males; while sixteen finds, numbering 10,000 individuals, did not contain a single male. I have found no record of a male *L. glacialis*, and none of the twenty odd specimens of the Spitzbergen variety I have as yet examined have been males. It is probable that throughout the whole genus self-fertilization is taking the place of cross-fertilization, but that some species have gone further than others in dispensing with males. Two species, for instance, *L. couesii*, Packard, and *L. macrurus*, Lilljeborg, are reported to have more males than "females (?)," but the finds in these cases seem hardly large enough to allow us to judge; it may have been purely accidental that more males than "females" were caught.

The males of the Apodidæ, with the doubtful exception of *L. productus*, seem to be smaller than the hermaphrodites, otherwise there is no very pronounced sexual dimorphism as there is among the Cirripedia. We are perhaps justified in concluding from this that the hermaphroditism of the Cirripedia is of much older date than that of the Apodidæ. No comparison is here, however, possible, since the two have nothing further in common beyond the fact that they are both hermaphrodite, and that this hermaphroditism is in both cases an adaptation against extermination through too wide dispersion of the individuals.

Jena, January 30. H. Bernard.

Stereoscopic Astronomy.

The note on this subject in Nature of January 22 (p. 269), regarding the perception of stereoscopic effect on examining properly-arranged photographs of Jupiter, recalls an observation which I published in one of a series of articles on physiological optics that appeared in the *American Journal of Science* in 1881 and 1882.

By taking advantage of the moon's librations, Mr. Lewis M. Rutherfurd, of New York, produced more than twenty years ago an excellent stereograph of this heavenly body. In examining this I found it possible to observe not merely the general convexity or concavity, according to the mode of stereoscopic combination, but also the inequalities upon the lunar surface. In an American text-book I have found the statement that Mr. Warren De la Rue had succeeded in obtaining a stereograph of the sun, from which, by stereoscopic vision, the ridges of the faculæ could be perceived in sharp relief. On application to Mr. De la Rue for a copy of this stereograph, I was disappointed to learn that the negative had, unfortunately, been destroyed, and hence no copies were attainable.

My own observations may be given by quoting from the article published in the *American Journal of Science* for May 1882. "On the stereograph of the moon, to which reference has been made, the elevation of mountain ranges and solitary peaks, and even the inequalities of the supposed dead sea bottoms can be clearly seen. The crater Copernicus, and the lunar Apennines, stand forth particularly boldly, and the ridge that divides the bed of the heart-shaped Sea of Serenity can be easily traced. Anyone who has undertaken the preparation of a stereograph with the pencil or pen, knows how very difficult it is to avoid the production of roughness in the combined image at places where smoothness is desired. No two impressions from the same type can be taken that will not present some inequalities when stereoscopically examined, and no two groups of type representing the same sentence can be so accurately adjusted as not to betray imperfection when subjected to this searching test."

For this statement regarding the moon, I was subsequently criticized by an English writer, well known in astronomical circles, who considered it to be extravagant. The test furnished by the photographs of Jupiter is probably even more delicate than that afforded by photographs of the moon's minor inequalities of surface. The observation of "W. J. H." is certainly very interesting. By experiments made in 1882 I found that a plane binocular image became noticeably convex or concave when the pair of diagrams under examination were so disposed as to produce an angular retinal displacement of only 47" (*Philosophical Magazine*, October 1882). By comparing the photographs of celestial objects whose distance is known, it may be possible yet to show that the minimum displacement measured in 1882 is really not quite a minimum. W. LE CONTE STEVENS.

22 Universitätsstrasse, Strassburg, Germany, February 4.

Notable Palæolithic Implement.

DURING the last five or six years I have lived at Dunstable, and many persons in the neighbourhood now know that I notice old things a little. The consequence is that various objects are now and again presented to me for purchase. These things are mostly no good—common fossils, pieces of "petrified water," shells, coins of the Georges, &c., but at times something worth notice comes to hand.

Late last autumn a number of stones of no value were brought to me; amongst them was a good, flattish, sub-triangular, Palæolithic flint implement which had been picked up in 1830 by a farmer named William Gutteridge on Dallow Farm, near Luton—the late Mr. Gutteridge's own land. The implement had been preserved by the farmer as a curious natural stone, and he had affixed a label to it with locality and date. The person of whom I secured the stone knew nothing of stone implements. I soon ascertained the name and date to be correct from a relative of the late William Gutteridge. In 1830 the Gutteridges had held Dallow Farm for over 150 years.

Dallow Farm is in the valley of the Lea, and three-quarters of a mile west of the river at Luton. The ground is, I think, about 50 feet above the Lea, and from 400 to 450 feet above the Ordnance datum, but the heights on the large-scale Ordnance map are here insufficient. I have never found a Palæolithic implement at Luton, but I have picked up a few drift flakes there, and found a good number of Palæolithic implements a few miles off.

The Dallow Farm Palæolithic tool was found by Mr. Gutteridge seventeen years before M. Boucher de Perthes published his discoveries in France (1847), and eleven or twelve years before he began to notice such objects.

The famous Gray's Inn implement was found in 1690; Mr. Frere's discoveries were made at Hoxne in 1800; the Dallow Farm implement comes next in 1830; and the Godalming implement (Evans, "Stone Implements," p. 529) about 1842.

Dunstable. WORTHINGTON G. SMITH.

Stereom.

AMONG wants long felt, at least by animal morphologists, is some word that shall express for Invertebrata the idea that the word *bone* expresses for Vertebrata. Words such as *skeleton*, *shell*, *test*, and *carapace* express the whole structure, not the substance of which it is made. Words such as *nacre* and *stereoplasm* express some particular form of hard substance strictly defined from a physical or morphological stand-point. *Sclerenchyma* is the only word that has yet been used in anything like the required sense; but that is confined to corals, and, from its affinity with *cœnenchyma* and the like, it is well that it should be so. Driven back on cumbrous periphrases, I therefore venture to suggest the adoption of the word *Stereom* (στερέωμα, that which has been made solid). This word was used by Aristotle ("De Anim. Part.," ii. 9) for the hard as opposed to the soft tissues of the body, and may, for the purposes of modern science, be thus defined: any hard calcareous tissue forming skeletal structures in Metazoa Invertebrata, and in Protozoa. F. A. BATHER.

February 9.

Destruction of Fish by Frost.

IN regard to Prof. Bonney's letter of January 26 (p. 295), I would ask whether the fish were not killed by want of air due to the stagnancy of the water in the canal?

The moat here abounds in fish, and several holes were kept open for their sakes during the frost. The first partial thaw set our land-drains running. Where one of these began to pour a little water into the moat, though no fish had been visible since summer, now the largest pike and carp were seen crowding to the aperture, seeming to be gasping for air, and seeking the fresh flow. When the frost departed, scarce half-a-dozen fish—all small—were found dead. It would seem, therefore, that a very slight flow of fresh water would suffice to save fish from death. But this can seldom be wanting in any natural body of water, for few are even the tarns into which no brook runs. So such a cause of destruction can seldom have acted on a scale visible to a geological eye. E. HILL.

The Rectory, Cockfield, Suffolk.

A DEDUCTION FROM THE GASEOUS THEORY OF SOLUTION.[1]

BEFORE passing on, let me briefly recapitulate the chief points in Van't Hoff's gaseous theory of solution and the experimental laws on which it it is based.

(1) In every simple solution the dissolved substance may be regarded as distributed throughout the whole bulk of the solution. Its total volume is therefore that of the solution, the solvent playing the part of so much space; and its specific volume is the volume of that quantity of the solution which contains 1 gramme of the substance. To avoid confusion, it is best to speak of this as the *specific solution volume* (v) of the substance. It is obviously in inverse ratio to the concentration.

(2) In every simple solution the dissolved substance exerts a definite *osmotic pressure* (p). This is normally independent of the nature of the solvent. It varies inversely as the specific solution volume (or directly as the concentration), and directly as the absolute temperature (T). We may then write for solutions, as we do for gases, the equation $p \cdot v = r \cdot T$, where p and v have their specialized meanings, and r is a constant for each soluble substance.

(3) The *molecular solution volume* of all dissolved substances is the same if they are compared at the same temperature and osmotic pressure. If m be the molecular weight, $m \cdot v = V$ is the molecular solution volume; and we can now write, as we do for gases, $p \cdot V = R \cdot T$, where R is the same constant for all substances.

(4) This constant R has the same value when the formula is applied to the dissolved state as when it is applied to the gaseous state itself.

(5) The gaseous laws, as I have stated them, are not absolutely true for dissolved matter in all circumstances. Dissociation often occurs, as it may occur in the process of vaporization, thus causing apparent exceptions. But apart from this there are and must be variations from the laws in the case of solutions of great concentration, just as there are in the case of gases and vapours of great concentration—for instance, in the neighbourhood of the critical point.

I wish now to ask your attention more particularly to

[1] Part of an address delivered by Prof. Orme Masson as President of Section B of the Australasian Association for the Advancement of Science, January 1891.

the actual process of dissolving, and then to lay before you a hypothesis which, as it seems to me, is a logical consequence of the general theory.

Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization. As the concentration increases, the osmotic pressure of the dissolved portion increases, and tends to become equal to that of the undissolved portion; just as, during vaporization in a closed space, the pressure of the accumulating vapour tends to become equal to the vapour pressure of the liquid. But if there be enough water present, the whole of the solid will go into solution, just as the whole of a volatile body will volatilize if the available space be sufficient. Such a solution may be exactly saturated or unsaturated. With excess of the solvent it will be unsaturated, and the dissolved matter will then be in a state comparable to that of an unsaturated vapour, for its osmotic pressure will be less than the possible maximum corresponding to the temperature. On the other hand, if there be not excess of water present during the process of solution, a condition of equilibrium will be arrived at when the osmotic pressure of the dissolved portion becomes equal to the pressure of the undissolved portion, just as equilibrium will be established between the volatile substance and its vapour if the space be insufficient for complete volatilization. In such a case we get a saturated solution in presence of undissolved solid, just as we may have a saturated vapour in presence of its own liquid or solid.

So far we have supposed the temperature to be stationary, but it may be raised. Now a rise of temperature will disturb equilibrium in either case alike, for osmotic pressure and vapour pressure are both increased by this means, and a re-establishment of equilibrium necessitates increased solution or vaporization as the case may be.

Now what will this constantly increasing solubility with rise of temperature eventually lead to? Will it lead to a maximum of solubility at some definite temperature beyond which increase becomes impossible? Or will it go on in the way it has begun, so that there will always be a definite, though it may be a very great, solubility for every definite temperature? Or will it lead to infinite solubility before infinite temperature is obtained? One or other of these things must happen, provided, of course, that chemical change does not intervene.

Well, let us be guided by the analogy that has hitherto held good. Let us see what this leads us to, and afterwards examine the available experimental evidence. We know that a volatile liquid will at last reach a temperature at which it becomes infinitely volatile—a temperature above which the liquid cannot possibly exist in the presence of its own vapour, no matter how great the pressure may be. At this temperature, equilibrium of pressure between the liquid and its vapour becomes impossible, and above this point the substance can exist only as a gas. This is the critical temperature. And so it seems to me that, if we carry our analogy to its logical conclusion, we may expect for every substance and its solvent a definite temperature above which equilibrium of osmotic pressure between undissolved and dissolved substance is impossible—a temperature above which the substance cannot exist in presence of its own solution, or, in other words, a temperature of infinite solubility. This may be spoken of as the *critical solution temperature*.

But a little consideration shows that in one particular we have been somewhat inexact in the pursuance of our analogy. For we have compared the solution of a *solid* body to the vaporization of a volatile *liquid*. We can, however, do better than this, for volatile solid bodies are not wanting. It is to these, then, that we must look in the first instance. Now a volatile solid (such as camphor or iodine) will not reach its critical point without having first melted at some lower temperature; and a similar change should be exhibited in the solution process. At some definite temperature, below that of infinite solubility, we may expect the solid to melt. This *solution melting-point* will not be identical with, but lower than, the true melting-point of the solid; and for the following reason. No case is known, and probably no case exists, of two liquids one of which dissolves in the other and yet cannot dissolve any of it in return. Therefore there will be formed by melting, not the pure liquid substance, but a solution of the solvent in the liquid substance. Hence the actual melting or freezing point must be lower than the true one, in right of the laws of which I have spoken when discussing Raoult's methods in the earlier part of this address.

From this solution melting-point upwards we shall then have to deal with two liquid layers, each containing both substance A and solvent B, but the one being mostly substance A and the other mostly solvent B. These may be spoken of as the *A layer* and the *B layer*. As temperature rises, the proportion of A will decrease in the A layer and increase in the B layer; and every gramme of A will occupy an increasing solution volume in the A layer (B being absorbed there) and a decreasing solution volume in the B layer. At each temperature the osmotic pressures of A in the two layers must be equal. The whole course of affairs, as thus conceived, now admits of the closest comparison with the changes which accompany gradual rise of temperature in the case of a volatile liquid and its saturated vapour. The liquid is like the substance A in the A layer; the vapour (which is the same matter in another state) is like the same substance A in the B layer. As temperature rises, the liquid diminishes in total qnantity, the vapour increasing; but the specific volume of the liquid increases, while that of the vapour decreases. The residual liquid is, in fact, constantly encroaching on the space of its vapour, just as the residual substance A in the A layer is constantly absorbing the solvent B from the B layer. Finally, in either case, the specific volume of the substance will become identical in both layers, which means that the layers themselves will become homogeneous and indistinguishable. Our system will then have reached its critical temperature—the temperature of infinite volatility in the one case and of infinite solubility in the other.

So much for hypothesis. Are there any facts in support of it? Well, in the first place the hypothesis demands that (in the absence of chemical change) increase of solubility with rise of temperature shall be as general a law as increase of vapour pressure; and we find that this agrees with the known facts, more especially since Tilden and Shenstone (Phil. Trans., 1884) cleared up certain doubtful cases. Secondly, the hypothesis seems to demand some connection between the true melting-points of salts and the rates of their increase of solubility; and such a relation has in a general way been established by the same observers. Thirdly, we have the fact, in complete accordance with the hypothesis, that while no case is known of a solid body having, as such, infinite solubility in any simple solvent, several cases are known of liquids of infinite solubility, and also of *solids which, after they have melted in presence of their own solution, become at some higher temperature infinitely soluble*. This last statement refers to the cases described by Alexéeff (*Wiedemann's Annalen*, 1886), of which I must say a good deal more directly. It would seem to apply also to the case of silver nitrate, which Tilden and Shenstone described as dissolving in water to the extent of 18·25 parts to one at so low a temperature as 130° C. The true melting-point of the salt is 217°; and I have seen it stated (but have been unable to find the published account) that Shenstone has himself shown it to be fusible in water, and of infinite solubility at quite reachable temperatures.

With regard to substances that are liquid under ordinary conditions, we have the well-known fact that some pairs are infinitely soluble in one another, while others exhibit the phenomenon of only partial solubility. The hypothesis would draw no hard and fast distinction between these cases, except the practically important one that such a mixture as that of ether and alcohol, which belongs to the first class, is usually above its critical solution point, while such a one as ether and water, which belongs to the second class, is usually below it. It should be possible, according to the hypothesis, to cool mixtures of ether and alcohol sufficiently to cause separation into two layers, similar to those observed at the ordinary temperature in the case of ether and water; but I do not know that this has yet been put to the test of experiment.

Alexéeff's experiments appear to me to be of the very highest importance, and to merit the closest attention in any inquiry into the nature of solution. As already stated, they afford the strongest support to the hypothesis which I have been discussing; indeed, had it not been for this support I should hardly have ventured to discuss it at all. They refer to solutions in water, below and above 100°, of phenol, salicylic acid, benzoic acid, aniline phenylate, and aniline, and to solutions in molten sulphur of chlorobenzene, benzene, toluene, aniline, and mustard oil. All these afford instances of reciprocal partial solution throughout a considerable range of temperature, leading eventually at a definite temperature to infinite solubility. Several of them afford instances also of solid substances with solution melting-points below their true melting-points.

Alexéeff experimentally determined the temperatures at which different mixtures of the same two liquids are just converted into clear solutions; or, in other words, he ascertained the strengths of the saturated solutions corresponding to different temperatures. For each pair of liquids he found that when a particular strength of mixture is reached, the temperature of saturation is lowered by further addition of either liquid. Thus a mixture of about 37 parts aniline to 63 parts water requires a temperature of 164°·5 to convert it into a homogeneous solution; but one of 21 of aniline to 79 of water assumes this condition at 156°, and one of 74 of aniline to 56 of water does so at 157°·5. He plotted his results in the form of curves, with temperature and percentage strength as the two co-ordinates. The curve for aniline and water is shown in Fig. 1; and this may be taken as a fair representative, the general form of all being similar. It is at once apparent that for every temperature up to a certain limit there are two possible saturated solutions, one of water in aniline and one of aniline in water. The limiting temperature at which there is but one possible saturated solution, and above which saturation becomes impossible, is called by Alexéeff the *Mischungs Temperatur*. It is what I have called the critical solution temperature. It is in the case of aniline and water about 167°, as nearly as one can judge from the curve without a greater number of experimental points than we have in this part; and the corresponding saturation strength is about 50 per cent. It is hardly necessary to say that this equality of the two ingredients is an accident which does not characterize all cases.

Now imagine a 50 per cent. mixture of aniline and water sealed up in a tube, shaken, and gradually heated. Let us assume that the tube is only large enough to contain the mixture and allow of expansion by heat, so that evaporation may be neglected as too small to materially complicate the result. The course of events will be exactly what I have already described with reference to the hypothetical A layer and B layer. There will be formed a saturated solution of water in aniline, which we may call the *aniline layer*, and a saturated solution of aniline in water—the *water layer*. Given the temperature, the percentage strength of each layer may be read off from the curve. As the temperature rises, the two layers will effect exchanges in such a way that the aniline layer will become poorer, and the water layer richer in aniline; and at about 167° the two layers will have attained equal strength and become merged into one. Were we to start with the aniline and water in any other proportions by weight, there would still be formed the two saturated solutions, but their relative amounts would be different, and one or other would be used up and disappear at a lower temperature than 167°. To attain the maximum temperature of complete solution you must start with the exact proportions which correspond to that temperature.

But it is possible to learn even more from Alexéeff's work than he himself has made evident. Let me call your attention to the curve shown in Fig. 2,[1] the data for which I have calculated in the following manner.

From Alexéeff's percentage figures was deduced the weight of water capable of dissolving, or being dissolved by, 1 gramme of aniline at each of his experimental temperatures, so as to form a saturated solution. Then from curves showing the expansion of pure water and pure aniline (the latter drawn from Thorpe's data, Trans. Chem. Soc., 1880) there were read the specific volumes of these substances at each of Alexéeff's temperatures; and from the combined information thus obtained, there was calculated the total volume of that quantity of the saturated solution at each temperature which contains 1 gram of aniline. This is what I have already called the specific solution volume. A slight error is involved by the fact that the volume of a solution is not exactly the sum of the volumes of its ingredients; but this error is necessarily small—too small to affect the general character of the curve or the nature of the lesson to be learned from it.

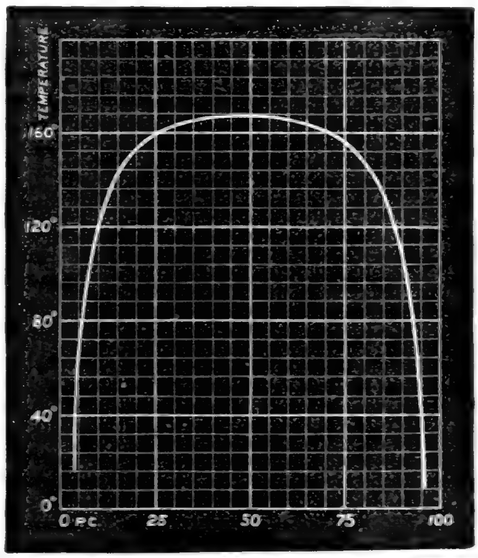


FIG. 1.—Percentage of aniline in its saturated aqueous solution (Alexéeff).

[1] In order to save space, only the upper portion of the curve is here represented, as it shows all that is essential to the argument. Of the twelve experimental points, one appears to be somewhat misplaced; but this does not affect that part of the curve shown in the figure.

The specific solution volumes of the aniline, calculated in this manner, were found to be as follows:—

Temperature.	Specific solution volumes of aniline	
	In aniline layer.	In water layer.
8°	1·015	—
16	—	32·16
25	1·036	—
39	1·053	—
55	—	28·27
68	1·087	—
77	—	19·55
137	1·297	—
142	—	7·696
156	—	5·248
157·5	1·498	—
164·5	—	3·412

These specific solution volumes are represented as abscissæ in Fig. 2, with the temperatures as ordinates. For the sake of comparison, I have placed side by side with it a specific volume and temperature curve (Fig. 3) for pure alcohol and its saturated vapour, plotted from the experimental data of Ramsay and Young (Phil. Trans., 1886). The reason that alcohol was chosen is simply that the data were convenient to my hand.

The two curves are strikingly similar in form and significance. In Fig. 3 we see the specific volume of liquid alcohol increasing slowly with rise of temperature, while that of the saturated vapour rather rapidly decreases. In Fig. 2 we see the specific solution volume of the aniline in the aniline layer slowly increasing, while that of the aniline in the water layer decreases more rapidly, with rise of temperature. In Fig. 3 we see that above the critical point the existence of liquid alcohol in presence of its vapour is impossible. In Fig. 2 we see that above the critical solution point the existence of an aniline layer in presence of a water layer is impossible. In Fig. 3 we see an inclosed area which represents

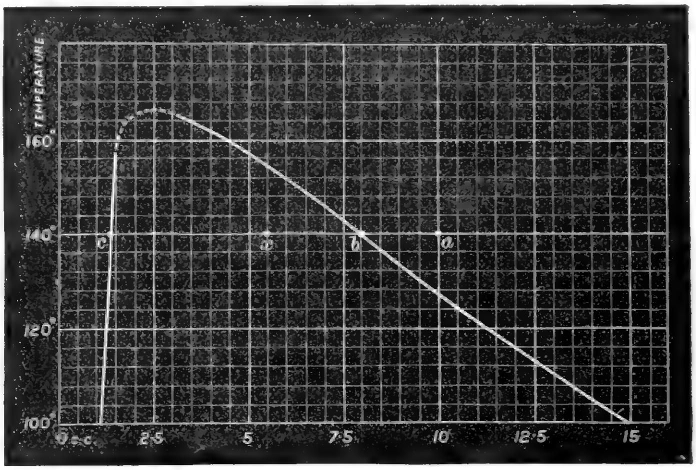


FIG. 2.—Volume of saturated aqueous solution containing one gram of aniline

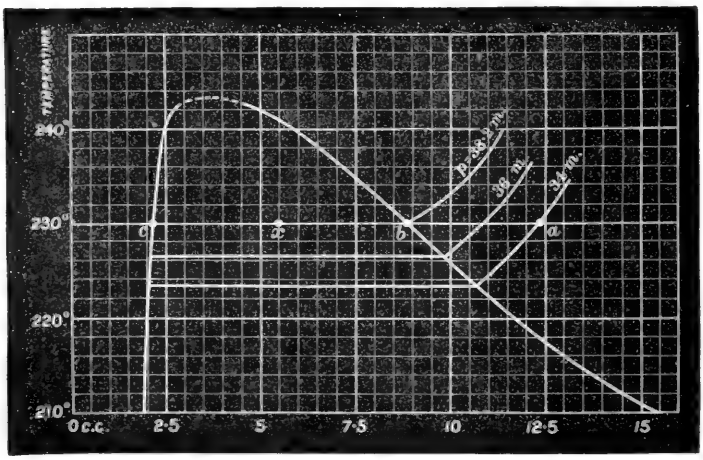


FIG. 3.—Volume of alcohol (liquid and saturated vapour) weighing one gram.

those temperatures and specific volumes which are mutually incompatible. In Fig. 2 we see an inclosed area which represents those temperatures and specific solution volumes which are mutually incompatible. In Fig. 3 we see that any two points on the curve which correspond to equal temperature must also, from the nature of the case, correspond to equal osmotic pressure. In Fig. 3 some of the pressures are indicated, as this can be done from Ramsay and Young's data. In Fig. 2 the value of the osmotic pressures cannot be given, as they have not been experimentally determined. In Fig. 3 any point outside of the curve and to the right, as at *a*, corresponds to the state of unsaturated alcohol vapour, whose temperature, specific volume, and pressure are indicated—the last by the isobaric line which passes through the point. In Fig. 2 any point outside the curve and to the right, as at a, must correspond to the state of an unsaturated aqueous solution of aniline, whose temperature and specific solution volume can be read, and whose osmotic pressure could be indicated by an isobaric line, had we the data for plotting it. A little thought makes it evident, too, that such isobaric lines would follow the same general course as those shown in the alcohol diagram.

Now consider what must be the effect of gradually decreasing the volume of the unsaturated vapour in the one case and the solution volume of the aniline in the unsaturated solution in the other, while temperature is kept constant. In the case of the vapour (Fig. 3), the point a will pass to the left across lines of increasing pressure, until the vapour becomes saturated at b. Then, if the diminution of volume continue, a portion of the vapour will condense to the liquid state, or be transferred to c, while the rest remains saturated vapour at b. With continued decrease of volume, the proportion condensed will constantly increase, but there can be no alteration of pressure till all is condensed; and after that nothing but a very slight diminution of volume is possible without a lowering of temperature. Well, how are we to diminish the solution volume of the aniline in the unsaturated aqueous solution? Clearly by depriving the solution of some of its water, so as to leave the same quantity of aniline distributed throughout a smaller space. And what will be the result of doing this while temperature is kept constant? Evidently, as in the other case, the point a (Fig. 2) will travel to the left, across lines of increasing osmotic pressure, until it reaches b—that is, until the solution is a saturated one; and after that, if more water be abstracted, some of the aniline will be thrown out or condensed, not as pure aniline but as a saturated solution of water in aniline, so that two layers will now co-exist—the aniline in one having the specific solution volume represented at b, and the aniline in the other having that represented at c. This tranference from b to c will continue, as water is abstracted, until the ratio of residual water to aniline is just enough to give the whole of the latter the specific solution volume shown at c. At this stage the water layer will disappear, and only a saturated solution of water in aniline will be left; and after that only a very small volume change can possibly result from further abstraction of water, as the specific solution volume is already not far from the specific volume of pure aniline itself at the same temperature.

To complete the comparison of the two curves, let me point out that, just as we can from Fig. 3 calculate the distribution of alcohol between its liquid and its vapour layers under given conditions, so can we calculate from Fig. 2 the distribution of the aniline between the aniline layer and the water layer under given conditions. In the former case, if the total volume of a tube containing n grammes of alcohol, at, say, 230°, be $n \times x$, and if x be marked off (Fig. 3) between b and c on the line of that temperature, then (x, b, and c standing for the volumes which can be read off on the horizontal base line) $n \cdot \frac{x-c}{b-c}$ is the weight of the alcohol in the vapour layer, and $n \cdot \frac{b-x}{b-c}$ is its weight in the liquid layer, and the volumes of the two layers in cubic centimetres are $n \cdot b \cdot \frac{x-c}{b-c}$ and $n \cdot c \cdot \frac{b-x}{b-c}$ respectively, which are together equal to $n \cdot x$. Just so also with the aniline and water mixture (Fig. 2). If $n \times x$ be the total volume of the mixture (both layers together) containing n grammes of aniline, at, say, 140°, and if x be marked off as it was in the other case, then $n \cdot \frac{x-c}{b-c}$ is the weight of aniline in the water layer, and $n \cdot \frac{b-x}{b-c}$ is its weight in the aniline layer, and the total volumes of the two layers are $n \cdot b \cdot \frac{x-c}{b-c}$ and $n \cdot c \cdot \frac{b-x}{b-c}$ respectively, together equal to $n \cdot x$.

If the actual weights of aniline and water in the mixture be given, the value of x can be calculated with a very fair approach to accuracy by the method adopted in plotting the curve; and thus all the facts with regard to the distribution at any temperature can be obtained.

Now if it be remembered that this case of aniline and water is not an isolated one, but typical of many cases experimented on by Alexéeff, and if it be remembered also that there exists no direct experimental evidence to show that the law which governs these cases is not the general law regulating all simple solutions, it must I think be granted that the facts do somewhat strongly support the hypothesis of a critical solution point which I deduced in the first instance from the general theory of solution. It may be summed up as follows:—

(1) In every system of solution which starts with a solid and its simple solvent, the solid has a solution melting-point which is lower than its true melting-point. Above this temperature the system consists of two separate liquids, each of which is a saturated solution.

(2) These two liquids become one homogeneous solution at a temperature which depends on the ratio of the original ingredients. There is one ratio which demands a higher temperature than any other. This is the critical solution temperature, above which either ingredient is infinitely soluble in the other.

THEORY OF FUNCTIONS.[1]

II.

WE now come to Dr. Schwarz's contributions to the theory of functions, which relate to the two theorems stated, and we begin with those which relate to the conform representation of various surfaces on a circle.

In the paper "Ueber einige Abbildungsaufgaben," we learn that these investigations date back to the time when the author was a student at Berlin. A fellow-student, Herr Mertens, observed to him how curious it was that Riemann had proved the existence of a function which would give a conform representation of the surface of a figure like a triangle on that of a circle, whilst the actual determination of the function in form of an expression seemed, on account of the discontinuities due to the corners, to be beyond the present powers of analysis. Dr. Schwarz thereupon tried to work out a special case, and selected the representation of a square on a circle, or, rather, first on the half of the plane which is bounded by a straight line; for the circle can be conformly represented on this half-plane by aid of reciprocal radii.

He next looks for a function which has such discontinuities as are introduced by the corners of the square. The right angle has to be represented by an angle of twice the magnitude, and therefore here the representation cannot be similar in the smallest parts. The known properties of the exponential function help at once to find it. This function is thus obtained by a happy guess, not by prescribed rules. It still contains an infinite number of constants. To determine these it is observed that if $u = f(t)$ gives a conform representation of a surface T in the plane t on a surface U in the plane u, then also will $u' = C_1u + C_2$ gives such representation, only the corresponding figure U′ is drawn to a different scale, and placed in a different position in the u plane. On differentiating this equation with regard to

[1] "Gesammelte Abhandlungen." Von H. A. Schwarz. Two Volumes. (Berlin: Julius Springer, 1890.) Continued from p. 323.

t, and eliminating the constants, it is found that the expression

$$\frac{d}{dt} \log \frac{du}{dt}$$

remains unaltered if we substitute u' for u, and is therefore independent of the absolute magnitude and position of the figure U in the plane. The value of this expression in the case under consideration is then shown to be a rational function of t, and on integration the value of u is got as an elliptic integral. On returning from the half-plane to the circle by the substitution $s = t - i/t + i$, we get ultimately

$$u = \int_0^s \frac{ds}{\sqrt{1-s^4}}, \quad s = \sin am\, u, \quad (k = i).$$

This lemniscatic function gives the required transformation, as is ultimately easily verified.

He also gives in form of a definite integral the function for the conform representation of a triangle on a circle, and the expression found at once suggests the function for the representation of any polygon.

These results he laid, in 1864, before the Mathematical Seminary of the Berlin University. They were, however, not published till 1869, and then with considerable additions. Meanwhile the objections raised against Riemann's theory, already mentioned, had been raised. These invalidated the results about the polygon; for the formula given contains a number of constants. The complete proof for the validity of the formula requires Riemann's theorem about the existence of a solution, or else a proof that the constants can in each case be determined. As the first had become doubtful, the author now states that he had been able to give the last proof in case of any quadrilateral, and adds that he has received a general proof from Weierstrass. But this proof is not given.

A few other examples are added, viz. the representation, always on a circle, of the part of a plane outside a square, of the space bounded by a parabola and by an ellipse. This last problem is more fully considered in a separate paper.

He next considers a polygon bounded by circular arcs. Such a polygon is transformed into another also bounded by circular arcs by aid of the substitution

$$u' = (c_1 u + c_2)/(c_3 u + c_4).$$

The differential equation obtained on eliminating the four constants, is of the remarkable form $\psi(u', t) = \psi(u, t)$, where $\psi(u, t)$ is the expression which Cayley has more suitably denoted by (u, t), and called the "Schwarzian derivative," and which, through one of Cayley's formulæ, has led to Sylvester's theory of reciprocants.

Some of the results obtained are extended to representation on a sphere by aid of stereometric projection, and finally the function is given which performs the conform representation of the surface of a polyhedron on that of a sphere.

The function obtained contains, again, a number of constants. The author states that these can at once be determined in case of a regular polyhedron, but that he has not been able to prove that this can always be done.

The case of a tetrahedron is considered and fully worked out in a special paper. The result is that the surface of a tetrahedron can always be conformly represented on the surface of a sphere, so that there is a one-one correspondence between the points, and this can be done in one way only if for any three points on the one surface the corresponding ones on the other have been arbitrarily selected.

The representation of a square on a circle is illustrated by a figure. The surface of the square is divided into smaller squares by lines parallel to the sides, and the curves representing these are drawn on the circle.

As this representation is performed by elliptic functions it gives interesting illustrations of these functions. In the paper attention is called to an illustration of a theorem of Abel, and in a note, added to the present reprint, a quotation from a paper on complex prime numbers, by Jacobi, is made, of which we repeat a part here. Jacobi says:—

"Eine ebenso interessante als schwierige Aufgabe dürfte es sein, dieser Theilung des Lemniscatenbogens in $a + b\sqrt{-1}$ Theile . . . einen geometrischen Sinn abzugewinnen."

This is done by the above representation.

The chief result of the papers considered is that the possibility of a conform representation of one surface on a circle has been proved for certain cases by the actual determination of the function which gives it. Especially it has been shown that a figure bounded either by straight lines or by arcs of circles can be thus represented. These results form the starting-point for the papers in which it is attempted to give strict proofs for Riemann's general theorems stated above.

In the paper "Zur Theorie der Abbildung," it is supposed that the plane surface U to be represented on the area of a circle consists of a simply connected single sheet whose boundary is everywhere convex. The plane is covered by a net of squares, and then that area is taken which contains all those squares that lie wholly or partly within the given area. For this new area U_1, a function u_1 exists, by aid of which the area of a circle is represented on that of the new figure, for it is bounded by straight lines. On halving the sides of the squares, a new area is formed, coming nearer the given one. After m repetitions of the process, we get a function u_m, representing the area of the circle on the last-formed area U_m. If m is now increased indefinitely, then U_m will become coincident with the given area U, and it is proved that the limiting value of u_m becomes an analytical function representing the area of the circle on the given area U. This proof, however, requires that the boundary of U is everywhere convex towards the outside.

It is worthy of notice that the boundary of the surface U is replaced by a broken line whose sides always remain at right angles, and which, therefore, never changes into a curve, which has, generally speaking, at every point a definite tangent. The boundary of U_m is and remains for an increasing m a broken line. This seems closely connected with Prof. Klein's speculations about the impossibility of representing $y = f(x)$ geometrically by a curve. All that a curve can represent is what he calls a *Functionenstreifen*. If we admit this, and consider the boundary of U as given by a *Streifen* (a strip of small but not vanishing breadth), we may divide the boundary into a finite number of parts, such that the strip representing each arc contains an arc of a circle or a straight line. The figure thus obtained can be conformly represented on a circle. Thus we get the theorem that every surface bounded by a *Streifen* can be conformly represented on a circle. This is all that is wanted for physical applications. In these, therefore, there need be no hesitation of applying Riemann's theorems. In fact, physicists never have hesitated, and have used Green's and Thomson's theorems, on which Riemann's depend, as if the boundaries of the solids and surfaces considered were continuous and without thickness, though neither can be true if matter itself is discontinuous in its smallest parts, if it is made up of atoms of small, but not infinitely small, size.

Having considered the papers which deal with the conform representation of one surface on another, we come to those in which the determination of an analytical function by aid of the partial differential equation $\Delta u = 0$ is treated.

We have seen that the determination of $w = u + iv$ as an analytical function of $z = x + iy$ can be made to depend on that of its real part u as a function of x and y, which satisfies $\Delta u = 0$.

According to Riemann, such a function, u, is completely determined if it has at the boundary of a given surface any prescribed values, whilst it is finite and continuous for all points within the surface, or has given discontinuities. It is required to prove that this assertion of Riemann's is true.

In the paper "Ueber einen Grenzübergang durch alternirendes Verfahren," Schwarz gives an outline of his method, but in so short a form that it is difficult to understand the reasoning, there being constant references to theorems which can only be known to those who are well acquainted with the literature of the subject. In the next paper, however, "Ueber die Integration der partiellen Differentialgleichung $\Delta u = 0$ unter vorgeschriebenen Grenz- und Unstetigkeitsbedingungen," we have the same problem treated in an exhaustive manner. All the propositions used are first explained, and only once reference is made to a theorem which Weierstrass has given in his lectures, but not published. This paper is very clear, and contains a complete introduction to the theory of analytical function, or, as Axel Harnack says, it contains "in gedrängter Kürze eine ganze Theorie der Potential functionen" (in a plane).

It is first pointed out that our problem, of determining u subject to given boundary conditions, can always be solved for a circle, and the function which solves it is given in form of a definite integral. The proof for this statement is given in a subsequent paper, "Zur Integration der partiellen Differentialgleichung $\Delta u = 0$," which contains, also, investigations about discontinuities, &c., and which should be read together with the one now under discussion.

It is next shown that the problem for any other surface is reduced to the above if the surface can be conformly represented on the circle. This greatly enhances the importance of such representation.

Three cases of surfaces for which this representation has been obtained are enumerated, viz. a *crescent* formed by two circular arcs, including a segment of a circle; a triangle bounded by arcs of circles (or by straight lines), provided two of the angles are right angles; and, lastly, such a triangle formed by circular arcs which has the third vertex a cusp.

Next an *analytical line* is defined, viz. if $z = f(t)$ is an analytical function, then the curve which the point z describes for real values of t is said to be analytical. Such a line is therefore, by aid of the function $z = f(t)$, conformly represented on a straight line, the axis of real t.

If now the boundary of a surface consists of a finite number of pieces of analytical lines, it will be possible to get, by aid of the equation $z = f(t)$, on the plane (t) a conform representation of one of these pieces, together with a part of the surface, and this may be bounded in such a manner that its representation on the plane (t) becomes, say, a segment of a circle. For this portion of the surface, therefore, our problem (of determining u) can be solved. From the whole surface we can thus cut off pieces next to the boundary for which the problem can be solved. It will therefore also be possible to place on the surface a number of figures such that for each of them the problem can be solved, and that no part of the given surface is uncovered, whilst none projects beyond the boundary. These figures will, of course, overlap.

Suppose, now, we could prove that our problem can be solved for a surface formed by such overlapping figures, if it can be solved for each of the component figures, the part where the figures overlap counting, of course, only once; then the proof would be complete that our problem of determining u as a solution of $\Delta u = 0$, so that it has prescribed values on the boundary of a given surface, has a solution if the surface is bounded by a finite number of pieces of analytical lines.

This is what Schwarz does by the "Grenzübergang durch alternirendes Verfahren." He supposes that T_1 and T_2 denote two surfaces, such that for each of them the problem has been solved, and places these so that they overlap, and their outer boundaries form a new surface, T.

The two figures have a region T^* in common; the figure T may therefore be expressed as $T = T_1 + T_2 - T^*$. Part of the boundary of T_1 is boundary of T; the other lies within T_2 and is boundary of T^*. The same is true of T_2. There is, first, a function u_1 determined for T_1, which has, on that part of the boundary belonging to T, the prescribed value, and on the other parts, within T_2, the value zero. Then a function u_2 is determined for T_2, satisfying the analogous boundary condition, only on the part of its boundary within T_1 it receives the values which the first function u_1 has there.

Then new functions are determined alternately for T_1 and T_2, each having on that part of the boundary which belongs to T the prescribed values, and on that part which belongs to T^* the values which the last function for the other area gives it. The effect is that, ultimately, two functions, u' for T_1 and u'' for T_2, are obtained, which coincide throughout the boundary of T^*, and which, therefore, must be identical for the whole common region T^*. But this implies, again, that they are values of the same function u, which satisfies, therefore, all conditions.

Riemann's principal theorem is thus proved, not for any surface, but for a case of very great generality. For the theory of analytical functions this seems sufficient, as it is difficult to conceive the necessity of having to use surfaces bounded by quite arbitrarily given boundaries.

In the theory of potential such boundaries might occur, but here Klein's investigations already referred to are pertinent. These, in fact, seem to brush away a great number of the difficulties which have been introduced by starting from too general definitions.

The theorems used by Riemann in his theory of Abelian functions, for instance, can all be proved by the above results of Schwarz, who, indeed, shows how the discontinuities which occur here can be treated by his method. He also extends some of his results to conform representation of surfaces on a sphere, and completes the investigation in his earlier papers.

Of the remaining papers we can only give a very short account. There are first two papers on developable surfaces of the first seven orders, and more particularly on those of order five.

The papers "Bestimmung der scheinbaren Grösse eines Ellipsoids für einen beliebigen Punkt des Raumes" and "Zur conformen Abbildung der Fläche eines Rechtecks auf die Fläche einer Halbkugel" contain applications of the use of Weierstrass's elliptic functions. Those who take an interest in the latter, which are in England known chiefly through various papers by Prof. Greenhill, will be glad to learn that Herr Schwarz promises in the preface of vol. ii. a new edition of his "Formeln und Lehrsätze zum Gebrauche der elliptischen Functionen."

In the important paper "Ueber diejenigen Faelle, in welchen die Gaussische hypergeometrische Reihe eine algebraische Function ihres vierten Elementes darstellt," we have applications of the theories just discussed, but we must forbear from entering into a discussion of it.

The investigation rests on a discussion of the well-known differential equation of the second order, of which the series is an integral. The results obtained are included in Forsyth's "Treatise on Differential Equation," though proved in a very different manner. They have enabled Klein to determine all differential equations of the second order which have algebraical integrals.

This paper, "Ueber algebraische Isothermen," contains this theorem :—

If a complex variable $w = f(z)$ has the property that the curves for which the real part u of w has a constant value are algebraical, then w is either an algebraical function of z, or μw, where μ or μi is some real number, is the logarithm of an algebraical function, or else μw is an elliptical integral of the first kind, with real modulus whose superior limit is an algebraical function of z.

All these investigations require a deep study of the very foundations of analysis. These cannot help to reveal a number of inaccuracies and gaps in the ordinary theories as contained in text-books. Accordingly we find several papers in the collection which contain such corrections. Thus there is one paper in which a complete system of independent conditions is given which underlie the proposition that $d^2u/dxdy = d^2u/dydx$.

In another paper the definition of the area of a curved surface in the first edition of Serret's "Calcul Différentiel et Intégral" is shown to be wrong. In another long paper it is proved that of all solids of given volume the sphere has the smallest surface. All previous proofs depend on the supposition that *one solid exists* which has a minimum surface; of this the present proof is independent.

The first volume contains papers relating to surfaces of minimum area. The original problem is one of the calculus of variation, viz. a given closed curve in space being given, it is required to determine that surface bounded by it which has the least area. One of the chief properties of such surfaces is that the principal radii of curvature at each point are equal but opposite, or the mean curvature is everywhere = 0. Hence all surfaces which have this property are called "surfaces of minimum area," though it is not any longer true that surfaces of this kind have the original property for every part cut out by any curve drawn on them, just as the arc of a great circle on a sphere ceases to be the shortest line between its ends as soon as the arc becomes greater than a semicircle. The question to decide whether this is the case for a given closed curve on the surface is considered in the paper "Ueber ein die Flächen kleinsten Flächeninhalts betreffendes Problem der Variationsrechnung." It is, of course, a problem about the second variation.

There is an interesting connection between the surfaces of minimum area and the conform representation of one surface upon another, viz. every such representation of the whole surface of a regular polyhedron on a sphere gives rise to a surface of minimum area, which in this case contains an infinite number of straight lines.

In the first paper the surface is considered which is thus obtained from the cube. This paper, when communicated to the Berlin Academy, was illustrated by models. In the present reprint, nicely executed shaded figures of these are added. The first gives the surface corresponding to one face of the cube. It is bounded by four edges of a regular tetrahedron. In the second we have the surface corresponding to all six faces of the cube. It forms one continuous sheet. If this surface be still further continued, a surface is obtained which extends throughout the whole space. Of this the third plate gives a part. Analytically the problem depends on elliptical functions.

These investigations are continued in the second paper (and several others), which obtained a prize of the Berlin Academy. The prize problem required the complete solution, by aid of elliptical or Abelian functions, of some important problem taken from almost any part of pure or applied mathematics. Herr Schwarz treats of the surface of least area bounded by any skew quadrilateral.

In "Fortgesetzte Untersuchungen über specielle Minimalflächen," a new problem is proposed, viz. there is given a closed chain consisting of straight lines and planes, it is required to find a surface of minimum area bounded by the lines and perpendicular to the planes.

Of the other papers we mention the "Miscellen aus dem Gebiete der Minimalflächen." It contains a highly interesting review of the whole subject, including Plateau's investigations, and is full of suggestions.

The volumes, which are dedicated to Weierstrass, are well printed on octavo pages sufficiently large to give room for the formulæ required, and not so large as to be unwieldy, as is the case with a recently published "Collection." But there is one point in which the edition might have been improved, trifling as far as editing and printing are concerned, but of great benefit to the reader. It is very desirable that in all editions of collected papers the examples set by Sir William Thomson and Prof. Cayley should be followed, of placing the date of the first publication of each paper both in the table of contents and at the head of each paper.

O. HENRICI.

GEOGRAPHICAL EXPEDITIONS.

M. GROMBCHEVSKY, now at St. Petersburg, has given the Russian Geographical Society a most interesting account of his last expedition. It is known that the Expedition left Marghelan in June 1889, and that having found the Alai Mountains deeply clothed in snow, they went to Kala-i-khumb through Karategin and Vakhia. They found that the khanate of Shugnan was at war with the Afghans, and as the latter refused to let the Expedition go further, M. Grombchevsky returned to Vakhia, after having crossed the Sytarghi Pass, which has on its western slope a great glacier, six miles long. In August, after having made a long circuitous journey over the Pamir (the well-known Pamir robber, Sahir-Nazar, being the guide of the Expedition), they reached the frontier of the Pamir khanates now occupied by the Afghans, and waited there for the Ameer's permission for further advance. A refusal was received in October, when the temperature already was from 20° to 24° C. below zero, and the Expedition could find no fuel of any kind. So they crossed the Mus-tagh ridge (yaks being used for the transport of provisions), and reached the valley of the Raskem River, where they met with Mr. Younghusband. During their fifty-five days' stay on the banks of the Raskem, they explored the passes of Shimshal, Mustagh, and Balti-davan, leading to Kashmir, as well as the passes across the Raskem ridge leading to Kashgaria. In November, M. Grombchevsky was at the Kashmir fort Shahidulla-kodja; but the fort was abandoned, and, the Expedition having no provisions, they asked permission to enter Kashmir and to winter there. But Colonel Nisbet refused admission to Kashmir, so that the Expedition had nothing to do, M. Grombchevsky says, but to move eastward, across the desert plateaus of Tibet, in order to reach some inhabited spot. Moving up the Kara-kash, the Expedition ascended the Tibet plateau. The thermometer fell as low as − 33° to − 35° C., all water was frozen, and two-thirds of the horses died; so that all natural history collections were abandoned, and, notwithstanding a frightful snowstorm, the Expedition re-crossed the mountains and went to Kashgaria. The first settlements were reached in February. Next month M. Grombchevsky went to Khotan, and thence to Niya, where he met with the commander of the Tibet Expedition, M. Pyevtsoff. At the end of March, he visited the Sourgak gold-mines in the south of Niya—where he found 3000 men busy in gold-washing—and Polu, whence he again ascended the Tibet plateau, and after some explorations he returned to Kashgaria again. In the autumn he visited the middle course of the Raskem River, making acquaintance with interesting tribes of mountaineers, and thence returned to Russia. The geographical results of the expedition seem to be very important. Surveys were made over a length of 5000 miles, and latitudes and longitudes were determined at 73 different spots; heights were measured throughout the journey, and photographic views taken; and rich geological, botanical, and entomological collections were secured.

On January 31, in the great amphitheatre of the Sorbonne, Paris, the French Geographical Society held a special meeting for the reception of M. Gabriel Bonvalot and Prince Henry of Orleans, whose travels in the heart of Central Asia have won for them an honourable place in the ranks of modern explorers. The chair was taken by M. de Quatrefages, who warmly congratulated the explorers on their achievements, and announced that the Society had conferred on the Expedition its large gold medal, the highest reward at its disposal.

M. Bonvalot, the chief of the Expedition, gave a full and interesting account of the journey. He left Paris with Prince Henry on July 6, 1889, and arrived on September 1 at the Russo-Chinese frontier, near which their caravan was organized. At Kuldja they met Father Dedékens, a Belgian missionary, who, to their great satisfaction, consented to accompany them, and rendered them important services. Having crossed the mountains of Tian-Shan, they arrived at Kurla, in Chinese Turkestan, where M. Bonvalot engaged fresh camels. At Lake Lob-Nor they reorganized their caravan, and laid in stores for six months. They then crossed the chains of Altyn Tagh, the Tshimen Tagh, and the Columbo Mountains, travelling sometimes at heights of more than 4000 metres. The region was wild and desolate, and the cold intense; and M. Bonvalot found it necessary to limit the Expedition to fourteen men, forty camels, and eighteen horses, the rest being sent back. Having followed for some time the traces of a caravan in the direction of Lhassa, he decided to keep to the same route as far as it could be made out; and in his address at the Sorbonne he gave a vivid description of the difficulties the party encountered in trying to discover the way the caravan had taken. On December 31, at a height of more than 5000 metres, a terrible storm caused them to lose sight of the marks by which they had been guided; whereupon they journeyed along the 90th degree of longitude. They found great chains of mountains, vast lakes, extinct volcanoes, geysers, and a Pass at an altitude of 6000 metres. Below 5000 metres they met with herds of wild yaks, antelopes, and other animals. Birds had wholly disappeared, and there was no vegetation. The only water they could obtain was melted ice, and cooking was impossible. Two men died, and the animals perished one after another. At last the traces of the route were discovered, and the Expedition arrived at Lake Ten'gri-Nor, where they met certain Tibetan authorities, who were accompanied by numerous horsemen. They had great difficulty in proving that they were Frenchmen, but after forty-five days of negotiation, at Dam, near Lhassa, the Tibetans provided them with the means of continuing their journey, as they had lost all their own means of transport.

The travellers followed what is called "the little route" from Tibet to China—a route still unexplored. They crossed the territory of independent tribes, who, in accordance with the wishes of the Llama, furnished them with yaks and horses. They were now in a region of valleys, and of wooded grounds well supplied with game and with large wild animals. In the course of three days they saw twenty-two bears. Some of the valleys are cultivated and occupied by villages. The Expedition followed the upper courses of the Salouen and the Mékong, and that of the Yang-tse-kiang, the sources of which they thought they recognized on the southern side of a colossal chain of mountains which they called "Monts Dupleix."

At Batang, which they reached on June 7, 1890, they met with Chinamen. They rested for a month at Ta-Tsien-Lou, on the Chinese frontier, where they received a cordial welcome from French missionaries; and on July 29, they started for Tonkin, arriving at Yunnan on September 5, where they found a letter from Europe, dated September 5, 1889. Reaching Manghao, on the Red River, they hired Chinese junks, and entered Tonkin at Lao-Kaï. Soon afterwards they were at Hanoï. Altogether, they had traversed 2500 kilometres on an unknown route.

Among the more important of the geographical results of the journey is the discovery of volcanic regions. On December 22, 1889, they observed on the plateau they were crossing a *coulée* of lava; and, looking towards the horizon, they saw in the west an isolated volcano, to which they gave the name of Mount Reclus, in honour of the well-known geographer. Further on, they came to other volcanoes, near which they saw great blocks of lava, which at a distance they took for yaks. One small chain reminded them of the mountains of Auvergne.

In the great chain of Dupleix they found fossils (bivalves), belonging to Tertiary strata, at a height of 5800 metres. In the same region they discovered various minerals, especially iron and lead. At the foot of the Dupleix chain, among rocks, they met with grey monkeys, with rather long hair and short tails. These creatures appeared to be isolated, as they had not been seen before, and were not seen afterwards.

At the meeting of the Royal Geographical Society on Monday, Mr. E. G. Ravenstein gave some account of the British East Africa Company's Expedition, under Mr. F. J. Jackson, from Mombassa to Uganda. The route up to Machako's, about 250 miles north-west of Mombassa, is already pretty well known from the narratives of Mr. Joseph Thomson and others. The portion between Machako's and Uganda had also been traversed to some extent by Mr. Thomson, as well as by Count Teleki and the late Dr. Fischer. Captain Lugard found that the plateau, which rises to about 6000 feet at Machako's, is much broken up by ravines, while there are numerous waterless stretches, where, however, water can generally be found by digging. There are numerous valleys and glades, with abundant vegetation; many patches of forest, mostly of soft-wood trees, and even several perennial streams. Iron and copper are abundant in some places, and indications of gold were found by Captain Lugard. From Machako's, Mr. Jackson's caravan had to make its way up the steep face of the Kinangop escarpment, 9000 feet in altitude, below which, in the valley between that and the equally steep and high Mau escarpment lay lake Naiwasha, and several other lakes, all without outlets, and yet all fresh. A descent of some 3000 feet has to be made to the lakes. These two escarpments, which may be said to extend more or less continuously from Abyssinia to Ugogo, are, Mr. Ravenstein pointed out, two of the most remarkable physical phenomena on any continent. The plateau between Machako's and Lake Victoria Nyanza is even more broken up by deep ravines than that between Machako's and the coast, so that travelling becomes of the most trying character. While the country here is to a large extent of a steppe character, still there are some districts of the highest fertility. In some cases the forest has been cleared away, and the country cultivated by the natives, some tribes being great cattle-rearers. Many of the gorges are still densely clad with forests, and beyond the Mau escarpment is a perfect network of rivers. Game was plentiful and buffaloes were seen in large herds. The north-east corner of Lake Victoria Nyanza has been laid down more accurately than on existing maps, and the contour given to it by Mr. Stanley is in all essential respects confirmed. Usogo, where the Expedition received a cordial welcome, is evidently one of the richest countries in Africa; a marked contrast to Uganda, which, owing to the strife which has prevailed since the death of Mtesa, has been converted into a wilderness

Before entering Usogo, Mr. Jackson made a detour to the north-east of Mount Elgon, but did not succeed in reaching Lake Rudolph, visited by Count Teleki. The country in this direction is of a barren steppe character, sparsely covered with bush, and with a few heights rising above the general level. On his way back, Mr. Jackson and his caravan travelled right across the summit of Mount Elgon, one of the most remarkable mountains in Africa. It is an extinct volcano, the crater of which is eight miles in diameter, its appearance reminding one of the great craters seen in lunar photographs. This mountain is over 14,000 feet high, and, taken in combination with Kilimanjaro, Kenia, and Ruwenzori, seems to indicate that at one period this must have been a region of intense volcanic activity. High up on the face of this mountain Mr. Jackson came upon the caves of which Mr. Thomson told us. These he found to be entirely natural, and not the work of man. One is so large that on its floor has been built a village of huts; for the caves are inhabited by natives who have been compelled to take refuge here from their enemies in the plains. Mr. Jackson's natural history collections are very extensive; very many new species of birds and insects have been sent home. Mr. Bowdler Sharpe stated that these collections have revolutionized existing notions as to the zoological geography of Africa. In the Mount Elgon region types are found similar to those of Abyssinia on the one hand and the Cape on the other; and Mr. Sharpe stated that the region most resembling that of Elgon is that of the Cameroons Mountains in West Africa; but this is based mainly on the ornithology of the two regions, the entomology leading to somewhat different conclusions. On the whole, the geographical and natural history results of the expedition are of high importance, and credit is due to the British East Africa Company for encouraging work of this kind.

NOTES.

Prof. Helmholtz, as we have already stated, will celebrate his seventieth birthday on August 31. In honour of the anniversary, a marble bust of Prof. Helmholtz will be prepared; and it is proposed that there shall be a Helmholtz Medal, to be bestowed on the most eminent German and foreign physicists. An international committee has been formed for the purpose of carrying out these schemes.

At its last meeting, January 28, the Russian Geographical Society awarded its two great gold Constantine Medals to Prof. A. Potebnya for his numerous ethnographical and philological researches, and to Prof. Th. Sloudsky for his geodetical work. The Count Lütke's medal was awarded to S. D. Rylke, also for geodetical work, and especially for the mathematical discussion of the results of the recent exact levellings. Gold medals were awarded to P. Rovinsky for his geographical and ethnographical description of Montenegro; to N. Filipoff for his work on the changes of level of the Caspian Sea; to V. Obrutcheff for a work on the Transcaspian region; and to V. Priklonsky for his manuscript, "Three Years in the Yakutsk Region." A number of silver medals were awarded to several persons for many years' meteorological observations, and various ethnographical works of minor importance.

The ninth German Geographentag, which will meet in Vienna on April 1, 2, and 3, will deal chiefly with the present state of our geographical knowledge of the Balkan peninsula, and with the investigation of inland seas. A geographical exhibition will be held in connection with the meeting.

A Royal Commission has been appointed to inquire into the effect of coal-dust in originating or extending explosions in coal-mines. Mr. Chamberlain is to be the Chairman, and his brother Commissioners are Lord Rayleigh, Sir William Lewis, Prof. Dixon, Mr. Emerson Bainbridge, and Mr. Fenwick, M.P. A small committee of experts has already investigated the subject.

On Friday last, in the House of Commons, in answer to Sir H. Roscoe, Mr. Plunket said it had been decided to proceed at once with the completion of the buildings in connection with the Science and Art Department on the east side of Exhibition Road. They would ultimately be devoted wholly to art collections, although for some years it was probable that some portions of them might be temporarily available for science collections. The buildings would, however, take several years to complete, and he was in communication with the Science and Art Department as to the best means of providing for the science requirements, and he hoped soon to be able to submit a proposal to the Treasury. As the buildings on the east side of Exhibition Road would cost some £300,000 or £400,000, it was obvious that any further immediate demands on the Chancellor of the Exchequer must be confined within as narrow limits as possible.

Some time ago Sir Joseph Fayrer announced his intention of retiring from the presidency of the Sanitary Assurance Association. At the tenth annual meeting of the Association on Monday, Mr. Rutherfurd referred to the great services Sir Joseph had rendered to the cause of sanitary improvement, and proposed a resolution expressing warm appreciation of the valuable services rendered by him during the ten years in which he had held the office of President, and assuring him of "their high admiration for the zeal and energy manifested in his disinterested and gratuitous discharge of those services (amidst numerous other public and private duties) to the undoubted promotion of the general health and public good." This was seconded by Surgeon-General Cornish, and warmly supported by Prof. Smith, and adopted. Sir Joseph Fayrer, in acknowledging the resolution, said he should continue to take a lively interest in the Association and its work, and would retain his seat on the Council. Surgeon-General Cornish, late Sanitary Commissioner with the Madras Government, was elected President; and Sir Joseph Fayrer and Prof. Roger Smith were elected Vice-Presidents.

A capital paper on decimal coinage, weights, and measures was delivered before the Society of Arts on February 4 by Mr. J. Emerson Dowson, and is printed in the current number of the Society's Journal. He urged that there is pressing need for a thorough investigation of the whole subject by a Royal Commission; and, as evidence of the opinion of important bodies of commercial men on the subject, he mentioned that all the seventy-two Chambers of Commerce of the Association of the United Kingdom have repeatedly pronounced themselves in favour of the decimal system, and that the four large Chambers which are not members of the Association (Edinburgh, Glasgow, Liverpool, and Manchester) have taken the same ground. Sir Henry Roscoe, who presided, remarked that he himself had largely benefited by the use of the metric system, which was employed of necessity by men of science in all countries. He had also seen the simplicity and ease with which arithmetic was taught in foreign schools, and could bear testimony to the way in which the old German system of weights and measures was entirely swept away in a few months, when the new system became the law of the land, and how readily it was adopted by the people. Mr. Goschen told Mr. Leng, in answer to the memorial sent from the Dundee Chamber of Commerce, that he was well aware of the strong case which the advocates of a decimal system had made out, but the difficulties were very great, and he could not undertake to bring in a Bill. They did not expect him to do so at present; the matter required further consideration; for although several Committees and Royal Commissions had sat upon it, the whole question was not then considered, and they wanted a new Royal Commission to consider the whole subject. He could not help thinking that if pressure were brought to bear on the Government by Chambers of Commerce, and by all interested in the simplification of the present confusion, not only would a Commission be granted, but the public would be ripe for a decisive step being taken that would put us on a level with all the rest of the civilized world.

AT the meeting of the Institution of Civil Engineers on February 3, a valuable paper on electric mining machinery, by Messrs. Llewelyn B. and Claude W. Atkinson, was read. The authors maintained that electric power was destined to become an important factor in mining mechanics, on account of (1) the facility with which it could be used with machines which required to be moved from time to time; (2) the great economy in first cost and reduced cost of working owing to its efficiency being higher than that of compressed air or any other medium of power transmission; (3) the smaller cost of maintaining the cables, as compared with piping on shifting floors in roadways, &c. Methods were described which the authors considered sufficient to obviate all objections to the use of electric motors in coal-mining, whether by excluding inflammable gases or by constructions which would allow of their safe combustion. Experiments, trials, and practical work, extending over four years, showed, it was contended, that—(1) electrical pumps might be used with advantage and economy for mine-draining; (2) electrical coal-cutters could replace hand-labour with saving in cost, and increased production of coal; (3) electrical drilling-machines were available in place of machinery worked by hand or compressed air.

AT the meeting of the Royal Botanic Society on Saturday last, a gift of seeds of the Parà rubber-tree suggested to Mr. Sowerby, the Secretary, some interesting remarks on india-rubber and gutta-percha. In the Society's museum was a specimen of the first sample of gutta-percha imported to Europe —viz. in 1842—and it was shortly after that date that it was used to insulate the first submarine telegraph cables. No substitute had been found to take its place. From some papers lately published in the *Electrical Review*, he gleaned that from the "wholesale cutting down of adult trees" and the "reckless clearing and burning of the forests" the trees furnishing the most valuable kinds of gutta-percha had become exceedingly scarce, and in most localities utterly extirpated. This was also rapidly becoming the case with the trees which supply the many varieties of india-rubber, and, sooner or later, all natural vegetable products used by man would have to be artificially cultivated, as the natural supply never kept pace with the artificial demand. Some few attempts had been made to cultivate india-rubber, but as yet not very successfully.

THE latest report of the Oxford University Extension Delegacy, ending with the last summer meeting, is highly satisfactory. It appears from it that the number of courses of lectures has grown from 27 in 1885–86 to 149 last year, that the number of centres at which lectures were delivered has risen from 22 in the same year to 100 in the last session, that, whereas in 1886–87 the average number of students attending the different courses was 9908, now no fewer than 17,904 receive in the different centres the instruction given. History seems from the return to be the favourite subject—85 courses were given upon it; on literature and art 28; on various branches of natural science 25; on political economy 10. "In August, 1889," the report continues, "the second summer meeting of Oxford University Extension students was held in Oxford. I was attended by 1000 students, and lasted rather more than a month, this period being divided into two parts to meet the convenience of those students whose duties prevented them from being present during the whole meeting. During the year the system of training lecturers on their appointment has continued." Finally the report acknowledges the receipt of various scholarships intended to enable poor students to avail themselves of the oppor tunities afforded by the summer meeting, Lord Ripon and Mr. J. G. Talbot, M.P., being the most considerable donors.

THE other day the members of the Manchester Geological Society attended a special meeting, which was held in the Geological Museum, Owens College, to hear an address by Prof. Williamson on the vegetation of the Carboniferous age. The special object of the meeting was to afford information to those engaged in the superintendence and working of coal-mines, which would enable them to assist in the collection of specimens likely to be of scientific interest. Prof. Williamson gave a lucid and interesting description (in which he was aided by some beautiful diagrams and specimens) of the families of plants represented in the coal measures. Speaking of the assistance he had received, and still hoped to receive, from geologists residing in Oldham and elsewhere, in working out their structure, he said that one great desire he had was to enrich the museum with as complete a collection as possible. All his own specimen would shortly be transferred to that building, and he therefore felt all the more confidence in asking the help of others. He invited members of the Society to preserve and send to him any fossil plant, however fragmentary, which came into their possession from the Carboniferous strata, taking care to note the character and position of the bed in which it was found. They wanted the most exact information obtainable with reference to the vertical range of species, and also as to their range horizontally. In getting the specimens together it was of importance that they should avoid mixing those from different seams. In this way most valuable information might be gained with regard to the life-history of many at present little-known plants of the coal measures.

THE Agricultural Department of Victoria has applied to the U.S. Minister of Agriculture for the services of an expert in the growth and manufacture of tobacco. According to the *Australasian*, there is a large field for the growth and manufacture of tobacco in Victoria, but so far the efforts made in that direction have met only with indifferent success, owing to the defects arising from want of knowledge in the drying and treatment of the leaf after it has been cut. It is hoped that, if the services of a thoroughly competent expert were secured, these defects would be removed, and that before very long tobacco would form one of the chief products of the colony.

A RECENT communication of Herr Büchner to the Imperial Academy of Sciences of St. Petersburg announces that, among other objects obtained in the Chinese Province of Kansu by Herrn Potanin and Beresowski, during their expedition of 1884-87, was a skin of *Æluropus melanoleucus*. This very remarkable Bear-like animal is hitherto known only from the specimens which were procured by Père David in the principality of Moupin, in the north of Szechuen, and which are now in the Paris Museum. Herr Beresowski met with it in the mountains of Southern Kansu, at an elevation of 10,000–12,000 feet, where it inhabits the Bamboo-bushes, and is known to the natives as the *Pei-ssjun* or *Chua-ssjun*, *i.e.* White, or Spotted, Bear. Few presents, we imagine, would delight the heart of the Director of the British Museum of Natural History more than examples of this rare and little-known Mammal. As France and Russia can now both boast of specimens, England, whose interests in China are so predominant, surely ought to be able to obtain some likewise.

AN interesting paper on the destruction of wolves in France appears in the current number of the *Revue Scientifique*. The law in virtue of which rewards are given for the killing of wolves was passed on August 3, 1882, and during the last four months of that year 423 were destroyed. In 1883 the number killed was 1316, the sum paid in rewards being 104,450 francs. The number was 1035 in 1884; 900 in 1885; 760 in 1886; 701 in 1887; 505 in 1888; 515 in 1889. The departments in which most animals have been slain are Dordogne and Charente. It is believed that very soon no specimens will be left in France except those which occasionally reach it from neighbouring countries.

IT has been a practical difficulty in the use of electrical accumulators, to know, at any given time, to what extent they are charged or discharged. M. Roux, we learn from *La Nature*, has devised an indicator of charge, based on the principle that the mean density of the liquid varies according to the quantity of electricity stored. He uses a flattened glass tube, of length about equal to the depth of the liquid, and weighted so as to be heavier than the thrust it receives. It is hung by means of a platinum wire to one end of a short lever, pivoted in the middle, and having a weight on the other arm; with full charge, this lever is horizontal. At right angles to the lever is a weighted pointer, which gives indication on a horizontal scale above, with 100 divisions; the pointer, vertical at 100 when the accumulator is charged, stands at 45 after discharge. The readings on the scale indicate directly the percentage ratio of the quantity remaining in the accumulator to that of saturation. By means of electric contacts, &c., the state of charge can be announced. M. Roux finds by experiments the differences between indications of his apparatus and those of an ampere-meter placed in the circuit to have been always less than 3 per cent., which is considered very satisfactory, the object being industrial.

THE *Annals of the Astronomical Observatory of Harvard College*, vol. xxi. Part ii., contains a summary of the meteorological observations made in the year 1889 under the auspices of the New England Meteorological Society, and three investigations dealing respectively with types of New England weather, land and sea-breezes, and the characteristics of the New England climate. These essays are of a very exhaustive nature, and should be of much use to meteorologists.

THE recent earthquake in Algiers did little damage in the town itself. The first shock, which occurred at 4 a.m. (on the morning of January 15), was followed by a more violent one at 4h. 10m. lasting 12 seconds. Slight shocks were also experienced at 5 a.m. and at 7 a.m. The direction of the shocks was from south-east to north-west. The inhabitants fled from their houses to the open places. The shocks were most violent at Gouraya, near Cherchel, which also suffered. In Philippeville two persons were killed and many injured by the fall of a house. In Villebourg many houses are in ruins; and numerous lives were lost there, and at Blidah, Medeah, and Orleansville.

THE journals of Nismes record three deaths in that neighbourhood resulting from poisoning by *Amanita citrina*, which appears, at least in the south of Europe, to be one of the most deadly of Fungi.

A "FLORA OF PALESTINE" is in progress edited by the Rev. G. E. Post, and is now completed as far as the end of the order *Umbelliferæ*. Several new species are described.

THE death is announced, on January 24, of Dr. Philip Carl, Professor of Physics at the Munich Royal Military College, and late editor of the *Repertorium für physikalische Technik* and the *Zeitschrift für Elektrotechnik*.

CAPTAIN ABNEY will deliver at the Society of Arts a course of five popular lectures on "The Science of Colour," on the following Friday afternoons, at half-past four o'clock:—February 13, 20, 27, March 6, 13. The lectures will be popular and elementary, and fully illustrated by experiments.

THE atomic weight of rhodium has been redetermined by Prof. Seubert and Dr. Kobbé, of the University of Tübingen, and an account of their results is published in the current number of *Liebig's Annalen*. During the last few years the atomic weights of the other five metals of the platinum group, ruthenium, palladium, iridium, platinum, and osmium, have formed the subjects of most careful investigations, mainly at the hands of Prof. Seubert, with the result that these values are considered as among the best determined of all atomic weights. The remaining metal, rhodium, must now be added to the list, for the present redetermination leaves no doubt whatever that the real value of this atomic weight has been arrived at within the ordinary limits of inevitable experimental error. The two principal former determinations afforded widely different results. That of Berzelius, in 1828, from an analysis of the salt $4KCl . Rh_2Cl_6$, gave the number 104·07; while Jörgensen, in 1883, employing the ammoniacal salt, $Rh_2(NH_3)_{10}Cl_6$, deduced the value 103·00 ($O = 16$) or 102·74 ($O = 15·96$). In the present redetermination, Seubert and Kobbé made use of the same ammoniacal compound as Jörgensen. Fine crystals of this salt are readily prepared in a state of purity, they are very stable in the air, and the salt lends itself readily to accurate analysis. It was thus in all respects eminently suitable for the purpose of an atomic weight investigation. The experimental method of treatment which was found to be attended with least possibility of error consisted in reducing the heated salt in a current of pure hydrogen to metallic rhodium, and thus determining the ratio of the weight of salt to its content of rhodium. The finely powdered crystals, after drying at 100° C., were weighed in a porcelain boat; the boat and its contents were then placed in a combustion-tube lined internally with a cylinder of platinum, the tube was gradually heated in the furnace, and a stream of hydrogen passed through. After complete reduction, and displacement of the hydrogen by a current of carbon dioxide, the metallic rhodium was found in the boat in the form of a bright grey rod. The boat and metal were then again weighed. Ten such experiments yielded values ranging, when $O = 15·96$, from 102·61 to 102·81. As the final mean, the number 102·7 is adopted; or, if $O = 16$, the round number 103·0. These values confirm those obtained by Jörgensen in an exceptionally satisfactory manner, thus setting the question finally at rest. Rhodium, therefore, retains the place in the periodic system marked out for it by its chemical behaviour, between ruthenium, of atomic weight 101·4, and Pd 106·3, and in the same vertical group as its analogue iridium.

THE additions to the Zoological Society's Gardens during the past week include two Malbrouck Monkeys (*Cercopithecus cynosurus* ♂ ♀) from West Africa, presented respectively by Mr. J. P. Heseltine and Mrs. Newton; two Common Peafowls (*Pavo cristatus* ♀ ♀), bred in Scotland, presented by Mr. Richard Hunter; two Globose Curassows (*Crax globicera* ♂ ♀), two Mexican Guans (*Penelope purpurascens*) from Central America, a Daubenton's Curassow (*Crax daubentoni* ♂) from Venezuela, deposited.

OUR ASTRONOMICAL COLUMN.

"ANNUAIRE DU BUREAU DES LONGITUDES."—This extremely useful and unique *Annuaire* for 1891 has just been published. The astronomical information is as complete as could be desired. The tables of physical and chemical constants are of the same comprehensive character. MM. Lœwy and Schulhof give an account of the comets that appeared between 1800 and 1826, and in 1889. This list completes those given from 1882 to 1890, and forms with them a catalogue that contains references to every published comet observation made this century. A table of sixty-two double stars, of which the elements are known, is given for the first time by M. Glasenapp. Another table, constructed by M. Bossert, contains the proper motions of sixty-nine stars. M. Cornu contributes a succinct description of three types of stellar spectra, and an interesting article on the astronomical applications of Doppler's or Fizeau's principle. M. Janssen gives an account of his ascent of Mont Blanc for the purpose of studying the telluric spectrum. M. Tisserand points out the uses of the minor planets, and discusses the communications made at the International Conference on Degree Measurements, held at Freiburg on September 15, 1890. In the portion of the work devoted to terrestrial magnetism, mention is made of anomalous disturbances similar to those found by Profs. Rücker and Thorpe in England. Many other important points are brought together, and the whole stands forth as a *vade mecum* having no equal.

UNITED STATES NAVAL OBSERVATORY.—Captain F. V. McNair, the Superintendent of this Observatory, has recently issued his report for the year ending June 30, 1890. Prof. A. Hall has used the 26-inch equatorial for observations of double stars, Saturn and its satellites, and Mars. The reduction of these observations is now in progress. Prof. Harkness has employed the transit circle in observations of the sun, major and minor planets, and certain stars for clock and instrumental corrections. The 9·6-inch equatorial has been used by Prof. Frisby for observations of comets, minor planets, and occultations of stars by the moon. Lieut. Hodges has made observations with the meridian transit instrument. Mr. Paul has continued the observations of his new Algol-type variable, S Antiliæ, referred to in the last report. The reductions show that the period deduced by Mr. Chandler in the *Astronomical Journal*, No. 190, viz. 7h. 46m. 48·0s., will be changed by only a very small fraction of a second. The magnetic observations and testing of chronometers have been continued as usual. As in many other Observatories, it is complained that the staff is insufficient to reduce and discuss the astronomical, geodetic, gravitational, and tidal observations that are made.

A NEW THEORY OF JUPITER AND SATURN.—In the Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. iv., Mr. G. W. Hill develops a new theory of the movements of Jupiter and Saturn. The guiding principle of the investigation was "to form theories of Jupiter and Saturn which would be practically serviceable for a space of three hundred years on each side of a central epoch taken near the centre of gravity of all the times of observation: theories whose errors in this interval would simply result not from neglected terms in the developments, but from the unavoidable imperfections in the values of the arbitrary constants and masses adopted from the indications of observation." It is seven and a half years ago since the necessarily laborious computations were commenced, and many years must elapse before any verdict can be given as to their accuracy. The theory appears, however, perfect, and the author is to be sincerely congratulated upon its development.

ARGELANDER-OELTZEN STAR CATALOGUE.—Dr. E. Weiss has completed his comparison of the stars contained in the catalogue constructed by Oeltzen from Argelander's southern zones and reduction-tables, with those contained in Schönfeld's Southern *Durchmusterung* and the Cordoba Catalogue. The work forms a supplementary volume to the Annals of the Observatory of Vienna University. The places of 18,276 stars are given for 1850, and the value of total precession which will enable them to be determined for the mean epoch 1875·0. About 1000 stars had to be examined with the Vienna equatorial, because they were not contained in the Cordoba zones, and 200 others for verification of position. Very few differences of magnitude have been found, and these were only of small amount. Argelander's Catalogue has always been of extreme value to astronomers. Prof. Weiss's revise renders its usefulness inestimable.

TECHNICAL INSTRUCTION IN ESSEX.[1]

THE Council of the Essex Field Club proposes to establish, at the cost of the Club, in Chelmsford (chosen not only as the county town, but also as being a central position in Essex), a public Museum to illustrate the natural productions, the geology and physiography, and the industries and manufactures of Essex, together with an educational series of specimens and preparations, which may be employed for teaching purposes. The Museum will also contain a library of books, maps, parliamentary papers, pictures, &c., treating of the natural history, geology, topography, history, and industries of Essex, as well as a general library of books necessary for the study of the before-mentioned subjects.

The Council of the Club is very desirous that the Museum shall be of the greatest possible service in promoting the study and love of science and its applications to industries and manufactures, and as a subject of general education. In the endeavour to carry out these objects, the Club most respectfully asks for the aid of the County Council of Essex, in accordance with the powers given by the above-mentioned Acts.

The leading features of the technical education scheme of the Essex Field Club are as follows:—

(1) The establishment of a central institution in Chelmsford in connection with the Club's Museum, with large laboratories and class-room, furnished with apparatus and preparations for practical teaching, and in which, as occasion may arise, examinations could be conducted; the institution being also amply provided with lecturing and class-teaching appliances (lanterns, slides, diagrams, apparatus, models, materials, and specimens) so arranged in travelling cases that they could be easily sent to any part of the county for use at the local lectures and classes.

(2) The arrangement of peripatetic courses of classes and lectures, conducted by specially qualified teachers (either supplemental to local efforts, or at the sole instance and cost of the institution) for imparting instruction in science and technology in any parts of the county, particularly in rural and maritime districts. The teaching given to be either elementary or more advanced, but always, as far as possible, of a thoroughly practical character, and such as will give a knowledge of things rather than words, and develop the faculties of seeing and doing.

The most important of the subjects proposed to be taught may thus be grouped:—

(*a*) Elementary drawing, practical geometry, carpentry, modelling, &c., and their applications in the study and practice of the following subjects.

(*b*) Practical elementary physics and chemistry, and their applications in agriculture, industries, &c.

(*c*) Biology, including practical botany and the principles of vegetable physiology, and their applications in agriculture, gardening, &c.

(*d*) The principles of geology and mineralogy, and their applications in agriculture, water-supply, &c.

(*e*) Human physiology and the laws of health or hygiene.

(*f*) Geography and physiography, including practical meteorology.

(*g*) The principles and practice of agriculture and agricultural chemistry, live-stock management, fruit-growing and preserving, dairy management, &c.

(*h*) Forestry, arboriculture, and gardening.

(*i*) The structure, life-histories, diseases, distribution, &c., of fish, molluscs, crustacea, &c., with special reference to the Essex fisheries, oyster culture, &c.

(*j*) Courses of instruction on the diseases of plants and animals, and on beneficial and injurious birds, insects, injurious fungi, &c.

(*k*) Special courses of instruction on the scientific principles and practice of any local industries.

(*l*) Navigation, fishing, &c.

(*m*) Cookery and minor domestic industries.

The stock of apparatus, models, preparations, maps, specimens, &c., in the central institution would allow of the teaching in these lectures and classes being illustrated and made practical in a way that would be impossible in the case of rural centres and villages under any other system. In schemes of elementary, scientific, and technical instruction hitherto put forward, towns and populous centres have alone been considered. The present scheme would permit of the best kind of instruction being given not only in towns but also in rural and maritime districts, and that at a minimum cost. It should be noted also that if in the future an extension of the institution in any direction should be considered desirable by the County Council of Essex, the plans proposed will readily allow of such development without any interference with the work then being carried on.

The museum, laboratories, and class-room would also be serviceable:—

[1] Abstract of scheme put forward by the Council of the Essex Field Club (under the Technical Instruction Act, 1889; the Local Taxation Act, 1890; and in accordance with the regulations of the Science and Art Department).

(1) As a means of giving general scientific information and practical education in the county, and as a local centre of nstruction for Chelmsford and its neighbourhood.

(2) As a place for instruction in the higher branches of any subject for advanced pupils, and as giving opportunities for individual practical work; the laboratories and class-room would also be of general advantage as an examination centre for any science or technical classes held in the county, whether under the Club's scheme or otherwise.

(3) It is submitted also that the museum, laboratories, and library at Chelmsford will be of considerable utility to the inhabitants of the county at large, to farmers, gardeners, fishermen, &c., and to members of the County Council, county officers, and others desirous of obtaining accurate information about Essex, its natural productions and industries, and also as affording facilities for any special technical investigations in the subjects above mentioned.

The Club would become affiliated to the Science and Art Department, so that Government examinations could be held, prizes and payments on results earned, and grants claimed towards the building fund, and for the purchase of apparatus, examples, &c. This affiliation would bring the Club clearly within the terms of the Technical Instruction Act, 1889.

In the work of carrying out the above scheme, the Essex Field Club would have special facilities; it would be in fulfilment of one of the highest objects of the Club, and the Council and members would have every incentive to carry out the scheme well and energetically. The ordinary meetings, serial publications, and circulars of the Club would also aid much in making the work widely known and appreciated, and in attracting students likely to receive benefit from the teaching afforded.

The grants from the County Council would be supplemented by (*a*) local contributions; (*b*) fees from students; (*c*) grants earned from the Science and Art Department; (*d*) special aid, both in money, specimens, and assistance by the Club and its members, the scheme being really complementary to the existing work of the Club.

The management of the classes would be in the hands of a special committee or committees, appointed by the Council of the Club, not necessarily chosen from the members, which committee or committees would have control over the apparatus during the continuance of the grants, and the Council of the Club would undertake, on its part, to carry out the above stipulations also during the continuance of the grants.

The Council claims that the scheme above set forth is of a wide-reaching character, embracing the whole county and not any particular district; that it will supplement in a very useful way the work of existing educational centres; and that it is calculated to be particularly serviceable in those districts not provided for by urban educational institutions. It has been formulated under the advice of some eminent practical educators; it is in accordance with the recommendations of the National Society for the Promotion of Technical and Secondary Education, and above all, it is perfectly workable provided sufficient funds are available for the purpose.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following is the speech delivered by the Public Orator, *Dr. Sandys*, Fellow and Tutor of St. John's, in presenting for the complete degree of M.A. *honoris causâ* Mr. J. A. Ewing, B.Sc. (Edinburgh), F.R.S., recently elected to the Professorship of Mechanism and Applied Sciences, vacated by the resignation of Professor Stuart, M.P.:—

Dignissime domine, domine Procancellarie, et tota Academia:—

Uni e professoribus nostris, Britanniae senatoribus adscripto, nuper valediximus, cuius merita de Academiae praesertim finibus Britanniae in oppidis magnis amplius prorogandis, animo grato in perpetuum recordabimur. Successorem autem eius hodie salutamus, qui, vitae humanae spatio dimidio vixdum decurso, quindecim iam annos, primum solis orientis inter insulas, deinde patriae septentrionalis in litore, professoris munere egregie functus est. Interim opera eius insignia, partim machinis vapore actis explicandis, partim scientiae magneticae investigandae dedicata, non modo doctrinae Britannicae inter thesauros, sed etiam Societatis Regiae inter annales relata sunt. Quid dicam de pulcherrimo eius invento, quo terrae motus etiam levissimi accuratissime indicantur? Nonnulli certe vestrum audivistis orationem eximiam, quâ nuper, munus suum auspicatus, scientiae machinali Academiae inter studia locum vindicavit, supellectilem ampliorem ei deberi arbitratus. Croesi divitiis si forte frueremur, Archimedis scientiam apparatu amplissimo libenter ornaremus. Interim civium munificorum liberalitatem exspectantes, his studiis in hac arce doctrinae denuo instaurandis (ut Vergili utar versu) *Dividimus muros et moenia pandimus urbis.* Quod si quis hodie loci eiusdem verbis male ominatis abuti velit, *Scandit fatalis machina muros;* omen illud in melius statim convertimus, recordati ex equo Troiano viros fortes, meros principes, exstitisse. Tali igitur viro, scientiae tantae inter principes numerato, non iam manus nostras velut devicti dedimus; foedere potius novo utrimque devincti, dextram dextrae libenter iungimus. Duco ad vos Professorem Ewing.

The Council of the Senate report that, in view of the dissent of ten of the Colleges therefrom, they have resolved to proceed no further with the proposed statute for relieving distressed Colleges from the contribution to the University funds.

E. A. T. Wallis Budge, M.A. of Christ's College, the distinguished Egyptologist of the British Museum, has been approved for the degree of Doctor in Letters.

A portrait of Prof. A. Newton, F.R.S., painted by Mr. C. W. Furse, has been presented to the University by the subscribers, and will probably be hung in the New Museum.

Mr. S. J. Hickson, M.A., the author of a recent work on Celebes, has been appointed to the Lectureship in the Advanced Morphology of Invertebrates, vacant by the resignation of Prof. Weldon, F.R.S., now of University College, London.

Mr. G. F. C. Searle and Mr. S. Skinner have been appointed Demonstrators of Experimental Physics at the Cavendish Laboratory.

Dr. Anningson, University Lecturer in Medical Jurisprudence, and Medical Officer of Health for Cambridge, announces a course of lectures and demonstrations in public health, suitable for candidates for the University diploma. The course will be given in the Long Vacation.

The Annual Reports of the Fitzwilliam Museum Syndicate and of the Antiquarian Committee contain long lists of valuable gifts and acquisitions of archæological and ethnographical interest received during the past year.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 22.—"The Passive State of Iron and Steel. Part II." By Thos. Andrews, F.R.SS.L. and E., M.Inst.C.E.

The experiments of Series III., in this paper, relate to the effect of temperature, and the observations of Series IV. refer to the influence exerted by nitric acid, of varied concentration, on the passive condition of iron and steel.

Series III.—Effect of Temperature on the Passivity of Iron and Steel.

The bars selected for these observations were unmagnetized polished rods, which had been previously drawn cold through a wortle; a pair of bars of each metal were cut adjacently from one longer bar, and then placed securely in the wooden stand; each bar was $8\frac{1}{4}$ inches long, 0·261 diameter. The **U**-tube, containing $1\frac{1}{4}$ fluid ounce of nitric acid, sp. gr. 1·42, was rigidly placed in an arrangement as shown on Fig. 3 in the paper. One limb was surrounded by a tank containing water, the other limb by a tank of the same capacity containing powdered ice; the arrangement was such that the water-tank could be heated by a Bunsen burner, and its temperature slowly raised, whilst the ice-tank was kept full of powdered ice.

The bars were in circuit with the galvanometer, and soon after immersing them in the nitric acid heat was applied to the water-tank, and the temperature of the nitric acid in that limb of the **U**-tube slowly raised to the temperatures required, whilst the acid in the other limb of the **U**-tube was meanwhile maintained at a temperature of 32° F.

The arrangement will be understood on reference to Fig. 3 in the paper, and the electro-chemical results obtained are graphically recorded on diagram I.

These electro-chemical experiments indicated that the wrought iron was less passive in the warm nitric acid than the soft cast steel: the average E.M.F. of 94 observations with wrought iron was 0·030 volt; whereas, in the case of the 94 observations on cast steel, the average E.M.F. was only 0·010 volt.

It was noticed that the behaviour of the steel, under the conditions stated, was more irregular than that of the wrought iron.

In the whole of the series of experiments on diagram I. the nitric acid was raised to a temperature of 175° F.; the cold nitric acid in the limb of the U-tube A remained perfectly colourless, and the steel or iron therein absolutely passive; but the steel or iron in the warm nitric acid in tube A, commenced to be gradually acted upon as the temperature increased, a pale yellow tint beginning to appear in the solution in the tube A, shortly after commencement.

The results showed that iron or steel does not fully lose its passivity up to a temperature even of 175° F., though the passivity is shown to have been considerably modified by temperature. The critical point of temperature of transition from the passive to the active state is therefore higher than 175° F., and is shown in the experiments of Part I., Series II., Table II., to have been about 195° F.

Series IV.—The Passivity of Iron and various Steels increases with the Concentration of the Nitric Acid.

Scheurer-Kestner considered that the passivity of iron was not dependent on the greater or less degree of saturation of the acid. In connection with this aspect of the subject, the electro-chemical experiments of Series IV. indicate, however, that the property of passivity in iron is not absolutely fixed or static, but that its passivity is modified to a certain extent in relation to the strength of the nitric acid used.

A current was observed between two bright "passive" wrought iron or various steel bars of the same composition, one in cold nitric acid, sp. gr. 1·5, the other in cold nitric acid, sp. gr. 1·42; the electro-chemical position of the bar in the weaker acid was positive. The mode of experimentation was generally similar to that previously employed. The chemical composition and general physical properties of the various steels used in the experiments are given in Tables IV. and V.

The results, the average of many repeated experiments in each case, are given in Table III., and show that the passivity of iron increases considerably with the strength of the nitric acid.

The results of the experiments of Series IV., Table III., show that wrought iron was less passive in the weaker acid than most of the steels, the soft Bessemer steel being found similar in passivity to the wrought iron.

The average E.M.F. was as follows:—With wrought iron, 0·054 volt; soft cast steel, 0·028 volt; hard cast steel, 0·036 volt; soft Bessemer steel, 0·059 volt; tungsten steel, 0·039 volt.

Geological Society, January 21.—A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On the age, formation, and successive drift stages of the valley of the Darent; with remarks on the Palæolithic implements of the district, and on the origin of the chalk escarpment, by Prof. Joseph Prestwich, F.R.S. i. *General Character and Age of the Darenth Valley.* The river is formed by the union of two streams, the main one flowing east from near Limpsfield, the other west from near Ightham, parallel with the ranges of Lower Greensand and Chalk, and flows northward into the Thames. The first indent of the valley was subsequent to the deposition of the Lenham sands, and, indeed, to the Red Clay with flints, and the old implement-bearing drift with which this is associated; and the same remark applies to a system of smaller valleys starting near the crest of the escarpment and running into the Thames. ii. *The Chalk Plateau Drifts and Associated Flint Implements.* Since the publication of the author's Ightham paper, Mr. Harrison and Mr. De B. Crawshay have found implements mostly of rude type (though a few are as well finished as those of Abbeville) in numerous localities on the plateau, where, owing to the gradients, the difference of level between plateau and valley-bottom is much greater than at Currie Farm. Evidence derived from the character and conditions of preservation of these implements is adduced in favour of their great antiquity. iii. *The Initial Stages of the Darent Valley.* The author has previously shown that in early Pliocene times a plain of marine denudation extended over the present Vale of Holmesdale, and that in pre-glacial times the plain was scored by streams flowing from the high central Wealden ranges. These streams centred in the Darent, and the excavation of the present valley then commenced. There is a gap in the sequence between the pre-glacial drifts and the earliest post-glacial drifts of the valley, which is probably covered by the extreme glacial epoch. It was a time of erosion, rather than of deposition in this area. Of the earliest drift of the Darent valley, little has escaped later denudation. The bank of coarse gravel on the hill on the west side of the valley between Eynsford and Farningham, certain flint-drifts in the upper part of the valley, and a breccia of chalk-fragments on the hill west of Shoreham, may be referred to this period. iv. *The High-Level or Limpsfield Gravel Stage.* The gravel at Limpsfield occurs on the watershed between the Darent valley and the Oxsted stream, but the author agrees with Mr. Topley that the gravel belongs to the Darent system, and Westheath Hill may be part of the original ridge separating the two valleys. This gravel is post-glacial, and the denudation of the area had made considerable progress at the time of its formation, for the chalk escarpment rises 200–300 feet, and the Lower Greensand 100–200 feet above the gravel-bed. The author traces outliers of this gravel down the valley at lower and lower points to the Thames valley at Dartford, and correlates it, not with the high plateau-gravel, but with the high-level gravel of the Thames valley, and shows that its composition indicates that it is derived from the denudation of the Chalk and Tertiary beds. Mr. A. M. Bell has discovered numerous implements in it, mostly of the smaller St. Acheul type, and the author hopes that they will soon be described by their discoverer. These implements agree in general type with the "Hill group" of the Shode valley, and not with the older group of the Chalk plateau, or those of the lower levels of the Thames and Medway. v. *Contemporaneous Drift of the Cray Valley.* Implements of this age have been found by Mr. Crawshay and by Mr. P. Norman, near Green Street Green, in gravel which is more than 100 feet below the Red Clay of the plateau. vi. *Brick-earths of the Darent Valley.* These are traced along the upper course of the valley from near Limpsfield. They seem to show glacial influence, and Mr. Bell has discovered a few implements in them. The Limpsfield deposit is from 10 to 30 feet below the adjacent gravel. Brick-earth, possibly of somewhat later date, also occurs near Dartford. vii. *Other Gravels of the Darent Valley: the Chevening and Dunton Green Drifts.* The relations of the gravels grouped under this head are more uncertain than those of the Limpsfield stage. Various features in the gravels point to the temporary return of glacial conditions during the period of formation of these and the brick-earths; and these are described in detail. viii. *The Low-level Valley-Gravels.* The correlation of these is also uncertain. West of Dartford is a bed corresponding with that at Erith, in which Mr. Spurrell found a Palæolithic floor. It contains land and fresh-water shells. The surface of the Chalk is here festooned under a covering of the fluviatile drift. The author attributes this festooning to the effects of cold. ix. *The Rubble on the Sides and in the Bed of the Valley.* The author describes this rubble, and rejects the view that it is rain-wash or due to sub-aërial action, and discusses the possibility of its having been produced by ice-action. x. *Alluvium and Neolithic Implements.* These occur chiefly between Shoreham and Riverhead. xi. *On the Chalk Escarpment within the Darent District.* The author, after discussing and dismissing the view that the escarpment was formed by marine denudation, criticizes the theory that it was due to ordinary sub-aërial denudation, and lays stress on the irregular distribution and diversity of the drift-beds in the Darent area; these do not possess the characters which we should expect if they were formed by the material left during the recession of the Chalk escarpment owing to sub-aërial action; and he believes that glacial agency was the great motor in developing the valleys, and, as a consequence, the escarpment, and that the denudation was aftewards further carried on in the same lines by strong river-action and weathering, though supplemented at times by renewed ice-action. By such agencies, aided by the influence of rainfall and the issue of powerful springs, he considers that the escarpment was gradually pared back and brought into its present prominent relief. After the reading of this paper there was a discussion, in which Mr. Topley, Dr. Le Neve Foster, Mr. De B. Crawshay, the President, and the author took part.—On *Agrosaurus Macgillivrayi*, Seeley, a Saurischian reptile from the north-east coast of Australia, by Prof. H. G. Seeley, F.R.S.—On *Saurodesmus Robertsoni*, a Crocodilian reptile from the Rhætic of Linksfield, in Elgin, by Prof. H. G. Seeley, F.R.S.

Royal Microscopical Society, January 21.—Annual Meeting.—Dr. C. T. Hudson, F.R.S., President, in the chair.—Mr. Swift exhibited and described a new form of petrological microscope which he had made under the instructions of Mr. Allen Dick. It differed from the ordinary patterns in having no revolving stage, but was so constructed that whilst the object remained fixed the eye-piece and the tube below the stage could be revolved.—Mr. E. M. Nelson exhibited a new apochromatic condenser, by Powell and Lealand, which gave a larger aplanatic solid cone than it had hitherto been found possible to obtain.—The report of the Council was read, showing an increase in the number of Fellows and in the revenue of the Society.—Dr. C. T. Hudson delivered his annual address.—Dr. R. Braithwaite was elected President for the ensuing year.

Paris.

Academy of Sciences, February 2.—M. Duchartre in the chair.—The death of General Ibañez on the 29th ult. was announced. An account of his life and works was given by M. J. Bertrand.—On the approximate development of perturbing functions, by M. H. Poincaré.—The photography of colours, by M. G. Lippmann. The conditions said to be essential to photography in colours by M. Lippmann's method are: (1) a sensitive film showing no grain; (2) a reflecting surface at the back of this film. Albumen, collodion, and gelatine films sensitized with iodide or bromide of silver, and devoid of grain when microscopically examined, have been employed. Films so prepared have been placed in a hollow dark slide containing mercury. The mercury thus forms a reflecting layer in contact with the sensitive film. The exposure, development, and fixing of the film is done in the ordinary manner; but when the operations are completed, the colours of the spectrum become visible. The theory of the experiment is very simple. The incident light interferes with the light reflected by the mercury; consequently, a series of fringes are formed in the sensitive film, and silver is deposited at places of maximum luminosity of these fringes. The thickness of the film is divided according to the deposits of silver into laminæ whose thicknesses are equal to the interval separating two maxima of light in the fringes—that is, half the wave-length of the incident light. These laminæ of metallic silver, formed at regular distances from the surface of the film, give rise to the colours seen when the plate is developed and dried. Evidence of this is found in the fact that the proofs obtained are positive when viewed by reflected, and negative when viewed by transmitted, light—that is, each colour is represented by its complementary colour.—Observations by M. E. Becquerel on the above communication. M. Becquerel called attention to the experiments made by him on the photography of colours in 1849. His researches, however, dealt more with the chemical than the physical side of the question.—General Derrécagaix read a memoir on a table of centesimal logarithms to eight decimal places, issued by the Service Géographique.—M. Faye presented the *Connaissance des Temps* for 1892 and 1893, and *L'Annuaire du Bureau des Longitudes* for 1891, and described the additions to the latter.—On the distribution in latitude of solar phenomena observed at the Royal Observatory of the Roman College during the latter half of 1890, by M. P. Tacchini. The results, in conjunction with those previously presented, show that in 1890, as in 1889, the prominences were more frequent in the southern hemisphere of the sun than in the northern hemisphere. The maximum frequency occurred in the zone $-40°$ to $-50°$. Faculæ and spots have been most frequent in the northern hemisphere.—Remarks on the displacement of a figure of invariable form in all the planes that pass through some fixed points, by M. A. Mannheim.—Complementary note on the characteristic equation of gases and vapours, by M. C. Antoine.—On the basicity of organic acids according to their electrical conductivity; monobasic and bibasic acids, by M. Daniel Berthelot.—On the reactions of the oxyalkyl derivatives of dimethylaniline, by M. E. Grimaux. A new class of colouring-matters is described.—On the composition and properties of *levosine*, a new substance obtained from cereals, by M. C. Tanret.—On the quantity of oxygen contained in the blood of animals living on elevated regions in South America, by M. Viault. The results indicate that the proportion of oxygen contained in the blood of men and animals (indigenous or acclimatized) living in the rarefied air of mountainous regions, is sensibly the same as that which is contained in the blood of men and animals living at lower levels.—On the amount of hæmoglobin in the blood according to the conditions of existence, by M. A. Müntz. The author, like M. Viault, finds that animals living at great altitudes—that is, in a medium where the pressure of oxygen is low—have the proportion of hæmoglobin in the blood increased, and it consequently acquires an absorbing power for oxygen which compensates the effect of rarefaction. It is also concluded that altitude is not necessary to produce these modifications, and that the same results may be obtained if, instead of diminishing the amount of oxygen, the quantity of combustible matter is increased.—On the larvæ of *Astellium spongiforme* and on the *Pœcilogonia* of *Ascidiæ*, by M. A. Giard.—On the anatomy of *Corambe testudinaria*, by M. H. Fischer. A new species of mollusk is described. The author notes: "J'ai recontré dans l'oreillette et dans les muscles de la radule des fibres striées transversalement." This confirms the idea that the striation of muscles is intimately connected with the mechanism of their contraction.—On the *Acridium peregrinum* from the extreme south of Algeria, by M. J. Kunckel d'Herculais.—On the influence of the nature of soils on vegetation, by M. G. Raulin.—On the respiration of cells in the interior of masses of tissue, by M. H. Devaux.—Influence of the hygrometric state of the air on the position and functions of the leaves of mosses, by M. E. Bastit.—On the siliceous clays of the Paris basin, by M. A. de Lapparent. The formation of *glaçons-gâteaux*, by M. F. A. Forel. These ice formations are said by the author to be "les *pan-cakes* des Anglais."—Remarks on the temperature of Marseilles, by M. J. Léotard.

Stockholm.

Royal Academy of Sciences, January 14.—Further notices on the molecular weight of the earth (oxide) of gadolinite, by Baron A. E. Nordenskiöld.—On the structure of the transversely striated muscular fibre, by Prof. G. Retzius.—The altitude of the clouds measured in the mountains of Jemtland during the summer of 1887, by Messrs. Hagström and Falk.—Photographs of the spectrum of iron taken with the aid of the Voltaic arc, and one of the "gitters" of Rowland, exhibited by Prof. Hasselberg.—On dinitro-diphenyl-disulphine, i., by Dr. A. Ekbom.—A short relation of a zoological tour to North Greenland during last summer, by Dr. D. Bergendæl.—Some observations on the water of the Gullmar fiord, by Dr. A. Stuxberg.

CONTENTS.

PAGE

THURSDAY, FEBRUARY 19, 1891.

REPTILIA AND BATRACHIA OF BRITISH INDIA.

The Fauna of British India, including Ceylon and Burma. Published under the authority of the Secretary of State for India in Council. Edited by W. T. Blanford. "Reptilia and Batrachia." By George A. Boulenger. (London: Taylor and Francis, 1890.)

MORE than a quarter of a century has passed since Dr. Günther's "Reptiles of British India"[1] appeared. Externally, it has little in common with the present book. Dr. Günther's work is large, and not very handy; but it is adorned with 26 plates, which, as unmatched gems of modern lithography, possess enduring artistic value. Mr. Boulenger's work, which may be taken as a sort of second edition of the older book, is a volume of moderate size, with a number of simply executed woodcuts, which, consisting for the most part of diagrammatic outlines, are intended merely to aid the student in comprehending the structure of the skull and pholidosis. In one respect, however, and that the most important, the two books have a remarkable resemblance to one another. Both trace new paths for the classification of the animals with which they deal.

While the herpetological fauna of the entire southeastern continental part of Asia is treated in Dr. Günther's "Reptiles of British India," Mr. Boulenger limits himself in the present work to British India, with Burma and Ceylon. Nevertheless, the number of species described by Mr. Boulenger is considerably greater than that described by Dr. Günther. The Reptiles described include 3 Crocodilia, 43 Chelonia, 225 Lacertilia, 1 Rhiptoglossa, and 264 Ophidia; the Batrachia include 124 Ecaudata, 1 Caudata, and 5 Apoda.

Looking more closely at the rich contents of the book, we need not say much about the majority of the orders of Reptiles and Batrachia, because the systematic as well as the descriptive treatment of the families, genera, and species does not differ greatly from the results he has set down in his masterly British Museum Catalogues—results which are now the common property of the naturalists of all nations. The only new contribution, corresponding better to present scientific needs, is the separation of the chamæleons from the lizards, and the consequent division of the Squamata into the three sub-orders of the Lacertilia, the Rhiptoglossa, and the Ophidia.

On the other hand, the classification of the snakes, which comprise nearly one-half of the Reptilian species known to occur in India, appears to be completely new; and all the descriptions of families, genera, and species have been prepared expressly for the present work. As no recent publication contains a complete synonymy of the Ophidia, the author explains that somewhat fuller references to the literature of the subject have been rendered necessary than in the other sub-orders of Reptiles and Batrachians. He has abandoned the old primary division of snakes into poisonous and non-poisonous. This division he properly regards as unscientific; "and," he adds, "although adopted almost generally, it is in so far incorrect that a number of forms (Opisthoglypha) usually ranked as harmless are really poisonous, although their bite may be without effect on man and large animals." Experiments recently made by Peracca and Deregibus on Cœlopeltis, and by Vaillant on Dryophis, "have shown that these snakes are poisonous, and that they paralyze their small prey before deglutition." Mr. Boulenger regards it as probable that "all snakes with grooved teeth will prove to be poisonous, to a greater or less degree, as it is clear, *a priori*, that these grooved fangs are not without a function." He has therefore "abandoned this physiological character in dividing the Snakes into families. Poisonous as well as harmless forms are arranged under Colubridæ." The difference between channeled and perforated teeth proves, moreover, to be but one of degree, and the term "perforated" is anatomically incorrect. In both these types the author finds the structure of the teeth essentially the same; they are folded over so as to form a duct to carry the poisonous secretion; when the edges meet and coalesce, a perforated fang is formed; when they merely approach each other, the channeled form results.

The learned author cannot, unfortunately, suggest a criterion by which it might be possible to distinguish at a glance between harmless and poisonous snakes. We wish, however, he had referred to a point which is stil involved in complete darkness. In British India, according to official reports, from 20,000 to 23,000 people die every year from snake-bite (20,067 in 1883; 20,142 in 1885; 22,134 in 1886); whereas in the French possessions in Farther India, and in the Dutch settlements in the Malay Archipelago, and in tropical Southern China, the deaths from this cause do not, in each, exceed ten in the year. Accurate statistics on the subject have been provided, especially by Dutch and German physicians in Java and Hong Kong. As the climatic conditions in all these countries are much the same, and as the frequency of snake-bite is not essentially different in the lands mentioned, and even many of the species (Naja, Bungarus, Trimeresurus) are alike, we have here a problem which has not yet been solved. To the present writer the number of yearly deaths from snake-bite in British India seems to be doubtful, and considerably exaggerated.

About 1500 species of snakes are known. Their systematic arrangement, as is well known, is extremely difficult. Mr. E. D. Cope's recent attempt in this direction[1] was not successful. Zoologists are much more likely to be satisfied with Mr. Boulenger's proposals. He arranges the Ophidia into nine families, viz. Typhlopidæ, Glauconiidæ (Menostomatidæ *olim*), Boidæ (including the Pythoninæ, Chondropythoninæ, and Boinæ) Ilysiidæ (for Ilysia and Cylindrophis), Uropeltidæ, Xenopeltidæ, Colubridæ, Amblycephalidæ, and the poisonous Viperidæ. Of these families and sub-families, those which are for the first time introduced into science are the Chondropythoninæ (Boidæ without præmaxillary teeth, but with a supra-orbital bone) and the Ilysiidæ (snakes with both jaws toothed, with transpalatine and coronoid present; præfrontals forming a suture with

[1] "The Reptiles of British India." By Albert C. L. G. Günther. London: Published for the Ray Society by Robert Hardwicke, 1864. Fol.

[1] "An Analytical Table of the Genera of Snakes." By E. D. Cope. Proc. Amer. Phil. Soc., 1886, pp. 479-499.

nasals; supratemporal small, intercalated in the cranial wall; vestiges of hind limbs), the latter a family which forms a passage from the Boidæ to the Uropeltidæ, agreeing with these in the physiognomy and scaling, with the former in the presence of vestiges of pelvis, whilst the skull is exactly intermediate.

The most interesting part of the book is that in which Mr. Boulenger divides and subdivides the family of Colubridæ. Of this, therefore, we may give a somewhat more detailed account. He divides this large family, containing the bulk of the Ophidia, into three parallel series:—

(1) Aglypha. All the teeth solid, not grooved. Harmless.

(2) Opisthoglypha. One or more of the posterior maxillary teeth grooved. Suspected, or poisonous to a slight degree.

(3) Proteroglypha. Anterior maxillary teeth grooved or perforated. Poisonous.

"In each of these series," says Mr. Boulenger, "we have a more or less perfect repetition of forms, due to adaptation to the various modes of life." Of the series represented in the Indian fauna, the Aglypha include the Colubrinæ and Acrochordina; the Opisthoglypha include the Dipsadinæ and Homalopsinæ; the Proteroglypha include the Elapinæ and Hydrophiinæ, all the former being adapted to terrestrial, all the latter to aquatic life. How deeply classification is affected by the new division here offered by Mr. Boulenger, we may see particularly in his record of the Indian genera of Colubrinæ, which we give, as a specimen, in the natural order:—Calamaria, Xylophis, Trachischium, Blythia, Aspidura, Haplocercus, Lycodon, Hydrophobus, Pseudocyclophis, Polyodontophis, Ablabes, Coronella, Simotes, Oligodon, Lytorhynchus, Zamenis, Zaocys, Coluber, Xenelaphis, Dendrophis, Dendrelaphis, Pseudoxenodon, Tropidonotus, Helicops, and Xenochrophis. Hitherto these genera have been included in the families and sub-families Calamariidæ, Lycodontidæ, Oligodontidæ, Colubridæ (with Coronellinæ, Trimerorhinæ, Colubrinæ, Dryadinæ, and Natricinæ), and Dendrophidæ; at least, therefore, in five different families. In how thorough a way the author has set to work in the limitation of genera also, we see from his combination of the genera Elaphis, Calopeltis, Cynophis, Compsosoma, Spilotes, and Gonyosoma in one great genus Coluber. The green, arboreal species referred to Gonyosoma stand in the same relation to Elaphis and Compsosoma as the green Ablabes (Cyclophis), Dipsas, or Trimeresurus to the other species of those genera. There can be no doubt that the phenomena can be more readily reviewed if these closely related forms are placed in one and the same genus than if genera or even sub-families are assumed, simply because some species have adapted themselves to arboreal or sub-arboreal habits, and have in consequence taken on a green colour. The group Acrochordinæ contains five genera—three with well developed ventral shields, viz. Stoliczkaia, from the Khási Hills; Xenoderma, from Java and Sumatra; and Nothopsis, from the Isthmus of Darien: two without ventrals, viz. Aerochordus, from the Malay Peninsula and Archipelago; and Chersydrus, from India to New Guinea. Mr. Boulenger deals with the Dipsadinæ in the same drastic manner. Of Indian genera, the following are placed by him in this subfamily: Dipsas, Elachistodon, Psammodynastes, Psammophis, Dryophis, and Chrysopelea—genera, therefore, which in the old classification were included in Dipsadidæ, Rhachiodontidæ, Psammophidæ, Dryophidæ, and Dendrophidæ. By devoting more attention than previous investigators to the structure of the skull and the dentition, to the number, form, and length of the teeth, and the form of the pupils, the pholidosis of the head, the structure of the scales, and the colour—to which, however, he does not attribute equal importance as a limiting principle—the author gives, it seems to us, a far better and more comprehensive arrangement than his predecessors. However, it cannot be denied that, satisfactory as the present attempt is, the problem of a systematic division of the *Colubridæ* is still far from being solved.

In the group of sea-snakes, Mr. Boulenger divides more sharply the genera Enhydris, Hydrophis, and Distira. The very differently formed dentition in these genera (in spite of their great resemblance in the form of the head, colour, and mode of life) seems to show that they have sprung from three different terrestrial genera, and that their present resemblances are only the result of their adaptation to their life in the water. Thus Platurus is to be regarded as a Hydrophid which may be traced as a direct descendant of the terrestrial Elapinæ.

Finally, Mr. Boulenger's work enriches science with a considerable number of new genera and species, especially in the sub-order of the Snakes; but space will not permit further reference to details. As the book will be indispensable to every naturalist and every layman who may wish to obtain information about the tropical Indian animal world, the few remarks we have given may suffice. The chief value of a work like this lies in its sharp diagnoses, and in its synoptical tables for genera and species, which enable the observer to determine with confidence the form which may be before him. The fact that the author gives a concise view of geographical distribution, and offers short biological remarks regarding some genera and even species, adds to the scientific importance of the work. For many years this will be the standard book for our knowledge of the Reptiles and Batrachia of India; and its moderate price ought to ensure for it a wide circulation both in India and in Europe. There are still many species about which we know little, and some of Mr. Boulenger's readers ought to be, and no doubt will be, stimulated to provide the desired observations and descriptions. The book should find a place in the luggage of every educated traveller who starts for India.

O. BOETTGER.

EDUCATION IN ALABAMA.

History of Education in Alabama, 1702–1889. By Willis G. Clark. (Washington: Government Printing Office, 1889.)

THIS volume forms one of the admirable series of historical monographs published by the Central Bureau of Education at Washington, and edited under the special supervision of Prof. Herbert Adams, of the Johns Hopkins University at Baltimore. It should be explained that in the United States there is nothing analogous to our Education Department, Congress and the Federal Government having no control or jurisdiction over schools, and each State and city making its own laws and administrating its own educational funds. But Congress established in 1866 at Washington a Central Bureau for the purpose of collecting and publishing statistics; and this Bureau, under the energetic direction of successive Commissioners, has sought to increase its own public usefulness by publishing from time to time valuable works on the history and philosophy of education, and particularly, as various practical problems have presented themselves relating to technical, musical, or physical education, it has gathered together, in pamphlets or circulars of information, the best testimony and opinion which could be obtained on the several subjects.

Prof. Adams's own contributions to the series, notably his "History of the College of William and Mary," and his "Essay on the Study of History in American Colleges and Universities," have been widely read and esteemed in the States; and among the other works included in the series, those of Dr. Bush on "Education in Florida," of Mr. Jones on "Georgia," and of Messrs. Meriwether and Smith on "North and South Carolina," have told with care and fulness the story of educational progress in the Southern States. The present volume relates to Alabama, and is written by Mr. W. G. Clark, of Mobile, who, as sometime President of the University, and as Chairman of the School Committee of the State, is exceptionally qualified for his task, and has been able, with the help of photographs and statistical tables, to present to the reader, not only an interesting historical sketch, but also a full account of the present organization and resources of education in the State. Many of the details in a book of this kind have necessarily a local and personal interest only. Mingled with them, however, are a few facts which may possess significance for English readers. The chief of these relate to the origin and history of the University, the present provision for scientific and technical training, and the general educational condition of the State.

The University was founded in 1821. The general law of the Union—that in all new States one-sixteenth of the public land shall be set apart for schools—dates from 1785, but it was not applied to the Alabama territory till the Act of 1818, which further provided that one entire township should be reserved for the support of a seminary of higher learning. The prudent administration of this large property would doubtless have secured for the State ample provision for learning in all its several departments; but the early history of the University is a melancholy record of mismanagement and neglect. The lands were unwisely sold, sometimes at 8 dollars per acre, the proceeds of the sale were not well invested, accounts were confused, and financial embarrassment followed. To add to these misfortunes, the University appears to have suffered grievously from frequent outbursts of lawlessness and even rebellion on the part of the students. Though the number scarcely exceeded 100 in its best days, and though the annual "output" of graduates averaged little more than 10, the University seems to have striven hard to keep up a high standard of teaching and scholarship, and has been officered from time to time by distinguished men. One is struck, in looking down the list, by observing that, though the University never professed any creed, or sought to maintain a pronounced religious character, many of its presidents and the majority of its professors appear to have been ministers of religion. It is very characteristic of the republican equality which prevails in the United States in regard to religious sects that in 1830, when the buildings of the University were completed, the offer of the principalship was at first made to a Presbyterian divine; that, on his refusal, Dr. Woods, a Baptist clergyman, accepted the office; and that the inaugural services were held in the Episcopal church, the minister of which, with the Governor of the State, took part in the proceedings. Additional buildings, comprising an observatory and a noble library, were added from time to time, but the war of 1861-65 completely interrupted the work of the University, and for a time well nigh ruined it. All the students enrolled themselves in the Confederate army, and after a disastrous struggle, the issue of which is well known, a body of the Federal cavalry, specially despatched for the purpose, set fire to all the public buildings of the University, and reduced them to smouldering heaps of ashes. The librarian, in the hope of changing the purpose of the commanding officer with reference to the destruction of the library, led him thither, unlocked the doors, and showed him the valuable collection of books. "It is a great pity," said the officer, "but my orders are imperative. But I will save one volume, at any rate, as a memento of the occasion." He entered, and seizing a copy of the Koran, withdrew from the building, and ordered it to be set on fire at once. In this way property valued at 300,000 dollars was hopelessly destroyed. It was not till 1870 that other buildings were completed and the College exercises were regularly resumed. As now constituted, the University includes two general departments, of which the first is academical and the second professional. In the former there are ten different schools—Latin, Greek, English, Modern Languages, Chemistry, Geology and Natural History, Natural Philosophy and Astronomy, Mathematics, Philosophy and History, and Engineering. In the department of professional education there are three schools—international and constitutional law, common and statute law, and equity jurisprudence. The rules of the Supreme Court of Alabama authorize the graduates of this department to practice in all the courts of the State, on simple motion, without further examination. The University also provides a special course of instruction under the heads of military art and science, military law and elementary tactics.

Besides the University, Alabama is provided with a remarkable institution of a technical character, called the Agricultural and Mechanical College. It is situated in the town of Auburn, and is endowed with the proceeds of the sale of 240,000 acres of land. Since its foundation in 1872 it has increased in popularity and usefulness, and has become a school of industrial science or a polytechnic institute. Its departments are agriculture and horticulture, mechanic arts, practical chemistry, physics and mineralogy, botany, engineering and surveying, drawing, and military tactics. In the mechanic arts and practical chemistry, its appliances are particularly excellent. There is a wood department in a large hall, provided with a 25 horse-power engine with indicator, planes, circular saw, band saw, scroll saws.

a buzz planer, twenty work benches, with full set of lathes and carpenters' tools required for instruction. A brick building with two large rooms has been constructed specially for instruction in working iron. One of these is equipped with twelve forges, and tools necessary for a forge department; the other with a cupola furnace, moulding benches, and special tools for use in a foundry. The machine is equipped with eight engine lathes with appropriate tools, and a chipping and filing department is arranged, with benches and vices for twelve students. A five horse-power dynamo furnishes light to the mechanic, art laboratory, and other halls, and it is designed to supply the different laboratories with electricity from this source.

Although there are these signs of enterprise, and although the public school system of the State includes elementary schools, high schools for girls, and normal colleges for teachers, Alabama proves, when its educational statistics are examined, to be one of the most backward States in the Union. The latest returns of the United States Commissioner show that the public schools are open on an average only 79 days in the year, and that the average attendance is only 63 per cent. of the number enrolled. The sum annually expended on education is not large, and amounts only to an average of 4 dollars 17 cents per head on the number of scholars. Of the total school revenue, 20 per cent, is derived from land and real estate, 55 per cent. from State taxation, and 24 per cent. from poll taxes, licenses, and other local imposts. Special provision was made in 1867 establishing separate schools for coloured children, who are numerous and increasing in number in this as in most of the Southern States. The last returns show a total of 212,821 coloured children of school age, of whom 98,919 are enrolled in the public schools set apart for them, while 66,424 are on an average in daily attendance during the school year. The year, however, is one of 67 days only. The average cost to the State of the coloured pupils is 2 dollars 10 cents.

These particulars sufficiently indicate the very exceptional conditions under which education is carried on in one of the most important of the South Central States of America, and will show to any student of the subject how much of interesting material has been accumulated in Mr. Clark's well arranged and well illustrated book.

CHEMISTRY FOR BEGINNERS.

Elementary Systematic Chemistry for the Use of Schools and Colleges. By William Ramsay, Ph.D., F.R.S. (London: J. and A. Churchill, 1891.)

AMONG the large number of text-books of elementary chemistry written for the use of schools, this volume will take its place with the very few that are not specially designed to prepare the student for some examination. It is further to be distinguished as having the classification and order of treatment of the elements based upon their periodic arrangement. The author deserves the thanks of those interested in the matter for making this experiment, whether the result be regarded as successful or otherwise. He states in the preface that he hopes "that the method of treating the subject of chemistry adopted in this short sketch may help to demonstrate the value of its study as a training in classification, and as a means of developing the reasoning powers." Certainly this is desirable, and the volume tends in this direction. We find, for example, the preparation of the elements treated under such headings as "Electrolysis of a Compound," "Decomposition of a Compound by Heat," &c., and there is much advantage in comparing the processes that serve for the isolation of the elements in this manner. But the first aim of a knowledge of chemistry is not to develop the reasoning powers. It is rather to give the student more exact ideas than he would otherwise have of the constitution and properties of matter, and it is important to get into his mind not simply the relationships that substances bear to one another, but a definite idea of each substance and its properties. We think that an inspection of the volume before us will confirm the experience of the past that an elementary text-book does well to aim, above all things, at presenting a distinct, and, as far as may be, complete description of each substance, leaving the classification of methods and reactions to the judgment of the teacher or the ingenuity of the student when it is impossible to include both aspects of the matter. Taking oxygen as an example, we are in the habit of finding a chapter in which the methods that serve for its preparation, and its more important properties are set forth in a connected and convenient manner. But in this volume the directions for the preparation of oxygen occupy a few paragraphs in a chapter entitled "Decomposition of a Compound by Heat," at p. 73, while the properties of oxygen are not described till we get to p. 93. To say the least of it, it would be very inconvenient for a student, after having prepared his oxygen, to busy himself with the preparation of sulphur from pyrites, the preparation of magnesium, aluminium, boron, silicon, hydrogen, several metals, the properties of hydrogen and about seventeen metals, besides boron, carbon, nitrogen, phosphorus, and arsenic, before he comes to a practical investigation of the properties of his oxygen.

The author has endeavoured to treat first of the elements, and subsequently of compounds. But elements and compounds are interdependent, and it is obvious that such a separation is practically impossible. We think it is unwise to attempt it, unless, indeed, chemistry is to be degraded into a mere "means of developing the reasoning powers." If education is for this purpose only, the student of a foreign language would do well to devote himself entirely to its grammar, and leave the vocabulary for those who desire knowledge for its own sake.

The difficulty of finding a good beginning place in the teaching of any science has been obvious to all those who have undertaken the task. If children are to be taught, they must be led by slow degrees from what they know to an expanded and fuller knowledge of the matters they are already acquainted with, and so onwards. The periodic law is beyond the grasp of a young student, and the only valid reason that should allow his course of work to be guided by it would be that the subject was thereby rendered more agreeable or more intelligible to him. It does not appear that the periodic law will be able to fulfil this new function.

Looking at the book more in detail, we notice that 60 pages are devoted to chemical physics, 144 pages to inorganic chemistry, and 141 to organic chemistry. The student who is already fairly acquainted with the subject will find the summaries of properties, &c., of much use. The analogous compounds of various groups of elements are treated together, and their differences are concisely remarked upon. He will, perhaps, be a little troubled at first to find names employed in an unusual manner. For instance, Ag_2O is called argentous oxide; AgO, argentic oxide; AgCl, AgBr, silver chloride, &c.; and there is a statement that "no *argentic* halides are known." If he looks up the paragraph on valency, he will find silver put down as a monad along with hydrogen, potassium, &c., and not with copper and mercury, which are marked monad and dyad. There are other inconsistencies that might have been eliminated, such as in the use of the words distill, and sublime. Directions are given to distill a fluoride with strong sulphuric acid, to distill a mixture of sand, fluor-spar, and sulphuric acid, and so on, and on turning to the description of the operation of distillation, in the first part of the book, we find that the word is used there in its correct sense. The solution of a carbonate in an acid is spoken of, and the sublimation of a mixture of mercuric sulphate and salt. Such operations are impossible according to the meaning of the words expressing them given in the earlier part of the work. The statement that chlorine peroxide is formed by distilling a chlorate with sulphuric acid at a low temperature might lead to serious accidents in the case of the young students for whom the volume is primarily intended. While the symbol Aq. is used to represent the water of solution, a refinement which is scarcely necessary in an elementary treatise, the reaction taking place when a jar of chlorine is inverted over a strong solution of ammonia is represented by an equation that demonstrates the liberation of hydrochloric acid. It is such inconsistencies as these that perplex and mislead the student.

C. J.

OUR BOOK SHELF.

Applied Geography. By J. Scott Keltie. (London: George Philip and Son, 1890.)

THE greater part of this volume consists of four lectures which were delivered last year at the Bankers' Institute, and given in part before the Society of Arts and the College of Preceptors. An article which appeared originally in the *Contemporary Review* is also included. The volume is one of great interest, both theoretical and practical, and ought to be of genuine service to various classes of readers, by showing how high is the place which must properly belong to geography in any complete scheme of education. Mr. Keltie has been especially successful in indicating some of the influences which geographical facts have exerted on the movements of races and the evolution of nations. On so great a subject it was impossible for him, within narrow limits, to develop any of his ideas fully; but his suggestions are excellent, and may perhaps be worked out by some of his readers for themselves. Mr. Keltie has much to say about the bearings of geographical conditions on the development of Africa, and about the relation of geography to the commercial prosperity of the British Empire. He also discusses various problems connected with the actual and possible geographical distribution of some of the common commodities of commerce. On all these subjects he writes with freshness and lucidity, displaying a thorough grasp of the principles of geographical science. Maps and diagrams, for which the author expresses his thanks to Mr. Ravenstein, add considerably to the value of the text.

The Autobiography of the Earth. By the Rev. H. N. Hutchinson, B.A., F.G.S. Pp. 290. (London: Edward Stanford, 1890.)

WE have rarely met with a popular work on geology or natural history so crowded with information as Mr. Hutchinson's little volume now before us. In less than 300 pages of large type, all the leading features of the "geological record" are passed in review, with special reference to the mode of reasoning by which the various facts and inferences are established. Nothing of importance seems to be omitted, from the nebular hypothesis and the birth of the moon, to theories of glacial epochs, the permanence of ocean basins, the origin of oolitic structure, and the method of discovering the hidden appendages of trilobites. Some of the sections, indeed, such as those relating to the nebular theory and the nature of geological agents, are seldom found more concisely arranged even in the pages of an examination "cram-book."

With all this wealth of material, however, the author succeeds in completely divesting his work of the "dry" and "uninteresting" formality which, in his opinion, repels the general reader from ordinary text-books. The several chapters are pleasantly written and illustrated with occasional woodcuts; while, although the accounts of the successive formations are systematically arranged from the Archæan to the Pleistocene, every occasion that permits of some digression into general principles is turned to good advantage An allusion to the igneous rocks of the Devonian period thus leads to a few brief paragraphs on the interest of denuded volcanoes; the Lower Carboniferous rocks afford an opportunity for discussing the origin of limestones; the chapter on the Oolitic rocks is followed by another on the organization of Mesozoic reptiles; and the short notice of the Glacial period forms a fitting occasion for digressing to the subject of ice as a geological agent. If, in any respect, the general reader's interest fails, it will be due to the too frequent use of inadequately defined technical terms, and the too rapid succession of unfamiliar ideas. Of such terms as "schist" and "Neocomian," for example, the definition seems to have been quite overlooked.

In a compilation of so wide a scope it is, of course, easy for a specialist to detect inaccuracies, but the present volume bears evidence of so much care bestowed in its preparation, that absolute errors appear to be unusually few. The bold statement on an early page that glaciers erode valleys like rivers is qualified in the last chapter; and the assertion that no Chelonian bones have been found in the Trias is corrected in a footnote on a later page. The most serious error is the frequent quotation of brachiopods and polyzoa as Mollusca; and nearly all the lesser misstatements relate to biological matters. The author, however, unlike many popular writers, has been trained in the modern school, and thus recognizes throughout the broad principles of organic evolution.

The volume is well printed, and issued in a style uniform with Miss Arabella Buckley's popular works, to which it forms a worthy companion.

A. S. W.

Spinning Tops. "Romance of Science Series." By Prof. John Perry, F.R.S. (London: Society for Promoting Christian Knowledge, 1890.)

IN September last Prof. Perry delivered the "Operatives' Lecture" of the British Association meeting held at Leeds, the subject of which was entitled "Spinning Tops." The present little work is a revised and much expanded edition of that lecture, and, instead of the moving apparatus originally displayed, the author has provided a very approximate equivalent in the elaborate illustrations and descriptions. He begins by showing the rigidity of flexible bodies when in motion: he then describes the behaviour of a common top by means of a balanced gyrostat, and explains the importance of giving a rotary motion to projectiles. The gymbal gyroscope and the curious movements that are connected with it are afterwards discussed, and the analogy of these movements to that of the earth in regard to precession, &c., is pointed out.

The next point referred to is the importance of balancing a rotating body, the author showing that for perfect balance not only must the axis of rotation pass through the centre of mass of the body, but it must be one of the three principal axes through the centre of mass of that body. Reference is made to the experiments of Prof. Milne, who has shown, by means of a modified seismograph, the want of balance in the wheels of locomotives.

In the last few pages Prof. Perry deals with the connection between light and magnetism and the behaviour of spinning tops, and among the experiments is Thomson's mechanical illustration of Faraday's rotation of the plane of polarization, which is performed by means of a number of double gyrostats placed in a line and connected by india-rubber joints, each instrument being supported at its centre of gravity, and capable of rotation in the horizontal and vertical planes. To many readers the book will be one of great interest, and a brief summary at the end will show them clearly the line of argument adopted throughout.

Wild Life on a Tidal Water. By P. H. Emerson. (London: Sampson Low, 1890.)

IT seems rather odd that an author should talk about "wild life" when he means simply life on a house-boat on Breydon Water in Norfolk. No one, however, who glances over the present volume will be disposed to criticize the title very severely, for Mr. Emerson has the art of describing even unimportant things in a way that makes them interesting. Above all, he has provided a series of thirty admirable "photo-etchings," which convey a wonderfully vivid impression of the various scenes reproduced. The photographic plates were taken by Mr. Emerson himself, but in selection of subject the majority are the result of a pleasant partnership with his friend Mr. T. F. Goodall, on whose house-boat he experienced the trials and delights of "wild life."

Arcana Fairfaxiana. With an Introduction by George Weddell. (Newcastle-on-Tyne: Mawson, Swan, and Morgan, 1890.)

THE manuscript of which a facsimile is presented in this volume was found some years ago by Mr. Weddell in a box of lumber. It contains much apothecaries' lore and housewifery dating from the first half of the seventeenth century, and was used, and partly written, by the Fairfax family. In a carefully-written introduction Mr. Weddell gives all necessary information about his treasure and about the persons with whom it has been associated. The facsimile is skilfully printed, and many of the medical receipts, and some of the instructions as to the baking of meats, are very curious and interesting.

Berge's Complete Natural History. Edited by R. F. Crawford. (London: Dean and Son, 1890.)

IN this volume some of the leading facts relating to the animal, the vegetable, and the mineral kingdoms are brought together. The descriptions, if not very interesting, are clear, and the reader is helped to understand them by means of no fewer than sixteen coloured plates, and over three hundred smaller 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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Bursting of a Pressure-Gauge.

WITH reference to Mr. Smith's letter in your issue of February 5 (p. 318) respecting the bursting of a pressure-gauge, will you allow us to point out a very simple method of preventing any serious consequences from such an accident, which must occasionally occur as the gauges wear out? There should be no cast-iron in the gauge: the tube and works should be mounted on a brass or gun-metal frame. The glass covering the dial should be mounted in a ring, fitting on the body of the gauge like a cap; when the gauge is in use this cap should be removed, thus avoiding all danger from the broken pieces of glass. The gauge should then be inclosed in a brass wire cage, so that, should the tube burst, any portions of metal would be caught by the wire network, and, if not stopped altogether, would at any rate be rendered harmless.

Our *employés* are constantly using gauges for testing gas bottles, and we have always had our gauges made in this way to avoid any risk from accident. The screw valve of the bottle should not be turned on full, one complete revolution of the screw is quite sufficient; this greatly minimizes the risk of fusion caused by friction, as the cylinder would probably take a quarter of an hour to empty itself. Of course the same gauge should never be used for both oxygen and hydrogen cylinders.

3 Fleet Street, February 11. NEWTON AND CO.

Modern Views of Electricity.

(1) THE first question that I raised in my former letter was how oxygen atoms come by the negative electricity which according to Dr. Lodge they have. I find in his pamphlet, "Seat of E.M.F." (p. 50), his view stated as follows, that whatever may be the case with molecules of oxygen, at least all dissociated atoms have a certain definite negative charge. If this be so, it seems to me to follow necessarily either that all molecules of oxygen are negatively charged, or that only those which are so charged can undergo dissociation.

(2) We are then to suppose a crowd of dissociated atoms of oxygen, all having negative electrification, "straining at," *i.e.* attracted by, the zinc. And this attraction, we are told (p. 50), exists independently of actual combination between zinc and oxygen, although it has its origin in the desire for such combination. It is a mechanical force arising from chemical affinity. Such a force would, according to the kinetic theory of gases, increase the density, and therefore the pressure, of oxygen in the neighbourhood of the zinc, causing a repulsive force, in addition, as it seems to me, to the repulsion due to like electrical charges. By this means the state of the gas in the neighbourhood of the isolated zinc would become one of equilibrium. And it may be that the variations of density would take place only within a distance from the zinc too small for our means of measurement. According to the statement (p. 110) of "Modern Views," only the repulsive forces arising from electrification are relied upon as producing equilibrium. I think, however, that Dr. Lodge would not exclude other mutual repulsive forces which may exist between the dissociated atoms of oxygen. And such other forces appear to me to be necessary, in order to explain satisfactorily the phenomena which ensue when the zinc is brought into contact with copper.

(3) When contact is made, a positive charge passes from the copper to the zinc. Let us call it σ per unit of surface. That disturbs the equilibrium previously attained about the zinc, because it introduces a new force of attraction on the oxygen atoms in virtue of their negative electrification. This force will be $2\pi\sigma$ per unit of surface. In order that there may again be equilibrium, we must introduce a counteracting force $-2\pi\sigma$. Now the new attractive force calls in a fresh influx of oxygen atoms, increasing their density, and therefore increasing the negative charges per unit of surface of zinc. If we had no repulsive forces, except those arising from electrification, the charges on these newly imported atoms would, I think, have to be $-\sigma$ per unit of area of the zinc, in order to give the required force $-2\pi\sigma$. This would exactly neutralize the positive charge

$+\sigma$ derived from the copper, and so would reduce the zinc to its original potential. Under these circumstances statical equilibrium could, as it appears to me, never be attained. But if there exist repulsive forces between dissociated atoms of oxygen other than those due to their negative electrification, we may obtain our repulsive force $-2\pi\sigma$ without introducing so many atoms as that their negative charges, referred to unit of surface of the zinc, shall be $-\sigma$. It may be $-\sigma'$, where σ' is less in absolute value than σ. And so we should, on making contact with copper, obtain a total charge on the zinc $\sigma - \sigma'$ per unit of surface. This is positive. The zinc may then be raised to the same potential with the copper, and at the same time the attractive and repulsive forces on dissociated oxygen atoms near the zinc may be in equilibrium. So it seems to me that, assuming the electrification of oxygen and its attraction by the zinc to be as stated by Dr. Lodge, the theory consistently explains phenomena up to this point, and may explain also the variations due to moisture of the zinc or other conditions.

(4) As regards the "intrinsic step" of potential relied upon by Dr. Lodge to explain the aluminium needle experiment, I cannot so easily follow him. Consider the surface of the isolated zinc, and let C be a point just so distant from it that the potential at C is sensibly unaffected by the presence of the zinc with its negative charges, and those of its attendant oxygen atoms. C is then at zero potential, that of the zinc being $-1·8$. Intermediate points are at intermediate potentials. When, on making contact, you introduce the positive electricity from the copper, you raise the potential of the zinc by half a volt. But the potential at C is, *primâ facie* at all events, no more affected by the positive than it was by the original negative electrification of the zinc, and remains zero. You have diminished the "step" by half a volt. As before, intermediate points would have intermediate potentials. In fact, so far as the potential at C is concerned, may we not suppose the newly introduced positive electricity simply to neutralize an equal quantity of the negative electricity previously found on the zinc?

S. H. BURBURY.

ON this head I would say that a point outside the surface-film, beyond the molecular range, is naturally unaffected by the chemical affinities of the surface; but it is by no means therefore uninfluenced by the ordinary dielectric strain of a static charge imparted to the zinc in any adventitious manner. Such a charge alters the potential of the whole neighbourhood, but does not alter the slope of potential existing in the surface-film. Nothing can alter that but a modification of the surface or of the adjacent medium.

Thus I state my position briefly in order that the sole remaining divergence of view between Mr. Burbury and myself may likewise disappear.

OLIVER J. LODGE.

PERHAPS the following slight elaboration of Dr. Lodge's views on the electrical condition of air films in contact with metals will commend itself to Mr. Burbury. It seems to me that by its means the difficulty of realizing the source of the negative charge on the oxygen atoms is to a certain extent got over without thereby forfeiting any of the essential features of the air film theory.

Assuming the truth of this theory, the fact to be explained is that when clean zinc is put into pure oxygen it becomes coated with a film of that substance which is at a higher potential than the zinc by 1·8 volt.

If the film be regarded as a conductor, this step of potential has to be brought about in the first instance by combination of a few zinc atoms with oxygen, so that the film becomes coated with + electricity on both its surfaces; the field set up by this and a corresponding − charge on the surface of the metal underneath representing the 1·8 volt step of potential between the two. It is at this point that the difficulty of the − charge on the oxygen atoms arises, as they have just been assumed to be +; and, also, this other smaller difficulty, that the + and − charges on the opposed film and metal surfaces, corresponding to 1·8 volt and at molecular distance apart, are such that every atom in each surface is charged with a quantity of electricity which is of the same order of magnitude as its electrolytic charge; a fact which necessitates the previous combination of every atom in the zinc surface with oxygen from the film if the ordinary laws of electrolysis are to be assumed; whereas according to the film theory, wholesale oxidation of the metal surface destroys the Volta effect altogether.

If, however, the film be regarded as a non-conductor, both these difficulties vanish.

The molecules which form the film would be gaseous except for the presence of the metal. The latter holds them to itself, and may be looked upon as, in a sense, polarizing them into chains or rows normal to its own surface. Supposing such chains to consist each of what were originally three distinct diatomic gas molecules, A_1A_2, B_1B_2, C_1C_2, with a metal atom M next to A_1, there will be a tendency to combination between A_1 and M, which will slacken the

$$M-A_1\equiv A_2-B_1\equiv B_2-C_1\equiv C_2-$$

bonds between A_1 and A_2, leaving A_2 partially free to hold on to B_1, which again will promote a holding on between B_2 and C_1, so that C_2 is partially uncombined. The bonds between A_2 and B_1, B_2 and C_1, represent in fact the cohesion of the film (or part of it), and are the result simply of the tendency to combination between M and A_1. If this be very great, actual combination may occur, which will set free A_2B_1 and B_2C_1 as gas molecules, leaving C_2 to find another partner. On the other hand, the tendency to combination between M and A_1 may be too small to do more than polarize the molecules, and this is perhaps what occurs with zinc in dry oxygen.

If, now, the essence of combination between zinc and oxygen is that the zinc atom is + and the oxygen −, we may assume, without entering on ultimate problems, that the row of molecules is polarized *electrically* as well as mechanically. $A_1B_1C_1$ all −; and $MA_2B_2C_2$ +; so that the film may be looked on as a dielectric plate, with a coating of metal on one side only, and a + charge on the other. The slope of potential between film and metal (*i.e.* the Volta effect) occurs now, not between their surfaces of contact, but right through the film, the 1·8 volt existing between the *outside* surface of the latter and the metal. No actual chemical combination is necessary to bring it about; and, indeed, any combination at all is prevented by the internal affinities in the film itself just described.

A. P. CHATTOCK.

University College, Bristol.

Pectination.

REFERRING to Mr. E. B. Titchener's letters in NATURE (see December 4, p. 103, and January 15, p. 248), I am able to confirm the view that pectinated claws are used by the birds possessing them for the purpose of scratching themselves.

On December 9 last I shot a cormorant, and found the fissures between the teeth of one of its pectinated claws choked up with fragments of down. This down corresponds with the bird's own down, and there can be little doubt that it is the bird's own down, and that it became thus situated through the claw being used for the above-mentioned purpose. I may also mention that I have since found minute fragments of feather in the claw of a barn owl.

Many birds use the middle claw to scratch themselves with, as Mr. Titchener remarks; and this claw appears to be very generally modified for the purpose. The modification consists in this, that the inner edge of the claw is bent out, and developed into a curved blade running along the inner side of the claw. Such a blade is well developed in guillemots and razorbills. It may also be easily seen in wild duck, teal, gulls (some at any rate), oyster catchers, golden plover, starlings, fieldfares, redwings, larks, and many others. In some birds the modification is very slight, but in all I have been able to examine (not an extensive collection) it seems to exist in some degree. In the divers, the claws are so flattened that the inner edge naturally forms a scraper, and the same may be said of other birds, such as partridges and pheasants.

Pectination is only a further modification of this blade, or inner edge of the claw, in that it becomes divided up by notches or fissures, placed at more or less regular intervals, into a comb-like structure. As is known, the middle claw is not pectinated in young nightjars. I have now in my possession a young, though almost full-grown male of this bird, and it has the middle claw provided with a well-developed blade, but there is no trace of pectination. I may mention too, that I have found the edge of the blade in a guillemot, slightly indented here and there, thus offering an approach, though but a very distant one, towards pectination.

The pectinated claw, then, should not be regarded as a structure peculiar to nightjars, owls, herons, cormorants, and gannets, and different from anything found in any other bird, but merely as a highly modified form of a structure found in a less modified form in many birds. Presumably these structures serve to rid the birds of troublesome parasites. If this is correct, it would be interesting to learn whether the birds possessed of pectinated claws are particularly liable to the attacks of hurtful parasites, or whether we may consider that in them only have variations in the direction of pectination arisen.

Treborth, Bangor, January 24. H. R. Davies.

Mr. Davies' confirmation of my results is very interesting, and in many ways, I think, conclusive. So far as my memory serves me (I have not here access to a collection), I can bear out all his facts. I regret that I have never taken the names of the different species in which I have observed the blade. The Pomarine Skua is one. A friend tells me that he has also noticed a jagged blade; and, he believes, in a guillemot.

It would appear that the list of pectinated birds given by Owen ("Anat. and Physiol. of Vertebrates," ii. 232) is too short. Indeed, we may hope to find many links between the blade and the serration. Mr. F. E. Beddard, in a paper on *Photodilus badius*—an owl considered by some ornithologists to be very near *Strix* (*Ibis*, 1890)—writes: "The claw is, however, produced laterally into a knife-edge, as in other owls. . . . I have examined an example of *Strix*, in which the jagged edge of the toe in question was very inconspicuous; and the question arises, whether it does not occasionally disappear altogether."

As regards the question of vermin, Owen says that each species of pectinated bird is infested by its peculiar louse (*Nirmus*). According to Hudson, the herons are especially free from vermin;[1] though the (roseate) spoonbill, which also has the pectination, is infested with them ("Argentine Ornithology," ii.). This author does not think that the herons could ever rid the entire plumage of vermin by means of the claw. It is curious that the herons were always in a miserable condition; the spoonbills plump and healthy.

Audubon once shot a frigate-bird which was scratching its head; and, on examining the pectination with a glass, found the racks of the comb crammed with the insects which occur on the bird's head, and especially about the ears. He also observed that the pectinated claws of birds of this kind were much longer, flatter, and more comb-like than those of any other species with which he was acquainted. He gives the tropic-bird (also a *Steganopod*) as having a knife-edge.

I am unable to say whether certain members of a genus are, as a rule, more infested by vermin than others.

E. B. Titchener.

Inselstrasse 13, Leipzig, February 11.

[1] The fact that my bird scratched himself immediately after a meal may be in point here.

On the Affinities of Hesperornis.

In Dr. Shufeldt's letter (Nature, Dec. 25, 1890, p. 176) no mention whatever is made of Prof. Fürbringer's studies on the point in question, and they appear, moreover, to have been partially misunderstood by Prof. Thompson. Prof. Fürbringer published his "Untersuchungen zur Morphologie und Systematik der Vögel" (2 vols. in folio; Amsterdam) in May 1888, and I beg to reproduce some of his results as to the family *Hesperornithidæ*.

Like Prof. Marsh, Prof. Fürbringer sees Ratite characters in the configuration of sternum, breast-girdle, and anterior extremity, but, in opposition to that author, finds nothing of a specifically Ratite description in the remaining parts of the skeleton. On the other hand, these parts—especially the pelvis and hinder extremity—correspond, as shown in detail, decidedly with the type *Colymbidæ*, *Podicipidæ*, and *Enaliornithidæ*. Herein lies the clue to the systematic position of *Hesperornis*.

Particular attention has, further, been paid to the dentition, on account of which Prof. Marsh has grouped under the S.C. *Odontornithes* the *Hesperornithidæ*, as O. *Odontolcæ*, with the *Ichthyornithidæ* (O. *Odontotormæ*), and with the *Archæopterygidæ* (O. *Saururæ*). In this Prof. Fürbringer does not follow him, but maintains that, in all probability, all ancestral ornithological forms possessed toothed jaws, and, consequently, that the dentition is of as little decisive genealogical importance in birds as in mammals, and that the three orders of toothed birds mentioned belong to completely different ornithological types, of which the Ratite *Hesperornithes* stand much nearer to the *Colymbo-Podicipites*, and the Carinate *Ichthyornithes* to the *Laro-Limicolæ*, than they do to one another.

The condition of the sternum, however, whether Ratite or Carinate, may not afford a point of more weighty genealogical significance. According to Prof. Fürbringer, the better-known *Ratitæ* form a perfectly artificial group—a medley of once Carinate birds sprung from the most dissimilar genealogical branches, which now possess nothing in common further than the purely secondary point of analogy that, with the advancing development of the hind-limb and increasing bulk, they have lost the power of flight. The representative forms of the so-named S.C. *Ratitæ* are as far, if not further, removed from one another, as are those of the S.C. *Carinatæ*, though, between this or that division of either S.C., certain points of affinity of a non-intimate nature are to be found. These are closely examined in Prof. Fürbringer's work.

After having set aside the higher taxonomic significance of the dental and sternal characters, there remains for Prof. Fürbringer only the decided agreement of the skeleton of the *Hesperornithidæ* with that of the *Colymbidæ*, *Podicipidæ*, and *Enaliornithidæ* as of true genealogical worth. The relations of these divisions are of a truly genetic description, but it is impossible to derive the *Colymbo-Podicipites* from the *Hesperornithes*, which were differentiated already in the Cretaceous period in the most one-sided manner. We might with better right trace the latter to some very ancient *Colymbo-Podicipite* form, though the safest course to follow is to regard both as independent branches of a common bough.

The avian system drawn up in chapter vi of Prof. Fürbringer's work, and represented in the accompanying genealogical trees (Plates xxvii.–xxx.), is based upon these considerations. Neither *Odontornithes* and *Anodontornithes* (*Euornithes*), nor *Ratitæ* (*Platycoracoideæ*) and *Carinatæ* (*Acrocoracoideæ*), are mentioned as genealogical divisions, but a S.O. *Podicipitiformes* is formed out of the *gentes Enaliornithes* (F. *Enaliornithidæ*), *Hesperornithes* (F. *Hesperornithidæ*), and *Colymbo-Podicipites* (F. *Colymbidæ* and F. *Podicipitidæ*).

All this proves that the penetrative researches and observations of Prof. Fürbringer on the position of *Hesperornis* had raised him no less than two and a half years ago in the chief question to the point attained by Prof. Thompson and Dr. Shufeldt in 1890. The latter writers differ from him only in that Prof. Thompson ascribes to *Hesperornis* as intimate a relationship to the *Colymbi* as, for instance, that of *Stringops* to the *Psittaci*; while Dr. Shufeldt holds it possible that the *Colymbi* are descended from *Hesperornis*. As is shown in a recent paper on the subject (cf. *Ornitholog. Monatschr. d. Deutsch. Ver. z. Schutze d. Vogelwelt*, 1890, No. 18), Prof. Fürbringer is unable to share these taxonomic views, but abides by the opinion maintained by him in 1888. F. Helm.

Royal Zoological Museum, Dresden, January 29.

Destruction of Fish by Frost.

I think it follows from the second clause in the question at the end of my letter of January 26 (p. 295) that I assume want of air to be the cause of the destruction of the fish in the Regent's Canal. Cases like that which Mr. Hill mentions (p. 345) have been so familiar to me from boyhood that it did not occur to me to be more explicit. Moreover, I did not say that the effect of this agent of destruction would often be "on a scale visible to the geological eye." This it is of which I was thinking: occasionally fossil fish are very numerous in a particular stratum. Various causes for this apparently sudden destruction have been suggested; it occurred to me that this one sometimes might have to be considered among the possible contingencies. Though, as he says, a slight flow of fresh water is "seldom wanting in any natural body of water," yet fish may be killed (as I have seen) in a pond through which a streamlet runs. The supply must be equal to the demand. Now the volume of streams in certain cases is greatly reduced in winter: the Reuss above Wasen on November 28, 1889, was a very different river from that which we have seen in summer. If, then, a lake were frozen, and the amount of water which entered it greatly reduced, the conditions of a pond might possibly be imitated during an exceptionally long winter, or even a river become almost like a canal. This remark applies especially to freshwater deposits, but as long frosts are sometimes followed by floods, the dead fish might be carried for a considerable distance. T. G. Bonney.

BABYLONIAN ASTRONOMY AND CHRONOLOGY.

BABYLONIAN astronomy has been investigated during the last year successfully by the Rev. Joseph Epping and the Rev. J. N. Strassmaier, S.J., who have explained and annotated two Babylonian calendars of the years 123 B.C. and 111 B.C. in their publication "Astronomisches aus Babylon oder das Wissen der Chaldæer über den gestirnten Himmel" (Freiburg, Herder, 1889). They have succeeded in giving a satisfactory account of the Babylonian calculation of the new and full moon, and have for the first time identified by calculations the Babylonian names of the planets, and of the 12 zodiacal signs and twenty-eight normal stars which correspond to some extent to the 28 *nakshatras* of the Hindoos. In the following passages, translated from their book, we give the general results they have obtained, but for many interesting details we must refer the reader to the work itself.

Astronomical Summary.

In discussing the condition of the astronomy of the Babylonians we must rely upon the wider knowledge which we have now attained in regard to their acquaintance with celestial bodies. In this respect the Cuneiform Inscriptions before us furnish much that is new: other already more familiar material is confirmed through them and receives a documentary foundation. Before entering into details it will be well to call to mind how much of the Babylonian astronomy was previously historically established. The reader need not fear that we shall lay before him one by one the conclusions of authors who have written about the Babylonians with more or less trustworthiness, or that we shall subject their works to the light of criticism; we are fortunately spared this trouble. Rudolf Wolf, utilizing all sources bearing upon the subject, has made the most thorough studies, and has accomplished the task in so satisfactory a manner in his well-known "History of Astronomy,"[1] that we may accept his judgment with the fullest confidence. He sums up his opinion of the Babylonians shortly in the following words: "It is beyond doubt that, as early as the time of the philosopher Thales, the Chinese and Babylonians possessed observations extending over several centuries of the most striking celestial phenomena, and that through them their attention had been directed to the periodical return of corresponding eclipses after a cycle of 223 moons, or 18 years and 11 days, which they called Saros, and employed for making predictions." In the discussion of the "Most ancient views upon the system of the world" (p. 23) he says further: "While the Babylonians, Chinese, and Egyptians contented themselves with collecting isolated experiences, fixing certain periods, &c., and scarcely a trace of any scientific system whatever is to be found in their work, the ancient Greeks struck out an entirely different line."

Speaking of the Metonic cycle, a period of nineteen years, at the expiration of which the lunar phases return partially to the same hour, but sometimes with a difference of twelve hours, Wolf mentions indeed the Indians and Chinese, but not the Babylonians; to the latter he simply denies the knowledge of precession, and the difference between the sidereal and tropical years. The same scholar says, referring to the Zodiac[2]:—"To which ancient people the priority in this respect belongs, whether to Indians, Chaldæans, Chinese, or Egyptians, &c., we are to this hour ignorant, in spite of all that has been suggested, and of very extensive researches; and we shall perhaps never know, since all dates are too uncertain, and all representations too crude and inexact; and we are equally far from ascertaining from which people this knowledge, even though with some transformation, passed over to the Greeks. The Zodiac was certainly not invented by the Greeks, who at first possessed only eleven signs because, through a misunderstanding of what came to them from abroad, they threw Libra and the claws of Scorpio together." R. Wolf's work records nothing of the knowledge of the Babylonians about the courses of the planets; the scanty mention of them is too obscure and general.

[1] "Geschichte der Wissenschaften in Deutschland: Neuere Zeit. Geschichte der Astronomie," von Rudolf Wolf (Munich, 1877).

[2] *l.c.*, p. 188.

So far the Cuneiform Inscriptions have not lifted the veil of darkness, but in one direction, with respect to observations, they have brought greater clearness. According to Dr. Kaulen,[1] the Babylonians had numerous observatories throughout the country which were specially fitted up for watching, and the observations must have been made regularly, for the astronomers had to send in their records at appointed times. Dr. Oppert[2] has examined and interpreted several astronomical reports of the Assyrians, and a few passages from one of them (the text is published in Rawlinson's "Cuneiform Inscriptions," vol. iii. plate 59, n. 9) are given here:—

"To the King, my lord, thy faithful servant, Mar-Istar."

"On the 27th day the moon disappeared. On the 28th, 29th, and 30th days we observed the moon's node of the eclipse of the sun: the time passed by: no eclipse occurred."

"On the first day, as the new moon's day of the month Thammuz declined, the moon was again visible over the planet Mercury, as I had already predicted to my master the King. I erred not."

"In the hour (kaš-su-ut) of Anu (Saturn[3]) she appeared at setting, in the circle of Regulus (chief of the heavenly host), but her track was not discernible in the mist of the horizon."

Even if observations of this kind were intended to serve astrological ends, they must also have helped to prepare the ground for astronomy, and that this ground was really trodden by the Chaldæans we have strong evidence in the tables which we have interpreted.

Before considering this point more closely, it might be well to answer the question, When does the recognition of a succession of natural phenomena become scientific?

Regular observation, with its corresponding record of such phenomena, certainly belongs to science, yet is not science in itself, but merely the necessary primary condition: science only begins when amongst these various forms the natural law comes to light. If the law discovered have such a firm and fixed form that it can be submitted, as it were, to a practical test, we have then to record a truly scientific victory. This practical test can, as a rule, be conducted in various ways according to the nature of the phenomena. If we are in a position to call into existence the conditions on which these phenomena depend, although on a smaller scale, then the law which has been discovered may be experimentally verified. Of course we must renounce the idea of experiment with regard to our celestial phenomena; but in its place another no less certain method is at our command. For instance, if the law of a planet's motion be discovered and completely ascertained, it must be possible to determine beforehand whereabouts in the ecliptic that planet will be found on a certain day.

We learn that the Babylonians applied these tests, and, considering the period, they accomplished them in a brilliant manner. With regard to the moon they were able to declare, not only the day of crescent moon, but also the time of her visibility on that evening, as well as the duration of visibility on the day of her disappearance. Their data for the risings and settings of the full moon are most satisfactory. As to eclipses, they did not con-

[1] *l.c.*, p. 172.

[2] "Die astronomischen Angaben der assyrischen Keilinschriften," von J. Oppert (1885).

[3] Dr. Oppert takes Anu for Saturn; we have proved that *An* is Mars.

fine themselves to their so-called Chaldæan period, for through this alone it would have been impossible to give the real visibility of an eclipse of the moon[1]; but the Babylonians determined the visibility, the hour, and the magnitude of the eclipse. They only once decidedly failed, when they announced an eclipse which never happened, or would certainly not have been visible in Babylon. Here, however, we may raise the question whether this failure should be attributed to the method employed or to an accidental miscalculation. The latter is the most likely, as in other data they are only guilty of comparatively small errors. They were well up in the paths of the planets. Their data for the heliacal risings and settings, for opposition, retrogression, and especially for the position of certain fixed stars, are within a few degrees of the reality. We can produce nothing similar from any other people of antiquity. We must, therefore, not neglect to rectify an error which, since the time of Biot, has appeared in books of history.

R. Wolf also says in his "History,"[2] speaking of stellar co-ordinates:—"Even the ancient Chinese must have observed the culmination times of the stars by the help of their water-clocks, and, according to Biot, 28 stars distributed in the circle of the heavens (*i.e.*, in the ecliptic) served their purpose: they compared these stars again and again with each other as firm points of reference from which to determine the positions of the remaining stars, and especially of the planets. By the help of this practice—unchanged from time immemorial—they derived the periods of revolution of the sun, the moon, and the planets with great accuracy, ascertained the periods which bring these bodies into conjunction with, or opposition to, each other, &c. On the other hand, the Chaldeans, like the ancient Greeks, observed the horizon almost exclusively."

It was not the Chinese but the Chaldæans to whom the whole merit should have been given in the above quotation, and they have further the credit of having erected for themselves, with the help of these normal stars, a scientific edifice of astronomy which suggested to them the means of announcing, for future times also, the places of the planets almost to a day. It is remarkable that here exactly 28 normal stars are spoken of, a number corresponding to their 28 constellations; yet it is unlikely that the Babylonians should not have also selected one or another star in the neighbourhood of the ecliptic, in Aquarius, or in the beginning of Pisces, even if of lesser magnitude, the more so as they made use of all twelve signs of the Zodiac for determining the positions of the planets at their heliacal rising and setting.

It might appear striking that the observations of the sun are scanty, and that even the direct ones appear somewhat faulty. As an instance, the autumnal equinox is correct with regard to the position of the sun, while the vernal equinox and the summer and winter solstices are not correctly indicated astronomically. We have already remarked that this deviation was probably intentional, in order to divide the year into equal parts. Moreover, the Chaldæans must have been very familiar with the path of the sun, since that was the first condition of being able to indicate accurately, as they did, the constellations for the planets and the Sirius phenomena. These two tables do not admit of any positive conclusions as to the accurate knowledge of the tropical year and the difference between it and the sidereal year, or of the retrogression of the vernal point: we ought to possess others more remote by some centuries than the above-mentioned ones.

Though we may only answer it by conjecture, we cannot dismiss the question which forces itself upon us here—namely, How did the Babylonians contrive to foretell the positions of the moon and the planets? They must have had some sort of theory concerning the movement of the moon, for we find a completely developed mechanism of calculation through which they developed the new moon from the preceding one, so that they were able to fix the time of crescent moon and its first quarter. Unfortunately we are unable to submit these calculations to a practical test because we do not know to what year they refer: but we can see from the whole process, and from the results of our three tables for the years 188, 189, and 201 S.E., that they accomplished what was possible at that time.[1]

With regard to the planets we find ourselves in a somewhat difficult position. The beginning of an explanation is, however, offered by another class of tables which contain the positions of single planets in detached sections. We have before us two of these tables, the copies of which Father Strassmaier had the kindness to transmit to us. The first (now published in the *Assyriological Journal* of Dr. Bezold, vol. v. p. 341 ff., marked R[m] 678) is from the Rassám collection in the British Museum, and contains all the planets, but each for a different year, the other was acquired last year by the American expedition of Pennsylvania University through Prof. Harper. The British table, which we have examined to some extent, contains, especially in the new copy, sufficiently clearly Jupiter (even for two years), Venus, Mercury, and Saturn—of course with few data; on the other hand, the data for Mars are for the most part damaged, but the year is visible with sufficient data to allow of its verification.

The deciphering of both tables of ephemerides first justified the wish to elucidate the matter, for the knowledge of these alone afforded a well-founded hope of revealing the partially analogous text of the others. Before attempting such a task, however, it should be established why, in the case of these last, different years for the different planets should be found together on the same tablet. This important association must have had some object beyond the scope of the tablet, because the positions of the planets are by no means in accordance. It was not far to the thought that, because the planets also have periods in their apparent paths, all data together should build up the foundation for the ephemerides of a coming year. Upon this supposition it was not difficult to determine the single years. Venus, for instance, had a period of 8 years, at the expiration of which the same apparent positions returned approximately. We will now place before the reader the years which are given for the individual planets (according to the era of the Seleucidæ), and below them the periods corresponding to those planets; which then, added to the number of the year, must in each case give the same year of that era, if the above view be correct.

Venus	228	Mercury	190	Jupiter	224	Saturn	177
Period	8	Period	46	Period	12	Period	59
	236		236		236		236

The agreement is so striking that no doubt the planetary data of these years have been used for the ephemerides of the year 236 S.E. How the transition was effected can only be recognized if for instance the ephemerides of the year 236 S.E. should be discovered.[2]

The American tablet is similarly constructed. On it the data for Saturn and Mars are completely broken off, but it contains a few more lunar dates, with the addition of the year 225 S.E., for which the combination is calculated. Jupiter is represented in this and the English one

[1] The lunar eclipse of the 2nd of August, 123 B.C. (*i.e.* −122), was 18 years before so small (magnitude 0·1 digit), that it can hardly be considered as having happened at all, the more so as the time was unfavourable for Babylon, being broad daylight.

[2] *l.c.*, p. 155.

[1] Compare J. Epping's interesting article "Die babylonische Berechnung des Neumondes" ("The Babylonian Calculation of the New Moon") in the *Stimmen aus Maria-Laach*, vol. xxxix., September 1890.

[2] In the meantime, these ephemerides of the year 236 S.E. have been found in the treasures of the British Museum, and the Rev. Jos. Epping is engaged in making the necessary calculations.

by two whole years, the one corresponding to the period 12, the other to one of 83 years, where above in the English tablet the number of the year is broken off.

A preliminary investigation makes it appear probable that we have to deal with the results of observation in both tablets. The longitudes calculated for Venus, for instance, according to the lists of dates given in the tables, differ as a rule in minutes only from the longitude of the annexed normal star, and differ always least when the latter is not far from the ecliptic, and consequently the agreement in longitude would be easiest to observe. It is further remarkable that in these cases the negative difference between ♀—✳ is the predominant one, as though the results which we receive from Le Verrier's tables for the geocentric positions of Venus were about 10′ too little. This stands out still more clearly in the case of the more scantily represented constellations of Mercury; here the negative difference rises to 1°.

The above-mentioned table of the 7th year of Cambyses (published in the *Assyriological Journal* of Dr. Bezold, vol. v. p. 281; and by Dr. Oppert, "Un Annuaire Astronomique Chaldéen, utilisé par Ptolémée," *Comptes rendus des Séances de l'Académie des Sciences*, tome cxi., séance du 17 novembre 1890; cf. Almagest, book v. ch. 14) contains no constellations at all of planets with fixed stars, but only data for the relative[1] positions of the planets, and may accordingly bring us further knowledge of their relative position at that time. This table is of greater importance for the path of the moon. The data for the time of eclipses are of less value, since their accuracy goes only to a half or a third *kas-bu* (= double-hour).

On the other hand, the data for the rising and setting of the full moon in a given case are to be estimated at a higher value. We have in this tablet clear and distinct data, for at least ten months, as to how many degrees of time before sunset the moon rose (1° = 4m.). As further subdivisions to one-sixth of a degree are given, and the observation and measurement of the slightest difference in time at the time of full moon hardly admit an error of more than 1m., the position of the moon with regard to the sun may be determined with an unusual degree of accuracy. This might raise the hope that the work of the Chaldæans might benefit even our advanced age. In the meantime it may suffice to have won again for the old astronomers the place of honour in science which was accorded to them in ancient times.

Chronological Summary.

The learned chronologist Ideler gives a convincing testimony[2] to the uncertainty which still reigned in the first half of our century with regard to the Babylonian chronology. He says, "Nowhere do we find peculiarly Chaldæan months, and in no author do we find the years reckoned according to a Chaldæan era. We shall therefore only be able to answer conjecturally the question, Of what nature was the Chaldæan chronology?"

As much may have been made clear in this domain since Ideler's time, it will be interesting to bring together the results which have been rendered certain through the interpretation of the Cuneiform Inscriptions.

First, the era of the Seleucidæ has acquired a firmness such as appertained to hardly any other before the Christian era. The data given in the tables for the years 189 and 201 S.E., indicate a unanimity of astronomical phenomena which can only belong to the years −122 and −110, and indeed with such an exclusiveness as would hardly allow of the recurrence of such a combination within a period to be measured by thousands of years. The Seleucidæan era is combined with the Arsacidæan in these tables, so that we are now able to determine both with equal certainty.

We have

−122 = 125 Arsac. E. = 189 S.E.

therefore

−246 = 1 ,, = 65 ,,

and

−310 = 1 ,,

Ideler distinguished a double Seleucidæan era, a Chaldæan, and a Syro-Macedonian[1]; he made the former begin in the autumn −310, the latter in autumn −311. Dr. Eduard Mahler[2] also places the beginning of the latter in the autumn of the year −311. If we put aside Ideler's first statement, then both scholars agree with our results, but the beginning of the year is to be placed six months later in the tables in question; therefore 1 S.E. = −310. The difference in the beginning of the year may be further explained through other inscriptions. Father Strassmaier has published some Arsacidæan inscriptions in the *Assyriological Journal* (vol. iii.), and amongst these the 11th (p. 137) begins—"Sanat 170 kan Di-mit-ri-su arah Adaru," &c. Then follows "Arah Airu 14 na," without the year's number suffering any change; therefore the year 170 of Demetrius began before the month Adar, with that of Thischri—that is to say, in the autumn. The table contains a horoscope, and gives constellations for the night of the 6th Adar which suit so exclusively for the 28th February −141, that they could hardly appear again in their entirety for 200 years before or after. It follows of itself that the year of the horoscope has not been reckoned from the beginning of the reign of one of the known Demetrius. As, on the other hand, 170 S.E. is −141 (that is, 142 B.C.), it follows that the tablet is dated according to the Seleucidæan era, but with the beginning of the year in the autumn, for which reason, perhaps, Demetrius is inserted as a distinguishing point.

Perhaps the first-named view of Ideler, that a Chaldæan era existed which began exactly a year later than the Syro-Macedonian, should not be dismissed simply with a wave of the hand. Certainly, no direct evidence can be produced in support of it, but we can advocate the grounds of probability. Father Strassmaier, speaking of the fourth of the above-mentioned inscriptions,[3] remarks: "We learn with certainty from the double dates of the inscription that the year of the Arsacidæan era began with the month Thischri, while the Seleucidæan began with the month Nisan: for it is only in this way that the above figures can be understood; the month Tebeth of the year 152 is the year 216, and the month Thammuz of the year 152 is the year 217 of the Seleucidæan era." Seeing, then, that there is the same difference—64—between 152 and 216 as in the data of our first planetary table—125 equal to 189—in the case of the new year being given with Nisan at the head, 124 equal to 189 should be written. Hence it follows that the Chaldæans made the Arsacidæan era begin half a year later than the Seleucidæan. It is not impossible that the Chaldæans, in order to bring both years into harmony, put off the beginning of the year from spring to the following autumn, and this becomes probable in case of Ideler's view having positive foundation.

Two difficulties which Dr. Oppert[4] advances against the Seleucidæan and Arsacidæan eras are removed by our tables. Firstly, he agrees with Dr. Mahler, and consequently with us, that the Seleucidæan era begins with the year −311, but holds that this era can only be supposed in question when the name of Seleukos is recorded. This objection is instantly refuted by the

[1] The calculations prove that these dates are based on observations; here Mars is called Ni-bat-a-nu, and Jupiter Sag-me-ša, or Sig-me-sa.

[2] "Handbuch der mathematischen und technischen Chronologie," von Dr. Ludwig Ideler, vol. i. p. 202.

[1] *l.c.*, pp. 223 and 224.

[2] "Chronologische Vergleichungstabellen," 1 Heft, von Dr. Eduard Mahler.

[3] *Zeitschrift für Assyriologie*, vol. iii. p. 132.

[4] *Comptes rendus des séances de l'Académie*, tome cvii. pp. 467 and 468.

tables before us, and also by two others, No. 9 and No. 11, published by Father Strassmaier in the *Assyriological Journal*, for in them the era is not named after Seleukos. Besides this, there exist some unpublished tables in which the same thing occurs; and yet, as above, the years are certainly reckoned according to the Seleucidæan era. Dr. Oppert feels justified in assuming further that the Arsacidæan era begins with

1 Arsac. E. = −255 in the autumn.

He applies it in the translation of the already-mentioned Strassmaier inscription, No. 9, which contains the course of an indicated eclipse [1] of the moon. Yet if we look at the year's data in this table, we shall easily find that they agree with our own. The text runs as follows:—"Šanat 168 kan ša ši-i Šanat 232 kan Ar-ša-ka-a." It is, as we see, completely analogous to those of 189 and 201 S.E., and the difference 232 − 168 is again exactly 64. Accordingly, in this table (No. 11) apart from its contents, the eras are none other than the Seleucidæan and the Arsacidæan.

The determination of the eras in use in Babylon at the time of the Macedonian rulers, will always be fraught with some difficulties which may be more easily and confidently disposed of if astronomical data are dated according to such an era.

The years in our Seleucidæan era are so-called "bound" [2] lunar years. We know that the Babylonians had lunar months—some of 30, some of 29 days—whose number was not determined according to the mean new moon, as was the case with the Greeks, but was reckoned from the crescent moon, in close connection with the real new moon. If they wished, with regard to the number of months in the year, to remain in harmony with the solar year, they would have been obliged to fix on an average in 11 years—7 years with 12 months, and 4 with 13 months.

Like the Jews to the present day, they had intercalary months, but without intercalary days in addition. The lunar year of the Chaldæans was so vigorously defended by Fréret that even Ideler, who preferred to attribute the Egyptian year to the Babylonians, could not do otherwise than pronounce Fréret's hypothesis probable. Yet here he differed from Fréret's view in that he could not bring himself to attribute to the Chaldæans an "unbound" year—that is to say, a year of 12 lunar months. He did not dispute the *saros*, *neros*, and *sossos* [3] of the Babylonians, but he denied that these great periods were the only means through which a connection with the solar year could be restored. Ideler was, if one ascribes lunar months to the Babylonians, absolutely in favour of the "bound" lunar year. In this he has remained perfectly right. The Babylonians had real intercalary months, as we have seen, in the case of the year 189 S.E. The many inscriptions which Father Strassmaier has published testify for other years. We refer the reader to his "Nabonidus," in which the 1st, 3rd, 6th, 12th, and 15th years had a second Adar, and the 10th had further a second Elul.

The fact is therefore established, but what was the nature of the intercalation? Had the Babylonians a fixed law for it, or did they allow a choice to be made in the single years within certain periods, for instance of eleven years? The latter appears so far the most probable, for, in the event of a fixed law, two intercalary years could hardly ever follow each other, and yet we do find such cases in the communications of Father Strassmaier, although very rare and perhaps uncertain: the years 2 and 3 of Cyrus, and 11 and 12 (?) of Darius were intercalary. Dr. Oppert is also of the opinion that the intercalation has been very arbitrarily accomplished. It would seem as though the Babylonians must have found themselves in no less a dilemma from the putting back of the date some few decennaries, unless official lists of all successive months and years were strictly kept. Even if this be assumed, Ideler's objection still subsists,[1] "that they must have had a well organized chronology: otherwise how could the Greek astronomers, to whom their observations served as a foundation for a theory of the moon, have accepted their data with so much confidence?" Certainly Ideler's view, which in his time was the most general, that the Chaldæans had no era of their own, but used the Egyptian, or at best reckoned according to the moon for civil life, is, as we have seen, falling into disfavour. Our tables are of an astronomical nature, and only permit an intimation of Egyptian chronology, in the Sirius phenomena, to glance through.

Moreover, the above-mentioned opinion requires but a slight modification of form in order to appear not only acceptable, but probable. We might be led to suppose that the astronomers made use of a double chronology, one which had its foundation in the movement of the moon, another which was in accordance with the path of the sun. Otherwise how could they have determined beforehand, within a few degrees, the exact position of the planets, even of Mercury? They must have been very familiar with the length of the solar year, and probably referred their calculations first of all to a solar year of some organized form in order to transfer the results afterwards to the lunar year.

Unfortunately we possess no documents which might afford us a positive insight into the matter. With regard to the beginning of the year we have already shown that the Seleucidæan era, as it is presented in our three tables, began the year with the month Nisan, therefore in spring. It would appear that in this an old tradition was held to, for in the inscriptions published by Father Strassmaier from the reigning years of Nabonidus and Nabuchodonosor we have an accession year [2] in each case.

In the first the year extends from Sivan beyond Thischri to Adar; in the latter Thammuz [3] is evidently represented, including Thischri and Kislev, so there is full evidence that Thischri, and consequently the autumn, did not form the beginning of the year. If, then, founding the commencement of the Babylonian year [4] in the month Nisan, we postpone it to spring, we cannot well go too far.

The transference to Thischri or autumn was probably a consequence of the Macedonian rule, for the Macedonians celebrated the beginning of the year in the autumn, and, previously to the Arsacidæan era there is no inscription known which places the new year in Thischri.

If we knew the beginning of the year of all the eras in use in those times, the exact determination of date of any data might still be a matter of difficulty, because the beginning point [5] in a "bound" lunar year is always uncertain. In the first table (189 S.E.) the 1st of Nisan fell on March 25; in the other one, of 201 S.E., on April 10; and in others the variation may be much greater.

[1] A translation of this description of the lunar eclipse is to be found in the *Assyriological Journal*, vol. iv. p. 76, which still holds good against the objections of Dr. Oppert, *l.c.*, p 174.

[2] The lunar year is *bound*, when it is kept in harmony with the solar year by intercalation; *unbound*, when it contains always only 12 months, so that the beginnings of the lunar and solar year coincide only after long periods, as in the Muhammadan era.

[3] There are many different opinions about these periods: some say *saros* = 3600, *neros* = 600, and *sossos* = 60 years; others only so many days; again, some think, and perhaps with greater probability, that *saros* signifies the Chaldæan period of 18 years and 11 days, but then they are uncertain about the meaning of *neros* and *sossos*.

[1] *l.c.*, p. 202.

[2] An accession year contained the rest of the months of the current year, whenever the beginning of the reign of a new king fell in the current year.

[3] Compare the inscriptions of Nabopalassar in the *Assyriological Journal*, vol. iv. p. 121, n. 19.

[4] Dr. Oppert, in his "Chronologie Biblique," pp. 11 and 12, puts the beginning of the Assyrian year in general in Nisan, but for weighty reasons he fixes the beginning of the eponym year in Thischri or in the autumn: "les éponymies vont de Thischri à Elul, et non de Nisan à Adar."

[5] E. von Herdtl, in his "Astronomische Beiträge zur assyrischen Chronologie," p. 43, thinks that the beginning of the Assyrian year falls on the first new moon before the vernal equinox.

It is only when astronomical data are combined with Babylonian dates that we can hope to determine the corresponding Julian date.

Another similar question which chronology has to answer is, whether the change of date was reckoned according to a point of time, or according to the civil day. According to the evidence brought forward by Ideler, the ancients were convinced generally that the Babylonians reckoned the day from one sunrise till the next, and therefore made the change of date take place in the morning. Ideler finds a difficulty here, in that such a supposition would hardly be compatible with a chronology depending on the changes of the moon. We must allow that he is right, and we think we have sufficiently proved that the first day of each month coincided with the first visibility of the crescent moon. Whether the Babylonians were first prompted by the Macedonians, at the outset of the Seleucidæan era, to put off the date till evening, cannot be learnt from the documents, but it is not probable, if we disregard Pliny, Censorinus, &c., for they reckoned in lunar months earlier. If we honour these historical statements as we are bound to do, there is yet a kind of explanation to be offered.

We have seen that the Babylonians referred their calculations for the new moon to midnight, so that astronomically speaking the change of date was accomplished at this hour, so it is not impossible that they did not forestall the civil day, but let it succeed the astronomical. This is nothing but a conjecture, in order not to reject the authority of the ancients.

The last chronological element upon which we have to speak is the division of the day. It is generally accepted that the Babylonians cut up the day into twenty-four hours—twelve day and twelve night hours. There is so much evidence here that we cannot doubt the fact. Our tables show another division which was in use among astronomers. It is completely demonstrated in the calculation tables, where the whole day is split up into six divisions, and each division into sixty subdivisions, so that the whole day falls, like the circle, into 360 parts, of which one equals four of our minutes. For the sake of calculation, the subdivision is carried on to sixty, and yet another sixty. The application of this method of calculation shows itself also in the ephemerides, first of all to express the duration of visibility with regard to the moon, and also to declare the time when, according to calculation, an eclipse should happen. It is remarkable that we have here two starting-points, sunrise and sunset; while in each case it is announced how many degrees of time before or after these terms the eclipse will happen. The extreme points, therefore, for both then touched at midday or midnight.

TEMPERATURE IN THE GLACIAL EPOCH.

THE late long frost has naturally suggested the question, What permanent fall of temperature would produce a recurrence of the Glacial epoch? It is a question not easily answered, for it is like a problem complicated by too many independent variables. It is not enough for us to ascertain the actual temperature of a district in order to determine whether it will be permanently occupied by snow and ice. There are regions where the ground, a short distance below the surface, is always frozen to a depth of several yards at least; and yet glaciers do not occur, even among the hills, because the amount of precipitation is so small that the summer rapidly dissipates what the winter has collected. There are other regions partly covered by ice though their mean annual temperature is distinctly above the freezing-point; as where glaciers descend to the sea from hilly districts, of which a considerable area lies above the snow-line, and on which there is much precipitation. In the case of Great Britain, at least, a further difficulty enters into the problem—namely, that much controversy still prevails as to the interpretation of the symbols upon which our inferences in regard to the temperature of these islands during the Glacial epoch must depend. Some authorities would concede no more than that the highland districts of Scotland, Wales, and England were enveloped in snow and ice, and the glaciers, whether confluent or not, extended from their feet for a few leagues over the lowlands—say, to some part of the coast of Lancashire and of Northumberland; while others desire to envelop a large part of the British Isles in one vast winding-sheet of ice, a corner of which even rested on the brow of Muswell Hill, above the valley of the Thames. The one school regards the Boulder Clay of England as a deposit mainly submarine, the product of coast-ice and floating ice in various forms; the other attributes it exclusively or almost exclusively to the action of land-ice. Into this thorny question we do not propose to enter. The approximation which we shall attempt—and it can only be a rough one—can be easily modified to suit the requirements of either party.

We will assume throughout that the annual isothermal of 32° coincides with the line of permanent snow. This, obviously, is an assumption; often, owing to small precipitation, it will be found to be erroneous, but we take it as the only simple approximation, for, under favourable circumstances, masses of ice may protrude beyond it.

The question, then, may be put in this form. Assuming a sufficient amount of precipitation, what changes of temperature are required in order to bring within the isothermal of 32° regions which are generally admitted to have been occupied by land-ice during some part of the Glacial epoch?

First, in regard to the British Isles. All will admit that in many places the Cumbrian and Cambrian glaciers descended to the present sea-level. The mean temperature of the Thames Valley near London is 50° F. This isotherm cuts the Welsh coast a little east of Bangor. Obviously, the whole region north of this line has a lower mean temperature, no part of the British Isles, however, being below 45°. Hence a general fall of 18° would give a temperature of 32° at most in the Thames Valley and on the shores of North Wales (except on the extreme west), while on the coasts further north the temperatures would range down to 27°. What would be the effect of this? Switzerland may enable us to return an answer. The snow-line in the Bernese Oberland may be placed roughly at 8000 feet above the sea, but it is obvious that the chief feeding-ground of the Alpine glaciers lies rather higher up in the mountains. In the case of such glaciers as the Great Aletsch, or the Aar, the lowest gaps in their upper basins are rather above 10,000 feet, while the surrounding peaks range, roughly, from 12,000 to 14,000 feet, though but few exceed 13,000 feet. Thus the feeding-ground of the Oberland glaciers may be regarded as equivalent to a mountain district the sky-line of which ranges from rather above 2000 to 5000 feet. In reality, however, not very much of it exceeds 4000 feet above the snow-line. This, indeed, rather overstates the case. We find practically that the effective feeding-ground, that which gives birth to glaciers, which protrude for some distance below their supply basins, may be placed about 1000 feet above the ordinary snow-line; so that the glacier-generating region of Switzerland may be regarded as equivalent to a mountain district with passes about 1500 feet, and peaks not often exceeding 3000 feet. It follows, then, that if the temperature at the sea-coast in North Wales were 32°, the whole of the Scotch Highlands, and a large part of the Cumbrian and Cambrian Hills would become effective feeding-grounds, and the glaciers would be able to descend into the plains. In the Alps, the larger glaciers terminate at present at altitudes of from 4000 to 5500 feet (approximately); that is, they descend on an average about 4000 feet below the effective feeding-ground, or 3000 feet below the snow-line. If the temperature of Bangor were not higher than 32°, then the Snowdonian district would be comparable with one of the Alpine regions where the mountains rise generally from about 1000 to 3000 feet above the snow-line; that is, with such a one as the head of the Maderanerthal, where none of the peaks reach 12,000 feet above the sea. Here the Hüfi Glacier leads to passes rather below 10,000, among peaks of about 11,000 feet in altitude, and it terminates a little above 5000 feet. That is to say, a region, rising roughly from 2000 to 3000 feet above the snow-line, generates a glacier which descends more than 2000 feet below it.

But what change is required to give a Glacial epoch to Switzerland? It is generally agreed that an ice-sheet has enveloped the whole of the lowland region between the Alps and the Jura. Let us assume that, other conditions remaining the same, this could occur if the mean annual temperature of this lowland were reduced to 32°. Its present mean temperature varies somewhat; for instance, it is 45°·86 at St. Gall, 49°·64 at Lausanne. Let us take 47°·5 as an average, which is very nearly the mean temperature of Lucerne.[1] So this lowland requires a fall of 15°·5. We may take the average height of the region as 1500 feet above the sea. If, then, we begin the effective gathering ground at 1000 feet higher, the valley of the Reuss from well below Wasen, and the valley of the Rhone from a little above Brieg, would be buried beneath *névé*. So that probably a fall of 16° would suffice to cover the lowland with an ice-sheet, and possibly bring its margin once more up to the Pierre-à-bot above Neuchâtel; at any rate, a fall of 18° would fully suffice, for then the mean temperature of Geneva would be slightly below 32°.

The line of 41° passes through Scandinavia a little north of Bergen; if, then, the climate of Norway were lowered by the same amount, which also is that suggested for Britain, the temperature at this part of the coast would be 23°, corresponding with the present temperature of Greenland rather south of Godhavn; and probably no part of Norway would then have a higher mean temperature than 26°.

The wants of North America are less rather than greater; though, as geologists affirm, an ice-sheet formerly buried all the region of the Great Lakes and descended at one place some fifty leagues south of the fortieth parallel of latitude. Its boundary was irregular; but if we strike a rough average, it may be taken as approximately corresponding with the present isotherm of 50°. The temperatures, however, in North America fall rather rapidly as we proceed northwards. Montreal is very nearly on the isotherm of 45°, and this passes through the upper part of Lakes Huron and Michigan; that of 39° runs nearly through Quebec and across the middle of Superior, while at Port Arthur, on the same lake, the temperature is only 36°·2. If, then, we assume sufficient precipitation, the maximum fall of temperature required for this North American ice-sheet will be 18°; but less would probably suffice, for the district north of the St. Lawrence would be a favourable gathering ground. This would be brought within the isotherm of 32° by a fall of 12° or at most of 13°.

It seems, then, that if we assume the distribution of temperature in the northern hemisphere to have been nearly the same as at present, we require it to have been lowered, at any rate in the regions named, by about 18° in order to bring back a Glacial epoch. For North Wales a reduction of about 20° might be needed, but if the isotherms ran more nearly east and west, 18° for the Thames Valley might suffice. If we assume the great extension of glaciers in Central and North-Western Europe to be contemporaneous with that in America, we must suppose that these parts of the northern hemisphere had a climate more nearly resembling, but even colder than, that which now prevails in the southern hemisphere. The isotherm of 40° runs a little to the south of Cape Horn; that of 45° passes north of the Straits of Magellan. The latter lie on parallels of latitude corresponding with those of North Wales, but their mean temperature is about 8° lower. If we could restrict ourselves to the British Isles, it would be enough to assume a different distribution of temperature from that which now prevails on the globe, for at the present time, and in the northern hemisphere, the isotherm of 32° twice comes down very nearly to the latitude of London; but it may be doubted whether this alone would account for the great extension of the Alpine glaciers, and the difficulties seem yet greater in the case of North America. Here, where even at present the temperature is rather abnormally low, we have to make a very considerable reduction. But this is too wide a question to discuss at the end of an article in these pages. We seem, however, fairly warranted in concluding that, whatever may have been the cause, a lowering of temperature amounting to 18°, if only the other conditions either remained constant or became more favourable to the accumulation of snow and ice, would suffice to give us back the Glacial epoch. T. G. Bonney.

[1] St. Gall, 45°·86 F.; Berne, 46°·58; Lucerne, 47°·48; Zurich, 48°·20; Neuchâtel, 48°·74; Geneva, 49°·46; Lausanne, 49°·64. St. Gall and Berne are rather high stations, the one being 2165 feet, the other 1760 feet. The lake of Lucerne is 1437 feet above the sea.

SURVEYING AND LEVELLING INSTRUMENTS.[1]

This volume fills a gap that has been long felt in consequence of the great dearth of good books treating of instruments used for surveying and levelling. Various books and pamphlets contain descriptions and methods of using instruments of this class, but there is no work in which each instrument is so completely treated as in the present volume.

The author's former work, which has been found to be a most useful help and guide, and is now in its sixth edition, was limited to drawing instruments, among which were those for plan drawing and calculation of areas. The present work is intended to complete the subject by describing theoretically and practically the different instruments that are required and used at the present day. Not only does the author give an excellent and complete description of each typical instrument, but in many cases he shows the methods of use adopted in the field; thus placing before us a good and trustworthy text-book.

Of the instruments treated therein, one is surprised at the many and various kinds that are in use. Among some of the less common instruments we may mention a tacheometer, our illustration (Fig. 1) showing the author's latest pattern of this instrument. As regards its general appearance, it differs very slightly from a theodolite, but, when closely examined, it will be found that the graduation of the arcs and circles is made upon the centesimal system, the circle reading 400 grades, and reading to 0·01 grade by a micrometer or vernier. The compass is of the cylindrical form, and is inside the small telescope placed below the horizontal circle, and is read by a very ingenious method. The telescope is made of a much larger size and of higher power than those generally employed in theodolites. To facilitate calculation, a logarithmic slide-rule forms part of the equipment of this instrument. A very neat and ingenious mining survey transit, the result of various improvements suggested by the author, is illustrated in Fig. 2. It should be found of

[1] "Surveying and Levelling Instruments." By William Ford Stanley. (London: E. and F. N. Spon, 1890.)

the utmost value when used in close working, for the circles are so arranged that they can be very easily read, the horizontal circle being so adjusted that the reading can be taken when the instrument is near the roof of a mine. The telescope has a wide field of view, and on it are

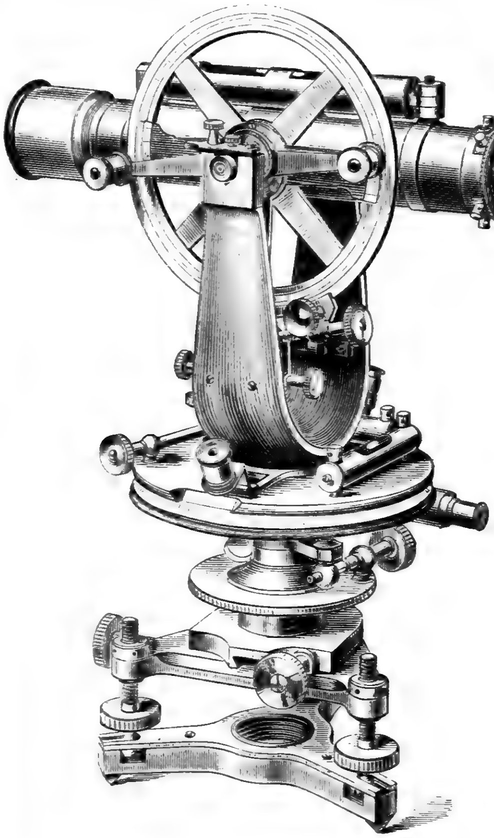


FIG. 1.

placed two pairs of sights made on a new principle for roughly sighting an object or station, or for use in difficult positions.

Another very compact little instrument which has been improved by the author is the box sextant with a continuous arc of 240°.

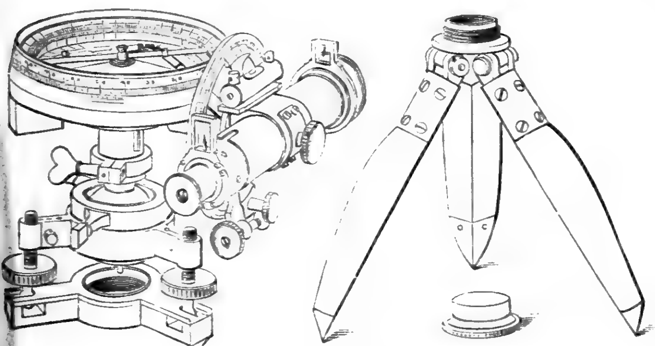


FIG. 2.

Among the miscellaneous instruments described at the end is mentioned a portable saw made of hardened steel plates riveted together in double series, and slack enough to allow the rivets to form joints. This saw "is equal to a 6-foot cross-cut saw, weight complete $2\frac{1}{4}$ pounds, including case, which measures over all $1\frac{3}{4}$ by 4 by 8 inches. Two men with this saw may cut a tree down, 12 inches in diameter, in about 10 minutes."

We need only say in conclusion that the work is deserving of the highest praise, and will be found invaluable to surveyors and others whose work makes it necessary for them to use or study instruments of this kind. For besides laying before the students of surveying or mining the principles and methods of use of each instrument of its class, and also the best tests for assuring the qualities of each, it provides information which will be of much service to skilful instrument makers. W.

PROFESSOR SOPHIE KOVALEVSKY.

THE Swedish papers bring us the sad news of the death of the lady-Professor of Mathematics at the Stockholm University, Mme. Sophie Kovalevsky. She spent her Christmas holidays in the south of France, returned to Stockholm on February 4, and began her course of lectures on the 6th. On the evening of that day she felt ill, and on the 10th she died of an attack of pleurisy. She was born in 1853 at Moscow, and spent her early childhood in a small town of West Russia, where her father, the general of artillery Corvin-Krukowski, was staying at that time; and afterwards on her father's estate in the same part of Russia. She received her first instruction from her father, but it seems that it was her maternal uncle, an engineer of some renown, Schubert, who awakened in her an interest in natural science. She early lost both her mother and her father, and, having ardent sympathy with the movement which was spreading among Russian youth, she applied for, and at last obtained, permission to study at St. Petersburg. The next year—that is, in 1869, when she was but sixteen years old—she was received as a student at the Heidelberg University, and began the study of higher mathematics. About this time, when extremely young, she married Kovalevsky, the well-known Moscow Professor of Palæontology. From 1871 to 1874, she was again in Germany, this time at Berlin, studying mathematics under Weierstrass; and at the age of twenty-one, she received the degree of Doctor of Philosophy at Göttingen. Her husband died in 1883, and the next year, in June, she was offered the chair of higher mathematical analysis at the Stockholm Högskola, on condition that she should lecture during the first year in German, and afterwards in Swedish. This she did, and most successfully too—some of her Swedish pupils already being professors themselves. Her chief mathematical papers were: "On the Theory of Partial Differential Equations" (in *Journal für Mathematik*, 1874, vol. lxxx.): "On the Reduction of a class of Abel's Integrals of the Third Degree into Elliptical Integrals" (in *Acta Mathematica*, 1884, vol. iv.)—both being connected with the researches of Weierstrass; "On the Transmission of Light in a Crystalline Medium" (first in the Swedish *Förhandlingar*, and next in the *Comptes rendus*, 1884, vol. xcviii.), being part of a larger work in which Mme. Kovalevsky shows the means of integrating some partial differential equations which play an important part in optics; and "On a Particular Case of the Problem of Rotation of a Heavy Body around a Fixed Point" (in the *Mémoires* of the Paris Academy: *Savants étrangers*, vol. xxxi., 1888). The third of these works received from the French Academy the Prix Baudin, which was doubled on account of the "quite extraordinary service" rendered to mathematical physics by this work of Sophie Kovalevsky. She was also elected a Corresponding Member of the St. Petersburg Academy of Sciences.

Besides her mathematical work, Sophie Kovalevsky had lately begun to give literary expression to her ideas

The autobiography of her early childhood ("Reminiscences of Childhood"), published last year in a Russian review, is one of the finest productions of modern Russian literature. In 1887 she published in the Swedish review *Norna* the introduction to her novel, "Væ Victis!" And in the last issue of the *Nordisk Tidskrift* she brought out, under the pseudonym of Tanya Rerevski, a fragment of a longer novel, "The Family of Vorontsoffs," which she left in manuscript entirely ready for the printer. In her last letter to the writer of these lines in December last, she spoke of bringing out an English version of this novel, which, though written in Russian, could not be published in her mother country.

It need not be said that so highly gifted a woman as Sophie Kovalevsky was modesty itself. She took the liveliest interest in Swedish intellectual life, and had many friends both in Stockholm and in this country, which she visited last year. She leaves a daughter eleven years old. The Swedish papers speak with the greatest sympathy and regret of *their* professor "Sonya" (the little Sophie) Kovalevsky.

In Mme. Kovalevsky's "Reminiscences of Childhood," she records a fact well worthy of note. She was then about ten years old, staying in her father's house in the country. The house was being repaired, and wall-papers were brought from St. Petersburg; but it so happened that there was no wall-paper for the nursery. So it was papered with the great Ostrogradski's lithographed course upon higher mathematical analysis—a survival of her father's student years; and the little Sophie, who devoured everything printed within her reach, to the despair of her English governess, was continually reading these mathematical dissertations covered with incomprehensible hieroglyphs. "Strangely enough," she says in her memoirs, "when, at the age of sixteen, I began studying the differential calculus, my teacher was astonished at the rapidity with which I understood him—'just as if it was a reminiscence of something that you knew before,' he said. The continual reading of the wall-papers certainly left some unconscious traces in my childish mind."

P. K.

NOTES.

THE Bureau of the International Congress of Geologists has decided that its fifth session shall be held at Washington, and the date of the session has been fixed for the last Wednesday (26th) of August 1891. The annual meeting of the American Association for the Advancement of Science and the summer meeting of the Geological Society of America will be held in the same city during the preceding week. A circular, signed by J. S. Newberry, chairman, and H. S. Williams and S. F. Emmons, secretaries, has been issued, cordially inviting geologists to take part in the labours of the Congress, and, if they desire to do so, to address their request for inscription as members of the Congress to the Secretary's office (1330 F Street, Washington, D.C.). The Committee of Organization will try to obtain from the ocean steamship lines the most favourable terms for the transportation of foreign members to and from the United States, and to arrange with the respective railroad companies for reduced rates for the geological excursions. They point out that, to accomplish this satisfactorily, it is important they should know beforehand the approximate number of members who propose to attend the meeting. They desire also to have an expression of opinion from these members in order to arrange in advance a series of excursions to places that will be of interest to the greatest number. Owing to the large number of points of geological interest, and to the great distances to be traversed, it would be impossible for the Committee to arrange these excursions, so that their expense should fall within reasonable limits, without some such previous information.

IT has been arranged that the afternoon meeting to celebrate the Chemical Society's jubilee, on February 24, at 3 p.m., shall be held in the theatre of the London University, Burlington Gardens, instead of at the Society of Arts, as the number of Fellows who have notified their intention of attending the celebration is larger than can be accommodated in the meeting-room of the Society of Arts. A number of distinguished foreigners are expected to be present—M. Gautier, President; M. Combes, Vice-President; and M. Haller, member of Council of the Société Chimique de Paris; Drs. Wichelhaus and Will, of Berlin, representing the Deutsche Chemische Gesellschaft; and Prof. Victor Meyer, of Heidelberg.

LORD LILFORD AND MR. WILSON NOBLE deserve the thanks of all genuine lovers of nature for calling attention to the scheme whereby an enterprising Birmingham Company proposed to take from the Shetland Islands, during the approaching spring, no fewer than 20,000 eggs, including "many beautiful and rare varieties." A question on the subject was asked in the House of Commons on Tuesday by Mr. W. James, in reply to whom the Lord Advocate suggested that Mr. A. Pease's Wild Birds' Protection Bill, which had been read a first time, might afford an opportunity for extending to the eggs of wild birds the protection at present given to wild birds themselves. In accordance with this suggestion, Mr. W. Noble proposes to move an instruction to the Committee on Mr. Pease's Bill extending the operation of the measure to birds' eggs. Meanwhile, the Birmingham Company has announced that the proposed "oological expedition" has been abandoned.

THE Cambridge Medical Graduates' Club are to give a dinner to Sir G. M. Humphry, F.R.S., at the Marlborough Rooms, Regent Street, next Tuesday.

ON Sunday, February 8, a *fête* was held at Naples in honour of the fiftieth anniversary of Prof. Arcangelo Scacchi's Professorship of Mineralogy in the University of that city. The large hall of the apartments of the Royal Academy was crowded by delegates from all the Universities of Italy, as well as some foreign ones. Nearly every scientific institution of Italy, and not a few institutions of other nations, were represented. The Geological Society of London and the Mineralogical Society of Great Britain had deputed Dr. Johnston-Lavis to convey their congratulations to the eminent mineralogist, who on that day attained the age of eighty-one years. Prof. A. Scacchi was deeply touched by the numerous speeches, and especially by that of the Mayor of Gravina di Puglia, where the Professor was born, and where a true hero worship has sprung up around his name. A large gold medal, bearing on one side the head of the veteran man of science, and on the other a suitable inscription, was presented to him, together with a beautiful illuminated parchment executed by Mr. L. Sambon. Much credit is due to the committee, and especially to Profs. Bassani and Oglialoro, the Secretary and Treasurer, for the succcess of the celebration. A pamphlet will be printed containing a biography of Prof. A. Scacchi, a list of his works, an account of the celebration, and the different congratulatory letters, telegrams, and speeches on the occasion; and a copy will be sent to each subscriber. Prof. A. Scacchi has resigned the Chair of Mineralogy, and his son, a promising young investigator, Prof. Eugenio Scacchi, takes his place in the University of Naples.

THE French scientific journals record the death of M. Jacques Armengaud, well known as an engineer, and formerly a professor at the Conservatoire des Arts et Métiers. He was the author of some important technological works.

THE interesting ethnographical collections brought by M. Charles Rabot from Siberia are now being exhibited in Paris.

THE veteran botanical explorer, Mr. C. G. Pringle, has brought back from Mexico a collection of about 20,000 specimens of plants, among which he believes there are a large number of new and rare species.

A SOCIETY of the Natural Sciences for the West of France is being founded at Nantes, under the auspices of the Director of the Museum of Natural History in that town. Its objects embrace the encouragement of the study of zoology, botany, geology, and mineralogy in the west of France, in connection with the Museum of Natural History at Nantes. It is intended to hold a meeting of the Society every month, and to publish a *Bulletin*, the first number of which is to appear in July.

ON Tuesday evening Lord E. Hamilton asked the First Lord of the Treasury whether his attention had been drawn to Mr. Elliott's invention for the annihilation of smoke, now on view on the Thames Embankment; and whether it was the intention of the Government to give the invention a trial, with a view to the possibility of taking some steps to abate the smoke nuisance in the metropolis. Mr. W. H. Smith, in reply, said he was not personally acquainted with the apparatus in question, but he was informed by the Metropolitan Police, whose duty it was to enforce the Acts relating to the smoke nuisance in the metropolis, that smoke had been observed to issue from the chimney on the Thames Embankment, from which it might be inferred that the apparatus was not at all times successful.

THE February number of the *Kew Bulletin* opens with an interesting paper on Ipoh poison of the Malay peninsula. In Java the Upas tree furnishes a very effective arrow poison; and, finding the same tree on the mainland, the Malays used its juice. Here, however, the juice is innocuous; and, according to Griffith, the defect is remedied with arsenic. The writer in the *Bulletin* says that "if this is really done it must be when the arrows are prepared, for two authentic specimens of Ipoh poison from the Malay peninsula were absolutely inert, and contained none of the poisonous principle antiarin." This article is followed by instructive papers on Kath or Pale Cutch, the production of cane-sugar in the sugar-cane, timber of Yoruba-land, phylloxera, the botanical station at Lagos, and the mealy bug at Alexandria. Along with the February number, Appendix I. for 1891 is issued. It consists of a list of such hardy herbaceous annual and perennial plants as well as of such trees and shrubs as matured seeds under cultivation in the Royal Gardens, Kew, 1889. These seeds are available for exchange with colonial, Indian, and foreign Botanic Gardens, and with regular correspondents of Kew. The seeds are for the most part available only in moderate quantity, and are not sold to the general public.

AT the last meeting of the scientific committee of the Royal Horticultural Society, the Rev. W. Wilks showed specimens of the injuries observed on shoots of peach-trees which were in contact with galvanized wire during the recent severe frost. The shoots at the point of contact with the wire were apparently blackened and frozen through, so that the distal part of a shoot, although for a short time it retains its healthy appearance, shortly dies of starvation. Similar illustrations have been before the committee on other occasions. At the same meeting Prof. F. Oliver displayed a number of water-colour drawings showing the effect of fog on the leaves and flowers of various plants; but he reserved a full statement of his observations till a future time.

MR. T. S. BRANDEGEE will publish, in the Proceedings of the Californian Academy of Science, the results of his recent botanical explorations in California.

MM. PORTA AND RIGO have returned from their botanical expedition to Spain, in which they were greatly hindered by the extraordinarily wet weather in the Peninsula last summer. Their chief explorations were in Murcia, and in the neighbourhood of Carthagena and Alicante; they have discovered some new species, and others previously unknown in Europe.

A BIOLOGICAL station will be opened in summer at the Plöner See, in East Holstein; and in view of the importance, scientific and practical, of the investigations which are to be carried on in connection with it, the Prussian Government has consented to set apart for it an annual grant during the next five years. Many private subscriptions have also been promised.

AN interesting case of the evidence of the northern and eastern extension of the Gulf Stream in Arctic regions is quoted in the *Times* of the 12th inst. A message inclosed in a bottle which was thrown, on July 3, 1890, from the s.s. *Magnetic* off the Westmanna Islands, lying to the south of Iceland, has just been returned to the writer in Liverpool, the bottle having been picked up in the Nufsfjord, Lofoden Islands, by the s.s. *President Christie*, on January 15. The bottle drifted 890 nautical miles in six months and a half.

MR. H. C. RUSSELL has published the results of meteorological observations made in New South Wales during 1888. There is a considerable reduction in the bulk of the volume for this year, as the details referring to rainfall are published in a separate work. The mean temperature of the whole colony for the past eighteen years is 61°·2, and the mean for the past year is 62°·3, or 1°·1 in excess of the mean. Anemometers are now erected at six stations, and show that at the inland stations the amount of wind recorded is little more than one-third of the amount shown by the Sydney record. The results of rain and river observations for 1889 show that the year was a very favourable one: the average rainfall for the whole colony for 1889 was 29·25 inches, being 21·7 per cent. above the average, obtained from the mean of all the complete records for the year. The number of volunteer observers is now 960, and Mr. Russell states that the interest in rainfall records is rapidly increasing. In addition to the meteorological observations published in these volumes, a complete system of weather telegraphy is maintained; two weather maps are issued daily, containing observations from eighty stations.

THE Hunterian Oration was delivered by Mr. Jonathan Hutchinson, F.R.S., last Saturday, in the theatre of the Royal College of Surgeons. Mr. Hutchinson drew a very interesting parallel between Aristotle and Hunter. That they exhibited great differences he admitted; and some of these he pointed out. He contended, however, that where the two men met on common ground they showed similar habits of thought, and intellectual powers not very unequal. Both were systematic and enthusiastic zoologists, in times when the study of zoology was the pursuit of very few. Both saw clearly that advance in natural history knowledge could be made only by the collection of facts, and, realizing that such advance was well worth the effort, both set themselves zealously to work. "It is probably not possible," said Mr. Hutchinson, "to mention a third name which can be placed in any sort of competition with either in respect to originality of effort in this direction. It has been asserted, by one well able to form an opinion in this matter, that between Aristotle and the German Kant no metaphysician of original power appeared. In like manner we might say, in reference to scientific zoology, that there was no one between Aristotle and John Hunter. There were, of course, many who made meritorious efforts with more or less success, but none whose achievements can in the least compare with Aristotle's account of the parts of animals or with John Hunter's museum. During the long twenty centuries that were passed through between these two men, there did not appear anyone so capable of applauding Aristotle's work as Hunter—not one who would have contemplated Hunter's preparations with more true interest than Aristotle. Nor must we, in admitting Hunter's inferiority in scope of attainment, forget the great difference in their occupations. Aristotle, although the son of a doctor, did not practice physic himself, and he was throughout liberally provided with funds. During the most prosperous part of his career, his pupil, Alexander the Great, not only supplied him with money, but collected specimens for him in distant lands—a zealous collector of specimens, as has been remarked by Sir Alexander Grant. Never before or since was the endowment of research on so liberal a scale. It is only justice to our own countryman to remember that no such fortune had fallen to his lot. He had to earn every pound that he expended in zoological science in the practice of a toilsome profession, to which also he was compelled to devote the better half of his thoughts. He, like many other devotees of science, was under the compulsion to earn a livelihood in other pursuits—a position not wholly unlike that of the Jews of old, who rebuilt the walls of Jerusalem with a trowel in one hand and a sword in the other."

At the Royal Academy of Lyncei on December 18, Signors Sella and Oddone gave an account of some researches on the distribution of magnetism in certain regions on the Alps. They have found a number of magnetic foci, and record that the rocks which present distinct magnetic properties are magnetite, serpentine, diorite, melaphyre, and syenite. A magnetic rock was observed by Signor Sella on Punta Giufetti, in the Monte Rosa group, and, as it presented traces of fusion on its surface, as if it had been struck by lightning, it is suggested that this circumstance has endowed the rock with its magnetic properties.

We are glad to see that the Natural History Museum at South Kensington has recently received some valuable additions. The skeleton of a whale, never before seen in this country, has been brought from the Behring Sea. The only places where this whale (*Rhachianectes glaucus*) is now found are in the northern parts of the Pacific and in the Sea of Kamtschatka; but the animal is supposed to have had a far wider range than this in consequence of the fossil remains of this species (or a nearly identical one) having been discovered in Norway and Sweden. Among other additions are pieces of amber, 600 in number, inclosing beetles, insects, and even some small lizards.

From the official report of the Japanese census, taken on December 1, 1889, it appears that the number of houses in the whole of Japan is 7,840,872, and the total population 40,702,020. The above population divided according to classes gives the following results: nobles and their families, 3,825; old military class, 1,993,637; common people, 38,074,558. These figures, compared with the census taken in 1888, show an increase of 38,046 houses, and of 464,786 persons. Statistics of ages are also given, and from them it appears that at the close of 1889 there were 65 persons who had attained their hundredth year in Japan; 45 their hundred and first year; 13 their hundred and second year; 11 their hundred and third year; 1 his hundred and fourth year; 9 their hundred and fifth year; 3 their hundred and sixth year; 1 his hundred and seventh year; and 1 his hundred and ninth year. The cities and prefectures having populations of over a million numbered 15, that of Tokio being given at 1,138,546, but this includes not only the city but also a considerable administrative area around.

During the present season an attempt is to be made to extend our knowledge of the wild tribes inhabiting the borderland of Burmah, between Bhamo and the Chinese frontier on the one hand, and between the Northern Shan States and the Chinese frontier on the other. Lieutenant Daly, Superintendent of the Northern Shan States, and Lieutenant Eliott, Assistant Commissioner, will spend the greater part of the next six months exploring these regions. The former will have with him an escort of fifty men of the military police, and will be accompanied by Mr. Warry, of the Chinese Consular Service, and Lieutenant Renny Tailyour, of the Survey Department. He starts from Lashio, and will visit the States on the Salween, including the important State of Kyaingyanyi, and will then return along the supposed Chinese border, ascertaining its situation as accepted on the spot, and the nature of the country and the tribes inhabiting it. Mr. Eliott will start from Bhamo, and will be accompanied by Major Hobday, of the Survey Department. These officers also will be supplied with an escort of military police. They will probably proceed up the right bank of the Irrawaddy to the bifurcation of the river, and then will cross and examine the country on the Chinese border on the left bank. The country is practically unknown at present, and it is expected that much information of an interesting nature will be collected by the exploring parties. The explorers will, of course, confine their attention to the British side of the border, and when the time comes for the formal demarcation of the frontier by a joint Commission of Chinese and British officials, the information now to be collected will, no doubt, prove useful.

A further paper upon azoimide or hydrazoic acid, N_3H, is published by Prof. Curtius, of Kiel, in conjunction with Herr Radenhausen, in the new number of the *Journal für praktische Chemie*. It will be remembered that in the earlier work upon this remarkable substance, a full account of which will be found in Nature, vol. xlii. p. 615, the free azoimide was obtained as a gas by the action of sulphuric acid upon the sodium salt N_3Na. It was not found possible to collect the gas in the anhydrous state, owing to its great affinity for water. Since the publication of the first communication, however, an improved method of preparing the solution of the acid in water by distilling a soda solution of the hippuryl compound with dilute sulphuric acid has been discovered, and the details of this process were described in Nature, vol. xliii. p. 21. In the present communication to the *Journal für praktische Chemie*, Prof. Curtius and Herr Radenhausen make the important announcement that they have at length succeeded in isolating pure anhydrous azoimide itself. They find that it is only a gas at temperatures above 37°C., at which temperature, under ordinary atmospheric pressure, it condenses to a clear, colourless, and very mobile liquid of phenomenally explosive nature. The liquid possesses the intolerable odour of the gas and the aqueous solution. It is readily miscible with either water or alcohol. It was obtained by the successive fractionation of the concentrated aqueous solution, the first fraction being condensed separately and refractioned. On repeating this process four times, an acid containing over 90 per cent. of N_3H was obtained. The last traces of water were finally completely removed by means of fused calcium chloride. The anhydrous liquid, when one is fortunate in carrying out a distillation in safety, is found to boil constantly without decomposition at + 37° C. But it explodes with extraordinary violence when suddenly heated, or when touched with a hot body, and also most erratically sometimes without apparent provocation at the ordinary temperature, with production of a vivid blue flame. It unfortunately explodes in the Torricellian vacuum at the ordinary temperature, thus preventing the determination of its density by Hofmann's method. The explosion of a quantity weighing only 0·05 gram was sufficient to completely pulverize the apparatus, the mercury being driven in fine particles into every corner of a large laboratory. Upon a subsequent occasion a quantity of the liquid amounting to no more than 0·7 gram suddenly exploded upon merely removing the tube containing it from the freezing mixture in which it had been immersed. Such was the force of this explosion that every glass vessel in the vicinity was completely shattered by the concussion, and it is a matter of great regret that Herr Radenhausen was seriously injured by it. As regards the relative strength of the acid, Prof. Ostwald, who has made determinations of its conductivity, finds that it is a little stronger than acetic acid. In reply to the recent suggestion of Prof. Mendeleeff that the ammonium salt of azoimide, N_3NH_4, might possibly undergo an isomeric change analogous to the conversion of ammonium isocyanate into urea, it is shown that this is not the case. The ammonium salt is a substance crystallizing in beautiful large prisms which possess the property of continually diminishing in size and eventually of entirely disappearing, owing to spontaneous sublimation. Neither sublimation nor boiling with water effect any change of constitution whatever.

THE additions to the Zoological Society's Gardens during the past week include a Red Deer (*Cervus elaphus* ♀), British, presented by Mr. C. J. H. Tower, F.Z.S.; six Night Herons (*Nycticorax griseus*), European, presented by Mr. A. A. van Bemmelen; a Spotted Eagle Owl (*Bubo maculosus*) from South Africa, presented by Mr. Julius Wilson; a Redwing (*Turdus iliacus*), British, presented by Mrs. J. B. Capper; two Yellow-throated Rock Sparrows (*Petronia petronella*) from Africa, deposited; seven Knots (*Tringa canutus*), two Bar-tailed Godwits (*Limosa lapponica*), European, purchased.

OUR ASTRONOMICAL COLUMN.

VARIABILITY OF THE ANDROMEDA NEBULA.—The January number of the *Monthly Notices of the Royal Astronomical Society* contains a note by Mr. Isaac Roberts, entitled "Photographic Evidence of Variability in the Nucleus of the Great Nebula in Andromeda." Between 1885 and 1890 a dozen photographs of this object were taken on several plates; and especially on three negatives taken with exposures of 5, 15, and 60 minutes in December 1890, the nucleus of the nebula has a decidedly stellar appearance. Other plates, exposed for both short and long intervals of time, show no trace of a stellar nucleus. It may therefore reasonably be inferred that the nucleus of the nebula is variable.

ECCENTRICITIES OF STELLAR ORBITS.—In the current number of *The Observatory* Dr. T. J. J. See points out that the arithmetical mean eccentricity of 50 of the best stellar orbits hitherto computed is 0·5, while the mean eccentricity for the orbits of the planets of our system is less than one-tenth of this fraction. A discussion of binary systems has led the author to the conclusion that the great eccentricities observed have been developed by the continual action of tidal friction. The elongated forms of most stellar orbits, and the relatively large mass-ratio of the components of a system, are so different from the orbits and relative masses in the solar system that "the development of the solar system seems to have been an exception and not the rule. From these considerations the writer would venture the opinion that investigators of cosmogony who have looked upon the solar system as typical of the general process of cosmic development, and proceeded therefrom to investigate stellar evolution in general, have pursued an erroneous path."

A NEW NEBULA NEAR MEROPE.—Mr. E. E. Barnard, of Lick Observatory, contributes a note "On the Nebulosities of the Pleiades, and on a New Merope Nebula," to *Astronomische Nachrichten*, No. 3018. Whilst examining the Pleiades on November 14, 1890, Mr. Barnard discovered a new and comparatively bright round cometary nebula close south and following Merope. Since this date the nebula has been observed several times and its position determined. The reason why such a comparatively bright object has never been photographed is that the exposure which it would require to impress itself upon the photographic plate would over-expose Merope so much that the light of the two would coalesce.

NAMES OF ASTEROIDS.—Dr. Palisa has given the following names to asteroids discovered by him last year:—

(290)	Bruna, discovered	March	20,	1890.
(291)	Alice, „	April	25,	„
(292)	Ludovica, „	„	„	„
(295)	Theresia, „	August	17,	„

THE BRITISH MOSSES.[1]

I.

I CANNOT lay the following paper before the readers of NATURE without repeating an apology which I addressed to my audience at the Royal Institution on this subject. I can make no pretence to speak with authority; I speak only as a learner who has devoted to the subject some leisure from amidst avocations of a very different kind. But the pleasure I have derived from the study, the sense, whenever I am in the country, that I am surrounded with a world of variety and beauty of which I was formerly only dimly conscious, and the hope of communicating some of this pleasure to others may, I hope, furnish some apology for my venturing to speak on the subject.

Classification.—Without entering into any question as to the best classification of the mosses, or the relative systematic value of the different groups, the following table, which is arranged in an ascending rank, will be sufficient to show the position of the mosses in the vegetable kingdom, and the principal groups into which they may be divided:—

TABLE A.

		Series.	Orders.	Examples.
Vascular Cryptogams				
Muscineæ	i. Musci	Pleurocarpæ		Hypnum
		Acrocarpæ	Stegocarpæ	Polytrichum
			Cleistocarpæ	Phascum
		Anomaleæ	Schizocarpæ	Andræa
			Holocarpæ	Archidium
	ii. Sphagnaceæ			
	iii. Hepaticeæ	Jungermanniaceæ		
		Marchantiaceæ		
Algæ, &c.				

From this table it will be gathered that the mosses, using that word in its wide signification, stand at the head of the cellular cryptogams, and that above them are the vascular cryptogams, of which the ferns are one of the best-known groups. From these vascular cryptogams the mosses are, however, separated by a distance which Goebel has described as a chasm "the widest with which we are acquainted in the whole vegetable kingdom."

From the table it will be further seen that the larger group of the Muscineæ divides itself into three principal smaller groups: the Hepaticæ or liverworts, the Sphagnaceæ or turf mosses, and the Musci or true mosses—urn-mosses, as they have been called, from the form of their capsule. Passing over the other subdivisions, it may be observed that the Acrocarpous mosses are those which carry their capsules at the end of the axis of growth, whilst the Pleurocarpous mosses bear their fructification on stalks, more or less long, proceeding from the sides of the axis. Amongst these Pleurocarpous mosses occurs the old (broken up by modern systematists into several genera) genus Hypnum, the largest of all the genera in these islands or in Europe—a vast group which occupies amongst mosses something like the place which the Agarics occupy amongst the Fungi.

Number of British Species.—If we were to try and ascertain the number of the British Muscineæ from the systematists of some few years ago, like Hooker and Wilson, the species would number between 500 and 600; but according to the views of more recent writers, the number would probably rise to something between 800 and 900. The true mosses are the most numerous, the turf-mosses by far the fewest.

Date of Flora.—What is the date of this moss flora of Britain? Three ancient collections enable us to give some

[1] The substance (with omissions and additions) of a Discourse by the Right Hon. Lord Justice Fry, delivered at the Royal Institution, January 23, 1891.

reply to the question. In an interglacial bed near Crofthead, in Renfrewshire, eleven species of moss were discovered, and with one possible exception all are well-defined British species of the present day. If we take Mr. Wallace's chronology, and hold that 80,000 years have passed since the Glacial epoch disappeared, and 200,000 years since the Glacial epoch was at its maximum, we may perhaps give from 100,000 to 150,000 years for the age of this little collection. Out of the eleven mosses discovered seven belong to the genus Hypnum, or the family Hypnaceæ. This collection, then, is evidence, so far as it goes, (1) that the existing moss flora is as old as the interglacial epoch; (2) that the Hypnaceæ were as dominant then as now; and (3) that the specific forms have remained constant since that epoch.

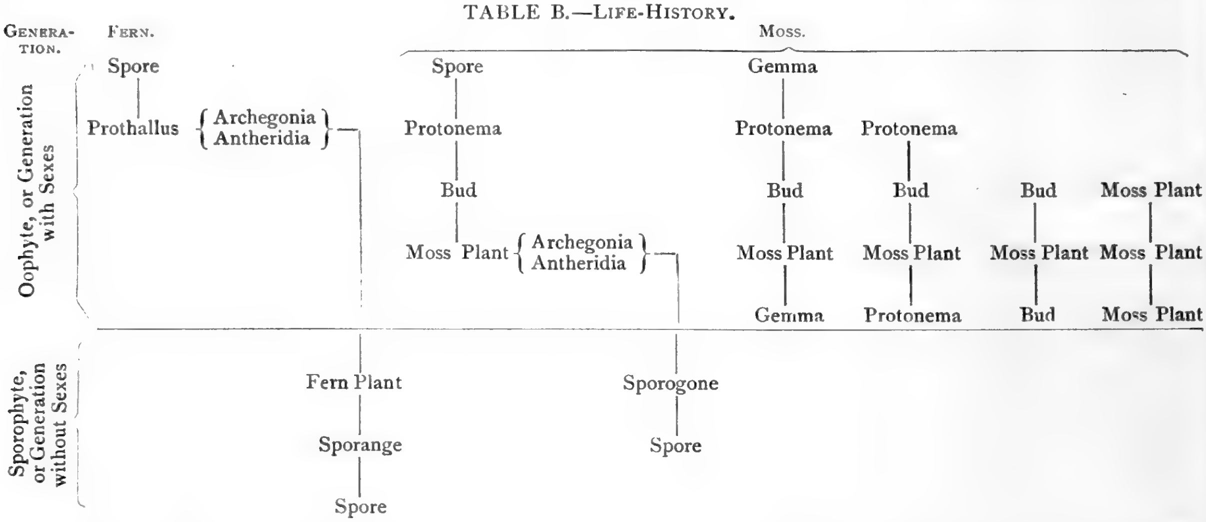
Another collection of fourteen mosses has been discovered in a drift in the Clyde valley above the Boulder drift, and tends to confirm the previous conclusions; as all the species are existing, all now inhabit the valley of the Clyde, and the Hypnaceæ are still predominant, though not in so great a proportion as in the Renfrewshire bed.

A third collection has been found at Hoxne, in Suffolk, in a lacustrine deposit, probably resting in a hollow in the boulder clay: together with phanerogams of an arctic habit have been found the remains of ten mosses, which are described by Mr. Mitten as looking "like a lot of bits drifted down a mountain stream." They are all still dwellers in our island, and exhibit, like the other collections, a preponderance of the family of Hypnaceæ.

The fossil remains of mosses are not numerous, or for the most part very ancient. Heer inferred their existence in the Liassic period, from the presence of remains of a group of small Coleoptera, the existing members of which now live amongst mosses—an inference which seems not very strong. But recently the remains of a moss have been found in the Carboniferous strata at Commentry, in France. It appears to be closely allied to the extant Polytrichum, the most highly-developed genus of mosses; so that we have here a phenomenon like that which occurs in reference to the Equisetaceæ and Lycopodiaceæ, viz. that the earliest fossil species known belong to very highly-developed forms of the group.

Life-History.—The following table is intended to illustrate the life-history of a moss in its fullest and in its abbreviated courses, and to bring this history into comparison with that of the ferns:—

TABLE B.—Life-History.

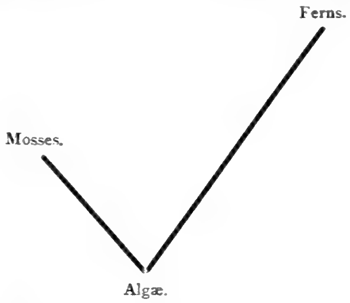


The reader's attention should first be drawn to the second column, which shows the life-history in its fullest form. It will be seen that it starts with a spore and returns to a spore.

From (1) the *spore*, which is a simple cell, proceeds (2) the protonema, a line of cells, extending by transverse divisions, so that it consists of single cells joined end to end to one another—an organism indistinguishable from the hypha of an Alga. At points this hypha throws off lateral branches which are always of less diameter than the principal ones. There is thus produced a tangled mat of fibres, running on or near the surface of the ground, and often coloured by chlorophyll. It is the green stuff so often seen in flower-pots which have been allowed to get too damp. At points in the primary hypha individual cells begin to divide in a new fashion—not by transverse septa as before, but by septa differently inclined, so as to produce the rudiments of leaves; and the direction of growth changes from horizontal to vertical. Thus is formed (3) the bud, which by growth gives rise to (4) the moss plant; on this plant, sometimes in close proximity to one another, sometimes in different parts of the same plant, sometimes on different plants, are formed (*a*) the female cell or archegonium, and (*b*) the antheridia or male organs, the antherozoids proceeding from which seek and find and fertilize the archegonium. This completes the first part of the life of the plant, the oophytic generation which results in a single sexual cell, viz. the fertilized archegonium. From this cell arises the next generation, consisting of the sporogone or stem bearing the capsule and the capsule itself, in which without fertilization are produced spores. The plant has thus started with the spore, an asexual cell, reached the point where its whole future is gathered up in a sexual cell, which has produced an organism again producing an asexual cell: we started with a spore, and have returned to a spore; we have travelled round a circle, divisible into two parts or generations, one sexual, the other asexual; and we have therefore a case of alternation of generations. To make this statement more clear, it may be observed that a generation is here spoken of as that part of the life of an organism which intervenes between the two points at which its whole future is gathered up into one cell; that such a cell is sexual when it is the result of the combination of two previously existing and independent cells; that such a cell is asexual when it is not the result of such combination; that an alternation of generations exists, whenever in the complete cycle of existence or life-history there are two points at which the whole organism is reduced to a single cell, and when the forms of the organism in the two intervals of its development are different. In the mosses, where the sporogone co-exists with and is organically connected with what I have called the moss plant, it is evident that the two generations are not such, according to the more popular notion of that word; they are not independent, nor necessarily successive.

A comparison of the first and second columns of the last table reveals at once the likeness and the unlikeness of the life-histories of the moss and of the fern. In each case the spore produces a growth of a form and nature entirely unlike the mother-plant—in one case a hypha, in the other a thallus. But whilst in the moss the protonema produces the moss plant, in the fern the prothallus itself is the home of the male and female organs, and of the sexual process, so that the fern plant belongs to the sporophytic and the moss plant to the oophytic generation; the fern plant is the result of the sexual union, whilst the moss plant is produced from the asexual spore; the fern plant produces spores asexually, the moss plant produces the sporogone as the result of the sexual union.

The observations which arise in connection with this comparison are numerous. (1) It is the belief of botanists, ever since the investigations of Hofmeister, that not mosses and ferns only, but all the phanerogams, go through an alternation of generations consisting of the oophytic and sporophytic generations. (2) It appears that the mosses and the Characeæ are the only groups of plants in which the conspicuous and vegetative organism—the plant, in ordinary parlance—belongs to the oophytic generation: (3) That, in consequence, the plant of the moss is in no sense the ancestor of the plant of the fern, or of the phanerogams, but belongs to a different generation from these; and further, that the leaves, the stem, and the epidermis of the moss have no genetic connection with the leaves, the stem, or the epidermis of our flowering plants, whilst the fibro-vascular bundles of the sporogone of the Polytrichum, and the stomata on the apophyses of some mosses will belong to the same generation which, in the vascular cryptogams and phanerogams, produces similar organs. (4) That the great chasm in the systematic arrangement of the vegetable kingdom between the mosses and the ferns is thus accounted for by their belonging to different generations, so that the ferns are not in any sense descendants of the mosses, but only collateral relatives, as thus—



(5) That, consequently, the mosses not only represent the highest development known of the cellular cryptogams, but the highest point in one line of development, in which the oophytic generation took the lead in importance; whilst the vascular cryptogams and phanerogams are the results of another and more successful line of development, in which the sporophytic generation took the lead as the prominent part in the life-history.

The appearance of similar organs in two independent lines of development—*i.e.* of the leaves, stem, and epidermis—in the mosses, and then in the ferns, without any relation of descent, is a thing well worthy of being pondered over by those who study evolution: it may suggest that the two lines of development, though independent, are governed by some principle which brings about such like results: it may be compared with the likenesses which occur in the animal kingdom between the placental and marsupial mammals.

The remaining columns of the table above given will best be understood after a study of the next succeeding table.

Modes of Reproduction.—Hitherto we have considered only the reproduction from a spore produced in the special organ for their production—the spore capsule. But, in fact, one of the most striking peculiarities of the mosses is the vast variety of their modes of reproduction.

In the following table, which is probably far from exhaustive, I have endeavoured to exhibit many of these modes of reproduction, dividing them into those cases in which it takes place with protonema, and those cases in which it takes place without.

TABLE C.—MODES OF REPRODUCTION.

A.—*With Protonema.*

i. Spores.	in capsule	
ii. Gemmæ	on end of leaf	*Leptodontium gemmascens.* *Orthotrichum phyllanthum.* *Grimmia Hartmani.*
	on midrib	*Tortula papiliosa.*
	in axils of leaves ...	*Bryum.*
	in balls	*Aulacomnion.*
	in cups	*Tetraphis.*
iii. Protonema ...	from rhizoids	*Phascum.* *Polytrichum.*
	from aërial rhizoids ...	*Dicranum undulatum.*
	from terminal leaves ...	*Oncophorus glaucus.*
	from base of leaf ...	*Funaria hygrometrica.*
	from midrib	*Orthotrichum Lyelli.*
	from margin	*Buxbaumia aphylla.*
	from stems...	*Dicranum undulatum.*
	from calyptra	*Conomitrium julianum.*

B.—*Without Protonema.*

iv. Leaf-Buds ...	on rhizoids	*Grimmia pulvinata.*
v. Leaf-Buds ...	on aërial rhizoids ...	*Dicranum undulatum.*
vi. Bulbs...	on stem	*Bryum annotinum.*
vii. Young Plants ...	at ends of branches ...	*Sphagnum cuspidatum.*
viii. Leafy Branches.	becoming detached ...	*Conomitrium julianum.* *Cinclidotus aquaticus.*
ix. Rooting of main axis	... ...	*Mnium undulatum.*

Weismann's Theory.—The consideration of this table is not without its interest in reference to Prof. Weismann's theory of the division of the cells and plasma of organisms into two kinds: the germ cells and germ plasma endowed with a natural immortality, and the somatic cells and somatic plasma with no such endowment. That the mosses are a difficulty in the acceptance of the theory as a universal truth, the Professor himself admits. The evidence of the mosses seems to amount at least to this: that in this whole group, the highest in this line of development, where the oophytic generation produces the principal plant, and where there are highly specialized organs for the production of spores or germ cells—that in this whole group either there is no effectual separation between the two kinds of plasma, or that the germ plasma is so widely diffused amongst the somatic plasma that every portion of the plant is capable of reproducing the entire organism.

Comparison with Zoological Embryology.—The table will further offer us some points of comparison with animal embryology.

In that branch of physiology, one of the most remarkable facts is what has been called recapitulation, *i.e.* the summary in the life of the individual of the life of the race, so that the development of the individual tells the development of the race—*e.g.* the gills of the tadpole tell us of the descent of the Batrachians from gill-breathing animals.

So here we cannot doubt that the protonema of the moss tells us of the descent of the whole group of mosses from the Algæ.

Another remarkable fact in animal embryology is the co-existence in exceptional cases of the mature and the immature form; so the axolotl retains both gills and lungs throughout its life. In like manner some mosses, *e.g.* the *Phascum*, retains its algoid protonema throughout its life.

Again, in zoological embryology, an attempt is often found, to use the language of Prof. Milnes Marshall,[1] "to escape from the necessity of recapitulating, and to substitute for the ancestral process a more direct method."

In like manner the preceding tables will show to how great an extent Nature has adopted the system of short-circuiting in the reproduction of the mosses; for in every mode of reproduction, except that through sporogone and spore, it will be observed that a shorter circuit is travelled, *e.g.* the *Orthotrichum phyllanthum* produces spores at the end of its leaves, which, falling to the ground, throw out a protonema which produces a bud, and then a moss plant, and then a spore at the end of the leaf, and the whole sporophytic generation is evaded; and so on in gradually shortening circles (see Table B), till we get the case of a Sphagnum, which produces a little Sphagnum plant at the end of its leaves without protonema—whether without bud, I do not know. In every case Nature seems to leave out the sexual reproduction if she can help it, and directs her whole attention to the production of the vegetative organism—the moss plant in the popular sense—which she never omits.

Another point of comparison arises, but this time it is one of contrast between the embryology of the two kingdoms.

In animals, to again quote Prof. Milnes Marshall, "Recapitulation is not seen in all forms of development, but only in sexual development, or at least only in development from the egg. In the several forms of asexual development, of which budding is the most frequent and the most familiar, there is no repetition of ancestral phases, neither is there in cases of regeneration of lost parts."

In mosses, on the contrary, the table last given shows that in most of the modes of reproduction, the ancestral form, the algoid protonema, is retained and reproduced, whereas in the growth from a sexual cell, *i.e.* in the sporogone, the ancestral form entirely disappears.

The peristome, or girdle of teeth round the orifice of the capsule, assumes very varying forms, often of great beauty and interest. In some of the mosses it is absent, in some it consists

[1] Address to Biological Section of British Association (NATURE, vol. xlii. p. 478).

of one ring of teeth, in many of two rings, and in one foreign genus (Dawsonia) there are as many as four circles of teeth.

The object served by this complicated structure is not, perhaps, very certain, but it seems to be intended to secure the retention or exclusion of the spores from the spore sac in such conditions of the atmosphere as will best conduce to their germination. In the gymnostomous mosses (*i.e.* those without peristome) it is observed that the spores sometimes germinate within the capsule, an event which is probably adverse to the prospects of the race. The following table will illustrate in a few cases selected as illustrations the different behaviour of the teeth of the peristome under different hygrometric conditions, and suggests what is the probable advantage in each case:—

TABLE D.

Genus.	Condition of teeth in dry weather.	Condition of teeth in wet weather.	Reason suggested.
Bartramia ...	Erect ...	Convergent	That spores require *dry* weather when first emitted
Orthotrichum.	Erect or reflexed ...	Ditto ...	Ditto
Funaria ...	Reflexed ...	Ditto ...	Ditto
Bryum ...	Convergent...	Expanded..	That spores require *wet* weather when first emitted
Fissideus ...	Ditto ...	Ditto ...	Ditto

The motion of the teeth of the peristome appears to be due to the action of a ring of specialized cells which surrounds the mouth of the capsule at the base of the teeth; and the opposite ways in which these cells act in the same condition of moisture in different genera, is a remarkable circumstance.

To anyone who studies the subject, the immense variety as well as beauty of the peristomes of mosses becomes very impressive. If the sole end be the protection and extrusion of the spores in the proper weather respectively, why is there this infinite wealth and variety of form and of colour? The question can be asked, but hardly can be answered, and the mind of the beholder is left, as it so often is, when contemplating the richness of Nature, in a state of admiration and wonder and ignorance. "Rerum natura tota est nusquam magis quam in minimis."

(*To be continued.*)]

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The regulations for the new Isaac Newton Studentships for study and research in astronomy and Physical Optics are published in the *University Reporter* for February 17, 1891. Mr. Frank McClean, the generous founder, has increased his benefaction to £12,500.

The Museums and Lecture Rooms Syndicate report that a sum of £1450 will be required for the fittings of the new buildings in the Departments of Human Anatomy and Physiology.

The following have been nominated as electors to the Professorships indicated;—Chemistry, Sir H. E. Roscoe; Plumian, Dr. Cayley; Anatomy, Prof. Liveing; Botany, Dr. Vines; Geology, Dr. Bonney; Jacksonian of Natural Philosophy, Prof. Ewing; Mineralogy, Dr. Bonney; Zoology and Comparative Anatomy, Sir G. M. Humphry; Cavendish of Physics, Prof. Liveing; Mechanism, Dr. Besant; Downing of Medicine, Sir G. M. Humphry; Physiology, Sir G. E. Paget; Pathology, Sir G. E. Paget; Surgery, Sir G. E. Paget.

Prof. A. G. Greenhill, F.R.S., and Dr. Routh, F.R.S., are nominated as adjudicators of the Adams Prize to be awarded in 1893.

Mr. Hickson, the newly-appointed Lecturer in Invertebrate Morphology, announces a course on *Cœlenterata* to be given during the remainder of the current term.

SCIENTIFIC SERIALS.

The *American Meteorological Journal* for January contains:—A report upon the features of tornadoes, and their distinction from other storms, considered in connection with the tornado of Lawrence, Mass., July 26, 1890, by Prof. W. M. Davis. He quotes a description of a tornado observed as early as 1687 at Hatfield, in this country, in which the writer (the Rev. A. de la Pryme) minutely describes the whirling motion of the funnel.—The meteorological observatory recently established on Mont Blanc, by A. L. Rotch. This is a reproduction of a paper read before Section A of the British Association last year, and contains a description of a meteorological observatory being erected by M. Vallot at the Rocher des Bosses, at an altitude of about 14,320 feet above sea-level.—The Gervais Lake tornado, by P. F. Lyons. It occurred on July 13, 1890, and did immense damage to buildings and crops, over an area of scarcely more than half a mile in length. The editors of the *Journal* have added what purports to be a photograph of the funnel, taken by an amateur photographer, who happened at the time to be occupied in taking views, about six miles off.—Rainfall in Michigan, by N. B. Conger, with a chart showing the annual fall in that State. This paper closes a series of monthly summaries by the same author.—Prof. H. A. Hazen concludes his account of observations on Mount Washington; and M. H. Faye concludes his articles on cyclones, tornadoes, and waterspouts, which were begun in the number for November 1889, and probably form the most complete exposition of his theory which has yet been printed. The editors of the *Journal* invite the criticism of English-speaking meteorologists.

The *Journal of Botany* has been recently distinguished for the unusual interest of its biographical notices of recently deceased botanists, or those interested in botanical pursuits. In the numbers for November and December 1890, we find such records of the late Miss Marianne North, whose beautiful flower-paintings are so familiar to visitors at Kew, and of the late Mr. James Backhouse, of York. The other papers in these numbers and in that for January 1891 are mostly either descriptive, or relate to the habitats of rare plants. Mr. John Roy gives a list of freshwater Algæ from Enbridge Lake and its vicinity in Hampshire; Mr. E. G. Baker continues his synopsis of genera and species of Malveæ; Mr. G. Massee describes and figures a remarkable new genus of Hymenomycetous Fungi from Madagascar, *Mycodendron*.

The two most important papers in the *Botanical Gazette* for January are a continuation of Mr. J. Donnell Smith's "Undescribed Plants from Guatemala," which include a new species of *Cephaelis*, and one by Mr. R. Thaxter on "Certain new or peculiar North American Hyphomycetes," in which a new genus of Fungi, *Sigmoideomyces*, is described, allied to *Oedocephalum*.

The number of the *Nuovo Giornale Botanico Italiano* for January is chiefly occupied by papers read at the Verona annual meeting of the Italian Botanical Society. Neither these, nor the independent papers printed in this number, present any features of special interest to the general botanist.

We have received the numbers for October, November, and December 1890 of the *Botanical Magazine* of Tokio, which give satisfactory evidence of the cultivation of botanical science in the Empire of Japan. The *Magazine* is published monthly, under the auspices of the Tokio Botanical Society, and is printed on rice paper. By far the greater number of the contributions are in Japanese, while others are in what we take to be Japanese printed in English characters. Those in English are chiefly by Prof. R. Yatabe, the President of the Tokio Botanical Society, and include descriptions of several new Japanese species, and of a new genus, *Kirengeshoma*, belonging to the Saxifragaceæ. The illustrations to a paper (in English) by Mr. N. Tanaka, on two new species of Japanese edible fungi, are particularly good.

SOCIETIES AND ACADEMIES.

London.

Zoological Society, January 20.—Mr. W. T. Blanford, F.R.S., in the chair.—Mr. Sclater exhibited specimens of three species of Purple Waterhens (*Porphyrio poliocephalus*, *P. cæruleus*, and *P. smaragdonotus*), of the Eastern Palæarctic Region, and made remarks on their nomenclature and geographical distribution.—Mr. F. E. Beddard described a new African Earthworm of the genus *Siphonogaster* from specimens transmitted by Sir A. Molony, from the Yoruba country to the north of Lagos, and proposed to call it *Siphonogaster millsoni*.—Mr. Oswald H. Latter read some notes on the Fresh-water Mussels of the genera *Anodon* and *Unio*, describing the passage of the ova from the ovary to the external gills, the mode of attachment of the *glochidia* to the parent's gill-plate, and some other peculiarities.—A communication was read from Mr. Roland Trimen, F.R.S., containing an account of a series of Butterflies collected in Tropical South-Western Africa by Mr. A. W. Eriksson. The collection contained examples of 125 species, of which 11 appeared to be new to science.—A communication was read from Mr. H. H. Brindley, containing an account of a specimen of the White Bream (*Abramis blicca*), in which the pelvic fins were entirely absent.—Mr. Boulenger read notes on the osteology of the poisonous Lizards *Heloderma horridum* and *H. suspectum*, pointing out the differences between the two species. He also remarked on the systematic position of the *Helodermatidæ*, which he held to be between the *Anguidæ* and the *Varanidæ*, but nearer the former; any close relationship with the *Mosasauridæ* was demurred to. It was incidentally mentioned that the Eocene genus *Thinosaurus*, Marsh, was probably a member of the family *Teiidæ*, and that the Cretaceous *Hydrosaurus lesinensis* was a *Dolichosaurus*. The Dolichosauria were considered as the probable common ancestors of the Lacertilia, Pythonomorpha, and Ophidia.—Prof. C. Stewart gave an account of some points in the anatomy of *Heloderma horridum* and *H. suspectum*, differing in some respects from the descriptions of these lizards given previously by Drs. Fischer and Shufeldt. The most interesting and important point was concerning the poison-apparatus. He believed that he had shown that in both species the ducts of the gland did not enter the lower jaw, but passed directly to openings situated under a fold of mucous membrane between the lip and the jaw. He thought that the structures previously described as ducts were only the branches of the inferior dental nerve- and blood-vessels.

Geological Society, February 4.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—The geology of Barbados and the West Indies; Part 1, the coral rocks, by A. J. Jukes-Brown and Prof. J. B. Harrison. The authors first discuss the reef growing round Barbados, and describe a submarine reef, the origin of which is considered; and it is pointed out that there is no sign of any subsidence having taken place, but every sign of very recent elevation. They then describe the raised reefs of the island, extending to a height of nearly 1100 feet above sea-level in a series of terraces. The thickness of the coral rock in these is seldom above 200 feet, and the rock does not always consist of coral *débris*. At the base of the reefs there is generally a certain thickness of detrital rock in which perfect reef-corals never occur. The collections of fossils made by the authors have been examined by Messrs. E. A. Smith and J. W. Gregory. Of the corals, 5 out of 10 species identified still live in the Caribbean Sea, and 1 is closely allied to a known species, whilst the other 4 are only known from Prof. Duncan's descriptions of fossil Antiguan corals. The authors are of opinion that the whole of the terraces of Barbados, the so-called "marl" of Antigua, and the fossiliferous rocks of Barbuda are of Pleistocene age. They proceed to notice the formations in other West Indian Islands which appear to be raised reefs comparable with those of Barbados, and show that these reefs occur through the whole length of the Antillean Chain, and indicate a recent elevation of at least 1300 feet, and in all probability of nearly 2000 feet. It appears improbable that each island was a region of separate uplift, and as a plateau of recent marine limestone also occurs in Yucatan, this carries the region of elevation into Central America; and it is reported that there are raised reefs in Colombia. The authors conclude that there has been contemporaneous elevation of the whole Andean Chain from Cape Horn to Tehuantepec, and of the Antillean Chain from Cuba to Barbados. Before this there must have been free communication between the Atlantic and Pacific Oceans, which is confirmed by the large number of Pacific forms in the Caribbean Sea. Under such geographical conditions the great equatorial current would pass into the Pacific, and there would be no Gulf Stream in the North Atlantic. The reading of this paper was followed by a discussion, in which the Rev. E. Hill, Mr. Attwood, Mr. Gregory, Mr. W. Hill, Mr. Easton, Dr. Blanford, and the President took part. The President remarked that the details supplied in the paper formed an important addition to the literature of the coral-reef question, showing as they did clear evidence of the elevation of old coral-reefs. He thought the speculations appended by the authors as to the changes in the level of the South American continent and Central America somewhat out of place, and hardly warranted by any of the observations recorded in the paper. No trifling submergence of the Isthmus of Panama would serve to divert the great equatorial current into the Pacific Ocean. Unless the downward movement had been more serious than the authors seemed to suppose, the bulk of the current would still sweep round into the Gulf of Mexico, only the upper waters passing into the Western Ocean.—The shap granite, and the associated igneous and metamorphic rocks, by Alfred Harker and J. E. Marr.

Entomological Society, February 4.—Mr. Frederick DuCane Godman, F.R.S., President, in the chair.—The President nominated the Rt. Hon. Lord Walsingham, F.R.S., Prof. R. Meldola, F.R.S., and Dr. D. Sharp, F.R.S., Vice-Presidents for the session 1891–92.—Mr. C. J. Gahan called attention to a small larva which he had exhibited at the meeting of the Society on October 1 last, when some doubt was expressed as to its affinities. He said that Prof. Riley had since expressed an opinion that the larva was that of a dipterous insect of the family *Blepharoceridæ*, and might probably be referred to *Hammatorrhina bella*, Löw, a species from Ceylon.—Mr. Tutt exhibited a long series of *Agrotis pyrophila*, taken last year by Mr. Reid, near Pitcaple, in Aberdeenshire, and he remarked that this species had been commoner than usual last year both in Scotland, the Isle of Portland, and the Isle of Man. He also exhibited long and variable series of *Melitæa aurinia* (*artemis*), *Triphæna orbona*, *Abraxas grossulariata*, and *Melanippe fluctuata*, all from the same locality in Aberdeenshire.—The Rev. Canon Fowler exhibited a cocoon of *Deiopeia pulchella*, recently received by him from Lower Burmah.—Mr. C. O. Waterhouse exhibited specimens of *Scyphophorus interstitialis*, a Mexican species, and *Aceraius comptoni*, a Ceylon species, recently taken by Mr. Bowring in his greenhouse. He also exhibited, on behalf of Miss Emily Sharpe, a specimen of *Daphnis hypothous*, Cramer, a native of Borneo, Java, and Ceylon, caught some years ago at Crieff, N.B. The specimen had long been confused with *Chærocampa nerii*, under which name its capture was recorded in the *Entomologist*, xiii. p. 162 (1880).—The Rev. Dr. Walker exhibited many species of Orthoptera and Scorpions recently received from Jerusalem.—Mr. Frederick Enock read an interesting paper entitled "The Life-History of the Hessian Fly." This paper was illustrated, by means of the oxy-hydrogen lantern, with a number of photographs of original drawings showing the fly in all its stages and transformations. Mr. G. H. Verrall said he believed the Hessian Fly was no more a recent introduction into this country than the Cabbage White Butterflies.—Mr. Roland Trimen, F.R.S., communicated a paper entitled "On some Recent Additions to the List of South African Butterflies."—Mr. Henry W. Bates, F.R.S., communicated a paper entitled "Additions to the Carabideous Fauna of Mexico, with remarks on species previously recorded."—Mr. W. F. Kirby read a paper entitled "Notes on the genus *Xanthospilopteryx*, Wallgr."—Dr. D. Sharp, F.R.S., contributed a paper entitled "On the Rhynchophorous Coleoptera of Japan," Part 2.

Linnean Society, February 5.—Prof. Stewart, President, in the chair.—Mr. Clement Reid exhibited and described some recent additions to the fossil Arctic flora of Britain.—Mr. Thomas Christy exhibited and made remarks on some specimens of honey: (1) "Arbutus honey," from Turkey, said to produce great drowsiness and sleep; (2) "Eucalyptus honey," from Mount Barker, Adelaide, said to possess valuable therapeutic properties; and (3) so-called "wool honey," from the Euphrates, collected by natives from the leaves of the oak, which would be more properly termed "honey-dew," being formed by Aphides, and not by bees.—Mr. J. E. Harting exhibited a living albino example of the Common Frog (*Rana temporaria*), captured in Wiltshire in September last, and remarked upon the infrequency of albinism amongst the Batrachia and Reptilia, of which he had only been able to find four or five recorded instances.—On behalf of Mr. Gammie, of Sikim, Mr. C. B. Clarke gave an abstract of an interesting paper on the tree ferns of Sikim, in which several moot points were discussed and difficulties cleared up.—The next paper was one by Prof. W. A. Herdman, on a revised classification of the *Tunicata*. Taking as a basis the scheme of classification adopted in his Report on the *Challenger* collection, he incorporated the various known genera and species not represented in this collection, and discussed the general principles to be recognized in classifying the *Tunicata*, especially dwelling on the value of the various modifications of the branchial sac, and of the tentacles. The polyphyletic origin of the group *Ascidiæ Compositæ* was pointed out, and the relations between Simple and Compound Ascidians were shown by means of a phylogenetic diagram.—A paper was then read by Prof. G. B. Howes, in which he gave a description of the genitalia of six hermaphroditic codfish examined by him, and a *résumé* of what is known on the general subject of hermaphroditism amongst fishes, more particularly referring to the *Teleostei*, which exhibited the most nearly primitive condition of the genital gland realized by living Vertebrata. He regarded the genital duct of the *Teleostei* as homologous in both sexes, representing a primitively hermaphrodite duct of the ancestral Chordata. He sought to homologize it with the proliferating mass described by Balfour and Sedgwick, Fürbringer and others, as entering into the formation of the base of the Müllerian duct proper, and regarded it as having been replaced by that structure on the advent of unisexuality. Several other points were touched upon of special interest to physiologists, and which want of space alone prevents being noticed.

Mathematical Society, February 12.—Prof. Greenhill, F.R.S., President, in the chair.—The Chairman informed the members present of the loss the Society had sustained by the recent death of Dr. Casey, F.R.S., and called upon Mr. Tucker to read a short obituary notice which had been drawn up by an intimate friend of the deceased. Dr. Larmor added a few sympathetic remarks.—Mr. Tucker communicated two notes on isoscelians, and Mr. Heppel read a paper on quartic equations interpreted by the parabola.—The Chairman read a note from Mr. W. E. Heal, of Indiana (communicated by Prof. Cayley, F.R.S.), on the equation of the bitangential of the quintic.—Mr. Tucker read an abstract of a paper by Mr. J. Buchanan, on the oscillations of a spheroid in a viscous liquid.

Cambridge.

Philosophical Society, January 26.—Prof. G. H. Darwin, President, in the chair.—The following communications were made:—On the electric discharge through rarefied gases without electrodes, by Prof. J. J. Thomson. A vacuum tube was exhibited in which an electric discharge was induced by passing the discharge of Leyden jars through a thread of mercury contained in a glass tube coiled four times along it. The induced discharge was found to be confined to the part of the vacuum tube which was close to the primary discharge, and it did not show striæ. It was also demonstrated that an ordinary striated discharge is strikingly impeded by the presence of a strong field of magnetic force.—On diffraction at caustic surfaces, by Mr. J. Larmor. A caustic surface is physically a result of diffraction, and consists of a series of parallel bright sheets whose distances apart are always in the same proportion, and at different places are absolutely proportional to the cube root of the curvature of the sheets in the direction of the rays, for light of given wavelength. The general character of the appearances is illustrated entoptically when the light that has passed through a water-drop on a glass plate is received into the eye, so that the caustic surface intersects the retina in a caustic curve, usually cusped.—The effect of temperature on the conductivity of solutions of sulphuric acid, by Miss H. G. Klaassen (communicated by Prof. J. J. Thomson. The viscosity of sulphuric acid solutions reaches a maximum at a dilution corresponding to a hydrate, $H_2SO_4 . H_2O$. The electric resistance shows the same general characteristics. The resistance curves are plotted for different temperatures, and show that the effect diminishes with rise of temperature, and finally tends to evanescence, a result which would be accounted for by gradual dissociation of the hydrate.

Paris.

Academy of Sciences, February 9.—M. Duchartre in the chair.—On Herr Wiener's experiment, by M. H. Poincaré. The author gave some mathematical objections to the theory proposed by M. Cornu to explain Herr Wiener's experiments on the direction of vibration in a beam of polarized light (*Comptes Rendus*, January 26).—Note by M. Berthelot, *à propos* of the preceding communication.—M. E. Becquerel submitted some of his photographic reproductions of the solar spectrum obtained more than forty years ago.—Determination of the masses of Mars and Jupiter by meridian observations of Vesta, by M. Gustave Leveau. The tabulated differences between observed and calculated places of Vesta show that the introduction of the secular perturbing influence of the asteroids does not sufficiently modify the residuals to take account of it in the formation of the tables of Vesta, but that the masses of Jupiter and Mars necessitate an appreciable correction.—On the conductivity of tribasic organic acids; a new characteristic of basicity, by M. Daniel Berthelot. It is shown that measurements of electrical conductivity furnish a new characteristic for the determination of the basicity of acids when their molecular weights are known.—On the combinations formed by ammonia with chlorides, by M. Joannis. Compounds of ammonia with chlorides of sodium, potassium, and barium, are described.—On the formation of isopurpurates, by M. Raoul Varet.—On the mode of combination of sulphuric acid in plastered wines, and on the detection of free sulphuric acid, by M. L. Magnier de la Source.—An olfactometer founded on the principle of diffusion across flexible membranes, by M. Charles Henry. The use of the instrument is to determine the weight of any odoriferous vapour per cubic centimetre of air, which corresponds to the minimum of perceptibility.—Action of certain substances used in medicine, and in particular of extract of valerian, on the destruction of glucose in the blood, by M. L. Butte.—On the manners and metamorphoses of *Emenadia flabellata*; facts relating to the biological history of Rhipiphori, by M. A. Chobaut.—On the development of the fins of *Cyclopterus lumpus*, by M. Frédéric Guitel.—A new fossil Cycad, by M. Stanislas Meunier.—On the coal basin of Boulonnais, by M. Gosselet.—On the presence of Upper Devonian in the Ossau valley (Lower Pyrenees), by M. J. Seunes.

CONTENTS.

PAGE

THURSDAY, FEBRUARY 26, 1891.

DOGS, JACKALS, WOLVES, AND FOXES.

Dogs, Jackals, Wolves, and Foxes: A Monograph of the Canidæ. By St. George Mivart, F.R.S. With Woodcuts, and 45 Coloured Plates drawn from Nature by J. G. Keulemans. (London: R. H. Porter, and Dulau and Co., 1890.)

THE group which Prof. Mivart has selected for a handsome, and, as far as materials are available, fairly exhaustive monograph, is one which has the advantage of being, in the present condition of the world's fauna, of strictly definite limits. On the other hand, the very numerous minor variations which occur within these limits give rise to many problems difficult of solution. While there is no difficulty in deciding what animals should be admitted into the *Canidæ*, the questions as to how many distinct specific modifications of the family should be accepted and what their relations are to one another are as difficult to answer in this case as in almost any other in zoology. It may indeed be doubted whether there will ever be any general accord upon such subjects in respect to any group of modern origin, and in which variation and differentiation are still rife. It is only in the ancient groups where extinction has played havoc among the members, and long-continued isolation has stereotyped certain definite forms that we can meet with species which will be universally recognized as such. In such a group as the *Canidæ*, therefore, opinions are sure to differ as to the number and limits of the species, and these opinions will be continually liable to modification in accordance with the amount of material upon which they are based.

We are glad to read in the preface of this work of "the rich and unrivalled stores of canine animals accumulated in the British Museum of Natural History," but we see ample evidence throughout the work that, far superior as this collection is at present to any which is available to a monographer of the group, much remains to be done before it can be considered exhaustive. The following observation under the head of *Canis azaræ*, is also applicable to other sections, and should be taken to heart by all concerned. "It is greatly to be desired that a numerous collection should be made of all the kinds of South-American dogs, the locality and sex of each individual being noted, as well as the time of year when it was obtained, the skull not being extracted from the skin, save at the Museum in which it may be deposited."

The present monograph has the great merit of placing in an accessible and attractive form all that is really known of the *Canidæ*, on what may be called a strictly conservative basis. If it only brings together, without greatly advancing our knowledge, it does absolutely nothing, as so many similar attempts have done, to confuse and retard it. Not a single new generic term, and only two new specific names are proposed—a mercy for which future zoologists may well be thankful. There are scarcely any speculations and certainly no assumptions on unknown and problematical subjects such as the ancestral history of the group treated of, or even on that very interesting but at present quite unsolved problem, the origin of the domestic dog. With regard to the first the author candidly avows, "Phylogeny, or the science of such evolution of forms of life, seems to us to be not merely in its infancy, but rather at a low stage of embryonic development. We have already seen the overthrow of a great many promising and carefully drawn out genealogical trees of life, and therefore feel little inclined to attempt now to construct the pedigree of the dog family."

The already established, though perhaps not universally admitted generic distinctions of the few aberrant forms, which can be definitely defined either by the number of the toes or of the teeth differing from those of typical *Canis*, are retained. These are *Lycaon*, with but four digits instead of five on the fore-feet, *Cyon* with two lower molars instead of three, *Icticyon* with a molar deficient, and *Otocyon* with one added, in either jaw. The whole of the other members of the group remain under the generic appellation of *Canis*, no value being attached to the numerous divisions of Gray, Burmeister, and others. Of this genus thirty species are admitted, including *C. domesticus*, upon the origin of which, as hinted above, Dr. Mivart is most cautious in hazarding any opinion of his own, though fairly stating the different views (amounting to eight in number) which have been or may be held by others, and adding:—"For our part we think that the evidence is as yet insufficient for us to enunciate any judgment in the matter. We have endeavoured to point out that it is possible that the origin of the dog may have been single or multiple, but we refrain from declaring that we regard either the one or the other as preponderatingly evident. Nevertheless our judgment inclines to the view that the domestic dog is a form which has been evolved by human effort from at least two, probably more, wild species, though it is possible it may be but a modification of one which has long become extinct save in its domestic and feral progeny."

Each of the recognized species is carefully described, mainly from specimens in the British Museum, both in its typical and varietal forms where such occur, and much useful information is given as to its general history, habits, and geographical distribution. Moreover, and this will be considered by many possessors of the book as one of its most valuable features, a coloured illustration is given of every species, and in some cases of several of the best marked varieties of each, there being forty-five of such plates. Fourteen of these are drawn from the actual type on which the species was founded. Of these figures, which are all original and hand-coloured, many are excellent, but it is difficult to say the same of all. In constructing drawings of animals from skins or indifferently stuffed specimens, although accuracy in detail of colouring may be secured, much must be left to the artist's skill as to the proportions, attitude, and expression, which in such a large number of figures of similar forms must tax his resources to the uttermost, and it is no wonder that some of them are rather stiff and wanting in life-like character. Perhaps we may be considered hypercritical in such matters, but we may instance Plate I., the common wolf, as a scarcely adequate representation of that familiar animal. On the other hand, the numerous woodcuts, giving details of cranial and dental characters, leave nothing to be desired, either in accuracy or artistic finish, and the book is, taken altogether, one which should be found in every complete zoological library.

PRIMITIÆ FL. SHAN.

On a Collection of Plants from Upper Burma and the Shan States. By Brigadier-General H. Collett, C.B., F.L.S., and W. Botting Hemsley, F.R.S., A.L.S. In Journal of the Linnean Society, vol. xxviii. (November 5, 1890). Pp. 1–150, tt. 1–22, with a Map.

UPPER BURMA is an arid plain, with an annual rainfall of about 30 inches, containing some isolated mountains. On the east of it are the Shan States, consisting of plateaus, mostly 3000–4000 feet above the sea, traversed by ranges of mountains, attaining 6000–7000 feet, running north and south, and having an annual rainfall estimated at 60 inches. The Shans occupy a large area of this hill country having Siam on the south, Tonquin on the east, and Yunnan on the north and northeast; they are a tribe of the great people denominated (generically) Kookies by Colonel MacCulloch, who extend (in the hills almost universally) from the bend of the Brahmapootra (at the west extremity of Assam) to the mountains north of Canton; the Kalangs in Java and the Mundas of Chota Nagpore may be outlying remnants of this people. The Shan States (or rather the south-west portion of them, for the political boundary of China is hereabout uncertain) had been more or less dependent on Burma. After the annexation of Upper Burma by the English, the Shans had to be got in; and, as is nearly always the case with imperfectly civilized peoples, it was found a necessary preliminary to the establishment of a beneficent despotism that a few should be shot. This duty, in 1887–88, fell to the lot of General H. Collett. No officer could have been selected who would have performed it more mercifully; he is a botanist. In the Shan States during this anxious service, and in the adjacent plain of Upper Burma (including the isolated Mount Popah, alt. 5000 feet), General Collett collected the 725 species of Phanerogams which form the *materies* of the paper we have under notice. Of these 87 are new to science, and several others were very imperfectly known previously. The richness of this collection is due to the fact that General Collett is not a mere collector, who gathers up everything in a new country that comes to his hand, and then sends it to Kew to be matched and named; but a botanist who could (in this remote ground almost cut off from books) determine the genus of most of the plants he lighted on *in the field*. But having to proceed to India to command the province of Assam, General Collett was obliged to leave the description of his collections largely to Mr. Hemsley.

The climate of the plain of Upper Burma is very much drier than that of Bengal and Assam; and the flora, only imperfectly known from the collections of Wallich and of Griffith, contains some plants of the north-west, central provinces, and south of India not known to inhabit the intermediate wet area of East Bengal, Assam, Chittagong, &c. Mr. Hemsley estimates that 25 species out of the 725 collected by General Collett are of this class; but this is perhaps rather an over-estimate, for out of the four *species* he names as absent in the connecting intermediate region, two, viz. *Priva leptostachya* and *Anisopappus Chinensis* are in quantity in the Khasi and Naga Hills (in Assam). But the fact, of a connection between the flora of the Upper Burma plain and that of the drier region of India, whether north-west, central, or south, is as Mr. Hemsley states it.

The flora of the Shan Hills is much richer, more interesting, and novel than that of the Upper Burmese plain. The country lies in a botanic *regio incognita* between the (more or less) known floras of Assam, Burma, and Tonquin (with Yunnan). The plateaus at 3000–5000 feet elevation are often bare; and, as is clear from General Collett's description, exceedingly like the open bare Khasi plateau (depicted by Sir J. D. Hooker in his "Himalayan Journal," vol. ii.). The flora is also very closely allied. It is difficult, without abstracting the entire list of new species, to give a worthy notion of them; but the most interesting having been selected for figuring, we give here the list of plates of the *new* plants:—

2. *Capparis Burmanica*, Coll. et Hemsl.
3. *Hypericum pachyphyllum*, Coll. et Hemsl.
4. *Impatiens ecalcarata*, Coll. et Hemsl.
5. *Crotalaria perpusilla*, Coll. et Hemsl.
6. *Neocollettia gracilis*, Hemsl.
7. *Phylacium majus*, Coll. et Hemsl.
8. *Bauhinia tortuosa*, Coll. et Hemsl.
9. *Rosa gigantea*, Coll.
11. *Lonicera Hildebrandiana*, Coll. et Hemsl.
12. *Inula crassifolia*, Coll. et Hemsl.
13. *Ceropegia nana*, Coll. et Hemsl.
14. *Brachystelma edulis*, Coll. et Hemsl.
16. *Strobilanthes connatus*, Coll. et Hemsl.
17. *Phacellaria caulescens*, Coll. et Hemsl.
18. *Sauropus concinnus*, Coll. et Hemsl.
19. *Bulbophyllum comosum*, Coll. et Hemsl.
20. *Cirrhopetalum Collettianum*, Hemsl.
21. *Polygonatum Kingianum*, Coll. et Hemsl.
22. *Lilium Bakerianum*, Coll. et Hemsl.

Of these *Rosa gigantea* is the largest wild rose (flowers 4 inches diameter) yet found; *Lonicera Hildebrandiana* is the largest honeysuckle (flowers 6½ inches long) yet found; the two orchids are extraordinary species, though located in old genera. *Neocollettia* (Hemsley, *genus novum*) is a 1-ovulate plant placed by Mr. Hemsley next *Phylacium*, but is probably as Mr. Hemsley suggests) one of the tribe *Phaseoleæ* (near *Rhynchosia*).

This collection enables us to estimate the wealth of the newly annexed territory; General Collett has added some observations on agriculture and economic botany. The Khasi pine is plentiful, and (as in several of the adjacent regions) valuable oaks are plentiful, 9 species having been collected by Collett. The area altogether included in General Collett's paper extends from 19° 30′ to 21° 30′ lat., and from 95° to 97° 30′ East long.; and contains probably not less than 4000 species of which Collett's collection gives us 725. Mr. Hemsley, in his part of the introduction, observes that the species are to the genera as 725 to 460, *i.e.* about as 1·6 to 1, and that these proportions closely approach those obtaining in many insular floras. But similar proportions usually occur when any area is imperfectly collected over. Thus in the very similar list of Muneypoor and Naga Hill plants (given in Journ. Linn. Soc., vol. xxv.) there are 924 Phænogams in 503 genera—*i.e.* a proportion of about 1·8 species to a genus. In this case the total flora would amount perhaps to 5000 species.

Mr. Hemsley concludes:—"The most interesting point, perhaps, connected with this collection has been left to the last. It is the large number of temperate types it contains from comparatively low elevations. Sir Joseph Hooker ("Himalayan Journ.," vol. ii. [ed. i.] p. 281) observed the same thing in the investigation of the Khasi Hills." This is a very curious point. Mr. Hemsley suggests that in the Shan Hills the comparatively small rainfall may have had much to do with it. But, near Cherra Poonjee, where Sir Joseph Hooker observed the fact, the annual rainfall varies from 400 to 650 inches. In both the Khasi and the Shan Hills the plateaus were probably once largely covered by jungle which has been (except patches) destroyed, while the district is kept in open grass (or small shrubs) by setting the grass on fire at the commencement of each cold season. In this way a large number of the temperate genera cited by Mr. Hemsley (Anemone, Delphinium, Silene, Stellaria, Hypericum, &c.) obtain a suitable habitat, which is denied them in the dense forest of the Eastern Himalaya at the same elevation (4000–7000 feet). But this is no adequate explanation. *Pinus longifolia*, which ascends to 7500 feet in the North-Western Himalaya, is rare above 3000 feet in Sikkim, and grows very fairly in the Bengal plain at Dacca. The Khasi pine, which grows mainly in the Khasi Hills at about 5000 feet altitude, descends nearly to sea-level in more tropical areas. And there are many such cases. The paper is put through the press by Mr. Hemsley with his usual literary finish. He might be asked why he prints *Physostelma carnosa* and *Brachystelma edulis*. He might reply that, according to "Propria quæ maribus," the names of all plants are female. Granting that, then why does Mr. Hemsley, who insists on uniformity from other botanic writers, write *Hypericum pachyphyllum*?

The present paper is produced by General Collett and the Linnean Society at their own private costs and charges. It would be a mistake to infer, therefore, that it is inferior in botanic merit to Aitchison's "Botany of the Afghan Delimitation Commission" or to Ridley's "Botany of Fernando Noronha"; and it has the additional value to Government that it deals with a territory (before unexplored) on its annexation to the English Crown. The thanks of all systematic, geographic, and economic botanists will be gratefully accorded to General Collett for his very valuable contribution to knowledge.

THE METHOD OF POLITICAL ECONOMY.

The Scope and Method of Political Economy. By John Neville Keynes, M.A. (London: Macmillan and Co., 1891.)

MR. KEYNES is known to the philosophical world as the author of what is in some respects the best treatise on a subject which has occupied the acutest intellects for above two thousand years—to wit, formal logic. Cultivated by so many skilful hands, that little branch of science might seem to have produced all the fruit of which it was capable. Yet it has been at least trimmed and pruned by Mr. Keynes not inelegantly. Nay, more; climbing up to that part where growth is still going on, he has gathered new fruits, or at leas brought down to earth those which had been before very difficult of attainment. We allude to the arduous and thorny problems of symbolic logic, which Mr. Keynes, in the work referred to, has translated into the language of ordinary life and common-sense.

Mr. Keynes's second claim to distinction is curiously similar to his first. His present subject has not, indeed, the venerable antiquity of formal logic. Yet the material logic of political economy has also occupied treatises which are already classical. Mill, the Aristotle of the subject, opened and almost exhausted it.

> "Quo nihil majus generatur ipso,
> Nec viget quidquam simile aut secundum;"

even if we concede to Mr. Keynes that Mill required to be corrected by himself: that the "Unsettled Questions" lag behind the "Political Economy." The general principles propounded by Mill appear to have been accepted by an influential majority of his successors; not excepting many names which are usually associated with the exclusively historical school. The remarks of Cohn, Roscher, and Wagner, which Mr. Keynes cites, are substantially in accord with Mill's. We say, substantially; for the phrase and emphasis have varied with the individuality of each writer, and the particular variety of heresy which he has had occasion to controvert.

Reviewing a long controversy, Mr. Keynes draws up, as it were, a confession of faith, so comprehensive and tolerant that, as it seems to us, only fanatics can find difficulty in subscribing to it.

The cardinal article is, of course, that which defines the relation between abstract reasoning and specific experience. Mr. Keynes holds the balance impartially between the *a priorists* and the historical school. He reasserts in modern words Mill's doctrine that "either acquirement without the other leaves one lame and impotent." In Prof. Cohn's words, cited by Mr. Keynes, "all induction is blind, so long as the deduction of causal connections is left out of account; and all deduction is barren, so long as it does not start from observation."

On the one hand, Mr. Keynes insists that all deductive reasoning rests on "hypotheses"; which do not indeed rest upon nothing, yet are seldom sufficient to support a practical conclusion. The person who confines himself to abstract reasoning must be content, as Mill says, to "have no opinion, or to hold it with extreme modesty, on the applications which should be made of his doctrines to existing circumstances." Mr. Keynes has no leaning towards the dogmatists who deduce *laisser faire* or any other practical rule from abstract notions. He deals another blow to, he speeds upon their road to extinction, those whom Dr. Sidgwick has described as "a scanty and dwindling handful of doctrinaires whom the progress of economic science has left stranded on the crude generalizations of an earlier period."

On the other hand, Mr. Keynes does not exaggerate the importance of experience and observation. He points out the uses of history in illustrating, criticizing and establishing economic theory. But at the same time he exposes the utter inadequacy of a merely inductive method to deal with the complex phenomena of industry and commerce. He quotes approvingly Bagehot: "If you attempt to solve such problems without some apparatus of method, you are as sure to fail as if you try to take a modern military fortress—a Metz or a Belfort—by common assault." Addressing extreme representatives of the exclusively historical school, Mr. Keynes condemns them out of their own mouth. Owing to that shifting character of economic conditions upon which they are always insisting, the study of the past becomes less applicable to the problems of the present day.

One difficulty in forming a judgment about the pretensions of the historical school is to know what they mean to assert beyond what every sensible person admits. "It is far from being easy," says Mr. Keynes, "to gain a clear idea of the form to be assumed by economics when its 'transformation' has been effected." His gentle reproof may be contrasted with the slashing sarcasm of a recent writer who applies to the historical school what has been said of the French, "that they don't know what they want, and will never be satisfied till they get it." Mr. Keynes, it must be admitted, is less incisive and epigrammatic than some preceding writers upon method. He is less entertaining to those who already agree with him; but perhaps he is more likely to convert those who differ from him.

The parallel which we have attempted to draw between the present and the earlier work of the author would not be complete if we did not point out that he has done more than merely consolidate the *dicta* of the earlier authorities. He has not only gone over the beaten road, mending it in places; but also, diverging into a comparatively fresh field, he has struck out a new path, or rather converted what was but a path into a highway. We allude to his remarkable chapter on symbolical and diagrammatic methods in political economy. Following upon Prof. Marshall's great work, in which these methods are so potently employed, Mr. Keynes's careful statement of their uses is likely to obtain general acceptance. Most of the previous writers who had taken much the same general view as to the functions of deductive reasoning had not considered that particular species of deduction which is effected through the channel of mathematical conceptions. Some eminent theorists were perhaps silent on this head from motives of discretion, knowing the hardness of readers' hearts. With others the cause may have been that which Dr. Johnson assigned for one of the mistakes in his dictionary, "Ignorance, madam, pure ignorance." Jevons, indeed, is a conspicuous exception. But his very zeal marred his advocacy. His personal connection with the cause impaired his authority. A plain man could hardly feel certain but that the new calculus was not a plaything like the "logical machine." Accordingly, when in the course of an impartial summing up of the claims of different schools, Mr. Keynes is far from condemning the mathematical method as trivial, his judicial utterances are likely to have a considerable effect. Very convincingly he dwells upon the appropriateness of mathematical symbols to represent the mutual dependence of variables and other leading conceptions of political economy. The peculiar genius of the economical calculus is thus seized:—

"Functions, while remaining numerically unknown, may possess known properties; and on the assumption that certain general relations between quantities hold good, it may be possible mathematically to deduce further relations that could otherwise hardly have been determined."

Altogether, it is a matter for rejoicing that the task of connecting and supplementing all the authorities on all the methods has devolved upon one so widely read, so impartial and exact. On a subject which it is undesirable to be always reopening, it may be hoped that Mr. Keynes's work will prove final. It may be expected that his second logical treatise will enjoy the same popularity as his first; for it deserves the same rare encomium—that, if a reader is under the necessity of confining himself to a single book upon the subject, the single book had best be that of Mr. Keynes.

F. Y. E.

OUR BOOK SHELF.

Mixed Metals, or Metallic Alloys. By A. H. Hiorns (London: Macmillan and Co., 1890.)

THIS is a useful little book, which will render good service to the craftsmen for whom it is evidently intended, and by whom such a work was much needed, for, with the exception of the translation of Guettier's "Guide Pratique des Alliages" and Brannt's "Metallic Alloys," there is no treatise on the subject, the information we possess being scattered through books and monographs which are difficult to obtain.

Mr. Hiorns begins with a reference to Gellert's "Metallurgic Chemistry," but he can hardly be familiar with even the English edition of Gellert's work published in 1776; he would have refrained from adopting the title of "Mixed Metals" for his book if he had had Gellert's clear indications as to the solvent action of metals on each other to guide him, supported as they are by Matthiessen's later experimental evidence, which led him to define alloys as "solidified solutions." It is true that in many cases, when masses of fluid alloys solidify, certain groups of constituents "fall out" of solution, but there is no known instance of a pure metal separating from the mass with which it was united, and remaining simply mixed. It is to be regretted that, for the sake of employing a term well known in the "metal trades," artisans should have an erroneous suggestion as to the nature of alloys conveyed by the very title of a hand-book.

In dealing with the effects of elements on metals, the indications are not quite as definite as could be wished. For instance, it is stated that "much arsenic is highly injurious" to copper, "making the metal hard and brittle"; but what should be considered *much* arsenic in a case of this kind? If certain metals were melted with gold, the ten-thousandth part of the added matter would impair the ductility of the gold, and would render it impossible for the jeweller to use it; but as regards the case in point—the action of arsenic on copper—a "trace" of arsenic would greatly diminish the value of the copper for electrical purposes, nevertheless the presence of as much as 1 per cent. would not be injurious if the copper had to be used for the fire-boxes of locomotives.

The author contributes the results of some important experiments of his own in connection with the manufacture and use of alloys of copper, nickel, and zinc, known as German silver. The sections which relate to alloys of iron with other metals are well done, as are those in which phosphor-bronze is dealt with. What is needed throughout the volume is fuller reference to authority, and graphic illustrations of the properties of alloys by means of curves. The author may, however, be said to have fully justified his claim to have offered practical men and students a book which will enable them to gain "a more intimate acquaintance with the nature and properties of metals in the alloyed state" than they have hitherto had for ready reference. W. C. ROBERTS-AUSTEN.

Grasses of the South-West: Plates and Descriptions of the Grasses of the Desert Region of Western Texas, New Mexico, and Southern California. Part I. By Dr. Geo. Vasey. Published by Authority of the Secretary of Agriculture. (Washington: Government Publishing Office, 1890.)

THIS work is issued by the United States Department of Agriculture, and is the twelfth Bulletin relating to botany which has been published by the Department. In this first part fifty uncoloured figures of the characteristic grasses of the south-west are given. The drawings are made by Mr. William Scholl, and the botanical determinations and descriptions are furnished by the veteran chief botanist of the Department, Dr. Geo. Vasey. The region of country immediately adjoining the northern boundary of Mexico, including the western part of Texas and the greater part of New Mexico, Arizona, and Southern California, is one of great heat and aridity. It is mainly a region of elevated plains, intersected by mountain ranges which occasionally run into high peaks, and is drained by comparatively few streams. In consequence of these climatic conditions the grasses become scanty, not in variety of species, but in individual quantity: some of them being short-lived, springing up rapidly after the summer rains, and soon dying away; others perennial, provided with deeply penetrating roots which enable them to bear the long droughts. Nowhere do the native grasses form a continuous herbage, as in our English meadows and pastures. The common grasses of the Northern and Eastern States are nowhere to be seen. This tract of country is getting more and more settled, and the most important agricultural problem before its inhabitants is how to increase the production of grasses and forage plants on the arid lands. It is very likely that this can best be done by bringing some of the native grasses into cultivation. The present work is issued mainly to give aid in this direction. A second part, containing fifty more plates, is in preparation; and this will be followed by a synopsis of all the grasses which grow wild in the district. Amongst the natives which are specially recommended for trial are *Panicum bulbosum*, *Stenotaphrum americanum*, *Hilaria mutica*, *Andropogon saccharoides*, *Bouteloua aristoides*, and *B. eriopoda*. There is a native species of millet, *Setaria caudata*.

The figures are very characteristic, and accompanied by botanical dissections. A large proportion of the species belong to Chlorideæ, a tribe which is scarcely represented in the European flora; and only two of them to Festuceæ, which contains the great mass of our European pasture grasses. On the agricultural bearings of the question it is likely that the Department might consult with advantage Dr. Schomburgk, Baron von Mueller, Mr. Bailey, and other botanists as to what has been attempted in Australia, which species have succeeded there as forage plants, and which have been tried and failed.

J. G. B.

Prodomus of the Zoology of Victoria. By Sir Frederick McCoy, M.A., &c. Decades 18, 19, and 20. (London: Trübner and Co., 1889.)

THESE three decades complete vol. ii. of this well-illustrated natural history of Victoria. Of the thirty coloured plates in these parts, four are devoted to Reptiles, seven to Fishes, three to Mollusca, nine to Polyzoa, two to Insects, four to Crustacea, and one to Echinoderms. Among the more noteworthy species figured may be mentioned—*Cyclodus occipitalis*, very rare in Victoria; the great red king crab (*Pseudocarcinus gigas*), from life; *Sepia apama*, which, though one of the commonest species of cuttlefish, does not appear to have been figured before; *Trachinops caudimaculatus*, McCoy, a little fish which created a great sensation by appearing in large numbers, about the middle of October 1884, off the piers at Williamstown, in Hobson's Bay, and being reported to the Commissioner of Customs as the young of the Californian salmon, introduced by Sir Samuel Wilson. The publication of such figures as are to be found in these decades will not only help to prevent such mistakes in the future, but will also be a direct means of calling attention to animals important from an economic point of view. Figures of *Pyrameis itea* and of *P. kershawi*, with their larval and pupal forms, are given; this latter species is very closely related to our own "painted lady," the three lower spots on the posterior wings in the Australian form are of a bright cobalt blue in their centre, instead of black. In the latter end of October and beginning of September 1888, this butterfly appeared in extraordinary numbers for two or three weeks, almost darkening the sky with their general flight towards the south-east, covering the gear and decks of ships many miles out at sea, and filling the air on land from the northern parts of the colony down south to Melbourne. They were accompanied by a day-flying moth (*Agrotis spina*).

Annals of a Fishing Village. Drawn from the Notes of "A Son of the Marshes." Edited by J. A. Owen. (Edinburgh and London: Blackwood and Sons, 1891.)

EVERYONE who has seen much of the marshlands is aware that to a lover of Nature they have a peculiar charm of their own, and that even now, when local individuality is everywhere being so rapidly effaced, there is something characteristic in the manners and customs of the marshmen. These special qualities are well brought out in the present volume, the substance of which, according to the editor, is "from the life," although real names are not given. "A Son of the Marshes," whose notes have been worked up by Mr. Owen, has had ample opportunities of becoming familiar with every phase of Marshland; and there are in the "Annals" many passages which show that he is a keen and accurate observer.

Solutions of the Examples in Elementary Algebra. By H. S. Hall, M.A., and S. R. Knight, B.A. (London: Macmillan and Co., 1891.)

THE authors of this book seem to have taken great trouble in securing accuracy: although we have worked out many of the examples taken at hazard, no errors have been brought to light. By making a judicious use of the examples, the student will find himself materially helped, especially if he is studying the subject without the aid of a teacher. We may also recommend this key to teachers, who will find much of their time saved by having it in their possession.

British Ferns, and where Found. By E. J. Lowe, F.R.S. (London: Swan Sonnenschein, 1891.)

THIS volume belongs to the "Young Collector Series," and presents an immense mass of carefully-arranged information on the subject with which it deals. The author has been a cultivator of British ferns since 1842, so that he is thoroughly and practically familiar with them, and knows exactly what are the kinds of facts for which a collector would be likely to look in a work of this sort. The book ends with a series of useful hints to fern cultivators.

LETTERS TO THE EDITOR.

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

An Assumed Instance of Compound Protective Resemblance in an African Butterfly.

Hamanumida dædalus, Fabr., generally quoted by its better known synonym *Aterica meleagris*, has been recorded as a good instance of protective resemblance.

Mr. Wallace ("Darwinism," p. 207) writes:—"A common African butterfly (*Aterica meleagris*) always settles on the ground with closed wings, which so resemble the soil of the district that it can with difficulty be seen, and the colour varies with the soil in different localities. Thus specimens from Senegambia were dull brown, the soil being reddish sand and iron-clay; those from Calabar and Cameroons were light brown with numerous small white spots, the soil of those countries being light brown clay with small quartz pebbles; while in other localities where the colours of the soil were more varied the colours of the butterfly varied also. Here we have variation in a single species which has become specialized in certain areas to harmonize with the colour of the soil."

Now in the Transvaal this butterfly never settles on the ground with closed wings, and the only example sent from Durban by Colonel Bowker to Mr. Trimen was described as "settled on a footpath with wings expanded" ("South African Butterflies," vol. i. p. 310). I have seen and captured a number of specimens in this country, and always found them with wings expanded and nearly always on greyish coloured rocks or slaty hued paths, with which the colour of the upper surface of their wings wonderfully assimilated. We have large tracts of bare ground of a reddish-brown colour with which the under surface of the wings would be in perfect unison, and for months I have watched to see a specimen thus situated and with its wings vertically closed, but without success.

If the reports as to its habits from Senegambia, Calabar and Cameroons are correct, and I believe for the last localities the authority was the late Mr. Rutherford (but I do not possess the necessary reference here), then we not only have a change of habit with difference of latitude, but also what I have ventured to style a "compound" condition of protective resemblance. For we thus see that while in Senegambia, Calabar and Cameroons, where according to report the butterfly always settles with wings closed, and which "so closely resemble the soil of the district that it can with difficulty be seen, and the colour varies with the soil in different localities," here in the Transvaal and Natal where it rests with expanded wings, its protection is almost equally insured by the assimilative colour of the upper wings to the rocks and paths on which it is usually found.

W. L. Distant.

Pretoria, Transvaal, January.

Cultivation of India-Rubber.

In Nature of January 15, in a note on p. 355, a statement is quoted, to the effect that some few attempts have been made to cultivate india-rubber, but as yet not very successfully. As, however, there are extensive flourishing plantations of *Ficus elastica* in Assam, a short account of their origin and present condition may prove interesting.

After some preliminary experiments on a small scale, the Government of Assam in 1873 determined to plant caoutchouc in the Charduar Forest at the foot of the Himalayas, north of Tezpur. Mr. Gustav Mann, the Conservator of Forests, gave me the necessary instructions to start the work in November of that year, and I remained in charge of the plantation till September 1875. The Charduar Forest has an essentially damp climate, the average rainfall at the caoutchouc plantation having been 94·65 inches during the years 1878–85, and during 1886–89, the annual rainfall was distributed as follows:—

Winter Rainfall.		1886–87.		1887–88.		1888–89.
November till March ...	Inches	4·87	...	7·38	...	4·78
Summer Rainfall.		1886.		1887.		1888.
April till October ...	,,	99·30	...	71·55	...	82·39
Total ...	,,	104·17	...	78·93	...	87·17

Data for the temperature of the Charduar Forest are not available, but the following average figures for ten years, for Sibsagar, which lies to the south-east of Tezpur, across the Brahmaputra River, will give sufficiently approximate results:—

Average annual temperature	73°·4 F.
Average monthly for January (lowest) ...	59°·0 F.
Average monthly for July (highest)	83°·7 F.

The absolute maximum and minimum temperatures for Sibsagar are not given in the meteorological tables from which the above figures are taken, but, quoting from memory, they are for Tezpur about 95° and 42° respectively.

The relative humidity for Sibsagar averages 83 per cent., being lowest in March, 79 per cent., and highest in January, September, and December, at 85 per cent. It is certainly not less than this in the Charduar Forest, where the moist hot atmosphere in the summer months resembles that of a forcing house. The Charduar Forest contains a vast number of woody species, both evergreen and deciduous, but chiefly the former, nearly pure woods of *Mesna ferrea* and *Altingia excelsa* prevail in the higher parts of the forest, and the undergrowth consists of dwarf palms, small bamboos, and evergreen shrubs, *Caffea bengalensis* being abundant in places, whilst cane palms are found in the damper parts of the forest, and festoon the trees in company with other huge climbers. A few enormous old rubber-trees are disseminated here and there throughout the forest. *Ficus elastica* has here been measured 129 feet high, with a girth around the principal aërial roots of 138 feet, whilst the girth of its crown was 611 feet.

As rubber-trees cannot stand shade, and the seedlings damp off unless fully exposed to light and well drained, the natural reproduction of *Ficus elastica* generally takes place in the forks of stag-headed or lightly foliaged trees high up in the crown, where they are left by birds; and from such a site the aërial roots in process of time descend to the ground, and develop into a vast hollow cylinder around the foster stem, which is speedily inclosed and completely killed by the vigorous crown of the epiphyte, which eventually replaces it in the forest. In its epiphytic growth, the aërial roots of *Ficus elastica* may take several years to reach the ground, but, once well rooted, nothing can probably surpass it in its native habitat for rapidity of growth and vigour.

As, owing to the above mode of growth, rubber-trees are so widely scattered in the Assam forests, and it is therefore extremely difficult to protect them from being tapped in a wasteful manner, the plan of concentrating them in artificial plantations, as proposed to the Government by Mr. Mann, was carried out as follows:—

At first, attempts were made to propagate by cuttings, which struck readily, but it was soon discovered that rubber-seed germinates freely on well-drained beds covered with powdered charcoal or brick-dust, and that the seedlings, though at first small as cress, grew rapidly, and became about 2 feet high in twelve months, and were much hardier against drought than plants produced from cuttings. The base of the stem of the seedlings swells out like a carrot, and this fact, no doubt, enables them to tide through the dry season in safety, for, in spite of the humidity of the air, the nearly constant sunshine from November till March is trying to young plants.

In order to imitate Nature as much as possible, some strong seedling rubber-plants were placed in the forks of trees in 1874, and by 1885 only a few of them had reached the ground and were growing most vigorously.

As this method, though much more economical than planting on the ground, gave such slow results, and it was found easy to produce plants in any quantity from seed, large nurseries were formed, in which the plants are now retained until they are 10 feet high, as smaller plants were browsed down by deer when planted out in the forest. The planting lines are cleared to a breadth of 40 feet in strips, separated by alternating strips of untouched forest 60 feet wide.

It was found that the rubber-plants did not get sufficient light with lines less then 40 feet broad, whilst the strips of forest kept the soil and atmosphere moist, and afforded side shelter to the plants, forcing them to grow upwards, instead of branching out near the ground. As this method involves considerable expense in clearing the lines, and wastes the wood, which is frequently unsaleable, Colonel, now General Keatinge, the Chief Commissioner of Assam, in 1874, directed that plantations of *Ficus elastica* should also be made in grass-land near Tezpur. It has been, however, found that large rubber-trees in Tezpur, when tapped, yield scarcely any rubber, the difference between them and the rubber-trees of the Charduar Forest being probably due to the greater dampness of the atmosphere and soil in the latter locality, as compared with the Brahmaputra Valley.

An area only of 8 acres was therefore planted out near Tezpur, whilst the area of the Charduar plantation in 1889, was 1106 acres, and contained 16,054 plants, besides large nurseries with 84,000 seedlings.

Local Governments in India, which have to find funds for all sorts of administrative purposes, are naturally inclined to economize, and Sir Charles Eliot, when Chief Commissioner of Assam, about ten years ago, proposed to stop further work on the Charduar plantation, but this was vigorously opposed by Dr. Schlich, the Inspector-General of Forests, and at his advice, the Government of India directed the further extension of the plantation. Apparently, however, little progress was made between 1881 and 1888, when an additional area of 63 acres was planted up. Regarding the growth of the plants, the following figures, taken from Mr. Mann's report on the Assam forest administration for 1888-89, give the average height and girth, up to April 1889, of 50 trees in each year's planting :—

Year when planted.	Average				Growth since last year.			
	Height.		Girth.		Height.		Girth.	
	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.
1874-75	61	11	11	5	6	1	0	9
1875-76	57	6	7	10	5	2	0	6
1876-77	55	10	7	5	3	7	0	6
1877-78	53	9	5	11	5	3	0	7
1878-79	46	2	4	6	4	0	0	5
1879-80	44	10	5	2	5	9	1	2
1880-81	38	7	4	2	6	7	0	8

Thus, we see that the present average annual growth in height and girth, taken from 350 plants, are respectively 5 feet 2 inches and 8 inches.

In the small Tezpur plantation, where there are now 794 plants, all of 1874, the average height and girth are 47 feet 3 inches and 10 feet 10 inches respectively, the average growth in one year being 4 feet 4 inches in height.

The up-keep of the plantation consists chiefly in clearing the lines round the plants, but four years after planting the undergrowth is well kept down by the shade of the rubber-trees.

Experimental tappings were made in 1883 and 1884 on 50 natural grown rubber-trees in the Charduar Forest, the total yield being 438 pounds in 1883, and 206 pounds in 1884, giving an average yearly yield of 6½ pounds per tree. Further information regarding the yield of rubber from trees in the Assam forests would doubtless be procurable from the Assam Forest Office, as well as statistics of the cost of the plantations, which are not given in the papers at present before me. W. R. FISHER.

Cooper's Hill College, February 18.

Snow on the Branches of Trees.

FOLLOWING upon the remarkable ice-storm of which I wrote you last month (NATURE, February 5, p. 317), we have had a wonderful and beautiful display of the amount of snow which the branches of trees can bear. There had been the beginning of an ice-storm on Sunday last; and on Monday, the 9th instant, there followed a damp but light snow which fell rapidly in a calm atmosphere. The whole appearance of trees and air and sky was very beautiful. Some of the trees caught a large quantity of snow, fastened to the branches in a form resembling elliptical cylinders, of which the lower lines of the branches were elements. I made some measurements on the extremities of drooping branches of an elm-tree on our lower campus. One twig with a diameter of 0·21 inch sustained a mass of snow with diameters of 2·50 and 2·33 inches; a second, 0·15 inch in diameter, carried snow the diameters of which were 2·30 and 1·93 inches; so that the area of the cross-sections of this snow was not far from 120 and 153 times that of the twigs which supported it. Two other measurements were still more remarkable. In one the twig was one-tenth of an inch in diameter, and the mass of snow had diameters of 2·40 and 1·75 inches, making the area of the sections 420 times that of the wood; and in the other, a twig 0·11 inch in diameter, carried snow with diameters of 2·50 and 2·05 inches, so that the area of the sections of the snow was 424 times that of the wood. The snow was so loosely attached to the branches that it seemed impossible to make accurate measurements of the weight of the twigs and of the snow which was piled up on them; but the ratio of the weights was, of course, by no means equal to the ratio of the sections—which was practically that of the bulks—of the wood and the snow. SAMUEL HART.

Trinity College, Hartford, Conn., U.S.A., February 12.

Elementary Systematic Chemistry.

I DO not as a general rule approve of a reply to a review, and so far as regards the part of the notice which refers to the short Chemistry which I have lately brought out, I have merely to thank your reviewer for the hints he has given in the last paragraphs, and for the friendly tone of his criticism.

But his first remarks open up a wide field, which appears to me to form a legitimate subject for discussion apart from the merits or demerits of any particular text-book.

The discussion turns on the age at which it is intended to begin the *study* of chemistry. I am all in favour of "children" playing with matter in all its forms; getting to know the appearance and properties of things in general; just as, indeed, very young children learn many useful facts from handling toys, and from the objects which daily come under their notice. But I do not acknowledge that playing with chemical materials constitutes a *study* of chemistry. To pursue your reviewer's simile, a child plays with language; he learns to speak it, read it, and possibly write it reasonably well. But when he begins to study language, he must learn grammar. I am equally in favour of learning a foreign language by conversation and by promiscuous reading; but to *know* French and German, we must study the classification and derivation of their words, their connection with each other, and the means whereby they may be combined to form correct sentences.

Now my experience has been that the ordinary text-books of inorganic chemistry convey a large amount of heterogeneous information—the facts grouped in a certain order, it is true, but not in such order as to lead the beginner to generalize and classify. And indeed this is tacitly acknowledged by your reviewer in his remark, "The student who is already fairly acquainted with the subject will find the summary of properties, &c., of much use," implying that such summaries are not easily obtained from ordinary text-books. The beginner in chemistry acquires a vast amount of information on isolated facts; great demands are made on his memory, and many students have formerly hinted to me that they find the study hopeless; there is so much to be remembered, and so little connection between the facts. This short text-book has been written with a view of removing this obstacle. In it facts are classified, general methods are set forth, and the properties of compounds common to all members of a group are to be found together. In the preface, I have emphasized these views; but I did not intend to exclude the acquisition of general knowledge of matter, which may be acquired as thoroughly by this arrangement as by any other.

I am quite aware that a complete comprehension of the periodic law is beyond the young student; but a boy of fourteen or fifteen will learn to understand it better by this method than by any other, and at first he may accept the statement that experience (the experience of the writer, at least) has proved it to be the best way of presenting the subject.

WILLIAM RAMSAY.

University College, Gower Street, W.C., February 20.

Frozen Fish.

IT is not uncommon for small fish to be frozen to ice in shallow water. In 1838 I put some of these frozen fish into tepid water, and they recovered. In 1852, in one part of the lake at Highfield House, there must have been hundreds frozen to the ice, but when it melted scarcely any dead fish were seen: they had either recovered or had been devoured by pike. In 1860 a number of gold-fish were hard frozen, but recovered in warm water.

When ice is transparent the fish seen beneath are apparently healthy; indeed, there always seem to be air-bubbles sufficient to sustain the life of fish.

I never saw pike, carp, tench, perch, or trout frozen to ice.

Shirenewton Hall, February 22. E. J. LOWE.

THE ZOOLOGICAL STATION OF NAPLES.

AT the recent meeting of the British Association at Leeds, much difficulty was experienced in obtaining the renewal of the vote for the occupation, by a British naturalist, of a table at the Zoological Station at Naples—a grant which has received the sanction of the Committee of Recommendations of the Association for many successive years. It was alleged, we believe, that the Zoological Station at Naples was used by those sent to it rather for educational purposes than as a place for original research, and objections were also raised, perhaps with greater force, to the policy of continuing to support an already thriving institution for an indefinite period. Had it not been for the munificence of an individual member of the Association, Captain Andrew Noble, C.B., F.R.S., who kindly offered to supply the debated sum of £100, the Association could not have continued, during the present year, to send naturalists to work at the Naples Station.

So far, however, from being mainly used for education, as was affirmed by some of its criticizers, Dr. Dohrn's station at Naples (for, as Dr. Dohrn is its founder, director, and proprietor, we may fairly call it by his name) is, it may be truly said, almost entirely devoted to original investigations. The investigators, no doubt, get a large amount of education out of their work, but the leading idea at Naples is *research*. All the minor works at the station are subordinated to this leading idea. As we have lately established in this country an institution with nearly similar aims and objects—we mean the Laboratory of the Marine Biological Association at Plymouth—it may be useful shortly to review the state to which the institution at Naples has arrived after a career of twenty years, and to show an ideal to which its British imitator may, we trust, hope to attain after a certain period.

The Zoological Station of Naples consists of two buildings, connected by a gallery, and placed on the Chiaja, in the beautiful public garden which occupies part of the strand of the world-renowned Bay of Naples. The lower portion of the larger building contains a long series of tanks arranged for public inspection, so as to give sightseers a sample of the fishes and other marine wonders of the Bay of Naples. This portion of the building is open to the public, at stated hours, at an admission fee of two francs, and produces a revenue of about £1000 a year to the institution. The large tanks which it contains are at the same time very useful as storehouses for the specimens required by the students. The whole of the upper stories of the larger building, and the whole of the smaller building are devoted entirely to scientific purposes. They contain the naturalists' working tables and tanks, the library, the studies and apartments of the Director and other officials, and the rooms used for the reception, preservation, and storage of specimens.

There is room in the buildings at Naples for about fifty naturalists' "tables," by which term is designated not merely the table itself, but the adjoining tank for specimens and every other sort of accommodation required for work in any branch of marine zoology. The tables actually rented continuously from year to year (at £100 each) are about twenty in number. Of these Prussia takes four, Baden one, Bavaria one, Saxony one, Hesse one, and Wurtemberg one, making altogether nine occupied by the various German Governments. Of nations foreign to Germany, Italy takes no less than seven, five of these being rented by the Ministry of Public Instruction, and two by that of Agriculture. France, in view of national prejudice, and having zoological stations of her own, could perhaps be scarcely expected to subscribe to what is essentially a German institution; but Switzerland, Hungary, and Holland each take a table, and the University of Cambridge occupies the only table rented in England, if, as now seems possible, that of the British Association will be given up. Besides these twenty "certain tables," others, varying in number from eight to sixteen, are let every year. These are taken by Russia, Belgium, Austria, Spain, and occasionally by some of the Italian provincial Governments. On the whole, this second "uncertain" series may be reckoned to number about ten on an average of years. Thus altogether thirty naturalists' tables are let and tenanted, and produce a revenue of about £3000 a year to the institution.

A certain number of these tables are occupied throughout the year, but in the height of summer the workers are reduced to a minimum, while in the early spring, perhaps, they attain their maximum number. At such an intermediate period as November 18 last, when the writer of this article had last the pleasure of visiting the establishment, eighteen naturalists were found to be in full work. The following list gives the names of these gentlemen, together with the names of the tables which they occupied and the objects of their various studies:—

Naturalists.	Residence.	Tables Occupied.	Objects of Study.
Dr. G. Jatta	Naples	Italy	Monograph of Cephalopods.
Dr. G. Cano	Sassari	,,	Crustaceans, system and embryology.
Dr. C. Crety	Rome	,,	Anatomy of Entozoa.
Dr. F. Monticelli	Naples	,,	Helminthozoa, system and anatomy.
Dr. G. Mazzarelli	,,	,,	Anatomy of Gastropods.
Dr. S. Pansini	Molfetta	,,	Bacteriology.
Dr. Salviati	Naples	,,	,,
Dr. M. Verworn	Jena	Prussia	Physiology of Protozoa and Cœlenterata.
Dr. M. Mendthal	Königsberg	,,	Anatomy of Nereidæ.
Dr. C. v. Wistinghausen ...	Berlin	,,	Embryology of Annelids.
Dr. A. Looss	Leipzig	Saxony	General study of the fauna.
Dr. J. Loeb	Strassburg	Strassburg	Physiological experiments on Cœlenterata and Worms.
Dr. M. Davidoff	Munich	Zoological Station	Monograph of Appendicularia.
Mr. W. Melly	Liverpool	British Association	Anatomy of Sponges.
Dr. J. Koningsberger ...	Utrecht	Holland	Embryology of Nemerteans and Nudibranchs.
Sr. Rioja y Martin	Madrid	Spain	General study of fauna.
Lieutenant Borja	,,	,,	Conservation of marine animals.
Lieutenant Anglada	,,	,,	,, ,, ,,

But these were by no means the only naturalists at work in the Zoological Station of Naples in November last. Besides the eighteen regular occupants of the "tables," the following members of the Station carried on scientific work whenever leisure from their ordinary duties permitted them to do so:—

Members of the Staff of the Zoological Station.	Subjects of Work.
Prof. A. Dohrn (Director)	Comparative embryology of Vertebrates.
Prof. H. Eisig	Anatomy and embryology of Annelids.
Prof. P. Mayer (Editor of Publications)	Morphology of Vertebrates.
Dr. W. Giesbrecht	Monograph of the Copepoda.
Dr. P. Schiemenz (Librarian)	Mollusks: monograph of the Pteropods.
Dr. F. Raffaele	General fishery questions. Development of the skeleton in Vertebrates.
Dr. W. Kruse	Bacteriology.
Dr. E. Herter	Physiology, chemistry.
Dr. Schöbel	Branchial apparatus of Selachians, microscopic drawing.

The whole number of naturalists that have occupied "tables" and worked at the Zoological Station of Naples since its opening, some twenty years ago, is 575. Of these, 228 have been Germans, 127 Italians, 52 English, 48 Russians, 32 Dutch, 26 Austro-Hungarians, 23 Swiss, 18 Spaniards, 14 Belgians, and 4 Americans, while the remaining 12 were of various other nationalities. Much of the good work that has thus been produced has been scattered abroad over the world in articles contributed to different scientific periodicals. But a portion of it, sufficiently solid to show its general character, has been published in a noble series of memoirs on various departments of the flora and fauna of the Bay of Naples, which now extends to sixteen elaborate and abundantly illustrated quarto memoirs. These are:—

(1) "Ctenophoræ," by Dr. C. Chun, 1880, with 18 plates.
(2) "Fierasfer," by Dr. C. Emery, 1880, with 9 plates.
(3) "Pantopoda," by Dr. A. Dohrn, 1881, with 17 plates.
(4) "Die Corallinenalgen," by Graf zu Solms-Laubach, 1881, with 3 plates.
(5) "I Chætognathi," by Dr. B. Grassi, 1883, with 13 plates.
(6) "Die Caprelliden," by P. Mayer, 1882, with 10 plates.
(7) "Cystoseiræ," by R. Valiante, 1883, with 15 plates.
(8) "Bangiaceæ," by Dr. G. Berthold, 1882, with 1 plate.
(9) "Le Attinie," by A. Andres, Vol. I., 1884, with 13 plates.
(10) "Doliolum," by Dr. B. Uljanin, 1884, with 12 plates.
(11) "Polycladidea," by Dr. A. Lang, 1884, 2 Parts, with 35 plates.
(12) "Die Cryptonemiaceen," by Dr. G. Berthold, 1884, with 8 plates.
(13) "Die Koloniebildenden Radiolarien," by Dr. K. Brandt, 1886, with 8 plates.
(14) "Polygordius," by Prof. J. Fraipont, 1887, with 16 plates.
(15) "Die Gorgoniden," by G. v. Koch, 1887, with 10 plates.
(16) "Die Capitelliden," by Dr. H. Eisig, 1888, 2 Parts, with 37 plates.

Besides these memoirs, eight successive volumes of a yearly journal entitled *Mittheilungen aus d. zoologischen Station zu Neapel*, containing smaller contributions to science, have been published during the past twelve years, and, since 1879, a *Zoologische Jahresbericht*, containing a summary of the advances made in the different branches of zoological knowledge during each year, has been regularly issued. In these three undertakings we have ample testimony to the great amount of work carried on at Naples by Dr. Dohrn and his coadjutors, and to the excellent results which they have arrived at.

Having described the nature of the business transacted at the Zoological Station at Naples, let us now consider the cost at which it is carried on, and the means whereby the necessary funds are obtained. Taking the expenditure of the last three years as a basis, we find, from figures kindly supplied to us by Dr. Dohrn, that about £2400 are required for the general expenses of management—that is, for stocking the tanks, preserving specimens, keeping up the laboratories, machinery and pumps, providing additions to the library, and paying taxes and other outgoings. A similar sum of about £2400 is spent on the salaries of the officials, the higher officers (twelve in number) receiving from £220 to £72 per annum, and the lower grades (34 in all) ranging from £72 to £15, the last-mentioned sum being the wages paid to boys

These are the two most serious items on the outgoing side, and make together £4800, besides which about £1300 are required for interest on the debt and sinking-fund, £200 for accumulation towards a pension-fund, which was commenced two years ago, and £100 for the publications, which cost about £1400 a year, and only produce a return of about £1300. Thus the total yearly expenditure of the Zoological Station at Naples, as at present carried on, may be reckoned at about £6400. To meet these annual requirements an income which has averaged about £4800 during the past three years is available. As already stated, the receipts from the admission of visitors amount to about £1000, while the thirty tables, let at £100 a year for each table, produce a revenue of £3000. Besides these principal items, the sale of specimens preserved at the Station produces about £700, and that of waste materials of various sorts another £100. Thus the whole income derived from the institution itself reaches only about £4800, and the Station would be carried on at a considerable annual loss were it not for the magnificent subsidy of £2000 a year granted to its support by the German Empire, which just covers the deficiency. This is a good example of the liberal way in which science is encouraged and supported in the "Fatherland," and is the more noteworthy because the object of its well-bestowed bounty in this instance is localized on foreign soil, and, though established and carried on by a German citizen, is by no means restricted for German subjects. We may appropriately contrast this with the conduct of the Government of our own country, which, in the case of the corresponding institution at Plymouth, situated in England, and founded and carried on mainly if not entirely for the benefit of British subjects, could only be persuaded to grant a subsidy of £500 a year for a limited period of five years. P. L. S.

ATTRACTIVE CHARACTERS IN FUNGI.

ON the recent introduction of this subject into the columns of NATURE it was understood, if not so expressed, that the inquiry was to be practically limited to the Hymenomycetal fungi, with the view of restricting it within a definite compass, and preventing too discursive a discussion. The limit was a very natural one, and included the best known and most appropriate objects for exhibiting the presumed attractiveness. Allusions have been made to another remarkable group, the *Phalloidei*, but facts applicable to this group would scarcely serve as illustrations of the *Agaricini*. Moreover, it must be admitted that with the *Phalloidei* the difficulties in the way of arriving at a conclusion are small. Strong fœtid odour and bright coloration are features almost universal, the object of which may fairly be accepted as attractive, to the end that the minute spores may be distributed, and the continuity of the species preserved. On this point nothing has been adduced beyond what is contained in Mr. Fulton's communication in the *Annals of Botany* for May 1889, on "The Dispersion of the Spores of Fungi by the Agency of Insects."

As regards the *Hymenomycetes*—that is to say, fungi of the mushroom type, with naked spores—the question appeared to resolve itself into this: Are such characters as colour, odour, &c., attractive, and if so to what members of the animal kingdom, and for what purpose? Incidentally, and apart from this, the side issue has been raised whether other characters, such as viscidity, inconspicuous coloration, &c., are not in some sense protective, for it has not been urged, nor do we think there was any ground for urging, that such latter characters were attractive, except perhaps the allusion to *Agaricus radicatus* by Mr. Worthington Smith, which does not seem to be at all conclusive.

Although not expressly stated, there seems to be an undercurrent of feeling amongst some correspondents that colour and odour are attractive to insects for the purpose either of fertilization or the dispersion of spores. This may be so, but there is no evidence, in the facts elicited, to support it—nothing to show that the attractive species are more in need of extraneous aid than unattractive. And, as to the subject of fertilization, the mystery is still unsolved, whether or no any special act of fertilization takes place, and if so, whether each individual spore is fertilized, or whether there is a fertilization of the young plant in the embryonic condition, or in its earliest stages, as some have contended, rendering all its spores fertile. This subject has been discussed over and over again during the past half-century, and has not been left, as one correspondent seems to think, disregarded. Dr. Karsten, in 1860, and M. Oersted, in 1865, held that upon the threads of the mycelium the male and female elements combined in the production of the rudimentary cap which developed into a fertile individual. On the other hand, Bulliard, Corda, Hoffmann, and others, both before and after this, contended for the fertilization of the individual spores by the cystidia. Mr. Worthington Smith supported this view, based upon experiments with *Coprinus radiatus*, and declares himself of the same opinion still. It is impossible to enter upon the details here, but this carefully written memoir by Mr. Smith merits attentive and unprejudiced perusal. The cystidia are borne upon the gills of the *Agaricini*, side by side with the basidia supporting the spores. Insect agency is not essential for direct fertilization, but, for cross-fertilization, some such aid would be necessary, assuming the theory of fertilization by the cystidia to be established. Here, again, another suggestion must be taken into account, for Mr. Smith contends that usually the cystidia fall out from the gills, and are scattered upon the ground beneath, and the fertilization of the falling spores takes place upon the ground, and not during the time that both spores and cystidia are attached to the gills. If this be the case, it explains why no foramen has been detected in the spore membrane, except the hilum of attachment, and suggests that through the hilum communication is established between the cystidia and the spore contents. On the other hand, fertilization usually takes place, in most organisms, at an early stage of the ovum, and not at its apparent maturity, and full coloration. It is not improbable, if fertilization takes place after the spores and cystidia have fallen to the ground, that the visits of insects, &c., to the gills may assist in releasing both organs and causing their precipitation, and, consequently, promoting fertilization. This hypothesis being true, a viscid stem would be of service to retain many of the spores and cystidia attached until fertilization is accomplished, so also would a woolly or hairy stem. As a rule, we imagine that the brightest coloured species (*Russula*, for example) have neither a viscid or woolly, but a dry and smooth stem, whereas a proverbially dull-coloured sub-genus, such as *Inocybe*, includes a great number of species with a rough scaly stem. We cannot pretend to discuss here the strength or weakness of this theory of fertilization, only to suggest, on the assumption of its being accepted, the probable advantages of insect visitations, and consequently of attractive characters favouring such visitations.

Closer habits of observation, and a larger number of observers, is the only hope for acquiring a more perfect knowledge of this subject, and this may be stimulated by some suggestions offered in the course of this discussion, indicating the direction in which observation is calculated to produce favourable results. However plausible it may appear to hint that bright colours in Agarics are attractive, we fail to recognize any evidence that such is the case. Do insects visit *Agaricus muscarius* more persistently than they do *Agaricus pantherinus*, or *Russula rosacea* more than *Russula consobrina?*

The persistency with which reference is made to the supposed passage of fungus spores through some animal host, in order to produce fertility, provokes as persistent a denial that there are facts to support such an hypothesis, and compels us to insist that such a supposition is untenable, and is not accepted nowadays by mycologists, however much it may have been tolerated in the past. Apart from the question of the attractive colours of fungi, we are reminded of instances in which colour, in the Hymenomycetal fungi, is evidently protective, although these are not numerous. *Cantharellus carbonarius*, as its name suggests, grows upon charcoal, or charred ground where charcoal has been burnt, and its smoky black cap so nearly resembles the soil that it may easily be overlooked. There are also two small species of *Collybia*, usually found growing together on burnt ground. *Agaricus atratus* and *Agaricus ambustus*, with dingy blackish-brown caps, which render them very inconspicuous. One of the most common of fungi on charred ground is *Agaricus* (*Flammula*) *carbonarius*, with a brown cap, so viscid that it is nearly always disguised by a coating of fragments of burnt soil and charcoal which adhere to it. The little *Agaricus* (*Pleurotus*) *acerosus* grows on the side of wet wheel-ruts in woods, and has such a dingy grey cap that it must be sought very carefully to be seen. Whenever we have found *Agaricus* (*Psalliota*) *hæmorrhoidarius*, it has been partly buried in clayey soil, and the whole fungus and soil so nearly of the same colour that even a practised fungus-hunter could easily pass without observing it. *Agaricus* (*Tricholoma*) *vaccinus* growing amongst pine-leaves would be mistaken for a fir cone, and *Agaricus* (*Tricholoma*) *imbricatus* is remarkably inconspicuous amongst dead pine-leaves and old fir cones; and every mycologist knows how very difficult it is to see the little *Hydnum auriscalpium* amongst old fir cones. *Agaricus* (*Tricholoma*) *sordidus*, with its dingy brown pileus, is almost indistinguishable on old dung-hills. *Agaricus* (*Collybia*) *fusipes*, which grows at the base of rotten stumps, is wholly of a dark chestnut brown colour, and, when wet, is not readily seen, unless specially hunted after. *Agaricus* (*Collybia*) *vertirugis* can scarcely be distinguished from the dead bracken on which it grows. Some of the pretty little species of *Mycena* are very difficult to find, because they resemble in colour the dead leaves and twigs amongst which they flourish. The small forms of the bright tawny *Agaricus* (*Galera*) *hypnorum*, with their conical caps, resemble the calyptra of the moss they grow among. Many of the species of *Cortinarius*, although possessing bright colours, are so in harmony with the bright tints of the freshly-fallen autumnal leaves, amongst which they grow, that they are hardly distinguishable. The colour of *Paxillus panuoides* is just that of the sawdust it inhabits. These are only a few instances to which our memory reverts at the moment, but there are many other examples which might be cited, if these were not sufficient, to show that, although some fungi exhibit a very bright and conspicuous coloration, there are others which harmonize with their surroundings to such an extent as to be practically inconspicuous.

There is another class of phenomena which might be alluded to in passing, and this consists of what could almost be termed imitative resemblances, between species otherwise remarkably distinct from each other. We do not allude to fancied resemblances, but deceptive resemblances, so close as to deceive even fungologists, until they apply the test of a scientific examination. Most of these are alluded to by Mr. Plowright in his paper on "Mimicry in Fungi." The renowned orange amanita (*Agaricus* (*Amanita*) *cæsareus*), which is so much praised as an esculent on the Continent, but is not an indigenous species, resembles closely the poisonous *Agaricus* (*Amanita*) *muscarius*, not only in colour and size, but also in the volva at the base of the stem, and the large pendulous ring. The edible *Ag.* (*Amanita*) *rubescens*, one of the safest and best of our white-spored species, has a counterpart in *Ag.* (*Amanita*) *pantherinus*, which has the reputation of being poisonous, and so closely like the edible species as to be liable to be confounded by the inexperienced. Then, again, the typical mushroom *Agaricus campestris* has its resemblance in the unwholesome *Agaricus melaspermus*, and another species, *Agaricus* (*Hebeloma*) *fastibilis*, which Mr. Smith reports came up in great numbers upon a mushroom bed, on one occasion, and might have caused a disastrous result had not the fact been detected by an adept. A no less remarkable similarity is that between *Lactarius deliciosus*, an excellent esculent, and the deleterious *Lactarius torminosus*: the latter, we know from experience, is sometimes not to be distinguished from the former when growing, and not until gathered and examined. We have gathered two or three times a very mild pleasant species of *Russula* which is perfectly innocuous, which in size, and its deep red colour, cannot be distinguished from the acrid and dangerous *Russula rubra*, and yet we can detect no difference between them, except in taste. The false chantarelle (*Cantharellus aurantiacus*) has a bad reputation, and yet it simulates the edible chantarelle (*Cantharellus cibarius*) sufficiently to caution novices against confounding them. Whether its ill name has good foundation or not, *Lactarius aurantiacus* so nearly resembles large specimens of *Lactarius mitissimus*, which is mild and good eating, that mycologists themselves have often confounded them. Then, again, there are other instances in which innocuous species closely resemble each other, as *Agaricus* (*Clitocybe*) *dealbatus* and *Agaricus* (*Clitopilus*) *orcella*, but the former has white spores and the latter pink, and both are edible. On the other hand, *Agaricus* (*Clitocybe*) *Sadleri* is the counterpart of *Agaricus* (*Hypholoma*) *capnoides*, and neither of them is fit to eat. These comparisons might be extended considerably, even to species systematically far remote from each other, but the above will suffice to show that good species have their counterparts, or imitators, in bad ones, and that both good and bad may have analogous resemblances in other distinct species.

We have purposely restricted ourselves in these observations to the Hymenomycetal fungi, but if Prof. Saccardo is correct that there is still to be found a *Clavaria ophioglossoides* with all the external features of *Cordyceps ophioglossoides*, and also a *Clavaria nigrita* which is the counterpart of *Geoglossum nigritum*, then we have two remarkable instances of species with naked spores simulating species in far-removed genera with a widely different structure and fructification, bearing large sporidia inclosed in asci. But in dealing with these groups we are in constant danger of mistaking conidial forms of ascigerous species for autonomous fungi, and we cannot help such a suspicion in these instances.

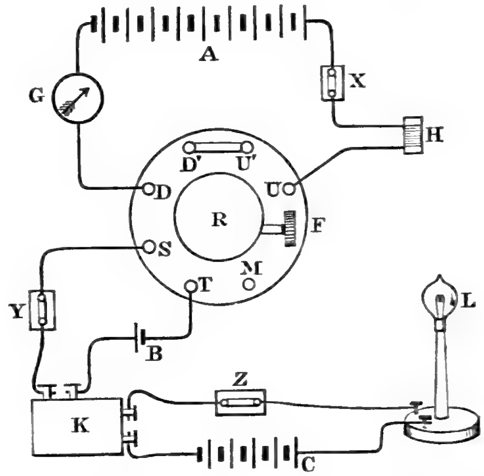
The whole subject is one of considerable interest, but surrounded by difficulties. Facts accumulate slowly because intelligent observers are few amongst the lower orders of Cryptogamia; still every decade of years exhibits some advance, although there is insufficient material for safe generalization at present. It is well to keep in view what has been written on the subject, and to that end we subjoin references, which may be consulted by those who are interested sufficiently to take the trouble. "Mimicry in Fungi," by W. G. Smith, in *Gardeners' Chronicle*, November 16, 1872, and February 10, 1877; also by M. C. Cooke, in *Grevillea*, vol. ix., June 1881, p. 151; and C. B. Plowright, in *Grevillea*, vol. x., September 1881, p. 1, and March 1882, p. 89.

M. C. COOKE.

AN AUTOMATIC LAMP-LIGHTER.

IN illustration of a paper on selenium, read at a recent meeting of the Physical Society (NATURE, vol. xliii. p. 262), I exhibited an electric lamp which was connected with a selenium-cell and a relay in such a manner that the lamp was automatically turned on in the dark and extinguished by the action of light. The details of the arrangement were, however, not described, and I propose to give here a few particulars for the assistance of those who may wish to repeat the experiment.

A scheme of the connections is shown in the annexed diagram. It will be seen that there are three circuits.



The first includes a battery A, of 24 small Leclanché cells, the selenium-cell H, and the magnet coils of the relay R. A Post Office tangent galvanometer G, capable of measuring milliamperes, is a convenient but not indispensable adjunct. In the second circuit is a Leclanché cell B, one pole of which is connected through the terminal T with the tongue of the relay R, and the other through the magnet-coils of the electro-magnetic switch K, and the terminal S, with one of the platinum stops of the relay. The third circuit contains the lamp-battery C, the incandescent lamp L, and the tongue and stop of the electro-magnetic switch K. The three simple switches, X, Y, Z, are useful for breaking any of the circuits.

The selenium-cell H has a resistance in the dark of about 50,000 ohms, which is diminished to one-half, or less, by the action of diffused daylight, or by the light of an ordinary gas jet at a distance of 1 foot. The relay R is a "standard relay," as used in the Postal Telegraph service. Its tongue, connected with the terminal T, oscillates between two adjustable platinum stops, which are connected respectively with S and M. (For brevity we will call these the S stop and the M stop.) It contains four magnet-coils which, as the instrument is sent out by the manufacturers, may be connected either all in parallel or two in series and two in parallel. For the present purpose it is desirable that the coils should be joined up all in series, thus greatly increasing the sensitiveness of the instrument to small currents at an immaterial sacrifice of rapidity of action. This can only be done by altering the permanent connections underneath the stand of the relay. The terminals D′ and U′ are joined to each other by a brass strap. Care must be taken to join the terminal D to the zinc pole of the battery. The electromagnetic switch K is simply an ordinary electric bell relay: it is used in order to avoid passing a strong current through the delicate relay R. The lamp L is an 8-volt lamp of 5-candle power. The battery C consists of 5 bichromate or Grove cells; secondary cells, if available, would of course be much better.

The connections being made as above, it only remains to adjust the relay. The platinum contacts must be clean and smooth; those in my instrument are occasionally cleaned with a watchmaker's "dead smooth" file, and then rubbed with a bright knitting-needle. The two stops are screwed up until there is only just room for the tongue to move between them. The milled head F, is first turned clock-wise, so as to make the tongue press against the M stop, and then (the selenium cell being in the dark, or only feebly illuminated) it is slowly and cautiously turned counter-clockwise, until the tongue passes over to the S stop, which causes the lamp to be switched on. If now the selenium cell is exposed to a sufficiently strong light, the tongue moves back to the M stop, owing to the increased current through the magnet-coils, and the lamp goes out. It is possible to adjust the relay so that the lamp is lighted automatically whenever the external illumination falls below any desired degree of intensity, and the adjustment when once properly effected, will remain perfect for days or weeks together.

The selenium-cell is made by winding two fine copper wires, which serve as electrodes, very close together around a slip of mica, one surface of which is afterwards coated with a thin film of selenium. I have given full instructions for the manufacture of these cells in NATURE, vol. xxiii. p. 58, and need not repeat them here. The cell used for the lamp experiment measures $2\frac{1}{4}$ inch by $\frac{3}{4}$ inch; the gauge of the copper wires is No. 36, and each wire makes 20 turns to the inch. The resistance of the cell in the dark is about 52,000 ohms.

The experiment may be shown by placing the cell near a window, and closing the shutters, when the lamp immediately lights up, going out again as soon as the shutters are reopened. When daylight is not available, the effect may be exhibited in a scarcely less striking way by alternately screening and exposing a gas burner placed at a distance of a foot or two from the selenium, or by turning the gas up and down. The following test illustrates the sensitiveness of the arrangement. The relay was adjusted so that the lamp circuit was just closed when a standard candle was burning at a distance of $7\frac{1}{2}$ inches from the selenium-cell. The candle was moved slowly towards the cell, until the distance between them was reduced to $6\frac{1}{2}$ inches, when the increased illumination caused the lamp circuit to be broken. The candle was then moved back, and when the distance was once more $7\frac{1}{2}$ inches, the circuit was again closed. By moving the candle backwards and forwards over a range of about 1 inch, the circuit could be alternately closed and broken as often as desired. The extreme difference in the strength of the currents passing through the selenium under these changes of illumination was shown by the galvanometer to be rather less than 0·1 milliampere.

Though the apparatus does not at present claim to be anything more than a pretty scientific toy, it is possible that it may turn out to be of some practical utility. To demonstrate its capabilities, I one day left the selenium-cell near the window, with the batteries joined up. At about 4 p.m., just when reading was becoming impossible through failing daylight, the lamp (which I had in fact forgotten all about) was automatically turned on.

SHELFORD BIDWELL.

REMARKABLE ANCIENT SCULPTURES FROM NORTH-WEST AMERICA.[1]

MR. JAMES TERRY has just published descriptions and photographs of some of the most remarkable works of prehistoric man yet discovered on the American continent. The title of his paper is sufficiently startling, but it is fully borne out by the beautiful full-size and half-size photographic prints with which it is illustrated. They represent three rude, yet bold, characteristic, and even life-like sculptures of simian heads, executed in basalt. One of these belongs to the author, one to Mr. T. Condon, and the third to Prof. O. C. Marsh, who referred to it, in his address "On Vertebrate Life in America," in the following terms:—"On the Columbia River I have found evidence of the former existence of inhabitants much superior to the Indians at present there, and of which no tradition remains. Among many stone carvings which I saw, there were a number of heads which so strongly resembled those of apes that the likeness at once suggests itself. Whence came these sculptures and by whom were they made?" Unfortunately we have no detailed information as to the conditions under which these specimens were found, except that "they would be classed as 'surface finds,' from the fact that the shifting sand-dunes, which were largely utilized for burial purposes, are continually bringing them to the surface and exposing them." This gives no indication of their antiquity, but is quite compatible with any age which their other characteristics may suggest.

The size of the heads varies from eight to ten inches in total height, and from five and three-quarters to six and a half inches in width. The three are so different from each other that they appear to represent three distinct animals; and, so far as I can judge, they all differ considerably from the heads of any known anthropoid apes. In particular, the nostrils are much farther from the eyes and much nearer to the mouth than in any of the apes. In this respect they are more human; yet the general form of the head and face, the low and strongly-ridged forehead, and the ridges on the head and cheeks seem to point to a very low type of anthropoid. In a letter to Mr. Terry, Mr. Condon suggests "that they were copied from the figure-head of some Malay proa that may have been wrecked on the coast;" but such a supposition is quite inadmissible, since nothing at all resembling these heads is ever carved on Malay proas, and there is no reason to believe that if such a carving did come into the possession of the natives they would ever think of copying it in stone; while these sculptures were found two hundred miles from the coast on the east side of the Cascade Mountains.

Taking into consideration the enormous antiquity of the stone mortars and human remains found in the auriferous gravels of California buried under ancient lava streams and associated with a flora and fauna altogether different from that of any part of America at the present time, Mr. Terry's own conclusion appears the more probable. It is, "either that the animals which these carvings represent once existed in the Columbia valley, or that, in the remote past, a migration of natives from some region containing these monkeys reached this valley, and left one of the vivid impressions of their former surroundings in these imperishable sculptures." The latter alternative appears to me, for many reasons, to be highly improbable; and though the former will seem to many persons to be still more improbable, I am inclined provisionally to accept it.

ALFRED R. WALLACE.

[1] "Sculptured Anthropoid Ape Heads found in or near the Valley of the John Day River, a tributary of the Columbia River, Oregon." By James Terry. (New York, 1891.)

NOTES.

THE celebration of the jubilee of the Chemical Society was in every way most successful. At the meeting at Burlington House, on Tuesday afternoon, admirable addresses were delivered, and the *conversazione* at the Goldsmiths' Hall in the evening was attended by about 800 guests, including many eminent men of science. The Fellows and their friends dined together on Wednesday. We shall have more to say about the celebration next week.

THE following have been appointed to preside over the Sections at the Cardiff meeting of the British Association in August next:—A—Mathematics and Physics, Prof. O. J. Lodge, F.R.S.; B—Chemistry, Prof. Roberts-Austen, C.B., F.R.S.; C—Geology, Prof. Rupert Jones, F.R.S.; D—Biology, Mr. Francis Darwin, F.R.S.; E—Geography, Mr. E. G. Ravenstein, F.R.G.S.; F—Economics, Rev. Dr. W. Cunningham; G—Mechanical Science, Mr. T. Forster Brown, M.Inst.C.E.; H—Anthropology, Prof. Max Müller.

THE first *soirée* of the Royal Society this year is to be held on Wednesday, May 6. The date of the *conversazione*, to which ladies are invited, is not yet fixed.

M. JEAN SERVAIS STAS is to have a jubilee next May. The Royal Academy of Belgium purposes to strike a gold medal on the occasion of his fiftieth anniversary as a "membre titulaire de la Classe des Sciences," and to present it to M. Stas at a general meeting of the Academy on May 5. Corresponding Societies will also probably send congratulatory addresses.

ON Monday afternoon a numerous and influential meeting was held at the Mansion House in support of the University College and King's College Extension Funds. The chair was taken by the Lord Mayor. The Bishop of London moved a resolution to the effect that it is essential to the welfare and dignity of London that its institutions for University teaching should be maintained at the highest standard and in the utmost efficiency. In another resolution, moved by Lord Reay, the meeting was reminded that the two Colleges had deserved well of London and merited liberal support. Mr. W. H. Preece, F.R.S., supporting this resolution, spoke of science in its relations to trade and industry. It was only a few days ago that, in common with the Lord Mayor, he was present at the inauguration of a system of lighting the City of London which would surpass what was done in any other city of the world. It was of the highest importance that London should not be behindhand as compared with our provincial friends or our foreign competitors. London, however, was at some disadvantage compared with the other great cities—it was so vast that the local spirit was practically inoperative, and it possessed no Whitworths, or Masons, Firths, or Owens, to leave all their wealth to education. Germany and Switzerland had made lavish outlays, and the United States were founding institutions which were the wonder of the age. In all the Universities of the United States large numbers were being taught in the practical classes, and it was our bounden duty that in this war of competition between the different nations of the world we should be able to hold our own. The third resolution, moved by Dr. Erichsen, was as follows:—"That a committee be formed, under the presidency of the Right Hon. the Lord Mayor, to receive subscriptions in support of the extension funds of the two Colleges, consisting of the committee of the University College Extension Fund, under the chairmanship of Lord Reay, and of such governors and other members of the council of King's College, under the chairmanship of the Bishop of London, as the Bishop may appoint."

PROF. ARMAND SABATIER, the well-known zoologist of Montpellier, is at present collecting money for the purpose of completing the zoological marine station of Cette. This laboratory will certainly be one of the most useful in France, the fauna there being very abundant and varied. It would be difficult to conceive of a station more favourably situated. Fresh waters are plentiful, brackish waters very abundant, in the *étangs* of the vicinity; in the harbour many forms are found which do not thrive or are seldom met with on the shore; and there is open water close by.

A SHORT Bill relating to technical instruction has been introduced into the House of Commons by Sir Henry Roscoe. He proposes that a local authority shall be enabled to make such provision in aid of the technical or manual instruction for the time being supplied in a school or institution outside its district, as may, in its opinion, be necessary for the requirements of the district in cases where similar provision cannot be so advantageously made by aiding a school or institution within its district. In the same way a local authority may provide, or assist in providing, scholarships for, or pay or assist in paying the fees of, students ordinarily resident in its district, at schools or institutions within or outside that district. In distributing the provision made in aid of technical or manual instruction, the local authority is empowered to consider the relative needs of the schools or institutions aided, as well as the nature and amount of efficient technical or manual instruction supplied by those schools or institutions.

MR. JAMES MUIR, Professor of Agriculture in the Royal Agricultural College, Cirencester, has been appointed to the new Professorship of Agriculture in the Yorkshire College, Leeds.

DR. ALEXANDER BUCHAN has for some time been at work on the effect of high winds on the barometer at the Ben Nevis Observatory. The result is believed to settle this much-debated question conclusively.

MR. MOSSMAN, who has just spent six weeks on Ben Nevis, investigated the cases of glazed frost which occurred while he was there. The results he obtained are said to promise well for the extension of our knowledge of cyclones and anti-cyclones.

SOME months ago Mr. James Britten, the editor of the *Journal of Botany*, and Principal Assistant in the Botanical Department, British Museum, issued a circular notifying that the "Biographical Index of British and Irish Botanists," compiled by himself and Mr. G. S. Boulger, which has appeared in sections in the last three volumes of the *Journal of Botany*, would be reprinted in an amended and augmented form, provided that a sufficient number of subscribers could be found to cover the cost of reproduction. We regret to learn that this project is likely to fall through in consequence of the very small number that have responded to the appeal for a very small subscription. The work is so obviously valuable and interesting to all concerned in botany and horticulture, that we think it has only to be known to be in great demand.

AT the last meeting of the Russian Geographical Society, on February 18, Th. Tchernysheff made a communication about the expedition which has been engaged during the last two years in the exploration of the *tundras* of North-east Russia, and especially the Timansk Mountains. The expedition had an excellent scientific staff, including, besides the geologist, M. Tchernysheff, Prof. Backlund for determining latitudes and longitudes, a mining engineer, and a botanist. About 25,000 square miles of this almost quite unknown territory, were carefully mapped, and the geological and orographical data gathered by the expedition proved especially valuable.

Mr. William Davies, F.G.S., died on February 13, at the age of seventy-seven. For forty years he was connected with the Geological Department of the British Museum, from which he retired as senior assistant two or three years ago.

M. C. Reinwald, the *doyen* of the publishers in Paris, died suddenly a few days ago. He was the publisher of a number of scientific works, including translations of those of Darwin and Haeckel.

We are sorry to hear from St. Petersburg of the death of K. I. Maximowicz, member of the Russian Academy of Sciences, the author of well-known botanical works. He had lately been engaged in the description of the floras of Tibet, Central Asia, and Mongolia, as represented by the rich collections of Prjevalsky and Potanin. The Russian Geographical Society has lost N. L. Puschin, well known in Russia for his hydrographical works, and especially for a work upon the hydrography of the Caspian Sea. We have also to record the death of Prof. Alexeyeff, of Kieff, one of the most active and able of Russian chemists.

The donations reported at the meeting of the Royal Botanic Society on Saturday last included one from Messrs. Dakin of an extensive and interesting collection of samples of the more curious forms of tea, many of which are never seen in use in Europe or known in commerce. To these kinds of tea, extraordinary virtues are attached in China and the East, and some command fabulous prices. Several varieties of growing tea-plants from the Society's greenhouses of economic plants were on the table in illustration.

On Thursday next, March 5, Prof. C. Meymott Tidy will begin a course of three lectures at the Royal Institution on "Modern Chemistry in Relation to Sanitation."

Archæologists have, of course, been profoundly interested by the recent discovery of a vault filled with mummies and funereal coffers at Deir Elbahiri, near the plain of Thebes. The Cairo correspondent of the *Times*, telegraphing on February 24, gives the following as the latest details:—The site of the discovery is east of the Temple of Queen Fatasou, in a small spot previously undisturbed, amidst the excavations made by the late Mariette Bey and Brugsch Pasha. A well-shaft of 15 metres leads to a doorway blocked with large stones, opening on a gallery 73 metres long, whence a staircase descending $5\frac{1}{4}$ metres conducts one to a lower gallery 12 metres in length, both lying north and south. The lower gallery gives access to two mortuary chambers, four and two metres square respectively. At the top of the staircase is a transverse gallery, 54 metres long, lying east and west, the object of which is unknown. The total underground area is about 153 metres, excavated in the limestone rock to over 65 feet below the surface. The same disorder reigned amongst the contents of the tombs as was found when the famous Royal mummies were discovered nine years ago. Sarcophagi were piled upon sarcophagi, and alongside were boxes, baskets of flowers, statuettes, funereal offerings, and boxes crammed with papyri. There is every indication that the place, though originally constructed as a vast tomb, was chosen for hurried concealment in time of tumult. Some of the exteriors of the mummy-cases are unusually richly decorated with religious subjects, carefully depicted; others of large size enclose mummies in a broken condition, and were apparently procured hastily, as the spaces for the occupants' names are left unwritten upon. The contents of the papyri are as yet unknown, but hopes are entertained that the writings are of permanent historical interest and have been thus hidden to avoid destruction. The mummies are priests and priestesses of Ammon, Anubis, Seti, Mentou, and Queen Aahotep, numbering 163, the latest belong, ing to the 21st dynasty. Seventy-five papyri were found in boxes in the form of statuettes of Osiris. Each mummy is also expected to contain more or less valuable manuscripts. The collection is *en route* in barges by the Nile, and will probably reach Cairo in a few days.

The *Université de Montpellier*, a paper recently started in the city of Montpellier, publishes an interesting series of articles by Prof. C. Flahault, on the organization of higher education in Sweden and Denmark.

Dr. G. v. Lagerheim, the Professor of Botany at Quito, has been entrusted with an investigation of the Cryptogamic Flora, of Ecuador, at the expense of the Government. In the course of the next eighteen months he hopes to have completed his monographs of the Uredineæ and of the Freshwater Algæ of Ecuador.

In the neighbourhood of Ascholtshausen, in Bavaria (according to the *Moniteur Industriel*), a species of coffee is successfully cultivated on sandy soil. It is sown in spring, and begins to flower in July (the flowers sky-blue); the fruit ripens in August, and is pale yellow, like Bourbon Island coffee. The taste of this coffee is said to be very pleasant, though slightly more bitter than foreign coffee. Several families in the district named grow all their own coffee.

As a small, isolated territory, the Andaman Islands, which have been under scientific observation since 1858, afford excellent scope for studying the growth of new plants, intentionally, accidentally, or naturally introduced. Dr. Prain, of Sibpur, has studied the subject, and gives some particulars in a long paper contributed to the Journal of the Asiatic Society of Bengal. Mr. Kurz made a list of the Andaman flora in 1866, recording 520 indigenous species, and the number has since been raised by persistent search to 600. The general points of Dr. Prain's discoveries are, that in 1866 fifteen intentionally introduced plants and "sixty-one weeds of cultivation" had become established as an integral portion of the Andaman flora, and that by 1890 twenty-three more of the first and fifty-six more of the second had been added. He also observes that four of the naturalized plants noted in 1866 have disappeared. By "weeds of cultivation" is meant weeds whose seeds have been carried thither unintentionally. With very few exceptions they are the commonest of Indian road-side and rice-field weeds. A common Indian butterfly has made its appearance since the plant on which its larva feeds became naturalized.

An instrument, called the *hæmatokrit*, has been lately invented by Herr von Hedin; it is for determining the volume of corpuscles present in blood, and is based on centrifugal action. A volume of blood and one of Möller's liquid (which prevents coagulation) are mixed together, and the mixture is brought into small thick-walled glass tubes, graduated in 50 parts. The tubes rest on a brass holder which is fixed on the axis of a rotation-apparatus. After some 8000 rotations, in 5 to 7 minutes, the process is complete. The separation between the corpuscles and the salt-plasma is more distinct, in that a narrow band of leucocytes appears between them. The instrument is useful in comparing the blood of different individuals. With a little practice, the total error is not more than one volume per cent.

Señor R. A. Santillan has published a meteorological bibliography for Mexico (Mexico, 1890) which—although comparatively little has been published in that country—will be very useful for reference. Until the middle of this century very few persons paid attention to that science; the first trustworthy observations were made by Alzate in 1769, and from that time none appear to have been taken until those by Dr. Burkart, in 1826. But since the foundation of the Central Observatory, in 1877, many stations have been established in all parts of the country. The publication in question includes the titles of 228 separate works and articles in Transactions, &c., arranged under (1) authors, (2) anonymous works and reports by institutions, and (3) articles relating to Mexican meteorology in foreign periodicals. There is also a list of works on earthquake phenomena, and a classified index of subjects for easier reference.

THE Burmah Administration Report for the past year says that the total approximate area topographically surveyed in Burmah last year was 31,680 square miles, of which nearly 9620 square miles were surveyed by the Anglo-Siamese Boundary party. The total cost of these surveys was £16,880. A cadastral survey party worked in Kyaukse from November 1889 till July 1890, and surveyed 550 square miles, at a cost of over £18,500. Surveys of State lands were undertaken in several districts in Upper Burmah by small detachments of local surveyors. These parties surveyed nearly 81,000 acres in six districts.

SODIUM amide, NH_2Na, forms the subject of a communication by M. Joannis to the current number of the *Comptes rendus*, This interesting compound was supposed to have been obtained by Gay Lussac and Thénard in the form of an olive-green readily fusible substance, by heating metallic sodium in ammonia gas. The product of this reaction, however, can scarcely have consisted of pure sodamide, for M. Joannis has now obtained the isolated compound in well-defined colourless crystals, and analyzed it. He first prepared the curious substance sodammonium, first obtained by Weyl, who liquefied ammonia by heating the compound of ammonia with silver chloride at one end of a sealed tube and allowed the liquid to collect at the other cooled end of the tube in contact with metallic sodium. It has been stated that the deep blue liquid thus formed is merely a solution of sodium in liquid ammonia. However this may be, M. Joannis finds that sodammonium decomposes spontaneously at the ordinary temperature into hydrogen and sodamide. NH_2Na. The decomposition occurs rather more quickly in sunshine than in the dark, but under any circumstances is very slow, only about a third of a cubic centimetre of hydrogen being evolved per gram of sodammonium in twenty-four hours. As the decomposition slowly progresses, however, small transparent colourless crystals make their appearance, whose average diameter is about one millimetre. Upon analysis, they yield numbers agreeing with the formula NH_2Na. When thrown into water a most lively action occurs, just as if the crystals had consisted of globules of red-hot metal, violent hissing occurring without the evolution of any gas except water vapour. The solution is found to contain only soda and ammonia. Sodamide may be prepared in much larger quantities and in very much less time by allowing saturated water solutions of sodammonium and sodium chloride to react upon each other at the temperature of melting ice. Under these conditions hydrogen is liberated to the extent of one equivalent for every equivalent of sodammonium. The solution remains blue until the sodium chloride is in excess, when it becomes colourless. The white solid product of this reaction is then washed with liquefied ammonia (NH_3) to remove the sodium chloride; the last traces of chlorine are found to be eliminated after several such washings. The residual substance is sodamide. This remarkable action of sodium chloride is due to the formation of an unstable intermediate compound, NH_2Na_2Cl, which is obtained, mixed with sodium chloride, when metallic sodium is treated with excess of sodium chloride in presence of a quantity of liquefied ammonia insufficient to dissolve the whole of the sodium chloride. On treating the compound with further quantities of liquefied ammonia it is decomposed into sodium chloride which dissolves, and sodamide which is left in a pure state. This compound, NH_2Na_2Cl, behaves quite differently with water to sodamide; it dissolves quietly without the least hissing, with formation of a solution of ammonia, soda, and common salt.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mr. Henry Williams; a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, presented by Mr. A. Mann; a Grey Ichneumon (*Herpestes griseus* ♀) from India, presented by Mr. J. Seymour Bartlett; a Jack Snipe (*Gallinago gallinula* ♂), a Common Buzzard (*Buteo vulgaris*), British, presented by Mr. W. H. St. Quintin; a Little Grebe (*Tachybaptes fluviatilis*), British, presented by Miss E. Bartlett; two Burbot (*Lota vulgaris*) from the Trent, presented by Mr. T. F. Burrows; a Scaup (*Fuligula marila* ♂), a Curlew (*Numenius arquata*), European, purchased.

OUR ASTRONOMICAL COLUMN.

THE SOLAR SPECTRUM AT MEDIUM AND LOW ALTITUDES.—Dr. Ludwig Becker, of the Royal Observatory, Edinburgh, has made a series of interesting observations of the solar spectrum at medium and low altitudes (Trans. Roy. Soc. Edin., vol. xxxii. Part 3, 1890). The region observed was between the wave-lengths 6024 and 4861. The method usually adopted for determining the wave-lengths of lines in the solar spectrum is by means of the reading of the vernier attached to the observing telescope. Dr. Becker has determined them by clamping the telescope, and recording the exact positions of a movable diffraction grating when the lines are successively brought to the fixed cross-wire in the field of view. Using one of Rowland's gratings, the angular interval between the two positions of the grating which bring the components of the E_1 line to the same direction was one second of arc in the second-order spectrum. This small angular movement has been multiplied 16,800 times by gearing several pairs of wheels and pinions together, and attaching the grating to the fastest wheel of the train. To make an observation, the observer turns the slowest wheel. The motion is transmitted by means of other wheels and endless screws, until it slowly rotates the grating and causes the lines of the spectrum to move across the fixed field of view. When the line under observation coincides with the intersection of the wires, the observer depresses one or more needles according to its degree of blackness, and the pricks thus made are recorded on a fillet of paper which is moved by another specially devised piece of apparatus. As to the linear distances between two lines on the paper, it is noted that the D lines are 19¾ inches apart, whilst the whole region of observation, λ 6024 to λ 4861, requires a strip 314 feet long. With respect to the work done, it need only be said that the memoir contains a catalogue of 3637 lines of the solar spectrum, including 928 telluric lines, between the above-mentioned limits. If 28 lines be excepted, the whole telluric spectrum is found by these observations to consist of three bands ranging from λ 6020 to λ 5666, λ 5538 to λ 5386, and λ 5111 to λ 4981, and containing respectively 678, 106, and 116 lines. Dr. Becker's investigations, combined with the results obtained by other observers, lead him to believe that the water-vapour lines of the first of the above-named bands are split into two distinct groups by a band of faint lines, which are probably due to oxygen. These two groups have been called the rain-band and the δ-band. They were known to Brewster, and his drawing of telluric-absorption bands gives also the other two bands under the designation ζ and ι, besides some other bands which do not appear to be due to atmospheric absorption.

The water-vapour band (ι) between *b* and F is described by Ångström as very strong in summer. It is the same which Mr. Maxwell Hall has utilized as a rain-indicator at Jamaica.

Dr. Becker's memoir is a most important one, and all the methods of observation and reduction of observations are fully explained. The probable error in the determination of wave-length from the fillet of paper is said to be about ± 0·02 $\mu\mu$, but, of course, differs slightly with the intensity of the line observed. The catalogue of lines contains oscillation frequencies, as well as wave-lengths, and many details of value. The maps have been well reproduced by photo-lithography, and show the intensities of blackness of the solar and telluric lines as they would appear at medium altitudes of the sun for an average amount of water-vapour, and the intensities of the telluric lines only when the apparent altitude of the sun is 1° or 2°. It is unnecessary to comment upon this addition to our knowledge of the telluric spectrum. The author must be glad that his laborious observations have led to such tangible results.

A METHOD OF MEASURING ATMOSPHERIC DISPERSION.—At the Paris Academy on February 16, M. Prosper Henry described a novel method for determining atmospheric dispersion and its variation with the wave-length of the light observed. A *reseau*, formed of Bristol board perforated very regularly with holes about 1 millimetre apart, was placed in front of the object-glass of a telescope as a diaphragm. When a luminous point, not affected by atmospheric refraction, is observed through this arrangement, a double series of spectra are seen besides the central image of the point. M. Henry shows that by knowing the zenith distance of a star, and the size of the meshes of the *reseau*, the value of atmospheric dispersion (D) may be found by determining the angular distance of any particular part of the spectrum from the central luminous point. A large number of stars have been observed at Paris Observatory, and from them D has been found to be 0″·723 for eye observations, whilst the value 0″·729 has been deduced from photographs. The most probable value may therefore be taken as 0″·726. If the wave-length of the maximum light intensity of the spectrum be taken as 575 $\mu\mu$, and the average amount of atmospheric refraction (A) for an object having a zenith distance of 45° be 58″·22, it is found that

$$A = 56''\cdot55 + \frac{0''\cdot726}{\lambda^2}.$$

This formula gives the following values for light of different wave-lengths:—

Wave-length. $\mu\mu$	Average atmospheric refraction. ″
700	57·79
600	58·11
575 (maximum light intensity)	58·22
500	58·66
430 (maximum chemical intensity)	59·13
400	59·42

It will be seen that the most intense chemical rays give a value of A 0″·91 higher than that furnished by light of maximum luminosity.

The comparison of the different dispersive effects of the atmosphere on light of different wave-lengths indicates that, in the latitude of Paris, the green portion of the solar spectrum ought to be visible about a second after the disappearance of the yellow. This explains the phenomena noted by the late M. Thollon and other observers of the telluric spectrum. It is found that, in the majority of cases, the last light visible at sunset is in the blue part of the spectrum; this green or blue light is a limit of the visible spectrum when the sun is on the horizon, the more refrangible rays being absorbed by the atmosphere.

WOLF'S "RELATIVE NUMBERS" FOR 1890.—*Comptes rendus* for February 16 contain the "relative numbers" obtained by Wolf from a reduction, by the usual method, of the solar observations made at various Observatories in 1890. The following table shows the mean values (r) obtained for each month in the year, the mean variations (v) in magnetic declination observed at Milan, and the increments (Δr, Δv) that these quantities have received since the corresponding epochs of 1889:—

	Solar Observations.		Magnetic Observations.	
	r	Δr	v	Δv
Jan.	5·3	+ 4·5	3·02	+ 1·27
Feb.	0·6	− 7·9	4·81	+ 0·82
March	5·1	− 1·9	7·49	+ 1·32
April	1·6	− 2·7	8·68	− 0·17
May	4·8	+ 2·4	7·70	− 0·49
June	1·3	− 5·1	8·84	− 0·02
July	11·6	+ 1·9	8·57	+ 0·32
Aug.	8·5	− 12·1	8·00	− 0·99
Sept.	17·2	+ 0·7	7·10	+ 0·26
Oct.	11·2	+ 9·1	8·72	+ 2·62
Nov.	9·6	+ 9·4	3·10	+ 0·55
Dec.	7·8	+ 1·1	2·54	+ 0·58
Means	7·1	+ 0·8	6·55	+ 0·51

NEW ASTEROIDS.—M. Charlois discovered (303) on February 11, Prof. Millosevitch (304) on the following day, and Dr. Palisa (305) on February 14.

THE BRITISH MOSSES.[1]

II.

LEAVES.—When we examine the leaves of mosses and compare them with the more familiar forms presented to us by the phanerogams, we find ourselves in a new world, and the interest with which we view them is increased when we remember that, according to the view usually accepted, they are, so to speak, a unique phenomenon: they are not the descendants of any earlier leaves nor the ancestors of any later ones; they appear thus once, as it were, in the history of the vegetable kingdom, and advance no further. They possess something of the charm which an ἅπαξ λεγόμενον exercises over the mind of a philologist.

We may first note what they are not. They are never opposite, never whorled, never on leaf-stalks, never truly veined, never lobed or compound, never furnished with epidermis or stomata.

When we turn to consider affirmatively what moss leaves are, we find them in some cases characterized by an extreme simplicity of form. They are single plates of similar cells without midribs, without veins, and without border; again, they stand in immediate connection with the atmosphere, absorbing moisture from it when moist, and shrinking and shrivelling when the air is dry. In some cases they are characterized by a marked difference in the form of the cells in the different parts of the leaf, and again in other cases by the unequal distribution of chlorophyll; in other cases we come across strange forms, the like of which we hardly know in the phanerogams: such are the thick border and double rows of teeth in some of the genus Mnium; the parallel plates in Polytrichum; and, stranger still, the third flange of the leaf in Fissidens, the true homology of which has proved a *crux* to bryologists.

In some cases the leaf is produced into a long thread or beak devoid of chlorophyll, and often with indented or toothed edges. This structure is found chiefly in mosses living on stones and rocks and in dry situations, such as Grimmia and Racomitrium, and the presence of these long white threads or beaks gives a grey tint to the whole moss; and in places where the moss is predominant (as, for instance, some parts of Dartmoor and North Wales, where Racomitrium abounds) a grey tint to the whole landscape. These long hairs and prominences, especially when armed with lateral teeth, no doubt retain the moisture which is necessary not only for the vegetative life of the moss, but also for the process of reproduction by archegonia and antheridia; hence it probably is that this form of leaf prevails in mosses living in dry situations, just as the thick leaves of succulent plants are found in similar situations.

The Roots or Rhizoids of the mosses are distinguished by the minuteness of their growing ends, by their pliancy, and by the presence on their exteriors of a balsamic or glutinous deposit. To these points of structure they owe their capacity to insinuate themselves into the minutest crevices of rock, to get, for instance, amongst the particles of the oolites, and also to fix themselves in the shifting sands of the sea-coast, and by so fixing themselves to give fixity in return to the sand, and so tend to produce the sand-dunes in many parts of the coast. At some parts of the Northumbrian coast the *Racomitrium canescens* may be found buried deep in the sand, from which it can scarcely be detached, and in like manner the sand-dunes of Holland and the west of France have in many places been fixed by mosses. The forests of firs on the North Sea and the Bay of Biscay thus owe their origin to humble mosses.

Sphagnaceæ.—Vast tracts of land in this country and throughout Northern Europe and America are covered with plants of this group, and large tracts which are now fertile agricultural land, where they have entirely ceased to grow, have in former times been occupied by them. The bogs of Ireland, which are mainly constituted of turf moss, were computed in 1819 by the Bog Commissioners to occupy 2,830,000 acres. No moss has probably ever, at least in the present state of the globe, played so large a part as the Sphagnum or peat moss.

Structure.—It is to the peculiar structure of the peat moss that this great part on the theatre of the globe is to be attributed.

Leaves.—In the young leaves the component cells are all alike; then by a differential growth we are presented with square cells surrounded by four narrow and oblong ones; then

[1] The substance (with omissions and additions) of a Discourse by the Right Hon. Lord Justice Fry, delivered at the Royal Institution, January 23, 1891. Continued from p. 382.

chlorophyll forms in these narrow cells, but is absent from the square cells; from these the contents disappear, and water or water-like fluid occupies the whole cell; subsequently annular and spiral threads develop on the walls of the square cells. The intimate structure of the leaf thus enables it to absorb great quantities of water.

But again, the shape of the leaves is in many species adapted to the retention of water. By a retardation of the lateral as compared with the mesial growth, the leaf assumes a boat shape. Often the edges of the leaves are turned over; the leaf thus affords means of holding water. Again, the lateral branches grow in groups from the stem, and some of these branches are generally pendent, and in close proximity to the stem, so that an immense capillary attraction is exerted by them.

Again, the *stem* itself is surrounded or rather is more than half occupied by large water-holding cells, and pitchers of a very peculiar form.

Again, the mode of growth of the plant, abandoning its moorings on the soil and throwing out roots into the water, and growing successively year after year, enables it not only to attain great growth, but also, when the occasion demands, to keep pace with the rise of the water in which it may be growing, "the individual thus becoming," it has been said, "in a manner immortal, and supplying a perpetual fund of decomposing vegetable matter."[1]

Physical Results from Structure.—The result of these peculiarities is that the entire plant of any species of Sphagnum is a perfect sponge. When dry it is capable (as may easily be found by experiment) of rapidly absorbing moisture, and carrying it upwards through the plant; and when growing in vast beds it acts thus on a great scale. Everyone who knows Scotland must know how on many a steep mountain-side, or on the bottom and sides of a gorge, these beds will hold up a great body of water against the force of gravity; and again, the Irish bogs are described as often ascending from the edges towards the interior, sometimes by a gradual, and sometimes by a sudden ascent, so that at times the bog is so high that it reaches the height of the church steeples of the adjoining country, without any rising ground intervening.

These peculiarities in the structure of Sphagnum have produced considerable physical effects.

(1) Everyone knows the different effects of rain falling on a land of bare rock or sand, like the Sinaitic desert, and on a porous soil; in the one case it produces a freshet or a flood, that leaves no trace behind; in the other it is held for a while in suspense, and only gradually passes into the streams. The glaciers and the Sphagnum beds of the mountains of Europe alike act as compensation reservoirs—receive large quantities of moisture as it falls, and retain it till the drier season comes, when it gradually passes away in part; but for these reservoirs, many of the rivers would exhibit a far greater shrinkage in summer and autumn than is now the case.

But (2) the Sphagnum beds have become peat, and have gradually filled up the ancient lakes and morasses, and turned water into dry land. It is true that peat appears under some circumstances to be formed by other vegetables than Sphagnum, and in all cases it has probably some other plants or roots growing amongst it. Mr. Darwin tells us that in Terra del Fuego and the Chonos Archipelago, peat is formed by two phanerogamous plants, of which one at least seems endowed with an immortality something like that of the Sphagnum; and the peat of the fens of Lincolnshire is formed mainly of *Hypnum fluitans*. But Sphagnum appears to be the main constituent of peat in Ireland, Scotland, and, so far as my researches have gone, in England; the peculiar spiral threads of the cells of the Sphagnum leaf being easily detected in the peat so long as it retains traces of its organic origin.

Ancient Forests.—The peat mosses, and the sea-shores of our islands and of the adjoining mainland, reveal, as it is very well known, traces of ancient forests. Many parts of England, nearly all the mainland of Scotland, the Hebrides, the Orkneys, and the Shetlands, Ireland, and Denmark, the shores of both sides of the English Channel, Normandy, Brittany, the Channel Islands, and Holland, and the shores of Norway, all bear evidence to the presence of these primæval forests; and what is more, to the successive existence of forests, each in succession living above the buried remains of the earlier ones.

[1] Macculock, "Western Islands," p. 130.

The following table will show the order of succession in the different species of trees in some of the places where this has been observed, the braces representing the co-existence of the trees:—

Island of Lewes.	England.	Scotland.	Denmark.	Danes Moor, near Macclesfield.
1. Oak.	1 { Oak.	1 { Oak.	1. Scotch fir	1. Scotch fir.
2. Elder.	Scotch fir	Scotch fir	2. Oak.	2. Larch.
3. Birch.	2 { Birch.	2. Birch.	3. Birch.	3. Oak.
4. Scotch fir.	Hazel.	3. Hazel.		4. Birch.
	3. Alder.	4. Alder.		5. Hazel.
		5. Willow.		6. Alder.
		6. Ash.		7. Willow.
		7. Juniper.		

What is the cause of the disappearance of these ancient forests one after the other? To this question various answers have been proposed.

The Romans, it has been suggested, in their inroads, cut ways through the forests and laid waste the land. But, wide as was the spread of the wings of the Roman eagle, the phenomenon in question is of far wider extension. They never conquered Denmark, or Norway, or Ireland, or the islands of Scotland: in Scotland, and even in England, their operations could never have covered the whole country; and as regards some of our peat mosses, we know that they must have existed long before the Roman invasion; for at least on the borders of Sedgmoor we have traces of their using peat for fuel as it is used there at the present day.

Still humbler agents have been invoked, in the supposition that the beaver and other rodents were the authors of the destruction of the forests. So far as I can judge, the cause suggested seems inadequate to the effect.

Again, changes in climate have been suggested. But, although there may be some evidence from the succession of the trees of a gradual amelioration in the climate, we know of no evidence of changes of so sudden and violent a character as would destroy the existing forests over large areas. Moreover, with few exceptions, the trees of the destroyed forests are such as are now found wild, or will grow easily in the spots where they lie buried.

The overthrow by storms has, again, been suggested as the cause of this wholesale destruction; and the fact that in some of the peat bogs of the west of Scotland the trees that have fallen lie to the north or north-east, and in some of those in Holland to the south-east, in the direction of the prevailing winds in those countries respectively, affords some reason to believe that wind has given the *coup de grâce* to the dying trees, and determined the direction of their fall. But it is much more likely that this was the work of the wind, than that successive forests should have been swept from the face of vast tracts of Europe by the agency of wind alone. Moreover, in some cases the trunks as well as the bases and roots of the trees are found standing or buried in the bogs.

Allowing that some or all of these agencies may have had their part in the destruction of the forests, I believe that the growth of Sphagnum has been the greatest factor in the work of destruction. "To the chilling effect of the wet bog mosses in their upward growth must be attributed," says Mr. James Geikie, "the overthrow of by far the greater portion of the buried timber in our peat bogs" (Trans. Roy. Soc. Edin., xxiv. 380).

In a letter written by Lord Cromarty, in 1710, on peat mosses, and published in the twenty-seventh volume of the Philosophical Transactions, we get a curious account of the swallowing up of a forest by a peat bog. In 1651 the Earl saw, in the parish of Lochburn (or, as Walker says, at Loch Broom, in West Ross), a plain with fir-trees standing on it, all without bark, and dead. Of the cause of their death he says nothing. Fifteen years after, he found the whole place a peat moss or "fog," the trees swallowed up, and the moss so deep that in attempting to walk on it he sank in it up to his armpits.

But, it will be said, assuming that this may be the case with one growth of forest, how about the successive destruction of successive forests? The answer is, I believe, to be found in the curious change which peat undergoes, and which converts it from a substance highly absorbent of water into one impervious to it.

The section exposed by a peat-cutting in, I believe, almost all cases exhibits two kinds of peat, the one known variously as red peat—or red bog, or fibrous bog, or in Somersetshire as white turf—which lies at the top, and the other, is a black peat, which lies at the bottom. The red peat retains visible traces of the Sphagnum of which it is mainly composed, and is highly absorbent of moisture; whilst the black peat has lost all, or nearly all, traces of the minute structure of the cells, and is not only unabsorbent of moisture, but is impervious to it. In fact, it constitutes an insoluble substance which is said to be scarcely subject to decay, so that it is used in Holland for the foundations of houses, and is found unchanged after ages, and when the buildings have fallen into decay. It is even said to have remained unchanged after three months' boiling in a steam-engine boiler. The broad difference between these two kinds of peat may easily be ascertained by anyone who will subject the two kinds to the action of water.

If we now take a section of a peat bog, with a succession of forests one above another, the history of the formation will be, I believe, much as follows:—

(1) We must get a water-tight bottom—sometimes this is a stiff clay, sometimes a pan, *i.e.* a stratum of sand or gravel made into a solid plate by the infiltration of insoluble iron oxides, themselves often due to decaying vegetable matter. The necessity of this water-tight bottom is well shown by the fact that in places in the Irish bogs where a limestone subsoil occurs the bog becomes shallow and dry.

(2) If on this clay bottom or sandy or gravel soil a forest arises, it may flourish for a considerable period, until the natural drainage of the area is stopped, whether by the choking up of the course of the effluent stream, or from the aggregation of vegetable matter, or from the fall in the course of nature of the trunks of the trees themselves. Everyone who will consider how much care our rivers require in order to make them flow with regularity to the sea—who thinks, for instance, of the works in the Thames valley, or in the upper valleys of the Rhine—will see how often and how easily, in a country in the condition of nature, stagnant waters will arise. In the morass thus formed the Sphagnum has grown, years after years, and if it has not destroyed the old trees it has prevented the growth of young ones. The stools of the trees buried in the antiseptic waters of the Sphagnum pools have been preserved, whilst the fallen trunks have, except when preserved by the like circumstance, rotted, and added their remains to the peat which the Sphagnum has been producing. It has been observed in several places in Scotland, that the under side of fallen trees which would be protected from decay by the tannin of the Sphagnum is preserved, whilst the upper side has decayed or rotted away. Year by year the process of decay on the lower parts of the Sphagnum goes on until the water grows shallower and at last disappears, leaving the original morass choked and filled up by the Sphagnum and the plants which it has nourished. On the top of this soil have grown first the heathy and bog shrubs which first succeed the Sphagnum, and in time, as the soil has grown more solid, forest trees. This is our second forest. This first peat deposit, or the lower part of it at all events, having been turned into the black peat impervious to water, plays the same part in the next stage that the clay or pan did in the earlier stage. Again, the drainage of this second level gets stopped, and the forest bottom is loaded with stagnant water, the home of the Sphagnum; together, the water and the Sphagnum kill the forest trees, which share the fate of their predecessors. The same history is gone through again—the Sphagnum filling up the morass and turning the water into dry land until it supports the third forest, and so on to the end.

Decay of the Moss.—Then comes, however, in many cases a time when this process is arrested; the artificial drainage of the soil, or the physical position of the area, prevent the re-formation of a morass, and the Sphagnum dies away. So in many parts, if not universally, in Sedgmoor, in Somerset, it is almost impossible to gather a bit of Sphagnum, and the peat is well known to the peat diggers not to be reproduced. Here the regulated drainage of the level maintains the surface in the condition of meadows or agricultural land. But in many cases, especially on mountain sides or tops, when the Sphagnum has died, and the peat undergone its last change into black earth, a process of decay sets in under the influence of air and water. The water lies in holes or "hags," or flows in sluggish streams, wearing away the dead peat; and the surface of the soil is broken and uneven, small patches of green surface with a rough growth of sedge or grass being surrounded by wider spaces of black earth. Such is, or was some years ago, the condition of the peat on the top of Kinder Scout in Derbyshire; on the parts of Dartmoor around Cranmere Port; and such also it is described to be on many of the Lowland hills of Scotland.

Sedgmoor.—In some cases the peat mosses have been originally arms of the sea, and the peat has only grown after the exclusion of the salt water. Such appears to be the history of Sedgmoor, the great plain of Central Somerset. Northward it is bounded by the Mendips; eastward lies Glastonbury with its Tor or hill; westward the Bristol Channel. The plain is intersected by the low line of the Pouldon Hills, once a long level-backed island in the estuary and afterwards in the morass: and the way in which the villages lie and the moor is apportioned between them suggests that the Pouldon Hills and some other spots which slightly rise above the level of the moss were the original seats of population. Originally this whole area appears to have been open to the Bristol Channel, of which it formed a bay or recess. The Burtle beds are a marine deposit well seen at the slight elevation on which the village of Burtle stands, which have been traced in various places along the borders of the moor and indicate the old line of beach. A curious confirmation of this geological fact is afforded by the presence—the one on Shapwick Heath, and the other near Glastonbury—of two plants (the *Rumex maritimus* and the *Vicia lutea*) which are shore plants, but which have until recently maintained their places as remains of the ancient marine flora, showing the retreat of the sea. The *Vicia lutea* has, I believe, recently succumbed in this interesting locality to the British collector. The description of Glastonbury as the Isle of Avalon, and the account of the bringing of the body of King Arthur from Tintagel to its resting-place at Glastonbury, are confirmations from tradition of the same fact.

Then a change came over the district, apparently by the formation of a barrier of sand or mud along what is now the shore of the Bristol Channel, and in that way the sea-water was shut out, and a depressed region left with a mud surface; on this the Sphagnum grew, and gradually filled it up, but leaving down to historic times spaces of fresh water from which the Abbots of Glastonbury formed their great fishing lake at Meare, by the side of which they erected the beautiful manor house and fish house which still remain. When the Romans occupied this part of England, they not only used the Burtle beds for plastic clay, but used the peat in their kilns, and the remains of the road which they constructed across the moor are now found some 6 feet below the present surface. In like manner, the Glastonbury monks formed a pathway across the moor from their own abbey to Burtle, where they appear to have had a chapel which they served. It consists of alder trunks laid crosswise so as to form a kind of corduroy road. Its remains near Westhay are said to be about 12 feet below the surface. Now, as I have already said, the system of drainage is so complete that the peat, when once cut, is not reproduced (though the lower soil is said to have a remarkable power of expansion and rises often to the old level), and the Sphagnum is to be found rarely if at all on many parts of the moor.

To the intimate structury of the turf moss are thus to be attributed great results in the history of the world. To look at our own island alone, but for it the primæval forests that once covered the land might still be standing; but for it large tracts of land would still be lake and mere; but for it every freshet in a highland river would be a flood; without it we should have had no mosses on the confines of England and Scotland, and where would have been the border warfare and the border minstrelsy? where the moss hags in which the hunted Covenanters sought for shelter and freedom of worship? Or, to come southward, by force of its growth the broad meadows of Somerset have been built up, and the dark waters on which the mysterious barge bore the dead Arthur from Tintagel to Avalon have been turned into the green pastures of Glastonbury and Meare and the battle-field of Sedgmoor.

FROM TONGKING TO CANTON.

THE paper read at the meeting of the Royal Geographical Society on Monday was on a journey from Haiphong in Tongking to Canton, overland, by Mr. A. R. Agassiz in the early part of 1890. He went by steamer from Haiphong to Phu-lang-thuong, thence to Langson and across the frontier into the Chinese province of Quang-si.

The country between Phu-lang-thuong and Langson is briefly described as follows:—First stage, to Kep: country perfectly flat and well cultivated. Second stage, to Bac-le: hilly, with numerous groves of bamboo and tropical trees. Third stage, to Than-moi: very hilly, some of the hills being well timbered. Last stage: cross mountain range, passing Thien-ho, situated at the highest point reached by the road, which is probably not less than 3000 feet above the sea-level. Langson stands in the centre of a small plain at the foot of these mountains; the French portion of the town and the citadel on the left bank of the Sung-chi-chiang river, and the native town on the right bank.

As to the Sung-chi-chiang river, its source is not exactly known, but Mr. Agassiz was assured by the officer in charge of the convoy with which he travelled that it enters Tongking from the province of Kuang-Tung. It then turns sharply to the north, passes Langson, and then flows on in the same direction to a place called That-khe, below which it is navigable for small boats. From here it takes an easterly course, and re-enters China (province of Quang-si) at Ping-erh-kuang. At Lung-chow, thirty miles farther, it unites with a river called the Kao-ping-ho, which rises in Yunnan, and crosses the north-east corner of Tongking, passing the French garrison town of Cao-bang.

At Chin-nan-kuan, after passing through a massive stone archway, Mr. Agassiz entered Chinese territory. There is a village here, but the town of Chin-nan is about two miles distant. Further on is Lung-chow, a walled city, said to contain 20,000 inhabitants. Many of the houses are of the bamboo-and-mud style of architecture; but the Yamens, and the residences of the better class of the people, are built of brick.

The only article produced in the district, that is not wholly consumed locally, is sugar, which is said to be cultivated with much success. Mr. Agassiz walked through many of the cane-fields, but could not see any cane that in point of size would compare with cane he has seen in the north of Queensland. The fields would have looked better for being trashed.

Nearly every planter possesses his own crushing machine, constructed in the following primitive way: two stout hard wood posts are placed firmly in the ground about 10 feet apart, and secured between them, at a height of 1 foot from the earth, is a plank 2 inches thick by 10 broad. On this plank, standing on their ends, and almost touching each other, with their lower axles fitted into holes bored in the plank, are two hard wood rollers, of 2 feet diameter. These rollers are connected at their upper edges by cogs, so that one cannot revolve without the other. Above them, with holes for their upper axles to fit into, is another plank, secured to the upright posts at its ends. The upper axle of one roller is longer than the other and protrudes a foot above the upper plank, with a hole bored through it, into which is fitted one end of a 15-feet pole. An ox, made fast to the other end of the pole, keeps the machinery in motion by walking round and round it, while a man, sitting on the ground, feeds the machine by placing the ends of the canes between the rollers, crushing five or six at a time. A trough beneath the machine catches the juice.

After remaining in Lung-chow for a fortnight, Mr. Agassiz started by boat for Canton on March 11, 1890.

Below Lung-chow the river is called the Tso-chiang or Left river; and is formed by the junction of the Sung-chi-chiang and Kao-ping-ho; none of which rivers are marked on any English maps of China.

Tai-ping, the only town of any size on this river, is situated on the left bank, three days' journey below Lung-chow.

On the 18th, four days after passing Tai-ping, Mr. Agassiz came to the junction of the Tso-chiang and the Yü-chiang, below which place, to its mouth near Macao, the river is called the Hsi-chiang, or West river.

The Yü-chiang, navigable up to the town of Pe-se, has long been one of the highways to the province of Yun-nan; but during the past year the opening of the Red River in Tongking has taken away much of its trade. Nan-ning, the most important town in Western Quang-si, is favourably situated about six hours' journey below the junction of the two rivers.

As to the general aspect of the country through which Mr. Agassiz travelled, its chief feature, and certainly a most striking characteristic, is the peculiar formation of certain hills, which, as Mr. J. G. Scott has said in his book "France and Tongking," are formed of a kind of prismatic limestone, and rise sheer out of the rice-fields to a height, in some cases, of 1000 feet. The first of these hills that Mr. Agassiz saw was a range on the southern side of the military post of Bac-le, in Tongking, after passing which he saw no more ranges, but numerous isolated hills. The mountains he crossed before entering Langson, which form the natural, although not the geographical, frontier of Tongking, are not of this formation. In the vicinity of Lung-chow, they are very plentiful, and in most cases have a peculiar turretted appearance. The river Tso-chiang passes frequently close under their perpendicular sides, and at times, when Mr. Agassiz was descending that river by boat, it required but a small effort to imagine one of these hills to be the ruin of a huge mediæval castle. Below Nan-ning, he saw no more of them, but at rare intervals.

Nan-ning is a large town, in fact, in point of population, it stands third on the list of the towns of this province.

Below Nan-ning, in some places ridges of rock stretched almost across the river, and the water, being thus partially dammed up would rush round the edge of a ridge, or through a breach in one, at a frightful rate.

At the junction of the Hung-shui-chiang and the Hsi-chiang, is situated the town of Hsun-chow.

With Hsun-Chow a traveller is likely to be disappointed, as its position induces one to expect to find a place of considerable commercial importance, which it is not. The city, surrounded by a wall, probably contains about 40,000 inhabitants; and as it is the chief town of one of the departments into which the province of Quang-si is divided, is a place of some official importance; but it has not the busy aspect of Nan-ning.

Below Hsun-chow the river is a splendid expanse of water, which might be rendered navigable to steamers by a slight expenditure of engineering skill.

In two days, after leaving Hsun-chow, Mr. Agassiz reached Wu-chow, the commercial capital of the province. This place is Canton on a reduced scale. Its houses, built, some of them, close to the water's edge; its boats, with an enormous floating population—for people here are born, live, and die in their boats; and the dress, and the general aspect of the people as they busily pass along its crowded streets, all recalled vividly to Mr. Agassiz the features with which he had formerly been familiar in the latter city.

The River Tan-chiang, on which is situated the town of Kwei-lin, the official capital of the province, here enters the Hsi-chiang, making Wu-chow a turning-point round which boats must pass when journeying between the chief towns of the two Kuangs, as the provinces of Kuang-Tung and Kuang-Hsi are frequently called. The Tan-chiang is, like so many of the rivers of China, variously named on different maps; on some it is called the Fu-ho.

Below Wu-chow the country is hilly, and the river deep to the Shao-hing gorges, through which it passes, with hills rising on either side to a height of 700 or 800 feet.

A short distance below the gorges the waters of the Hsi-chiang are augmented by the Pei-chiang, or North River; which is a very considerable stream, rising in the mountainous country on the borders of the Kuang-Tung and Kiang-Si provinces. Below this junction the Hsi-chiang begins to split up, and finds its way to the sea through many mouths. The great branch enters the ocean near the Portuguese colony of Macao, and rightly bears the name of the parent river whose first-born it is. Another branch, only second in size to the one already mentioned, is the Canton river, which enters the sea close to the historical Bogue Forts. Opposite to Canton this river divides into two branches, which lower down unite, forming the Island of Honam. The larger of these branches, called the Back Reach, was the one most used for the purposes of steam navigation; but since the time of the late Franco-Chinese troubles it has been closed by a barrier, which makes it impossible for ocean steamers to reach Canton, as the other branch, called the Front Reach, is too shallow for any but light-draught river steamers to ascend. Ocean steamers have consequently, for the past five years, been obliged to discharge their cargoes at Whampoa, twelve miles below Canton. The name "Canton

River" is by some map-makers already erroneously applied to the whole of the Hsi-chiang river. It would be as correct to call the River Ganges the "Hoogli."

THE ST. PETERSBURG ACADEMY OF SCIENCE.

WE have before us the yearly Report of the St. Petersburg Academy of Science, drawn up by its new secretary, Prof. A. A. Strauch; it is full of interest, as it gives a careful analysis of the scientific work done by the Academy. After having mentioned the losses sustained by the Academy, and the new members elected, Prof. Strauch passes in review the scientific institutions connected with the Academy. The Pulkova Observatory is now under the directorship of the Moscow Professor, Th. Bredichin, well-known for his researches into the structure of comets; the Physical Observatory, under H. Wilde, has added to its former weather warnings a system of warnings of snowstorms, which are sent to the Russian railways. A new laboratory for researches into the physiology and anatomy of plants has been opened; while the remarkable ethnographical and anthropological collections of the Academy (which contain the collections brought in by Krusenstern, Lütke, Junker, Miklukho-Maclay, Polyakoff, and so on), have been lodged in a separate museum, now opened to the public. Rich collections, especially zoological, from Caucasia, Turkestan, and Mongolia, were received during the past year. Among the recent acquisitions of the library, Mr. Friedland's collection of Hebrew printed works, old and new, some of which are very rare, is especially valuable.

As to the scientific work done during the last year, the following are especially worthy of notice.

In mathematics, Prof. Ishmenetsky, continuing his researches into the functions of Bernoulli, has shown the use which may be made of them to explain the geometrical meaning of Euler's formula for the approximate calculation of surfaces limited by curves; Prof. Markoff's work on the transformations of slowly convergent series into rapidly converging ones, and M. Bortkevitch's researches into the average duration of life in Russia, are also valuable contributions.

In astronomy, Prof. Backlund, besides geodetical work in the north of Russia, continued his calculations of the ephemerides of Encke's comet, which will reappear this year.

In physics, O. D. Chwolson's work upon the conductivity of metals at various temperatures is mentioned.

In meteorology, we find, besides a review of the already known publications of the Central Physical Observatory, special reference to H. Wilde's memoirs on a new (very practical) instrument of his own invention for measuring magnetical inclination, as also on his anemograph, registering pluviometer, and atmograph.

In chemistry, Prof. N. Beketoff continued his work upon the physical and chemical properties of cæsium and its oxides.

In geology, Dr. Rogon published an interesting work upon the Ganoid fishes of the Upper Silurian deposits of Oesel, as also on the Jurassic fishes of Ust-Balei in East Siberia. The six species discovered in these last deposits are intermediate forms between the Mesozoic Ganoids and the *Teleostei*. M. Tschersky's work is especially interesting: taking advantage of more than 2500 specimens (70 species) of fossil Mammalia discovered in Northern Siberia, he prepared a most elaborate monograph on Post-Pliocene Mammalia, which contains, first, a full account of what is already known about the Quaternary mammals in Siberia, a description of the Post-Pliocene formations of Siberia generally, and their mammalian fauna, with incidental remarks upon the fauna of the caves, and, finally, a very good systematic description of 25 Post-Pliocene mammals.

In botany, the work of Prof. Maximowicz on the flora of Tibet is prominent. This flora is of high antiquity, and consists, besides its own endogenous species, of immigrants from both the Himalayas and the mountains of Mongolia. Many of those immigrants have already evolved into distinct species. Later immigrants came from China, and, later on, the Tibet flora was completed by our familiar northern plants. The orographical division of Tibet into a plateau in the west, and Alpine tracts in the East holds good for the flora as well. As to the flora of Mongolia, it is an impoverished continuation of the flora of South Siberia. Prof. Famintsyn continued his researches into the symbiosis of Algæ with Infusoria. The green grains often seen in several Infusoria proved to be Algæ having a nucleus, chromatophores, and covered with a jelly-like envelope; their structure is identical with that of monocellular Algæ, and they multiply within Infusoria by partition. But they are incapable of an independent life, and die out soon after the death of the Infusorium they have lived in. Further research is now being carried on to ascertain in what conditions they might live independently.

In zoology, the chief work of the Academy consisted in the publication of the zoological results of Prjevalsky's expeditions. Two fascicules have now been issued containing the description of the Rodents, by E. A. Bichner, and the description of the families of *Silviidæ*, *Timeliidæ*, and *Accentoridæ*, by Th. Pleske. The chief interest of the latter fascicule is in the new genus of birds, *Lophobasileus*, which appears to be a connecting-link between the *Sylviæ* and the *Regulus*. S. M. Hertzenstein described some new fishes from the Russian Pacific coast, and E. A. Bichner made a preliminary review of a small but very interesting zoological collection brought in by MM. Potanin and Berezovsky from the Chinese province of Kansu, and now lodged at Irkutsk. Th. Pleske issued the fourth fascicule of his "Ornithographia Rossica," which contains the description of ten Russian species of *Acrocephalus*.

In anatomy and physiology, Prof. Owsiannikow continued his researches into the striation of some nerves, and Dr. Tarenetski described forty-four Aino skulls from the island of Saghalien. The author is inclined to admit that they belong to a race quite different from the Mongolian.

In ethnography the Report mentions the following works:—Dr. Bilenstein has terminated an important work upon the geographical distribution of the Letts, now and in the thirteenth century, in Courland and Livonia. In view of the capital interest of this work, it will be published by the Academy separately, with an atlas of maps. Prof. W. Radloff has published a *facsimile* of a most important document, the "Kudatku-bilik," which is the oldest representative of the Uigur language, and has, for Turkish dialects, almost the same importance as Ostromir's Gospel has for the Slavonian languages. To complete the historical and linguistic materials which will be associated with this publication, M. Radloff consulted the Eastern manuscripts of the British Museum, and is now preparing a general work upon the subject. In connection with the above, Prof. Eitling, of Strasburg, prepared for the Academy a table of Uigur, Mongol, and Mantschu alphabets, which shows that they originated from the Syrian alphabet. The likeness between Syrian and Uigur letters also permits us to guess the sounds which separate letters had in the Uigur language. Prof. Wasilieff's notes on his journey to West Siberia are also worthy of note. The learned Professor is now preparing a work on the geography of Tibet, as well as the second volume of his great work on Buddhism. Finally, M. Katanoff (of Sagai origin) visited, last year, Northern China and Turkestan, and collected a good many interesting materials relative to the Tartars, and especially the now rapidly disappearing Soyotes. His collection of tales, songs, Shaman prayers, &c., is remarkably interesting, the more so, as all has been written down in the Soyote language (with Radloff's Turkish alphabet), and transcribed for print, on the spot, among the Sagais, who speak the Soyote language correctly.

P. K.

A METHOD OF DETERMINING SPECIFIC GRAVITY.

THE specific gravity of a single Foraminifer, such as a Globigerina, of the scales from a butterfly's wing, or of a drop of its blood, might seem a difficult task to ascertain, as indeed by the ordinary gravimetrical methods it would be plainly impossible. Yet nothing can be easier, given the following method. And to conduce to brevity we shall describe its application in a particular case, say to the spicules of the common shore sponge (*Halichondria panicea*). A quantity of one of the well-known heavy fluids, such as cadmium-boro-tungstate, or potassium-mercury-iodide solution, or methylene iodide, is diluted down to a density of about 2·25 (which is known to be above that of the spicules), and introduced into a small glass tube, about one-quarter of an inch in diameter, and with two opposite flattened faces. This is cemented by one of its flat faces to an ordinary microscope slide, the axis of the tube being set at right angles to the length of the slide.[1] The tube being about half-filled with heavy fluid, water (or in the case of methylene iodide,

[1] See Proc. Roy. Dublin Soc., vol. iv. p. 374, 1885, and Journ. R. Micr. Soc., vol. v. p. 879.

benzole) is poured in, and this not too carefully, since partial admixture will serve to expedite the process. The vessel is now left to stand for a few hours or over-night. In the morning the change in the specific gravity of the column of fluid will be found to increase *uniformly* on passing from the top downwards. To make certain of this three small indexes of different specific gravities, say 2·15, 2·03, and 1·98, are thrown in: naturally they sink till they each reach that level in the column where the specific gravity is identical with their own—"their own level," as we may briefly term it. The distances between them may be determined by bringing them successively into the focus of the microscope, and then reading off the position of the edge of the microscope slide by means of two parallel scales attached to the stage of the instrument.

If it be found that these distances are exactly proportional to the known differences of specific gravity, then the increase in density of the column must be uniform. It will save arithmetic if the distances and densities be referred to two rectangular axes, and a curve constructed; evidently when the change in density is uniform, the curve will be a straight line.

The sponge spicules are next introduced (in practice they are added at the same time as the indexes), and, like the indexes, sink to their own level. The position of this is read off as in the case of the indexes, and being referred to the axis of distances in the diagram, the specific gravity will be found on the corresponding axis of density. In this way it is easy to determine the specific gravity of individual spicules, and with a certainty that does not always attach to specific gravity determinations on large bodies by ordinary methods. The specific gravity of the spicules as a result of actual experiment is found to be 2·036, the great bulk of them floating at a level indicating this, but individual examples may sink to 2·09, while others do not fall below the level of 2·02.

It is only rarely that one needs to employ a microscope in this method; usually a test tube serves admirably to hold the heavy fluid, and a graduated glass mirror affords the speediest means of reading off the distances of the levels. So easy, accurate, and sure, has experience shown this method to be, that I now use it whenever practicable in preference to any other: unfortunately its range is limited to specific gravities below 3·45.

The indexes may be small fragments of minerals, or chemically pure substances of known density, such, for example, as yellow phosphorus (1·85), sulphur crystallized from carbon disulphide (2·07), and the like; or little floats may be made by manipulating short lengths of capillary glass tube. The ends of the bit of tube are successively sealed, the last to be fused generally blowing out into a little bubble during the process, and thus giving a form to the float which causes it to assume the vertical position when suspended in the heavy fluid. The quantity of solid glass in the float as compared with hollow, is determined chiefly during the sealing of the first end, since this merely fuses together without blowing out. Having constructed a good supply, some dozens, of these floats, they are thrown into a diffusion column along with three mineral indexes of carefully determined density: by reference to these, the specific gravity of each float can be found, and it can then be fished out from the fluid with a dipping tube, and preserved in a labelled box for future use.

The density of fluids can be ascertained in the same way as that of solids; but if the fluid be watery, an oily fluid such as methylene iodide, with benzole to dilute it, must be used for the diffusion column; while if it be oily, the diffusion column must be an aqueous solution, it may be of cadmium-boro-tungstate, or some other heavy salt.

There is, finally, one other case in which density can be determined by this method, while all others fail: given a gelatinous precipitate, like some of the silica hydrates, in which one has a sponge of loosely combined hydrate with additional water mechanically present filling the meshes, to find the specific gravity of the hydrate. This is not possible by ordinary methods, for one cannot be sure that no combined water is lost in the process of drying, which must precede the application of gravimetrical processes. But if a small clot of the gelatinous substance be introduced into a diffusion column, the mechanically mixed water will slowly diffuse out, and its place be taken by heavy fluid till the hydrate has reached its own level, where it will rest, its meshes then being filled, no longer simply by water, but by solution of the same specific gravity as its own. I am at present investigating the composition of the silica hydrates by a process based on this method. W. J. SOLLAS.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The General Board of Studies propose to appoint Mr. J. J. Lister, M.A., of St. John's College, Assistant to the Superintendent of the Museum of Zoology and Comparative Anatomy (Mr. J. W. Clark) for a period of three years. He will give special attention to the preparation of a new Catalogue of the Museum.

The Mechanical Workshops Inquiry Syndicate make proposals which virtually amount to the establishing of an Engineering School and Laboratory under Prof. Ewing, by recommending an annual expenditure of some £700 for demonstrations and apparatus. Another Syndicate report in favour of reserving a considerable area of ground between the Chemical and the Physical Laboratories for buildings in which the Engineering School may be accommodated. But the erection of these buildings and their equipment with suitable plant depend on a handsome response being made to the appeal for funds from outside the University which Prof. Ewing has in contemplation. It is to be hoped that his energy and fervour in the cause of engineering education in Cambridge may be adequately rewarded by generous benefactions to the Engineering School.

The Sites Syndicate propose that the remainder of the ground available in the New Museums area should be assigned (1) to the necessary extension of the Cavendish Physical Laboratory; (2) to the Botanical Department for new class-rooms, &c.; (3) to the Departments of Medicine, Surgery, &c.; (4) to the Sedgwick Memorial Museum of Geology; and (5) to the temporary accommodation of the Classical and Modern Language Lecturers. The Syndicate have been unable to assign special rooms to the Lecturer in Geography.

SCIENTIFIC SERIALS.

American Journal of Science, February.—A solution of the aurora problem, by Prof. Frank H. Bigelow. The question considered is the location in space of the visible aurora arch and streamers, referred to the surface of the earth, as seen by an observer. The observations required to test the theory which is developed consist in measuring the angle of inclination of a streamer to the vertical plane passing through the station, together with the azimuth of the ray. A simple piece of apparatus, suitable for such measurements, is described. The problem is of importance, because it bears upon the physical connection between the sun and the earth, as communicated through the medium of the ether.—Columbite and tantalite from the Black Hills of South Dakota, by W. F. Headden. A full description is given of the constitution of these minerals.—Note on the geology of the Florida phosphate deposits, by N. H. Darton.—Record of a deep well at Lake Worth, Southern Florida, by the same author. The well penetrated the great sand mantle at Lake Worth, and extended down into the Vicksburg limestone to a depth of 1212 feet. The section obtained from the borings is an important one, inasmuch as it throws some light on the general stratigraphy of a portion of Florida of which little was hitherto known.—On the chemical composition of aurichalcite, by S. L. Penfield.—The compressibility of hot water and its solvent action on glass, by Carl Barus. As a general deduction from the experiments, the author infers that in many instances a definite dissociation temperature of the solid must first be surpassed before solution will set in.—An attempt to harmonize some apparently conflicting views of Lake Superior stratigraphy, by S. R. Van Hise.—Powellite calcium molybdate, a new mineral species, by W. H. Melville.—Brückner's "Klimaschwankungen," by Frank Waldo. This is an extended review of Dr. Edward Brückner's important work on oscillations of climate, published last year.—The gigantic Ceratopsidæ, or horned Dinosaurs of North America, by O. C. Marsh (with ten plates); read at the Leeds meeting of the British Association for the Advancement of Science, 1890.

Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, new series, vol. i. No. 4.—On the products of condensation of benzaldehyde and the phenols, by A. Rusanoff (in German).—List of the (thirteen) *Scotylus* species in the Museum of the Academy, by P. Shevyreff Two species (*S. ventrosus* and *S. unispinosus*) are new.—On some observations made by Winnecke at Pulkova with the meridian circle in 1861–63, by O. Backlund (in German).—

New anemograph and anemoscope, by H. Wild (in German). The former has been at work at St. Petersburg since 1887, and gives satisfactory results; the second, which is a simplification of the former, has been in use at St. Petersburg and Pavlovsk for more than ten years, and also works quite satisfactorily.—On the law of reciprocity of quadratic residues, note by Ed. Lucas (in French).—On the structure of nerve-filaments, by Ph. Owsjannikoff.—Apocryphal Acts of Apostles in Coptic language, by O. Lemm (in German).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 5.—"On a Membrane lining the Fossa Patellaris of the Corpus Vitreum." By Prof. T. P. Anderson Stuart.

The existence of a membrane here had been the subject of discussion *pro* and *con.* till 1886-87, when the matter was considered by some to have been finally set at rest by Schwalbe, who decided against it in very clear and explicit terms. According to his description the vitreus jelly itself lies against the lens capsule and forms the posterior boundary of the canal of Petit—if such it could be called, for the canal, he says, is merely to be compared with the other clefts in the vitreus. Any membrane that had been seen he declares to be an artificial product, the result of the action of reagents. The author finds, however, that in the perfectly fresh, unaltered eye, after the removal of the lens in its capsule, there may be raised off the surface of the jelly a membrane which, when stained and mounted, does not show any structure. When the membrane from the four-year-old ox eye was isolated and tied over the mouth of a test-tube ½ inch wide, it sustained a column of water 40 inches high. A smaller column than this may be sustained for days together. When isolated, it may be dried to form a delicate membrane. It is thinner in the centre where it lies against the lens capsule, thicker peripherally where it forms the posterior wall of the canal of Petit, which is thus a true canal. Thus, when the entire vitreus is squeezed, the centre of the anterior face bulges more than the periphery. The line of demarcation of the two parts is fairly sharp. The sun's rays concentrated upon it show fluorescence as marked as in the case of the hyaloid, and at a puncture the sharp fluorescent edge is strikingly different from the jelly showing through the hole. Treated with picrocarmine by the Gross method (to be described in the next number of the *Journal of Anatomy and Physiology*), the membrane is red, the jelly is yellow, and now its wrinkles are seen just as in the case of the hyaloid. In successful meridional sections the membrane is seen *in situ.* For ophthalmological practice, a knowledge of the existence of the membrane is most important, and the observations of ophthalmologists strongly support the author's observations.

"On the Connection between the Suspensory Ligament of the Crystalline Lens and the Lens Capsule." By Prof. T. P. Anderson Stuart.

The common teaching is that there is a direct continuity of substance between the suspensory ligament and the capsule of the lens, but an observation by the author of the paper seems to indicate that the ligament is only cemented to the capsule. On opening eye-balls in an advanced state of decomposition—putrid—he found the lens in its capsule perfectly free, and no indication of any rupture of tissue along the line of attachment of the suspensory ligament. This ligament was found perfectly intact projecting from the collapsed vitreus body as a sort of frilled ring with a free edge. These points are best seen after the Gross staining of the structures, as described by the author in the next number of the *Journal of Anatomy and Physiology.* The observation bears upon the still unsettled question of the development of the lens capsule, and upon cases of detachment of the ligament from the capsule sometimes met in ophthalmological practice; also on cases of atrophy and solution of the suspensory ligament, and cases of luxation of the lens.

"A Simple Mode of Demonstrating how the Form of the Thorax is partly determined by Gravitation." By Prof. T. P. Anderson Stuart.

Remembering how constant and how potent is the action of gravitation, and arguing that the segments of the thorax were so many rings of more or less elastic matter, the author concluded that, if similar rings of any other elastic material were suspended in the same way, the form of the thoracic segments should be reproduced, provided there intervened no other condition strong enough to counteract the action of gravitation. The author has found crinoline steel most convenient, though bands of paper do very well. The form of the thoracic segment of the quadruped, of the human fœtus, and of the human adult, are reproduced in succession if the ring be held between finger and thumb, and turned, from lying in the vertical, till it lies in the horizontal plane. The complete reproduction of the different features of the adult human thorax at its most characteristic level is most striking. This is when the steel, as usually sold, is about 6 feet long and ½ inch wide. As the ring is made smaller, the forms of the higher segments appear in succession. The points which are thus reproduced are so numerous and simultaneous that the author cannot believe them to be mere coincidences, and he therefore concludes that gravitation has had a greater influence in determining the typical form of the thorax than would be generally admitted. This is supported by the shapes assumed by the steel rings when the mode of suspension is varied from the normal, as in deformities of the vertebræ: here the particular form in the individual—the thoracic deformity—is more or less accurately reproduced.

Physical Society, February 13.—Annual General Meeting.—Prof. A. W. Reinold, F.R.S., Past-President, in the chair.—The reports of the Council and Treasurer were read and approved. From the former it appears that there has been a satisfactory increase in the number of members, and in the average attendance at the meetings. During the year a translation of Prof. Van der Waals's memoir on the continuity of the liquid and gaseous states of matter has been issued to members, and it is hoped that the translation of Volta's works, now in hand, will be published before the next general meeting. The Council regret the loss, by death, of Mr. W. H. Snell and Mr. W. Lant Carpenter, and obituary notices of these late members accompany the report. The Treasurer's statement shows that the financial condition of the Society is very satisfactory, and that the sales of the Society's publications have increased considerably.—A vote of thanks, proposed by Mr. Whipple and seconded by Dr. Gladstone, was unanimously accorded to the Lords of the Committee of Council on Education for the use of the room and apparatus. Dr. Atkinson proposed a vote of thanks to the auditors, Prof. Fuller and Dr. Fison, which was seconded by Dr. Thompson, and passed unanimously. The proposer, in referring to the satisfactory nature of the accounts, recommended that the publications of the Society should be brought before physicists and other students of physical science, and Dr. Thompson heartily concurred in this recommendation. A third unanimous vote was accorded to the President and Officers for their services during the past year, the proposer and seconder being Dr. Waller and Prof. Minchin.—The following gentlemen were declared duly elected to form the new Council: President: Prof. W. E. Ayrton, F.R.S.; Vice-Presidents: Dr. E. Atkinson, Walter Baily, Prof. O. J. Lodge, F.R.S., Prof. S. P. Thompson; Secretaries: Prof. J. Perry, F.R.S., T. H. Blakesley; Treasurer: Prof. A. W. Rücker, F.R.S.; Demonstrator: C. V. Boys, F.R.S.; other members of Council:—Shelford Bidwell, F.R.S., W. H. Coffin, Major-General E. R. Festing, R.E., F.R.S., Prof. G. F. Fitzgerald, F.R.S., Prof. J. V. Jones, Rev. F. J. Smith, Prof. W. Stroud, H. Tomlinson, F.R.S., G. M. Whipple, James Wimshurst.—The meeting was then resolved into an ordinary science meeting, and a paper on the change in the absorption spectrum of cobalt glass produced by heat, by Sir John Conroy, Bart., was read by Mr. Blakesley. The absorption spectrum of cobalt glass, when cold, consists of three dark bands in the red, yellow, and green, with a considerable amount of absorption between the first two. When a piece is heated to nearly red heat, the absorption between the first two dark bands diminishes, and the band in the red moves towards the least refrangible end of the spectrum, whilst those in the yellow and green retain their position, but become less distinct. During the heating of the glass the intensity of its colour diminishes, and as the glass cools its original colour and absorption spectrum returns. Diagrams and numbers showing the character and positions of the bands in hot and cold glass accompany the paper, together with the numbers obtained by Dr. W. J. Russell (Proc. Roy. Soc., xxxii. p. 258) for cold cobalt glass. In conclusion, the author says that these observations, and those of Feussner on solutions, show that the absorption spectra of some substances vary with temperature. In solutions, this may be due to formation of different hydrates or to partial dissociation, but in a solid like cobalt glass an actual change in its chemical constitution at a temperature considerably below its fusing point does not seem probable. Dr. Gladstone said it was generally known that heat affects the colouring power of substances, and that in solutions absorption is greater the higher the temperature. Different solvents sometimes produce effects analogous to heat, for cobalt salt dissolved in water and in alcohol gives pink and blue solutions respectively, and rise of temperature makes the aqueous solution more blue. He concurred with the author as to the causes of the phenomena in liquids, and that the same explanation would not apply to glass. Prof. S. P. Thompson thought Sir John Conroy's results agreed with the experiments which Mr. Ackroyd showed before the Society some years ago, when he demonstrated that the colours reflected by opaque bodies, such as porcelain, &c., when heated, tend towards red.—Prof. Minchin showed some experiments in illustration of his paper on photo-electricity read at the previous meeting. In one of these, a seleno-aluminium battery, illuminated by the light of a taper, deflected an electrometer needle, thereby actuating a relay and ringing a bell. He afterwards exhibited one of his "impulsion cells" in action, and showed the change from the insensitive to the sensitive state produced by a Hertz oscillator at a distance. In the discussion, Mr. Tunzelmann said Kalischer and von Uljanin had worked at the same subject, the former being the first to make experiments on a photo-E.M.F. in selenium. His cells were made by winding brass wires on glass tubes and coating them with selenium, which was subsequently annealed. These cells lost their power after some time, and would not respond to feeble lights. By using two wires of different metals he obtained better results. Fritts, in 1883, used brass and gold plates coated with selenium, and Uljanin employed platinum plates deposited so thin as to be transparent. The latter experimenter found that the E.M.F. was proportional to the square root of the intensity of the light. He also observed that the orange-yellow of the prismatic spectrum produced the greatest effect, whereas the yellow-green and green rays of the diffraction spectrum gave the maximum E.M.F. Comparing these results with Langley's observations on the energy of the spectrum, it would appear that the E.M.F. bears no relation to the maximum energy falling on the surfaces. Speaking of the cause of the phenomena, he said the electrolytic idea of von Uljanin seemed inapplicable to Prof. Minchin's results, and he inquired whether a mixture of selenium and aluminium would undergo a gradual change by exposure to light. Dr. Gladstone said such a change, if it occurred, would be very slow, for nearly all difficult chemical reactions take time to complete. The fading of colours was adduced as an instance of slow chemical change produced by light. Dr. Waller thought the subject might throw light on the changes occurring in the retina, and asked if it was possible to separate thermo-electric and photo-chemical effects. Dr. Burton said he had suggested that the action of light on the retina was a photo-chemical one some time ago. But hitherto it had been difficult to obtain substances sensitive to any but the blue and violet rays, whereas the eye was most sensitive to green and yellow light. In the photo-electric batteries, however, the E.M.F. may generate a current and therefore energy, and the important question seemed to be, Where does this energy come from? Is it a chemical change precipitated by the action of light, or does a direct conversion of light into electric energy occur? Prof. Minchin, in his reply, said he thought his cells really transformed the incident energy. They were usually kept on open circuit, and there appeared to be no deterioration with time, the only change being a sluggishness in developing the maximum E.M.F.

Zoological Society, February 3.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of January 1891; and called special attention to a Yellow-crowned Penguin (*Eudyptes antipodum*) from Stewart Island, New Zealand, presented by Sir Henry Peek, Bart., new to the collection.—A letter was read from Dr. Emin Pasha, dated "Bussisi, October 6, 1890," announcing the despatch to the Society of a collection of birds which he had made on his way up from the coast.—The Secretary exhibited, on behalf of Mr. J. W. Willis Bund, a specimen of the Collared Petrel (*Œstrelata torquata*), which had been shot off the Welsh coast in Cardigan Bay in December 1889, and was new to the British Avifauna.—A communication was read from Dr. R. W. Shufeldt, containing remarks on the question of saurognathism of the Woodpeckers and other osteological notes upon that group.—Count T. Salvadori pointed out the characters of two new species of Parrots of the genus *Platycercus*, which he proposed to call *P. xanthogenys* and *P. erythropeplus*, both believed to be from Australia.—Mr. P. L. Sclater, F.R.S., gave an account of a collection of birds, from Tarapacà, Northern Chili, which had been made for Mr. H. Berkeley James by Mr. A. A. Lane. Fifty-three species were recorded as represented in the series, amongst which was a new Finch, proposed to be called *Phrygilus coracinus*.—Mr. F. E. Beddard gave an account of the pouch of the male Thylacine, from a specimen recently living in the Society's Menagerie. Mr. Beddard also described the brain of this animal, and pointed out its differences from the brains of other Marsupials.

CAMBRIDGE.

Philosophical Society, February 9.—Prof. Darwin, President, in the chair.—The following communications were made:—On rectipetality, by Mr. F. Darwin and Miss D. Pertz.—On the occurrence of *Bipalium kewense* in a new locality, by Mr. A. E. Shipley. The specimens exhibited came from an orchid house in the garden of Mr. Lawrence Birch, at Wiley, near Bath. They were apparently introduced in a miscellaneous lot of orchids whose origin was unknown. The species was first described by Prof. Moseley in 1878, from specimens obtained in the Kew hot-houses. Since then, individuals have been found at Haslemere, Welbeck Abbey, Clapham Park, and finally at Bath; they occur nearly always in hot-houses and a few at a time. So far the species does not seem to be becoming acclimatized. Abroad it has been recorded in the Botanic Gardens of Berlin, Frankfort, Cape Town, and Sydney, in the latter place in considerable numbers. The animals are very soft and extensible; they require a moist atmosphere, and die quickly in uncongenial surroundings. They are hermaphrodite, and reproduce both sexually and by transverse fission. They seem to be entirely carnivorous, devouring earthworms and small snails, &c. There is no reason to believe that they are ever harmful to plants. The mucus which is secreted from the skin, chiefly in the anterior region of the body, leaves a slimy trail along the track of the animal, which moves by means of ventrally placed cilia. The skin contains two kinds of urticating organs, (i.) simple rod-like bodies, the rhabdites; and (ii.) somewhat similar bodies one end of which is drawn out into a long thread.—The medusæ of *Millepora* and their relation to the medusiform gonophores of the *Hydromedusæ*, by Mr. S. J. Hickson. In *Millepora plicata* no medusiform structures of any kind were observed. The spermaria are simple sporosacs on the sides of the dactylozooids. The eggs are extremely minute, and show frequently amœboid processes: they are found irregularly distributed in the cœnosarcal canals of the growing edges of the colony. In *Millepora murrayi* from Torres Straits, large well-marked medusæ, bearing the spermaria, were observed lying in ampullæ of the cœnosteum. Even when free from the cœnosarcal canals and ready to escape they show no tentacles, sensory bodies, radial or circular canals, velum or mouth. They are formed by a simple metamorphosis of a zooid of the colony. The eggs of this species, like those of *M. plicata*, are extremely small and amœboid in shape. They are not borne by special gonophores. In the *Stylasteridæ*, the eggs are large, contain a large quantity of yolk, and are borne by definite cup-like structures produced by foldings of the cœnosarcal canals. In *Allopora* the spermarium is inclosed by a simple two-layered sac composed of ectoderm and endoderm. The endoderm at the base is produced into the centre of the spermarium as a simple spadix. In *Distichopora* the male gonophores are similar to those of *Allopora*, but there is no centrally placed endodermal spadix. In both genera a two-layered tube (seminal duct) is produced at the periphery of the gonophore when the spermatozoa are ripe. Neither the gonophores of *Allopora* and *Distichopora* nor the medusæ of *Millepora murrayi* show any traces in development of being degenerate structures like the adelocodonic gonophores of the other *Hydromedusæ*.—The development of the oviduct in the frog, by E. W. MacBride. The previous work on this subject was sketched; the only important paper being one by Hoffmann in the *Zeitschrift für wissenschaftliche Zoologie*, 1886. The development of the abdominal funnel of the oviduct was then described: this arises as a groove in the peritoneum, ventral to the only remaining nephrostome of the pronephros, this latter being the persistent first and not the third of the tadpole's head-kidney, as stated by Hoffmann. This groove is subsequently carried round the root of the lung to the ventral surface, and this extension persists in the adult, though the length of the orifice is increased. It does not atrophy as suggested by Marshall. The main body of the duct grows back as a solid rod of cells in close connection with and apparently derived from a strip of columnar peritoneal epithelium on the outer border of the kidney. Hoffmann is incorrect in stating that the most anterior part of this rod is split off from the Wolffian duct. At the time when this solid rudiment first appears the Wolffian duct in front of the persistent part of the mesonephros has completely disappeared. The general conclusion reached is the complete independence of the oviduct, in its development, from the Wolffian duct.

PARIS.

Academy of Sciences, February 16.—M. Duchartre in the chair.—On the objections raised to the interpretation of Herr Wiener's experiments, by M. A. Cornu. Further evidence is adduced in support of the conclusion drawn from some experiments made by Herr Wiener (*Comptes rendus*, January 26 and February 9), viz. that in a beam of polarized light the vibrations take place in a direction perpendicular to the plane of vibration, as is indicated by Fresnel's theory.—History of the Ibañez-Brunner apparatus, by M. Rod. Wolf. It is shown that the idea of utilizing optical, instead of real contacts, for the determination of a base line in geodetic observations was acted upon by Tralles and Hassler in 1797.—On solar statistics for 1890, by the same author. (See Our Astronomical Column.)—The Mont Dol elephants, by M. Sirodot. The author describes Quaternary strata exceedingly rich in the *débris* of elephants, he having found as many as 758 teeth within an area of 1400 square metres. Those of *Elephas primigenius* predominate, but with such variations that a great number of the specimens would have been classed as *Elephas antiquus*, or as *Elephas indicus* if they had not been found isolated in particular strata.—Observations of the asteroid discovered by M. Charlois on February 11, made at Paris Observatory, by Mdlle. D. Klumpke. Observations for position were made on February 13 and 14.—On a method for measuring the atmospheric dispersion of light of different wave-lengths, by M. Prosper Henry. (See Our Astronomical Column.)—On the resistance of various gases to the movement of a pendulum, by Commandant Defforges. It has been previously shown that

$$\frac{\Delta T}{T} = Pd + R\sqrt{d},$$

where T = time of oscillation of a pendulum, P = the hydrostatic impulse during motion, d = the density of the surrounding air, and R = its resistance. Six series of experiments in carbon dioxide, three in oxygen, and three in hydrogen, show that the coefficients P and R are the same with the same pendulum for all three gases and also for air. They depend on the form of the pendulum, but not on the nature of the surrounding gas.—Remarks relative to M. Poincaré's note on Herr Wiener's experiment, by M. A. Potier.—Variability of the number of vibrations of musical notes according to their functions, by M. Mültzer.—On the conductivity of organic acids and their salts, by M. Ostwald.—Reply to M. Ostwald's preceding note, by M. Daniel Berthelot.—On some compounds of pyridine, by M. Raoul Varet.—On amide of sodium, and on a chloride of disodammonium, by M. Joannis.—Researches on *l'huile pour rouge*, by M. Scheurer-Kestner. This compound has been previously described. Some of its combinations are now given.—On the action of excessive cold on animals, by M. G. Colin. The rabbit appears to be able to live through considerable cold. Adult specimens have lived in ordinary hutches suspended from a branch of a tree or standing on a heap of snow, and their temperature has only been lowered about 1° in five or six days, when the outside temperature varied from − 10° to − 15° C. Other specimens have lived in perfect health for two months in cubical hutches completely open on one side, when the temperature ranged from −10° to −25°. Sheep and pigs are also able to live through severe weather, but the dog and horse are killed by it.—Observations on the development of some Ascidiæ, by M. A. Pizon.

AMSTERDAM.

Royal Academy of Sciences, January 31.—Prof. van der Waals in the chair.—Dr. Hoogewerff and Dr. van Dorp dealt with the reaction of hypochlorites and hypobromites on phtalimide and phtalamide. When molecular quantities of phtalimide and hypobromite or a hypochlorite are brought together in an alkaline solution, anthranilic acid is obtained in a quantity nearly approaching that given by theory. Under the same circumstances Griess's benzoylencerea is formed from the amide.—Mr. H. A. Lorentz discussed the application of Maxwell's principles to electrical phenomena in moved bodies, the ether being supposed to remain at rest. By the aid of certain assumptions the author establishes the equations of motion for a system of electrified particles. The results may be applied to all phenomena which it is permitted to explain by the hypothesis of such particles. It is found that, by imparting to a dielectric a velocity v in the same direction in which it is traversed by light-waves, the velocity of these latter relatively to the ether is increased by $\left(1 - \frac{1}{n^2}\right)v$, n being the index of refraction. The co-efficient $1 - \frac{1}{n^2}$ is the same which was introduced by Fresnel into the theory of aberration.

STOCKHOLM.

Royal Academy of Sciences, February 11.—On the execution of the measuring of meridian degrees on Spitzbergen, by Prof. Rosén.—A new work, "Biologische Untersuchungen," Neue Folge, I., by Prof. G. Retzius, exhibited by himself.—An examination of the new tables of definite integrals of Bieren de Haan, by Dr. C. F. Lindman.—Anatomical studies of the Scandinavian Cestodea, by Dr. E. Lönnberg.—On the structure of *Ogmogaster plicata*, Creplin, by Hr. Jägerskiöld.—Bromeliaceæ Herbarii Regnelliani, described by Dr. C. A. M. Lindman.—Determination of the maximum elasticity of the vapour of water at low temperatures, by Dr. J. Juhlin.—Comparison between the methods of Ångström and of Neuman for determination of the caloric conductivity of different bodies; i. theoretical foundation, by Dr. Hagström.—Notes on the superficial strata of Scania, by Prof. B. Lundgren.—On *Anthella Wrightii*, n.gen. et n.sp., a singular Actinia, by Hr. O. Carlgren.

CONTENTS.

THURSDAY, MARCH 5, 1891.

AN ANTI-DARWINIAN CONTRIBUTION.

The Darwinian Theory of the Origin of Species. By Francis P. Pascoe, F.L.S., &c., ex-President of the Entomological Society. (London: Gurney and Jackson, 1890.)

TO avoid any misconception to which the somewhat ambiguous title of this work may give rise, let it be at once stated that this is not an exposition of the Darwinian theory; neither is it a contribution to that theory in the constructive sense, nor does it attempt to deal with the subject in any other than a destructive sense. The author has, in fact, played the same part in the present little book of some 113 pages that he played some years ago in a similar production which is now out of print, and which bore the title "Notes on Natural Selection and the Origin of Species." Mr. Pascoe has hardly placed himself in the position of "counsel for the opposition," inasmuch as he can scarcely be said to plead anywhere against the Darwinian theory; he acts rather as solicitor for the opposition, and by virtue of his special knowledge of certain groups of insects and his general knowledge of other groups of animals, he has been enabled to collate a large number of difficulties and objections which have occurred to himself and to other naturalists. It is hardly necessary to say that many of the most weighty objections raised by the author have been culled from the writings of Darwin himself. The result has been the production of a brief which is valuable as coming from a systematist of recognized authority, and which the counsel for the opposition may and no doubt will make use of.

In so far as the author quotes from other writers, it is difficult to ascertain how far he has himself grasped the Darwinian principles. Where he does appear in his own personality, he leaves some doubt as to whether he really does understand these principles. For example, there is a passage fronting the title-page which is not given as a quotation, and which we may therefore suppose to be the author's own, and which commences with the statement:—

"Natural selection is assumed to depend on a power in every organism—past and present—intently watching every variation, rigidly destroying any in the least degree injurious, and picking out with unerring skill all that in the future, by gradual accumulations, give a better chance in the struggle for life."

It is difficult to see how natural selection can depend upon a power in an organism either present or past; in fact, selection as understood by most Darwinians is an operation from without, *i.e.* external to the organism; and if the author means to imply that the selection is due to an internal agency, then he has failed to understand the spirit of Darwinism. But the whole passage is so ambiguous that the most favourable view that can be taken of it is that when Mr. Pascoe's counsel comes to consider his brief he may be told that the "power in the organism" is used as a metaphorical expression only.

In a few pages of introduction we have a series of quotations, or rather fragments of quotations, from Herbert Spencer, Agassiz, Cope, Wallace, Huxley, Flower, Mivart, and Tyndall. These are strung together without any connecting argument, but are presented in a form which is, to say the least, unfair to the authors in question. They are the odds and ends of various passages which would in most cases bear a quite different construction from that which the author intends his readers to put upon them. He quotes from Darwin to the effect that "with all beings there must be much fortuitous destruction, which can have little or no influence on the course of natural selection." Then he adds:—

"It cannot be contended, for instance, that out of the ten million ova of a codfish the few that attain maturity owe their escape to something not possessed by those that perish."

Certainly not; all that Darwinians would contend for is that out of the millions of codfish whose ova do reach maturity, there would be quite a sufficient amount of material for natural selection to work upon. For this agency can obviously deal only with the organism which is already in existence, and if by fortuitous destruction a very large proportion of the ova are destroyed, that percentage is *hors de combat* so far as natural selection is concerned. Moreover, the production of large numbers of ova shows that natural selection has been at work in the direction of producing an increased fertility. This "objection" does not, therefore, appear to be of a very serious character.

After the introduction the real business of the book begins with a brief historical sketch. The statement of the Darwinian theory is composed chiefly of quotations, and in so far as these are Darwin's own words, they are fairly presented. But when the author offers his own summing up, we again meet with that indistinctness of expression which is one of his most unsatisfactory characteristics:—

"Natural selection, then, is assumed to be a power acting by gradually accumulating small modifications, which, in the future, when increased by inherited modifications, is to the 'advantage of each being.'"

Mr. Pascoe's counsel must be left to put this into form. Sexual selection is, of course, not received by the author, but his objections to this part of the theory are very meagre, and he does not refer to any of the recent contributions to the subject, such, *e.g.*, as the masterly series of observations on spiders by George and Elizabeth Peckham. It can hardly be supposed that a writer possessed of such an extensive knowledge of the literature of the Arthropoda, should not be aware of the existence of this work. In connection with the subject of use and disuse, there is a discussion of the question of the wingless beetles of Madeira, which is of importance, because it deals with a group of insects upon which the author is entitled to pronounce an opinion as an expert. Darwin's explanation of this fact is familiar to all students of the "Origin of Species." The "objections" brought against this explanation are thus stated:—

"Mr. Wallace says that the same species, wingless in Madeira, are winged on the Continent. It may be so. Some of our Hemiptera—*Nabis*, *Pithanus*, *Pyrrhocoris*, &c.—ordinarily wingless, are sometimes found in hot summers to have well-developed wings. But the beetles of Madeira belong, without exception, to European forms, and in Europe few species are habitual fliers, while wingless, or nearly wingless, species are found far inland, in all parts of the world, belonging principally to the same families as those in Madeira (Curculionidæ, Tenebrionidæ, Carabidæ, Lamiidæ, &c.). It is difficult always to determine, without injury to the insect, what may be a wingless species, but in these four families alone it is safe to say that there are a few thousands. In many cases the elytra are soldered together, and have even grown to the sides of the thorax. In some Lamiidæ allied to *Dorcadion*, and probably in some others, there are vestiges of wings. But how is the union of the elytra to be explained? . . . There is a tendency to be wingless in all the insect orders, either in one or both sexes. Neuters, unlike males or females, are always wingless. Barren females are only neuters from want of food."

That is to say, that because winglessness occurs in other insects through other causes than those which obtain in Madeira, therefore this is an "objection" to the wind-selection theory of Darwin. Since Mr. Pascoe quotes Huxley to the effect that "in science scepticism is a duty," I may be permitted to express my scepticism as to whether the author has really grasped the Darwinian explanation. I must confess that to me the reasoning appears to be of the nature of a *non sequitur*, but I have given the extract in full in order that readers may form their own opinion. It is typical of the whole spirit of the "objections" which the author has marshalled against the Darwinians. In the first place, the above statement does not fairly represent Mr. Wallace's views. He says only that *some of* the wingless Madeiran species are winged on the Continent. "In other cases the wingless Madeira species are distinct from, but closely allied to, winged species of Europe" ("Darwinism," p. 105). But the fact that Darwin sought to explain is not that there occur wingless beetles in Madeira, but that such a large number (over 34 per cent.) out of the whole Coleopterous population should be thus deprived of the means of flight. Moreover, the confirmation which this explanation receives from the insects of Kerguelen, which belong to three different orders, and which are *all* wingless, is not alluded to as it should have been in fairness to the argument. Even the "union of the elytra" may disappear as an objection on further consideration, for if, through wind-selection, *or any other cause*, flight became unnecessary or dangerous, it is quite conceivable that the possession of loose elytra would become a source of positive danger to beetles which bury or live among loose stones and the crevices of bark, and natural selection would in such cases bring about the character in question on account of the superior protection from bodily injury which would be thus furnished.

The next writers whom the author puts into his anti-Darwinian witness-box are Dr. Romanes and Prof. Mivart, the latter of whom he considers to be the "most damaging" opponent. Then we have a whole array of Darwin's own difficulties, which are familiar to all who have read the later editions of the "Origin of Species." In his anxiety to pin Darwinians down to the literal statements of their master, Mr. Pascoe has not done justice to, or has wilfully ignored, the later developments of the theory. It is true that Darwin admitted that, unless "many individuals were similarly and simultaneously modified 'rarely single variations could be perpetuated.' . . . Variation, however useful, if confined to one individual, would be 'generally lost by subsequent intercrossing with ordinary individuals.'"

But ideas on this head have become broadened to an extent that the author does not seem to be aware of. The whole number of individuals composing any species may at any period of its existence be divided into two portions presenting variations above and below a line of mean variability. Of these portions one must possess characters more or less advantageous so far as concerns the external conditions, and the other portion must possess characters more or less disadvantageous with respect to those same conditions. On the first portion—which may be considered above the mean line—natural selection will act as a preserving agent; on the second portion—below the mean line—natural selection will act as a destructive agent. That is to say, that the whole number of individuals surviving in the struggle for life is supplied from the portion *above* the mean line of variability. The proportion of individuals of this upper portion which survive will depend upon two factors, the intensity of the action of selection for the time being, and upon the range of variability of the species, *i.e.* the height of the extreme variations above the mean line. Mr. Pascoe occasionally gives extracts from Wallace's "Darwinism," but he cannot have properly digested the third chapter of that work, although he quotes from it, if he still considers the isolated fragments of Darwin's writings which he gives above in the light of "objections." It is obvious that many individuals *are* similarly and simultaneously modified, and since the action of selection in the conservative sense is exerted upon individuals above the mean, while the loss by destruction falls upon those which are below the mean, it follows that in the next generation the line of mean variability will be raised, *i.e.* the species will have come into closer harmony with the external conditions, and so on in successive generations till equilibrium is reached.

Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—

"On the converting 'an ordinary hoofed quadruped' into a giraffe, Mr. Romanes observes: 'Thousands and thousands of changes will be necessary.' . . . 'The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters.' The chances of such changes are 'infinity to one' against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident."

I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it. But surely it is not essential to the Darwinian explanation of the form of the giraffe that there should have been any "tapering down" at all. The particular advantage which the form of the animal confers upon it is its *height;* the neck has been elongated and the fore-quarters raised for this purpose, and, as far as I know, neither Darwin nor any of his adherents have asked us to believe that the hind-quarters have been lowered. The other difficulty presented in the above passage is one which is very frequently urged, viz. the chances against concurrent favourable variations in complex and co-ordinated structures. To many impartial critics, however, this class of difficulty will not appear so formidable when it is remembered that the complex co-ordinations which are now witnessed are the final results of long series of modifications superimposed from generation to generation through long periods of time. We are not called upon to believe that co-ordinated characters were developed at once by the occurrence of a sufficient number of individual variations having the necessary co-ordination of distinct variations. Mere elongation of neck would be an advantage to an animal occasionally driven to browse upon the foliage of trees. The most rabid anti-Darwinian will not deny that animals do vary in this character. But if an elongated neck is an advantage, natural selection would seize upon it and perpetuate it in the species presenting the necessary variations in this direction. Then, superimposed upon this character would be other modifications, say strength of neck-muscle, which would give its possessors an advantage in reaching up to their food. Natural selection would, in such a case, be acting upon two distinct characters—length of neck and strength of muscle—which are now co-ordinated, inasmuch as they have both been developed for a common purpose. But it is not necessary to suppose that the variations in these two characters have always been "concomitant"; there is no reason for believing that the individuals with the longest necks were necessarily those with the strongest muscles. All that is claimed is, that among the individuals with the longest necks there would be variability in the strength of the neck-muscles, and that muscular strength has thus been superadded little by little to length of neck through successive generations, until the co-ordination which we now observe has been reached. Whole classes of difficulties in the way of accepting incipient "concomitant variations," which occur throughout the present work and others of a like character, will, I venture to think, disappear, or at any rate become much diminished, if looked at from this point of view.

About one-half of the whole book is devoted to objections of this and other kinds, but the reader, whether Darwinian or not, will be struck by the total want of method with which the facts are marshalled. This is certainly a surprising defect in a writer of such acknowledged skill as a systematist. Large numbers of the paragraphs are bare statements of facts, and their bearing upon the questions at issue is often so obscure that it is impossible to decide whether the author is writing in favour of Darwinism or against it. The first indication of methodical treatment appears about the middle of the book, where begins a short synopsis of the Invertebrata, which occupies from thirty to forty pages, and of which the object is thus explained:—

"The invertebrate classes are so little noticed by writers on the origin of species, that a short sketch of some of their characters and peculiarities may be useful to show their classification and affinities, and to give some idea of their often extremely exceptional forms and modes of reproduction. It will bring into special notice the statements already given in general, and illustrate still further the difficulties of the theory."

This synopsis is certainly useful as far as it goes, but it contains only the ordinary zoological information (very much compressed) to be found in any modern text-book, and therefore might well have been dispensed with in a special work like the present where compactness appears to have been aimed at. If the author had only devoted the same amount of space to the detailed discussion of some special classes of difficulties he would have done his own cause more service. To say that "the invertebrate classes are so little noticed by writers on the origin of species" is a distinct injustice to such writers as Wallace, Bates, Fritz Müller, Haeckel, Lubbock, Weismann, Ray Lankester, and Poulton, all of whom have based their contributions to the theory in question, either partially or wholly, on work done in connection with the invertebrates.

After the summary of the characters of the Invertebrata, Mr. Pascoe introduces the subject of colour as a means of protection. He objects (apparently) to natural selection as the effective agency in this case, quoting Mr. Butler to the effect "that no insect in any shape was ever refused by all birds; what one bird refused another would eat." This objection is based on a misapprehension; no Darwinian has ever asserted that protective colouring is *absolute*, and that the concealment is so perfect that the species thus protected are altogether free from persecution. The utmost that is claimed is a relative advantage. In a similar way Bates's theory of mimicry is lightly disposed of because "it does not appear that butterflies are anywhere the food of birds." There is, however, much evidence to the contrary, and the theory of mimicry, moreover, does not require us to believe that birds are the only foes of butterflies. "Another butterfly (*Hypolimnas bolina*) apparently not mimetic," is a quite erroneous statement, as the female presents one of the most remarkable instances of mimicry known. The leaf-butterfly (*Kallima*) is described as "one of the most perfect cases of mimicry"; but this is rather a case of protective resemblance (Poulton's "procryptic" group). To say of mimicry that, "after all, the majority of such resemblances are superficial," is no objection, but would be conceded by the most advanced advocate of Darwinism. The author records a very interesting case of a Queensland beetle (*Saragus*) which is covered with a waxy secretion which is identical in appearance with a fungus (*Isaria*). The bent of the writer's mind will be gathered from the statement that the fact that the beetle is found on the trees on which the fungus grows is regarded as "the oddest thing" (!). Then he shuts his eyes to the only reasonable explanation, because the beetle belongs to a family which have a hard exoskeleton, and "none of which are likely to be touched by birds." The reasoning is inconclusive, because hardness of covering is not always a protection, and birds are not the only beetle destroyers. With respect to this whole question of colour adaptation, Mr. Pascoe makes a most astonishing statement: "My experience leads me to the conclusion that, as a rule, animals think very little of concealment." If this is his experience, it is certainly at variance with that of every naturalist who has observed animals among their natural surroundings.

The concluding portions deal with Weismann's theory of heredity, instincts, geographical distribution, and other miscellaneous subjects, which are all treated of in the fragmentary way that prevails throughout. But in spite of any hostile criticism which the work may call forth, it must be recognized that the author has a distinct claim to make himself heard. He is an authority on certain groups of Coleoptera, and is a veteran among systematists who had won his spurs before his present critic knew a beetle from a moth. To attempt to answer all the objections and difficulties which he has accumulated would necessitate a volume. With respect to these objections, some are old, and have already been answered; others are trivial, and require no answer; others answer themselves; others are strained; others arise from misapprehension, as I have endeavoured to show; while others remain as real difficulties, for the solution of which we may have to wait patiently for years. It must not be forgotten, however, that difficulties are not necessarily disproofs. Astronomers tell us that there are many difficulties connected with the motions of the heavenly bodies, but I am not aware that these difficulties have shaken their faith in the theory of gravitation. The man of science accepts that theory, although he might find it impossible to explain why a particular pebble found its way to the top of a particular hill.

In justice to Mr. Pascoe, from whom I have long ago "agreed to differ" on these questions, it must be pointed out that he is, or at least appears to be, an evolutionist. He says so more than once in this work:—"The objections now are confined to natural selection. No objection can be advanced against the theory of descent. A separate creation for each species would admit of no blood-relationship" (p. 107). "We are grateful to Mr. Darwin for having freed science from the bonds of the old theology" (p. 111). It is with the Darwinian form of evolution that he wages war, and if I have availed myself of the opportunity afforded by the Editor of NATURE for discussing the contents of this volume at greater length than its bulk might appear to demand, it is because the author is the representative of a large class of systematic entomologists in this country, who hold similar views, and who, by their constant study of and search for minute characters upon which to found "species," have become biased in judgment with respect to the broader problems of modern biology. Writers of this school are in the habit of forcing difficulties upon natural selection which that theory has never professed to deal with. The Darwinian doctrine will not collapse under this last attack, and however much we may differ from the author, it cannot be denied that, as a stimulus to further research, such compilations as that which Mr. Pascoe has produced are distinctly useful.

R. MELDOLA.

THE HISTOLOGY AND PHYSIOLOGY OF GRANITE.

Contributions to a Knowledge of the Granites of Leinster. By W. J. Sollas, LL.D., D.Sc., F.R.S., Professor of Geology and Mineralogy, University of Dublin. Transactions of the Royal Irish Academy, Vol. XXIX., Part XIV., January 1891. (Dublin.)

THE convenient system of the Royal Irish Academy, by which each paper in their Transactions constitutes a separate part, makes this quarto memoir of nearly a hundred pages virtually an independent work. As such it should be possessed by all engaged in serious petrographic research; while to workers in other branches of geology, it will give a clear idea of the patient methods and precise observations by which the history of rock-masses is gradually being brought to light. The merchant and the *connoisseur* of ornamental stones will find little in these pages concerning the broad features of granite, the joints that so often limit the exposed masses, the modes of disintegration, or even the life-history of the Leinster mountain-chain; while the lover of scenery will miss with regret the handsome plates, connecting surface features with geological structure, which adorned the Transactions of Societies at the commencement of the present century. But we would refer such readers to the limitations and restraint shown in the title of the present paper. It is the work of one who appreciates the labours of his predecessors; yet, despite the bibliography which Prof. Sollas has here drawn up, we soon become aware that our knowledge of the familiar rock, granite, is greatly in need of these "contributions." The detailed chemical and numerical observations of Dr. S. Haughton, so freely quoted from by geological writers during the last thirty years, have clearly borne good fruit in Dublin on their natural soil.

The minerals of the Leinster granite are considered in this paper individually. In obtaining the specific gravity of isolated grains, the author ingeniously employs a "diffusion-column," composed of heavy liquid, the density of which increases regularly from above downwards (see NATURE, February 26, p. 404). It is found that a column prepared by pouring a less dense upon a heavier solution acquires by diffusion the necessary regularity after standing for some six or seven hours. Minerals of known density are introduced as index-marks, and the density of any layer of the liquid between those at which they float can be read off against a graduated mirror. The convenience of this method, as compared with the ordinary plan of uniformly diluting a sample of liquid for the examination of each separate mineral, will be apparent to all who have worked at the determination of rock-constituents; the purity, moreover, of a group of grains, which have been isolated by the ordinary method, may be ensured by transferring them to the diffusion-column, when some may be found to float above and some below the horizon of the pure material.

We notice that Prof. Sollas uses in his investigations the solution of iodides of mercury and potassium: without being contentious over matters of priority, we must protest against his styling this the Thoulet solution, since its inventor, Mr. Sonstadt, was the first, not only to employ it for the determination of specific gravities, but to suggest its utility in processes of isolation.

The author, when dealing with the biotite of the Aughrim granite, discusses in some detail the constitution of the micas, and graphically represents their molecules in the form of sexradiate rings. Now that the forms of molecules are playing a prominent part in crystallographic considerations, chemists will be attracted by these symmetrical diagrams, which are, of course, mainly speculative. Prof. Sollas himself regards them merely as suggestive; but we can imagine some fascinated student, gazing on graphic formulæ which so skilfully adapt themselves to the reactions of the mineral, and feeling himself at last in touch with the affinities of the impenetrable atom. Let such consider and beware.

The description given of the contact-phenomena (p. 480) has at present an especial interest, and the author freely uses the term "schist" for the distinctly foliated rocks which are common along the flanks of the great chain, although such rocks are undoubtedly of Ordovician age. This is mere justice to the structures developed, and an encouragement to petrologists in the field, who are apt to resent a terminology based in any way upon geological age. While Prof. Sollas points out how the foliation in this case preceded the intrusion of the granite, we may take it that the same earth-movements produced both the one and the other at no long interval, the foliation being a prelude to the final yielding of the uptilted rocks.

The phenomena of foliation in the schists, and of flow-structure in the granite, may be studied by any visitor to Dublin upon the airy summits above Killiney Bay; but in some spots the granite itself has assumed a gneissic character through pressure subsequent to consolidation. On this point pp. 496–502 of the present memoir may be commended to workers in metamorphic areas; especially noteworthy is the insistance that the formation of cracks at right angles to the direction of flow, so often seen throughout the elongated constituents of a rock deformed by pressure, will serve under the microscope to distinguish cases of secondary from those of primary and igneous flow.

Among the interesting series of conclusions, we may remark how the soda-lime felspars predominate over the potash varieties in the Leinster granite. This only confirms what microscopic examination has taught us of a host of "granites"; so that we may have to fall back upon the comfortable definition of the rock as consisting of "quartz, felspar, and mica," unless we are willing to hand over many old acquaintances to the increasing group of the quartz-diorites.

Although the author states that we have no trace of volcanic ejectamenta emanating from the Leinster granite, we should be inclined to connect with it the great series of Ordovician "felsites," and associated tuffs stretching along its flanks from Wicklow southwards. Volcanoes characteristically break out upon the margins of an uplifted area, not upon its crest, where the eruptive energy may be presumed to become less and less; and highly silicated lavas, ancient obsidians and pumiceous tuffs, abound among the Ordovician rocks of Leinster, probably as the direct precursors of the granite.

Contributions such as these furnished by Prof. Sollas will be estimated at their full merit by those familiar with the long processes of isolation and analysis. The work of careful weeks may occasionally come before us in a page; and the results may appeal more immediately to the philosophic chemist and the crystallographer. But from such a memoir, as from that recently presented to the Geological Society by Messrs. Marr and Harker, we may learn what work lies before us even on familiar granite fells; and we may turn with renewed zest from the streets of Dublin to its highlands, to the moorland white with hoar-frost, and the broad glens reaching to the sea. G. C.

THE FLORA OF WARWICKSHIRE.

The Flora of Warwickshire. "The Flowering Plants, Ferns, Mosses, and Lichens," by James E. Bagnall, Associate of the Linnean Society." "The Fungi," by W. B. Grove, M.A., and J. E. Bagnall. 518 + 34 pages, with a Map. (London: Gurney and Jackson. Birmingham: Cornish Brothers, 1890.)

THE interest to outsiders in the plants of Warwickshire lies in the fact that we have here a typical English Midland county, the botany of which is not in any way affected by proximity to the sea or high mountains. Although it is the central county of England, and forms the watershed of the Severn, Trent, and Thames, no portion of its surface rises above 855 feet. Its area is 885 square miles, or 566,458 acres, and it contains 4 hundreds, 2 cities, 1 county town, 10 market towns, and 209 parishes. In 1888 the crops of corn, beans, and peas occupied 106,000 acres; green crops, 38,602 acres; permanent pasture land, 312,000 acres; fallow, 8161 acres; and woodland, 16,650 acres. The soils are fertile, but varied, comprising nearly all kinds but those containing chalk and flints. All the southern and south-eastern part of the county is occupied by a strong clay resting on limestone. A soil of similar kind occupies the north-west. Over a large portion of the county, extending from Warwick to its western boundary, are strong clay loams resting on marl and limestone. Westward and southwestward of Warwick there is a strong clay over limestone. About Rugby and in the valleys of the Blythe and Tame are light sandy soils mixed with gravel. The remaining extensive portions of the county consist of a red sandy loam and a red clay loam, resting on freestone or limestone, and sometimes on gravel. The extent of uninclosed land is very inconsiderable, the only extensive commons being those of Sutton Coldfield and Yaningale. The subjacent sedimentary rocks begin with the Cambrian and end with the Inferior Oolite, with a little volcanic tuff with intrusions of diabase and quartz porphyry in the north-east, near Atherstone.

The author of this book, Mr. James Bagnall, is one of the most meritorious and best-known of our working-men naturalists. He lives in Birmingham, and has devoted himself specially for the last twenty years to the study of the botany of his native county, and in the present work the result of his long and diligent labours is carefully summarized. He has taken rank as one of our best critical British botanists, and has been selected by the Linnean Society as one of its fifty Associates, and has been awarded the Darwin Medal given by the Midland Union of Natural History Societies for the encouragement of original research. His fellow-townsmen are justly proud of him, and when the present work was planned, a number of local gentlemen, with Mr. Joseph Chamberlain at their head, undertook to guarantee him against pecuniary loss. There was, however, no need to call upon them, as 430 out of the 500 copies printed were subscribed for before the book was issued.

The work consists of an introduction of thirty-one pages, which contains the needful explanations of the plan followed in the enumeration of plants and their distribution, together with a sketch of the physical geography, meteorology, and geology of the county, the latter contributed by Mr. A. Bernard Badger. The great body of the work—328 pages—is taken up by the enumeration of the flowering plants and vascular Cryptogamia that grow in the county, and an account of their distribution and the special stations of the rarities. The county is divided into ten districts founded on the river drainage, and each of these has been worked separately. The last edition of the "London Catalogue" has been followed as a standard of nomenclature and species limitation. The county is specially rich in Rubi, and in studying them Mr. Bagnall had the advantage in starting of the oversight in the field of the Rev. A. Bloxam, who was one of the best practical authorities in this difficult genus that we have had in this country. The flowering plants of the county have been worked so thoroughly that it is not likely that any material additions will be made. Then follows the enumeration of the mosses, of which 236 species are known in the county. The Hepaticæ and lichens have not been worked so carefully, and in these orders there is ample scope for further research. The enumeration of the fungi is confined to the Hymenomycetes and Gasteromycetes. The enumeration of the lower Cryptogamia occupies 130 pages. Then follows a table showing the distribution of the plants through the ten drainage districts of the neighbouring counties of Leicester, Northampton, and Oxford. The book concludes with a sketch of the progress of botanical investigation in the county; the principal botanists who have worked within its limits being Withering, Stokes, Perry, Purton, Bree, and Bloxam.

The flowering plants and vascular Cryptogamia of the county summarize as follows:—Out of 532 plants generally diffused throughout Britain, Warwickshire has 501. Out of 409 species concentrated towards the south of Britain, Warwickshire has 285. Out of 127 plants concentrated in the eastern counties, Warwickshire has 31. Out of 70 plants concentrated in the western counties, Warwickshire has only 8. Out of 37 plants concentrated in the centre of Britain, Warwickshire gets 7. Out of 208 plants which represent the boreal element in the British flora, Warwickshire has only 19.

The book is not too large to be conveniently carried, which is a great advantage in a county flora. From every point of view it is thoroughly satisfactory, and will be a lasting memorial of the ability and diligence of its author.

J. G. Baker.

OUR BOOK SHELF.

A Hand-book and Atlas of Astronomy. By W. Peck, F.R.S.E., F.R.A.S. (London and Edinburgh: Gall and Inglis, 1890.)

As Astronomer and Public Lecturer to the City of Edinburgh, the author of this work might reasonably be expected to be familiar with the requirements of a popular hand-book of astronomy. His aim, however, has not been to give a mere outline of the subject, but to give "complete and accurate" information in the principal departments of modern astronomy. In this endeavour he has compiled the volume before us, consisting of 170 pages, and embellished with 20 large plates and numerous smaller diagrams. For the ordinary reader who does not possess even a small telescope, the book has not much to recommend it. The descriptions are often very meagre, and the spectroscopic work which is now engrossing the attention of so many astronomers is scarcely touched upon. The star maps and the tables which accompany them are excellent, but it is questionable whether they would not have been more convenient if issued separately, instead of forming part of a rather bulky volume. Yet, if these were taken away, there could be little excuse for the existence of the remainder. That is to say, there would be little left that is not already available in much cheaper forms. The author has fallen into the common error of attempting to combine a popular work, suited to the general reader, with one more especially adapted for those wishing to acquire a comprehensive knowledge of the subject. From either point of view, the deficiency of spectroscopic astronomy is very conspicuous.

Some of the large diagrams are really excellent, but others are very indifferent. Amongst the latter the most striking are Plate 9, illustrating solar phenomena, and Plate 19, depicting various spectra. In the latter the colours are unsatisfactory, and the spectrum of hydrogen is represented as consisting of two bright lines and numerous dark ones. It would have been a great improvement if, instead of the drawings of some of the brightest nebulæ, photographs had been reproduced. The reproductions of photographs of the moon, taken by the author, are excellent.

Biographisch-litterarisches Handwörterbuch der wissenschaftlich bedeutenden Chemiker. By Carl Schaedler. (Berlin: R. Friedlaender und Sohn, 1891.)

Biographical notes of some hundreds of chemists and physicists are here collected together, the names being arranged in alphabetical order. In the majority of cases there are given the date and place of birth, and, in cases where it has occurred, of death, besides the offices held, the most important of the work done, and the books, &c., written by the individual. The period covered extends from before the Christian era, and among the most recent names may be found those of Thomas Carnelley and Sydney Gilchrist Thomas. The collection cannot fail to be useful and interesting, but its value to historians would have been greatly enhanced if references had been given to the authorities from which the statements are derived. To do this would have probably added but little to the trouble of compilation, and would have made the volume a standard work of reference.

Round Games with Cards. By Baxter-Wray. (London: George Bell and Sons, 1891.)

In this little treatise, Mr. Baxter-Wray deals with all the most popular round games with cards. Among them may be mentioned nap, loo, poker, vingt-un, commerce, pope-joan, spin, together with eight others which are played at the present day. With regard to each game the reader receives sound advice as to the methods he should adopt, and those he should not. The variations of each game are well described, but mention might have been made—in the variation of nap called "misery, or misère"—of playing with all the hands on the table face upwards, which affords, when more than three are playing, an excellent game requiring much skill and tact.

Many suggestions and rules are given pertaining to the stakes, deals, &c.; and those who read the book will find in it all that will enable them to learn a new game.

Elementary Science Lessons: Standard II. By W. Hewitt. (London: Longmans, Green, and Co., 1891.)

This book is intended to be in the hands of teachers, who, by making a judicious use thereof, should be able to engraft much in the minds of young people in a sound and practical manner. The principle on which it is written is excellent. The work is drawn up on the same lines as the first series, only it is of a slightly more advanced character. The idea throughout is to place objects before the children, by means of which they may be able to recognize the general properties relating to them. Thus, in the first few lessons certain substances are exhibited, from which the general idea of solids, liquids, and gases can be gathered. The general characters of iron and steel, and those of a variety of other metals, are then illustrated, the metallic surfaces of which suggest the principles of the reflection of light, which are consequently treated of. The remaining lessons deal with sunlight, colour, motion, and the forces that produce it. The appliances for the experiments are of the most simple kind, and there are notes for the use of the teacher, from which the necessary information can be gathered.

LETTERS TO THE EDITOR.

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

Darwin on the Unity of the Human Race.

HAVING had occasion last year to allude as a fact to the circumstance that Charles Darwin assumed mankind to have arisen at one place, and therefore in a single pair, I was surprised to find that this fact was doubted, or at least very doubtfully accepted, by some of my scientific friends; and I was asked for a reference to his works in confirmation of it. My principal reliance, however, was in the recollection of a private letter to myself from the illustrious naturalist, which I had unfortunately mislaid. Having now recovered this letter, I send a copy of it to NATURE for publication, simply explaining that this letter was in reply to a letter from me in which I put the direct question, why it was that he did assume the unity of mankind as descended from a single pair? It will be observed that in his reply he does not repudiate this interpretation of his theory, but simply proceeds to explain and to defend the doctrine.

ARGYLL.

"*Down, Beckenham, September* 23, 1878.

"DEAR DUKE OF ARGYLL,—The problem which you state so clearly is a very interesting one, on which I have often speculated. As far as I can judge, the improbability is extreme that the same *well-characterized* species should be produced in two distinct countries, or at two distinct times. It is certain that the same variation may arise in two distinct places, as with albinism or with the nectarine on peach-trees. But the evidence seems to me overwhelming that a well-marked species is the product, not of a single or of a few variations, but of a long series of modifications, each modification resulting chiefly from adaptation to infinitely complex conditions (including the inhabitants of the same country) with more or less inheritance of all the preceding modifications. Moreover, as variability depends more on the nature of the organism than on that of the environment, the variations will tend to differ at each successive stage of descent. Now it seems to me improbable in the highest degree that a species should ever have been exposed in two places to infinitely complex relations of exactly the same nature during a long series of modifications. An illustration will perhaps make what I have said clearer, though it applies only to the less important factors of inheritance and variability, and not to adaptation—viz. the improbability of two men being born in two countries identical in body and mind. If, however, it be assumed that a species at each successive stage of its modification was surrounded in two distinct countries or times by exactly the same assemblage of plants and animals, and by the same physical conditions, then I can see no theoretical difficulty to such a species giving birth to the new form in the two countries. If you will look to the sixth edition of my 'Origin,' at p. 100, you will find a somewhat analogous discussion perhaps more intelligible than this letter. "Yours faithfully,

"CHARLES DARWIN."

Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

THERE are many, no doubt, who will be pleased to have the English translation of some of the papers of Prof. Van der Waals which has recently been published by the Physical Society. There are those at any rate who will be glad to satisfy themselves, without overmuch labour, as to how much there is of real importance in these much-discussed memoirs, published originally in a language too little studied in this country. I do not propose to criticize the papers, though I do not think that either the thermodynamics or the conclusions will bear examination; but I cannot avoid the task, however ungracious, of pointing out that they do not show a proper appreciation of the work of Andrews.

I will make but two or three quotations, and they shall be as brief as possible.

On the first page of the author's preface appears this sentence: "These latter [theoretical considerations] led me to establish the connection between the gaseous and liquid condition, the existence of which, as I afterwards learned, had already been suspected by others." The author's preface concludes as follows: "That my conception has shown itself to be a fruitful one cannot be denied, and it may be the incentive to further inquiry and experimental investigation."

The claim put forward in these sentences appears to me absolutely untenable. This connection, or relation as it might better be called, was not only "suspected" by Andrews, but was clearly and explicitly stated by him in the Bakerian Lecture for 1869; a paper published under the very title which Van der Waals, in 1873, has taken (without a word of acknowledgment) as the title of his essay.

On p. 430 a description is given of the mode of altering the gaseous condition of a substance (carbonic acid) into the liquid condition, and *vice versâ*, by a continuous process devoid of any abrupt change. At the end of the description come the two sentences: "Now, we cannot but call this substance a gas, though formerly we called it a liquid. I have borrowed this remark from Maxwell." The whole description was given by Andrews in the Bakerian Lecture (read June 17, 1869); and was referred to and accentuated by its author in the Royal Society Proceedings abstract of his complete paper.

The curves, Plate v., Fig. 3, are taken, says Prof. Van der Waals (p. 416), from Maxwell (Maxwell's "Theory of Heat" I understand from a reference a few lines higher on the same p. 416). This is to me unintelligible. The curves seem certainly not taken from Maxwell, but are somehow obtained from the original curves of Andrews (after a transformation, which Maxwell also makes, of turning Andrews's curves right for left); and they contain the peculiarity (purposely omitted by Maxwell, for simplicity) of a bend instead of a sharp corner at the bottom of the low temperature curves. In any case Maxwell credits Andrews with the construction of these remarkable curves, which contain, indeed, the germs of the whole *discovery of continuity* made by Andrews and James Thomson. As to the curves themselves, it is utterly unintelligible that anyone with a true perception of their physical meaning should allow the isothermal marked $25^{\circ}\cdot5$ to stand as part of the diagram. The translators ought to have corrected or cancelled this on the ground which led them to object, in the footnote, p. 416, to an ill-judged remark in the text.

Throughout this essay on a subject which, by patient labour and consummate experimental skill, crowned with a rich harvest of results, Andrews made incontestably his own, there is not a single reference to the title or date, or existence even, of the Bakerian Lecture; nor, with the solitary exception of a very questionable reference on p. 421, is there a hint given that Andrews ever gave any attention to the question of continuity; and no uninformed reader would guess from this essay that Andrews had done anything more than supply a quantity of numbers which afterwards turned out to be convenient for the purpose of affording such confirmation as numbers can to the "discoveries" and "laws" of Prof. Van der Waals.

Whatever weight may be given to Van der Waals' investigation, no one who knows the subject as it was known in 1869 can fail to see that neither the idea nor the proof of continuity is in any sense whatever due to him. In their ultimate form they are due to Andrews and James Thomson; though of course it must never be forgotten that the whole subject was opened up by the investigations of Faraday and Cagniard de la Tour; and that Donny, Dufour, and others made important contributions to the subject. I can find no vestige of a novel idea on the subject of continuity in the essay of Van der Waals. If he has succeeded in extending thermodynamic formulas to fit in with Andrews's discoveries, so far the work is praiseworthy and valuable; but an essay "On Continuity" ought certainly to contain a suitable acknowledgment to the author of the discovery.

J. T. Bottomley.

Rainbows on Scum.

I have several times noticed "rainbows" on the black scum upon the pond in a park in this town, and have imagined all of them to have been formed on *dew* deposited upon a film of soot. It is out of the question, however, that yesterday there can have been any *dew* to produce the phenomenon, as there had been a thick hoar frost on the grass, all melted by the warm sun by 10.30 a.m., at which time there was a very vivid double "rainbow," which seemed exactly like an ordinary bow upon rain, except that there were none of the supernumerary arcs due to diffraction, and that the outer bow was fainter than usual in proportion to the inner one. The pond was thinly frozen over, but, it being a cloudless day, the surface of the ice was by that time covered with water. On closely examining the scum, I found it was composed of floating black particles, I presume of soot (the weather being rather foggy), and to many of these, minute drops were adhering, which varied much in size, the largest being probably $\frac{1}{50}$ of an inch in diameter. It was surprising to find distinct drops upon water, but I suppose it must have been the particles of soot that kept them separate. It seems probable they were a portion of the melted hoar frost; but it is rather curious that in such a situation this can produce a rainbow, seeing that usually melted hoar frost does not do so at all, or at most gives a very slight one; so decidedly is this the case, that one may distinguish in the morning between dew and melted hoar frost by noticing whether a "rainbow" and white anthelion are formed; dew being capable of producing bright ones owing to the roundness of the drops composing it, while hoar frost when it melts usually turns into irregular drops. I may say, however, that this "rainbow" on the pond was far more brilliant than any ordinary dew bow, and therefore it would appear that there is some property in the particles of soot to perfect the roundness of drops adhering to them, and so produce a striking phenomenon even from melted hoar frost.

At 0.30 p.m. I noticed the bow was fainter, but still fairly bright, and I estimated there were about 100 drops to each square inch of the surface of the water; it seems this number of drops, averaging probably not more than 0·003 to 0·005 inch in diameter, is sufficient to produce a pretty bright "rainbow." When I placed my eye close to individual drops I found that supernumerary arcs were visible.

T. W. Backhouse.

Sunderland, February 20.

Wild Swine of Palawan and the Philippines.

Dr. A. Nehring has recently (*Abhandl. Mus. Dresden*, 1889, No. 2, p. 14 *et seq.*) characterized the wild pig of Luzon under the title of *Sus celebensis* var. *philippensis*, and the animal found in the island of Palawan (Paragua) as *Sus barbatus* var. *palavensis*. It would seem that both these local races or species have been already characterized and figured by M. Huet from specimens collected by M. Alfred Marche—the first-named as *Sus marchei* (*Le Naturaliste*, 1888, p. 6), and the second as *Sus ahenobarbus* (*op. cit.*, p. 5).

It is interesting to note that the wild pig of Palawan is a representative of the well-marked *Sus barbatus* of Borneo, and not of the wart-faced animal of the Philippines proper.

No specimens of wild pig from the large island of Mindanao appear to have been examined as yet. The following note of the external characteristics of a boar's head brought to me at Zamboanga may therefore be worth being transcribed:—"It was black with a white bar across the snout half-way between the eyes and the nose, with a black spot at the inner corner of the eyes; two tufts of coarse white bristles on the fleshy protuberances on the cheeks on either side, and two singular fleshy black knobs (warts) on the sides of the snout just below the white band."

A. H. Everett.

41 York Terrace, Regent's Park.

A Beautiful Meteor.

On February 25, about 7.30 p.m., I chanced to see, at Coombe, near Woodstock, Oxfordshire, a globular meteor start from the Pointers, and fall with a slightly northerly inclination. When it was near the horizon, my garden wall hid from my sight the close of its brilliant career. Big as Venus at her brightest, it was substantially of a yellow colour, but shot over with flashes of glowing scarlet.

John Hoskyns-Abrahall.

Coombe, near Woodstock, Oxfordshire.

INFECTIOUS DISEASES, THEIR NATURE, CAUSE, AND MODE OF SPREAD.[1]

I.

We read in Homer that "Phœbus Apollo, offended by mortals, sent a pernicious plague into the camp of the Greeks; the wrathful god with his arrows hit first mules, then dogs, and then also the Greeks themselves, and the funeral pyres burned without end." If we expressed this in less poetical language, but more in conformity with our modern realistic notions, we would say, that the deity of health and cleanliness, having been offended by mortals, sent his poisonous but imperceptible darts or bacilli into them, and caused an epidemic of a fatal disease, communicable to man and animals.

In whatever form we meet with this simile—whether an epidemic be ascribed to a wrathful providence, or to a sorcerer or a witch that put their spells on man or on cattle, thereby causing numbers of them to sicken and to die; whether this happened amongst the nations of old, or amongst the modern Zulus, whether amongst the peasants of Spain or in Italy—we now know that it always means that the offended deity of cleanliness, and the outraged laws of health, avenge themselves on mortals by the invasion of armies of imperceptible enemies, which we do not call arrows, nor sorcerers' or witches' spells or incantations, but microbes.

From Homer's Trojan epidemic among the Greeks to the epidemic in the camp of Cambyses, from the plagues carried and spread by the Crusaders of old to the plagues carried and spread in modern times by pilgrims to and from Mecca, the plagues following the ancient armies and those of more recent times, the plagues attacking a country debilitated by famine or by superstition have been in the past, and will be in the future, due in a great measure to neglect and ignorance, on the part either of individuals or of a whole population, of the principles of the laws of health: and it is chiefly to this neglect, ignorance, and indolence that the spread and visitations of epidemic infectious disorders must be ascribed. It is, therefore, with justice, that these disorders are called preventable diseases, and one cannot imagine a greater contrast than that between the knowledge we possess at the present time of communicable diseases, as to their cause, mode of spread and prevention, and the views of former generations, as to their spontaneous origin.

Although the notion that all epidemic diseases are communicable, *i.e.* spread from one individual to another, is not a new one, since many writers of former generations have had clear ideas about them, yet the actual demonstration of the fact that the different infectious or communicable diseases are due to definite species of microbes, which, having invaded a human or animal organism, are capable therein of multiplying and of causing a particular infectious illness; further, the identification of these living germs in the blood and tissues of an invaded individual, and the recognition of their many and intricate migrations outside the animal body; the study of the microbes in artificial cultures, *i.e.* out-

[1] Lecture delivered at the Royal Institution on Friday, February 20, 1891, by E. Klein, M.D., F.R.S., Lecturer on Physiology at St. Bartholomew's Hospital Medical School.

side the human or animal body; further, the best means to do battle with them, to neutralize them, to prevent their growth and to destroy them; then the *modus operandi* of the different species, each appertaining to, and causing, a definite kind of disorder—in short, all that is exact and precise in the knowledge of the causation, nature, and prevention of infectious diseases, is an outcome of investigations carried on during the last twenty-five years. Modern research has not only definitely demonstrated these microbes, it has also shown that a number of diseases not previously suspected as communicable have a similar cause to the above, and are therefore now classed amongst them. It need hardly be emphasized that a knowledge of the causes must lead, and as a matter of fact has led, to a clearer and better understanding of the recognition, prevention, and treatment of these disorders, an understanding obviously directed towards, and followed by, the alleviation and diminution of disease and death in man and animals. I may point to a few special examples to illustrate these propositions. The disease known as splenic apoplexy or malignant anthrax is a disease affecting man and brutes. In some countries the losses to agriculturists and farmers owing to the fatal character of the disease in sheep and cattle is enormous. In man it is chiefly known amongst wool-sorters and those engaged in the handling of hides. This disease has been definitely proved to be due to a bacillus, the *Bacillus anthracis*, which, after its entry into the system of an animal or human being, multiplies very rapidly in the blood and spleen, and, as a rule, produces a fatal result, at any rate in sheep and cattle. Now, the bacillus having been proved to be always associated with this disease, anthrax, it was then shown that this bacillus can grow and multiply also outside the animal body: its characters in artificial media have been carefully studied and noted, so that it can be easily recognized; and by the pure cultures of the bacillus the disease can be again reproduced in a suitable animal. Such cultures have been subjected to a number of experiments with heat, chemicals, or antiseptics; the chemical function of the *Bacillus anthracis* has been and is being accurately studied in order to give us an insight into the mode in which it is capable of producing the disease; it has been further shown by Koch that the bacilli are capable of forming seeds or spores which possess a very high degree of resistance to various inimical conditions, such as heat, cold, chemicals, &c., and that it is precisely these spores, entering the system by the alimentary canal, through food or water, or by the respiratory organs through the air, to which the disease in most instances is due. Further, it has been shown that a trace of the blood of an animal affected with or dead from the disease, when introduced into an abrasion of the skin of man or animal, produces at first a local effect (carbuncle) followed by a general and often fatal infection. But the most important result of the cultivation of the bacillus outside the body, in artificial media, was the discovery that if subjected to or grown at abnormally high temperatures, 42°·5 C., *i.e.* above the temperature of the animal body, its power to produce fatal disease—that is, its virulence—becomes attenuated, so much so that, while the so-altered bacilli on inoculation into sheep or cattle produce a mild and transitory illness, they nevertheless furnish these animals with immunity against a fatal infection.

The recognition and identification of the *Bacillus anthracis* as the true cause of the disease, splenic fever or splenic apoplexy, the knowledge of its characters in the blood and spleen of man and animals, and of its peculiarities in artificial cultures, have enabled us to make a precise diagnosis of the disease, which previously was not always easy or even possible. The knowledge of its forming spores when grown under certain conditions, and of the manner in which experimentally the disease can be reproduced in animals by the bacilli and its spores, has led to a complete understanding of the means and ways in which the disease spreads both in animals and from them on to man; and last, but not least, the methods of the protective inoculations first indicated and practised by Pasteur have been solely the result of the studies in the laboratory of the cultures of the *Bacillus anthracis*, and of experiments with them on living animals. I could add here a number of other diseases, such as glanders, fowl cholera and fowl enteritis, erysipelas, scarlet fever and diphtheria in man, actinomycosis in man and cattle, swine fever and swine erysipelas, grouse disease, symptomatic charbon in cattle, and other diseases of animals—we have been brought to a fairly advanced understanding of one and all of these by methods such as those indicated above; and hereby not only in the diagnosis and recognition, but also in the treatment and prevention of these disorders, an immense amount of valuable progress has been achieved.

[1. Demonstration: lantern slides of anthrax, fowl cholera, fowl enteritis, grouse disease, typhoid, cholera, pneumonia, diphtheria, actinomycosis, scarlatina, and glanders.]

As examples of the second proposition, viz. that the modern methods of study of disease germs, of their nature and action on living animals have led to the recognition as communicable diseases of some disorders which previously were not known or even suspected to be of this character, I may mention amongst several the disease known as tuberculosis or consumption, tetanus or lockjaw, and acute pneumonia. Not until Klencke and Villemin had shown by direct experiment on animals that tuberculosis is inoculable was it grouped amongst the infectious diseases. Since these experiments were first published a large amount of work has been done, proving conclusively that tuberculous material—that is, portions of the organs containing the tubercular deposits (*e.g.* lung, lymph gland, spleen, &c.)—by inoculation, by feeding or by introducing it into the respiratory tract, can set up typical tuberculosis in the experimental animals; the tubercular deposits in these experimental animals again are endowed with the power to propagate the disease in other animals. Further, it was shown that the disease in cattle called "Perlsucht" was in all respects comparable to tuberculosis in man, and it is accordingly now always called tuberculosis.

A further, and perhaps the greatest, step was then made by Koch's discovery in 1882 of the tubercle bacillus, and his furnishing the absolute proof of its being the true cause of the disease. The demonstration and identification of this microbe is now practised, I might almost say, by every tyro, and it is of immense help to diagnosis. In former years, and before 1882, the diagnosis of tuberculosis was not by any means an easy matter in many cases of chronic lung disease; since that year every physician in such cases examines the expectoration of the patient, and the demonstration of the tubercle bacilli makes the diagnosis of tuberculosis absolutely certain. Not only in medical, but also in many surgical cases, *e.g.* certain forms of chronic disease of bones and joints, particularly in children, the demonstration of the tubercle bacilli is of essential importance, and by these means diseases like lupus of the skin, scrofula and certain diseases of bones and joints not previously known as tuberculosis, are now proved to be so. The same applies to animals; wherever in a diseased organ of man or animal the tubercle bacilli can be demonstrated, the disease must be pronounced as tuberculosis.

The proof that the tubercle bacillus is the actual cause of the tubercular disease was established by Koch beyond possibility of doubt. Cultures in artificial media were made from a particle of a tubercular tissue either of a human being, or of cattle affected with tuberculosis, or of an experimental animal tubercular by ingestion, or by injection with tuberculous matter, and in all cases crops of the tubercle bacilli were obtained. Such cultures were then carried on from subculture to subculture, through many generations, outside the animal body; with a mere trace of any of these subcultures, however far removed from the original source, susceptible animals were infected, and all without fail developed tuberculosis, with the tubercle bacilli in the morbid deposits of their organs. The discovery of the tubercle bacilli and the demonstration that they are constantly present in the tubercular deposits of the typical tuberculosis, and the proof by experiment on living animals that they are the actual cause of the disease, are not all that we have learned, for it has also been shown that certain diseases, like the dreaded and disfiguring disease known as lupus—at any rate some forms of it—and further the disease scrofula, so often present in children, are really of the nature of tuberculosis, the former in the skin, the latter in the lymph glands.

Now see what an enormous step in advance this constitutes:—

(1) We can now diagnose tuberculosis with much greater accuracy in man and animals, even in cases in which this was formerly difficult or impossible.

(2) We have accepted rightly that all forms of tuberculosis are infectious or communicable diseases, communicable by inoculation, by ingestion, *i.e.* by food, or by respiration, *i.e.* by air.

(3) We have learned to recognize that, as in other infectious disorders, there exists a risk to those susceptible to tuberculosis, of contracting the disease from a tubercular source, and it is the recognition of these facts which ought to regulate all efforts to prevent its spread.

Tetanus or lockjaw, not previously known to be so, has likewise been fully demonstrated to be an infectious disease: we now know that it is due to a bacillus having its natural habitat in certain garden earth; that this bacillus forms spores, that these spores gaining access to an abrasion or wound of the skin in man or animals are capable of germinating there and multiplying, and of producing a chemical poison which is absorbed into the system, and sets up the acute complex nervous disorder called lockjaw. The recognition of the disease as an infectious disease and caused by a specific microbe has taught us at the same time the manner in which the disease is contracted, and thereby the way in which the disease is preventable.

[2. Demonstration: lantern slides of tubercle and tetanus.]

The study of disease germs by the new and accurate methods of bacteriology has also led to a clearer and better understanding of the manner in which at any rate some of the infectious diseases spread. While it was understood previous to the identification of their precise cause that some spread directly from individual to individual (*e.g.* small-pox, scarlet fever, diphtheria), others were known to be capable of being conveyed from one individual to another indirectly, *i.e.* through adhering to dust, or being conveyed by water, milk, or by food-stuffs (*e.g.* cholera, typhoid fever). But we are now in a position to define and demonstrate more accurately the mode in which infection can and does take place in many of the infectious diseases. By these means we have learned to recognize that the popular distinction between strictly contagious and strictly infectious diseases—the former comprising those diseases which spread as it were only by contact with a diseased individual, while in the latter diseases no direct contact is required in order to produce infection, the disease being conveyed to distant points by the instrumentality of air, water, or food—is only to a very small extent correct. Take, for instance, a disease like diphtheria, which was formerly considered a good example of a strictly contagious disorder; we know now that diphtheria, like typhoid fever or scarlet fever, can be, and, as a matter of fact, is, often conveyed from an infected source to great distances by the instrumentality of milk. In malignant anthrax, another disease in which the contagium is conveyable by direct contact, *e.g.* in the case of an abrasion or wound on the skin coming in contact with the blood of an animal dead of anthrax, we know that the spores of the anthrax bacilli can be, and, as a matter of fact, in many instances, are, conveyed to an animal or a human being by the air, water, or food. The bacilli of tubercle, finding entrance through a superficial wound in the skin or mucous membrane, or through ingestion of food, or through the air, can in a susceptible human being or an animal produce tuberculosis either locally or generally. The difference as regards mode of spread between different diseases resolves itself merely into the question, Which is, under natural conditions, the most common mode of entry of the disease germ into the new host? In one set of cases, *e.g.* typhoid fever, cholera, the portal by which the disease germ generally enters is the alimentary canal; in another set an abrasion or wound of the skin is the portal, as in hydrophobia, tetanus, and septicæmia; in another set the respiratory organs, or perhaps the alimentary canal, or both, are the paths of entrance of the disease germ, as in small-pox, relapsing fever, malarial fever; and in a still further set the portal is just as often the respiratory tract as the alimentary canal, or a wound of the skin, as in anthrax, tuberculosis. But this does not mean that the virus is necessarily limited to one particular portal, or that it must be directly conveyed from its source to the individual that it is to invade. All this depends on the fact whether or not the microbe has the power to retain its vitality and virulence outside the animal or human body.

Anthrax bacilli are killed by drying: they gradually die off if they do not find sufficient nutriment in the medium into which they happen to be transferred; they are killed by exposure to heat far below boiling-point; they are killed by weak carbolic acid. But if these anthrax bacilli have been able to form spores, these latter retain their vitality and virulence when dried, when no nutriment is offered to them, and even when they are exposed for a few seconds to the heat of boiling water, or when they are exposed to the action of strong solutions of carbolic acid. Similarly, the bacillus of diphtheria when dried is killed also by weak solutions of carbolic acid; it is killed when kept for a few days in pure water on account of not finding sufficient nutriment; fortunately the diphtheria bacillus is killed in a few minutes at temperatures above 60° or 65° C., for this bacillus does not form spores. The same is the case with the microbe of scarlet fever.

The tubercle bacillus forms spores; these are not killed by drying, they are killed by the heat of boiling water of sufficiently long duration, two or more minutes; they are not killed by strong carbolic acid.

While, therefore, we know in these cases on what the conditions of infection depend, we have also learned to understand the conditions which favour or prevent the infection.

Not all infectious diseases which have been studied are due to Bacteria: in some the microbe has not been discovered, *e.g.* hydrophobia, small-pox, yellow fever, typhus fever, measles, whooping-cough; in others it has been shown that the disease is due to a microbe which belongs, not to the Bacteria, but to the group of those simplest animal organisms known as Protozoa. Dysentery and tropical abscess of the liver are due to Amœbæ; intermittent fever or ague is due to a protozoon called Hæmoplasmodium; a chronic infectious disease prevalent amongst rodents, and characterized by deposits in the intestine, liver, and muscular tissue, is due to certain forms known as Coccidia, or Psorospermia. A chronic infectious disease in cattle and man known as actinomycosis is due to a fungus, the morphology of which indicates that it probably belongs to the higher fungi; certain species of moulds (*e.g.* certain species of Aspergillus and Mucor) are also known to be capable of producing definite infectious chronic disorders, and so also is thrush of the tongue of infants; ringworm and certain other diseases of the hair and skin are known to be due to microbes allied to the higher fungi.

The microbes causing disease which have been studied best, are those belonging to the groups of Bacteria or Schizomycetes or fission fungi (they multiply by simple division or fission); most species of these have been cultivated in pure cultures, and the new crops have been utilized for further experiments on animals under conditions variable at the will of the experimenter.

[3. Demonstration: cultures of Bacteria in plates and in tubes.]

(*To be continued.*)

RECENT PHOTOGRAPHS OF THE ANNULAR NEBULA IN LYRA.

ANNULAR nebulæ can no longer be regarded as a class completely apart. They should rather be described as planetaries in which one special feature predominates over the rest. The progressive improvement of telescopes has tended to assimilate the two varieties by bringing into view peculiarities common to both. It is only when they are ill seen that planetary nebulæ appear really such. The uniformity of aspect at first supposed to characterize them disappears before the searching scrutiny of a powerful and perfect instrument. The usually oval surfaces which they present are then perceived to be full of suggestive detail. They are broken up by irregular condensations, or furrowed by the operation of antagonistic forces; betray here the action of repulsive, there of attractive, influences; and bear as yet undeciphered inscriptions of prophetic no less than of commemorative import. Among the various modes of diversification visible in them, however, two are especially conspicuous—first, the presence of a nucleus; next, the emergence of a ring, or even of a system of rings.

Now the nebula in Lyra, when distinguished by Sir William Herschel in the annular form, of which it is the most perfect exemplar, seemed completely perforated by a dark opening; but interior nebulosity, constituting the object essentially a disk with annular condensation, was noticed by Schröter in 1797, and is depicted as strongly luminous in the drawings of Lassell (reproduced in *Knowledge*, November 1890), Trouvelot (*Harvard Annals*, vol. viii., Plate 34, 1876), and Holden.[1] Moreover, a minute central star, visually discerned at intervals, has lately been photographed in unmistakably nuclear shape. The entire formation, then, consists of a disk, nucleus, and ring, and differs from many planetaries only in the proportionate lustre of its parts.

The records concerning the central star include some curious anomalies. They go back to the year 1800, when Von Hahn was struck with its disappearance. The change was in his opinion due, not to loss of light in the star, but to the veiling with delicate nebulous clouds of the dark background upon which, in former years, it had been seen projected (*Astr. Jahrbuch*, 1803, p. 106). His observations were made at Remplin, in Mecklenburg, with a 12-inch Herschelian speculum, somewhat impaired in brilliancy (Lisch, "Gesch. des Geschlechts Hahn," Bd. iv. p. 282). "Hahn's star" was next seen by Lord Rosse 1848 to 1851 (Trans. R. Dublin Society, vol. ii. p. 152), and drew the notice of Father Secchi in 1855 (*Astr. Nach.*, No. 1018). Twice observed by M. Hermann Schultz at Upsala in 1865 and 1867 ("Observations of 500 Nebulæ," p. 99), it unaccountably, ten years later, evaded the deliberate scrutiny with the Washington 26-inch of Prof. Asaph Hall, who nevertheless perceived the nebula as exteriorly surrounded by a "ring" of nine faint stars (*Astr. Nach.*, No. 2186). The same great instrument, however, displayed the missing star to Mr. A. C. Ranyard, August 23, 1878 (*Astr. Journal*, No. 200), while to Dr. Vogel, equally in 1875 with the Newall refractor, and on several nights in 1883 with the Vienna 27-inch, it remained imperceptible (*Potsdam Publicationen*, No. 14, p. 35). Very remarkable, too, is its non-appearance to Dr. Spitaler at Vienna in 1885, when he carefully delineated the nebula, as well as in 1886, during repeated verifying observations. The interior seemed then to contain only dimly luminous floccules; yet the star thus persistently invisible caught his eye at the first glance, July 25, 1887 (*Astr. Nach.*, No. 2800). It had in the meantime, September 1, 1886, been photographed by Von Gothard; and having committed itself to this *adsum qui feci* avowal, has, now for four years past, abstained from capricious disappearances.

Its variability, then, is still unproved; since observational anomalies, even of a very striking kind, may be explained in more ways than one; and it is very easy *not* to see a fifteenth-magnitude star. More especially when the sky behind it is—perhaps intermittently—nebulous. Luminous fluctuations in the diffused contents of the ring certainly suggest themselves to the student of its history. At times a bare trace of nebulosity is recorded; at others, the whole interior is represented as filled to the brim with light—mist, coagulated, as it were, into a cirrous or striated arrangement. Under such conditions, the effacement of quasi-stellar rays, feebly seen at the best, is not surprising.

The "gauzy" stuff within the ring possesses very slight actinic power. The best drawings of the nebula represent what might be described as an oval disk with a brighter border, while in all photographs hitherto taken it comes out strongly annular. The interior does not fill up even with such abnormally long exposures as might be expected to abolish gradations by giving faint beams time to *overtake* the chemical effect more promptly produced by brighter rays.

The photographic record of the Lyra nebula goes back a very few years. It opens in 1886, with some Paris impressions showing a small, nearly circular ring, starless, and perfectly black within. A decided advance was marked by Von Gothard's picture of a pair of nebular parentheses—thus, ( )—inclosing a very definite, though probably non-stellar, nucleus. The failure of light at each extremity of the major axis, which makes one of the most significant features of this object, was already in 1785 noticed by the elder Herschel (*Astr. Jahrbuch*, 1788, p. 242; Phil. Trans., 1785, p. 263); and Lord Rosse in 1863, and Schultz in 1865, were struck with its accompaniment, *on the north-eastern side*, by "nebulous radiations in the direction of the longer axis, which seemed momentarily almost to destroy the annular form." This appearance of an equatorial outflow, however, had shifted to the opposite or *south-westerly side* of the nebula when Holden observed it at Washington in 1875;[1] while a photograph taken by Mr. Roberts, with twenty minutes' exposure in July 1887, showed a very decided protrusion in the place indicated by Schultz, but only an abortive attempt at Holden's appendage. The suggestion of real changes of an alternating character, affecting luminosity perhaps, rather than figure, meets some confirmation in Prof. Holden's remark upon a further incompatibility between his own and Lord Rosse's observations on a different part of the same object. "It is a little curious," he wrote in 1876, "that that end of the minor axis which Lord Rosse has represented as the best terminated, viz. the *south*, is precisely that one which to-day, and with the Washington telescope, is least so" (*Monthly Notices*, vol. xxxvi. p. 63). And the Lick 36-inch similarly disclosed the whole of the bright southern edge as filamentous (*Monthly Notices*, vol. xlviii. p. 387) in opposite correspondence with the views obtained at Parsonstown of the northern edge.

[1] Executed in 1875, with a power of 400 on the 26-inch Washington refractor (*Wash. Observations* for 1874, Pl. vi.).

[1] *Monthly Notices*, vol. xxxvi. p. 66: and pastel drawing in *Wash. Obs.*, 1874.

The central star was recorded in fifteen minutes on Mr. Roberts's plates (*Monthly Notices*, p. 29); it waited 1h. 50m. to appear at Bordeaux. A picture, however, taken there in three hours by M. Courty in July 1890 included all Lord Rosse's seven exterior stars (Rayet, *Comptes rendus*, 7 Juillet, 1890); and Admiral Mouchez's suspicion that the nuclear one was inclosed in a quadrilateral of much fainter objects was verified, as regards three of the four, by a subsequent photograph obtained at Algiers by MM. Trépied and Rabourdin, with two exposures of three hours each, for which the international charting instrument of 13 inches was successfully employed.[1] In the resulting impression, Hahn's "star" is distended into a nucleus far more luminous than the fainter parts of the ring, and contrasting powerfully with a black intermediate space, usually grey and glimmering to telescopic vision. The ring itself comes out broad, hazy, and far from uniformly illuminated. Two somewhat unequal maxima and two conspicuous minima of brilliancy mark the extremities respectively of its minor and major axes. Its outline, though blurred, is tolerably symmetrical. No effusions interrupt, no "fringes" obliterate it.

The method of multiple exposures (introduced by Mr. Roberts) enabled, in November last, the Toulouse astronomers, MM. Andoyer and Montangerand, to get sittings from the Lyra nebula for a single portrait, summing up to nine hours, with the result of bringing out, over an area of three square degrees, about 4800 stars, including, of course, the nuclear *punctum saliens*. And the registration of this stellar multitude is the more valuable from the probability that some of them may belong to the same system with the strange object round which they swarm. Its inclosure by a "ring of stars" was remarked by Prof. Hall in 1887; Prof. Holden in 1888 perceived indications of a second similar but interior ring, evidently forming part of a complex nebular structure. The places of twelve of its minute components were measured by him; and it will be interesting to learn whether they can be identified in the Toulouse photograph.

An irresistible inference from the data collected, both visually and photographically, concerning the Lyra nebula, is that its ellipticity is genuine, and not merely an effect of foreshortening. For it would be absurd to suppose the plan of construction of a sidereal body related in any way to its situation as regards the line of sight from the earth; hence, geometrical shape that is emphasized by inequalities of light must correspond to a physical reality. It is certainly by no accident that the transverse axis of the object in question terminates in maxima, its longitudinal axis in minima of brightness; and this alone conveys a positive assurance that it *is*, and does not simply appear oval.

More especially when we find that this very peculiarity is shared by several other nebulæ of the same class (Secchi, "Les Étoiles," t. ii. p. 13), and forms a link of a profoundly suggestive kind between them all and the great Dumb-bell Nebula. For this remarkable object, too, shows minima of illumination evidently homologous with those of the pattern ring-nebula; they affect corresponding parts of the structure, and depend, there can be little doubt, in a similar manner, upon internal organization. They are besides in each case attended by a symptom which may prove instructive as to their mode of origin. Mr. Roberts's photographs of the Dumb-bell, no less than of the annular nebula, bring into view dim effusions of nebulosity in the directions of its greatest extent. The elliptical outline is transcended at the two opposite points where it is partially interrupted by gaps of comparative obscurity. Now, the late Mr. Roche, of Montpellier, demonstrated that the "limiting surface" of a cometary mass falling towards the sun must assume the shape of an oval pointed along the line of junction with the sun (*Mémoires de l'Acad. de Montpellier*, t. ii. p. 426). This surface, representing the boundary of the atmosphere controllable by the comet, necessarily contracts in proportion as the solar attraction gains predominance; while the matter thus progressively abandoned, instead of escaping indifferently in all directions, flows away solely along recurving lines from the nodal points (as they might be termed) of the cometary envelope. The question then presents itself whether both the oval figure, and the apparent effluences along the major axes of the nebulæ we have been considering, may not be due to attractive influences in their neighbourhood. A gradual abandonment of nebulous material, at some previous time fully incorporated with the central mass, seems at any rate indicated, and cannot otherwise be easily accounted for.

A. M. Clerke.

[1] *L'Astronomie*. t. ix. p. 441; Ranyard, *Knowledge*, November 1890; *C. rendus*, t. cxi. No. 15.

ZITTEL'S "PALÆONTOLOGY"—REPTILES.

In the annual address to the Geological Society delivered last year by the retiring President, the work of which a portion is now before us was referred to as "the superb palæontological compendium now being published by our distinguished Foreign Member, Prof. K. v. Zittel, a book that, I fear, no English publisher at present would feel justified in undertaking." It is with a feeling of admiration mingled with regret—of admiration for this splendid work, and of regret that its like cannot be produced in this country—that we quote these words; for it is unfortunately but too true that palæontological science, or, in other words, the zoology of past epochs of the earth's history, is not cultivated among us with anything like that zeal which its importance and interest merit. Indeed (although there are signs that this spirit is now passing away), we too often hear palæontology and palæontologists mentioned by those who ought to know better with a covert, if not with an open sneer. Palæontology, however, if studied in a proper philosophic spirit, must be the very groundwork of all our systems of zoological classification; and therefore all students of zoology—both recent and past—owe a deep debt of gratitude to the author of this "Palæontologie," which may be said to be the only work which is so comprehensive as to deal, not only with the general outlines, but also with the details of the science of which it treats.

The two parts forming the subject of the present notice deal with that division of Vertebrates for which Prof. Huxley proposed the name Sauropsida, or, in other words, Reptiles and Birds. No less than 427 pages of letterpress, illustrated by 298 woodcuts, are devoted to this portion of the work, which is alone sufficient to give some idea of the fulness with which the subject is treated. Since, however, there are some repetitions of the figures, the total number is somewhat less than that given above. A considerable proportion of these woodcuts are original, but others have been copied from the memoirs of Owen, Cope, Marsh, and others, as well as from the British Museum Catalogues.

In dealing with the difficult question of the arrangement of the orders of Reptiles, and the number of such orders which it is advisable to adopt, we think that in the former respect the author has not been altogether well advised. The orders adopted are nine in number, and are arranged in the following sequence, viz.: (1) Ichthyosauria; (2) Sauropterygia; (3) Testudinata; (4) Theromorpha; (5) Rhynchocephalia; (6) Lepidosauria; (7) Crocodilia; (8) Dinosauria; and (9) Pterosauria.

Now the first point that strikes us as incongruous in this arrangement is the position assigned to the Theromorpha (otherwise known as Theromora or Anomo-

[1] K. v. Zittel, "Handbuch der Palæontologie," Vol. III. Vertebrata, Parts 3 and 4 (Munich and Leipzig, 1889–90).

dontia). Thus if any one point more than another is firmly established in herpetology, it is the relationship of the Theromorphs to the Amphibians; and there can, accordingly, be no question but that the position of this order should be immediately following the last-named class. We venture, indeed, to think that the author has by no means sufficiently realized the peculiar structural features and the high morphological importance of the Theromorphs, which are so closely allied, on the one hand to Amphibians, and on the other to the primitive Mammalian stock. It must, however, be conceded in his favour that several memoirs on this group have appeared since this portion of the work before us was written; while the confusion that has existed up till a late period between the bones of the shoulder and pelvic girdles of these reptiles renders certain errors in regard to these points perfectly excusable. Still, the absence of any definite mention of a distinct third bone—the precoracoid—in the shoulder-girdle of many members of this group, which is of such extreme morphological importance, is an omission which cannot be lightly passed over. A fourfold division of the Theromorphs into suborders is adopted—namely, into Anomodonts, Placodonts, Pariasaurians, and Theriodonts. The Anomodonts are represented by the well-known African and Indian Dicynodonts, but the inclusion of *Phocosaurus* is a glaring error, since the pelvis so named should be referred to quite another group. Indeed, the author—probably misled by others—does not appear to be acquainted with the true construction of the shoulder and pelvic girdles of the Dicynodonts. For instance, the bone represented on p. 560, Fig. 506, *b*, as a Dicynodont coracoid (which could not, by the way, possibly fit on to the scapula in the same figure) is really a right ilium. Then, again, the pelvis and sacrum shown in Fig. 510 (p. 562) as belonging to *Dicynodon*, are referable to *Pariasaurus*. Again, the thrusting of the Placodonts between the Anomodonts and Pariasaurians is an arrangement that will certainly not commend itself to those who have attentively studied these groups; while the Pariasaurians, as the lowest and most generalized representatives of the whole order, should certainly have come before all the others. The absence of full information on the structure of this group at the date of publication must, however, here again defend the author. The Owenian division of the Theriodonts into single-nostriled and double-nostriled groups has not stood the test of fuller examination; and the mingling of the American Permian types with the typical African ones is certainly an ill-advised proceeding. A wise discretion is, however, exercised in not assigning any definite position to that most remarkable African genus described by Sir R. Owen as *Endothiodon*, the skull of which presents such a curious approximation to a Mammalian type in the structure of the palate. If we may venture to hazard a conjecture on this point, we think it probable that the mammal-like humerus described by another writer under the name of *Propappus*, together with a pelvis which has been provisionally associated with it, will both eventually prove to belong to *Endothiodon*; when this genus will take a place near *Pariasaurus* as one of the most mammal-like reptiles yet known to us.

In criticizing thus freely Prof. v. Zittel's treatment of the Theromorphs, we do so because this is obviously by far the weakest part of this section of the work, and one where the student ought to be advised of the necessity of seeking for other aids to his studies.

The pestilent doctrine that the Ichthyosaurs are primitive reptiles directly descended from fishes would seem to be the sole reason for placing them first of all the reptilian orders. We have, however, but little doubt that the position assigned them by Dr. Baur next the Rhynchocephalians best expresses their true relationship. In the classification of this order the species are arranged under the genera *Mixosaurus*, *Ichthyosaurus*, *Ophthalmosaurus*, and *Baptanodon*, according to the British Museum Catalogue of Fossil Reptiles, from which (as in the case of the next order) a large proportion of the illustrations have been borrowed.

In the Sauropterygia, as represented by the Nothosaurs and Plesiosaurs, the systematic arrangement is likewise practically the same as the one adopted in the work last mentioned, with the important exception that the small Triassic reptile known as *Mesosaurus* is transferred to the Rhynchocephalia, of which more anon. In connection with the illustrations of the osteology of this order, we may observe that the two figures of the shoulder-girdle given on p. 777, as well as the first of the two on the succeeding page, are not altogether correct; while Fig. 463, on p. 487, described as the skeleton of *Plesiosaurus dolichodirus*, really represents the type specimen of *P. hawkinsi*.

There is nothing especially to notice concerning the Nothosaurs. In massing under the name *Cimoliosaurus* all those post-Liassic Plesiosaurs in which the ribs of the neck unite with the bodies of the vertebræ only by a single articulation (Fig. 1), the author once more follows the plan proposed in the British Museum Catalogue. It might, however, have been stated somewhat more definitely that this is to some extent merely a provisional arrangement for the convenience of working, and that it may subsequently be found advisable to refer those forms in which the vertebræ have more or less concave terminal faces (like the one in the figure reproduced) to a different genus, or even to several genera. The separation of the Triassic genus *Pistosaurus* from the typical Nothosaurs to form a new family at the top of the order is an entirely new departure, the necessity for which we are not altogether prepared to admit.

We are not sure that in adopting the name Testudinata, in place of the more usual Chelonia, for the tortoises and turtles, the author is well advised, although we are not disposed to find fault for such a trifling matter. At the same time, we are glad to see the view entertained that the so-called Leathery turtles are really nothing more than an extreme development of the type to which the true turtles belong, and that they are, therefore, not entitled to form a distinct sub-order. The separation of the turtles of the London Clay as a family distinct from the *Chelonidæ* is, however, certainly not justifiable, since they come exceedingly close to the living Loggerhead; and even if such separation were justifiable, the name of the family would surely not be *Chelonemydidæ*. A more serious error is the inclusion of the generalized genus *Platychelys* in the *Chelydridæ*, with which it has nothing whatever to do, being allied to the well-known *Pleurosternum*, of the English Purbeck, which appears to represent the ancestral group from which both the modern Cryptodira and Pleurodira have taken rise. Prof. v. Zittel places this genus among the Pleurodira, where he makes no attempt to separate the various genera into families, although a workable classification has been already proposed elsewhere.

Omitting the Theromorphs, to which we have already referred, the next group in the series is that of the Rhynchocephalians, followed by the Lepidosauria (Lizards and Snakes). In the near association of these two orders we think the author is decidedly in the right, for it appears impossible to believe that Lizards are not the direct greatly modified descendants of the more primitive Rhynchocephalians. In the latter order the remarkable Permian genus *Palæohatteria*—the prototype of the New Zealand *Sphenodon*—is placed in the same family with *Proterosaurus*, which some authors even make the type of a distinct order. This family is followed by the *Mesosauridæ*, the members of which undoubtedly show signs of affinity with *Palæohatteria*, although we venture to think that their Sauropterygian relationships are sufficiently manifest to override this. We cannot, moreover, refrain from surprise when we see that the author still retains the genus *Stereosternum* as distinct from *Mesosaurus*, when those that have examined specimens of each are puzzled to find even points of specific distinctness.

In including Lizards, Snakes, and Mosasaurs under one ordinal heading, the author follows the lead of some of those who have given especial attention to these groups. The substitution of the name Lepidosauria for the more generally used term Squamata is, perhaps, an improvement, since the latter has been also applied to the loricated Edentate Mammals. In placing, however, the Cretaceous *Dolichosaurus* with the true Lizards, we notice a marked error in judgment, since this genus clearly indicates the ancestral type of the Mosasaurs. We must equally deprecate the retention of the genus *Palæovaranus*, of the French Phosphorites, among the *Varanidæ*, since it has been fully proved that all the remains so named belong to *Anguidæ*; the *Varanidæ*, so far as we know, being unknown in the earlier Tertiaries of any part of the world.

The Crocodiles are divided into the three sub-orders of Parasuchia, Pseudosuchia, and Eusuchia, the recent proposal to separate the first (and probably the second) group as a distinct order not having been published at the time that the work went to press. The tempting view that the true Crocodiles (Eusuchia) can be divided into a Longirostrine and a Brevirostrine section—the one terminating in the Gharial and the other in the Alligator—is, we regret to see, adopted. The close structural resemblance in all essential details of these two reptiles is, however, on the face of it almost sufficient proof that they could not have originated from totally independent stocks. And the truth indeed appears to be that every group of Crocodiles at every epoch has developed both long- and short-jawed forms. Thus in the *Metriorhynchus* group, which is placed among the Longirostres, the genus *Suchodus* is as short-jawed as any Alligator; while *Geosaurus* itself can scarcely be called more long-jawed than many species of the genus *Crocodilus*. We fail also to see how the genera *Plesiosuchus* and *Dacosaurus* are to be distinguished from *Geosaurus*. Here may be mentioned incidentally a small error, very excusable in a foreigner, into which the author has fallen in regard to the magnificent series of reptilian remains from the Oxford Clay collected by Mr. A. N. Leeds, of Eyebury, near Peterborough. Confusing this gentleman's

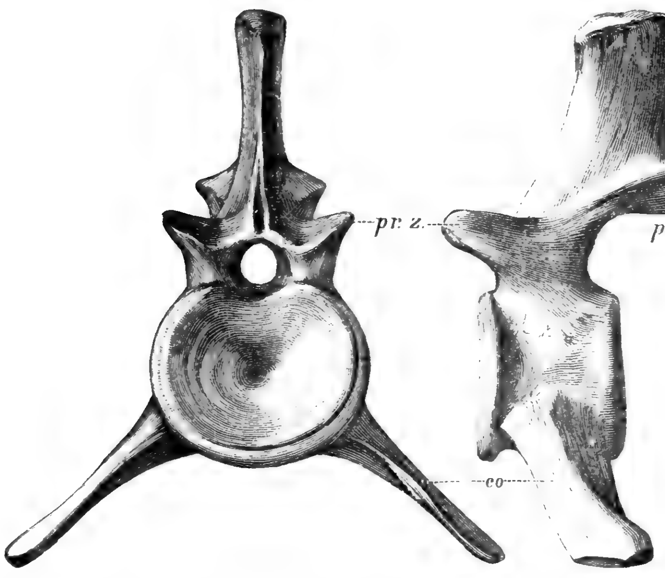


Fig. 1.—Front and left views of a cervical vertebra of *Cimoliosaurus*, from the Oxford Clay. ⅓. *pr.z.*, *pt.z.*, pre- and post-zygapophyses; *co*, rib.

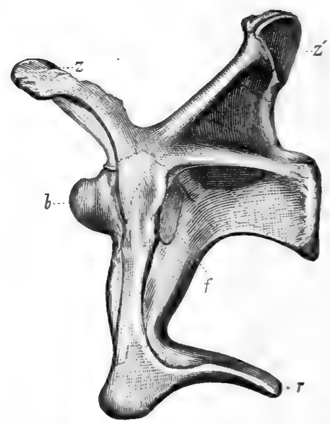


Fig. 2.—Left view of cervical vertebra of *Brontosaurus*. 1/12. *z*, *z'*, zygapophyses; *b*, body; *r*, rib; *f*, lateral aperture.

name with that of the Yorkshire town, we find the author, on p. 670, referring to "Eyebury, near Leeds," and on p. 714, "Eyebury, Yorkshire." Among the so-called Brevirostres we meet with some very interesting, and to us entirely new, information concerning certain small reptiles, from the lithographic limestones of the Continent, known as *Alligatorellus* and *Atoposaurus*. The former is clearly a crocodile, and its resemblance to the latter (which has long been known to possess typical crocodilian feet) points to the conclusion that it should also be regarded as a member of the same order which (like *Metriorhynchus*) had entirely lost its dermal bony armour.

The Dinosaurs, as their importance and interest demand, are treated of very fully. This order, in contravention of some recent proposals, is retained in its widest sense, in which respect we are in full accord with Prof. v. Zittel. It is divided into three sub-orders, for which the terms Sauropoda, Theropoda, and Orthopoda are adopted. The beautiful illustrations (some of which we reproduce) are largely taken from Prof. Marsh's memoirs.

In regard to the vexed question of the relationship of the huge herbivorous forms known as Sauropoda (of which a cervical vertebra is shown in Fig. 2) of Europe and the United States, we certainly do not consider that the author has advanced matters by referring the English *Cetiosaurus* to a distinct family, following this by the American *Atlantosauridæ*, and going on with the *Morosauridæ*, which is taken to include the American *Morosaurus* and the English Wealden forms. *Cetiosaurus* appears to be decidedly inseparable from the *Morosauridæ*, while in the opinion of more than one English writer the Wealden forms should be included in the *Atlantosauridæ*. It is to be feared that the writer of this review is primarily responsible for the relegation of the oldest name *Pelorosaurus* to the rank of a synonym; and it is to be regretted that the name *Ornithopsis* again crops up, after having been shown that it has no title to stand.

In the Theropoda, or carnivorous types, we meet again with the same redundancy of families which cannot possibly be justified. Thus we have *Zanclodontidæ*, *Megalosauridæ*, *Ceratosauridæ*, and *Anchisauridæ*; of which, in our opinion, only two should stand. The reference of *Thecodontosaurus* to the *Zanclodontidæ* cannot for a moment be maintained, since it is a question whether it is even distinct from *Anchisaurus*, while *Zanclodon* appears too close to *Megalosaurus* to be separated as a family. Marsh's figure of the coalesced metatarsals of *Ceratosaurus* is once again reproduced, but, in the face of the note quoted from Dr. Baur to the effect that this is caused by disease, its omission would have been preferable. The author will probably correct the assignation of the name *Epicampodon* to Prof. Huxley.

In those remarkable little Dinosaurs known as the *Cœluridæ*, it is unfortunate that Prof. Cope's redetermination of the American forms now known as *Cœlophysis* did not appear in time to prevent their reference to the problematical *Tanystrophæus*. In refusing to regard the European *Compsognathus* and the American *Hallopus* as anything more than the representatives of distinct family groups the author is to be commended. The peculiar backward prolongation of the calcaneum of *Hallopus* (as shown in our reproduction of Prof. Marsh's original figure) is, however, so remarkable as to indicate that its owner is a very aberrant member of the sub-order.

In grouping the Ornithopoda and Stegosauria of Prof. Marsh in a single sub-order, the author follows the lead of Dr. Baur, and the adoption of the name Orthopoda for this group is, perhaps, an improvement on the nomenclature of some other writers. This sub-order is again divided into three sections, the Armoured forms, or Stegosauria, the Horned types (Ceratopsia), and the Iguanodonts (Ornithopoda). In the *Stegosauridæ*, so markedly differentiated by their hoof-like feet (Fig. 4), the author seems to have some doubts as to the identity of the European *Omosaurus* with the American *Stegosaurus*; and he has unfortunately been misled in stating that *O. durobrivensis* is from the Kimmeridge Clay, its real horizon being the Oxfordian. We need say nothing of the wonderful Horned Dinosaurs, as they have been noticed in an article recently published in this journal. In the Iguanodonts the separation of the *Camptosauridæ* from the *Iguanodontidæ* is not supported by any sufficient structural differences; the "pendant" inner trochanter of the femur of *Camptosaurus* being likewise found in some species of *Iguanodon*. In stating, moreover, that *Camptosaurus* differs from *Iguanodon* by the shorter anterior process of its ilium, the author relies on a figure published by Prof. Marsh, which a recent writer states to have been drawn from an imperfect specimen. The disregard of the rights of priority in nomenclature, to which we have already alluded, is again manifest in the retention of the name *Hadrosaurus* instead of the earlier *Trachodon*. In including that most bird-like Dinosaur only recently described under the name of *Ornithomimus*, the work is well up to date.

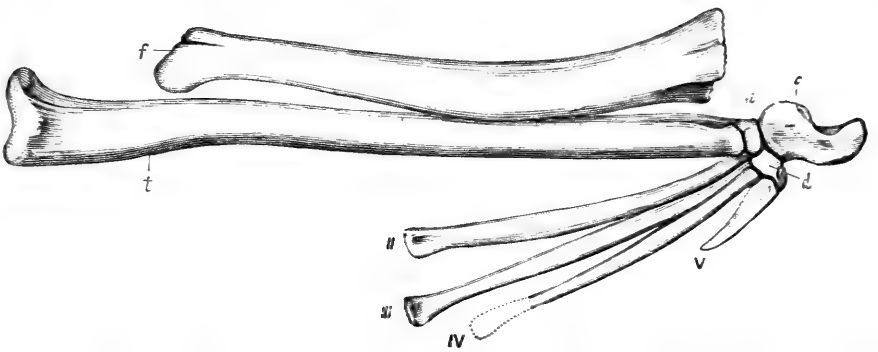


FIG. 3.—Part of left hind limb of *Hallopus*. Nat. size. *t*, tibia; *f*, fibula; *c*, calcaneum; *a*, *d*, distal tarsals; II–V, metatarsals.

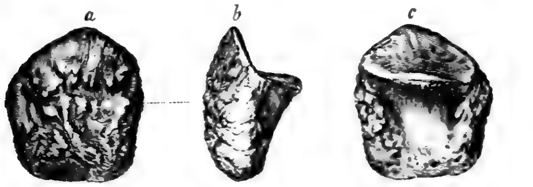


FIG. 4.—Three views of one of the terminal joints of the foot of *Stegosaurus*.

The chapter devoted to the Pterodactyles calls for no special notice; the only innovations being the creation of a distinct family for the reception of the genus *Ornithochirus* of the Cambridge Greensand, and the abolition of the sub-order Pteranodontia, made for the toothless American forms.

The Birds, as their minor palæontological importance demands, have but a comparatively small amount of space allotted to them. They are divided into the three orders Saururæ, Ratitæ, and Carinatæ; the minor groups, usually reckoned as orders by ornithologists, being regarded as sub-orders. The extinct toothed *Hesperornis* is included in the Ratitæ—an arrangement which the author may see reason to revise when he has had the opportunity of studying the memoir on the affinities of this genus recently published by Prof. D'Arcy Thompson. In referring the fragments of Tertiary bones described as *Macrornis* and *Megalornis* provisionally to the Ratitæ, Prof. v. Zittel has been misled by their original description; and he has unfortunately not seen the notice that the so-called *Dromæus sivalensis* was founded upon Mammalian bones. The provisional reference of *Dasornis* to the *Rheidæ* rests on no solid foundation; the genus being apparently allied to *Gastornis*, which is placed—in our opinion incorrectly—among the Carinatæ. The Kiwis and Moas are included in one sub-order; the latter being divided into three genera. If, however, the author were to study carefully the literature of the *Dinornithidæ*, he would find that the name *Palapteryx* cannot be legitimately employed in the sense in which he uses it. There is nothing of special interest to record in the treatment of the fossil Carinatæ.

Throughout the foregoing comments we have endeavoured to find as much fault with the work as we could; the result being that our criticisms are mainly concerned either with small and unimportant points of systematic arrangement, or with slight errors for which the author is frequently not the responsible party. From personal experience we are fully aware of the enormous difficulty of steering clear of errors in a work of this description, and also of arriving at a satisfactory compromise in cases of a conflict of authorities. In both these respects Prof. v. Zittel has passed triumphantly through the ordeal; and his work may truly be said to be indispensable, not only to the palæontologist, but likewise to every student of zoology who desires to know something more of his subject than can be gleaned from the study of the animals of the present day. R. L.

THE SUNSHINE OF LONDON.

For some few years past sunshine recorders have been in operation at four stations situated in various parts of London; and in attempting to gain some idea as to the average duration of sunshine in the metropolis, one is met at the outset by the somewhat perplexing question as to which of these four is best calculated to yield a fair result. One recorder is placed in the heart of the City, at Bunhill Row; and exposed as it is to a maximum amount of smoke and fog there can be little hesitation in saying that its indications are, for the metropolis as a whole, greatly below the mark. Another is stationed somewhat more favourably at Westminster, on the roof of the Meteorological Office, but even there the influence of the surrounding chimneys is felt to a very serious extent, and many a fair winter's day has been known to pass without the registration of so much as a trace of bright sunshine. In a third instance the conditions are reversed, for at the Kew Observatory the air is almost as free from smoke and mist as it is in the open country, so that as a London record the sunshine instrument gives us too high a value. The fourth station appears, however, to be one which strikes a fairly even balance between the meteorological features of the City and those of the more open suburbs; for, although Greenwich is influenced to a greater extent by the impurities of London than Kew, it is sufficiently removed from the central parts of the metropolis to escape much of the fog and smoke which affect the recording instruments both in the City and at Westminster. From a careful examination, we are inclined to think that the Greenwich record supplies a very fair idea of the conditions which prevail over the metropolis as a whole; and as the observations of bright sunshine have now been made at the Royal Observatory for rather over fourteen years, sufficient material has accumulated for the deduction of average results. It is only such sunshine as is strong enough to burn the papers that is here dealt with.

The general results of an examination of the Greenwich records for the fourteen years 1877 to 1890 are given in the following table, which shows for each month, for each season, and for the entire year—firstly, the average number of hours of bright sunshine; secondly, the percentage of the possible amount; thirdly, the average number of hours per day; fourthly, the average amount of sunshine on the brightest day; and fifthly, the number of days on which no bright sunshine was registered. The spring season comprises the months of March, April, and May; the summer those of June, July, and August; the autumn those of September, October, and November; and the winter those of December, January, and February.

From an examination of the table we see that the sunniest month in the year is May, with a total of 179 hours, or 37 per cent. of the possible quantity. June, however, runs it very close with a total of 174 hours, or 35 per cent. of the possible; and, in fact, owing to the slight difference which exists between the length of the two months, the daily average (five hours and three-quarters) is the same in each. It is a somewhat singular fact that the sunniest individual month in the course of the whole fourteen years was neither May nor June, but July. In the July of 1887 the aggregate number of hours recorded was 277, or 56 per cent. of the possible quantity, the daily average being very nearly nine hours. In both May and June we may look for at least one day with brilliant sunshine continuing for about 13 hours, but on an average there are in each month three days on which the solar rays are altogether absent. As regards sunless days, July is the most highly favoured month, for on an average of fourteen years' observations there is then only one day that is continuously overcast. The finest day experienced in the course of the entire period was June 13, 1887, when no less than 15 hours of bright sunshine were recorded.

Months.	Average number of hours of bright sunshine.	Percentage of the possible amount.	Average number of hours of sunshine per day.	Average amount of sunshine on the brightest day.	Number of sunless days.
January	26	10	0·8	5·4	19
February	42	15	1·5	6·9	12
March	95	26	3·0	8·9	6
April	122	29	4·1	11·0	4
May	179	37	5·8	13·0	3
June	174	35	5·8	13·1	3
July	167	34	5·4	12·5	1
August	154	34	5·0	11·6	2
September	115	31	3·8	10·1	3
October	77	23	2·5	7·8	9
November	43	16	1·4	5·8	12
December	20	8	0·7	3·9	20
The Spring	396	31	4·3	13·2	12
The Summer	495	34	5·4	13·2	6
The Autumn	235	24	2·6	10·1	25
The Winter	89	11	1·0	7·0	50
The entire Year ...	1214	27	3·3	13·7	94

The dullest month of the year is December, with a total of only 20 hours of sunshine, or 8 per cent. of the possible, and with 20 sunless days. January is very little better, the total number of hours being then 26, or 10 per cent. of the possible, and with an average of 19 sunless days. In each of these winter months the daily average of sunshine is only about three-quarters of an hour, but after January the weather rapidly improves, February being twice as sunny as its predecessor, and March twice as sunny as February. The dullest month in the course of the whole fourteen years was last December, when the total duration of sunshine was less than two hours and a half, or about 1 per cent. of the possible amount. On 28 days there was a complete absence of sunshine, and of these 28 no fewer than 18 were consecutive. On the brightest December day we can hardly expect four hours of sunshine, but we may certainly look for more than we had last December, when the finest day produced less than an hour and a half.

Turning to the various seasons we find, as we might expect, that the maximum amount of sunshine is recorded in the summer, the average number of hours being 495, or 34 per cent. of the possible amount. The finest summer of the whole fourteen years was the Jubilee season of 1887, when there were 715 hours, or about 50 per cent. of the possible. The dullest summer of the entire series was that of the following year (1888), when the total number of hours was only 373, or 26 per cent. of the possible. The average number of sunless days in the summer months is only six, but in 1888 there were 16, or six more than in any other year of the series. The spring is of course far more sunny than the autumn, the total amount of sunshine in the former season being 396 hours, as against 235 in the latter. In the winter the aggregate amount is only 89 hours, or an average of about one hour per day. The number of sunless days advances from the sunniest to the dullest of the seasons in fairly approximate geometrical progression, being twice as great in spring as in summer, twice as great in autumn as in spring, and twice as great in winter as in autumn. In a London winter there are more days without sunshine than with it.

The values for the entire year show that the average number of hours of bright sunshine is 1214, or 27 per cent. of the possible amount. The largest number recorded in any year of the fourteen was in 1887, when there were 1407, while the smallest was in that notoriously dismal year 1879, when there were only 984. Taking the year through, the average daily amount of sunshine at Greenwich is little more than three hours and a quarter, or less than half the quantity possible on the shortest December day. Thus, if the sun were to shine all the year through for the same number of hours as the highest possible in mid winter, we should get twice as much bright weather as we actually experience; and the results of an average of fourteen years' observations show that there are 94 days out of the 365, or more than a fourth of the year, upon which the solar rays are either altogether absent or are too feeble to leave any mark on the recording instrument.

FREDK. J. BRODIE.

NOTES.

AT a meeting of the Academy of Sciences held in Paris on Monday, the 2nd inst., Mr. Archibald Geikie, F.R.S., Director-General of the Geological Survey of the United Kingdom, was elected Correspondent of the Institute of France.

ON March 1 a meeting was held in the large hall of the Berlin Rathhaus to do honour to the memory of Dr. Schliemann. The meeting was summoned by the Geographical, the Anthropological, and the Archæological Societies of Berlin. Profs. Virchow and Curtius were the chief speakers, and they eulogized the character and achievements of the famous explorer and archæologist.

THE office of Colonial Bacteriologist at the Cape of Good Hope, has been offered to Dr. Edington of the University of Edinburgh.

REPLYING to Mr. John Ellis in the House of Commons, on Tuesday, Mr. Plunket said that the official guide to the Royal Gardens at Kew, which was at present out of print, was under revision. During the last two years the changes consequent on the rearrangement of the collections had been so extensive, that it had been thought better to suspend the publication of the guide, as in such a period of transition it would mislead, and be a cause of disappointment. Now that the rearrangement of the collections was nearing completion, a new guide had been put in hand, and Mr. Thiselton Dyer hoped to have it ready by next summer.

WE regret to have to record the death of Mr. George Bertin, who was well known as an able and learned student of Assyriology. He died on February 18, in his forty-third year.

PROF. VICTOR HORSLEY will give a discourse at the Royal Institution on "Hydrophobia," on Friday, March 20, in place of Prof. W. E. Ayrton, who is unable at present to give his promised lecture on "Electric Meters, Motors, and Money Matters."

ON Friday last, Mr. Goschen received a numerous deputation representing the University Colleges of England, who presented a memorial in favour of the annual grant, which is now £15,000, being increased. The deputation was introduced by Mr. Chamberlain, who, with Mr. Mundella, Sir Henry Roscoe, and others, strongly supported the memorial. Mr. Goschen, in the course of his reply, said they would not expect him without consultation with his colleagues, or at once after the reception of a deputation such as that, to give any final answer, but he was disposed to admit that in many ways they had made out a strong case. At all events they had made out this case, that the Colleges had been doing an increased good work, and that they thought that with additional sums they would be able to widen still the area of their usefulness. One point had struck him while the discussion was going on—and he might some time discuss it with Mr. Chamberlain, who would be a good authority on the question—namely, whether, instead of relations being established between the Imperial Government and these Colleges, it would not be better that the tie should exist between the County Councils and the Colleges, which would stimulate local interest. That was a matter which was too wide for discussion then; but he threw out the idea, in case it should become at any time the subject of their future consideration. It might be that the County Councils would throw their hearts into this work, as he believed they were doing in many parts of the country into the question of technical and other education, and for his part he should see no objection to any course that would tend to increase local interest in these Colleges.

IN several journals attention has lately been called to the fact that for women there exists at the present moment at Bedford College the very facilities for study which University College and King's College propose to offer when the present scheme for the development of their scientific teaching is carried out. Bedford College, one of the earliest established for women only, was founded as long ago as 1849; and when the University of London admitted women to its degrees, Bedford College students were the first to graduate there, while of those women who have since taken degrees in arts and science, a large proportion have belonged to the same institution. A new wing has just been added to the College, so that four separate laboratories are now open daily for the use of women desirous of carrying on practical work in biology, botany, chemistry, or physics. Special advantages are provided for those who have any bent for higher work or for original research. As the public opening of the new wing has been unavoidably postponed, the Council of the College desire it to be known that since Easter, 1890, the new laboratories have been in full working order, and that they can now offer accommodation to a greatly increased number of women students.

AT a meeting of the Biological Society of Washington, on February 7, Mr. Charles D. Walcott, of the U.S. Geological Survey, announced the discovery of vertebrate life in the Lower Silurian (Ordovician) strata. He stated that "the remains were found in a sandstone resting on the pre-Palæozoic rocks of the eastern front of the Rocky Mountains, near Canon City, Colorado. They consist of an immense number of separate plates of placoganoid fishes, and many fragments of the calcified covering of the notochord of a form provisionally referred to the Elasmobranchii. The accompanying invertebrate fauna has the facies of the Trenton fauna of New York and the Mississippi valley. It extends into the superjacent limestone, and at a horizon 180 feet above the fish beds seventeen out of thirty-three species that have been distinguished are identical with species occurring in the Trenton limestone of Wisconsin and New York. Great interest centres about this discovery from the fact that we now have some of the ancestors of the great group of placoderm fishes which appear so suddenly at the close of the Upper Silurian and in the lower portion of the Devonian groups. It also carries the vertebrate fauna far back into the Silurian, and indicates that the differentiation between the invertebrate and vertebrate types probably occurred in Cambrian time." Mr. Walcott is preparing a full description of the stratigraphic section, mode of occurrence and character of the invertebrate and vertebrate faunas, for presentation at the meeting of the Geological Society of America, in August next.

A Society for the Study of the Flora of France has been instituted by M. Camus, of Paris, and M. Magnier, of Saint-Quentin (Aisne), somewhat on the lines of our Botanical Exchange Club. Its special object is the distribution of authentic specimens of rare and critical species, remarkable varieties, and hybrids.

The State of Dakota has established a Botanical Experiment Station at Fargo, under the charge of Mr. Henry L. Bolley, of the Indiana Experiment Station.

We learn from the *Botanical Gazette* that Dr. J. T. Rothrock had arranged a biological expedition to the West Indies and Yucatan, and was to spend the months of November, December, and January in those countries. The party was provided with an excellent ship with abundant storage room. Mr. A. S. Hitchcock was to accompany the expedition in the interests of the Missouri Botanical Garden.

The fifth Annual Report of the Ornithological Observation Stations in the kingdom of Saxony has been issued. It is edited by Dr. A. B. Meyer and Dr. F. Helm. The Report records the observations made in the year 1889 by 47 observers at 45 stations, on 209 species of birds.

Mr. G. J. Symons, F.R.S., writing to the *Times*, from 62 Camden Square, N., speaks of the extraordinary dryness of the month of February. He says:—"My observations here have been absolutely continuous for more than thirty years, and hitherto the driest February was that of 1863, when 0·31 inch fell. In 1891 we have less than one-thirtieth of that; we have only 0·01 inch. And if we examine all the other months of the whole thirty-three years we find that the driest was May 1885, with 0·26 inch. These two facts sufficiently indicate the exceptional character of the past month at this station. We had one slight sprinkle in the forenoon of February 7, immediately after one of those intense darknesses (arising from high fog) which are becoming so sadly more frequent in this wilderness of chimneys. It had been dark—actually darker than on a clear moonless night. Fine mist began to fall. I put some sheets of note paper in the garden for the rain to fall upon. The shower, if such it could be called, was over in an hour, and every drop had left its inky mark upon the paper. I inclose a portion that you may have one more proof of the need for drastic measures if London is to be clean enough to live in." Mr. Symons has received only one return from England exceeding 0·10 inch, and this was from the hills above Ullswater.

A Bill introduced into the House of Lords by Lord Stratheden and Campbell for the abatement of the smoke nuisance in London was read a second time on Monday, and afterwards referred to a Committee. On the same evening, in the House of Commons, Viscount Wolmer asked the First Lord of the Treasury whether, considering the serious injury to health, the disturbance of business, and the hardships inflicted on many of the poorest wage-earners of the metropolis by the curtailment of their working hours, caused by the increasing prevalence of fogs, Her Majesty's Government would consider the advisability of appointing a Royal Commission to examine and report how far the evil was one which could be mitigated by legislation. Mr. W. H. Smith replied:—"I have to assure my noble friend that Her Majesty's Government are, in common with other inhabitants of the metropolis, extremely sensible of the serious injury, disturbance, and hardship inflicted by the increasing prevalence of fog. They are, however, sceptical of the value of a Royal Commission to investigate the subject. It is notorious that the evil mainly arises from the smoke emitted by ordinary domestic fires, and the problem to be solved is, whether it is possible by legislation to prohibit and prevent the production of smoke in this way. A Bill was introduced into the House of Lords in 1887 with this object, and was referred, after a second reading, to a Select Committee, which took evidence that went to show that smoke could be prevented by the use of non-bituminous coal, by the substitution of coke or gas for coal for heating purposes, and possibly by the adoption of an improved grate, and by great care in the lighting and feeding of fires in the metropolis. The Bill was not proceeded with beyond the Committee stage of 1887. Another Bill has been introduced with the same object, and stands for second reading in the House of Lords this evening. If it should come down to this House, the noble lord will have an opportunity of considering whether it is possible by penalty on occupiers of houses and tenements to secure the object he has in view." Sir H. Tyler had a question on the subject of an inquiry into the various descriptions of coal used in the metropolis. Mr. W. H. Smith said the matter had already had attention from a Committee of the House of Lords, and he doubted whether it would be possible to carry it further.

Ciel et Terre of February 16 contains an article by A. Lancaster, of the Brussels Observatory, on the severe frosts of this century. He has examined the statistics with the view of finding some clue to the probable future weather, and finds: (1) that a cold winter has never been followed by a very hot summer; and (2) that in the great majority of cases the summer following a severe winter has been cold. The same opinion has been expressed by Humboldt in his *Cosmos*, and by others. M. Lancaster concludes from the comparisons he has made that there is a great probability of a cold summer this year, and that June and July particularly will have a low temperature. Further, that as every prolonged period of cold in summer coincides with rainy weather, we may expect a wet summer. In support of this opinion he quotes the fact that out of fifteen cold winters between 1833–90 all but two have been followed by wet summers.

The Government have chartered the steam yacht *Harlequin*, 160 tons, for fishing-experiments off the Irish coast. She will be in commission three months. She has a powerful electric search light, and her steam appliances include a winch capable of lifting a 7-ton trawl.

A telegram from Washington says it has been decided that the Arctic Expedition under the command of Civil Engineer Peary, of the United States Navy, will leave Inglefield Gulf, on the west coast of Greenland, and take a north-easterly course by sledges. The party will consist of picked men, and will start from New Bedford, Massachusetts, on May 1. The scientific and geographical Societies of the United States will defray the expenses.

The Journal of the Camera Club, vol. iv., Nos. 40–52, for the year 1890, contains many articles that will be read with interest by photographers, both amateur and professional. Among them we notice those on the density of negatives, on pin-holes, &c., by Captain Abney; limitations in the treatment of subjects, practical interpretation of the law of conjugate foci, by T. R. Dallmeyer; eclipse photography, by A. A. Common; photography by the light of the electric spark, by Lord Rayleigh. Besides these, there are many notes, reviews of books, and accounts of excursions made to various places of note for the purposes of photography, and several other items of photographic news. Illustrations of the new premises that are being specially built for the Club are given, showing plans of the various floors, and from them we see that ample provision is being made for the requirements of photography in the form of dark rooms, studios, enlarging rooms, and rooms for meeting. In this volume, also, is inserted a very pretty illustration, entitled "Twitch Burning," the print being by the Praphstone Company, Limited.

IT is commonly believed that the aboriginal Aino race, inhabiting the Island of Yezo in Northern Japan, is rapidly dwindling away before the advance of civilization. But, according to the information lately published in Japan, this popular belief does not appear to be altogether corroborated by facts. Under the old Japanese *régime*, the population of Yezo emigrants from the other Japanese islands was estimated at 40,000, but in consequence of protection and encouragement given by the reformed Government, this number increased to 350,000 in 1888. As a natural consequence, the Ainos' means of sustenance were encroached upon, and they have been popularly believed to be going down before the advance of the dominant race. But the latest statistics show that the truth is far from the common hypothesis, and that this curious and interesting race is not dying out, as has been supposed. The exact figures are as follows :—

Aino Population in Yezo.

Year.	Male.	Female.	Total.
1872	7964	7311	15,275
1873	8167	8032	16,199
1874	8171	8160	16,331
1875	8547	8583	17,130
1876	8579	8598	17,177
1877	8483	8483	16 966
1878	8537	8521	17,058
1879	8513	8515	17,028
1880	8566	8575	17,141
1881	8476	8457	16,933
1882	8546	8652	17,198
1883	8617	8615	17,232
1884	8702	8770	17,472
1885	8635	8687	17,322
1886	8464	8571	17,035
1887	8437	8525	16,962
1888	8475	8587	17,062

MESSRS. CASSELL have published the first part of what promises to be a book of considerable value. The work is entitled "Our Own Country," and will present, with illustrations, a geographical and historical description of the chief places of interest in Great Britain and Ireland. The same publishers have just issued the first part of a work on "Familiar Trees," by G. S. Boulger, with 80 coloured plates ; and Part 29 of their excellent "New Popular Educator."

A WORK entitled "Memorials of John Gunn" will shortly be issued. It will be edited by Mr. Horace B. Woodward, and will contain notes by the late Mr. Gunn, presenting some account of the Cromer forest bed and its fossil Mammalia, and of the associated strata in the cliffs of Norfolk and Suffolk. Mr. Gunn, while acting as a country clergyman, had for many years devoted a large share of his attention to the geology of Norfolk. The "Memorials" will be accompanied by a portrait and biographical sketch of the author.

THE following lectures will be given at the Royal Victoria Hall : March 10, Mr. J. E. Flower on "Spain" ; 17th, Mr. A. W. Clayden on "Thunderstorms" ; 24th, Prof. Roger Smith on "St. Paul's."

PROF. SEUBERT contributes an important paper to the current number of *Liebig's Annalen*, in which are presented the final results of his redetermination of the atomic weight of osmium. A preliminary account of the earlier portion of this work was published in the *Berichte* in June 1888, and a short notice concerning it was given in these columns (vol. xxxviii. p. 183). It was then shown that the atomic weight of osmium was certainly not higher than 191, and was probably a few decimals less. Owing, however, to lack of material, Prof. Seubert was not able to complete the work in the unimpeachable manner characteristic of his other atomic weight determinations. Since that time, however, thanks to the kindness of Prof. Lothar Meyer, a sufficient quantity of pure osmium has been placed at his disposal, and the work has been completed in a manner which leaves nothing to be desired. The salts analyzed were potassium and ammonium osmium chloride, K_2OsCl_6 and $(NH_4)_2OsCl_6$. The final mean value derived from all the experiments is 190·3, a number which fully justifies the expectations of Prof. Seubert that it would fall slightly below 191. The importance of the settlement of this question cannot be over-rated, for it removes the last outstanding exception to the periodic generalization. The metals of the platinum group—osmium, iridium, platinum, and gold—when arranged in the order of their chemical and physical properties unmistakably take the relative precedence just quoted. If these properties are, as everyone now agrees, periodic functions of atomic weight, the atomic weights of these metals should increase from that of osmium upwards to that of gold. Previous to the year 1878, however, the accepted atomic weights were : gold 196·2, iridium 196·7, platinum 196·7, and osmium 198·6—a relation which, if correct, was diametrically opposed to the principle of periodicity. In that year Seubert attacked the subject, and the first outcome of his labours was to correct the atomic weight of iridium, which he found to be 192·5, instead of 196·7. It was a most remarkable tribute to the accuracy of Seubert's work, and likewise of his own, that Joly a short time ago obtained for the same constant the identical number, 192·5. In 1881, Seubert took up the case of platinum, and finally adjusted its atomic weight to 194·3—a number which was confirmed by a subsequent determination of Halberstadt. In 1887 the position of gold was finally decided by the remarkably agreeing and almost simultaneous determinations of Thorpe and Laurie on the one hand, and Krüss on the other, the value arrived at in both cases being practically 196·7. Finally, we have the just completed work of Seubert upon osmium ; and the four metals, when arranged in order of atomic weight, now take the order : osmium 190·3, iridium, 192·5, platinum 194·3, gold 196·7—an order of precedence in full accord with the order of their chemical and physical properties.

THE additions to the Zoological Society's Gardens during the past week include a Red-throated Diver (*Colymbus septentrionalis*), British, presented by Mr. E. J. Gale ; a Herring Gull (*Larus argentatus*), British, presented by the Rev. C. A. Berry ; twelve Grayling (*Thymallus vulgaris*) from British fresh-waters, presented by Messrs. Howard L. Cooper and — Jukes ; a Wapiti Deer (*Cervus canadensis* ♂) from North America, a Long-tailed Weaver Bird (*Chera progne*) from South Africa, two Chinese White-Eyes (*Zosterops simplex*) from China, deposited ; four Upland Geese (*Bernicla magellanica* ♂ ♂ ♀ ♀) from the Falkland Islands, purchased ; a New Zealand Parrakeet (*Cyanorhamphus novæ-zealandiæ*) from New Zealand, two Wonga-Wonga Pigeons (*Leucosarcia picata*) from New South Wales, received in exchange ; a Gayal (*Bibos frontalis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE PERMANENCE OF MARKINGS ON VENUS.—In *Bulletin No. 12 de l'Académie Royale des Sciences de Belgique*, Dr. Terby contributes a paper entitled, "Facts demonstrating the permanence of the dark spots on Venus, and the slowness of their motion of rotation." Dr. Terby made a series of observations of Venus between April and August 1887, and sent some of the results, in a sealed packet, to the Academy in the same year. A similar series of observations was made by M. Perrotin from May to September 1890, and the results were communicated to the Paris Academy (*Comptes rendus*, October 27, 1890). The object of the present paper is to call attention to the fact that the drawing made by M. Perrotin in 1890 agrees in every respect with those made by Dr. Terby in 1887. Each observer made drawings of two types of markings, and in each case the change from one type to the other took place about two months after the first observation. A comparison shows that Venus occupied very nearly the same position in her orbit during the time that the drawings were made by the two observers. The earth's position was also little different in both cases. This agreement between independent observers seems therefore to justify the following conclusions:—

(1) The observations were made on the same portion of the planet's surface.

(2) Venus, in travelling over the same part of her orbit, after an interval of three years, or after five complete revolutions, presented the same part of her surface to the same part of the heavens during the respective periods of observation.

(3) Therefore the planet at these two epochs also presented very nearly the same face to the sun.

(4) Venus has therefore a very slow rotational motion, as has been suggested by Signor Schiaparelli, and confirmed by M. Perrotin.

(5) There exist not only light spots on the planet's surface, but also dark ones having a fixed character similar to those on Mars; these spots are fixed enough to be recognized after an interval of three years, but are difficult to see, on account of their undefined edges, due, probably, to a dense atmosphere full of clouds. Up to now, the permanence of the dark spots was much questioned; these spots, however, do not appear to resemble those drawn by Bianchini and De Vico.

Observations made by various other observers are quoted in support of these conclusions, and in favour of the probability that the time of revolution is equal to that of rotation. A plate containing forty-three drawings of Venus accompanies the paper.

Observations of Mars.—The above-named *Bulletin* contains also some observations of Mars made by M. J. Guillaume last year, by means of a With's reflector about 9 inches in aperture, and a power of about 195. Twenty-five drawings of the planet are given, and with each the times of observation and the longitude of the central meridian calculated from the ephemerides furnished by Mr. Marth. This facilitates the comparison of the drawings with those made by other observers. In spite of the bad atmospheric conditions under which M. Guillaume, like many other astronomers, has had to labour, he has had the good fortune to see a certain number of canals, and gives a drawing of their gemination. The variations in the appearance of some of the seas observed on different occasions leads to the conviction that they can only be explained by the presence of clouds in the Martian atmosphere.

New Variable.—*Wolsingham Observatory Circular*, No. 29, reads:—A star, 8·5 mag., red, third-type spectrum, not in the D.M., was observed here on January 31 and February 1 and 2, in R.A. 5h. 32m. 37s., Decl. + 31° 57′ (1855). The star is perhaps variable.

New Asteroid.—M. Charlois, of Nice Observatory, discovered another asteroid on February 16. Its designation will be (306); its magnitude is 11·5.

THE MAMMALIAN NERVOUS SYSTEM.[1]

1. *Introduction.*

In the Proceedings of the Royal Society, No. 273, November 1888 (vol. xlv., 1889, p. 18), we published a preliminary account of some of the experiments of which the results are given in detail in our full paper.

In that communication we stated that the object of our work then was to endeavour to ascertain the character of the excitatory processes occurring in nerve-fibres when either directly, *i.e.* artificially excited, or when in that state of functional activity which is due to the passages of impulses along them from the central apparatus. The most important way in which such a method could be applied was, obviously, one which would involve the investigation of the excitatory changes occurring in the fibres of the spinal cord when the cortex cerebri is stimulated. We must at once assume that the motor side of the central nervous system is practically divisible into three elements:—(1) *Cortical centres.* (2) *Efferent* (pyramidal tract) *fibres*, leading down through the internal capsule, corona radiata, and spinal cord. (3) *Bulbo-spinal centres* contained in the medulla and the spinal cord, and forming the well-known nuclei of the cranial and also of the spinal motor nerves.

It had already been determined, both by direct observation and by the graphic method, (1) that certain areas of the cortex were connected with definite movements of various parts of the body, and (2) that while the complete discharge of the cortical apparatus was followed by a very definite and characteristic series of contractions of the muscles in special relation with the particular point excited, the effectual removal of the cortical central mechanism and subsequent excitation of the white fibres passing down through the internal capsule, &c., led to the production of only a portion of the effect previously obtained from the uninjured brain.

This method of observation in no wise showed what processes were actually occurring in the spinal and other nerve-fibres, and although the ablation of the cortical centre to a certain degree suggested the extent to which the cortex acted, nevertheless it did not afford an exact demonstration of the same. Moreover, the data which the graphic method furnished were precluded, through their being muscular records, from determining what share, if any, the lower bulbo-spinal central nerve-cells took, either in the production of the characteristic sequence of contractions, or in the modification, whether in quality or in force, of the descending nerve-impulses during their transit. It seemed to us that the only way to approach this subject would be to get, as it were, between the cortex and the bulbo-spinal system of centres. This would be accomplished if some means were devised of ascertaining the character of the excitatory processes occurring in the spinal fibres of the pyramidal tract when, upon excitation of the cortex, nervous impulses were discharged from cortical cells, and travelled down the cord.

The question as to the extent to which it is possible to obtain physical evidence of the actual presence in nerve-fibres of excitatory processes, and thus to arrive at trustworthy data for the comparison of their amounts, is one which, up to the present, has been answered only indirectly, and that in two ways: first, by the extension of Helmholtz's classical experiment of determining the rate of transmission; and secondly, by observing those variations in the electrical state of nerve-fibres which Du Bois-Reymond discovered to be an invariable concomitant of the excitatory state. As will subsequently be shown in the historical retrospect, it is well known, through the researches of Du Bois-Reymond and others, that the fibres of the spinal cord, just as nerve-fibres in the peripheral trunks, are characterized by showing, when unexcited, an electrical difference between their longitudinal surface and cross-sections; and, furthermore, that when excited, a well-marked diminution of this resisting electrical state is produced in the fibres of the cord, as in those of nerve-trunks. Now, since such excitatory variations in the electrical state are presumably parallel in time and amount with the presence in the nerve of the series of unknown processes, termed excitatory, which a series of stimuli evokes, it was reasonable to presume that, if the cortex were discharging a series of nerve-impulses at a certain rate down the pyramidal tract, there would be a series of parallel changes in the electrical condition of the fibres in the cord tract, and that, with a suitable apparatus for responding to such changes, these might be both ascertained and recorded. The accomplishment of a further purpose, viz. the localization of both paths and centres by ascertaining the excitatory electrical effects in relation with them, was one of the main objects we had in view. In carrying it out, we found it was unnecessary to employ the electrometer, and, in fact, that it was advantageous to use the galvanometer, the record of which would be more easily and more accurately noted, since its graduation admits of far higher magnification. Moreover, with this instrument it was possible, by employing a series of stimuli, of known number and duration, to obtain quantitative results of definite comparative value, as will be shown further on; and thus, to compare both the size of different central paths and the amount of nervous energy discharged along the same path from different sources.

The plan upon which the full paper is framed is, first, to give an historical retrospect of the work of authors who have opened up the study of electrical changes in the central and peripheral nervous system; second, to describe at length our mode of experimentation, with special reference to the modifications which we have introduced; then to compare roughly

[1] "On the Mammalian Nervous System; its Functions and their Localization determined by an Electrical Method." By Francis Gotch, Hon. M.A., Oxford, and Victor Horsley, F.R.S. (From the Physiological Laboratory of the University of Oxford.) Croonian Lecture read before the Royal Society on February 26.

the results we have obtained by our present method with those which had been previously ascertained by the graphic method, and so introduce the description of the facts which we have discovered, elucidating the physiology of the spinal cord, both in its relation to the higher centres, and to the peripheral nerves.

2. *Experimental Procedure.*

The observations were in all cases made on etherized animals (cat and monkey), with due regard to the special influence of the anæsthetic. The operative procedure was so designed as to provide for suitable exposure of a particular region of the nervous system for excitation, and of another part in which the electromotive changes evoked by the stimulation may be observed. The relative parts were as follows:—

Part exposed for excitation.	Part exposed for observation.
Brain (cortex and corona radiata) ...	and spinal cord.
,, ,, ,, ...	and mixed nerve.
Spinal cord	and spinal cord.
,, ...	and mixed nerve.
Mixed nerve...	and spinal cord.
Spinal roots	,, ,,
Posterior roots	and mixed nerve.

The excitation was either electrical, chemical (*i.e.* with absinthe and strychnine), or mechanical. In the former instance the duration and intensity were specially determined. The records were made by a Thomson high-resistance reflecting galvanometer and a Lippmann's mercurial capillary electrometer.

The tissue, whether nerve or spinal cord, was so arranged for observation as to be always suspended in the air, one end remaining in connection with the animal; consequently any error due to current derivations from the rest of the body could only have a slight and unipolar effect.

3. *Resting Electrical Difference between the Cut Surface of the Tissue and its Uninjured Longitudinal Surface.*

The average amounts of this difference in the tissues observed were as follows:—

Cat.		Monkey.	
Nerve (69 cases),	0·01 Daniell ...	(12 cases),	0·005 Daniell.
Root (5 cases),	0·025 ,, ...		—
Cord (50 cases),	0·032 ,, ...	(9 cases),	0·022 ,,

We have observed that the cord difference is greater when that tissue is in connection with the higher centres, and that it rises after each excitation. An important fall of the difference is to be remarked in all three tissues as a direct result of systemic death.

4. *Electrical Changes in the Spinal Cord evoked by Excitation of the Cortex Cerebri and Corona Radiata.*

We further discuss in our full paper the following points additional to those described in our previous communication, and which have resulted from the observation of the above changes:—

(*a*) Localization of cortical areas of representation in relation to the various regions of the cord.

(*b*) Bilaterality of representation in the central nervous system, as evidenced by the electrical changes in the two halves of the spinal cord, consequent upon excitation of the brain or cord.

5. *Electrical Changes in the Spinal Cord when evoked by Direct Excitation of its Fibres, after Severance from the Encephalon.*

We have by employment of this method ascertained the proportionate existence of direct channels in the various columns of the spinal cord, our design embracing the quantitative comparison of the electrical changes (and so indirectly of the nerve-impulses) which are transmitted as a result of minimal excitation of the fibres. To further control our observations on these points, we have also determined the extent of interruption in any given channel by intervening sections of the same.

As an extension of this subject, we have investigated the concurrent spread of nervous impulses to collateral paths, and probably to centres, when this further condition is introduced by increase in the stimulus.

The above results have been obtained in the case of both ascending and descending impulses.

Among other general conclusions from this division of our research are the following:—

(1) High degree of unilaterality of representation in the spinal cord.

(2) Spread of impulses from one posterior column to another and from one posterior column to its neighbouring lateral column through centres.

6. *The Relation of the Paths and of the Bulbo-Spinal Centres in the Spinal Cord to the Peripheral Nerves and their Roots.*

We have investigated this important relationship in the following modes:—

(I.) *The Electrical Changes in the Spinal Cord evoked by Excitation of a Mixed Nerve or its Roots.*—The chief conclusions which have been deduced from the results of these experiments, by means of minimal excitation and the employment of the method of blocking by intervening sections, include the following:—

(1) Complete obstruction offered to centripetal impulses reaching the cord by the central end of the anterior root.

(2) Mode of conduction, direct and indirect, in the cord of centripetal impulses passing up the posterior root.

(3) Localization of the direct path of afferent impulses in the posterior column of the same side as that of the nerve or root excited.

(4) Localization of the indirect path of afferent impulses in the posterior columns of the same and the opposite side and the lateral column of the same side as that of the nerve excited.

(5) Proportionate development of both systems of paths in the two sides of the cord.

Expressed in percentages of the total transmission, this proportion is as follows:—

Posterior column of same side as the excited nerve ...	60	p.c.
Lateral column of same side as the excited nerve ...	20	,,
Posterior column of opposite side to the excited nerve	15	,,
Lateral column of opposite side to the excited nerve...	5	,,

(II.) *The Electrical Changes in a Mixed Nerve or its Roots evoked by Excitation of the Spinal Cord.*—Whereas in the foregoing series (I.) we dealt only with ascending impulses, we proceeded to investigate the distribution of descending impulses by observing the above-named changes when the individual columns of the cord are excited by minimal and later with more intense stimuli, controlling our results by the method of intervening sections.

We can summarize the effects observed as follows:—

(1) Marked quantitative diminution suffered by impulses leaving the spinal cord by the anterior roots, whether originating in the cortex cerebri, corona radiata, or the lateral columns of the cord.

(2) Localization of direct transmission of impulses in the posterior column and passing out into the posterior roots of the same side.

(3) Proportionate development of the direct and indirect paths in the individual columns of the cord, passing out into the mixed nerve of the one side.

(4) Effects observed in the posterior roots when the bulbo-spinal centres are excited either by strychnine or electrically (kinæsthesis).

Finally, the chief general principles elucidated by this research may be stated as follows:—

(1) Unilateral character of the representation of function in the paths of the central nervous system.

(2) The physiological characteristics of the regions of a nerve centre:—

(*a*) The kinæsthetic activity of the afferent region of the centre.

(*b*) The obstruction offered by the efferent region, including the field of conjunction, to the transmission of impulses through the centre.

THE CITY AND GUILDS OF LONDON INSTITUTE.

An important memorandum on the examination, inspection, and payment of grants in aid of local technological and trade classes has been issued by the City and Guilds of London Institute to County Councils and municipal bodies. So many changes are likely to be effected by means of the funds which have lately been granted to local authorities for the promotion of technical education, that it was necessary for the Institute to consider the various ways in which it might hope to meet most effectually the altered conditions. In the following statement it has summed up clearly the main facts of the situation :—

(1) Recent legislation has placed funds at the disposal of County Councils for the purposes of technical education, and has imposed on local authorities the responsibility of determining the best means of utilizing those funds in accordance with the provisions of the Technical Instruction Act.

(2) Whilst the Science and Art Department pays grants on results on all subjects of instruction included in its Directory, no such payments are made by the Department on the technological and trade subjects included in the Institute's programme. Hitherto, grants on such subjects have been paid by the Institute, but these grants will be discontinued after the year 1892.

(3) The Institute is prepared to continue, and to defray the cost of its present system of technological examinations, and to improve it from time to time—

(*a*) By the addition of further practical tests.

(*b*) By adapting the examinations to a still greater extent to local requirements and to the different sections into which many trades are now divided.

(*c*) By the addition of new subjects of examination.

(4) The administration of the present system of examinations involves—

(*a*) The organization of technical classes adapted to different trades.

(*b*) The selection and appointment of competent examiners and local superintendents.

(*c*) Inquiry into the qualifications, and the registration, of teachers.

(*d*) The preparation of syllabuses of instruction and examination.

(*e*) The recommendation, of books of reference, for the use of students and for the formation of technical libraries in connection with separate trades.

(*f*) The examination of registers of attendance of students at classes, and of certificates as to their occupations.

(*g*) The award of different grades of certificates, of silver and bronze medals, and of the money prizes, provided by the separate Livery Companies of London.

(5) The present system of examinations would be further improved by the inspection of classes by competent experts in different branches of trade, and by other persons possessing the necessary scientific knowledge and educational experience. Information might thus be obtained as to the efficiency of the teaching, the provision made for practical instruction in the laboratory or workshop, and the adequacy of the equipment as regards apparatus, models, and machinery. The Institute has under consideration the expediency of organizing such a system of inspection.

(6) The Institute would be prepared to submit to County and Borough Councils co-operating with it, reports on the attendance of students at technological classes, and on the results of the examination of such students, which would serve to guide County Councils in making grants in aid of the maintenance of such classes.

(7) It is suggested that the grants in aid might be of two kinds :—

(*a*) Capitation grants on students in regular attendance at classes certified by the Institute.

(*b*) Grants on the results of the Institute's technological examinations.

(8) No grants have been hitherto given under section (*a*), although the want of such assistance has been generally felt and fully recognized. The grants under section (*b*), given by the Institute, consist of £2, and £1, on behalf of every candidate who takes a first-class or second-class certificate, and is actually engaged in the trade connected with the subject of examination.

(9) The Institute possesses the administrative machinery required for furnishing local authorities with the necessary detailed reports for the award of grants under each of the above sections, and is prepared to place its technical and educational experience at the service of County and Borough Councils for the purpose hereinbefore indicated.

(10) The Institute would be further prepared to make arrangements with County or Borough Councils co-operating with it, for submitting reports on the general efficiency of technological classes, and for affording assistance in the organization of new schools or classes for the promotion of technical education in connection with local trades and industries.

These suggestions are made with the view of indicating the position which the Institute has occupied during the last twelve years, and that which it is prepared to occupy in future, in relation to technical education throughout the country, and of enabling County and Borough Councils, if they so desire, to avail themselves of the assistance thus offered in improving, and in subsidizing, by the payment of grants, the technological and trade classes now established in the chief centres of industry, many of which, it is feared, without such help, will be unable to continue to exist.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—Sir Alfred C. Lyall, K.C.B., K.C.I.E., has been appointed Rede Lecturer. His subject is "Natural Religion in India."

Prof. Roy announces for the Long Vacation a course of instruction in "Bacteriology," suitable for candidates for the University diploma in public health. Dr. A. Gamgee delivers in this term and the next a course in pathological chemistry.

The Special Board for Medicine have issued revised schedules defining the range of the examinations in physics and chemistry for the M.B. degree. Johns Hopkins University is recognized by the Board as a school of medicine.

SOCIETIES AND ACADEMIES.

London.

Royal Society, February 12.—"On the Demonstration by Staining of the Pathogenic Fungus of Malaria, its Artificial Cultivation, and the Results of Inoculation of the Same." By Surgeon J. Fenton Evans, M.B. Communicated by Prof. Victor Horsley, F.R.S. (From the Laboratory of the Brown Institution.)

The discovery of organisms constantly concomitant with manifestations of malaria was made by Laveran in 1880.

His researches have since been corroborated and amplified by numerous observers in different parts of the world, among whom must be mentioned, Marchiafava Celli, Golgi, and Guarnieri, in Italy ; Councilman, Osler, and James, in America : and Vandyke Carter, in India. The foreign structures which all of the above-named investigators agree in finding in the blood during or after attacks of ague may be grouped into the following classes :—

(1) "Cystic" bodies or spores, 2 to 11 μ in diameter, round, transparent, encapsuled bodies of variable dimensions.

(2) Crescentic bodies, 8 to 9 μ long and 3 μ broad.

(3) Plasmodia malariæ, organisms as variable in size as the "cystic" bodies or spores, possessing the power of amœboid movement, and so closely associated with the red blood corpuscle that hitherto the majority of observers have considered them to be parasites situated within the red blood cells.

(4) Mobile filaments, 21 to 28 μ long.

Despite the general concord of the observations, the subject has not advanced beyond the stage of recognition of these structures in the blood, and that, too, only while in the fresh state.

No method had hitherto been discovered of preparing permanently stained specimens of the organism.

It had never been isolated or classified, nor when thus separated had its pathogenic qualities ever been tested by experiments on lower animals.

It was thus clear that much remained to be done, and in the paper are recounted the attempts made to place the subject on a satisfactory footing. The author has found that it is possible to stain the organisms with an anilinized alkalized solution of rosanilin hydrochloride after treatment with bichromate of potash, and after treatment with dilute sulphuric acid by an anilinized alkalized solution of Weigert's acid fuchsin.

Another method of staining consisted in the saturation of the tissue with a copper salt and its reduction by sulphuretted hydrogen previous to coloration with anilinized alkalized acid fuchsin.

By these staining methods the organisms have been demonstrated in the blood, and also in the tissues. And some new, hitherto unrecognized features are described, among which may be mentioned what appears to be the germination of the spore in the blood, the existence of a comma-shaped body and of mycelium in the spleen and Peyer's glands, and the localization of the plasmode, *i.e.* in relation to the blood corpuscles.

The isolation of the organism and its artificial cultivation have been successfully carried out, and it is shown that this result entirely depends for its success upon the fact that the nutrient media must be previously treated with living blood, *i.e.* before *rigor mortis* has set in.

Alteration in the chemical composition of the nutrient medium, consisting in the addition of glucose, together with iron or hæmoglobin or fresh blood, to the non-peptonized beef broth, elicited the interesting fact that, under these circumstances, the organism can pass to a more highly developed state, displaying the structure and fructification of a highly organized fungus, but differing in certain important features from any fungus hitherto described.

Inoculation of guinea-pigs, monkeys, and rabbits with the growths in various nutrient media has produced a frequently fatal disease, which, although not characterized in these animals by the symptoms of classical intermittent fever, yet displayed in a number of instances a definitely intermittent character. It was further, whatever its clinical character, invariably accompanied by the appearance of the characteristic organisms in the blood drawn after death from the right ventricle.

It is accordingly concluded that the malarial fungus is capable of being cultivated outside the body, and has been proved to possess pathogenic qualities.

Zoological Society, February 17.—Prof. Flower, F.R.S., President, in the chair.—Mr. Edward Gerrard, Jun., exhibited an extraordinarily large head of a Koodoo Antelope (*Strepsiceros kudu*), which had been shot by Mr. F. C. Selous, near the river Macloutsie, Khama's Country, South Africa, in May last.—Mr. T. D. A. Cockerell exhibited and made remarks on a curious and rather noteworthy monstrosity of a Land-shell (*Clausilia rugosa*) with two apertures.—Mr. G. A. Boulenger exhibited and made remarks on the renewed left pectoral fin of an African Lepidosiren (*Protopterus annectens*) from a living specimen in the Society's Gardens.—Mr. Boulenger also exhibited young specimens and eggs of a South African Siluroid fish (*Galeichthys feliceps*), sent to him by Mr. J. M. Leslie, of Port Elizabeth. They had been taken from the mouth of the male parent, which carries its eggs in this extraordinary manner.—Prof. G. B. Howes read a paper on the probable existence of a Jacobson's organ among the Crocodilia, and made observations upon the skeleton of that organ in the Mammalia, and upon the basimandibular elements in the Vertebrata.—Mr. R. H. Burne made some observations on the variation and development of the Leporine sternum.—Mr. Scott B. Wilson read a paper on *Chasiempis*—a genus of Muscicapine birds peculiar to the Sandwich Islands. He described one of the species inhabiting the island of Oahu as new, and named it *Chasiempis gayi*, after Mr. F. Gay, of Kauai. The author further gave a key by which the five species of this genus inhabiting the various islands may be distinguished.—Mr. Wilson also read the description of a new bird of the genus *Himatione*—based on a single specimen obtained on the island of Maui—naming it *Himatione dolii*, after Mr. S. B. Dole, of Honolulu.—Mr. G. A. Boulenger read a paper on some British specimens of the remains of *Homœosaurus*, and made remarks on the classification of the Rhynchocephalia.—Mr. F. E. Beddard read a preliminary account of an Earthworm from West Africa, referable to a new genus and species, which he proposed to call *Libyodrilus violaceus*.—Mr. Frank Finn gave an account of a functional *ductus botalli* which he had observed in specimens of two birds (*Nycticorax violaceus* and *Dafila spinicauda*) dissected in the Society's Laboratory.

Royal Meteorological Society, February 18.—Dr. C. T. Williams, Vice-President, in the chair.—The following papers were read:—The great frost of 1890–91, by Mr. C. Harding. This paper dealt with the whole period of the frost from November 25 to January 22, and it was shown that over nearly the whole of the south-east of England the mean temperature for the 59 days was more than 2° below the freezing-point, whilst at seaside stations on the coast of Kent, Sussex, and Hampshire, the mean was only 32°. In the extreme north of Scotland as well as in the west of Ireland, the mean was 10° warmer than in the south-east of England. In the southern midlands and in parts of the south of England the mean temperature for the 59 days was more than 10° below the average, but in the north of England the deficiency did not amount to 5°, and in the extreme north of Scotland, it was less than 1°. The lowest authentic reading in the screen was 0°·6 at Stokesay, in Shropshire, but almost equally low temperatures occurred at other periods of the frost. At many places in the south and south-west of England as well as in parts of Scotland and Ireland, the greatest cold throughout the period occurred at the end of November; and at Waddon, in Surrey, the thermometer in the screen fell to 1°, a reading quite unprecedented at the close of the autumn. At Addington Hills, near Croydon, the shade thermometer was below the freezing-point each night, with one exception, and there were only two exceptions at Cambridge and Reading; whilst in the Shetlands there were only 9 nights with frost, although at Biarritz frost occurred on 31 nights, and at Rome on 6 nights. At many places in England the frost was continuous night and day for 25 days, but at coast stations in the north of Scotland it in no case lasted throughout the 24 hours. On the coast of Sussex the temperature of the sea was about 14° warmer than the air throughout December, but on the Yorkshire coast it was only 6° warmer, and in the Shetlands and on parts of the Irish coast it was only 3° warmer. The Thames water off Deptford, at 2 feet below the surface, was continuously below 34° from December 23 to January 23, a period of 32 days, whilst the river was blocked with ice during the greater part of this time. In Regent's Park, where skating continued uninterruptedly for 43 days, the ice attained the thickness of over 9 inches. The frost did not penetrate to the depth of 2 feet below the surface of the ground in any part of England, but in many places, especially in the south and east, the ground was frozen for several days at the depth of 1 foot, and at 6 inches it was frozen for upwards of a month. In the neighbourhood of London the cold was more prolonged that in any previous frost during the last 100 years, the next longest spell being 52 days in the winter of 1794–95, whilst in 1838 frost lasted for 50 days, and in 1788–89 for 49 days.—The problem of probable error as applied to meteorology, by Mr. T. W. Backhouse.

EDINBURGH.

Royal Society, February 2.—Sir Douglas Maclagan, President, in the chair.—Prof. W. Rutherford, by request of the Council of the Society, gave an address on the sense of hearing. He criticized Helmholtz's theory of the manner in which the cochlea is affected by sound-vibrations, and showed the great anatomical difficulties of any theory that regards the basilar membrane as the transmitter of sound-vibrations to Corti's cells, and as an analyst of complex sound-vibrations. The basilar membrane is heavily damped by the cellular elements above and beneath it; and, in the case of the rabbit, by its division into two layers with a homogeneous tissue between them, and also beneath the lower layer—an arrangement that must greatly interfere with any localized sympathetic vibration of its fibres. The hair cells of Corti are the true nerve terminations, and are placed in a favourable position for receiving the sound-waves transmitted to them through the superjacent membrane of Corti. The sound-wave is probably considerably damped, and fine vibrations therefore destroyed, by the granular protoplasm at the lower ends of Corti's cells and the granular cement substance between their lower ends and the supporting cells of Deiters. He stated the arguments opposed to the theory of Helmholtz, and explained his own theory of sound-sensation suggested by the telephone, and communicated by him to the British Association in 1885. Although no theory is free from difficulty, he maintained that the most feasible theory is that the great extension of the organ of Corti in the mammal is for the purpose of increasing the number of hair cells, so that the appreciation of qualities of sound may be more acute, and therefore more intelligent; that the hairs of all the cells of Corti are affected by tones of every pitch, and that the sound-vibrations are translated by Corti's cells into nerve-vibrations corresponding in frequency, amplitude, and form to those of the sound; and that the different sensations of tone are due to nerve-vibrations of different frequency and form, periodic or aperiodic, arriving in the sensorium. He illustrated his address by numerous diagrams and experiments, and was awarded at the close, on the motion of Sir William Turner, a special vote of thanks by the Society.

PARIS.

Academy of Sciences, February 23.—M. Duchartre in the chair.—M. Fremy, in presenting his recently published work on the synthesis of rubies, remarked that he had been able to obtain numerous rhombohedric crystals identical with those found in nature. This result has been obtained, after many trials, by calcining a mixture of aluminium, red lead, and potassium bichromate for several hours in an earthenware crucible.—M. Chauveau presented a work having the title "Le Travail musculaire et l'énergie qu'il représente."—On coloured interference rings, by M. Mascart.—On the isolation of the glycolytic ferment of the blood, by MM. Lépine and Barral.—On the spectrum of α Lyræ, by M. H. Deslandres. Some photographs of the spectrum of α Lyræ were taken at Paris on the same dates as those taken by Mr. Fowler (*Monthly Notices R.A.S.*, December 1890). M. Deslandres's negatives, however, show K as a single, and not as a double line.—Observations of two new asteroids discovered at Nice Observatory on February 11 and 16, by M. Charlois. The magnitudes of the asteroids are 11·5 and 12 respectively.—Observations of the asteroid discovered by Charlois on February 11, made with the Brunner equatorial of Toulouse Observatory, by M. B. Baillaud. Observations for position were made on February 16 and 18.—Observations of sun-spots, made in 1889 and 1890 with the Brunner equatorial (0·18 metres aperture) of Lyons Observatory, by M. Em. Marchand. The following conclusions are deduced from the observations:—(1) The monthly numbers of groups did not vary much from January 1889 to January 1890; they increased from February 1890, this year presenting a total of 38 groups more than the previous one. (2) The total spotted surface per month varied little from January to August 1889; it then began to diminish, and passed a well-marked minimum in November of the same year. It afterwards increased more or less regularly up to the end of 1890, and in this year presented a total spotted surface of 103·3 thousandths of the area of the visible hemisphere, against 73·4 thousandths in 1889. (3) These facts place the minimum of solar activity in November 1889, a result in conformity with the absence of spots from October 10 to December 4 of the same year. (4) The distribution in latitude of the regions of activity changed completely about the time of minimum activity. Whilst at the beginning of 1889 spots were most frequent in the zone $-10°$ to $+10°$, in 1890 the maximum frequency occurred in the zones 20° to 30° in each hemisphere. What is more, the zones 30° to 40° north and south, in which only nine groups appeared in 1889, included thirty-two groups in 1890. (5) The southern hemisphere contained more active regions than the northern before the minimum (1889); the contrary was the case in 1890.—On the movement of a rectilinear vortex in a liquid contained in a rectangular prism of indefinite length, by M. Andrade.—On the representation of equations with four variables, by M. M. d'Ocagne.—On a class of harmonic surfaces, by M. L. Raffy.—On the compressibility of mixtures of air and hydrogen, by M. Ulysse Lala. The observations show that the compressibility of mixtures of air and hydrogen, in which the proportion of the latter gas varies from 16 to 31 per cent. is intermediate between those of air and hydrogen respectively for small pressures (about 100 cm. of mercury). With larger proportions of hydrogen and higher pressures the mixture appears to be less compressible than hydrogen itself. This interesting fact seems fully proved, for the experiments have been controlled by making a series of measures of the compressibility of single gases.—On the compression of quartz, by M. Monnory.—Direction of luminous vibrations; Fresnel's and Sarrau's systems, by M. E. Carvallo.—On the solubility of potassium bitartrate, by M. Ch. Blarez. It is shown that cream of tartar is completely insoluble at the ordinary temperature in a mixture of 100 parts of alcohol at 20°, 900 parts of water, 4 parts of neutral potassium sulphate, and 2 parts of tartaric acid, but is dissolved if all or part of the neutral sulphate is replaced by the acid sulphate.—On the transformation of the fecula in dextrine by butyric ferment (*Bacillus amylobacter*), by M. A. Villiers.—On normal butylamines, by M. A. Berg. The author has used Hoffmann's method for the preparation of these compounds by acting on normal chloride of butyl with an alcoholic solution of ammonia.—Determination of the form of parts of the sternum of vertebrate animals, by M. Lavocat.—The structure of the pancreas and intra-hepatic pancreas of fishes, by M. Laguesse.—Anatomy of *Cerianthus membranaceus*, by M. L. Faurot.—On the differentiation of the liber in the root, by M. P. Lesage.—On the native silver and the dioptase of the French Congo, by M. E. Jannettaz. Some mineralogical specimens brought from the Congo appear to contain native silver.—On the distribution of sea-salt according to altitude, by M. A. Muntz. According to analyses made by the author, the proportion of sodium chloride in the air decreases with the altitude. This result is easy to understand. Its importance is brought out by analyses of plants, rain-water, and the blood of different animals, which appear to vary in saltness in a similar manner.—M. G. Stefanesco announced the discovery of a manuscript containing an account of a fall of carbonaceous meteorites in 1774.

Brussels.

Academy of Sciences, December 6, 1890.—M. Stas in the chair.—Facts demonstrating the permanence of the dark spots on Venus, and the slowness of their movement of rotation, by M. F. Terby. (See Our Astronomical Column.)—On some mollusks and post-Pliocene fossils found during a voyage up the Congo in 1887, by M. E. Dupont.—On the mollusks found by M. Dupont between the mouth of the Congo and the confluence of the Kassai, by M. Ph. Dautzenberg. The description of the specimens is accompanied by three plates.—Note on the physiology of Branchiostegans, by M. Léon Fredéricq.—On the preservation of hæmo-cyanine when isolated from the action of air, by the same author.—Physical observations of the planet Mars in 1890, by M. J. Guillaume. (See Our Astronomical Column.)—On the influence of the exterior temperature on the production of heat in warm-blooded animals, by M. G. Ausiaux. M. Ausiaux has placed guinea-pigs in an Arsonval's calorimeter, submitted them to temperatures comprised between 3° and 32° C., and measured their production of heat for each degree. He finds that the minimum production of heat takes place at a temperature nearly equal to the mean diurnal temperature in spring, viz. 20° C. At temperatures above or below this the production of heat is increased.—New method for the quantitative determination of the quality of bread, flour, albumen, &c., by Dr. J. Barker Smith.

CONTENTS.

THURSDAY, MARCH 12, 1891.

HUYGENS AND HIS CORRESPONDENTS.

Œuvres Complètes de Christiaan Huygens. Tome Troisième, "Correspondance, 1660-61." (La Haye: Martinus Nijhoff, 1890.)

THE great edition of the works of Christian Huygens now in course of publication at the Hague (NATURE, vols. xxxviii. p. 103, xl. p. 591) continues to issue from the press, at a leisurely rate indeed, yet, all things considered, with creditable punctuality. A slight delay in the appearance of the third volume, now before us is fully accounted for by the discovery, in the National Library at Paris, of some documents bearing on the history of the pendulum-clock, which it was judged expedient to incorporate with it in the form of an appendix. Their interest is considerable, although their import be not subversive of received ideas. They serve to confirm the originality of Huygens, while illustrating the zeal displayed in contesting it. Such debates recur in every chapter of scientific history. They sometimes rouse impatience by their apparent triviality, but seldom fail, none the less, to bring out curious and suggestive facts. It is well when they are conducted with as little acrimony as in the present case. Huygens's brilliant success in applying the pendulum to the regulation of clocks in 1657 gave the signal for the raising of adverse claims to priority. Those of Galileo were the best founded; and they were championed by Prince Leopold de' Medici, brother of Ferdinand II., the reigning Grand Duke of Tuscany. Several of his letters on the subject to Boullaud are now published for the first time; he employed Viviani, a disciple of Galileo, to draw up a "statement of claim" on his behalf; and he sent to Paris, for communication to Huygens, a drawing of a model for a time-piece begun under Galileo's directions in the last year of his life (1641). Its reproduction (at p. 8 of the work under notice) shows indeed a pendulum connected with wheelwork, but no clock in the proper sense—means for continuing the motion, either in the shape of weights or springs, being totally absent. Galileo, in fact, was on the track of an invention which eluded him. He saw that the thing was to be done, but never quite achieved the doing of it. Old age and infirmity precluded him from this final triumph. He died, *re infectâ*, leaving his ideas to be perfected by his son Vincenzo, one of the "about to be's" of the world, whose dilatoriness, as usual in such cases, marred his ingenuity; his over-drawn account with time being at last peremptorily closed by the intervention of death. Huygens succeeded to the task ignorant of these previous attempts to deal with it; to which, indeed, attention was only directed by the effective completeness of his resulting discovery.

The present volume includes, besides supplementary matter, the correspondence of two years, 1660-61. Should the thirty-three still to be traversed prove equally productive, we may look forward to seventeen further volumes—making twenty in all—devoted to letters written by, to, or about the Dutch Archimedes. The practically endless vista of huge tomes thus opened out before us is no phantom of the imagination. There seems no reason to expect that the treasures of the Leyden archives will be found less rich as they are more deeply delved into. For the genius of Huygens never lapsed into inertness. Some of its most splendid fruits were gathered late in life. His career continued to the end closely interwoven with the seething scientific activity of his age; and his eagerness for information as to the fruits of that activity doubtless retaining its keen edge, his communications with the learned are not likely to have become less frequent or less copious. Their value, moreover, judging from the samples thus far printed, is so great as to render suppression or selection undesirable, so that we can only wonder at and admire the prodigious scale of this rising literary monument to the memory of a great man. And while bearing in mind that superfluity of information, as of other things, becomes at a certain point equivalent to penury, we willingly admit that the evils of extreme voluminousness are, in this stately publication, reduced to a minimum by the admirable care with which its contents are indexed and assorted.

The figure of Saturn continued to be the leading topic of astronomical discussion for several years after the publication of the Huygenian "System." The hypothesis there expounded still lacked some final touches of confirmation by fact; and its validity was accordingly regarded in many quarters as problematical. Experience alone could answer the question whether the predicted phases of the ring would manifest themselves as time went by; and observers eagerly scanned the ill-defined planetary contour displayed by their imperfect instruments for evidential *pros* and *cons*. Chief among them was the discoverer himself. He had described what was to be expected; it remained for him to announce what was to be seen; and he had the advantage over his contemporaries in the command of superior defining powers, both mental and telescopic. Eustachio Divini, it is true, was then popularly supposed to "hold the field" in optical art; yet his best glasses, favoured by the clear Roman air, still exhibited the "triple planet" under the aspect it had assumed to Galileo, as a globe with two detached spherical appendages. He naturally regarded his own observations as conclusive; but his "Brevis Annotatio," designed for the annihilation of a fictitious ringed Saturn, served only to demonstrate their deceptiveness; and Huygens, in his "Brevis Assertio Systematis Saturni," made a triumphant reply.

The Accademia del Cimento from the first unanimously supported him. At Florence it was said, "tutti erano Hugeniani." Borelli, less prejudiced, or better optically armed, than Divini, succeeded in tracing the connection of the ansæ with the disk; and a model was constructed of the strange Saturnian machine, by which, for the special benefit of Prince Leopold, all its varying appearances, well or ill observed in the sky, were experimentally and convincingly reproduced. Premature speculations as to the nature of the ring, indulged in, among others, by Magalotti and Roberval, made it the result of condensed vaporous exhalations from the body of the planet; while Neuræus recognized in it a unique contrivance for rendering habitable an otherwise desolate world at the confines of the solar system. The bizarre idea of its composition like a lens out of some highly refractive material enabling it to concentrate light and heat upon an underlying zone, thereby brought up to the terrestrial standard of comfortable accommodation for living beings, appeared then scarcely more extravagant than any other supposition regarding a structure of which the existence, antecedently to proof, might well have been thought incredible. Fine distinctions of probability vanished in the presence of so astounding a specimen of celestial workmanship.

An imperfect anticipation of "this kind of Saturn was hatched" (it is curious to learn) early in 1658, at White-Waltham, in Berkshire, by the combined exertions of Sir Christopher Wren and Sir Paul Neile. An elliptical "corona" was fitted to the planetary globe, meeting it at two places, and rotating with it once in its period of revolution round the sun, on an axis coinciding with the plane of that revolution. Thus, after a fashion, the observed phases were accounted for; but the superiority of the Huygenian *rationale*, divulged in the following year, was perceived by none more clearly than by Wren himself; and he wisely allowed his own abortive attempt to sink into quasi-oblivion. The paper embodying it is now printed in full (p. 419), accompanied by drawings testifying very creditably to the skill of early English opticians.

Huygens was the first to make definite observations of the markings on Mars. Some of his drawings have even proved available in the most recent determination of the planet's rotation, roughly estimated by him, from his views of the Kaiser Sea, November 28 and 30, and December 1, 1659, to occur in a period of twenty-four hours. The desire to confirm a discovery which would have been the earliest of its kind quickened his zeal for the further improvement of telescopes. The main obstacle lay, he thought, in the defective quality of the glass then fabricated. And the best, which was procured from Venice, was certainly bad, judging from the collection of Huygenian lenses preserved at Leyden (Kaiser, *Astr. Nach.*, No. 592). Nor did his tubeless telescopes, when actually brought into operation, realize all that was expected from them. Concurrent evils outweighed their theoretical superiority. The disk of Mars was, however, measured by Huygens with remarkable accuracy on December 25, 1659, by means of a metal slip, of graduated width, inserted at the telescopic focus. This was the first Continental example of the use of a micrometer.

An amusing glimpse behind the scenes of pseudo-scientific life in the seventeenth century is afforded by some communications included in the present volume relative to the horoscope of one of the Princesses of Orange. Huygens was the intermediary; Boullaud the prophet, whose dignity as an interpreter of mystic influences contrasts ludicrously with his childish petition for an "Indian jewel" as the guerdon of his services. On both sides, too, there is evidence of failing faith. Huygens, apprehensive of a fiasco if the enjoined secrecy were observed as to the name and quality of the lady, imparted them, under the rose, to an astrologer who had at least the merit of choosing a low level for his pretensions. He made no claim to the divination of particular circumstances. "Temperaments" only, in his opinion, were rained down, according to the rules of science, from the skies; and temperaments are vague entities, intangible, undefinable, defiant of positive affirmations or denials; to say nothing of the saving possibility of their lying latent for any convenient length of time. The horoscope of Albertina Agnes, Princess of Nassau, was, then, at any rate safe from confutation by facts.

In the spring of 1661, Huygens visited London. He was, however, far from sharing his brother Ludovic's enthusiasm for the city by the Thames. The smell of smoke there he found "insupportable," and concluded to be insalubrious; mean architecture, narrow and ill-paved streets, unstable dwelling-houses, shabby public gardens, constituted the chief part of his impressions. The lower orders struck him as melancholy, the upper classes as naturally unsocial, their affability to strangers notwithstanding, the women as deficient in conversational charm, and falling a long way behind their French sisters in sprightliness; and he echoed the hope that an advance in refinement of manners would ensue upon the re-establishment of the Court. He was presented at Windsor to the King, whom he found somewhat curt and pre-occupied; but he left it to Pepys to chronicle the "brave sight" of the coronation in Westminster Abbey on May 3, choosing for his own share the competing celestial spectacle of Mercury's transit across the sun. It was the first phenomenon of the kind which had been at all generally observed, and Huygens watched its progress from Long Acre with one of Reeves's excellent telescopes. Some of his own were set up in the garden behind Whitehall, and occasionally served to display Saturnian marvels to the gaze of the Duke and Duchess of York. The method of their fabrication was a secret until Huygens gratified the Royal Society with its disclosure; and he was, on the other hand, deeply interested in the experiments on vacua exhibited before him at Gresham College. The hospitality and politeness, indeed, with which he was received both in public and private, did not fail to win his acknowledgments; nor could he remain insensible to the high capacity of many of his learned entertainers, "most of whom," he added, "had travelled in France and elsewhere." Unmistakably, he had by this time fallen under the spell of our neighbours' subtle charm. French had become a second native language to him, and, although he acquired enough English to make himself understood when occasion required, he did not prosecute the study very zealously. Nor was it necessary. His correspondence with Sir Robert Moray, Boyle, and Oldenburg, was carried on in French; Wallis used Latin by preference; Boyle's new tracts were promptly conveyed into French or Latin. Even his Dutch vernacular was in a measure discarded by the astronomer of the Hague in favour of more cosmopolitan tongues. Insensibly and inevitably he had grown beyond the range of a single country. He belonged henceforth primarily to Europe; only secondarily, and by a tie which was soon to be still further loosened, to Holland.

A. M. Clerke.

FORCE AND DETERMINISM.

The Philosophical Basis of Evolution. By James Croll, LL.D., F.R.S. (London: Edward Stanford, 1891.)

AGAIN and again is the physicist, in the course of his researches, brought face to face with philosophical questions. It then depends upon the bent and bias of his mind whether he is content to leave these questions as he finds them, or is impelled to adapt them to some more or less plausible metaphysical solution. Dr. Croll, whose death we have so recently had occasion to lament, made his mark by a skilful application of physical reasoning to sundry difficult problems connected with climate and time. It need hardly be said that the whole tendency of his thought and work was in support of the doctrine of evolution in its widest sense. In the volume before us he discusses, after forty years of meditation, the fundamental principles which underlie this doctrine.

At the outset, Dr. Croll draws and emphasizes what he terms "the radical and essential distinction between force and the determination of force." The *production* of motion, he says, is one thing; the *determination* of its direction, another and perfectly distinct thing. When a molecule is to be moved, there is an infinite number of directions in which force may be conceived to move it. But, out of the infinite number of different paths, what is it that directs the force to select the right path? Force produces motion, but what determines it and gives it its thusness? "In the formation of, say, the leaf of a tree, no two molecules move in identically the same path. But each molecule must move in relation to the objective idea of the leaf, or no leaf would be formed. The grand question therefore is, What is it that selects from among the infinite number of possible directions the proper one in relation to this idea?"

Dr. Croll states in his preface that his volume is not of a speculative or hypothetical nature. But we venture to think that, notwithstanding this disclaimer, the "objective idea" of a leaf comes perilously near a metaphysical as opposed to a scientific conception. But let that pass. We venture to think, further, that physicists may be a little impatient with the "radical and essential distinction between force and the determination of force." This, too, they may be disposed to regard as rather a metaphysical than a physical distinction. And this the more, since physicists are accused of attributing everything to force, and little or nothing to the far more important determination of force. Let us, however, endeavour to see what Dr. Croll's contention really amounts to. And let us take the simpler case of a crystal of alum, instead of the more complex case of a leaf, involving, as this latter does, complicating elements such as heredity and natural selection.

From a solution of potassium aluminium sulphate, octohedral crystals of alum are obtained on evaporation. Concerning them, Dr. Croll says, in effect: Granted that the molecules run into crystalline figure under the stress of certain forces, *why this particular figure?* Force accounts for their motion, but what determines the direction of motion so as to give rise to this particular form and not a scalenohedron or a rhombic pyramid? Many of us are content, at this point, to confess our ignorance, and to say that it is a way they have; it is part of the constitution of Nature as presented for our study. But possibly, others may descend to more recondite physical principles. Let us, then, for the nonce, grant that if we only knew the full bearings of that fundamental physical principle, that force is the product of mass into acceleration, we should find the crystalline figure of the alum inevitably contained therein. Few, if any, are likely to go so far. But if such there be, even of them Dr. Croll will still ask: What, then, has determined that force should be the product of mass into acceleration, rather than the product of mass into momentum, or an indefinite number of other conceivabilities? If it be replied that such is the constitution of things, the answering question will still be, But what determined that the world should be so constituted?

It will now be seen that Dr. Croll's question, though couched in new terms, is an old, old question. Nor are the solutions suggested other than those which have of old been put forward. And if Dr. Croll adopts a Theistic solution, he does not adduce other than the well-worn arguments, which it is not our province to discuss.

Two chapters are devoted to "Determinism in relation to Spencerianism," in which Dr. Croll inquires whether a rational explanation of evolution can be derived from the persistence of force. The conclusion arrived at is that no such rational explanation can be deduced from this vaguely definite axiom; and in this we are disposed to agree. With all our admiration of Mr. Herbert Spencer's power as a thinker, we believe that a synthetic philosophy, *quâ* synthetic, is of little, if any, practical or speculative value. If the synthesis leads us back to the universe from which our analysis started, nothing is gained thereby; if it brings us to a different universe, we can afford to neglect it. Other chapters are devoted to "Determinism in relation to Darwinism," in which Dr. Croll points out that natural selection does not and cannot explain the origin of variations; but this, though true enough, is an old story.

Some of the ablest sections of the work, including an appendix of three chapters, are devoted to "Determination in relation to Free-will." We fancy that some of those who may welcome Dr. Croll's argument for Theism will be unwilling to accept his argument for determinism in the matter of human conduct. They will, however, find his discussion of the matter well worthy of careful consideration, as, indeed, are many other parts of the little treatise.

In conclusion, we must state that we have felt some diffidence in dealing with this work of an author who has so recently passed beyond the reach of either praise or criticism. We could not, however, pass it by in silence; and since we were impelled to speak, we have not hesitated to speak frankly; and we trust that Dr. Croll's many friends will not blame us for the course we have adopted.

C. LL. M.

NATURAL HISTORY OF THE ANIMAL KINGDOM.

Natural History of the Animal Kingdom, for the Use of Young People. In Three Parts. With 91 Coloured Plates and numerous additional Illustrations in the Text. Adapted from the German of Prof. von Schubert by W. F. Kirby. (London: The Society for Promoting Christian Knowledge, 1889.)

THE number of books on natural history, written for the purpose of attracting persons young in years or knowledge is truly astonishing; in most cases they are adaptations from French or German sources, but sometimes they are "original" compilations. They are always illustrated; they come, and, what is more truly wonderful, they go, in the trade acceptation of this word. The demand for them would seem to be great—so great that few are able to resist the temptation of adding to their number, not knowing beforehand what pains of remorse are caused to the authors of such writings, in after days.

Times there were when, in the infancy of knowledge, such books as Patterson's "Zoology for Schools" and Milne Edwards's "Elementary Course of Natural History" served a useful purpose and had their day; but the rapid piling up of additions to that knowledge soon left it impossible for any single person to keep up with it, and even to write a good popular treatise on one small group of animals required the combined labour of a Kirby and a Spence. However, when the demand went on as before, the hint conveyed by such a fact was quite wasted, and the publishers took steps to meet it. Natural history for the people is now in as great favour as ever, and it must be brought out in a manner not only to attract the crowd, but it must be within their pecuniary re sources From some experience we have learned that works of the popular natural history class are not written with the view of being criticized; indeed, it would appear to be scarcely fair to subject them to any such ordeal, at least from a scientific point of view. A compiler who had a good working knowledge of, say, the mammals and birds, would be a more or less exceptional creature; if he knew something of the whole group of the Vertebrates, he would be far out of the common; but even such a one would flounder when he came to treat of the remaining great groups; we laugh at Oliver Goldsmith's "Animated Nature," but a learned entomologist might be as ignorant of the Vertebrates and the Mollusca as Goldsmith was of the difference between a cow and a horse. Therefore instead of criticism, we venture to think that commiseration were the more needed, and perhaps to this the advice might be added to beware not to repeat the folly.

In the "Natural History of the Animal Kingdom," for the use of young people, as adapted from the German of Prof. von Schubert, by Mr. W. F. Kirby, there are no such absolute blunders as are to be met with in Goldsmith's work; its shortcomings are more in the direction of omissions and lack of explanation of technical terms. If the adapter had handed say the first part, "On Mammals," to some young and fairly intelligent youth, and then examined him about what he read, he would probably have been astonished at the result; we tried the experiment, the critic perversely turned to the last page, about the "duckbill and spiny anteater," the position of the marsupial bones (nowhere described as having any connection with the pouch) puzzled him, and his ideas fell far from realizing the fact, but when he came to the description of the cavity of the mouth of the duckbill as "a closed weir," his speculations became hopelessly absurd, and we inquired no further. Mr. Kirby, we feel, is not accountable for the illustrations, which may amuse many, possibly instruct some.

The next time the Society makes an attempt in this direction we hope it will succeed better.

OUR BOOK SHELF.

Commercial Botany of the Nineteenth Century. By John R. Jackson, A.L.S. (London: Cassell and Co., 1890.)

The general public are so little aware of the sources and history of the many familiar vegetable products which they use daily, that a short description of them in a readable form will naturally be welcome. To provide this is the object of the little book under review: it contains within its 160 pages an epitome of the results achieved by Kew and the colonial gardens in vegetable economics during the present century. The more important attempts to introduce plants of commercial value into new areas, their success or failure, and the consequent effect upon the imports of raw materials, and the prices of manufactured articles are discussed. The facts, in themselves interesting enough, will appeal with additional weight to the reader since they come from head-quarters, the author being the Curator of the Museums in the Royal Gardens at Kew.

Mr. Jackson has wisely avoided the dictionary form, which makes books of this nature so dry and disconnected. He has devoted separate chapters to distinct classes of products, *e.g.* india-rubber, drugs, oils, dyes, fibres, &c., each chapter thus gives a succinct account of the steps taken to advance the interests of a separate industry. Perhaps the most interesting are the pages which describe the rise and progress of the trades in india-rubber (pp. 10–26) and quinine (pp. 60–71); these illustrate admirably the methods pursued by the Directorate of Kew, and it is highly desirable that such statements should be put before the general reading public. It is desirable, not merely for their information in matters which more or less directly concern every one of them, but in order that they may duly appreciate the importance of the work carried on by Kew, in the introduction of economic plants into new areas, and the effect which such experiments have already had upon supply and prices. Mr. Jackson is to be congratulated on having produced a book at once short, interesting, and useful: the facts which he puts forward so closely affect the whole community that they lose little or nothing in weight by the plainness of the style in which the book is written.

F. O. B.

Fresenius's Quantitative Analysis. Translated by Chas. E. Groves, F.R.S. Vol. II., Part 3. (London: J. and A. Churchill.)

This third part is especially welcome after the long time that has elapsed since the second was to hand, as we hope it indicates that the rest of the volume is likely to follow without delay. The present part continues the subject of acidimetry, and goes on to alkalimetry, compounds of the alkalies, and alkaline earths (including bleaching powder), aluminium compounds, silicates, and chromium and zinc ores.

The Design of Structures. By S. Auglin, C.E. (London: Charles Griffin and Co., 1891.)

This work can be confidently recommended to engineers. The author has wisely chosen to use as little of the higher mathematics as possible in his treatment of the different branches of the subject, and has thus made his work of real use to the practical engineer. It must not be imagined that the author has not thoroughly dealt with his subject. The work is a very good example of the way in which the subject can be adequately treated without the use of abstruse formulæ and complicated calculations. In a volume of 500 pages, we find most of the usual points dealt with, and illustrated by a large number of practical examples such as occur in the every-day experience of the engineer.

The volume is divided into thirty-one chapters, and concludes with a good index. Although the work has been designed for students of engineering and architecture—at least this is the modest claim of the author—he also hopes that it may prove a useful book of reference to those engaged in the profession generally. There is little doubt that these hopes will be fulfilled, for after careful perusal we have nothing but praise for the work.

On pp. 409 and 414, "Mr. B. Baker" is quoted. In a future edition it will be as well to give this eminent engineer his proper title. N. J. L.

LETTERS TO THE EDITOR.

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

Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

I CANNOT but think that my friend Mr. Bottomley is a little hard on Prof. Van der Waals. I am not aware that there is any dispute as to the fact that the methods he employed are open to criticism, and that his formula is only approximately true. In spite of its defects the treatise was regarded by Maxwell at the time of its publication as of very great interest. If, however, Van der Waals is accused of not showing a "proper appreciation of the work of Andrews," the following facts should be considered before judgment is passed:—

(1) The celebrated Bakerian Lecture of Andrews is not directly referred to, but the full account of it which appeared in *Poggendorff's Annalen* (Ergänzungsband v. p. 64, 1871) is quoted (p. 406).

(2) This reference is followed by a long section headed "Experiments of Andrews" (p. 407).

(3) On p. 420 the following passage occurs:—"The signification of the temperature—the critical temperature of Andrews—is clear from what precedes. Below it the substance can exist in the so-called gaseous as well as in the so-called liquid state, &c. The honour of this remarkable discovery, which alters our views as to the so-called permanent gases, and the liquefaction of gases generally, belongs to Andrews. That it was not so easy to reach this conclusion from experiments appears, amongst other circumstances, from Regnault giving in good faith maximum pressures for carbonic acid above 40°."

(4) The phrase, "I have borrowed this remark from Maxwell," which follows the description of the continuous transformation from gas to liquid, is at all events a proof that Van der Waals did not claim priority in the conception of the possibility of such a transformation.

He can therefore have had no possible reason for desiring to credit Maxwell, rather than Andrews, with this idea, especially in view of the facts that Maxwell himself (p. 119, first edition, "Theory of Heat") laid no claim to it, and that it is most clearly expressed in the abstract of the work of Andrews (*Pogg. Ann., loc. cit.*), to which Van der Waals himself refers his readers.

(5) The preface is not happily worded, but I think that the phrases employed do not necessarily bear the interpretation which Mr. Bottomley attaches to them.

The context shows that the "connection between the gaseous and liquid condition," which Van der Waals claims to have established, is not the possibility of a continuous transformation from one to the other through a series of stable states, but that "both portions of the isotherms belong to one curve, even in the case in which these portions are connected by a part which cannot be realized."

He is referring to the work of James Thomson, not to that of Andrews, and his claim, as I read it, is to have deduced "from theoretical considerations" a form of the isothermal which, as the passage on p. 416 shows, he fully admits that James Thomson was the first to suggest and to support by sound argument. Again, I do not understand that Van der Waals claims to be the originator of the "conception" of the continuity of the liquid and gaseous states. He only says that his conception of their *identity*, which, in the sense in which he uses the word, he admits to be doubtful, has proved a "fruitful" hypothesis. He defines *identity* to mean that the molecule is not more complex in the liquid than in the vaporous state. His calculations are based on this assumption, and he fully admits that they only apply in cases where it is justified.

While, then, I agree with Mr. Bottomley that an explicit tribute in the preface to Andrews and to James Thomson would have been graceful on the part of Van der Waals, I do not think that there is any evidence of an attempt to claim for himself credit which is due to others. A. W. RÜCKER.

SINCE my letter which was published in your last issue was written, I have found that the first edition of Maxwell's "Theory of Heat" contains a diagram, intended to represent the isothermals of carbonic acid substance, with all, or almost all, the faults of the diagram of Prof. Van der Waals; and from this, no doubt, Van der Waals's diagram was taken. Consequently I beg leave to withdraw absolutely the words used in my letter, viz. "The curves seem certainly not taken from Maxwell," and also a succeeding sentence which gave my reason for this opinion. I am sorry for my error; but I was not aware, or rather had quite forgotten, that Maxwell's first edition contained this faulty diagram.

My criticism of Van der Waals's essay is in no way altered, however, unless perhaps it is a little strengthened. Maxwell became alive to the faultiness of his diagram, at any rate prior to 1875, and corrected it. Unfortunately, Prof. Van der Waals and the translators had not reached a clear understanding of the physical meaning of these curves in 1890, even with the aid of Maxwell's second edition. J. T. BOTTOMLEY.

13 University Gardens, Glasgow, March 10.

Surface Tension.

I SHALL be obliged if you can find space for the accompanying translation of an interesting letter which I have received from a German lady, who with very homely appliances has arrived at valuable results respecting the behaviour of contaminated water surfaces. The earlier part of Miss Pockels' letter covers nearly the same ground as some of my own recent work, and in the main harmonizes with it. The later sections seem to me very suggestive, raising, if they do not fully answer, many important questions. I hope soon to find opportunity for repeating some of Miss Pockels' experiments. RAYLEIGH.

March 2.

Brunswick, January 10.

MY LORD,—Will you kindly excuse my venturing to trouble you with a German letter on a scientific subject? Having heard of the fruitful researches carried on by you last year on the hitherto little understood properties of water surfaces, I thought it might interest you to know of my own observations on the subject. For various reasons I am not in a position to publish them in scientific periodicals, and I therefore adopt this means of communicating to you the most important of them.

First, I will describe a simple method, which I have employed for several years, for increasing or diminishing the surface of a liquid in any proportion, by which its purity may be altered at pleasure.

A rectangular tin trough, 70 cm. long, 5 cm. wide, 2 cm. high, is filled with water to the brim, and a strip of tin about 1½ cm. wide laid across it perpendicular to its length, so that the under side of the strip is in contact with the surface of the water, and divides it into two halves. By shifting this partition to the right or the left, the surface on either side can be lengthened or shortened in any proportion, and the amount of the displacement may be read off on a scale held along the front of the trough.

No doubt this apparatus suffers, as I shall point out presently, from a certain imperfection, for the partition never completely shuts off the two separate surfaces from each other. If there is a great difference of tension between the two sides, a return current often breaks through between the partition and the edge of the trough (particularly at the time of shifting). The apparatus, however, answers for attaining any condition of tension which is at all possible, and in experiments with very clean surfaces there is little to be feared in the way of currents breaking through.

I always measured the surface tension in any part of the trough by the weight necessary to separate from it a small disk (6 mm. in diameter), for which I used a light balance, with unequal arms and a sliding weight.

I will now put together the most important results obtained with this apparatus, most of which, though perhaps not all, must be known to you.

I. *Behaviour of the surface tension of water.*—The surface tension of a strongly contaminated water surface is *variable*—that is, it varies with the size of the surface. The minimum of the separating weight attained by diminishing the surface is to the maximum, according to my balance, in the ratio of 52 : 100.

If the surface is further extended, after the maximum tension is attained, the separating weight remains *constant*, as with oil, spirits of wine, and other normal liquids. It begins, however, to diminish again, directly the partition is pushed back to the point of the scale at which the increase of tension ceased.

The water surface can thus exist in two sharply contrasted conditions; the *normal* condition, in which the displacement of the partition makes no impression on the tension, and the *anomalous* condition, in which every increase or decrease alters the tension.

II. *Mobility.*—Upon the purity of the surface depends its mobility, and in consequence the persistence of a wave once set in motion. So long, however, as the water surface is in its anomalous condition, the damping of the waves is constant,[1] and just at the degree of purity at which *the tension ceases to alter the decrease of the damping begins.*

If the balance is loaded with just the maximum weight which the surface tension can hold, and the normal surface is contracted till the weight breaks away, a measure is obtained of the relative amount of contamination by the ratio of the length of the surface before and after contraction; for, the purer the surface, the smaller must be the fraction to which it is reduced before it begins to enter the anomalous state. By counting, with different relative contaminations, how often a wave excited by a small rod at the end of the trough passed along the surface adjusted to a length of 30 cm. before it ceased to be visible, I obtained approximately the following values for the number of the passages:—

Relative contamination...	0	5	10	15	20	25	30
Number of visible wave passages	17	17	17	17	12	8	3

The numbers of the upper row indicate the length at which the surface becomes anomalous in 30ths of its whole length; those of the second row are, as may be imagined, rather uncertain, particularly the greater ones, although they are the mean of many observations.

A *perfectly clean* surface, whose tension remains constant, even under the greatest contraction, can be approximately produced with the adjustable trough, by placing the partition quite at the end, and pushing it from thence to the middle. The surface on one side is thus formed entirely afresh, from the interior of the liquid.

III. *Effect on a water surface of contact with solid bodies.*—Every solid body, however clean, which is brought in contact with a newly formed surface, contaminates it more or less decidedly, according to the substance of which the body consists. With many substances, such as camphor or flour, this effect is so strong that the tension of the surface is lowered to a definite value; with others (glass, metals) it is only shown by the increase of relative contamination. The contaminating current which goes out from the circumference of a body—for example, of a floating fragment of tinfoil—is easily made visible by dusting the water with Lycopodium or flowers of sulphur. I will call it, for the sake of brevity, "the solution current."

The solution current of a body which is introduced into a perfectly clean water surface lasts until the relative contamination produced by it has attained a definite value, which is *different for every substance.*

Thus the solution current for wax ceases at a relative contamination of 0·55; that of tinfoil at a still smaller one; but that of camphor, not until the surface has become decidedly anomalous, and the separating weight gone down to within 0·80 of the maximum. If, on the other hand, the surface surrounding a small piece of tinfoil be restored to its previous purity, the current begins again with renewed strength, and it appears that this process may be repeated as often as desired without the solution current ever quite disappearing.

From this effect of the contact of solid bodies, it follows that a perfectly pure surface cannot be maintained for long in any vessel, since every vessel will contaminate it. Whether the air and the matter contained in it have a share in the gradual increase of relative contamination which occurs on water left standing, I know not; but the influence of gases and vapours does not appear to me important in general. The contamination by the sides of the vessel does not, however, always go so far as to diminish the tension, which remains normal, for example, in a glass of water, after four days' standing.

With a rising temperature the contamination from all substances seems to increase considerably; but I have not yet investigated this in detail.

IV. *Currents between surfaces of equal tension.*—Between two normal surfaces, which are unequally contaminated by one and the same substance, a current sets in from the more to the less contaminated when the partition is removed; much weaker, indeed, than that exhibited in the anomalous condition by differences of tension, but, all the same, distinctly perceptible. With *equal* relative contamination by the same substance, no current of course sets in. It is otherwise when the contamination is produced by *different* substances.

I contaminated the surface on one side of the partition by repeated immersion of a metal plate, on the other by immersion of a glass plate, which had both been previously carefully cleaned and repeatedly immersed in fresh water surfaces. I then made the relative contamination on the two sides equal (*i.e.* $= \frac{1}{2}$) by pushing in the outer partitions by which the surfaces were inclosed. After the water had been dusted with Lycopodium, the middle partition was removed. I repeated this experiment eight times, with different changes devised as checks.

On the removal of the partition a *decided current* set in each time, *from the surface contaminated by glass to that contaminated by metal;* and when I replaced the partition after the current had ceased, and investigated the contamination on both sides, I always found it greater on the metal than on the glass side.

Thus equal relative contamination by different substances does *not* indicate equality of that (osmotic?) pressure which is the cause of the current between surfaces of equal tension.

For further proof of this result I have made experiments with other substances; for example, with a floating piece of tinfoil on one side, and of wax on the other, when, after they had been acting for a long time, and then the relative contaminations had been equalized, a current resulted from the wax to the tinfoil; and again, with camphor on the one, and small pieces of wood and wax on the other side, which showed a current from the wax and wood to the camphor.

Since, therefore, the water surface assumes dissimilar qualities from contact with different substances, the conviction is forced upon me that it is these bodies themselves (glass, metal, wax, &c.) which are dissolved, though only feebly in the surface, and thereby render it capable under sufficient contraction of becoming anomalous.

V. *Further observations on solution currents.*—The following facts agree with this view. If a newly formed water surface be contaminated by small floating slices of wax until the latter cease to give solution currents, the relative contamination amounts to 0·55. If now another fresh surface is brought to the same relative contamination by tinfoil and a corresponding contraction, and then a slice of wax from the first surface be introduced, it will develop a considerable solution current. This therefore depends on the substance with which the surrounding surface was previously contaminated.

Substances which are properly soluble in water, such as sugar and soda, exhibit a similar behaviour when immersed in the surface, only they continue to act in the anomalous condition.

A crystal of sugar placed in a normal but not perfectly pure surface produces a great fall of tension. If the surface be then made normal again by immersing and withdrawing strips of paper, and if this process be repeated several times, a normal surface is at last attained, which is contaminated *by sugar only*, and on the tension of this the sugar produces no further effect. A piece of soda held in the surface containing sugar greatly lowers the tension; and on the other hand on a surface rendered repeatedly anomalous by soda, soda acts but slightly, and sugar powerfully.

[In this experiment the sugar and soda crystals being instantly

[1] This is not quite exact. I found the number of visible passages constant = 3 in the anomalous state; but the velocity of transmission varying in some degree with the tension, the time required for the vanishing of the wave must really become a little longer when the tension is lowered.—*February* 26.

wetted, they do not really act by solution-currents, for the latter can only be produced by a dry body. The action here is an indirect one by intervention of the deeper layers.—*February* 26.]

VI. *Behaviour of the surfaces of solutions.*—The effect of soluble matter on the surface tension has absolutely nothing to do with the change which the cohesion of the water undergoes, through matter dissolved in the body of the liquid, for both sugar and soda solutions have a *higher* maximum tension than pure water, and yet these same substances introduced into the surface produce a fall in the separating weight.

In order to investigate the behaviour of the surfaces of solutions more closely, I introduced a saturated solution of common salt into the adjustable trough. The freshly formed surface of the solution of salt maintained its normal separating weight, (1·154 of that of water) even when most contracted, though it must necessarily have contained as much salt as the interior of the liquid. The entrance of the anomalous condition, then, does not depend on the absolute quantity of the contaminating substance contained in the surface; but when I placed some salt in contact with the normal surface of the saturated solution, it gave a solution current and lowered the tension, as in the case of pure water. I obtained similar results with a solution of sugar. From these experiments I concluded (*a*) that the surface layer of water can take up *more* of soluble substances than the internal liquid; (*b*) that the surface of a solution is capable of becoming anomalous under contraction, always and only, when it contains more of the dissolved substance than the interior of the liquid.

That the surface layer really possesses a higher dissolving power is further shown by the experiment, which is well known to you, in which a thin disk of camphor, so hung that it is half immersed in the cleanest possible water surface, is cut through in the course of a few hours. I will add by the way, that a newly formed surface of a saturated solution of camphor is *normal* according to my observations, *i.e.* that its tension remains nearly constant under contraction, and that small pieces of camphor floating on it still give solution streams and have slight motions. The solution stream seems in this case to cease just when the surface begins to be anomalous.

What I have further observed regarding solutions in the surface and the like, seems to me less remarkable, and part of it still very uncertain. I therefore confine myself to these short indications, but I believe that much might be discovered in this field, if it were thoroughly investigated. I thought I ought not to withhold from you these facts which I have observed, although I am not a professional physicist; and again begging you to excuse my boldness, I remain, with sincere respect,

Yours faithfully,

(Signed) AGNES POCKELS.

Modern Views of Electricity.

DR. LODGE'S doctrine of the slope of potential, explained in his note to my letter in NATURE of February 19 (p. 367), still presents great difficulties. A plate of zinc is covered by a film of air or oxygen in a different state from the surrounding atmosphere. We first consider a point outside of the film. Dr. Lodge says this point is influenced by the ordinary dielectric strain of a static charge imparted to the zinc in any adventitious manner. That is evident. Now, when the zinc was isolated, we had a negative charge upon it, or in the film, and therefore, at the point in question, a positive slope of potential upwards from the zinc. Call it R. When we make contact with copper we introduce a positive static charge on to the zinc. The effect of this at the point in question is a negative slope of potential—that is, downwards from the zinc. Call it $-R'$. Then, as the final result we have an upward slope of potential, $R - R'$, which is less than before contact was made.

Dr. Lodge further says that the static charge imparted to the zinc does not alter the slope of potential *within the film*. By that, I understand the average slope of potential over a line drawn from end to end of the film at right angles to the zinc. Now if the new static charge—suppose σ per unit of surface—be placed close upon the zinc, so as to have the film outside of it, it will diminish the upward slope of potential at all points within the film by exactly $2\pi\sigma$. If we are at liberty to place the new static charge at some distance from the zinc, we may modify this result in any way we please.

Mr. Chattock suggests that the essence of combination between zinc and oxygen is that the zinc atom is + and the oxygen −. By this, I understand him to mean that two atoms of zinc assume equal and opposite charges, and two atoms of oxygen assume equal and opposite charges; and then positive zinc combines with negative oxygen, forming a neutral compound as regards electrification; but the remaining zinc is negative, and the remaining oxygen positive; hence the step of potential from zinc to oxygen. But how would he explain the permanence of these states of electrification?

S. H. BURBURY.

The Flying to Pieces of a Whirling Ring.

HAVING had occasion lately to devise a high-speed whirling-machine, I examined the speed at which it might be safe to work, and some of the results surprise me. For instance, it is easy to show (by equating the normal component of the tension to the centrifugal force of any element) that the critical velocity at which a circular ring or rim of any uniform section will fly, unless radially sustained, is given by $T = v^2\rho$, where T is the tenacity, and ρ the density of its material. Thus a band of steel just able to bear a load of 30 tons to the square inch will fly to pieces at a peripheral speed of about 800 feet a second; and this without reference to its angular velocity, or radius of curvature. It may be objected that no such accident could occur with purely rectilinear motion, but such motion at the critical speed would be very unstable—the slightest shiver of a vibration running along it would precipitate a catastrophe.

Hence a steel girdle round the earth's equator would burst, however thick it might be, were it not for its weight. Again, an Atlantic cable is only held together by its weight. In the early days of cable-laying, it was suggested to ease matters by attaching floating matter to the cable till it was of the same average density as sea-water; but we now see that such a cable, if lying parallel to the equator, could not hold together, unless it were made of 30-ton steel and laid north of latitude 60°.

OLIVER J. LODGE.

Cutting a Millimetre Thread with an Inch Leading Screw.

IT is possible that many who possess a screw-cutting lathe with a leading screw of so many threads to the inch may wish to use it for cutting millimetre screws. While, of course, it is too much to expect that the absolute value of the millimetre, as given in terms of the inch, can be obtained by ordinary change wheels—and this is not of great importance, since, among other reasons, the two determinations of the value of the millimetre in inches differ by one part in a hundred thousand—yet it may not be well known that a most remarkable degree of accuracy may be obtained with wheels in ordinary use. After some trouble I lighted upon the following numbers, which, with a leading screw of eight threads to the inch, give as a result 25·3968, whereas the inch is 25·3995 millimetres. The wheels are 28 on mandril, 100 and 36 on stud, and 32 on screw. The error would therefore, with a perfect lathe, be less than one part in nine thousand, so that a screw cut in this way would for almost all purposes be correct; in fact, it is doubtful if in the case of short screws many lathes could be trusted to cut inch threads more accurately. For leading screws of other pitches, such as 4, 5, 6, or 10 threads to the inch, the wheels can easily be altered so as to give the same result.

Of course it may be the case that this or an equally good arrangement is known to some; but as I had to start working out the combinations of thirteen wheels taken four together, in which each combination contained six sub-combinations, in order to obtain the result, it is possible that it may be appreciated by those to whom it may be of use, but who would rather be saved so much trouble.

C. V. BOYS.

Royal College of Science, London.

P.S.—It may be worth while to add that the wheels taken in order—

	28 ...	*100* ...	36 ...	*32*	with	*8*	threads to the inch
are the same as	28 ...	*32* ...	36 ...	*100*	,,	*8*	,, ,, ,,
or as	7 ...	*8* ...	9 ...	*25*	,,	*8*	,, ,, ,,
or as	7 ...	*8* ...	9 ...	*10*	,,	*20*	,, ,, ,,

where the followers or multipliers are printed in italics. The last sequence of figures is sufficiently curious, and is one that can easily be remembered.—C. V. B.

A Green Sun.

I RECOLLECT reading some years ago in NATURE an observation of Mr. Norman Lockyer, to the effect that he had seen the sun green through the steam escaping from the funnel of a boat on Lake Windermere. In May 1888, I spent a considerable time one day viewing the sun through steam escaping at various pressures from the boilers of a colliery in Monmouthshire. In no case could I, or the friends with me, succeed in seeing the sun of a green colour through the steam, although we viewed it in a very great variety of ways. All we saw was the usual orange or red coloration.

But, this month, I have been watching the sun through steam puffed out from locomotives, and have, on five or six occasions, seen a bluish-green coloration extending over the whole disk. But sometimes the sun appeared simply white, and sometimes it was coloured orange-red. I cannot exactly determine the circumstances which produce the bluish-green; but I have seen it best with freshly puffed steam which had not risen very far above the funnel.

If the vapour particles are assumed of fairly uniform size, the following may be a possible explanation. The rays, coming through particles of vapour (really water particles in suspension) may be retarded as compared with the rays passing between the particles; and, if this retardation is such as to delay the red light passing through the particles one half wave-length, as compared with the red light not so passing, the result would be the destruction of the red element in the white light, and the light left would then appear bluish-green. This suggestion I owe to the courtesy of Sir G. G. Stokes, who also communicated to me the following very interesting observation. When a jet of transparent steam is escaping from a tube, we know, from Mr. Shelford Bidwell's experiments, that the steam becomes visible vapour, if an electrified point is brought near the jet. Sir G. Stokes noticed that the permanent shadow of the vapour on a screen was orange; but that, for a fraction of a second after the commencement of the electrification, a faint greenish shadow, preceding the orange one, was frequently seen. Water globules, about one ten-thousandth of a centimetre in diameter, might produce the requisite retardation.

I shall be glad to hear of other observations of this bluish-green coloration. After the great eruption of Krakatão we know that the sun was seen coloured green and blue.

CHAS. T. WHITMELL.

18 Park Place, Cardiff, February 26.

Frozen Fish.

THAT fish suffer, when imprisoned under a covering of ice in comparatively shallow ponds or lakes, goes without saying. But do they necessarily die when inclosed for lengthened periods in solid ice? My own opinion is that the latter condition is far less injurious to them than the former. It is a question of importance, for it concerns the conditions under which fish probably exist in comparatively shallow waters in high latitudes.

In one of the "Arctic Voyages" (I am not able, at this moment, to give the reference, and rely upon memory) it is distinctly stated that fish (carp, I think), frozen so hard as to necessitate the use of an axe in order to divide them, revived when thawed before the cabin fire, and "jumped about," as is usual with fish out of water.

There are fish and fish. Has the severe winter of 1890–91 caused any important mortality; if so, to which in particular?

R. McLACHLAN.

Lewisham, March 6.

Zittel's "Palæontology"—Reptiles.

IT has been pointed out to me that in my review of Prof. v. Zittel's "Palæontologie" (March 5, p. 420) I have omitted to mention that, although other writers have placed the Dolichosauria next to the Pythonomorpha, it is only in a paper recently read by Mr. G. A. Boulenger before the Zoological Society, but not yet published, that the one group was considered to be the ancestor of the other. R. L.

March 9.

THE CHEMICAL SOCIETY'S JUBILEE.

AT the meeting in celebration of the Jubilee of the Chemical Society, held in the theatre of the London University on Tuesday, February 24, 1891, the proceedings were opened by the following address from the President, Dr. W. J. Russell:—

We meet to-day to celebrate the fifty years' existence of our Society, a time, if measured by the progress which our science has made, equal to centuries of former ages, but which in years is so brief a space that we have, I am happy to say, with us to-day some of those who were present and who took an active part in the foundation of the Society, and I need hardly say with how much interest we shall listen to their reminiscences of the time and circumstances connected with the birth of our Society.

I would, by way of introduction, say a few words, first, with regard to our Society, and afterwards with regard to the state of chemistry in England when our Society was founded. We boast, and I believe rightly, that our Society holds the distinguished position of being the first which was formed solely for the study of chemistry. Chemistry and physics, twin sisters, had hitherto always dwelt together, and many were the societies, both in this country and abroad, devoted to their joint study and development.

In London there was the Royal Society, which had hitherto received the most important chemical papers; there was also the Society of Arts, which is 110 years, and the British Association, which is ten years, senior of our Society. In Manchester the Literary and Philosophical Society had been founded and actively at work since 1781; and we admit that our neighbours at Burlington House, the Astronomical, Antiquarian, Linnean, and Geological Societies, are all our seniors: they had a distinct individuality and literature of their own, which called them into existence some forty to eighty years before the commencement of our Society. Small private chemical societies, no doubt, existed: they are the natural forerunners of a large society, and become merged into it. The Chemical Section of the British Association, which is an ephemeral and peripatetic Chemical Society, had existed from the founding of that body. If we turn to other countries, we find that, much as our science had been cultivated on the Continent, it did not until later times engross a whole society to itself, the French Chemical Society not having been formed until 1857, and the now great Berlin Chemical Society not until 1868. Our interest, however, at the moment is rather in the growth of chemistry in this country than in what occurred elsewhere.

To-day we may learn how it came about that the first Chemical Society was established in England. I may, however, state that the reason for our meeting depends on the official record that on February 23, 1841, twenty-five gentlemen "interested in the prosecution of chemistry" met together at the Society of Arts to consider whether it be expedient to form a Chemical Society. Of the twenty-five who then met I am happy to say three are present—Sir W. Grove, Sir L. Playfair, and Mr. Heisch; and Mr. J. Cock is another of this band who is still alive but is not present.

These twenty-five gentlemen appear without dissent to have come to the conclusion that it *was* expedient to form a Chemical Society, and appointed a committee of fourteen to carry this resolution into effect. So expeditious were they in their work, that in little more than a month the first general meeting was held, and the provisional committee brought forward a report embodying a plan for the constitution and government of the Society, and this plan remains essentially the same, save in one point, to the present day. I refer to the formation of a museum of chemical specimens; this project was abandoned some years ago. It is worth recording that at this first general meeting Thomas Graham was elected President; Messrs. W. T. Brande, J. T. Cooper, J. F. Daniell, R. Phillips, Vice-Presidents; Mr. Arthur Aikin, Treasurer; Messrs. Robert Warington, E. F. Teschemacher, Secretaries; Council—Dr. T. Clarke, Rev. J. Cumming, Dr. C. Daubeny, Messrs. T. Everitt, T. Griffiths, W. R. Grove, H. Hennell, G. Lowe, W. H. Miller, W. H. Pepys, R. Porrett, Dr. G O. Rees. Also that the Society then numbered seventy-seven members. We hail Sir W. Grove as being the most active member who is still among us in founding our Society, for he was a member of the first Council, was present at the first meeting, and was a member of the provisional committee. I must here add to the official record, for it does not tell us how these twenty-five gentlemen "interested in the prosecution of chemistry" were collected together at one time and place. Obviously some special force was required to build up this complicated molecule; that special force was embodied in and exercised by Robert Warington. By his activity and energy he brought about this meeting, and we can imagine how difficult and troublesome a work it probably was, how some of these gentlemen had to be instigated to action, others repressed, some convinced that the aim was desirable, others that it was feasible. But whatever the difficulties were, Mr. Warington succeeded, and to him we are indebted for the formation of our Society. Although he has passed away, he is ably represented here to-day by his son. The love for the Chemical Society has proved to be hereditary: Mr. Warington of to-day is a most active and valued member; is one of our Vice-Presidents: and, as our programme shows, is about to present to us records connected with the early history of our Society which are of great interest now and will become of increasing value as time goes on.

I turn now at once from these matters immediately connected with our Society to the consideration of what was being done in chemistry in this country fifty years ago. At that time public laboratories for the systematic teaching of chemistry did not exist in London. The number of real students of chemistry in this country was very small. They were looked upon by their friends as being eccentric young men, who probably would never do any good for themselves, and these few students found practical instruction in the private laboratories of some of the London teachers.

The practical teaching of chemistry appears to have been undertaken in Scotland much earlier than in England, for Dr. D. B. Reid held practical classes at the University of Edinburgh as early as 1832. Graham came to London from Glasgow in 1837, and until the opening of the Birkbeck Laboratory, in 1846, he had from time to time private students working in his laboratory. And so with the other teachers, who all had private or articled pupils. I doubt whether the pupils received much systematic instruction, but they gained an insight into laboratory work, saw how apparatus was put together, and how analyses were made. We have indeed to wait some years before public laboratories are established, for not till 1845 is the College of Chemistry opened, and this appears to have been really the first public laboratory in London, and its object, as stated by its founders, is "to establish a practical School of Chemistry in England." About the same time both University and King's College established laboratories. The Council of our Society recognized the importance of these occurrences, for in the Annual Report in 1847, they say, "although an event not immediately connected with the Society, the Council has much pleasure in commemorating the late successful establishment in London of chemical laboratories expressly designed to further the prosecution of original research. The new laboratories of the College of Chemistry, and of the two older Colleges of the London University, now offer facilities for practical instruction and research not surpassed we believe in any foreign school."

While speaking of laboratories in London, I should however mention that the Pharmaceutical Society established a laboratory especially if not exclusively for its own students as early as 1843.

It was not till several years later, till 1850 and 1851, that the medical schools in London established classes of practical chemistry.

If we consult the scientific journals of the time immediately preceding the formation of our Society, we find it was by no means a period of chemical activity in this country, but rather a dull time, given more to the study and slow development of the science than to discovery. Methods of analysis, both organic and inorganic, had been much improved, and the dominant idea was the determination of the empirical composition of bodies, and the preparation of new compounds, whose existence was predicted by a study of Dalton's "Atomic Theory." Graham, Kane, and Johnson of Durham were the leaders in scientific chemistry, and the authors of the most important chemical papers of the time. Graham had very lately published his notable paper on the constitution of salts, a paper which gained for him, some years after its publication, a Royal medal. Kane was an active worker and a bold theorist, and at this time his reputation was much increased by a paper on the chemical history of archil and litmus. Johnson was also a most active chemist. His contributions relate to many branches of the science, but especially to the chemical composition of minerals. In 1841, however, he is engaged on a long series of papers on the constitution of resins. He will probably be best known and remembered as an agricultural chemist. Faraday we can hardly claim as a chemist at this time, for he was then rapidly publishing his long series of experimental researches in electricity. While speaking of electricity I should state that it was in 1840 that Smee described his battery, and the Society of Arts awarded him a gold medal for it. An important branch of our science was, however, coming into existence, a branch which has found many and sucessful investigators in this country. I mean photography. It was in 1840 that Herschel published in the Philosophical Transactions his elaborate paper on the chemical action of the rays of the solar spectrum, a paper in which he recognizes a new prismatic colour beyond the violet, and chemical activity in the spectrum beyond the red, and besides discussing many other matters, establishes his previously discovered hyposulphite of soda as the best agent for the fixing of sun pictures. Fox-Talbot had previously given an account of photogenic drawing, and claims that as far back as 1835 he took pictures of his house by means of a camera and chloride of silver paper, but it is not till 1838 that the Secretary of the Royal Society extracts from him a clear account of the details of his process, and it is in 1841 that he is granted a patent for improvements in obtaining pictures or representations of objects. Again, in the following year Herschel publishes another paper of much importance. I can here only mention how actively this line of research was prosecuted by Robert Hunt, how many, ingenious, and interesting were the experiments he made, and how valuable was the account he afterwards gave of this subject in his "Researches on Light." Thus the work done in this branch of chemistry at the time of which I am speaking is certainly noteworthy, probably more so than in other branches of chemistry. In fact, of other advances in chemistry there is little to record, but I may mention that Clarke's process for determining the hardness of water also holds its jubilee this year, for it was in 1841 that a patent was granted to Dr. T. Clarke for a new mode of rendering certain waters less impure and less hard.

Not a single chemical paper appears in the Phil. Trans. for 1841, but there are two papers which were much discussed at this time, and although they were readily shown to be erroneous, still are interesting as indicating the chemical ideas of the day. One is by Robert Rigg, who is carrying on an experimental inquiry on fermentation, and is termed "Additional Experiments on the Formation of Alkaline and Earthy Bodies by Chemical Action when Carbonic Acid is present"; it is published in the Proceedings of the Royal Society. The other is a paper by Dr. S. M. Brown, entitled "The Conversion of Carbon into Silicon," and is published in the Transactions of the Royal Society of Edinburgh.

With regard to the first paper, Mr. Rigg believes that he has demonstrated that, when fermentation takes place, a great and direct increase in alkaline and earthy salts, viz. of potass, soda, and lime occurs, an increase varying from fifteen to nineteen times the original amount. Denham Smith, who has only very lately passed away, showed that the theory simply rested on inaccurate experiment.

The object of the other paper is to demonstrate that, on heating paracyanogen, nitrogen is given off, and a residue of silicon remains. Dr. Brett and Mr. Denham Smith controverted this, and, in a paper in the *Philosophical Magazine*, proved that the supposed silicon was simply carbon in a very incombustible state. So important an experiment was this alleged conversion of carbon into silicon considered to be at the time of its publication, that it attracted Liebig's attention, and in a letter to Dr. Playfair, which was communicated to the meeting of the British Association at Plymouth, in 1841, Liebig says he has repeated Dr. Brown's experiment on the production of silicon from paracyanogen, but has not been able to confirm one of his results.

As far as pure chemistry is concerned it was rather a time of repose. The beginning of the century had been a brilliant time for chemistry in England. Dalton had published his atomic theory; Davy had decomposed potash and soda, and had demonstrated that chlorine was an element; and Cavendish and Wollaston were then still at work. In fact the most important discoveries of that time were made in this country, but I fancy that during this later period a feeling grew up that the age of brilliant discoveries was over, and that, apart from the preparation of a few new compounds, the essential work of the time was analysis and the determination of the percentage composition of bodies. Still much quiet study of the science was going on, as is indicated by the considerable demand which existed for good text-books. Henry's, Turner's, Kane's, and Graham's "Chemistry"—all these, without mentioning others, went through numerous editions, and played a very important part in the spread of chemical knowledge in our country.

Another text-book, which is interesting as showing how little organic chemistry was studied in this country, is Dr. Thomas Thompson's work on "Vegetable Chemistry." Dr. Thompson states in his preface that the object of the book is to lay before the British public a pretty full view of the present state of the chemistry of vegetable bodies, and further, he says, "that the ultimate analyses he gives have, with very few exceptions, been made upon the Continent, and principally in Germany and France. British chemists have hardly entered on the investigation." Evidently then at this time organic chemistry had been but little studied in this country.

When our Society was founded, Thomas Graham was certainly the most distinguished chemist in England. He came to London in 1837 as professor of chemistry at University College, succeeding Edward Turner. The work he had already accomplished was of a high order, and he was now occupied in writing his book, which appeared in 1842.

The book was an admirable account of the chemistry of the time; it contained a well arranged and clearly written introduction, describing the principles and latest discoveries in those branches of physics which bear most directly on chemistry. There was also an able and succinct account, probably the best which had then appeared in this country, of organic chemistry; and with regard to physiological chemistry, he states in the preface that he gives a "condensed view of the new discoveries in this department, which now enters for the first time into a systematic work on chemistry."

There are, however, indications that a knowledge of the discoveries and discussions going on on the Continent only slowly reached this country. This is strongly insisted on in the *Phil. Mag.* of 1841, by Messrs. Francis and Croft, who state that "but little of what is done abroad, especially in Germany, seems to find its way into England, or at least until the lapse of some years." In proof of this statement they mention results lately published by Dr. Apjohn, Prof. Johnston, and Dr. Golding Bird, all of which had been known on the Continent some time previously. A valuable series of communications described as "Notes of the Labours of Continental Chemists," is afterwards communicated by these chemists to the *Phil. Mag.*, and continued for several years.

The visit of Liebig in 1837, when he attended the meeting of the British Association at Liverpool, must have given some stimulus to the study of organic chemistry in England, and we find that he undertook to report to the British Association on "Isomeric Bodies," and also on organic chemistry, and this great undertaking resulted in his two works, the one "Chemistry, in its application to Agriculture and Physiology," and the other, "Chemistry, in its applications to Physiology and Pathology." Both books were dedicated to the British Association, the first appearing in 1840, the second in 1842. It is very difficult for us now to realize the importance of these works, and properly to appreciate not only the large amount of new knowledge which they contained, but, what is of still greater importance, the novelty of treating such subjects in a truly scientific spirit. Gradually this treatment of the subjects became understood and appreciated, and people took a higher view of chemistry, and regarded it as a true science, and not merely as a study which might lead to useful results.

If then it be true that chemistry at this epoch was not rapidly progressing in this country, we naturally ask how it came about that our Society from its very foundation was so successful. The explanation is not difficult to find, nor doubtful, for we have only to turn from our own country to the Continent and learn what is happening there. Liebig is at Giessen, Wöhler at Göttingen, Bunsen at Marburg, Dumas, Laurent, Gerhardt, and a host of distinguished and active chemists in France, and at this time even Berzelius and Gay Lussac are alive. Liebig, with his wonderful energy and ability, was powerfully advocating the theory of compound radicles, and was extending in every direction our knowledge of organic chemistry, and inspiring all who came within the range of his influence with a love for investigation. Dumas, at the same time, both as a chemist and a finished advocate, was advancing his views on substitution and chemical types. Laurent, and afterwards Gerhardt, were with conspicuous ability showing how these theories were to be extended and modified so as to assume a form which has even with the lapse of time been but little altered. Thus on the Continent it was a time of wonderful activity; chemistry was every day becoming more of a true science, and the constitution as well as the composition of bodies was actively being discussed and investigated. This activity on the Continent took time to reach and really affect us here. The older chemists thought the new theories were visionary and unsound, the simple theories of their younger days were being swept away, and only slowly did they realize the meaning of the newer form of their science; but the wave of progress could not be stopped, and in this country we had been ripening for the change. Clearly the immediate cause of this sudden increase of chemical activity in England was Liebeg. His famous school had now been established for several years at Giessen, and if the older men in this country did not altogether put their trust in him, the younger men, breaking through all restraint, flocked from this country to his laboratory, there to become indoctrinated with his enthusiasm for the study of chemistry, and to learn how scientific investigation was to be carried on. At this epoch our Society was founded, and our Journal shows how successful Liebig's teaching was, how a new spirit was instilled into English chemistry, and how much valuable work his students did. Our Society gave them a ready means of publishing their discoveries, and a meeting-place for discussion and mutual interchange of ideas. Thus do I explain the success which from the first has attended on our Society; and having now led you to this point I stop, for my part was merely to speak the prologue, and I leave the story of the development of our Society in other hands.

INFECTIOUS DISEASES, THEIR NATURE, CAUSE, AND MODE OF SPREAD.[1]

II.

ONE of the earliest and most important discoveries was that made by Pasteur as to the possibility of attenuating in action an otherwise virulent microbe—that is to say, he succeeded in so growing the microbes, that when introduced into a suitable animal they caused only a mild and transitory illness, which attack, though mild, is nevertheless capable of making this animal resist a second virulent attack. Jenner, by inoculating vaccine, inoculated a mild or attenuated small-pox, and by so doing protected the individual against a virulent small-pox Pasteur succeeded in producing such an attenuated virus for two infectious diseases—chicken cholera and splenic apoplexy or anthrax; later on also for a third—swine erysipelas. For the first two he produced cultures grown under certain unfavourable conditions, which owing to these conditions lose their virulence, and when inoculated fail to produce the fatal disease, which they would produce if they were grown under normal conditions. What they produce is a transitory mild attack of the disease, but sufficient to protect the animal against a virulent form; thus in anthrax he showed that by growing the *Bacillus anthracis* at a temperature of 42°·5–43° C. for one week, the bacilli become slightly weakened in action; growing them for a fortnight at that temperature, they become still more weakened, so much so, that if this culture (*première vaccine*) be injected into sheep or cattle (animals very susceptible to anthrax) the effect produced is slight; then injecting the culture which has been growing only eight days at 42°·5, the effect is a little more pronounced, but not sufficient to endanger the life of the animal. Such an animal, however, may be regarded as having passed through a slight attack of anthrax, and as being now protected against a second attack, however virulent the material injected. In the case of swine erysipelas, Pasteur found that the microbe of this disease, transmitted through several rabbits successively, yields a material which is capable of producing in the pig a slight attack of swine erysipelas, sufficient to protect the animal against a second attack of the fatal form. Passing the anthrax virus from however virulent a source through the mouse, it becomes attenuated, and is then capable of producing in sheep only a mild form of disease protective against the fatal disorder. Attenuation of the microbes has been brought about outside the body by growing them under a variety of conditions somewhat unfavourable to the microbe.

Attenuation of the action of the anthrax microbes has been produced by adding to the cultures some slightly obnoxious material (*e.g.* mercuric bichloride 1 : 40,000), by which the growth is somewhat interfered with; or subjecting an otherwise virulent culture for a short time to higher degrees of temperature (anthrax to 56° C. for five minutes; fowl enteritis, twenty minutes, 55° C.); or exposing them for short periods to some obnoxious chemical substance (*e.g.* anthrax to carbolic acid, anthrax to bichloride of mercury 1 : 25,000 for twenty minutes); or the microbes are passed through, *i.e.* are grown in the body of certain species of animals, whereby the microbes become weakened as regards other species (swine erysipelas, anthrax, diphtheria, and tetanus); finally, some microbes become attenuated spontaneously, as it were, by growing them in successive generations outside the animal body, *e.g.* the pneumonia microbe, the erysipelas microbe, and others. However good the nutritive medium, these microbes gradually lose their virulence as cultivation is carried on from subculture to subculture; in diphtheria the culture which was virulent at first loses its virulence as the same culture becomes several weeks old.

All these facts are of considerable importance, inasmuch as they enable us to understand how, in epidemics, the virulence of the microbe gradually wears off and becomes ultimately *nil*, and because they indicate the ways of attenuating microbes for the object of protective inoculations.

Another important step in the study of Bacteria was this: it was shown that they have, besides their special morphological and cultural characters, definite chemical characters. Specific chemical characters (specific ferment actions) of Bacteria have been known for a long time through the earlier researches of Pasteur—*e.g.* the Bacteria causing the acetic acid fermentation of alcohol, the mucoid fermentation, *e.g.* when beer becomes ropy, the lactic acid fermentation of milk-sugar, when milk becomes spontaneously sour, &c.

Similarly, it has been shown that when animal or vegetable matter undergoes the change known as putrefaction or putrid decomposition, substances are produced which resemble alkaloids in many ways, and which, introduced into the circulation of man or animals, act poisonously, the degree of action depending, *cæteris paribus*, on the dose, *i.e.* the amount introduced. These alkaloids—called ptomaines of Selmi—have been carefully investigated and analyzed by Brieger; they are different in nature according to the organism that produces them, and according to the material in which this organism grows: neurin, cadaverin, cholin, &c., are the names given to these substances.

Recent research has shown that pathogenic Bacteria, *i.e.* those associated with, and constituting the cause of specific diseases, are capable of elaborating poisonous substances—toxalbumins or toxins, as they are called—not only in artificial culture media, but also within the human or animal body affected with the particular pathogenic microbe. Thus, in anthrax or splenic apoplexy, Hankin and Sidney Martin have shown this to be the case; in diphtheria (Fraenkel and Brieger), in tetanus (Kitisato), similar toxins have been demonstrated. We can already assert with certainty that a microbe that causes a particular disease causes the whole range of symptoms characterizing the disease by means of a particular poisonous substance or substances it elaborates in and from the tissues of the affected individual.

Another important fact ascertained about some of the toxic substances produced by the different pathogenic Bacteria was this: that if, after they are elaborated in an artificial culture fluid, and, by certain methods of filtration, separated from the Bacteria, and injected into a suitable animal, they are capable of producing the same

[1] Lecture delivered at the Royal Institution on Friday, February 20, 1891, by E. Klein, M.D., F.R.S., Lecturer on Physiology at St. Bartholomew's Hospital Medical School. Continued from p. 419.

disease as their microbes, the rapidity and intensity varying with the amount introduced, so that it became evident that also in the human and animal body the intensity of the particular disease depends, amongst other things, on the amount of poisonous substance elaborated by the Bacteria in the tissues. A further important step made was this: that if the poisonous substance be introduced in such doses that only slight disturbance would follow, and the dose be repeated several times, the body of the animal eventually becomes refractory to the growth and multiplication of the particular Bacteria.

Wooldridge's researches on septicæmia and on anthrax, Roux's researches on septicæmia and diphtheria, Beumer and Peiper, Salmon, and many others, have shown the same for a variety of infectious diseases: in all these instances it has been proved that, when the chemical products of a specific microbe, elaborated in an artificial culture medium or in the animal body, are injected into a healthy animal, this latter is rendered refractory against the specific microbe, so that, if the specific microbe be introduced into such a prepared animal, the microbe cannot grow and multiply, and cannot therefore produce the disease. Pasteur's brilliant researches on protective inoculation against hydrophobia are based on this principle.

The same explanation applies also to those diseases, like scarlet fever, anthrax, fowl cholera, swine fever, certain forms of septicæmia, in which a first, even mild attack is sufficient to protect the animal against a second attack, however large the number and however great the virulence of the particular Bacteria introduced.

For it must be obvious that it is practically the same, whether the protective amount of the toxic substance is produced by the Bacteria in the animal body, as is the case during a mild first attack of the disease, or whether the protective amount of toxin is elaborated outside the body, *i.e.* in an artificial culture, and is then introduced into the animal body. In both instances the effect is the same, viz the animal body is hereby rendered capable of withstanding the growth and multiplication of the particular Bacteria when a new invasion takes place.

What is the cause of this immunity or refractory condition?

In order to explain this, I wish first to draw your attention to the familiar fact that different species of animals, and even different individuals of the same species, offer a different degree of resistance to the different infectious diseases. Whereas splenic fever or anthrax is communicable to man, rodents, and herbivorous animals, it is only with difficulty communicable to carnivorous animals or birds; cholera and typhoid fever are not communicable to any but man; diphtheria is communicable to the human species, to guinea-pigs, cats, and cows, it is not communicable to some other animals; tubercle or consumption is communicable to man and herbivorous animals, in a less degree to carnivorous animals, though these also take it but in a smaller intensity; certain other diseases are common to animals, but are not communicable to the human species.

If we inquire into the cause of this different susceptibility, we find some very striking facts. Take anthrax: cold-blooded animals, *e.g.* frogs, are unsusceptible as long as they are in their natural conditions of temperature; but if a frog be kept at the temperature of a warm-blooded animal, it is found susceptible to anthrax (Petruschki). Birds are not susceptible to anthrax, but if its temperature be lowered a few degrees it becomes susceptible to anthrax (Pasteur).

Or take another instance: rats are not susceptible to anthrax, but if the animals be kept for some time under severe muscular exercise, they become susceptible to the disease. Tame mice are unsusceptible to glanders, but if phlorizin is administered to them for some days, whereby a deposit of sugar takes place in their tissues, they become susceptible to glanders. The susceptibility and unsusceptibility are expressed by saying that the living tissues in an animal offer in the one case a favourable, in the other an unfavourable, soil for the growth and multiplication of the microbe, and that this unfavourable condition can be altered in a variety of ways, *e.g.* temperature, muscular fatigue, sugar in the tissues, &c. But also a primary favourable condition can be rendered unfavourable: for instance, a human being that has passed through one attack of scarlatina offers tissues unfavourable for the growth of scarlatina microbes; attenuated anthrax protects against virulent anthrax; an animal that has been first treated with repeated small doses of the chemical products of a particular microbe becomes unsusceptible to that microbe.

In order to explain the whole group of phenomena of refractory state, immunity, and protection, a theory has been put forward which is as simple as it is fascinating. There can be truly no greater satisfaction and no greater aim in any branch of science than to express a great number of facts and phenomena by the simplest possible formula; the greatest minds and the most successful philosophers have achieved this. Now, in regard to the numerous and extremely complex phenomena that we have under consideration, a simple formula has been put forward which is supposed to cover all the facts and to explain all the phenomena; this formula is comprised in a single word, "phagocyte." This word is put forth whenever and wherever a difficulty arises in explaining or understanding the complex problems involved in the intimate pathological processes, the refractory condition, the unsusceptibility to and immunity from an infectious disease. To any and every question referring to infectious diseases the answer is simply "phagocyte." By a "phagocyte" is understood one of those elementary microscopic corpuscles abounding in the animal and human body, possessed of spontaneous or amœboid movement, and occurring in the blood as white blood-cells or leucocytes; in the lymph and lymph glands and most tissues, as lymph corpuscles; in all acute and chronic pathological processes, as pus-cells or round cells. The cells, by their protoplasmic or amœboid movement, have the power to take up into their interior all manner of minute particles or granules, living or non-living; it seems as if these granules and particles were being swallowed up, eaten, and destroyed by the cells—hence the name of eating globule, or "phagocyte," given to them.

These cells—white blood-cells, lymph-cells, or round cells—are supposed to have the important function to act as the sanitary police against the invading Bacteria, to be always on the look-out for them, and where they meet them to at once engage in battle with them; that is to say, to do as the giants do—to eat their victims. If the phagocytes are victorious—that is, if they succeed in eating up the Bacteria—no harm is done to the animal body; no disease is produced; but if, for one reason or another, the Bacteria succeed in evading the grasp of the phagocyte police, then the Bacteria grow and multiply, and cause the disease. Sometimes this latter result follows on account of the phagocytes not being capable of moving sufficiently briskly to the places of mischief, or, for some inherent reason, not being able to cope with the Bacteria, or being altogether indifferent to the presence of the enemy; when this is the case in an animal or human body, the phagocytes being powerless to destroy the Bacteria, we are supposed to be dealing with a body that is susceptible to the disease; but when the phagocytes do their duty, then the body is unsusceptible to the disease. Again, when an animal or human being, by a mild first attack, or by protective inoculation of one kind or another, becomes unsusceptible to a second attack, this is explained by saying that, though the phagocytes have not done, or have not been able to do, their duty during that first attack, they have now been rendered capable of doing it.

Now, if you ask what is the evidence on which this theory of phagocytosis is based, you will find that it is of the most slender kind, and you will further find that there is an overwhelming number of observations which directly negatives this theory of *universal* phagocytosis, and, moreover, proves conclusively that if phagocytosis has any share in producing a refractory condition on the part of animals towards a particular infectious disease—be that a primary unsusceptibility or an acquired immunity against a second attack—this share is of a remarkably small degree. The whole theory was started by Metschnikoff by the interesting and fundamental observation that, if anthrax bacilli are introduced into the dorsal lymph-sac of the frog—an animal unsusceptible to anthrax while living under normal conditions—the bacilli become inclosed in the lymph-cells, and are gradually broken up; they do not multiply, and do not therefore set up the disease anthrax. This observation, which is easily verified, was the starting-point for the theory. Metschnikoff and others have described similar appearances in other conditions of refractory states. Now the above observation is explained by Metschnikoff in this way: the lymph-cells are acting the part of guardians, swallowing up the bacilli and preventing them from entering the circulation, and thereby preventing the outbreak of the disease. It must seem very extraordinary that this should be really a true explanation of the refractory state of the frog towards anthrax, considering that the bacilli, like other minute particles, when injected into the lymph-sac, would be absorbed and brought into the circulation in a few minutes, nay, seconds—at any rate some hours before the phagocytes have got into the lymph-sac in sufficient numbers to do battle with the bacilli. That the bacilli really enter the circulation in this and other cases, but are destroyed by the blood, not by the leucocytes but by the fluid part of the blood—the plasma—has been abundantly proved; and it has likewise been proved that the fluid part of the blood and of the lymph in general has a remarkable germicidal action, independent of any cellular elements, leucocytes, or other cells. The observation of Metschnikoff admits of an explanation different from that given by him; it may mean, and probably does mean, that the bacilli cannot exist in the fluid of the lymph and of the blood; they are destroyed here, but they *take refuge in the leucocytes or lymph-cells in which they can live and exist*—for a time, at any rate—these cells offering to them more favourable conditions of existence. In the case of the normal frog, this is also only of a temporary nature, since the substance of the lymph-cells suspended in the lymph or in the blood-plasma becomes likewise permeated with the germicidal influence of the fluid lymph and the fluid plasma, and hence the bacilli soon die, even in the substance of the cells. This explanation is in perfect harmony with the large number of carefully conducted experiments of a host of workers (Fodor, Nutall, Buchner, Petruschki, Lubarsch, and others), according to whom the refractory condition of an animal to a particular infectious disease is due to a chemical germicidal action of the tissue juices, the lymph, or blood plasma, and independent of any cellular elements. The more pronounced this germicidal action of the juices is, the more refractory the animal. Hence we find that, for instance, when in an animal even a very small number of Bacteria introduced are sufficient to produce disease, the germicidal action of the living blood fluid is very small indeed; but when a considerable number of bacilli are required to produce infection, as in animals possessing only a slight refractory power, this germicidal action of the blood and tissue fluid is greater, and it is greatest of all in those bodies in which not even a large number of bacilli can produce infection, as is the case in animals possessed of immunity. As stated just now, this part of the subject, as to the germicidal action of the fluid of the tissues and the blood, has been worked out very carefully, and it has been shown that, as regards the destruction of Bacteria, the leucocytes of the lymph and blood, or other similar cells, might just as well be absent altogether. Quite recently the whole argument has been clenched by showing[1] that if an animal susceptible to a particular disease be first infected with the bacilli causing this disease, and then into such an infected animal the cell-free serum or plasma of the blood of an animal refractory to that disease be injected, the development of the disease in the former animal is stopped or prevented. Thus mice are very susceptible to anthrax; if they are infected with anthrax bacilli they die of virulent anthrax within thirty-six to forty-eight hours; but if simultaneously with, or soon after, the introduction of the virulent anthrax bacilli, blood of frog or blood of dog (both animals very refractory to anthrax) be injected into these mice, no fatal anthrax follows. Guinea-pigs, very susceptible to diphtheria, when infected with virulent culture of the diphtheria bacilli, die in the course of a day or two, but rats are little or not at all susceptible to the diphtheria bacilli; and therefore if the guinea-pigs, after infection with the diphtheria bacilli, are injected with rat's blood they recover, this blood being a powerful destroyer of the diphtheria bacilli. Tetanus is easily communicable to mice, in which it produces fatal tetanus in one to three days, but it is not communicable to rabbits; mice infected with the tetanus bacilli and then injected with rabbit's blood do not become affected with tetanus and remain alive.[2]

While on the one hand, then, the tissue juices and the blood, independent of the cellular elements, possess this germicidal action—small or *nil* in susceptible, larger in animals less susceptible, and largest in unsusceptible animals—there is, on the other hand, a considerable body of evidence to show that the least germicidal action seems to be possessed by those very cells themselves which figure in the theory as the destroyers of Bacteria, as phagocytes; that is to say, that of all the tissues the so-called phagocytes are the materials offering to the Bacteria the best means of existence. Even in cases in which the lymph and blood fluid have against particular Bacteria the greatest germicidal power, the so-called phagocytes are for a time the last refuges for the Bacteria. I will illustrate this by a number of examples both of acute and chronic infectious diseases, as gonorrhœa, Egyptian ophthalmia, Koch's mouse septicæmia, leprosy, and tuberculosis; this latter is particularly instructive, as it demonstrates the absurdity of the alleged phagocytosis of the cells of the spleen in tuberculosis, for it is the latter cells in which the tubercle bacilli thrive well, and which they choose pre-eminently.

[4. Demonstration: tubercle cells and leprosy cells.]

Nay, more than this: non-pathogenic Bacteria cannot exist in the normal blood and in the tissues, in the wall of the alimentary canal, in the tonsils, in the tongue; they are destroyed and are therefore absent in the living tissue. But they can, for a time at any rate, exist in the cells of those parts, and in these, and these only, they are met with; these cells are therefore just the reverse of phagocytes, being the last refuges of the Bacteria.

These facts seem to show that cells containing in their substance living Bacteria is no evidence whatever of a battle going on between the cells and the Bacteria, but rather the reverse. The assumption of the presence in the so-called phagocytes and similar cells of a "defensive proteid" seems therefore opposed by these facts. The cells seem to possess a particular chemical attraction for the Bacteria, just as is possessed by certain chemical substances; such attraction is spoken of as *positive chemotaxis* in contradistinction to *negative chemotaxis*—that is, the opposite or repulsive interaction between

[1] Ogata and Tasuhara, *Mitth. der Med. Facultät d. Kais. Japan Universität, Tokio.*

[2] Behring and Kitisato, *Deutsche Med. Woch.*, 1890, N. 49.

Bacteria and certain substances. This line of inquiry is of quite recent date, and promises to produce important and interesting results.

From all this we conclude, then, that in some cases the blood and tissues are, or include, a natural antidote ; in others the antidote is not present naturally, and is only furnished by the Bacteria themselves, and still in others the tissues, though possessed of this antidote, may lose it owing to altered conditions.

Another point worth considering is the peculiar inimical action exerted by one microbe on the other : this practically means that the products of one microbe either prevent the growth of another microbe or neutralize its toxic action. It is perfectly well established that while the products of one microbe exert an inimical action, when present in sufficient amount, on the growth and life of the same microbe (the protective inoculation by chemical products of the Bacteria cited above), the accumulation of the products of the particular microbe interferes with, and eventually altogether stops the further growth of its microbe. Outside the body this is easily proved in artificial cultivations. Inside the body it is proved by those cases in which recovery takes place.

It has been shown that while some pathogenic microbes can well thrive side by side in the same culture, inside or outside the body, there are others where the growth of one is antagonistic to the action of the other: erysipelas and anthrax (v. Emmerich), swine erysipelas and swine fever, anthrax and *Bacillus pyocyaneus* (Charrin), anthrax and *Staphylococcus aureus;* this is due to the fact that the chemical products of one species inhibit or neutralize the other species. That this antagonism is really of a chemical nature is shown in the case of anthrax and *Bacillus pyocyaneus;* if the chemical products of this latter microbe be injected into the animal simultaneously or soon after inoculation with the anthrax bacilli, no anthrax disease ensues, the anthrax bacilli do not multiply and do not produce the disease. Apart from specific antiseptics, there exist, then, in the battle against the action of pathogenic microbes which have already entered the system of an animal, the following means : (1) the chemical antagonism offered by the healthy tissues themselves —in some cases this is nought, in others very great and powerful, alterable by various conditions ; (2) the germicidal action of the blood and tissue juices of unsusceptible animals on the multiplication of pathogenic Bacteria within a susceptible animal ; (3) the antagonism existing between the Bacteria and their own chemical products ; (4) the antagonism of one species and its chemical products against another species. These principles have, then, to be borne in mind as indicating the ways in which the microbes can be prevented from producing eventually the disease in a body into which they have found access. Pasteur's hydrophobia inoculations, and many of the recently published results of curative inoculations against tetanus, against anthrax, and against diphtheria, are illustrations of these principles.

The principle on which Koch's antituberculous lymph acts is apparently of a different nature. Koch has found by experiment on guinea-pigs that if the chemical products or an extract of the substance of the tubercle bacilli be injected into a body affected with tuberculosis, the tubercular tissue becomes necrotic, while the tubercle bacilli themselves remain unaffected ; at the same time a remarkable reactive inflammation sets in, by which the necrotic tissue becomes eliminated, either spontaneously, *e.g.* where the tubercular process is superficial, as in lupus of the skin, or it may be removed by surgical aid, as in tuberculosis of the bones and joints. All who have followed the numerous cases treated in this manner must agree that it is an immense advance on all previous methods of treatment of some forms of lupus and of bone tuberculosis.

After having shown you what an enormous amount of accurate knowledge about the nature and causation, about the prevention and treatment of infectious diseases has been gained by the experimental method and by this alone, it will hardly be credited that a number of persons, as well-meaning as they are ill-instructed, are still maintaining the contrary ; it is they who have succeeded in inducing Parliament to pass a law restricting, if not in some cases altogether prohibiting, the use of that method. This law is interfering with research in this country to a large extent, and has even put a stigma on those who are engaged in elucidating truths that are for the benefit of mankind, and of the animals themselves : what can be of greater benefit in the battle against diseases than the knowledge of their causes, and the devising of means for their prevention and treatment ?

Fortunately for progress in general, this country is the only one in which such restrictions disfigure the statute-book ; other countries, more enlightened and able to recognize the value of researches of this kind, have wisely resisted the clamour for restrictions.

In connection with all this knowledge, of which I have only been able to give you an outline, I have heard it stated that "ignorance" (meaning the ignorance of former times) "is bliss" as compared with the present knowledge of the dangers surrounding us ; but I have also heard it stated that the wise man, knowing and recognizing the nature of these dangers, has the possibility given him of avoiding and preventing them, and no truer words have been spoken than these, that "he who cures a disease may be the skilfullest, but he that prevents it is the safest, physician."

My best thanks are due to my friend Mr. Andrew Pringle for the admirable photographs prepared by him of the microscopic slides illustrating the different pathogenetic microbes exhibited, and to my friend and pupil Mr. Bousfield for the fine lantern slides of tube cultivations.

THE ROYAL METEOROLOGICAL SOCIETY'S EXHIBITION.

THE twelfth annual Exhibition of the Royal Meteorological Society was opened on Tuesday evening, March 3, in the rooms of the Institution of Civil Engineers, 24 and 25 Great George Street, Westminster. This year's Exhibition is devoted to rain-gauges and evaporation-gauges, and also such new instruments as have been constructed since the last Exhibition. It might at first be thought that an exhibition of rain-gauges would be a very small and insignificant affair, and would not be of any interest to the general public. Anyone, however, visiting the Exhibition will at once see that a very large collection of various forms of rain-gauges has been got together by the Society, and that the information obtained from the records of these simple instruments is of the highest importance. There are altogether fifty-six different forms of rain-gauges shown in the Exhibition, and it is interesting to compare the old with the new patterns.

Side-tube rain-gauges of various diameters are exhibited. In this instrument the water passes into the body of the gauge, and also into a glass tube in the front, and stands at the same level in each. As the combined area of these tubes is very much less than that of the receiving surface, the natural depth of the rain is proportionally increased, and thus the scale is lengthened in proportion—usually about eight or ten times—so that the quantity can be read off to hundredths of an inch. The objection to this form of instrument is that the glass tube is liable to be burst by frost, and the record lost.

Messrs. Negretti and Zambra exhibit a contracted float-gauge, the receiver of which contains a copper float, to the upper side of which a rod is attached. When rain falls, the rod is lifted, and, owing to the small area of the body of the gauge as compared with that of the rim, the float rises about eight times the natural depth of the rain.

Mr. Symons shows Fleming's rain-gauge, which is a very small float-gauge. This pattern was formerly much used in Scotland, but is now nearly abandoned, because when the quantity of rain collected exceeds 2 inches, rain which ought to pass over the gauge is caught by the measuring-rod, and runs down it into the gauge. It was also usually placed so nearly level with the ground that surface water occasionally entered.

Various modifications of Howard's rain-gauge are exhibited. This pattern was designed by Luke Howard, F.R.S., and engraved in the first edition of his "Climate of London," published in 1818. It simply consists of a funnel 5 inches in diameter, with a long tube at the bottom, which fits into the neck of a glass bottle. The area of the funnel is about eleven times that of the measuring-jar, so that minute measurements can easily be made. In 1850 a stone-ware bottle was substituted for that of glass, with the view of reducing the frequency of breakage. Mr. H. H. Treby modified this form of gauge somewhat by having rough divisions put upon the glass bottle, so that an approximate idea of the amount of fall might be obtained without the gauge being interfered with. Mr. Symons further modified this gauge by attempting to protect the glass bottle with an outer cylinder having two slits in it, so as to allow of inspection, as in Mr. Treby's gauge. This gauge, however, had two faults, viz. (1) the bottle did not hold enough; and (2) if it burst, the can, being pierced, could not save the water. This gauge was subsequently so modified as to remove the above-mentioned evils, the bottle being larger, and the can water-tight.

Glaisher's rain-gauge is 8 inches in diameter, and has a bevelled rim and curved pipe. The rim round the gauge, about two-thirds of the way up, was designed to make a water-tight joint, so as to prevent any of the rain inside escaping by evaporation. The same object was aimed at by the curved tube or inverted siphon, in which the last few drops of rain remained, and (until they dried up) formed a water-seal. This gauge has subsequently been modified by the substitution of a vertical rim (to cut the rain-drops) for the original bevelled one on which they would break, and by the substitution of a long straight pipe for the curved one, which was found to be frequently choked with leaves, &c.

In the autumn of 1864 the late Major Mathew undertook to provide a number of gauges for the district round Snowdon; for that district Mr. Symons provided gauges 5 inches in diameter, with cylinders rising 4 inches vertically from the edge of the cone of the funnel. These are called "Snowdon rims," and funnels so provided are gradually displacing all others, because they are so much better in time of snow. A gauge of this kind in copper is nearly indestructible and independent of frost, because two vessels (one of glass and one of copper) must burst before the water can be lost. Specimens of this form of gauge, Symons's Snowdon, are shown both in copper and in galvanized iron.

The Meteorological Office gauge, which is 8 inches in diameter, is generally regarded as the best gauge for ordinary observers to whom cost is not a primary object; it has all the good features of the Glaisher and of the Snowdon patterns, and being of copper is very durable.

Several mountain rain-gauges are exhibited. As these are for use in places where the rainfall is heavy, and where they can only be periodically examined, they are made to contain 40 or 50 inches of rain. The rod is detached from the float (to avoid error from its intercepting the rain), and is only dropped into the cup when an observation has to be made. The instrument has an outer cylinder to guard against frost, and to facilitate emptying.

The Manchester, Sheffield, and Lincolnshire Railway company, who for many years past have had regular rainfall observations made at about fifty of their stations, exhibit a specimen of the gauge they employ, which is 8½ inches in diameter, also a map showing the sites of their rain-gauge stations, and specimens of their forms and publication.

Mr. Symons shows a number of the original gauges which were employed about twenty-five years ago in the experiments carried out by Colonel Ward and others to determine (1) the effects of placing gauges at different heights above the ground, not (as had been done previously) on buildings, but on posts; (2) to ascertain whether there is any difference in the indications of gauges ranging in diameter from 1 to 24 inches, and including square ones of 25 and 100 inches area; and (3) to test the effect of various receiving surfaces, such as tin, copper, glass, porcelain, and ebonite.

Among the other old pattern gauges may be seen:—Stevenson's, which has the rim of the gauge brought to the level of the ground, and is surrounded by a brush to avoid in-splashing; FitzRoy's, in which the amount of rain is ascertained by a graduated dipping tube which has a hole at each end; the old copper gauge, with square funnel, used at the Kew Observatory from 1850 to 1890; and the coffee-pot rain-gauge—so called from its shape.

Mr. Symons shows both the first and second pattern of his storm gauges. These are not intended for general use, but to enable observers to watch the most minute details of heavy rain during thunderstorms. Carefully attended to, they yield information of the very highest importance, both for architects and for engineers, as to the rate at which rain falls. With one of these instruments in London on June 23, 1878, rain was ascertained to be falling for 30 seconds at the rate of 12 inches an hour.

The earliest registering rain-gauge is probably that known as Crosley's. The area of this gauge is 100 inches, and beneath the tube leading from the funnel there is a vibrating divided bucket; when one compartment has received a cubic inch of water, *i.e.* 0·01 inch of rain has fallen, the bucket tips, the index advances on the first dial, and the other bucket begins to fill, and so on indefinitely. Messrs. Yeates and Son's electrical self-registering rain-gauge is simply a Crosley gauge, in which electrical contact is made at each turn of the bucket, and the index hand moved one division. The advantage of this instrument is that the funnel may be placed in any exposed position out of doors, while the registering part can be fixed indoors.

The Kew Committee exhibit Stutter's registering rain-gauge, which has twenty-four collecting jars, one for each hour, and also Beckley's self-recording rain-gauge, which is the pattern in use at the Observatories in connection with the Meteorological Office; its funnel has a receiving area of 100 square inches, and is provided with a lip 1¼ inch deep, to retain the splashes. The rain flows down into a copper receiving vessel, which, floating in a cistern of mercury, sinks and draws down with it a pencil which marks on a cylinder moved by a clock. When the receiving vessel is full, a siphon comes into action and rapidly draws off the whole of the water, the vessel rising almost at a bound, the action being recorded by a vertical line on the cylinder. Mr. Casella shows the recording portion of his self-recording gauge, which is another mode of effecting the same object.

MM. Richard Frères, of Paris, exhibit two of their self-recording rain-gauges, which are very ingenious instruments and promise to yield good results. They are already at work at many stations on the Continent, and to our own knowledge at four in this country.

Several rain-gauges as used in foreign countries are included in the Exhibition. Prof. Mascart, of the Bureau Central Météorologique de France, has sent four specimens of gauges as used in France; and Dr. Hellmann, of the Kon. Preussisches Meteorologisches Institut, Berlin, has forwarded the first and second patterns of his rain and snow gauge (the latter of which is now used in the Prussian Meteorological Service), and also his gauge for measuring the density of snow. Wild's rain-gauge, as used in Russia, and Nipher's protected snow-gauge, as used in the United States, are also exhibited.

Among the miscellaneous gauges may be mentioned the marine rain-gauge, mounted on gimbals for use on board ship; Livingstone's rain-gauge, with funnel 3 inches in diameter, as made for the late Dr. Livingstone; tropical rain-gauge, with funnel 4½ inches in diameter, and receiver large enough to hold 40 inches of rain; Colladon's gauge for determining the temperature of hail; Sidebottom's snow-melting rain-gauge; and Mawley's snow-gauge.

Perhaps the largest rain-gauge that has ever been made is that employed by Sir J. B. Lawes and Dr. J. H. Gilbert on their experimental farm at Rothamsted; this has an area of one-thousandth of an acre. The funnel portion of this large gauge is constructed of wood lined with lead, the upper edge consisting of a vertical rim of plate glass, bevelled outwards. The rain is conducted by a a tube into a galvanized iron cylinder underneath, and when this is full it overflows into a second cylinder, and so on into a third and fourth, and finally into an iron tank. Each of the four cylinders holds rain corresponding to half an inch of depth, and the tank an amount equal to 2 inches. Each cylinder has a gauge-tube attached, graduated to 0·002 inch. Of course, this gauge itself could not be exhibited, but two of the collecting gauge cylinders are shown, as well as a coloured view of the gauge *in situ*, drawn by Lady Lawes. A coloured view of the Rothamsted drain or percolation gauges, drawn by Lady Lawes, is also shown. There are three drain-gauges, each one-thousandth of an acre area, which are used for the determination of the quantity and the composition of the water, percolating respectively through 20, 40, and 60 inches depth of soil (with the subsoil in its natural state of consolidation). Sir J. B. Lawes and Dr. Gilbert exhibit a table giving the results of rainfall and drainage at Rothamsted for the twenty harvest years ending August 31, 1890. The annual means are as follow:—

Rainfall.		Drainage through soil (uncropped).				Difference approximately = evaporation.		
		20 in.	40 in.	60 in.		20 in.	40 in.	60 in.
in.		in.	in.	in.		in.	in.	in.
30·29	...	14·38	15·16	13·61	...	15·91	15·13	16·68

The Exhibition also includes a number of evaporation gauges for determining the amount of evaporation from a free surface of water or from plants. The Meteorological Council exhibit von Lamont's atmometer, and Wild's, De la Rue's, and Piche's evaporimeters; Mr. Casella shows Babington's atmidometer, and an 8-inch pedestal evaporator. Dr. W. G. Black shows his floating rain-gauge and evaporating cup for use on ponds; and Mr. W. H. Dines exhibits the apparatus used by the late Mr. G. Dines for measuring evaporation. Mr. Symons shows the following from the series of evaporators constructed under the supervision of Mr. Rogers Field, and used in experiments at Strathfield Turgiss about twenty years ago, viz. Fletcher's, Watson's, Miller's wet-sand, tin, tin with overflow, Casella's can and bottle; also Field's hook gauge, used for determining the depth of water evaporated from the large tank, 6 feet square and 2 feet deep, which was used as the standard wherewith the foregoing and some other forms of instrument were compared. The Cambridge Scientific Instrument Company exhibit a self-recording evaporimeter, designed for use with growing plants in a botanical laboratory; and MM. Richard Frères show their self-recording evaporation gauge for use with either water or plants.

Several new instruments are also shown in the Exhibition, among which may be mentioned the following:—Latham's self-recording apparatus for wells, rivers, and reservoirs; Dines's and Munro's helicoid and Robinson's anemometers, and helicoid air-meter; and Richard Frère's statoscope (which is a very sensitive atmospheric barometer) and anemo-cinemograph. Mr. Clayden exhibits a small and large camera for meteorological photography, showing a simple method of attaching a mirror of black glass for photographing meteorological phenomena. The Kew Committee exhibit the frames designed by General Strachey and Mr. Whipple for measuring cloud pictures for determining the height and drift of clouds.

The Exhibition also includes a large number of photographs illustrating meteorological phenomena, &c., as well as a number of maps and diagrams showing the distribution of rainfall over various parts of the world.

The Exhibition will remain open till Thursday, the 19th instant.

William Marriott.

THE PTOLEMAIC GEOGRAPHY OF AFRICA.

At the meeting on Monday of the Royal Geographical Society, Dr. H. Schlichter read a paper on "Ptolemy's Geography of Eastern Equatorial Africa." Ptolemy, as a geographer, has received very different treatment at different times at the hands of his critics. At one time it was the fashion to sneer at the industrious Alexandrian geographer as entirely untrustworthy, as a mere imaginative arm-chair geographer, without critical discrimination. That Ptolemy was an arm-chair geographer no one denies, but in geography, at least, it should be remembered that the looker-on often sees most of the game. Basing his system on that of his predecessor, Marinas of Tyre, Ptolemy seems diligently to have collected the itineraries of all travellers that came within his reach, and his position at the great port of Alexandria was highly favourable for work of that kind. Of course his methods were faulty, his fundamental data erroneous, and the observations with which he had to deal often of the vaguest kind. Still, when all due allowance is made for these drawbacks, there is no denying that Ptolemy's map of North-Eastern Africa bears a wonderful resemblance to reality—just the resemblance that might be expected in the infancy of cartography, before the invention of instruments of precision, and ere travellers had learned to make good use of their eyes. Recent discoveries in Central Africa have attracted increased attention to the geography of Ptolemy, and make one wonder how he came so near the truth. It has been recently attempted by Dr. Meyer (who in this case is merely the mouthpiece of Mr. E. G. Ravenstein) to show that Ptolemy's knowledge of East Africa did not extend beyond Abyssinia; that his Nile is simply the Abyssinian River, and his lakes the lakes of that country, projected downwards, to suit later knowledge, into the heart of Africa. However that may be, Dr. Schlichter, in his paper, gives the result of an ingenious method adopted by him to test Ptolemy's accuracy, and to prove that he must have somehow obtained information about the lakes which we now know give origin to the Nile, and about the snow-clad mountains that cluster round them, and which are all that remain to us of the once famous Mountains of the Moon that extended like a barrier across the continent. After discussing Ptolemy's cartographical methods, and making allowances for his error as to the length of the degree (600 instead of 500 *stadia*), Dr. Schlichter's *modus operandi* is as follows:—

1. To look for the basis on the coast which Ptolemy used in order to fix the position of this part of Africa; and to eliminate his error of geographical latitude.

2. To reduce the positions of his points to modern graduation.

3. But in all other respects to leave the distances from the basis of the map intact with the exception of the itineraries round the Victoria Nyanza.

Dr. Schlichter rightly takes the Rhapta of Ptolemy and his Periplus as the central point of his calculations. Besides Rhapta, Ptolemy mentions a promontory called Rhaptum, and a river called Rhaptus. The "metropolis of Rhapta" must have been somewhat inland, but Dr. Schlichter has no difficulty in identifying the Pangani River with the Rhaptus, and Ras Mamba Mku, a cape to the south of Zanzibar as Ptolemy's Rhaptum. Taking this as his starting-point, and making due allowance for Ptolemy's mistakes as to the length of the degree, Dr. Schlichter measures off with his compasses the distances given by Ptolemy, and in this way identifies most of the places in East Central Africa mentioned by Ptolemy with well-known places of the present day. He measures off, for the sake of minute accuracy, his distances in millimetres. He has constructed two maps—one based merely on Ptolemaic data and another showing the latest knowledge: the coincidences are striking. In this way Dr. Schlichter identified the coast places marked by Ptolemy with such well-known places as Melinda, the mouth of the Tana, the towns of Brava, Marka, Magdishu, Warsheikh, and other places. Applying the same method to the positions in the interior given by Ptolemy, Dr. Schlichter identifies Ptolemy's Eastern Nile Lake with the Victoria Nyanza; the circle, with Rhaptum as the centre and the position given by Ptolemy in the interior as the other end of the radius, cuts the south-east shore of Victoria Nyanza. Following the same method, Dr. Schlichter finds that the position given by Ptolemy for the eastern end of the Mountains of the Moon coincides with a point a little to the south of Mount Kenia. Again, in a similar manner he identifies the Western Nile Lake with Lake Albert or Lake Albert Edward, the western end of the Mountains of the Moon with Ruwenzori, and the confluence of the two rivers which form the Nile with the place where the Somerset Nile flows into Lake Albert.

These instances are sufficient to indicate the method followed by Dr. Schlichter, and its success in identifying the positions given by Ptolemy with features which we know now really do exist. In the subsequent discussion, Mr. Ravenstein endeavoured to prove that Dr. Schlichter's method was entirely misleading, even although he admitted that the position adopted for Rhaptum was approximately correct. Mr. Ravenstein's arguments cannot, however, be regarded as convincing; and although we are not interested in upholding Dr. Schlichter's position, still we think that, in justice to Ptolemy, and in the interests of historical truth, his methods and results deserve serious consideration.

CARL JOHANN MAXIMOWICZ.

CARL JOHANN MAXIMOWICZ, who died at St. Petersburg on February 16, after a few days' illness, was born at Tula in 1827. He went early to St. Petersburg, where he was brought up at the St. Annenschule, a renowned German Lutheran College. In 1844 he left the Russian capital for the University of Dorpat. After completing his studies, he was appointed director's assistant at the botanical garden of Dorpat, a post he held until 1852, when he was made Conservator of the Imperial Botanical Garden at St. Petersburg. The following year he set out on a voyage around the world on board the frigate *Diana*, his chief task being to make acquisitions of living plants for the botanical garden at St. Petersburg. The *Diana* visited Rio de Janeiro, Valparaiso, and Honolulu. But when war was declared by the Western Powers against Russia, she was compelled to call at the nearest Russian harbour, De Castries, on the coast of Mantchuria, at that time the youngest, and scarcely an organized, Russian colony. Maximowicz had to leave the frigate, and decided at once to go up the River Amur, and to explore its banks and the adjoining country, which was then little known. Though furnished with only limited means, he carried out his task under great difficulties and severe privations in a very successful manner. He returned to St. Petersburg by way of Siberia in 1857. The next two years he devoted entirely to the working out of his "Primitiæ Floræ Amurensis: Versuch einer Flora des Amurlandes," a thick quarto volume, which appeared in 1859, and contained a full enumeration of his botanical collections, and a most clear exposition of the general physical features of the country visited by him, and particularly of its phytogeographical character. Immediately after, the full Demidoff Prize was awarded to him in acknowledgment of the excellence of his work. At the same time he was directed to proceed again to the far East. In 1859 and 1860 he travelled in Mantchuria; in 1861 he visited the island of Yesso; 1862, Nipon; 1863, Kiu-siu. He returned to Europe by the sea-route in 1864. It was then that he first visited England. He was at that time in a bad state of health, in consequence of an obstinate fever he caught in Japan, and from the effects of which he suffered from time to time throughout his life. In 1869 he was appointed Botanicus Primarius at the Imperial Botanical Garden at St. Petersburg, and he was a Fellow of the Imperial Academy of Science from 1864. Consequently he was also entrusted with the direction of the Herbarium of the Academy. After 1866 he published many contributions to the flora of Eastern Asia in the *Mémoires* and the *Bulletins* of the Academy, the most important being a monograph of the rhododendrons of Eastern Asia, the "Diagnoses breves Plantarum Novarum Japoniæ et Mandshuriæ, Dec. i.-xx."; the "Diagnoses Plantarum Novarum Asiaticarum, i.-vii.," &c. It was in the latter that he began to work out the large and exceedingly important collections made by Prjevalsky, Potanin, &c., in Central Asia. In consequence, however, of the extreme thoroughness of his work, and his highly critical method, combined with overwhelming official duties, the first parts of these important works did not appear before the end of 1889. These are the "Flora Tangutica" and the "Enumeratio Plantarum hucusque in Mongolia, &c., Lectarum," each comprising only the Thalamifloræ and the Discifloræ of the collections. A general review of the phytogeography of Central Asia, founded on the collections of Prjevalsky and other Russian explorers, however, was submitted by him to the Botanical and Horticultural Congress at St. Petersburg, 1884; it is a model of lucidity of style and arrangement. Now, we fear, these two works, so comprehensively planned, will proceed no further, although Maximowicz's preparations for the remaining parts were considerably advanced and a large number of most beautiful plates are ready for press. But we look in vain for the man in Russia who could take up the work. Russia was so unfortunate as to lose her great explorer by sudden death at the very moment when he was setting out to gather new laurels, and now his most famous interpreter has breathed his last not less unexpectedly. Deeply as we must regret that he was not permitted to finish his work himself, one thing is certain—that whatever he completed will last. He was of a noble, high-minded nature, a highly cultivated scholar in almost every branch of learning, and a gentleman in the truest sense of the word.

OTTO STAPF.

NOTES.

THE next ordinary general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, the 19th, and Friday evening, the 20th, at 25 Great George Street, Westminster. The chair will be taken by the President, Mr. Joseph Tomlinson, at half-past seven p.m. on each evening. The following papers will be read and discussed, as far as time permits :—Fourth report of the Research Committee on Friction : experiments on the friction of a pivot-bearing. On recent trials of rock drills, by Mr. Edward H. Carbutt, past-President, and Mr. Henry Davey, of London.

THE annual general meeting of the Linnean Society of New South Wales was held on January 28. Dr. J. C. Cox, Vice-President, delivered the annual address. Pointing out that Australia offered an unrivalled opportunity of working up completely, and under the most favourable circumstances, the flora and fauna, especially interesting in itself, of one of the great tracts of the globe, he inquired how it was that with such a splendid harvest still waiting to be gathered in spite of all that had been accomplished, the number of workers was relatively so few. He attributed the slow increase in the number of enthusiastic naturalists partly to defective educational methods which left children blind to the beauties and attractions of Nature ; partly to the want of descriptive catalogues, and well-illustrated hand-books, written from the Australian stand-point ; and partly to the very slender inducement to young men in Australia to qualify themselves for the serious pursuit of science.

AT Oxford, on Tuesday, Convocation voted the sum of £150 per annum for three years for a scholarship to be held by a student in the Marine Biological Institute at Naples.

THE General Board of Studies at Cambridge has recommended the appointment of a Demonstrator in Palæobotany in the department of geology.

LAST week Mr. Leon Warnerke delivered before the Photographic Society of Great Britain a lecture on a simplified photo-collographic process. Instead of a glass or metal plate, as is usual in the collotype process, Mr. Warnerke makes use of a gelatine film supported upon vegetable parchment. He demonstrated the method of sensitizing and drying the film, explained the process of printing, and passed round specimens for inspection before and after printing and after washing with water. He then demonstrated the method of printing, and showed its application to wood engraving, to canvas, and to the decoration of china and porcelain. The walls of the room were hung with between two and three hundred examples of collotype printing sent for exhibition by the Autotype Company, the London Stereoscopic Company, and Messrs. Bemrose and Sons, of Derby, Waterlow and Sons, Römmler and Jonas, of Dresden, L. Rouillé, of Paris, F. Thevoy and Co., of Geneva. It was announced that the exhibition would remain open for a month.

THE first electrical launch ever built for the English Government was sent off the stocks into the Thames at Messrs. Woodhouse and Rawson's yard, near Kew Bridge Station, on Tuesday afternoon. The *Electric*, as the pinnace is called, is to be used for the conveyance of troops between the dockyards of Chatham and Sheerness. The *Daily News* describes the motion of the boat as "delightfully smooth—the very poetry of motion." With her forty fully-equipped soldiers, she can run at a speed of eight knots an hour. A single charge of electricity enables her to run for ten hours.

AS various erroneous statements have been made with regard to Dr. Nansen's Arctic expedition, the *Times* gives the following account of what has actually been arranged. Dr. Nansen's desire is to leave Norway in February 1892, but it is doubtful whether the special vessel which is being built will be ready by that time. Outside of Norway not a farthing has been contributed by anyone. The expedition is purely Norwegian, and will remain so. The Norwegian Government contribute 200,000 kroner ; King Oscar, 20,000 ; twelve private individuals (all Norwegians except one Englishman who has lived in Christiania for many years), 90,000 : in all, 310,000 kroner, equal to £17,200. That, Dr. Nansen believes, will be sufficient. The ship, of course, is being specially constructed for the peculiar conditions which exist between the New Siberian Islands and the Pole. Dr. Nansen will be accompanied by probably not more than eight young men, all as stalwart and strong in physique as himself, and all equally confident of success.

AT a meeting of the Royal Botanic Society of London on Saturday, a report by Mr. Lecky was read, giving a summary and digest of the sun record in the Gardens during the year 1890, showing the percentage of each month. As compared with the returns for the previous year, this shows an increase of 156 hours of bright sunshine, a result due to the latter half of the year (the earlier months being comparatively sunless). The total recorded for the year amounts to 1092 hours, as against a possible total of 4455 hours. A noticeable feature was the predominance of afternoon sunshine, due, it seems, to the position of the Gardens in the north-west of London, and the difficulty the sun has in piercing the smoke and mists from the eastern districts as it rises. Not the slightest trace of sunshine is recorded as having occurred during December.

A propos of the recent severe weather, *La Nature* has a representation of an ice colonnade, made near Rheims in January, which is about 14 feet high, and bends in half circle round an ice figure of Father Christmas. M. Caron describes a peculiar kind of ground ice found in rivers in the Jura region. In clear cold nights crystals form at the bottom, and rise in groups to the surface. They consist of small needles and lamellæ of ice held by mutual attraction, but not adherent to each other. They offer no resistance to boats or oars, but may sometimes cause much inconvenience : thus they accumulate and stop water-wheels ; or, rapidly collecting on a dam, they may cause flooding. M. Franger (an Alsatian engineer) points out that all circumstances unfavourable to radiation from a river bed, are also against formation of ground ice, which is rarely, if ever, met with in a stream that is muddy or too deep, so as not to transmit the heat-rays, or where the motion is sluggish enough for surface ice to form, and for gravel at the bottom to be covered with sediment. At the sluices near Maigrange, according to M. Cuony, ground ice forms about the iron work largely used in the sluices, and is got rid of by heating the upper part of the structure with wood fires. M. Cuony produced ground ice experimentally, by cooling an iron bar 10° to 15° below zero C., and plunging it in cold water ; thus illustrating the part played by the piles of bridges.

THE Report of the Smithsonian Institution for 1888 (Washington, 1890) contains *inter alia* a review by Prof. Cleveland Abbe on "Recent Progress in Dynamic Meteorology," giving a brief, but careful, summary of the most important works which have appeared up to December 1888, on the movements of storms and the general motions of the atmosphere. The works summarized are classified under the following heads : (1) laboratory experiments on fluid motion ; (2) statistics of actual storms ; (3) theoretical hydrodynamics applied to the motion of the air ; (4) thermodynamics of atmospheric phenomenon ; (5) prediction of storms and weather. The author considers that it is generally conceded that meteorological phenomena, at least those which depend on the motions of the atmosphere, are too difficult to be unravelled at present, although much progress has been made during the last few years. His summary will be a good guide for persons wishing to study the science.

THE Royal Meteorological Institute of the Netherlands has published a work upon the tides off the Dutch coast, based on the observations of the light-keepers on the various banks, showing graphically the average times of the turning of the tides, and also the means of sea temperature at and below the surface, deduced from hourly observations. The mean annual temperature of the air is slightly lower than the sea-surface temperature, but in some months the reverse occurs, and the temperature at depths of 5 and 10 fathoms differs very slightly from the sea-surface temperature. Tide rips are most frequent in April, during the first four days of full and new moon, while the least number occur in January. With a strong south-west wind, the tide runs eight miles an hour.

WE learn from the *Oesterreichische botanische Zeitschrift* that Herr H. Leder has undertaken a three years' zoological and botanical expedition to Siberia. During the first year he proposes to visit the Tunka and Sajan mountains to the south of Irkutsk; in the second year he will follow the course of the On-on into Transbaikal; and during the third year he has planned a visit to the upper course of the Amur and Argun in the Katunsk Altais.

THE editors of the *Botanisches Centralblatt*, finding it impossible to keep pace with the ever-increasing mass of botanical literature, have arranged for the publication of *Beihefte*, of which it is intended that seven, of eighty pages each, shall appear annually, with only a moderate increase in the annual subscription. By this means it is hoped that the delay which has hitherto occurred in the *résumés* of important botanical papers may in the future be avoided. The first of these supplements has appeared, and contains notices of several English papers which have appeared in the *Annals of Botany* and elsewhere.

TEA at £10 12*s.* 6*d.* per pound ought to have very special qualities. A small parcel of Ceylon tea, from the Gartmore Estate, was sold at this price at the London Commercial Tea Sale Rooms on Tuesday. The tea is composed almost entirely of small "golden tips," which are the extreme ends of the small succulent shoots of the plant.

THE new number of the Journal of the Institution of Electrical Engineers (No. 91) contains the Presidential address on "Electricity in Transitu," delivered by Prof. Crookes on January 15. There are also papers on "Electric Lighting from Central Stations," by General Webber, and on the "Early History of the Telegraph in India," by Mr. P. V. Luke.

THE use of oil at sea continues to be regarded by experienced masters of vessels as an excellent means of protection in stormy weather. A number of recent reports on the subject, printed in America, afford striking testimony to the efficacy of the method. Captain Rogers, Br. s.s. *Congo*, for instance, writes thus:—"Left Liverpool, January 18, 1891, for New York; arrived February 8. At 4 a.m., February 4, it was blowing a hurricane, ship taking some very heavy seas aboard. Put her before the wind, stopped the engines, and put an oil-bag out on each quarter. Ship rode out the hurricane splendidly, and without shipping any water whatever." Chief Officer Miller, Am. brig. *Marena*, writes:—"During the gale which began on January 10, lat. 32° 56′ N., long. 75° 20′ W., and ended on January 12, lat. 34° 40′ N., long. 74° 45′ W., wind from south to west-north-west, the vessel was hove-to and a hemp-canvas bag was partly filled with oakum, saturated with fish oil. The bag was replenished every two hours, and was allowed to just dip into the sea from the lee quarter. The oil was used for twenty-four hours, and as the vessel lay quite easy and shipped no heavy seas it proved a great success. The captain is always provided with a full supply of fish oil (which he considers the best), and all appliances for instant use." Captain Leseman, Br. s.s. *Miranda*, reports:—"Used oil with most excellent effect in the gale encountered on December 1, 1890, between St. John's and Halifax. The oil was a combination of kerosene and linseed, and was used from the closets forward and from bags on each side amidships. Waves would come bearing down in the direction of the steamer as though to crush her, but they no sooner reached the oil than they rolled harmlessly past. To its use we owe our lives and the safety of the ship."

SOME interesting remarks on squirrels are made by various writers in the current number of the *Zoologist*. It is often said that squirrels are torpid during winter, but there is no really sound evidence for this view. Mr. Masefield, writing from Cheadle, Stafford, says:—"I have seen squirrels abroad on fine days in, I think I may say, every one of the winter months; and while pheasant-shooting near here on a sunny day (January 6 last), which was about the middle of the most severe frost we have had for many years, with several inches of snow on the ground, I saw a squirrel jumping from tree to tree, before the beaters, in the most lively condition." Mr. Blagg, also writing from Cheadle, has "frequently seen squirrels abroad in the middle of the winter, when there has been deep snow on the ground and a keen frost in the air." "I remember," he adds, "once seeing a squirrel abroad during a severe storm of sleet and rain in winter-time, and he appeared to be not at all inconvenienced by the rough weather." Mr. Blagg's idea is that the squirrel probably does sleep a good deal more in winter-time than in summer, as do many other wild animals, but that he has to be continually waking up and taking nourishment. The period of reproduction is unfavourable to the notion of an almost complete state of torpidity. The editor of the *Zoologist* records that he has notes of "finding newly-born squirrels on March 21 (three young), April 9 (three young), April 26 (four young), and April 29 (two young). Those found at the end of March and beginning of April were naked and blind; those taken at the end of April were about three parts grown." According to the editor, "the old squirrels, in case of danger, remove the young from the nest, or 'drey,' to some hole in a tree, whither they carry them one by one in the mouth, just as a cat carries her kitten. One of the prettiest sights in the world is to see an old squirrel teaching a young one to jump."

IT has been recently shown by Dr. Marcet (*Arch. des Sciences*) that different persons respire different volumes of air to furnish to the body the oxygen required, and to yield a given weight of carbonic acid. Thus, to produce one gramme of carbonic acid, three persons were found to need, on an average, 9·29, 10·51, and 11·30 litres of air respectively. The first was 23 years of age, the third 60; and no doubt the less the air required for a given combustion, the better the conditions of respiration. The influence of food on formation of carbonic acid in the body begins in the first hour after a meal, and increases for two or three hours, the period of maximum respiration of CO_2 varying in this time. After a certain time, the weight of CO_2 expired decreases more rapidly than the required volumes of air decrease. The influence of local variations of air-pressure appears in less air being needed, for a given amount of CO_2, with low pressures than with high; but the degree of the influence varies in individuals.

AS it is possible that circumstances may arise during the next few years which may make it desirable to lay down a cable to the Andamans, the Meteorological Reporter to the Government of India has, according to the Indian press, recorded his opinion that it is almost absolutely necessary for the warning of the Bengal and Madras coasts that such a cable should be laid, and that he believes it to be almost essential to the proper and complete protection of the Burmah coast. Observations from Port Blair would rarely give earlier information of the commencement of disturbed weather in the Bay than is shown by the present battery of stations round the Bay. They would, however, in the majority of cases, give earlier definite information as to the storm in process of formation, whether it was a feeble or powerful one, or of small or large extent. They would also enable the probable path of the storm to be determined with greater certainty than can be done under the present arrangements. Telegraphic communication to Port Blair would hence tend to make the work of storm-warning more exact and trustworthy, and enable the Meteorological Department to state with greater certainty than at present the probable path or intensity of storms approaching the Bengal and Madras coasts. It would also give certain and strong indications of the formation of storms in the Andaman Sea, and enable the Department to warn the Burmah coast more efficiently than at present.

Prof. Dubois, of Berne, has been lately studying the physiological action of electric currents and discharges (*Arch. des Sciences*); and he has some interesting observations on the human eye, which, it is known, has luminous sensations under the action of galvanic currents. Sudden variations of intensity, especially at making and breaking the circuit, produce such flashes. With a moistened plate at the nape of the neck, and a pad on the eye, a slight flash was distinctly perceived even with a Leclanché cell of about 1·20 volt, and measuring in the galvanometer 4/100 of a milliampere. Raising the intensity to five-tenths, the observer could tell which pole was applied to the eye. On the other hand, the retina responds much less readily to discharges from condensers or induction coils. Not till a capacity of 0·037 microfarad and a tension of 21 volts was reached, was a true retinal flash perceived; and not even with 10 microfarads were the durable sensations characteristic of the two poles produced. The retina reacts to quantity.

An important new mineral, titanate of manganese, $MnTiO_3$, isomorphous with the well-known titanate of iron or titanic iron ore, $FeTiO_3$, has been discovered in the neighbourhood of Harstigen by Dr. Hamberg, and is described by him in the current number of the *Geol. Foeren. i. Stockholm* (12 Band, 598). The crystals were found embedded in calcite, and were readily isolated by removing the latter by means of dilute hydrochloric acid, the titanate of manganese being but very slightly attacked by acids. They were of tabular habitus, and possessed an exceptionally brilliant metallic lustre of a deep red tint. In thin sections the colour is seen to be orange-red, and is not pleochroic. When ground to powder a yellow ochre is produced, possessing a slight tinge of green. The crystals are as hard as apatite, and have a specific gravity of 4·537. They consist of almost pure manganous titanate, containing 50·49 per cent. of titanium dioxide and 46·92 per cent. of manganous oxide. Like titanic iron ore, the crystals belong to the rhombohedral-hexagonal system, the basal plane (0001) being the principal face, upon which the crystals are tabular. The edges of the tables exhibit faces of the negative rhombohedron $(02\bar{2}1)$, and the deuteroprism $(11\bar{2}0)$. The angle of the rhombohedron is very near that of titanic iron ore, and if the ratios of the axes of the two compounds are compared the similarity is very striking indeed. In the case of titanate of iron $a : c = 1 : 1·385$, and in the new mineral $a : c = 1 : 1·369$. Indeed, the isomorphism is even more complete, for titanic iron ore is not only rhombohedral, but is also tetartohedral; and a study of the corrosion figures produced by boiling hydrochloric acid upon the basal plane of titanate of manganese shows that this mineral is likewise tetartohedral. The optical properties of the new mineral are also rather remarkable. The refractive index of the ordinary ray for sodium light is no less than 2·4810, almost as high as the refractive index of the diamond, for the extraordinary ray somewhat less, 2·21. The dispersion is likewise large, the respective indices for the lithium red and thallium green rays being 2·44 and 2·54 respectively. The crystals cleave readily parallel to the rhombohedral and prism faces. Dr. Hamberg gives the name pyrophanite to the mineral. He is of opinion that specular iron ore, hæmatite, Fe_2O_3, which he writes $FeFeO_3$, is also truly isomorphous with pyrophanite, and this assumption is certainly supported by the similarity of the axial ratios, those of hæmatite being $a : c = 1 : 1·359$. The ruby and sapphire, Al_2O_3, may perhaps also be included in the series, for their axial ratios are almost identical with those of pyrophanite, $a : c = 1 : 1·363$. Hæmatite, crystallized alumina, pyrophanite, and titanic iron ore, would thus form an isomorphous series with gradually ascending axial ratios.

The additions to the Zoological Society's Gardens during the past week include a Serval (*Felis serval* ♀) from East Africa, presented by Mr. D. Wilson; two Red-backed Weaver Birds (*Quelea sanguinirostris*) from West Africa, presented by Mrs. Hastings; a White Frog (*Rana temporaria*, var.), British, presented by Mr. W. Hannaford; a Rhesus Monkey (*Macacus rhesus* ♂) from India, a West African Python (*Python sebæ*) from West Africa, deposited; a Snow Leopard (*Felis uncia*) from the Himalayas, a Collared Peccary (*Dicotyles tajaçu* ♀) from South America, two North American Turkeys (*Meleagris gallo-pavo* ♂ ♀) from North America, six Shore Larks (*Otocorys alpestris*), British, purchased; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Photographic Spectrum of the Sun and Elements.—The *Johns Hopkins University Circular*, No. 85, issued last month, contains Prof. Rowland's report of progress in spectrum work. The spectra of all known elements, with the exception of a few gaseous ones, or those too rare to be yet obtained, have been photographed in connection with the solar spectrum, from the extreme ultra-violet down to the D line, and eye-observations have been made on many to the limit of the solar spectrum. A table of standard wave-lengths of the impurities in the carbon poles extending to wave-length 2000, has been constructed to measure wave-lengths beyond the limits of the solar spectrum. In addition to this, maps of the spectra of some of the elements have been drawn up on a large scale, ready for publication, and the greater part of the lines in the map of the solar spectrum have been identified. The following rough table of the solar elements has been constructed entirely according to Prof. Rowland's own observations, although, of course, most of them have been given by others :—

Elements in the Sun, arranged according to Intensity and the Number of Lines in the Solar Spectrum.

According to intensity.		According to number.	
Calcium	Zirconium	Iron (2000 or more)	Magnesium (20 or more)
Iron	Molybdenum	Nickel	Sodium (11)
Hydrogen	Lanthanum	Titanium	Silicon
Sodium	Niobium	Manganese	Strontium
Nickel	Palladium	Chromium	Barium
Magnesium	Neodymium	Cobalt	Aluminium (4)
Cobalt	Copper	Carbon (200 or more)	Cadmium
Silicon	Zinc	Vanadium	Rhodium
Aluminium	Cadmium	Zirconium	Erbium
Titanium	Cerium	Cerium	Zinc
Chromium	Glucinum	Calcium (75 or more)	Copper (2)
Manganese	Germanium	Scandium	Silver (2)
Strontium	Rhodium	Neodymium	Glucinum (2)
Vanadium	Silver	Lanthanum	Germanium
Barium	Tin	Yttrium	Tin
Carbon	Lead	Niobium	Lead (1)
Scandium	Erbium	Molybdenum	Potassium (1)
Yttrium	Potassium	Palladium	

Doubtful Elements.

Iridium, Osmium, Platinum, Ruthenium, Tantalum, Thorium, Tungsten, Uranium.

Not in Solar Spectrum.

Antimony, Arsenic, Bismuth, Boron, Nitrogen, Cæsium, Gold, Indium, Mercury, Phosphorus, Rubidium, Selenium, Sulphur, Thallium, Praseodymium.

With respect to these tables Prof. Rowland adds :—"The substances under the head of 'Not in the Solar Spectrum,' are often placed there because the elements have few strong lines or none at all in the limit of the solar spectrum when the arc spectrum, which I have used, is employed. Thus boron has only two strong lines at 2497. Again, the lines of bismuth are all compound, and so too diffuse to appear in the solar spectrum. Indeed, some good reason generally appears for their absence from the solar spectrum. Of course, there is little evidence of their absence from the sun itself; were the whole earth heated to the temperature of the sun, its spectrum would probably resemble that of the sun very closely."

The powerful in strument used at Baltimore for photographing spectra, and the measuring engine constructed to fit the photographs so that its readings give the wave-lengths of lines directly within 1/100 of a division on Angström's scale, give the foregoing results a weight superior to many others published.

A VARIABLE NEBULA.—Mr. Roberts has recently shown that the nebula in Andromeda is variable (*Monthly Notices R.A.S.*, January 1891). Previous to this, the only nebula whose variability could be accepted as proved was N.G.C. 1555, in the constellation Taurus, discovered by Dr. Hind on October 11, 1852, and observed by D'Arrest four times in 1855-56, but which has since been looked for in vain by a number of astronomers.

In *Comptes rendus* for March 2, M. Bigourdan has a communication on the variability of a nebula, N.G.C. 1186, situated near Algol. Sir William Herschel discovered this nebula in 1785 (Phil. Trans., 1789, p. 247). Sir John Herschel observed it in 1831 (Phil. Trans., 1833, p. 376); but Lord Rosse looked for it without success in 1854 and 1864 (Phil. Trans., 1861, p. 745, and Trans. Roy. Dub. Soc., vol. ii. p. 34). On November 8, 1863, D'Arrest could not see the nebula, although he looked for it assiduously and the atmospheric conditions were most favourable. He therefore concluded that the object did not exist ("Siderum Neb.," p. 56). M. Bigourdan finds that the nebula is again visible in the position indicated by the two Herschels, viz. R.A. 2h. 54m. 20s., Decl. + 42° 10', he having observed it on January 31 and February 26 of the present year. It is difficult to believe that this object could have escaped the scrutiny of Lord Rosse and D'Arrest in 1854, 1863, and 1864; hence the variation is probably real, and merits the attention of astronomers. The nebula may be easily found, as it is very near the binary 694 B.D + 42° (1123 G.C.), the position of which for 1891 is R.A. 2h. 58m. 6s., Decl. + 42° 29'. Photographic evidence of the variability would be most interesting.

FEBRUARY SUNSHINE.

AS everybody is aware, February 1891 was remarkable for its excessive dryness and for the absence of anything approaching stormy weather. Many will also be disposed to remember it as a month in which we had more than our ordinary share of fog, particularly during the second half, when fog seemed to be very general over the eastern and south-eastern parts of England. It will be a surprise, therefore, to learn that, in spite of the exceptionally foggy character of the month, the amount of bright sunshine which was registered, over England especially, was altogether abnormally large. And what is still more surprising is that the second half, which included the days when the fogs were reported as most dense and widespread, was very much more sunny than the first half. According to the statistics published weekly by the Meteorological Office, the average duration of bright sunshine in the twelve forecasting districts for the month of February is 89 hours in the Channel Islands, 72 hours in the south of Ireland, 46 hours in the extreme north of Scotland, and in the other nine districts it varies between 60 and 69 hours. In the period now under review, however, the recorders registered 167 hours in the Channel Islands; 126 hours in England, S.W.; 108 hours in England, S.; 102 hours in England, E., and Midland Counties; 97 hours in England, N.W.; 90 hours in Scotland, E.; 88 hours in England, N.E.; 80 hours in Ireland, S.; 73 hours in Ireland, N.; 59 hours in Scotland, W.; and 54 hours in Scotland, N. With the exception, therefore, of the West of Scotland, where there was a deficiency of one hour, every district showed a considerable excess of sunshine, England and Wales taken as a whole having 104 hours against an average of 65 hours, the increase being 60 per cent., the south-western counties showing an excess of 83 per cent.; while the Channel Islands had 88 per cent. more than the average. The records for the individual stations in the several districts are even more interesting. Out of forty-one stations the following twenty had at least 100 hours of bright sunshine for the month, the excess above the average being shown in the second column of figures. The corresponding figures for the second half of the month are given in the third and fourth columns:—

Station.	Feb. 1-28. Hours.	Excess. Hours.	Feb. 15-28. Hours.	Excess. Hours.
Jersey	167	78	120	72
Hastings ...	135	53	105	59
Plymouth ...	134	58	97	53
Torquay ...	128	?	82	?
Falmouth ...	126	50	80	37
Pembroke ...	125	48	94	51
Cirencester ...	124	55	81	42
Southamptom	123	53	88	49
Cullompton	120	54	81	43
Llandudno ...	120	56	93	58
Eastbourne ...	118	?	89	?
Stowell ...	118	?	86	?
Churchstoke	115	54	88	54
Aberdeen ...	113	?	72	?
Hillington ...	104	40	71	35
Cambridge ...	103	37	68	31
Newton Reigny	102	45	79	47
Marchmont ...	101	?	73	?
Geldeston ...	100	31	67	28
Rothamsted	100	?	71	?

A glance at the above table shows that this wonderful outburst of sunshine was not confined to the south coast stations. Llandudno had a better record than Eastbourne, Aberdeen came very near, while Newton Reigny and Marchmont, both northern stations, fall into the list of high totals. It will be seen from the second column that the average daily excess ranged from more than 1 hour at Geldeston to nearly 3 hours at Jersey. The only station in the kingdom which had a deficiency of sunshine was Glasgow, the total duration being 34 hours, or 12 less than the normal. Fort Augustus had 40 hours, but the average is not known for this station; London comes next with 42 hours, and, small as was the total, it was 5 hours above the average. Ireland did not have a large excess: Dublin, with 91 hours, was 22 hours to the good, and Armagh, with 73 hours, was 12 hours above the average; but elsewhere the normal was exceeded by from 2 to 10 hours only. The figures quoted for the month as a whole are quite exceptional for so early a period in the year; but the third and fourth columns of the table show that about three-fourths of the total sunshine was registered in the last fourteen days. Indeed, in the first week the amount recorded fell below the average in eight out of the twelve districts, and in the following week four districts were still deficient; and this fact accounts for the excess for the last fortnight at Hastings, Pembroke, and other places being actually larger than that for the entire month. During the fourteen days, 15-28, there were several stations other than those included in the above table which had from 50 to 80 hours of sunshine. At Dublin, Durham, Geldeston, and Oxford the excess was as much as 2 hours per day above the average; Llandudno and Hastings were favoured with an extra 4 hours per day, and Jersey rather more than 5 hours. For London the Meteorological Office gives 18·9 hours in the first half, and 23 hours in the second half of the month; total, 41·9. The Royal Observatory had respectively 24·2 hours and 46·5 hours; total, 70·7. The influence of the fog on the western districts is seen in the difference for the last fortnight, when the south-eastern quarter had rather more than twice as much sunshine. The Greenwich record is an excess of 29 hours on the average for the month, or a little over 1 hour per day. This is very good under the circumstances, but Londoners cannot help envying the more fortunate districts beyond the limits of metropolitan fogs; even distant Aberdeen, although entitled to less sunshine owing to latitude, having nearly three times as much brightness as western London.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 12.—"On the Organization of the Fossil Plants of the Coal-Measures. Part XVIII." By Prof. W. C. Williamson, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester.

On three preceding occasions the author has directed attention to the existence in the older Carboniferous rocks of a remarkable form of fructification which seemed to belong to the Calamarian family of plants, though presenting features distinct from any that had hitherto been described. In the first instance, in 1871, he placed this fructification in Sternberg's provisional genus *Volkmannia*, under the name of *V. Dawsoni*. Some small fragments of the same type, obtained at a later period by the late Prof. Weiss, of Berlin, led him to identify the plant with Binney's hitherto very obscure genus *Bowmanites*, an identification which is accepted by Prof. Williamson. Still more recently, a number of additional specimens have been obtained from the Ganister Carboniferous beds of Lancashire and Yorkshire, which not only throw further light upon the plant, but have made it possible to re-write its history in an almost complete form.

Like all the other Calamariæ, *Bowmanites* was a plant with a distinctly articulated stem, each node of which bore a verticil of lateral appendages. In the vegetative organs each of these nodal appendages consisted of a verticil of the linear, uninerved leaves characteristic of the old, ill-defined genus *Asterophyllites*. In the fructification these foliar verticils are replaced by a broad circular disk, the margin of which sustained a verticil of leaf-like "disk-rays." These rays can scarcely, at present, be identified with true leaves, since they have not only no midrib, but they seem to contain no traces whatever of a vascular bundle.

The centre of the axis of the strobilus is occupied by a conspicuous bundle of barred and reticulated tracheids of the scalariform type, the transverse section of which bundle is triangular, with concave sides. Each of the three prominent angles is abruptly and broadly truncated. A thin inner cortex seems to have originally surrounded this bundle, but all traces of its tissues have disappeared. The thick outer cortex is composed of a mixture of rather coarse, strongly defined parenchymatous and prosenchymatous cells. At each node this cortex expands into the lenticular disk already referred to. This disk is thickest at its inner border, thinning gradually towards its outer margin, where it subdivides into the verticil of elongated disk-rays already mentioned. Though no vascular bundles can be discovered connecting the central axial one with the surrounding disk, some such must have once existed, since we find them both in the cortex of the internodes and in the nodal disks.

The entire upper surface of each disk has given off numerous very slender sporangiophores, destined to reach three or four concentric circles of sporangia, which were arranged in a single plane in the internodal interval between each two disks. Each sporangiophore, unlike what is usual amongst the Calamariæ, only sustained a single sporangium. In order to reach the more external ranges of the latter organs, the sporangiophores were prolonged outwards in a distinct layer between the upper surface of the disk and the sporangia which rested upon it. Not only was this the case, but when each sporangiophore reached the sporangium with which it was destined to become organically united, it did not at once do so; but it passed under, and even beyond that organ, where it bent back upon itself and became united to the sporangium on its distal side. The outer, or epidermal, layer of the sporangium was merely an extension of that of the sporangiophore.

The numerous spores of *Bowmanites* have also a distinctive form. Each has a rather thin exosporium, but this is thickened along a few reticulate lines, and from each junction of these reticulations a strong radiating spine is projected. It is in the very distinctive features of these reproductive organs that the marked generic individuality of *Bowmanites* chiefly resides.

The second plant described in the memoir, under the name of *Rachiopteris ramosa*, is one of the several fern-like organisms which the author has included in his provisional group of Rachiopterides. Considerable doubt exists respecting the true affinities of at least some of these plants. The one now described may prove to be a less hirsute, more fully developed condition of the *Rachiopteris hirsuta* described by the author in his Memoir XV.

Chemical Society, January 15.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—On magnetic rotation, by W. Ostwald. The magnetic rotation of organic compounds, according to Perkin, is an additive function of their composition, and equal to the sum of the rotations of the components, but this is not the case with the rotation of inorganic compounds, which is usually found greater than that calculated on such an assumption. The author points out that these exceptional values are only obtained in the case of electrolytes, and that they must therefore be referred to a fundamental difference between the constitution of electrolytes and that of non-conductors. The author claims that the facts established with regard to magnetic rotation are in perfect accordance with Arrhenius's theory of electrolytic dissociation, and that any exceptional values in the magnetic rotation of electrolytes are due to the occurrence of electrolytic dissociation.—The vapour density of ammonium chloride, by Frank Pullinger and J. A. Gardner. The authors have made experiments on the vapour density of ammonium chloride at various temperatures. The apparatus used was that of Victor Meyer. The ammonium chloride was vaporized into an atmosphere of ammonia and into air. At a moderate red heat, and at 448° C., complete dissociation took place; at 360° C. in an atmosphere of ammonia it was not wholly dissociated. It was found impossible to vaporize the salt into ammonia at 300° C.—Chlorinated phenylhydrazines, by J. T. Hewitt.—A new modification of phosphorus, by H. M. Vernon. Observations on the rate of rise of temperature of phosphorus and other experiments have led the author to the conclusion that one or other of two different modifications of phosphorus may result when fused phosphorus solidifies.

February 5.—Dr. W. J. Russell, F.R.S., President, in the chair.—It was announced that the following changes in the Council list were proposed by the Council: President, Prof. Crum Brown, *vice* Dr. Russell; Vice-Presidents: Mr. S. Pattinson and Prof. Tilden, *vice* Profs. Crum Brown and Mallet; Foreign Secretary: Prof. Meldola, *vice* Prof. Japp; Members of Council: Dr. Atkinson, Mr. Boverton Redwood, Prof. Perkin, and Dr. J. Voelcker, *vice* Mr. Cross, Prof. Dunstan, Prof. Meldola, and Dr. Plimpton.—The following papers were read:—On the formation of an explosive substance from ether, by Prof. P. T. Cleve. The author describes a remarkable explosion occasioned by impurities in commercial ether. On distilling about 250 c.c. of the ether, it was noticed that a viscid residue remained; after drying on the water-bath, this formed a transparent, amorphous mass. Prof. Cleve states that, having poured a little water on to the substance, he proceeded to stir it gently with a rounded glass rod; this occasioned a most violent explosion. The explosive substance was probably ethyl peroxide, as it gave the well-known perchromic coloration, besides liberating iodine and discharging oxygen from silver oxide; it was at once destroyed by reducing agents.—Does magnesium form compounds with hydrocarbon radicles?, by Prof. Orme Masson and U. T. M. Wilsmore, University of Melbourne. The authors state that they have in vain endeavoured to prepare magnesium ethide (1) from magnesium and ethyl iodide; (2) from magnesium-copper couples and ethyl iodide; (3) from an alloy of magnesium and sodium and ethyl iodide; (4) from magnesium and zinc ethide; (5) from magnesium and mercury ethide; and (6) from anhydrous magnesium iodide and zinc ethide.—Compounds of the oxides of phosphorus with sulphuric anhydride, by R. H. Adie. The author has endeavoured to prepare compounds of phosphorus similar to those which the other elements of the group form with sulphuric anhydride. By the action of sulphuric anhydride on H_3PO_3, he obtained a compound very nearly of the composition

$$H_3PO_4 \cdot 3SO_3 = (SO_3H)_3PO_4.$$

Sulphuric anhydride and phosphorus were found to interact violently to form a compound represented by the formula $3P_2O_4 \cdot 2SO_3$.—The combustion of magnesium in water vapour, by G. T. Moody. The author describes a way in which the combustion of magnesium in water vapour may be performed as a lecture experiment, by carrying out the operation in a piece of hard glass tube, about 10 mm. wide and 250 mm. long, bent at an angle of 120°, so as to leave one arm nearly twice as long as the other. The shorter arm is inserted through a cork closing the mouth of a "tin can" or other convenient vessel in which steam can be generated; and the longer arm, which contains a few strips of magnesium ribbon, is connected by a fairly wide delivery-tube with the pneumatic trough, at which the liberated hydrogen may be collected. The air being displaced by a slow current of steam, the arm of the tube containing the magnesium is heated by means of a Bunsen burner; this is then replaced by a blowpipe flame, which is moved about so that the whole arm becomes very hot; then, on allowing the flame to impinge on a portion of the tube against which the magnesium rests, the metal takes fire and burns with great brilliancy.

Geological Society, February 20.—Annual General Meeting.—Dr. A. Geikie, F.R.S., President, in the chair.—The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1890. In the former the Council once more congratulated the Fellows upon the continued prosperity of the Society, as evinced by its increasing number and by the satisfactory condition of its finances. The Council's Report also referred to the publication of the late Mr. Ormerod's Third Supplement to his Index to the Publications of the Society, to the editing of Nos. 183 and 184 of the Journal by Prof. T. Rupert Jones, to the deaths of the late Foreign Secretary and the late Assistant-Secretary, and in conclusion enumerated the awards of the various Medals and proceeds of Donation Funds in the gift of the Society. The Report of the Library and Museum Committee included a list of the additions made during the past year to the Society's Library, and announced the completion of the glazing of the Inner Museum.—After the presentation of the Medals and the balance of the Wollaston Fund, and the Murchison and Lyell Geological Funds, the President read his anniversary address, in which he first gave obituary notices of several Fellows, Foreign Members, and Foreign Correspondents deceased since the last annual meeting, including the late Foreign Secretary, Sir Warington W. Smyth, the late Assistant Secretary, Mr. W. S. Dallas, M. Edmond Hébert and M. Alphonse Favre (Foreign Members, both elected in 1874), Mr. Wm. Davies, Mr. Robert Wm. Mylne, Mr. Samuel Beckles, Dr. H. B. Brady, Mr. Samuel Adamson, and Prof. Antonio Stoppani (Foreign Correspondent, elected in 1889). He then dealt with the history of volcanic action in Britain during the earlier ages of geological time. He proposed to confine the term "Archæan" to the most ancient gneisses and their accompaniments, and showed that these rocks, so far as we know them in this country, are essentially of eruptive origin, though no trace has yet been found of the original discharge of any portion of them at the surface. Passing to the younger crystalline schists, which he classes under the term "Dalradian," he pointed to the evidence of included volcanic products in them throughout the Central Highlands of Scotland and the north of Ireland. The Uriconian series of Dr. Callaway he regarded as a volcanic group, probably much older than the recognized fossiliferous Cambrian rocks of this country. The Cambrian system he showed to be eminently marked by contemporaneous volcanic materials, and he discusssd at some length the so-called pre-Cambrian rocks of North Wales. He reviewed the successive phases of eruptivity during the Arenig and Bala periods, and described the extraordinary group of volcanoes in Northern Anglesey during the latter time. The volcanoes of the Lake District were next treated of, and reference was made to the recent discovery by the Geological Survey that an important volcanic group underlies most of the visible Lower Silurian rocks in the south of Scotland. The last portion of the address was devoted to an account of the volcanoes of Silurian time in Ireland, and it was shown that during the Bala period a chain of submarine volcanic vents existed along the east of Ireland from County Down to beyond the shores of Waterford; while in Upper Silurian time there were at least two active centres of eruption in the extreme west of Kerry and in Mayo.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—Council: Prof. J. F. Blake; W. T. Blanford, F.R.S.; Prof. T. G. Bonney, F.R.S.; James Carter; James W. Davies; John Evans, F.R.S.; L. Fletcher, F.R.S.; C. Le Neve Foster; A. Geikie, F.R.S.; A. Harker; J. C. Hawkshaw; H. Hicks, F.R.S.; G. J. Hinde; W. H. Hudleston, F.R.S.; Prof. T. McKenny Hughes, F.R.S.; J. W. Hulke, F.R.S.; J. E. Marr; H. W. Monckton; F. W. Rudler; J. J. H. Teall, F.R.S.; W. Topley, F.R.S.; Prof. T. Wiltshire; H. Woodward, F.R.S. Officers:—President: A. Geikie, F.R.S. Vice-Presidents: W. T. Blanford, F.R.S.; Prof. T. G. Bonney, F.R.S.; L. Fletcher, F.R.S.; W. H. Hudleston, F.R.S. Secretaries: H. Hicks, F.R.S.; J. E. Marr. Foreign Secretary: J. W. Hulke, F.R.S. Treasurer: Prof. T. Wiltshire. The thanks of the Fellows were unanimously voted to the retiring Members of Council: Prof. A. H. Green, Rev. Edwin Hill, Major-General C. A. MacMahon, E. T. Newton, and Rev. G. F. Whidborne.

February 25.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—A contribution to the geology of the Southern Transvaal, by W. H. Penning. The following table shows the author's classification of the sedimentary rocks of this region, as compared with those of Messrs. Dunn and Stow and Prof. Rupert Jones:—

DUNN. (Map, 1887.)		STOW.	T. R. JONES.		PENNING.		
Coal-measures: *Upper Karoo* (formerly Stormberg Beds, above Upper Karoo).	SILURIAN. CARBONIFEROUS TO TRIASSIC.	*Upper Karoo.*	*Lower Karoo.*	TRIASSIC FORMATION.	*High Veldt Beds.*		OOLITIC.
Kimberley Shales: *Lower Karoo* (formerly Upper Karoo).		....			*Kimberley Beds.*		
					Klip River Series.	*Megaliesberg Formation.*	DEVONIAN.
Lydenburg Beds.					*Witwatersrand Series.*		
Namaqualand Schists.					*De Kaap Valley Beds.*	SILURIAN.	

The De Kaap Valley beds consist of schists, shales, cherts, and quartzites, with some conglomerates, chloritic and steatitic beds of great thickness, faulted, according to the author, against the granite. They contain a few obscure corals, and are provisionally referred to the Silurian. The Witwatersrand series consists chiefly of sandstones, shales, cherts, and quartzites, having an estimated thickness of 18,000 feet, possibly formed in a hollow of the granite, and perhaps of marine formation. The Klip River series is formed of shales, flagstones, cherts, and quartzites, with numerous interstratified traps, and is at least 18,000 feet thick. Near its base is the "Black Reef," and a chalcedonite like that described by the author in connection with the Lydenburg district, which confirms his opinion that this area is formed of part of the Megaliesberg formation. The base of the series is generally conformable to the underlying rocks. The whole of the lower half of the Megaliesberg formation is let down against the north side of the granite south of Pretoria. The author divides the formation, which he described in 1884 under the heading of "High-level Coal-fields of South Africa," into the Kimberley beds and the High Veldt beds. The former thin out eastward, and are overlapped by the latter, the estimated thickness of which is 2300 feet. A volcanic rock overlies the coal-formation. Near the base of the formation is a bed of loose, calcareous, sandy clay, inclosing many water-worn pebbles, some of large size, derived from the quartzites and "bankets" of the underlying formation. The author is convinced that the region was under glacial influences at some time during the long period which intervened between the deposition of the Megaliesberg formation and of the coal-bearing rocks of the High Veldt, which latter, he maintains, are certainly Oolitic; the latter contain *Glossopteris* (?) and fishes which he considers to be nearly allied to *Lepidotus valdensis*, the latter being from the Free State. The High Veldt rocks are of fluviatile origin, and there appears to have been continuity of fluviatile denudation on the close of the Oolitic period until now. The reading of this paper was followed by a discussion, in which Mr. Gibson, Mr. Alford, Prof. Rupert Jones, Mr. Smith Woodward, and Dr. Blanford took part.—On the lower limit of the Cambrian series in North-West Caernarvonshire, by Miss Catherine A. Raisin. Communicated by Prof. T. G. Bonney. A discussion followed, in which Prof. Blake, Dr. Hicks, Prof. Hughes, Prof. Bonney, the President, and Mr. Peach took part. —On a Labyrinthodont skull from the Kilkenny Coal-measures, by R. Lydekker.

Paris.

Academy of Sciences, March 2.—M. Duchartre in the chair.—Observations of asteroids, made with the great meridian instrument of Paris Observatory during the second quarter of 1890, by Admiral Mouchez. The asteroids which have been observed are Pallas, Ceres, Juno, and Melete.—On metallic reflection, by M. H. Poincaré. Further objections are adduced against MM. Cornu and Potier's interpretation of Herr Wiener's experiment on the direction of vibration in a polarized beam of light (*Comptes rendus*, January 6 and February 9).—On an attempt at oyster-culture in the experimental fish-pond of the Roscoff Laboratory, by M. de Lacaze-Duthiers. Oysters measuring from 1·5 to 2 cm. in April 1890 had attained a greatest length of 5 cm. in June, and in September 1890 and March 1891 reached a length of 7 and 8 cm. Of the 8500 young oysters placed in the fish-pond, only 50 died during the severe weather experienced this winter. This is an extremely low rate of mortality when compared with the loss at ordinary oyster-beds. The reason of this success is probably due to the fact that the boxes containing the oysters could be protected from the cold air during low tides by running them down into the sea by means of chains.—On the composition of drainage waters, by M. P. Dehérain.—On a variable nebula, by M. G. Bigourdan. (See Our Astronomical Column.)—History of apparatus for the measurement of base-lines, by M. A. Laussedat.—On the transformation of a geometrical demonstration, by M. A. Mannheim.—On the minima surfaces limited by the four corners of an irregular quadrilateral, by M. Schoenflies.—Results of actinometric observations made at Kief, in Russia, in 1890, by M. Savélief. Observations made from the beginning of June to the end of November give the following results:—(1) In summer and in autumn, the real value of the absolute heat intensity of solar radiation, for an apparently clear sky, reaches a maximum about 10 o'clock; a secondary maximum occurs between 1 p.m. and 2 p.m.; between these two maxima a well-defined minimum may be observed at midday. In autumn, the calorific intensity of solar radiation is greatest between 9 a.m. and 2 p.m., and reaches a higher value than in summer. (2) In summer, the hourly mean of absolute intensities—that is, one-sixtieth of the quantity of heat received normally in one hour by a surface having an area of 1 sq. cm.—reaches a maximum about 10 a.m., and a secondary maximum about 5 p.m. In autumn, the curves are more regular than in summer, and present only a single maximum about 11 p.m.—Remarks on M. Savélief's communication, by M. A. Crova. Variations similar to those described by M. Savélief have been registered on M. Crova's actinometer at Montpellier.—On duplex-beating metallic reeds, by M. A. Imbert.—On some alkaline derivatives from erythrite, by M. de Forcrand. The author has obtained crystallized alkaline erythrates by the action of erythrite on aqueous solutions of potash and soda.—On the dyeing of cotton, by M. Léo Vignon. —On a vegetable hæmatin, aspergillin—a pigment of the spores of *Aspergillus niger*, by M. Georges Linossier. The author shows that the fruits of *Aspergillus niger* contain a pigment having the same characteristics as the hæmatin of the blood of animals.—Idiosyncrasy of certain species of animals for carbolic acid, by M. Zwaardemaker. Small doses of carbolic acid have no effect on dogs or rabbits, but intoxicate and subsequently kill cats and rats, the deaths being always preceded by convulsions lasting several hours.—On the hepatic epithelium of the testacle, by M. J. Chatin.—On the conglomerate of Gourbesville containing fossilized bones, by M. A. de Lapparent.—On the age of the strata cut by the Panama Canal, by M. H. Douvillé. Some fossils that have been collected in Panama Canal cuttings belong to the Miocene and Eocene periods.—On the relation of earth tremors to the seasons, by M. de Montessus. It has been asserted that earth tremors occur more frequently in winter than in summer, and are therefore connected with meteorological phenomena. M. Montessus has investigated 63,555 tremors with respect to their time of occurrence, and finds that the astronomical seasons bear no relation to them.—On the action of running water on some minerals, by M. J. Thoulet.

Brussels.

Academy of Sciences, January 10.—M. Stas in the chair. —M. Folie was elected President for 1891.—Researches on the velocity of evaporation of liquids at the temperature of ebullition, by M. P. de Heen. The author has used specially devised apparatus for the determination of the influence exercised on the velocity of evaporation: (1) by the velocity of a dry current acting on its surface; (2) by temperature; (3) by the nature of the liquid; (4) by the nature of the gaseous current; (5) by the pressure of the gas in motion. He finds that the velocity of evaporation is proportional to the square root of the velocity of the gaseous current, and that for a given velocity of the current the quantity of liquid vaporized is proportional to the vapour tension. Experiments on water, benzine, chloroform, acetic acid, alcohol, ethyl bromide, carbon bisulphide, and ether indicate that, *cæteris paribus*, the amount of liquid vaporized varies as the product of the vapour tension into the molecular weight. The interior friction of hydrogen, carbon dioxide, and air are respectively represented by 95, 163, and 194. Experiments with these gases as currents show that the vaporizing influence is greater when the interior friction of the gas is greater. The amount of liquid vaporized appears to depend on the velocity of the current of gas, but is independent of the pressure.—Preliminary notes on the organization and development of different forms of Anthozoa, by M. Paul Cerfontaine. —Crystallographical notice on the monazite of Vil-Saint-Vincent, by Dr. A. Franck.

CONTENTS.

PAGE

THURSDAY, MARCH 19, 1891.

WOOD'S HOLL BIOLOGICAL LECTURES.

Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl, in the Summer Session of 1890. Pp. v. + 250. Illustrated. (Boston, 1891.)

AS we are informed in the preface, the ten addresses and lectures contained in this little volume were, with two exceptions, delivered at the Marine Biological Laboratory of Wood's Holl, as a continuation of a previous course. The volume is issued under the editorship of Prof. C. O. Whitman, the Director of the Laboratory, but each article appears to have been seen through the press by its respective author, as we notice some differences in the degree to which phonetic spelling has been adopted.

It is stated that one object of these lectures—which deal with some of the higher problems of biology, and among them those with a more or less pronounced metaphysical leaning—was to bring specialists into mutual communication and helpful relations with one another, and, at the same time, to make their work and thought intelligible and useful to beginners. It strikes us, however, that some of these lectures are altogether over the heads of most beginners; this being, indeed, practically acknowledged in a later paragraph of the preface. And, considering the generally practical tendency of the American mind, it also strikes us as somewhat remarkable that in scientific circles there should be such a marked tendency towards speculations which, to put it mildly, contain such a large amount of theory based on such an extremely small modicum of fact.

The lectures cover a very wide expanse of ground—from the Gastræa theory to Weismann on the origin of death, and from the relationship of the sea-spiders to ocean temperatures and currents. All of them are, indeed, practically reviews of the current opinions on the topics with which they respectively deal; and to review them critically would therefore be practically reviewing a review. Moreover, since each lecture or article is the work of a specialist on a more or less highly abstruse subject, it would require a committee of specialists on the subjects dealt with to venture on such a critical review. We shall, therefore, content ourselves with giving the titles of the various lectures, and calling attention to those points which appear to us to be of more than average interest. We heartily commend the volume as a first-rate *résumé* of the current opinions regarding several interesting biological problems, and we shall look forward to seeing it followed next year by a second volume containing the lectures which, we presume, are to be delivered during the coming summer.

The first lecture, on "Specialization and Organization," is by the editor; who first shows how specialization is met with in nature, and then mentions how necessary it is for the right study of nature. While, however, admitting the necessity of specialization in natural studies, the author urges the importance of organization and co-operation. Co-operation, indeed, is considered to be fairly well up to the requirements of the times, owing to the number and efficiency of our scientific journals; but the need for organization is strongly urged, and the author waxes eloquent on the advantages which would be derived from the establishment and endowment of a national biological station in the United States.

In the course of this lecture, Prof. Whitman refers to the recent Japanese experiments on *Hydra*. These tend to show that this creature is more specialized than was originally supposed to be the case; Trembley having omitted to notice that when turned inside out the Hydra again turns itself back to its normal condition, so that the functions of its inner and outer surfaces are not interchangeable, as was first supposed.

The second lecture is likewise by Prof. Whitman, and has for title "The Naturalist's Occupation." The chief object of the first part of this lecture appears to be to inculcate the importance of discovering what are really fundamental characters in animals, and of using these, and these only, in classification, which the lecturer urges should be based on genealogical lines. The Vertebrate affinities of the Ascidians are strongly insisted upon, the author even going so far as to say "that we are compelled to place them in the same great family;"—an opinion with which many zoologists will be by no means disposed to agree, even if they admit an affinity between the two groups. The second part of this lecture takes the special problem presented by the Vertebrate head—as to whether or no it is composed of a number of segments serially homologous with those of the body. After reviewing the original theory of the segmentation of the whole head, it is stated that embryology has now clearly shown that, although wide of the mark in its entirety, the theory rests on a fundamental basis of truth, there being conclusive evidence to show that at least the hinder part of the head is segmented, and the problem to decide being how far forwards this segmentation extends, or how many segments the head contains. The difficulty of determining which is head and which vertebral column in embryos and the Lancelet, is then referred to; this being followed by a discussion of the view that the fore-brain alone represents the original ancestral brain, while the mid- and hind-brain are formed from modified trunk-segments pressed into the service of the head. That this view is true, so far as concerns the mid- and hind-brain, is regarded as practically established: and the conflict is accordingly now restricted to the origin of the fore-brain. Here we are limited to three hypotheses: either the fore-brain is the inherited, unsegmented, ancestral brain of the Invertebrates; or it is an entirely new formation; or it represents a number of fused trunk-segments, among which the ancestral brain is unrecognizable. The third hypothesis is the one which appears to be steadily advancing in favour, but, as the author rightly observes, we are here mainly or entirely dealing with conjectures, and have at present no solid ground to work from. In regard to the genesis of the eyes of Vertebrates, it is stated that all the evidence points to their derivation from paired segmental sense-organs; and from this point of view the author seems inclined to support the doubt which Leydig has thrown on the pineal eye, since it is argued that the existence of such an organ would imply the coalescence of at least one pair of eyes, and we have no example of any analogous fusion in the Vertebrata.

The third lecture dealing with "Some Problems of

Annelid Morphology," is by Prof. E. B. Wilson. In this lecture the chief points dealt with relate to the larval Annelid form known as the *Trochophore*, and the subjects of metamerism and apical growth. The circumstance that the Trochophore corresponds only to the head of the adult Annelid, the trunk of the latter being formed as a bud growing from the lower end of the larva and then segmenting, and thus being a typical instance of apical growth, suggests that the trunk is a linear colony of sexual individuals budded off from the asexual head. And if this be so, then all metameric animals must be regarded as colonial organisms, comparable, so far as regards individuality, with a polyp-colony. Some zoologists have actually accepted this view, while others have propounded alternative hypotheses, none of which the author regards as satisfactory explanations of metamerism; and he concludes by stating that we are not at present in a position to offer any adequate intepretation of this difficult problem. With regard to the nature of the Trochophore, opinion is still undecided; but the author's own inclination is to regard it as a secondary production, and thus of no importance in genealogical questions: a similar view is expressed by Prof. T. H. Morgan in the seventh lecture.

We shall venture to pass over Prof. J. P. McMurrich's admirable summary of the Gastræa theory and its successors, in the fourth lecture, and pass on to the fifth and sixth of the series. These are on kindred subjects, the former being by Mr. E. G. Gardiner, and bearing the title of "Weismann and Maupas on the Origin of Death;" while the latter is by Prof. H. F. Osborn, of Princeton College, and is entitled "Evolution and Heredity." Prof. Osborn has given a more recent epitome of his views of the latter question in a paper read before the American Society of Naturalists at Boston, on December 31, 1890, entitled "Are Acquired Variations Inherited?"

Weismann's theory of the origin of death, our readers need scarcely be reminded, is based on his observations on the Protozoa, in which he finds that since one of these creatures reproduces itself by dividing into halves, each of which starts again as a young animal, there is normally no decay or death, but rather a potential immortality. In the Metazoa, on the other hand, each individual passes through a period of youth, adult life, and old age, finally ending its existence in death. And since the Metazoa have been derived from the Protozoa, it is argued that death is something acquired with the development of the one from the other. It is further urged that when death had once made its appearance it was received as a distinct advantage by the animal world, or nature. And we have further the well-known "continuity of the germ-plasm" theory, in which it is considered that the germ-cells of the Metazoa are those which have inherited the Protozoan immortality; the so-called somatic cells being, so to speak, an addendum to the germ-plasm which have lost their potential immortality; this loss being perhaps due to what is termed the neglect of natural selection, owing to the somatic cells (the body) being merely the protective case to the germ-plasm.

Mr. Gardiner states that since Prof. Weismann started this remarkable theory much opposing evidence has been brought against it; one of his strongest opponents being M. Maupas, of Algiers, who enters the lists by declaring that death and decay do occur normally among the Protozoa, and thus, if right, cutting away the very ground from Weismann's feet. This leads Mr. Gardiner to conclude that we are not at present entitled to regard the Protozoa as potentially immortal, nor to claim that the somatic cells of the Metazoa have no representatives in the Protozoa. It is added, "Nevertheless, it is too soon to declare that the idea that death is an adaptation is altogether erroneous"—a sentence that to our thinking tends to convey the idea that the lecturer considers that a theory based on false premises may yet probably be true.

Other objections to the theory are then considered; among which it will suffice to mention the one founded upon the various ages to which different animals attain; but the arguments employed appear somewhat difficult to follow, and not to advance the question much in one way or another.

Prof. Osborn's lecture is intimately connected with the preceding one, treating of the question whether the neo-Darwinism of Weismann or the neo-Lamarckism of Eimer best explains the phenomena of heredity, in which the chief *crux* is whether acquired characters are or are not transmissible. The neo-Lamarckians maintain that special and local variations in function and structure induced by environment and habit in the life of a parent tend to reappear in some degree in the offspring. On the other hand, Weismann and the neo-Darwinists contend that special individual variations are not transmitted from parent to offspring.[1] The lecturer states that a complete theory of heredity must account for the repetition phenomena (including reversions); for the non-repetition phenomena, or the appearance of new characters; and for the phenomena of physical transmission, which can be only worked out by the embryologist.

In regard to Weismann's views it should be observed that especial importance is attached to the "continuity of the germ-plasm," to which we have already alluded, and still more so to his doctrine of the isolation of the germ-cells from all influences which affect the body, or somatic cells; so that the former cannot be reached by slight acquired variations. Inheritance being the unbroken transmission of racial and ancestral characters by subdivision of the germ-plasm, only changes which affect the body as a whole can be added to the characteristics of the germ-plasm.

Prof. Osborn expresses his own opinion to the effect "that upon the side of evolution, or non-repetition in inheritance, the neo-Lamarckians have much the best of it; while upon the side of repetition and embryology, their opponents are strongest." But he significantly adds that in analyzing the arguments of some of the neo-Lamarckians he finds nearly as much against as in favour of the principle for which they contend. Finally, the necessity of moderation in the discussion is strongly urged, the present spirit displayed by the two sides not being conducive to the settlement of these apparently irreconcilable opinions. "I claim," the lecturer continues, "if the neo-Lamarckians can demonstrate by palæontological or other evidence that acquired characters are

[1] See Prof. Lankester's article in Nature of March 6, 1890, p. 415.

inherited, it rests with the embryologists to furnish a theory of physical transmission. On the other hand, the embryologists may show conclusively that such inheritance is impossible."

At the end of his address on acquired characters, Prof. Osborn observes that "it follows as an unprejudiced conclusion from our present evidence that upon Weismann's principle [the non-transmission of special individual variations] we can explain inheritance but not evolution, while with Lamarck's principle [the transmission of acquired variations], and Darwin's selection-principle, we can explain evolution, but not, at present, inheritance. Disprove Lamarck's principle, and we must assume there is some third factor in evolution of which we are now ignorant."

A totally different subject is taken up in the seventh lecture, by Prof. T. H. Morgan, on the relationships of the sea-spiders. In this it is considered that the Pycnogonida, as the group to which these curious creatures belong has been termed, come nearest to the Arachnida, both as regards many peculiarities of the adult and also of the various stages of development. After discussing the morphology of the peculiar "Pantopod" larva of these forms, and its relations to the "Trochophore" of the Annelids, and the "Nauplius" of the Crustaceans, the lecturer has some observations upon the initial differences in the development of various groups of animals which are of sufficient general interest to be quoted at length. He observes:—

"If we remember that during the time in which the groups of Annelids and Crustacea have been evolved, the larval forms themselves have been acted upon in an increased degree, there seems every reason to believe that the young may have been much more acted upon and suffered far greater changes. On the other hand, when we see in such a group as the Vertebrates that in the higher forms the young have been removed to a large extent from the action of surrounding conditions—as, for instance, by being enclosed within a shell as in the Sauropsida, or retained within the uterus in Mammals—then can we understand why the young resemble each other more closely than do the adults, for the obvious reason that the adults have had to adapt themselves to more numerous external conditions, while the embryo has remained fixed. Indeed, this may be pushed a step further, it seems to me, and explains why such young retain the characteristics of lower forms, while the adults have lost such structures. This may be due to the young having been removed to a greater extent than the adults from a process of active selection. Hence, in such a group, when we say that the ontogeny tends to repeat the phylogeny, we mean that the embryos have retained more of the ancestral features than have the adults. But in such groups as the ones we are discussing—Annelids, Crustacea, &c.—we ought to expect, if what I have said be true, the reverse of what we find in such a group as the higher Vertebrates; viz. that the young forms diverge far apart, and the adults come nearer together."

Our remarks on the foregoing lectures have extended to so much greater length than we had at first intended, that space allows little more than the quotation of the headings of the three remaining ones; although neither fails in attracting as much interest as their predecessors. The eighth lecture is by Prof. S. Watase on "Caryokinesis"—a term which it may be well to explain is derived from κάρυον, a walnut, and is applied to a peculiar kind of nuclear cell-division, as met with in Cephalopods and Echinoderms. In the ninth lecture Prof. Howard Ayers gives us an elaborate dissertation on "The Ear of Man, its past, present, and future." The lecturer concludes that the ear of the higher Vertebrates is derived by the invagination of part of a system of canal sense-organs, like the lateral line of fishes; and he ventures to predict certain lines of modification which he considers will probably arise in the human ear with the course of time. The volume concludes with an article by Prof. W. Libbey, of Princeton College, on "Ocean Temperatures and Currents."

We have already expressed our high opinion of the volume before us as containing valuable *résumés* by specialists on many moot biological topics; but we cannot conclude without recording the pleasure and interest to which its perusal has given rise in ourselves, or without heartily commending it to the attention of our readers.

R. L.

PHYSICAL GEOGRAPHY FOR SCIENCE STUDENTS.

Elementary Physical and Astronomical Geography. Specially designed for Pupil-Teachers, Students in Training, and Science Students. By R. A. Gregory. With Original Illustrations. (London: Joseph Hughes and Co., 1891.)

THE general conception and arrangement of this volume are very good, and the same may be said of the detailed treatment of most of the subjects discussed. There is, however, great inequality in the author's work. The portions dealing with mensuration and astronomy are for the most part excellent, though there is sometimes a want of appreciation of the difficulties felt by beginners, and a consequent deficiency in the explanations given. The geological and meteorological parts are less thoroughly treated, while the chapter devoted to plants and animals is so imperfect that it had better have been altogether omitted. In case the work goes to a second edition, it will be as well to call attention to a few of the points where some alteration or further elucidation would be advisable.

At p. 21 the fact that the earth has been circumnavigated is given as a proof that it is not flat. But this is no proof at all; for if the earth were a flat disk, with the North Pole as its centre and the equator midway between the pole and the southern circumference—as represented by "Parallax" and other flat-earth men—circumnavigation might be performed exactly as at present.

The fact that degrees of latitude are longer near the poles than near the equator, although the polar axis is shorter than the equatorial, is often a stumbling-block to learners. It is, in fact, one of the paradoxes, and some half-century ago Mr. Von Gumpach published a pamphlet, and wrote letters to Prof. Airy and to the Astronomical Society, demonstrating, as he thought, that the earth is elongated at the poles—is, in fact, a prolate spheroid, because, the degrees being measurably longer at the poles, shows, he maintained, that the radius is there longer. Mr. Gregory takes no notice of this difficulty, which is a very real one, and requires some explanation in a text-book for learners.

At p. 107 we have the causes which lead to the difference between solar and mean time accurately stated, and a table given of the equation of time at different periods of the year. But though the causes are stated, they are not explained, and many students will fail to see, without explanation, why the apparent motion of the sun is greatest at the solstices, and least at the equinoxes.

At p. 133, the cause of dawn and twilight is well explained, but the fact that the length of day itself, as measured by the time the sun is visible above the horizon, is also increased by refraction, is not referred to.

In the chapter on the atmosphere we have a statement which, though founded on a fact of physics, is erroneous in the form in which it is given. It is stated that "When water vapour is transformed into the liquid state, a certain amount of heat is liberated, and therefore rainfall must have a considerable effect in warming the air, and conveying heat to higher latitudes." This is quite correct, but the author goes on to say that "one gallon of rain gives out latent heat suffici ent to melt 75 pounds of ice, or to melt 45 pounds of cast-iron, and every inch of rainfall is capable of melting a layer of ice upwards of 8 inches thick spread over the ground." Here, supposing the figures to be correct as regards the amount of heat given out during condensation, yet the latter part of the statement as it stands is incorrect, and very misleading. The heat of condensation is transferred to the cold current which causes the condensation high up above the surface of the earth, and does not remain in the rain, which therefore is not "capable of melting" any definite quantity of ice, since its temperature may be very little above the freezing-point when it reaches the earth. Water in freezing also parts with its latent heat of liquefaction, which goes to reduce the extreme cold of the air currents which froze the aqueous vapour, but no one would say that the snow brought this heat to the earth. The two cases are exactly parallel.

As illustrations of the ignorance or carelessness exhibited in the chapter on plants and animals, a few extracts will suffice. The characteristic plants of the Arctic zone are said to be "rhododendrons, lichens, and azaleas," while the vegetation of the torrid zone is thus described: "the palms, bananas, and other trees are densely packed, climbing vines entwine around their branches, and interlace with enormous tree-ferns and grasses." No doubt all these plants are found in the torrid zone, but the manner in which they are grouped shows that the description was not written by a person conversant with tropical vegetation. The lists of plants and animals given as characteristic of the different regions are always inadequate, and sometimes ludicrously erroneous: as when "plants yielding spices," and "bamboos," are given among the characteristic flora, and "numerous insects" among the fauna, of South America; "olives," "tobacco," "oranges," and "vines" as characteristic of the African flora; "hares" among the Australian fauna, the American "custard-apples" among the Oriental flora, while "grasses" appear as characteristic plants of Australia alone.

Among the less important errata for correction in a future edition are the following: at p. 50, in describing the noonday shadow at different seasons, the words "increase" and "decrease" are misplaced; at p. 170, "the Friendly Islands" are given as examples of conical isolated volcanic peaks; at p. 174, "the late Professor Darwin" is referred to; at p. 250 the dark heat-rays are said to be *reflected* from the earth's surface, instead of *radiated*, while, a few lines above, "radiation" is omitted as an important means of heating the atmosphere; and, at p. 280, the table gives "classes" instead of "genera" of Mammalia and birds.

In pointing out some deficiencies of Mr. Gregory's work, it must be remembered that many of the observations will apply equally to other works, some of them of considerable reputation; while the present volume is by no means without special merits. The chapter on the rotation of the earth and consequent phenomena is exceedingly good, as is the following one on its revolution. The account of eclipses and of the tides is also good, and well calculated to render these phenomena intelligible to learners. The chapters on the atmosphere and its movements are also clear and instructive, as are those on oceans and ocean currents. The numerous illustrations, though rather coarse, are clear, and elucidate some of the more difficult problems discussed; and if the author will carefully revise his work for another edition, it may be rendered a very useful and trustworthy guide to students and teachers.

OUR BOOK SHELF.

Whence comes Man; from "Nature" or from "God"? By Arthur John Bell. (London: Wm. Isbister, 1888.)
Why does Man Exist? By Arthur John Bell. (London: Wm. Isbister, 1890.)

In these volumes Mr. Bell brings an acute and ingenious mind to bear on matters of science and the philosophical questions to which the study of science leads. Unfortunately there are many indications that Mr. Bell has no thorough and adequate first-hand acquaintance with the branches of science concerning which he writes. And when the science is mere scissors-and-paste-work, the conclusions are not likely to have the value which attaches to those of even the humblest practical observer. The first volume deals chiefly with the writings and opinions of Mr. Herbert Spencer, Prof. Huxley, and others, and introduces incidentally some quaint notions concerning matters physical and metaphysical. In the second volume, apart from theological questions, with which it is not our province to deal, the main thesis is that the fundamental cause of evolution is psychological. Every cell of the metazoan body is the seat of an intelligence or life which is capable of being conscious, and is therefore a person; and when a given life multiplies and divides, it gives rise to another life like itself without being diminished. Thus is constituted a *cell-patriarchy*. But a patriarchy involves a patriarch; and the patriarch is found in the parent-cell from which all others are produced. At the first division of the fertilized ovum there is constituted a parent-cell and a child-cell (Mr. Bell does not tell us how to determine which is which). The parent-cell may produce other child-cells, and the child-cell give birth to grandchild-cells, and so on; but throughout all cell-division the patriarch parent-cell remains. When invagination has taken place, we may be sure that the position of the parent-cell is at the mouth of the gastrula, for though it has subordinated its children to itself and to its service, that service has to be paid for. Later, however, we find it safely ensconced in the *lamina terminalis* of the brain, whence it directs the proceedings of its faithful servants and children. For the patriarchal parent-cell is the habitation of the Ego, the I, of the organism, while the child-cells are inhabited by subordinated egos. The protoplasm of a cell is a machine, and the inhabiting Ego its engineer. Reflex action is not merely reflex, but chiefly determined by the purposive action of the cell-ego, and so forth. We need not follow Mr. Bell further. Enough has been said to show the nature of his speculations, and to enable the biological or psychological reader in some measure to decide whether it will repay him to read Mr. Bell's pages for himself. C. LL. M.

Elementary Botany. By J. W. Oliver. (London: Blackie and Son, 1891.)

IN these days, when the pursuit of a "pass" is more keen than that of knowledge, the confession that an elementary text-book has been written for the use of students who are studying in classes under the Science and Art Department, and has been prepared on the lines of the syllabus of the first stage or elementary course, is apt to awake criticism, especially when, as in the present case, ten years' examination papers are printed at the end of it. But this book, though not an ideal elementary text-book, is tolerably free from the vices of cram: its merits are, in short, chiefly negative. What is urgently required at present is an elementary book of positive excellence, written (not compiled, as the present work appears to be) by an author who walks himself near the limits of our present knowledge; under these circumstances, the beginner would receive, from the very first, side lights, whether from terminology or from positive statement, which would prepare him for his more extended study. Such side lights are singularly absent from this work: take, for instance, the andræcium and gynæcium; in connection with these the word sporangium is not mentioned, nor is the word spore, except in the statement (p. 163) that the Cryptogamia "reproduce themselves by spores which contain no embryo": thus the attempt is not made to pave the way for subsequent progress to the study of the homologies in the lower forms. Again, in describing various types of corolla, the old terms such as papilionaceous, hypocrateriform are trolled out (p. 126) with only the minimum of explanation of the romance of insect agency (p. 147); and though function is put in relation to form in treating of the stem, the chapter on leaves is singularly dry, owing to its dealing simply with form and terminology. As regards terms, *fibro*-vascular should not be applied generally to bundles (p. 63), and the term *oospore* may well give place to zygote; while "acropetalous" is, we believe, a new enormity. Many of the figures are old friends: some of the new ones are bad: for instance, Fig. 54, of the wood of pine, which is full of inaccuracies; Fig. 73 (C), in which the cambium in a two-year old shoot is as thick as the phloem; and Fig. 75, in which the bordered pits are entirely omitted; while the difference between spring and autumn wood depends mainly upon filling up the lumen of the tracheids with printer's ink. It must not be concluded from these remarks that the book is worse than others of its class: in some respects, it is above the average, but none the less the field is yet open for a book suitable for beginners, and written by a master hand, which shall deal with the elements of the science, in its modern development, in such a way as to lay a secure foundation for the future progress of the beginner, and leave him with nothing to unlearn.

F. O. B.

Household Hygiene. By Mary Taylor Bissell, M.D. (New York: N. D. C. Hodges, 1890.)

THIS little volume consists of a series of papers which originally appeared as contributions to the Art Interchange Company. They deal with the sanitary regulation of the home. Every chapter is written in excellent style, and considering that much of the matter with which the author is concerned is technical, the manner in which it is presented is exceedingly clear. Dealing as it does with every requirement of the home from a sanitary and scientific point of view, the book contains much information of the utmost value to women of the household. It would be well if the essential conditions for a healthy home, so well laid down by Dr. Mary Bissell, were more carefully remembered, for they would be the means of saving life, now often sacrificed by ignorance of the ordinary laws of health. We recommend the book most cordially to the class of readers for whom it has been written, and we feel sure that everyone upon whom the care of a home devolves would be the better for bearing in mind the lessons which it teaches. H. BROCK.

Lessons in Applied Mechanics. By J. H. Cotterill, F.R.S., and J. H. Slade, R.N. (London: Macmillan and Co., 1891.)

THIS is one of the best little books on the subject that have come under our notice for some time. It is of a thoroughly practical character. Although most of the matter has been selected from the larger work by the first-named author, it is presented here in a more elementary manner, having been for the most part re-written, with considerable additional illustration.

The work is divided into three parts. In the first part the principle of work is dealt with, and among the more important chapters in it we may mention that on pulleys, belts, and wheel-gears, and that on the direct-acting engine, the latter being thoroughly well examined in detail, with explanatory diagrams. In the second part, which treats of the strength of materials and structures, we have some well-prepared chapters on the bending moments and shearing forces under distributed loads, open beams, lattice girders, and frame-work structures, &c., together with some experimental facts relating to elasticity, strength, and resistance to impact of various materials. The last part consists of the fundamental laws relating to hydraulics, and although rather short, contains a sufficient amount of information for an elementary work.

Altogether, the book is an excellent treatise for students of engineering, and others taking up the subject. The examples, all of which are practical and original, are numerous, and add greatly to its utility.

LETTERS TO THE EDITOR.

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

The Flying to Pieces of a Whirling Ring.

IN order not to mislead students, it may be well to point out that the words "parallel to the equator" in my letter on p. 439 (March 12) are emphatic, and that a less paradoxical statement is simply that the tension needed to lay a cable whose true weight is buoyed, precisely along a parallel of latitude, is more than it can stand.

It will be noticed that a horizontal rope must sag either downwards or upwards, according as it is in a liquid lighter or heavier than itself; and that to stretch a thread, straight in the one case, or curved like the earth in the other, needs too much tension for anything but a quartz fibre to stand; unless the liquid is so delicately adjusted as to buoy the body's apparent weight without buoying its true. OLIVER J. LODGE.

WE may imagine how anxious the practical man would be to test the extraordinary numerical results given by Dr. Lodge for the tension in a steel telegraph cable, due to the whirling effect of the earth's rotation, amounting, according to the formula, to a tension of 30 tons per square inch in latitude 60°, and 120 at the equator.

Dr. Lodge gives the formula $T = \rho v^2$, and at the equator, $v = 1526$ f.s., and now our practical man looks out the density of steel in a table, and finds it given as about 8.

With $\rho = 8$, $v = 1526$, he finds T is very nearly 20 million, so there is a misconception somewhere; however, the result is given in tons, so, dividing by 2240, he finds T is now about 8000, still very far from the result. He next tries dividing by 144, as the result is given in tons per square inch, and now T is about 60—only half the true result.

By this time he remembers that he ought to have taken $\rho = 8 \times 62{\cdot}4$, and T is now about 3700; and as this looks like 32 times the true result, he now thinks of dividing by g, so that, finally, the formula which gives the true result is, not the simple $T = \rho v^2$, but $T = \frac{62{\cdot}4}{2240 \times 144}\frac{\rho v^2}{g}$.

What does the formula $T = \rho v^2$ then mean? With C.G.S. units we may put $\rho = 8$, and $v = 4 \times 10^9 \div (24 \times 60 \times 60)$, and now $T = 10^{10} \times 1{\cdot}7$ *barads*; and as a stress of one pound per square inch is about 70,000 barads, we divide by 70,000 and 2240, and find $T = 110$ tons per square inch.

But with the ordinary British legal units of the foot and the pound weight, the formula $T = \rho v^2$ is meaningless; and a practical man has a just complaint against our vague system of theoretical teaching in dynamics, when he comes across a formula such as $T = \rho v^2$, where it is merely stated that T is the tension, ρ the density, and v the velocity.

It is in the interest of those to whom dynamics is a reality, and not a mere combinational analysis, that I am encouraged to write this criticism; but now returning to the telegraph cable, which, as a girdle round the equator, ought to rise out of the ocean bed, and stand like an arch under a tension of 120 tons per square inch, in consequence of the whirling effect of the earth's rotation.

But, on taking into account the gravity due to the earth's attraction, which is about 289 times the whirling effect, our cable, if it still stood as an arch, would have a pressure of about 32,000 tons per square inch; and now we are confronted with the old Ostrogradsky paradox. A. G. GREENHILL.

March 16.

PROF. LODGE has invited me to follow up his letter on "The Flying to Pieces of a Whirling Ring" (NATURE, March 12, p. 439), by sending to NATURE a note about the strains and stresses in a whirling disk—a matter which has lately been the subject of some correspondence between us. Before speaking of the disk, however, let me (as an old cable hand) confess that I do not follow the reasoning which leads Prof. Lodge to say that a submerged cable of the average density of sea-water, if lying parallel to the equator, would be subject to a stress of 30 tons per square inch, or more (in latitude less than 60°), in consequence of the earth's rotation. This is in startling disagreement with one's recollection of the behaviour of say a "caya" rope (which satisfies the condition as to density). But surely a cable that is wholly supported by water is as much protected by gravity from flying to pieces as a cable that is partly supported by the mud or rocks of the bottom.

The strains in a revolving disk have been discussed by Mr. Chree (*Quart. Journal of Pure Math.*, No. 89, 1888; see also Proc. Camb. Phil. Soc., vols. xiv. and xv.) and by the late Prof. Grossmann (*Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, Berlin, 1889, p. 216). Prof. Grossmann—for the reference to whose paper I am indebted to Mr. J. T. Nicolson—treats the case of a disk with a central hole in it, and points out that the hoop tension is greatest just round the hole.

There is, however, an important difference between the values of the hoop tension when there is and when there is not a hole. The difference in question appears to have escaped notice, and my object in writing this note is to point it out.

The case supposed is that of a thin disk, homogeneous, isotropic, and uniformly thick. We have to consider two principal stresses—namely, the radial tension p_1, and the hoop tension p_2.

Let ω be the angular velocity,
- ρ the density,
- E Young's modulus for the material,
- μ Poisson's ratio of lateral to longitudinal strain in a simple stress.
- u the radial displacement at any point where the stress is considered, and to which the radius is r.

The equilibrium of any small element of mass under the two radial tensions at its inner and outer surfaces, the two hoop tensions on its front and back faces, and the "centrifugal force," requires that

$$p_1 = \frac{d(p_2 r)}{dr} + \omega^2 \rho r^2 \quad \ldots \ldots \quad (1)$$

The strain in the direction of the hoop stress p_1 is $\frac{u}{r}$. The radial strain is $\frac{du}{dr}$. Hence

$$\frac{Eu}{r} = p_1 - \mu p_2; \quad \ldots \ldots \quad (2)$$

and

$$E\frac{du}{ar} = p_2 - \mu p_1 \quad \ldots \ldots \quad (3)$$

From (2) and (3),

$$p_1 = \frac{E}{1-\mu^2}\left(\frac{u}{r} + \mu\frac{du}{dr}\right) \quad \ldots \ldots \quad (4)$$

$$p_2 = \frac{E}{1-\mu^2}\left(\frac{\mu u}{r} + \frac{du}{dr}\right) \quad \ldots \ldots \quad (5)$$

And, substituting in (1),

$$\frac{rd^2u}{dr^2} + \frac{du}{dr} - \frac{u}{r} + \frac{(1-\mu^2)\,\omega^2\rho r^2}{E} = 0 \quad \ldots \quad (6)$$

From which,

$$\frac{u}{r} = \frac{C}{r^2} + C_1 - \frac{(1-\mu^2)\,\omega^2\rho r}{E} \quad \ldots \ldots \quad (7)$$

In this I have simply followed Grossmann, who goes on to find the stresses in a disk *with a hole* by applying the boundary conditions that $p_2 = 0$ when $r = a_1$, the radius of the disk, and also when $r = a_2$, the radius of the hole. The result is—

$$p_1 = \frac{\omega^2\rho}{8}\left\{(3+\mu)\left(a_1^2 + a_2^2 + \frac{a_1^2 a_2^2}{r^2}\right) - (1+3\mu)r^2\right\};$$

$$p_2 = \frac{\omega^2\rho}{8}\left\{(3+\mu)\left(a_1^2 + a_2^2 - \frac{a_1^2 a_2^2}{r^2} - r^2\right)\right\}.$$

From this it is clear that the maximum hoop tension occurs close to the hole, with the value

$$\text{Max. } p_1 = \frac{\omega^2\rho}{4}\left\{(3+\mu)a_1^2 + (1-\mu)a_2^2\right\};$$

and the maximum radial tension occurs when $r = \sqrt{a_1 a_2}$, with the value

$$\text{Max. } p_2 = \frac{\omega^2\rho}{4}(3+\mu)(a_1 - a_2)^2.$$

In the special case when the hole is very small

$$\text{Max. } p_1 = \text{Max. } p_2 = \frac{\omega^2\rho a_1^2(3+\mu)}{4}.$$

With a given material, the stresses depend simply upon the peripheral velocity.

Next take the case of a disk which has no hole. The boundary conditions are $p_2 = 0$ when $r = a$ (the radius of the disk), and $u = 0$ when $r = 0$. Hence, by equation (7), $C = 0$, and then, by equation (5), $C_1 = \frac{(1-\mu)(3+\mu)\omega^2\rho a^2}{8E}$.

The stresses in the disk without a hole are therefore,

$$p_1 = \frac{\omega^2\rho}{8}\left\{(3+\mu)a^2 - (1+3\mu)r^2\right\};$$

$$p_2 = \frac{\omega^2\rho}{8}(3+\mu)(a^2 - r^2).$$

Each of these is a maximum at the centre, namely,

$$\text{Max. } p_1 = \text{Max. } p_2 = \frac{\omega^2\rho a^2(3+\mu)}{8};$$

and this is just half the value which the intensity of stress reaches when there is a very small hole.

If we take 15 tons per square inch as the greatest safe stress in steel, a thin disk of uniform thickness, with a hole at the centre, may be whirled with a peripheral velocity of about 620 feet per second. If there were no hole, the peripheral speed might be about 870 feet per second.

A plate intended to whirl at a high speed should evidently have its thickness increased in the neighbourhood of the nave.

Cambridge, March 14. J. A. EWING

I IMAGINE that many experimentalists who have had to employ whirling apparatus running at a dangerously high speed must have come to the same conclusion as Dr. Lodge in finding the limit of safety. When designing the magnetic ring, which the late Dr. Guthrie and I used in investigating the conductivity of liquids, I arrived at the same result—namely, that each material when in the form of a ring has a limiting linear speed depending only on its tenacity and density. The same is true of a portion of a ring held by the ends moving about its centre of curvature, provided that it is so long that its stiffness is not a material factor. It did not, however, occur to me that an Atlantic cable of the same density as sea-water would fly to pieces; and I don't now clearly understand why this should be so, or, if so, why the ocean would in such a case hold together, having the same density as the cable.

Of course in the case of a disk, such as a grindstone, higher speeds are possible, because, to use Dr. Lodge's expression, the outer parts are radially sustained. The investigation of the subject will be found in the reprint of Clerk Maxwell's scientific papers, vol. i. p. 60, where the effect on polarized light of a transparent revolving cylinder is also considered.

C. V. BOYS.

IN his letter on p. 439, Dr. Lodge points out that the tension due to centrifugal forces in a rotating band is independent of the curvature; but the deductions which he draws from this are, I think, mistaken. He argues, in the first place, that a straight band of 30-ton steel moving with a velocity of 800 feet per second in the direction of its length is in a state of very unstable equilibrium, and that the slightest shiver of a "vibration running along it would precipitate a catastrophe."

To be sure, if the band is already stretched to its breaking-strain, the equilibrium is unstable *whether it be in motion or not.* But if the band be not so stretched, and it need not be, there is no instability whatever on account of the motion, and a vibration will travel along a bar of steel advancing with this or any other velocity precisely as if the bar were at rest, and without exciting among its particles any rebellion against the second law of motion.

Further on Dr. Lodge asserts that a cable of the same average density throughout its length as sea-water, and lying across the ocean parallel to the equator in latitude lower than 60°, could not hold together unless of 30-ton steel, and the suggestion to relieve the tension of a telegraph-cable by floating it is pronounced infeasible on account of the centrifugal forces. Surely Dr. Lodge has forgotten that the buoyancy of the sea-water is already itself modified by the centrifugal force, so that such a cable would be in perfect equilibrium. Moreover, and quite apart from this consideration, since the centrifugal force on a body even at the equator is only about $\frac{1}{300}$ of its weight, there remains $\frac{299}{300}$ of the weight which might be relieved by flotation in the manner suggested without encroaching on the remaining $\frac{1}{300}$, which would balance the centrifugal force, and thus free the cable from all tension.

A. M. WORTHINGTON.

Devonport, March 15.

ONE of Prof. Lodge's results would surprise all mathematicians were it correct: unfortunately this is not the case. A submarine cable would have *no* tendency to break if supported by floating matter in the manner described by Prof. Lodge. Every particle of the cable would be under the influence not only of "centrifugal force," but also of gravity, and the upward pressure of the water would just balance the difference of these opposed forces, hence there would be *no tension whatever* in the cable, and it would remain in neutral equilibrium, no matter what the latitude.

Prof. Lodge's other results are well-known to most students of dynamics. His general statement that "an Atlantic cable is only held together by its weight," is merely a particular case of the fact that all bodies, whether cables or otherwise, would fly off or burst away from the earth if gravity did not exist. Had Prof. Lodge realized this fact, he could hardly have made such an obvious mistake with regard to the behaviour of a *supported* cable.

G. H. BRYAN.

Peterhouse, Cambridge, March 14.

Modern Views of Electricity (Volta's Force).

I THINK that the difficulty which Mr. Burbury expresses on p. 439 (March 12), under the above heading, probably rests on a misapprehension. He says: "When zinc is isolated, a negative charge is on it, and therefore at an outside point there is a positive slope of potential upwards from the zinc." My statement, on the contrary, is that a piece of zinc immersed in an oxidizing medium possesses no charge so long as it is isolated, but experiences a lowering of potential by reason of the chemical tendencies of its surface-film—*i.e.* the contiguity of a number of negatively charged oxygenations. In this film, indeed, there is an electrical double-layer, consisting of equal opposite charges, the negative facing the zinc, the positive facing outwards; but there is no charge such as will produce the slightest effect at an external point. Contact with copper of course changes all this; displacing negative electricity from zinc to copper across the junction, from copper to zinc through the air. It is this displacement which affects all external points; and it is this which electroscopic experiments have displayed. There is nothing whatever to be detected in the neighbourhood of a piece of isolated zinc, unless its surface-film itself be explored. The range of its effect is sharply bounded by the thickness of its infinitesimal air-film, on which the whole of the molecular strain is thrown: much as is expressed by Mr. Chattock in the latter half of his letter on p. 367.

If Mr. Burbury does not object to contemplate a piece of isolated zinc surrounded on all sides by straining oxygen atoms, each negatively charged, he can have no difficulty in realizing its depression of potential; nor can he fail to appreciate the momentary transfer of electricity, accompanying the sudden approach of the crowd of oxygen atoms, which occurs as soon as a way of escape for negative electricity is opened by the sweeping away of some of them by copper.

OLIVER J. LODGE.

Ratio of Centimetre to Inch.

PROF. BOYS' letter on p. 439 (March 12), reminds me that I have never seen stated a very simple approximate relation between centimetres and inches, viz. 33 to 13, which is correct to one part in 1700.

OLIVER J. LODGE.

Potassium Salts in Sea-Water.

A CORRESPONDENT in NATURE of January 1 (p. 199), in asking why it is that the water of the ocean contains such a large proportion of sodium and so little, comparatively, of potassium salts, raises one of the most instructive inquiries in the whole range of mineral physiology. The waters which flow into the sea convey the soluble salts derived from the land, and these often include a considerable proportion of potassium. The sources of these salts are two-fold: (1) the sub-aërial decay of crystalline rocks, which give up their alkalies as carbonates; (2) saline solutions and solid salts which have come from evaporated seas or lake basins, and have thus been withheld or abstracted from the ocean's waters. In the latter case they are fossil sea-waters, as in many saline springs from the older sediments. These waters show that the proportion of potassium salts was then not greater but less than at present. Of the alkaline salts of the St. Lawrence River estimated as chlorides, the potassium equalled, by my analysis, 16 per cent., and the Ottawa 32 per cent., the remainder being, of course, sodium chloride. In the numerous saline and alkaline springs which rise from the Palæozoic strata throughout the great valley drained by these rivers the proportion of potassium chloride is seldom over 2 or 3 per cent. of the alkaline salts, and often less, while in the waters of the modern ocean it is found to be not far from 3 per cent.

There are, then, two questions before us: (1) Why do saline springs and ordinary potable spring-waters contain so small a proportion of potash salts? and (2) What prevents their accumulation in the waters of the ocean? The evaporation of sea-water in limited basins gives at first pure sodium chloride, and it is only in the mother-liquor that the potassium salts are found, and, as in the Stasfurth beds, are deposited above the rock-salt. The researches of various chemists have long since shown that surface waters, in filtering through the soil, give up potash, ammonia, silica, and phosphates, retaining, however, lime, magnesia, and soda—a beautiful provision by which the earth retains the elements necessary for the life of plants, while the filtered water thereby becomes purified and fit for ordinary uses. A process not unlike this goes on in the sea. It is well known to chemists that the ashes of sea-weeds abound in potassium salts, and contain, in most cases, from 15 to 25 per cent. of potassium oxide; so that kelp is valuable, not only as a source of iodine, but of potash, and the fertilizing effects of sea-weed, due to the presence of the alkali, are recognized. The marine plants thus select from the sea-water the potash, and by their subsequent decay in the ooze of the bottom, as long since pointed out by Forchhammer, restore this element again to the insoluble sediments. These plants, at the same time, take from the sea-water the minute quantity of iodine which it contains, and also the dissolved metals, as silver, copper, and gold, traces of which are found in their ashes, and, as I have long since shown, are thus agents in the production of metalliferous strata, and, finally, of mineral veins.

Thus, while the soil removes from the surface waters the elements necessary for the nutrition of land plants, the marine vegetation itself performs more directly a similar function in the waters of the ocean, and in the accumulating sediments restores it to the solid earth the alkaline element. In the sea, as on the land, the great process of terrestrial circulation goes on unceasingly. The reader who wishes may see the whole matter discussed at greater length in the author's "Chemical and Geological Essays," pp. 95, 96, 135; and again, in a lecture on "Metalliferous Deposits," *ibid.*, pp. 220–236.

New York. T. Sterry Hunt.

Bright Crosses in the Sky seen from Mountain Tops.

On March 21, 1881, I made an ascent of the Grossneuediger (3673 m.), and observed at midday from the summit a curious phenomenon. Yesterday, March 8, 1891, I made an ascent of the Patscherkofl (2214 m.), and observed, also at midday, the same phenomenon from the summit. As I have never seen it at any other time from any mountain top, it may be considered rare, and as it was in both cases observed in March, with fine weather, south wind, and relatively high temperature, it may be more or less restricted to that time of the year, when Alpine ascents are rarely made.

The phenomenon is a combination of two rings of light, one of which has its centre in the line connecting the observer with the sun, and appears to have the same dimensions as the large ring sometimes observed round the moon and the sun at low levels. The other ring has its centre in the observer and passes through the zenith and the sun. Both rings are pale, the latter paler than the former, and not visible near the northern horizon. Where these two rings cross each other, they are much brighter than elsewhere; and so it appears at first sight, if one does not look carefully, as if there were merely two bright crosses, one below and one above the sun, the arms of both crosses being vertical and horizontal.

This observation may be interesting to meteorologists, and seems to be but rarely made. R. v. Lendenfeld.

Innsbruck, March 9.

Iridescent Clouds.

A brilliant iridescent zenith arc was seen to-day at 3.15 to 3.30 p.m., without either the primary or secondary solar halos which usually accompany it. A faint "mock sun" was, however, observed in the west at 4 p.m.

A remarkable and distinct display of iridescent cirro-stratus was seen on March 4. The cloud-layer, which appeared to be very high, and was apparently advancing from the west-northwest, was evenly tinted in a striking manner. The hues were most strongly marked in the foremost band, whilst in the far perspective the cloud was still faintly illumined with iridescent colours; these were especially beautiful at 5.45 p.m. Low scud cumulus from the west partially obscured the view at 6 p.m. It is not unusual to see iridescent colouring in clouds, chiefly when a low bank of cloud reflects the light on the higher cirrus. It is also occasionally seen fringing the edges of cirro-cumulus when they are in an azimuth near that of the sun.

York Road, Driffield, March 11. J. Lovel.

Frozen Fish.

Mr. McLachlan's opinion that fish suffer comparatively little injury when inclosed for lengthened periods in solid ice is fully borne out by an occurrence here in the year 1873. In the early part of the month of July a boy was seriously ill in one of the large boarding-houses; ice-bags had to be applied to his head; the ice was procured from the ice-house, which had been filled in the previous December from a pond in the neighbourhood. On pouring off the water from one of the bags after it had been used, a small fish was seen swimming merrily about. My informant (the master of the house in which the boy lay ill) tells me "the fish was very small, and so transparent that a large portion of its internal organization was clearly visible"; he thinks it was minnow, but is doubtful as to the accuracy of the opinion. At all events we have here a well-authenticated case of a fish surviving inclosure in solid ice for a period of between six and seven months.

I may perhaps mention the effect of the recent winter on the Unionidæ of this neighbourhood. They lie dead in shoals round the margins of several large ponds I have examined, particularly in those which are very shallow round the edges. The dead are far more numerous than I have ever seen in previous years. Two *A. cygnæus* which I exposed in an open vessel to the entire severity of the frost were killed, and both their shells split from dorsal to ventral surface on one side. On the other hand, Unionidæ, even when out of their shells, can be frozen and thawed for two successive nights at least without injury; neither do the contained Glochidia suffer in any way. This last point I chanced to discover last year accidentally, by one of my dissecting dishes containing a living Unio getting frozen solid on two successive nights. Oswald H. Latter.

Charterhouse, Godalming, March 15.

Mr. McLachlan asks (1) Whether fish necessarily die when enclosed for lengthened periods in solid ice?

To this I can definitely reply that there were many small carp (3 inches to 9 inches), and innumerable sticklebacks, embedded in the ice in a pond here, within a week of the commencement of the long frost (beginning about December 8), and that when pieces of ice containing them were broken up (as was done at that time) and the fish put into water, they showed no signs of life.

(2) Whether this winter caused any important mortality?

Up to the frost, the pond swarmed with sticklebacks, and contained hundreds of small carp (3 inches to 6 inches), and, probably, two or three dozen of the same fine fish from 6 inches to 20 inches long. Since the frost, there has been no sign of fish-life in the pond.

The pond is about 70 feet by 50 feet, and at the time of being frozen had a depth of 2 feet of water, and (in places) over 1 foot of soft mud. Unfortunately, the ice was not kept broken.

Of course, it is possible that the old carp may be still alive at the bottom of the pond or in the mud, but we should have seen the smaller carp and sticklebacks if there had been any still alive. James Turle.

North Finchley, March 14.

Eskimo Art Work.

Among the many objects of interest seen in a brief journey to the cryolite mines in the Arsuk fjord, Greenland, last September, none was more attractive than a collection of what may be called Eskimo works of art, belonging to Assistant-Superintendent Edwards, of the mine. Three of the specimens were photographed with a tourist's camera. Although the photograph was not a very good one, it shows a degree of skill in sculpture that would probably surprise those familiar only with those specimens found in such museums as the Washington and the Berlin.

In the collection, besides candlesticks and cigar-holders, were a number of ash-receivers, anchors, paper-weights, &c. They were all made of green stone (weight stone, the Danes call it), of the variety used in making the Eskimo lamps. Of course, every article was made with the intention of selling it to the Danish rulers; the Eskimos, so far as I could learn, never using their artistic skill for decorating their own homes, although such articles as weapons, toggles for dog harnesses, &c., are often fashioned with an eye for beauty, as well as utility.

Files, purchased of the Danes, were about the only tools used by the Eskimo artists, although the form of the object to be made was first rudely blocked out with a pointed piece of iron used as a chisel.

Some of the objects had a jewellery finish, as founders in bronze would say; while others (and the more beautiful) showed plainly the marks of the file. The art centre—if one may call it so—of Greenland is Godthaab, where Heinrich J. Rink lived when Inspector of South Greenland, although one or two men at Frederickshaab have, by their skill, made reputations among the whites along the coast. John R. Spears.

New York, February 26.

THE ZOOLOGICAL STATION AT NAPLES.

IN NATURE of February 26 (p. 392) a friend of the Zoological Station of Naples has raised his voice to correct one or two misconceptions which, as he thinks, have been the cause of the difficulties, experienced at the last meeting of the British Association in Leeds, in obtaining the renewal of the vote for the occupation by British naturalists of a table in the Zoological Station. While thanking him, I should wish to be allowed to add some remarks to the arguments used in that article.

If opposition to the continuance of the table was really based on the ground that the Zoological Station is in the main an educational institution, nothing would be easier than to show that this is a fundamental error. In fact, the whole conception of the Naples Zoological Station was to found an institution meant *exclusively* for research, and this conception has been carried out in every way. Not only more than six hundred naturalists of various nations have worked for months and years in the laboratories of the Station; not only from six to ten assistants have been occupied with research all these years through; but the Zoological Station has sent ever-increasing numbers of well-preserved marine animals to almost all the greater and many smaller European and other laboratories for pure ends of research. By all this the Zoological Station has almost revolutionized the conditions of biological research; it may yet be the cause of greater changes through the arrangements that it is just now finishing to enable physiologists to carry out experimental and chemical studies on marine animals and plants.

One educational exception is, perhaps, worth recording. The Zoological Station has admitted during the last ten years naval officers and physicians from Italy, Germany, Russia, and Spain, for the purpose of instructing them in the art of collecting and preserving marine organisms on their voyages through the oceans, and I am glad and proud to say that the collections brought home by the Italian corvette *Vittor Pisani* have earned not only well-deserved fame for Captain Chierchia, but have proved to be really the solution of the problem how to add numberless treasures of the oceans to the stock of inland laboratories for research, and to do this by the simple expenditure of a few thousand francs. The example set by the Italian naval authorities has been followed by the Russian Navy, after a visit to Naples by the present Minister of the Navy at St. Petersburg, Admiral Tchichatchoff; and splendid collections from the Pacific and the Indian Oceans, made by the naval physician, Mr. Isnaeff, have been added to the stores of the Moscow and St. Petersburg collections. I still hope that other navies may follow in this line, and I am sure that naval officers and physicians on board as well as naturalists at home would be greatly satisfied if the Italian and Russian examples became more generally imitated.

I am pleased at this opportunity of calling attention to the only case where the Zoological Station made use of special instruction as the most effective way to promote research. All the six hundred naturalists who have worked during eighteen years in the Zoological Station have done so relying only on their previously acquired education in Universities at home and abroad, and if even they went away from Naples better instructed than they came, it is simply because no one is more fitted to profit by example than he who already understands.

Let me now treat of the second objection, of which the author of the article speaks, regarding the "policy of continuing to support an already thriving institution for an indefinite period."

It is obviously more difficult for me to discuss this objection, and especially so after the author of the said article has once more most distinctly called the Zoological Station at Naples "Dr. Dohrn's Station." The author is right in calling me the founder, director, and proprietor of the Station, but I wish most distinctly to point out that my proprietorship involves only a burden and responsibility, and no advantage whatever of a material kind. I am a creditor to the Zoological Station, like other creditors, but with the clear distinction that my material liability is unlimited towards the other creditors, and my moral liability limited to that *imponderabile* called the public opinion of the whole scientific, and a great part of the unscientific, public, which takes an interest in or contributes to the maintenance of the Zoological Station.

But this same unlimited liability may excuse me if I take the liberty to state unrestrictedly the necessities and the conditions under which I hoped to succeed in an enterprise which, when I began it, was considered fantastical, almost Utopian, by many, perhaps by most, of my fellow-workers in biology. I meant from the very beginning to create an international institution, and I counted upon the loyal and lasting co-operation of all those, in whatever country, who understand the extraordinary importance of seaside studies, and who know from experience how difficult progress in biology had become from want of appropriate laboratories near the sea. I hoped, further, to enlist as supporters of the Naples Station all those naturalists who, with me, put the general interests of biology higher than the personal predilection for this or that branch of biological pursuit, and who could help me in securing the material support of Governments and learned bodies for the new institution, which was created under considerable difficulties, and for which I had undertaken to act as a responsible manager. To find myself without that co-operation could alone make me regret the labour and loss that I so incurred.

The author of the article calls the Zoological Station "an essentially German institution," and seems to believe France justified, "in view of national prejudice and having zoological stations of her own," in not having subscribed for one or more tables in the Naples Station. In fact I am German both by birth and culture, and shall remain so to the end of my days, and so are the greater part of my assistants, who have staked like me their existence on the prosperity and efficiency of the Zoological Station of Naples. But the very name of Naples indicates that one might quite as well call it an essentially Italian institution, and the more so as among my assistants there are several Italians of no less importance and service to the Zoological Station than my compatriots, and as Italy like Germany has behaved most generously in supporting the Station.

But I think the time has come when one must raise one's voice most distinctly against the narrowing limits of national prejudice, which nowadays has grown to almost overwhelming and even pernicious importance in many provinces of material and—I am sorry to say—also moral and intellectual existence. Science at any rate ought to be exempt from that morbid exclusiveness which refuses to act in rational community regardless of political or ethnographical boundaries. When I left my country to found the Zoological Station at Naples, I acted simply in the interest of science. I would certainly have preferred to found the Zoological Station in Germany if Germany had offered the same scientific advantages as Naples; or I would have gone to the North Cape or Ireland, if I had been convinced that biology were best served by building a station there instead of in Naples. My choice fell on Naples because I was and am still convinced that no place in the world combines so many advantages for biology as Naples, and no other place would so readily induce others to follow the lead which—it was, perhaps, presumptuous in a young man of thirty years of age—I, with the daring of just these thirty years, believed myself capable of taking, and even entitled to take.

As for France not following the example of almost all the other European nations, allow me to state that Claude Bernard, the great physiologist, asked the Minister of Public Instruction, M. Bardoux, to rent four tables for French naturalists at Naples; and if this has not been achieved, there comes in a greater obstacle than national prejudice—the untimely death of the great physiologist. I believe, indeed I know, that even now a view is predominant among some of the highest authorities of the French biological school, that France ought to be represented at Naples, and it is regretted in some quarters that "national prejudice" is allowed to triumph over those higher aims of the French mind, to which science, as we all know, owes such splendid manifestations and such grand achievements.

I do not know whether it is a better position to plead for the abstinence of France in view of the several French zoological stations; but as Austria has not ceased to rent tables at Naples though in possession of a national station at Trieste, so France might have found it well worth the outlay of an annual £100 to have a share in the maintenance and profitable use of the largest and the only international biological laboratory existing.

If it be alleged that the Naples Station is now a thriving institution, and not any more in need of being supported, as in the case of the table rented by the British Association, I am glad that the author of the article in NATURE gives the account of the receipts and expenditure of the Naples Station, and finishes with the statement, that "the Station would be carried on at a considerable annual loss were it not for the magnificent subsidy of £2000 a year, granted to its support by the German Empire, which just covers the deficiency." I think this statement answers more than fully the question of the desirability of the "international" support of the Naples Station. If it were true that the Station was essentially a German institution, the German Empire would certainly not ask for the support of any other State or foreign Association, but would receive foreign naturalists as guests in a laboratory maintained for the benefit of its own subjects. But the scientific and international importance of the Naples Station is so unrestrictedly recognized at Berlin, that whilst there is a movement on foot to create in Heligoland a "Prussian" biological station for home interests, I am distinctly told that this will in no way interfere with the generous subsidy given by the German Government to the Naples Station.

I believe myself to have been the first to suggest the formation of a net of zoological stations round the globe, and have been either actively or morally helpful in the formation of most of those now existing. If I have not carried out an old plan to assist personally in the creation of a Zoological Station at Sydney, which I considered, and consider still, of the highest importance to science, it was in deference to the remonstrances of my late friend Prof. F. M. Balfour, who insisted even more than myself upon the supreme necessity of a powerful central establishment of the kind, and opposed, even for a time against my own opinion, the plan for the foundation of a British biological station, on the ground that it was too early, and would so interfere with the thorough development and maintenance of the Naples Station.

And I think I ought not to conclude without once more respectfully and gratefully recording the splendid gifts of some British naturalists, headed by the late Mr. Darwin, to the Zoological Station, which, in a dangerous moment, went far to protect my, at that time, still isolated and not generally recognized efforts from falling short of the end in view. May these two names be suffered to test the high and purely scientific character of the Naples Station, and may this reference to them help to maintain the ties which, from the beginning, have been established between it and the British biologists.

ANTON DOHRN.

THE HIGH-PRESSURE AREA OF NOVEMBER 1889 *IN CENTRAL EUROPE, WITH REMARKS ON HIGH-PRESSURE AREAS IN GENERAL.*

UNDER this heading Dr. Hann, of Vienna, has recently had a memoir published,[1] in which he gives in detail and discusses the meteorological conditions and circumstances in the high-pressure area which remained nearly stationary over the Alps and the circumjacent territory in November 1889, during fourteen days. On November 6 there was high pressure over the Atlantic Ocean, France, and the southern part of England. On the morning of the 11th the centre lay over the North Sea, and on the 12th it was transferred to Central Europe, and nearly the whole of Europe was comprised within the high-pressure area, which continued until the 25th. During this time there was low pressure over the extreme north-west, north, and north-east of Europe, but no distinct storm-centre up to and even beyond the 60th parallel of latitude. The centre of high pressure, 780 mm. reduced to sea-level, lay over the eastern part of the Alps. The wind, as shown by the chart, seemed to blow gently out from this centre, and at the same time to turn toward the right, indicating an anticyclonic motion. The charts also show that the region of high barometric pressure corresponded with that of low temperature, the latter, however, without any reduction to sea-level.

After reducing the pressure and temperature observations of twelve high-level stations of the Alps and adjacent territory, with altitudes ranging from 1400 to 3100 m., to the level of 2500 m. of altitude, the centre of high pressure is found to correspond, at that level, with that at the earth's surface, and the temperatures, with little variation between stations, to be a little below that of incipient freezing.

The temperature on the earth's surface first sank under the influence of the high pressure below the normal. Before this, a temperature prevailed which was considerably above the normal, which first sank to the mean on the 11th and 12th, as the centre of high pressure was first transferred to Central Europe.

The dryness of the air at the mountain stations in the centre of the region of high pressure was extraordinary during the whole time from the 12th to the beginning of the stormy west winds on the 25th, and the daily mean of the relative humidity from the 19th to the 23rd ranged from 17 per cent. on the Wendelstein (1730 m.) to 49 per cent. on the Schneeberg near Vienna (1460 m.), while on the low lands with lower temperatures the air was nearly or quite saturated with aqueous vapour. In the higher strata of the air, therefore, during the high pressure, and especially during the latter part of it, there was very great dryness, while near the earth's surface the reverse was the case.

By comparing the observations of the lower stations above 500 m. and over, from the 19th to the 23rd, with the higher ones, it was found that through a range of 2050 m. there was an increase of $0^{\circ}.8$ in the daily mean; but for the lower intervals of altitude, the increase of temperature with altitude, for an average range of 680 m., was $7^{\circ}.1$. This indicates that the air was very cold near the earth's surface only, and that in ascending it rapidly became abnormally warm, and remained so up to the level of the upper stations, and, we have reason to think, much higher. This warm and dry air came not from the south, since, at a few high stations, as Sonnblick, Schneeberg in Tyrol, and Obir, the prevailing winds were northerly. It was a real *foehn*, with its characteristics of great warmth and dryness, arising from the gradual descent of air in the interior of the high-pressure area.

The departures of barometric pressure from the normal

[1] "Denkschriften der mathematisch-naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften," Band lvii.

of thirty years (1851–80) in the valley stations from the 19th to the 23rd, ranged from plus 15·2 mm. to 17·9 mm., and so there was but little difference between the several stations. For the high stations the range was from 13·1 mm. to 15·2 mm.; and so here, likewise, the variations between stations was small, and the departures differed but little on the average from those at the low stations. They were somewhat in proportion to the whole pressures at the high and low stations. This was a consequence of the almost uniform temperature from the highest of the lower stations up to the highest.

While the temperature departures on the earth's surface from the normal were about $-3°$, they amounted at the high stations to $+8°$. The region of positive departures had a much greater vertical extent than that of the negative ones, which perhaps, on the mean, had not over 300 to 500 m. of depth. The mean temperature, therefore, in the central region of high pressure up to 3100 m. above sea-level, was warmed certainly about 6° over the normal, and as the air on the Sonnblick was 8° too warm, it is reasonable to suppose that, up to a great altitude, the air of the central region of high pressure must have had an unusually high temperature.

The limits between the warmer upper air strata and the under ones, which were relatively very cold and moist, seemed to be sharply drawn. It was principally made visible by the upper limits of the fog-formation. As this did not remain strictly at the same level, but oscillated a little vertically, places situated at about the same altitude were sometimes above and sometimes below this limit, and so were subject to very sudden and great changes of temperature. Places at all times above this limit had very dry air, and, during the day, continual sunshine, while the low stations were mostly in a very cold and moist air, with little or no sunshine. The upper warm strata floated over the cold under ones without mixing with or disturbing them.

Tables of the meteorological conditions for most of the high stations during the time of the high pressure are given, all of which show that there was an increase of temperature with increase of pressure, and that the highest temperature occurred generally soon after the time of highest pressure. A few tables for other times and places are given, which indicate the same thing. In fact, this matter has been so frequently and so ably worked up by Dr. Hann, and the results are now so well known and understood, that further researches in this direction seem almost superfluous.

The same memoir likewise gives the results of a similar investigation of a low-pressure area which lay over Central Europe, and centrally over the east side of the Alps, on October 1, 1890. The isobar of 755 mm. at this time inclosed the west part of the Mediterranean Sea and the northern part of Adria. The lowest pressures were at Vienna, Budapest, Prague, Bozen, and Riva, varying but little from 752 mm. at these places. The winds were gentle, and all Europe had rainy weather, and so the typical weather of a low-pressure area. On the mountain tops of the eastern Alps moderate and variable winds prevailed. The mountain tops were enveloped in clouds, and snow fell there; in the valleys, on both the north and the south sides of the Alps, there was rain.

The temperature departures of October 1 from the normal of three years of observations were negative at all the stations. By forming groups of stations of nearly the same altitudes, the following results were obtained:—

Altitudes (metres)	410	850	1700	2160	3100
Departures	−2°·7	−3°·3	−5°·5	−4°·8	−3°·8
Mean temperatures	8·6	6·0	0·8	−1·7	−6·5

The average departure of the whole air-column, therefore, was about $-4°$.

In a comparison of these temperatures with those from November 19 to 23, in order to not over-estimate the temperature in the high-pressure area the morning temperatures only of the latter were used, and thus the following results were obtained:—

Altitudes (hectometres)	5	10	15	20	25	30	35
Minimum— Oct. 1 ..	7°·9	5°·1	2°·3	−0°·6	−3°·4	−6°·2	−9°·1
Maximum— Nov. 19–23	−2·7	+6·3	4·4	2·5	0·6	−1·3	−3·2
Max.–Min.	−10·6	+1·2	2·1	3·1	4·0	4·9	5·9

From this showing Dr. Hann concludes that at least from an altitude of 1000 m. up to 3500 m. the air in the high-pressure area was much warmer than that of the low-pressure area of October 1, although the latter was more than 1·5 months earlier.

He then estimates the mean temperatures of the air-columns up to an altitude of 3 kilometres, and obtains the following results:—

In the minimum of Oct. 1 ... ... −0°·6 C.
In the maximum of Nov. 19–23 ... +1°·6 C.

This seems to be a very fair presentation of the matter, and really, as he thinks, an under-estimate of the difference in the two cases.

A result obtained from the discussion of the temperatures on the Sonnblick, for both high and low pressures, is here referred to, which is that cyclones of the summer half-year cause a great cooling of the air up to at least 3000 metres, and that the mean temperature of the whole air-column in a summer cyclone, from the earth's surface even up to and above 5000 metres, is lower than that in an anticyclone.

Some of the deductions from the results of the memoirs by the author, on account of his prominence as a meteorologist, require here some special notice. With reference to these results he says, "So much at least is established, that an inquiry into the cause of the cyclonal and anti-cyclonal motions of the air must take into account the fact *that up to at least 4 or 5 km. the mean temperature of the air in the centre of an anticyclone may be higher* (perhaps, indeed, always higher) *than that in the centre of a cyclone.*"

He further says, in view of this: "Herewith fall the views which have sought the cause of these motions in the difference of specific gravity of the air in a cyclone in comparison with that of an anticyclone; in the impulse to which the air in a cyclone is said to be subject."

This seems to be said with reference to Espy's condensation theory, according to which the ascending air in a cyclone is warmer from the latent heat given out in the condensation of the aqueous vapour, and consequently lighter than the surrounding air. The argument, of which the conclusion merely is given, seems to be somewhat as follows. The temperature in the cyclone of October 1, 1889, was several degrees lower than the three years' normal, and also a little lower even than that of the anticyclone from November 19 to 23, more than a month and a half later. Also the temperatures of summer cyclones over the Alps, and a few other places, have been found to be less than the average temperature or normal. Therefore the temperature in a cyclone is less, and the specific gravity greater, than in the surrounding air at the time of the occurrence of a cyclone.

With regard to taking into account that the temperature in an anticyclone, in the sense in which it is used in the memoir, may be greater than that of a cyclone up to a considerable altitude, the writer does not see how that has anything to do with the cause of the motions of a cyclone, except, perhaps, in a few rare and special cases. During the fourteen days of the anticyclone over the Alps, even if the temperature within had been very much greater, he does not see why Espy's conditions of a cyclone could not have existed in America, on the Atlantic Ocean, or even in the north of Europe. These conditions, as is well known, are that the upper part of the atmosphere shall be cooled down below the normal, so as to give rise to a greater vertical gradient than usual, the gradient required being less for air entirely saturated, or nearly so, than for drier air. Whatever may have been the cause of the anticyclone, that of the cyclone was of course different; and in the case of the supposed cyclone in the north of Europe at the same time, there might have been a little overlapping and mingling of cause and effect on the adjacent sides, but neither cyclone nor anticyclone would have interfered materially with the other; and the complete conditions of a cyclone not being interfered with on all the other sides, these would have controlled the cyclone. Even the conditions of a small cyclone can exist in a large area of pressure considerably above the normal pressure, since it is only necessary that the vertical distribution of temperature be such that the ascending air, by its law of cooling, is kept a little warmer than the surrounding air. It is admitted that, in the case of a high-pressure area being formed and maintained for a considerable time, until the descending currents have caused an abnormal heating in the air within at some distance above the earth's surface, these conditions cannot be fulfilled.

Of course, if there was a ring of high pressure formed all around a cyclone, and maintained for some time, the *foehn* effect of the descending current would soon destroy it. A broad ring of this sort with a very moderate increase of pressure is always formed around the cyclone, but the ascending current of the interior of the cyclone comprises so small an area in comparison with the broad area all around when the air very gently settles back again towards the earth, that the *foehn* effect of the latter may be regarded as being almost insensible. So far as it goes, however, it tends to work the destruction of the cyclone, as well as the ascending currents do, by gradually decreasing the vertical temperature gradient in conveying heat from the lower to the upper strata of the atmosphere. Cyclones, in their nature, are not perpetual; for they are the means by which the atmosphere in an unstable state, which is necessary to give rise to a cyclone, is restored again to the stable state, and when this is brought about the cyclone must necessarily cease.

In the case of the anticyclone, so called, over the Alps, and all similar cases, the air comes in from a great distance on all sides, and descends over a comparatively small area towards the earth in the interior. Here the circumstances are reversed, and the *foehn* effect is very great, while the cooling effect all around, from the very slow ascent of air to compensate the downward current, is extremely small.

It is assumed by Dr. Hann that the high-pressure area over the Alps was an anticyclone, but he does not explain how an anticyclone gyration around the centre of this area could be originated, and maintained for so long a period. With an anticyclonal motion the air is pressed from all sides towards the centre by the force arising from the earth's rotation, and as the gyratory velocity below, at the earth's surface, must be diminished by friction, and this force made less there than that above, of course the descending air in the interior escapes there. If this anticyclonal gyration could be explained, it would furnish an explanation of everything else in the production of the *foehn* effect within. So far as it concerns our preceding arguments it is no matter whether it was an anticyclone or not, and want of space forbids the entering into a discussion of that subject here.

As in the case of anticyclones, so called, so it is with that of normals. They have nothing to do with the conditions of a cyclone, and the comparison of the temperature of the cyclone of October 1 with the three years' normal is not pertinent in an argument against the condensation theory of cyclones. As has been stated, this theory requires simply that the temperature of the interior of a cyclone shall be greater than that of the adjacent surrounding air at the time, without regard to what a three years' normal or any other normal may be. The three years' normal may be in error 1° or 2°, and the observed temperature may have been 5° below a true normal, and yet 5° or more above the prevailing temperature at a distance all around at the time. It is well known that the temperature departures from the normals are often very large and of long continuance. The excess of temperature in a cyclone above the surrounding temperature need not be large. If a column of air were 1° (1°·8 F.) warmer, from the earth's surface to the top, than the surrounding air, it would give rise to an ascending current at an altitude of three miles of about thirty-five miles per hour, not considering friction. Of course considerable allowance must be made for this, but, making ample allowance, there is still a velocity left much greater than that which usually exists in the ascending currents of cyclones.

It is true that the products of condensation, falling from high and cold altitudes, cool the earth's surface and the lower part of the atmosphere in summer cyclones. But this is an effect of the cyclone, and does not enter into the conditions which give rise to it. It affects surface temperatures mostly, and these are the temperatures observed in the Alps. The power of the cyclone is mostly above, in the cloud region, and surface temperatures have little or no effect. The air below in the cyclone may be colder than the surrounding air, yet, as soon as the gyration is started above, by which the pressure is decreased in the interior of the cyclone and increased around it, this lower colder air is brought into circulation by the difference of pressure thus produced, and the action upon it of that above by means of friction. But, as has been said, cyclones are not perpetual, and, so far as the cooling of the body of the air is concerned, it is one amongst others of the causes which retard and gradually destroy the motions of the cyclone after they have been once started. The principal part of the observed cooling in summer cyclones is, no doubt, due to the exclusion of the solar heat by the clouds, but this is compensated by its absorption in the upper part of the cloud.

Dr. Hann seems to have strangely overlooked the fact that cyclones do not occur in a normal, but an abnormal, state of the atmosphere; that is, when the upper strata are so cooled down as to give an unusually great vertical temperature gradient, and so an unstable state of the atmosphere. This has been illustrated in great detail by the writer elsewhere,[1] both in the case of dry and moist air. Since, then, cyclones can occur only when the upper strata are abnormally cold, and the temperature in the interior of cyclones, as we have seen, is but little above that of the surrounding adjacent air, the average temperature at high stations of a large number of cyclones must necessarily fall below the general average of temperature of the month, or season of the year, in which they occur, and in any special cases could hardly be expected to come up to this average. Not only in no special cases, therefore, but also in no cases of averages, can any argument be based upon the comparisons of cyclone temperatures at high stations with normals.

Another quotation which we make from Dr. Hann's memoir reads as follows:—"Ferrel's views with regard to the nature of anticyclones, as still maintained in his latest work, 'A Popular Treatise on the Winds' (London, 1889), are likewise in manifest opposition to the facts. A stationary anticyclone during 14 days over the whole of Central Europe, as in November 1889, and so many others of longer continuance (December 1879, January, February, 1882, &c.), cannot still be regarded as satellites or dependencies of cyclones, and support the

[1] "A Popular Treatise on the Winds, &c.," pp. 36 and 232.

position, 'The duration of the area of high pressure depends upon that of the cyclone' (p. 343). Rather the reverse is the case—the cyclones depend upon the anticyclones; these last rule the general character of the weather, and control the directions of the former. The anticyclones, also, do not depend directly upon the lower temperature, as Ferrel thinks (p. 344); the anticyclone of November 1889 disproves this in a striking manner, as well as the more general position, that the cause of the anticyclone is the increased density of the air through the diminution of its temperature."

In the first of these references to the writer's work, the anticyclone and ring of high pressure which accompanies the cyclone are spoken of. These are clearly deduced from theory, and always observed where not obscured by other irregularities. Many references could be given to show this,[1] and it is not, therefore, "in manifest opposition to the facts." It is stated that this ring of high pressure is often superimposed upon so many other irregularities that it is broken up into areas of high pressure, and a complete ring of high pressure is not observed. With regard to these it is said that their duration depends upon that of the cyclone which has caused them. Now, what argument can be drawn from the area of high pressure over the Alps, or any similar ones, against this reasoning the writer is entirely unable to see.

In forty-four cases of ridges, or areas of high barometer with an area of low barometer between them, passing over the United States, the late Prof. Loomis found the average distance from the centre of low barometer to that of the areas of high barometer preceding and following to be about 1000 miles. Now it is very reasonable to suppose—in fact, absolutely certain—that these ridges of high pressure are caused by the centrifugal forces of the gyrations of the cyclones, which drive the air from their centres and cause low pressures there, and at the same time give rise to the ridges of high pressure between them. Yet Dr. Hann would have us believe that these areas of high pressure do not depend upon the cyclones, but rather the reverse—the cyclones between are caused by the areas of high pressure.

Again, in this reference it is stated that "the anticyclones do not depend directly upon a lower temperature." In the high-pressure areas which immediately follow the great winter cyclones of the northern part of the United States and of British America, the writer took the position that the high pressure is not wholly due to dynamic causes, but in a great measure also to the lower temperature of the air on the north-west side of the cyclone, both from being brought down from a higher and colder latitude by the cyclonic gyration, and also from the increased radiation of heat into space, on account of the great clearness and dryness of this air in comparison with that of the cyclone which had just passed over. But Dr. Hann says this has nothing to do with the matter, and also that the increased high pressure in no way depends upon the cyclone; but he does not inform us what the cause of it is. A sudden change of 30° F. is no unusual change immediately after the passage of a cyclone, and if this extended to the top of the atmosphere, and without any diminution of the height of the atmosphere, this would cause an increase of nearly two inches in the barometric pressure. But it is not to be supposed that this great difference of temperature extends to the top of the atmosphere, and besides we must make some allowance for a little settling down and contraction of the height of the column, or rather disk, of cold air. But still it is very reasonable to suppose that the pressure is very much increased from the lowering of its temperature.

Of course, if this high-pressure area were a stationary one of 14 days' continuance, there would be a large body of heated air formed in its interior, commencing a little above the earth's surface and extending up to a considerable height, which would change the initial temperature conditions and tend to diminish the downward pressure, and finally to bring it to the normal pressure. This is what took place in the anticyclone, so called, over the Alps. It will be remembered that the high temperature and all the *foehn* characteristics took place from November 19 to 23, during the last part of the 14 days' continuance, and just before its sudden breaking up, which was caused by this increase of temperature within it. The vertical circulation being once established, of course it was continued by the momentum beyond the point where the temperature was equal to that of the surroundings, just as the pendulum does not stop when it has reached its lowest point, but continues on, even to the distance whence it started, if there are no frictional or other resistances. The same is true in the ascending air in a cyclone. It continues, by virtue of its momentum, not until its temperature is reduced to that of the surroundings merely, but to a still lower temperature, and until the diminished temperature and increased density gradually destroy the ascending velocity, and cause the air to be deflected laterally on all sides above. This is another reason why the observed temperature in a cyclone at high altitudes may sometimes be found to be below the normal temperature, for it may even be lower than the surrounding temperature, which, it has been shown, must itself be lower than the normal or average.

It is well known that the writer does not hold that areas of high pressure are anticyclones, and that his anticyclones surround and accompany the cyclones. It is, therefore, not logical to make observations upon one thing, as the long-continued high pressure on the Alps, and then to deduce arguments from them against something entirely different. And this is especially the case here, where it is not shown that the thing observed is an anticyclone in any sense. The writer's view is that high-pressure areas of long duration in winter depend mostly upon the diminished temperature, though dynamic causes may come in at first in the origin of the smaller ones, such as that over the Alps, already so often referred to. The areas of highest pressure and of the greatest extent are found over Europe-Asia, and only in the winter season. Of 81 cases investigated by Loomis,[1] 74 were found to have a barometric pressure of 31 inches and above, and their average extent to be 3800 miles from north to south, and about 4900 miles from west to east, the limits being determined by the isobar of 30 inches. It is unreasonable to consider these as anticyclones, and that the high pressures are due to dynamic causes. It is not only impossible to give any explanation of a supposed anticyclonal motion around so large an area, but the high pressure is very satisfactorily explained by the diminished temperature. In the northern and eastern part of Asia the normal January temperature is 45° (81° F.) below freezing; and this, from what has already been shown, is amply sufficient to give an increase of barometric pressure of more than an inch, making all the due allowances. Of course, in this high-pressure area there is a very gradual settling down of the air, and consequent increase of temperature in the lower strata of the atmosphere, a little above the earth's surface, but this is not sufficient to overcome the effect of the powerful radiation from and through the clear air during the long and clear winter nights of these high latitudes.

In the conclusion of the memoir Dr. Hann ascribes the origin of cyclones to the general circulation of the atmosphere, and considers them as being simply subordinate parts of this circulation, and independent of any local causes. It is somewhat different from the old hypothesis which prevailed before Espy's time, according to which

[1] "Popular Treatise on the Winds, &c.," pp. 269–270.

[1] Memoirs of the National Academy of Sciences, vol. iv. Part 2, p. 39.

all kinds of whirlwind phenomena originate in the meeting of counter currents of the air, and then continue often for days and weeks increasing in power and dimensions, without the apparent expenditure of any energy. He also thinks that the barometric maximums and minimums arise mostly from the descending and ascending motions of the air in the vertical circulation of the atmosphere. He says:—"There can scarcely be any longer a doubt that the pressure relations in the barometric maximums and minimums are generally explained for the most part by these kinds of motion. The forces which we really see in the atmospheric circulation in the higher latitudes, especially in winter, arise from the warmth of the tropics; that is, from the difference of temperature between the polar regions and the equatorial zone. The cyclones and anticyclones are merely subordinate parts of the general atmospheric circulation. The masses of air set in motion polewards by the upper gradients are resolved in part into great whirls, the principal progressive motion of which is controlled by the prevailing west component of the former. The influence of the inequalities of the earth's surface, the different heating and cooling of the land and ocean, and the bringing in of aqueous vapour and its condensation, come thus into account as matters of secondary importance."

Of course, nothing more can be claimed for most of this than hypothesis, but a mere hypothesis is not to be despised if it is a reasonable one. Let us now consider whether these hypotheses are of this character; and first, the hypothesis which ascribes the origin of cyclones to the general atmospheric circulation. The poleward velocities of the upper parts of the atmosphere are very small. The cirrus clouds of these regions, when not agitated by abnormal disturbances, have an apparently very gentle motion toward the east with scarcely any perceptible poleward motion. The velocity of this motion, especially in winter, may, at a maximum, amount to as much as three or four miles per hour, and that of the downward descent in the higher latitudes to a few inches per minute. Such motions are supposed to throw the atmosphere into a great whirl, several hundred miles in diameter, say in the north-west part of the United States. This whirl passes eastward, over the lakes and on to Newfoundland, with destructive and increasing violence all along its course, then passes into the Atlantic Ocean, and often into Europe, causing shipwrecks and other disasters on the way. And all this forms an incipient whirl produced by the gentle currents of the general atmospheric circulation described above. No attempt is made to show how a couple of forces could be formed from those which cause the general circulation, so as to give rise to a whirl, for this can originate in such a couple only, nor to explain where the energy comes from which keeps such a whirl in motion. It is true, Dr. Hann says the latent energy made free in condensation comes in as a matter of secondary consideration, which may either accelerate or retard, but he leaves it to be inferred that, in either case, the whirl would continue without it.

Again, a whirl originates in low latitudes in the Atlantic Ocean, over near Africa, where there are no poleward gradients; it progresses westwardly, and increases in diameter and violence; the partially condensed vapour rushes out from its top like the smoke and heated air from the flue of a great furnace, or the crater of a volcano, giving rise to a hazy cloud seen at a great distance towards the east; the wave of high pressure which precedes and surrounds such meteors is observed, while yet the air is very little disturbed at the place of observation; it arrives at the Windward Islands and then at Cuba, destroying factories and sugar plantations on the way; it passes over to Florida, and along the whole eastern coast of the United States, increasing in dimensions and violence as it goes, causing great injury to plantations and commerce; it finally ends in a great gyrating storm covering the most of the northern part of the North Atlantic Ocean. All this, this new hypothesis, now brought forward by Dr. Hann, requires us to believe originated in, and is a part of, the very gentle motions of the general circulation of the atmosphere, without any inherent power upon which the astonishing mechanical effects depend.

In order to have a gyration in the air, there must be a couple of forces to cause it, but no such couple can in any way arise from the general motions of the atmosphere, or the forces which give rise to them. And if such a gyration could arise in this way, it would have to be followed up by this couple to overcome the friction, and to continue the gyration. The general circulation of the atmosphere being once established as the result of certain forces, these same forces, according to sound principles of mechanical philosophy, can no more give rise to whirls, or any kind of local disturbances of these general motions, than the force of the sun's attraction, from which results the elliptic orbit of a planet, can cause perturbations in the motion in that orbit. Local effects must have local causes; and a cyclone can no more exist in the atmosphere, independent of any local cause, but dependent only upon the forces which give rise to the general circulation, than the motions of the satellites of Jupiter can be independent of the attraction of that planet, and depend simply upon the attraction of the sun.

As the old hypothesis required the whirl, once produced, to be followed up by counter-currents, so the new requires it to be followed up by a couple of forces, to keep it going. Scarcely anything, therefore, could be better or more appropriately said with regard to the new hypothesis, and in favour of the condensation theory of cyclones, than what Dr. Hann (*Zeitsch. für Meteorologie*, B. x., p. 82), has himself said with regard to the old hypothesis, namely:—

"No one can deny that whirls may arise from the meeting of currents at an angle, but it may be difficult, indeed impossible, to explain in this way the exclusive occurrence of gyrations from right to left in the northern, and the contrary in the southern, hemisphere. It is still more difficult to explain by this theory the fact that the whirl, once formed, progresses hundreds of miles; and meanwhile it continually draws into its motion new masses of air, overcomes a great amount of frictional resistance along the whole path, and produces powerful mechanical effects. Without a constant supply of energy it would be physically impossible—it would be the perpetual motion. To assume a continual new meeting of winds throughout in the requisite direction along the whole course of diversified wind regions, must appear to everyone, however, as a mere play of the imagination.

"The more recent theory of the origination of cyclonic motion through the deflecting force of the earth's rotation upon the air flowing in toward the minimum pressure entirely clears up the unexceptionable gyration of it from right to left in the northern hemisphere, and the contrary in the southern hemisphere. It also especially satisfies us in that it points out the source from which new energy always comes to the gyration when once originated. This comes from the fact that every large cyclone is accompanied by a rich condensation of aqueous vapour. The latent heat set free in condensation causes an accelerated ascension of the air in the cyclone, and so continually produces an underflow of the air from all sides. We now see that the gyration can advance forward, indeed that it must do so, if it continues to exist. The force which is necessary for overcoming the frictional resistance, for the drawing in of the hereto quiet air, for its powerful mechanical effects—this already is laid up in store in the air along the path over which it shall pass. It is still latent, but it is freed in being drawn in. When the cyclone does not find enough of aqueous vapour in the air, and at the same time has to overcome great frictional resistance, then must it soon come to a stand.

Since this more recent theory, in this way, gives us a conclusive physical reason of one of the most important phenomena coming into consideration in storms, I have allowed myself to designate it as the 'physical' theory."

Let us now consider the hypothesis that the maximum and minimum barometric pressures depend upon descending and ascending currents. Dr. Hann does not seem to confide wholly in the anticyclone hypothesis of areas of high pressure, though he still calls them anticyclones. He therefore devised another hypothesis to account for them—namely, that they result from an increase of pressure by the downward current which he supposes exists in these areas, and so regards them as the cause, at least mostly, and not as the effect of the high pressures. He still adheres to the old hypothesis that the zones of high pressure a little beyond the tropics in both hemispheres are caused by the crowding of the upper poleward currents into intermeridional spaces gradually becoming narrower toward the poles, and so by their being deflected down towards the earth's surface, although these high-pressure zones have long since been satisfactorily accounted for, without any mere hypothesis, upon true mechanical principles. Starting out from this hypothesis, he says that even beyond these zones there must be local obstructions and a damming up of the air in places in the higher latitudes, and a consequent deflecting of the currents down toward the earth's surface (*Zeitsch. für Meteorologie*, B. xiv., p. 39). This seems to be what is meant by the forms of motion, in the quotation above, to which is ascribed mostly the temperature relations in the barometric maximums and minimums. It does not appear, however, how the minimums of pressure can be explained by this hypothesis, for both ascending and descending currents require an increase of pressure at the bottom, where there are no literal differences of temperature and density.

The preceding hypothesis, unlike many others, can readily be tested by means of the well-known formula showing the relation between pressure and velocity, which is based upon true and undisputed principles of mechanics. If there were a perpendicular wall around the globe on the 35th parallel of latitude, extending up to the top of the atmosphere, so that any poleward motion would have to be entirely stopped, and we suppose the upper half of the atmosphere between it and the equator to have a poleward motion toward this wall of 10 miles per hour, and that the whole is stopped, turned downward, and deflected back on the lower half of the atmosphere, the greatest increase of barometric pressure, according to the formula, which could arise from this, would be less than 0·004 of an inch. But a very small part only of the air in these high-pressure zones is stopped and turned downward, and the rest passes on to higher latitudes, so that the real effect must be very much less than this. But the observed excess of pressure in these zones is about 0·3 of an inch on the average. Hence the hypothesis could not account for the one-seventy-fifth part of it if all the kinetic energy were there converted into pressure; but considering the very small part which is so changed, it scarcely accounts for the one-thousandth part.

With regard to high-pressure areas being caused by descending currents, it would require a downward velocity of more than 170 miles per hour to cause an increase of 1 inch in the barometric pressure. The same effect would be produced by a horizontal current of that velocity if the kinetic energy were all converted into pressure by a total stoppage of the current; but where the velocity is only slightly hindered by a damming up through obstructions, the velocity would have to be many times more. Hence the hypothesis is entirely inadequate to cause even any measurable increase of barometric pressure.

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Martinsburg, W. Va., U.S.A.

THE FLORA OF THE REVILLAGIGEDO ISLANDS.

THE somewhat peculiar flora of Lower California, as revealed by comparatively recent American explorations, aroused the curiosity of botanists concerning the probable composition of the vegetation of the Revillagigedo group of islands, situated between 18° and 19° N. lat., off the west coast of Mexico. During the spring of 1889, the United States Fish Commission steamer *Albatross* visited the two principal islands, Socorro and Clarion; and Dr. G. Vasey and Mr. J. N. Rose have just published the results of their investigations of a collection of dried plants made in these islands by Mr. C. H. Townsend, the ornithologist of the expedition. A less interesting flora could hardly be imagined, if this be a fair sample of it; but on this point the report in question affords no information whatever. Considering the distance of the islands from the nearest points of the continent, and the size of the principal island, a flora possessing some peculiarities might have been expected, and possibly the few dried plants brought away by Mr. Townsend by no means represent the flora, either as to quantity or as to quality.

Socorro is described as the largest of the group, about twenty-four miles long by nine broad, with elevations up to 2000 feet; and the position is given as 18° 43′ 14″ latitude and 110° 54′ 13″ longitude, being about 260 miles south of Cape San Lucas, Lower California, and nearly the same distance from the nearest point of the Mexican coast. Clarion, a much smaller island, in nearly the same latitude, lies about 4° to the west.

"The total number of species found on the two islands was twenty-six; eighteen are from Socorro, and twelve from Clarion Island, four of which they have in common." The sentence quoted is preceded by the statement that the flora of these islands is doubtless tropical and similar to that of Mexico; a statement that is a little ambiguous, because, although these islands are situated within the north tropic, the plants collected are mostly characteristic of warm temperate and sub-tropical regions rather than of the tropics. In this apparently poor flora, for there is no mention of the existence of any other plants besides those enumerated, are such widely-dispersed plants as *Portulaca pilosa*, *Waltheria americana*, *Tribulus cistoides*, *Dodonæa viscosa*, *Sophora tomentosa*, *Elytraria tridentata*, and *Lantana involucrata*. Ten of the others are undetermined species of common genera, and may be common species; three are described as new, one of which had been previously collected in Lower California—*Cardiospermum Palmeri*, *Perityle socorroensis*, and *Teucrium townsendii*. The Mexican *Aristolochia brevipes*, and the widely spread tropical American parasite *Phoradendron rubrum*, are also recorded as doubtful identifications, the material being too scanty to admit of certainty. This is all the information one can extract from the report; perhaps a more detailed account of the islands and their natural history may appear in some other publication connected with the expedition.

W. BOTTING HEMSLEY.

ON LOCAL MAGNETIC DISTURBANCE OF THE COMPASS IN NORTH-WEST AUSTRALIA.

AS the subject of how far the compasses of a ship, when near land, are affected by local magnetic disturbance has hitherto been more frequently one of controversy rather than a study of facts, it seems important that full publicity should be given to well-authenticated observations.

In September 1885, on board H.M. surveying-vessel *Meda*, when passing Bezout Island near Cossack, North-West Australia, a steady deflection of her compass of 30° was observed, whilst the ship was running over half a mile in a north-north-west direction and in a depth of eight fathoms of water. This remarkable result has since been exceeded by observations made in H.M. surveying-vessel *Penguin* on November 6, 1890.

On this occasion, the *Penguin*, being 2 miles N. 79° E. from Bezout Island, a deflection of 22° was observed in her compass. The ship was immediately anchored, and some hours of the next day were spent in drifting backwards and forwards near that position, and the following results were obtained.

On Bezout Island the absolute values of the variation and dip were normal, the dip being 50° 1'·7 S. But at a position N. 79½° E., distant 2·14 miles from that on Bezout Island, the observed dip on board was 83° S., with a very small deflection of the compass. This may be considered the central point of the disturbing force. At 900 feet to the westward of this the dip was normal, and it decreased rapidly as the centre was quitted in any direction. At about 100 feet south of the centre of disturbance, the compass was deflected 55°. This was the largest deflection observed, but the compass was disturbed over an area of about a square mile. The general depth of water in this area was nine fathoms, and the quality of the bottom quartz sand.

The observations of the magnetic elements at Cossack and the neighbourhood showed little or no disturbance from local magnetic effects. It is therefore evident from the remarkable results now related, which have been derived from observations of undoubted accuracy, that the deflections of the compass in the *Meda* and *Penguin* were due to magnetic minerals at the bottom of the sea adjacent to the ship.

All well-authenticated observations point to a similar source, when deflections of the compass are caused by local magnetic forces external to a ship navigating near a coast.

E. W. Creak.

NOTES.

A general meeting of the International Congress of Hygiene and Demography was held on Monday. The Prince of Wales, who presided, announced that the Queen had consented to act as patron of the Congress. In the report of the Organizing Committee, which was read by Sir Douglas Galton, it was stated that the opening meeting would take place at St. James's Hall on August 10, under the presidency of the Prince of Wales, and the sectional meetings would be held on the five following days in the rooms of the learned Societies at Burlington House. A special committee had been formed to call attention to the Congress in India, and special representations and invitations had been forwarded to foreign Governments (through the Foreign Office), the colonies (through the Agents General), and also to county, municipal, and local councils throughout the country. The Corporation of London had voted £2000 for an entertainment to the Congress, to be given in the Guildhall, and there could be no doubt that much hospitality would be offered to the Congress during its session. The organization of the Congress involved a very large expenditure, first for making the Congress known throughout the world and making the necessary arrangements for its sections, and secondly for the publication of the transactions. It was estimated that between £5000 and £6000 would be necessary, and for this sum an appeal would be made. Sir Spencer Wells read a report from the Reception Committee, Dr. Corfield a report from the Foreign Committee, and Mr. Digby a report from the Indian Committee. It appeared that the Governments of France, Italy, and Switzerland, and several of the colonies, had already accepted invitations to send delegates to the Congress. The reply of the Indian Government had not yet been received, but Lord Cross was fully alive to the importance of India being represented at the gathering. In replying to a vote of thanks for presiding, the Prince of Wales said he had little doubt the seventh Congress would be even more useful than any of its predecessors.

Mr. Edward Bartlett, lately Curator of the Maidstone Museum (son of Mr. A. D. Bartlett, Superintendent of the Zoological Society's Gardens), has been appointed by Rajah Brooke to be Curator of the new Museum at Sarawak. Mr. Bartlett will shortly leave England for Sarawak to take up his new duties. The series to be placed in the Museum at Sarawak will be entirely confined to Borneo objects, which Mr. Bartlett will make every exertion to render as complete as possible. He will have the assistance of some excellent native collectors, hy whose aid he hopes to make many discoveries in the still imperfectly known fauna of Borneo.

We regret to have to record the death of Captain Daniel Pender, R.N., Assistant Hydrographer to the Admiralty. He died on the 12th inst., at the age of 58. Captain Pender had a remarkably extensive knowledge of the Pacific, where he served on various ships for a good many years.

Dr. J. Eric Erichsen was one of the speakers at the meeting held at the Mansion House on February 23, for raising a fund in aid of the extension of University and King's Colleges. His speech has now been printed separately, and ought to do excellent service to the movement on behalf of which it was delivered. Dr. Erichsen has much to say as to the good work accomplished by both institutions, and pleads eloquently for the support needed to enable them to comply adequately with the intellectual conditions of the present age.

The Council of the Society of Arts will consider, early in May next, the award of the Albert Medal, and invite members of the Society to forward to the Secretary, on or before April 18, the names of such men of high distinction as they may think worthy of the honour. The medal was struck to reward "distinguished merit for promoting arts, manufactures, or commerce."

The Shaen Memorial Wing of the Bedford College for Ladies (York Place, Baker Street), with the new science lecture-rooms and laboratories, to which we have already referred, will be formally opened by the Empress Frederick on Tuesday, March 24, at 4 p.m.

The second International Ornithological Congress is to be held in Budapest at Whitsuntide. The opening ceremony and exhibition is on Sunday, May 17, and the concluding session on May 20, after which various excursions, beginning on the 21st, will be made. It is intended to divide the Congress into seven Sections, viz.: (1) Systematy; (2) Biology; (3) Anatomy; (4) Avigeography; (5) Oology; (6) Migration; (7) Economical Ornithology. The President of the General Committee is Count Bethlen, the Minister of Agriculture, and the President of the Scientific Committee, Dr. Otto Herman. The official head-quarters will be the National Museum, Budapest.

An Electrical Exhibition was opened by the Lord Mayor on Monday in the St. Pancras Vestry Hall. The electric light is to be supplied to the parish by the Vestry, and the object of the present exhibition is to familiarize the inhabitants with the various conditions of electric illumination.

Among the deaths registered in the Zoological Society's Gardens last week, was that of a European Crane (*Grus cinerea*), which was acquired by purchase on May 13, 1848, and had thus lived nearly forty-three years in the Gardens. This is not quite so good a case as that of the Black Parrot (*Coracopsis vasa*), which died in 1884, after having lived fifty-four years in the Regent's Park.

THE Geologists' Association has made arrangements for an excursion to the Isle of Wight at Easter. Head-quarters for the first two days will be at the Totland Bay Hotel, and for the remainder of the excursion at the Blackgang Hotel.

THE National Congress of French Geographical Societies will hold its twelfth session this year at Rochefort.

THE Société Botanique de France has decided to hold an extraordinary meeting at Collioure, a small town in the Pyrénées Orientales Department, from May 16 to 24. Many excursions will be made. The region is interesting to botanists, and has been rather neglected.

IN the *Kew Bulletin* for March there is a valuable list of the orchids which flowered at Kew in 1890. It enumerates 766 species and varieties, and is published to afford data as to the time and duration of the flowering period of orchids cultivated in England. In the Kew collection no attempt is made, by the cultivation of a large number of examples, to give prominence to the most showy-flowered. On the other hand, as the *Bulletin* explains, every effort is made to obtain and cultivate even small and unattractive kinds of scientific interest, such as the ordinary collector would consider beneath his notice. In the limited space available for orchids as comprehensive a collection of species as possible is aimed at. Consequently, whilst there is never a great display of orchid flowers at Kew, at no time of the year is the collection wanting in flower interest. Thus, whilst the highest number of species flowered in any one month was 125 in May, the lowest was 85 in January. The average for each month was a fraction over one hundred. In 1811 the number of species in cultivation at Kew was only 37. There are now 1342 species, comprised in 158 genera. These figures do not include 174 varieties, and over 100 undetermined plants. The collection is kept up by means of exchange, and a small outlay, about £20 annually, for plants which can be obtained only by purchase.

THE same number of the *Kew Bulletin* contains an official correspondence on the experimental cultivation of Egyptian cotton on the Gold Coast, and a note on "Dammar from New Caledonia."

IN the second of the Cantor Lectures on Photographic Chemistry, delivered on Monday, the 16th, at the Society of Arts, Prof. Meldola called attention to the importance of the principle of associating other substances with the compound undergoing photo-chemical decomposition so as to increase the sensitiveness of the latter. As an illustration of this principle the lecturer alluded to a discovery which has recently been made, and which was practically demonstrated by the discoverer, Mr. F. H. Varley, at the conclusion of the lecture. By associating iron salts with suitable sensitizers, it has been found possible to prepare films quite as sensitive as any of the modern gelatine emulsions, and containing no trace of any silver compound. The advantage of such films, from an economical point of view, is obviously very great, and a new departure in the applications of photography to scientific and other purposes is likely to originate with the exaltation of the sensitiveness of iron salts.

AT the meeting of the Academy of Natural Sciences, Philadelphia, on January 27, Prof. Heilprin exhibited a specimen of *Porites astræoides* from the Caletta Reef, harbour of Vera Cruz, Mexico, which gave some interesting data regarding the rate of growth of coral structures. The specimen in question was received through Captain J. Powell, Chief of Construction of Piers of the Mexican Railway, and is said by that gentleman to have been removed from an anchor which was cast in the autumn of 1885 and drawn in November 1890. The extreme period of growth is thus somewhat over five years, but naturally it is impossible to state how soon after the casting of the anchor attachment of the polyp was made. The coral is a mammillated sheet or crust measuring four inches in longest diameter, and somewhat less than three inches on the shorter diameter. The general thickness of the basal mass is not over $\frac{1}{8}$–$\frac{1}{4}$ inch, although through involution and secondary crustage knobs of considerable prominence have been added to the surface. Assuming the basal growth as the index of actual development, then the annual accretion would be (if we allow full five years for the process) scarcely $\frac{1}{20}$ of an inch. Observations recently made on other species of corals have yielded somewhat similar results.

M. AMÉDÉE BERTHOULE has published a useful little work on the lakes of Auvergne. The subject is considered especially from the pisciculturist's point of view, but some very good photographs of scenery are given, and the author touches on various geological topics.

AT Mont-Dol, in Brittany, already well known to geologists and palæontologists, the remains of about a hundred elephants have been discovered, gathered on a small surface of about 1900 square metres. All the bones are broken, and it is thought that the animals must have been eaten by prehistoric men.

THE French Society of Physiological Psychology has appointed a Committee to investigate cases in which persons suppose themselves to have seen or heard some one who was not really present. M. Sully Prudhomme, member of the Academy of Sciences, will act as President.

THE Quarterly Reports of the Palestine Exploration Fund contain monthly meteorological observations taken at Sarona, Syria, beginning with July 1888, the results of which are deduced by Mr. James Glaisher. The statement for January last contains a comparison of the atmospheric pressure in Palestine and in England for the ten years ending 1889. Mr. Glaisher shows that the mean monthly readings at Sarona are highest in the winter months, but very seldom so high as 30 inches; the lowest are in the summer months, but none so low as 29·6 inches, so that the mean monthly atmospheric pressure is very uniform. The highest absolute reading was 30·285 inches, and the lowest 29·442. Both these readings have occurred several times in the month of January.

THE injury from hail in Würtemberg during the sixty years 1828-87 has been recently investigated by Herr Bühler (*Met Zeits.*). The yearly average of days with hail is 13, and about 0·92 per cent. of the cultivated land was affected, damage being done to the extent of about £120,000. July had most hail (34 days), June coming next (with 30·1 days). There is no evidence of increase of hail in the course of decades. The Black Forest district seems to have specially suffered. The author makes out 17 paths of the hailstorms. One very often frequented is that on the Danube, from Scheer to Ulm (70 km. long and 15 broad). All the paths seem connected with the configuration of the ground, and limited in many cases by quite low heights. Slopes with a western exposure are more in danger than those with an eastern; and plains suffer much less than hilly ground. The frequently affirmed influence of forest on hail-fall is not distinctly proved by the Würtemberg data. Herr Hellmann has made a further study of the figures, and finds that in Würtemberg, as in the Rhone Department and in Carinthia, the chief maximum falls in the second half of July. A secondary one, nearly as high, occurs in June 20–24; this holds also for Carinthia, while in the Rhone Department this maximum is earlier, in the first half of June.

THERE seems to be some room for improvement in the methods adopted at the institutes which teachers in Indiana are compelled to attend. This year, the study at these establishments is botany, and the work is laid out for each month. According to the *American Naturalist*, the teachers will have to study the dog-tooth violet in November; in December they will be searching their gardens for flowering tulips, and scanning the orchards for the blossoms of the apple and peach; and in January the flower and fruit of the strawberry will form the subjects of discussion.

THE Government of New Caledonia proposes to establish a Museum at Noumea, and has appealed for support to members of the Civil Service, native chiefs, persons who are known to occupy themselves with scientific inquiries, and colonists generally. It is hoped that the authorities may be able to form important collections, not only of natural products, but of objects interesting to anthropologists.

MESSRS. MACMILLAN AND CO. are issuing a new and thoroughly revised edition of "A Treatise on Chemistry," by Sir H. E. Roscoe and C. Schorlemmer. In the part recently issued (Part III. of Vol. III.) the authors deal with the chemistry of the hydrocarbons and their derivatives. The whole of the subject-matter has been revised, but they draw attention especially to the renewed discussion of the constitution of benzene, and to the researches of Nietzki and his co-workers on the higher substitution-products of benzene, which have explained the constitution of the remarkable substances derived from the explosive compound of potassium and carbonic oxide.

A NEW edition of the "Year-book of New South Wales" has been issued. It contains much well-arranged information as to the history and resources of that colony.

MESSRS. GEORGE BELL AND SONS have published "The School Calendar, and Hand-book of Examinations and Open Scholarships, 1891." This is the fifth year of issue, and the present volume will certainly not be less welcome than its predecessors. In a preface Mr. F. Storr sums up the educational events of 1890.

MR. L. UPCOTT GILL has published an excellent "Book of Aquaria," which will be most welcome to many students of the ways of aquatic creatures. It consists of two parts, one dealing with fresh-water aquaria, by the Rev. G. C. Bateman, the other with marine aquaria, by Reginald A. R. Bennett.

MESSRS. D. C. HEATH AND CO., of Boston, are publishing a series of "Guides for Science-Teaching." We have received the eighth volume, which treats of "Insecta." It is by A. Hyatt and J. M. Arms, and contains a series of replies to questions which have arisen in the minds of the authors while teaching.

WE learn from the *Journal of Botany* that the series of British plants exhibited in the Botanical Gallery of the Natural History Museum has recently been extended by the addition of a series of British mosses, consisting of 576 species arranged in 129 genera. The arrangement is that adopted by Hobkirk in the second edition of his "Synopsis," and the descriptions have been taken from that work. The Museum has also acquired the extensive herbarium of the late M. Triana, containing upwards of 8000 plants, as well as a large collection from the province of Atacama, Chili, made by MM. Borchers and Philippi.

THE same journal informs us that, at the request of the Irish Land Commissioners, Mr. W. Carruthers, F.R.S., is preparing a plain account of the potato disease, with illustrations drawn by Mr. W. G. Smith, which will be reproduced in chromolithography as a wall-diagram for schools and farm-houses. A reproduction of Bauer's water-colour drawings of the germination of wheat, in the form of six wall-diagrams for educational purposes, is also being prepared, under the direction of Mr. Carruthers, for publication by the Royal Agricultural Society, at a sufficiently low price to bring them within the reach of the poorest schools.

SILICON bromoform, $SiHBr_3$, has been obtained in the pure state by M. Besson, and an account of its mode of preparation and more important properties is given by him in the current number of the *Comptes rendus*. It is well known that hydrobromic acid exerts an action upon crystalline silicon of a somewhat similar nature to the action of hydrochloric acid, which forms a mixture of silicon chloroform and tetrachloride; but hitherto, M. Besson states, the products obtained have never been separated. Buff and Wöhler, the discoverers of silicon chloroform, $SiHCl_3$, long ago obtained a colourless fuming liquid by the action of hydrobromic acid upon silicon, resembling the corresponding chloroform in properties, but which was certainly contaminated with other products, especially silicon tetrabromide. M. Besson has isolated the compound by repeated fractional distillation of the product obtained by passing a stream of dry hydrobromic acid gas over crystals of silicon heated to a temperature just below redness. The main bulk of the liquid product consisted of silicon tetrabromide, boiling at 153°, but 5 per cent. distilled constantly at about 110°, and gave numbers, on analysis, closely agreeing with the formula $SiHBr_3$. Pure silicon bromoform is a colourless liquid, most difficult to work with. In the first place, it fumes exceedingly strongly at the first contact with air, and in a few minutes spontaneously inflames. Again, the vapour forms highly explosive mixtures with air which occasionally suddenly detonate with great violence. It is only possible, of course, to distil it in an atmosphere of an inert gas, when it boils at 110°. It still remains liquid at temperatures as low as −60°. Water at once decomposes it, and with solutions of alkalies the decomposition is very violent. Strong potash liberates twice as much hydrogen as is contained in the compound, the reaction proceeding as follows:—

$$SiHBr_3 + 5KOH = K_2SiO_3 + 3KBr + 2H_2O + H_2.$$

Dry ammonia gas also reacts in a very lively manner with silicon bromoform, and if the reaction is not moderated it is accompanied by incandescence. The white product appears to consist of a definite compound mixed with more or less of its products of decomposition. Phosphoretted hydrogen is without action under ordinary circumstances, but when compressed to 25 atmospheres in a Cailletet apparatus in contact with a few drops of silicon bromoform at the ordinary temperature, a white solid body is formed, which persists for some time after the pressure is removed, and which loses phosphoretted hydrogen in a stream of carbon dioxide. Silicon chloroform, when in contact with compressed PH_3 under like circumstances, forms an analogous substance in definite isolated crystals, which rapidly grow as long as the pressure is maintained.

THE additions to the Zoological Society's Gardens during the past week include a Passerine Parrakeet (*Psittacula passerina*) from South America, presented by Miss Edith B. Burrell; a Markhoor (*Capra megaceros* ♂) from North-East India; a Bennett's Wallaby (*Halmaturus bennetti* ♀) from Tasmania; an Indian White Crane (*Grus leucogeranos*) from India, deposited; a Striped Hyæna (*Hyæna striata* ♂) from North Africa; a Maguari Stork (*Dissura maguari*); a Brazilian Teal (*Querquedula brasiliensis* ♂) from South America, purchased; an Indian Muntjac (*Cervulus muntjac* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE "CAPTURE THEORY" OF COMETS.—A memoir by M. O. Callandreau, on the capture theory of periodic comets, has recently been published (*Annales de l'Observatoire de Paris*, vol. xx.). It is generally known that the periodic comets are distributed in groups which depend in some manner on the major planets. Jupiter's family of comets is at least fifteen in number, and all the members of it have direct motion, orbits only slightly inclined to the orbit of Jupiter, and aphelion points near

Jupiter's aphelion; what is more—one of the two points where each of them intersects the plane of Jupiter's orbit is generally very near to the trajectory of this planet. The theory which best explains such distribution is that which regards the comets of which the groups are composed as having come under the perturbing influence of the major planet to which they are respectively related. If a comet arrives from interstellar space into the solar system with a sensible parabolic velocity, and passes near a major planet, the velocity will be either diminished or increased. In the former case, the parabolic orbit would be transformed into an elliptical one, and the comet would be, as it were, incorporated into our system—captured by the planet. If, on the other hand, the velocity is accelerated, the orbit becomes hyperbolic, and the comet moves away from our system, never to return. The results of a research on this subject were given by M. Tisserand a few months ago (*Bulletin Astronomique*, July 1889, and NATURE, vol. xlii. p. 31).

M. Callandreau has at present only investigated the strong perturbations which a comet experiences when passing in the neighbourhood of a major planet—that is, a particular case of the problem of three bodies. He has considered the perturbations when a comet approaches very near to the disturbing body, and examined the difficulties connected with the capture theory. The theory that periodic comets are "ejects" from the major planets is mathematically discussed, and shown to be an improbable one. But it is not sufficient to show that periodic comets may be produced by capture; it is necessary to explain why the hyperbolic comets which the capture operation ought to engender escape observation. M. Callandreau proves that such comets are not seen either because their perihelion distance is very great, or because they only pass perihelion once, and then move to infinity on the hyperbolic orbit. Many other conditions are treated, and similarly interesting results obtained. An accurate knowledge of the formation of comets is of great importance in cosmogony. Such a discussion as the one before us is a decided advance in the matter, the demonstrations being in accordance with M. Callandreau's established reputation.

ANNUAIRE DE L'OBSERVATOIRE DE BRUXELLES.—This interesting *Annuaire* for 1891 has just been received. It is composed of ephemerides containing astronomical data for the ensuing year, statistical, geographical, and meteorological information, and articles on various scientific subjects. The mean positions of the principal stars, with the right ascension for every tenth day, occultations of stars by the moon, and eclipses of Jupiter's satellites are tabulated, as in previous years. Tables are also given of physical units and constants, and a detailed note on absolute measures, on the definition of different electrical units, and on their expression in absolute units. Another section contains a large amount of physiographical information. Dr. Folie contributes an article on diurnal variations in the height of the Pole; M. Spée, one on solar activity in 1890; and M. Lancaster gives an extended account of the climate of Belgium in the same year. An important article on the similarity between maps of the earth and other planets is from the pen of M. W. Prinz. Elements of the planets, and of some of the asteroids discovered in 1890, are also given. The obituary notices refer to the late MM. Montigny, Fievez, and Pirmez.

NEW ASTEROIDS.—Prof. Millosevich discovered the 307th asteroid on March 1, and M. Charlois the 308th on March 5.

THE LONDON-PARIS TELEPHONE.

LONDON and Paris are now connected by means of a telephone, and the completion of so great an enterprise deserves to be specially noted. The scheme was originally proposed by the French Government. It was at once taken into favourable consideration in England, and, when Mr. W. H. Preece had proved that it was practicable, it was adopted by the Postmaster-General.

The following details are taken from the *Times*, which printed on Tuesday a full account of what had been done in the matter. The scheme involved the construction of a trunk telephone line between the two cities, with a telephone cable across the Straits of Dover, the first ever made for the open sea. It was decided to have two separate circuits, so that if one should fail at any time, the other might be in use. The route for the English land line was chosen by Mr. Edward Graves, the Engineer-in-Chief to the Post Office, who has taken a keen personal interest in the whole work. It runs along the South-Eastern Railway to a point near Sidcup, and thence by road and rail through Swanley, Maidstone, and Ashford to the cable-house on the beach at St. Margaret's Bay, between Dover and Deal. The building, which began in September last, was continued throughout the severe frost, except when it snowed too hard to see, and the work was completed by the first week in March. The wire is of copper, the best material for the purpose, and weighs 400 pounds to the mile. The connection between the last pole on the chalk cliff at St. Margaret's Bay and the cable hut on the beach is effected by lengths of the cable core inclosed in an iron pipe and buried in a trench down the face of the cliff. The whole line is 85 miles long, and its excellence is proved not only by the electrical tests, but by the wonderfully clear and loud speaking through it between the cable-hut and the General Post Office. The voice of the speaker in London can be recognized at the hut, and the ticking of a watch distinctly heard.

The French land line follows the direct route of the Chemin de Fer du Nord, through Montdidier and Calais to the cable-house at Sangatte, between Calais and Boulogne. It is similar in construction to the English line, except that only one circuit is run at present, and the copper wire weighs about 600 pounds a mile. Its length is about 204 miles, and the speaking with the D'Arsonval apparatus employed in France is also excellent.

The connecting cable, which is the joint property of the two Governments, was designed by Mr. Preece, and contains four separate conductors, two for each circuit. It was taken on board Her Majesty's telegraph ship *Monarch*, on Monday, March 2, and the following day, in order to be laid when the weather was favourable. On Tuesday evening, March 3, the *Monarch* left her moorings near the *Warspite* and put to sea. Next morning she arrived off St. Margaret's Bay, and afterwards she steamed across to Sangatte; but for several days there was a nasty swell on the sea and a disagreeable haze. After waiting nearly a week in hopes of better weather, the morning of Monday, March 9, broke fine and clear. The long-expected opportunity seemed to have come, and preparations were made for landing the shore end of the cable into the hut at Sangatte. The two lifeboats were lowered, and a strong platform placed across them to form a raft, on which a length of cable sufficient to reach the shore was quickly coiled by the cable hands. The steam launch took the boat raft with the black coil of the cable in tow, the men paying it by hand as she went along to ground. She cast off and gave place to the men, who, in their white overalls and sea-boots, dragged the cable up the sand, along the trench, and into the cable hut. It was half-past 9 when the lifeboats were launched, and 12 minutes to 11 when the end was landed. No time was lost in returning to the ship, which immediately started paying out towards St. Margaret's Bay. The cable ran smoothly out of the tank, through the iron "crinoline," which keeps it from lashing about with the rolling of the ship, it glided along the guides, took three turns round the huge revolving iron drum, with its friction brake which controls the speed of egress, and passed over the starboard sheave or pulley projecting from the bows, then dived into the sea, just grazing the hull about the water line. Mile after mile was traversed in this way, and all was going on well. As yet there were no signs of an approaching storm. A drizzling rain began to fall, and the breeze freshened, but it was not until towards 3 o'clock, when 10 miles of cable had been paid out, and the *Monarch* was half seas over, that the gale came on, and the water became rough. At length it was decided to anchor until a lull in the storm should reveal the land, if only for a little while. The cable was fastened, and the anchor rattled out soon after 4 o'clock. The snow cleared about 5 o'clock, and it was then discovered that the *Monarch* was lying off St. Margaret's Bay, about a mile from the shore, and eastward of the cable hut. An attempt was made to lift the anchor and pay out all the cable, but the strong tide, aided by the furious wind, had driven the cable foul of the anchor, and after a fruitless attempt to clear, the anchor was slipped with 14 fathoms of chain. It was now a quarter past 8 at night, and very dark, but the *Monarch* paid out the rest of the cable to avoid cutting it, and buoyed the end well off the shore to the east of St. Margaret's Bay, about 20 minutes past 9, then ran for the Downs, where she anchored soon after 10 o'clock. Next morning the weather made further operations impossible. Wednesday was not much better, for, although it brightened up, the glass was still unsettled. The

Monarch was now lying at Dover, where she went to land a visitor and take in stores. Thursday was fine, and after picking up the cable from the buoy, she proceeded to clear it from the lost anchor. The line was coiled four times round the anchor, and could only be released by cutting out the damaged part. This was done, the anchor and chain being recovered, and the end of the cable buoyed. She returned to Dover. On Friday nothing could be done owing to the high wind and sea; but Saturday morning was as quiet as a lamb, the blue sky smiling through fleecy clouds. The *Monarch* was early astir, and although the sea was a little hazy, and a strong easterly breeze blowing, the glass was very steady. The ship had spliced the cable by 20 minutes past 11, and then picked up some 5 miles of cable from the buoy, towards Sangatte, relaying it so as to clear a bight in the Calais-Dover line, arriving off St. Margaret's Bay about 20 minutes past 3 in the afternoon, where she anchored 1000 yards away from the landing-place. A raft was speedily formed with the lifeboats, and the shore end landed in the same way as at Sangatte. It was now getting dusk, but groups of spectators had collected on the beach to watch the operations, and a local photographer, deputed by a London illustrated paper, took a picture of the scene. The end was hauled ashore by the sailors at 10 minutes past 6, and 12 minutes later brought into the cable hut. Lieutenant O'Meara called up St. Martin's-le-Grand and announced the good news. Three cheers were given at the Post Office and in the hut through the land line, and those from London sounded so lustily that the lieutenant declared they had split the drum of his telephone. The end of the cable was then stripped and the sheathing filed off, the rasping of the file being plainly heard in London. The cores were then pared, and the cable connected to a Morse apparatus, by which the hut was put in communication with Sangatte. The French electricians there telegraphed a "hurrah for the telephone," and the work was done.

COCO-NUT BEETLES.

THE destruction of coco-nuts in the Straits Settlements by insects has been so great that of late much attention has been given to the question. Perhaps the most important contribution that has yet been made to our knowledge of these pests is a recent report by Mr. H. N. Ridley, Director of Forests and Gardens at Singapore, on the destruction of coco-nut palms by beetles, which has been printed by the Government and issued from the Colonial Secretary's Office. There are, Mr. Ridley says, two species of beetles which are especially destructive to coco-nut palms. The first is the *Oryctes rhinoceros*, commonly known as the rhinoceros, elephant, or black beetle, and the other the *Rhynchophorus ferrugineus*, known as the red beetle. Two other larger species of Calandra attack some palms at Singapore, but Mr. Ridley has not received any notice of their attacking coco-nuts.

The *Oryctes rhinoceros* belongs to the group of Lamellicornia. The parent beetle usually deposits its eggs in decaying coco-nut trees. The identification of the larvæ is very difficult, for the grubs of all the larger Lamellicorn beetles are very much alike. The larva is white and fleshy, and when full grown is about three inches long; the head is round and hard, and is of a dark chestnut colour. It is covered with short bristles; the legs are about half an inch long; the antennæ are short and hairless, and the jaws thick and strong. The chrysalis has the form of the perfect insect; but the insect is very rarely found in this state. The beetle itself is sometimes two and a half inches long; it is very broad, and is of a dark-brown or black colour, and its chitinous coat is very hard. The head of the male is small, and has a horn, about half an inch long, curved towards the back. The wing-cases do not quite cover the body; they are broad and oblong, and covered over with minute punctures. The legs are strong, and the second joint is armed with teeth, by means of which the beetle cuts its way into the tree. The female is usually much smaller, and is readily distinguishable from the male. The grub is quite harmless, but the perfect insect is most destructive. It always works at night, attacking the base of a leaf-stalk, burrowing into the heart of the cabbage, where, as a rule, it remains all the next day. The attack is generally renewed till the rain finds its way in and rots the palm. The destruction of the tree is hastened by the fact that when once a tree has been attacked it appears to become popular. Besides the coco-nut palm, very many other palms, a list of which is given by Mr. Ridley, are destroyed by this insect, but, so far as is known, it does not attack other trees. The present methods adopted for destroying the *Oryctes rhinoceros* are described and criticized in the report. The usual mode is to search for the beetles in the palms, and spear them with a flexible iron wire. Large fires are also made in the plantations at night, and the beetles, flying towards the light, are beaten into the flames by men and boys with branches of trees. Mr. Ridley does not hope to exterminate the pest, but he thinks that its numbers can be greatly reduced by destroying in all the plantations rubbish and vegetable refuse of all kinds. Dead trees should be burnt, and the law should prevent any planter from allowing any heap of vegetable matter, in which the insects always breed, accumulating, and also from keeping any dead trees on his land. By this simple measure the ravages of the beetle can be minimized, if not quite abolished.

The second species of beetle spoken of in this report is the *Rhynchophorus ferrugineus*, the red beetle, which is, perhaps, even more destructive than the other. In the case of *Oryctes rhinoceros*, it is the perfect insect which is destructive; in the present instance, it is the grub. It attacks the trees at night, and having perforated the base of the leaf-stalk, it pushes the egg deeply into the body of the tree. The grub is white and footless, and tunnels through the soft portion of the palm. Unfortunately the presence of this insect in the tree is not so easily detected as in the former case. The grub is a thick, cylindrical, white larva, without feet or antennæ. The head and jaws are small, and the body curved and wrinkled. The perfect insect is usually about two inches in length. The head is small and usually red; the wing-cases are black, sometimes ornamented with red, and a good deal shorter than the body. The legs are black and long, and have a strong claw at the end of the second joint, and two small ones on the feet. The methods of destruction used by the planters are very similar to those used in the case of the rhinoceros beetle, but on account of the difficulty of tracing the red weevil they are not so efficacious. If the black beetle is much reduced in numbers, the effect will be to reduce the red beetle also very much, for the latter will not then be able to take advantage of the holes which have already been made by the former. In dealing with this beetle also, the report urges the necessity of making the destruction of all vegetable refuse compulsory, particularly in the neighbourhood of the palm plantations.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The General Board of Studies have re-appointed Mr. J. E. Marr, Sec.G.S., Fellow of St. John's College, to the Lectureship in Geology, for five years from Lady Day.

The subject of the Adams Prize Essay of 1893 is "The Methods of Determining the Absolute and Relative Value of Gravitation and the Mean Density of the Earth." Candidates must be graduates of the University. The value of the prize is about £170.

Mr. S. F. Harmer, Fellow of King's College, has been nominated to the use of the University table at the Naples Zoological Station.

The Mechanical Workshops Enquiry Syndicate have issued an important memorandum, setting forth a scheme of practical and theoretical instruction in engineering within the University; and state that a sum of £20,000 will be needed for the establishment and equipment of the necessary laboratories. As the funds are not to be had in Cambridge, they propose to make an appeal for benefactions outside the University.

The Agricultural Education Syndicate have issued a voluminous report, containing a complete plan of education and examination in agricultural science and practice, leading either to the B.A. degree or to a diploma in agriculture. It involves the formation of a Board of Agricultural Studies, and the foundation of readerships or lectureships in agricultural botany, chemistry, physiology, &c. Without the amplest pecuniary assistance, the plan is likely to fall by its own weight, but the Syndicate plainly indicate their expectation that adequate subsidies will be forthcoming from the County Councils and from the Government. The report is signed by the fourteen members of the Syndicate, including the Vice-Chancellor, Lord Hartington, Lord Walsingham, Canon Browne, Profs. Liveing and Foster, and Mr. Albert Pell.

SCIENTIFIC SERIALS.

Studies from the Johns Hopkins University Biological Laboratory, vol. iv., No. 7, October 1890, contains:—Notes on the anatomy of *Sipunculus gouldi*, Pourtales, by E. L. A. Andrews (plates xliv. to xlvii.). The very detailed anatomical account is prefaced by an interesting history of the habits of this little sipunculoid, which is very abundant at Wood's Holl, Mass. Comparisons are instituted between the author's accounts of this species and those by Andreæ on *Sipunculus nudus* and by Shipley on *Phymosoma varians*.—On the relationships of Arthropods, by H. T. Fernald (plates xlviii. to l.). Though in working on the problem of the phylogeny of the Arthropods, much labour has been bestowed on the anatomy of the Crustacea, Arachnids, and on Limulus and Peripatus, next to nothing has been done among the Hexapods and Myriapods. Among the Thysanura, the section containing Campodea, Iapyx, Lepisma, &c., is now well known; but for the anatomy and histology of the Collembola the author knew of only Sommer's article on *Macrotoma plumbea*, he therefore has devoted himself to a patient investigation of *Anurida maritima*, and from this standpoint he considers the existing views of the relationships of Arthropods, which are passed in review and commented upon.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 5.—"Preliminary Notice of a New Form of Excretory Organs in an Oligochætous Annelid." By Frank E. Beddard, M.A., Prosector of the Zoological Society. Communicated by Prof. E. Ray Lankester, M.A., LL.D., F.R.S.

So far as our knowledge of the Oligochæta goes at present, the excretory system appears to consist either of one or more pairs of separate nephridia in each segment, or of a diffuse, irregularly arranged system of tubules with numerous external pores upon each segment, and often with numerous cœlomic funnels in each segment; there may or may not be a connection between the tubes of successive segments. All the aquatic Oligochæta have nephridia of the first kind; a large number of the terrestrial Oligochæta have nephridia of the second kind; there is occasionally in the latter forms a specialization of part of the diffuse nephridial system into a pair of large nephridia; these species connect the two extremes. But in all these worms the nephridia are contained in the cœlom, though some of the connecting branches may be retroperitoneal; the ducts which lead to the exterior may branch in the thickness of the body wall, but there does not seem to be any extensive ramification and anastomosis of the tubes in the muscular layers of the body wall (*Quart. Journ. Micr. Sci.*, vol. xxviii., Pl. xxx., Fig. 1, *n*, and Fig. 2).

I have recently found a remarkably different arrangement of the nephridia in an Annelid belonging to a new genus of Eudrilidæ. This family is chiefly noteworthy on account of the remarkable modifications of the reproductive organs, and the present genus is no exception to the rule in that particular; but it shows a further peculiarity—in the structure of the nephridia; the arrangement of these organs in the clitellar region of the body is unique among Annelids, and is to a certain extent suggestive of the condition of the organs supposed to be nephridia in certain Nematoidea. Throughout the body generally, as in other Eudrilids, the nephridia are paired; in the genital region I was struck, on dissecting the worms, by the apparent absence of nephridia. Sections through the body wall in this region show that the longitudinal and transverse muscular layers are traversed by a system of peculiar canals not at all like nephridia in appearanee. These canals are not mere clefts between the muscular fibres, such as Kükenthal has described in his paper "Ueber die Lymphoidzellen der Anneliden" (*Jenaische Zeitschr. f. Naturw.*, vol. xviii., 1885, p. 319); such lymph spaces I have found in a good many Oligochæta, but they never possess a definite wall. On the contrary, the canals which I describe here have a definite darkly-staining wall, with nuclei here and there. They resemble the blood-vessels very closely, and might easily be confounded with them.

These vessels are arranged in a longitudinal and a transverse series with numerous branches and interconnections. The longitudinal muscles are embedded in a nearly homogeneous, transparent, connective tissue, which is of some thickness between the peritoneal epithelium and where the muscular fibres end. It is in the latter tract of tissue that the four principal longitudinal trunks run, corresponding in position to a line connecting the four successive pairs of setæ; there appear to be smaller longitudinal trunks, but the four principal ones run through several segments without a break; these longitudinal trunks are connected with a metamerically repeated system of transverse vessels; these lie between the transverse and longitudinal muscular coats, and appear to run right round the body. They are of considerable calibre, but not so wide as the longitudinal trunks; I could not detect any ciliation anywhere, and their walls are extremely thin. They give off numerous branches, which traverse the body wall in every direction, and form a finer meshwork of tubules; some of the branches run towards the epidermis, and although I could not detect in transverse sections the actual orifices, on account of the fineness of the tubes, I could make out at frequent points a slight modification of the epidermis which seemed to correspond to an external pore.

Upon fragments of the chitinous cuticle being stripped off and examined with a high magnifying power, the orifices were quite plain. They were much smaller than the nephridiopores of *Perichæta*, but not so minute as to be confounded with the pores of the gland cells of the epidermis.

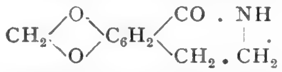
The system of tubes was everywhere accompanied by blood-vessels; but, it is perhaps unnecessary to remark, there was nowhere any connection between these tubes and the capillaries; no coagulated blood was in a single instance found in the excretory tubules.

In spite of their very different appearance, as well as arrangement, from the nephridia of other types, such as *Perichæta*, which possess a diffuse nephridial system, the excretory nature of these tubes seems probable, without any further description. A connection with the body cavity must be proved in order to remove all doubts as to their nature; in each segment, just behind the pair of setæ, the longitudinal duct gives off a branch, which passes through the peritoneum and comes to lie in the cœlom; this branch continues for a short distance, and then abruptly ceases; whether it is furnished with an actual orifice or not I am unable to say. In a few cases, the branch entering the cœlom became connected with a very small coiled nephridial tubule, so small that it was not, as already mentioned, recognizable in dissection.

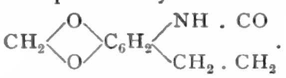
I am inclined to refer the atrophy of the intra-cœlomic part of the nephridia to their having been used up in the formation of the genital ducts. I have recently communicated to this Society a notice of the development of the genital ducts out of nephridia in *Acanthodrilus* ("Roy. Soc. Proc.," vol. xlviii., 1891, p. 452); and that mode of development is possibly general. In any case *the nephridial system of the genital segments of this Eudrilid consists almost entirely of a complex system of tubes, which ramify in the thickness of the body wall, which open by numerous pores on to the exterior, and are connected by a few short tubes with the body cavity.* If the tubes leading to the cœlom became obliterated, and they are very short as it is, the excretory system would consist only of the network in the body walls.

This system of tubes in the skin may perhaps be more comparable to the nephridial network of Cestodes and other flat Worms, than the intracœlomic network of other Oligochæta; its presence, however, in the body walls suggests a comparison with the Nematoidea, which appear to possess at least the remains of a cœlom. In some of these Worms a system of fine tubes connected with the excretory pore permeates the interspaces between the longitudinal muscles. In *Echinorhynchus* the tubes connected with the lemnisci also ramify in the integument, and the lemnisci themselves are processes of the body wall depending into the cœlom.

Chemical Society, February 19.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—The action of reducing agents on αα′-diacetylpentane: synthesis of dimethyldihydroxyheptamethylene, by F. Stanley Kipping and W. H. Perkin, Jun., F.R.S. On reducing αα′-diacetylpentane dissolved in moist ether with sodium, it is converted into a colourless liquid of the composition $C_9H_{18}O_2$. This compound is formed by the addition of two atoms of hydrogen to diacetylpentane, and from its behaviour the authors conclude that it is *dimethyldihydroxyheptamethylene*. The authors also describe the properties and some derivatives of this reduction product.—The osmotic pressures of salts in solution, by R. H. Adie. The osmotic pressures were determined by direct observation by the method of Pfeffer. The results are arranged under the following five headings:—(1) *Boyle's law, applied to solutions.* The results show that the osmotic pressures do not vary directly as the concentration, and when represented graphically give a definite curved form, approaching Ostwald's line of no dissociation at two points, and receding from it elsewhere. Potash alum gives the nearest approach to a straight line of the salts examined. (2) *Charles's law, applied to solutions.* Only one experiment yielded any result, and that was confirmatory of the correctness of applying Charles's law to solutions. (3) *Influence of bases on osmotic pressures.* No definite influence can be attributed to the base, except that the salts of some bases, *e.g.* soda, appear to be more readily dissociated by water than those of others, *e.g.* potash. (4) *Influence of acids on osmotic pressures.* In the case of inorganic acids no definite influence can be detected. In the case of organic acids, an increase in molecular weight of homologous acids increases the osmotic pressure, and a ring nucleus gives a higher osmotic pressure than an open chain. (5) *Avogadro's law, applied to salts in solution.* The author suggests that the differences between the observed results and those calculated on the assumption that Avogadro's law applies to salts in solution, may be due to the existence of simple and complex molecules, as well as to the formation of water compounds.—A direct comparison of the physical constants involved in the determination of molecular weights by Raoult's method, by R. H. Adie. The author shows that the constants of Raoult's method of determining molecular weights, and those ascertained by direct observation of the osmotic pressure do not agree. An attempt is also made to connect, by the same method of Raoult, the osmotic pressure with the gaseous pressure of any substance in solution.—Derivatives of piperonyl, by Frederick M. Perkin. The compound $C_{10}H_9NO_3$, obtained from berberine by the author's brother, is shown to have the constitution



The author also describes the preparation from piperonal of the isomeric compound represented by the formula



—Studies on the constitution of tri-derivatives of naphthalene; No. 9, Andresen's β-naphthylaminedisulphonic acid, by Henry E. Armstrong and W. P. Wynne. The authors have examined the acids obtained by reduction of the products of nitration of naphthalene-1 : 3′-disulphonic acid. The chief product on reduction gives the so-called α-naphthylamine-ε-disulphonic acid $[NH_2 : SO_3H : SO_3H = 1' : 1 : 3']$. Of the other products, one on reduction yields a β-naphthylaminedisulphonic acid $[NH_2 : SO_3H : SO_3H = 2 : 4 : 2']$ (Andresen's acid). This is the first instance in which the formation of a β-nitro-acid by the nitration of a naphthalenesulphonic acid has been observed.

Physical Society, February 27.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were read:—Proof of the generality of certain formulæ published for a special case by Mr. Blakesley; tests of a transformer, by Prof. W. E. Ayrton, F.R.S., and Mr. J. F. Taylor. In 1888 Mr. Blakesley published a number of formulæ relating to the measurement of power, &c., in alternating current circuits by means of electro-dynamometers, one of which had its two coils independent, and placed in different circuits. These formulæ were deduced on certain assumptions, the chief ones being that the currents and magnetizations varied harmonically, and that the magnetic stress in the iron was proportional to the ampere-turns. The present paper shows that the above assumptions are not necessary to the truth of the resulting formulæ. To take the case of a transformer, let alternating current ammeters be placed in the primary and secondary circuits, and a direct-reading "split dynamometer" have a coil in each circuit. Let D_p, D_s, and D_{ps}, be the respective readings of these instruments, then, whatever be the law of variation of the currents,

$$D_p = \sqrt{\frac{1}{T}\int_0^T A_p^2 dt}$$

$$D_s = \sqrt{\frac{1}{T}\int_0^T A_s^2 dt}$$

and

$$D_{ps} = \sqrt{\frac{1}{T}\int^T A_p A_s dt},$$

where A_p and A_s are the values of the primary and secondary currents at any instant, and T the time of one complete alternation. If i be the total induction in the core, P and S the numbers of turns of wire on the primary and secondary respectively, V_p and V_s the terminal pressures, p the resistance of the primary coil, and s the resistance of the whole secondary circuit, then the following equations hold at any instant—

$$V_p = pA_p + P\frac{di}{dt}$$

$$\frac{di}{dt} = \frac{s}{S}A_s.$$

Therefore

$$V_p = pA_p + \frac{P}{S}sA_s.$$

Multiplying both sides by A_p, we get

$$A_p V_p = pA_p^2 + \frac{P}{S}sA_p A_s,$$

and integrating from $t = 0$ to $t = T$, and taking the mean,

$$\frac{1}{T}\int_0^T A_p V_p dt = \frac{p}{T}\int_0^T A_p^2 dt + \frac{P}{S}\frac{s}{T}\int_0^T A_p A_s dt;$$

or

$$\text{watts in primary} = pD_p^2 + \frac{P}{S}sD_{ps}$$

quite independent of the laws of variation of A and V. The power lost in heating the iron core was also shown to be

$$s\left(\frac{P}{S}D_{ps} - D_p^2\right).$$

Other formulæ, such as that given by Mr. Blakesley, expressing the primary volts in terms of the dynamometer readings, are shown to be true generally. On the subject of transformer magnetizations, the authors state that it is desirable not to speak merely of the ampere-turns, but also of the self-induction, for they have reason to believe that the magnetizing value of an ampere-turn varies. Defining the self-induction of the secondary by the equation

$$L_s = \frac{dN}{dA_s},$$

N being the total flux through the secondary, they show geometrically (assuming harmonic variations) that if L_s diminishes, the efficiency increases, and at the same time the phase angle θ between the primary and secondary currents, and the magnetic lag φ, decrease. On comparing this theoretical deduction with the results of experiment, they find a good agreement, as will be seen from the following table:—

Frequency.	Efficiency. Per cent.	L_s	θ	φ
160	96·3	0·0017	113°	0° 24′
160	92	0·0368	169	18 24
160	85·4	0·1241	174·6	45 36

Similar results were obtained in different sets of experiments. It was also noticed that, as the secondary current is increased, the efficiency rises to a maximum and then diminishes, whilst L_s, θ, and φ diminish and then rise again, the current which gives the maximum efficiency coinciding almost exactly with that which gives minimum values to L_s, θ, and φ. The methods employed in making the tests are described in the paper, and the formulæ used in working out the results are there demonstrated. Amongst the facts deducible from the experiments are the following: (1) with constant frequency, the ratios $\frac{V_s}{V_p}$ and $\frac{D_s}{D_p}$ increase as the primary volts rise; (2) with frequency constant and primary volts constant, $\frac{V_s}{V_p}$ decreases, and $\frac{D_s}{D_p}$ increases as the secondary current increases; (3) with constant secondary current, L_s decreases as the primary volts are increased; (4) with small constant secondary currents, the efficiency diminishes as the frequency increases; (5) for large secondary currents the efficiency is approximately independent of the frequency; (6) shunting the secondary with a condenser increases the efficiency. The transformer in which the above tests were made was one of the Mordey type kindly lent by the Brush Electric Engineering Corporation.—

Further contributions to dynamometry, by Mr. T. H. Blakesley. The object of Mr. Blakesley's paper was, in the first place, to show what sort of physical quantities could be advantageously evaluated by using electro-dynamometers of two coils of low resistance in circuits conveying electric currents. The meaning of a dynamometer reading was explained to be the mean value of the product of two currents, either steady or undergoing any periodic variations with sufficient rapidity. In mathematical language such an instrument measured $\frac{1}{T}\int_0^T C_1C_2dt$, where C_1 and C_2 were the instantaneous values of the currents in the two coils, including, of course, the common case where these currents are identical. Any physical quantity whose value was such a product C_1C_2 multiplied into something which was independent of the time, and which, therefore, on integration, came outside the integrator, was well adapted to have its mean value given by such instruments. Power was such a quantity, being merely $(\text{current})^2 \times$ resistance. The square of an E.M.F. was another such quantity, but he did not wish to restrict the method to such evaluations. It follows that any quantity whose instantaneous value can be expressed by terms each quadratic in current, and whose other factor was independent of time, could have its mean value expressed in dynamometer readings. In addition, the particular place and mode of coupling of the dynamometers was indicated by the instantaneous equation, as well as the factor to be applied to each reading. *Thus the equations are made to indicate the practical arrangement to be adopted, and the use to be made of the observations* in each case. Examples were given for the cases of transformers in series and parallel, and special applications of the method were suggested in the measurement of the power employed in such diverse apparatus as voltameters subject to direct or variable currents of any sort, welding machines, parallel generators, tuning-fork circuits, vacuum discharges, and imperfect condensers. For parallel generators the power of each could be separately estimated, and in the case of electric welders, the power employed in the welding circuit was shown to be measurable without introducing any resistance whatever into that circuit. Mr. J. Swinburne said the author's assumption that there is no back or forward E.M.F. in the primary and secondary circuits of transformers except that due to $\frac{di}{dt}$, where i is the total induction in the core, was unwarrantable, for in all real transformers there was a "drop" due to waste field, and this made the split dynamometer method useless. It makes the full load efficiencies too high, and this, he thought, accounted for the extraordinary results obtained by Prof. Ayrton and Mr. Taylor. If a dynamometer be used at all, it should, he said, be used as a wattmeter, the moving coil of one turn being joined in series with a non-inductive resistance and put as a shunt to the primary. The power absorbed by the instrument itself should be then determined, and the power given out by the secondary measured by the same instrument, if the secondary be not non-inductive. Any errors due to self-induction in a wattmeter are, he said, equally present when it is called a split dynamometer, and in addition to this the wattmeter as a split dynamometer precludes the possibility of measuring power. Mr. Mordey said the results obtained by Prof. Ayrton and Mr. Taylor confirmed experiments he had made himself by an entirely different method, for he found that the losses in the iron decreased considerably as the secondary current increased, and this gave increased efficiency. In his experiments he kept the load constant until the transformer attained a steady temperature, and then substituted a direct current for the alternating one, varying its strength until the same steady temperature was maintained. The power thus supplied is a measure of the loss in the transformer under the working condition. A 6 kilowatt transformer tested by this method gave a loss of 110 watts at no load, and at full load 205. Of this 205, 176 was accounted for by the loss in the copper coils, leaving only 29 watts as the iron losses at full load. Figures which he quoted from Prof. Ayrton and Mr. Taylor's paper showed the same general result.—A note on electrostatic wattmeters, by Mr. J. Swinburne, and a paper on Interference with alternating currents, by Prof. W. E. Ayrton, F.R.S., and Dr. Sumpner, were postponed.

Linnean Society, February 19.—Prof. Stewart, President, in the chair.—Mr. Thomas Christy exhibited a number of food nuts utilized by the natives of Northern Queensland, and gave the native names for them. The species, however, had not been determined, since no flowers nor foliage of the trees producing them had been obtained.—On behalf of Mr. A. R. Hunt, the Secretary exhibited a curiosity in the shape of an orange within an orange, and remarked that, although by no means of common occurrence, a similar abnormality had been described and figured by Dr. Perrier (*Bull. Soc. Linn. Normand.*, ix. tab. 2).—Mr. G. C. Druce gave an account of the Dillenian Herbarium at Oxford, prefacing his remarks with some particulars of Dillenius's life and labours, and of the botanists of his day with whom he was in correspondence.—Prof. Stewart exhibited and described a remarkable hermaphrodite trout, explaining, by means of the blackboard, the normal structure of the genital organs in both sexes of the fish, and pointing out in what respects the specimen in question differed.—A paper was then read by Dr. John Lowe, on some points in the life-history and rate of growth in yew-trees, and some excellent photographs and drawings of celebrated yews were shown in illustration of his remarks.

Zoological Society, March 3.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1891.—Mr. Sclater exhibited the typical and unique specimen of Macgregor's Bower-bird (*Cnemophilus macgregorii*) from the Queensland Museum, Brisbane, which had been kindly lent to him by the authorities of that institution.—A report was read, drawn up by Mr. A. Thomson, the Society's head keeper, on the insects bred in the insect-house during the past season.—Mr. O. Thomas gave an account of a collection of small Mammalia made by Mr. F. J. Jackson in Eastern Central Africa during his recent expedition through the territories of the British Imperial East African Company. Fifteen species were represented in the collection, of which three appeared to be new to science. These were named *Nyctinomus lobatus*, *Otomys jacksoni*, and *Rhizomys annectens*.—A communication was read from Miss E. M. Sharpe on the Butterflies collected by Mr. F. J. Jackson during the same expedition. Twelve new species were described in this paper, and a general account of the whole collection was promised on a future occasion.—A communication was read from Dr. R. W. Shufeldt, containing observations on the comparative osteology of the Columbidæ of North America.

Mathematical Society, March 12.—Prof. Greenhill, F.R.S., President, in the chair.—Dr. Hirst, F.R.S., drew the attention of the members present to the loss the mathematical world had sustained by the recent death of Madame Sophie Kovalevsky (see NATURE, February 19, p. 375), and gave many personal reminiscences. The President also touched upon some of her mathematical work.—The following papers were communicated:—On cusp-loci, which are enveloped by the tangents at the cusps, by Prof. M. J. M. Hill.—On the partitions of a polygon, by Prof. Cayley, F.R.S.—Some theorems concerning groups of totitives of n, by Prof. Lloyd Tanner.—Mr. Love and Prof. Hill spoke at some length on four theorems, connected with the motion of a liquid ellipsoid under its own attraction, more particularly concerning the surfaces which always contain the same particles.—Dr. Larmor made an impromptu communication on the collision of two spherical bodies with reference to Newton's experiments.

EDINBURGH.

Royal Society, February 16.—A. Forbes Irvine, Vice-President, in the chair.—Prof. Tait read a further note on the virial. In a former communication he deduced from the expression for the virial a general equation connecting the pressure, volume, and temperature of a substance, and also gave numerical values of the constants in the equation which enabled it to roughly represent the isothermals of carbonic acid. In the present note he gives values of the constants which enable the equation to represent these isothermals with great accuracy in the neighbourhood of the critical point.—Dr. Berry Haycraft discussed the adverse criticism of Salkowski and Jolin regarding his process for the estimation of uric acid. He also reviewed the favourable notices of Hermann, Czapek, and Camerer. He showed that the results obtained by the methods of Salkowski and Jolin cannot be accepted in disproof of the utility of his method.—Dr. Haycraft also described a method for the determination of the density of a liquid, when only small quantities of it can be obtained. A drop of the liquid is placed in another liquid of greater density than it, and a liquid of less density is added until the drop will neither float to the surface nor fall to the bottom of the mixture.—Prof. Tait communicated a paper, by Prof. Cargill G. Knott, on the interaction of longitudinal and circular magnetizations in iron and nickel wires. In a former paper on this subject, Prof. Knott described effects which were observed when a constant current of electricity was made to flow along a wire which was subjected to a cyclical variation of longitudinal magnetization. He has since found that some of the results were due to a slight amount of twist which had been given to the wire previously to its magnetization. A twist of not more than a few minutes of arc per centimetre of length causes a profound modification in the magnitude of the average polarity which is developed in the wire by the cyclical process when the constant current is maintained. The effect of the current is to reduce hysteresis.—Prof. Tait read a paper, by Mr. Robert Brodie, on the value of the method of demonstration by superposition.—Dr. Hugh Marshall described the method of formation of, and exhibited a specimen of, potassium persulphate. The unexpected discovery of the stability of a salt of persulphuric acid is of great theoretical importance.—Dr. John Murray communicated a paper on the temperature of the Clyde sea-area. Among other points, he described the action of an in-shore breeze in summer in accumulating the warm surface-water on the lee shore, and the action of an off-shore breeze in blowing away the warm surface-water from the coast, and thus causing the cold water to rise to the surface. This action is reversed in winter. In one case a variation of temperature of a number of degrees was observed within two days from a reversal in the direction of the wind.

Paris.

Academy of Sciences, March 9.—M. Duchartre in the chair.—On some experiments made in 1890 at the Aubois sluice, by M. Anatole de Caligny.—Observation of the asteroid (308) discovered at Nice Observatory on March 5, by M. Charlois. The asteroid is of the 13th magnitude. Its position was R.A. 10h. 1m. 26s., N.P.D. 70° 17′ 50″, at 8h. 46m. 45s. mean time at Nice on March 5.—Observations of the asteroid (307), made at Toulouse Observatory by MM. Baillaud and Cosserat, and of (308), made by M. Andoyer with the great equatorial. Observations for position were made on March 3, 4, 5, 6, and 7.—Observations of (307) made at Paris Observatory with the East Tower equatorial, by Mdlle. Klumpke. Observations for position were made on March 3, 5, and 6.—On the measure of the 52nd parallel in Europe, by M. Vénukoff. In presenting to the Academy "Mémoires de la Section Topographique de l'État-Major Général Russe," vols. xlvi. and xlvii. (St. Petersburg, 1891), M. Vénukoff remarked upon the results obtained from the Russian triangulation, and those obtained in England by Clark. The Russian stations give 68·6412 kilometres as the mean length of a degree of longitude in latitude 52°; the English measures give the value 68·6880. A similar variation is seen if only Russian stations are considered. It appears, therefore, that the terrestrial surface under the 52nd degree of latitude is not that of an ellipsoid of revolution. This conclusion depends, of course, upon the accuracy of the results cited. Measures of the 42nd parallel in the United States lead, however, to a similar conclusion. M. Vénukoff therefore adds that the earth is not a perfect sphere.—On equations of two minimum periodic surfaces possessing the symmetry of an octahedron, by M. A. Schœnflies.—On harmonic spirals, by M. L. Raffy.—On the compatibility of the laws of dispersion and double refraction, by M. G. Carvallo.—Longitudinal and transverse superposed magnetizations, by M. C. Decharme.—On the hydrated manganites of sodium, by M. G. Rousseau. The author treats of the decomposition of potassium permanganate by heat, and of the formation of manganites by heating sodium manganate with sodium chloride.—On the transformation of sodium pyrophosphite into hydrogen sodium phosphite, by M. L. Amat. The solution of sodium pyrophosphite gradually changes according to the following equation:—

$$Na_2H_2P_2O_5 + H_2O = 2(HPO_3NaH).$$

The law according to which the transformation takes place is given by

$$\frac{1}{x} \log \frac{l-\phi_0}{l-\phi} = k \log e,$$

where k is a constant, x is the duration of the experiment in hours, ϕ is volume in c.c.s of standard soda required for neutralization at end of experiment, ϕ_0 = value of ϕ at beginning of experiment, l = limiting value of ϕ when transformation is complete. The experimental values of the constant $k \log e$ vary from 0·000327 to 0·000369. In the presence of very dilute acid (H_2SO_4), $k \log e$ varies from 0·00149 to 0·00122; with stronger acid, and taking x in minutes, $k \log e$ varies from 0·0125 to 0·0059.—On silicobromoform, by M. A. Besson.—A study of the thermochemical properties of some alkaline derivatives of erythrite, by M. de Forcrand.—On some ammoniacal compounds of cyanide of mercury, by M. Raoul Varet.—On the fermentation of starch by the action of the butyric ferment, by M. A. Villiers. The writer claims to have discovered a new carbohydrate, differing from the sugars, and crystallizing from alcohol as

$$(C_6H_{10}O_5)_6 \cdot C_2H_6O \cdot 5H_2O.$$

—The histological changes of the skin during measles, by M. Catrin.—On the existence of "attractive spheres" in vegetable cells, by M. Léon Guignard. Some time ago M. van Beneden gave the name *sphères attractives* to certain small bodies playing an important part in the segmentation of the cells of animals. M. Guignard now announces that vegetable tissues show attractive spheres similar to those found in animal tissues. He thinks the bodies merit the name of *sphères directrices*, since they govern the division of the nucleus, and move without discontinuity from one cell to another during the whole life of the plant.—On the classification and history of *Clusia*, by M. J. Vesque. The histological characteristics of those Guttiferæ known as *Clusia* are described.—On the chalk of Cotentin, the white Meudon chalk, and the Maestricht tufa, by M. A. de Grossouvre.—Skull of a cave-bear, having traces of a blow inflicted by a flint axe, by M. Wanzel.

CONTENTS.

PAGE

THURSDAY, MARCH 26, 1891.

SCIENTIFIC WORTHIES.

XXVII.—Louis Pasteur.

LOUIS PASTEUR was born on December 27, 1822, at Dôle, where his father, an old soldier who had been decorated on the field of battle, worked hard as a tanner. He was an earnest, industrious, and thoughtful man, fond of reading, and very desirous that his son should be well educated and should gain renown in some branch of learning. Father and mother, alike in their enthusiasm and ambition, devoted themselves to their son—they would "make a man of him," they said.

In 1825 they removed to Arbois, and as soon as he was old enough to be admitted as a day-boy, Pasteur began his studies in the Communal College, and there, after the first year or two, he worked hard and gained distinction. Thence he proceeded to Besançon, and thence, after a year's successful study, to the École Normale in Paris, to which, after gaining a high place in the entrance-examination, he was admitted in 1843. His devotion to chemistry had begun while he was a pupil of Prof. Darlay at Besançon, and now he studied it under Dumas at the Sorbonne, and Balard at the École Normale. His love of science became intense: he spent every day in attendance on the lectures, in reading, and in practical work in both chemistry and physics.

Among the teachers in the École by whom he was most encouraged was M. Delafosse, who was especially studying molecular physics, and it was after a conversation with him that Pasteur was guided to the careful study of crystals, and to the inquiry which led him to his first important discovery.

It was known that the tartrate and the paratartrate of soda and ammonia, though exactly isomeric, similar in their atomic composition, specific gravity, and crystalline form, yet differed, not only in many of their chemical relations, but, as Biot had shown, in the fact that a watery solution of the tartrate deflected the plane of polarized light, and that of the paratartrate did not. Pasteur could not believe that bodies apparently identical in atomic composition and construction could thus differ in their relations to light. He had already observed a dissimilarity between the crystals of the tartaric and of the paratartaric acids, in that the latter were, and the former were not, symmetrical; and he found the same difference between the crystals of their salts. Now, by a laborious study and repeated measurements of the crystals of these and other similar compounds, he showed that in the paratartaric (now called racemic) acid there are two distinct forms of tartaric acid, of which the one dextro-tartaric) is identical with the ordinary tartaric, and like it, in solution, deflects light to the right; the other (lævo-tartaric) deflects it to the left. The two in combination, as they occur in the ordinary paratartaric acid, neutralize one another in their influences on polarized light, and do not deflect it in either direction. Similarly, he next found that the crystals of the two acids and of their respective salts are different in their forms; those of the dextro-tartaric and its salts are similar to those of the ordinary tartaric and its salts in their dissymmetry; those of the lævo-tartaric are also dissymmetrical, but in the opposite direction; those of the two combined in the paratartaric and its salts are symmetrical.

Thus was explained the seeming anomaly of so distinct a difference between bodies exactly isomeric. A great problem was solved; and it was among its results that, gradually, the way was made clearer by which the synthesis of organic alkaloids and of sugars has been achieved.

These researches had occupied six years, and Pasteur had gained by them an early high renown, and he hoped that, by continuing to work on the same lines, he might obtain results yet more important, especially in the contrast of the symmetrical forms of crystals derived from the chemistry of dead substances, and the dissymmetrical of those derived from the chemistry of living bodies. He had invented instruments and methods for experiments. But it chanced that at this period of his work he was, in 1854, appointed Dean of the Faculty of Sciences at Lille, and, though he gave up crystallography with great regret, he determined to investigate and teach a subject of more direct utility. The chief industry of the town was in the manufacture of alcohol from beetroot and corn, and he decided to teach the scientific methods of improving it, and to promote the scientific brewing of beer that might compete with those of Germany and Austria.

Hence, fermentation became his chief study, and in this was the beginning of the researches which led to the most important of his discoveries. But, although it may have seemed like entering on a new subject, the change was an illustration of the regular sequence that may be observed in the order of all Pasteur's work, for, among the facts which had most influence on the course of his investigations, was one which he had observed in his study of the tartaric salts. He had traced the fermentation of the ordinary right tartrate of ammonia in a solution containing albuminous matter, and had observed the coincident appearance of a distinct micro-organism. Then, he had shown the breaking up of the composition of the paratartaric acid by a similar process of fermentation, by putting a minute portion of green mould into water containing phosphate of potassium and ammonia, but no albuminous matter. Here also there was an abundant formation of organisms, and, with this, the gradual disappearance of the dextro-tartaric acid, of which, as other experiments showed, the carbon was taken for the sustenance and growth of the organism of the mould. In both cases the fermentation had seemed due to the action of a living organism.

Pasteur's belief that these and all processes of fermentation were primarily and essentially due to the presence and action of minute living organisms in the variously fermentescible fluids led him to study them with the combined chemical and microscopical research which no one before him had ever used with the same constancy or the same skill. It was, indeed, a combination of methods of research which had hardly ever before been used. There were excellent chemists and there were excellent microscopists; but they usually worked apart, and hardly helped one another. Pasteur—well instructed in exact methods of research, and both chemist and microscopist—attained results far beyond what any before him had reached. It was no easy thing for him to justify the study of fermentation on the lines suggested by what was called the vitalistic or germ-theory, when the hypothesis of "spontaneous generation" was held by many, and when the whole process of fermentation seemed so well explained by the chemical theory of Berzelius, or by that of communicated molecular motion held by Liebig and others who agreed with him in thinking that the organisms in fermenting liquids might be considered as accidental, except in so far as they might supply organic matter to the liquid. But Pasteur did more than justify the germ theory; for he proved its truth by a multitude of facts previously unobserved, and from which he constructed general principles of which both the scientific value and the practical utility are now beyond estimate.

It is not possible to describe in detail or in chronological order the course and methods of Pasteur's researches on the various fermentations and on processes closely allied to them. Only their chief results can here be told; only the things which he discovered, or which, since he studied them, have been deemed not merely probable, but sure. Thus, he proved the constant presence of living micro-organisms, not only in yeast, in which Cagniard-Latour, and Schwann, especially, had studied them, but in all the fermenting substances that he examined; he proved the certain and complete prevention of fermentation, putrefaction, and other similar processes in many substances, however naturally subject to them, by the exclusion of all micro-organisms and their germs, or by their destruction if present; and he proved the constant presence of various micro-organisms and their germs in the air and water, in the earth, in dust and dirt of every kind—their abundance "everywhere." By the proof of these things the evidence became complete, that fermentations and similar processes are primarily and essentially due to the action of organisms living in the fermenting substance. But, more than this, it was ascertained that each method of fermentation—vinous, lactic, acetic, putrefactive, or whatever it may be—is due to a distinct specific micro-organism appropriate to that method, and by its own vital processes initiating the changes which lead to the formation of the "specific products" of the fermentation, the alcohol, the vinegar, the virus, or whatever else. For each specific micro-organism has to live on the constituents of an appropriate fermentescible fluid which it decomposes, assimilating to itself such elementary substances as it needs for its own maintenance and increase, and leaving the rest to combine in the forming of the "specific products." Further, and most importantly, it was made certain by Pasteur's investigations that, by various appropriate methods of "cultivation" which he invented, the individuals of each species of ferment may be separated from others with which they are mingled, and that, thus separated, the individuals of each species may, by means of a "pure cultivation" be multiplied indefinitely, as, especially, in the production of "pure yeast." And in certain instances it was shown that the active power of micro-organisms may, by cultivation, be so "attenuated" that they will produce only comparatively slight changes in substances which, in their natural state, they potently affect.

The most direct applications of the results of Pasteur's studies of fermentation were, naturally, made in the manufacture of wine and vinegar, and, at a later time, of beer. The "diseases," as they were fairly called, of these fermented liquors, the "thickness," "ropiness," "sourness," and others, could be traced to the disturbing influences of various other micro-organisms mingled with those of pure yeast, the true alcoholic ferment, and each producing an injurious method of fermentation. This being made sure, the prevention of the diseases became possible, by the exclusion or the destruction of every organism other than the true one; and of this one the proper cultivation could insure the sufficient production. The methods of thus preventing or arresting the diseases have become too various to be described here. Among them is that of heating wines and beers sufficiently to destroy all the germs remaining in them; and the uses of this, which has been specially called Pasteurization, and of many others, have made brewing and wine-making nearly secure against the great losses to which, before Pasteur's time, they were always liable from the disturbances of their fermentations.

The utility of the proof that fermentations, including the putrefactive, are absolutely dependent on the action of living micro-organisms was speedily shown in the prevention or remedy of diseases far more important than those of wine and beer. The discovery of the causes and processes of what were justly called the "diseases" of fermented liquids enlarged the whole range of a great section of pathology, showing, as it soon did, so intimate relations between contagious diseases and disordered fermentations that it became safe to apply the facts found in the more simple to the study and treatment of the most complex. The study of the diseases of fermented liquids led straightway to the practice of antiseptic surgery. Its first practical application in medicine was in 1862, when Pasteur, who had proved the existence of a living organism in fermenting ammoniacal urine from a diseased bladder, recommended the washing of such bladders with a solution of boracic acid, now a well-known "germicide." This was done successfully by M. Guyon, and the practice is still commonly followed.

For the treatment of wounds and of parts exposed by wounds, the whole subject of the complete exclusion of micro-organisms, and of their destruction if present, was, in and after 1865, studied with especial care by Sir Joseph Lister, and the results obtained by him and others following his example were so convincing that the antiseptic practice, which many justly call Listerism, is followed everywhere by every surgeon of repute. The methods of excluding micro-organisms from all wounds, the antiseptic substances employed, the modes of applying them are more than need be told, and they increase every year; but they have all the same design—the complete exclusion or destruction of all living germs and micro-organisms capable of exciting putrefaction or any other fermentation in parts exposed by wounds; their exclusion from air, water, sponges, surgical instruments, dressings, and everything that can come in contact with such parts. And the system is not nearly limited to the treatment of wounds. In various methods and degrees it is applied in the construction of hospitals and infirmaries, in the prevention of the spread of infection and of every form of blood-poisoning. It is impossible to estimate the number of the thousands of lives that are thus annually saved by practices which are the direct consequences of Pasteur's observations on the action of living ferments, and of Lister's application of them. In the practice of surgery alone, they are by far the most important of the means by which the risks of death or serious illness after wounds are reduced to less than half of what they were thirty years ago; and of the means by which a large number of operations, such as, at that time, would have been so dangerous that no prudent surgeon would have performed them, are now safely done.

In antiseptic practice, and in the manufactures connected with fermentation, the design is to exclude or destroy all micro-organisms. Many kinds might be harmless, but at present there are no ready and sure means of excluding only those from which mischief might arise, and therefore, in the schemes for complete exclusion, no account is taken of the great general principle established by Pasteur, that each method of fermentation is due to the action of one special living organism. This in relation to contagious diseases, would now have become one of the chief subjects of his study. But again, he was obliged to change the subject of his work; for in 1865 he was urged by Dumas, and was forced to consent, to undertake the investigation of a disease of silkworms in the south of France. Since 1849 this disease, called *pébrine*, had been ruinously prevalent. In 1865 the loss due to it was estimated at four millions sterling, and it had spread to many other countries from which silkworms' eggs had been brought to France. M. Quatrefages had reported to the Academy that, among the results of many previous injuries, especially by Italian naturalists, minute "corpuscles" had been found in the bodies of diseased silkworms and in the moths and their eggs. Pasteur, of course, suspected that these were disease-giving organisms, and directed his chief studies to them, though not without making a careful investigation of the whole disease—a task in which he proved himself to be, not only a chemist and microscopist, but an excellent clinical observer.

He soon found that, among several diseases of silkworms, the two most important were *pébrine*, which was specially called *the* silkworm disease, and was the most destructive, and another, often very prevalent, called *flacherie*. He found "corpuscles" in both, and he confirmed and widely extended the observations of those who had already studied them. But the most important facts, both for the prevention of the *pébrine* and for general pathology, were those by which he showed that it was not only contagious but hereditary. The proof of its being contagious was chiefly seen, as by others, in the inoculations of silkworms through accidental wounds, and in the consequences of their feeding on mulberry-leaves on which the disease-germs had been deposited. Many of the worms thus, or in any other way, infected soon died; in many the material for their silk was spoiled; in others, the disease continued, after, their spinning, in their chrysalides and in the moths developed from them. These moths laid diseased eggs and of these were born diseased worms, which died young, or, at the most, were spoiled for the production of silk, and so long as they lived were sources of contagion. Thus the disease passed on by inheritance from year to year. The germs in eggs laid by diseased moths survived; but those left on leaves, or in the dust, or in the bodies of dead moths, soon perished; only in the diseased and living eggs was the contagion maintained.

These things were proved by repeated experiments, and by observations by Pasteur in his own breeding-chamber, year after year; and they made him believe that the disease might be put an end to by the destruction of all diseased eggs. To this end he invented the plan which has been universally adopted, and has restored a source of wealth to the silk-districts. Each female moth, when ready to lay eggs, is placed on a separate piece of linen on which it may lay them all. After it has laid them and has died it is dried and then pounded in water, and the water is examined microscopically. If "corpuscles" are found in it, the whole of the eggs of this moth, and the linen on which they were laid, are burnt; if no corpuscles are found, the eggs are kept, to be, in due time, hatched, and they yield healthy silkworms.

Pasteur continued these studies for four years, going every year for several months to a little house near Alais, in which he watched every step in the life of the silkworms bred and fed by himself and others. His other investigations had thus been in no small degree interrupted; but worse interruption came in October 1868, when, as it seemed from overwork, he had a paralytic stroke. For a time his life was in great peril; but, happily, he recovered, and without mental impairment, though with permanent partial loss of power on his left side. For nearly two years he could do very little beyond directing experiments for repeating and testing his researches at Alais in 1869, and in Austria in 1870. Then came the French-German war, the misery of which, added to that of his paralysis, made him utterly unfit for work. At the end of the war he returned to work, and, after careful researches on the diseases of beer, similar to those by which he had studied the diseases of wines, he gave himself especially to the study of the "virulent diseases" of animals—the diseases which might reasonably be suspected to be due to different kinds of "virus" derived from different species of micro-organisms. The suspicion was already justified by what he had observed in the diseases of wine and beer, and by some more direct facts: for, in 1850, Rayer and Davaine had found organisms in the blood of animals with anthrax; and in 1865, Davaine, stirred by Pasteur's recent demonstrations of organisms much like these as agents in the butyric fermentation, had gathered evidence of the absolute dependence of anthrax on the presence of these organisms in the blood. But his observations were disputed, and his conclusions not accepted, till Pasteur proved that they were correct, and then extended his researches over a far wider range of subjects. The objects of research in this wider range included, indeed only a small proportion of the diseases connected with micro-organisms; but in a few years it was made certain that some, and probable that all, of the diseases usually classed as virulent, contagious, or specific, are due to distinct living micro-organisms, and to the products of the changes which are initiated by them in the blood or other fluids or substances in which they live. Thus, that which had often been an ingenious hypothesis of a *contagium animatum* became an accepted general law; diseases that had been called virulent were now more often called parasitic; and it may justly be said that Pasteur's researches were the efficient beginning of the vast science of bacteriology—vast alike in natural history and in pathology and in its intimate relations with organic chemistry.

Besides ascertaining the micro-organisms appropriate to several diseases, Pasteur, still working on the lines which he had followed in his studies of fermentation and of the diseases of beer and wine, found various means of "cultivating" the germs, separating them, multiplying them, and then testing their different influences on different animals, or on the same animals in different conditions, or after various changes induced in themselves. Among these changes, the most important and most fruitful in its further study were the various means of "attenuation" by which the virulence of disease-producing micro-organisms can gradually be so diminished that at last they can, without harm, be inoculated or injected into an animal which they would have rapidly killed if similarly inserted in their natural state. And some of these injections were shown to be better than harmless, for, by conveying the disease in a very mild form, they rendered the animal, for some considerable time, insusceptible of that same disease in a more severe form; they conferred an immunity similar to that given by mild attacks of the contagious fevers which, as it is commonly and often truly said, "can be had only once." Or, as Pasteur held, the inoculation with the attenuated virus was similar to vaccination, which gives protection from small-pox by producing similar disease in a milder form. Hence began the practice of "protective inoculation" for many diseases besides small-pox.

In studying the methods of attenuation, Pasteur found many facts which are not only valuable in bacteriology, but are likely to help to the knowledge of important principles in general pathology. To cite only a few examples—he found marked differences among the micro-organisms of different ferments in their degrees of dependence on air. The great majority need oxygen for the maintenance of life; but unlike these, which he named aërobic, were some anaërobic, the first examples ever known of organisms capable of living without oxygen. He showed that the bacilli of anthrax, being aërobic, soon perish and disappear in the blood of the animals that have died of the diseases due to them, and that in the same blood the anaërobic septic bacilli needing no oxygen now appear and multiply. In anthrax, also, he showed that the attenuation may best be attained by keeping the cultivated bacilli at a high temperature (about 42° C.) for a certain number of days, regulated according to daily tests of the reduction of their virulence. In the end they become incapable of killing even mice, and are protective for sheep and cattle, and other animals, which in their natural intensity they would rarely fail to kill. In chicken-cholera, the disease for which the first experiments in protective inoculation were made, he showed that the due attenuation can be obtained by a series of successive cultivations of the micro-organisms in pure air, provided that intervals of several days or weeks are allowed between each two of the cultivations in the series. In experiments on the transmission of the virus of a disease of one species through a succession of animals of another species, he showed that the virulence of the bacilli of swine-erysipelas was increased by transmission through pigeons, but diminished by transmission through rabbits. And, as to the varying susceptibility of the same animal under different conditions, a fact so commonly observed in man, he showed that chickens, which are ordinarily insusceptible of anthrax, could be made susceptible by lowering their temperature. They became again susceptible when their natural temperature was restored; and when apparently dying of anthrax in the cold, they recovered if warmed.

It was a step far beyond what had been obtained by protective inoculations when Pasteur invented and proved the utility of his treatment of rabies. Here he proved that when a virus has been inoculated or in any way so inserted that it may justly be deemed sure to destroy life, this result may, at least in the case of rabies, be prevented by a daily or otherwise gradual series of inoculations, beginning with the same virus very attenuated, and diminishing the degree of attenuation till it is used in such intensity as, without the previous graduated inoculations, would certainly have been fatal. The results of the treatment of rabies on this principle are well known; its success is certain, and is enough to justify the hope that by similar treatment, whether with virus simply attenuated, or with some "lymph" derived from a cultivated virus, or from the chemical products of its action on the liquids in which it has grown, other specific diseases may be similarly controlled. This is especially probable for those in which, as in rabies, there is a clear interval between the entrance of the virus and the first outbreak of the disease; and it is becoming very probable that tuberculosis will be one of these. But it would be useless to imagine the probabilities of what will now follow from the researches that have already followed the discoveries of Pasteur.

It hardly need be said that this summary of Pasteur's life and works, and of the chief results to which they have led, can give no fair estimate of the number and the variety of his experiments and observations. Only a complete personal study of his published works, and especially of those in the *Comptes rendus de l'Académie des Sciences*, can give this. Yet even a mere summary may indicate the most notable points that may be studied in his scientific character: of his charming personal character there is no need to speak here. Clearly, he had a native fitness and love for the study of natural science, and these were well educated, and have been manifest in his whole life. But with this loving devotion to science, he has shown not only a very rare power both of thinking and of observing, but that spirit of enterprise which stirs to constant activity in the search after truth, especially by way of experiment. With the power of accurately thinking what is likely to be true, he shows a happily adjusted ingenuity in the invention of experiments for tests of thoughts, and the habit of doubting the value of any scientific thought, even of his own, which does not bear experimental tests. Especially, the thoughts of what may be true in biology seem to have been always submitted, if possible, to tests as strict as those that may be used in chemistry and physics; and they appear to have been repeated and varied with admirable patience and perseverance whenever any doubt of previous conclusions was felt by himself or reasonably expressed by others. He has practised what he urged on his younger colleagues at the opening of the Pasteur Institute: "N'avancez rien qui ne puisse être prouvé d'une façon simple et décisive." Besides, with all his mental power and caution, we can see, in the course and results of Pasteur's work, the evidence of rare courage and strong will, and of singular skill in the use of the best means of scientific investigation. He has been chemist, microscopist, and naturalist, and has applied all the knowledge thus gained to the practical study of pathology. It is not strange that he has attained the results of which the best, and only the best, have here been told.

The honours that have been bestowed on Pasteur need not be mentioned. His chief reward may be in the happiness of seeing some of the results of his life-long work; and, indeed, very few scientific men have lived to see their work bear such good and abundant fruit. No field of biological study has in the last twenty years been so effectually studied as that which he opened, and in which he showed the right methods of research. Now, wherever biology is largely taught, the bacteriological laboratory has its place with the chemical and the physiological; and, for a memorial of the gratitude not only of France, but of many other nations, there is in Paris the Pasteur Institute, which was constructed at a cost of more than £100,000, and was opened in 1889. Here, he may not only see the daily use of his treatment for the prevention of rabies, but may observe and still take his part in the extension of the vast range of knowledge in which there has been constant increase ever since the first sure steps were made by his discoveries.

JAMES PAGET.

THE PAST HISTORY OF THE GREAT SALT LAKE (UTAH).

Lake Bonneville. By Grove Karl Gilbert. "Monographs of the United States Geological Survey," Vol. I. (Washington, 1890.)

WEST of the Rocky Mountains, inclosed by regions which drain to the Pacific, is the extensive area which bears the name of the Great Basin, for from it there is no outflow. This basin in form is rudely triangular, the most acute angle pointing southward, and its greatest length is about 880 miles. At the broader end the general elevation of the wide valleys or plains, which intervene between a series of parallel ridges, is about 5000 feet above the sea; at the narrower end the ground descends gradually till it is about on, or even below, the sea-level. Streams empty themselves into inland lakes in different parts of the Basin, the most important of these being familiar to everyone as the Great Salt Lake of Utah. This, however, is only the shrunken representative of a grander predecessor, a mere brine-pan compared with its fresh and far-spread waters. To a height of about 1000 feet above the present surface, the evidence of lacustrine wave-work and lacustrine sedimentation can still be traced, and to the lake thus indicated the American geologists have given the name of Lake Bonneville. This, in general outline, was rudely pear-shaped, but its shore-line was very irregular, a succession of jutting headlands and deep bays; its surface also was broken with islands. Its area measured about 19,750 miles, not much less than that of Lake Huron. This is now a region of arid deserts, spotted here and there with a salt marsh or a lagoon, and diversified by the Great Salt Lake and two others of smaller size. The greatest depth was originally 1050 feet, for the Great Salt Lake does not exceed 50 feet in any part. Then the waters of Lake Bonneville found an outlet at the northern end, not far away from the mouth of the Bear River, which is now the principal affluent of the Salt Lake. The rainfall then in the northern part of the Great Basin must have been much heavier than it is now; as it diminished, Lake Bonneville contracted in size and increased in saltness. The annual rainfall in this district is now only about 7 inches, while over the region between the Appalachians and the Mississippi it is 43 inches. In the latter the average moisture in the air is about 69 per cent. of saturation, in the former it is only 45; while the evaporation from the surface of Lake Michigan is only 22 inches per annum, for the Great Salt Lake it amounts to 80 inches. The level of the water in the latter is subject to oscillations, dependent partly on variations in the rainfall, partly on the results of extended cultivation, and appears likely in the future to fall somewhat below its present height.

In the disappearance of the ancient lake, epochs—sometimes perhaps rather long—of stability appear to have alternated with eras of change; at any rate, shore-cliffs, terraces, spits, and bars of detritus are very distinctly grouped at intervals above the present water-level. Owing to the scanty rainfall, and the absence, until late years, of any attempt at cultivation, these natural features are preserved with unusual distinctness. In the admirable plates by which the memoir is illustrated, we can see the enclosing hills, bare and arid, but furrowed into a thousand gulleys by the transient storms of myriad years; the wave-worn cliffs which overlooked the margin of the vanished lake; the long shelving slopes which formed its bed; the water-worn *débris*, here accumulated in a long spit, and indicating the general set of the waves; there piled up in a bar, which now runs, like a railway embankment, from headland to headland across the opening of a bay. The plates in themselves are an object-lesson in physical geography.

The origin of the basin of Lake Bonneville, as of all other large lake basins, is undoubtedly, as Mr. Gilbert points out, deformation of the earth's crust, or *diastrophism*, as he proposes to call it. But there is evidence to show—and this is a point of much interest—that during the process of desiccation, this crust has not remained absolutely at rest; these "bench marks" afforded by the lake margins have undergone movements which are not uniform in amount. They are found, on examination, to exhibit variations amounting in some cases to about 350 feet in altitude. Faults, also, may be traced for considerable distances which are later in date than the desiccation of Lake Bonneville. These in places are made very distinct by scarps, crossing lines of terraces or alluvial fans, and facing outwards towards the lower ground. These faults, however, do not indicate any great displacement. The maximum throw does not exceed about 60 feet, and it is often less. Certain localities also have been disturbed by volcanic eruptions. These have occurred before, during, and since, the epoch of the greatest extension of Lake Bonneville. Craters of scoria, as well as flows of lava, remain as monuments—the former occasionally three or four hundred yards in diameter, the latter sometimes a little more than three miles long; the materials are all basaltic. Rhyolitic lavas also occur in the region, but these are long anterior in date to the epoch of the lake. Organic remains are not common in the marls and other deposits of the old lake-bed. This is not surprising in the later period of its history, but they might have been expected in greater abundance in that when the waters were still fresh. The earliest deposits do not carry us back beyond the Pleistocene age, so that, geologically speaking, both the formation and desiccation of the lake are modern events.

This is a bare outline of the last pages of the story of a remarkable district in America, the like of which can be found in more than one other locality on the earth, though, perhaps, in none of them is the record so clearly preserved. Mr. Gilbert's memoir is not only a most careful description and full discussion of the various phenomena presented by this singular dried-up region, but also he turns, not seldom, to questions of wider import, on which, however, want of space forbids us to touch. Moreover, the second chapter of the volume is occupied by a very full discussion of the various topographical features of lake shores. Perhaps in this the author errs occasionally on the side of prolixity, but he brings together so much valuable information that the book will be indispensable to all who wish to study the history and phenomena of lakes and inland seas. We lay it down with a deep sense of gratitude to him for the loving labour which he has evidently bestowed upon this memoir, and will only add that, high as the standard already attained by the publications of the American Geological Survey may be, this monograph, especially in the work of the printer, and in the number, interest, and excellence of its illustrations, more than attains to it. T. G. Bonney.

ON DUCKS AND AUKS.

On the Morphology of the Duck and Auk Tribes. By W. Kitchen Parker, F.R.S. With Nine Plates. (London: Williams and Norgate, 1890.)

When the grave, a few months ago, closed over the remains of W. Kitchen Parker, there still remained with those who knew him the memory of an excellent man and brilliant anatomist. None save one devoted to the science could have worked on as he did, often amid many cares and troubles, feeling a high delight in his work, and considering the attainment of knowledge to be its own reward. From a very early period of his life all his spare moments were devoted to anatomical research, and the hour of death overtook him while as yet, though full of years, he was labouring still.

One of his latest, if not his last work, lies before us. It treats of the morphology of the Anatidæ and the Alcidæ, and has been published by the Royal Irish Academy, as one of its "Cunningham Memoirs." It may be necessary to add that these memoirs are published from the resources of a fund left to the Irish Academy by a Mr. Cunningham, and that great care is taken that the memoirs published therein shall be of a high order of merit.

Most of the materials for this memoir had been in the late Dr. W. K. Parker's hands for many years, but those which he needed to complete his investigations had been only obtained during the last few years from several friends. In a brief introduction he states that for many years after the publication of his first two papers on the osteology of birds (1850-60), his attention was directed solely to the skull. The burden of the anatomy of the skull was placed upon his willing shoulders by Prof. Huxley, who then by degrees tempted him into the investigation of the organs of support, which have proved to be of as great interest and profit as the anatomy of the skull itself.

The two families of birds whose morphology is treated of in this memoir are very distantly related, and the true position and genealogy of the duck tribe present as tough a problem as those of the auk tribe; indeed, herein is enough, Mr. Parker writes, to task the ingenuity and strength of two or three generations of biologists. The cranium in Cygnus and its vertebral column are described from an early stage; the wings of Cygnus and Anas and the hip-girdle of Anas in various stages of development are noticed. Among the auks, *Uria troile* has been selected for description.

In a summary of nearly seven pages, it is pointed out that the Anatidæ manifestly converge towards the Gallinaceous group; that they have the Struthious division of the Ratitæ obliquely below them; whilst the Alcidæ are related to a large and varied group of existing families, but, in their ancestry belong somewhere between those two extremely dissimilar extinct families, the Hesperornithidæ and the Ichthyornithidæ. The revelations made by the precious remains of those two toothed types throw a bright light on one side of these questions of origin and relationship, but intensify the darkness of the other side.

Though both groups are adapted for an aquatic life, they are very sharply defined from each other. The ducks are more or less terrestrial, but are also swimmers; while the auks are not adapted for a land life, but are at home and at ease in the denser or rarer medium—they can dive and fly.

From the ontological standpoint, it will be conceded that that which has dominated the whole bird form is the wing, and embryology shows that this is merely the modified fore-paddle of a low gill-breathing amphibian—a nailless fore-paw. But the nails or claws do appear; yet, in the wing, they are out of place; and this reptilian stage is only transient. If the bird is, indeed, the child of the reptile, it must forget its father's house; it must proceed beyond its progenitor. But if we are willing to see the bird's wing grow, not out of a perfect and typical cheiropterygium, but out of an ichthyopterygium in an unsettled state, ready for transformation into the higher type of limb, then the difficulty is solved. It was a fish paddle; it was not to become a fore-foot; it did change into the framework of a bird's wing; in that respect it is a perfect thing; as a paw, it is an abortion. But an organism moves together in all its parts, if it moves at all; and thus we see that, in correlation to the profoundly modified fore-limb, every other part of this feathered creature has suffered changes.

The whole memoir is devoted to a detailed account of the changes which are thus brought about during this beautiful metamorphosis, the interest of which is increased by the peculiarly fascinating manner of their description, and to which in a brief notice it would be impossible to do proper justice.

OUR BOOK SHELF.

A Dictionary of Metric and other Useful Measures. By Latimer Clark. (London: E. and F. N. Spon, 1891.)

THIS dictionary will be found to be a most valuable and useful *vade-mecum* by all those who have occasion to employ metric and other physical measures. The arrangement of the tables in this form is the most convenient that could have been adopted, and for uniformity and facility of reference could hardly be excelled. One great feature, which is generally lacking in ordinary sets of tables, is the setting forth of the relations of the different metric units to each other: thus, for instance, on looking under the heading gramme-centimetre, we find its equivalent in kilogramme-metres, foot-grains, foot-pounds, joules, ergs, &c., while the latter are indexed under their respective titles. Not only have the French measures with their factors for conversion into British measures been given, but physical, electrical, and other modern units which are so numerous and indispensable.

With regard to some of the fundamental units we may mention that the value of the cubic inch of water, adopted here, is that which "was recently determined with great care by the Standards Department of the Board of Trade"; and in consequence of its being recently legalized, the values of the cubic foot, gallon, &c., have been revised. Throughout the work the logarithms of all the chief factors have been inserted, and at the end there is a short table of logarithms and anti-logarithms adapted for use with any number of figures up to five.

A Text-book of Geometrical Deduction. Book I., corresponding to Euclid, Book I. By James Blaikie and W. Thomson. (Longmans, Green, and Co., 1891.)

THIS work forms an excellent supplement to the first book of Euclid, and by its means a systematic course of training in the art of solving geometrical deductions can be obtained. The arrangement adopted is good, and of a very progressive character. The propositions are divided into sections, and each section is subdivided into three parts: in the first a deduction is worked out in full to serve as a guide to the student; deductions similar to the one already mentioned then follow, in which the figures are in each case given and such notes as are deemed necessary for a beginner. In the last part no figures or notes are added, but occasionally references are given to the propositions on which the proofs depend. The deductions in the last two parts should be written out by the student, and the proofs made to depend on the preceding propositions of Euclid. Additional parts, corresponding to the remaining books of Euclid, are in preparation, and if they are up to the standard of the present one, the series will be found generally useful.

Elementary Algebra. By W. W. Rouse Ball. "Pitt Press Mathematical Series." (Cambridge: University Press, 1890.)

IN this book all those parts of the subject which are usually termed "elementary" are dealt with. It is a sound and well-written treatise. No deviation of importance has been made in the general order of arrangement that has been lately adopted, but many articles and examples which might profitably be left for a second reading have been marked with an asterisk. Permutations and combinations, the binomial theorem and the exponential theorem—subjects which are sometimes included in an elementary treatise, and sometimes excluded—have here only been lightly touched upon, and will serve as an introduction to the more detailed discussions contained in more advanced text-books. Numerous examples are interspersed in the text of each chapter, and here and there are papers and questions that have been set in various examinations. The table of contents is fuller than usual, and will enable the student to find readily any particular article to which he may wish to refer.

A Ride through Asia Minor and Armenia. By Henry C. Barkley. (London: John Murray, 1891.)

THE "ride" described in this book came off in 1878, but the author writes so brightly that only very exacting readers will complain of any lack of freshness in his narrative. His journey from Constantinople occupied ninety-six days, of which fifty-three were spent in the saddle. He rode fourteen hundred miles, the average distance done each day being about twenty-two and a half miles; and, says Mr. Barkley, "if the miserable mountain roads are taken into consideration, I think this was very fair work for a lot of ponies." Apart from the personal incidents of the journey, Mr. Barkley was interested chiefly in the character, manners, and customs of the inhabitants of the districts through which he passed; and on these subjects he records a good many acute observations. It is worth noting that he speaks in high terms of the spirit of hospitality displayed in the parts of the Turkish dominions he has visited. Of course, the Turk is most hospitable to the Turk, and the Christian to the Christian; but "it often happens that the Turk receives the Christian as his guest, and the Christian the Turk." If a respectable traveller finds a want of hospitality on the part of either Turk or Christian, Mr. Barkley cannot but think it is the traveller's own fault.

LETTERS TO THE EDITOR.

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Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

WITH regard to Mr. Bottomley's criticism, I should like to add to what Prof. Rücker has said that Prof. Van der Waals's book is not in any sense a *treatise* on the continuity of the liquid and gaseous states, but a *thesis* wherein is put forward the author's own work on which he claims a doctor's degree.

The preface explains that, in the attempt to determine the value of one of Laplace's capillary constants, the author was forced to proceed by theory, and that the course of these theoretical investigations led him to see that there must be continuity between the gaseous and liquid states. He was, in fact, led to his well-known characteristic equation for a substance in a *fluid* state, an equation in no way depending on the character of the fluidity.

This characteristic and its application are for this thesis the important things, and Prof. Van der Waals proceeds therefore to show that the results deducible from it are in complete agreement with Dr. Andrews' experiments and Prof. James Thomson's suggestions. It is not a point with him to discuss the question of continuity except as bearing on his characteristic; but this continuity is doubtless taken for the title of the thesis as being the most important deduction from his theory.

There is no question of priority: the author gives full information as to where the experiments bearing on the subject are recorded, and only claims to have shown this continuity as a consequence of known laws.

Prof. Van der Waals has been unfortunate in that the English dress in which his thesis appears is a translation from a translation. A literal rendering would have shown that he took his descriptions and diagrams from Maxwell's "Theory of Heat" because this is a "little book which is certainly in the hands of every physicist": it would have prevented the insertion of that footnote on p. 416 alluded to by Mr. Bottomley, since the text runs, "That Maxwell joins the points C and G by a straight line I do not think happy. It is apt to lead off the track and not on to it." The first of Mr. Bottomley's quotations—and with this, I might add, the scientific part ot the preface of the original concludes—should read: "These considerations have led me to perceive continuity between the gaseous and liquid states, the existence of which, as I saw later, had been already surmised by others." *Vermoed* (surmised) certainly seems a weak word in the light of Dr. Andrews' experiments, but it may possibly point to an earlier date for Prof. Van der Waals's theoretical conclusion than that of his thesis.

Christ Church, Oxford. ROBERT E. BAYNES.

The Flying to Pieces of a Whirling Ring.

DR. LODGE having set the ball of paradox rolling, perhaps I may be allowed to point out some of the paradoxes of his critics on the subject of revolving disks, of the well-known "grindstone problem." Prof. Ewing refers to two treatments of this problem, which, however, stand upon quite different footings. Prof. Grossmann's discussion reduces the problem to one in *two-dimensions*, and leaves an unequilibrated surface stress over both faces of the disk. Even if the disk be moderately thin, the solution cannot be considered satisfactory till the *degree of approximation* has been measured by comparison with the accurate solution of the problem. But Grossmann's method is precisely that of Hopkinson (*Messenger of Mathematics*, vol. ii., 1873, p. 53), except that the latter has dropped by mischance an r in his equation (1) [or Grossmann's (6)]. This slip I pointed out in 1886; and Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations. Between Hopkinson and Grossmann this theory has several times been reproduced in technical books and newspapers without comment on its want of correctness. Such first-class technical authorities as Ritter and Winkler have also given quite erroneous solutions of the "grindstone problem."

Prof. Boys refers to Clerk Maxwell's solution. Unfortunately the editor of his scientific papers has given no word of warning about the difficulties of that solution. It involves the paradox of an equilibrated shearing stress on the faces of the disk, and this stress is comparable with the stress which Maxwell supposes to burst the stone (see "History of Elasticity," vol. i. p. 827). Thus both the solutions suggested by Profs. Ewing and Boys suffer from the same defect of unequilibrated stress on the faces. Their difference leads to the fact that Maxwell's causes a hollow disk to burst first at the outer rim, and Grossmann's at the hole.

The solution by Mr. Chree, to which Prof. Ewing refers, seems to me to lie on a higher plane than the other two, and to have been better worth reproducing than Grossmann's, although it cannot be considered as final. Mr. Chree recognizes that for his form of solution normal stresses over the faces of the disk would be necessary, and he proceeds to find their values. Grossmann failed to notice this paradox of his supposed solution, and therefore gives *no measure of the amount of its error*. Some years ago Mr. Chree kindly provided me for lecture purposes with a solution of the disk problem in which the stress on the disk face was zero over a circle of given radius. This was a closer approximation to the facts of the case, but as the stress was still unequilibrated at other points of the face the solution was not of course final.

If all these solutions are therefore paradoxical, where is the correct one to be sought? I fear it has yet to be worked out. Some progress can easily be made with it. It involves four series of Bessel's functions, two of either type, but the surface conditions lead to equations so complex that they will, I think, puzzle the ingenuity of our best Cambridge analysts. When solved, the work to be of practical value must be reduced to numerical tables and not left in the form of infinite series—a type of solution of elastic problem which is so common and yet so technically useless. An Italian has recently solved, by a finite number of definite integrals, the problem of the elastic spherical shell under given surface forces: possibly something might be done for the grindstone problem in the same direction. At any rate, my object in writing to NATURE is to point out that the solutions referred to by Profs. Ewing and Boys are incorrect, and to express a hope that no competent analytical elastician will, owing to these paradoxical solutions, hesitate to try his hand at a very important problem. I am quite certain that no real solution (the paradoxical are myriad) exists prior to 1860, and pretty nearly certain that none has been achieved since, although my bibliography of papers on the strength of materials for the last twenty years is not so complete as I could wish.

University College, March 20. KARL PEARSON.

Deductions from the Gaseous Theory of Solution.

FROM the gaseous theory of solution, Prof. Orme Masson has concluded (see NATURE of February 12, p. 345) that there must be some temperature above which two mutually soluble bodies will be infinitely soluble in each other. This, no doubt, is a fact, and it may be interesting to show that precisely the same conclusion can be drawn from the hydrate theory of solution.

Take first the case of a solution from which a solid separates on cooling. The body which separates, say solid water, does so owing to the tendency of its molecules to coalesce and form solid aggregates; and their tendency to do so is, we know, increased by lowering the temperature: on introducing any substance which possesses an attraction for the water molecules, the attraction of these for their fellows will be in part counterbalanced, and to get them to coalesce a lower temperature will be necessary, and the lower will this temperature be, the more foreign substance there is present; thus the freezing-point of the water will fall as the amount of, say, any salt present in it is increased, as in ADC, Fig. 1. Similarly, if

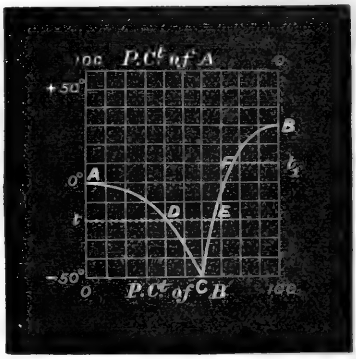


FIG. 1.

we start with the pure salt at B, its freezing-point will be lowered by the addition of water, giving us a curve such as BFEC, which meets or cuts the first curve at some point C—the miscalled cryohydric point. This is precisely what does occur; the woodcut in fact represents the crystallization of water and the hexhydrate of calcium chloride from solutions of this salt, and may be taken as a typical example of the figures obtained in all cases. A solution of the composition D will be the one containing the most water of any which can exist at the temperature t, while E is the one containing the most salt at this temperature, all solutions of intermediate composition being capable of stable existence at t. At t_1 any solution weaker than F will be able to exist, since there is no inferior (*i.e.* for weak solutions) limit of stability, while above B there is neither superior nor inferior limit, and the two substances will be infinitely soluble in each other.

The same general results will obtain when the substances separate on cooling in the liquid instead of the solid condition, but they may be expressed in another form. From Fig. 1 we see that the maximum amount of B which the liquid A can hold at different temperatures is represented by CB, and that this maximum increases with the temperature; it may be represented by AC, Fig. 2; similarly, the maximum amount of A which B can contain is represented by CA, Fig. 1, or BC, Fig. 2, and we thus get in Fig. 2 a double curve which shows that at any temperature below C, such as t, the two substances on being mixed will form two solutions of the composition D and E respectively, whereas at and above C they will form but one homogeneous liquid, their mutual solubility being infinite. This figure is similar to that for aniline and water reproduced by Prof. Orme Masson in Fig. 1, p. 347.

In the above I have purposely avoided using the terms solvent and dissolved substance, since there is much confusion as to their meaning, and, indeed, it is perhaps impossible to differentiate them: when talking of the freezing-points, that substance which crystallizes from the liquid is generally termed the solvent, whereas, when talking of solubilities, the crystallizing substance is termed the dissolved substance.

The want of sound logic displayed in the arguments of the advocates of the gaseous theory of solution, must, I think, be a matter of surprise to many. Their chief argument is this: so-called osmotic pressure is (roughly and with many palpable exceptions) numerically equivalent to the gaseous pressure which the dissolved substance might be expected to exert if it could be gasified; *therefore*, the dissolved substance is a gas. They forget that the osmotic passage of water through a membrane in order to arrive at a solution on the other side, though it might be caused by the dissolved substance bombarding the membrane which it cannot penetrate, might also be caused by other means, such as an attraction of the solution for more water, or by the effective pressure of the solution being less than that of pure water. Surely before building up such a vast superstructure on the foundation of the gaseous nature of the dissolved substance, it would be well to see whether that foundation is real or imaginary. If the dissolved substance is truly gaseous, and if the

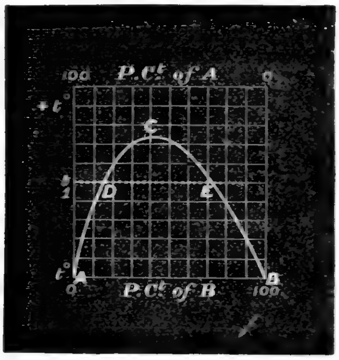


FIG. 2.

solvent, as Prof. Orme Masson says, only "plays the part of so much space," then the dissolution of a gas at constant pressure in a liquid should neither evolve nor absorb heat, but this, I can confidently assert, is not the case: if, again, the so-called osmotic pressure is due to gaseous bombardment, it will be $\frac{1}{2}mv^2$, and we should be able to deduce it from independent measurements of m and v, whereas preliminary experiments of my own lead me to feel fairly certain that $\frac{1}{2}mv^2$ does not give the osmotic pressure. Surely these are fundamental points, the investigation of which should have been the first duty of the advocates of the theory. The flimsy nature of the foundation will account for the number of props which the building requires. The osmotic pressure is not a constant independent of the nature either of the solvent or of the dissolved substance, it is not a rectilineal function of the concentration, and the solvent most palpably does not act as "so much space." The theory has, consequently, to be bolstered up on the side of weak solutions by a supplementary theory of dissociation into ions (with all its inconsistencies), and on the side of strong solutions by the never-failing resource of the destitute—the disturbing influence of hydrates and molecular aggregations.

Van't Hoff started his theory by talking of osmotic phenomena as due to the attraction of the solution for more water. The proximate numerical equivalence of this attraction to gaseous pressure soon caused the substitution of "pressure" for "attraction." The latter has now been entirely lost sight of, and "pressure" has become a catch-word which has blinded his followers to the most patent facts, and has led them to press the supposed analogies of gaseous and osmotic pressure to the most absurd consequences. Prof. Orme Masson's address contains the latest development in this direction. He says, "Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization." Now what can be the meaning of these sentences? What sort of pressure is it that a stable solid exercises? It is not ordinary vapour pressure, for that, where it does exist, does not render a solid soluble, and, indeed, we are told that it is only "analogous to vapour pressure," nor can it be osmotic pressure, for that is a property confined exclusively to (supposed) gases in solution; it can only be some novel property of solids which has yet to be revealed to an expectant world. This, I believe, is the only attempt which has been made to explain on the physical theory *why* a substance dissolves at all, why the solvent which, it is said, has no attraction for it and acts only as "so much space" should not only perform the mighty work required to liquefy and gasify a solid, but should also be able to retain it as a gas under enormous pressure; and if the physical theory is capable of giving no more satisfactory explanation of *the* fundamental fact of dissolution than the above, the sooner that theory is abandoned the better.

Harpenden, March 2. SPENCER U. PICKERING.

Co-adaptation.

THERE is one point in Prof. Meldola's review of Mr. Pascoe's book on the origin of species touching which it seems desirable that I should say a few words. The matter is introduced by the following passage:—

"Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—'On the converting "an ordinary hoofed quadruped" into a giraffe, Mr. Romanes observes: "Thousands and thousands of changes will be necessary." . . . "The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters." The chances of such changes are "infinity to one" against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident.' I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it."

The "dissected" sentences here referred to have been taken from an article on Mr. Wallace's "Darwinism," which I published in the *Contemporary Review* for August 1889. It is, perhaps, needless to say that the "pleonasm" does not occur in the original, and that I do not there hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, but likewise entertained such aid to natural selection as of "importance" in other cases where the phenomena of "co-adaptation" are concerned. Whether or not he underrated the power of natural selection in regard to such cases, it is in my opinion too early to dogmatize. But I am quite sure that "the well-known difficulty" in question cannot be met by the "Neo-Darwinians" with any appeal —explicitly or implicitly—to what is here the false analogy supplied by artificial selection. For example, suppose that there are n different parts which are required to vary, each in one particular way, but all to vary together in the same individual, if any of the variations is to confer an advantage in the struggle for existence. Suppose, further, that there is nothing but "chance" to lead to the simultaneous variation of all these parts in the same individual. Upon these data it is sufficiently evident that the happy combination would not occur with sufficient frequency to admit of being perpetuated in progeny—even if n be only equal to 4 or 5. Now I say that this "difficulty," be it great or small, cannot be met by what Mr. Wallace has called "the best answer"—namely, "the very thing said to be impossible by variation and natural selection has been again and again effected by artificial selection." For there is no "difficulty" at all in understanding how artificial selection is able to choose the separate congenital variations A, B, C, D, &c., as they severally occur in different individuals, and, by suitable mating, to blend them all in a single individual. Here the "selection" is *intentional;* and therefore the whole ground on which the "difficulty" stands is absent. This ground is the supposition of *fortuity*, with regard (*a*) to *all* the variations A, B, C, D, &c., happening to occur in any one individual to begin with, or (*b*) being afterwards preserved (by suitable mating) from obliteration by free intercrossing. Therefore, thus to appeal explicitly from natural selection to the analogy of artificial selection is to be cheated by a metaphor.

How, then, does it fare if the appeal be made implicitly, as in Prof. Meldola's review, by supplying *utility* in the one case as corresponding to *intelligence* in the other? Obviously, here again, the element of *fortuity* is ignored, and therefore, as previously, the "difficulty" is not met, but evaded. For no one who believes in natural selection could deny, that if *each* of the variations, A, B, C, D, &c., is of advantage *per se*, they would all be preserved as they severally happened to arise in this, that, and the other individual, till, by general intercrossing, they would eventually coalesce in single individuals—as in the case of artificial selection. But all this is quite wide of the mark. Indeed, intercrossing is here a necessary condition to, instead of a fatal impediment against, the blending of co-operative modifications; and therefore Mr. Spencer would have been a fool had he brought his "difficulty" to bear upon this case. This case, however, is not that which is meant by "co-*adaptation*": it is the case of a confluence of *adaptations*. Or, otherwise stated, it is not the case where adaptation is *first initiated in spite of intercrossing*, by means of a fortuitous concurrence of variations, each in itself being without any adaptive value; it is the case where adaptation is *afterwards increased by means of intercrossing*, on account of the blending of variations each of which has always been of adaptive value in itself.

The "difficulty," therefore, remains just where it was before; and the only way of meeting it is to show that the phenomenon of co-adaptation does not occur in nature. In other words, it must be shown that the difficulty is fictitious, by showing that, as a matter of fact, there are no cases to be found where *n* modifications, each being useless in itself, become useful in association. Whether or not the difficulty does admit of this the only rational solution, I will not occupy space by discussing; but I have thought it desirable to state what I have always understood to be the real nature of Mr. Spencer's "well-known objection."

Oxford, March 10. George J. Romanes.

Neo-Lamarckism and Darwinism.

It has been sometimes said that it is difficult to tell the difference between the supposed effects of the environment upon an organism, and the accumulation of favourable variations. There can be no difference; for they are but two explanations or theories to account for the same thing. A species is characterized by certain features; it is *these* which have to be accounted for; and any number of theories may be propounded as to the cause. It is simply a question as to which can be "proved" to be either the most probable or actually true.

At one time it was thought satisfactory to account for everything by a direct creative act. A man is exactly the same, whether he was created as he is, or evolved from animals; and if evolved, whether by the direct action of the environment or by natural selection or any other way. We may say with Burns, "A man's a man for a' that."

It is also said that the value of a theory depends upon the number of phenomena it can satisfactorily explain or account for. This is not altogether the case. The theory of creation accounted for everything; but we have abandoned it, nevertheless. The value of a theory really depends, not so much on what it can explain, as upon *the number of facts* on which it is based.

Now, are not many theorists forgetting the importance of this? I have just read Mr. Cockerell's paper on the "Alpine Flora" (Nature, January 1), which will illustrate my contention. He has studied the flora of the mountains of Colorado, and finds that, as a whole, the plants are characterized by certain features. These are the same as are noticeable, not only on European and the Rocky Mountains, but in Arctic and Antarctic regions as well. He comes to the conclusion that "If this [lack of nourishment] were the only cause of dwarfing, the Alpine flora would present clear evidence for the transmission of acquired characters, as the character has undoubtedly become a *specific* one in several mountain plants." He here alludes to *one*, viz. a dwarf habit. The cause, however, which he gives is not the only one, nor is it in this case probably always the right one. If it were, then all mountain and all Arctic and Antarctic regions must have poor soils, for which there is no evidence. All these regions, however, have a relatively lower temperature.

Here, then, we have *two coincidences* of *universal application*—a dwarf habit and a low isotherm. Now we all know from experience how suddenly cold weather instantly checks growth in spring, &c.; therefore, we can infer, or draw the deduction, that the constantly low temperature of the Alps and Lapland perpetually check growth in those regions.

This alone would be a perfectly legitimate conclusion; as the probabilities of there being a distinct cause and effect underlying these coincidences are so great as to amount to a "moral conviction" of the truth.

Though this is logically sufficient, the deduction has been "verified by experiments." When seed is gathered from Alpine plants and sown at low altitudes,[1] and *vice versâ*, the plants raised after a few years begin to assume the characters, respectively, of the same species which are natives of the places. *Now* the argument is complete.

The preceding facts, therefore, warrant one in stating the theory thus: "That Alpine plants have acquired their special characteristics, by the responsive power of their protoplasm under the influence of their environment."

Having lived generation after generation under that same influence their characters have become relatively fixed, hereditary and "specific" as Mr. Cockerell believes. Such plants, however, probably never lose the power of changing again, as experiment shows.

To this scientific explanation Mr. Cockerell superadds the theory of natural selection. He endeavours to explain how natural selection "may" come into play as well. He says:—

"(1) They may escape the violence of high winds which prevail at those altitudes; taller plants being broken off before the seed matures."

Instead of appealing to facts, as he did before, he now begins with an hypothesis. Has he ever seen a taller plant broken off (as often occurs at lower altitudes)?

A mere suggestion is scientifically of little value, unless it be founded upon something which actually occurs.

"(2) They may obtain some additional warmth from their close proximity to the ground and partial shelter."

Here is a remark which ought to have been tested experimentally before being given to the world. Why should not a close proximity to the ground give a chill as well as, or instead of, warmth? As a fact, radiation at night begins on the ground, as the presence of hoar-frost tells us; and therefore we might ask, Are not dwarf plants just as likely (*a priori*), if not more so than tall ones, to suffer as well as to be benefited?

To what *facts* does "partial shelter" refer? Alpine plants are particularly exposed.

"(3) The short summer of the mountain tops necessitates very rapid development; and requires every energy to be thrown into the essential function of producing flowers and seed, leaving nothing to spare for the production of branched stems and diffuse foliage."

This seems like putting the cart before the horse; for how can a seedling plant know that the summer is going to be very short, and that it must, therefore, put forth all its energies? If it understood its own functions, it would know that the flowering depends entirely on the foliage; and, since M. Bonnier, for example, has shown[2] that the chlorophyllous tissue is increased in Alpine plants, this justifies us in looking to it as probably a sufficient cause of Alpine plants having fine flowers.

Finally, has Mr. Cockerell observed any plants which have "failed in the race, and so have been ruthlessly cut off by the autumn storms?" If so, will he give examples? If not, I would refer him to the paragraph italicised above.

To refer once more to the difficulty mentioned above. It must not be forgotten by those who feel that difficulty, that, while the action of the environment on plants is a thing which can be tested, and in many cases admits of easy proof by experiment, the accumulation of many useful variations which mark any living species must ever remain an *a priori* assumption, which is absolutely incapable of verification.

Cairo, February. George Henslow.

[1] As by M. Bonnier, see ref. *infra*.
[2] *Bull. Soc Bot. de Fr.*, 1886, p. 467.

Formation of Language.

SEVERAL years ago, being interested in speculations on the development of language, and having a son a few months old, I instituted a series of minute observations on the part of the entire family as to his utterances. The result, curious at the time, has received a new interest from a later observation. The nursery maid who had charge of the boy did not understand a word of English, Italian being the language spoken with the domestics exclusively. The first articulations of the child were evidently meaningless mimicry of what he heard from us, and it had so much the character of English speech that the maid supposed he was speaking English. There was no attempt to catch or repeat any word—only a gabble, a gibberish, in which we were not able to detect any resemblance to any word of any language. This continued for several weeks, when we perceived that he began to repeat certain sounds to which we found that he attached definite meaning, and as this progressed he left off his incoherent imitation of our language, and he soon had coined a small vocabulary for himself, comprising words for bread, water, milk, &c. The first word we distinguished was as nearly as I can render it "bhumbhoo," meaning water. This phase continued some weeks also, when he began to couple our words for his objects with his own—as "bumbhoo-aqua," when he wanted water. Little by little he dropped his own words and began speaking only Italian. The three stages of the development of language were perfectly distinguishable, but I supposed that the words the child contrived were purely arbitrary, and am inclined to think so still; but during a late visit to Greece I went over to Crete, and visiting in the family of an old Cretan friend, I was interested in a little boy—his young son—who was in the state of development of speech which I have noted in ours as the second. He had only got two or three words, but that for water was precisely the same as that which my own little boy had invented. Have any of your readers who have the watching of child-talk made any analogous observations?

Rome, March 15. W. J. STILLMAN.

Force and Determinism.

IN case any philosophers who do not happen to be physicists feel a doubt about the orthodoxy of what I understand to be one of the main doctrines in Dr. James Croll's recent book, reviewed in your issue of March 12 (p. 435), viz. that although expenditure of energy is needed to increase the speed of matter, none is needed to alter its direction (and the doubt has been already expressed to me); perhaps it will not be regarded as intrusive if I say that this statement is perfectly correct.

Determining the direction of motion involves no expenditure of energy or performance of work. Energy may be guided along desired channels without altering its quantity in the least—just as can matter. The rails which guide a train do not propel it, nor do they necessarily retard it; they have no essential effect upon its energy except a guiding effect. A force at right angles to motion does no work.

It is a function of living organisms thus to direct the path of transference of energy, but they add nothing to its quantity. There is no more energy in a live animal than in a dead one—in a lighted fire than in one ready to be lit. There is activity of transference and transformation in the one case, and stagnation in the other; but the law of conservation has nothing whatever to say against a live animal, or a mind, controlling the motion of molecules; although it would have everything to say against motion being produced *de novo* by an act of will. Life is not energy, it is a determiner of the paths of energy. That is its natural and principal function: it is a director, not a worker. Food and fuel work: life directs. It has control over triggers and sluice-gates. It is not the main-spring of the clock, it is the touch which sets it going. Its best analogue is flame: life is the spark which ignites a conflagration.

The distinction between generating motion and directing motion is evidently one useful to remember. If anyone has thought that an arbitrary alteration of, say, the weather, would necessarily involve a contradiction of the principle of conservation of energy, I think I am right in saying that he has been mistaken.

OLIVER J. LODGE.

Modern Views of Electricity.

MR. BURBURY asks for an explanation of the permanence of the atomic charges in air films, but this carries the question further than I can follow it.

My suggestion is simply that the chemical attraction of zinc for oxygen is necessarily accompanied by + electricity on the zinc and – on the oxygen. The permanence of the charge is, on this view, bound up with the permanence of chemical affinity. It is perhaps only completely to be explained by a knowledge of the mechanism of the latter; and that is one of those "ultimate problems" which I was careful to avoid in my letter.

I fancy, however, that Mr. Burbury has not quite followed me in one point. There is to be no actual combination of zinc with oxygen—only a tendency thereto; and it is to this tendency that the polarized condition of the molecular chain is due.

University College, Bristol. A. P. CHATTOCK.

CHEMICAL SOCIETY'S JUBILEE.

WE have already given the address of the President, Dr. W. J. Russell, at the afternoon meeting. Sir Lyon Playfair and Sir William Grove, two of the five remaining original Fellows of the Society, at the same meeting recited their recollections of the state of chemistry at the time of the foundation of the Society, and we now reproduce their speeches, as they forcibly serve—that of Sir Lyon Playfair in particular—to bring home to us the great changes which have taken place during the fifty years.

At the dinner at the Hôtel Métropole on the Wednesday evening, the Marquis of Salisbury delivered a speech remarkable for the emphasis which he laid on the importance of the work done by the Society in cultivating the higher study of chemistry rather than its industrial applications, and it is noteworthy that Sir William Grove had on the previous day expressed his preference for the abstract rather than the applied side of the science. Such a consensus of opinion is most significant and hopeful. Sir Lyon Playfair, in responding to the Marquis of Salisbury's speech, showed that he was fully aware of the latest discoveries, and able to appreciate their high theoretic import.

At the afternoon meeting Sir Lyon Playfair said:—

It is a sad feeling that there are now living among us only five of the original founders of the Chemical Society. I am one of those five, and have therefore been selected to address a few words to you to-day. You have learned from the excellent discourse of our President that before 1841 chemistry was being both rapidly developed and rapidly evolved. New methods of research were being created; organic chemistry had almost been created. There were many luminaries in the chemical firmament all over the world at that time, and if I mention a few names they will appear to many of you as milestones representing mere discoveries and progress, though they are names well known to the older members of the Society and the few founders who are left as strong personalities with whom we connect much kindness, hospitality, and encouragement. Liebig was then *facile princeps* chemist of the world. He formed a school, and showed how to advance chemistry by original research. At that time, in 1841, the year of our foundation, his brilliant pupil Hofmann had scarcely risen above the horizon. Kopp and Bunsen had made researches, but were still young. There were in Germany names of the highest importance in our science: at Göttingen there was Wöhler, the dear friend of Liebig, and associated with him in his work; in Berlin there was Mitscherlich, the aristocrat of chemistry; there was Rose, the most lovable of our fraternity, who had raised analysis to a high platform by improving methods of research; there was Dove, the jolliest of companions, who had joined physics to chemistry; and lastly, there was Rammelsberg, who took mineralogy out of the domain of physics, and made it part of the domain of chemistry. In France, at that time—I speak only of those whom I personally knew, and whose friendship has ever been valuable to me—there was a man who died only the other day, but who was a veteran then, and famous for his researches on the fatty bodies, Chevreul; there was Balard, the discoverer of bromine; there was

Baron Thenard, the king among lecturers; there was Dumas the eloquent, who established the doctrine of substitutions; and there were other good workers who had not yet acquired the reputations which they afterwards gained —men like Pelouze, Fremy, and Regnault. These were the great luminaries on the Continent; but whom had we at home? There was my old teacher, and to all old chemists devoted friend, Graham, who founded one of the first laboratories of research which existed in this country; who by his profound philosophical views did so much to promote the advancement of chemistry. There was, at Manchester, Dalton, who did as much for chemistry as Kepler did for astronomy. There was Faraday, that prince of electricians; and my dear friend Grove, who now sits beside me, who formulated the correlation of forces; and Joule, who discovered the mechanical equivalent of heat. These names show that the great science of chemistry was active in our country. But it required association to bring the chemists together; it required association to encourage young men in research, and to give them that support which united science always adds to the promotion of investigation. Fifty years, gentlemen, is a long time in the history of an individual, but it is a mere mathematical point in the history of a science. We are sometimes told that chemistry is a modern science: that is not true. The moment that men's minds began to experiment on the constitution of matter, there was a science of chemistry. Tubal Cain was a chemist because he was skilled in brass and iron. Thales was a chemist when he declared that everything was made of water. Anaximines was equally a chemist when he said that everything was made of air. Aristotle was a very advanced chemist when he got out four elements—fire, air, earth, and water. So chemistry has progressed from those days to the present time by the investigation of the laws which govern the combination of the elements, and by examining into the constitution of matter. Now, chemists and microscopists have often been taunted with the fact that they are content to rely on those small particles of matter which we call atoms, and that they are narrow as men of science compared with astronomers, who sweep the skies and examine the motions of large masses of matter. But the astronomers have been obliged to take us into partnership. We have helped them to know the constitution of the stars, and we are now helping them to discover how new worlds are formed. It is unnecessary for me to detain you longer upon the subject of the progress of chemistry; for that has been ably done by the President. But I would like to hold out some encouragement with regard to the future of chemistry. There are periods of great activity in the progress of every science, and that has been manifested during the period terminating in our jubilee. When this Society meets to celebrate its centenary, what a different chemistry it is likely to be from the chemistry of to-day! Already analysis has led to synthesis, yet we know very little with regard to the processes that go on in organic bodies. With regard to the elements, we are beginning to doubt what they are, and even to hope for their resolution. When we find such an important law as the one that the properties of the elements are periodic functions of their atomic weights, what a field is thrown open for investigation! It is a field of discovery the borders of which we have scarcely yet crossed. The motions of the elements may ultimately be known to us, and even the ultimate elements themselves. We call them elements still, because they have a certain fixity, and we are at present unable to decompose them. But recollect that sometimes there comes a man who changes the whole features of a science. What did Newton do for astronomy? With one fell swoop he cleared away the vortices of Descartes, and the tremendous system of "monads," "sufficient reason," and "pre-established harmony" of Leibnitz, by his philosophy; and we may hope that during the next fifty years there will arise a chemical Newton, who will enable us to know far more than we now know, who may bring under one general law the motions of atoms, and even the rupture of those which we now call elements simply because they have acquired a fixity in the order of things and are able to resist changes in the struggle for existence. Let us have hope in the future. Veterans like myself and my friend Grove will not live to see these great discoveries, but some of our younger men will participate in the chemistry of the future, and will look back with interest to the chemistry of the fifty years we are now celebrating. There is no heart here so cold as to doubt the rapid and continuous progress of our science. I express my own thought, and I believe that I express the conviction of each person present, when I conclude in the words of Tennyson:—

> "And men through novel spheres of thought
> Still moving after truth long sought,
> Will learn new things when I am not.
>
> Thou hast not gained a real height,
> Nor art thou nearer to the light,
> Because the scale is infinite."

Sir W. R. Grove spoke as follows:—

My qualification for being here this afternoon is not one of great distinction. It is that of old age, and the privileges of old age are such as nobody envies. With old age come impaired faculties, and one of its effects is loss of memory. When I promised to take part in this celebration I thought that I should have some reminiscences of my early connection with it to bring before you; but when I came to look up the subject I found that my recollections of it were but slender. So that although, as I have said, my main qualification is that of old age—a sort of survival of the unfittest—I am afraid that I shall not be able to assist you very much. Still I do remember some few incidents of the early formation of the Society. I do not remember who was the actual initiator of it; but the most active man in its formation was undoubtedly Prof. Graham. There was a good deal of discussion as to who should be the first President of the Society. We were anxious to get a man of considerable distinction; and I spoke to Faraday. But he thought that he could do more good in research than in assisting in the construction of such a body; and so he declined the honour. Then the matter gradually advanced, until we got the names which appear in the charter of the Society as its original members. Among those names the only ones that I can now recognize are those of my old and good friend Sir Lyon Playfair, Prof. Graham, and my own. After considerable discussion it was agreed that Prof. Graham should be invited to become President, and he accepted. I think that Mr. Phillips's name was previously suggested, but he declined, and proposed Prof. Graham. However, among the names I do not recognize more than those I have mentioned. I am surprised not to see one name among them. Perhaps he was then too young; but he afterwards took an active part in the Society. I refer to Jacob Bell, a very able, gentlemanly, and agreeable man, and also a good chemist. He was the means of introducing into this country the system of selling pure drugs. I could wish that my memory enabled me to tell you more about the origin of the Society; but I do not know that I can give you much information. There were of course discussions among the best chemists of the day. The name of Dalton has been mentioned by our President. I was present at the lecture which Dalton gave at the Royal Institution upon the atomic theory. He was then somewhat aged, of great simplicity of character, and thoroughly devoted to his subject. I well recollect the paper, and his drawings of atoms—little circles to represent atoms as minute spheres grouped together to show their action in uniting to form a molecule of a body. Illustrations were given of the combinations of nitrogen and oxygen, the spheres being arranged in symmetrical little groups around one central sphere. The most compact forms consisted of six spheres around one, for in that case they all touch, and thus pressed together give us a hexagon, as shown in the honeycomb, to the explanation of which a great deal of mathematics has been devoted. I have no doubt that it is caused by the pressure of the bees in crowding into the honeycomb; for each bee with closed wings being cylindrical or nearly so, and somewhat elastic, they convert the spherical cylinders which they make into hexagons by mutual pressure. Conversely you will find that pressure from without acts in the same way, as by winding a band round a bundle of soft clay tubes and gradually tightening it the tubes become hexagons. I think the name atomic theory was an unfortunate one. We talk fluently about atoms as the smallest particles that exist, and chemists regard them as indivisible in the sense of being so hard as to be incapable of further division. To my mind the infinitely small is as incomprehensible as the infinitely great. I use the word incomprehensible advisedly. I do not say that you may not believe in the infinity of the universe; but we cannot comprehend it, we cannot take it in. And so with the atom. Therefore I think that it would have been better to have taken a different word—say minim—which would have been a safer term than atom. As it is, different people think differently as to what an atom really is. However, that was Dalton's theory, deduced from the definite proportions of combining bodies, which is now universally regarded as the keystone of the constitution of matter enabling us to comprehend its combination into definite masses. After the elaborate survey you have just heard from our President, I will not attempt anything approaching to a summary of the chemical discoveries made during the lifetime of the Society; the more so as you can get them in the *Standard* of this morning, or at any rate a very large number of them. There are two ways of regarding science: first, as seeking natural revelations; secondly, practically, as applied to the arts and industries. For my own part, I must say that science to me generally ceases to be interesting as it becomes useful. Englishmen have a great liking for the practical power of science. I like it as a means of extending our knowledge beyond its ordinary grasp, leading us to know more of the mysteries of the universe. The little we can see of it even telescopically is a mere nothing, while what we call an atom is gigantic if its divisibility is infinite.

The spectroscope has been discovered during the lifetime of our Society; and I ought to have been its discoverer. I had observed that there were different lines exhibited in the spectra of different metals when ignited in the voltaic arc; and if I had had any reasonable amount of wit I ought to have seen the converse, viz., that by ignition different bodies show in their spectral lines the materials of which they are formed. If that thought had occured to my mind, I should have discovered the spectroscope before Kirchoff; but it didn't. I cannot recall to my mind any further points sufficiently interesting to speak to you about. Alphonse of Castile is reported to have said that if he had had the making of the universe he would have done it much better. And I think so too. Instead of making a man go through the degradation of faculties and death, he should continually improve with age, and then be translated from this world to a superior planet, where he should begin life with the knowledge gained here, and so on. That would be to my mind, as an old man, a more satisfactory way of conducting affairs. However, it is not so, and we must put up with things as they are. I have been sometimes reproached for having to a great extent given up science for my profession. I need not say that I should have preferred the former. But the necessities of a then large family gradually forced me to follow a more lucrative pursuit. I have said that I prefer contemplative science to science applied to the arts; we are overdone with artificial wants, and life becomes in consequence a constant embarrassment. But there is one practical problem which I would venture to urge upon the attention of the members of this Society: and that is that they should endeavour to prevent the existence of London fogs, even under a constitutional and representative Government.

At the dinner, the Marquis of Salisbury, in proposing "Prosperity to the Chemical Society," coupled with the name of the Right Hon. Sir Lyon Playfair, said:—

I have been, though most unworthy, selected to propose this toast. In vain I pleaded that it would be better in the hands of somebody who knew something about the subject, for those to whom I pleaded were hard-hearted and would hear no excuse. I must therefore proceed, hoping that my distinguished friend who sits next me [Sir Lyon Playfair] will supply that element of knowledge which, perhaps, you will find missing on the present occasion. What naturally strikes me is the importance—the enormous importance—of the science which you cultivate, to the community as a whole. Some hundred years ago, the President of a celebrated tribunal, who was a man of rather advanced opinions, informed Lavoisier that a Republic had no need of chemists. But though a man of advanced opinions he was behind his age. It was the beginning of a time when chemistry more and more as each decade rolled by asserted its vital importance to every class and every interest of every community in the world. I thought—if it is possible to pass any criticism upon the learned and able and most interesting discourse to which we have just listened—I thought that our President was a little too apologetic for chemistry in the early part of the century. Annals which contain the names of Davy and Faraday have no reason to be ashamed. But from my point of view—from the social point of view—chemistry undoubtedly has this claim, that it is one of the most powerful agents that has moved the world. But that is common-place. There is no need for me to tell you what Roger Bacon and Volta have done in the history of the world. But it seems to me that as an educational instrument upon the minds of the community it is one of the most valuable that we possess, because more than any other science it is brought into close communion with pure, real fact. Science is a word that is elastic; and in our days we hear many definitions of it. We hear something of the scientific imagination, a most valuable quality, which I would be the last to depreciate; only I think that, like many valuable concentrated essences, it ought to be indulged in only in small quantities. When there is a proportion in its admixture similar to that which Falstaff observed in his mixture of bread and sack, you feel a desire for more of the solid nutriment and less of the stimulating spirit. But chemistry has an enormous deal of bread and very little sack; it has a large amount of solid fact, and comparatively little of scientific imagination. For the chemist can always be certain of his discoveries; all he has to do is to repeat the experiment, and there is no doubt of his discovery. But when a man discovers what happened fifty millions of years ago, it is not easy to ascertain the exact accuracy of his discovery; and when he discovers all that is going on fifty billions of miles from us, although there may be much probability in what he teaches, still its certainty is not the same in character with the certainty of the man who can go back to his laboratory and repeat the experiment which he has made. I should say that astronomy is largely composed of the science of things as they probably are; geology consists mainly of the science of things as they probably were a long time ago; but chemistry is the science of things as they actually are at the present time. Now the application of a science of that kind to the national mind by constant familiarity with its teachings, by constant knowledge of its achievements, is of the highest human value. It teaches the mind the immortal difference between guessing and knowing. And the farther chemistry goes on, and the more it asserts the superiority of its ways and canons in all departments of human thought, so far shall we drive guessing to a distance and be satisfied with nothing but what we can know. But my task is to say something about the Chemical Society, and perhaps the most suitable course I can pursue, following the Chairman, is to take the other side from what he took, because that will at least give variety to our proceedings, and will also give you an opportunity of testing the superior value of his remarks. Now he dilated much, and most fitly and justly, upon the enormous value from a material point of view which chemistry has been to society in the rapid development which has marked the present reign. I am far from disputing its splendid services to the people of all Europe during that period. But I do not think that it is for the purpose of securing those services that this Society exists. My Right Honourable friend Sir Lyon Playfair did quite right to go to Manchester and stir them up there and teach them their business, and he was a benefactor of mankind in doing so. But when that impulse had been given, you may trust the self-interest of mankind to be sure that the material interests of chemistry will not suffer in the race. But there are other aspects of chemistry, higher aspects, which it is the function of a Society such as this to protect. It is your duty to keep up its intellectual spirit, to teach that not only those things which are demanded by the interests and industries of this country shall be cultivated, but those things also which carry us nearer to the essences of truth. I am not going to carry that pretension too high. We are beings of a mixed character, and our pursuits must bear a trace of the mixture which we give to them. I am not going to imitate the Oxford Professor of my youth, who said that the one thing he valued in the system of quaternions was the certainty that it could never be defiled by any utilitarian application. But still you will observe that the industrial part of chemistry has been that which has received the highest development. Our distinguished President gave us a touching and pathetic history of what I may call the loves and the vicissitudes of benzene. But why is benzene so famous? Why is she lifted up among so many of her compeers who appear in the chemical lists with formulas as imposing and with histories quite as difficult to follow? It is because the products drawn from benzene, or at least from coal-tar, have had the good fortune to produce colours which catch the female eye. Therefore, it is that benzene is famous. But I plead for her humbler sisters who have produced no colours, but the study of whom may yet be steps to the discovery of mighty laws and phenomena which may interest the world. And this, in my humble judgment, is one of the advantages of this Society, that it tends, by bringing men of different researches and pursuits and different intellectual qualifications together, to prevent the science from becoming the mere "handmaid of industry," and ensures that its higher claims shall secure recognition from its votaries. And now I must say a word about the future. Our President has prophesied great things, and is imbued with a just confidence as to the future that awaits us. I believe that there is plenty of room for discovery in the future, and that our forefathers have by no means monopolized the glory that our descendants may win. I rather feel as an outsider—looking at what science is and has achieved—that it is like an Alpine prospect in the early morning, when you see here and there a few peaks bathed in light, but separated from each other by depths and chasms of the unknown. And that is what we all of us feel who look with very little skill or very superficially at the history of science in our own days. It seems evident to me that chemistry is entering upon a new stage, in which it may win splendid victories and learn things of which our forefathers never dreamed. Perhaps it will be best to describe the difference between chemistry as it is now and as it was when I was a young man. In those days the atom reigned supreme; but now the atom has been dethroned, and the bacillus reigns in its stead. But that means that you are approaching, with more and more chance of solving, it the vast problem that separates organic and inorganic nature. Your President has claimed that Nature has no longer the monopoly of creating organic substances. That is true; but Nature does still a great many things that you cannot do. And still less can you tell me the reason of the vast difference between organic and inorganic nature. You are all of you familiar with the tremendous vegetable poisons which produce the most fearful and astounding effects upon the human frame; but if I asked you to explain their effects you would show me formulæ showing that they consisted of the most vulgar and commonplace elements, but giving no explanation of the tremendous powers they assume. I am an agriculturist, and a disciple of Dr. Gilbert and others. I compass sea and land in order to get manure to make our products grow. And what is manure? It is an impure form of the carbon and nitrogen in which those products are bathed in the circumambient air every day of their lives. I trust that the chemistry of the future may tell us why we have to go to Chili, and why we cannot take the nitrogen from the air around us. I believe that these and other problems are now approaching nearer to their solution than ever they were before, because we have seen chemistry grapple more closely with the mysteries which separate organic and inorganic life. I believe that in the future—some fifty years hence it may be—in this or some other room, the President of the Chemical Society of that day will congratulate the Associates of that Society on victories and on achievements of which we cannot now dream the nature. And I am quite sure that when he does so he will attribute no small share of that progress to the existence and labours of the Chemical Society.

Sir Lyon Playfair, in responding, said:—

I quite understand that the reason for selecting me for the honour of acknowledging the toast of "The Chemical Society," is the privilege of old age, and of my having been one of its first members. But I am sure that you will agree with me, that we owe a debt of gratitude to the noble Marquis. He has, as Prime Minister, to bear the weight and responsibility of this great Empire, and it is a proud fact that he has recognized so much the influence and the benefits of chemistry as to honour us by appearing here this evening to propose this toast. If Lord Salisbury had not unfortunately become a great statesman, and had followed the inclination of his own mind, he would have been a great chemist. The education of the upper classes in this country has for a long time been too restricted. Science has not formed that element in education which is so necessary for its progress, and I trust now that the Universities, and the various institutions throughout the country which are doing so much for the advancement of science, will produce great results in the future. But we cannot but regret that the education of the past has not given to us that amount of hereditary talent which our old families possess, and which they have generally given to the benefit of the State. It must be recollected that we have had in the past several instances of descendants of noble families becoming great men of science. We all remember that the famous Boyle was called "the father of modern chemistry, and brother of the Earl of Cork"; and he showed us in his work that we must not trust to authority, but must use acumen as a means of demonstration in all questions brought before us. It was a great delight to me to see, in the exhibition at the Goldsmiths' Hall yesterday, those interesting instruments which Boyle used in his researches. There was another member of a noble family whom we are always glad to claim as a master among chemists: I mean Cavendish, who discovered the composition of water. He did much more than that, however, for he taught us that all experiments should be made with absolute accuracy as regards weight and measure. But what am I to say in answer to this toast? It is a large and important subject. I recollect it fifty years years ago. I am glad to say that not many of you have such an antique recollection of our science as that. The changes that have taken place in the science during that time have been vast indeed. Of course, our main object is to study chemical affinity, to understand the relations of the elements, and the families into which they group. One of the results of fifty years' advance in chemistry is that you have introduced a great deal of profligacy into the elements. When I was young we always taught that oxygen was the universal lover, and joined freely with almost every body, while nitrogen was a confirmed bachelor, and could only be put into union under great difficulty. But now, how completely this is all changed. Oxygen is now a respectable bigamist, while nitrogen, which acts so meekly in the atmosphere, when it gets out of it becomes a terrible polygamist, for it takes three and sometimes even five conjugates at a time, and produces bodies of a remarkable character. I have two friends, one of whom, Hofmann, is not here, but the other, Dr. Perkin, is, and they have done very much to corrupt the morality of the nitrogen of my youth. They have not only taught us what it can do in the way of conjugates, but have shown it to be a most fickle body, from whom you may take one conjugate and readily replace it by another, and thus produce most remarkable compounds. Sometimes they carried their efforts so far that nitrogen became apparently ashamed of itself, and blushed as rosanilin or became scarlet as magenta, and even, when moved by strong emotion, became purple as mauve. Occasionally chemists have tried to get nitrogen back to good habits, to be content with more simple conjugates, and be content with fewer elements in combination. But see how it revenges itself. Curtius and Radenhausen have lately described a most extraordinary compound—azoimide—in which three atoms of nitrogen unite with one atom of hydrogen. This was most unfair, for three atoms of nitrogen ought to have at least nine atoms of hydrogen. But they compelled it to do with one, and what is the consequence? They had to make it take the form of a liquid, and when in that condition it exploded with such violence as to break every glass vessel in the laboratory, and, I am sorry to say, injure one of the persons who tried to force it into this unnatural union. I have therefore some right to complain that the respectable nitrogen of my youth has become a most profligate element under your tuition. And what shall I say of carbon? How different was the carbon of 1841 from the carbon which we now know. At that time we knew, of course, that it was combined in most organic bodies, and Liebig had determined the constitution of the bodies into which it entered, but then we did not require to puzzle ourselves with those fearful complications of diagrams and graphic methods by which we now represent the tenacity of carbon for various substances. These methods are very difficult for the pupil to follow, and I am sure that if Cullen, who invented the system of chemical diagrams, could come to life again, and see the wonderful methods by which chemical combinations are now represented, he would ask to go back to his grave again and rest. Chemical substances now have such astounding properties. If there are two bodies which I thought I knew most thoroughly they are the quiet and respectable compounds called in my old professional days carbonic oxide and carbonic acid. But the respectable quiet carbonic oxide of 1841 was shown the other day by Mond to run away with nickel in the state of a gas—a quiet stable element like nickel. And then when it was followed in hot pursuit, by raising the temperature a few degrees, it dropped the nickel like a hot potato. Well, I am speaking of the changes which strike a man looking backwards, and comparing the chemistry of his day with that of the present time. But though I have been chaffing in an after-dinner speech, do not think that I do not appreciate the vast progress that has been made in the discoveries relating to the valency of the elements. That has been the great distinguishing feature of modern chemistry. There is a great future before the chemistry of this country; and when the centenary of this Society takes place, the members will look back not without respect to the efforts we made in the first fifty years of the Society's existence. In conclusion, I must again thank Lord Salisbury for having honoured us on this occasion in the midst of his great and incessant duties, to show his appreciation of a science in which he has often laboured, and the value and importance of which he has recognized in the excellent speech before us.

THE SCIENCE MUSEUM.

THE question of the Science Museum which has been on the *tapis* for the last eighteen years, has moved —backwards—during the past week. The following question and answer will show how:—

"Sir Henry Roscoe asked the Chancellor of the Exchequer whether it was the case that an unknown donor had offered £80,000 to build an art gallery on a site at South Kensington, the erection of which would materially interfere with the purposes for which land was recently bought by Government for housing the science collections, and for the necessary erection of suitable chemical and physical laboratories in connection with the Royal School of Science; whether another site at South Kensington had been offered which would not interfere with the object for which Parliament granted the purchase-money for the land; and whether the Government would give an assurance that those objects would be maintained.

"The Chancellor of the Exchequer—It is true that a public-spirited gentleman has offered £80,000 to build an art gallery on a site at South Kensington; but with regard to the further points raised by the hon. member, I may say at once that the offer only affects about one-tenth of the land recently bought by Government, and the remainder would still be left available, if required, for science collections. No pledge was given that the whole of the land would be appropriated to science collections. On the contrary, the Treasury, in accepting the offer of the Commissioners of the 1851 Exhibition, stated that the land was in excess of even future requirements of the science collections. It would be possible to make adequate provision for chemical and physical laboratories on the land between the Imperial Institute Road and the Technical Institute. This site adjoins the east galleries, and it is in these galleries, together with the west and southern galleries, and a proposed cross gallery joining the east and west galleries, that the science collections may ultimately be housed. The interests of the Royal School of Science, and of the science collections, are being carefully kept in view, and the hon. member will understand that the acceptance of this generous offer will enable us to provide adequate space for exhibition purposes more rapidly than would have been possible under the old scheme."

It thus appears that the ground which was bought to house collections illustrating science is to be used for some other purpose, since the present collections are to be permanently located in those galleries which, rightly or wrongly, are not considered by "the unknown donor" to be good enough for his pictures.

Further, since the new Art Gallery takes up nearly all the frontage of the Royal College of Science, the extension of that building, instead of being opposite, is to be built about 100 yards away on the opposite side of Exhibition Road, exactly over the projected railway. It is true the railway is postponed for this year, but that is no guarantee that it will not be proposed again.

Mr. Goschen appears to have given in to a caprice of an unknown donor without having any notion of the effect of his action. But if this be so, why does not Mr. Goschen abolish the Royal College of Science and the Science and Art Department altogether? This would be more statesmanlike than omitting to ask the opinions of people who are paid to advise on such matters. Can Mr. Goschen be of the same opinion as another great official who maintained that, even if it were conceded that there should be national collections of physical-science objects, as there are of pictures, books, beasts, birds, and the like, still little space would be required, "because there was no instrument a man of science used which could not be put into a hat?"

Certainly, if the absurd scheme sketched in Mr. Goschen's answer is carried out, the intelligent foreigner will have a good time. He will have to determine whether English statesmanship has succeeded best in putting a physical laboratory over a railway which will prevent nine-tenths of the instruments being used, or in sandwiching a building devoted to art between the two halves of a science school.

NOTES.

THE third session of the Australasian Association for the Advancement of Science was held in Christchurch, New Zealand, and began on January 15, 1891. Sir James Hector presided. The meeting was a successful one, the attendance being about 470, and the number of papers read 74. Prof. Goodale, of Harvard University, represented the American Association, but no member of the British Association attended from England. A revised code of laws was adopted for confirmation at the next session. The evening lectures were: (1) "The Glaciers of the Tasman Valley," by G. E. Mannering; (2) "Oysters and Oyster-culture in Australasia," by W. Saville Kent; and (3) "A Short History of Vocal Music," by G. F. Tendall. Ten Research Committees were appointed to report on different subjects to the next meeting, and a grant of £25 was made towards measuring the rate of motion of the New Zealand glaciers. As great inconvenience is often felt from the want of a special name for the sea between New Zealand and Australia, a recommendation was adopted that the Lords of the Admiralty be requested to name this sea the Tasman Sea. The Committee also recommended the appointment, by the British and American Associations, of a conjoint Committee to define the terms of general importance in biology; and that the Little Barrier Island north of New Zealand, and Resolution Island in Dusky Sound, be set apart as reserves, where the native fauna and flora of New Zealand may be preserved from destruction. The next session will be held at Hobart, Tasmania, with Sir Robert Hamilton, Governor of the Colony, as President.

THE Vatican Observatory, which, according to a circular letter from Father Denza, the Director, "now revives under the protection of His Holiness the Pope Leo XIII.," is bestirring itself to come into closer communication with other scientific establishments, especially by way of the exchange of publications.

THE half-yearly general meeting of the Scottish Meteorological Society was held in Edinburgh yesterday. The Report from the Council of the Society was presented, and the following papers were read: on the winter of 1890–91, by Dr. Buchan; silver thaw at the Ben Nevis Observatory, by R. C. Mossman.

THE Shaen Wing of Bedford College for Women, York Place, was opened by the Empress Frederick on Tuesday. On entering the College, the Empress was received by the visitor, Mr. N. Story Maskelyne, and the Chairman of Council, Dr. W. J. Russell. The ceremony took place in the large lecture-room, where there were present, among many others, Mrs. Shaen, Miss E. A. Shaen, Miss E. Shaen, Sir Henry Roscoe, Sir Lyon Playfair, General Donnelly, and Dr. Gladstone. Her Majesty having ascended the daïs, Mr. Maskelyne read an address, in the course of which he said:—"The literary and the art divisions of the College having been sufficiently provided with the necessary space and appointments, an addition to the buildings of the College for the teaching of science became imperative. This has been achieved, and laboratories for complete instruction in chemistry and the several branches of physics have been built, and in part equipped. This addition has been called the Shaen wing, in order to connect permanently with the College the name of one who was always interested in its welfare, and who for 20 years had served on the Council, being elected Chairman of that body in 1880, an office which he held until his death. The sciences, with their vast influence on the one hand as intellectual triumphs, and on the other as mighty means for the material prosperity of the human race, are, and will be for ever, among the great monuments of the Victorian era. It is an encouragement for all interested in the higher training of women throughout the Empire and that of our kinsfolk in Germany to feel that the heart of the Royal race of England is with them in the effort to make this higher training a heritage of our women as well as of our men." The Empress spent some time in the building after the ceremony was over, and expressed herself as greatly pleased with the College and the work it is doing.

LAST week, at the monthly general meeting of the Zoological Society, it was announced that in recognition of the effective protection accorded for sixty years to the Great Skua (*Stercorarius catarrhactes*) at two of its three British breeding stations—namely, in the island of Unst, by the late Dr. Laurence Edmondston, and other members of the same family, and in the island of Foula, by the late Dr. Scott, of Melby, and his son, Mr. Robert Scott—the silver medal of the Society had been awarded to Mrs. Edmondston, of Buness House, as representative of that family, and to Mr. Robert Scott, of Melby. The medals will be delivered to the medallists or their representatives after the close of the anniversary meeting on April 29 next.

THE following are the lecture arrangements at the Royal Institution after Easter:—Mr. J. Scott Keltie, three lectures on the geography of Africa, with special reference to the exploration, commercial development, and political partition of the continent; Dr. E. E. Klein, three lectures on Bacteria, their nature and functions (the Tyndall Lectures); Mr. William Archer, four lectures on four stages of stage history (the Betterton, the Cibber, the Garrick, and the Kemble periods); Prof. Dewar, six lectures on recent spectroscopic investigations; Dr. A. C. Mackenzie, four lectures on the orchestra considered in connection with the development of the overture; Prof. Silvanus P. Thompson, four lectures on the dynamo; Mr. H. Graham Harris, three lectures on the artificial production of cold; Prof. A. H. Church, three lectures on the scientific study of decorative colour. The Friday evening meetings will be resumed on April 10, when a discourse will be given by Sir William Thomson, on electric and magnetic screening; succeeding discourses will probably be given by Prof. A. W. Rücker, the Rev. Canon Ainger, Mr. J. E. Harting, Prof. W. Ramsay, Prof. G. D. Liveing, Prof. J. A. Ewing, Dr. David Gill, Prof. Harold Dixon, and other gentlemen.

THE fourth annual exhibition of the Photographic Society of Philadelphia is to be opened on May 25. One of the three medals is to be for scientific or technical photography.

AT the request of the Minister of Education, Mr. C. Todd, the Government Astronomer for South Australia, has published the results of his weather forecasts for the year 1890, showing that, out of 305 forecasts issued, 250, or 82 per cent., were verified, and 13 per cent. were partially verified; an amount of success which must be considered highly satisfactory. Mr. Wragge has also sent forecasts from Brisbane with a percentage of success amounting to 55, which, considering that his information cannot be so good as that of the local office in South Australia, may be regarded as a creditable success. But, as pointed out, such a double system of forecasting can only lead to occasional conflicting reports, in addition to which, it is contrary to the principle laid down by the Meteorological Conference held in Melbourne in 1888, that forecasts should be issued for a particular colony only by the local authorities.

THE United States National Museum lately received from Dr. W. L. Abbott a large zoological collection from the vicinity of Mount Kilimanjaro, East Africa. The collection includes about ninety skins of mammals, and an equal number of skulls, representing about thirty-eight species. A description of two of the species, an antelope and a tree-coney, which had apparently not been described before, is given by Mr. F. W. True in the Proceeedings of the United States National Museum, and has been separately printed.

MR. F. W. TRUE was requested in 1886, by Prof. Baird, to investigate and report upon the porpoise fishery carried on at Hatteras, North Carolina. With the industrial aspects of the subject he has dealt in the *Bulletin* of the United States Fish Commission; and now, in the Proceedings of the United States National Museum, he has brought together some interesting notes regarding the habits and structure of porpoises. The species captured at Hatteras is *Tursiops tursio* (Bonnaterre). The fishermen informed Mr. True that the young porpoises remained near their mothers when the latter were entangled in the nets, as sometimes happens. He himself saw this in the case of one female which became entangled near the beach. He did not, however, find the young porpoise among those captured. It probably escaped by diving under the net, as the adult porpoises often do. Mr. True was informed that the mothers helped their young in their efforts to breathe, by bearing them up to the surface of the water on their "flippers," or otherwise. The spiracle, or blow-hole, appears to be a sensitive part of the head. When Mr. True touched it with his hand, the porpoises invariably showed signs of discomfort by lashing the tail violently.

THE Smithsonian Institution has reprinted from the Report of the U.S. National Museum for 1887–88 an excellent account of the coast Indians of Southern Alaska and Northern British Columbia, by Mr. Albert P. Niblack. Among the subjects dealt with in this treatise are regulative organization, mutilations, food, land-works, arts and industries, mortuary customs, feasts, dances, and ceremonies.

M. LIÉVIN COPPIN, director of the Brussels *Économiste*, describes (in an article quoted in the *Board of Trade Journal*) the new commercial museum at Rome. The museum will include a permanent exhibition of national products and of such products of other countries as are capable of being used in Italy. The exhibition also aims at making known in every place in Italy the useful products of foreign countries, and in foreign countries Italian products, in order to bring about a more active exchange in the commercial movement of the country. A bulletin-catalogue of the museum will be exchanged with the similar publications of other countries, and sent to all the national and international exhibitors, as well as to all chambers of commerce and geographical societies; it will be put on board the vessels of the chief navigation companies, &c. A library will be attached to the museum, and this will contain all the commercial journals of the world, and every variety of information concerning customs tariffs, treaties of commerce, &c.

IN his recent presidential address to the Engineers' Club of St. Louis, Mr. F. E. Nipher mentioned that there were in St. Louis about 50 isolated electric lighting plants, having in all about 27,000 lights, and representing about 2700 horse-power. The cost of these plants was on the average 9 dollars per lamp.

A "CATALOGUE of the Library of the Royal Meteorological Society," compiled by Mr. J. S. Harding, Jun., has been published by Mr. E. Stanford. It is complete to September 1, 1890. The work has been prepared with great care, and will be of essential service to students of meteorology. A preface is contributed by Mr. Symons and Dr. Tripe, the Secretaries of the Society. The Council, they say, are glad to show the Fellows how extensive and valuable the library has become, and how worthy it is of the better accommodation which, it is hoped, will shortly be provided for it. The Council trust that those Fellows who possess, or may be able to procure, meteorological works not yet in the library will do what they can towards its augmentation.

THE first edition of Lord Lilford's "Coloured Figures of the Birds of the British Islands," with the exception of a few of the earlier parts, has all been subscribed for. He is therefore making preparations for the issue of a second edition in every respect equal to the first.

MR. R. H. PORTER has nearly ready "The Birds of Sussex," by Mr. William Borrer. The author claims that the volume will contain an account of all the birds now to be found in the county, with mention and careful verification of the occurrences of the rarer species during the last fifty years.

WE have received from Mr. R. H. Porter Part I. of "Aves Hawaiienses: the Birds of the Sandwich Islands," by Scott B. Wilson, assisted by A. H. Evans. The work promises to be one of great value. It is finely illustrated.

MR. H. LING ROTH has just completed a translation of "Crozet's Voyage to Tasmania, New Zealand, the Ladrone Islands, and the Philippines in the years 1771–72." It will be published shortly by Truslove and Shirley.

MESSRS. WHITTAKER AND CO. have published the sixth edition of Sir David Salomon's practical hand-book on "Electric Light Installations, and the Management of Accumulators." Few changes have been made in the text, but a new chapter, containing a considerable amount of fresh matter, has been added. The same publishers have issued the third edition of "Electric Transmission of Energy," by Mr. Gisbert Kapp. The work has been revised and slightly enlarged.

A NEW edition of Mr. V. T. Murché's "Elementary Text-book of Physiology" has been issued by Messrs. Blackie and Son in their series of science text-books. It is intended for classes studying the first stage of the Science and Art course in physiology. A supplement has been added, dealing with those subjects which are included in the curriculum of the Science Syllabus, but are either not discussed at all in the earlier parts of the book, or are not treated there with sufficient fulness.

WE have received from the publishers, Messrs. Percival and Co., both the elementary and advanced stages of their examination sheets on practical plane and solid geometry, by A. Godfrey Day, and edited by E. J. Cox. The sheets are similar in form and style to the Government Science and Art papers. The questions embrace a complete course on the subject in an intelligent and compact manner, those on each sheet treating of different portions of the syllabus. For students who are reading the subject a second time, they will be especially useful. Teachers will also find them a great help, forming as they do an excellent series of examination papers.

Messrs. G. W. Bacon and Co. have published a "New Geological Map of England and Wales." It is "compiled from the best authorities."

The Royal University of Ireland has published the Examination Papers, 1890, as a supplement to the University Calendar for the year 1891.

An important communication upon the colour and absorption spectrum of liquefied oxygen is made by M. Olszewski to the January number of the *Anzeiger der Akademie der Wissenschaften in Krakau*, and a brief abstract is published in the current number of the *Chemiker Zeitung*. Liquid oxygen has hitherto been described as a colourless liquid. In thin layers it certainly appears to be colourless; but M. Olszewski in the course of his investigation of the absorption spectrum, has obtained a sufficient quantity of the liquid to form a layer thirty millimetres thick, and makes the somewhat unexpected and very important discovery that it possesses a bright blue colour resembling that of the sky. Great precautions were taken to ensure the purity of the oxygen employed, the absence of ozone, which in the liquid state possesses a deep blue colour, being especially ascertained. Carbon dioxide, chlorine, and water vapour were also completely eliminated, the oxygen having been left in contact under pressure with solid caustic potash for a week. In view of this fact that oxygen in the liquid state transmits a preponderating quantity of blue light, M. Olszewski's latest experiments upon its absorption spectrum are specially interesting. In a former paper to the *Monatshefte*, an account of which was given in Nature, vol. xxxvi. p. 42, the absorption spectrum of a layer 7 mm. thick was shown to exhibit two strong dark bands, one in the orange, extending from wave-length 634 to wave-length 622, distinguished for its breadth, and one in the yellow, wave-length 581–573, distinguished for its intensity. When the thickness of the layer was increased to 12 millimetres, two further bands appeared, a very faint one in the green, about wave-length 535, and a somewhat stronger one in the blue, extending between wave-lengths 481 and 478. M. Olszewski now finds that his layer 30 millimetres thick, which possesses the blue colour, exhibits a fifth band in the red, corresponding with Fraunhofer's A. This band is rendered still more apparent when a plate of red glass is held between the source of light and the slit of the spectroscope. It is stronger in intensity than the band of wave-length 535, but fainter than the other three bands. This observation of the coincidence of an oxygen band with the telluric band A of the solar spectrum is of considerable interest. For Ångström, in 1864, expressed the opinion that this band A was not due to the aqueous vapour of the atmosphere; and Egoroff and Janssen, who examined the spectrum of long layers of compressed gaseous oxygen, were of opinion that it was due to oxygen. In conclusion, M. Olszewski remarks that the colour exhibited by his 30-millimetre layer is exactly what one would expect from the nature of its absorption spectrum. He also suggests that the blue colour of the sky may be simply due to the atmospheric oxygen, which in gaseous layers of such extent may exhibit the same colour as when compressed into a few centimetres of liquid. Apart from the discussion of this debatable subject, the fact is certainly of interest to chemists, that ordinary oxygen and its condensation allotrope ozone, when compressed into the liquid state, are thus related as regards colour, the former possessing a bright blue and the latter a deep blue tint.

The additions to the Zoological Society's Gardens during the past week include a Common Otter (*Lutra vulgaris*) from Suffolk, presented by Mr. G. C. Edwardes-Ker; a Common Rhea (*Rhea americana*) from South America, presented by Mrs. Hatfield; a Brazilian Caracara (*Polyborus brasiliensis*) from Brazil, presented by Mr. J. D. Spooner; a Green-cheeked Amazon (*Chrysotis viridigenalis*) from Columbia, presented by Miss Julia Crooke; two Leopard Tortoises (*Testudo pardalis*), five Angulated Tortoises (*Chersina angulata*), a Tuberculated Tortoise (*Homopus femoralis*), four Areolated Tortoises (*Homopus areolatus*), a Hygian Snake (*Elaps hygiæ*), four Smooth-clawed Frogs (*Xenopus lævis*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a L'huy's Impeyan Pheasant (*Lophophorus l'huysi* ♂) from Western China, deposited; two White-throated Capuchins (*Cebus hypoleucus* ♂ ♂) from Central America, a Coqueral's Lemur (*Cheirogalus coquerali* ♂) from Madagascar, a Small-clawed Otter (*Lutra leptonyx*) from India, a Collared Peccary (*Dicotyles tajaçu* ♂) from South America, two Griffon Vultures (*Gyps fulvus*), a Ruddy Sheldrake (*Tadorna casarca*), European, six Amherst's Pheasants (*Thaumalea amherstiæ* 6 ♂) from Szechuan, China, purchased.

OUR ASTRONOMICAL COLUMN.

Determination of the Constant of Aberration.—*Comptes rendus* for March 16 contains an abstract of a memoir by MM. Lœwy and Puiseux, on determinations of the aberration constant, in which some of the results obtained by M. Lœwy's method are given. Up to 1828, astronomers accepted, as the constant of aberration, values which were comprised between 20″·255 and 20″·708, these being respectively due to Delambre and Bessel. Richardson then obtained the value 20″·446 from a discussion of 4000 observations made with the Greenwich mural circle by his predecessors. In 1843, W. Struve proposed a value almost identical with this, viz. 20″·445, as the result of a discussion of his careful observations made in the prime vertical. He estimated the probable error as 0″·011, and remarked that he did not think any astronomical element had been determined with so great an accuracy. Struve's work was received with much favour, and appeared to render unnecessary, for a number of years, all researches on the same subject. However, in 1844, Baily deduced 20″·419 as the most probable value, and in after years, Peters, Lundhal, and Lindhagen subjected to a minute discussion all the meridian observations, made at Dorpat and Pulkova, of circumpolar stars. From their researches, a value a little greater than that of Struve was found. Still, when these results were taken in conjunction with the determinations the most worthy of confidence the values 20″·45 and 20″·46 were obtained, thus supporting Struve's work. Nyren, from a discussion of observations made by Struve in the prime vertical as material for the study of nutation, derived the value 20″·43. In 1853, Struve himself proposed to increase his number to 20″·463 with a probable error of 0″·017, but the reasons given to justify the change do not appear to be sufficient. Gylden, Wagner, and Nyren's ulterior observations at Pulkova of circumpolar stars gave the higher value 20″·49. Later, in 1879–82, Nyren made another determination by Struve's method, and used a large number of stars for the investigation. He then found 20″·540 or 20″·517, according to the method of grouping adopted. More recently, in 1885, Küstner, of Berlin Observatory, found 20″·313 by Horrebow and Talcott's method. Between these two last numbers, both of which represent a large amount of work executed with much care, the difference is somewhat greater than 0″·2—that is, about twenty times the probable error estimated by Struve in 1843. This seems to indicate that the astronomers have taken a step backwards. MM. Lœwy and Puiseux do not enumerate the work done on the same subject at Greenwich, the Cape, Washington, and other Observatories, but point out that similar sources of error exist in all the methods employed. Lœwy's method, as is now well known, consists in placing before the object-glass of an equatorial a double plane mirror formed by silvering the sides of a prism of glass. This acts as a sort of compass of strictly constant opening, and brings to the eyes rays which make a constant angle with each other. Pairs of stars separated by a wide angle on the celestial sphere, but which together appear in the field of view, can easily be found, and the variations in relative position due to refraction or aberration can be measured micrometrically with great precision. The adoption of this method leads the authors to the following tentative conclusions:—(1) The number 20″·445, proposed by Struve, is very near to the truth. It would be premature, in our opinion, to wish to modify it. (2) As M. Fizeau supposed, reflected rays behave in the same manner as direct rays from an aberration point of view. (3) The new method for the investigation of aberration may be regarded as proved and definite.

In a future communication the authors will give some details of the method, the observations made on four couples of stars, and the numerical value they find for the aberration constant.

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON Thursday and Friday of last week, the spring meeting of the Institution of Mechanical Engineers was held in the theatre of the Institution of Civil Engineers, by permission of the Council of the latter Society, the President, Mr. Joseph Tomlinson, being in the chair. There were but two items in the programme—namely, the fourth Report of the Research Committee on Friction, and a paper on rock drills, contributed by Messrs. Carbutt and Davey. The meeting suffered a good deal, especially on the second evening, from the fact that the Institution of Naval Architects was in session at the same time. On both evenings very interesting papers on engineering subjects were being read before the latter Society, where the attraction appeared to be greater, for, whilst the Mechanical Engineers meeting was very thinly attended, the Naval Architects had, we hear, an overflowing house on both evenings. It is a pity the secretaries of two Societies having objects so nearly akin, cannot arrange for their meetings not to clash. There is this to be said in favour of the Naval Architects, however, that they were adhering to a time-honoured fixture.

FRICTION OF A PIVOT BEARING.

The Friction Committee's report was taken charge of by Mr. Beauchamp Tower, who was practically the author. The experiments were carried on last year at Simpson and Co.'s engine works, Pimlico, &c. The thanks of the Institution, and of the engineering world at large, are due to this firm for the assistance they have lent, and perhaps the name of Mr. Mair-Rumley should be especially mentioned in this connection.

The pivot bearing operated upon was 3 inches in diameter, and flat ended. The vertical shaft carrying the footstep was geared to a horizontal shaft, which was driven by a belt from the works shafting. Variations of speed were obtained by varying the size of pulley. The bearing was pressed upwards against the footstep by an oil press with a 6-inch diameter plunger. This plunger was made a good but perfectly free fit in its cylinder for a length of 9 inches, a number of grooves being turned in the cylinder throughout its whole length at close intervals. The pressure was applied by means of a small hand-pump, provided with an air-vessel, pumping oil out of a tank into the press. It was found that the leakage of the oil past the plunger, even with the highest pressures, was exceedingly slow, requiring only an occasional stroke of the pump to keep the pressure constant; and at the same time the friction was practically *nil*. Into the top of the plunger was let a piece of hard steel, having a conical depression, wherein rested a hard steel conical centre, which was formed on the bottom of the plate L that carried the bearing. This plate was circular, and had a groove turned in its periphery; a small chain was fastened to the plate and lay in the groove round a portion of the circumference, from whence it led off to a spring-balance attached to the fixed frame of the apparatus; so that the rotation of the plate stretched the spring-balance, and the force tending to turn the plate was thereby indicated. The upper end of the vertical shaft that carried the footstep had a piston fixed on it, which revolved in a cylinder 6 inches diameter. This upper cylinder was connected by a pipe with the cylinder of the lower press, so that, whatever oil-pressure there was in the lower cylinder pressing the bearing upwards, there was the same in the upper cylinder pressing the footstep downwards. This was a convenient way of providing for taking the upward thrust upon the experimental bearing. The footstep having been set running at the desired speed, the hand pump was worked until the pressure gauge on the oil press indicated the desired pressure; and the friction was then read off the spring balance connected with the bearing plate. The load could be quickly removed from the bearing by opening a cock for discharging the oil from the air-vessel of the pump. This method of applying the load was found to be exceedingly convenient. Efficient automatic means of lubrication were provided, which are well worth following, but which we have not space to describe. In the results the coefficient of friction was obtained by dividing the friction in inch-pounds by the product of the load multiplied by the area of the bearing.

The results of the experiments were given in the report by means of a table and in a graphic form. From these we extract the following outline particulars; and must refer our readers to the report itself, which will be published in the Proceedings of the Institution, for fuller details upon this important and interesting subject.

Experiments on the Friction of a Pivot Bearing. Steel Footstep on Manganese Bronze Bearing.

Revolutions per min.	Load: lbs. per sq. in.	Oil drops per min.	Friction. Total.	Friction. Coefficient.
			In. lbs.	
50	20	20	2·77	0·0196
	120	56	18·72	0·0221
128	20	79	1·13	0·0080
	160	84	12·82	0·0113
194	20	196	1·44	0·0102
	160	168	7·69	0·0068
290	20	Continuous stream	2·51	0·0178
	140	,, ,,	4·51	0·0046
	160	200	5·03	0·0044
353	20	Continuous stream	2·36	0·0167
	160	,, ,,	6·15	0·0054

The friction given is that of one face of the flat circular bearing surfaces, at the effective radius of the face, viz. 1 inch.

A white metal bearing surface was next substituted for the manganese bronze. The coefficient of friction was a little larger, but the difference was so small that the results may be looked upon as practically identical.

That the coefficient of friction is less at the higher speeds is doubtless due to the more perfect action of the lubricating device. After the completion of these experiments, the endurance of the manganese bronze and white metal bearings were tested. The former heated and seized at 260 pounds per square inch load on one occasion, and 300 pounds on another, running at 128 revolutions per minute without lubrication. The white metal bearing heated and seized in a load of 240 pounds per square inch at 128 revolutions per minute, without lubrication.

These experiments should be studied with those on the same subject which have preceded them.[1] A short but interesting discussion followed the reading of the paper.

ROCK DRILLS.

The paper on rock drills does not call for an extended notice at our hands. It grew out of some trials made last year at the Crystal Palace, in connection with the Mining Exhibition there held.

One cannot help comparing the carefully thought-out trials last described with those now before us. The only point upon which we can commend those responsible for the present competition is that they awarded no prize. Perhaps one of the most difficult subjects to decide by competition would be the superiority of any one rock drill over its fellows, and the conditions of trial would require careful planning and elaborate preparation. We were not present at the trials, but, to judge by the description, they seem to have been organized by persons having a very elementary knowledge of the conditions under which these machines are called upon to work. One of the judges stated that his qualification to act arose from the fact that he had been in the steam-hammer business, and the

[1] For previous reports see Proceedings of the Institution, 1883, p. 632; 1885, p. 58; and 1888, p. 173.

blow of the rock drill is similar to that of the steam-hammer. *Ex pede Herculem!* It appeared, however, that the makers of the machines framed the conditions of trial, so that, presumably, every one concerned was satisfied.

THE INSTITUTION OF NAVAL ARCHITECTS.

THE annual meeting of the Institution of Naval Architects was held last week, on Wednesday, Thursday, and Friday, at the rooms of the Society of Arts, lent by the latter Society for the purpose. The meeting in question was one of the most successful held for many years; the merit of the papers and the large attendance of members speaking volumes as to the flourishing state of this excellent Society. As there were just a dozen items in the programme, including the President's Address, it will be evident that we can do no more than mention some of the papers read.

The one fault we have had to find in the management of this Institution is that it gives us too many good things at once. It holds but one meeting a year, and that is divided into five sittings. In this way matters that would supply a whole season's programme for many kindred institutions have been crowded into the sole meeting of the year, which has to be rushed through in three days. We have dwelt on this subject before, and know for a fact that our remarks have met with the approval of a considerable number of members. We are glad, therefore, to learn that it is proposed in future to hold two meetings every year. If an effort is made by the Council to improve the quality of the discussions—which can only be done by giving them more time—rather than by adding to the number of papers, the new departure will, we feel sure, be additionally welcome.

The following is a list of the papers read and discussed:—
1. "Future Policy of War-ship Building," by Lord Brassey. 2. "On some recent American War-ship Designs for the American Navy," by J. H. Biles. 3. "On Boiler Deposits," by Prof. Vivian B. Lewes. 4. "Study of Certain Phenomena of Compression," by M. Marchal. 5. "Boiler Construction suitable for withstanding the Strains of Forced Draught," by A. F. Yarrow. 6. "Recent Improvements in Armour for Vessels," by M. Barba, Chief Engineer of Schneider and Co., Creusot. 7. "On the Alteration in form of Steel Vessels due to Different Conditions of Loading," by Thomas Phillips. 8. "The Internal Stresses in Steel Plating," by J. A. Yates. 9. "Certain Details of Marine Engineering," by Thomas Mudd. 10. "On Combined Crank, Crank and Intermediate Shafts, for Marine Engines, and on their liability to Fracture," by C. H. Haswell. 11. "An Assistant Cylinder for Marine Engines," by David Joy. The President, Lord Ravensworth, occupied the chair throughout.

The two great features of the meeting were undoubtedly Mr. Yarrow's paper on boiler construction, and Lord Brassey's contribution on war-ship policy. The respective values attached to these memoirs naturally depended on the walk in life of those appraising them; the Admirals mustering in unusual force to hear Lord Brassey, whilst there was a tremendous gathering of engineers to listen to Mr. Yarrow; indeed, we have seldom seen the theatre in John Street more crowded than it was last Thursday. Each of these papers had an addendum, Lord Brassey's in Mr. Biles's contribution, and Mr. Yarrow's in Mr. Mudd's paper, which gave some very valuable practical additions to our knowledge of the science of boiler construction.

We have used the term "science of boiler construction" advisedly. Last week we should have hesitated to apply it, as being a subject almost non-existent. Steam engineers have woefully neglected the source of their power in time past. The engine has been like a favourite child, no trouble too great to expend upon it; but the boiler has been, figuratively speaking, left out in the cold. Such improvements as have been made in its construction have been due to inventive ability of the ingenious mechanic order. Hardly anyone has thought of treating the boiler philosophically; at least hardly any one before Mr. Yarrow. The boiler has had its revenge. It has been the uncertain factor, and, in marine engineering, the prime source of trouble. We wish we could give all the beautiful experiments by which Mr. Yarrow illustrated the reason of the ills to which boilers are subject when they are pressed to a high rate of duty. Everyone has heard of the difficulties that have arisen in our own and foreign navies from the endeavour to apply forced draught to war-vessels. The curious fact has remained that whilst time after time the larger vessels of the navy came back from abortive trials with boilers leaking at every tube, Mr. Yarrow could run the trials of his torpedo boats, having a high forced draught pressure, with almost unvarying success. The prime reason for all which was made apparent by the paper of Thursday evening last. It may be explained in a few words: Mr. Yarrow has treated his subject in the true spirit of scientific research. He has taken each difficulty as it arose, and investigated it to the bottom, dealing with material he had to use, and the method of construction, upon a basis of scientific reasoning. A good example of this was shown in the manner in which he explained the ovalling of tube plate holes, one of the most fruitful sources of trouble to those who run marine boilers with forced draught. Mr. Yarrow first gathered together all the known facts on the subject. He took the two metals of which tube plates are composed—namely, copper and steel—and tabulated their rates of expansion under various temperatures, and their ratio of conductivity of heat. By the facts so ascertained, and the analogy of a well-known blacksmith's operation—that of reducing the size of a tire by repeated heatings and coolings on one side only—he formulated certain hypotheses, which he proved by experiment to be well founded. His reasoning was clearly set out in his paper, and his experiments were successfully repeated before the meeting. The conclusions involve some interesting problems of molecular physics, and we regret we cannot give the matter the space it deserves; but a satisfactory explanation would involve the reproduction of Mr. Yarrow's diagrams and illustrations of his apparatus. We have dwelt somewhat at length on this paper, partly because it is likely to be of especial interest to our readers, but more especially because it affords a most welcome precedent which we hope many other principals of engineering factories will follow.

Turning to the other papers, we find them all at least of moderate merit, and many of them excellent. Mr. Phillips's contribution on the alteration in form of steel vessels was a praiseworthy effort to put an important branch of ship construction on a more satisfactory basis. From his exceptional position he was able to carry out a series of practical investigations as to the alteration of form of ships under certain conditions of stress, which are so far satisfactory that they go to prove the existing regulations in force on this subject are sufficient. The paper did not pass without criticism, and indeed gave rise to one of the best discussions of the meeting. The paper of Mr. Yates was a more philosophical effort on a cognate subject. A consideration of the internal stresses in steel plating due to water pressure involves some very debatable matter, and the author's mathematics did not pass without criticism. It is characteristic of the time that Mr. Bryan, whose admirable paper on the buckling of a thin steel plate will be remembered, journeyed up from Cambridge purposely to speak on this paper. His mathematical analysis of the subject will form a valuable page in the Transactions.

Prof. Lewes's paper on boiler deposits was eminently practical, and a most welcome addition to a too little studied subject. The Institution and the engineering world in general are fortunate in getting a competent chemist to turn his attention to these matters. M. M. Marchal's paper was taken as read. The paper by M. Barba was somewhat disappointing, and the discussion which followed it was decidedly "shoppy." The two remaining papers which were read, those of Mr. Mudd and Mr. Joy, were of a practical engineering interest; more especially Mr. Mudd's, which was full of instruction for working marine engineers. Mr. Haswell's contribution was not read.

SCIENTIFIC SERIALS.

American Journal of Science, March.—On gold-coloured allotropic silver, by Mr. M. Carey Lea. The present paper is in continuation of one published in this *Journal* in June 1889, and has for its object the description of the reactions of gold-coloured allotropic silver. It is shown that there exists a well characterized form of silver, intermediate between the allotropic silver previously described and ordinary silver, differing in a marked manner from both. All forms of energy act upon allotropic silver, converting it either into ordinary silver or into the intermediate form. Mechanical force (shearing stress) and high tension electricity convert it directly into ordinary silver. Heat and chemical action convert it first into the intermediate form, then into ordinary silver. The action of light is to produce the intermediate form only, and even the most prolonged action at ordinary temperatures does not carry it beyond this. A remarkable parallelism appears to exist between the action of these forms of force on allotropic silver and their action on the silver haloids, indicating that it is not improbable that in these haloids silver may exist in the allotropic condition. Three coloured plates accompanying the paper illustrate the changes described.—The flora of the Great Falls coal-field, Montana, by J. S. Newberry.—High-level shores in the region of the Great Lakes, and their deformation, by J. W. Spencer.—On the composition of pollucite and its occurrence at Hebron, Maine, by G. H. L. Wells.—The volumetric composition of water, by Edward W. Morley. A description is given of the apparatus used to obtain results which will be published in the next number.—On the intensity of sound : a reply to a critic, by Charles K. Wead.—The fire-ball in Raphael's "Madonna di Foligno," by Prof. H. A. Newton. The fire-ball painted by Raphael in his picture, the "Madonna di Foligno," is most probably representative of one that fell at Crema in September 1511. Some political events of importance to Italy and the Pope, which transpired in 1512, were supposed to be connected with the fall of stones that occurred. It appears natural, therefore, that Raphael should introduce the Crema fire-ball into the altar-piece he was painting at the time.

Reale Istituto Lombardo, December 4, 1890.—Observations on the results of mechanical and chemical analyses of some soils in the neighbourhood of Pavia, by Prof. T. Taramelli.—The mean linear coefficient of expansion by heat, between the limits of temperature 0° and $t°$, of a homogeneous and isotropic solid body, is inversely proportional to the difference which exists between the temperature of fusion T and the temperature t, by Prof. A. Sayno.—On the dynamics of storms, by Prof. L. de Marchi.—On the classification of rational transformations of space, and in particular on transformations of the zero class, by Signor G. Loria.—Note on the calcite of some localities in the Grand-Duchy of Baden, by Prof. F. Sansoni.—On a theorem of differential geometry, by Prof. G. Padova.

December 18, 1890.—The coefficient of elastic expansion of a homogeneous and isotropic solid body at a temperature t, between two given limits, is inversely proportional to the difference which exists between the temperature of fusion T and the temperature t, by Prof. A. Sayno.—On some ancient and modern lavas from Stromboli, by Prof. G. Mercalli.—General formulæ for the representation of a field of force by means of elastic tension, by Dr. C. Somigliana.

January 15.—On the theory of the potential function of surfaces, by S. C. G. A. Maggi.—Results of observations made at the Royal Observatory at Brera during the years 1889–90.—On the diurnal variation in magnetic declination, by M. E. G. V. Schiaparelli. It is shown that the magnitude of the diurnal variation in magnetic declination is connected with the amount of spotted surface on the sun as exhibited by Wolf's relative numbers.—On calculations of condensation and on some applications of them, by E. Cesaro.

Notes from the Leyden Museum, vol. xii., No. 3, July 1890, contains :—P. C. T. Snellen, note on *Tyana superba*, Moore.—C. Ritsem, on *Cyriocrates zonator*, Thoms.—Ed. Lefevre, new Coleoptera belonging to the Eumolpidæ.—Dr. W. van Lidth de Jeude, on a large specimen of Orthragoriscus washed ashore at Ameland. The specimen is figured, and attention is called to discrepancies in the published descriptions and figures.—M. Schepman, on *Oliva semmelinki*, n.sp.—J. Büttikofer, zoological researches in Liberia ; birds collected in the district of Grand Cape Mount. *Zosterops demeryi* and *Z. obsoleta* described as new, and eighty-six species enumerated.—W. Roelofs, *Ectatorhinus alatus*, n.sp., described.

No. 4, October 1890, contains :—J. D. Pasteur, on telegraph poles pierced by woodpeckers (*Picus analis*).—Dr. F. A. Jentink, on *Strepsiceros kudu* and *S. imberbis*, rectifies several mistakes made by various authorities about these species.—On two very rare, nearly forgotten, and often misunderstood Mammals from the Malayan Archipelago, *Pithecia melanurus*, S. Muller, the history of which species is quite a romance ; but as facts it may be stated that it lives in Sumatra and West Java, that Duvaucel's drawing (reproduced in F. Cuvier's "Histoire naturelle des Mammifères") represents the animal of its natural size, that it has been accurately and satisfactorily drawn, and, finally, that the animal is a true mouse. Two specimens are in the Leyden Museum. *Tupaja dorsalis*, Schlegel. Dr. Jentink justifies Schlegel in keeping this as a species distinct from *T. tana*, contrary to the opinion he expressed in 1888.—On *Rhinoceros simus*, Burch. The Leyden Museum possesses a fine stuffed adult female, and an unstuffed skin of this species. Quotations are given from modern writings, which suggest that the species is not so rare as Dr. P. L. Sclater seemed to think (NATURE, vol. xlii. p. 520). The question is asked, Is the Quagga extinct? If so, it would be well to take stock of the existing specimens.—Dr. R. Horst, on *Perichæta vordermanni* and *P. sluiteri*, new species from the Island of Billiton.—W. Roelofs, on two new species of Poteriophorus, *P. van de polli*, and *P. sellatus*.—E. Candeze, *Melanoxanthus nigrosignatus*, n.sp., Java.—C. Ritsema, *Coloborhombus auricomus*, Java ; *Thermonotus pasteuri*, Sumatra ; and *Atossa bipartita*, Borneo, all described as new species.—Dr. de Jeude, on some reptiles from Nias.

Vol. xiii., No. 1, January 1891, contains :—Dr. J. G. de Man, carcinological studies in the Leyden Museum (plates i. to iv.). This is No. 5 of a series of studies. Several new species are described, a conspectus of fifteen Indo-Pacific species of the genus Gelasimus is given.—M. Schepman, *Fusus sieboldi*, n.sp., from Japan.—A. B. Meyer, *Cercopithecus wolfi*, n.sp., a beautiful species from Central West Africa, living in the Dresden Zoological Gardens.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 5.—"Some Points in the Structure and Development of Dentine." By J. Howard Mummery. Communicated by C. S. Tomes, F.R.S.

The purpose of the present paper is to show that there are appearances in dentine which suggest that it is formed by a connective tissue calcification, and that the process is more closely analogous to the formation of bone than has usually been supposed.

Processes or bundles of fibres are seen, incorporated on the one side with the dentine, and on the other with the connective tissue stroma of the pulp ; some of the bundles give evidence of partial calcification, reminding one of similar appearances in the calcification of membrane bone. Cells are seen included in the bundles and lying parallel to their course ; these cells, it is concluded, forming with the odontoblasts the formative cells of the dentine, the calcification of which should be looked upon as in part, at least, a secretion rather than a conversion process, the cells secreting a material which calcifies along the lines of and among the connective tissue fibres, the cells themselves not being converted into dentine matrix. These appearances are seen in the rapidly forming dentine of a growing tooth, as well as in more fully developed specimens. An examination of other Mammalian teeth reveals similar appearances. The dentine of the incisor of the rat (*Mus decumanus*) shows with great distinctness the incorporation of the connective tissue fibres with the dentine, and a marked striation of the dentine near the pulp cavity, parallel with these fibres. The ivory of the elephant's tusk shows the same relation of connective tissue to formed dentine. Vaso-dentine exhibits a very well defined connective tissue layer surrounding the pulp. This layer has hitherto been looked upon as consisting of odontoblasts, but this tissue shows no nuclei, and has the characters of a layer of flattened connective tissue fibres—a layer of nucleated cells in close apposition to the dentine, probably being the odontoblasts of vaso-dentine.

Physical Society, March 6.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Mr. James Swinburne read a note on Electrostatic wattmeters. After referring to the history of the electrometer method of measuring power developed by alternating currents, the author pointed out that the necessity for taking two readings from which to determine the watts might be obviated by having the quadrants separated instead of connected in pairs, as in the ordinary method. Non-inductive resistances are connected to the transformer, motor, or other apparatus in which the power is to be measured, so as to be in series with the apparatus, and on opposite sides of it, and the four ends of these two resistances are connected with the four quadrants. Under these circumstances the deflection of the needle is a measure of the watts. To increase the maximum deflection obtainable, so as to make an instrument capable of being read by a pointer, the needle is made in two halves, and fixed one below the other on the same stem, and, instead of quadrants, semicircular boxes are employed. In this way a range of about 130° is obtainable. Prof. Perry inquired as to what kind of *law* the instrument had, and Mr. Blakesley asked whether it was convenient to use. Mr. E. W. Smith pointed out that there was no necessity to take two observations in the ordinary electrometer method, for, by using a false zero, one deflection gave the watts. Further, the use of the false zero rendered it unnecessary to employ any other voltmeter when experimenting at constant pressure. The President said the historical part of the paper was not quite correct, and recalled attention to the fact that, when high pressures were used, a single reading obtained with the ordinary method of connecting up gave the power. For ordinary low voltages, however, the false zero method described by Mr. Smith was very convenient. Mr. Swinburne, in reply to Mr. Smith, said the observation of the false zero really meant another reading. As to the law of his wattmeter, referred to by Prof. Perry and the President, he said he never calculated a law, but calibrated the instruments directly.—Prof. S. P. Thompson now took the chair, and a paper on Interference with alternating currents, by Prof. W. E. Ayrton, F.R.S., and Dr. Sumpner, was read by the latter. The paper relates to the phenomena which occur when alternating electric pressures are impressed on circuits made up of various combinations of resistances, condensers, arcs, and inductive coils; to the characteristics of alternators; to the properties of transformers; and to the peculiarities exhibited by the Ferranti mains. In one of the experiments, an inductive coil and a condenser were connected in series, and a pressure of 25 volts, as measured by a Cardew voltmeter, impressed on its terminals; the pressures on the two parts, measured in the same way, were 110 and 104 volts respectively, thus showing that each of the two parts was much greater than the whole. On joining a condenser and inductive coil in parallel, an ammeter in the main circuit indicated 5·5 amperes, whilst those in the branches showed 6·4 amperes passing through the condenser, and 10 amperes through the coil. Other experiments of a similar nature were described, and it was pointed out that the ratio of the sum of the two parts to the measured total may be large, being about 8 in the case first mentioned. Theoretically this ratio might be anything, depending as it does on the phases of the pressures in the two parts, and these phases are determined by the ratio of the impedance of the coil to its resistance; practically, however, it was not easy to get a coil of large self-induction and very small resistance. Alternate current arcs and condensers gave results of the same general character as those above described, as also did arcs and inductive coils, such as the regulating coils of lamps; this may cause considerable error in estimating the power supplied to such lamps. The magnitude of the error was found to depend greatly on the quality of the carbons and the character of the arc. With bad carbons and a hissing arc the error was very great, but with good cored carbons burning steadily it was not very serious. Combinations of inductive and non-inductive coils exhibit marked peculiarities, particularly if the inductive one be a transformer coil. This last arrangement gave distinct evidence of interference, or difference of phase, when the secondary of the transformer was open, but when closed and with a moderate load, the difference of phase disappeared, thus showing that under these circumstances the primary coil had no appreciable self-induction. On the subject of alternator characteristics, a graphical method of drawing the E.M.F. curve from the terminal curve was described, and the dependence of the terminal curves on the character of the external circuit pointed out. Keeping the speed, exciting current, and armature current constant, the pressure between the terminals was shown to be dependent on whether the external circuit consisted of condensers, resistances, or inductive coils, the pressure being greatest in the former case, and least in the latter. The true E.M.F. of the dynamo, however, was the same in all the cases, but it became less as the armature current increased. From these results the authors conclude that the drop in E.M.F. with large currents is due to reaction on the field, but that the change of terminal-pressure cannot be all attributed to this cause. Transformers, it was shown, are powerful controllers of phase, for the primary and secondary currents are nearly always circulating in opposite senses; the phase-angle for a 1 to 1 Mordey transformer experimented on varying from 170° at no load, to 180° at full load. The relation between the strength of the primary and secondary currents A_p and A_s was found to be a linear one, of the form $\frac{P}{s} A_p = \alpha + \beta A_s$, where P and S are the numbers of turns in the primary and secondary respectively, and α and β constant, α representing the exciting current, and β being nearly unity. The phase-angle, ϕ, between the currents was given by the equation—

$$-\cos\phi = \frac{\frac{1+\beta^2}{2}A_s + \alpha\beta}{\beta A_s + \alpha}.$$

The results of numerous experiments on a transformer of the Mordey type, in which coils having different numbers of turns were put in parallel with each other, were given. In some cases resistances were put in circuit with the coils, and in others one or more of these resistances were cut out. Remarkable interference effects were thus produced, for in some combinations the volts or currents were additive, whilst in others they were nearly differential. In connection with the Ferranti effect, experiments had been made by putting a condenser on the terminals of the secondary of a transformer, and noting the resulting increases in pressure, both in the primary and secondary circuits. The results obtained with a given condenser and approximately constant secondary pressure show:—(1) That whether the transformation be up or down, the percentage rise in the secondary is greater than that in the primary. (2) That these percentage rises diminish as the secondary current increases. (3) That they increase with the ratio of transformation. (4) That the rise in the secondary may be considerable without that in the primary being appreciable. (5) That the rise in the secondary still persists even when large currents are flowing. These facts lead the authors to believe that the "Ferranti effect" is due to some kind of interaction between the cable condenser and the self-induction of the transformer, and that it is not only due to armature reactions in the dynamo. In the discussion on the paper, Mr. Swinburne said the character of the "Ferranti effect" had been wrongly stated, for he understood Prof. Ayrton to say that the pressure between the Ferranti mains was greater at the London end than at Deptford. This, he contended, was impossible. He also said the effects now described, of putting a condenser on the secondary of a transformer, and noticing the rise in both primary and secondary volts, were due to the waste field of the transformer. In large transformers, such as those at Deptford, the magnetic leakage was proportionately much less, and no such effects would be noticeable with them. Referring to the relation between the primary and secondary currents in transformers, he said it was convenient to look at the primary from two points of view, and consider one part of it as producing the magnetization, and the other as neutralizing the effect of the secondary current. Mr. Mordey described an experiment on armature reactions in alternators, in which one of the armature coils of a 50 h.p. machine was isolated from the rest and connected directly to a Cardew voltmeter. On varying the load from 0 to the full output (20 amperes), the voltmeter on the isolated coil was quite stationary, thus showing that no appreciable armature reaction occurred. Mr. E. W. Smith remarked that the formula connecting the primary and secondary currents holds true over a very wide range, for he had experimented on a Kapp transformer which gave α constant and $\beta = 1$ for frequencies varying from 20 to 200. Mr. Blakesley gave the following formula as applying to transformer currents generally,

$$\left(\frac{A_p}{A_s}\right)^2 = \frac{n^2}{m^2}\left\{(1 + kr_2^2) + \frac{qr^2}{v}\right\},$$

where r_2 = resistance of secondary, v is proportional to the frequency, m and n the primary and secondary turns, and k and q are constants depending on the disposition of the iron. This formula he found to very nearly represent Ferrari's results, and he hoped others would test its applicability to various types of transformers. He believed himself to be the first who carried out an interference experiment, when he put a condenser in parallel with an inductive coil six years ago. He mentioned the matter now, because that arrangement had been put forward by Mr. Glazebrook in explanation of the "Ferranti effect." Mr. Glazebrook, however, had made an error in stating that under certain conditions one of the currents may become infinite. Dr. Thompson thought the word interference had been used in a somewhat different sense from its ordinary usage in optics, and asked for an authoritative definition of its meaning in the paper under discussion. The experiments on the alternating arc he considered very remarkable, as well as those on the dynamo, which gives various terminal pressures when its E.M.F. and current were the same, but the external circuit of different character. Prof. Ayrton pointed out that the measurements on the rise of volts by putting on condensers, mentioned in the paper, were not strictly analogous to those made by Mr. Ferranti, for his were made by pilot transformers, one placed between the primary terminals of the Deptford transformer, and the other on the secondary of the London transformer; whereas in the cases now brought forward the volts measured were those at the primary and secondary of the same transformer. On the subject of arc lamps, he said that Messrs. Kolkhorst, Thornton, and Weekes, who made the experiments, found that great lag only occurred when the carbons were bad and the arc hissing. If the apparent and the true power spent in such an arc were measured, a great difference existed between them. Dr. Hopkinson, he said, had shown that this might be expected, if the arc had a constant back E.M.F. which changed sign with the direction of the current.—A paper on the theory of dissociation into ions and its consequences, by Prof. S. U. Pickering, F.R.S., and another on some points in electrolysis, by Mr. J. Swinburne, were postponed.

Entomological Society, March 4.—The Right Hon. Lord Walsingham, F.R.S., Vice-President, in the chair.—Mr. F. P. Pascoe exhibited, and made remarks on, a curious Coleopterous larva, with a case somewhat resembling that of the Lepidopterous genus *Psyche*, which was found at the Theatre of Bacchus, Athens.—Mr. J. W. Douglas sent for exhibition specimens of *Icerya ægyptiaca*, which, through the kindness of Mr. A. D. Michael, he had received from Alexandria on January 19 last. It was stated that in travelling most of them had become loose, and had lost their waxen appendages; but a few still remained on the stems of their food-plant. In connection with this subject Mr. G. H. Verrall alluded to a Dipterous parasite of *Icerya* from Adelaide—*Lestophonous iceryæ*, Williston—which had been bred from *Icerya purchasi*, last February.—Mr. R. Adkin exhibited a long and interesting series of *Triphæna comes* from various parts of the south of England, Yorkshire, Forres, the Isle of Man, the Isle of Lewis, and the north of Ireland.—Mr. G. F. Hampson exhibited a series of varieties of *Plotheia frontalis*, Walk., which was the only species in the genus, and confined to Ceylon. He said that the varied forms of this species had been described under twenty-one different names by Walker, Felder, and Moore.—Mr. F. Merrifield showed a number of specimens of *Selenia illustraria*, of three different stocks, proving that the spring brood of this species, which passed the winter in the pupal stage, was, like the summer pupa, materially affected in colouring by the temperature to which the pupa had been exposed in its later stages. He thought this fact, coupled with similar results ascertained with respect to the single-brooded *Ennomos autumnaria*, indicated that the operating cause was one of wide general application. Captain Elwes said that in his experience in many parts of the Palæarctic region, where there was a combination of heat and moisture, all the commoner species of Lepidoptera occurring in this country attained a larger size and a greater brilliancy of colouring than in colder and drier regions; and he referred to such species, amongst others, as *Pieris brassicæ* and *Argynnis paphia*. The discussion was continued by Mr. Jacoby, Mr. Fenn, and others.—Mr. W. H. B. Fletcher exhibited a long series of *Zygæna lonicerœ* from York, and *Zygæna filipendulæ* from Shoreham, Sussex; also a series of hybrids obtained by crossing these two species. He stated that the eggs obtained from these hybrids were all infertile. Lord Walsingham said this latter fact was extremely interesting.—Mr. F. W. Frohawk exhibited a living specimen of an ichneumon which had just emerged from a chrysalis of *Papilio taunus*.—Mr. C. J. Gahan exhibited a number of species belonging to the genera *Lema* and *Diabrotica*, and read a paper on them, entitled "On mimetic resemblances between species of the Coleopterous genera *Lema* and *Diabrotica*." Lord Walsingham, Mr. Jacoby, Colonel Swinhoe, and Mr. Champion took part in the discussion which ensued.

Chemical Society, March 5.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Crystalline form of the calcium salt of optically active glyceric acid, by A. E. Tutton. This paper presents the results of a complete crystallographical investigation of the calcium salt, $Ca(C_3H_5O_4)_2$, $2H_2O$, of the optically active (dextro-rotatory) form of glyceric acid described by Frankland and Frew. The crystals are monoclinic, and are hemihedral. The ratio of the axis is $a : b : c = 1{\cdot}4469 : 1 : 0{\cdot}6694$.—Fermentations induced by the *Pneumococcus* of Friedländer, by P. F. Frankland, A. Stanley, and W. Frew. Brieger has pointed out that this micro-organism is capable of fermenting suitable solutions of glucose and cane-sugar. The authors have confirmed Brieger's observations, and have found that the organism also ferments maltose, milk-sugar, raffinose, dextrin, and mannitol, but not dulcitol. The fermentations of glucose and mannitol were specially studied. The products are, in each case, ethyl alcohol, acetic acid, generally a little formic acid, and a trace of succinic acid, carbon dioxide, and hydrogen. The glucose is less readily attacked than mannitol and cane-sugar, and both are only partially fermented. Quantitative results of the products of fermentation are given. For mannitol, the several products are in very close accord with the molecular proportions—$9C_2H_6O : 4C_2H_4O_2 : 12CO_2 : 8H_2$.—The volumetric estimation of tellurium, Part 2, by B. Brauner. When a solution of tellurium dioxide in sulphuric acid is titrated with permanganate, the following interchange occurs:—

$$2KMnO_4 + 4H_2SO_4 + 4TeO_2 = K_2SO_4 + Mn_2(SO_4)_3 + 4H_2O + 4TeO_3.$$

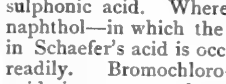
The manganic salt is destroyed with decinormal ferrous sulphate or oxalic acid, and then permanganate is added in slight excess. The quantity of tellurium dioxide is calculated from the volume of permanganate used, *minus* that corresponding to the amount of ferrous salt or oxalic acid added, as if the following action had taken place:—

$$2KMnO_4 + 3H_2SO_4 + 5TeO_2 = K_2SO_4 + 2MnSO_4 + 3H_2O + 5TeO_3.$$

The result is greater by 1 per cent. than that calculated, owing to the evolution of some oxygen. In alkaline solution, the change which takes place is

$$2KMnO_4 + 3TeO_2 = K_2O + 2MnO_2 + 3TeO_3.$$

Excess of sulphuric acid is added, and oxalic acid used for re-titration; the results are 0·35 per cent. too high.—Chloro- and bromo-derivatives of naphthol and naphthylamine, by H. E. Armstrong and E. C. Rossiter. The preparation of 1 : 4-dichloro-β-naphthol and 1 : 4 : 4′-trichloro-β-naphthol is described. The latter melts at 157°–158°; its acetyl derivative at 129°. 1-chloro-, 1 : 3- and 1 : 4-dichloro-, and 1 : 3 : 4- and 1 : 4 : 4′-trichloro-β-naphthol are all sulphonated with great ease, and yield derivatives of 1 : 3′- (Schaefer's) β-naphthol-sulphonic acid. Whereas 3′ : 1-dibromo- and bromochloro-β-naphthol—in which the position occupied by the sulpho-group in Schaefer's acid is occupied by bromine—are sulphonated less readily. Bromochloro- and dibromo-β-naphthol yield, on oxidation, 1 : 3 : 4-bromophthalic acid; and the bromochloro-β-naphthol, when distilled with PCl_5, gives 1 : 2 : 3′-trichloronaphthalene as one of its products. The authors find that, contrary to Smith's statement, the preparation of tetrabromonapththol is attended with considerable difficulty. The product of the action of excess of bromine on β-naphthol consists chiefly of tribromonaphthol. Nearly all the chloro- and bromo-derivatives of β-naphthol yield quinone derivatives when submitted to the action of nitric acid. The final product of the action of nitric acid is a phthalic acid. Alkaline permanganate gives a series of oxidation products: for example, the main product of its action on dibromo-β-naphthol is the acid,



, which, on distillation, yields bromo-phthalide.

Linnean Society, March 5.—Prof. Stewart, President, in the chair.—Mr. D. Morris exhibited a dwarf species of *Thrinax* which he found growing plentifully in the Island of Anguilla, West Indies, and which was apparently undescribed.—Mr. T. Christy exhibited the fruit of some undetermined species of plant which had been introduced into commerce by the name of Monchona, but the origin of which had not been ascertained.—Mr. J. E. Harting exhibited several instantaneous photographs (taken by Mr. W. H. St. Quintin in Yorkshire) of a living Great Bustard (*Otis tarda*), and gave a brief account of the recent visitation of several of these birds to England. Between December 9 and February 5 no fewer than seven had been shot, in Norfolk, Suffolk, Essex, Sussex, Hants, Wilts, and Carmarthenshire.—On behalf of Miss E. Barton, Dr. D. H. Scott gave the substance of a paper communicated by her, and entitled a morphological and systematic account of the fucaceous genus *Turbinaria.*—Mr. George Murray described some new species of *Caulerpa*, with observations on the position of the genus. In elucidation of this paper Mr. E. M. Holmes exhibited a large series of specimens, showing the extreme variability of the species of seaweeds which had been referred to this genus.—A paper was then read by Dr. John Lowe, on the specific identity of two forms of parasitic Crustacea, *Lerneonema spratta*, Sowerby, and *L. eucrasicholi*, Turton, the only two of the genus which had been hitherto recognized in Britain. A third species had been described by Dr. Salter (*Ann. Nat. Hist.*, 1850, p. 56), from the eye of the herring, and to this he gave the name of *L. Bairdii*, but his figures show clearly that they were drawn from imperfect specimens of *L. spratta*, which had been forcibly removed from the fish's eye, leaving the head behind. The parasites in question had only been found on the sprat, herring, and anchovy.

Royal Meteorological Society, March 18.—Dr. C. T. Williams, Vice-President, in the chair.—Mr. G. J. Symons, F.R.S., read a paper on the history of rain-gauges. It appears that Sir Christopher Wren, in 1663, designed not only the first rain-gauge, but also the first recording gauge, although the instrument was not constructed till 1670. The earliest known records of rainfall were made at the following places: Paris, 1668: Townley, in Lancashire, 1677; Zürich, 1708; and Londonderry, 1711. Mr. Symons gave a very full account of the various patterns of rain-gauges, and in most instances pointed out the merits or defects of each.—Mr. A. W. Clayden showed, on the screen, a number of interesting transparencies of photographs of clouds, lightning-flashes, and other meteorological phenomena.—The meeting was adjourned at 8.30 in order to allow the Fellows to inspect the exhibition of rain-gauges, evaporation-gauges, and new instruments, which had been arranged in the rooms of the Institution of Civil Engineers.

Paris.

Academy of Sciences, March 16.—M. Duchartre in the chair.—Determination of the constant of aberration, by MM. Lœwy and Puiseux. (See Our Astronomical Column.)—On the equilibrium of fluid dielectrics in an electric field, by M. H. Poincaré.—On the different manifestations of phosphorescence of minerals under the influence of light and heat, by M. Henri Becquerel. The phosphoroscope contrived by the father of the author of this paper for the investigation of phosphorescence is well known. M. H. Becquerel has placed various specimens and varieties of fluor-spar in the phosphoroscope, and has observed the spectra they emit when illuminated by the electric spark or subjected to heat. A number of luminous bands are seen under either condition, and the wave-lengths of these have been determined. Chlorophane was one of the most interesting bodies examined. In rotating the disks of the phosphoroscope with increasing velocity, this substance emitted light of different tints, which tints corresponded to the appearance of bands of different refrangibilities in the emission spectrum. For a very slow movement of the disks the spectrum extended from about $\lambda 543$ to $\lambda 478$ with a maximum from about $\lambda 531$ to $\lambda 497$. On more rapid rotation, bands appeared at wave-lengths 557, 592, 606, and 492-478; afterwards, the velocity increasing, a band appeared at $\lambda 542$, and this became more brilliant than any of the others, whilst the maximum at 531-497 was replaced by a band at 492-478. Similar variations were observed when chlorophane and other varieties of fluor-spar were subjected to changes of temperature. One of the conclusions deduced from the results obtained is that the different spectra given by the same body are due to the presence in the body of different substances which form definite compounds under certain conditions of illumination and temperature.—On a new method for the determination of critical temperatures and pressures, and in particular, the critical temperatures and pressures of water, by MM. L. Cailletet and E. Colardeau. The determination of the critical temperature of water is extremely difficult by ordinary methods, because the tubes of glass burst under the enormous pressures required. The authors have devised a method which permits the experiment to be made without observation of the disappearance of the terminal surface of the liquid. The method not only gives the critical temperature but also the critical pressure, and the curve of tensions of the saturated vapour of the liquid up to the critical point. The manometer used for the determinations of the high pressures necessary registers up to 400 atmospheres, and is installed in the Eiffel Tower. No results are as yet given.—On the fossils found at Gourbesville by M. de Lapparent, by M. Albert Gaudry. As a complementary note to a previous one, M. Gaudry states that he has discovered the teeth of *Palæotherium magnum*, *Mastodon angustidens*, *Carcharodon*, and others, in the Gourbesville conglomerate.—Effect of cold on marine fishes, by M. A. F. Marion. The author gives some observations made at Provence on the resistance of certain species of fish to severe cold.—On the application of M. Lie's groups, by M. L. Autonne.—Graphical method for the determination of the relative values of the force of gravity in different places, by M. Alphonse Berget. One of the arrangements proposed consists of a pendulum carrying a small lens which concentrates the light from a lamp upon a movable sensitized film. A continuous, sinuous curve is thus obtained; which may be compared with that given by a standard pendulum at Paris or some other centre.—On the degree of complexity of gaseous molecules, by M. Marcel Brillouin.—On the transformations which accompany the carburation of iron by diamond, by M. F. Osmond. Some experiments which are described show—(1) that diamond itself is not cemented by iron, but first undergoes, from contact with this metal, a molecular transformation which renders it capable of cementation; (2) that the diffusion of carbon in iron has for a corollary a diffusion of the iron in the transformed diamond.—On the formation of coloured lacs, by M. Léo Vignon.—Researches on the dispersion in organic compounds (ethers), by MM. P. Barbier and L. Roux.—On the ptomaines, by M. Œchsner de Coninck.—Influence exercised by the extractive matters on the alcohol in spirits, by M. Ch. Blarez.—On the toxicity of the soluble products of tuberculous cultures, by MM. J. Héricourt and Charles Richet. A healthy rabbit was not affected by an injection of two grammes of the lymph, but died when the dose was increased to three grammes. A rabbit suffering from tubercle, but otherwise healthy, was killed in forty-eight hours with a dose of about the eighth part of the latter. It would therefore appear that much care should be taken in administering Dr. Koch's lymph to patients.

CONTENTS.

THURSDAY, APRIL 2, 1891.

EXTERMINATION.

EXTERMINATION is a process which has attracted far less attention from naturalists than it deserves. Signifying literally a driving out of bounds, it may in some cases indicate that in certain parts of the range of a species, whether plant or animal, that species may have ceased to exist, however abundant it may remain elsewhere; while in other cases, especially if the species have but a limited distribution, it easily becomes equivalent to extirpation. The older school of zoologists seems hardly to have contemplated the possibility of a whole species becoming extinct within the period since man appeared upon earth, or to have supposed that a species could by human efforts be utterly swept away. We have but to look back into the days before Duncan and Broderip and Strickland wrote upon the Dodo to learn that this was so. There were zoologists of established reputation who doubted whether such a creature had lived, because they could not find that it still existed; while there were others of no inferior rank who were prepared to believe in the reality of its scanty remains, but only because, in the profound ignorance of the facts of the geographical distribution of animals that then prevailed, they were always expecting that it would turn up again as the inhabitant of some hitherto undiscovered country. Neither class was prepared to admit that the Dodo, or indeed any other species of animal, had flourished within a comparatively few years, and had somehow or other since become extinct through the direct or indirect agency of man.

We now know better; but far be it from us to blame our predecessors for not knowing what they could not possibly have known. Indeed there was much to be said in support of the view they took. They knew how that by many nations war had been declared for centuries against this, that, or the other animal, which notwithstanding maintained its existence. There was the notorious case of the Wolf. The *caput lupinum* had passed into a proverb in every European language, and yet there was scarcely a country on the Continent in which Wolves did not defy the price set on their heads, and persistently carry on their retaliations, ravaging flocks and herds, and even at times mangling or destroying men, women, and children. This is still true. Though extirpated in the United Kingdom, the Wolf is yet found throughout Europe, except in the Netherlands, an artificial country—

> won from the raging deep
> By diligence amazing,

and liable to be overwhelmed by its waves on any intermission of watchfulness—and Denmark, consisting of a peninsula and several islands, in which effectual methods of destruction could be easily employed. That these methods were rather indirect than direct is pretty plain from what we know of its extirpation in Great Britain and Ireland. It was effected much more by the gradual curtailment of and inroads upon the forests which grew on so much of our soil, than by the doing to death of this or that individual of the species who made its presence known. Direct persecution hardly reached the robbers of the folds.

> With their long gallop, which can tire
> The hound's deep hate, and hunter's fire,

the power they possessed of covering vast distances, so that the marauder of the night was far distant from the scene of its depredations before they were discovered in the morning, was enough to baffle successful pursuit. When its haunts were invaded by the axe and the plough, it was easily traced to its lair, and the use of firearms soon completed the business; but so long as it could lie under the greenwood tree, it could safely snarl in scorn of Acts of Parliament passed for its extirpation.

In nearly the same tones could we write of the Birds-of-prey, and those which are detrimental to crops of grain or fruit. For centuries past war has been almost incessantly waged against Kites and Crows, and *hoc genus omne*. In this country the *Accipitres* have at last pretty well succumbed to persecution, carrying the harmless Owls in their train; but it has taken years to bring about this almost entire destruction, and we may safely say that without the system which places almost every wood under the guardianship of one or more gentlemen in velveteen, just as every shrubbery is under that of a gardener, this extermination would not have been effected. The Crow-kind would have undergone the same fate—indeed, the king of their race has practically met it—but for a divided opinion among farmers as to the agricultural utility of some of the species, aided by the delusion which in some quarters obtains that Rook-shooting (notwithstanding the example set by Mr. Winkle) is a sportsmanlike recreation, and therefore that Rooks should be reserved for slaughter in a peculiar way. The effective cunning of the two lower forms of *Corvidæ*, though not proof against the temptation of a poisoned egg, just enables them to maintain a position as denizens.

Conversely, human efforts often fail signally when they try to continue the existence of a species. Confining ourselves for the present to our own island, there is an ancient Act of Parliament (25 Hen. VIII., cap. 11) well known, and still, we believe, in force in England and Wales, prohibiting, under what was then a heavy penalty, the taking of the eggs of certain Birds, of which six kinds are expressly named. Despite this protecting law, four of these six have long since ceased to breed in this country, and the reason is plain. They have been exterminated by the agricultural improvements that have changed the face, nay, the substance of the land in so many parts—that is to say, they have been exterminated by human means, but means acting indirectly, and without intention of hurting them. To the category of these might easily be added many others that have undergone the same fate, if it were now our object to give a complete account of the effects of extermination in the British Islands, for it ranges from the Bear to a Butterfly; while if we turn to other parts of the world we immediately find such a crowd of examples as would take a volume to enumerate. Nobody in New Zealand wished to destroy its Quail, which was in some places abundant enough to afford a good day's shooting. Yet it is gone—perished by fires which were lighted for other purposes. In many British colonies the baleful effects of what some thirty years ago was highly lauded as "Acclimatization" by half-informed persons calling themselves naturalists have been felt with fearful force. Nearly all the most interesting animals of New Zealand and Australia will disappear before the ravages of the *Mustelidæ*, so foolishly if not wickedly introduced to check the great but obviously transient pest of a superabundant population of Rabbits—themselves an inconsiderate importation. The Mongoose has been set free in Jamaica to kill the Rats that spoil the sugar-canes. Its effect on the Rats is inappreciable. It prefers another kind of country and a different food. The result is the destruction of every Turkey-Buzzard's egg it can find; the Turkey-Buzzard, commonly known as "John Crow"—a species cherished and protected by colonial law for its cathartic character—being a bird that makes its nest upon the ground, and ignorant of the danger it thereby incurs from the interloper. To the Mongoose is also laid the charge of extirpating the peculiar Petrel of the island, *Œstrelata jamaicensis*, which breeds in holes on the mountain sides, and proof of the crime seems likely enough after what Colonel Feilden has told us[1] of the extinction of its congener, *Œ. hæsitata*, the Diablotin in Dominica by a species of Opossum, imported with no evil intention, so far as is known, into that island.

It may surprise some persons who imagine that, because Sea-fowls can and do roam almost illimitably they are, therefore, the safer from danger. The fate of the Diablotin is proof to the contrary. No birds possess the "homing" influence more strongly than Petrels, and of many species the home is a narrow one. The Diablotin has been known to wander to Long-Island Sound, the English Channel, the county of Norfolk, and even to Hungary, but its only home was in the Islands of Dominica and Guadeloupe, and in each it has apparently become extinct. In the early days of the colonization of the Bermudas, another species of Petrel, then and there known as the Cahow, which was presumably the *Puffinus auduboni*, was so abundant, upon at least some of the islands, that many of the settlers being at the time (between 1612 and 1615) suffering from famine and sickness, they were set ashore there by the Governor that they might feed on these birds. They did so, and nearly extirpated the species, so that a few years later (1616–1619) a succeeding Governor issued "a Proclamation against the spoile of Cahowes, but it came too late, for they were most destroyed before."[2] They never recovered their numbers, and when in 1849 Sir John Orde landed on the only island where the Cahow was reported still to exist, he found it represented by but two adults, a down-clad nestling, and an egg![3] Whether the species yet maintains itself there is unknown.

We have been led to make these remarks in consequence of the award, announced at the last general meeting of the Zoological Society, as already stated in our columns, of two of its silver medals to as many recipients, in recognition of the long-continued and successful efforts made by members of their respective families for more than one generation to protect from extermination one of the most remarkable species of our native birds. This species, the Great Skua, seems never to have had more than three breeding-stations in the British Islands, and all of them situated in Shetland where it is known familiarly as the "Bonxie." On one of these stations, Roeness Hill (commonly called Rona's Hill), in Mainland, it is said to have been exterminated some fifteen or twenty years ago. On each of the others it has been preserved—in the case of Foula, the most westerly of the group, and in that of Unst, the most northerly—by the exertions of the late Dr. Robert Scott and of the late Dr. Laurence Edmondston respectively. Though it has pleased some ornithological writers to give this bird the name of the "Common" Skua, the fact only shows how little they knew about it and its rarity. Widely ranging at other times of the year, in the breeding season it has but three resorts in the North Atlantic, and, though represented by other species in other parts of the globe, this is strictly a North Atlantic bird. The first of these resorts is in the Shetlands, as just stated; the second is in the Faeroes, where it was once fairly numerous, but latterly has been "minished and brought low"—since in 1872 a good observer computed the number upon all the islands in the group to be about thirty pairs; the third is in Iceland, where it seems to breed sporadically but not numerously along the southern shore, but is more abundant on the Westman Islands. Its existence, therefore, is exposed to much hazard, and if it is to survive in Shetland it will have to be protected. The protection afforded by the gentlemen above-named has saved it hitherto, and the recognition of their service to science thereby will be hailed, we are sure, by all as a graceful act on the part of the Zoological Society. True it is that the recognition is posthumous, and that the reward goes to the successors of the real benefactors, but the good effect of their example should not on that account be the less. Medals are struck and bestowed for preservation by posterity, and we trust that a long line of each of the families of Scott and Edmondston will look back with gratification on these memorials of good action on the part of their predecessors. We also congratulate the Zoological Society, the foremost of its kind in the world, on its discrimination as being the first to reward a service of this kind, and thereby to recognize the importance of attempting to ward off the dire effects of the process of extermination.

PSYCHOLOGY IN AMERICA.

Outlines of Physiological Psychology: A Text-book of Mental Science for Academies and Colleges. By George Trumbull Ladd, Professor of Philosophy in Yale University. (London: Longmans, 1891.)

The Principles of Psychology. By William James, Professor of Psychology in Harvard University. 2 Vols. (London: Macmillan, 1890.)

WHILE the "Outlines of Physiological Psychology" is not a mere abridgment of Prof. Ladd's larger work, entitled "Elements of Physiological Psychology," it is cast on the same general lines, and has a similar object in view. At the time of its appearance, we noticed at some length (NATURE, vol. xxxvi. p. 290) the longer and more elaborate treatise. This relieves us of any

[1] Transactions of the Norfolk and Norwich Naturalists' Society, vol. v. p. 24.
[2] See Lefroy's "Memorials of the Discovery and Early Settlement of the Bermudas or Somers Islands," vol. i. pp. 76, 137.
[3] Jones's "The Naturalist in Bermuda," p. 96.

necessity for saying more than a few words of commendation and criticism of the later and shorter work. In it will be found a careful and accurate description of the results which have been reached through the experimental and physiological study of those neural processes which are regarded as the correlates of mental phenomena. What is known of the neural processes, and what is inferable concerning their psychical concomitants, are clearly and fairly presented; and concerning this part of Prof. Ladd's work we have naught to offer but praise of the impartial consideration of the facts and the judicial presentation of the results. But there are higher and more abstract regions of psychology, where exact correlation of mental processes with neural processes is at present impossible. From the point of view of physiological psychology these should either be left alone, or indicated as regions in which the nature of the correlation, if such exist, is at present unknown. Careful and judicious speculation in this region, if duly presented as such, is fairly admissible. But dogmatic assertions, positive or negative, are an occasion of stumbling to the student, and a serious dereliction of duty on the part of the teacher. When Prof. Ladd says that "a thorough analysis of mental life discloses other forms of activity than those properly called sensory-motor," he is well within his province; but when he proceeds to lay it down "that these forms not only do not find their full explanation in the changing state of the brain, but are not even conceivable as correlated with such states," he is building too confidently on his ignorance. It is noteworthy that, where our ignorance concerns a conclusion which he rejects, the conclusion is said to be "inconceivable"; but where it concerns a conclusion which he accepts (as in the case of the causal connection between the mind and the body as distinct entities), it is called a "mystery," and we are gravely admonished that "because of its mystery it is no less to be acknowledged as a fact." What is here spoken of as a "fact" is one solution out of several of one of the most difficult philosophical problems. Just where the scientific spirit, as opposed to that which is the outcome of other considerations, demands increased caution and modesty, since the grounds of perceptual verification are far distant, and since investigators of admitted knowledge, insight, and fairmindedness have reached other conclusions, Prof. Ladd assumes a dogmatic tone, which seems to us a serious error in an otherwise excellent work.

In his "Principles of Psychology," Prof. James presents us with a work of unusual ability and power. His sturdy individuality, his keen insight, his wide range of learning and culture render him especially fitted for the task which he has undertaken. His pages abound in pithy epigrams, and observations which indicate that the author is a keen student of the individualities as well as the generalities of human thought and conduct. His style is at once lucid and flowing. Not a little of the matter which he has embodied in his pages has seen the light before in periodical publications; and this it is, perhaps, that has led to one of the defects we find in his work. It is too long. It many parts it would be rendered better and more effective by wise and judicious condensation. After reading a hundred and fifty pages on the perception of space, or a hundred and ten on the consciousness of self, one wishes that the author had seen fit to give us the essence of his thought in one-third of the space. Some of the longer quotations might go, or be considerably curtailed. In view of future editions, we would counsel Prof. James to make experiments on the following lines: to read ten pages over night, sleep over them and condense them to five on the following morning. We believe his work will thus gain in power what it loses in bulk.

The method of arrangement of the subject-matter is somewhat peculiar and not altogether satisfactory. Perception, with which so much of our daily life is concerned should have an earlier place; and the chapters on the stream of thought and the consciousness of self, with those on the automaton theory and the mind-stuff theory might well have been deferred till towards the close of the treatise. This is, however, a matter of no great importance, since the work is presumably not intended for those who come fresh to the subject of psychology with no previous acquaintance with its methods and results; and every author of an advanced text-book has a perfect right to develop the subject as seemeth to him best.

It is obviously impossible in the space at our command to attempt anything like an analysis or detailed criticism of a work of well-nigh 1400 pages of closely-printed matter. Easy and pleasant would it be to dwell on the many arguments and passages which seem to us wholly satisfactory—which are, that is to say, in conformity with our own views. We think it better, however, to indicate some of the points in which we find Prof. James's treatment less satisfactory.

The description and definition of "perception" is on the whole excellent. It is clearly seen and enunciated that while part of what we perceive comes through the senses from the object before us, another part, and it may be the larger part, always comes, in Lazarus's phrase, out of our own head. The fact, also, that a pure sensation is an abstraction—that it answers to the presentative as isolated from the representative elements which the analysis of percepts discloses—is shown and insisted on. So far good. But the distinction thus satisfactorily laid down is not steadily adhered to. An hallucination is called "as good and true a *sensation* as if there were a real object." In the chapter on the perception of space we read in one place: "When we speak of the relation of direction of two points toward each other, we mean simply the *sensation* of the line that joins the two points together;" and again, "rightness and leftness, upness and downness, are pure *sensations*." But further on we find it stated that "for the distance between the 'here' and the 'there' to be felt, the entire intervening space must be itself an object of *perception*." Again, in the chapter on sensation we find it written: "And though our *sensations* cannot so analyze and talk of themselves, yet at their very first appearance, quite as much as at any later date, are they cognizant of all those qualities which we end by extracting and conceiving under the names 'objectivity,' 'exteriority,' and 'extent.'"

Now we venture to think that there is in all this a good deal of confusion, if not of thought at least of statement; though we must bear in mind Prof. James's view that "the words 'sensation' and 'perception' do not carry very definitely discriminated meanings in popular speech, and in Psychology also their meanings run into each other." A line, as such, is not an object of abstract sensation but of perception. Rightness and leftness are concepts; and the practical right and left or up and down which we observe in daily experience are the outcome of the perceptual process, are part of the perceived relations between sensations, and are not themselves sensations. This Prof. James himself practically admits when he tells us that "the fuller of relations an object is, the more it is something classed, *located*, measured, compared, assigned to a function, &c., the more unreservedly do we call the state of mind a perception." The title of the chapter on "The Perception of Space" is a misnomer. Space is a concept, not a percept: what we perceive is extension and distance, or rather that objects are extended and distant, out of which by abstraction thought reaches the conception of space. The same is true of time: what we perceive is duration, or rather that phenomena are enduring, and from this our thought rises to the conception of time. His confusion of terms makes Prof. James's discussion of space (in which he strives to establish an original *sensation of space*), and those parts of his chapter on sensation, in which he runs a tilt against the doctrine of "eccentric projection," rather difficult reading. With regard to the former question his contention, unless we misunderstand its essence, seems to come to this—that in our given perception of external things there are special relations of extent, direction, and distance, which are *sui generis*, and behind or beyond which we cannot get in the process of analysis. If this represent the core of his argument, we consider it sound; while, at the same time, we deny the sensation of space. With regard to "projection," again, the whole question seems to us to depend very much upon our definition of terms. Few will be likely to deny the fact that, in the immediate perception of adult life, the objects which we see are given to consciousness as external to us. We then proceed to analyze. And in our analysis we reach sensations, and the relations in which sensations lie to one another. The sensations are pure abstractions; they have been abstracted from their normal relationships; their outness is part of their relationship; therefore *quâ* sensations they have no outness. Moreover, analysis discloses, or seems to disclose, that consciousness, whether sensational or relational, is a concomitant of certain neural vibrations in the cerebral cortex. And the question arises, How does the consciousness accompanying these molecular processes in the brain give us the outness of the object of perception? The answer seems to us to be that certain of these neural processes are directly symbolic of this outness. The question thus shifts to one of origin: How have these neural processes come to be symbolic of outness? Prof. James himself tells us, in italics, that "every perception is an acquired perception." How, then, has the perception of outness been acquired? Whether we believe in use-inheritance, or, with Prof. James, rely on the selection of chance variations, the question is a valid and not an idle one.

Prof. James is insistent, as against the crude Associationalists, that a compounded idea is a new psychic fact to which the separate ideas stand in the relation, not of constituents, but of occasions of production. He denies, and in our opinion rightly denies, that we can explain the constitution of the higher mental states by viewing them as identical with the lower ones merely summed together. No more can we do so than we can explain the constitution of grey nerve-matter as identical with certain material atoms merely summed together. In each case the nature of the summation is determined by the fixed laws of the constitution of thoughts and things. Whether we have any right to say more than this, from the scientific standpoint, is open to question.

An interesting chapter is devoted to instinct. In Prof. James's acceptation of the term, instinct means innate impulse, or what we have elsewhere termed innate capacity for response in action, as opposed to any special and more or less definite manifestation of this impulse performed without intelligence and without practice. We question whether this wide extension of the field of instinct is advisable. In any case, it is in opposition to the tendency of recent thought on the subject.

In the last chapter there is an interesting and able discussion of the range and limits of experience. We propose merely to indicate the nature of the problem, and to refer our readers to Prof. James's work for his ingenious treatment of it. The modes of external noumenal existence are represented in the mind of man in mental symbolism. Whether the products of this mental symbolism in any sense *resemble* the noumena symbolized is wholly immaterial and altogether beside the question. To us it appears that the only phenomenal which can be colorably supposed to resemble the noumenal is that which we designate sequence. By Prof. James both time and space are so regarded. "The time- and space-relations between things," he says, "do stamp copies of themselves within." If we are right in understanding him to contend that our ideas of concrete things in any sense resemble their noumena, we altogether disagree with him. But this, as we said, is immaterial.

Now, experience may be described as the adjustment of the symbolism for the practical purposes of life and thought. And in a series of organic beings this adjustment may be developed and perfected, either by the inheritance of individual increments of adjustment (use-inheritance), or by the elimination of failures in adjustment (natural selection), or by both combined. We may therefore speak of (1) experience as developed through use-inheritance, and (2) experience as developed by natural selection. Some people would have us restrict the term to the former category, which appears to us somewhat unsatisfactory. The more satisfactory plan is to distinguish between experience as a guide, determining what varieties spontaneously arising shall survive, and experience as a source of origin. If use-inheritance be disproved, experience as a source of origin goes by the board. But at the same time we may quite legitimately say that it is by experience that our mental symbolism has been *moulded*. For origin we are then thrown back either upon accidental variation (which is but a phrasal cloak for our ignorance), or upon the view that psychical development has obeyed certain fixed laws of our mental being, just as a crystal is developed in accordance with certain fixed laws of physical being.

When we have to deal, not with perceptual symbolism, where verification is within easy range, but with conceptual symbolism, the moulding by experience is indirect. Hence the divergent theories of things, ethical types, and æsthetic standards. Prof. James stoutly maintains that with these experience has had little or nothing to do. And he is so far right in that they rose to the clouds (or, if it sound better, the empyrean) of conception, from the solid (*i.e.* verifiable) ground of experience, and can only be tested in so far as their conclusions can be brought down to the same practical and mundane sphere. For the rest, as we have elsewhere suggested, our theories of things and interpretations of nature are subject to a process of elimination through incongruity, whereby conceptions incongruous with any individual system of mental symbolism strive in vain to enter therein and become incorporated therewith.

We have presented this question rather as it had formulated itself in our own mind before we had read Prof. James's work, than as it is set forth in his final chapter, to which we must now refer our readers. And we have dwelt upon the point at some length that our biological readers may see the bearing of the use-inheritance question in the mental field. Prof. James is on the side of Weismann, and rejects use-inheritance. Our own position is that of anti-dogmatism. We think use-inheritance is still upon its trial, and we desire to see fair play. Mr. Platt Ball's valuable discussion does not get down to the root of the question, which is really one of the origin of variations. We want more experimental evidence. But crucial experiments are hard to devise.

We regard Prof. James's work as an exceedingly valuable contribution to psychological literature; and trust that in a second edition he will make the index in some degree worthy of the volumes, the treasures of which it at present very inadequately indicates.

C. Lloyd Morgan.

THE ANNUAL REPORT OF THE UNITED STATES GEOLOGICAL SURVEY.

Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1887–88. By J. W. Powell, Director. (Washington, 1889.)

A CONSIDERABLE part of this volume is occupied by the reports of the Director and of his principal assistants on the progress in work for the year 1887–88. Of these reports, several touch upon topics of general interest, but as they are more or less of a preliminary nature, we shall restrict ourselves to a notice, necessarily brief, of the larger and later part of the volume; that containing the papers accompanying the official report. The first of these, drawn up by Captain C. E. Dutton, gives a history of the Charleston earthquake on August 31, 1886, with a discussion of the observations collected in order to determine the position, depth, and velocity of the shock. The memoir begins with three accounts of the earthquake, contributed by eye-witnesses. The first is written in a graphic, perhaps over picturesque style, but as it was originally composed by one of the staff of a daily newspaper for public consumption, the roar of the young lion is to be expected; the others are shorter, simpler, but in some respects more valuable. At Charleston the first and principal shock occurred about 9.50 p.m. At that time the writer of the popular account was at work, as usual, in the office of the *Charleston News and Courier*, in a room on the second floor. His attention was first attracted by a sound, apparently from below, which was accompanied by a perceptible tremor, much as if a heavily-loaded dray were passing by. For two or three seconds the occurrence excited no surprise or comment:—

"Then, by swift degrees, or all at once—it is difficult to say which—the sound deepened in volume, the tremor becoming more decided; the ear caught the rattle of window-sashes, gas-fixtures, and other movable objects.... The long roll deepened and spread into an awful roar that seemed to pervade at once the troubled earth and the still air above and around. The tremor was now a rude, rapid quiver that agitated the whole lofty strong-walled building, as though it were being shaken—shaken by the hand of an immeasurable power; . . . there was no intermission in the vibration of the mighty subterranean engine. From the first to the last it was a continuous jar, adding force with every moment, and as it approached and reached the climax of its manifestation, it seemed for a few seconds that no work of human hands could possibly survive the shocks. . . . For a second or two it seemed that the worst had passed, and that the violent motion was subsiding. It increased again, and became as severe as before. . . . The uproar slowly died away in seeming distance. The earth was still."

The earthquake also was very severe at Summerville, a village 22 miles to the north-west, where some slight preliminary shocks had been felt on August 27 and 28, which, however, were not perceived at Charleston. Other shocks occurred at Charleston during the month following, but fortunately none were severe. But the devastation caused by the great shock was lamentable: 27 persons were killed; at least thrice that number died afterwards from hurts, or the indirect results of the earthquake; not more than half-a-dozen houses escaped injury; many buildings were practically destroyed. The numerous illustrations, taken in many cases from photographs, enable the reader to judge of the devastation. "Hibernian Hall," with two broken columns, projecting from a mass of *débris*, the sole remnants of a portico, might, at the present day, suggest to some minds a political interpretation. The work of the jerry-builder—who evidently exists in Charleston as in London—was revealed by the earthquake, which proved to be a satisfactory, though expensive test of the relative value of different building materials. Fissures and craterlets were formed in the surrounding country, from which water and sand were ejected. The tracks of railways were distorted and otherwise injured; and in one case a train was wrecked by being thrown off the rails.

Captain Dutton's observations have enabled him to construct a map of the isoseismal lines, and to estimate the depth and velocity of the shock. These "isoseisms" are arranged about two "epicentra," which lie on a straight line running rather west of south, about 13 miles apart, and nearly as much west of Charleston. The curves around the northern one are almost circular; around the southern one they are somewhat elongated and rather distorted ovals. To the focus of the former a depth of 12 miles is assigned, with a probable error of less than 2 miles; to that of the latter, a depth of nearly

8 miles, but here the data are less complete. The velocity of the shock is estimated at nearly 5200 metres a second. Large numbers of observations are recorded and discussed in their practical and theoretical bearings, and the memoir is enriched by numerous illustrations, which, as usual, are admirably executed.

The second memoir, by Prof. N. S. Shaler, describes the geology of the district about Cape Ann (Massachusetts). This district consists entirely of igneous rocks, but it affords exceptional advantages for the study of dykes, of the phenomena of joints, and of the superincumbent drift, while on its coasts the relation of wave-work to ice-work can be readily examined. This memoir, as might be anticipated, is not of such exceptional interest as the preceding one; the illustrations, however, are excellent, and in several cases will be valuable to students who cannot travel far afield.

The third memoir, by Mr. W. H. Weed, "On the Formation of Travertine and Siliceous Sinter by the Vegetation of Hot Springs," is one of much general interest. A large number of facts have been collected and co-ordinated by the author, which establish, as he believes, the following conclusions: (*a*) that the plant-life of the calcareous Mammoth Hot Springs waters causes the deposition of travertine, and is a very important agent in the formation of such deposits; (*b*) the vegetation of the hot alkaline waters of the geyser basins eliminates silica from the water by its vital growth, and produces deposits of siliceous sinter; (*c*) the thickness and extent of the deposits produced by the plant-life of thermal waters establishes the importance of such vegetation as a geological agent. The effect of organisms in promoting mineral accumulation, indirectly by decomposition after death, or directly during life, has long been recognized, but it is interesting to find that some algæ, like some corals, may claim to be called "reef-builders."

A short memoir on the geology and physiography of a part of North-Western Colorado, Utah, and Wyoming concludes the volume. As may be inferred from our brief outline of its contents, it is not less interesting than any one of its eight predecessors, and it abounds in illustrations admirably executed. In this respect, unfortunately, geological works published in Great Britain cannot be compared with those of the United States of America.

T. G. Bonney.

OUR BOOK SHELF.

On the Eggs and Larvæ of some Teleosteans. By Ernest W. L. Holt. (Transactions of the Royal Dublin Society, Vol. IV. Part 7, February 1891.)

In 1890 the Royal Dublin Society appointed a Committee for the purpose of investigating the fishing grounds on the west coast of Ireland, and they voted a considerable sum of money for the cost of the expedition. This sum was increased by a vote in aid from the Irish Government, who thus properly assisted the efforts of a private Society in attempting a work of great general utility. Here it is not our intention to refer in any detail to the general work done during the last year by the expedition, which was under the charge of several well-known biologists, and which we are glad to learn is being continued this year, under even more favourable auspices than before; but to call attention to a report by Mr. Ernest Holt on the eggs and larvæ of some Teleosteans met with in the first year's cruise. The ova were collected between June 12 and July 11, 1890, and the observations made thereon, with the series of beautiful drawings now before us, were laid before the Royal Dublin Society in November of the same year, while the memoir itself was published in February last. All the observations and drawings of the living forms were made on board ship, but the bad weather generally encountered during the cruise left much to be desired in the way of facilities for microscopical study. It must also be remembered that the main object of the expedition was an inquiry into the presence of "fish grounds," into the condition of the reproductive organs and food of adult fishes, and that these special researches of Mr. Holt were conducted only in such time as could be spared from these other duties.

Under all these circumstances it is surprising that so much good work was done, the ova of twenty-two forms having been more or less carefully investigated. Most of these were pelagic ova. The methods used for their capture were: (1) towing at the surface small ring-nets of fine cheese cloth at the sides of the vessel whilst trawling; (2) sinking larger ring-nets and a large triangular midwater net, after Prof. McIntosh's pattern, to a fathom or so below the surface, and allowing the ship to drift with them for a short time; (3) trawling from the ship's boats with a small naturalist's trawl with muslin net—owing probably to some defect in the latter, this method was not very successful. The first method proved by far the most productive. The following were the species examined. The ova and larvæ of those with an asterisk are figured in detail in the plates. *Scomber scomber*, **Trachinus vipera*, *Trigla gurnardus*, **Gobius niger*, **Callionymus lyra*, **Cepola rubescens*, **Lepadogaster bimaculatus*, *Labrus maculatus*, *Crenilabrus melops*, *Merluccius vulgaris*, *Macrurus* (?), **Rhombus lævis*, **Pleuronectes microcephalus*, *P. cynoglossus*, *Clupea sprattus*, **Solea* (?), **S. lutea* (?), **A. Motella*-like form, **Ctenolabrus rupestris* (?), **A Coris*-like form, and four unidentified forms which are figured. There is also a table showing the comparative dimensions of the ova.

This memoir forms a very important contribution to the history of the development of our native fish, and is exceedingly well printed and illustrated.

The Hand-book of Games. Vol. II. Card Games. New Edition. (London: George Bell and Sons, 1891.)

The first volume of this work dealt with table games, while the present one treats entirely of card games. Of the twenty-two games here described, eleven are new, or we should rather say did not appear in the first edition, and several of the old games, such as Boston, quinze, lansquenet, &c., which have become obsolete, are omitted.

The game that stands above all others is undoubtedly whist, and to play it well requires no small amount of scientific reasoning. Bearing this in mind Dr. William Pole treats of it in a very able manner, laying before the reader a good account of the game as it is played to-day. In a general view of the modern game, he points out that it was Edmund Hoyle's book which, in 1743, was the first to bring the game to a definite shape. Then followed William Payne's "Maxims," published in 1770, and T. Mathews's "Advice to the Young Whist Player," written in 1800. It is in these three books that the game as then played is fully described. In the later works of "Cavendish" and Clay, the game now known as "modern whist" is expounded, and the author, by making a judicious use of all the above-mentioned authorities, has, in the space of about 100 pages, written both an instructive and refreshing essay on the subject in question.

In regard to the other games treated of, we may note that they are all written by the best authorities. Thus solo whist is dealt with by Robert F. Green, piquet, écarté, euchre, bézique, and cribbage by "Berkeley," and the round games, including napoleon, loo, &c., by Baxter-Wray. Although we have only just mentioned these games by name, it must be understood that each is thoroughly well described, and numerous illustrations for the explanation of difficult hands are inserted. The work will be most popular as a book of reference; and we may add that those who wish only to be instructed in one or two games in particular can obtain different sections in separate volumes.

Object-Lessons from Nature: a First Book of Science. By L. C. Miall, Professor of Biology in the Yorkshire College, Leeds. (London: Cassell and Co., Limited, 1890.)

THIS little volume is a very laudable attempt to form a first guide to the study of Nature for children; the lessons are meant to serve as guides to the intelligent teacher, who should in a half-demonstrative, half-catechetical way, bring the various subjects treated of before the young pupil's mind. Throughout there is no attempt to teach the student that there are a number of different sciences, but the various natural phenomena witnessed in the history of a "summer shower," in the "burning of a candle," or the "growth of a plant or insect," are gradually led up to, the various stages in the story of each being explained as they arise. Teachers must use their personal experience in illustrating their teachings, as based on these lessons, and those who are able to use the chalk and blackboard will find an immense advantage in doing so, as the little student's attention is thereby doubly attracted, and a much greater impression is made upon his mind.

LETTERS TO THE EDITOR.

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

The International Congress of Hygiene and Demography.

As I notice that you give an excellent account of the meeting of the General Committee of this Congress recently held under the presidency of H.R.H. the Prince of Wales, I have no doubt that your readers will be interested to know how the Congress came to be invited to hold its session in London this year.

In 1884 I was asked by the Organising Committee of the Congress to be held at The Hague that year to give an address at a general *séance* of the Congress; I did so, selecting as my subject "La Science Ennemie de la Maladie." During the meeting of that Congress, one of the most pleasant that I ever attended, I was frequently asked why we had never invited the Congress to London, and I was obliged to answer that our Government would give no pecuniary assistance for such a purpose, and that we had no Hygienic Society strong enough to undertake it; but I determined that anything I might be able to do to bring about that result should be done, as I felt that it was a disgrace to this Country that this Congress, of all others, had not met here.

The next Congress was to be held in Vienna in 1886, but was postponed until 1887, and in that year my opportunity came in the proposed amalgamation of the Sanitary Institute of Great Britain and the Parkes Museum. I urged the importance of the matter on the Councils of both those bodies, and induced them to pass resolutions inviting the Congress to hold its next session in London. Sir Douglas Galton (as Chairman of both Councils) and I were appointed delegates to attend the Congress at Vienna, and present the invitation; the Society of Medical Officers of Health was also asked to co-operate, and appointed Mr. Shirley Murphy as its delegate. We three went to Vienna, and presented the invitation, which was accepted. After our return to this country we set to work to form, with the aid of the other English members of the Vienna Congress, especially Sir Spencer Wells, Prof. (now Sir George) Humphry, Dr. Cameron, M.P., Prof. Frankland, and Dr. Mapother, the large and influential General Committee.

In the early days it was thankless and rather trying work, especially as the money did not come in as readily as we could have wished; but now, thanks to the patronage of Her Majesty the Queen, to the presidency of His Royal Highness the Prince of Wales, and to the support of the Lord Mayor and Corporation of the City of London, it is comparatively plain sailing, and there can be no doubt that the Congress will be a great success, to which no one will have contributed more than Sir Douglas Galton, the indefatigable Chairman of the Organising Committee. To ensure this, however, and to enable us to bear the heavy expense of printing the Transactions, &c., funds are still urgently needed. I may add that all general correspondence relating to the Congress should be addressed to Dr. G. V. Poore, the Hon. General Secretary, at the offices, 20 Hanover Square, W.

W. H. CORFIELD,
Hon. Foreign Secretary.

19 Savile Row, W., March 28.

On the Rôle of Quaternions in the Algebra of Vectors.

THE following passage, which has recently come to my notice, in the preface to the third edition of Prof. Tait's "Quaternions," seems to call for some reply:

"Even Prof. Willard Gibbs must be ranked as one of the retarders of quaternion progress, in virtue of his pamphlet on 'Vector Analysis,' a sort of hermaphrodite monster, compounded of the notations of Hamilton and of Grassmann."

The merits or demerits of a pamphlet printed for private distribution a good many years ago do not constitute a subject of any great importance, but the assumptions implied in the sentence quoted are suggestive of certain reflections and inquiries which are of broader interest, and seem not untimely at a period when the methods and results of the various forms of multiple algebra are attracting so much attention. It seems to be assumed that a departure from quaternionic usage in the treatment of vectors is an enormity. If this assumption is true, it is an important truth; if not, it would be unfortunate if it should remain unchallenged, especially when supported by so high an authority. The criticism relates particularly to notations, but I believe that there is a deeper question of notions underlying that of notations. Indeed, if my offence had been solely in the matter of notation, it would have been less accurate to describe my production as a monstrosity, than to characterize its dress as uncouth.

Now what are the fundamental notions which are germane to a vector analysis? (A vector analysis is of course an algebra for vectors, or something which shall be to vectors what ordinary algebra is to ordinary quantities.) If we pass over those notions which are so simple that they go without saying, geometrical addition (denoted by +) is, perhaps, first to be mentioned. Then comes the product of the lengths of two vectors and the cosine of the angle which they include. This, taken negatively, is denoted in quaternions by $S\alpha\beta$, where α and β are the vectors. Equally important is a vector at right angles to α and β (on a specified side of their plane), and representing in length the product of their lengths and the sine of the angle which they include. This is denoted by $V\alpha\beta$ in quaternions. How these notions are represented in my pamphlet is a question of very subordinate consequence, which need not be considered at present. The importance of these notions, and the importance of a suitable notation for them, is not, I suppose, a matter on which there is any difference of opinion. Another function of α and β, called their product and written $\alpha\beta$, is used in quaternions. In the general case, this is neither a vector, like $V\alpha\beta$, nor a scalar (or ordinary algebraic quantity), like $S\alpha\beta$, but a quaternion—that is, it is part vector and part scalar. It may be defined by the equation—

$$\alpha\beta = V\alpha\beta + S\alpha\beta.$$

The question arises, whether the quaternionic product can claim a prominent and fundamental place in a system of vector analysis. It certainly does not hold any such place among the fundamental geometrical conceptions as the geometrical sum, the scalar product, or the vector product. The geometrical sum $\alpha + \beta$ represents the third side of a triangle as determined by the sides α and β. $V\alpha\beta$ represents in magnitude the area of the parallelogram determined by the sides α and β, and in direction the normal to the plane of the parallelogram. $S\gamma V\alpha\beta$ represents the volume of the parallelopiped determined by the edges α, β, and γ. These conceptions are the very foundations of geometry.

We may arrive at the same conclusion from a somewhat narrower but very practical point of view. It will hardly be denied that sines and cosines play the leading parts in trigonometry. Now the notations $V\alpha\beta$ and $S\alpha\beta$ represent the sine and the cosine of the angle included between α and β, combined in each case with certain other simple notions. But the sine and cosine combined with these auxiliary notions are incomparably more amenable to analytical transformation than the simple sine and cosine of trigonometry, exactly as numerical quantities combined (as in algebra) with the notion of positive or negative quality are incomparably more amenable to analytical transformation than the simple numerical quantities of arithmetic.

I do not know of anything which can be urged in favour of the quaternionic product of two vectors as a *fundamental* notion in vector analysis, which does not appear trivial or artificial in comparison with the above considerations. The same is true of the quaternionic quotient, and of the quaternion in general.

How much more deeply rooted in the nature of things are the functions $S\alpha\beta$ and $V\alpha\beta$ than any which depend on the definition of a quaternion, will appear in a strong light if we try to extend our formulæ to space of four or more dimensions. It will not be claimed that the notions of quaternions will apply to such a space, except indeed in such a limited and artificial manner as to rob them of their value as a system of geometrical algebra. But vectors exist in such a space, and there must be a vector analysis for such a space. The notions of geometrical addition and the scalar product are evidently applicable to such a space. As we cannot define the direction of a vector in space of four or more dimensions by the condition of perpendicularity to two given vectors, the definition of $V\alpha\beta$, as given above, will not apply *totidem verbis* to space of four or more dimensions. But a little change in the definition, which would make no essential difference in three dimensions, would enable us to apply the idea at once to space of any number of dimensions.

These considerations are of a somewhat *a priori* nature. It may be more convincing to consider the use actually made of the quaternion as an instrument for the expression of spatial relations. The principal use seems to be the derivation of the functions expressed by $S\alpha\beta$ and $V\alpha\beta$. Each of these expressions is regarded by quaternionic writers as representing two distinct operations; first, the formation of the product $\alpha\beta$, which is the quaternion, and then the taking out of this quaternion the scalar or the vector part, as the case may be, this second process being represented by the selective symbol, S or V. This is, I suppose, the natural development of the subject in a treatise on quaternions, where the chosen subject seems to require that we should commence with the idea of a quaternion, or get there as soon as possible, and then develop everything from that particular point of view. In a system of vector analysis, in which the principle of development is not thus predetermined, it seems to me contrary to good method that the more simple and elementary notions should be defined by means of those which are less so.

The quaternion affords a convenient notation for rotations. The notation $q(\ )q^{-1}$, where q is a quaternion and the operand is to be written in the parenthesis, produces on all possible vectors just such changes as a (finite) rotation of a solid body. Rotations may also be represented, in a manner which seems to leave nothing to be desired, by linear vector functions. Doubtless each method has advantages in certain cases, or for certain purposes. But since nothing is more simple than the definition of a linear vector function, while the definition of a quaternion is far from simple, and since in any case linear vector functions must be treated in a system of vector analysis, capacity for representing rotations does not seem to me sufficient to entitle the quaternion to a place among the *fundamental* and *necessary* notions of a vector analysis.

Another use of the quaternionic idea is associated with the symbol ∇. The quantities written $S\nabla\omega$ and $V\nabla\omega$, where ω denotes a vector having values which vary in space, are of fundamental importance in physics. In quaternions these are derived from the quaternion $\nabla\omega$ by selecting respectively the scalar or the vector part. But the most simple and elementary definitions of $S\nabla\omega$ and $V\nabla\omega$ are quite independent of the conception of a quaternion, and the quaternion $\nabla\omega$ is scarcely used except in combination with the symbols S and V, expressed or implied. There are a few formulæ in which there is a trifling gain in compactness in the use of the quaternion, but the gain is very trifling so far as I have observed, and generally, it seems to me, at the expense of perspicuity.

These considerations are sufficient, I think, to show that the position of the quaternionist is not the only one from which the subject of vector analysis may be viewed, and that a method which would be monstrous from one point of view, may be normal and inevitable from another.

Let us now pass to the subject of notations. I do not know wherein the notations of my pamphlet have any special resemblance to Grassmann's, although the point of view from which the pamphlet was written is certainly much nearer to his than to Hamilton's. But this is a matter of minor consequence. It is more important to ask, What are the requisites of a good notation for the purposes of vector analysis? There is no difference of opinion about the representation of geometrical addition. When we come to functions having an analogy to multiplication, the product of the lengths of two vectors and the cosine of the angle which they include, from any point of view except that of the quaternionist, seems more simple than the same quantity taken negatively. Therefore we want a notation for what is expressed by $-S\alpha\beta$, rather than $S\alpha\beta$, in quaternions. Shall the symbol denoting this function be a letter or some other sign? and shall it precede the vectors or be placed between them? A little reflection will show, I think, that while we must often have recourse to letters to supplement the number of signs available for the expression of all kinds of operations, it is better that the symbols expressing the most fundamental and frequently recurring operations should not be letters, and that a sign between the vectors, and, as it were, uniting them, is better than a sign before them in a case having a formal analogy with multiplication. The case may be compared with that of addition, for which $\alpha + \beta$ is evidently more convenient than $\Sigma(\alpha, \beta)$ or $\Sigma\alpha\beta$ would be. Similar considerations will apply to the function written in quaternions $V\alpha\beta$. It would seem that we obtain the *ne plus ultra* of simplicity and convenience, if we express the two functions by uniting the vectors in each case with a sign suggestive of multiplication. The particular forms of the signs which we adopt is a matter of minor consequence. In order to keep within the resources of an ordinary printing-office, I have used a dot and a cross, which are already associated with multiplication, but are not needed for ordinary multiplication, which is best denoted by the simple juxtaposition of the factors. I have no especial predilection for these particular signs. The use of the dot is indeed liable to the objection that it interferes with its use as a separatrix, or instead of a parenthesis.

If, then, I have written $\alpha . \beta$ and $\alpha \times \beta$ for what is expressed in quaternions by $-S\alpha\beta$ and $V\alpha\beta$, and in like manner $\nabla . \omega$ and $\nabla \times \omega$ for $-S\nabla\omega$ and $V\nabla\omega$ in quaternions, it is because the natural development of a vector analysis seemed to lead logically to some such notations. But I think that I can show that these notations have some substantial advantages over the quaternionic in point of convenience.

Any linear vector function of a variable vector ρ may be expressed in the form—

$$\alpha\lambda.\rho + \beta\mu.\rho + \gamma\nu.\rho = (\alpha\lambda + \beta\mu + \gamma\nu).\rho = \Phi.\rho,$$

where

$$\Phi = \alpha\lambda + \beta\mu + \gamma\nu;$$

or in quaternions

$$-\alpha S\lambda\rho - \beta S\mu\rho - \gamma S\nu\rho = -(\alpha S\lambda + \beta S\mu + \gamma S\nu)\rho = -\phi\rho,$$

where

$$\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu.$$

If we take the scalar product of the vector $\Phi . \rho$, and another vector σ, we obtain the scalar quantity

$$\sigma.\Phi.\rho = \sigma.(\alpha\lambda + \beta\mu + \gamma\nu).\rho,$$

or in quaternions

$$S\sigma\phi\rho = S\sigma(\alpha S\lambda + \beta S\mu + \gamma S\nu)\rho.$$

This is a function of σ and of ρ, and it is exactly the same kind of function of σ that it is of ρ, a symmetry which is not so clearly exhibited in the quaternionic notation as in the other. Moreover, we can write $\sigma.\Phi$ for $\sigma.(\alpha\lambda + \beta\mu + \gamma\nu)$. This represents a vector which is a function of σ, viz. the function conjugate to $\Phi.\sigma$; and $\sigma.\Phi.\rho$ may be regarded as the product of this vector and ρ. This is not so clearly indicated in the quaternionic notation, where it would be straining things a little to call $S\sigma\phi$ a vector.

The combinations $\alpha\lambda$, $\beta\mu$, &c., used above, are distributive with regard to each of the two vectors, and may be regarded as a kind of product. If we wish to express everything in terms of i, j, and k, Φ will appear as a sum of ii, ij, ik, ji, jj, jk, ki, kj, kk, each with a numerical coefficient. These nine coefficients may be arranged in a square, and constitute a matrix; and the study of the properties of expressions like Φ is identical with the study of ternary matrices. This expression of the matrix as a sum of products (which may be extended to matrices of any order) affords a point of departure from which the properties of matrices may be deduced with the utmost facility. The ordinary matricular product is expressed by a dot, as $\Phi \,.\, \Psi$. Other important kinds of multiplication may be defined by the equations—

$$(\alpha\lambda) \overset{\times}{\times} (\beta\mu) = (\alpha \times \beta)(\lambda \times \mu), \qquad (\alpha\lambda) : (\beta\mu) = (\alpha.\beta)(\lambda.\mu).$$

With these definitions $\frac{1}{6}\Phi \overset{\times}{\times} \Phi : \Phi$ will be the determinant of Φ, and $\Phi \overset{\times}{\times} \Phi$ will be the conjugate of the reciprocal of Φ multiplied by twice the determinant. If Φ represents the manner in which vectors are affected by a strain, $\frac{1}{2}\Phi \times \Phi$ will represent the manner in which surfaces are affected, and $\frac{1}{6}\Phi \overset{\times}{\times} \Phi : \Phi$ the manner in which volumes are affected. Considerations of this kind do not attach themselves so naturally to the notation $\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu$, nor does the subject admit so free a development with this notation, principally because the symbol S refers to a special use of the matrix, and is very much in the way when we want to apply the matrix to other uses, or to subject it to various operations.

J. WILLARD GIBBS.

New Haven, Connecticut.

The Meaning of Algebraic Symbols in Applied Mathematics.

PROF. GREENHILL, on p. 462 (March 19), gives a naïve and most instructive description of the straits to which a "practical man" is put when he wishes to interpret the simplest general formula.

I have always held, not in sarcasm but in sorrow, that students brought up on the system of specialized and limited numerical formulæ used by Prof. Greenhill and some other Professors of Engineering in this country, must necessarily go through the tentative trial-and-error sort of process which he so graphically describes, whenever they have to obtain a numerical result from anything not already arithmetical. In other words they are unable to deal with complete algebraic symbols or concrete quantities.

The symbol "v" to them does not completely represent a velocity, it only represents a number; and to make it represent a velocity some words, such as "in feet per second," must be added. Whereas, since it is plain that the velocity of light does not vary with its numerical specification, nor the size of a room change according as it is measured in feet or in inches, it is surely better to make a symbol express the essential and unchanging aspect of the thing to be dealt with, *i.e.* the thing itself, and not merely the number of some arbitrary and conventional units which the thing contains.

May I, then, assure Prof. Greenhill very seriously, *and with entire appreciation of and accord with his insistence that all expressions should be complete and capable of immediate practical numerical interpretation,* that the equation $T = \rho v^2$ is perfectly complete, and that it is true and immediately interpretable in every consistent system of units that has been or that can be invented? T is the tenacity, ρ the density, of the material of a ring, and v is its critical or bursting velocity. There is no need to say a word more. And no properly taught student ought to have the slightest difficulty in obtaining a numerical result directly in any system of conventional units that may be offered him.

It is the frequent recurrence of such ghastly parodies of formulæ as—

$$T = \frac{62 \cdot 4}{2240 \times 144} \cdot \frac{\rho v^2}{g}.$$

in many engineering treatises which makes them such dismal reading. It is a standing wonder to physicists how a man of Prof. Greenhill's power can fail to see the inadequacy and tediousness of expressions which are only true in one particular system of units, and which to be true even in that require the special statement of every unit employed. For not only is Prof. Greenhill's expression long in itself, but it is incomplete without the tiresome addition, "ρ being measured in so and so, v in &c., T in something else, and g meaning nothing more than the pure number 32·2." All this has been needlessly put into the formula, and so has to be wearisomely taken out again.

If there be any physicist who does *not* contend for the concrete interpretation of algebraic terms (wherein each symbol is taken to represent the quantity *itself*, and not merely a numerical specification of it in some conventional unit—see, for fuller explanation, NATURE, vol. xxxviii. p. 281), I trust he will write and uphold his position on the side of Prof. Greenhill.

I suspect that the cause of Prof. Greenhill's failure at present to recognize the extreme simplicity and reasonableness of the physicist's procedure is to be found, partly in a vague idea that in order to get numerical results in British units from a general expression it is necessary to work it out in C.G.S. units first, and then translate, which I assure him is not the case; and partly in the general difficulty which most people feel in thinking it possible that they can be mistaken.

I would gladly convince Prof. Greenhill if I could, because he would carry with him so many other teachers, and thus a mass of waste labour would be saved annually to several thousands of students. Would it be too much to ask him to consider the matter with care, and, if possible, from our point of view; setting me a sum to do if that would be any assistance towards bringing him to the desired point of view?

OLIVER J. LODGE.

Tension of a "Girdle of the Earth."

IT is perfectly true, as Prof. Lodge has asserted, that a cord or chain running on its own track as an endless band in a frictionless groove *of any form* will not require the sides of the groove to keep it in that form. But whatever velocity it moves with, such a tension will exist all round it as to resist the centrifugal forces of its windings; and to preserve them by virtue of the curvature and constant tension, invariable in shape however the speed of coursing of the belt may be increased, without any external guidance and assistance. If w is the cord's mass per unit of its length, and v the velocity with which it pursues its course, wv^2 is the tension in dynes which will be set up all round it. The speed may of course be so increased as to tear the cord or chain to pieces; and this will occur in steel tires of railway wheels, for example, if the train's velocity on which the wheels are carried is much more than 120 miles an hour. Mr. Bourne long ago pointed out, in his works on the steam-engine, that a very low limit of speed in railway trains is enforced for safety in view of this dynamical condition so as not to approach and exceed working and proof-stresses at least in the material of which steel wheel-tires are formed.

But the truth of the proposition $T = wv^2$ rests entirely on the supposition that the running cord or cable pursues *exactly its own curve* in its motion. For a tension of 30 tons per square inch to be reached in a maritime cable in virtue of its being carried round either at the equator or at any distance of latitude from the equator, it must be presupposed that while buoyed with its own submerged lightness so as to be practically weightless in the water, it must from one end point of support to the other *follow accurately* the equator's circle of curvature, or the circle of curvature of the small circle of latitude along which it is laid, because this is *the line of motion* along which its parts are carried along by the earth's rotation. These circles are practically straight lines for any mile or two of cable, and truly enough, if in the presence of even the weak force acting "centrifugally" on the cable's mass by the earth's rotation, it is attempted (supposing it to be quite weightless otherwise) to pull it as nearly straight as the hardly sensible curvature of the earth requires, mathematics would not yield its point an inch, and a pull of something like 30 *tons per square inch would be required.* If the natural weight of the cable is nearly 400 times as great as the counter-force on it produced "centrifugally," we could imitate this outwards, by supposing the earth to revolve about 20 times faster than it is actually doing; and then the end pulls required to make a telegraph cable relieved from gravitation and acted on only by this outward "centrifugal force" just equal to the cable's weight, to stretch it to as flat a curve as that of the earth's circumference and of its circles of latitude, would be about 20 × 30 tons per square inch, or vastly more, of course, than any cable could withstand! But what telegraph-engineer would ever dream of trying to stretch telegraph wires on poles along the banks of a canal so that even between poles they should everywhere *be absolutely parallel to* the water level surface in the canal? With their natural weight acting outwards against him he would know that the attempt would be sure to result in rupture! And even if the centrifugal force due to the earth's velocity in this latitude of about 1250 feet a second acted alone on the wires instead of the above supposed one of about 5 miles a second to represent an equivalent to gravity, he would surely not be surprised to find his attempt to pull the wires as straight as the surface of still water, end only unsuccessfully in breaking them?

A. S. Herschel.

New Athenæum Club, London, March 20.

Spinning Disks.

Mr. Wehage, of Berlin, has independently obtained Prof. Ewing's results for a solid disk by putting $p_1 = p_2$ for $r = 0$, in Grossmann's equations.

As Prof. Boys points out, Maxwell attacked the problem in 1850, at the age of 19. I have gone over his solution ("Papers," vol. i. p. 61), and find that, in consequence of two slips he has made, his results do not agree with Ewing's. His first equation of (57) should read $\frac{c_1}{2r^2}$, instead of $\frac{c_1}{r^2}$; and his third equation of (57) will then read $-\frac{c_1}{2r^2}$, vice $-\frac{c_1}{r^2}$. His final result (59) is not affected by this alteration, which only changes c_1 and not c_2; but, owing to a wrong sign, that equation should read

$$q = h_1 + \frac{\pi^2 k}{2gt^2}\left[-2(r^2 + a_1^2) + \frac{E}{m}(3r^2 - a_1^2)\right];$$

which, remembering that Maxwell's E and m are E and $E/(1+\mu)$ respectively in Ewing's notation [or Thomson and Tait's $9nk/(3k+n)$ and $2n$, respectively], is seen to be the same as

$$p_1 = h_1 + \frac{\omega^2\rho}{8}\left[(3+\mu)a^2 - (1+3\mu)r^2\right];$$

except that Maxwell takes tensions negative.

Maxwell's equation (60) is correct; but is not deducible from his (59).

As Prof. Pearson remarks (Todhunter and Pearson's "History of Elasticity," vol i. p. 827), Maxwell seems to have thought the disk would yield first at the rim.

Cambridge, March 23. J. T. Nicolson.

P.S., March 28.—Since writing the above, I have read Prof. Pearson's letter (Nature, March 26, p. 488), in which he says that "Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations." They flow, however, also at once from Maxwell's equations, if these are corrected in the manner I have shown above. Prof. Pearson is not correct in supposing that Maxwell's and Grossmann's solutions lead to different results for the position on the disk of the maximum hoop-tension. Maxwell probably thought the disk would yield first at the rim from his putting $r = a_1$ in his final result; but his corrected results are identical with Grossmann's for a hollow, and with Ewing's for a solid disk.—J. T. N.

The Stresses in a Whirling Disk.

By an unfortunate accident my letter on this subject was printed (Nature, March 19, p. 462) without correction in proof. There are several *errata*, due to slips in the manuscript.

In column 1, p. 462, line 10 from bottom, the words "hoop" and "radial" are transposed. Read "the hoop tension p_1 and the radial tension p_2."

In column 2, equation (7) should read

$$\frac{u}{r} = \frac{C}{r^2} + C_1 - \frac{(1-\mu^2)\,\omega^2\rho r^2}{8E} \quad \ldots\ldots (7)$$

Further down, the expression for the maximum radial tension in a disk with a central hole should read

$$\text{Max. } p_2 = \frac{\omega^2\rho}{8}(3+\mu)(a_1 - a_2)^2.$$

And in the special case when the hole is very small,

$$\text{Max. } p_1 = 2 \times \text{Max. } p_2 = \frac{\omega^2\rho a_1^2(3+\mu)}{4}.$$

Cambridge, March 20. J. A. Ewing.

The Poison Apparatus of the Heloderma.

Our largest United States lizard, the *Heloderma suspectum*, is too well known to science to require any special description here. In the Proceedings of the Zoological Society of London, for April 1, 1890, the writer published quite an exhaustive memoir upon the entire morphology of this famous reptile, and among other parts of its anatomy reference was made to its poison apparatus. Fig. 4 of Plate xvi., of the contribution in question, showed a superficial dissection of the under side of the head of a large Helođerm, and upon the left side of the same the submaxillary gland is turned outwards, thus rendering it possible to be seen the four structures leading from (or to) this gland to separate and as many foramina opposite them, which exist upon the external aspect of the ramus of the lower jaw. Heretofore, these four structures have very generally been looked upon as the four poison ducts leading from this gland to the hollow space in the mandible, which latter in turn had its upper outlets in the minute foramina, found one each at the base of the teeth of this jaw. The poison was supposed to pass from the gland through these four ducts into the body of the ramus, then through the above-noted foramina, whence it was conducted into any wound the reptile might inflict with its teeth, along the grooves which have for many years been known to exist upon them. Without making any especial microscopical description, this is practically the view I supported in my memoir in the Proceedings of the Zoological Society, and I added that "Fischer found in his specimen that these ducts branched as they quit the gland; this was not the case in the reptile examined by me. Each duct passes obliquely upwards and inwards through the lower jaw, and its internal opening within the mouth is found at the base of the tooth it supplies, near the termination of the groove of the tooth" (p. 207). Dr. Fischer's paper was published in Hamburg in 1882, and in it he also gives a figure showing the ducts I have just mentioned; and he is largely responsible for the view that has been adopted in regard to them. As early as 1857, however, John Edward Gray evidently leaned in the same direction, and he speaks of "*Heloderma horrida*, in which all the teeth are uniformly furnished with a basal cavity and foramen,"—structures which he compared with the poison fangs and associated parts of venomous serpents. Dr. C. K. Hoffmann had the same ideas about the poison apparatus of the *Helodermas* (Bronn's "Klassen des Thier-Reichs," Bd. vi., iii. Abth., 30–32 Lief., pp. 890–892), and he republished Fischer's figures; and thus running through the similar views of numerous other leading herpetologists, we find, even as late as 1890, Prof. Samuel Garman, of Harvard University, quoting Fischer's description of the poison glands of *Heloderma* without question (author's reprint from Bull. Essex Inst., vol. xxii., Nos. 4–6, 1890, p. 9), and he adds that "no glands have been found on the upper" jaw. With all this in my mind, I was not a little surprised when Prof. C. Stewart, on January 20 last, in a paper read before the Zoological Society of London, claimed that he believed "that he had shown that in both species [*H. horridum* and *suspectum*] the ducts of the gland did not enter the lower jaw, but passed directly to openings situated under a fold of mucous membrane between the lip and the jaw. He thought that the structures previously described as ducts were only the branches of the inferior dental nerve and blood vessels." Upon hearing this, I at once took a large specimen of *Heloderma suspectum* to my friend Dr. E. M. Schaeffer, the well-known microscopist in Washington, D.C., who kindly examined the structures, and found them to be exactly as Prof. Stewart had described. It now remains to be said of what use are the foramina at the base of the teeth in these lizards. Why are the upper teeth grooved when there is no poison gland upon the upper jaw? Would not such a severe bite as a *Heloderma* is enabled to give kill "frogs and insects," even were no poison injected into such wounds? Are the grooves on the teeth there to conduct a poison into the wounds inflicted by the teeth of this lizard? Here in America the evidence would seem to be rapidly leading to the demonstration of the now entertained theory that the saliva of this heretofore much-dreaded reptile is possibly almost entirely innocuous. If it be proven, it is unnecessary to add that it leaves the great order *Lacertilia*, in so far as our present acquaintance goes with it, without a single representative possessed of the power of inflicting a venomous wound by the means of its bite.

R. W. SHUFELDT.

Smithsonian Institution, February 24.

The Recession of the Niagara Gorge.

BEFORE the survey of 1842, the only data for estimating the rate of recession of the Niagara Gorge were the observations of the people of the neighbourhood. Mr. Bakewell, in 1829, "was informed by Mr. Forsyth, the proprietor of the Pavilion Hotel on the Canada side, that during his residence of forty years the Falls had receded forty yards" (*American Journal of Science*, May 1857, p. 85). It is well-known that Sir Chas. Lyell, at the time of his visit to the Falls, came to the conclusion that the rate of recession was not over one foot a year. He based his estimate upon the statement of his guide, that between 1815 and 1841 the American Fall had receded forty feet. Modern investigators, basing their calculations on the surveys of 1842, 1875, and 1883, estimate the recession of the Canadian Fall at from three to five feet per year, and the age of the gorge from seven to ten thousand years (see Mr. Wesson's article in NATURE, vol. xxxii. p. 229; and Prof. Wright's "Ice Age in North America," p. 452 *seq.*). A recent American publication, "The Journal of William Maclay," a member of the Senate in the first Congress, 1789-91, brings to light the result of local observation of the recession of the Falls for the thirty years previous to the beginning of Forsyth's observations, which have generally been cited as the earliest. The passage in Maclay's journal reads:—"February 1st [1790]. . . . . Mr. Ellicott's accounts of Niagara Falls are amazing indeed. I communicated to him my scheme of an attempt to account for the age of the world, or at least to fix the period when the water began to cut the ledge of rock over which it falls. The distance from the present pitch to where the Falls originally were, is now seven miles. For this space a tremendous channel is cut in a solid limestone rock, in all parts one hundred and fifty feet deep, but near two hundred and fifty at the mouth, or part where the attrition began. People who have known the place since Sir William Johnson took possession of it, about thirty years ago, give out that there is an attrition of twenty feet in that time. Now, if 20 feet = 30 years = 7 miles, or 36,960 feet; answer, 55,440 years." In view of the fact that since 1842 the rate of recession has varied widely, this earlier testimony, so far as it can be relied upon, is especially interesting. It is possible that the observation related to the American Falls, as did that of Sir Chas. Lyell's guide; but there is nothing to indicate it, except that this is about the present rate of recession of the American Fall as calculated by Mr. Wesson. The intelligent attitude and fertile scientific suggestion of Senator Maclay are very remarkable in 1790. It will be remembered that Hutton's "Theory of the Earth" was not published separately till 1795. Maclay seems to have anticipated men of science by many years in proposing to use the recession of the gorge as a scale for measuring geological time. Perhaps even more noteworthy is the unconcern with which he sets down his conclusion that the gorge was over 55,000 years old, at a time when geology was still confined by the narrow and seemingly impassable limits of traditional Biblical chronology. Maclay's informant, Ellicott, was a Government surveyor, and, according to the account of his life in Appleton's "Cyclopædia of American Biography," "he was selected by Washington in 1789 to survey the land lying between Pennsylvania and Lake Erie, and during that year he made the first accurate measurement of the Niagara river from lake to lake, with the height of the falls and the descent of the rapids." This early survey seems entirely unknown to geologists, who all refer to Hall's of 1842 as the first. Ellicott was a man of considerable attainments, a professor in West Point from 1808 to 1820, and the correspondent of European learned societies. He left some published writings, and works still in manuscript. Cannot the data of this survey of a century ago be found? If it proved a careful one, it would double the period of scientific observation of the recession of the gorge, and increase the certainty of any generalization. The map of Evershed's survey of 1883 was published in *Science*, vol. v. p. 398. In NATURE, vol. xxxii. p. 244, Mr. Garbett quotes some interesting observations from the *Gentleman's Magazine*, January 1751, from Kalm's description of the Falls as seen shortly before. Mr. Garbett tries to identify an island that Kalm mentions as intersecting the Falls as Luna Islet. This would give a definite point for calculation. It seems to me that an attempt to test Mr. Garbett's conjecture by the Evershed map must show that it is untenable. If I have understood him, and correctly measured in accordance with his suggestions, the recession of the Canadian Falls will have been about half a mile in the 133 years, or about twenty feet a year. Notwithstanding Kalm's small dimensions, I think he meant Goat Island, for he says the island lies parallel with the river, and Luna Islet prolonged would lie almost at right angles to the river at its lower end.

EDWARD G. BOURNE.

Adelbert College, Cleveland, March 5.

Modern Views of Electricity.

I WILL reply to the last paragraph of Dr. Lodge's letter in NATURE of March 19 (p. 463) in his own language. I have no objection to contemplate a piece of isolated zinc surrounded by straining oxygen atoms, each negatively charged. And on the hypothesis that such atoms exist, I have no difficulty in realizing the depression of potential of the zinc, nor do I fail to appreciate the momentary transfer of electricity accompanying the sudden approach of the crowd of oxygen atoms when contact is made between the zinc and copper.

If we assume that atoms of oxygen have a negative charge, and if we further assume that, whether or not actual chemical combination takes place, they are attracted by zinc, and in a less degree by copper, then I admit that we have, on Dr. Lodge's principles, a consistent explanation of certain facts, viz. that, in an oxidizing medium at zero potential, isolated zinc has potential $-1{\cdot}8$ volts, and isolated copper has potential $-0{\cdot}8$ volt, and that, when metallic contact is made between them, both assume a common potential $-1{\cdot}3$ volts. So far I understand, and with only slight modification can accept, the theory developed in "Modern Views."

But I cannot, on this hypothesis, account for the slope of potential assumed (p. 111) to explain the aluminium needle experiment. Suppose the zinc and its contiguous air film to be exactly inclosed by a surface S. There is then within S an excess of negative electricity. Dr. Lodge says it is wholly on the oxygen atoms within the film, and not on the zinc—which statement I accept. Then, at every point in external space we have lines of force converging towards S, as to any negatively charged body—that is, a slope of potential downwards towards S, and at all points not too distant this will be appreciable. When contact is made with the copper, the excess of negative electricity within S is diminished, and the slope of potential at any external point is diminished in the same proportion.

Another, and, I think, quite distinct view which Dr. Lodge seems disposed to entertain is, that the atoms (or molecules) of oxygen within the air film on the surface of the zinc, without having any actual charge, are polarized—that is, have one pole positively and the other negatively charged, and present the negative pole to the zinc. So that the zinc is inclosed in a shell exactly analogous, *mutatis mutandis*, to a closed magnetic shell with its negative face inwards. I admit that such an arrangement, could it exist, would possess the properties attributed by Dr. Lodge to the air film. It would have no influence at any external point, and would give a negative potential to the inclosed zinc. But a difficulty arises *in limine*. To polarize the atoms in this manner, or to arrange them in this manner if independently polarized, requires work to be done. And from what source is the energy derived, no actual combination taking place between the atoms in question and the zinc?

S. H. BURBURY.

Mud Glaciers of Cromer.

BETWEEN Cromer and Trimmingham, it is well known, the cliffs are largely made up of boulder clay and chalky loam, in the contorted drift. The lower part is, generally speaking, composed of more compact boulder clay, while the upper portion, sand and loam, is eroded backwards more rapidly by constant slipping; this slacks by the action of rain and springs, and becomes a feeding-ground for a number of mud-streams, which creep down the gulleys or fall as mud-avalanches over the cliffs of the more resistant boulder clay below. This coalesces, and forms mud glaciers, and flows outwards upon the sands. The ends spread out, and assume the convex form usual with ordinary glaciers. The edges are crevassed longitudinally by being stretched open by the more rapid central currents, while the centre is well crevassed in the process of fanning out. The largest stream measured 60 feet across the fan, where it would be about 12 feet deep as far as exposed above the sands, for it sinks some distance into it, with a front of 6 feet.

Its slow advance causes the sand upon the shore in front to pucker into folds or convolutions, which crack, and large flakes or cakes of sand are pushed forwards in a slanting direction over those in front.

Every tide obliterates these marks and inequalities, but the distance the flanges of these flakes overlap between each tide indicates a movement of about 5 or 6 inches every twenty-four hours.

Here we have no fracture and regelation, or molecular disturbance in the exact sense of ice movement, yet the forms and currents are so similar, that with a slight covering of snow it would be difficult or impossible to detect any difference between mud and ordinary ice streams.

The convolutions and overlapping of the sands in front may not be without its value when studying the forms of the contorted drift, where folding is frequent, and the sliding of one bed over another apparent from the frequent repetition of the same bed in a vertical section. The slanting and concentric laminæ so frequently seen in our boulder clays, inclosing beds of stratified sands and gravels, may also, in some cases, be explained by the running down and overspreading of reconstructed boulder clay in a similar way to the mud-streams of Cromer, where it can be seen now in progress.

Sutton Coldfield. WILLIAM SHERWOOD.

On Frozen Fish.

DURING the last few weeks there have appeared in NATURE several letters containing extraordinary statements of the vitality of fish that have been frozen in ice. I have just found in a quaint old book—the "Anatomy of Sleep"—two further statements on this subject, and have therefore extracted them, thinking that they might prove of interest to readers of NATURE. The first statement is quoted by the author (Dr. Binns) from Franklin's "Journey to the Polar Seas," p. 248:—"The fish froze as fast as they were taken out of the net, and in a short time became a solid mass of ice; and, by a blow or two of the hatchet, were easily split open, when the intestines might be removed in one lump. If, in the completely frozen state, they were thawed before the fire, they recovered their animation. We have seen a carp recover so far as to hop about with much vigour, after it had been frozen six-and-thirty hours."

The second statement is quoted from Isaak Walton ("Complete Angler," p. 257), who "quotes Gessner for the fact of some large breams being put into a pond, which was frozen the next winter into one mass of ice, so that not one could be found, and all swimming about again when the pond thawed in the spring," which phenomenon seemed to Walton "a thing almost as incredible as the resurrection of an atheist."

Dr. Binns further quotes, from the *Quarterly Review*, a statement which goes to show that *mosquitos* have been frozen on to the surface of a lake in the evening, and thawed again by the morning sun into animation. I should like, therefore, to inquire, firstly, whether there are any records of cold-blooded vertebrates other than fish being thus frozen and recovered to life; secondly, whether any such thing is known of the invertebrates, such as Mollusca, Echinodermata, and Vermes; and thirdly, whether this reported freezing and subsequent recovery of insects can be confirmed? In connection with this last class, I ought perhaps to mention that caterpillars have, I believe, been reported to recover animation after being frozen.

March 27. F. H. PERRY COSTE.

On the Presence of a Sternum in *Notidanus indicus*.

I HAVE just found that the omosternum of *Notidanus*, described by me in a recent number of NATURE (vol. xliii. p. 142), was originally described by my friend Prof. W. A. Haswell, of Sydney, who, in his paper "Studies on the Elasmobranch Skeleton" (Proc. Linn. Soc. N.S.W., vol. ix., 1884), has the following passage:—

"The shoulder-girdle is remarkable for the presence in the middle ventral line of a distinct four-sided lozenge-shaped cartilage let in to the arch, as it were, in front. This is a condition which I have not observed or seen described in any other form; it does not seem to occur either in *Heptanchus cinereus* or in *Henanchus griseus*. The intercepted cartilage is temptingly like a presternal, but the absence of such an element in any group nearer than the Amphibia seems to preclude this explanation."

Dr. Haswell gives no figure of the shoulder-girdle, but from the above description it would appear that the post-omosternum was absent in his specimen. T. JEFFERY PARKER.

Dunedin, N.Z., January 27.

Cackling of Hens.

I HAVE recently published the following letter in the *Field*; but not having so far received any answer to the question which it presents, I should like to republish it in your columns, in order to ascertain whether any ornithologists to whom I have not already applied may have any information to give upon the subject. GEORGE J. ROMANES.

Christ Church, Oxford, March 28.

Cackling of the Hens of Jungle Fowl.

"Can any of your readers inform me whether or not the hens of the wild jungle fowl (*Gallus bankiva*) cackle after laying their eggs, in the manner of their domesticated descendants? I cannot find any literature upon the subject; but if wild hens do cackle in the jungle, surely somebody must have heard them. Mr. A. P. Bartlett informs me that, when confined in shrubberies of the Zoological Gardens, they do not cackle; and, therefore, if nobody has ever heard them do so in a state of nature, we may fairly infer that the instinct is a product of domestication. If this should turn out to be the case, it would be a somewhat remarkable fact, and would, moreover, lead to the further question whether there are any parts of the world where domesticated poultry do not cackle."

Wood's Holl Biological Lectures.

A REVIEW of some lectures given by American naturalists at the Wood's Holl Laboratory appeared in your columns on March 19 (p. 457), signed with the initials "R. L." As there are statements made in that review for which I should much regret to be held responsible, I beg you to allow me to prevent any mistake which might arise from the similarity of the reviewer's initials to my own, and to state that the review was not written by me. E. RAY LANKESTER.

Bembridge, I.W., March 30.

New Comet.

AT 9 p.m. on Monday, March 30, I picked up a bright, round nebulous object in Andromeda which I could not identify. I soon found it in rapid motion to the south, and its cometary nature was therefore placed beyond a doubt.

The position of the object is approximately R.A. 14°, Decl. 43° north, and its motion is carrying it quickly towards the sun, so that it becomes important it should be immediately observed for place and its orbit computed. It will probably disappear in two or three weeks.

At 4.30 on the morning of Tuesday, March 31, the new comet was very distinctly visible, though the gibbous moon was shining brightly at the time.

At the end of the present week the comet will be near β Andromedæ, and a little sweeping in the region of this star will almost certainly reveal it, for it is too conspicuous to be overlooked even in a comparatively small telescope.

Bristol, March 31. W. F. DENNING.

THE ASTRONOMICAL CONGRESS.

OUR readers will be glad to know that the invitations issued by Admiral Mouchez to the Directors of the various Observatories interested in prosecuting the photographic chart of the heavens have met with a very ready response. It is confidently expected that this Congress will be the last that will be found necessary, and before the astronomers have separated it is hoped that the scheme will have received its final shape, and that no important detail will have escaped attention.

The English contingent is represented by the Astronomer-Royal, Dr. Gill from the Cape of Good Hope, Mr. Plummer from Oxford, Captain Abney, and Mr.

Knobel. Of German astronomers, while the Congress regrets the absence of Prof. Vogel, it will welcome from the Potsdam Observatory Dr. Scheiner. Prof. Kopteyn of Groningen is also expected. Prof. Bakhuyzen comes from Leyden. Italy is represented by Prof. Tacchini of the Collegio Romano, Padre Denza, the representative of the Vatican, and M. Riccò of Palermo. Russia sends two astronomers, Drs. Donner of Helsingfors and Belopalsky of Pulkova. The Chilian troubles have not prevented the Government of that country accrediting M. Maturana; while Brazil illustrates its interest in the scheme by despatching M. Beuf of Rio de Janeiro.

Of course the French astronomers are strongly represented: M. Rayet of Bordeaux, M. Trépied of Algiers, and M. Baillaud of Marseilles, represent the more distant French Observatories; while of the Paris men of science, in addition to MM. Mouchez and Janssen, we may confidently expect the co-operation of MM. Faye, Wolf, Cornu, and Henri *frères*.

An informal meeting was held on Saturday last at the Observatory to draw up a paper of agenda containing the points which it was thought that the Congress should consider. This does not exclude any other points which may prove of interest in the course of the debate.

The following programme, to be submitted to the General Congress, was drawn up:—

1. Verbal reports on the astrophotographic installations.
2. Exhibition of the photographs obtained, and the discussion of the most desirable point of exposure with regard to the focus.
3. The mode of printing the *réseau*.
4. Report on the various kind of plates and photographic processes and developments.
5. On the formation of a catalogue of guiding stars.
6. (*a*) On the desirability of increasing the distance of the guiding stars from the centre of the plate.
(*b*) On the best form of micrometer for the guiding telescope.
7. On the orientation of the plate.
8. Is it desirable that the plates for the catalogue and the chart be taken on the same evening?
9. To determine the length of the exposure in the catalogue plates, and whether it be desirable to increase it.
10. Is it desirable to make three exposures on the same plate?
11. To fix the minimum diameter which the 11th and 14th magnitude star images shall have on the plate.
12. To discuss the possibility of determining a relation between the diameter and time of exposure.
13. To obtain a definition of the 14th magnitude.
14. The method of measuring the negatives and the employment of the *réseau*.
15. To discuss
(*a*) The number of fundamental stars for each negative.
(*b*) The choice of these stars.
(*c*) The proper plan to assure the meridian observation of these stars.
16. On the method of reproduction of the plates for the chart.
17. (*a*) To discuss the desirability of a Central Bureau for measuring and discussing the plates.
(*b*) Shall Observatories possessing measuring instruments begin to measure at once?
18. To discuss when the work should be begun.
19. Should there be plates of longer exposure in the neighbourhood of the ecliptic?

No. 10 seems to require some explanation. It has been found by MM. Henry that the visibility of minute stars is much assisted if there be three exposures a line; and it is possible that a proposition will be made to do away with the very short exposures and to substitute three separated from each other in length of exposure by an equal or nearly equal interval of time.

PHOTOGRAPHIC PERSPECTIVE AND THE USE OF ENLARGEMENT.

It is not uncommon to hear it remarked that photographs make hills look low, or that they make things look "such a long way off"; and that they do so in a great many cases is perfectly true.

In explanation of the apparent lowness of photographed mountains, I have heard it suggested that the eye judges horizontal and vertical distances by different standards. and this, too, is probably the case; but since there is a horizontal and a vertical in a picture as well as in nature, the eye ought to form similar judgments on both.

The true meaning of the appearances alluded to, though they admit of a most simple explanation, is not as generally understood as might be expected.

The fact is that they depend merely on perspective.

In elementary books on drawing there often appears a diagram in which imaginary threads are supposed to be stretched from every point of an object, through an upright sheet of glass, and to intersect in some point behind it. The trace of these threads on the glass will there form a picture of the object which is in true perspective, when viewed from the intersection of the threads; and if the proper amount of light, shade, and colour be supposed to be added, this picture, to the single eye so placed, would be absolutely undistinguishable from the object itself.

But now suppose the eye is not at the place of intersection of the threads, but a certain distance further off or nearer to the glass. It is evident that the apparent angular magnitude of every object in the picture is altered in the ratio of the distance of the intersection of the threads to the distance of the eye from the glass. But this is exactly what would be the case, if, keeping the eye at the intersection of the threads, a new picture were formed on the glass either by altering the size of the real objects in this ratio, or their distance from the glass in the inverse ratio.

For instance, let the objects forming the picture be two towers, one say half a mile off and the other a mile, and suppose that the intersection of the threads is one foot behind the glass; to the eye placed at that distance the towers in the picture will subtend the same angle as they do in reality; but if the eye be moved a foot further from the glass these angles will be halved, and the same picture will then fall on the retina as would be formed there were the eye one foot from the glass and the towers only half their actual size, or if they were removed to the distances of one mile and two miles respectively.

Thus by viewing the picture from the wrong distance, either the apparent size of the objects represented by it is multiplied by the ratio $\frac{\text{true distance}}{\text{wrong distance}}$, or their apparent distances by $\frac{\text{wrong distance}}{\text{true distance}}$.

Putting this in symbols, for the sake of simplicity and brevity, we have, if D = true distance of an object from the point of view, A = its real linear magnitude, F = distance at which the picture must be viewed in order to convey a correct impression of D and A. Then if *d*, and *a*, are the values corresponding to D and A, when the picture is seen from the distance *f*, we have $d = \frac{f}{F} D$ when A is judged correctly; $a = \frac{F}{f} A$ when D is judged correctly. Of course both A and D may be misjudged, but apparent and true distances and sizes are still connected by the relation

$$ad = AD.$$

In a photograph, F is the focal length of the lens with which it was taken, and *f* the distance at which it is looked at. Thus, if, as is generally the case with all moderate sized pictures, the focal length of the lens is less than the distance one would naturally hold the picture at for convenient view, the inevitable result is either that the apparent distances of the picture are greater than the real ones in the proportion of $\frac{f}{F}$, or that the apparent sizes of the things represented in it are reduced in the proportion $\frac{F}{f}$, or a combination of both these wrong impressions is produced.

Which of these effects or what combination of them is suggested depends much on the nature of the picture itself.

In interiors taken with a wide-angle, short-focussed lens, distances are enormously exaggerated, while in landscapes it is generally the sizes of things which seem diminished.

As a rule, it may be said that objects which do not themselves suggest any scale will be made to look small, while those which do, such as men, houses, &c., will appear distant.

When $\frac{F}{f}$ is greater than unity, *i.e.* when the picture is viewed too near, the reverse of the above effects is seen; and as far as the perspective is concerned, the scene is being viewed through a telescope.

The magnifying power of a telescope is the focal length of the object-glass divided by the focal length of the eye-piece, or, in other words, the distance from the lens at which the image is formed divided by the distance from which it is viewed.

If the focal length of the eye-piece is the same as that of the object-glass, there is no magnification, and in the field of the telescope will be seen an exact reproduction of the natural view.

When, however, by shortening the focal length of the eye-piece, magnification is obtained, foreshortening of all the distances in the ratio $\frac{F}{f}$ naturally takes place.

This may be practically illustrated in rather a striking way by looking from a railway bridge along a straight piece of line at an approaching train.

Supposing the train to be travelling at forty miles per hour, if the telescopic power be forty, the apparent rate of approach will be only one mile per hour.

From what has been said, it will be clear that just the same laws apply to photographic pictures (or any pictures in true perspective) as to telescopic images, and that there is only one distance at which they will convey a correct impression to the eye.

This being so, it is evident that any photograph taken with a lens of less than about a foot focal length, must exaggerate all the distances, or make objects in the picture look smaller than they should, and the only remedy for this is to enlarge the picture until the right distance to view it from becomes also the convenient distance.

Even if this be done, however, there is still a tendency to view the picture too far off; for few lenses, except those for portraits embrace an angle so small as to be taken in at a single glance, and people are naturally inclined to stand far enough from a picture to see the whole of it at once.

Still, a proper amount of enlargement offers the best means of making a photograph give a true idea of the scene which it represents; and this is especially true of the small pictures taken by so-called "detective" cameras, having lenses varying from four to six inches in focal length; and it is for this end, and not, in general, to enable more detail to be seen, that the enlarging process is most useful.

Of course, negatives for enlargement must be well enough defined to bear being examined from the focal distance of the lens which took them, or less than this (since detail is lost in the enlarging process), and many which would pass muster well enough when held a foot or more off will be found imperfect when looked at from the lesser distance.

In a subsequent article I will, if the Editor permits, enter more fully on the subject of photographic definition and its limits, both as they depend on the nature of the various sensitive films, and on the lenses by which the image is formed.

A. Mallock.

ÉMILE GAUTIER.

Colonel Gautier was born on April 18, in the year 1822, at Geneva, where he made his first studies. When he had concluded his course at the ancient Academy, his tastes and natural talents inclined him to astronomy, the science to which he had been early initiated by his uncle, Alfred Gautier, then Director of the Observatory. In order to perfect himself in his studies, he went to Paris, where, thanks to the recommendations of his uncle, and more especially of Frederic Maurice, a member of the Institute, with whom he was on excellent terms, he entered the Observatory and became the pupil of the celebrated astronomer Leverrier. One of the early recollections that Émile Gautier loved particularly to recall was the time when he worked under the direction of the illustrious *savant* at the calculations of the perturbations of the planets; he went over again in duplicate all the calculations relative to the perturbations of Uranus, calculations which Leverrier had presented to the Academy of Sciences in November 1845.

After a stay of about two years in Paris, he returned to Geneva, and published in December 1846 a thesis prompted by the mathematical works in which he had taken part; it was entitled "An Essay on the Theory of the Perturbations of Comets." In the year 1847, he sojourned for several months in England, where he made the acquaintance of several eminent English astronomers, among whom were Airy, Challis, Adams, and perhaps others.

On his return he spent some months at Paris, where he made various astronomical researches, and determined among others the elements of the planet Metis. He afterwards returned to Geneva, where, in conjunction with Emile Plantamour, then Director, he worked at the Observatory, especially at magnetic observations.

In 1849 he married Mdlle. V. Sarazin Maurice, granddaughter of Frederic Maurice, already mentioned. He leaves two sons: the elder, Lucien, is at present Professor of Hebrew at Lausanne; the younger, Raoul, in the beginning of 1890 was nominated Professor of Astronomy at the University of Geneva, and succeeded his father at the commencement of the same year as the Director of the Observatory.

In the early part of the year 1850, pressed by Colonel Aubert and General Dupour, Gautier turned his attention more especially towards a military career, for which he possessed remarkable qualities. His advancement was rapid, and he was considered one of the best officers in the Swiss army. We need not here follow this part of his life, nor note the various administrative functions in connection with which he introduced useful innovations. This subject would lead us far from the limits of this article. We may say, nevertheless, that in his numerous occupations he never lost sight of astronomy, but constantly made himself acquainted with its current progress.

In 1860 he went to Spain to observe the total eclipse of the sun on July 18 at Taragona (Aragon), and gave an account of his observations in the *Archives des Sciences Physiques et Naturelles*, a publication in which the majority of his works were printed. About this time he had recognized the nature of solar prominences, and defended his opinion strenuously against astronomers who still regarded them as appendages of the moon. Afterwards he occupied himself chiefly with the constitution of the sun, and with the study of spots and prominences. He presented the Observatory with a direct-vision spectroscope by Hoffman, the instrument with which he pursued his researches up to the time when he was confined to his bed in October 1890.

On the death of Émile Plantamour, Gautier was nominated Director of the Observatory of Geneva, and from the outset displayed great activity in endowing this establishment with new instruments. In meteorology the eye observations have been completed by a self-recording barograph by Hipp, reversing thermometers by Negretti and Zambra, self-recording thermometer and hygrometer by the brothers Richard, and during the last year an anemograph by the same makers. The Meteorological Station of Great Saint-Bernard, which is a sub-station to the Observatory of Geneva, had been likewise supplied with a barograph. In chronometry, an important branch of the Observatory, apart from the usual tests installed by É. Plantamour, Gautier instituted two competitions for the studies of errors of compensation. He has presented the Observatory with an electrical pendulum of extreme accuracy made by Hipp.

If we speak of the man, all astronomers with whom he was acquainted, and they were many, will agree in saying that he was the type of a true gentleman. His cordial welcome, his frankness, his good nature, and his readiness to help, gained for him in succession the love of everyone. A day of great happiness for him was that on which the Astronomische Gesellschaft resolved to make Geneva the place of meeting in 1885; those who took part in this Congress were able to enjoy his good nature and frank hospitality, and yet a few days before he experienced a cruel grief.

Émile Gautier died from heart disease, by which he was carried suddenly away on the night of February 24-25.

A. R.

NOTES.

THE "Flora of British India" has reached the seventeenth part—in other words, the first part of the sixth volume has appeared, and Sir Joseph Hooker may be congratulated on having so nearly accomplished his great task. Since his retirement from the Directorship of Kew Gardens, Sir Joseph has worked single-handed, and several large families yet remain to be done, notably the grasses, which are very numerous, and, in some respects, more difficult than the petaloid monocots, and mainly so because the majority of the species have a much wider area of distribution, thus entailing more literary research. The last published part of "The Flora of British India" is of more than ordinary interest, inasmuch as it contains the conclusion of the descriptive account of the orchids of India. About 1400 species are described; they are referred to upwards of a hundred genera, and they constitute about ten per cent. of the flowering plants of that vast country. This is a larger proportion than that recorded for the rich orchid flora of Mexico and Central America. Among epiphytal, or tree orchids, the beautiful genus *Dendrobium* contributes upwards of 150 species, and *Habenaria* among ground orchids numbers 106 species. All lovers of orchids will welcome this masterly synopsis of the Indian species; and all botanists will wish the eminent author health to finish his great work.

THE Camera Club has issued the programme of the fifth annual Photographic Conference, which will be held in the theatre of the Society of Arts on Tuesday and Wednesday, April 7 and 8, under the presidency of Captain Abney. On Tuesday Captain Abney will deliver his presidential address, and papers will be read by Mr. L. Clark, Mr. Joseph Pennell, and the Rev. F. C. Lambert. Major Nott, Mr. C. V. Boys, and Sir H. T. Wood will read papers on Wednesday, and in the evening the members and their friends will dine together at the Criterion Restaurant. All photographers are invited to attend the Conference. The annual exhibition of photographs by members will be on view in the Club-house meeting-room, Charing Cross Road, from 10 a.m. to 4 p.m., and it will remain open for about six weeks.

THE Congress of German Geographers was opened in Vienna yesterday. A Geographical Exhibition in connection with the Congress was on view at the University. The exhibits consisted chiefly of photographs, maps, and charts.

THROUGH the death of Mr. Henry Groves, on March 1, at the age of fifty-six, of paralysis, the English colony at Florence has lost one of its most active scientific members. Mr. Groves had been in business in Florence as a pharmaceutical chemist for about thirty years, and had given the whole of his leisure time to the investigation of the flora, not only of Tuscany but of nearly the whole of Italy, of which he possessed an exceptional knowledge. His vast stores of information were always at the service of English and other visitors to Florence. His dried collection of Italian plants was probably unrivalled in extent, and has, we are informed, become the property of the Central Botanical Society of Tuscany.

ON Thursday last, about midday, two distinct shocks of earthquake were felt at Boscastle, North Cornwall. The first shock was very severe, shaking the windows and furniture in some of the houses. The second shock followed in about two minutes. It was not quite so severe, but was distinctly felt. Some people state that they felt the ground trembling under their feet for several seconds.

DR. H. WILD read a paper before the Imperial Academy of Sciences of St. Petersburg on January 16 last on the use of the electric light for photographic self-registering instruments. He finds that it is more economical than gas or petroleum, and gives more uniform and sharper curves. Also that it reduces the possibility of loss of continuous registration to a minimum, and completely prevents the disturbance caused by increase of temperature when gas or oil is used.

MR. C. CALLAWAY has submitted to the Shropshire County Council a report on technical education; and now the Staffordshire County Council has invited him to prepare a similar report on technical instruction in the district occupied by the industries of North Staffordshire.

MR. R. L. WEIGHTON has been appointed Professor of Engineering and Naval Architecture at the Durham College of Science, Newcastle-upon-Tyne.

DR. WILLIAM SOMERVILLE, having been appointed to the chair of agriculture and forestry recently founded in the Durham College of Science, Newcastle-upon-Tyne, will begin his duties early in the summer. The College has acquired 15 acres of land at Gosforth for the purposes of an experimental station, and it is hoped that smaller stations will be established in other parts of the district. It is the desire of the College that the members of the staff of its agricultural department should assist in the establishment of a system of agricultural education throughout the adjoining counties, partly by a system of "extension lectures" and partly by conducting special classes for teachers.

THE *Revue Biologique du Nord de la France* publishes a paper by M. J. de Guerne on the new steam yacht recently built in London for the Prince of Monaco. The *Princess Alice* is, in fact, a sailing vessel with an engine and screw-propeller to help it; the proprietor much prefers sailing to steaming. The boat contains three laboratories very well equipped for zoological work, as well as photography and oceanography; and a short trial cruise is to be made in the autumn.

AT the Royal Institution, Mr. J. Scott Keltie will on Tuesday, April 7, begin a course of three lectures on the geography of Africa, with special reference to the exploration, commercial development, and political partition of the continent; Prof. Dewar will on Thursday, April 9, begin a course of six lectures on recent spectroscopic investigations; and Prof. Silvanus P. Thompson will on Saturday, April 11, begin a course of four lectures on the dynamo. The Friday evening meetings will be resumed on April 10, when Sir William Thomson will give a discourse on electric and magnetic screening.

PROF. KARL PEARSON proposes to deliver at Gresham College lectures on the following subjects: April 14, the geometry of motion; April 15 and 16, matter and force; April 17, the classification of the sciences. These lectures begin at 6 o'clock p.m., and are free to the public.

THE following lectures on scientific subjects are to be given at the Royal Victoria Hall:—April 7, "Minute Things in Nature," by Prof. Rupert Jones; April 14, "Extinct Volcanoes of France," by Dr. Crosskey; April 21, "Mountain Exploration in the Caucasus," by Mr. Dent.

LAST August various gentlemen connected with agricultural industry in different parts of the United Kingdom were invited by the High Commissioner for Canada to visit the Dominion, to report upon its agricultural resources, and the advantages the country offers for the settlement of farmers and farm labourers. The reports of these gentlemen have now been issued. If published together, they would have made a rather bulky volume; so it was decided that they should be divided into four parts. They present a great mass of useful and well-arranged information, and are carefully illustrated.

THE *Revue Mensuelle de l'École d'Anthropologie de Paris*, of which three numbers have now been published, is likely to do much good work in extending among the educated classes of France a knowledge of anthropological science. Among the contributors are MM. G. de Mortillet, André Lefèvre, and C. Letourneau.

ACCORDING to a telegram sent through Dalziel's Agency, a magnificent grotto has been discovered near Ajaccio. It is entered with difficulty, owing to the smallness of the aperture; but upon his entrance the explorer finds himself in a vast and lofty hall, the sides of which are some 25 yards in height. From this there are several passages leading to an indefinite number of other chambers. A thorough investigation of the grotto has not yet been made.

THE following are some results of a study of 197 thunderstorms in Russia in 1888, with reference to their speed of travel. The author (Herr Schönrock) obtained as mean velocity about 28·5 miles an hour, with variation from 13 to 50 miles. In the hot season the velocity was less than in the cold (28 m. against 32 m.). It was least in the early morning, then increased, at first slowly, then faster, reaching a maximum between 9 and 10 p.m. Thunderstorms travel most quickly from south-west, west, and north-west. An interesting geographical difference was observed. From west to east the velocity increased at first; but about 30° to 35° east longitude a maximum was reached, and further east the speed declined; the decline showing, however, a secondary maximum between 45° and 50°.

THE Annual Report of the Berlin branch of the German Meteorological Society contains the results of rainfall observations at a number of stations in and near Berlin for the year 1890. This year is among the driest experienced since 1848, when regular observations were begun; the months of February and September, especially, are the driest on record. Dr. Hellmann, the Secretary, has carried on some useful experiments to determine the influence of the height of rain-gauges upon the records of rainfall—a matter of considerable importance in towns, owing to the difficulty of obtaining a good exposure at a low level. He finds that about a quarter of the rainfall is lost in an elevated exposure, such as on the roof of a house, during strong winds; but he arrives at the important conclusion that an elevated exposure is permissible if the gauge can be protected from the disturbing influence of the wind. The Report also contains a list of the severe winters since 1728. The coldest winter was 1788; on December 28 a minimum of $-21°·6$ was recorded.

A KNOWLEDGE of how water behaves with regard to passage of different light-rays through it is important, not only for determining the colour of the substance, but also for ascertaining what rays penetrate to the inhabitants of ocean depths. This matter has been studied lately by Herren Hüfner and Albrecht, with the aid of the new spectro-photometry (*Wied. Ann.*). Above or below the spectrum obtained directly from a beam of sunlight passing to the spectro-photometer, was thrown one from an equally strong beam which had passed through a water column of known length. The intensities of the two spectra were then compared in a series of sections by means of the polarization apparatus, and the coefficients of extinction determined. It was proved that the light-extinction by water is in general greater the longer the wave-length. But the curve of transmitted light has not a regular course; in the region of D and G, where broad absorption bands have been observed, it shows sudden rises.

ABOUT 18° C., according to Herr Kleinstrick (*Beibl.*), Japanese and ordinary wax sink in water, but above 18° they float. This is because the wax has a much greater coefficient of expansion than water, and under 18° it has a higher specific gravity.

THERE is reason to believe that the indications of tromometers are sometimes vitiated by wind. This matter has been lately investigated by M. Carcani, who made inquiry at four Observatories (Rome, Mestre, Florence, and Rocca di Papa), the tromometers of which were quite isolated from the floors or walls of buildings. At the last-named station, the instrument is in a cavern several metres under the surface. M. Carcani finds, in many cases, synchronism between the maxima of wind and of tremors; on an average, in 84 per cent. of the observations. As it is difficult to suppose this coincidence fortuitous, it is inferred that wind-pressure enters as an important factor into the movements of the tromometer. It is pointed out that, in the case of a tromometer isolated in a building, the oscillations caused in the latter by wind exert pressure on the ground, and so affect the instrument. In a case like that of Rocca di Papa, the direct pressure of the wind on sloping ground has to be considered; the vibrations so produced may be transmitted to some distance from their place of origin, like tremors of earthquake nature. Mr. Carcani finds, in this particular case, many instances of rise and fall in the tromometric intensity, corresponding with rise and fall of wind. In view of these facts, tromometric observations, he considers, should be made only when the wind is not very high.

WITH reference to observed changes in the earth's axis of rotation, it has been pointed out that through changes in distribution of air-pressure and movement of water masses, considerable differences of level in the ocean may be produced. Herr Lamp notes (*Astr. Nachr.*) the displacement northwards of the maxima of air-pressure in the trade-wind region, and of ocean currents, as the sun rises in summer. Thus a certain quantity of water passes over in summer from the southern to the northern hemisphere; and it is improbable that compensation takes place by means of under-currents. As the year advances, water passes back to the southern hemisphere, reaching there a maximum in our winter. This periodical transference of mass is supposed to cause periodical variation in the earth's axis. Herr Lamp calculates that to cause a change of latitude of $0''.5$, it would be sufficient that at 180° longitude from Berlin, a water-mass of 2500 cubic metres should move in a meridional direction from 30° S. lat. to 35° N. lat.; and that with reference to the oceanic area concerned, we need only suppose a mean elevation of 10 cm. (or 4 inches) in the sea-level.

THE Director of the Agricultural Experiment Station of the Agricultural and Mechanical College for the State of Alabama has issued his Bulletin No. 13, which is devoted to an exhaustive account of the different varieties of cotton grown in the State, by Mr. P. H. Mell, the botanist and meteorologist to the station. According to Mr. Mell, only three species of *Gossypium* are of special commercial importance, viz. (1) *G. Bahma*, or Egyptian cotton; (2) *G. barbadense* or *nigrum*, Sea-Island cotton, or long staple, or black-seed cotton; (3) *G. herbaceum* or *album*, short staple, or upland, or green-seed cotton. These three species have been multiplied into 20 or 30 so-called varieties, by certain kinds of cultivation and careful selection. *G. Bahma* is supposed to be originally a hybrid between the native Egyptian cotton-plant and a species of *Hibiscus*. The "Sea-Island" cotton requires a salt atmosphere, and is mainly used in the manufacture of lace. Mr. Mell gives the microscopic characteristics of 25 varieties of cotton, and his descriptions are accompanied by photographic illustrations made with a photo-micro camera and micrometer. The Bulletins are supplied free on application to any citizen of the State.

ACCORDING to the *Japan Mail*, the Japanese Government has made an award of $1000 to Dr. Shohei Tanaka, a graduate in science of the Tokio University and of the Berlin University, for the invention of a new musical instrument, as to which the following information is given. In Germany, Dr. Tanaka devoted himself specially to the study of sound and of music, and is no doubt the first Japanese who has obtained an intimate knowledge of Western music on its practical, theoretical, scientific, and historic sides. On the purely scientific side he has added to our knowledge of the laws of vibrations of plates, and has also communicated to musical literature several papers of interest. One of these contains an account of a harmonium which he has devised, and which is tuned in practically pure intonation. From a cursory glance at the contents of this pamphlet, it is difficult to pick out the really original matter. Judging from the references and foot-notes, the author has read widely, and appears to be warranted in claiming to be the first who has constructed a keyed instrument capable of giving practically pure chords in all the usual keys, and of being played almost exactly as a piano or an organ is played. The manual is, to a first glance, very similar in appearance to the ordinary organ or piano manual. But a closer inspection shows that a short black note is introduced between E and F, and that the other black notes are divided into two or even three. In all, there are twenty distinct notes within the compass of an octave, instead of the usual twelve in our instruments of equal temperament. Dr. Tanaka's white notes are tuned to the perfect major scale of C, the E being therefore considerably flatter than the note of the same name on the piano. If it is desired to play on the scale of D, this E, the true major third to C, must not be used. A slightly but appreciably sharper note must be used, and this is inserted between D and E, in front of the ordinary black note known as D sharp or E flat. Strictly speaking, as on Bosanquet's organ for instance, D sharp and E flat are really different notes, but the difference is too slight to be practically appreciable. In Dr. Tanaka's scheme, however, the requirements of modern transposition in music necessitate a C sharp distinct from D flat.

THE additions to the Zoological Society's Gardens during the past week include a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by Mrs. Sutton Sams; a Sooty Mangabey (*Cercocebus fuliginosus* ♂) from West Africa, presented by Miss Kathleen Hill; an Indian Civet (*Viverricula malaccensis*), two Malabar Squirrels (*Sciurus maximus*) from India, presented by Colonel A. Bloomfield; a Two-spotted Paradoxure (*Nandinia binotata*) from West Africa, presented by Dr. J. Galbraith Westlake; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Charles C. Barton; five Summer Ducks (*Æx sponsa* 5 ♀) from North America, four Gadwalls (*Chaulelasmus streperus* ♂ ♂ ♀ ♀), European, purchased; a Black-headed Lemur (*Lemur brunneus*), born in the Gardens.

SCIENCE IN NEW ZEALAND.

THE following is the Presidential address delivered by Sir James Hector, at the recent meeting of the Australasian Association for the Advancement of Science:—

When I rashly replied in the affirmative to the cablegram which I received from our Secretary in Melbourne, asking me to undertake the honourable and responsible duties which I have to commence this evening, I fear I did not fully realize the difficulties of the position, but since then the sense of my unfitness for the task has become very oppressive. To address an assembly of this kind on general science must involve unusual difficulties, owing to the audience being largely composed of those who, only taking a casual interest in scientific discussions, look chiefly to the results; while, at the same time, there are present professional specialists in almost every branch of knowledge. I feel that on this occasion I must be ruled by the interest of the majority, and claim the forbearance of my fellow-workers in science if I have to refer in a sketchy way to subjects in which they are deeply interested, and far more learned than I profess to be.

Seeing that I am addressing a Christchurch audience, I hope I may be permitted, in the first place, to say a word concerning one whose scientific services should, without doubt, have obtained for him the position of first President in New Zealand of the Australasian Association. We naturally recall the name of Sir Julius Von Haast on this occasion, and mourn for the loss the colony has sustained of one who for thirty years occupied a most prominent position. His early researches in the North Island in company with Von Hochstetter, were followed by the exploration of the remote districts on the west coast of Nelson, after which Canterbury secured his distinguished services, and enabled him to leave that monument of his varied scientific knowledge, shrewd capacity, and indefatigable industry which is to be found in the Canterbury Museum. There are others of our fellow-colonists whose wide range of experience would have peculiarly fitted them to act as your President, and I am able to say that had our veteran colonist and explorer Sir George Grey felt more assured in health and strength, it would have been your pleasure this evening to listen to a flood of eloquence on all scientific topics that relate to the future development of Australasia. There is another name I feel must be mentioned as one who should have been in this position had his health permitted. I refer to the Rev. William Colenso, who is not only the greatest authority on the folk-lore of the Maoris, on whom he was among the first to confer a printed literature in their own language. His long-continued work as a field naturalist, and especially as a botanist, is exceedingly interesting, seeing that it forms a connecting link that has continued the early spirit of natural history research in New Zealand, that commenced with Banks and Solander, and was continued by Menzies, Lesson, the two Cunninghams, and Sir Joseph Hooker, prior to the arrival of colonists. Thus we still have in my esteemed friend, Mr. Colenso, an active veteran naturalist of what we may call the old school of explorers.

It is wonderful to reflect that little more than fifty years ago this European colony was represented by a few fishing hamlets on the seaboard of a country occupied by a considerable native population. To the early explorers, and even down to a much later date, the obstacles that beset their path were very different from those of the present time: often obstructive natives, no roads, no steamers, no railways. Had an Association then existed and desired to promote science by giving our visitors an opportunity of visiting the remote parts of the islands, the same excursions which have on this occasion been planned to occupy a few days, would have occupied as many months, and then been accomplished only with great hardship and difficulty. I must ask the young and rising generation of colonial naturalists to bear this in mind when they have to criticize and add to the work of their predecessors. Such names of early colonists as Bidwill, Sinclair, Monro, Mantell, Travers, and many others should ever be held in esteem as those who, amidst all the arduous trials of early colonization, never lost sight of their duty towards the advancement of science in New Zealand. I will not attempt to particularize other names from amongst our existing, and, though small in number, very active corps of scientific workers. They are here, or should be, to speak for themselves in the sectional work; and I have no doubt some of those who did me the great honour of placing me in my present position are secretly congratulating themselves that they have secured for themselves the position of free lances on this occasion.

This is now the third annual gathering of this Association, and New Zealand should feel honoured that it has at so early a date in the Association's history been selected to the turn in rotation as the place of meeting among so many divisions of the great colony of Australasia. The two volumes of the Transactions of the Association, already in the hands of members, are quite sufficient to prove that the hopes of its founders, or rather, I may almost say, the founder—Prof. Liversidge, of Sydney—have been amply fulfilled. The papers read before the different Sections, and the addresses delivered, have, in my opinion, to a most remarkable extent embodied information and discussions which were not likely to have been produced as the result of any of our local scientific organizations. The authors seemed to have felt it incumbent on them to place their subjects in the environment of Australasia, and not in relation to the colony they happened to represent. This, I take it, is the first truly effective step towards Federation which has yet been achieved, and I trust that all our members will continue to be imbued with this spirit. Politicians should take this well to heart. Let them continue to aid all efforts that will tend to bring scientific accumulations in these colonies into a common store, so that each may discover for what purpose it has been best adapted by nature, and by paying proper political respect in fiscal policy to one another, each may prosper to the full extent of its natural advantages.

But it is not alone in the value of the papers communicated the Association contributes to advance true civilization in the colonies. The face to face conference, the personal contact of the active workers in different lines of scientific work, must greatly facilitate the more thorough understanding of the work which has been done and which is still undone. A vague idea, simmering in the brain of one man of science, who thinks light of it because it has no special application in his particular environment, may, by personal converse, flash into important results in the mind of another who has had the difficulties facing him, but without the happy thought occurring. It would be rather interesting for someone with leisure to endeavour to recount how many great discoveries have eventuated in this manner.

In casting my thoughts for a particular subject on which to address the Association I felt perplexed. Presidents of similar Associations in the Old World, who are in constant contact with the actual progress in scientific thought, feel that a mere recital of the achievements during their previous term is sufficient to command interest; but in the colonies most of us are cut off from personal converse with the leading minds by whom the scientific afflatus is communicated; and in our suspense for the tardy arrival of the official publications of the Societies, we have to feed our minds with science from periodical literature. But even in this respect my own current education is very defective, as I reside in the capital city of New Zealand, which has no College with a professorial staff whose duty, pleasure, and interest it is to maintain themselves on a level with the different branches of knowledge they represent. I therefore decided that, instead of endeavouring to review what had been done in the way of scientific progress, even in Australasia, it would be better to confine my remarks to New Zealand—the more so that this is the first occasion that there has been a gathering of what must, to some extent, be considered to be an outside audience for the colony. To endeavour to describe, even briefly, the progress made in the science of a new country, is, however, almost like writing its minute history. Every step in its reclamation from a wild state of nature has depended on the application of scientific knowledge, and the reason for the rapid advance made in these colonies is chiefly to be attributed to their having had the advantage of all modern resources ready to hand.

As in most other matters in New Zealand, there is a sharp line dividing the progress into two distinct periods, the first before and the second after the formation of the colony in 1840. With reference to the former period it is not requisite that much should be said on this occasion. From the time of Captain Cook's voyages, owing to his attractive narrative, New Zealand acquired intense interest for naturalists. His descriptions of the country and its productions, seeing that he only gathered them from a few places where he landed on the coast, are singularly accurate. But I think rather too much is sometimes endeavoured to be proved from the negative evidence of his not having observed certain objects. As an instance, it has been asserted that if any of the many forms of the moa still survived, Captain Cook must have been informed of the fact. Yet we find that he lay for weeks in Queen Charlotte Sound, and in Dusky Sound, where all night long the cry of the kiwi must have been heard just as now, and that he also obtained and took home mats and other articles of native manufacture, trimmed with kiwis' skins; and that most likely the mouse-coloured quadruped which was seen at Dusky Sound by his men when clearing the bush was only a gray kiwi; and yet the discovery of this interesting bird was not made till forty years after Cook's visit. As a scientific geographer Captain Cook stands unrivalled, considering the appliances at his disposal. His longitudes of New Zealand are wonderfully accurate, especially those computed from what he called his "rated watches," the first type of the modern marine chronometer, which he was almost the first navigator to use. The result of a recent measurement of the meridian difference from Greenwich by magnetic signals is only two geographical miles east of Captain Cook's longitude. He also observed the variation and dip of the magnetic needle, and from his record it would appear that during the hundred years which elapsed up to the time of the *Challenger's* visit, the south-seeking end of the needle has changed its position $2\frac{1}{2}°$ westward, and inclines $1\frac{1}{2}°$ more towards the south magnetic pole. Captain Cook also recorded an interesting fact, which, so far as I am aware, has not been since repeated or verified in New Zealand. He found that the pendulum of his astronomical clock, the length of which had been adjusted to swing true seconds at Greenwich, lost at the rate of 40 seconds daily at Ship Cove in Queen Charlotte Sound. This is, I believe, an indication of a greater loss of the attraction of gravity than would occur in a corresponding north latitude.

The additions to our scientific knowledge of New Zealand acquired through the visits of the other exploring ships of early navigators, the settlement of sealers and whalers on the coast, and of Pakeha Maoris in the interior, were all useful, but of too slight a character to require special mention. The greatest additions to science were made by the missionaries, who, in the work of spreading Christianity among the natives, had the services of able and zealous men, who mastered the native dialects, reduced them to a written language, collected and placed on record the traditional knowledge of the interesting Maori, and had among their numbers some industrious naturalists who never lost an opportunity of collecting natural objects. The history of how the country, under the mixed influences for good and for evil which prevailed almost without Government control until 1840, gradually was ripened for the colonist, is familiar to all.

The new era may be said to have begun with Dieffenbach, a naturalist who was employed by the New Zealand Company. He travelled and obtained much information, but did not collect to any great extent, and, in fact, appears not to have anticipated that much remained to be discovered. For his conclusion is that the smallness of the number of the species of animals and plants then known—about one-tenth of our present lists—was not due to want of acquaintance with the country, but to paucity of life forms. The chief scientific value of his published work is in the appendix, giving the first systematic list of the fauna and flora of the country, the former being compiled by the late Dr. Gray, of the British Museum. The next great scientific work done for New Zealand was the Admiralty survey of the coast-line, which is a perfect marvel of accurate topography, and one of the greatest boons the colony has received from the mother country. The enormous labour and expense which was incurred on this survey at an early date in the history of the colony is a substantial evidence of the confidence in its future development and commercial requirements which animated the Home Government. On the visit of the Austrian exploring ship *Novara* to Auckland in 1859, Von Hochstetter was left behind, at the request of the Government, to make a prolonged excursion to the North Island and in Nelson; and he it was who laid the foundation of our knowledge of the stratigraphical geology of New Zealand. Since then the work of scientific research has been chiefly the result of State surveys, aided materially by the zeal of members of the New Zealand Institute, and of late years by an increasing band of young students, who are fast coming to the front under the careful science training that is afforded by our University Colleges.

In the epoch of their development the Australasian colonies have been singularly fortunate. The period that applies to New Zealand is contemporaneous with the reign of Her Majesty, which has been signalized by enormous strides in science. It has been a period of gathering into working form immense stores of previously acquired observation and experiment, and of an escape of the scientific mind from the trammels of superstition and hazy speculation regarding what may be termed common things. Laborious work had been done, and many grand generalizations had been formerly arrived at in physical science; but still, in the work of bringing things to the test of actual experiment, investigators were still bound by imperfect and feeble hypotheses and supposed natural barriers among the sciences. But science is one and indivisible, and its subdivisions, such as physics, chemistry, biology, are only matters of convenience for study. The methods are the same in all, and their common object is the discovery of the great laws of order under which this universe has been evoked by the great Supreme Power.

The great fundamental advance during the last fifty years has been the achievement of far-reaching generalizations, which have provided the scientific worker with powerful weapons of research. Thus the modern "atomic theory," with its new and clearer conceptions of the intimate nature of the elements and their compounds that constitute the earth and all that it supports, has given rise to a new chemistry, in which the synthetical or building-up method of proof is already working marvels in its application to manufactures. It is, moreover, creating a growing belief that all matter is one, and reviving the old idea that the inorganic elementary units are merely centres of motion specialized in a homogeneous medium, and that these units have been continued on through time, but with such individual variations as give rise to derivative groups, just as we find has been the case in the field of organic creations. The idea embodied in this speculation likens the molecule to the vortex rings which Helmholtz found must continue to exist for ever, if in a perfect fluid free from all friction they are once generated as a result of impacting motion. There is something very attractive in the simplicity of the theory of the constitution of matter which has been advocated by Sir William Thomson. He illustrates it by likening the form of atoms to smoke rings in the atmosphere, which, were they only formed under circumstances such as above described, such vortex atoms must continue to move without changing form, distinguished only from the surrounding medium by their motion. As long as the original conditions of the liquid exist they must continue to revolve. Nothing can separate, divide, or destroy them, and no new units can be formed in the liquid without a fresh application of creative impact. The doctrine of the conservation of energy is a second powerful instrument of research that has developed within our own times. How it has cleared away all the old cobwebs that formerly encrusted our ideas about the simplest agencies that are at work around us; how it has so simplified the teaching of the laws that order the conversion of internal motions of bodies into phases which represent light, heat, electricity, is abundantly proved by the facility with which the mechanicians are every day snatching the protean forms of energy for the service of man with increasing economy.

These great strides which have been made in physical science have not as yet incited much original work in this colony. But now that physical laboratories are established in some degree at the various College centres, we shall be expected, ere long, to contribute our mite to the vast store. In practical works of physical research, we miss in New Zealand the stimulus the sister colonies receive from their first-class Observatories, supplied with all the most modern instruments of research, wielded by such distinguished astronomers as Ellery, Russell, and Todd, whose discoveries secure renown for their separate colonies. I am quite prepared to admit that the reduplication of Observatories in about the same latitude, merely for the study of the heavenly bodies, would be rather a matter of scientific luxury. The few degrees of additional elevation of the South Polar region which would be gained by an Observatory situated even in the extreme south of New Zealand could hardly be expected to disclose phenomena that would escape the vigilance of the Melbourne Observatory. But star-gazing is only one branch of the routine work of an Observatory. It is true that we have a moderate but efficient Observatory establishment in New Zealand sufficient for distributing correct mean time, and that our meridian distance from Greenwich has been satisfactorily determined by telegraph; also, thanks to the energy and skill of the Survey Department, despite most formidable natural obstructions, the major triangulation and meridian circuits have established the basis of our land survey maps on a satisfactory footing, so that subdivision of the land for settlement, and the adoption and blending of the excellent work done by the provincial Governments of the colony, is being rapidly overtaken. Further, I have already recalled how much the colony is indebted to the mother country for the completeness and detail of the coastal and harbour charts.

But there is much work that should be controlled by a physical Observatory that is really urgently required. I may give a few illustrations. The tidal movements round the coast are still imperfectly ascertained, and the causes of their irregular variations can never be understood until we have a synchronous system of tide-meters, and a more widely extended series of deep-sea soundings. Excepting the *Challenger* soundings on the line of the Sydney cable, and a few casts taken by the United States ship *Enterprise*, the depth of the ocean surrounding New Zealand has not been ascertained with that accuracy which many interesting problems in physical geography and geology demand. It is supposed to be the culmination of a great submarine plateau; but how far that plateau extends, connecting the southern islands towards the great Antarctic land, and how far to the eastward, is still an unsolved question. Then, again, the direction and intensity of the magnetic currents in and around New Zealand require further close investigation, which can only be controlled from an Observatory. Even in the matter of secular changes in the variation of the compass we find that the marine charts instruct that an allowance of increased easterly variation of 2′ per annum must be made, and as this has now accumulated since 1850, it involves a very sensible correction to be adopted by a shipmaster in making the land or standing along the coast; but we find from the recently published work of the *Challenger* that this tendency to change has for some time back ceased to affect the New Zealand area, and as the deduction appears only to have been founded on a single triplet observation of the dip taken at Wellington and one azimuth observation taken off Cape Palliser, it would be well to have this fact verified. With regard to the local variation in the magnetic currents on land and close in shore, the requirement for exact survey is even more imperative. Captain Creak, in his splendid essay, quotes the observations made by the late Surveyor-General, Mr. J. T. Thomson, at the Bluff Hill, which indicate that a compass on the north side was deflected more than 9° to the west, while on the east side of the hill the deflection is 46° to the east of the average deviation in Foveaux Strait. He adds that if a similar island-like hill happened to occur on the coast, but submerged beneath the sea to a sufficient depth for navigation, serious accidents might take place, and he instances a case near Cossack, on the north coast of Australia, when H.M.S. *Medea*, sailing on a straight course in eight fathoms of water, experienced a compass deflection of 30° for the distance of a mile. A glance at the variation entered on the meridian circuit maps of New Zealand shows that on land we have extraordinary differences between different trigonometrical stations at short distances apart. For instance, in our close vicinity, at Mount Pleasant, behind Godley Head lighthouse, at the entrance to Lyttelton Harbour, the variation is only 9° 3′ east, or 6° less than the normal; while at Rolleston it is 15° 33′, and at Lake Coleridge 14° 2′. In Otago we have still greater differences recorded, for we find on Flagstaff Hill, which is an igneous formation, 14° 34′; while at Nenthorn, thirty miles to the north, in a schist formation, we find an entry of 35° 41'.

In view of the fact that attention has been recently directed to the marked effects on the direction and intensity of the terrestial magnetic currents of great lines of fault along which movements have taken place, such as those which bring widely different geological formations into discordant contact, with the probable production of mineral veins, this subject of special magnetic surveys is deserving of being undertaken in New Zealand. In Japan and in the United States of America the results have already proved highly suggestive. A comparison between this country and Japan by such observations, especially if combined with systematic and synchronous records by modern seismographic instruments, would be of great service to the physical geologist. There are many features in common, and many quite reversed in the orographic and other physical features of these two countries. Both are formed by the crests of great earth waves lying north-east and south-west, and parallel to, but distant from, continental areas, and both are traversed by great longitudinal faults and fissures, and each by one great transverse fault. Dr. Naumann, in a recent paper, alludes to this in Japan as the *Fossa Magna*, and it corresponds in position in relation to Japan with Cook Strait in relation to New Zealand. But the *Fossa Magna* of Japan has been filled up with volcanic products, and is the seat of the loftiest active volcano in Japan. In Cook Strait and its vicinity, as you are aware, there are no volcanic rocks, but there and southward, through the Kaikouras, evidence of fault movements on a larger scale is apparent, and it would be most interesting to ascertain if the remarkable deviations from the normal, in direction and force, of the magnetic currents, which are experienced in Japan, are also found in New Zealand. For it is evident that, if they are in any way related to the strain of cross fractures in the earth's crust, the observation would tend to eliminate the local influence of the volcanic rocks which are present in one case and absent in the other. With reference to earthquakes, also, few, if any, but very local shocks experienced in New Zealand have originated from any volcanic focus we are acquainted with, while a westerly propagation of the ordinary vibrations rarely passes the great fault that marks the line of active volcanic disturbance. In Japan, also, out of about 480 shocks which are felt each year in that country, each of which, on an average, shakes about 1000 square miles, there are many that cannot be ascribed to volcanic origin.

There are many other problems of practical importance that can only be studied from the base-line of a properly equipped Observatory. These will readily occur to physical students, who are better acquainted with the subject than I am. I can only express the hope that the improved circumstances of the colony will soon permit some steps to be taken. Already in this city, I understand, some funds have been subscribed. As an educational institution, to give practical application to our students in physical science, geodesy and navigation, it would clearly have a specific value that would greatly benefit the colony.

Another great branch of physical science, chemistry, should be of intense interest to colonists in a new country. Much useful work has been done, though not by many workers. The chief application of this science has been naturally to promote the development of mineral wealth, to assist agriculture, and for the regulation of mercantile contracts. I cannot refrain from mentioning the name of William Skey, analyst to the Geological Survey, as the chemist whose researches during the last twenty-eight years have far surpassed any others in New Zealand. Outside his laborious official duties he has found time to make about sixty original contributions to chemical science, such as into the electrical properties of metallic sulphides—the discovery of the ferro-nickel alloy *awaruite* in the ultra-basic rocks of West Otago, which is highly interesting as it is the first recognition of this meteoric-like iron as native to our planet—the discovery that the hydrocarbon in torbasic and the gas shales is chemically and not merely mechanically combined with the clay base—of a remarkable colour test for the presence of magnesia, and the isolation of the poisonous principle in many of our native shrubs. His recent discovery, that the fatty oils treated with aniline form alkaloids, also hints at an important new departure in organic chemistry. His suggestion of the hot-air blow-pipe, and of the application of cyanide of potassium to the saving of gold, and many other practical applications of his chemical knowledge, are distinguished services to science, of which New Zealand should be proud. In connection with the subject of chemistry, there is a point of vast importance to the future of the pastoral and agricultural interests of New Zealand, to which attention was directed some years ago by Mr. Pond, of Auckland. That is, the rapid deterioration which the soil must be undergoing by the steady export of the constituents on which plant and animal life must depend for nourishment. He calculated that in 1883 the intrinsic value of the fixed nitrogen and phosphoric acid and potash sent out annually was £592,000, taking into account the wool and wheat alone. Now that we have to add to that the exported carcasses of beef and mutton, bones and all, the annual loss must be immensely greater. The proper cure would, of course, be to bring back return cargoes of artificial manure, but even then its application to most of our pastoral lands would be out of the question. I sincerely hope that the problem will be taken in hand by the Agricultural College at Lincoln as a matter deserving of practical study and investigation.

I have already referred to several great generalizations which have exercised a powerful influence in advancing science during the period I marked out for review, but so far as influencing the general current of thought, and almost entirely revolutionizing the prevalent notions of scientific workers in every department of knowledge, the most potent factor of the period has been the establishment of what has been termed the doctrine of evolution. The simple conception of the relation of all created things by the bond of continuous inheritance has given life to the dead bones of an accumulated mass of observed facts, each valuable in itself, but, as a whole, breaking down by its own weight. Before this master-key was provided by the lucid instruction of Darwin and Wallace, it was beyond the power of the human mind to grasp and use in biological research the great wealth of minute anatomical and physiological details. The previous ideas of the independent creation of each species of animal and plant in a little Garden of Eden of its own must appear puerile and absurd to the young naturalists of the present day; but in my own College days, to have expressed any doubt on the subject would have involved a sure and certain pluck from the examiner. I remember well that I first obtained a copy of Darwin's "Origin of Species" in San Francisco when on my way home from a three years' sojourn among the Red Indians in the Rocky Mountains. Having heard nothing of the controversies, I received the teaching with enthusiasm, and felt very much surprised on returning to my *alma mater* to find that I was treated as a heretic and a backslider. Nowadays it is difficult to realize what all the fuss and fierce controversy was about, and the rising school of naturalists have much cause for congratulation that they can start fair on a well-assured logical basis of thought, and steer clear of the many complicated and purely ideal systems which were formerly in vogue for explaining the intentions of the Creator and for torturing the unfortunate students. The doctrine of evolution was the simple-minded acceptance of the invariability of cause and effect in the organic world as in the inorganic; and to understand his subject in any branch of natural science, the learner has now only to apply himself to trace in minutest detail the successive steps in the development of the phenomena he desires to study.

With energetic leaders educated in such views, and who, after their arrival in the colony, felt less controversial restraint, it is not wonderful that natural history, and especially biology, should have attracted so many ardent workers, and that the results should have been so good. A rough test may be applied by comparing the number of species of animals and plants which had been described before the foundation of the colony and those up to the present time. In 1840, Dr. Gray's list in Dieffenbach's work gives the number of described species of animals as 594. The number now recognized and described is 5498. The number of Mammalia has been doubled, through the more accurate study of our seals, whales, and dolphins. Then the list of birds has been increased from 84 to 195, chiefly through the exertions of Sir Walter Buller, whose great standard work on our avifauna has gained credit and renown for the whole colony. The number of fishes and Mollusca has been more than trebled, almost wholly by the indefatigable work of our Secretary, Prof. Hutton. But the greatest increase is in the group which Dr. Gray placed as Annulosa, which, chiefly through the discovery of new forms of insect life, has risen from 156 in 1840 to 4295, of which over 2000 are new beetles described by Captain Broun, of Auckland.

When we turn to botany, we find that Dieffenbach, who appears to have carefully collected all the references to date in 1840, states the flora to comprise 632 plants of all kinds, and, as I have already mentioned, did not expect that many more would be found. But by the time of the publication of Hooker's "Flora of New Zealand" (1863), a work which has been of inestimable value to our colonists, we find the number of indigenous plants described had been increased to 2456. Armed with the invaluable guidance afforded by Hooker's "Handbook," our colonial botanists have renewed the search, and have since then discovered 1469 new species, so that our plant census at the present date gives a total of 3355 species. It would be impossible to make mention of all who have contributed to this result as collectors, and hardly even to indicate more than a few of those to whom science is indebted for the description of the plants. The literature of our post-Hookerian botany is scattered about in scientific periodical literature, and, as Hooker's "Hand-book," is now quite out of print, it is obvious that, as the new discoveries constitute more than one-third of the total flora, it is most important that our young botanists should be fully equipped with all that has been ascertained by those who have preceded them. I am glad to be able to announce that such a work, in the form of a new edition of the "Hand-book of the Flora of New Zealand," approved by Sir Joseph Hooker, is now in an advanced state of preparation by Prof. Thomas Kirk, who has already distinguished himself as the author of our "Forest Flora." Mr. Kirk's long experience as a systematic botanist, and his personal knowledge of the flora of every part of the colony, acquired during the exercise of his duties as Conservator of Forests, point to him as the fitting man to undertake the task.

But quite apart from the work of increasing the local collections which bear on biological studies, New Zealand stands out prominently in all discussions on the subject of geographical biology. It stands as a lone zoological area, minute in area, but on equal terms as far as regards the antiquity and peculiar features of its fauna, with nearly all the larger continents in the aggregate. In consequence of this, many philosophical essays—such, for instance, as Hooker's introductory essay to the early folio edition of the "Flora," the essays by Hutton, Travers, and others, and also the New Zealand references in Wallace's works, have all contributed essentially to the vital question of the causes which have brought about the distribution and geographical affinities of plants and animals, and have thus been of use in hastening the adoption of the doctrine of evolution. But much still remains to be done. Both as regards its fauna and its flora, New Zealand has always been treated too much as a whole quantity, and in consequence percentage schedules prepared for comparing with the fauna and flora of other areas fail from this cause. It is absolutely necessary to discriminate not only localities, but also to study more carefully the relative abundance of individuals as well as of species before instituting comparisons. The facility and rapidity with which change is effected at the present time should put us against rashly accepting species which may have been accidental intruders, though wafted by natural causes, as belonging to the original endemic fauna or flora. Further close and extended study, especially of our marine fauna, is urgently required. We have little knowledge beyond the littoral zone, except when a great storm heaves up a gathering of nondescript or rare treasure from the deep. Of dredging we have had but little done, and only in shallow waters, with the exception of a few casts of the deep-sea trawl from the *Challenger*. When funds permit, a zoological station for the study of the habits of our sea fishes, and for the propagation of such introductions as the lobster and crab, would be advantageous. I observe that lately such an establishment has been placed on the Island of Mull, in Scotland, at a cost of £400, and that it is expected to be nearly self-supporting. With respect to food-fishes, and still more with respect to some terrestrial forms of life, we, in common with all the Australian colonies, require a more scientific and a less casual system of acclimatization than we have had in the past. One must talk with bated breath of the injuries that have been inflicted on these colonies by the rash disturbance of the balance of nature. Had our enthusiasm been properly controlled by foresight, our settlers would probably not have to grieve over the losses they now suffer through many insect pests, through small birds and rabbits, and which they will in the future suffer through the vermin that are now being spread in all directions.

Sir James Hector then went on to say that there were many other points that he had intended to touch upon, but all had been forestalled by the remarks of His Excellency the Governor and Mr. Goodale. He was the better pleased that these gentlemen had spoken upon them, as they were remarks relating to the advantages of this Association. He felt, however, that he would like to have given a description of what had been ascertained relating to the geology of New Zealand. He might state that the early explorers appeared to have had only most vague ideas of the geologies of the countries they explored. Indeed, the whole science of geology seemed to have been almost brought into existence during the last fifty or fifty-five years. It existed only as drawing its knowledge from other branches of science; it barely existed as a science until these branches had become established. In New Zealand our geological explorations had been since the matters he had referred to had been settled, and the result had been that we had attained more rapidly, competent and tolerable, more complete knowledge of the structure of the country. New Zealand was probably the outcrop of a great earth-wave, the hollow of which formed the submarine plateau lying to the east. New Zealand appeared to have first originated as dry land in the Palæozoic times, merely as volcanic islands rising in a sea of moderate depth. After the Palæozoic period there appeared to have been a great blank in the geological formation. It was a period during which no deposits took place, and it was probable that all which had been deposited was removed. He went on to trace the various formations, referred to the first traces found of the moa at Timaru, and then leaving that subject stated that at the sectional meeting on ethnology there would be presented to the Association the first proof-sheets of a great lexicon of the languages spoken in the Pacific Islands, especially by the natives of the Sandwich Islands, of Tonga, and of New Zealand. It was being prepared by Mr. Tregear, one of the most profound workers in New Zealand in Maori mythology.

There was another subject on which he would like to touch. It was concerning the great Antarctic continent, but as he understood that Baron von Mueller wished the discussion of it should be deferred for Saturday forenoon, he would say no more upon it. He had to apologize for the very feeble manner in which he had attempted to discharge his duties, though he had the most perfect confidence in the success of the Association. He thought it was about twenty-four years since Mr. Travers got the Act passed which established the New Zealand Institute. In a small way it was an association of scientific men, and it was founded to absorb and render permanent the active endeavours in all parts of the colony to advance science. How well it had succeeded was known by all. Baron Von Mueller had kindly attributed its success to him, but he must really disclaim that, and say its success was due to the wise framer of the special Act. He hoped to see the colonies united together as one whole in this matter; the whole of the Australasian colonies were not too large to combine for the purpose, and he hoped that the inclusion of New Zealand in the magic circle would come about in time. In conclusion he expressed a wish that the visitors might have a pleasant sojourn in New Zealand; and trusted that he had succeeded in proving the claims of New Zealand as a place for the meeting of the Association, and that he had shown there was enough scientific work to merit such recognition as they had received; and he thought he had shown that New Zealand had great capabilities for scientific research, and that there was still a great deal to be done.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 12.—"On the Bisulphite Compounds of Alizarin-blue and Cœrulin as Sensitizers for Rays of Low Refrangibility." By George Higgs. Communicated by Lord Rayleigh, Sec. R.S.

The determination of the relative wave-lengths of the Fraunhofer lines, by photographing all the orders of spectra given by any particular grating, includes certain subjects which present more or less difficulty, and that of selecting or producing a dye-bath adapted to the requirements of the two or more orders comprising the subject is intimately connected with that of the choice of absorbing media.

Having been engaged for some time in investigations of this nature, I had occasion, during the summer of 1889, to require an impression of the second order, about w.l. 3300, contiguous with that of the red end of the first order; and finding that the ordinate of an actinic curve for a plate immersed in a very dilute alcoholic ammoniacal solution of cyanin (1 : 30,000), reduced to about one-fourth of that for an unprepared plate, I abandoned its use for this purpose. The results appeared to be unaffected by the addition of quinine.

Subsequently, induline, cœrulin, alizarin-blue, and the bisulphite compounds of the two latter were used, and when obtained in a state of sufficient purity the alizarin-blue S leaves little or nothing to be desired, for, whilst possessing, in a high degree, sensitizing properties for rays throughout the region comprised between w.l. 6200 and 8000, it does not, like cyanin, lower the sensitiveness to the violet and ultra-violet.

The following is one of the processes I employed in the preparation of the dye-stuff in a pure state :—

To a saturated solution of sodium bisulphite in a mortar is added alizarin-blue paste, this is disintegrated with a pestle, and poured into a glass vessel capable of holding an additional quantity of sodium bisulphite, in all 10 parts of the paste to 20 parts of bisulphite, and another 10 parts of water. The vessel is well stoppered, set aside in a cool place for five or six weeks, and shaken daily, but left undisturbed during the last eight or ten days.

The solution is decanted, filtered, and treated with alcohol, to precipitate the greater portion of the remaining sodium bisulphite. 50 parts of water are now added with a sufficiency of sodium chloride to form a concentrated solution. Again set aside in an open-mouthed glass jar, covered with bibulous paper, for seven or eight days, a deposition of the dye in a crystalline state, together with sulphite of calcium, will take place, which latter, owing to its insolubility in water, may be removed by filtration.

The alizarin-blue S is separated from any unaltered substance left in the original stoppered vessel by solution, and added to the brine, now purified from lime salts, and once more set aside to crystallize, the final purification being effected in a beaker containing alcohol and a small percentage of water to remove the last traces of sodium chloride, collecting the crystals on a filter-paper and drying at ordinary temperatures.

The needle-shaped crystals are of a deep-red. Dilute solutions are of a pale-sherry colour, changing, with the addition of a few drops of ammonia, to a green, which immediately gives way to magenta and every shade of purple, till oxidation is complete, when it assumes a blue colour, the absorption spectrum of which is continuous and strongest in the least refrangible end, presenting the appearance of extending into the infra-red.

Plates immersed in a solution containing 1 : 10,000 and 1 per cent. of ammonia give the most perfect results the day after preparation, but rapidly deteriorate unless kept quite dry.

With a slit 1/1000 inch in width, and an exposure of forty minutes, results have been obtained in the region of great A of the second order which possess all the detail and definition usually so characteristic of the violet end. Numerous lines are sharply depicted which were previously not known to exist. W.l. 8400 has been reached, giving almost equal detail.

The process for the preparation of pure cœrulin S is a slight modification of the preceding. The results obtained, as well as the actinic curve, are almost identical. The pure substance is almost white ; dilute solutions pass rapidly from pale yellow to a bright green ; a trace of ammonia produces an olive-green.

For several samples of paste I am indebted to the kindness of Messrs. Schott, Segner, and Co., of Manchester, agents to the Badische Anilin- und Soda-Fabrik, Ludwigshafen, who hold the patent rights for the manufacture of alizarin-blue S. It is hoped this company may be induced to manufacture this substance free from the minute crystallizable impurities which render it unsuitable for use in investigations of such delicate nature.

Geological Society, March 11.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read :—Manod and the Moelwyns, by A. V. Jennings and G. J. Williams. The area described by the authors is on the north side of the Merionethshire anticlinal of Lower Cambrian rocks, and contains Lingula Flags, Tremadoc, and Arenig rocks. The authors correct what they think is an inaccuracy of some importance in the correlation of beds in different parts of the range, as interpreted in the map and memoir of the Geological Survey, and trace with greater completeness the position and constancy of the beds of slate in the Arenig series—a point of considerable local and practical importance to those engaged in slate-quarrying. They offer also what seems to them to be conclusive evidence to show the intrusive nature of the great crystalline mass known as the syenite of Tan-y-Grisiau, and to its intrusion are due, in their opinion, the peculiar physical characteristics of the surrounding country. Though in the immediate neighbourhood of Festiniog there is no direct evidence of unconformity between the Tremadoc and Arenig series, it seems probable that an unconformity does exist ; for when traced toward the west the Tremadoc beds thin out and the Lingula Flags are overlain by graptolite-bearing slates of Arenig age, while eastward, near Llyn Serw, the grit comes close upon Upper Lingula Flags. The division of the Arenig volcanic rocks into Lower Ashes, Felstone, and Upper Ashes, while true of some districts and useful as a generalization, conveys an idea of uniformity of strata all round the anticlinal which more detailed examination of different districts does not support. After the reading of this paper there was a discussion, in which Prof. Hughes, Dr. Hicks, Mr. Sherborn, and the President took part.—The Tudor specimen of *Eozoon*, by J. W. Gregory.

Zoological Society, March 17.—Prof. G. B. Howes in the chair.—Mr. Sclater exhibited and made remarks on some horns with scalps attached of an Antelope sent to him from Somali-Land by Captain H. G. C. Swayne, R.E., which he referred to the lately described *Cervicapra clarkii* of Mr. Oldfield Thomas. —Mr. Sclater also exhibited two skins of the Ounce (*Felis uncia*) in reference to the specimen of this Cat lately acquired by the Society, and made some remarks on the geographical range of the Ounce in Central Asia.—Mr. A. Smith Woodward gave an account of some dermal plates of *Homosteus* from the Old Red Sandstone of Caithness, lately sent to him by Mr. Donald Calder, of Thurso, the examination of which had enabled him to advance our knowledge of some points in the structure of this remarkable form of extinct fishes.—Mr. G. A. Boulenger gave a detailed description of Simony's Lizard (*Lacerta simonyi*) from the large specimen lately living in the Society's Gardens, which had been brought from the rock of Zalmo, Canaries, by Canon Tristram. —Mr. W. F. Kirby gave an account of a small collection of Dragon-flies made by Mr. E. E. Green in Ceylon. The series contained examples of sixteen species, of which three appeared to be new to science.—Mr. Oldfield Thomas read some notes on the specimens of Antelopes procured by Mr. T. W. H. Clarke in Somali-Land, which had been submitted to his examination by Messrs. Rowland Ward and Co. The specimens were referred to eight species. One of these, already preliminarily described as *Cervicapra clarkii*, was now regarded as constituting a new generic form allied to the gazelles, and proposed to be called *Ammodorcas clarkii*.

Royal Microscopical Society, March 18.—Dr. R. Braithwaite, President, in the chair.—A letter from Colonel O'Hara, dealing with sundry points connected with photomicrography, was read.—Mr. E. M. Nelson exhibited and described a new design of student's microscope recently brought out by Mr. Baker, the idea of which was to provide a microscope of this class, fitted with some of the more important accessories usually only applied to instruments of an expensive character. It had a good coarse adjustment, a differential fine adjustment, a centring sub-stage with rack-work, and a Wright's finder. The stage was of the horseshoe shape, and solid and well made, so that the instrument answered to the description of it as a cheap microscope, capable of doing all ordinary microscopic work.—Mr. T. Charters White read a paper on a new method of demonstrating cavities in dental and osseous tissues, and exhibited specimens in illustration.—Mr. E. M. Nelson exhibited an enlargement of a photomicrograph.—Mr. E. M. Nelson read a paper on the optical principles of microscope bull's-eyes, illustrating the subject by drawings on the black-board.—Mr. Mayall read a communication from the authorities of the Antwerp Microscopical Exhibition to be held during August and September next.—Prof. Bell announced that the President and the Rev. Dr. Dallinger had been appointed delegates to represent the Society at the forthcoming International Congress of Hygiene.—The President announced that the next *conversazione* would be held on Thursday, April 30.

Linnean Society, March 19.—Special General Meeting.—Prof. Stewart, President, in the chair.—The Secretary having read the minutes of the last meeting, the President announced that the sense of the meeting would be taken by ballot on the proposed alteration of certain bye-laws, of which due notice had been given as prescribed by the charter of the Society ; and after explaining the nature and object of such alterations, he invited those present to express their opinions. A discussion followed, in which twenty-two of the Fellows took part, and on the votes being counted it was found that a portion only of the proposed alterations were assented to, the remainder being negatived by 40 to 29.—The following papers were then read :—Researches on earthworms belonging to the genus *Lumbricus*, by the Rev. H. Friend.—The Hemiptera and Heteroptera of Ceylon, by W. F. Kirby.

CAMBRIDGE.

Philosophical Society, March 9.—Dr. Lea in the chair.—The following communications were made:—Observations upon the cerebral heat centres, by Mr. J. G. Adami.—On the nature of supernumerary appendages in insects, by Mr. W. Bateson. The author exhibited a number of specimens in illustration of this subject. The evidence related to about 220 recorded cases of extra legs, antennæ, palpi, or wings, and particulars were given as to the mode of occurrence of these structures. Speaking of cases in which the nature of the extra parts could be correctly determined, it was found that the following principles were followed (amongst others):—I. Extra appendages arising from a normal appendage usually contain all parts found in the normal appendage peripherally to the point from which they arise, and never contain parts central to this point. II. A. Extra appendages of double structure are the commonest. (1) Whether separate or in part compound, they consist of a pair of complementary parts, one being right and the other left. (2) Of the two extra appendages, that which is adjacent to the limb from which they arise, is a limb of the other side of the body. (3) If the pair of extra appendages arise from the anterior surface of the normal appendage, the surfaces which they present to each other are structurally posterior; if they arise posteriorly, the adjacent surfaces are anterior; if they arise ventrally, the adjacent surfaces are dorsal, &c. II. B. A single extra appendage is rarely perfect. (1) If it arises from the body, it is formed as an appendange of the side on which it is placed. (2) If it arises by peripheral division of an appendage, the parts central to the point of division are commonly right or left as the case may be, while the peripheral part may be a symmetrical and complementary pair. It was pointed out that these phenomena are important as an indication of the physical nature of bodily symmetry, and in their bearing upon current views of the character of germinal processes. The author expressed his indebtedness for information, or the loan of specimens, to Messrs. H. Gadeau de Kerville, Pennetier, Giard, Kraatz, L. von Heyden, Dale, Mason, Westwood, Waterhouse, N. M. Richardson, Janson, Reitter, &c., and especially to Dr. Sharp for much help and advice in examining the specimens.—On the orientation of *Sacculina*, by T. T. Groom.—Some experiments on blood-clotting, by Albert S. Grünbaum. The author described the results of six experiments in which the cœliac and mesenteric arteries were ligatured. He finds that the effects of the ligature on the clotting of blood removed (after an interval of four hours) from the body is less pronounced than has been stated by Bohr (*Centralb. f. Physiol.*, 1888, s. 263). The clotting was slightly delayed, in one case by twenty minutes, but in the others by a few (three to four) minutes only. The clots, when formed, were less firm than normally. Bohr has stated that, in two experiments conducted as above, the clotting of blood was delayed for about two hours.

EDINBURGH.

Royal Society, March 2.—Sir Douglas Maclagan, President, in the chair.—Dr. Alexander Buchan read a paper on the relation of high winds to barometric pressure at Ben Nevis Observatory. The Observatory at the top of Ben Nevis is exposed to winds from all quarters, while the Observatory at the foot of Ben Nevis is well sheltered from all winds. It is found that the variations of barometric pressure at the high-level Observatory are practically proportional to the speed of the wind throughout a range varying from zero speed to a speed of 110 miles per hour. The difference is practically proportional to the square of the speed of the wind, which agrees with the well-known result regarding the reduction of pressure in the interior of a moving fluid.—Dr. A. Bruce read notes on a case of cyclopia in a child. There was a single median lozenge-shaped ocular cavity furnished with two upper and two lower eyelids. The nose was represented by a short pedunculated process of skin and subcutaneous tissue attached to the skin of the forehead above the median eye. On microscopic section, two rudimentary eyes were found, the two retinæ of which evidently arose from a single optic vesicle. The brain was nearly normal below the cerebrum, which was imperfectly divided into two membranes, and contained only a single ventricular cavity. The two optic thalami were fused together, apparently in consequence of the pressure of thickened membranes around them, which was considered to be the probable cause of the deformity. The premaxillary and ethmoid bones, the vomer and turbinated bones, and the pre-sphenoid were absent.—Dr. Byron Bramwell read a paper on cases illustrating the position of the visual centre in man. He first referred to the experimental investigations of Ferrier, Munk, Schäfer, and other observers, and emphasized the difficulty of determining the exact position of the visual centre by experiments on the lower animals. He then quoted Ferrier's analysis of the recorded cases of lesion of the visual centre in man; and finally detailed some cases which had come under his own observation which have important bearings on the subject. In the first case the patient was seized, two and a half years before her death, with symptoms indicative of cerebral embolism. From the presence of right-sided homonymous hemianopsia, the author diagnosed an embolic enfarction of the left posterior cerebral artery, with resulting softening of the left occipital lobe—probably the cuneus and adjacent white matter. The hemianopsia, which was complete, which passed just outside the fixing-point, and which affected light, form, and colour, persisted absolutely unchanged until the patient's death from a second embolism of the right internal carotid artery, two and a half years after the first attack. A sharply-defined softening of the posterior and inferior part of the cuneus and of the inferior occipital convolution was found after death. The angular gyrus was absolutely unaffected. The white matter of the occipital lobe was only involved to a limited extent from before backwards. Except in the top of the occipital lobe, the optic radiations were in no way directly implicated by the lesion. The author claimed that the case conclusively proved the presence of a half-vision centre in the top of the occipital lobe. In the second case a localized irritative lesion in the surface of the left occipital lobe produced flashes of light referred by the patient to the right eye, but in reality projected to the right side of the middle line (visual area corresponding to the left side of each retina). The third case was a typical example of sensory Jacksonian epilepsy, the left half of each retina being completely blind (right-sided homogonous hemianopsia). In the fourth case a localized softening beneath the left angular gyrus had produced temporary word-blindness and hemianopsia, the latter symptom being apparently due to arrested function in the optic radiations. A fifth case, still under consideration, was briefly referred to. In it complete right-sided hemianopsia, complete word-blindness, and temporary mind-blindness, without any other symptom whatever, had resulted from a sudden cerebral lesion, which was thought to be an embolism of the left middle cerebral artery, with resulting softening of the angular gyrus.—Prof. Crum Brown communicated a paper by Mr. F. Beddard on the anatomy of Ocnerodrilus (Eisen).

March 16.—Rev. Prof. Flint, Vice-President, in the chair.—Emeritus-Professor Blackie read a paper on bistratification in the living Greek language. The author asserts that modern Greek has been but slightly altered, since the time of Coraes, from classical Greek. The first thirty-one verses of the Gospel of St. John, as published in Athens in 1855, contain only nine departures from the classical type; while the corresponding portion of the Romaic version, published 200 years ago, contains twenty-eight. In the higher walks of Greek literature this purity of literary style is very marked. In thirty-one pages of Tricoupis' "History of the Greek War of Independence" (published in London in 1853), only fifteen deviations from the standard of ancient Greek appear; and in two chapters of Paspati's "History of the Capture of Constantinople by the Turks" (published in Athens in 1890), only ten deviations appear. The standard to which the author appeals is the Greek, not only of Plato and Xenophon, but of Diodorus, Lucian, Polybius, and Chrysostom. In the lower colloquial Greek of common life, very great divergence from classical literary style is evident. Thus, in the first twenty-six lines of the dialogues in a primer of colloquial Greek, published this year in Leipzig, thirty-three deviations from classical style occur. But, in even this lower form of Greek, very few words borrowed from other languages are found; and the accented syllable still remains as it was fixed by the Alexandrian grammarians.—Dr. John Murray read a paper, written by Mr. Robert Irvine and himself, on silica and siliceous formations in modern seas. There is great difficulty in accounting for the number of organisms which secrete silicic acid, and for the remains of such organisms which occur in the ocean and on the bed of the ocean. The amount of silicic acid which exists in solution in sea-water is far too small to account for the immense development of such organisms in various parts of the ocean. The authors have proved that clay and mud, carried down by rivers to the sea, are to be found in even the least disturbed parts of the ocean. And the diatoms can extract from these clays sufficient material for the formation of their siliceous sheaths. The authors have also proved that the suspending power of sea-water for such clays diminishes very markedly as the temperature rises. This appears to account for the great abundance of diatoms in the colder seas. —A communication, by Dr. W. G. Aitchison Robertson, on the relation of nerves to odontoblasts, and on the growth of dentine, was read.—Mr. A. Silva White read a paper, illustrated by a map, on the comparative value of African lands. The chief points dealt with in this paper are the relative value of African lands to the European Powers which control them; the progressive value of these lands from strategic bases on the coasts; and the lines of least resistance, and tracts of highest resistance, to European domination.

Paris.

Academy of Sciences, March 23.—M. Duchartre in the chair.—The President announced the loss sustained by the Academy in the death of M. Cahours on March 17, and gave a short account of the life and works of this eminent chemist.—Action of heat on carbonic oxide, by M. Berthelot. It is known that carbonic oxide shows indications of decomposition at a red heat, with production of traces of carbon and carbon dioxide. A discussion of the facts leads M. Berthelot to the conviction that the phenomena are not due to the direct dissociation of carbonic oxide, but to molecular condensation, the condensed product separating into carbon dioxide and sub-oxides.—On a reaction of carbonic oxide, by the same author. In the course of researches on the preceding subject, M. Berthelot observed a characteristic reaction of carbonic oxide, due to its reducing action on an ammoniacal solution of silver nitrate. If bubbles of carbonic oxide, or an aqueous solution of it, be added to the nitrate solution, an abundant black precipitate appears upon boiling.—On the proper odour of earth, by MM. Berthelot and G. André. The authors have endeavoured to determine the origin of the odour which is so marked an emanation from vegetable mould after a fall of rain. They find that the essential principle resides in an organic compound of the aromatic family. Its odour is very penetrating, and analogous to that of camphors; its proportion in mould is only a few millionths, but one three-millionth of a gram is sufficient to produce a sensible smell. The new principle is neither acid, alkali, nor a normal aldehyde; its concentrated aqueous solutions may be precipitated by potassium carbonate, with the production of a resinous ring. When heated with potash, an acrid odour, analogous to that of the resin of aldehyde, is developed. It does not reduce ammoniacal silver nitrate. Under certain conditions—that is, by the employment of potash and iodine—iodoform is produced. This property is common, however, to many other substances, but the authors have not found furfurol, acetone, nor ordinary alcohol, although M. Muntz has stated that these bodies existed in some vegetable mould he examined.—Contribution to the biology of parasitic plants, by M. A. Chatin. The author contests the idea that parasitic plants nourish the hosts.—On the glycolytic power of the blood of man, by MM. R. Lépine and Barral. The glycolytic power is defined as the percentage loss of sugar which occurs when blood is kept for an hour in a water-bath at 38°–39° C. Determinations have been made of the glycolytic power of the blood of persons suffering from diabetes, pneumonia, and uræmia.—Observations of Millosevich's asteroid (304) made at Paris Observatory with the East Tower equatorial, by Mdlle. D. Klumpke. Observations for position were made on March 13 and 17.—On the theory of applicable surfaces, by M. J. Weingarten.—On the changes which are presented after imbibition in a system formed by the superposition of two thin, homogeneous, hygroscopic laminæ having different properties, by M. J. Verschaffelt.—On the action of hydroiodic acid on silicon chloride, by M. A. Besson. A part of the chlorine of silicon chloride has been replaced by iodine; the products obtained are described.—On the transformation of pyrophosphite of soda into phosphite, by M. L. Amat.—On bromo-nitrite salts of platinum, by M. M. Vèzes.—On the disaggregation of neutral amine salts by water, by M. Albert Colson.—New combinations of pyridine, by M. Raoul Varet.—On the theory of dyeing phenomena, by M. Léo Vignon.—On a method of simultaneously measuring the time of electrical excitation, and the resulting muscular contraction, by M. A. d'Arsonval.—On the action of phenic acid on animals, by MM. Simon Duplay and Maurice Cazin.—Actinometric observations made at the Observatory of the Petrowsky Academy, near Moscow, by MM. R. Colley, N. Michkine, and M. Kazine.—Some remarks on these observations, by M. Crova.

Amsterdam.

Royal Academy of Sciences, February 28.—Prof. van de Sande Bakhuyzen in the chair.—Mr. D. T. Korteweg discussed the spinodal and connodal curves of a surface having a slightly deformed conical point.—Mr. van Bemmelen showed two new compounds of mercuric oxide—a crystalline sulphate of mercury with one molecule of water, $HgO.SO_3.H_2O$, and a colourless basic sulphate, $(HgO)_3.(SO_3)_2.(H_2O)_2$, both obtained by Mr. C. Hensgen in the inorganic chemical laboratory of Leyden, during his researches on the chemical equilibrium between mercuric oxide and diluted sulphuric acid. He shows also that a homogeneous solution of chloride of antimonium, S_6Cl_3, in diluted sulphuric acid is separated into two layers of liquid by heating it to a certain temperature. The two layers disappear again by cooling. The temperature of the separation depends on the composition of the mixture. Mr. Hensgen is carrying on his researches on the subject.—Mr. L. M. J. Stoel, on the influence of temperature on the viscosity of fluid methyl chloride (communicated by Mr. Lorentz). Mr. Stoel has measured the transpiration times for a fixed volume of the liquid, the capillary tube being always the same. The pressure at one end was equal to that of the saturated vapour; that at the other end was greater by an amount which is measured by a column of mercury of a fixed height. The temperatures range from − 28° C. to + 123° C., the boiling-point being − 23°, and the critical temperature + 143°. The transpiration times (in seconds) may be calculated with a fair approximation by the formula

$$\log D = \frac{896 - T}{250},$$

T being the absolute temperature. The experiments were executed in the laboratory of Mr. Kamerlingh Onnes, at Leyden.

CONTENTS.

THURSDAY, APRIL 9, 1891.

ANOTHER DARWINIAN CRITIC.

On the Modification of Organisms. By David Syme. (London : Kegan Paul, Trench, and Co.)

THIS little book is one of a class that was more common twenty years ago, when any acute literary critic thought he could demolish Darwin. Mr. Syme has, however, the advantage of having read some of the best works both for and against Darwinism, and is thus able to support his views by quoting writers of eminence. He begins boldly. In the table of contents of the first chapter we find such headings as, "A fatal admission—Darwin's definition misleading—Refutes his own theory." But when we look for the proof of these statements we find they rest on misconception, misrepresentation, or misquotation. A few examples will show that this is the case.

At p. 3, after quoting Darwin's definition of natural selection as "The preservation of favourable individual differences and variations, and the destruction of those that are injurious," Mr. Syme remarks: "Natural selection is therefore another name for the struggle for existence, and I cannot help thinking that the latter is much the better expression of the two, being less ambiguous." What are we to think of a critic who thus, at the very outset, misrepresents his author, by stating without qualification that of the three factors which lead to natural selection—rapid multiplication, heredity with variation, and the struggle for existence—the first two may be left out of consideration, and the last taken by itself as synonymous with the resultant of the whole? And this misrepresentation he makes use of again and again in his argument. At p. 8 he tells us that "Darwin never acquired the art of using precise language"; and, after quoting some of his statements, adds: "Had he substituted for natural selection the expression 'struggle for life,' there would, it is true, have been less novelty about it, but there would also have been less liability to error, both on his own part and on the part of his readers." And now, having repeated his own erroneous definition twice, he seems to have convinced himself that it is Darwin's also, for he says, in the same paragraph: "We have seen that he defines natural selection as 'the struggle for existence,' and again as 'the survival of the fittest.'" Mr. Syme actually gives both these terms between inverted commas as if they were Darwin's own words, and then goes on to show that elsewhere he speaks of the two as different things; and concludes by informing us that "Such inaccuracies of expression occur in almost every page of his writings"!

One more example of this system of criticism. At p. 10, after quoting a passage from Darwin about the origin of the eye, and of organs used only once in a lifetime, as within the power of natural selection, the critic goes on to say: "It is evident that we have here two kinds of natural selection. We have a natural selection which selects or preserves only, and we have another which adapts, modifies, or creates"; and then there is a quibble about the two being fundamentally different. But what we have to observe here is the word "creates," which Mr. Syme has brought in with an "or," and which he very soon imputes to Mr. Darwin himself. For example, at p. 15, he says: "in other places he insists that variations are created by natural selection"; and again at p. 17, he says:—"We have seen that Darwin has put forth two distinct and contradictory theories of the functions of natural selection. According to the one theory natural selection is selective or preservative and nothing more. According to the other theory natural selection creates the variations, and we are left to infer that it afterwards selects them." He adds that Darwin evidently favoured this latter view, and therefore he (the writer) "shall assume that it is a creative as well as a preservative and destructive process"!

Having thus, by means of various misconceptions and misquotations, shown his readers how inaccurate, illogical, and inconsistent Darwin often is, Mr. Syme surveys the position from his own superior stand-point, and points out the road over which he is about to lead them in a passage which, for its amazing statements and supreme self-confidence, deserves to be quoted.

> "I venture to dissent altogether from Darwin on the question of the functions and tendency of natural selection. I maintain that natural selection does not create the favourable variations, at all events in the sense understood by him, and that it does not even preserve them. I go further than this, and assert that it does not even exterminate the unfavourable variations. I shall endeavour to show that it is neither creative, preservative, nor greatly destructive; that it neither produces nor preserves the fit, nor exterminates the unfit, and that, so far from being beneficent in its operation, as Darwin and his followers represent, the struggle for existence is, on the whole, pernicious, and tends to produce disease, premature decay, and general deterioration of all beings subjected to its influence."

How Mr. Syme establishes all this must be studied in the pages of his book. He appears to satisfy himself, and may perhaps satisfy such of his readers as know nothing from any other source of the subjects he discusses. Those who have such knowledge may estimate the value of Mr. Syme's teaching by his explanation of mimicry, which is, that natural selection has nothing to do with it, but that insects choose environments to match their own colours. He tells us that these extraordinary resemblances only occur among insects that are sluggish, and that, "to account for these likenesses to special objects, animate or inanimate, we have only to assume that these defenceless creatures have intelligence enough to perceive that their safety lies in escaping observation."

In a similar manner he deals with the supposed adaptations of flowers for cross-fertilization by insects. After quoting from Darwin the curious mode in which *Coryanthes macrantha* is fertilized by bees, he says that it is "utterly incredible" that this complex arrangement has been provided for the purpose of securing cross-fertilization, adding: "It is far more probable that the insects made use of the existing apparatus than that it had been expressly provided for them in order to get the alleged purpose effected." What use it can be to the insect to be imprisoned in a floral water-cistern he does not deign to explain: neither does he tell us how the flower comes to possess this complex structure. Topsy's explanation, that "it growed," is perhaps thought sufficient.

But though Mr. Syme believes that he has utterly smashed Darwinism, he still professes himself an evolutionist, and in his last chapter gives us an alternative theory in the intelligence of the vegetable and animal cells.

"They are," he says, "the sole agents employed in the construction, and afterwards in the maintenance, of the most complex organisms, and their economic and social organization is both comprehensive and complete. When an injury occurs to any part of the organism, they collect in force on the spot for the purpose of effecting repairs, which they execute with singular skill and judgment, varying the means employed according to the circumstances of each particular case."

This theory will be found much more thoroughly as well as more amusingly set forth in Mr. Samuel Butler's "Life and Habit"; but, whatever may be thought of its merits, few evolutionists will accept it as a complete and sufficient substitute for the Darwinian theory of natural selection.

Mr. Syme has a considerable reputation in other departments of literature as a powerful writer and acute critic; but he has entirely mistaken his vocation in this feeble and almost puerile attempt to overthrow the vast edifice of fact and theory raised by the genius and the life-long labours of Darwin.

Alfred R. Wallace.

METALLURGY.

An Introduction to the Study of Metallurgy. By Prof. W. C. Roberts-Austen, C.B., F.R.S. (London: Charles Griffin and Co., 1891.)

THE well-known efforts of Prof. Roberts-Austen in leading students to appreciate the application of correct principles to the metallurgic art, led to high expectation when the publication of his "Introduction to the Study of Metallurgy" was announced, and this expectation has not been disappointed. Although, as regards minute and accurate description of detail and general thoroughness, the volumes of Percy stand alone, and although more condensed works, such as Phillips and Bauerman's "Elements of Metallurgy," are available for the student, yet there has been a distinct want of a systematic exposition of the general principles of metallurgy, and of clear statements as to the physical characters of metals and alloys. These are more especially needed by students on the threshold of metallurgy, who desire to enter profitably on the study of the more or less disconnected details of the art as applied to the several metals, such as are to be found in the monographs of Sir Lowthian Bell, and of the late Sir William Siemens. The evident purpose of the volume is to meet this want, the author having deliberately subordinated details of smelting operations, in order that he might deal at length with the physical properties of metals and the constitution and characters of alloys, modified as these properties often are by thermal treatment, and by the presence of small quantities of foreign elements. Such questions are treated with much wealth of research, and abundant reference to authority. The book will hardly be popular with the class of students who merely attempt to "cram."

The importance of the amount of impurity, which may either be valuable or prejudicial, in the application of metals and alloys in the arts, is strikingly shown by the aid of elaborate curves, among which may be noticed one indicating the influence of minute proportions of phosphorus on steel (p. 24), and others showing the action of nickel on iron, and of foreign elements on gold. A remarkable example of the effect of minute variations in the proportions of an added element is noted (p. 117) in the case of die-steel, which when containing 0·8 per cent. of carbon, may be made into dies that will strike 40,000 coins each, but which would be rendered practically useless by variations of under 0·2 per cent. of the carbon.

In noticing the special section of the work comprised in the first four chapters, the evidently strong points of the author should not be overlooked. He rightly regards it as of much importance that the student should be made conversant with the observations and works of the early metallurgists, with the reasoning which led to their practice, and with the advances which have, up to the present day, resulted from their labours. The treatment of metallurgy, as embodied in this section, is a novel feature, and must have involved much more labour and research than would at first sight be gathered from the fact that it has been possible to compress the conclusions into little more than a hundred pages. In the adoption of this treatment, the author has marked out for himself a course that cannot be too highly commended. The student will now be able to attack with advantage the difficulties he will have to grapple with later, and to discount erroneous statements and false reasonings, which, if presented under the guise of authority, prove to be veritable stumbling-blocks in practice, until the stern school of experience teaches better things.

Metallurgical processes are not treated in the detail usual in metallurgical text-books, and here the essential character of the author's method comes into prominence. Furnaces and apparatus, though classified and illustrated in the previous chapters, hardly appear again, but the general scope of metallurgical procedure is exemplified by means of typical processes, and these occupy thirty-five pages.

The classification of processes (pp. 238–241) well deserves praise. It presents to the student, in a few pages, and in a way not be found elsewhere, the essential and distinctive characters of the whole of the methods of metallurgy, whether by "dry" or furnace processes, by the solvent action of mercury, by solution and precipitation (the so-called "wet" processes), or by the latest and already important methods which involve electrolysis. The more typical of these have been selected for descriptions, which are illustrated by the aid of diagrams showing the essential steps and sequence of operations. By the aid of these diagrams, the student has clearly presented to his eye such excellent but apparently complicated processes as the smelting of copper ores by the Welsh method (p. 242), and the Freiberg method of smelting complex ores (p. 250), which have hitherto been found very confusing, even when the descriptions have been very carefully written. With such guidance, the details of furnaces, of successive roastings and fusions, as fully elaborated in other works, can be studied without confusion or difficulty.

Wet processes are made equally clear in the same way, and the only fault to be found with this division of the work is that it is too brief. Whilst the work merits ungrudging approval, it may be observed that the author, who has done well in giving special prominence to the influence of foreign matter on the metals and alloys, has perhaps exaggerated the relative value of the study of thermal and mechanical treatment of metals as compared with improvements in smelting operations. This has led him to subordinate and curtail descriptions of metallurgical processes, which is to be regretted, because his diagrammatic methods of description are most effective. This part of the work will well bear expansion in the next edition, which will doubtless soon be called for.

THOMAS GIBB.

THE RELATIVITY OF KNOWLEDGE.

The Prevailing Types of Philosophy: Can they Logically reach Reality? By James McCosh, LL.D., Litt.D., &c. (London: Macmillan and Co., 1891.)

WHAT is reality? The plain man when he opens his eyes and stretches forth his hands never questions the practical reality of the objects which surround him. The walnuts which he sees on his plate are resistant to the fingers, the wine in his glass has a *bouquet* the reality of which he can readily put to the satisfactory test of the palate. But the psychologist seeks to analyze these intuitions and perceptions, the validity of which no sane man doubts. He follows the intuitions and perceptions home, and finds that they are in some way associated with certain material transactions within the ivory casket of the human skull. And then he begins to speculate about an ultimate reality behind the reality of perceptions. The blush on the peach—is it really inherent in the fruit, or is it merely a mode of vibration of my own brain-molecules? And these brain-molecules—is their so-called matter aught but a figment of the mind? Or is this mind merely a subtle secretion of the grey matter of the cortex? Thus, many questions can be asked, and many answers given.

Dr. McCosh, in the little volume under notice, inquires, What do the leading philosophic systems of the day make of reality? "I am to put this question," he says, "to each of them. Do they acknowledge it, or do they deny it? Do they accept it in whole, or only in part? Do they attempt to prove it, or simply assume it?"

What, we naturally ask, is the "reality" concerning which Dr. McCosh asks these questions? He replies:—

"The only way of showing its nature is to point to examples of it. We look on the wall of the room in which we sit, and know it to be real. We see a bird flying, and know it to be an actuality. We are conscious of ourselves in pain, and we are sure of our own existence in a state of pain."

In other words, by "reality" Dr. McCosh means that practical reality which no man in his senses denies or thinks of denying.

The question Dr. McCosh sets himself to consider is therefore this:—How far do the leading philosophic systems of the day agree to regard *as absolute and ultimate* that practical reality, *relative to man as an organism*, in which every man out of Bedlam implicitly believes? Such being the true nature of his question, it is scarcely a matter of surprise that Dr. McCosh should find that neither the experiential and sensational schools nor the *a priori* or Kantian school, nor the Scottish school will agree to do anything of the sort. They all see too clearly the difference between relative and absolute reality to identify them after the fashion of the ex-President of Princeton College. Or, if individual members of any of these schools do so identify them, they are for the most part honest enough avowedly to confess that they do so on theological and not on philosophical grounds.

Into the theological aspects of the question we cannot enter here. Suffice it to say that we have never expressed aught but respect for those who believe that the world was created by Divine fiat, and that man was endowed with faculties which enable him directly to apprehend its existence and nature. But we do not think that philosophy apart from revelation would have been led to this conclusion; and we doubt the wisdom of those who appeal to philosophy in its support.

We would have it clearly understood that it is not to the appeal which Dr. McCosh makes, but the tribunal to which he makes it, that we take exception. "Agnosticism," he says, "is upheld and propagated in the present day by several influential men, such as Mr. Herbert Spencer and Prof. Huxley. It is in the air, and our young men have to breathe it and suffer the consequences. It is evidently exercising a relaxing influence on the faith and doctrinal convictions of the rising generation. It is in my view the grand office, at present, of the higher philosophy, to meet and expose this doubting spirit." We would counsel Dr. McCosh to substitute in future editions for "higher philosophy" other words, and to boldly assume at the outset the theological position on the grounds of fundamental and unalterable conviction. Any conclusions of philosophy, whether experiential, Kantian, or Scottish, which run counter to this fundamental assumption will then, for him and his followers, stand *ipso facto* condemned. If the relativity of human knowledge be one of these unorthodox conclusions, Dr. McCosh is assuredly right for his own part in rejecting it on the grounds of unalterable conviction, though mere unaided and uninspired philosophy has, as his little book plainly shows, conspired with one voice to sanction it.

Dr. McCosh writes clearly, tersely, and forcibly, and gives in a short space an excellent account of the conclusions reached by the prevailing types of philosophy so far as they concern the question of reality.

C. LL. M.

OUR BOOK SHELF.

An Explanation of the Phonopore, and more especially of the Simplex Phonopore Telegraph. By C. Langdon-Davies. (London: Kegan Paul, Trench, Trübner, and Co., Limited, 1891.)

THE present work contains a plain explanation of the phonopore, and is written in a manner that will be intelligible to all who are acquainted with the elements of electric technics and practical telegraphy. The phenomena which led to the invention of this instrument were the sounds generally known to telephonists as induction noises: many attempts seem to have been made to overcome them, and success has been achieved only at a great cost. The author, in investigating the causes of these disturbances, was led to make use of the force from which they were derived, and in the phonopore we have an instrument which is based on the results of the investigation.

This system of telegraphy is used in conjunction with the ordinary system, with the addition of certain instruments and apparatus. Short and rapid electrical impulses (which have no effect on the telegraphic instruments as usually employed) being sent successively along the ordinary wire by means of other apparatus forming part of the system, this pulsatory current is enabled to work relays and other telegraphic instruments; no interruption is caused in the ordinary telegraphic signals, which may be worked as usual and transmitted through the same conductor at the same time.

Of the instruments in the above-mentioned circuit, the principal one is that called by the author the phonopore; its action consists in allowing the vibratory impulses to pass freely through it, while ordinary currents, such as those used in telegraphic working, are stopped by it.

Previous to the description of the simplex phonopore telegraph, the author relates some of the experiments that led him to devise these ingenious instruments. The technicalities pertaining to the principles of their construction and to the methods of arranging them in circuit are then entered upon, and finally a comparison is drawn between the ordinary telegraph and the phonopore telegraph.

The five reports written by well-known men and inserted at the end speak of this new system as highly valuable. The following brief extract shows in a few words Prof. Silvanus P. Thompson's view of the utility of the system:—"In my opinion the experiments and demonstrations have been successful in establishing the entire practicability of satisfactorily working the simplex phonopore telegraph simultaneously with the ordinary telegraphs (both needles and Bright's bells) upon the same ordinary telegraph line."

In concluding our remarks, we must add that the subject-matter is presented in English and French, printed side by side; numerous illustrations are given showing the actual instruments, while others, in diagrammatic form, explain the methods of arranging them in circuit. Altogether the work is one that will be read with interest by electricians and by all those connected with practical telegraphy.

The Naturalist of Cumbrae: being the Life of David Robertson. By the Rev. T. R. R. Stebbing. (London: Kegan Paul, Trench, Trübner, and Co., 1891.)

Mr. Robertson, who is now in his eighty-fifth year, has done much good work as a marine zoologist; and the present record of his career will interest not only his personal friends, but all readers who admire energy, enthusiasm, and intellectual resource. It is no easy task to write a biography of one who is still alive, but Mr. Stebbing has done his work skilfully and with good taste, erring only occasionally by reference to small details which are scarcely worthy of a place in a serious narrative. For many years, as a boy and young man, Mr. Robertson worked as a farm labourer; but, having much intelligence, he missed no opportunity of cultivating his mind, and he contrived to fit himself for the study of medicine in Glasgow. Although he passed through the regular medical course, he preferred business to the life of a doctor; and he was successful enough to be able, in 1860, to retire with a competency. He had long been interested in various branches of natural science, and now he had leisure to gratify his tastes to the utmost. He settled on the Island of Cumbrae, and there he has since worked at marine zoology so steadily, and with so ready a power of interpreting observed phenomena, that he has added considerably to our knowledge, and has had many opportunities of being of service to eminent naturalists, with whose requests for specimens or information he has always been delighted to comply. It is pleasant to read of the friendships thus formed, and of the fine qualities which are so cordially appreciated by all to whom Mr. Robertson is personally known. The book belongs in some ways to the class which Mr. Smiles has made popular, but Mr. Stebbing's hero is distinguished by geniality and modesty of character from the type which most people are apt to associate with the idea of "self-made men."

By Track and Trail: a Journey through Canada. By Edward Roper. (London: W. H. Allen and Co., 1891.)

This book is rather too long, but it contains many interesting passages, and will give much pleasure to any readers who may have special reasons for desiring to obtain information as to the prospects of settlers in the Dominion. Everything set down with regard to agricultural matters is a repetition of what the author heard from men whom he either knew or believed to be trustworthy. His general conclusion is that there is no country under the British flag where a prudent settler has better chances than in Canada. In the course of his narrative he gives some very bright descriptions both of places and people; and he has brought together some valuable notes on the aborigines. Numerous reproductions of original sketches by the author illustrate his story.

An Etymological Dictionary of the German Language. By Friedrich Kluge. Translated from the Fourth German Edition by J. F. Davis. (London: George Bell and Sons, 1891.)

Prof. Kluge's work, of which this is a translation, is well known to all students of the etymology of the German language. The author has not only extensive knowledge, but a sound and penetrating judgment; and he displays a remarkable power of presenting concisely and lucidly results at which he can have arrived only after careful and elaborate research. Mr. Davis's translation is in every way worthy of the original, and no one who may have occasion to use it will fail to appreciate the skill with which he has accomplished a difficult and useful piece of work.

Nature's Wonder-Workers. By Kate Lovell. (London: Cassell and Co., 1890.)

In this book, Miss Lovell tells the life-histories of various insects, her object being "not so much to teach as to give fresh interest to the living, often despised creatures which constantly cross our path." She writes clearly and gracefully, and the information she presents has been derived from the best and latest authorities on entomology. The volume is well illustrated, and will stimulate the interest of its readers in some of the more curious facts of natural science.

LETTERS TO THE EDITOR.

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

Phosphograms.

If a glass plate (or a celluloid film) is evenly coated with phosphorescent calcium sulphide in a suitable menstruum, and then exposed for a few minutes in a photographic camera, a phosphorescent negative can be taken of a sunlit view. When this plate is carefully applied to an ordinary photographic film for a short period in the dark room, on developing the latter a reversed negative (or *phosphogram* as I prefer to call it) of the original picture is permanently produced. If the picture consists (as in my case) of a vista of smoke-stained chimney-pots, the slated roofs and stuccoed walls of adjacent houses, the negative, under favourable circumstances, will show considerable traces of natural colours. In the case of phosphorescent calcium sulphide, which glows with a beautiful purple violet light, it is not difficult to understand why it acts on the gelatine film so readily as it does. Both the phosphorescent rays and the most active photo-chemical rays belong (as is well known) to the most refrangible rays of the solar spectrum; that the one should thus react on the other is not surprising.

If, instead of using the colourless calcium sulphide, as above described, we stain the mixture with an alcoholic solution of rosanilin acetate before applying it to the glass plate, on exposure to daylight it glows with a deeper intensity, and it still acts, although with less vigour, on a photographic film. M. Verneuil (*Comptes rendus*, civ. 501), and M. Becquerel (*ibid.*, 551), have investigated the causes of phosphorescence in calcium sulphide from a chemical standpoint, and the latter has shown that the colour of the light may be changed to a bright green by the admixture of a trace of lithium carbonate or of potassium persulphide, and to an orange-yellow by the addition of manganese peroxide. The green phosphorescence acts feebly, the orange-yellow scarcely at all, upon the ordinary gelatine film. The bromo-iodide plates are clearly only imperfectly responsive to coloured rays. To attempt coloured photography with the present dry plates is eminently unsatisfactory. By staining the film some makers have succeeded in increasing the sensitiveness of the plate to the less refrangible red rays, but my own experience leads me to conjecture that the green and green-yellow rays are quite as difficult to intercept by our modern plates as the deep-red, if they are not more so.

Brighton, March 28. W. AINSLIE HOLLIS.

Neo-Lamarckism and Darwinism.

UNDER the above heading (NATURE, March 26, p. 490) Prof. Henslow gives some criticisms of my paper on the Alpine flora, which seem to show that, for the sake of brevity, I unintentionally rendered my views obscure to him and possibly to others who share his opinions. It is therefore fortunate that he should have stated his difficulties, in order that I may try to explain what it was I intended, taking the various points in succession.

(1) I wrote "dwarfing may be—doubtless often is—the direct result of environment, as lack of nourishment." This, I believe, is strictly true as regards individual plants, and I do not think it affects the argument that dwarfing may also be due to other causes. Certainly, insufficient nutriment may cause extreme dwarfing altogether apart from cold: for instance, I very well remember some minute but flowering specimens of *Matricaria*, only an inch or so high, found by Mr. J. W. Horsley in a dry place near Chiswick, where the normal fully-developed form of the plant is abundant.

And if dwarfing is the result of cold, how is it explained that plants on different soils at the same altitude vary so much in this respect? Thus, along Swift Creek, in Colorado, *Mertensia sibirica*, which grows in very damp ground and in the rich vegetable deposits on the immediate banks of the creek, is large, tall, and rank, with broad leaves; while a species of the same genus, growing close by on the more exposed, dry, and more barren prairie land, is very much smaller, more compact, and with narrow leaves. Again, at the same place, *Oxytropis lamberti*, when growing in the damper ground by the creek, is tall, rank, and has white or very pale flowers; while on the adjoining prairie the same species is low, depressed, altogether smaller, and with crimson flowers, which turn purple in drying for the herbarium.

I did not say, and certainly would not say, that dwarfing was always the result of bad or insufficient nourishment; but neither is it always or normally the direct result of cold.

(2) When dwarfed varieties or forms have been produced by environmental conditions or otherwise, it is natural enough that they should tend to revert to the original type when removed again to the original conditions. Nevertheless, true Alpine dwarfed *species* do retain their characters when grown at low altitudes artificially. Florists' catalogues are full of allusions to dwarf varieties and species, most of which are very constant. As I write, I can see a clump of *Scilla sibirica* out of the window, and certainly it appears to me to have the true characters of the species, and the long cultivation which this species has undergone has not changed it into a large and more diffuse species like *Scilla nutans*. Minute species of *Narcissus*, *Gentiana*, and *Silene*, which come from mountain regions, may surely be grown in England without starting up and simulating the large temperate-region forms of those genera. I don't mean to say that dwarfing, like other specific characters, may not become changed or lost in time if the circumstances are favourable, or the utility of the peculiarity ceases, but I think it will be generally admitted that low stature, as a specific character, often proves as constant and hereditary as other specific characters.

(3) Surely it is not necessary to demonstrate that tall plants would be likely to be injured by the winds on Alpine summits? The trees at timber-line in Colorado would, I should think, convince anybody of this. Those which are exposed frequently bear branches only on the side towards the valley, the others having succumbed at an early stage to the violence of the winds. The trees at their highest limit are shorn off almost as if with a knife, so that, going down the slope, one does not meet with a full-sized tree until the topmost branches are able to obtain some shelter from the tree above it; so that, although the trees grow to timber-line, they do not raise their summits above it. Also, one may find some old stumps, perhaps fifty yards above the present timber-line, on the Sangre de Cristo Range, showing that, for some reason, trees which formerly grew there have since succumbed.

(4) I have not myself made any experiments to test the additional warmth that might be derived from close proximity to the ground, but I think the point is well established. The rocks and the surface of the ground would surely retain a certain amount of heat, and beds of vegetable tissue, such as peat, do not readily cool. Thus, Mr. E. J. Lowe relates (*Conchologist*, 1891, p. 4) that, in the great frost of 1860–61, the River Trent was frozen over; but a drain, cut through a bed of organic soil, though only a foot wide, and containing less than 12 inches in depth of water, remained unfrozen.

(5) As to "partial shelter": is it not clear that dwarfed plants, which grow close to the ground, and often between rocks or under projecting ledges (as in the manner of Alpine plants), will be partially sheltered, and thereby advantaged?

(6) Of course, I do not suppose that plants *know* anything about the coming season! Only this, those plants which naturally grew quickly and through rapid metabolism came to early maturity, would survive, while those which did not do so would perish. It is a simple example of natural selection. Various weeds and other wild plants, notably *Cleome integrifolia*, are in Colorado often accidentally sown as seeds dropped from hay, and thus appear round human habitations some hundreds of feet above their proper altitude. They grow and flourish, and duly flower, but they cannot mature their seeds in time, and hence never perpetuate themselves at these altitudes. The Indian corn, *Zea maïs*, varies considerably in its period for maturing. It can be grown in England, but does not sufficiently often mature to be a paying crop. In America, the quickly-maturing varieties can be grown at greater altitudes and latitudes than the others. Given such variation, is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short? Let anyone take varieties of Indian corn differing in the period of maturing, and grow them at the extreme altitude at which this species can be cultivated, and it will not take a botanist to predict the success of the one and the failure of the other.

T. D. A. COCKERELL.

The Whirling Ring and Disk.

I REGRET that the interjection of a paradoxical rider to my proposition regarding a whirling ring should have had the effect of embedding in a storm of protest Prof. Ewing's interesting point concerning a disk. Let me do the best I can to set matters straight and clear.

First, it is plain that my rider concerning a cable was not well worded, and whereas it is quite true that a weightless steel girdle round the earth would be broken by its whirling tension, it would have been better to avoid any appeal to flotation as a practical means of securing weightlessness. The simplest plan is to abandon the cable illustration altogether as a not very pertinent or useful one. Nevertheless, it is plain that a cable fixed at two distant points and hanging free between them is liable to break, whether it be hanging downwards or floated upwards, *i.e.* whether it be supported too little or too much; and it further appears that if it be stretched to the curvature of the earth the slight difference between its true and apparent weights, *i.e.* its centrifugal force alone, is sufficient to break it.

But now returning to the main proposition, that the critical velocity at which a whirling ring breaks is the square root of the ratio of its tenacity to its density, no one calls it in question; some, indeed, say it is well known. That it was known, or at least so easily knowable as to be practically known, to applied mathematicians, is manifest; but that it is well known to practical men I have some reason for doubting. However that is of no consequence. What I write to point out is how singularly nearly the result for a ring agrees with the result for a disk of the same size.

For, as Prof. Ewing's letter shows, the critical peripheral speed of a uniform disk with a very small hole in the centre, when it is on the point of flying, is

$$\sqrt{\left(\frac{4T}{(3+\mu)\rho}\right)},$$

whereas for a ring it is the same expression without the $4/3\frac{1}{4}$.

Thus, then, a perforated disk of uniform thickness, although its greatest stress occurs round its central aperture, yet can stand only 11 per cent. greater angular spin than a ring of the same material fitting its circumference and devoid of all radial support.

A disk without a hole can, as Prof. Ewing says, stand 40 per cent. more rotations per minute than a perforated disk can stand (*i.e.* $\sqrt{2}$ times the speed), even though its perforation is a mere needle-prick or indeed is absolutely infinitesimal, so long as it destroys radial coherence across the centre.

I learn also from Prof. Ewing that Mr. Chree has obtained results for an oblate spheroid, which, reduced to an approximate disk, show that the stresses and strains at any point of the ellipsoid are $\frac{8}{11+\mu}$ths of what they are at the corresponding point of an unperforate disk of the same size but of uniform thickness (μ being Poisson's ratio).

I am obliged to Mr. Boys for calling attention to Maxwell's early memoir on the subject ("Scientific Papers," vol. i. p. 61), with which I was not acquainted. Since Maxwell uses an unusual notation, it may be convenient to note that his μ is volume-elasticity, his m is twice rigidity, and E = Young's modulus; so that his E/m is merely 1 + Poisson's ratio. There are, however, a few misprints with regard to signs.

Prof. Worthington's remarks refer to a straight *bar* with free ends, not to an endless band. It is true, however, that there was no need to drag in vibrations; the dangerous tension will be set up in a straight portion of any endless band, running in the direction of its length with the critical speed $\sqrt{\left(\frac{T}{\rho}\right)}$, by the agency of the curved portions, which necessarily exist somewhere.

It may save some confusion to mention that the suffixes of Prof. Ewing's radial and tangential tensions have become interchanged in the first column of his letter on p. 462 (March 19).

The observations of Prof. Greenhill are best dealt with under a separate heading. OLIVER J. LODGE.

March 21.

Formation of Language.

PERMIT me to reply to your correspondent Mr. W. J. Stillman, on the "Formation of Language" (NATURE, March 26, p. 491). The interesting fact he records of the spontaneous invention and use of child-names for objects is not unknown to philologists. The phenomenon has been previously noticed, among others, by Miss Watson, of Boston, and Dr. E. R. Hun, of Albany, U.S.A.; by Archdeacon Farrar, in the case of Indian children left by themselves for days together in Canadian villages; and by M. Taine, in his work "De l'Intelligence." Numerous examples of children's language are given by Dr. Horatio Hale (philologist to the U.S. Wilkes Exploring Expedition), who has made a special study of the spontaneous development of roots among children the basis of his remarkable theory of the origin of linguistic stocks. Full details will be found in a paper on the "Development of Language," read before the Canadian Institute of Toronto, April 1888, and in an address on the "Origin of Languages and the Antiquity of Speaking Man," in the Proceedings of the American Association for the Advancement of Science, Buffalo, 1886, vol. xxxv. The occurrence has been often noticed in the families of philologists—the most noteworthy instance being that of the young nephew of the well-known Sinologist Dr. George von Gablentz. This boy, before he learned his mother-tongue, called things by names of his own invention. The constant elements were the consonants, the vowels being varied, and employed as they were deeper or higher to denote greatness or smallness. The root for round objects was *m-m;* a watch, plate, and the moon was *mem*, a large round dish or table, *mum*, and the stars *mim mim mim;* an ordinary chair was *lakail*, a great arm-chair *lukull*, and a little doll's chair *likill*. A distinguished Accadian, Chinese, and Semitic scholar, the Rev. C. J. Ball, makes no secret of the fact that, between the ages of six and eight, he and his young brother had names of their own devising—perfectly arbitrary monosyllables and dissyllables for several of the small tools and toys they valued most. Mary Howitt relates in her autobiography (edited by her daughters) that the silent sadness of the Quaker home circle extended to the nursemaids, and that in consequence of this the eldest child, her sister Anna, did not learn to talk until she was four years old. Long after they could talk, "being left chiefly to converse together, our ignorance of the true appellations for many ordinary sentiments and actions compelled us to coin and use words of our own invention. To sneeze was to us both *okis-kow*, the sound which one of our parents must have made in sneezing." Here we get a true *onomatopœia*, as in the monosyllable *mea*, employed by one American child for "cat"; in another child's vocabulary the extraordinary trisyllable *shindikik* designated that animal. The association of ideas and extension of meaning are often very suggestive—viz. *migno migno* = water, wash, bath; *waia waiar* = black, darkness, Negro. As in the case of the name for water, *bhumboo*, cited by Mr. Stillman, the vocables are often of two syllables, rarely of three. It is interesting to note the continued use of the little boy's own name for water as a means of identifying the acquired Italian *aqua* for the same object, as frequently happens with adults struggling to express themselves in a foreign tongue. Reduplication seems also to characterize these "child languages" like those of some savage tribes, and plurals are formed by repetition. The syntax, Dr. Hale remarks, resembles that of deaf-mutes and gesture language. If left to themselves there seems no reason why children with this aptitude should not develop a vocabulary at least as extensive as that of Dr. Farrar's three peasants, "who conversed for a long while without employing more than one hundred words." Many cases of "child language," no doubt, have passed away unrecorded. Soon after the children mix much with adults, the special vocabulary begins to die out. It is possible that the use of such spontaneously developed root-words might be prolonged among the children of the poorer classes, so often cared for by children but little older than themselves. The *crèches* of our large towns might afford further evidence of abnormal developments of this apparently inherent inventive linguistic faculty.

Brighton, April 2. AGNES CRANE.

MR. W. J. STILLMAN winds up his interesting letter on the above subject by inquiring whether any of the readers of NATURE have made observations analogous to his own on the development of language in children.

I have on several occasions requested parents to note down the sounds emitted by their children from their birth to the time when those sounds seemed to become developed into articulate words with a certain amount of intelligent meaning, but I have not obtained any satisfactory results. The subject has, however, been studied, and described with considerable detail, by that well-known member of the French Academy, M. H. Taine, in his work, "De l'Intelligence" (2 vols., 3rd edition, Paris, 1878). I would particularly refer to chap. ii., sec. 5, and also to that part of a long note at the end of vol. i., entitled "Acquisition du Langage par les Enfants" (pp. 356–383). C. TOMLINSON.

Highgate, N., March 31.

On Frozen Fish.

MR. F. H. P. COSTE (*ante*, p. 516) supplies the reference to the statement I had in memory as to "one of the Arctic voyages" (*ante*, p. 440), and my necessarily imperfect quotation does not materially differ from the actual statement.

With regard to Mr. Turle's letter (*ante*, p. 464), I would remark that my words, "comparatively shallow" waters, were not intended to apply to "two feet of water," and I would suggest that the innumerable sticklebacks embedded in the ice, and which did not revive, were probably dead (from suffocation) before they became so embedded.

It is well known that insects which habitually hibernate as larvæ or pupæ do not suffer from being frozen for a lengthened period. On the other hand, they suffer greatly in "open" winters with frequent alternations of wet, warmth, and cold. Therefore, from an entomological point of view, the season of 1891 promises to be an unusually productive one.

It is not my intention to return to this subject.

Lewisham, April 3. R. MCLACHLAN.

IN a letter appearing under the above heading in your last issue, the writer asks for well authenticated instances of the recovery of frozen insects. All climbers have, at one time or another, met with butterflies, lying frozen on the snow, on Alpine passes, and many persons have brought down some of these insects, which, on reaching the warmth of the lower regions, invariably recover animation, though when picked up they are so completely frozen, and consequently so brittle, that they break to pieces unless carefully handled. I have frequently, when climbing, placed these frozen butterflies on my hat, and, on descending, have noticed them always fly away. It must often have been a considerable time from the frozen stage till recovery. E. MAIN.

Grand Hotel, Montreux-Territet, April 4.

Quaternions and the Algebra of Vectors.

MY remark about Prof. Willard Gibbs was meant in all courtesy, and I am happy to find it so taken by him. The question between us, being thus a scientific one only, can afford to wait for a fortnight or so:—until my present examination season is past. P. G. TAIT.

THE MULTIPLE ORIGIN OF RACES.

IN NATURE of March 5 (p. 415), the Duke of Argyll has printed a very interesting letter of Mr. Darwin's, from which he drew the inference that the writer "assumed mankind to have arisen . . . in a single pair." I do not think myself that the letter bears this interpretation. But the point in its most general aspect is a very important one, and is often found to present some difficulty to students of Mr. Darwin's writings.

Quite recently I have found by accident, amongst the papers of the late Mr. Bentham at Kew, a letter of friendly criticism from Mr. Darwin upon the presidential address which Mr. Bentham delivered to the Linnean Society on May 24, 1869. This letter, I think, has been overlooked and not published previously. In it Mr. Darwin expresses himself with regard to the multiple origin of races and some other points in very explicit language. Prof. Meldola, to whom I mentioned in conversation the existence of the letter, urged me strongly to print it. This, therefore, I now do, with the addition of a few explanatory notes.

W. T. THISELTON DYER.

Royal Gardens, Kew, March 27.

Down, Beckenham, Kent, S.E.,
November 25, 1869.

MY DEAR MR. BENTHAM,—I was greatly interested by your address, which I have now read thrice, and which I believe will have much influence on all who read it. But you are mistaken in thinking that I ever said you were wrong on any point. All that I meant was that on certain points, and these very doubtful points, I was inclined to differ from you. And now, on further considering the point on which some two or three months ago I felt most inclined to differ, viz. on isolation, I find I differ very little. What I have to say is *really* not worth saying, but as I should be very sorry not to do whatever you asked, I will scribble down the slightly dissentient thoughts which have occurred to me. It would be an endless job to specify the points in which you have interested me; but I may just mention the relation of the extreme western flora of Europe (some such very vague thoughts have crossed my mind, relating to glacial period) with South Africa, and your remarks on the contrast of passive and active distribution.

P. lxx.—I think the contingency of a rising island, not as yet fully stocked with plants, ought always to be kept in mind when speaking of colonization.

P. lxxiv.—I have met with nothing which makes me in the least doubt that large genera present a greater number of varieties relatively to their size than do small genera.[1] Hooker was convinced by my data, never as yet published in full, only abstracted in the "Origin."

P. lxxviii.—I dispute whether a new race or species is necessarily, or even generally, descended from a single or pair of parents. The whole body of individuals, I believe, become altered together—like our race-horses, and like all domestic breeds which are changed through "unconscious selection" by man.[2]

P. do.—When such great lengths of time are considered as are necessary to change a specific form, I greatly doubt whether more or less rapid powers of multiplication have more than the most insignificant weight. These powers, I think, are related to greater or less destruction in early life.

P. lxxix.—I still think you rather under-rate the importance of isolation. I have come to think it very important from various grounds; the anomalous and quasi-extinct forms on islands, &c., &c., &c.

With respect to areas with numerous "individually durable" forms, can it be said that they generally present a "broken" surface with "impassable barriers"? This, no doubt, is true in certain cases, as Teneriffe. But does this hold with South-West Australia or the Cape? I much doubt. I have been accustomed to look at the cause of so many forms as being partly an arid or dry climate (as De Candolle insists) which indirectly leads to diversified [?] conditions; and secondly, to isolation from the rest of the world during a very long period, so that other more dominant forms have not entered, and there has been ample time for much specification and adaptation of character.

P. lxxx.—I suppose you think that the *Restiaceæ*, *Proteaceæ*,[3] &c., &c., once extended over the world, leaving fragments in the south.

You in several places speak of distribution of plants as if exclusively governed by soil and climate: I know that you do not mean this, but I regret whenever a chance is omitted of pointing out that the struggle with other plants (and hostile animals) is far more important.

I told you that I had nothing worth saying, but I have given you my THOUGHTS.

How detestable are the Roman numerals; why

[1] Bentham thought "degree of variability . . . like other constitutional characters, in the first place an individual one, which . . . may become more or less hereditary, and therefore specific; and thence, but in a very faint degree, generic." He seems to mean to argue against the conclusion which Sir Joseph Hooker had quoted from Mr. Darwin that "species of large genera are more variable than those of small."

[2] Bentham had said: "We must also admit that every race has probably been the offspring of one parent or pair of parents and consequently originated in one spot." The Duke of Argyll converts the proposition.

[3] It is doubtful whether Bentham did think so. In his 1870 address he says: "I cannot resist the opinion that all presumptive evidence is against European *Proteaceæ*, and that all direct evidence in their favour has broken down upon cross-examination."

should not the President's addresses, which are often, and I am sure in this case, worth more than all the rest of the number, be paged with Christian figures?

My dear Mr. Bentham,

Yours very sincerely,

CH. DARWIN.

HERTZ'S EXPERIMENTS.

I.

"OH! yes; I understand it all now. Electricity is the ether;" or, "Yes; it's just like everything else: electricity is a vibration." These are the remarks one hears made by those who think that a few scattered words picked up at a popular lecture make things quite clear. It is no doubt unfortunate that repeating a form of words is a different matter from understanding them, and still more different from understanding the subject they are intended to explain. In this case there is the added misfortune that the form of words is not accurately repeated, and in its inaccurate form does not mean what is true. It is often hardly worth while remarking this to those who make these statements, because the words convey to them little or no signification, and are to them as true as any other unmeaning sentence. The connection between electricity and the ether is certainly not, as far as is known, well described by saying that "electricity is the ether," and we cannot say with any certainty that electricity is or is not a vibration. Hertz's experiments have given an experimental proof of Maxwell's theory that electrical phenomena are due to the ether, and Hertz's experiments deal with vibrations. One cannot, however, say, because the pressure of 15 pounds per square inch exerted by the atmosphere is due to the air, that therefore "pressure is the air": nor even, because a person who studied the properties of the air had studied them by means of sounds propagated through it, can one assert that "pressure is a vibration." It is to be hoped no one will now assert that "electricity is pressure." The example is given to illustrate the absurdity of the statements made as deductions from recent experiments, and not to teach any new theory. And yet one comes across people who, after listening to an interesting lecture Lord Rayleigh might give, illustrated by Mr. Boys's sound-pressure-meter, would make the above statements, and really think they understood them. That blessed word "Mesopotamia" comforted the soul of an old lady with some reason, for religion is to some extent a question of feeling; but in science it is high treason to truth to be comforted by unmeaning sounds—they should produce despair.

It is to be hoped after this tirade that any reader of these articles who comes across statements he cannot understand will not tell himself the lie that he does understand them, nor pretend to others that he does. The subject is very difficult: one that has engaged the attention of thoughtful and clever men for many years, and is still in many parts, even to the most acute, shrouded with difficulties, uncertainties, and things unknown, so that nobody need be the least ashamed of not following even as far as others can go into this wonderful region. If the articles can give to most who read them glimpses which unfold intelligible ideas of even the outskirts of this region, it is all that any writer can reasonably expect who is not one of those masters of exposition who combine the highest scientific and literary abilities.

Consider for a minute the question at issue. That electric and magnetic phenomena are due to the same medium by which light is propagated—that all-pervading medium by whose assistance we receive all the energy on this earth that makes life here possible, by which we learn the existence of other worlds and suns, and analyze their structures and read their histories; that medium which certainly pervades all transparent bodies, and probably all matter, and extends as far as we know of anything existing: this wonderful all-pervading medium is the one we use to push and pull with when we act by means of electric and magnetic forces; and remember that we can pull molecules asunder by this means, as well as propel trains and light our houses. The forces between atoms are controlled by this all-pervading medium, which directs the compass of the mariner, signals round the globe in times that shame e'en Shakespeare's fancy, rends the oak and terrifies creation's lords in the lightning flash. It was a great discovery that proved all concord of sweet sounds was due to the medium that supplies the means of growth to animals and plants, and deals destruction in the whirlwind; and yet the 80 miles depth of our air is but a puny thing compared with the all-pervading illimitable ether.

That there is a medium by which light is transmitted in a manner somewhat analogous to that by which the air transmits sound has been long held proved. Even those who held that light was due to little particles shot out by luminous bodies were yet constrained to superpose a medium to account for the many strange actions of these particles. Now, no one thinks that light is due to such particles, and only a very few of those who have really considered the matter think that it can be due to air, or other matter such as we know. How does light exist for those eight minutes after it has left the sun and before it reaches the earth? Between the sun and earth there is some matter, no doubt, but it is in far-separated parts. There are Mercury and Venus, and some meteors and some dust no doubt, and wandering molecules of various gases, many yards apart, that meet one another every few days, perhaps, but no matter that could pass on an action from point to point at a rate of thousands of miles each second. Some other medium must be there than ordinary gross matter. Something so suble that the planets, meteors, and even comets—those wondrous fleecy fiery clouds rushing a hundred times more quickly than a cannon-ball around the sun—are imperceptibly impeded by its presence, and yet so constituted as to take up the vibrations of the atoms in these fiery clouds and send them on to us a thousand times more rapidly again than the comet moves to tell us there is a comet toward, and teach us what kinds of atoms vibrate in its tail. How can a medium have these contrary properties? How can it offer an imperceptible resistance to the comet, and yet take up the vibrations of the atoms? These are hard questions, and science has as yet but dim answers to them, hardly to be dignified by the name of answers—rather dim analogies to show that the properties supposed to coexist, though seeming contradictory, are not so in reality. One of the most beautiful experiments man knows—one fraught with more suggestions than almost any hundred others—is that by which a ring of air may be thrown through the air for many yards, and two such rings may hit, and, shivering, rebound. These rings move in curved paths past one another with almost no resistance to their motion, urged by an action not transmitted in time from one ring to another, but, like gravitation, acting wherever a ring may be, and yet the air through which they move *can* take up vibrations from the rings, showing thus that there is no real contradiction between the properties of things moving through a medium unresistedly in certain paths round one another, and yet transmitting other motions to the medium. This same air can push and pull, as when it sucks up water-spouts and deals destruction in tornadoes. Hence there seems no real contradiction between a medium that can push and pull and transmit vibrations, and yet offer no resistance to such fragile, light, and large-extended things as rings of air.

It is important to understand something about the properties that this medium must have in order to explain light, electricity, and magnetism, because there is no use expecting a medium to possess contradictory properties. It is also well to recollect that for about two hundred years the existence of a medium by which light is propagated has been considered as certain, and that it would be very remarkable if this medium, which can be set in vibration by material atoms, acted on matter in no other way. It seems almost impossible but that a medium which is moved by atoms, and which sets them into motion, should be able to move such armies of atoms as we deal with in material bodies. Even if we knew nothing of electricity and magnetism, it would be natural to look for some important phenomena due to the action of this medium on masses of matter. The medium is a *vera causa*, and if it can be shown that the same set of properties by which electric and magnetic forces are explained will also enable it to transmit vibrations that have all the properties of light, it will surely be beyond a doubt but that these electric and magnetic actions are those very ones we would naturally expect from the medium that propagates light. Clerk Maxwell some years ago showed that this was so, but as far as any facts known at that time could prove, there were other theories of electric and magnetic actions which explained their known phenomena without the intervention of a medium. The matter stood somewhat thus. The older theories of electric and magnetic force explained all phenomena then known. These older theories assumed that electric and magnetic forces were propagated instantaneously throughout space. That if the sun became electrified, it would instantaneously begin to induce electricity on the earth. That there would be no delay of eight minutes such as occurs between a light occurring on the sun and its acting on the earth. Similarly in the case of magnetic actions, they were supposed to be propagated instantaneously throughout space. It was, no doubt, known that it took time for an electric signal to be transmitted along a conducting cable. This is, however, a very much more complicated problem than the simple one of supposing a body surrounded by a non-conductor to be electrified. Will it or will it not instantaneously act on all conductors in space, and begin to induce electrification on them? As far as was known, such actions as this, actions through non-conducting space, were instantaneous. Such an instantaneous action could not be transmitted by the air. Air cannot send on from point to point any effect more rapidly than a molecule of air can moving carry it forward, and that is only a little faster than the velocity of sound; and there was every reason to know that electric induction through air was propagated much more rapidly than that. There was every reason to believe that electric and magnetic forces acted without any material intervention.

In fact, in these older theories there was no thought of any medium to transmit the actions; it was supposed that electricity acted across any intervening space instantaneously. There is no real difficulty in such a supposition: as far as we know, gravitation is just such an action, and as far as was then known, there was no experiment that disproved the supposition in the case of electric and magnetic actions. It was known that no experiment had ever been devised that could test whether this action was instantaneous or whether it was propagated at a rate such as that of light. It was known that this action was enormously more rapid than sound, but as light goes about 300,000 times as fast as sound, there was plenty of spare velocity. These older theories explained all that was known, and they supposed nothing as to the existence of an intervening medium. Any theory that assumed that induction was not instantaneous, but that energy having been spent on electrification at one place work would be done at another after some time, as in the case of light generated on the sun not reaching the earth for eight minutes, any theory that assumed such a disappearance of energy at one place and its reappearance at another after the lapse of some time must assume some medium in which the energy exists after leaving the one place and before it reaches the other. A theory that only supposes instantaneous action throughout space need not assume the existence of a medium to transmit the action, but any theory that supposes an action to take time in being transmitted from one place to another must assume the existence of a medium. Now, Maxwell's theory assumed the existence of a medium, and along with that led to the conclusion that electric and magnetic actions were not propagated instantaneously, but were propagated with the velocity of light. According to his theory an electric disturbance occurring on the sun would not produce any effect on the earth for about eight minutes after its occurrence on the sun. No experiments were known to test the truth of this deduction until the genius of Hertz brought some of the most beautifully conceived, ingeniously devised, and laboriously executed of experiments to a brilliantly successful conclusion, and demonstrated the propagation of electric and magnetic actions with the velocity of light, and thereby proved experimentally that they are due to that same wonderful, all-pervading medium, by means of which we get all the energy that makes life here possible.

The problem to be solved was, Are electric and magnetic actions propagated from place to place in a finite time, or are they simultaneous everywhere? How can experiments be made to decide this? Consider the corresponding problem in sound. What methods are there for determining the rate at which sound is propagated? An experiment that measures the rate can tell whether that rate is finite or whether it is infinitely great. There are two important methods employed for measuring the velocity of sound. The second is really only a modification of the first direct method, as will be seen. The direct method is to make a sudden sound at a place, and to find how long afterwards it reaches a distant place. In this method there is required some practically instantaneous way of communicating between the two places, so that the distant observer may know when the sound started on its journey. A modification of the method does not require this. It depends on the use of reflection. If a sound be made at a distance from a reflecting surface, the interval of time between when the sudden sound is made and when the reflected sound, the echo, returns, is the time the sound took to travel to the reflector and back again. A well-known modification of this method can be applied if we can secure a succession of sudden sounds, such as taps, at accurately equal intervals of time. We originate such a regular succession of taps, and alter the distance from the reflector until each reflected tap occurs simultaneously with the succeeding incident tap. Or if the distance at which we can put the reflector be sufficiently great, we may arrange it to be such that a reflected tap is heard simultaneously with the second, third, fourth, or any desired succeeding tap. The coincidence of the taps with their reflections can be fairly accurately observed, and a fairly accurate estimate formed of the velocity of sound, *i.e.* the velocity at which a compressing or rarefying of the air is propagated by the air. Instead of altering the distance of the source of sound from the reflector, we may ourselves move about between the source and the reflector, and we can find some places where the reflected taps occur simultaneously with the incident taps, and some places where they occur between the incident ones. This is pretty evident, for if we start from the source towards the reflector, as we approach it we get the reflected taps earlier and the incident ones later than when we were at the source. How far must we go towards the reflector in order that the original and reflected taps may again appear simultaneous? We must go half the distance that a tap is propagated during the interval between two taps: *half* the distance, because in going away from the source we are approaching the reflector and so make a double change—we not only get the original ones later, but we also get the reflected ones earlier, and so coincidence will have again been reached when we have gone half the distance between any pair of compressions travelling in the air. Now, if the taps succeed one another slowly, the distance in the air between any two of them travelling through it will be considerable; any one of them will go a considerable distance from the source before its successor is started after it. If, on the contrary, they succeed one another rapidly, the distance between the travelling taps will be small. In general, if V be the velocity with which a tap travels, and T be the interval of time between successive taps, the distance apart of the taps travelling in the air will be $\lambda = VT$. By arranging, then, that the taps shall succeed one another very rapidly, *i.e.* by making T small, we can arrange that λ may be small, and that consequently the distance between our source of sound and the reflecting wall may be small too, and yet large enough to contain several places at distances of $\frac{1}{2}\lambda$ apart between the source and the reflector where the incident and reflected taps occur simultaneously. Now, a very rapid succession of taps is to us a continuous sound, and where the incident and reflected taps coincide we hear simply an increased sound, while at the intermediate places where the incident taps occur in the intervals between the reflected taps we do not hear this effect at all. In the case of a succession of sharp taps we would hear in this latter place the octave of the original note, but if the original series be, instead of taps, a simple vibration of the air into and out from the reflector, the in and out motions of the incident waves will in some places coincide with the in and out motions of the reflected wave, and then there will be an increased motion, while at intermediate places the *in* and *out* motions of the incident wave will coincide with the *out* and *in* motions of the reflected wave, and no motion, or silence, will result, so that at some places the sound will be great and at intermediate places small. This whole effect of having an incident and reflected wave travelling simultaneously along a medium can be simply and beautifully illustrated to the eye, by sending a succession of waves along a chain or heavy limp rope or an india-rubber tube fixed at the far end so as to reflect the waves back again. It will then be found that the chain divides up into a series of places where the motion is very great, called loops, separated by points where the motion is very small, called nodes. The former are the places where the incident and reflected motions reinforce, while the latter are where these motions are opposed. If we measure the distance between two nodes, we know that it is half the distance a wave travels during a single vibration of the string, and so can calculate the velocity of the wave if we know the rate of vibration of the string. This is the second method mentioned above for finding the velocity of sound. There are so many things illustrated by this vibrating chain that it may be well to dwell on it for a few moments. We can make a wave travel up it, either rapidly or slowly, by stressing it much or little. If a wave travels rapidly, we must give it a very rapid vibration if we wish to have many loops and nodes between our source and the reflector; for the distance from node to node is half the distance a wave travels during a vibration, and if the wave goes fast the vibration must be rapid, or the distance from node to node will be too great for there to be many of them within the length of the chain. Another point to be observed is the way in which the chain moves when transmitting a single wave and when in this condition of loops and nodes, *i.e.* transmitting two sets of waves in opposite directions. There are two different motions of the parts of the chain it is worth considering separately. There is in the first place the displacement of any link up or down, and in the second place there is the rotation of a link on an axis which is at right angles to this up and down motion. Now, when waves are going up the chain those links are rotating most rapidly which are at any time most displaced: it is the links on the tops and bottoms of waves that are rotating most rapidly. On the other hand, in the case of loops and nodes the links in the middle of loops never rotate at all; they are much displaced up and down, but they keep parallel to their original direction all the time, while it is the links at the nodes where there is no displacement up and down that rotate first in one direction and then back again: there is, in the loops and nodes condition, a separation of the most rotating and the most displaced links which does not occur in the simple wave. There is a corresponding relation between the most rotated and the most rapidly moving links. These are the same links half-way up the simple waves, but in the loops and nodes the most rapidly moving links never rotate at all, while those at the nodes that get most rotated are not displaced at all. These remarks will be seen hereafter to throw light on some of the phenomena observed in connection with Hertz's experiments: hence their importance.

It will be observed that the method of measuring the velocity at which a disturbance is propagated along a string and which depends on measuring the distance between two nodes is really only a modification of the direct method of finding out how long a disturbance takes to go from one place to another; it is one in which we make the waves register upon themselves how long they took, and so does not require us to have at our disposal any method of sending a message from one place to another more quickly than the waves travel, and that is very important when we want to measure the rate at which disturbances travel that go as fast as light. If the wave travels very fast, we must have a very rapid vibration, unless we have a great deal of space at our disposal; for the distance between two nodes is half the distance the wave travels during one vibration, and so will be very long if the wave travels fast, unless the time of a vibration be very short. Hence, if we wish to make experiments in this way, in a moderate-sized room, on a wave that travels very fast, we must have a very rapid vibration to start the waves.

(*To be continued.*)

METEOROLOGY OF BEN NEVIS.[1]

THIS work, just issued by the Royal Society of Edinburgh as vol. xxxiv. of its Transactions, is a quarto volume of 467 pages, giving in detail the hourly observations at the Ben Nevis Observatory from December 1883 to the close of 1887, together with a log containing much that is interesting and novel in meteorological observing; the five daily observations made in connection therewith at Fort William, and a report by Dr. Buchan on the meteorology of Ben Nevis.

This is, it is believed, the only existing combined high and low level Observatories sufficiently near each other in horizontal distance as to be virtually one Observatory: completely placed in one of the greatest highways of storms in the world; and at such a height as to be occasionally above the storms that sweep over that part of Europe, and frequently, as the winds show, within a geographical distribution of pressure at this height widely different from what obtains at the same time at sea-

[1] "Meteorology of Ben Nevis." By Alexander Buchan, LL.D., Secretary of the Scottish Meteorological Society. Transactions of the Royal Society of Edinburgh, vol. xxxiv.

level. Further, the Observatory affords unique facilities for supplying, through its observations, those physical data which are absolutely indispensable in any discussion of the problems involving the relations of height to temperature, humidity, and pressure in the free atmosphere.

Last summer the Low Level Observatory was equipped by the Meteorological Council with a complete set of self-recording instruments, and the regular observing work began on July 14; and it is intimated that the discussion of the hourly observations made at the top and bottom of the mountain in their bearing on many of the more important meteorological inquiries has been commenced. As intimated in NATURE of February 26 (p. 397), a first instalment of this work is completed, showing the influence of high winds on the barometer at the Observatory on the top of the Ben.

The report summarizes the results in diurnal, monthly, and annual means of temperature, humidity, pressure, wind-force, rainfall, cloud, and sunshine. The latter part of the report deals with miscellaneous observations and discussions, which have been carried on, mostly of a novel character, for which Ben Nevis offers exceptional facilities.

The mean annual temperature of Ben Nevis for these four years is 30°·5, the lowest monthly mean being 23°·2 in March, and the highest 40°·9 in July. The mean annual difference between Ben Nevis and Fort William is 15°·9, the least monthly difference 14°·2 in November, and the greatest 17°·8 in May. These results show the rate of decrease of temperature with height to be 1° for every 275 feet of ascent for the year; the rates are most rapid in April and May, and least rapid in November and December, these being 1° for each 247 feet and 307 feet respectively. From a discussion of the daily maxima and minima it is seen that in winter the decrease of temperature with height is nearly as great during the night as during the day, but from April to September the rate of fall during the day is about a half more than that of the night.

But the rates of decrease from day to day differ widely from these mean values and from each other. The greatest difference between the two Observatories was 28°·1 at 2 p.m. of June 8, 1885. The temperature of the top, however, has frequently stood higher than that of Fort William. The greatest deviation occurred at 8 a.m. of November 18, 1885, when, while the temperature of Fort William was 22°·2, that of the top was 35°·1, or 12°·9 higher. These inversions of temperature are significant phenomena, from their obvious and intimate relations with anticyclones, and their connection with neighbouring cyclones; from the extraordinarily dry states of the air which frequently accompany them; and the unusually high barometer at the top when reduced to sea-level, as compared with the sea-level pressure at Fort William.

The repeated occurrence of excessive droughts is one of the most striking features of the climate of Ben Nevis. On July 30, 1885, there occurred a good example of these droughts, when, from 1 a.m. to 4 a.m., the dew-point fell from 44°·8 to 21°·8. But by far the greatest drought was in March 1886, commencing at 1 a.m. of the 11th, and ending at midnight of the 12th; the mean relative humidity of the first 24 hours was 19, and of the second 15, the lowest being 6 at 8 p.m. of the 12th. Such low dew-points and humidities, frequently marked off sharply from high dew-points and humidities, often occur; and, it may be remarked, their occurrence suggests an explanation of the irregular geographical distribution of those disastrous frosts which occur the following night in some districts, although not at all in districts closely adjoining.

The mean annual pressure at Ben Nevis is 25·300 inches, the lowest monthly mean being 25·106 inches in January, and the highest 25·516 inches in June, the difference thus being 0·410 inch. As compared with the sea-level pressure at Fort William, the mean annual difference is 4·562 inches, the greatest monthly difference 4·626 inches in March, and the least 4·483 inches in July, these being, it need scarcely be added, the months of lowest and highest mean temperature respectively. The difference between the mean monthly maximum and minimum is thus 0·143 inch. For these two months the mean temperatures of the stratum of air between the top and bottom of the mountain are 31°·6 and 49°·0, on the assumption that the mean temperature of the intervening air stratum is the mean of the temperature at the top and bottom. Hence the vertical displacement of the mass of the atmosphere for a temperature difference of 17°·4 is represented by a barometric difference of 0·143 inch. Considering the successful arrangements which have been made to minimize the effects of solar and terrestrial radiation at both the upper and lower Observatories, and their close proximity to each other, this result may be regarded as the most important datum hitherto contributed by meteorology for the discussion of inquiries into the relations of height to temperature and pressure in the free atmosphere.

A table of corrections for height for the barometric observations of the Observatory has been prepared, chiefly empirically, for each degree of air temperature and each tenth of an inch of sea-level pressure. With the two sea-level pressures of the upper and lower Observatories thus obtained, various questions may be investigated, such as the effect of high winds on the barometer; the varying relations of pressure and temperature to anticyclones, cyclones, and particularly the regions in front or rear of cyclones; and the relation of pressure to heavy falls of rain.

For the period dealt with, the mean annual amounts of rainfall at the top and bottom are 129·47 inches and 77·33 inches, or about a half more at the Observatory than at Fort William. The observations show that on Ben Nevis, one day in from three to four days has been without rain; and that in one day out of nine, 1 inch of rain, or upwards, has been collected. The time of the year when the great annual fall of temperature takes place is coincident with the time of heaviest rainfall; and the time of least rainfall with the time of the greatest increase of temperature.

The mean minimum temperature of the day occurs from 5 to 6 a.m., and the maximum from 1 to 3 p.m., according to season, the daily range in winter being only 0°·8, and in summer 3°·5. The great daily variations of temperature observed on Ben Nevis are not due to the sun, but they are the temperatures which accompany the cyclones and anticyclones as they successively appear and disappear. The great importance of the hygrometric observations lies in the part they play in the occurrence of daily changes of weather attendant on these cyclones and anticyclones; and in investigating these relations, it is not mean humidities, but the individual observations which are so valuable.

In all seasons, the barometric curves show the double maximum and minimum pressure. Their times of occurrence are, first minimum from 5 to 6 a.m.; first maximum from 11 a.m. to 2 p.m., second minimum 3 to 7 p.m.; and second maximum 8 to 9 p.m., according to season. Like all high level Observatories situated on peaks, the largest of the variations from the daily mean is the morning minimum, and the least, the afternoon minimum, the former being 0·016 inch below the mean, and the latter 0·001 inch above it, on the mean of the year. The relatively large minima in the early morning is due to the cooling of the atmosphere during the night, by which the air contracting sinks to a lower level, thus lowering pressure at high levels, and, consequently, this effect is greatest in the summer months when the diurnal range of temperature is at the maximum. The expansion of the air from the increase of temperature during the day raises its mass to a higher level, thus maintaining the barometer at a higher point during the time of the afternoon minimum.

The maximum velocity of the wind occurs during the night, and the minimum during the day, in all months of the year. This peculiarity in the diurnal variation, being just the reverse of what obtains at low levels, is explained by the circumstance that during the night cold currents of air descend the slopes of the mountain on all hands, and the air currents from higher levels which take their place bring with them their higher velocities. On the other hand, during the day, air currents ascend the slopes of the mountain, carrying with them to the summit their velocities diminished from various causes during the ascent. One effect of these ascending currents carrying with them the higher humidities of lower levels, is illustrated by the partition during the day of the sunshine. Thus, in the nine months of spring, summer, and autumn, there are 306 hours of sunshine before noon, but only 285 hours after it.

The concluding portion of the report deals with the miscellaneous discussions and investigations on the formation of snow crystals, winds and rainfall, diurnal variation of wind direction, the thermal wind-rose, rain-band observations, St. Elmo's fire, thunderstorms, mean temperature of each day in year, hygrometry, optical phenomena, and earth currents in the telegraph cable; but as the results of these have from time to time appeared in NATURE, further reference to them is unnecessary.

We would draw special attention to the Observatory log-book, pp. 316–56, which is a remarkably readable, as well as an instructive, document. The following extract presents an interesting side of the Observatory work in this isolated and exposed situation:—

"*January* 26, 1884.—In forenoon wind backed from south to south-east and blew hard. As the drift made it impossible to read thermometer at 1 p.m., Mr. Omond and Mr. Rankin went out tied together, but found it impossible to go farther than the end of the snow porch with safety; at 4 p.m. they got as far as the box, but could not see instruments as the drift blew up in their faces. No temperature observations were taken till 10 p.m., when wind moderated and observations were resumed. There was thus a gap in the temperature observations from noon to 9 p.m. inclusive. Quarter and half hourly barometer observations were taken in afternoon. Lowest recorder reading (corrected to 32°) 23·173 inches."

Optical phenomena, including after-glows, halos, mock suns, mock moons, glories, &c., have been fully observed, and the results are in many respects of the greatest interest. The following graphic account of St. Elmo's fire will be read with interest:—

"*October* 29, 1887.—At 1h. 5m. a.m. St. Elmo's fire was seen in jets 3 to 4 inches long on every point on the top of the tower and on the top of the kitchen chimney. Owing to the number of jets on each cup of anemometer, the instrument was quite ablaze. On the kitchen chimney the jets on the top of the cowl were vertical, and those on the lower edge of same horizontal. The fizzing noise from the different places was very distinct. While standing on office roof watching the display, the observer felt an electric sensation at his temples, and the second assistant observed that his (the observer's) hair was glowing. While standing erect the sensation and glow lasted, but on bending down they always passed off. On raising the snow-axe a little above his head, a jet 2 to 3 inches long shot out at the top. At 1h. 15m. a.m. the fire vanished from every place at the same instant. A shower of snow and snow-hail (conical) was falling at the time, and the wind was variable south to south-west breeze—very flawy."

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

THE Permanent International Committee for the prosecution of the photographic chart of the heavens, has met, deliberated, and adjourned. It will be generally admitted, when the result of their discussions has been digested, that the promise of much good work has been given, and the accomplishment of a photographic chart been brought nearer within the scope of practical astronomy. At a future opportunity we hope to give in some little detail an outline of the arguments that have succeeded in causing deviations from the plan originally contemplated. Now, we can only give a brief account of the report of Admiral Mouchez, and a summary of the more important results which have received the support of the Committee.

Admiral Mouchez's report may be considered as divided into three parts: first, that relating to the installation of the photographic instruments; second, to the methods and apparatus necessary for the measurement of the negatives; and third, the manufacture and employment of *réseaux*. From the first of these sections we learn that all the participating Observatories are provided with satisfactory telescopes; but, unfortunately, the grave political events which have disturbed the Chilian Government are likely to operate adversely on the scientific advance of that country, and the station at Santiago may possibly have to be abandoned, M. Maturana, the Chilian astronomer, having received orders to report himself to his Government for military service. Some delay has been occasioned at Rio de Janeiro by the removal of the Observatory to a distance of six or eight kilometres, where, under more favourable atmospheric conditions, the photographic equatorial is being erected. With these exceptions, the Observatories declare themselves all ready for work.

With regard to the second section, we learn that the construction of the measuring instrument, generously offered by M. Bischoffsheim, has been delayed, owing to difficulties of correspondence between MM. Gill and Kapteyn; but now that these gentlemen have had an opportunity of deciding the particular form the instrument should take, it will be rapidly proceeded with, and it is believed that the employment of such an instrument will greatly abridge the difficulties of measurement and calculation.

The manufacture of the *réseaux* has been definitely abandoned by the authorities at Berlin, but M. Gautier has constructed an instrument for the ruling of these necessary adjuncts, and he is now in a position to supply those Observatories which have failed to obtain *réseaux* from M. Vogel.

Three important points have been before the Committee, and it has been found necessary on these points to modify the decisions of previous Committees. The first of these referred to the desirability of obtaining on the plates of shorter exposure, destined to form a catalogue, impressions of star images with one-fourth of the exposure found necessary to secure the impression of an eleventh magnitude star. The result of this decision would have been to give a picture of the heavens obtained with an exposure of a minute and a half, or a minute, or even less. This project has now been abandoned in favour of one arranged to give impressions, very feeble in character, of stars of the eleventh magnitude, while on the same plate will be exhibited a picture of the heavens obtained with an exposure of twice the length necessary for producing star images in the former case. Thus we may expect on these catalogue plates, exposures of three to four minutes, and also of six to eight minutes.

Another interesting point, over which excitement waxed keen, arose on a proposal of M. Henry to substitute three exposures of equal length, arranged at the angles of an equilateral triangle whose sides are about 5″, instead of one exposure of three times the length. It was asserted that a photograph obtained in this manner, in which each of the exposures was continued for twenty minutes, would exhibit more stars than a photograph with a single exposure of one hour. It was eventually decided that the chart plates taken at the even degrees should have a single exposure, while the exposures of the plates taken at the odd degrees should be left to the discretion of the observers.

The third and last point to which reference may be made here is the length of time to be generally devoted to securing a plate for the chart. It was generally felt that, if the exposures were made long enough to secure the impressions of a fourteenth magnitude star, as generally understood in photometric work, the length of time required would entail too great a strain upon the capacity of the observer. It has therefore been recommended by a sub-committee to confine the exposures to about forty minutes, which, it is believed, will be about eight or ten times the exposure required to impress the image of an eleventh magnitude star on plates as at present employed, and under the ordinary meteorological conditions of the Paris Observatory.

CRYSTALS OF PLATINUM AND PALLADIUM.

I HAVE found that crystals of the metals platinum and palladium are easily prepared as follows:—

A ribbon of the pure metal is stretched horizontally between two binding screws. Upon the ribbon, some topaz, reduced to a fine powder, is dusted. A current is now passed through the ribbon, of a strength sufficient to raise it to a bright red heat. In about half an hour's time, if the current be stopped and the ribbon examined with a microscope, it will be found that very small brilliant crystals cling here and there to projecting points on the partially decomposed topaz. If the heat be maintained, these crystals steadily grow; in about two hours some will have attained to a size of about 0·1 mm.

Platinum was the first substance which I crystallized in this manner. When the topaz was scraped off the ribbon, the under surface of the caked fragments was found covered with a grey deposit of the small crystals. Under the microscope, using a 1-inch objective, these present a very brilliant appearance. They are opaque, and show a high metallic lustre, like that of clean mercury, but are somewhat whiter in colour. The faces are clean, and sharply defined. Their crystallographic system is very evidently cubic. The prevailing form is the octahedron, or some modification of it. When the octahedron is perfect, the triangular faces are equilateral. Strings of octahedra occur, after the manner of magnetite. A common modification of the octahedron is that well known in the case of alum, in which two opposite faces of the solid are dominant, giving a tabular form. Very thin hexagonal tables also occur, suggesting, at first sight, dimorphism. These, in the opinion of Prof. Sollas, might be referred to an extreme development of the alum habit. Many observations support this view, which seems preferable to supposing dimorphism.

Hemihedral forms occur, principally the tetrahedron and modifications of it. The combination of tetrahedron and cube, the tetrahedron being dominant, is observed. Acicular forms are also present, but are scarce. The following observations bear upon the nature of these crystals.

Treated in hot hydrochloric, sulphuric, or nitric acid, they are unattacked. They also appear unaffected in cold hydrofluoric acid. In hot hydrofluoric acid they seem slightly attacked. They are slowly but completely dissolved in boiling aqua regia, from which a precipitate of ammonium platinic-chloride is obtained on the addition of ammonium chloride. They are not attracted by the electro-magnet, and conduct electricity (shown by touching the opposite extremities of a crystal with fine needles in circuit with a battery and galvanometer). When crushed between two glass slips they spread out, giving the glass the appearance of a mirror. Although in this way much extended, they show no appearance of cracking upon the edge. They are therefore very malleable. Their melting-point approximates to that of platinum, for a ribbon with some of these crystals clinging to it may be raised to its melting-point, when some of the crystals will be found partially fused. The ribbon upon which crystals have been formed presents a roughened appearance.

The bodies present at their formation are platinum, fluorine, silica, alumina, and a trace of iron. From the physical characters observed, however, the crystals are certainly metallic, and in fact can only be platinum, probably in a very pure state. It would appear that fluorine, liberated at a high temperature from the topaz, attacks the platinum, forming a fluoride, which again breaks up, depositing the crystals.

M. Moissan has already observed that the tetrafluoride of platinum when reduced leaves distinct crystals of the metal (NATURE, vol. xli. p. 118). M. Moissan obtained the fluoride by the direct action of dry fluorine on platinum at a low red heat, and mentions that this body splits up again at a higher temperature. The position of the greater part of the crystals in the present experiment, close to the surface of the platinum, is in agreement with the instability of the fluoride at a high temperature. It seems necessary to suppose the formation of the fluoride to occur further away from the ribbon where the temperature is suitable. So that it appears that a considerable volatilization of the platinum takes place. It is remarkable that, if the conditions are such that the temperature throughout the mass of the mineral is uniform, the crystals of platinum are not formed. Thus, if powdered topaz is rolled up tightly in a tube of platinum, and this tube heated for a considerable time by the passage of a strong current, the mineral is decomposed, there is considerable loss of weight, but no platinum crystals are formed. On the other hand, acicular crystals, which are colourless and transparent, are found under these conditions plentifully intermixed through the partially fused *débris*.

It appeared probable that some related metals might afford crystals in the same manner. Palladium and iridium suggested themselves. The latter I have not yet been successful with, owing to the difficulty of obtaining a piece of suitable dimensions. Palladium ribbon was easily obtained, Messrs. Johnson, Matthey, and Co. supplying this and the other pure metals.

The ribbon of palladium was treated in the same manner as the platinum. Crystals were easily formed.

These are very similar in colour and lustre to the crystals of platinum. In form they appear isomorphous; I have not been able to detect any difference, so that the foregoing remarks on the form of the platinum crystals apply to these also. It seems reasonable to suppose that a tetrafluoride of palladium is concerned in their formation.

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DR. NANSEN ON GLACIATION.

IT may be worth while to call attention to one or two points of scientific interest in connection with the results obtained by the heroic band of Norwegians, whom Dr. Nansen led across Greenland, as set forth by the chief of the expedition in the appendix to the "First Crossing of Greenland" (Longmans, 1890).

(1) The importance of the internal heat of the earth and its conductive transmission to the contact-plane, as a factor in the mechanics of glaciers, for which I contended eight years ago, may be said to be now established. The

views of Tyndall and Helmholtz as to the important part played by liquefaction of the ice under pressure and regelation, with all that follows from this law, have also received important corroboration.

(2) All that Dr. Nansen has recorded of the snow-covered "inland-ice," goes to show how important a factor a loose covering of snow is in dispersing the solar rays, and so preventing the transmission of a large part of the *luminous* energy into the ice, where, on the "greenhouse principle" (see NATURE, vol. xxvii. p. 554) it would otherwise be converted into dark heat by absorption, and so facilitate the "flow" of the ice.

(3) The interpretation of the observed facts of the Waigatz Sound (about 71° N. lat.), quoted by Dr. Nansen from Prof. Helland, must be received with some caution. It is based largely on the assumption that the basalt capping of the district referred to was once a *continuous sheet*, whereas it is just as likely that the portions of the basalt cap now observed may be only originally separate tongues of the lava-flow, and that the soft Tertiary strata have been worn out by the action of frost and running water as an ordinary "fjord." Erosive action of glacier-ice seems here unnecessarily invoked. Even supposing these strata to be of an age so late as the Miocene, there is a wide berth in *time* for the action of running water, aided and supplemented by the disintegrating action of frost, which Dr. Nansen's own record shows, in the present condition of things, to be (in the interior) often some degrees below the freezing-point of mercury in such latitudes. Then, again, there is the important consideration, which Dr. Nansen touches upon but slightly, that the Miocene period of Greenland may not have synchronized with that period in more southern latitudes. The *relative* value of the different periods of the Tertiary age, as affected by latitude, is a matter, which, so far as I know, has not received from geologists anything like that consideration to which it is entitled, though the reason may not be far to seek.

(4) The extension of the lower parts of the ice-sheet into the valleys to form glaciers has, I think, been over-estimated as an agent of *erosion*, insufficient account having been taken of the expenditure of mechanical force in the shearing of the ice. This argument has been more fully worked out by myself in my paper on the "Mechanics of Glaciers" (*Q.J.G.S.*, vol. xxxix. pp. 64, 65).

(5) The summary manner in which the formation of "*åsar*" is dealt with (*op. cit.*, p. 494) seems to be based upon the assumption that they were formed beneath the ice-sheet; but this can scarcely be said to be established. Against the results arrived at by Dr. N. O. Holst, of Stockholm, from an extended investigation of these phenomena in Sweden, the non-observance of surface-rivers on the Greenland ice-sheet, rather late in the season of the year, by Nansen's party, can scarcely be said to militate very strongly, seeing that such rivers were observed by Nordenskiöld. The *åsar* are best known in formerly glaciated regions of more southern latitudes, successive zones of which would constitute the *marginal* areas of the ancient ice-sheet during its gradual diminution and the retreat of the ice from lower to higher altitudes; while a different configuration of the country would furnish the necessary moraine material, which would appear, from Dr. Nansen's observations, to be almost wanting in Greenland, though Dr. Holst saw a good deal in the summer of 1880 (see the *American Naturalist* for August 1888, p. 707).

A. IRVING.

NOTES.

LAST week men of science and the general public were sincerely sorry to receive bad reports as to the state of Prof. Tyndall's health. On Wednesday there was a severe crisis in his illness, but afterwards hopeful symptoms manifested themselves, and these, we are glad to say, have since been maintained.

THE vacancy in the position of Director of the Royal Meteorological Institute of the Netherlands, caused by the death of Prof. Buys-Ballot last year, has been filled up by the appointment of Dr. Maurice Snellen, who has for many years been connected with the Institute. Dr. Snellen represented Holland at the Meteorological Congress of Rome in 1879. He was the chief of the Dutch Expedition to Dickson Harbour in 1882, to take part in Weyprecht's scheme. It will be remembered that the Expedition was caught in the ice, and eventually had to abandon the ship and return without ever reaching its destination.

THE forthcoming International Ornithological Congress at Budapest is expected to be a great success. It will be held from May 17 to 20. There will be various interesting exhibitions, and the following lectures will be delivered:—On the life of West African birds, by Alexander Homeyer, of Greifswald; on the results of observations upon the passage of birds, by Otto Herman; on the life of birds in Arctic Norway, by Robert Collett, of Christiania; and on the life of Alpine birds, by Victor zu Tschudi Schmidthoffen. After the close of the proceedings expeditions will be made to various parts of Hungary.

THE Medical Faculty of Queen's College, Birmingham, with the contents of the museums and other property belonging to it, is to be transferred to Mason's College, where for some time a part of its work has been carried on. This has been formally decided by the Council of Queen's College, whose action has been ratified by the governors of the institution. The decision is an important one, and may prove to be the first step towards the establishment of a University for the Midlands. During the last few years there has been a great increase of medical students, and this has rendered the present accommodation much too small, whilst the Queen's College authorities have no funds available which would permit them to carry out the necessary reconstruction. Moreover, it is felt that the systematic part of medical education should be carried out under the control of one Board, and not as at present under the direction of two independent Councils. New buildings, adjoining those of the Mason College, and connected with them, but having also an independent entrance, will be erected for the School of Medicine, and in the plans due provision will be made for the largely-increased number of students whom it is expected the more perfect equipment and enlarged facilities of the new school will attract.

THE Council of the Society of Arts, acting under the provisions of the Benjamin Shaw Trust, have offered two gold medals, or two prizes of £20 each, to the executive committee of the Congress of Hygiene and Demography, for any inventions or discoveries of date subsequent to 1885, exhibited at or submitted to the Congress, and coming within the terms of the Trust. Under the conditions laid down by the donor, these prizes are to be offered for new methods of obviating or diminishing risks incidental to industrial occupations.

STEPS are being taken in Paris to prepare the way for the holding of an International Colonial Exhibition next year on the Champ de Mars. According to the Paris correspondent of the *Times*, the sections would be geographical, not political, all the West Indies, for instance, forming one section, all India another, and so on. Specimens of all the native populations would be brought over and housed as at their homes, and two Congresses—a Colonial and an Ethnographical—would be held.

AN International Agricultural Congress will be held at the Hague in September next, from the 7th to the 12th. A Commission will be appointed at the Hague to arrange for the reception of the members.

WE regret to have to record the death of Mr. Tuffen West, on March 19, at his residence, Furnell House, Frensham, in his sixty-eighth year, after a very painful illness. Having lived in great seclusion for many years, owing to infirmities of various kinds, Mr. West was but little known to the present generation of naturalists. Twenty or thirty years ago, a descriptive work on natural history was scarcely considered complete unless illustrated by him; and scientific magazines and the publications of the learned Societies bear testimony alike to the marvellous accuracy of his pencil and to the artistic feeling which characterized all his productions.

THE law transferring the U.S. National Weather Service from the War Department to the Agricultural Department will take effect on July 1. The *New York Tribune*, commenting on this change, says that Congress, in directing it, was probably "governed by the obvious fact that meteorological work is essentially civilian, and not military, in character." The *Tribune* urges that great care should be taken in the selection of a new chief. "Congress," it says, "has properly provided that the daring and skilful commander of the Fort Conger Arctic Expedition of 1881-84 shall, after the Signal Corps is relieved of its meteorological duties, retain his present military rank and emolument. No element of personal reproach or hostility has entered into the movement for the transfer, so far as we know. The change thus involves no indignity to that gallant and accomplished officer and popular hero. This is a fortunate phase of the matter. And since several of General Greely's most valued assistants either gain promotion in the reorganized branch of military service in which they intend to remain, or else go with the observers and clerks into the new civil bureau, the President will experience less embarrassment than would otherwise be the case in selecting the new Superintendent."

MR. C. S. MIDDLEMISS, of the Geological Survey of India, now in Hazara, has been appointed geologist with the Black Mountain Expedition.

THE *Pioneer* of Allahabad expresses a hope that the opportunity offered by the expedition into the Black Mountain will be utilized for exploring the unknown country lying to the north. Beyond Thakot the very course of the Indus is not certain, and exploration in this direction will be full of interest. It is quite certain that there is no easy passage into Cashmere from the west, or southwards from Banji towards Thakot, but British surveyors must feel it a reproach that the maps should be blank for even 150 miles. The valleys are said to be sparsely populated, and the hills rugged in the extreme; but a party with an energetic leader and a sufficient escort should find no difficulty in making their way up the Indus Valley. There will not on this occasion be any necessity for evacuating the Black Mountain district in a hurried manner, and it would have a good effect along the border if the country northwards to Gilgit were explored.

THE South London Entomological and Natural History Society will hold its annual exhibition at the Bridge House Hotel on April 15 and 16. According to the *Entomologist*, there is little doubt that this Exhibition will be "quite equal to, if it does not surpass, any of the Society's previous achievements."

WE have to record the publication of a new Italian monthly scientific journal, *Neptunia*, which takes the place of *Notarisia*. It is devoted to "the study of science pure and applied to the sea and to marine organisms." It is edited by Dr. D. Levi-Morrenos, and three numbers have already appeared. Fresh-water as well as marine Algæ are also taken under its cognizance; and, in the numbers already published, appear papers on Conjugation in the Zygnemaceæ, and on the Cystocarps and antherids of *Catenella Opuntia*, by our countrymen Mr. W West and Prof. Harvey-Gibson.

THE editor of *Neptunia* announces a project for the establishment of a marine station at Sebastopol. The plan followed in the erection will be that of the Zoological Station at Naples, but on a smaller scale. The building will consist of three stories, of which one will be devoted to the aquarium, another to the laboratory, and the third to the library, the museum, and the private rooms of the Director.

THE Shanghai papers report that an extensive collection of rare and beautiful butterflies, collected by the late Captain Yankowsky, the commander of a merchant steamer on the China coast, was recently sold by public auction. There was a good attendance and some spirited bidding. But the merchant princes and millionaires of the settlement were conspicuous by their absence, and the labour of a life-time was scattered and sold piecemeal. Strange to say, some of the cases containing butterflies which are not very rare went higher than those from the most distant parts of China, from Omiesan, Wusan, and Tachin-lu in the west of Szechuen. The chief purchasers were Mr. Carl Bock, who was present on behalf of the Shanghai Museum, and Inspector Howard, who bought on his own account. The entire collection fetched only Tls. 262 (about £50).

PERHAPS the most important addition that has been made to the interesting collection of animals in the Calcutta Zoological Gardens since its establishment fifteen years ago is the Greater Bird of Paradise (*Paradisea apoda*, Linn.). The committee of management had long been desirous of acquiring a specimen, but owing to the great rarity of the species, and the very great cost of those that are on rare occasions offered for sale, their efforts to procure one had hitherto proved unsuccessful. At last, however, they have succeeded, thanks to the liberality of the Maharajah Bahadur of Dumraon, who is a great patron of the institution.

MR. LITTLEDALE, of Baroda, has made a proposal that the Bombay Natural History Society should carry out certain interesting experiments with a view of ascertaining whether several of the game birds which are common in other parts of the country could be successfully introduced into the neighbourhood of Bombay, such as the chukor, black partridge, and Bengal florican. A special meeting of the Society was convened for the consideration of this proposal, which also includes the possible introduction of several of the African antelopes, and it was unanimously agreed to. Experiments of this character are of the greatest interest to naturalists as well as sportsmen.

A CLIMATOLOGICAL table issued by the German Meteorological Society shows that the mean temperature for the year 1890 ranged from 44°·1 F. at Königsberg to 50°·2 at Aix-la-Chapelle and Cologne, and 50°·4 at Strassburg, the mean for the Empire, deduced from the observations at twenty-five selected stations, being 47°·9. In January the mean ranged from 26°·2 at Königsberg to 35°·8 at Aix-la-Chapelle, the general mean being 31°·1. July had from 63°·1 at Hamburg to 67°·1 at Frankfort-on-the-Maine and Freiburg, the mean for the whole being 65°·2. The rainfall varies between 19·69 inches at Posen to 50 inches at Freiburg, but only four stations have more than 30 inches, and only eleven receive more than 25 inches, the mean amount for the Empire being 26·58 inches. For the sake of comparison we give the following results calculated from the publications of the Meteorological Office. The mean tempera- of the British Isles for the year 1890 was 48°·0, ranging from 45°·7 in the north of Scotland to 51° in the Channel Islands.

January had a mean of 42°·6 (38°·3 at Hawes Junction and 38°·4 at Braemar, to from 46° to 48°·3 at several stations in the south-west of England), and July 56°·9 (from 51°·5 at Sumburgh Head and Glencarron to 60°·9 at Southampton). The two months it will be remembered were exceptional in these islands, the former being unusually mild, and the latter exceptionally cold. Although, therefore, there was practically no difference between the mean temperature of the two countries for the whole year, Germany was 11°·5 colder in January and 8°·3 warmer in July. The rainfall of the United Kingdom was 33·4 inches, the eastern districts of England having from 22 inches to 25·4 inches, and elsewhere from 32·9 inches in the Channel Islands to 52·8 inches in the west of Scotland, and 55·1 inches in the north of Scotland. London was rather more than a degree warmer than Berlin for the year, and nearly 13° warmer in January, the means being, London, January 43°·7, July 60°·4, year 49°·5; Berlin, respectively 31°·1, 66°·2, and 48°·4. The former had 22·78 inches of rain, the latter 23·63 inches.

At the meeting of the Linnean Society of New South Wales on February 25, Mr. R. Etheridge, Jun., in continuation of former notes, read a paper on Australian aboriginal stone weapons and implements. Among the objects he exhibited and described were stone knives from Northern Australia; small and beautifully-formed spear-heads from Kimberley; larger lanceolate spear-heads from Nicholson River and Settlement Creek, North-West Carpentaria; and talismanic stones from New England and North Queensland, the latter a very interesting tael formed of two rock crystals joined by a gum-cement mixed with human hair.

A Royal Commission on Vegetable Products has just been at work in Victoria, and the result has been to bring together a valuable array of facts regarding what is called the "scent farming" industry in that colony. Mr. Shillinglaw, the secretary to the Commission, has just published a *précis* of the proceedings, which is extremely interesting, as conveying some idea of the extent to which the cultivation of perfume plants has been carried in Victoria. The Commissioners during the course of their inquiries, investigated the Dunolly Perfume Farm, and also drew largely from the experiences of Mr. Joseph Bosisto, the originator of the scent farming industry in the Wimmera district. The general result of the inquiry seems to be that the soil and climate of Victoria are particularly well suited for the cultivation of scent-producing plants.

Messrs. B. Westermann and Co., New York, have sent us the first part of what promises to be an important work on "The Fishes of North America that are caught on Hook and Line." The author is Mr. W. C. Harris. The coloured illustrations are executed most carefully.

A "Fauna of Normandy," by M. Henri Gadeau de Kerville, has been in course of publication in Paris since 1888. Two volumes have been issued—"Mammalia" and "Birds." There are no engravings, and the work consists essentially of an enumeration of the animals observed in Normandy, with bibliographical references, and some notes on modes of life, instincts, and general biology.

The fourth volume of the *Internationales Archiv für Ethnographie* opens with a double number of great interest. Dr. Masanao Koike gives an excellent account of the observations made by him during a residence of two years in Corea. This paper, written originally in Japanese, has been translated into German by Dr. Rintaro Mori. A description of the Corean collections in the ethnographical museum of Leyden is contributed by Dr. J. D. E. Schmeltz; and Dr. A. Baessler makes some valuable additions to our knowledge of the ethnography of the East Indian Archipelago. Prof. H. H. Giglioli has an interesting paper on two ancient Peruvian masks made with the facial portion of human skulls. The number, as usual, is admirably illustrated.

Mr. Philip B. Mason has an interesting little note in the new number of the *Zoologist* on a question which has lately been discussed in that periodical—whether squirrels remain active during the coldest weather. Mr. Mason is able to state that during the whole of the recent severe and prolonged frost several squirrels which had been accustomed to climb to the nursery window of Drakelowe Hall, Burton-on-Trent, where they were fed, continued their visits during the whole of the time, and seemed to be as lively as usual.

The first number of a new quarterly magazine for conchologists has been issued. It is called the *Conchologist*, and promises to be of much service to the class of students to whom it is specially addressed. If adequate support is received, the magazine will be issued as a bi-monthly in 1892.

We note the appearance of a new trade journal, the *Optician*. It is intended to act as the organ of the optical, mathematical, philosophical, electrical, and photographic instrument industries, and also to present a review of the jewellery and allied trades.

The Royal University of Ireland has issued its Calendar for the year 1891. We have already noted that the papers set at the examinations in 1890 are published in a separate volume, forming a supplement to this Calendar.

Messrs. Dulau and Co. have issued a catalogue of the botanical works which they now offer for sale.

At the last monthly meeting of the Manchester Geological Society Mr. Tonge read an interesting paper entitled "Coal-Mining in 1850 and 1890: a Few Contrasts." He said that during the forty years from 1850 to 1890 progress had not been more marked in any direction than in the industry of coal-mining. Not only had new fields been discovered which previously were not even suspected, not only had greater depths been reached, but far greater quantities of coal had been raised; and, what was more important, the collier's life had been made more secure and his work more pleasant, whilst his health, mental powers, and moral character had been more carefully studied and protected. In 1851 there were rather more than 50,000,000 tons of coal and other minerals raised; in 1889, 185,187,266 tons. In 1851 there were 216,217 persons employed above and below ground in coal-mining; in 1889, 563,735. In 1851 there were 984 deaths caused by accidents in and about mines, being at the rate of 4·56 persons per 1000 employed; in 1889 there were 1069 deaths, being at the rate of 1·88 per 1000 employed. In 1851 there was one death from accident for every 219 persons employed; in 1889, one for every 530, the degree of safety being two and a half times greater in 1889 than in 1851. He attributed the improved condition of things largely to beneficent legislation, to scientific and mechanical discoveries, bringing improvements in machinery, ventilation, and lighting, and to greater care in the use of explosives. The collier's labour had been considerably lightened. He had not such long distances to waggon his coal; he had not to go to the surface, as in former times, to look for props, and perhaps saw them for himself. The travelling roads and working places were in much better condition; and the system of working, being mostly long-wall, enabled him to get his coal with much greater ease, and to get a greater quantity than by the pillar and stall system which in 1851 was so much practised.

At the meeting of the Société Chimique of February 27, M. Hanriot communicated the fact that he had repeated the work of Messrs. Mond, Langer, and Quincke, an account of which was given in NATURE, vol. xlii. p. 370, upon the remarkable compound of nickel and carbon monoxide, $Ni(CO)_4$. He finds that it is a most highly poisonous substance, far more deadly than carbon monoxide itself. Blood poisoned by means of it exhibits the characteristic absorption-spectrum of blood containing carbon monoxide. The oxygen of the air diminishes somewhat the poisonous action of the compound, owing to the fact that it rapidly promotes dissociation into metallic nickel and carbon monoxide.

THE mineral hornblende has been artificially reproduced in well-formed crystals by M. Kroustchoff, and an account of his experiments is communicated to the current number of the *Comptes rendus*. The last few years have been most fruitful in mineral syntheses, so much so indeed that there remain very few of the more commonly occurring rock-forming minerals which have not been artificially prepared in the laboratory. M. Kroustchoff, who not long ago described a mode of preparing most perfect crystals of quartz, has made many attempts to reproduce hornblende, and has at length succeeded by the adoption of the following somewhat remarkable process. This process essentially consists in digesting together for a long period of time, *in vacuo*, and at a high temperature, the various oxides contained in natural hornblendic amphiboles, in presence of water. Small flasks of green glass were employed, each of which was exhausted by means of a Sprengel pump after the introduction of the substances to be digested together. The ingredients digested consisted of (1) a dialyzed three per cent. aqueous solution of silica; (2) an aqueous solution of alumina obtained by dissolving aluminium hydrate in an aqueous solution of aluminium chloride and subjecting the solution to dialysis; (3) an aqueous solution of ferric oxide obtained by the addition of ammonium carbonate to ferric chloride in such quantity as to redissolve the precipitate first formed, and dialyzing the solution; (4) carefully prepared pure ferrous hydrate; (5) lime water; (6) freshly precipitated hydrate of magnesia; and (7) a few drops of caustic soda and potash. The mixture presented the appearance of a gelatinous mud. The exhausted and sealed flasks were placed in a specially constructed iron many-chambered furnace, and heated for three months to a temperature of 550° C. At the expiration of this time the appearance of the contents had entirely changed, having become much darker in colour, and distributed throughout were numerous brilliant little crystals, almost black in colour, and reminding one forcibly of natural hornblende. On systematic examination they were found to consist of flattened prisms identical in character with hornblende. Under the microscope they exhibited the hornblendic yellowish-green colour and pleochroism. Their index of refraction was the same as that of natural hornblende, about 1·65. The angle between their optic axes was found to be 82°; that of natural crystals varies from 80° to 85°. Analyses gave the characteristic amphibolic percentages, that of SiO_2 being 42·3. In addition to these crystals of hornblende it is interesting to note that pyroxenic crystals resembling those of the augite family were also found in the flasks, together with crystals of a zeolite and of a variety of orthoclase felspar. And finally, some exquisite little quartz crystals were observed, showing cavities containing liquids and bubbles resembling those of natural rock crystals.

THE additions to the Zoological Society's Gardens during the past week include an Arctic Fox (*Canis lagopus*) from Iceland, presented by Mr. H. Sacheverell Bateman; a Squirrel-like Phalanger (*Belideus sciureus* ♂) from North Australia, presented by Mrs. FitzGerald; a Lacertine Snake (*Cœlopeltis lacertina*), South European, presented by Mr. J. C. Warburg; a Rhesus Monkey (*Macacus rhesus* ♂) from India; a Common Wolf (*Canis lupus* ♀), European, deposited; two Violaceous Plantain Cutters (*Musophaga violacea*) from West Africa, four Cape Colys (*Colius capensis*) from South Africa, a Carpet Snake (*Morelia variegata*) from Australia, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), a Vulpine Phalanger (*Phalangista vulpina* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

STARS HAVING PECULIAR SPECTRA.—*Astronomische Nachrichten*, No. 3025, contains a note entitled "A Fifth Type of Stellar Spectra," by Prof. E. C. Pickering, and one by Mrs. Fleming, in which she describes the following objects of interest, discovered during an examination of the photographs of stellar spectra taken at Harvard College Observatory with the 8-inch Draper telescope:—

Designation.	R.A. 1900.	Decl. 1900.	Mag.	Description of spectrum
	h. m.	° ′		
D.M. + 63°·83 ...	0 37·5	+64 14	9·5	Bright lines.
Cord. G.C. 23416	17 12·1	−45 32	7·2	,, ,,
[Aquarius]	20 41·2	− 4 26	Var.	III. Type; G and *h* bright.
[Delphinus]........	20 43·1	+18 58	Var.	,, ,, ,,

The similarity of the spectra of the two stars in Aquarius and in Delphinus to that commonly shown by variables of long period led Mrs. Fleming to suspect the variability, which was confirmed by an examination of photographs taken on various dates. With respect to this point, Prof. Pickering remarks, in the note to which reference has already been made:—"Probably most of the variable stars of long period give a spectrum resembling that of *o* Ceti, and having the hydrogen lines G, *h*, α, β, γ, and δ bright about the time of maximum. When the photographic spectrum is faint, only the brighter lines G and *h* are visible. Photographs have been obtained of forty-one of these objects, ten of which have been discovered by means of this peculiarity of their spectrum."

Following the classification suggested by Prof. Lockyer in the Bakerian lecture for 1888, spectra of Secchi's third type are divided into sub-groups, of which α Orionis, α Herculis, S.D. − 2°·3653, and *o* Ceti, are given as examples.

The fifth type of stellar spectra which Prof. Pickering proposes to establish will contain planetary nebulæ and bright-line stars, and is therefore equivalent to Lockyer's Group I. Photographs of the spectra of sixteen planetary nebulæ taken at Harvard resemble each other closely, and consist mainly of bright lines and bands. The bright-line stars are divided into three classes, in the first and second of which λ 469 is the most conspicuous line, whilst the third class is distinguished by the distinctness of a line at λ 464. A comparison shows that the position of the lines of these stars and planetary nebulæ is the same as in Orion stars, but the lines are dark instead of bright in the last-named bodies. Only a three-figure reference is now given to the positions of the lines, and more precise measures of wave-length will be required to show whether the slight differences which exist are real or not. The irregular distribution of these objects in the heavens is another point of resemblance. Four-fifths of the stars of the Orion type are found in the Milky Way. A similar distribution of the planetary nebulæ has long been recognized. By arranging thirty-three bright-line stars according to their distances from the pole of the Milky Way (R.A. 12h. 40m., Decl. + 28°), and its ascending node (R.A. 18h. 40m., Decl. 0°), Prof. Pickering shows that their distribution is also strikingly irregular. The average value of the galactic latitude of all the stars is 2° 6′. Groups occur in Argo, Scorpio, and Cygnus. The approximate longitudes are 257°, 313°, and 42°. The number of stars in these groups is 7, 5, and 8, or 20 in all. Each group is included in a circle 8° in diameter, and of all the members only three are isolated, being distant 10° or more from any of the others. It therefore follows that one half of the known bright-line stars are included in an area of about one three-hundredth of the entire sky. It is gratifying to find the suggestion, that stars with bright lines are physically similar to planetary nebulæ, supported by the numerous observations made at Harvard College Observatory. The anomalous character of the spectra of the Orion stars has long been a matter of discussion. Perhaps the agreement as to position of the dark lines in these stars with the bright lines in others will enable their place in stellar evolution to be determined.

The Nebula near Merope.—In *Astronomische Nachrichten*, No. 3024, Prof. Pritchard states that the nebula close south and following Merope, which Prof. Barnard observed on November 14, and announced as a discovery in *Astr. Nach.*, No. 3018, is plainly impressed on photographic plates taken at Oxford Observatory, with exposures varying from 20 to 120 minutes. Prof. Barnard believed that the exposure which would be necessary to produce an image of the nebula would so extend the diameter of the image of Merope that the two objects would coalesce. Prof. Pritchard, however, finds that stars of the 14th magnitude are perfectly distinct on plates taken with the above-mentioned exposures, and that the stellar disks of Merope in such cases vary from 20″ to 30′ in diameter. Since the supposed new nebula is some 40″ distant from Merope, the separation of the two objects is always plainly marked.

Comet *a* 1891.—The comet of which Mr. Denning announced the discovery in the last issue of Nature (p. 516) is probably identical with one independently discovered by Prof. Barnard, of Lick Observatory, on Tuesday, March 31.

BIOLOGICAL NOTES.

The Eyes in Blind Crayfishes.—The minute structure of the eyes of two species of blind crayfish have been very thoroughly investigated by G. H. Parker, under the direction of Prof. Mark. Mr. Parker is the Instructor in Zoology at the Harvard College Museum. The species were *Cambarus setosus*, a new species recently described by Dr. Walter Faxon, from caves in South-western Missouri, and *C. pellucidus*, Tellk., the well-known species from Mammoth Cave. After a short *résumé* of the investigations of Newport and Leydig, the author states that the principal questions concerning the eyes of blind crayfishes turn on their amount of degeneration; not only has the finer structure of the retina been affected, but the shape of the optic stalks has been altered. The optic stalks are not only proportionally smaller than those of crayfishes possessing functional eyes, but they have in these two cases characteristically different shapes. In crayfishes with fully developed eyes the stalk is terminated distally by a hemispherical enlargement; in the blind crayfishes it ends as a blunt cone. In both forms of crayfishes the optic ganglion and nerve were present, the latter terminating in some way undiscoverable in the hypodermis of the retinal region. In *C. setosus* this region is represented only by undifferentiated hypodermis, composed of somewhat crowded cells, while in *C. pellucidus* it has the form of a lenticular thickening of the hypodermis, in which there exists multinuclear granulated bodies; these are shown to be degenerated clusters of cone-cells. (*Bulletin of the Museum of Comparative Anatomy at Harvard College*, vol. xx. No. 5, November 1890.)

A New Stalked Crinoid.—The United States Acting Fish Commissioner was engaged during 1887 in dredging operations between Panama and the Galapagos Islands. One haul, taken from a depth of 392 fathoms, off Indefatigable Island, one of the Galapagos group, brought up three imperfect specimens of a most interesting stalked Crinoid. At a first glance, it might readily pass for a living representative of the fossil Apiocrinus; but on a closer scrutiny it showed some features which ally it with Millericrinus, and others with Hyocrinus and Rhizocrinus. Prof. Alexander Agassiz, who hopes very soon to publish a detailed account of this form, with figures, thinks that it shows structural features of all the above mentioned genera. It has, like Hyocrinus and Rhizocrinus, only five arms; they are, however, not simple, but send off, from the main stem of each arm, three branches to one side and two to the other. The system of interradial plates is highly developed, as in Apiocrinus and Millericrinus, six rows of solid polygonal imperforate plates being closely joined together, and uniting the arms into a stiff calyx as far as the sixth or seventh radial and to the third or fourth joints of the first and second pinnules. The interradial calycinal plates extend along the arms for a considerable distance beyond the first branch. The stem tapers very gradually, and in its general appearance recalled that of Apiocrinus; it expanded towards the base, but the actual attachment was not found. The stem must have been about from 26 to 27 inches in height; the height of the calyx to the interradials is 7/16 of an inch; its diameter at the inner base of the second radials is 11/16 of an inch, and at the height of the third joint of the second pinnule 1 inch. The arms were about 8 inches in length. It is named *Calamocrinus diomedæ*, after the steamer *Albatross*.

British Marine Algæ.—Almost forty years have passed since the issue of the last part of Harvey's "Phycologia Britannica." The publication of this work extended over five years, and it enumerates 388 species. In the preface to the first volume the author not obscurely hints at the existence of other forms, "some few but distinct looking, preserved in my own and other herbaria," remaining over for further research; but the time for this never came. During the years that have elapsed since 1851, though the labourers in this field of botany do not appear to have greatly increased, the methods of modern research have added to our exact knowledge of the subject, and the investigations of Agardh, Bornet, Flahault, Gomont, Grunow, Schmitz, Reinke, and others, have thrown a flood of light on the minute structure, fructification, and classification of the species of this group. Although the time has not yet, in our opinion, come for a new edition of Harvey's work, yet it is with extreme satisfaction that we notice the publication of "a revised list" of our British marine Algæ, which is the result of the joint labours of Messrs. E. M. Holmes and E. A. L. Batters. None but those who have engaged in such work can appreciate the immense amount of labour and care which is needed to bring such a list even fairly up to date, and when, as in the present instance, most of the additions to the known native species have been made by the individual efforts in collecting of the authors, it must add to the appreciation with which this revised list will be received by botanists. As a matter of necessity, for the present, any classification must be regarded as only provisional, but the authors have been enabled to give the very best one possible, and one quite in accordance with recent investigations. Exclusive of all varieties, the number of species recorded in this revised list is 536, after excluding some six forms, like *Sargassum bacciferum* and *S. vulgare*, which have only been met with as waifs. In the classification of the Cyanophyceæ, the authors have as to the Nostochinæ followed, with few exceptions, the guiding of Bornet, Flahault, and Gomont. In the Chlorophyceæ, while the researches of Agardh and Wille have not been overlooked, yet it can scarcely be doubted but that much new work is needed ere the numerous species of Chætomorpha and Cladophora can be satisfactorily described or arranged. Reinke has been in part followed in the arrangement of the Phæophyceæ, but in some orders of this group a good deal of additional information is needed ere a generally acceptable determination of even the limits of its genera is obtained. In the Rhodophyceæ the classification of Schmitz has been generally adopted, but we still wait expectingly for the final views of Schmitz on the group. In the working out of the details a great deal of trouble has been taken in the determination of the often very numerous varieties, and this will be an important help to the worker. An attempt has been made to give a general idea of the geographical distribution by dividing the coast-line of Great Britain and Ireland into fourteen sections, and then indicating in which of these the species or its varieties are or have been found. In an appendix a list of species known to occur either on the Atlantic shores of France or of Norway, or in the Baltic, and which one might expect to meet with on our own shores, is given, and will prove useful. Possibly Mediterranean forms may yet be met with, other than waifs, on the south-west shore of Ireland, a part of the coast-line hardly as yet investigated. Miss Hutchins collected in portions of Bantry Bay, and Miss Ball in the Youghal district, but the intervening coast has only as yet been casually visited. It is to be hoped and expected that one result of the publication of this most useful list will be to stir up enthusiasm in the collecting and study of our native species. This list appears in the *Annals of Botany* for December 1890.

Comparative Zoology Museum, Harvard.—The Annual Report of the Curator for the session 1889–90 has just reached us, and like all the annual reports from the pen of Alexander Agassiz it is full of interest. The geological section of the Museum, containing the exhibition-rooms and additional laboratories of the department, is now ready for use. On the first floor it contains a large lecture-room, capable of seating 320 students. On the second floor are placed the petrographical laboratories, one for general use, the other for advanced students. In the basement are the chemical laboratories, photographic rooms, &c.; while on a fourth floor are laboratories for the physical geography department. A view of the University Museum, as seen facing the north-west corner, including the newly erected mineralogical sections, is given. On the very evident ground of safety to the collections it has been arranged that no specimens be loaned even to specialists, and that for the future specialists must visit the collections, which will be freely placed in the Museum at their disposal. The reports of the various officers of departments are given, and all will congratulate the Curator on the great progress that this splendid Museum has made.

MECHANICAL TRISECTION OF ANY ANGLE.

THE following discussion gives simple methods of directly trisecting angles which do not exceed 180°. The relations on which the methods are based, hold, however, up to 270°, but it would be difficult to apply them far beyond 180°.

For angles greater than 180°, the excess over 180° can be trisected, and 60° added by laying off on the arc a distance equal to the radius.

Fundamental Relations.

With any angle, PCK, less than 180° (Fig. 1), if one-third of the angle is represented by KCX, and if, from any point I on the prolongation of one side PC, an arc, IKC′, is described with the vertex C as a centre, the chord IC′ makes with CC′ an angle equal to half PCC′, and therefore equal to KCC′, one-third the original angle.

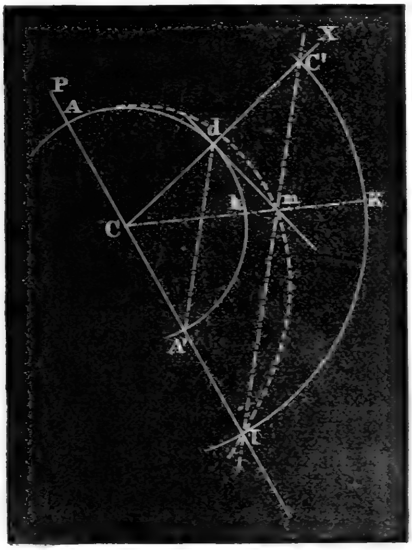


FIG. 1.

The intersection m of the radius CK and the chord IC′ is hence :—

(i.) Equidistant from the points C and C′.

(ii.) On a perpendicular to CC′ at its middle point.

The first relation (i.) gives a means of readily plotting the point of trisection, whether on paper or in the field; the second relation suggests a simple instrument for trisecting an angle without describing an arc.

(1) To lay off one-third a given angle.

Assume any angle, PCK, less than 180° (Fig. 2), and describe an arc with any radius, CI, the vertex C being taken as a centre.

From the point I, where the arc intersects the prolongation of one side, PC, of the angle, draw right lines intersecting the other side, CK. From the points of intersection, m', m'', m''', &c., lay off, in a direction away from I, distances m'E′, m''E″, &c., equal to the distances of m', m'', m''', &c., respectively, from the centre C.

Only such lines need be drawn that the extremities will lie near the arc, both within and without, and the curve plotted by these points intersects the arc at the point C′, trisecting the arc PK.

The trisecting point of the supplementary arc KI lies 60° from C′.

(2) From the second relation (ii.) it is evident that the following simple instrument would accomplish the trisection.

Two equal arms, CI and CC′, are jointed at C (Fig. 3): one arm has a projection forming a right angle at d, and the other arm is prolonged at CA beyond the joint. Points I and C′ on the two arms, at twice the distance Cd from the centre, are connected by an elastic cord.

Placing the point C over the vertex of any angle, PCK, less than 180°, with the edge CA extended along one side, CP, the other arm is moved until the point of intersection, m, of the cord with the perpendicular, dm, is over some point on the other side, CK.

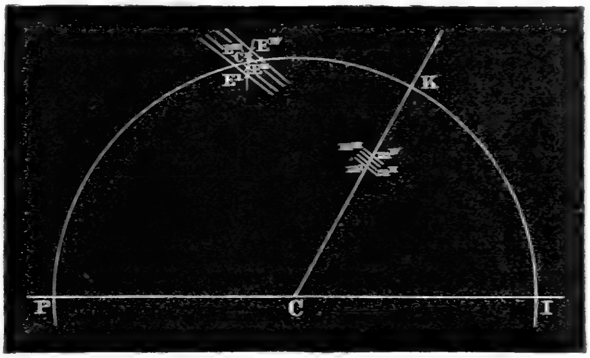


FIG. 2.

A line ruled along the edge CC′ will form an angle with CK equal to one-third PCK.

Instead of a cord connecting I and C′, a ruler may be used. The edges AI and CC′ of the two arms are radial from the centre of motion C.

The angles made by the lines Cm and Im with the right-angled projection dm, correspond to angles of incidence and reflection if a plane mirror facing towards C is substituted for the projecting piece.

An observer sighting on an object K, the arm CA being directed upon another object P, swings the arm Cd until he brings the reflection of the point I in the direction of the point K.

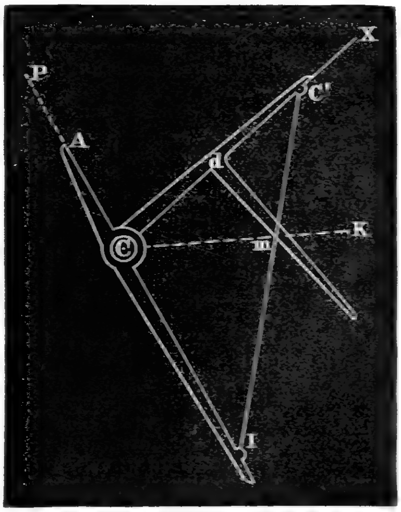


FIG. 3.

Then, sighting in a direction perpendicular to the mirror, he can lay off the angle KCX, one-third the angle PCK.

An instrument of this character suggested the above constructions.

The locus of the point m is partially shown in dotted lines in Fig. 1. Its polar equation is evidently $r = a \sec \frac{1}{3}\phi$, a representing the distance Cd (Fig. 1); ϕ, the angle PCK.

For $\phi = 0$, $r = a$, corresponding to the point A.
For $\phi = 180°$, $r = a \sec 60° = 2a$, corresponding to the point I.
For $\phi = 270°$, $r = \infty$, and the curve becomes asymptotic with the line *dm*, now tangent at A′ to the arc A*d*A′.

The curve, extended beyond A, will have two symmetrical branches with reference to the line AI.

The portion shown in Fig. 1 need alone be considered.

This curve possesses the following property :—

If a line is drawn making any angle, PCK, with CP, and from its intersection with the curve, at *m*, as a centre, an arc be struck with a radius equal to C*m*, its intersection with the arc described with radius CI will determine a point C′, the line from which to the centre C will trisect the angle ACK.

To Prof. John Peirce, of Providence, the suggested use of the curve is due.

A. H. Russell,
Captain U.S. Ordnance Corps.

Providence, R.I., April 13, 1889.

THE ACTION OF THE NERVES OF THE BATRACHIAN HEART IN RELATION TO TEMPERATURE AND ENDOCARDIAC PRESSURE.

(Preliminary Note.)

This work, which has been carried on during the past four or five months, has been chiefly concerned with the influence of temperature on the action of the sympathetic and the vagus, and only incidentally with the effect of endocardiac pressure. A re-investigation of the heat standstill of the heart has also been connected with the work. In most of the experiments tracings of the auricular and ventricular movements were taken simultaneously, and these were supplemented by a series of electrical observations.

Omitting details, the chief results are as follows. The term "vagus" is here used in its anatomical sense to denote the mixed vago-sympathetic trunk.

(1) Both the vagus and the sympathetic have their activity diminished as the temperature is lowered and increased as the temperature rises, whether changes in the rhythm or in the amplitude of the beats be taken as the test of activity. The sympathetic curve, however, falls more steeply with falling temperature than does the vagus curve, so that the vagus is generally still active with a temperature at which the sympathetic has ceased to react.

The secondary augmentor effects of vagus stimulation are extremely well marked at the higher temperatures, and become less conspicuous as the temperature is lowered. Not only are both nerves active on the very threshold of the heat standstill, but during the actual standstill, when it has not been obtained at too high a temperature, stimulation of the sympathetic may rouse the heart to vigorous contraction.

(2) An increase of endocardiac pressure sufficient to abolish the inhibitory action of the vagus leaves the sympathetic still active, and the primary augmentation which, under these conditions, has been seen as a result of vagus stimulation, may be attributed to the sympathetic fibres in the mixed nerve.

When a high pressure is suddenly reduced, standstill of the heart may occur, and this can be removed by stimulation of the sympathetic.

(3) Heat standstill of the heart, when there is no constant stimulus acting, such as a high endocardiac pressure, is always diastolic, and can never be described as "heat tetanus."

(4) Electrical effects, analogous to those described by Gaskell in the quiescent auricle of the tortoise on stimulation of the vagus, and in the toad's ventricle on stimulation of the sympathetic, have been looked for in vain in the heart of the frog and toad during heat standstill and the standstill caused by sudden relief of a high endocardiac pressure. The effects observed were always related to coincident or consequent mechanical changes.

So far as the sympathetic is concerned, I do not know of any previous work on the subject of this note; nor has the influence of temperature on the vagus been before studied by a suitable graphic method, but only to a small extent in any way and by methods yielding very imperfect information.

New Museums, Cambridge. G. N. Stewart.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—An examination for Minor Scholarships in certain branches of Law and Natural Science will be held at Downing College on Wednesday, July 8, and subsequent days. Candidates for Minor Scholarships in Natural Science must be under the age of nineteen at the time of commencement of the examination, and must send a certificate of birth with their other papers. There is no such limit of age for candidates in Law. Further information will be furnished by the Rev. J. C. Saunders, Tutor of the College.

SCIENTIFIC SERIALS.

The *American Meteorological Journal* for February contains :—An account of a lecture on the State Weather Service, delivered by Prof. F. E. Nipher at the request of the Board of Agriculture of Missouri. These services aim at weather predictions for the use of agriculturists and others, and the forecasts are of a more local character than can be attempted by the Central Office. The general use of the telephone is recommended for the purpose.—Wind-pressures and the measurement of wind velocities, by Prof. C. F. Marvin. This paper recapitulates the experiments which have been carried on in the United States, concurrently with others in this country and elsewhere, to determine more accurately than was at first done the constants of the Robinson cup-anemometer. The instrument used was the small pattern adopted by the Signal Service, the cups of which are 4 inches in diameter, and the arms 6·72 inches long, and for this form of anemometer the equation

$$\log V = 0·509 + 0·9012v \log v$$

is recommended to be used instead of Dr. Robinson's original formula $V = 3v$, which is commonly applied to all cup anemometers, regardless of size of cup and length of arm. An account is also given of some experiments carried out at the summit of Mount Washington, with a view to determining the wind-pressure upon large plates, and comparing it with the wind velocity recorded by the cup-anemometer. The writer finds that, with ordinary barometric pressures, the wind-pressure can be satisfactorily obtained from the wind velocity by the following formula :—

$$P = 0·0040V^2S,$$

where V is the wind velocity in miles per hour, P is the pressure in pounds per square foot, and S is the area of the plate.—Meteorological observations taken in four balloon voyages, by W. H. Hammon. The ascents were made in January, March, and April, 1885, by direction of the Chief Signal Officer. The Report contains full descriptions of the instruments and detailed observations during the ascents, which were made during conditions of the atmosphere that were considered likely to elucidate certain points. The ascents were only of slight elevation, and the observations are to be considered only as contributions to the special weather of the respective dates, and not as bearing particularly upon the questions involved in the general circulation of the air.

SOCIETIES AND ACADEMIES.

London.

Physical Society, March 20.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Prof. S. U. Pickering, F.R.S., read a paper on the theory of dissociation into ions, and its consequences. According to this theory electrolytes are entirely dissociated into their ions in weak solutions. This dissociation was held by Arrhenius to absorb heat, and although heat is evolved by the dissolution of hydrochloric acid, &c., it is now maintained that dissolution evolves heat, but that the heat absorbed by the decomposition of the molecule into its atoms is more than counterbalanced by the heat supposed to be evolved by the combination of the atoms with their electric charges. These actions the author considered improbable, and thought that before being accepted, the theory must give satisfactory answers to the following questions: How can matter combine with an affection of matter to produce heat? Whence do the electric charges originate? Why does not the opposite electri fication of the different atoms make them cling more firmly together, instead of dissolving the union between them? And why should an atom which possesses a strong attraction for a negative charge (such as chlorine) go to the positive electrode during electrolysis? When a dilute solution, which is supposed to contain some gaseous molecules, is further diluted, then, according to the theory, some of the molecules are dissociated, and, if heat is absorbed, it follows that the dissociation, and therefore dissolution, of the gas must absorb heat; yet, he said, it can be shown that in some of these cases the dissolution of a gas evolves a large amount of heat. The antagonism between the present and the old electro-chemical theory, according to which the atomic charges are identical with the free energy of an atom, and are the cause of combination, not decomposition, was commented on, as well as the disagreement between the present theory and Clausius' view that there are a few ions or atoms present in a liquid, owing to accidental superheating of some of the molecules. Reasons, however, were adduced for believing the presence of even a few atoms in a solution to be improbable.—A communication on some points in electrolysis was made by Mr. J. Swinburne. Considering a reversible single fluid cell, the author, by a process of reasoning based on Carnot's principle and the conservation of energy, arrives at Helmholtz's equation—

$$E = E_c + \theta\frac{dE}{d\theta},$$

where E is the electromotive force, E_c the part due to chemical action, and θ the absolute temperature. Writing the equation in full, using suffixes n and p to denote the negative and positive plates, it becomes

$$E_n'' + E_p = E_{nc} + E_c + \theta\frac{dE_n}{d\theta} + \theta\frac{dE_p}{d\theta}.$$

He then shows that, by having the two plates in different vessels, and heating them to different temperatures, the Peltier effects represented by $\theta\frac{dE_n}{d\theta}$ and $\theta\frac{dE_p}{d\theta}$, can be determined separately. Similarly, those of a two-fluid battery might be found by arranging the junction of the fluids in a third vessel. After pointing out the desirability that the conditions under which all thermo-chemical data have been obtained, should be clearly stated, he proceeded to show that any cell in which secondary actions occur (as, for example, if the zinc oxide primarily formed by electrolysis were to dissolve in sulphuric acid) must necessarily be non-reversible. He also contended that, in a secondary battery, the formation of lead sulphate on both plates is the essence of the cell's action, and that there is no intermediate formation of PbO. On the subject of so-called "nascent" hydrogen or oxygen, he said that reasoning from the conservation of energy showed that neither could exist. Taking the case of persulphate of iron in dilute sulphuric acid, which is said to be reduced to protosulphate by the "nascent hydrogen" liberated on putting a piece of metal (say magnesium) into the liquid, he said a better explanation of the phenomena would be, that the metal dissolves, if it either reduces the metal or evolves hydrogen; and as the former requires less energy, the reduction takes place, and, when no reducible salt is available, hydrogen is evolved. Evolution of hydrogen and reduction of the persalt are thus *alternative* and *not consecutive* actions. Examining Dr. Lodge's views on the contact E.M.F. between metals, he remarked that, if the tendency of a metal (such as zinc) to oxidize can produce an electric stress or difference of potential which prevents further combination, actual combination must charge the metal if it be insulated. A piece of sodium, however, oxidizes continuously, and therefore should become charged to an enormous potential. As this effect is not known to occur, the author suggested that the Volta effect may be due to films of water, and, in support of this view, adduced the fact that metals when perfectly dry do not combine with chlorine, and that even sodium is not attacked by dry chlorine.—In the discussion on the two papers, Prof. Pickering said the idea of nascent elements had, to a large extent, been given up by chemists, and pointed out that the fact of one reaction taking place rather than another was not merely a question of heat energy, but that a kind of chemical selection was involved. Prof. S. P. Thompson recalled attention to the fact that the products of electrolysis depend on the E.M.F. employed in producing it, and thought the E.M.F. required to produce the various products might be taken as a measure of their affinities. He did not agree with Mr. Swinburne's method of finding the E.M.F. of a secondary battery from thermochemical data, for he failed to see how two similar actions going on at the two plates of a cell could add anything whatever to the E.M.F. of the cell. The President said the question whether the potential difference between two dissimilar substances was due to oxidation or to mere contact could only be decided by direct experiments made in a vacuum, from which all traces of moisture and oxygen had been removed. Without agreeing with Dr. Lodge's view on the subject, he pointed out that the continuous oxidation of a piece of insulated sodium need not necessarily produce a great potential difference, for the combination might merely produce heat.—After Prof. Pickering and Mr. Swinburne had replied to the points raised, Mr. Walter Baily took the chair, and Prof. Perry read a note on the variation of surface-tension with temperature, by Prof. A. L. Selby. Considering unit mass of liquid at constant volume, but variable surface (S) and temperature (t), the author expresses the gain of intrinsic energy due to changes of the variables by

$$dH + dW = kdt + (l + T)\,dS,$$

where dH is the heat absorbed, dW the work done on the film, k the specific heat at constant volume, l the latent heat of extension, and T the surface-tension. This being a perfect differential, it is shown that $T = c - bt$, and $l = bt$, c and b being constants. Supposing c and b to be independent of the specific volume of the liquid it is shown that at the critical temperature $t = \frac{c}{b}$, hence this temperature may be determined by finding the surface-tension at two very different temperatures. Since also $l = bt$, the latent heat of extension is proportional to the absolute temperature. Reasons for supposing b to be independent of the specific volume are given in the paper. Mr. Blakesley described an effect of temperature on surface-tension which he had observed in sensitive spirit-levels. By warming one end of the tube, even by the hand, the bubble immediately moves towards that end. This effect, which might produce considerable error in engineering operations, was, so far as he was aware, not mentioned in the text-books. Prof. Perry remarked that although the volume, temperature, and surface of the liquid had been referred to in the paper, pressure was not mentioned, and on this point he inquired whether the results arrived at were true, quite independent of the pressure.—Prof. S. P. Thompson, read a paper on magnetic proof pieces and proof planes. The distribution of magnetism over magnets has been examined in various ways by different observers, but mostly by observing the force of detachment of either rods, ellipsoids, or spheres, &c., used as proof pieces. In all these cases it was, the author said, difficult to see exactly what was measured, for the presence of the proof pieces altered the thing to be tested. The pull exerted must also depend on the permeability of the piece used as well as on its shape and disposition with respect to the magnetic circuit. He had therefore investigated the subject, by finding the actual distributions, by means of a flat exploring coil and ballistic galvanometer, both with and without the presence of proof pieces of various shapes and sizes. The results show that the perturbations produced by the proof pieces are always large, in some cases the perturbed field about a point being four to six times the unperturbed field. In most cases, however, the ratio of the perturbed to the unperturbed field was constant, so long as the former did not exceed 6000 C.G.S. units. The amount of perturbation was also found to depend on the saturation of the magnet and on whether it was a permanent or an electromagnet. The numbers obtained in various experiments, and curves plotted from such results, were shown. In conclusion, the author said that in using proof pieces, much depended on the accuracy of the contact, but in any case the results attained were not very trustworthy. The flat exploring coil, or magnetic proof plane, however, furnished a satisfactory method of examining magnetic distributions.

Chemical Society, March 19.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Molecular refraction and dispersion of various substances, by Dr. J. H. Gladstone, F.R.S. This paper contains a number of observations of refractive indices, with the optical constants calculated from them, and remarks on their bearing on chemical or physical theory. Observations on carbon bisulphide and benzene confirm the old conclusion that the specific refractive energy of a liquid is a constant not affected by temperature. Data relating to a number of carbon compounds are given; the molecular refraction and dispersion calculated from experiment are compared with those calculated from the values already assigned to the constituent elements. In most cases the experimental and theoretical numbers agree fairly well; in some cases, however, there is a discrepancy. This, of course, points to some difference in constitution. The molecular dispersion of amylene shows the increase for double-linked carbons characteristic of the allyl compounds previously examined, and the allyl and diallyl acetic acids. The xylenes differ considerably in their actual indices, but have almost identically the same molecular refraction and dispersion. The optical properties of safrol agree with the normal value calculated for four double-linked carbons; but anethol, cinnamene, and butenyl benzene, which have the same number of double linkings, give a refraction and especially a dispersion far above that required by theory, thus indicating the presence of carbon in a still more refractive and dispersive condition, to which the author drew attention in 1881. Some of the results afford fresh confirmation of the analogy between molecular refraction and dispersion and molecular magnetic rotation. The nitrogen compounds give results confirmatory of the two values previously assigned to that element in combination. Stannic ethide gives a refraction equivalent for tin 18·1; zincic ethide gives a value for zinc 15·9. The halogen compounds of silicon and titanium gave results confirmatory of the former refractive equivalents; the atomic dispersion of silicon came out about 0·32, and that of titanium about 8·7.—Contributions to our knowledge of the aconite alkaloids: on the crystalline alkaloid of *Aconitum napellus*, by W. R. Dunstan and W. H. Ince. The alkaloid was extracted from the root of *Aconitum napellus* by amyl alcohol. The yellowish crystals melted at 188°·4, and were found to be associated with a small quantity of a gummy amorphous base. They gave, on combustion, numbers agreeing fairly well with the formula $C_{33}H_{43}NO_{12}$, which is that proposed for aconitine by Wright and Luff. The alkaloid is purified by recrystallization from a mixture of alcohol and ether, or by conversion into its hydrobromide and regeneration of the alkaloid from the salt, or by regeneration from its crystalline aurochloride. It crystallizes in tabular prisms belonging to the rhombic system, is very slightly soluble in water and light petroleum, more soluble in ether and alcohol, most soluble in benzene and chloroform, and melts at 188°·5 (corr.). Previous observers state aconitine to be lævo-rotatory; the authors find an alcoholic solution to be dextro-rotatory $[\alpha]_D = +10°·78$; the aqueous solution of the hydrobromide is, however, lævo-rotatory $[\alpha]_D = -30°·47$. The pure alkaloid gave, on analysis, numbers corresponding to the formula $C_{33}H_{45}NO_{12}$. Two crystalline *aurochlorides* are obtained, $C_{33}H_{45}NO_{12}HAuCl_4$ (m.p. 135°·5), and $C_{33}H_{45}NO_{12}AuCl_3$ (m.p. 129°). *Dehydra-conitine* or *apo-aconitine*, $C_{33}H_{43}NO_{11}$, is readily prepared by heating aconitine with saturated aqueous tartaric acid in closed tubes, melts at 186°·5, forms crystalline salts, and closely resembles the parent alkaloid. Three *aurochlorides* are obtained, $C_{33}H_{43}NO_{11}HAuCl_4$ (m.p. 141°). This salt on crystallization from aqueous alcohol forms a hydrate, $C_{33}A_{43}NO_{11}HAuCl_4H_2O$ (m.p. 129°), isomeric with aconitine aurochloride, into which it is easily converted. The third aurochloride, $C_{33}H_{43}NO_{11}AuCl_3$, melts at 147°·5. An *amorphous base* is obtained from aconitine, together with benzoic acid, by prolonged heating with water in a closed tube, and appears to be identical with the *aconine* of Wright and Luff. The amorphous base and its amorphous aurochloride gave, on analysis, numbers agreeing with the formulæ $C_{26}H_{41}NO_{11}$ and $C_{26}H_{41}NO_{11}HAuCl_4$.—The crystallographical characters of aconitine from *Aconitum napellus*, by A. E. Tutton. Aconitine crystallizes in the rhombic system. Habit prismatic; $a : b : c = 0·5456 : 1 : 0·3885$. It is a highly dispersive substance. The apparent acute angle of the optic axes in air 2E, for lithium light is 47° 0′, for sodium light 56° 10′, and for thallium light 65° 5′. Hence there is a dispersion of 18° 5′. The double refraction is positive.—The asymmetry of nitrogen in substituted ammonium compounds, by S. B. Schnyver. The primary object of the work was to produce stereo-isomers of pentavalent nitrogen compounds. The author has prepared quaternary ammonium compounds containing three different radicles: (1) by adding methyl iodide to diethylisoamylamine; (2) by adding isoamyl iodide to diethylmethylamine; (3) by adding ethyl iodide to ethylmethylisoamylamine. The platinochlorides of (1), (2), and (3) gave, on evaporation by heat, needle-shaped crystals belonging to the rhombic system. The platinochlorides of (1) and (2), on evaporation *in vacuo*, gave needle-shaped crystals belonging to the monoclinic system, which are easily converted into the rhombic variety; whilst (3), on evaporation *in vacuo*, gave needles which resembled those obtained on evaporation by heat. From these results the author concludes that this difference in crystalline form is due to the various arrangements of the radicles ethyl, methyl, isoamyl, and chlorine round the nitrogen atom, and therefore that the stereo-isomers are possibly capable of existence in the quaternary ammonium compounds.—Acetylcarbinol (acetol), $CH_3 . CO . CH_2OH$, by W. H. Perkin, Jun., F.R.S. Acetylcarbinol can be prepared in large quantities by adding freshly precipitated barium carbonate, in small quantities at a time, to a boiling aqueous solution of acetylcarbinyl acetate ($CH_3CO . CH_2 . OC_2H_3O$, prepared by digesting monochloracetone with potassium acetate in alcoholic solution). The product is purified by repeated fractional distillation under reduced pressure. Acetylcarbinol boils at 120°–122° (250 mm.), and at about 147° under ordinary pressures. When cooled in a mixture of ice and chlorhydric acid, it solidifies to a hard crystalline mass; its relative density at 15° = 1°·07915; magnetic rotation, 3·650. Sodium amalgam converts it quantitatively into methyl glycol.—The action of ethyl dichloracetate on the sodium derivative of ethyl malonate, by Dr. A. W. Bishop and W. H. Perkin, Jun., F.R.S. When a mixture of ethyl malonate (2 mols.), sodium ethylate (2 mols.), and ethyl dichloracetate (1 mol.) is digested in alcoholic solution in a reflux apparatus, and the product fractionally distilled, two new compounds are obtained: α-ethylic ethylenetricarboxylate $(CO_2Et)_2C : CH . CO_2Et$, and β-ethylic propanepentacarboxylate $(COOEt)_2 : CH . CH(COOEt) . CH(COOEt)_2$.—Benzoylacetic acid and some of its derivatives, Part 5, by W. H. Perkin, Jun., and J. Stenhouse. The preparation and properties of the following compounds are described in this paper:—Ethylic allylmethylbenzoylacetate, ethylic methyldibenzoylacetate, ethylic benzoylbenzoylacetate, benzylacetophenone, α-methyl-β-phenyllactic acid, α-ethyl-β-phenyllactic acid, and ethylic purpuralbenzoylacetate.—Syntheses with the aid of ethyl pentanetetracarboxylate, by W. H. Perkin, Jun., F.R.S., and B. Prentice. Ethylic pentanetetracarboxylate yields a disodium derivative, from which, by the action of alkyl iodides, higher homologues may be obtained, and these, on hydrolysis and subsequent splitting off of two molecules of carbon dioxide, yield higher homologues of pimelic acid,

$$(COOEt)_2 : CR . CH_2 . CH_2 . CH_2 . CR' : (COOEt)_2 + KOH = (COOK)_2 : CR . CH_2 . CH_2 . CH_2 . CR'(COOK)_2 + 4Et . OH,$$

and

$$(COOH)_2 : CR . CH_2 . CH_2 . CH_2 . CR' : (COOH)_2 = COOH . CHR . CH_2 . CH_2 . CH_2 . CHR' . COOH + 2CO_2.$$

The preparation and properties of ethyl dibenzylpentanetetracarboxylate, dibenzylpentanetetracarboxylic acid, and dibenzylpimelic acid are described.

Geological Society, March 25.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—Notes on Nautili and Ammonites, by S. S. Buckman.—On the drifts of Flamborough Head, by G. W. Lamplugh. The author describes in detail the characters and distribution of the glacial deposits on Flamborough Head, and classifies them as follows:—

Alluvial wash, fresh-water marls, &c.	Recent.
Late glacial gravels	Glacial.
Upper boulder clay	Glacial.
Intermediate series. Stratified beds, with bands of boulder clay	Glacial.
Basement boulder clay	Glacial.
Chalky rubble	Glacial.
"Infra-glacial" beds of Sewerby and Speeton.	

He discusses their relationship with other drifts, and arrives at the following conclusions:—(1) The glacial deposits are divisible into upper and lower boulder clay, with an intermediate series. (2) The lower clay is a continuation of the basement clay of Holderness, and is the product of the first general glaciation of the area. The intermediate series passes laterally into the purple clays of Holderness, and has been deposited at the edge of the ice-sheet. The upper clay includes the Hessle clay of Holderness, and marks the latest glaciation of this region. (3) The fossiliferous beds of Sewerby ("buried cliff beds") and Speeton ("estuarine shell bed") are older than the basement clay, and therefore than the earliest glaciation. (4) The glaciation was effected by land-ice of extraneous origin, which moved coastwise down the North Sea, and did not overflow the greater part of the Yorkshire Wolds. (5) Neither the boulder clays nor the intermediate gravels are of marine origin, the shells which occur in them being derivative. (6) The ice-sheet seems to have filled the North Sea basin in this latitude from the commencement of the glaciation until its close. There is no clear evidence here for a mild interglacial period, but only for extensive fluctuations of the margin of the ice. After the reading of this paper there was a discussion, in which Mr. Clement Reid, Mr. Whitaker, Mr. E. T. Newton, Prof. Hughes, the President, and the author took part.—On a phosphatic chalk with *Belemnitella quadrata* at Taplow, by A. Strahan. (Communicated by permission of the Director-General of the Geological Survey.) Two beds of brown chalk in an old pit near Taplow Court owe their colour to a multitude of brown grains. These grains are almost entirely of organic origin, foraminifera and shell prisms forming the bulk of them. Mr. Player has analyzed specimens of the brown chalk, and finds that it contains from 16 to 35 per cent. of phosphate of lime. The tests as well as the contents of the foraminifera seem to have been phosphatized, the phosphate appearing as a translucent film in the former case, and as an opaque mass in the latter. In the case of the prisms of molluscan shells, the whole of the phosphate appears to be in the opaque form. Minute coprolites also occur, together with many small chips of fish-bone, in which Dr. Hinde has recognized lacunæ, while some have been identified by Mr. E. T. Newton as portions of fish teeth. Mr. Player observes that the phosphate occurs in such a condition that it would not improbably serve as a valuable fertilizer, without conversion into superphosphate. This condition is probably due to the partial replacement of carbonate of lime by phosphate in the organisms. The removal of the remaining carbonate leaves the phosphate in a honeycombed state, peculiarly favourable for attack by the acids in the soil. The author comments upon the resemblance of the deposit to the phosphatic chalk with *Belemnitella quadrata* which is largely worked in Northern France, and upon a less striking resemblance with that of Ciply, which is at a higher horizon. Some remarks on this paper having been made by Dr. G. J. Hinde, Mr. Whitaker, and Prof. Judd, the President, alluding to the geological and economic interest of the discovery described in the paper, said that, though the area occupied by the phosphatic layers seemed to be small, there was good reason to hope that somewhere else in the upper chalk districts the same or similar bands might yet be found. The search for such deposits would now be stimulated by the information so fully supplied by the author, who himself would no doubt follow up his observations at Taplow by a thorough examination of the higher members of the chalk in the east of England.

Entomological Society, April 1.—Prof. R. Meldola, F.R.S., Vice-President, in the chair.—Captain H. J. Elwes showed a small but very interesting collection of butterflies from Laggan Alberta, North-West Territory of Canada, taken by Mr. Bean at high elevations in the Rocky Mountains. Amongst them were *Colias elis*, Streck., which seemed to be very close to, if not identical with, *C. hecla* of Europe, *Argynnis alberta*, W. H. Edw., and *Chionobas subhyalina*, W. H. Edw. The resemblance between the butterflies of this locality and those found on the Fells of Lapland was very striking, some of the species being identical, and others very closely allied. Captain Elwes said that it was another proof, if one were wanted, of the uniformity of the butterflies found throughout the boreal region in the Old and New Worlds.—Mr. G. C. Champion exhibited several insects recently received from Mr. J. J. Walker, from Hobart, Tasmania. The collection included a curious species of *Forficulidæ*, from the summit of Mount Wellington; two mimetic species of *Œdemeridæ* belonging to the genus *Pseudolycus*, Guér., and the corresponding *Lycidæ*, which were found with them; also specimens of both sexes of *Lamprima rutilans*, Er.—Mr. N. M. Richardson exhibited a specimen of *Zygæna filipendulæ* with five wings; four specimens of *Gelechia ocellatella*, including a pink variety, bred from *Beta maritima*; four specimens of *Tinea subtilella*, a species new to Britain, taken last August in the Isle of Portland; also specimens of *Nepticula auromarginella*, a species new to Britain, bred from larvæ taken near Weymouth on bramble.—Mr. C. Fenn exhibited a series of *Tæniocampa instabilis*, which had been bred out of doors during the recent severe weather. They were all bred from ova laid by the same female, and many of them were of an abnormally pale colour. Mr. Fenn said that, according to Mr. Merrifield's theory, these pale specimens, in consequence of the temperature to which they had been subjected in the pupal state, ought to have been very dark. Mr. Jenner Weir said he had never before seen any specimens of so light a colour.—Mr. W. Dannatt exhibited a butterfly belonging to the genus *Crenis*, recently received from the Lower Congo. He said he believed the species was undescribed.—Mr. G. A. J. Rothney sent for exhibition several specimens of an ant (*Sima rufo-nigra*), from Bengal, together with specimens of a small sand wasp (*Rhinopsis ruficornis*) and a spider (*Salticus*), both of which closely mimicked the ant. It was stated that all the specimens exhibited had lately been received from Mr. R. C. Wroughton, of Poona. Mr. Rothney also communicated a short paper on the subject of these ants, and the mimicking sand wasps and spiders, entitled "Further Notes on Indian Ants."—Mr. G. C. Champion read a paper entitled "On the Coleoptera collected by Mr. J. J. Walker, R.N., in the neighbourhood of Gibraltar, with descriptions of new species." At the conclusion a discussion ensued, in which Mr. Kirby, Captain Elwes, Mr. McLachlan, Mr. Jenner Weir, Dr. Sharp, and Mr. Crowley took part.

Dublin.

Royal Society, March 18.—On the cause of double lines in the spectra of gases, by Dr. G. Johnstone Stoney, F.R.S. The alternations of electro-magnetic stress in the ether which constitute light form an undulation which is propagated through the ether under the same laws as the transverse vibrations of an incompressible elastic solid (see McCullagh's papers, *passim*). It is convenient first to regard certain points in the molecules of the gas as acting dynamically on such a medium, to inquire what oscillations within the molecules would impart to the medium the vibrations which correspond to the observed lines in the spectrum, and afterwards to correct the investigation so as to transfer it from the dynamical hypothesis of light to the electro-magnetic theory. Let us then suppose that a point P in the molecule acts on the ether, and is set oscillating within the molecule by the inter-molecular encounters. Its undisturbed path will be represented by the simple elliptic motion

$$\left.\begin{array}{l} x = a \,.\, \cos\theta \\ y = b \,.\, \sin\theta \end{array}\right\} \quad \ldots\ldots\ldots \quad (1)$$

θ being written for $2\pi mt/\tau$, in which m is the oscillation-frequency in the time τ. Perturbating forces operating within the molecule would cause an apsidal motion of the ellipse in its own plane, and perhaps a motion of the nodes of this plane upon the invariable plane. The apsidal motion is represented by

$$\left.\begin{array}{l} X = a \,.\, \cos\theta\cos\omega - b \,.\, \sin\theta\sin\omega \\ Y = a \,.\, \cos\theta\sin\omega + b \,.\, \sin\theta\cos\omega \end{array}\right\} \quad \ldots \quad (2)$$

where $\omega = 2\pi nt/\tau$, n being the oscillation-frequency of the apsidal revolutions. Equations (2) are equivalent to

$$\left.\begin{array}{l} X = \dfrac{a+b}{2} \,.\, \cos\,(\theta + \omega) + \dfrac{a-b}{2} \,.\, \cos\,(\theta - \omega) \\ Y = \dfrac{a+b}{2} \,.\, \sin\,(\theta + \omega) - \dfrac{a-b}{2} \,.\, \sin\,(\theta - \omega) \end{array}\right\} \quad . \ (3)$$

a motion which, when imparted to the surrounding medium, represents a pair of lines in the spectrum with oscillation-frequencies $(m + n)$, and $(m - n)$, and intensities which are to one another in the ratio of $(a + b)^2$ to $(a - b)^2$. These quantities are not altered when the various positions in which the molecules chance to be at any one instant are taken into account. Equations (2) and (3) represent an apsidal motion in the same direction as the primary elliptic motion, and in this case the more refrangible line is the brighter. If the apsidal motion is in the opposite direction, we shall have to change the sign of ω in equations (2) and (3). This does not alter the positions, but it is now the less refrangible line which is the brighter. A third case occurs when the primary motion is an oscillation in a straight line instead of in an ellipse: here $b = 0$, and the intensities of the two lines become equal. All three cases occur in the observed spectra of gases. A motion of the nodes on the invariable plane will give rise to additional pairs of lines, and if the inclination of the two planes is small, one of the pairs only is conspicuous. Such a motion may be what occasions the faint lines that have sometimes been called satellites. It would at the same time cause a shifting of the position of the bright lines—a matter of importance in grouping the lines of a spectrum into series. To pass from the dynamical investigation to the electro-magnetic, attention must be given to Faraday's law of electrolysis, which is equivalent to the statement that in electrolysis a definite quantity of electricity, the same in all cases, passes for each chemical bond that is ruptured. The author called attention to this form of the law in a communication made to the British Association in 1874 (see Scientific Proceedings of the Royal Dublin Society of February 1881, and *Philosophical Magazine* for May 1881, p. 385); and showed that the amount of this quantity of electricity is about the twentiethet (that is, $1/10^{20}$, or a unit in the twentieth place of decimals) of the usual electro-magnetic unit of electricity, *i.e.* the unit of the ohm series. This is the same as three eleventhets (or $3/10^{11}$) of the much smaller C.G.S. electrostatic unit of quantity. A charge of this amount is associated in the chemical atom with each bond. It appears to be irremovable from the atom, but becomes for the most part disguised when atoms chemically unite. If this charge be lodged at the point P of the molecule which undergoes the motion described above, the oscillation of the charge will cause an electro-magnetic undulation in the surrounding ether. The only change that has to be made in our investigation to adapt it to this state of things is to change θ into $\frac{\pi}{2} + \theta$, *i.e.* a mere change of phase. We in this way represent the fact that it is the direction and velocity of the motion of P, not the direction and length of its radius vector, which determine the direction and intensity of the electro-magnetic stresses in the surrounding ether. We have further to correct for the change of phase, about one-fourth of a vibration-period, consequent upon what takes place in the immediate vicinity of the moving charge. Within the molecule itself, the oscillation of the permanent charge probably causes electric displacements in other parts of the molecule, and to the reaction of these upon the oscillating charge we may perhaps attribute those perturbations of which the double lines in the spectrum give evidence. The periodic times of all the molecular motions dealt with in this paper, of the primary and apsidal motions, of the motion of the nodes, of change of ellipticity, &c., and also the form of the ellipse, can be deduced from what may be observed in the spectrum.

Paris.

Academy of Sciences, March 31.—M. Duchartre in the chair.—On the third meeting of the Permanent International Committee for the execution of the photographic map of the heavens, by Admiral Mouchez (see Nature, April 2, p. 516).—A new gyroscopic apparatus, by M. G. Sire.—New observations on the Marseilles sardine, by M. A. F. Marion.—The earthquakes at Algiers on January 15 and 16, by M. A. Pomel. It is remarked that no apparent relation appears to exist between the seismic movement and the geological structure of the region affected.—New nebulæ discovered at Paris Observatory, by M. G. Bigourdan. A list is given of new and interesting nebulæ discovered by M. Bigourdan between 0 hours and 9 hours of right ascension in the years 1887–90.—On the variations observed in the latitude of a single place, by M. A. Gaillot. Diurnal variations in latitude have been found from a discussion of meridian observations made at Paris during the years 1854–57. An annual variation does not appear very definitely marked. The results indicate that all hypotheses relative to the cause of the phenomenon are premature, for in all probability there is no real variability in the direction of the earth's axis, the question being merely one of temperature and refraction.—On the theory of conformable representations, by M. Paul Painlevé.—On the pressure in the interior of magnetic or dielectric media, by M. P. Duhem.—Propagation of Hertz's electrical undulation in air, by MM. Édouard Sarasin and Lucien de la Rive. The most important result obtained is, that the velocity of propagation of Hertz's electrical undulations across air is very sensibly the same as that with which they are transmitted along a wire conductor.—New method for the investigation of feeble bands in banded spectra; application to hydrocarbon spectra, by M. H. Deslandres. It is shown that the bands or flutings characteristic of hydrocarbon spectra occur periodically. M. Deslandres has discovered three new bands at λ 438·19, λ 437·13, and λ 436·5. These bands are not given by the combustion of hydrocarbons, but they may be seen with the ordinary bands of hydrocarbon and cyanogen in the electric arc and in the cyanogen flame. By the application of the above law, M. Deslandres has been able to determine that these bands really belong to hydrocarbon.—On the origin of the higher alcohols contained in industrial ferments, by M. L. Lindet.—On vegetable hæmatine, by M. T. L. Phipson.—On the employment of liquefied carbonic acid for the rapid filtration and sterilization of organic liquids, by M. A. d'Arsonval.—The males of the Ostracoda of sweet water, by M. R. Moniez.—On the influence of salinity in the formation of starch in chlorophyllian vegetable organs, by M. Pierre Lesage. According to the author, the presence of salt in waters is an obstacle to the production of starch in vegetable tissue.—Note on the simultaneous disengagement of oxygen and carbon dioxide in Cacti, by M. E. Aubert. Some observations show that Cacti, when submitted to a temperature of about 35°, and a light of moderate intensity, simultaneously emit oxygen and carbon dioxide.—The artificial reproduction of amphibole, by M. K. de Kroustchoff. The author has been able to produce crystals of this rock by synthesis.—Some magnetic anomalies observed in the centre of Russia in Europe, by General Alexis de Tillo. Anomalous magnetic disturbances are described, similar to those found by Profs. Thorpe and Rücker in England.—A definite depression in the centre of the Asiatic continent, by the same author. Some barometric readings indicate that a little south of the town of Tourfan, in the centre of the Asiatic continent, there exists a depression 50 metres below the level of the ocean.

CONTENTS.

THURSDAY, APRIL 16, 1891.

OUTLINES OF PSYCHOLOGY.

Outlines of Psychology. By Harald Höffding, Professor at the University of Copenhagen. Translated by Mary E. Lowndes. (London: Macmillan, 1891.)

THIS volume forms one of Messrs. Macmillan's familiar manuals for students. Our first feeling on receiving it was one of regret that for a text-book of psychology in England it should be necessary to have recourse to a translation of a German rendering of a Danish work. But on second thoughts, after perusal, the excellence of the work itself threw our previous regrets into the background.

The first chapter deals with the subject and method of psychology, and it is here at the outset indicated that in the time-honoured classification of psychical states under cognition, feeling, and will, it is not the actual concrete states themselves which are classified, but the elements out of which a closer examination shows them to be formed. Hence these psychological divisions, if they are to have scientific value, must be based on a thorough-going analysis. With this we are in full accord. That analysis should precede synthetic speculation we hold to be sound method in psychology; and for this reason, if for no other, we think it an error of judgment in Prof. Höffding's work to introduce at the outset a somewhat scrappy psychogenesis in the child, starting with the debateable assertion that "the beginning of conscious life is to be placed probably before birth," followed by a section to show that the psychology of primitive races teaches that the idea of the mental has passed through the same stages in the history of the human race as in the individual. It would have been better to start with the apparently naïve deliverance of consciousness in the average European man or woman, and to defer the question of genesis either in the individual or the race till a later part of the work.

In the second chapter the interrelation of mind and body is considered. Laying it down that the popular mode of apprehension is distinguished from the scientific in being a compound of experience and metaphysics—a statement so true that it will surprise and annoy many practical common-sense folk—the author points out that, in considering the four hypotheses logically possible, we are in psychology concerned with them only from the point of view of experiential science. The four possible hypotheses are: (1) that consciousness and brain, mind and body, act upon each other as two distinct beings or substances (dualism); (2) that the mind is only a form or a product of the body (materialism); (3) that the body is only a form or a product of one or several mental beings (idealism or monistic spiritualism); and finally (4) that mind and body, consciousness and brain, are evolved as different forms of expression of one and the same being (scientific monism or the identity hypothesis). The consideration of these four views is careful and searching. As an empirical working hypothesis, scientific monism is adopted, but it is wisely noted that "the empirical formula, with which we here end, does not exclude a more comprehensive metaphysical hypothesis."

The third chapter is devoted to the conscious and the unconscious. Here, again, the treatment is clear and guarded, and the very reverse of dogmatic. The conclusion is—and it is the only logical conclusion open to an evolutionist—that the conscious emerges from the unconscious. Prof. Höffding is, however, too careful a reasoner to say that it emerges from neural vibrations; he sees too clearly that consciousness and energy belong to distinct orders of being. But he says: "As the organic world is built up of elements and by means of activities which make their appearance, though more scattered and without unity and harmony, in inorganic nature also, so in the sensations, feelings, and thoughts of conscious beings we should have higher forms of development of a *something* that, in a lower degree and in a lower form, exists in the lower stages of nature." To avoid the awkwardness of the vague "something" which we have italicized, we have (NATURE, vol. xliii. p. 101) suggested the term metakinesis for the whole range of that mode of being which reaches the clearness and intensity of consciousness in the human mind. The conclusion reached, therefore, is that below the level of consciousness there are metakinetic states which are unconscious. And from this it follows that the mental world—as compared with the physical world—is to us a fragment; it is possible to complete it only by means of hypothesis, and even such completion has great difficulties. Moreover, "this fragmentary character of the psychological phenomena, as known to us, makes it impossible for psychology ever to become an exact science, such as physics is already, and as physiology is in process of becoming."

A short chapter on the classification of the psychological elements brings to a conclusion what may be regarded as the general and introductory part of the work. Then follow three long chapters on the psychology of cognition, the psychology of feeling, and the psychology of the will. Into these we do not propose to follow the author. Suffice it to say that they may safely be placed in the hands of psychological students as presenting a logical and well-thought-out development of the scientific or experiential as opposed to the metaphysical study of the human mind. The pages abound in pithy epigrammatic conclusions reminding one of the psychological gems scattered through the writings of George Eliot. There are, too, many references to the instances of keen psychological insight to be found in the works of the poets, and pre-eminently in Shakespeare.

The translation has evidently been done with great care. The sharp crispness of the sentences, and the idiomatic purity of the style, except in a few inconspicuous exceptions, make us forget that we are reading a rendering of a German work. There are, however, one or two words, phrases, or sentences, to which we would direct the translator's attention. On p. 30 we read: "The very first principle on which natural science is based is, that the state of a material point (rest or movement in a straight line) can be altered only through the *movement* of another material point." Should not "movement" be "influence"? Again, would not "stimulus" be more appropriate than "irritant" on p. 50 (in brackets). The word "illusion" is used several times where "delusion" would be more correct (*e.g.* pp. 88, 341).

On p. 147, in speaking of an observation on after-images, it is said that "they disappeared gradually, but in the spot where they had vanished from the *orbit* of the closed eye," &c. Here the *field* of vision seems to be intended. When we read (p. 107) of "the *marginal* elements to which we are led by analysis of compound states of consciousness," the *ultimate* elements are presumably intended. Is it in accordance with Prof. Höffding's original Danish that the corpora striata and optic thalami are placed in the mesencephalon? These are but slight and scattered blemishes in an excellent piece of work. C. LL. M.

THE FOUNDATIONS OF GEOMETRY.

The Foundations of Geometry. By E. T. Dixon. (Cambridge: Deighton, Bell, and Co., 1891.)

"OUGHT there not to be a perfect subjective geometry, as well as an applied objective one, the applicability of the former to the latter being a matter to be determined by induction from experiment? It is the object of this book to show that such is the case, to establish the perfect geometry, and to examine the grounds on which we may believe that it applies to the objective space in which we live." Such, stated in the author's own words, is the object he has proposed to himself, and he appeals with confidence to geometricians to criticize his system. The work is divided into three parts, devoted respectively to (1) a discussion on the logical status of geometry; (2) the development of the author's subjective theory; and (3) an investigation into its application to material space. It is to part (2) that we shall chiefly direct our remarks; for though the author's views, as laid down in part (1) (see pp. 20, 21), on definition as the basis of a deductive system, have an important bearing on his geometrical theory, and seem in opposition to those of at least one school of logicians, they will be best tested by an examination of the definitions of the two concepts "direction" and "sameness of direction" laid down at the beginning of part (2). With much of the author's criticism of Euclid's treatment of the plane we are in agreement, but he seems to lay himself open to the chief objections urged against his predecessors. With regard to part (2), *granting the assumptions tacitly made at the outset*, we are willing to admit the formal accuracy of most of the proofs of the propositions in the "subjective theory," and the possibility of supplying it, without any serious derangement of the system, to those which seem to want it. But we do take serious exception to the way in which the foundations of the new edifice are laid. The system is based partly on three axioms, explicitly stated, as to (1) the possible transference of geometrical figures, (2) the possible extension of a straight line, (3) the three-way extension of space; and partly on what are styled, not inaptly, the "implicit" definitions of *position* and *direction*. With that of "position" (used in the sense of "fixed point") we are not much concerned, merely remarking that in it the word *position* is used to explain *position*. This defect, under which Bardolph's more famous definition of "accommodated" also labours, could probably be easily rectified. But so much depends on that of "direction" and "sameness of direction," that we give it in full:—

"Implicit definition of direction.

"(*a*) A direction may be conceived to be indicated by naming two points, as the direction from one to the other.

"(*b*) If a point move from a given position constantly in a given direction, there is only one path, or series of positions, along which it can pass. (Such a path may be called a 'direct path,' and a continuous series of points occupying such positions, a straight line.)

"(*c*) If the direction from A to B is the same as that from B to C, that from A to C is also that same direction.

"(*d*) If two unterminated straight lines, which intersect, are each intersected by a third straight line in two separate points, any unterminated straight line extending in the same direction as this last one, which intersects one of the two former, shall also intersect the other."

Now, we mistrust an "implicit" definition. It seems to be a compound of "axiom and definition" or "postulate and definition," which ought in each case to be carefully analyzed into its elements. As to (*a*) after repeated mental effort, we are still unable to realize the meaning of the "direction from A to B" without using the concept of the "straight line AB." Hence (*b*) seems merely to tell us that moving along the straight line from A to B may be called "moving constantly in the same direction from A to B," and to lay down the *axiom* or *postulate* that "only one straight line can be drawn from A to B," while the only additional explanation given by (*c*) is that if two different straight lines meet their directions are said to be different. As yet we have not received any information as to what is meant when two non-intersecting straight lines are said to have the "same direction" or to have "different directions." We proceed to (*d*) to find what direction from C is that which may be said to be the same as that from A to B. Following the instructions, we must imagine two straight lines OP, OQ, both of which intersect AB, and one of which passes through C; then the straight line which passes through C, and intersects OQ in some point D, such that CD does not intersect AB, however far they are both produced, is said to have the "same direction" as AB.

Surely there is a tacit assumption here *that there is one and only one straight line through C which intersects OQ, and being produced does not intersect AB;* without it the definition seems unmeaning. Yet this afterwards appears as Proposition 2. There is certainly a further one which may be enunciated thus: *If there are two non-intersecting straight lines AB, CD, such that CD meets each of two given intersecting straight lines which each meet AB, then if CD meets either of any other such pair of intersecting straight lines it meets each of them.* If this axiom be not admitted the supposed demonstration of Proposition 2 fails.

It must be clearly understood that we are not objecting to the axioms themselves as fit foundations for a logical system; they seem to be tantamount to those which Euclid either openly or tacitly assumes, or, as it would be better to say, *postulates*. We do not think, indeed, that the author has done his own method justice, for it might be simplified by laying down its αἰτήματα explicitly; and a comparison of them with those tacitly or openly made by Euclid in his definition of a plane, in "Axioms 10 and 12," and in XI. 1–3, would serve to clear up much of the obscurity which it must be confessed has been generally allowed in text-books to hang over this part of the "Elements." It may be incidentally remarked that in Henrici's "Congruent Figures" and Hayward's "Elements of Solid Geometry" these matters receive due discussion. It will serve as a concise summary of the points of contrast between Mr. Dixon's system and that of Euclid as elaborated by commentators to say that, whereas Euclid, starting from the concepts *straightness* and *flatness*, deduces from certain αἰτήματα as to these concepts the concept *parallelism*, Mr. Dixon, starting from the concepts *straightness* and *sameness of direction*, deduces from certain αἰτήματα as to these concepts the concept *flatness*.

While open to the objections we have urged, and to some others on the treatment of the plane angle (we do not, for instance, see any logical cogency in the demonstration of Proposition 7), this part of the work seems to have been elaborated with skill and care.

Part (3) contains some acute remarks on the possibility of a "fourth dimension," and on a method of forming diagrams in material space which shall be "orthogonal projections of the true figures."

We heartily recommend the book to the attention of all those interested in the presentation of the "Elements" to beginners, whether they are authors, teachers, or examiners. Authors may find suggestions for many a note on fundamental difficulties; teachers may pick up a frequent hint for class-work with their more intelligent pupils; and examiners might do worse than set an occasional critical question on some of the assumptions which the author reminds us it is usual to make tacitly. We notice with interest the appearance of a proof of "Axiom 12," which the author, after Prof. Newman, ascribes to M. Vincent, of Paris. A similar one occurs in Dr. Thomson's edition of the "Elements," and is said to be due to M. Bertrand, of Geneva, and it is also to be found in chapter xv. of De Morgan's "Study and Difficulties of Mathematics," with the remark that it "is more than probable that had the same come down to us sanctioned by the name of Euclid it would have been received without difficulty." E. M. L.

OPTICAL PROJECTION.

Optical Projection. By Lewis Wright. (London: Longmans, Green, and Co., 1891.)

MR. WRIGHT has earned the warm thanks of all teachers and students who use the lantern for lecture or demonstration purposes by this excellent book. It contains about 400 pages. Of these, the first half is devoted to descriptions of the various parts of a lantern, and of apparatus accessory to its use. The principles of projection are clearly explained; then follows an account of the different forms of condensers and their relative advantages. Mr. Wright's criticisms of the various forms are clear and to the point. He recommends, as the one which is generally most useful, two plano-convex lenses with their plane surfaces turned outwards, the lens nearer the radiant being rather the smaller. A discussion of the various forms of objectives comes next, with practical hints for testing both them and the condensers. After this we have several chapters devoted to possible sources of light.

The form of jet for the lime-light which the author recommends is a mixed jet devised by himself, and described on p. 63; this, he says, can sometimes be pushed as bright as 1000 candles; while for the arc light a modification of the Brockie Pell lamp, described on p. 163, seems to have given the most satisfactory results. Full details for using these and other forms of light are given, and the possible sources of danger to which each may give rise are carefully discussed.

The book is throughout thoroughly practical. The best method of preparing the gas, the best form of gas bag, and of pressure boards, the various adjustments required in setting up the burner so as to give the best result, the details of focussing, and many other points, are considered in turn. The best screen appears to be a smooth surface of plaster of Paris, after that a whitewashed wall finished with whitening.

In chapter xii. we have an account of lanterns for special purposes, scientific demonstrations, and the like; among others the elaborate lantern used at the Royal Institution is described.

Considerable space is devoted in chapter xiii. to the projection-microscope, of which the important development in the past ten years is largely due to Mr. Wright.

This concludes the first half of the book. The second part deals with demonstrations in the various branches of natural science. Experiments are described in physics—including heat, light, sound, electricity and magnetism—chemistry, physiology, &c.

With regard to many of these a general criticism may be made. Mr. Wright, in fact, almost suggests it himself on p. 215. There may be occasions on which apparatus and experiments can be shown only by means of projection. The room may be too large and the audience too numerous to permit of the lecture-table being well seen, and the details of an experiment as it is actually performed being followed by all; or, again, a lecturer may have to travel about, as in the case of University Extension lecturers, to various centres; he can fairly easily carry with him slides and small apparatus, whereas it would be impossible to transport all that is required for the performance of the same experiments on a large scale. In such cases as these Mr. Wright's practical details will prove invaluable, but for smaller audiences, or in lecture-rooms attached to permanent laboratories, more will, we think, be learnt from seeing the same experiments performed on a larger scale, rather than with apparatus specially devised to render projection possible.

Another difficulty of a practical kind in the use of the lantern, is that the room must be darkened. Students cannot easily see to make diagrams or take notes; and the lecturer cannot see them, and discover from their faces, as he proceeds, how far he is making himself understood and is interesting the class. However, as was pointed out at the last meeting of the British Association, in an interesting paper by Profs. Barr and Stroud, on the use of the lantern, the room may for many experiments be comparatively bright, provided that the screen is well shaded from the light.

As might be expected from the nature of the case, the section on Optics is the most complete. Mr. Wright's earlier book on "Light" is extremely instructive; the suggestions it contains are in many cases amplified and improved in the present work, and much that is new is added. There is one point, however, on which Mr. Wright does not express his meaning with his usual clearness. He says, when treating of the spectrum (p. 304), that it is in most experiments permissible to converge more light upon the slit by the use of a lens This statement is hardly sufficiently strong: it is not only a permissible course, but also the best course, to form on the slit an image of the source of light, using a lens of such a focal length that all the rays which pass through it, after converging to the slit, diverge so as all to pass through the lens and prism used to form the spectrum. In some cases it may be best to dispense with the condensers and lantern, and merely place the source of light close up to the slit itself. And again, in section 179, the rays emerging from the slit should not be a narrow parallel pencil, but a divergent pencil of such an angle as to fill the lens completely; while in Fig. 171 the light should not be shown as focussed on the prism, but on a screen behind the prism, and at about the same distance as the screen on which the spectrum is formed.

DRY DENUDATION.

Die Denudation in der Wüste und ihre geologische Bedeutung: Untersuchungen über die Bildung der Sedimente in den Ägyptischen Wüsten. Von Johannes Walther, a. o. Professor an der Universität, Jena. (Leipzig: S. Hirzel, 1891.)

THIS book forms the third part (being paged continuously) of the sixteenth volume of the Transactions of the Königlichen Sächsischen Gesellschaft der Wissenschaften. The questions propounded by the author for investigation are the following:—What meteorological forces are active in the deserts? What is their destructive effect on the rocks? What are the ultimate results of this? Is the present sculpturing of the deserts due to the influence of other forces than those which are now active? How can fossil deserts be recognized? The last is left unanswered; the others are considered in the light of information collected from books of travel, and from the author's own studies during two visits to the Egyptian desert.

In regard to rainfall, he points out that no part of the African desert is absolutely rainless, and that, as the storms, though rare, are heavy, the mechanical effects of water are more marked than they would be in a region where precipitation was more uniform. But in a desert, where the absence of plants and of soil exposes the rock to the effects of atmospheric variations, changes of temperature are yet more potent in causing denudation. These changes, owing to the dryness of the air, are great. The diurnal range may be full 30° C., the annual as much as 70° C. By the constant expansion and contraction due to these variations, the rocks are split, and the results are more important in producing denudation than are chemical changes. The author gives a number of illustrations to show how rock-masses in the desert are destroyed by the action of heat and cold, wind, and drifting sand. The surfaces of old walls are corroded; strata of different hardness in the face of a cliff are worn bac unequally; masses of rock are isolated, and the bloc and pillars are carved into strange forms; denudation, short, seems to proceed as actively in a desert as in damp climate, and along very much the same line Isolated hills of tabular form are also very characterist of desert denudation. These may be either on a lar scale—outliers of an extensive plateau—or on a sma one, like models, but a few feet high. In each case t cause is the same: a harder stratum at the top has pr served the softer material below. The author also d scribes the valleys of the desert, usually dry, and t cirques which, as was pointed out some years since Mr. Jukes Browne (*Geol. Mag.*, Decade 2, vol. iv. 477), seem to occur in the deserts of Egypt even as regions where ice may be supposed to have acted. T description of the latter is important, since it indicat that there is not that necessary connection between gl ciers and cirques which some geologists appear to ha imagined.

In regard to the excavation of the valleys, and perhaps some other physical features on a larger scale, we thi that the author is disposed to attribute rather too mu to the sculpturing effect of heat, cold, and wind (wi which of course drifting sand is included), and to ove look the probability that many of the bolder physic features may be memorials of an age when the regi was well watered. It is often more than probable th deserts have not always been deserts, but that they a districts of local desiccation, which sometimes may still in progress. So it is with the Aralo-Caspian are the Great Salt Lake of Utah and the Dead Sea are me remnants of fresh-water lakes on a far grander sca When a large part of the present Jordan valley was o sheet of water, when glaciers replaced the cedars Lebanon, and the peaks of Sinai were white with pere nial snow, the Egyptian desert must have been no long arid, and permanent streams must have occupied t wadis. Probably since that time the change in t physical features, though doubtless not unimporta has been in the main superficial; the effects of the ar epoch through which Egypt and some other regions a passing may be compared to those of the Glacial epo further north. The work has been that of the file rath than of the chisel.

That changes of temperature and the action of drifti sand have been agents of denudation of considerable i portance was, of course, well known, and is stated most text-books of geology, but Prof. Walther has do good service in emphasizing the fact, and in collecti together a number of interesting observations and illust tions which will be useful for reference, especially teachers.

T. G. B.

OUR BOOK SHELF.

The American Race. By Daniel G. Brinton. (N York: N. D. C. Hodges, 1891.)

By "the American race," Dr. Brinton means the abo ginal race of America, and in the present work he ma what he believes to be the first attempt to classify systematically on the basis of language. Whether guage is, as he contends, "the only basis on which the subdivision of the race should proceed," is a question which needs rather fuller discussion than Dr. Brinton has devoted to it. It may, however, be admitted that in the study of the American aborigines, at the present stage of ethnographical science, no other test works so well upon the whole as the linguistic; and that the results attained by the use of it are interesting and instructive, whether they exactly correspond to racial distinctions, in the strict sense of the expression, or not. Dr. Brinton divides the American race into five great groups—the North Atlantic group, the North Pacific group, the Central group, the South Pacific group, and the South Atlantic group. Taking each of these in turn, he describes the various stocks included in them, paying attention especially to those portions of the continent about which ethnographers are least united. The facts are sometimes presented in a style unnecessarily dry; but the author has taken immense pains to arrange them clearly, and the work will be of genuine value to all who wish to know the substance of what has been found out about the indigenous Americans. We may note that Dr. Brinton does not agree with those who hold that the Red Men originated in America. On the other hand, he sees no reason to suppose that they came from Asia. His theory is, that they were an offshoot from the race which in the earliest times occupied Western Europe, and that they crossed from the one region to the other on a land-bridge. The former existence of this land-bridge he accepts chiefly on the evidence collected by various English geologists.

Charts of the Constellations. By Arthur Cottam, F.R.A.S. (London: Edward Stanford, 1891.)

THE large edition of Mr. Cottam's magnificent charts has met with a well-deserved success, and the smaller edition before us will doubtless be received in a similarly favourable manner. The thirty-six charts in the large edition measure 30 × 22 inches, and were projected on the scale of one-third of an inch to a degree of a great circle; the size in the reduced form is 15 × 12 inches, and the scale is one-half as great. In this handy size the charts will be sure to have a wide circulation among beginners in the study of astronomical science, and to possessors of the smallest telescope with alt-azimuth mounting they are invaluable. The road to an intimate knowledge of the stellar universe is considerably straightened by showing each constellation on a separate chart, whilst three key maps exhibit the relative positions of the various constellations, and will be extremely useful to show alignments over larger spaces than are included on the separate charts. Some notes on the constellations, in conjunction with an excellent introduction explanatory of various astronomical measurements and methods, enhance the value of the charts and help to render the work superior to any hitherto published. G.

LETTERS TO THE EDITOR.

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

Co-adaptation.

THE remarks on this subject made by Dr. Romanes (NATURE, March 26, p. 489) do not appear to me to present the case in a fair light, and I therefore think it advisable to make some further observations. In the first place, I do not allow that there is any "fortuity" in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual, for every variation, of whatever kind, must of necessity entail numerous structural and functional modifications. If variations in the length of neck or leg or wing, in the strength of certain muscles, or even in colour, arise "fortuitously" (meaning thereby through some unknown cause antecedent to birth), there must be corresponding variations in the length of the bones, nerves, blood-vessels, &c., as well as in the physiological processes upon which depends the supply of nutriment to the parts in question. If any variation survives, or is perpetuated by breeding, it must therefore possess not only the external visible character A, but also all the concomitant and less obvious or altogether undiscoverable modifications, B, C, D, &c. However complex the correlated modifications may be, the mere existence of the variation implies the existence of such modifications, and the survival of the variation implies the survival of these correlated modifications. If, therefore, it is argued that any variation is a simple case of modification in some one character only—as might be inferred from Dr. Romanes's letter—this is an unwarranted assumption entirely opposed to fact.

But the case we have to consider is that in which the variations A, B, C, D, &c., are not physically or physiologically correlated, but in which they are supposed to be "independent variables." It is to such cases that Mr. Spencer applied his objection. He says ("Factors of Organic Evolution," p. 14):—"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?" Then comes the case of the giraffe, in which Mr. Spencer endeavours to show that the fore- and hind-quarters of this animal must be regarded—if I may venture to express his views briefly—as, "independent variables."

In reply to arguments of this kind, it may be pointed out that mere remoteness does not preclude the possibility of there being correlated variability between structures or functions. And in the next place even if it be admitted that certain parts now co-operative were independently variable at first—as in many cases they no doubt were—it is a gratuitous assumption to suppose that they must have originated from the very beginning in individuals in which the parts were co-operative, *i.e.* in individuals in which the non-correlated variations, "A, B, C, D, &c.," occurred simultaneously. The chances against such variations occurring in any one individual are, I concede most fully, "infinity to one." But if A (with its correlated physical and physiological modifications) survives through natural selection (utility) or is bred by artificial selection, that form is fixed by virtue of its possessing certain characters. If it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio*. Thus with the giraffe it must be shown that the heightening of the fore-quarters not only conferred no particular advantage, but may even have placed the animal at a disadvantage unless associated with the necessary modifications of the hind-quarters, the necessary modifications being, according to Mr. Spencer, those required to enable the animal to escape pursuit, "since any failure in the adjustment of their respective strengths [the fore- and hind-quarters] would entail some defect in speed and consequent loss of life when chased." To me—and I speak with all deference to the views of Mr. Spencer—this seems like an imposition on Nature of certain conditions which might not be true. It cannot be dogmatically laid down that at the time of its development from the ancestral form the necessity for escaping pursuit was of equal or greater importance than the faculty of high-reaching. In fact, the heightening of the fore-quarters is just as likely to have *diminished* the necessity for great speed, by enabling the animal to take a wider survey of its surroundings, and so to get sight of its enemies in time to escape. But, if speed were essential for the preservation of the species, I must confess that I see no difficulty in the belief that this character also might be superadded subsequently to the heightened fore-quarters in the same way that other associations of characters arising from "independent variables" are fixed by artificial selection. I do not admit therefore that "all this is quite wide of the mark"; on the contrary, I believe that it is *the* mark, and that the particular case of co-adaptation quoted by Mr. Pascoe (I am afraid in a somewhat garbled form) may resolve itself into a "confluence of adaptations." In fact, I do not think that it would be going too far to put forward the proposition that all cases of co-adaptation may be ultimately explained in the same way, *i.e.* that they arise from the coalescence (by intercrossing) of *n* modifications each *useful* (not useless) in itself, and acquired at successive periods in the phylogeny of the race.

The remarks with which I have endeavoured to meet the arguments advanced by Dr. Romanes ignore the "difficulty" which he have interposed with respect to the want of analogy between artificial and natural selection. I have left this out of consideration advisedly, because it raises one of the questions which have so long divided Dr. Romanes from those whom he has thought proper to label "neo-Darwinians." It is the old difficulty of "the swamping effects of intercrossing" under another form. I do not believe in the reality of this difficulty, and it has been so amply treated of by Dr. Wallace and others that I do not feel warranted in trespassing upon these columns at any greater length in order to rediscuss the question. I can only assure those who have read the comments made upon my review of Mr. Pascoe's book that I have not been made the subject of any metaphorical fraud, but that I accept the analogy between artificial and natural selection as real.

R. Meldola.

The Meaning of Algebraic Symbols in Applied Mathematics.

Dr. Lodge's remarks on p. 513 (April 2), ought not, I think, to pass without protest. He very reasonably objects to being asked to use a formula which is adapted to one particular set of units, and is not convenient for any other set, and prefers the greater freedom which is usually indulged in, as regards units, in mathematical physics. But he goes further than this, and maintains that it is best (he almost suggests that it is necessary) that Prof. Greenhill's practical man, if he wishes to avoid the somewhat mild difficulties which at present beset him, should adopt the system set forth in Nature, vol. xxxviii. p. 281. It may be a reasonable thing to do to base the interpretation of physical equations upon the method of multiplication of concrete quantities described in the article referred to; but that the practical man, whose difficulties are in question, is likely to take the trouble to understand it, may be confidently denied.

However, I think that the system of interpretation advocated involves other inconveniences. Suppose, for some purpose, we chose to measure the velocity of light by the distance of the light of that colour from a given line in a standard spectrum, thus giving velocity for this purpose the dimension length. This quantity would have different properties from the same velocity measured in the usual way; it would practically be a different quantity; yet the velocity of light is independent of the mode adopted for measuring it. Does it not make confusion to insist upon one of these two quantities being the concrete velocity itself, some other name having to be invented for the other? The same confusion of language would arise even if we desired to depart so slightly from the usual practice as to use our velocity symbol for the specification of it in miles per second, using a centimetre for the unit of length.

I have found that, occasionally, a good way of clearing up difficulties of pupils about dimensions is to say that it is no more intrinsically absurd to equate an area to a length than to equate a length to a product of a velocity and a time, all the symbols being numbers; but that the latter equation can be employed without abandoning the particular freedom as to subsequent choice of units which we wish to retain, and that the former cannot. I have heard a lecturer appeal, in a similar case, to the intrinsic absurdity of saying that the area of a field is equal to the distance from here to London: this appears to me to be not so clear a way of talking of the distinction between the two cases.

Finally, what are the disadvantages of considering the symbols of quantity to be mere numbers? Dr. Lodge, while making out his case as against Prof. Greenhill, is, to my mind, quite unconvincing on this more general point.

King's College, Cambridge, April 11. W. H. Macaulay.

Force and Determinism.

Dr. Oliver J. Lodge has reminded us, in your issue of March 26 (p. 491), that under physical constraint or control the direction of motion of material particles may be changed without expenditure of energy or performance of work.

It does not follow from this that the direction of motion can be changed under metaphysical control or constraint—that is to say, by vital force, mind, will-power.

If mind controls the motion of molecules, then mind must be reckoned among the physical forces, differing, however, from all other modes of physical force in that it always acts at right angles to motion.

It is right that your readers should be informed that there are many philosophers (I allude especially to those who accept the hypothesis of scientific monism) who regard the supposed control of mind over matter, or of matter over mind, as an assumption which is unnecessary and unsatisfactory.

An arbitrary alteration of the weather might involve no contradiction of the principle of the conservation of energy, and yet at the same time completely upset our notions of physical causation by the introduction of metaphysical interference.

If the distinction between generating motion and directing motion is one useful to remember, that between physical and metaphysical control is still more important.

University College, Bristol. C. Lloyd Morgan.

The Influence of Temperature on the Vagus.

In Nature of April 9 (p. 548) I notice that Dr. G. N. Stewart says: "nor has the influence of temperature on the vagus been before studied by a suitable graphic method." It appears to me that this sweeping assertion is not in accordance with facts, for in my paper upon the "Influence of Temperature on the Pulsations of the Mammalian Heart and on the Action of the Vagus," published in the St. Bartholomew's Hospital Reports for 1871, p. 216, I described a graphic method invented by Prof. Stricker, of Vienna, and employed by me with very satisfactory results. The apparatus may have been ruder than that employed by Dr. Stewart, but the tracings obtained by it were sharp and clear, and allowed the beats of the heart to be easily counted, even when the pulse rate was 470 per minute. The important fact that the peripheral ends of the vagus are not paralyzed by heat, but retain their power to the last, was clearly demonstrated in this paper. T. Lauder Brunton.

Antipathy of Birds for Colour.

At this season the yellow crocus is coming up, and again being destroyed, either playfully or maliciously, by the house sparrow. Has the bird an aversion to yellow? This could be tested by throwing bits of coloured wool about, and finding what colours are used in nesting. I should be glad to learn if this has been tried, and the result. M. H. M.

April 13.

The Discovery of Comet *a* 1891.

It is stated that Prof. Barnard, of the Lick Observatory, Mount Hamilton, anticipated me in the discovery of comet *a* 1891, as he first saw the comet on the evening of March 29, whereas my observation was made on March 30. My information on the point is somewhat meagre and uncertain, but it is very likely to prove correct. If so, I must relinquish my claim to priority, and comfort myself with the reflection that my discovery was an independent one, and that it conveyed the first notification of the comet received by the chief Observatories of Europe.

The *Times* of April 1 published a telegraphic message, dated Mount Hamilton, March 31, as follows:—

"A small but fairly bright comet, with a tail of 15 minutes in length, was discovered by Prof. Barnard at 8.34 on Tuesday night at the Lick Observatory, the position being right ascension one hour ten minutes and ten seconds, north declination 44 degrees 48 minutes, moving rapidly southwards in the direction of the sun at the rate of one degree per day."

Now this telegram, if correct, would show that Prof. Barnard's discovery followed, not preceded, my own (made at 9h. on Monday night, March 30), and several scientific journals have alluded to the matter on the basis of the paragraph above quoted. But I think it highly probable the *Times'* telegram contained several inaccuracies, and that Prof. Barnard first saw the comet on Sunday night, March 29. Further particulars from America will no doubt shortly come to hand. If Prof. Barnard is entitled to priority, let it be freely accorded to him, and he will have my sincere congratulations on another success in a field where he has previously earned such high distinction.

Bristol, April 11. W. F. Denning.

ON SOME POINTS IN THE EARLY HISTORY OF ASTRONOMY.[1]

I.

IN the last astronomical course given in this theatre the subject touched upon was our present knowledge of the sun and stars. Some of you may possibly have been present at the lectures, but in any case the point made was something like this: an enormous advance had recently been made in our astronomical knowledge and in our powers of investigating the various bodies which people space, by the introduction of methods of work and ideas from other branches of science: the purpose was to show that the recent progress was to a large extent dependent upon the introduction of methods from other sciences.

The present course will also deal with the sun and stars, but the point of view will be changed. What I shall endeavour to do in the present course will be to show that a knowledge of even elementary astronomy may be of very great assistance to students of other branches of science; in other words, that astronomy is well able to pay her debt. Those branches to which I shall now chiefly refer are archæology, history, folk-lore, and all that learning which deals with man's first attempts to grasp the meaning and phenomena of the universe in which he found himself before any scientific methods were available to him, before he had any idea of the origins or the conditionings of the things around him. You can quite understand that for that we have to go a considerable distance back in time.

It seems to be generally accepted by archæologists that the first civilizations with which we are acquainted were those in the Nile valley and in the adjacent countries.

The information which we possess concerning these countries has been obtained from the remains of their cities, of their temples, even of their observatories and of the records of their observatories: of history on papyrus we have relatively little.

But when we come to the other two groups of nations to which I shall have to refer, China and India, there we have paper records; but, alas, no monuments of undoubtedly high antiquity. It is true that there are many monuments in India in the present day, but, on the authority of Prof. Max Müller, these monuments are relatively modern. We go back in Egypt to a period, as estimated by various authors, of something like 6000 or 7000 years; we go back in Babylonia certainly 4000 years; in China and in India we go back as certainly to more than 4000 years ago.

When one comes to examine the texts, whether written on papyrus, burnt in brick, or cut on stone, which archæologists have obtained from these sources, we at once realize that man's earliest observations of the heavenly bodies may very fairly be divided into three perfectly distinct stages; I do not mean to say that these stages follow each other exactly, but at one period one stage was more developed than another, and so on.

For instance, in the first stage, wonder and worship were the prevalent features; in the second, there was a desire to apply the observation of celestial phenomena in two directions, one the astrological direction, and the other, the direction of utility—for instance, the formation of a calendar and the foundation of years and months and days.

[1] From shorthand notes of a course of lectures to working men delivered at the Museum of Practical Geology, Jermyn Street, in November 1890. The notes were revised by me at Aswan during the month of January. I have found, since my return from Egypt in March, that part of the subject-matter of the lectures has been previously discussed by Herr Nissen, who has employed the same materials as myself. To him, therefore, so far as I at present know, belongs the credit of having first made the suggestion that ancient temples were oriented on an astronomical basis. His article is to be found in the *Rheinisches Museum für Philologie*, 1885.

Only more recently—not at all apparently in the early stage—was any observation made of any celestial object for the mere purpose of getting knowledge. We know from the recent discoveries of Strassmaier and Epping that this stage was reached at Babylon at least 300 years B.C., at which period regular calculations were made of the *future* positions of moon and planets. This of course is now practically the only source of interest in astronomy to us: we no longer worship the sun; we no longer believe in astrology; we have our calendar; but we must have a nautical almanac calculated years beforehand, and some of us like to know a little about the universe which surrounds us.

It is very curious and interesting to know that this first stage, this stage of worship, is practically missing in the Chinese annals; the very earliest Chinese observations show us the Chinese, a thoroughly practical people, trying to get as much out of the stars as they could for their terrestrial purposes.

In Assyria and Babylonia it is a remarkable thing that from the beginning of things—so far as we can judge from the monuments—the sign for God was a star; and although the information is perhaps not so precise as in the case of Egypt, yet no doubt in Babylonia the worship stage existed in equal activity.

We may take the Vedas to bring before us the remnants of the first ideas which dawned upon the minds of the earliest dwellers in Western Asia—that is, the territory comprised between the Mediterranean, the Black Sea, the Caucasus, the Caspian Sea, the Indus, and the waters which bound the southern coasts, say, as far as Cape Comorin. Of these populations, probably the Chaldæans may be reckoned as being the first. According to Lenormant—and he is followed by all the best scholars—this region was invaded in the earliest times by peoples coming from the steppes of Northern Asia. Bit by bit they spread to the west and east. There are strange variants in the ideas of the Chaldæans recovered from the monuments and those preserved in the Vedas. Nevertheless, we find a sun-god—Utu, Babbar[1]—and the following hymn:—

> "Oh Sun, in the most profound heaven thou shinest. Thou openest the locks which close the high heavens. Thou openest the door of heaven. Oh Sun, towards the surface of the earth thou turnest thy face. Oh Sun, thou spreadest above the surface, like a mantle, the splendour of heaven."

The earth to these peoples was a round boat, bottom-uppermost, surrounded on all sides by ocean (Apsu) near the edge; on the edge itself rested the hemispherical sky (Anna). As the sky thus rested on the earth, the earth rested on an abyss, the home of death and darkness.

Let us consider for a moment what were the first conditions under which the stars and the sun would be observed. There was no knowledge, but we can very well understand that there was much awe, and fear, and wonder. Man then possessed no instruments, and the eyes and the minds of the early observers were absolutely untrained. Further, night to them seemed almost death, no man could work; for them, there was no electric light, to say nothing of candles, so that in the absence of the moon the night reigned like death over every land. There is no necessity for us to go far into this matter by trying to put ourselves into the places of these early peoples; we have only to look at the records: they speak very clearly for themselves.

Let us begin with India, whence the first complete revelations of this kind came. Max Müller and others during recent years have brought before us an immense amount of most interesting information, some of which I hope now is to be found in our free libraries.

They tell us that 1500 years B.C. there was a ritual, a set of hymns called the Veda (Veda meaning knowledge).

[1] Maspéro, "Histoire ancienne des Peuples de l'Orient," p. 136.

These hymns were written in Sanskrit, which a few years ago was almost an unknown language; we know now that it turns out to be the nearest relation to our English tongue. The thoughts and feelings expressed in these early hymns contain the first roots and germs of that intellectual growth which connects our own generation with the ancestors of the Aryan races; "those very people who, as we now learn from the Vedas, at the rising and the setting of the sun listened with trembling hearts to the sacred songs chanted by their priests. The Veda, in fact, is the oldest book in which we can study the first beginnings of our language and of everything which is embodied in all the languages under the sun." The oldest, most primitive, most simple form of Aryan Nature-worship finds expression in this wonderful hymnal, which doubtless brings before us the rituals of the ancient Aryan populations represented also by the Medes and Persians. There was, however, another branch, represented by the Zend-Avesta, as opposed to the Vedas, among whom there was a more or less conscious opposition to the gods of Nature, to which we are about to refer, and a striving after a more spiritual deity, proclaimed by Zoroaster under the name of Ahura-Mazda, or Ormuzd. The existence of these rituals side by side in time tends to throw back the origin of the Nature-worship of both. Now, what do we find? In the Veda the gods are called Devas, a word which means bright; brightness or light being one of the most general attributes shared by the various manifestations of the deity. What were the deities? The sun, the sky, the dawn, fire, and storm. It is clear, in fact, from the Vedas that sunrise was, to those from whom the ritual had been derived, the great revelation of Nature, and in time in the minds of the poets of the Veda, "deva," from meaning "bright," gradually came to mean "divine." Sunrise it was that inspired the first prayers of our race, and called forth the first sacrificial flames. Here, for instance, is an extract from one of the Vedas. "Will the sun rise again? Will our old friend the Dawn come back again? Will the power of Darkness be conquered by the God of Light?"

These three questions in one hymn will show what a questionable stage in man's history is thus brought before us. We find very many names for Sun-gods—

Mitra,
Indra (the day brought by the sun),
Sûrya,
Vasishtha,
Arusha (bright or red);

and for the Dawn-gods—

Ushas,
Dyaus,
Dyotanâ,
Ahanâ,
Urvasī,

We have only to consider how tremendously important must have been the coming of the sun in the morning, bringing everything with it, and the dying away of the sun in the evening, followed at once by semi-tropical quick darkness, to cease to wonder at such worship as this. Here is an extract from one hymn to the Dawn (Ushas):—

(1) She shines upon us like a young wife rousing every living being to go to his work; when the fire had to be kindled by men she made the light by striking down darkness.

(2) She rose up spreading far and wide, and moving everywhere, she grew in brightness, wearing her brilliant garment [the mother of the cows (the mornings)], the leader of the days, she shone gold-coloured, lovely to behold.

(3) She, the fortunate, who brings the eye of the gods, who leads the white and lovely steed (of the sun), the *Dawn*, was seen revealed by her rays, with brilliant treasures, following everyone.

(4) Thou art a blessing when thou art near. . . . Raise up wealth to the worshipper, thou mighty *Dawn*.

(5) Shine for us with thy best rays, thou bright Dawn. . . .

(6) Thou daughter of the sky, thou high-born Dawn. . . .

In addition to the Sun and the Dawn, which turn out to be the two great deities in the early Indian Pantheon, other gods are to be met with, such as Prithivī, the Earth on which we dwell; Varuna, the Sky; Ap, the Waters; Agni, the Fire; and Maruts, the Storm-gods. Of these, Varuna is especially interesting to us. We read:—

"Varuna stemmed asunder the wide firmament; he lifted up on high the bright and glorious heaven; he stretched out apart the starry sky and the earth."

Again—

"This earth, too, belongs to Varuna, the king, and this wide sky with its ends far apart. The two seas (the sky and the ocean) are Varuna's loins."

Finally, the result of all this astral worship was to give an idea of the connection between the earth and the sun and the heavens, which are illustrated in later Indian pictures, which bring before us modernized and much more concrete views of these early notions, ultimately transformed into this piece of poetic thought—that the earth was a shell supported by elephants which represent strength, the elephants being supported on a tortoise which represents infinite slowness.

This poetical view subsequently gave way to one less poetical—namely, that the earth was supported by pillars; on what the pillars rested is not stated, and it does not matter. We must not consider this as ridiculous, and pardonable merely because it is so early in point of time, because, coming to the time of Greek civilization, Anaximander told us that the earth was cylindrical in shape, and every place that was then known was situated on the flat end of the cylinder; and Plato, on the ground that the cube was the most perfect geometrical figure, imagined the earth to be a cube, the part of the earth known to the Greeks being on the upper surface. In these matters, indeed, the vaunted Greek mind was little in advance of the predecessors of the Vedic priests.

We now turn to Egypt. Here, as I have said, the main source of information consists no longer in writings like the Vedas, but in the inscriptions on the monuments. It is true that, in addition to the monuments, we have the Book of the Dead, and certain records found in tombs; but, in the main, the source of information which has been most largely drawn upon consists in the monuments themselves.

It has been impossible, up to the present time, to fix with very great accuracy the date of the earliest monuments. If any of you will get from your public libraries any books on Egypt, I am sure you will feel that it is not a question of knowing so little—it is a question of knowing anything at all. When one considers that at the beginning of this century not a sign on any of these monuments was understood, and that now the wonderful genius of a small number of students has enabled Egyptologists to read the inscriptions with almost as much ease and certainty as we read our morning papers; *this* is what is surprising, and not the fact that we as yet know so little, and in many cases lack certainty.

I have told you that probably some of these monuments are 6000 or 7000 years old. How has this been determined? One of the many points investigated by Egyptologists has been the chronology of the kings of Egypt from their first king, whom all students recognize as Mena or Menes. All these students have come to the definite conclusion that there was a King Menes, and that he reigned a long time ago; but with all their skill the total result is that they cannot agree to the date of this king within a thousand years, for the reason that in these early days astronomy was a science still to be cultivated, and therefore the early Egyptians had not a perfect mode of recording; they had no idea of a hundred years as we have. All their reckonings were the reckonings of the reigns of kings. We now, fortunately for us, have a calendar which enables us to deal with large intervals of time, but still we reckon, in Egyptian fashion, by the reigns of kings in our Acts of Parliament. Furthermore, Egypt being then a country liable to devastating wars, and to the temporary supremacy of different kingly tribes, it has been very difficult to disentangle the various lists of kings so as to obtain one chronological line, for the reason that sometimes there were lines of kings existing together in different regions. The latest date for King Menes is, according to Bunsen, 3600 years B.C.; the earliest date, assigned by Boeckh, 5702 years B.C.; Unger, Brugsch, and Lepsius give, respectively, 5613, 4455, 3892. For our purpose we will call the date 4000 B.C.—that is, 6000 years ago.

We come now to deal with the ideas of the early inhabitants of the Nile valley. We find that in Egypt, as in India, we are in presence absolutely of the worship of the Sun and of the accompanying Dawn. The early Egyptians, whether they were separate from, or more or less allied in their origin to, the early Chaldæans, had exactly the same view of Nature-worship as the others had, and we find in their hymns and the lists of their gods that the Dawn and the Sunrise were the great revelations of Nature, and the things which were most important to man; *and therefore everything connected with the Sunrise and the Dawn was worshipped.*

Renouf, one of the most competent of living writers on these subjects, says: "I fear Egyptologists will soon be accused, like other persons, of seeing the dawn everywhere," and he quotes with approbation, and applies to his own subject, the following passage from Max Müller relating to the Veda:—

"I look upon the sunrise and sunset, on the daily return of day and night, on the battle between light and darkness, on the whole solar drama in all its details, that is acted every day, every month, every year, in heaven and in earth, as the principal subject."

Among the names given by the Egyptians to the sun are:—

Hor, or Horus.
Chepera (morning sun).
Rā (noon).
Tmu (evening sun).
Osiris (sun when set).

The phenomena of morning and evening twilight gave rise to the following divinities:—

Isis represents the Dawn and the Twilight; she prepares the way for the Sun-god. The rising sun between Isis and Nephthys = morning.

Nephthys is the Dawn and the Twilight, sometimes Sunset.

Shu is also the Dawn, or sunlight. Tefnut represents the coloured rays at dawn. Shu and Tefnut are the eyes of Horus. Shu was also called "Neshem," which means green felspar, in consequence of the green colour observed at dawn. The green tint at dawn and sunset are represented further by the "sycamore of emerald." Sechet is another goddess of the Dawn, the fiery Dawn.

The *red* colours at sunset were said to be caused by the blood flowing from the Sun-god when he hastens to his suicide. A legend describes Isis as stanching the blood flowing from the wound inflicted on Horus by Set.

Hathor is, according to Budge, identified with Nu or Nut, the sky, or place in which she brought forth and suckled Horus. She is the female power of Nature, and has some of the attributes of Isis, Nut, and Maāt.

I have not time to quote the many hymns to the Sun-gods which have been recovered from the inscriptions, but the following extracts will show that the worship was in the main at sunrise or sunset—in other words, that the *horizon* was in question:—

"Thou disk of the Sun, thou living God! There is none other beside thee. Thou givest health to the eyes through thy beams, Creator of all beings. *Thou goest up on the eastern horizon of the heaven* to dispense life to all which thou hast created—to man, four-footed beasts, birds, and all manner of creeping things on the earth where they live. Thus they behold thee, and they go to sleep when thou settest."

Hymn to Tmu—

"Come to me, O thou Sun,
Horus of the horizon, give me help."

Hymn to Horus—

"O Horus of the horizon, there is none other beside thee,
Protector of millions, deliverer of tens of thousands."

Hymn to Ra-Tmu-Horus—

"Hail to thee of the double horizon, the one god living by Maat. . . . I am the maker of heaven and of the mysteries of the twofold horizon."

Hymn to Osiris—

"O Osiris! Thou art the youth *at the horizon* of heaven daily, and thine old age at the beginning of all seasons. . . .

"The *ever-moving* stars are under obedience to him, and so are *the stars which set.*"

Hymn to Ra—

"O Ra! in thine egg, radiant in thy disk, shining forth from the horizon, swimming over the steel (?) firmament.

"Tmu and Horus of the horizon pay homage to thee (Amon-Ra) in all their words."

We next have to gain some general idea of the Egyptian cosmogony—the relation of the sun and dawn to the sky; this is very different from the Indian view. The Sky is Nu (Fig. 1), represented as a female figure bending over Seb, the Earth, with her feet on one and her finger-tips on the other horizon. The Sun-gods, and even the stars, were supposed to travel in boats across the firmament from one horizon to the other. The under-world was the abode of the dead, and daily the sun and the stars which set died on passing to the regions of the west, or Amenti, below the western horizon, to be born again on the eastern horizon on the morrow. In this we have the germ of the Egyptian idea of immortality.

Among other gods which may be mentioned are Chnemu, the "Moulder," who was thought to possess some of the attributes of Rā; and Patah (or Ptah), the "Opener," who is at times represented with Isis and Nephthys, and then appears as a form of Osiris.

We can now begin to glimpse the Egyptian mythology. Seb, the Earth, was the husband of Nut, the Sky; and the Sun- and Dawn-gods and goddesses were their children, as also were Shu, representing sunlight, and Tefnut, representing the flames of dawn.

Maāt, the goddess of law, was the daughter of Ra.

When one comes to consider the Rig-Veda and the Egyptian monuments from an astronomical point of view, one is struck by the fact that the early worship and all the early observations related to the horizon. This was true not only for the sun, with which so far we have exclusively dealt, but it was equally true of the stars which studded the general expanse of sky.

We must be perfectly clear before we go further what this horizon really is, and for this some diagrams are necessary.

The horizon of any place is the circle which bounds our view of the earth's surface, along which the land (or sea) and sky appear to meet. We have to consider the relation of the horizon of any place to the apparent movements of celestial bodies at that place.

We know, by means of the demonstration afforded by Foucault's pendulum, that the earth rotates on its axis, but this idea was of course quite foreign to these early peoples. Since the earth rotates with stars, infinitely removed, surrounding it on all sides, the apparent movements of the stars will depend very much upon the position we happen to occupy on the earth: this can be made quite clear by these diagrams. An observer at the North Pole of the earth, for instance, would see the stars moving round in circles

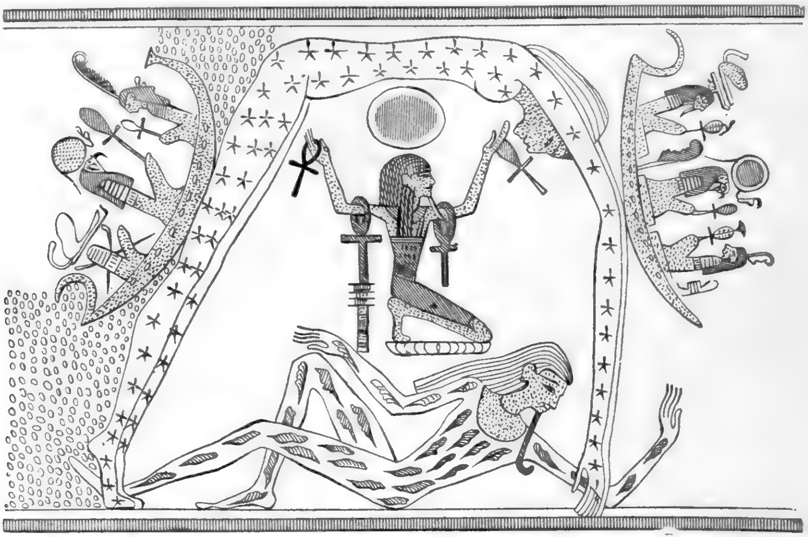


Fig. 1.—Egyptian representation of the Earth (Seb), a recumbent figure; with Nu stretching over the Earth from horizon to horizon, and the boats of the Sun-gods floating over her.

parallel to the horizon. No star would either rise or set—one half of the heavens would be always visible above his horizon, and the other half invisible; whereas

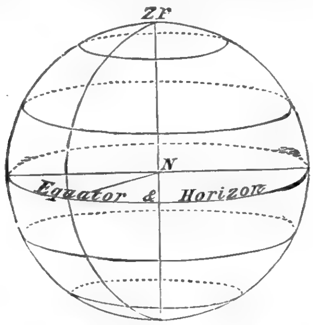


Fig. 2.—The celestial sphere, as viewed from the North Pole. A parallel sphere.

an observer at the South Pole would see that half of the stars invisible to the observer at the northern one, because it was the half below his horizon. If the

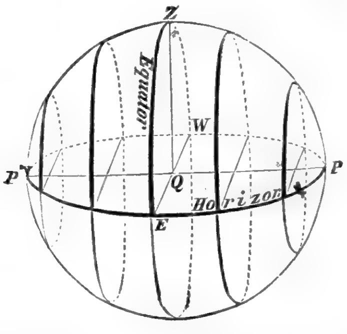


Fig. 3.—The celestial sphere, as viewed from the Equator. A right sphere.

observer be on the equator, the movements of the stars will appear to be as indicated in this diagram (Fig. 3)—that is, all the stars will rise and set, and each star in turn will be half its time above the horizon, and half its time below it. But if we consider the position of an observer in middle latitude, say in London, we find that some stars will always be above the horizon, some always below—that is, they will neither rise nor set. All other stars will both rise and set, but some of them will be above the horizon for a long time and below for a short time, whereas others will be a very short time above the horizon and a long time below it.

Wherever we are upon the earth we always imagine that we are on the top of it. The idea held by all

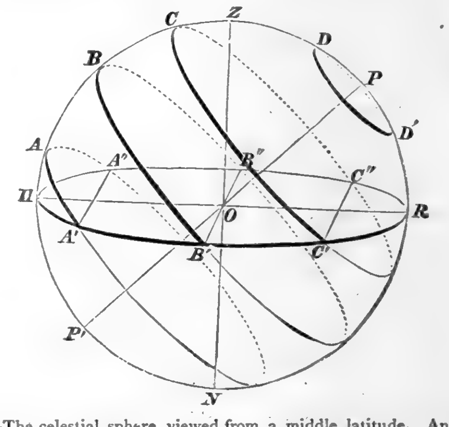


Fig. 4.—The celestial sphere, viewed from a middle latitude. An oblique sphere. In this woodcut DD′ shows the apparent path of a circumpolar star; BB′B″ the path and rising and setting points of an equatorial star; CC′C″ and AA′A″ those of stars of mid declination, one north and the other south.

the early peoples was that the earth was an extended plain: they imagined that the land that they knew and just the surrounding lands were really in the centre of the extended plain. Plato, for instance, was content to put the Mediterranean and Greece upon the top of his cube, and Anaximander placed the same region at the top of his cylinder.

By the use of the globe we can best study the conditions of observation at the poles of the earth, the equator, and some place in middle latitude. The wooden horizon of the globe is parallel to the horizon of a place

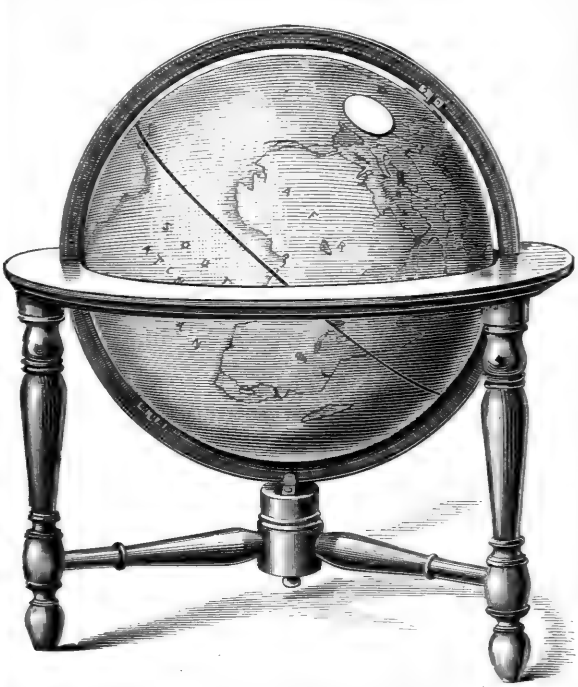


FIG. 5.—Diagram to show how the inclination of the horizon of London will change with the rotation of the Earth.

at the top of the globe, which horizon we can represent by a wafer. In this way we can get a very concrete idea of the different relations of the observer's horizon to the apparent paths of the stars in different latitudes.

J. NORMAN LOCKYER.

(*To be continued.*)

ADDITIONAL RESULTS OF THE UNITED STATES SCIENTIFIC EXPEDITION TO WEST AFRICA.

WRITING a year ago from Ascension Island, I gave to NATURE a partial account of the work of the Expedition from October 1889 to March 1890. Subsequently the U.S.S. *Pensacola*, Captain A. R. Yates, U.S. Navy, commanding, arrived at Barbados on April 28, at Bermuda on May 18, and terminated her cruise with the Expedition on reaching New York on the 23rd of the same month.

My previous communication to NATURE (vol. xlii. p. 8), relates the great obligation of the Expedition to His Excellency Governor Sir H. B. Loch, and to Dr. David Gill, H.M. Astronomer at Cape Town, to His Excellency Governor Antrobus, at Saint Helena, and to Captain Napier, R.N., then in charge of Ascension, acting in accordance with the very liberal instructions of the Admiralty. Although our arrival at Barbados was entirely unexpected, all necessary facilities were at once provided by direction of His Excellency Governor Sir Walter Sendall.

The gravity and magnetic work at Bridgetown occupied nearly a fortnight. Mr. E. D. Preston, in special charge of this department of the Expedition, occupied in all fourteen magnetic stations, of which five were on the West Coast of Africa, and eight on islands of the North and South Atlantic. The area of the gravity stations extends from Cape Town to Washington, covering a range of 73° in latitude, and 96° in longitude. The elevations of these stations range from 7 feet to 2250 feet above sea-level. These observations are now in progress of reduction by Mr. Preston, under the direction of Prof. Mendenhall, Superintendent of the U.S. Coast and Geodetic Survey; and the definitive results will, it is hoped, be available at the conclusion of the present year. "Whether," says Mr. Preston, "they show the Atlantic islands to be heavy or light as compared with the continental masses, they will at least add considerable new material for the determination of the earth's figure."

By direction of the Hon. Secretary of War, and General Greely, Chief Signal Officer of the Army, Prof. Cleveland Abbe was detailed as meteorologist of the Expedition. His observations and researches, assiduously conducted during the entire cruise of the *Pensacola*, are so significant that no brief statement can do them full justice. From the forthcoming Report of the Expedition I excerpt the following sections of Prof. Abbe's preliminary report :—

(1) I should mention at the outset that the work with small balloons was carried on sufficiently to demonstrate the practicability of this method of getting the actual velocity of the wind; but, by the advice of the maker of the balloons, I had omitted taking up with me the varnish ordinarily used, as being probably unnecessary and possibly dangerous to manipulate on ship-board. The result was that I found it would become necessary to use more balloons than originally contemplated. Still I deem it a decided gain to have been able to make enough observations to show that the method of determining the velocity of the wind by small balloons is accurate and practicable, both on land and sea, and but little, if at all, more expensive than the use of self-recording anemometers.

(2) The mirror nephoscope used by me rested on the Ritchie liquid compass (U.S. Navy standard), which was established on the after-bridge, and which compass I was free to use at all times. Although free in its gimbals, yet the compass is by no means exactly horizontal; so that more numerous observations must be made at sea than on land, if the same resulting accuracy is desired. I was able to maintain daily observations, and I can but consider that the opportunity thus offered me has been entirely unique in the history of meteorology; and unless I am greatly mistaken in my knowledge of the literature of the subject, it will be found that the results attained will form a valuable basis for further study. My observations have in fact led to some change in my own views as to the general circulation of the atmosphere, and every form of fluid motion has been found by me to be visibly illustrated in the cloud-structure.

(3) I have attempted a simple and rational classification of clouds, based, not on shapes and appearances, but primarily on their modes of formation.

(4) A method of using the nephoscope has been elaborated for the determination of actual heights and velocities of clouds (or balloons) by combining observations made when the observer is moving successively in two different directions, or with two different velocities. The instrument can be used in a waggon, or car, or boat. I call this "*the aberration method*," as distinguished from ordinary parallax methods.

(5) My work shows that the observed movements of the clouds and the wind have such a relation to the presence of a storm centre, an island, or to any other disturbance, that one should be able to locate the position of such a disturbance by observations made at one locality.

(6) At Fayal, on the summit of Pico, I first observed the formation of a cloud, with which I afterwards became

familiar in other similar localities, and which must be well known in mountainous countries. Although not often clearly described, this is a slender white stream, which is sometimes likened to a table-cloth or to a soft hood. It has nothing of the boiling vertical motion characteristic of the cumulus; nothing of the curling and rolling motions characteristic of the scud-cloud. It is a smooth stream, of soft appearance, of very gentle curvature, and illustrates an elementary form of fluid motion. It is the simplest cloud that can be formed by the slow steady rise and fall of undulating streams of air. As seen from Fayal, this cloud marked the lowest limit at which the densest cirrus could form in an atmosphere having the temperature and moisture then prevailing. When a mountain obstructs the lower wind, it forms a scud-cumulus, or cumulus whose ascending currents are due to obstructed wind rather than to internal heat. These cumuli themselves obstruct the wind prevailing at their own higher levels. The latter is therefore pushed up, but it rises slowly and smoothly, being a case of "steady motion" without forming discontinuous eddies; and thus generates the smooth white hood, or arched crescent cloud.

The hood will form on the peak itself if the air below the summit is calm, so that scud-cumuli do not form by reason of horizontal winds, or if the lower air be cold or dry and dense, so that the cumuli will not form by reason of heat. In such cases the summit of the peak can itself be considered as a small obstacle to the gentle current at its level. When large cumuli form in open regions, their summits then become like the mountain, comparatively small obstacles to the upper current; and, under these circumstances, beautiful hoods form in the warm moist air ascending on the sunny sides of the cumuli. For the formation of these, it is simply necessary that the upward deflection of the disturbed horizontal motion of the upper current shall be so gentle as not to introduce sudden curves and eddies. Even without the formation of a visible cumulus there may be a large invisible mass of rising air, not yet cooled to saturation; and in such a case the crescent hood may even be seen to form in mid-air, and the cumulus may subsequently become visible. At Green Mountain, on the Island of Ascension, the heavy south-east trade strikes an abrupt surface, and is thrown up to a great height, whence it descends; and, like a standing wave in the river, flows off many miles to leeward. Several times I was able to observe the crescent cloud forming in mid-air in a clear blue sky for a short space only, and evidently at the very summit of the first of these standing waves. On Table Mountain, at Cape Town, the so-called "table-cloth" was, at least during our visit, a mass of rolling scud-cumuli; but above these there were occasionally to be seen small beautiful crescent hoods.

(7) At Freetown, Sierra Leone, the study of two arched squalls showed, as I expected, that they and the African tornado and the afternoon thunderstorms of the eastern coast of the United States are the same in nature and laws, differing only in degree.

(8) At Elmina the study of the Harmattan wind, through which we had been passing for several days, was combined with notes made by a good observer on shore, showing unmistakably that this is a mass of dry, and therefore dense, air flowing from the interior of Northern Africa at this season. It brings with it the fine white ashes due to innumerable local bush-fires in the interior, commonly called "dust from the desert"; and the whole phenomenon is analogous to the flow of dry, cold air from Canada, south-eastward over the United States, making our so-called "north-westers." The Bora of Russia and the Pampero of South America have a similar physical origin.

(9) At Cape Ledo I found the finest imaginable opportunity to study the formation of the overflow from the summit of the cumulus clouds, and its gradual transformation into slowly-dissolving stratus. It was a daily phenomenon at Cape Ledo to observe the large cumuli ascend bodily, lose their flat bottoms, become pear- or balloon-shaped, and then spread out into fields of most beautiful spots, vortices, striæ, curds, and other forms. The "mackerel sky" did not occur at this time. The motion of the lower cumuli was with the lower southwest winds; but the overflow into the stratus and cirrus moved from the east in the afternoons out westward over the sea. But if these clouds endured over night, as they usually did, then in the morning they were found to have descended, and to be moving from the west toward the land; while below them the cool, easterly land breeze existed for a few hours with a slight scud or cloud-rip on its upper surface.

(10) At Cape Town we experienced an unusually heavy south-easter. There had also been a remarkably destructive north-westerly storm in the previous July. I made detailed maps illustrating the south-easter, and obtained from Mr. A. G. Howard his own maps illustrating the storm of July, all based on the data of the Meteorological Commission for South Africa. These maps were made the basis of an address to the South African Philosophical Society on the practicability of storm warnings, and I think it may safely be anticipated that within a few years there will be such a system in active operation at all the ports on the South African coast. The remarkable development of rare and beautiful flowers over the whole of Table Mountain is, of course, possible only by virtue of the damp atmosphere and the steady drip of fog-particles from leaves and rocks, quite as much as by virtue of the rainfall, as ordinarily measured.

(11) At Saint Helena my attention was attracted first of all to the matter of rainfall. A previous study of the subject had convinced me that the local temperature of the air over the land could have little or nothing to do with the rainfall in a small isolated tropical island; that the extraction of the moisture as rain from the air must be done in cumuli that are formed by the impinging of the trade-wind upon the island as an obstacle to its progress, and not in cumuli carried up by heat of an overheated land-surface; and that therefore the rainfall of Saint Helena was a direct indication of the average wind striking against the island, since the moisture is so uniform. I consider the Saint Helena rainfall as the best index to the average movement of the air, and as depending therefore, like the movement of the air, on the meteorological conditions over a large surface of the ocean. Similar considerations apply to other islands, as Ascension and Barbados, at both of which I have therefore made the rainfall a matter of special solicitude. At Saint Helena my attention was attracted by "the rollers," as being an unsolved problem in ocean meteorology, and I made arrangements for future observations to be kept by Mr. George Bruce, of the Customs Department, rather more fully than hitherto.

(15) At the Island of Ascension I was so fortunate as to be allowed to occupy the signal station at the summit of Cross Hill, a steep conical hill of volcanic cinder rising to 870 feet above the sea-level, and immediately overlooking Garrison and the landing-place on the leeward side of the island, as well as the lowlands of the interior of the island to windward, with Green Mountain bearing east-south-east about three miles distant. Here I observed, not only the motions and phenomena of clouds above me, but also observed their shadows on the ground and sea beneath me, so that I was able to determine the heights and linear velocities by the simplest mathematical methods. Certainly there are few, if any, spots in the world where the phenomena of the trade-winds with their annual and their irregular changes can be more successfully observed. I have certainly never before occupied a position so favourable for all manner of studies of the

clouds, winds, twilight phenomena, and the "rollers"; nor could any place be found where these particular phenomena are more highly developed.

(16) With regard to the "rollers," I would say that previous studies at Saint Helena had led me to think that undoubtedly the heavy surf constituting their prominent characteristic as described by all previous observers must be due to distant winds, not necessarily storms; and that, as an earthquake wave can traverse the whole of an ocean in a day, so the heavy swell attending a hurricane or a "norther," could easily pass to Ascension and Saint Helena from the most distant parts of the North Atlantic Ocean.

Within a week the "single-roller flag," and finally the "double-roller flag" was displayed at the Garrison landing, and I found myself looking down upon the phenomenon that had so long been a mystery. The first glance showed that the character of the phenomenon had been wholly mistaken; and on subsequent inquiry I found that probably no observer of intelligence had ever undertaken the slight hardship of a solitary residence on Cross Hill for the purpose of studying the rollers.

Otherwise it would be inconceivable that some one should not ere this have recognized a feature that certainly could never be seen from the deck of a vessel or from a lower station—namely, that the rollers are essentially the deflection around to the leeward side of what would be merely a heavy swell on the windward side of the island; and that the double rollers are simply the interference of the two sets of rollers coming around the island by the right hand and by the left. A swell such as would be caused in the open sea by a trade-wind of force 4, blowing for two days over a limited part of the ocean, will, on reaching the windward side of Ascension, or Saint Helena, produce a phenomenon on the leeward side precisely such as anyone can reproduce by studying the interference of waves in shoaling spots in a small tub of water. The angle at which the rollers interfere to produce double rollers at Garrison landing is about 135°; and as you proceed further from the island to leeward, the angle diminishes. When we sailed away from the island I was able to determine the angle as being about 40° at about fifteen miles distance. It will be at once a matter of surprise that rollers are peculiar in their severity at Ascension and Saint Helena; for other islands might be expected to show similar phenomena. But the fact is that the severity of the rollers depends first on the shape of the island; second, on its size; third, on the location and character of the shoals which surround it, all taken in connection with the length and height of the original swell; so that we should not expect many islands to possess the necessary combination of peculiarities. In fact, I have thus far learned of only one other island—namely, Saint Paul de Noronha, where the roller phenomena are conspicuous. At the Island of Barbados I particularly observed an appreciable swell curl around to the leeward side of the island from both its northern and southern extremity, and was told that occasionally these (which might be so-called single rollers) would be troublesome; but the island is so shaped that before these rollers interfere and conspire together, they have dwindled into an unimportant swell.

(17) Our second approach to and passage through the doldrums, in April 1890, served to give me the clue to a satisfactory solution of the question as to how it is that the north-east trades interact at the equator—namely, whether they pile up over the doldrums and flow back as massive upper currents, according to the ancient theory; or whether they interpenetrate each other, according to Maury's theory; or whether they revolve around each other in horizontal curves, slipping past each other to the north and south; or whether they meet and conspire together in a powerful upper easterly wind, as Abercromby maintains. This question is now readily settled by recognizing that the important general features of the circulation in the lower atmosphere are as follow:—As the trades approach the doldrums from either side, there is a continual diurnal uprising and return flow taking place, so that the returning upper anti-trades are perpetually being supplied by new air, and derive scarcely any of their material from the central region of the doldrum itself. Each successive ascending mass diminishes the inertia of the matter in the lower trade wind by necessitating the descent of a little air from the anti-trade, so that the inertia of the lower trade considered as a whole is all used up by the opposition of these descending waves, some time before it can reach the doldrums. This causes the broad irregular calm space near the equator to have no horizontal motion, and only a diurnal vertical interchange. The motion of this doldrum region horizontally in either direction depends upon the balance of pressure on the great areas of moving air around it; and it can, I believe, be deduced from anemometer records from a few such island stations as Ascension and Saint Paul de Noronha. A vertical section of the trades would show them to be wedge-shaped, being shallower at the high latitudes, while the overlying anti-trades are deeper at high latitudes. In the doldrums, as high up as clouds are formed, the prevailing characteristic of the circulation is a vertical one repeated diurnally for months and years without any systematic interchange of air to the north or south.

(20) At Barbados I was able to secure a large amount of manuscript meteorological data, and was delighted to find that the magnificent system of rainfall stations developed by Sir Rawson W. Rawson, Governor at Barbados 1866-75, is still maintained by the Government; and although the number of stations had fallen from 250 to 80, yet the system remains one of the best in the world.

Cleveland Abbe.

The unfortunate appearance of "the grip" on board the *Pensacola*, when two days out from Barbados, caused our prompt quarantine on reaching Bermuda. Meteorological work at the latter place was, therefore, necessarily much abridged. Mr. Preston was, however, permitted to land with his instruments at Quarantine, Nonsuch Island; and by remaining after the departure of the *Pensacola*, he was enabled, through the courtesy of His Excellency Governor Lieut.-General Newdigate-Newdegate, to carry on the magnetic and gravity work as elsewhere, thus preserving the chain of Expedition stations unbroken.

David P. Todd.

Amherst College Observatory, Massachusetts, March 24.

VEGETATION OF LORD HOWE ISLAND.

THERE is nothing absolutely new to announce concerning the flora of this remote islet; but what has been published is in the form of Government reports, which have a comparatively restricted circulation, and many persons who would be interested in their contents are unaware of their existence. And even when one knows of the existence of such reports, it is often difficult to procure them. Through the intermediary of Sir Saul Samuel, Agent-General for New South Wales, the library of the Royal Gardens, Kew, has just received a copy of a report on the state and prospects of Lord Howe Island, with a number of photographic illustrations of the scenery and vegetation of the island; and it is on account of some of these illustrations that I have thought it worth while making known to the readers of Nature the existence of such a report, though it was published as long ago as 1882. Unlike the majority of such documents, this report is too meagre; "Thompson's farm" and other matters being mentioned and illustrated in such a manner as to take for granted an amount of previous knowledge that very few readers could possibly have possessed.

Although so remote and so small, Lord Howe Island supports an indigenous flora of a highly interesting character, especially interesting because it includes some plants whose nearest allies are natives of New Zealand. The island is about 300 miles from Port Macquarie, the nearest point of the Australian mainland, in 31° 30′ S. latitude. It is seven miles long, with an average breadth of about a mile, and the basalt mountains rise to a height of nearly 3000 feet. The soil is fertile, and is, or rather was, everywhere covered with vegetation. The scenery is beautiful; the climate is described as unsurpassable, and a great future is predicted for the island as a sanatorium, "when the Australian colonies become more densely inhabited." Without waiting for the time when Australia will be crowded with inhabitants, Lord Howe Island might be made a pleasant holiday resort, involving just enough of a sea voyage to be exciting and exhilarating, and not long enough to be monotonous.

The most complete account of the flora yet published is by Mr. Charles Moore, Director of the Botanic Gardens, Sydney, N.S.W., though many of the new plants then—1869—collected by him have since been published in various books and periodicals. The dominating feature in the vegetation is composed of palms, of which there are three or four species peculiar to this island—a condition of things paralleled in remote insular floras only in the Seychelles. Next in interest and prominence are the four or five endemic species of tree ferns, which, however, we are informed, in the illustrated report referred to, by the Hon. J. Bowie Wilson (botany by Mr. J. Duff), are fast disappearing from the lowlands, and will soon be extinct if their removal is not absolutely prohibited. In this connection one is gratified to find both the chief of the Commission of Exploration, and the botanist attached thereto, strongly urging the Government to take active steps to preserve the beautiful vegetation of the island, and especially to make no concessions, nor grant any leases that might entail any further destruction of the woods. Commonest among the other trees are *Hibiscus Patersonii*, *Myoporum acuminatum*, and *Ochrosia elliptica*—all three Australian trees; one or more species of *Ficus*, and one or more endemic species of screw-pine. One of the vegetable wonders of the island is a huge banyan-tree (*Ficus* sp.), said to cover three acres of ground; but no particulars are given of this remarkable tree, beyond a photograph of a portion of it. This is rather disappointing, because of all the famous banyan-trees in India, some of which are encouraged by artificial means in the development of the aërial descending roots, which eventually become auxiliary trunks, few surpass in size this one, on such a speck of an island. The celebrated banyan between Poona and Kolapore, in the Bombay Presidency, is, indeed, the only one, of which I have found a record, that covers a greater area than the Lord Howe Island banyan, and that, according to measurements given of the spread of its branches, must cover between six and seven acres.

In striking contrast to the flora of Australia, the flora of Lord Howe Island, like that of New Zealand, contains exceedingly few species of the large natural order Leguminosæ. Out of five species collected, three are common sea-side plants that often establish themselves on a shore from seeds cast up by the waves. Of the other two, one belongs to the otherwise exclusively New Zealand genus *Carmichaelia*, and the other, *Sophora chrysophylla*, is also a native of the mountains of the Sandwich Islands, and has hitherto been found nowhere between these two distant parts of the immense Pacific Ocean, and nowhere else in the world. From the foregoing notes may be gathered what an interesting flora that of Lord Howe Island is, and it is to be hoped that the recommendations of the Commissioners for its preservation have been carried out by the Government of New South Wales.

W. Botting Hemsley.

NOTES.

At the meeting of the Royal Geographical Society on Monday, it was announced that the following awards had been made:—To Sir James Hector, M.D., F.R.S. (Director of the Geological Survey, &c., of New Zealand), Royal Medal; to Dr. Fridtjof Nansen, Royal Medal; to Mr. William Ogilvie, the Murchison grant; to Mr. W. J. Steains, the Back grant (one year); to Dr. David Kerr Cross, the Back grant (one year); to Lieutenant B. L. Sclater, R.E., the Cuthbert Peek grant; to Mr. A. E. Pratt, the Gill memorial.

The next ordinary general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, April 30, and Friday evening, May 1, at 25 Great George Street, Westminster. The chair will be taken at half-past seven p.m. on each evening by Mr. Joseph Tomlinson, the President. The following papers will be read and discussed, as far as time permits:—Research Committee on marine-engine trials: report upon trial of the steamer *Iona*, by Prof. Alexander B. W. Kennedy, Chairman; on some details in the construction of modern Lancashire boilers, by Mr. Samuel Boswell, of Manchester. The anniversary dinner will take place on Wednesday evening, April 29.

At the Physical Society, on Friday, in addition to the papers already announced, an account will be given, by Prof. Silvanus P. Thompson, of erecting prisms for the optical lantern. A new prism by Mr. Ahrens will be exhibited.

The Kew Committee of the Royal Society have given notice that they are prepared to examine, at the Kew Observatory, photographic lenses, for the purpose of testing them and of certifying their performance.

The twenty-second annual meeting of the Norfolk and Norwich Naturalists' Society was held in the Norwich Museum on March 31 last, Mr. Henry Seebohm, the President, in the chair. Dr. F. D. Wheeler was elected President for the ensuing year. The Treasurer's report showed that the Society was in a sound position financially, and that the present number of members is 254. Mr. Seebohm read his presidential address, and referred to the loss the Society had sustained by the death of Mr. J. H. Gurney and Mr. John Gunn.

Writing on April 4, a Constantinople correspondent of the Vienna *Vaterland* says that, on the preceding day, the hamlet of Adil-Djevas, in the district of Van, in Armenia, had been destroyed by an earthquake. One hundred and forty-six houses had been destroyed, 240 other buildings had been much injured, and hundreds of lives had been lost.

According to a telegram sent through Reuter's Agency, a large body of water has been discovered at El Golea, in the Sahara Desert, about 120 feet below the surface. It throws up nearly forty gallons per minute at present, and it is anticipated that the yield will be much greater when more perfect access to the water is attained. The discovery is regarded as of high importance, as this is the first time that water has been found in the Sahara at such a slight depth underground.

A lady sends the following note on a meteor seen by her to pass across the sky from north-west to south-east about 7.20 p.m., March 26. It was seen from the bridge over the River Fal, at Ruanlanihorn, Cornwall:—"Body very bright, brilliant white light, towards the back a slight flame-like appearance, a slight delicate tail rather more inclined to reddish light.

It travelled fast across the sky, and I saw it first when just about the highest centre, and it disappeared to the south-east behind the hill (our copse and glebe), which was just in front of me as I was crossing the sette on my way home. It travelled fast, but not nearly so fast as any meteor I have ever seen before; but with a quick rate, of steady perceptible time."

AMONG the results already obtained from the oceanographic expedition of the *Pola*, organized by the Academy of Sciences of Vienna, are the following:—The water of the central basin of the Mediterranean was found to be warmer, denser, and richer in dissolved salts, than the western basin. As regards the penetration of light into the sea, a white disk was visible only at a depth of 43 m., but photographic plates were affected at a depth of 500 m. Starting from the surface of the sea, the quantity of oxygen dissolved at first increases with the decrease of temperature; but then again decreases, so that at a depth of 3000 m. the proportion is the same as that at the surface. In no case was any free carbonic acid found. The nitrogenous substances in solution vary in inverse proportion to the depth; that of ammonia varies but slightly, but is greater in the lower strata.

IN the course of excavations which are being carried out in the neighbourhood of Vienna by the Academy of Sciences, a cavern was lately discovered on the slope of the mountain at Baden. A correspondent writes to the *Times*:—"It was plain on a cursory inspection that the cavern had been used not only in the Middle Ages, but long previously. At the time of the Roman occupation Baden was the encampment of a veteran legion who were well acquainted with the good qualities of the waters. Decided remains of the foundations of a vestibule were found at the entrance of the cave. In a niche hewn out of the rock was an altar with the sacrificial stone table. In front of the cavern was a regularly-constructed building, fully 10 feet below the surface of the ground above, designed probably to conceal the cavern behind, which was most probably employed as a temple to Mithras. There were two stalls for horses, fragments of utensils, knives, flint arrow-heads, carved bones, mixed up with Roman coins, lamps, and stamped tiles."

AT the meeting of the French Meteorological Society on March 3, a communication from M. Marès showed that the weather in Algeria had been as remarkable during the last winter as in Europe. The author stated that in many localities the excessive rainfall had prevented the sowing of seeds, and in the mountainous districts, where the sowing had taken place early, the seed had been swept away by the torrents. About the third week in January a heavy fall of snow lay on the Mitidja and the Sahel for two whole days; the writer states that for the last thirty-five years, although he had sometimes seen snow fall, it did not lie an instant on the ground. The effects had been disastrous to early crops and to many animals.

THE April number of *Himmel und Erde* contains an article by Dr. W. J. van Bebber, of the Deutsche Seewarte, on the typical weather conditions of winter. The writer shows very clearly by means of charts how the disposition of barometric pressure over the Atlantic and the continent of Asia regulates the weather over Western Europe. Nearly twenty years ago the synoptic charts issued by Captain Hoffmeyer, of the Danish Meteorological Institute, showed that there were three large areas of very low barometric pressure in the Atlantic, the most important being south-west of Iceland, and two smaller areas, one on the eastern side towards the northern Arctic Ocean, and another on the west side towards Davis Straits. These areas cause the westerly and south-westerly winds which bring the damp and warm air over Europe. The shifting of their positions causes the variations in our wind-system. M. L. Teisserenc de Bort has pointed out the important part also played by areas of high barometer, which has given greater importance to the Danish synoptic charts. The most important area of high pressure for Western Europe is that stretching eastwards over the Azores and Madeira. If this area shifts north-eastwards towards France, it blocks the way of the air over the ocean, and the weather becomes foggy and cold. Another important barometric maximum persists over Central Asia. This maximum is subject to frequent modifications; sometimes it splits up into two parts, one of which not rarely shifts as far westwards as Scandinavia, and produces a persistence of cold easterly winds over Western Europe—especially when the pressure over Southern Europe is low, which was generally the case in the past winter.

THE new number of the *Mineralogical Magazine* opens with a paper on cassiterite, "sparable tin," from Cornwall, by R. H. Solly. The other papers are: twins of marcasite in regular disposition upon cubes of pyrites, by C. O. Trechmann; the tetartohedrism of ullmannite, by H. A. Miers; a student's goniometer, by H. A. Miers; on an eclogite from Loch Duich, by J. J. H. Teall, F.R.S.; on a micro-granite containing riebeckite from Ailsa Craig, by J. J. H. Teall, F.R.S.; on occurrences of riebeckite in Britain, by Grenville A. J. Cole; on a rapid method for the accurate recognition of sulphides, arsenides, antimonides, and double compounds of these bodies with metals, by Charles A. Burghardt; a system for constructing crystal forms by the plaiting of their zones, by J. Gorham. There are also reviews and abstracts.

OUR young contemporary *Neptunia*, published at Venice, includes in its programme an account of the existing biological stations in various parts of the world. In its first number (January 1891) there is an historical sketch of the work done in the Marine Laboratory of Luc-sur-Mer, Normandy, attached to the Faculty of Sciences at Caen. Established in 1883 at the suggestion of Prof. Deslongchamps, of Caen, and supported by a grant of 30,000 fr. from the Council-General of the Calvados, it has since been under the direction, successively, of Profs. Delage and Laffine. During the seven years of its existence some admirable biological work has been done in this laboratory, the results of which have been published in the French scientific journals. The researches carried on in 1888–89 include a "Memoir on the Organization of the Chætoptera," by Prof. Laffine; "Researches on the Lower Algæ," by M. E. Dangeard; "Researches on the Sponges of the Manche," by M. Topset; "On the ink-bag of the Mollusca," by M. Letellier. In a later number of the same journal there are accounts of the movable zoological station of the Committee for the Exploration of Bohemia, and of the marine zoological laboratory at Rapallo.

IN a recent report by Lord Vaux of Harrowden, Secretary to the British Legation at Stuttgart, on agriculture in Würtemberg, reference is made to agricultural education in that State. This is cared for by numerous schools and societies, and appears to be fully appreciated by the peasants and others. Almost every institution of this sort had greater demands made upon it in 1889 (the year to which the report specially refers) than in the previous year. The Agricultural and Gardening School had its normal number of students; the School of Vineyards, in consequence of increasing demands for tuition in this branch, was again forced to exceed its statutory number of pupils; the Agricultural Winter School was attended by 103 scholars, being an increase of six on the previous year. A sixth school was added to the five already in existence for teaching country girls and young women farmhouse and dairy work. The travelling teachers of husbandry, as well as those specially devoted to orchards and vineyards were in great request among local societies and by the communal authorities. Sixty-four students attended the lectures upon orchard cultivation; ninety-two farriers were taught at the various veterinary colleges and schools throughout the country. The winter evening agricultural schools, reading clubs, and local libraries all showed a considerable increase both in numbers and in attendance. Altogether some 23,400 persons attended agricultural schools or lectures on husbandry during the year. This is rather more than 1 per cent. of the total population of the country, and is good evidence that the people as a rule do not neglect the opportunities given them of becoming successful agriculturists.

BORON IODIDE, BI_3, has been prepared by M. Moissan, and its properties, which are of a somewhat remarkable nature, investigated. It may be obtained in three ways: (1) by leading gaseous hydriodic acid mixed with the vapour of boron chloride, BCl_3, through a porcelain tube heated to redness; (2) by the action of iodine upon boron at a temperature of 700°–800° C.; (3) by the action of hydriodic acid gas upon amorphous boron. The third method affords the best means of preparing the new substance in quantity. A current of gaseous hydriodic acid is first well dried by passage through porous calcium iodide; it is then led over amorphous boron contained in a hard glass tube heated to a temperature approaching that of the softening of the glass. At the commencement of the experiment a small quantity of iodine vapour makes its appearance, and is allowed to escape. When this ceases a dry receiver is attached to the end of the combustion tube, and a more or less deeply coloured crystalline product begins to collect. In a short time the reaction becomes very energetic, large quantities of the crystalline body being deposited, of a bright reddish-purple colour, and almost pure hydrogen escaping. The coloration is due to a small quantity of admixed iodine, for if the solid product is treated with carbon bisulphide it entirely dissolves, forming a solution of the same colour as that of iodine in carbon bisulphide, and which is rendered colourless by agitation with mercury. The solution in carbon bisulphide deposits on evaporation colourless transparent tabular crystals, somewhat nacreous in appearance, of pure boron iodide. The crystals are very sensitive to light; their colourless solution in carbon bisulphide becomes deep red in half an hour, owing to the liberation of iodine under the influence of diffused daylight. The crystals melt at 43° to a liquid which boils undecomposed at 210°, without any appearance of free iodine vapours. The crystals are exceedingly hygroscopic, attracting moisture with great rapidity, and thereby suffering decomposition. In contact with water itself the decomposition is instantaneous, boric and hydriodic acids being formed, $BI_3 + 3H_2O = H_3BO_3 + 3HI$. When heated in air or oxygen, boron iodide burns readily with a brilliant flame deeply coloured with iodine vapour, clouds of boric anhydride, B_2O_3, being also produced. Melted sulphur attacks it likewise with considerable energy, iodine volatilizing, and a substance being formed which on the addition of water yields a precipitate of sulphur and evolves sulphuretted hydrogen, presumably a sulphide of boron. Phosphorus reacts in the cold with incandescence. With oxychloride of phosphorus, $POCl_3$, a crystalline compound appears to be formed with considerable rise of temperature. Silver fluoride at once invokes incandescence, a violent evolution of gaseous fluoride of boron, BF_3, occurring together with formation of silver iodide. Ethyl alcohol likewise reacts with rise of temperature, a product being obtained which on distillation yields ethyl iodide, the residue consisting of boric acid, $3C_2H_6O + BI_3 = H_3BO_3 + 3C_2H_5I$. Ethyl ether forms with boron iodide a brown liquid, also with considerable evolution of heat, which appears to consist of ethyl iodide and boric ether: $3(C_2H_5)_2O + BI_3 = 3C_2H_5I + B(OC_2H_5)_3$; for on the addition of water to the product, ethyl iodide, boric acid, and alcohol are obtained.

THE additions to the Zoological Society's Gardens during the past week include a Spiny-tailed Mastigure (*Uromastix acinthinurus*) from Algeria, presented by Mrs. W. Williams; two Chipping Squirrels (*Tamias striatus*) from North America, presented by Mr. A. W. Jutson; a Brown Milvago (*Milvago chimango*) from South America, presented by Mr. J. Mand; three Puff Adders (*Vipera arietans*) from the Cape of Good Hope, three Egyptian Cobras (*Naia haje*) from Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Mr. Thos. E. Phillips; a Red-spotted Lizard (*Eremias rubro-punctatus*) from the Pyramids of Dashoor, presented by Dr. Drewitt; two Scorpions from Alexandria, presented by Mr. Sidney R. Carver; three Partridge Bronze-winged Pigeons (*Geophaps scripta*), a Brush Bronze-winged Pigeon (*Phaps elegans*), three Maned Geese (*Bernicla jubata*) from Australia, purchased.

OUR ASTRONOMICAL COLUMN.

THE SOLAR CORONA.—The fourteenth number of the Publications of the Astronomical Society of the Pacific contains several notes on the solar corona. Prof. Charroppin gives the results of an investigation into photographs of the corona, and recommends the following methods in order to obtain greater extension of coronal streamers: (1) to use orthochromatic plates; (2) the greatest precaution to guard from all foreign light; (3) short exposures to obtain the polar filaments and the inner corona; (4) long exposures to secure the extension of the outer corona; (5) photographing each wing separately, and keeping the brighter part of the eclipse out of the field. Prof. Frank H. Bigelow gives a brief summary of the result of a discussion of a photograph of the corona streamers of the eclipse of July 29, 1878, according to the law of equipotential surfaces. He shows that the repulsion of the surfaces of infinitesimally small particles, obeying this law, is all that is required as a fundamental conception, in order to arrive at a physical interpretation of the facts. Lastly, Mr. Schaeberle gives a short account of the principles and results of the mechanical theory of the corona, to which reference has been made in a previous number (NATURE, vol. xlii. p. 68). The full account of the investigation will appear in a report to be issued by the State Legislature. Beginning with the theorems, that (1) the eruptions on the sun's surface are most active and numerous in the sun-spot zones; (2) the sun rotates about an axis passing through its centre; (3) this axis is inclined to the plane of the earth's orbit at an angle of about 82¾°, Mr. Schaeberle derives formulæ which give results showing "that the observed shapes of the equatorial extension or wings of the corona can be satisfactorily accounted for by supposing them to be the envelopes of systems of streamers ejected from the sun-spot zones with initial velocities of less than 380 miles per second (such velocities as have been observed in the higher regions of the prominences, for example)." He then shows on mechanical principles that the curious forms of the "polar rays," which are seen in any corona can be produced by the perspective overlapping of systems of nearly right line streamers originating within the sun-spot zones, and have therefore no objective existence. The *résumé* given by Mr. Schaeberle is a proof that he has thoroughly worked out his theory, and tested its capability of explaining the numerous coronal forms. We therefore look forward with interest to the publication of the Eclipse Report, containing the extended argument in favour of a theory which certainly rivals all others in simplicity.

THE PHOTOGRAPHIC CHART.—A recently published *Bulletin du Comité international permanent pour l'exécution photographique de la Carte du Ciel* is distinguished by several interesting and important articles. M. Kapteyn and M. Gautier contribute notes on the construction and use of the apparatus for the measurement of the photographs, and Dr. Scheiner describes a simple method used at Potsdam for the exact orientation of the telescope. In a paper on the law of photographic diameters of stellar disks, Max Wolf compares the disks of Polaris and neighbouring stars, and the group G.C. 4410, obtained by exposures varying, in geometrical progression, from 1 to 1024 seconds, and deduces the conclusion that the increase in diameter gets smaller and smaller as the exposure increment is augmented. M. Dunér discusses the vexed question of the determination of the photographic magnitudes of stars by means of measures made on the negatives, and propounds the following definition :—"The relation between the light of two stars which differ from each other by a photographic magnitude is expressed by the factor with which the time of exposure of a given plate must be multiplied or divided in order to render the diameter of the image of a star on the new *cliché* equal to the image of another star on the given *cliché*." A paper by M. Prosper Henry, on the value of atmospheric refraction for different portions of the spectrum, has previously been noticed (NATURE, vol. xliii. p. 400).

COMET BARNARD-DENNING (*a* 1891).—Prof. Berberich gives the following elements in *Astronomische Nachrichten*, No. 3027, for the comet discovered by Mr. Barnard, of Lick Observatory, on March 29·695, G.M.T. and by Mr. Denning at Bristol on March 30·417 G.M.T.

Mean epoch = 1891 April 27·730 Berlin mean time.

$$\left.\begin{array}{r} \text{Longitude of perihelion} = 178^\circ\ 14'\!\cdot\!30 \\ \text{Longitude of ascending node} = 194\ \ 13\!\cdot\!14 \\ \text{Inclination} = 120\ \ 30\!\cdot\!52 \end{array}\right\}\ \text{Mean Eq.}$$

Perihelion distance = 0·40652 earth's mean distance.

On the 18th inst. the comet is in R.A. 1h. 42m. 9s., Decl. + 23° 41'·6, and is therefore not well situated for observation, although it is increasing in brightness.

THE PLANET MERCURY.—At the present time the planet Mercury is in a position most favourable for observation, and will continue so until about the 25th of this month. Appearing as an evening star, it will be found near the western horizon just after sunset, and those who have no optical means at their disposal should look out for it at about 8 o'clock on the 19th or 20th of this month, when it will resemble a star of about the first magnitude, and will be a little to the westward of the Pleiades. Although the planet Mars is also situated near this region, the detection of Mercury can easily be made, by reason of its colour, which is of a far whiter hue than that of the first-mentioned planet. During the latter end of the present month and the first week of May the planet will be almost invisible, being lost in the rays of the sun, and its next appearance will take place as it transits the disk of the sun on the 9th of the same month. At Greenwich the transit will only be partial, as the sun will rise at 16h. 19m. (Greenwich mean time), so that only the internal and external contacts at egress can be observed.

For the benefit of those wishing to observe the planet during the present week, the following extract from the *Nautical Almanac* may be useful :—

		Apparent R.A. Noon. h. m. s.		Apparent Declination. Noon. ° ' "
April 16	...	2 50 13·46	...	N. 19 2 59·0
„ 17	...	2 54 42·75	...	19 27 55·9
„ 18	...	2 58 52·66	...	19 50 6·0
„ 19	...	3 2 42·52	...	20 9 29·4
„ 20	...	3 6 11·74	...	20 26 5·4
„ 21	...	3 9 19·79	...	20 39 54·3
„ 22	...	3 12 6·27	...	20 50 56·5

NEW ASTEROID (308).—M. Borelly discovered the 308th asteroid on March 31.

THE WHEAT HARVEST IN RELATION TO WEATHER.

THE general law of wheat production in England was stated in the *Times* of August 30, 1881, as follows : "The yield of wheat is proportional to the summer temperature, with the modifying conditions of rainfall, prevalence of cloud, character of the weather at blossoming time and during the harvest, and the state of growth at the commencement of the summer"; and it was added, "The growing influence of a high or low thermometer is established by the observations of many years." To test the law, superior and inferior harvests may be correlated with their summer temperatures and rainfall. For this purpose the meteorological records of the Royal Observatory, Greenwich, will be used. The mean temperature of June, July, and August, and the total rainfall for these months, will be taken for the summer.

I.—*Superior Wheat Harvests.*

Year.	Character.	Temperature.	Rainfa
		°	inches
1775	Plentiful	62·0	?
1779	Plentiful	62·3	?
1791	Abundant	59·5	Dry
1818	Most abundant	64·3	1·4
1819	Fine	60·3	4·6
1820	Productive	58·0	8·2
1825	Early and good	62·0	3·3
1826	Remarkably early and very great	64·0	5·1
1827	Good	60·0	2·9
1833 (*a*)	Abundant	59·4	6·7
1834 (*b*)	Early, very productive	62·5	11·3
1835	Good	62·6	4·5
1840	Fine yield	59·8	3·9
1849	Above the average	61·0	3·8
1851	Above the average	61·0	7·2
1854	Extremely good	59·0	5·6
1857	Above the average	63·9	6·0
1858	Above the average	62·5	5·7
1863	Abundant	60·3	6·6
1864	Good	59·6	2·5
1868	Productive	64·4	4·1
1874	Very good	60·9	6·4
1888 (*c*)	Above the average	58·4	13·8
	Mean	61 2	5·6

II.—*Inferior Wheat Harvests.*

Year.	Character.	Temperature.	Rainfal
		°	inches
1789	Very deficient	59·7	Wet
1792	Inferior	58·3	Wet
1795	Very defective	57·8	?
1800	Bad	60·7	Wet
1810	Scanty	60·0	?
1811	Very scanty	59·0	?
1812	Very defective	56·0	?
1816	Very great deficiency	55·2	8·4
1817	Deficient	57·4	7·9
1821	Inferior	57·8	7·0
1823	Deficient	57·8	7·1
1828	Bad	60·3	12·0
1829	Inferior	59·0	9·4
1838	Late, unproductive	59·1	7·3
1839	Damaged	59·3	7·6
1848	Very bad	59·5	10·6
1852	Below the average	61·7	11·4
1853	Bad	60·1	11·0
1860	Very deficient	56·7	11·6
1867	Deficient	59·8	10·2
1873 (*d*)	Very deficient	61·7	7·6
1875	Very unsatisfactory	60·3	9·8
1876 (*e*)	Unsatisfactory	62·7	3·7
1877 (*f*)	Unsatisfactory	62·0	6·0
1879	Worst known	58·5	13·3
1880	Deficient	60·6	7·1
1881	Deficient	61·1	7·9
1886 (*g*)	Deficient	61·0	4·1
	Mean	59·4	8·6

(*a*) May was very dry.
(*b*) The winter was very mild ; the spring very dry.
(*c*) The winter and early spring were very cold ; May wa very dry, with much sunshine.
(*d*) Frost occurred at blooming-time.
(*e*) and (*f*) The spring was cold.
(*g*) The winter and early spring were very cold ; May wa very wet.

It is not easy to understand how to correlate the harvests with any specified meteorological datum, for the harvest itself may vary greatly in different counties. But if it is possible to differentiate the meteorological conditions with reference to the harvest, it is quite impracticable to integrate them, or to consider them all together. We shall not, however, be far wrong if we infer from the preceding simple tabulations that: good harvests of wheat accompany hot and dry summers; bad ones, cold and wet. The yield of wheat in England probably depends much more upon the summer dryness than the high temperature. A mean temperature above the average, and small rainfall during the months of June, July, and August imply much clear sky and bright sunshine. A mean temperature below the average for these months implies prevalence of cloud intercepting sunshine, but does not always or necessarily imply large rainfall. Excessive rainfall generally, unless it is due to local thunderstorms, implies overcast weather. Of course, mischief to the growing crop may be of too early date to admit of good yield from even the most favourable summer weather.

The largest wheat harvests have been in those years in which the sun exerted most power, and when, from midsummer until the full ripening, intermittent glowing heat, with fewest interruptions of cloudy weather, or humidity, was experienced. Of the heavy-yielding wheat years, 1854 was a dry summer, 1857 and 1858 had summers of exceptionally long-continued heat. The large wheat crop of 1863 was connected with a fine dry summer; that of 1864 was related to a prolonged drought from July 4 to August 21. The hot summer of 1868 brought a bulky wheat yield. As regards the abundant harvest of 1874, July was much above its average temperature. Good wheat crops resulted from very fine hot summers in 1846, 1847, 1870; and good wheat crops attended the droughty summers of 1885 and 1887. Bad harvests seem rather to depend upon large summer rainfalls than upon low mean temperatures, as in 1828, 1852, and 1853. The years 1886 and 1888 contradict the law, and would seem to point to the effect of the weather in May, which was of opposite character in these two years. Again, the temperature and rainfall indicate good, not bad, harvests for 1876 and 1877. The good harvest year 1851, and the bad one 1873, were on a par meteorologically; and 1849 and 1876 might exchange places, so far as the weather seems concerned. The hottest and driest summer, 1818, had apparently the best harvest; the wettest, 1879, the worst; the coldest, 1812, a very defective one; and 1860, with its cold and wet summer, had a very deficient harvest.

In estimating the influence of the weather upon the resulting crops, the character of the winter and spring ought to be taken into consideration, for, according to Sir J. B. Lawes, "The great influence upon the subsequent growth of wheat of the weather before the period of active above-ground growth, was clearly illustrated in 'Our Climate and our Wheat Crops,' in the case of the season of 1854. The summer of that year was comparatively cold and sunless, yet the wheat crop was one of the best of the present century. The early winter had been unusually cold, but the remainder and the early spring were warmer than the average, and the season was extremely dry from seed-time to harvest, the mild spring and the dryness obviously compensating for the deficiency of temperature during the summer months." The year 1890, like 1854, had high temperature winter and spring; and, according to Sir J. B. Lawes, "The produce of both seasons clearly illustrates the fact that prevailing high temperature during the period of active growth and even of ripening, are not essential for the production of large crops of wheat."

The features of the winter 1890-91 make it the most extraordinary winter of the century in England; its effects, therefore, upon agriculture will be watched with more than passing interest. A writer in *Ciel et Terre* propounds the law that cold winters are followed by cold summers, and thereupon predicts that the summer of 1891 will be cold. Now, low summer temperature is usually attended with rainy weather, so the summer may be wet. The Greenwich observations apparently bear out these deductions, but not without exceptions. For instance, the summer of 1847 was warm and dry, after a very cold winter. However, the probabilities seem in favour of a cold and wet summer. Nevertheless, it should be pointed out that, assuming a cold summer and given a cold preceding winter, it follows that the spring and autumn must be either mild or seasonable, otherwise the year altogether will be remarkably deficient in temperature. Protracted winters were followed by cold wet summers and bad harvests in 1811–12, 1813–14, 1815–16, 1859–60, 1878–79; very cold winters by cold and wet summers in 1816, 1820, 1823, 1830, 1836, 1838, 1841, 1845, 1847. There are not wanting weather-wise people who predict that 1891 will be a dry year, on the theory of the sapient meteorologist, taken *per contra*, that rainy weather prolongs itself, that the more rainy weather you have the more you may expect. They argue, 1889 and 1890 were exceptionally dry years, so 1891 may be even drier. Last spring, up to the end of May was curiously rainless, and, from August onward, every month has shown less than the average quantity of rain. December and January had already parched the ground; February made it moistureless. Some rain came on March 7, but the fall for the month was far below the average in all parts of the British Isles except North Scotland. Want of moisture would gravely affect the prospects of the harvest.

In conclusion, one or two inferences remain to be drawn from the foregoing tabulation. Between the mean summer temperature of the superior harvest years and that of the inferior there is only a difference of 1°·8 in favour of the former; but this means so much more heat daily over 92 days. The mean rainfall for the summers of inferior harvest years exceeds that of the superior by 2·9 inches, which means that the wet summers had half as much more rain than the dry ones. Hence, it would seem that rainy summers rule the harvests much more potently than the mean temperatures. This influence seems conformable to the well-known character of rainy summers, in England, as regards sunshine, for they are wofully deficient in that vital element in the growth and maturity of the crops. The wheat yield in England follows the summer rainfall inversely. Good wheat years are those of hot dry summers. Bad wheat years are those of very wet sunless summers.

A JOURNEY IN SOUTH-WEST CHINA.

At the last meeting of the Royal Geographical Society the paper read was on "Two Journeys to Ta-tsien-lu on the Eastern borders of Tibet," by Mr. A. E. Pratt, whose main object was the collection of natural history specimens. Ta-tsien-lu is a mountain village about 8400 feet above the level of the sea, in the province of Sz-chuen in West China—five days' journey from the borders of Tibet, and ten days' journey south-west from the Roman Catholic missionary station of Mou-pin, where Père David lived for some years, and whence he sent to Europe the valuable collections of mammals and birds which have made his name famous throughout the world. In the year 1889 Mr. Pratt spent three months in this district with Mr. Kricheldorff, making collections in natural history, and again in 1890 about the same time. The first stage of the journey to this remote district was from Shanghai to I-chang. The river Yang-tze is navigated for this distance of 1200 miles by steamers built especially in Britain for the river service, and commanded by English or American captains. Passengers change steamers at Hankow, and the whole journey occupies from ten days to three weeks, according to the state of the river and the time lost in waiting at Hankow for the next boat.

The journey from I-chang to Chung-king is generally made in Chinese house-boats, but Mr. Pratt had a boat specially built. At Chung-king the river (1600 miles from Shanghai), at high water, is considerably over a mile in width. This is a great opium-growing district. At Sui-fu, the great centre of trade for Yunnan, Mr. Pratt left the Yang-tze and entered the Min, one of its largest tributaries. The great industry of this thickly populated district on the banks of the Min is the manufacture of salt. On May 14 the party anchored for the night at a place some fifteen miles below Kia-ting-fu, reaching the city in the course of the afternoon. They left Kia-ting-fu on May 19. Their way lay through a really lovely country, beautifully watered by innumerable streams, reminding Mr. Pratt very much of Hampshire. Here, for the first time, he saw that beautiful orchid, *Dendrobium nobile*, growing wild—a mass of pink bloom. Eight hours' travelling brought them to the town of Omei-hsien, seven miles from Mount Omei, the celebrated sacred hill so well described by M. Colborne Baber. They left Omei-hsien on the 21st, and on their way met many coolies carrying the eggs of the celebrated wax insect down from the mountains. These eggs hatch out if exposed to the sun, so they are generally carried by night. The method of production of this wax has been fully described by Mr. Hosie.

After travelling through a very wild region, the party reached an elevation of 5000 feet, where Mr. Pratt gathered a lovely fragrant honeysuckle and a fine mauve-coloured primula, and saw some feathers of the famous Amherst pheasant. On May 24 they struck the main stream of the Tung river, which appears to divide the territory of the independent Lolos from that of the portion of this interesting people subject to the Chinese. Passing by the side of a range of mountains the party followed an affluent of the Tung river, and on May 26, thirty-two days after leaving Chung-king, reached Ta-tsien-chih, a long straggling village of detached clusters of houses. It stands at an elevation of 5980 feet above the sea. The mountain ends in a series of fourteen precipices, each some 200 feet high, the highest being only accessible by ladders. The climate is very much like that of England—cold, rainy, and changeable; the roses very pretty, but single, and strawberries were plentiful; and there is good shooting—wild ox, two species of antelope, two species of bear, and five of pheasant.

The party went on to Ta-chien-lu, the road leading over a pass some 10,000 feet in height. Ta-tsien-lu is a most interesting town. All sorts of Asiatics may be met in its streets, and Europeans, therefore, attract less attention here than in other places where strangers are seldom seen. The natives of the place are the wildest-looking people, invariably armed to the teeth; some of fine physique, tall and handsome, with long matted hair hanging over their faces.

The following year, 1890, Mr. Pratt made a second expedition to Ta-tsien-lu, to increase his collections. This time he carried out the intention he had formed on his previous journey, of ascending Mount Omei. This mountain is 11,100 feet high, and is regarded throughout the neighbouring countries as a spot of peculiar sanctity. There are between sixty and eighty temples on it, and about two thousand priests, and it is continually visited by many thousands of pilgrims. The mountain rises abruptly like a promontory, and can only be ascended from one side. The others are extremely steep, one of them being a precipice nearly a mile and a third high, the highest sheer declivity, perhaps, in the world. As the party approached the mountain, they passed many fine trees, of the species allied to the banyan. One particularly fine specimen, with a magnificent spread of foliage, Mr. Pratt measured, and found it to be 30 feet in circumference. The path led them at first through a wide fertile valley of rice fields, with clumps of trees scattered here and there as in a park. The mountain is covered from head to foot with undergrowth and forest, pines, hollies, and other evergreens predominating. Flowers were very abundant, wild roses, anemones, asters, yellow violets, and two species of hydrangea. Here Mr. Pratt noticed *Paxia begonia*, which he believes has no representative in Europe, but which he believes is represented in America. Near the top he found a primula and a dwarf azalea with fragrant foliage, the latter, so far as he knows, a unique specimen.

During this visit, Mr. Pratt more than once witnessed the curious phenomenon known as the glory of Buddha. Standing on the edge of the precipice, and looking down into the sea of mist which generally fills the valley below, he saw, about 150 feet beneath him, the golden disk surrounded by rainbow-coloured rings of light, which is the chief marvel of Mount Omei, and the clearest evidence of its sanctity. Every year many pilgrims commit suicide by throwing themselves down from this cliff. On May 1, accompanied by Father Soulié, Mr. Pratt made an excursion from Ta-tsien-lu to the snow-capped mountains, and pitched his tent in a forest of rhododendrons just coming into bloom, about two hours below the region of perpetual snow.

By way of summary of the vegetation, Mr. Pratt divides the country here briefly into four regions or zones:—(1) Above 16,000 feet we have perpetual snow. (2) Between 16,000 feet and 10,000 feet, rhododendrons, anemones, primulas, rhubarb, many lilies, a few asters, grass, and wild onions; of birds, *Crossoptilon thibetanum*, *Lophophorus lhuysii*, and Père David's small blue bird. (3) From 10,000 to 5000 feet—rhododendrons, coniferous trees, gooseberries, several species of currant (including one very large black currant with bunches of fruit a foot in length), undergrowth, and several species of birds. (4) Below 5000 feet there is cultivation on a few farms, and pasturage.

M. GRZIMAILO'S EXPEDITION.

ON March 25, the Russian Geographical Society held an extraordinary meeting to listen to a communication by G. E. Grum-Grzimailo about his expedition to Central Asia. The expedition consisted of M. Grum-Grzimailo, his brother, a collector, an interpreter, six Cossacks, and two men. The luggage was transported on some fifty horses and donkeys. After having crossed the Russian frontier on June 8, 1889, they soon reached Kulja, and thence went north-east, towards the spurs of the Boro-Khoro Mountains. By the way they visited Central Djungaria, in order to obtain specimens of the wild horse discovered by Przevalsky, and described as *Equus przevalski* by the late Polyakoff from one single specimen brought in by the great traveller. Four specimens more were obtained. Returning from Djungaria, the expedition proceeded, in September, to the Eastern Tian-Shan, and completed the exploration of its remotest eastern parts. The well-known oasis of Turfan proved to be a desert which has been recovered for industry only by the hardest imaginable labour. It has no water, notwithstanding the proximity of the snow-mountains of Bogdo; and its inhabitants have dug out a whole system of underground canals and wells (some of which are 300 feet deep) to irrigate the desert. The canals collect the water underground, and then bring it to the surface in the lower grounds. The whole work is so colossal that the members of the expedition compare it with the colossal works of Egypt. As to the absolute height of the oasis, M. Grum-Grzimailo pointed out that parts of it appear to be *below the level of the sea*. Of course, this conclusion of the Russian traveller, being based upon barometrical measurements only, cannot yet be taken as quite certain; but it shows that the oasis of Turfan is extremely low, and that it in no case rises more than from 200 to 300 feet. It thus must represent the bottom of a great lake, which occupied on the border of the Central Asian plateau the same position as Lake Baikal occupies now; and this quite unexpected fact is one of great importance for the physical geography and geology of the whole region.

In February 1890 the expedition reached Hami, and thence proceeded to Mor-gol. Heavy snowfalls, however, prevented their further advance eastwards; so they turned towards the south, and went to the Nian-Shan ridge, which had already been crossed in three different places by Przevalsky and Potanin. MM. Grum-Grzimailo studied that interesting ridge over a length of 300 miles, and crossed it in a picturesque gorge which brought them to the Babo-ho river, and thence to the Chinese town Yunan-tcheu. After having explored the Alps of Si-nin, they reached the Hoang-ho, and thence began their return journey. The snowstorms rendered travelling difficult, so they rested for a while at Su-tcheu, and thence, crossing the Be-tchan Mountains, went to Gu-tchen, thence to Urumtchi, and finally reached the Russian frontier on November 25, 1890. Survey has been made over a length of 4840 miles, of which 4000 miles were previously untrodden ground; latitudes and longitudes were often determined during the journey; so also were altitudes. Nearly 200 photographs were taken, and the natural history collections are sure to be very interesting.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xiii., Nos. 2 and 3.—In No. 2 is concluded Part I. of a lengthy article by O. Bolga on the theory of substitution-groups and its application to algebraical equations; the final section discusses groups of operations, especially those obtained from the "groups" of rotations of a regular polyhedron which leave it congruent with its first position. Part II. deals with Galois' theory of algebraic equations."—The following papers also appear:—"Quelques propriétés des nombres K_m^p," by M. M. d'Ocagne. These numbers have been discussed in a previous article (1887, p. 353), where they were defined by means of a triangle analogous to Pascal's.—"Sur les lois de forces centrales faisant décrire à leur point d'application une conique quelles que soient les conditions initiales," by P. Appell.—On certain identities in the theory of matrices, by H. Taber.—Systems of rays normal to a surface, by W. C. L. Gorton.—On the epicycloid, by F. Morley. Some interesting results of

Wolstenholme's and others are here obtained by the use of circular co-ordinates.—The reduction of

$$dx/\sqrt{A(1 + mx^2)(1 + nx^2)} \text{ to } Mdy/\sqrt{(1 - y^2)(1 - k^2y^2)}$$

by the substitution $x^2 = a + by^2/a' + b'y^2$, by H. P. Manning. A table of available forms is added, and attention drawn to those forms in it given by Cayley ("Elliptic Functions," p. 316).—A simple statement of proof of reciprocal theorem, by J. C. Field.—Related expressions for Bernouilli's and Euler's numbers, by J. C. Field.——In No. 2 appears a third memoir, on a new theory of symmetric functions, by Major P. A. MacMahon, R.A. Attention is drawn to a fundamental theorem in operations, given without proof. It is a generalization of a theorem by Sylvester which is itself a generalization of Taylor's theorem; "it enables us from any linear function P of the operators to determine another linear function Q, such that exp. P = $\overline{\text{exp}}$. Q," the bar in $\overline{\text{exp}}$. u being used by the author to indicate that the multiplication of operators that occur in u is symbolic.—M. Joseph Perrott also contributes a paper entitled "Remarque au sujet du théorème d'Euclide sur l'infinité du nombre des nombres premiers."

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, April 9.—"The Measurement of the Power supplied by any Electric Current to any Circuit." By Prof. W. E. Ayrton, F.R.S., and W. E. Sumpner, D.Sc.

I.—During the meeting of the Electrical Congress in Paris in 1881, one of us[1] devised a method of using an electrometer for measuring the power given to any circuit by any current. This method is the only electrical one hitherto published, the accuracy of which does not depend on assumptions either as regards the character of the current variations or as regards the nature of the circuit the power given to some part of which we desire to measure.

In view then of the present wide use of alternating currents for industrial purposes, it might have been expected that this electrometer method of measuring the power given by any intermittent or alternating current to an inductive circuit would have been extensively employed. Unfortunately, however, as pointed out by one of us in conjunction with Prof. Perry (Journal of Soc. of Tel. Engs. and Elects., vol. xvii., 1888), the use of this method is restricted by the fact that Sir W. Thomson's quadrant electrometers do not generally obey the mathematical law given for these instruments in text-books,[1] as it was supposed they did when this electrometer method of measuring power was first suggested. And hence the main result that has, up to the present time, followed from the publication of this method has been the stimulation of inventive minds to devise forms of electrometers in which the text-book law is strictly fulfilled.

In 1888, Mr. Blakesley published a very ingenious method for measuring the power supplied by alternating currents to the primary coil of a transformer, by the use of three dynamometers. The proof originally given was geometrical, and was based on several assumptions, amongst others that the currents and magnetic fluxes varied with the time according to a simple sine law. An analytical proof has recently been given (meeting of Physical Society, February 27, 1891) by one of us, in conjunction with Mr. Taylor, showing that the method gives equally good results however the currents and magnetic fluxes vary, but there still remains a serious objection to the method, as it is assumed that there is no magnetic leakage in the transformer, or, in other words, every line of force is supposed to thread each convolution of both primary and secondary coils. Further, the method cannot be used with a single circuit as the coils of one of the dynamometers must be placed in different circuits.

The employment of an electro-magnetic wattmeter for the measurement of power is well known, but errors are introduced when alternating currents are used, owing to the self-induction of the fine wire coil. Several investigators have considered the magnitude of this error, and have suggested various devices for reducing it to a minimum.

II.—Several months ago, however, while working at alternate-current interference, we noticed that it was possible to employ an extremely simple method for measuring the power supplied by *any* current to *any* circuit. This method, which has since been in regular use in the laboratories of the Central Institution, is quite independent of any assumptions as to the nature of the current, or of the circuit, the power given to which it is desired to measure, and it has the further great advantage that the only measuring instrument required is the ordinary alternate-current voltmeter of commerce.

In series with the circuit *ab* (Fig. 1), the power given to which we desire to measure, connect a non-inductive resistance *bc* of *r* ohms.

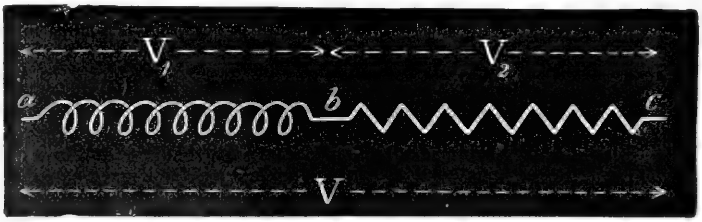


FIG. 1.

Let V_1, V_2, and V be the readings of the voltmeter when applied between a and b, b and c, and a and c respectively; then, if W be the mean watts supplied to the circuit ab, we have in all cases, whatever the nature of the current, or of the circuit ab—

$$W = \frac{1}{2r}(V^2 - V_1^2 - V_2^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

For, let v_1, v_2, and v be the instantaneous values of the P.D. between a and b, b and c, and a and c at some moment t, then

$$v = v_1 + v_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If a be the current in amperes flowing through the circuit at time t, then av_1 equals the watts w given to ab at that time. But

$$a = \frac{v_2}{r},$$

since the resistance bc is non-inductive;

$$\therefore w = \frac{v_1 v_2}{r}.$$

Then, squaring (2), we have—

$$v^2 = v_1 + 2v_1v_2 + v_2^2;$$

$$\therefore w = \frac{1}{2r}(v^2 - v_1^2 - v_2^2).$$

As this equation is true at every moment, it must also be true for the mean values of w, v^2, v_1^2, and v_2^2.

So that

$$\int_0^T w dt = \frac{1}{2r}\left(\int_0^T v^2 dt - \int_0^T v_1^2 dt - \int_0^T v_2^2 dt\right),$$

and

$$W = \frac{1}{2r}(V^2 - V_1^2 - V_2^2),$$

which is the equation given above.

If the resistance of bc be not known, or if there be any fear that it may be changed by the passage of the current, then an

[1] This method was simultaneously arrived at independently by Prof. Fitzgerald.

[1] We may mention that an investigation on quadrant electrometers has been going on from time to time at the Central Institution for the last five years, and we had hoped to have communicated the complete report long before this to the Royal Society.

ammeter (an alternate-current ammeter, of course, if alternate currents be employed) can be inserted in the circuit. Let the reading of this ammeter, which represents the square root of the mean square of the current be A, then, for r in (1) we may substitute V_2/A, or

$$W = \frac{A}{2V_2}(V^2 - V_1^2 - V_2^2) \quad . \quad . \quad . \quad . \quad . \quad (3)$$

When employing this last formula, the non-inductive resistance bc may be that offered by incandescent lamps, since there is no objection to the resistance varying with different mean strengths of the current employed.

This voltmeter method of measuring power was arrived at quite independently of the electrometer method referred to above, but an examination of the electrometer method shows that it is practically equivalent to simultaneous measurements of three P.Ds.

An analysis of the equation (1) shows that the value of the non-inductive resistance r, which it is best to adopt in order to reduce to a minimum the error in W arising from errors in the voltmeter readings, is such that the potentials V_1 and V_2 are equal to each other. It can also be shown that the percentage error in estimating the power W due to errors in the voltmeter measurements arising either from faulty graduation of the scale, or from inaccurate readings, is from four to five times the percentage error of a single reading of the voltmeter.

As all instruments that are graduated for measuring the square root of the mean square of an alternating P.D., such as a hot-wire voltmeter, an electrostatic voltmeter, &c., really measure the mean square and not the square root of the mean square directly, it would be better, if such an instrument were to be employed for the method of measuring power described in this paper, that it should be graduated in mean squares of P.Ds. and not in the square roots of the mean squares. In that case the probable percentage error in the measurement of power by the method would be from 2 to 2·5 times the error in the measurement of each of the P.Ds.

It is, of course, clear that these errors to which we have been referring are not errors in any way essential to the method proposed for measuring power, since by the employment of an accurately graduated voltmeter, by exercising care in taking the readings, and, if necessary, by repeating the measurements two or three times and taking the means of the observations, the power can be measured to any degree of accuracy desired.

As in practical cases the sum of the two potentials V_1 and V_2, will not often be much in excess of V, we may conveniently express the true power W in the following way.

If A is the current in ab (Fig. 1) as read by an alternating current ammeter, the apparent power absorbed by ab is

$$V_1A.$$

The true power W, when V_2 is made equal to V_1, can be shown to be

$$W = \left(1 - 2y + \frac{y^2}{2}\right)V_1A,$$

where

$$y = \frac{V_1 + V_2 - V}{V_1},$$

or as y is usually a small number,

$$W = (1 - 2y)V_1A.$$

Thus the error made in assuming that V_1A represents the true power is 8 per cent. if $(V_1 + V_2 - V)$ is 4 per cent. of V_1, also if, due to unsteadiness of the currents, or to error in the voltmeter readings, the value of $(V_1 + V_2 - V)$ is uncertain to the extent of 1 per cent. of V_1, the uncertainty in estimating W is twice this, or 2 per cent.

III.—The method we have just described is well adapted for measuring the power supplied to an alternating current arc lamp, and is, moreover, likely to be of service in investigating the phenomena of the alternating current arc. Three of our senior students, Messrs. Kolkhorst, Thornton, and Weekes, have made a number of experiments on arc lamps by the use of this method, and from their experiments it would appear that the quality of the carbon employed affects materially the difference in phase between the currents passing through the arc and the P.D. between the carbons. If the arc be quite steady and only give out the rhythmic hum that accompanies a well-formed arc, such as can be obtained with cored carbons of good quality, the arc appears to act practically as a simple resistance, but if the arc be maintained between uncored carbons of poor quality, and be hissing, there is considerable difference in phase between the current and the P.D. between the terminals; further, the experiments show that the current is very far from being a sine function of the time, although produced by a dynamo whose E.M.F. normally follows a harmonic law.

In addition to the difference of phase of P.D. and current that may be produced in the arc itself, there is the electro-magnet to be considered, by which the distance between the carbons is usually regulated in arc lamps. This electro-magnet will introduce lag between the P.D. at the terminals of the lamp and the current passing through the electro-magnet and the arc in series; and hence, even although the arc be perfectly steady, we find, even in the case of a Brush lamp especially intended for alternate currents, that the true power supplied to the electro-magnet and arc is 20 per cent. less than the product of the readings of the ammeter and the voltmeter attached to the lamp terminals, and which gives the square root of the mean product of the squares of the current and P.D.

If, however, the arc be between common carbons and be hissing, the difference, we find, is much greater. With cored carbons this Brush lamp requires a P.D. of about 35 volts to be maintained between its terminals, but if these cored carbons be replaced by common carbons and the arc be hissing, the P.D. between the terminals of the lamp at once rises to 45 or even 50 volts, although the current passing through the lamp and the amount of light given out remain practically as before. And then we find that the true power supplied to the lamp may be only one-half of the square root of the mean product of the squares of the current and P.D., so that the readings of the ammeter and voltmeter alone make the apparent power twice as great as the true power.

For the purpose of easily estimating the ratio of the true to the apparent power supplied, formula (3) may be thus written—

$$W = AV_1\left\{1 - \frac{(V_1 + V_2 - V)(V_1 + V_2 + V)}{2V_1V_2}\right\} \quad . \quad . \quad (4)$$

from which we see that the expression in the brackets represents the ratio of the true to the apparent power supplied to the lamp or other circuit ab (Fig. 1). Hence the percentage error made in assuming that the power supplied to any circuit was the product of the ammeter and voltmeter readings would be in all cases, whatever the nature of the current or of the circuit,

$$100\frac{(V_1 + V_2 - V)(V_1 + V_2 + V)}{2V_1V_2} \quad . \quad . \quad . \quad . \quad (5)$$

The following are samples of the results obtained with a hand-regulated lamp, there being no electro-magnet at all in series with

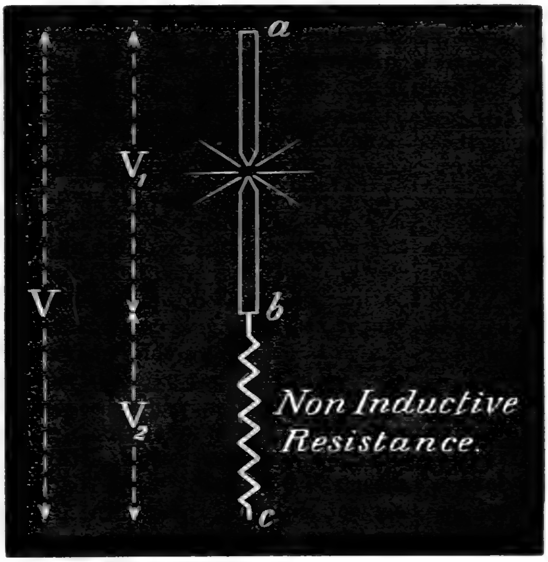


FIG. 2.

the arc (Fig. 2). The carbons were not cored, and the arc was hissing. The frequency was maintained at 200 periods per second.

Table I.

Square root of mean square				
Of P.D. in volts between			Of current in amperes.	Percentage error in estimating power formula (5).
a and *b*. V_1.	*b* and *c*. V_2.	*a* and *c*. V.	A.	
55·0	60·0	108·0	12·3	24·0
45·4	75·4	107·3	11 8	45·8

For the purpose of obtaining an idea of ϕ, the angle of phase difference produced by the hissing arc, between the current and the P.D., we may assume that the P.D. and current are sine functions of the time; then, as may be easily proved,

$$\cos \phi = \frac{V^2 - V_1^2 - V_2^2}{2V_1V_2} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and the values of ϕ for the two tests given above come out as 40° 20′ and 57° 10′. It will, of course, be observed that this assumption of a harmonic law for the P.D. and current for the purpose of obtaining some idea of the value of ϕ, in no way affects the generality of the method for the measurement of the power, since this is based on no such assumption.

The following are samples of the results obtained with a Brush alternate-current lamp regulated by an electro-magnet (Fig. 3),

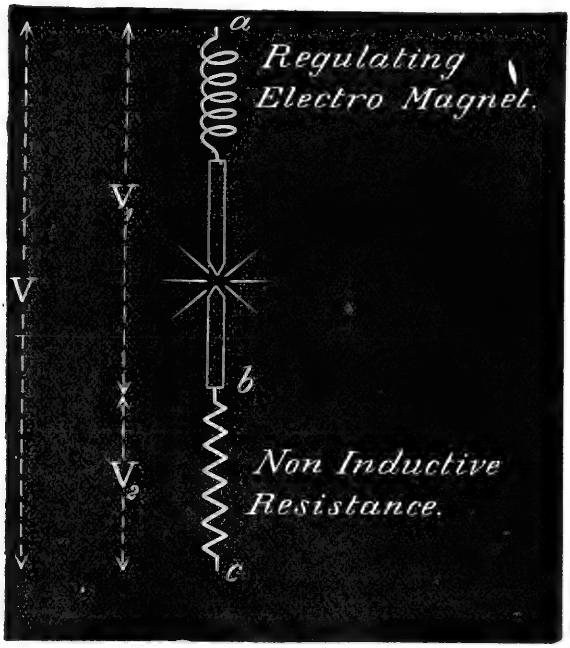


Fig. 3.

the carbons not being cored, and the arc hissing. The frequency was maintained at 200 periods per second.

Table II.

Square root of the mean square				Percentage error in estimating power formula (5).	Lag between current and P.D.
Of P.D. in volts between			Of current in amperes.		
a and *b* V_1.	*b* and *c*. V_2.	*a* and *c*. V.			ϕ.
64·8	58·0	108·4	13·0	44·0	56° 0′
59·8	64·2	107·4	12·0	50·5	60 20
55·0	67·3	107·4	10·6	47·0	58 30

The experiments already described tell us that a hissing arc may cause a considerable phase difference between the P.D. and the current, but they do not enable us to decide whether such an arc causes the current to lag behind the P.D., or to lead in front of it. To decide this point—that is, to decide whether a hissing arc acts like an inductive coil, or a condenser—a variety of experiments were made by putting induction or capacity in series with the arc. The following gives the result of one such experiment:—In series with a hand-regulated lamp (and, therefore, containing no electro-magnet), was placed a condenser of 89 microfarads (Fig. 4). Uncored carbons were used, and they were adjusted so that the arc was very short at first; the carbons were then not touched, and, as they burnt away, the arc grew longer and longer until it finally went out. The frequency was maintained at 200 periods per second.

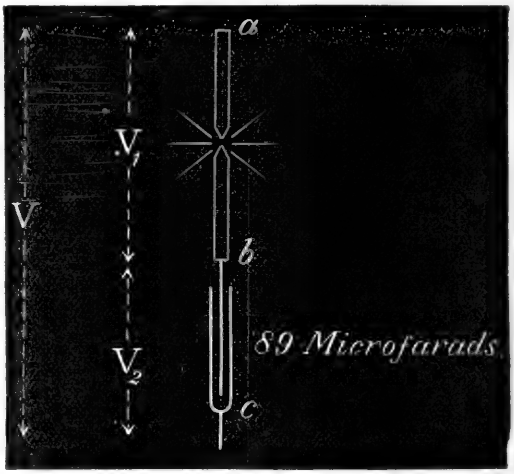


Fig. 4.

Table III.

E.M.F. of dynamo in volts.	Square root of mean square				Sum of $V_1 + V_2$.	Lag between current and P.D.
	Of P.D. in volts between			Of current in amperes.		
	a and *b*. V_1.	*b* and *c*. V_2.	*a* and *c*. V.	A.		ϕ.
59	35·4	89·0	72·3	12·0	124·4	129°
	38·0	92·0	73·3	12·5	130·0	133
	51·2	104 5	74·3	14·0	155·7	135
	69·2	86·5	67·5	13·4	155·7	131

Comparing V with the E.M.F. of the dynamo, we see that the arc and the condenser together acted as a condenser on the whole; but, comparing V with $V_1 + V_2$, we see that the arc acted as an induction and not as a capacity.

Calculations from the measurements made on the assumptions that the arc acts like an induction coil, and that the current follows a simple sine law, show that the inductance of the arc itself is as great as that of the regulating electro-magnet used in the lamp. When, however, the inductances of the arc and electro-magnet in series is observed, it is found to be less than the sum of the two inductances. This shows conclusively that the current does not vary according to a simple sine law.]

Linnean Society, April 2.—Prof. Stewart, President, in the chair.—The Rev. Prof. Henslow exhibited specimens of *Oxalis cernua*, Thunberg, a native of the Cape of Good Hope, and gave an interesting account of its introduction into the countries bordering the Mediterranean and the Canaries and Madeira, tracing its present northern distribution, so far as he had been able to ascertain it. A discussion followed, in which Messrs. A. W. Bennett C. B. Clarke, W. Bateson, and B. D.

Jackson took part.—Mr. A. B. Rendle, having examined the specimens of "Monchona" exhibited by Mr. Christy at a previous meeting, expressed the opinion that this trade product was the preserved fruit of a palm, belonging to a species apparently undescribed. It was stated somewhat vaguely by the importer to have come from the South Pacific.—Mr. Rendle also exhibited another specimen of an orange within an orange, which differed from that shown at a former meeting in that the inner orange possessed a rind, and was not entirely enveloped by the outer one.—The President exhibited an abnormal specimen of a butterfly (*Gonepteryx rhamni*) possessing five wings, or two hinder wings on one side.—Mr. W. Bateson then gave the substance of a paper by himself and Miss A. Bateson on variations in floral symmetry of certain plants with irregular corollas. He described the variations in number of parts and of symmetry occurring in the flowers of *Gladiolus*, *Veronica*, *Linaria*, and *Streptocarpus*, and showed that, although in these varieties there is considerable departure from the normal form, yet the resulting variety is often as definite as the normal form, and not less perfect in symmetry. It was suggested that the variations by which specific forms of symmetry are produced may also be thus distinct, and not of necessity involving transitional forms; and, for example, that the process by which the 4-petalled symmetry of *Veronica* arose from that of a 5-petalled ancestor was perhaps similar in kind to that by which the 3-petalled variety of *Veronica* is formed from the type, transitional forms being in such cases rare, or even absent. An interesting discussion followed, in which the President, Prof. Henslow, Messrs. C. B. Clarke, and A. W. Bennett took part.—The Secretary then read a paper by Mr. H. N. Ridley, of Singapore, on two new genera of orchids from the East Indies.

Zoological Society, April 7.—F. Du Cane Godman, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of March; and called special attention to a young example of the Ounce or Snow-Leopard (*Felis uncia*), new to the collection, and to a Small-clawed Otter (*Lutra leptonyx*) from India, being the second specimen of this Otter acquired by the Society; also to a specimen of a Lhuys' Impeyan (*Lophophorus lhuysi*) from Szechuen, Western China, obtained by Mr. A. G. Pratt, during his recent visit to that country, being the first example of the species that has reached Europe.—The Secretary exhibited the drawing of a female Antelope (*Tragelaphus gratus*), with a young one, now living in the Zoological Garden, Amsterdam, which had been obligingly sent to him by Heer C. Kerbert, the Director of that Garden.—The Secretary exhibited (on behalf of Mr. W. L. Sclater, Deputy Superintendent of the Indian Museum, Calcutta) a specimen of a Duck, apparently a hybrid between the Mallard (*Anas boschas*) and the Gadwall (*A. strepera*), which had been lately obtained in the vicinity of Calcutta.—Mr. T. D. A. Cockerell read a paper on the geographical distribution of Slugs. The author divided the known Slugs into six families: Succineidæ, Vaginulidæ, Arionidæ, Limacidæ, Testacellidæ, and Selenitidæ, under which he grouped fifteen sub-families. The Janellidæ were reduced to a sub-family of Succineidæ, and the generic nomenclature of the whole group was revised, two new genera and one new sub-genus being named. The Philomycidæ were made a sub-family of the Arionidæ. The distribution of each sub-family, and of all recognizable genera, was discussed in some detail. Under the *Veronicellinæ* a new sub-genus *Imerinia*, from Madagascar, was indicated.—A communication was read from Dr. Alcock, Surgeon-Naturalist to H.M. Indian Survey steamer *Investigator*, containing a description of *Saccogaster maculatus*, a viviparous Bathybial Fish from the Bay of Bengal.—Prof. F. Jeffrey Bell read some observations on *Bathybiaster vexillifer*, a Star-fish originally described by Sir Wyville-Thomson, of which the typical specimen had lately been received by the British Museum.—Mr. G. A. Boulenger gave an account of the Siluroid fishes obtained by Dr. H. von Ihering and Herr Sebastian Wolff in the Province of Rio Grande do Sul, Brazil.—Mr. F. E. Beddard read a paper giving some account of the anatomy of the Patagonian Cavy (*Dolichotis patagonica*) from specimens recently living in the Society's Gardens.

Mathematical Society, April 9.—Major MacMahon, R.A., F.R.S., Vice-President, in the chair.—Major MacMahon (Mr. J. J. Walker, F.R.S., taking the chair *pro tem.*) read a paper on the analytical forms called trees, with applications to the combinations of certain electrical quantities and to the compositions of multipartite numbers. A discussion followed, in which Mr. Kempe, F.R.S., Mr. S. Roberts, F.R.S., and Mr. J. Hammond took part.—Mr. Kempe spoke on the flaw in his proof of the map-colour theorem which had been pointed out by Mr. P. J. Heawood (see Appendix to vol. xxi. of the Society's Proceedings), and stated that he was still unable to solve the problem fully.—Mr. Tucker (Hon. Sec.) communicated a paper by Mr. Culverwell on compounded solutions in the calculus of variations.

EDINBURGH.

Royal Society, April 6.—The Hon. Lord Maclaren, Vice-President, in the chair.—Sir Douglas Maclagan read an obituary notice, by the Right Hon. Lord Moncrieff, of Prof. Campbell Swinton.—Prof. Crum Brown read a paper, written by himself and Dr. James Walker, on the synthesis of dibasic acids by means of electrolysis. In a previous paper on this subject the authors described the behaviour of the ethyl-potassium salts of normal dibasic acids on electrolysis. These they found to yield always the diethyl esters of normal acids of the same series. In the present paper, extending their investigation to acids with side chains, they give an account of the alkyl derivatives of succinic acid as obtained by the electrolysis of ethyl-potassium methyl-malonate and ethyl-potassium ethyl-malonate. The esters formed according to the general equation,

$$2EtO(CO) . R'' . (CO)O = EtO(CO) . R'' . R'' . (CO) . OEt + 2CO_2,$$

are evidently always symmetrical, so that from methyl-malonic acid one might expect to obtain symmetrical dimethyl-succinic acid,

$$[2EtO(CO) . CH(CH_3) . (CO)O = EtO(CO) . CH(CH_3) . CH(CH_3) . (CO)OEt + 2CO_2].$$

This dimethyl-succinic acid contains two similarly situated asymmetrical carbon atoms, and is thus, like tartaric acid, capable of existence in four isomeric forms—two optically active, and two optically inactive, one of these latter (corresponding to racemic acid) being a compound or mixture in equal proportions of the two opposite optically active acids. As the optically active forms are produced in equal proportions by any purely chemical process from inactive materials, the authors were justified in expecting to obtain, by electrolysis, a mixture of the esters of the two inactive symmetrical dimethyl-succinic acids. The synthesis was conducted in precisely the same manner as in the previous experiments. The authors succeeded in separating and purifying two acids—one, the less soluble, having the melting-point 193° C., the other melting at 120°–121° C. These acids on analysis proved to have the same composition—both corresponding to the formula $C_6H_{10}O_4$—the acid melting at 193° C. being apparently identical with the para-s-dimethyl-succinic acid of Bischoff (melting-point 194° C.), while the other seems to be identical with his anti-s-dimethyl-succinic acid. This conclusion was further confirmed by measurements of the electrolytic conductivity of solutions of the acids. In a similar manner the authors performed the electrolysis of ethyl-potassium ethyl-malonate. As before, they obtained, in the pure state, two acids, one melting at 192° C. with decomposition, the other at 130° C. Analysis showed that these acids have the composition of diethyl-succinic acid, and, from their mode of formation, are symmetrical. These are evidently identical with the para-s-diethyl-succinic acid (melting-point 192° C., with decomposition), and the anti-s-diethyl-succinic acid (melting-point 129° C.) of Bischoff and Hjelt. This conclusion also was further confirmed by measurements of electrolytic conductivity.—Prof. Tait read a paper on the Virial, with special reference to the isothermals of carbonic acid. He showed that the usual mode of writing the equation, with $p(v - \beta)$ as the left-hand member (where the term βp is part of the virial), is incorrect except in the absence of molecular force. The true form of the (approximate) virial equation is

$$p = \bar{p}\left(1 - \frac{(v - \bar{v})^3}{v(v + \alpha)(v + \gamma)}\right) + \left(R + \frac{e}{v + \alpha}\right)\frac{t - t}{v},$$

where $\bar{p}$, $\bar{v}$, t belong to the critical point. A first rough estimate, based on Amagat's recent work, gives for carbonic acid the values $\bar{p} = 72{\cdot}6$ atm., $\bar{v} = 0{\cdot}0046$. Andrews long ago carefully determined $t = 30°{\cdot}9$ C. The approximate values of the other constants are $a = 0{\cdot}001$, $\beta = 0{\cdot}0008$, $R = 0{\cdot}00371$, and $\epsilon = 0{\cdot}000021$. With these, the above formula gives fair representations of the isothermals from 0° C. to 100° C., for ranges of pressure from 1 to 500 atm. These are, however, to be regarded as provisional values only. Further numerical work is required to determine them more exactly. The formula above is based on the assumptions (1) that the particles are hard spheres, and (2) that the absolute temperature is measured by the average energy of a *free* particle; and its agreement with experiment is regarded as strong evidence in favour of the truth of the second of these assumptions; which, in its turn, throws strong light upon the nature of the liquid, and even of the solid, state.

PARIS.

Academy of Sciences, April 6.—M. Duchartre in the chair.—On a system of equations from partial derivatives, by M. Émile Picard.—Transformation *in vitro* of lymphatic cells into *clasmatocytes*, by M. L. Ranvier. It is shown that the lymphatic cells of the frog may be transformed into ramified, immobile cells—that is, into *clasmatocytes*—by making a preparation of the peritoneal lymph and keeping it in a glass cell at a temperature of 25° C. for one hour.—On vaccination by minimum doses of vaccinating matter, by M. Ch. Bouchard. The results of numerous experiments indicate that vaccinating matters act efficaciously when the amount employed is only a small fraction of a milligram. In one experiment complete immunity was obtained by the total injection of 0·026 c.c. of the culture per kilogram of the subject.—Interpretation of the fire-ball painted by Raphael in his picture the "Madonna di Foligno," by M. Daubrée (see NATURE, vol. xliii. p. 500, and *American Journal of Science*, March 1891).—The law according to which the sum of the distances from the moon to two certain stars varies in the function of time, by M. L. Cruls.—New nebulæ discovered at Paris Observatory, by M. G. Bigourdan. This is a continuation of lists previously given, and contains a description of fifty-five new objects situated between nine and sixteen hours of right ascension.—Observations of the asteroid (308) discovered at Marseilles Observatory with the Eichens equatorial, by M. Borrelly. Observations for position were made on March 31 and April 1 and 4.—On the theory of surfaces applicable to a given surface, by M. J. Weingarten.—On the theory of applicable surfaces, by M. E. Goursat.—On an analytical problem which is connected with dynamical equations, by M. R. Liouville.—On regular continuous fractions relative to e^x, by M. H. Padé.—On the mode of vibration of membranes, and the *rôle* of the thyro-arytenoidean muscle, by M. A. Hubert.—Preparation and properties of iodide of boron, by M. Henri Moissan. (See Notes, p. 568.)—On a new compound of molybdenum, by M. E. Péchard. A description of permolybdic acid, Mo_2O_7, is given.—On a new method for the separation of iron from cobalt and nickel, by M. G. A. Le Roy. An electrolytic method is proposed for the separation of iron from cobalt and nickel.—On the asymmetry and the production of the rotatory power in the chlorides of the compound ammoniums, by M. J. A. Le Bel. The author shows that when four alcoholic radicals are substituted for the hydrogen in ammonium chloride, the molecule appears to attain an invariable geometrical form. This is experimentally proved by the existence of isomers and the appearance of rotatory power when these four radicals are different.—On the nitro-derivatives of dimethyl-ortho-anisidine, by MM. E. Grimaux and L. Lefevre.—On the transformation by heat from campho-sulpho-phenols to homologues of ordinary phenol, by M. P. Cazeneuve.—On terebenthene, by M. Raoul Varet.—On ethyl malonate, and ethyl-potassium malonate, by M. G. Massol.—On the micro-organisms found on grapes, and on their development during fermentation, by MM. V. Martinand and M. Rietsch.—Contribution to the study of the bleaching effect of the air, by MM. A. and P. Buisine.—Law of position of nervous centres, by M. Alexis Julien.—New observations on the oceanic sardine, by M. G. Pouchet.—On the supposed crane of Moctezuma II., by M. E. T. Hamy.—On the existence of tuffs of andesite in the flysch of La Clusaz (Upper Savoy), by M. P. Termier.—On the phenomena consecutive to the alteration of the pancreas determined experimentally by an injection of paraffin in the Wirsung canal, by M. E. Hédon.—On the phenomena consecutive to the destruction of the pancreas, by M. E. Gley.—Chemical researches on microbian secretions: transformation and elimination of the nitrogenous organic matter by the pyocyanic bacillus in a medium of a given culture, by MM. Arnaud and A. Charrin.

STOCKHOLM.

Royal Academy of Sciences, March 11.—On symbiosis as causing accessory secretions in the shells of marine Gastropoda, by Dr. Carl Aurivillius.—Researches on the amount of blood expelled from the heart, by Prof. Tigerstedt.—On pendulum observations made in the mines of Sala during 1890, by Prof. Rosén.—A report on a foreign tour undertaken to study constructions suitable for maritime purposes, by Herr Nystedt, marine engineer.—On the respiration of the Algæ, by Miss H. Lovén.—On the hydrography of the fiord of Gullmar, by Miss A. Palmqvist.—Observationes mycologicæ; i. de genere Russula, by L. Romell.—On the African species of the genus Xyris, by Dr. A. Nilsson.—An elementary demonstration of the fundamental proposition of the equation theory, by Dr. E. Phragmén.—On the cyclic system of Ribaucour, by Prof. Bäcklund.—The radiation of the clouds around the thermometric minima, by Dr. H. Hamberg.—Geological observations on Snaefellenesand in the environs of the Faxebay in Iceland, by Dr. Th. Thoroddsen.—Derivation of a formula within the mathematical statistic, by Dr. G. Eneström.—Observations on the ephippiæ or the hivernal egg-capsules of *Daphnia pulex*, by Baron G. C. Cederström.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Dictionary of Applied Chemistry: Prof. T. E. Thorpe; vol. ii. (Longmans).—The Missouri Botanical Garden (St. Louis).—A Treatise on Plane Trigonometry: E. W. Hobson (Cambridge University Press).—Elementary Lessons in Heat, Light, and Sound: Prof. D. E. Jones (Macmillan and Co.).—The "Progressive" Euclid, Books 1 and 2: A. T. Richardson (Macmillan and Co.).—Magnetic Observations at the U.S. Naval Observatory, 1888 and 1889: J. A. Hoogewerff (Washington).—The Elements of Statics and Dynamics; Part 2, Elements of Dynamics: S. L. Loney (Cambridge University Press).—Traité Élémentaire d'Électricité: J. Joubert; 2me. edition (Paris, G. Masson).—The London and Middlesex Note-book (E. Stock).—Elementary Chemistry: W. J. Harrison (Blackie).—Familiar Objects of Every-day Life: J. Hassell (Blackie).

CONTENTS.

THURSDAY, APRIL 23, 1891.

CATALOGUE OF FOSSIL FISHES.

Catalogue of the Fossil Fishes in the British Museum, Natural History. Part II. By Arthur Smith Woodward, F.G.S., F.Z.S. (London: Printed by order of the Trustees.)

IT is a matter for very warm congratulation to all British zoologists that such excellent work as Mr. Smith Woodward's "Catalogue of Fossil Fishes" should proceed from the great National Museum in Cromwell Road. When one remembers the dingy rooms, the inadequate staff, the poor library of the old Natural History Department at Bloomsbury, it seems scarcely credible that in so few years' time so great a change has been effected. In the new Museum there is ample space for the collections, working rooms for the curators, an increased staff of competent naturalists, and an admirable library with the latest memoirs from all parts of the world for their use. The increased opportunities for genuine scientific work now afforded to the younger members of the staff of the Museum, as well as the improved system of making appointments which, as well as much else which is good in the Natural History Museum, we owe to the Director, are bearing fruit in work like that done by Mr. Smith Woodward. His "Catalogue of Fossil Fishes" is not a mere enumeration of the specimens in the National Collection. Though the series of fossil fishes there deposited is by far the finest and most extensive in any Museum in the world—now that the collections of the late Earl of Enniskillen and of the late Sir Philip Egerton have been incorporated with the older possessions of the Museum—yet of many groups or of important genera the best specimens are as far away as St. Petersburg on the one side and Philadelphia on the other. Mr. Woodward has for some years spent his vacations in visiting (one hopes at the expense of the Trustees of the British Museum) the various Museums of Europe and America in which fossil fish-remains are preserved: he is therefore well acquainted, not only with the literature of his subject, but with all the existing material. Further than this, he shows in his treatment of the remains with which he has to deal a sound knowledge of anatomy, and that care and precision as to systematic arrangement and detail which are the most important qualifications of a museum curator. The "Catalogue of Fossil Fishes" in the British Museum thus becomes, in Mr. Smith Woodward's hands, a valuable treatise on ichthyology, indispensable to every comparative anatomist.

In the matter of the general classification of fishes, Mr. Smith Woodward follows neither the tradition of Huxley nor of Günther, but leans to Cope; at the same time, his two main lines of piscine pedigree, the Hyostylic and the Autostylic, are distinguished by a character first insisted upon by Huxley. He recognizes two sub-classes of Hyostylic fishes—the ELASMOBRANCHII, with three orders, Icthyotomi, Selachii, and Acanthodii; and the TELEOSTOMI, with two orders, the Crossopterygii and the Actinopterygii (these latter including all modern Ganoids and Teleostei). In the other main branch of descent, the Autostylic, Mr. Smith Woodward places also two sub-classes—the HOLOCEPHALI with only one known order, the Chimæroidei; and the DIPNOI, with two orders, viz. (1) Sirenoidei (including Dipterus, Phaneropleuron, and Lepidosiren), and (2) Arthrodira (including Coccosteus and Dinicthys). In addition to these four sub-classes, the author recognizes a fifth—the OSTRACODERMI, with the three orders Heterostraci, Osteostraci, and Antiarcha (why not —i ?), typified respectively by the genera Pteraspis, Cephalaspis, and Ptericthys. There is no evidence to show what may have been the nature of the jaw-suspensor in the Ostracodermi—hence the sub-class cannot be referred to either hyostylic or autostylic branch.

There are several noteworthy features in this classification. The Palæicthyes of Günther are rejected, and, more startling still, we miss the familiar "Ganoids" of the palæontologist. Every anatomist must have been struck with the clear indications of a line of descent from primitive fishes through forms like Chimæra to the Dipnoi and so to the Amphibia. But one is not quite prepared for some of Mr. Smith Woodward's pedigree-making. He adopts the view that the Icthyotomi—those delightful extinct sharks with crossopterygial fins—are the most ancient forms of the hyostylic series, and is led to the conclusion that, in both the autostylic and the hyostylic series, the so-called archipterygium (crossopterygium, I should prefer to call it) is really archaic and primitive, and that the "actinopterygium" of Chimæra in the one series, and of modern Elasmobranchs, and, again, of modern Teleostomi in the other series, is a secondary departure, occurring thus at least at three separate and widely remote points in the pedigree. As a result of this mode of viewing the facts, we get Phaneropleuron together with Dipterus associated at the remote end of the autostylic branch with Ceratodus and Protopterus, whilst as far off as possible, at the remote end of the hyostylic branch, we find the "Crossopterygian Ganoids" Holoptychius and Osteolepis—the former of which was so closely associated by Huxley, as previously by the sands of Dura Den, with Phaneropleuron.

It is difficult to persuade oneself that the wide separation of these two old associates now proposed is a natural one. Without proposing another theoretical pedigree, we may point out two facts: (1) that it is somewhat easier to suppose that the change from autostylic to hyostylic suspensorium (or from an amphistylic neutral condition to either) has taken place more than once, than it is to suppose this of the change from crossopterygium to actinopterygium; (2) that the evidence adduced by Balfour and by Thatcher as to the primitive identity of construction of the lateral fins and the median fins of fishes is very strong, whereas the assumption of a primitive fish-ancestor with two pairs of crossopterygia has really not a leg to stand on, because the argument from palæontological succession is faulty, the Icthyotomi being really modern fish (Carboniferous), compared with the primitive fish of Silurian times. There is no sufficient ground for concluding that the actinopterygium has always developed from the crossopterygium rather than that the change has been in the other direction.

However these matters may be settled hereafter, it is probable that the fusion of the Ganoids with the Teleostei as one group (Teleostomi) containing older and newer grades of structural evolution, will be found acceptable by many systematists.

It would be impossible to follow Mr. Smith Woodward in these columns into the details of his work, but there are one or two points to which I may direct attention. The wide separation of Coccosteus from Pterichthys is a bold stroke, and one for which much solid argument is adduced, Coccosteus and its allies appearing to fit in with the Dipnoi, whilst Pterichthys finds allies in Cephalaspis, Pteraspis, and Didymaspis. Dr. Traquair's valuable researches are largely used and acknowledged by Mr. Smith Woodward, and he does not hesitate to accept the former author's association of the large family of the Palæoniscidæ with the Sturgeons, which, as the Chondrostei, form the first sub-order of the Actinopterygian Teleostomi, the point up to which this valuable work has now been brought.

Of my old friends Cephalaspis, Pteraspis, and their allies, Mr. Smith Woodward has a great deal which is extremely interesting to say. It appears that the conclusion (which I resisted in my monograph published by the Palæontographical Society, and elsewhere) is now pretty well demonstrated by new and well-preserved specimens (described by von Alth), viz. that the plates to which I gave the name Scaphaspis are really the ventral plates of the associated species of Pteraspis. Mr. Smith Woodward is able by means of this view to give a more correct interpretation of my specimen of the scales of Pteraspis than I was able to do. It is now in the collection of which this catalogue treats, and remains the only specimen of the body-covering of these fish. The possibility of this relationship of Scaphaspis and Pteraspis was often discussed and considered at the time when I wrote on these fossils, and I was careful to point out the constant association of *Sc. Lloydii* with *Pteraspis rostratus*, of *Sc. rectus* with *Pt. Crouchii*, and of *Sc. truncatus* with *Cyathaspis Banksii*. But no British specimens were forthcoming (although we had hundreds to examine) showing any connection of the two shields as parts of one animal. The remarkable genus to which I gave the name Holaspis (which, being preoccupied by the late Dr. Gray, has had to give place to Mr. Claypole's Palæaspis of ten years later date) seemed to confirm the view that Scaphaspis was a simplified shield of the same character as Pteraspis, Holaspis or Palæaspis being intermediate in complexity. But the specimen described and figured by von Alth leaves no doubt in my mind as to the interpretation of Scaphaspis adopted by him. The absence of the canal system, so well developed in Pteraspis and Palæaspis, is an argument against Scaphaspis being, like those forms, a dorsal cephalic plate.

There are matters with regard to Didymaspis and Eukeraspis (genera of mine which I am glad to see surviving Mr. Smith Woodward's crucible) and Cephalaspis on which new light is thrown in the "Catalogue," which, however, I must not take space to detail. A very interesting thing connected with our Herefordshire cornstones is the discovery, by Dr. Traquair, of a Coccostean, *Phlyctænaspis anglica*, in specimens which I had supposed might be portions of the large *Cephalaspis salweyi*. Quite a series of these specimens are recorded in the "Catalogue" as being in the national collection, whilst in Oxford we have some excellent specimens amongst the other treasures of the Grindrod Collection, which has been purchased for the University Museum.

Sixteen beautifully lithographed plates and numerous woodcuts illustrate this second volume of the "Catalogue of Fossil Fishes."

One last word as to a matter of word-making. I congratulate Mr. Smith Woodward on having the good sense to write "Pteraspidæ" instead of "Pteraspididæ," and "Asterolepidæ" instead of "Asterolepididæ." It would *not* have been more correct to write the longer form, and would have been pedantic.

E. Ray Lankester.

THE HONEY BEE.

The Honey Bee: its Natural History, Anatomy, and Physiology. By T. W. Cowan, F.L.S., F.G.S., F.R.M.S., F.S.Sc., &c. Illustrated with Seventy-two Figures of 136 Illustrations. (London: Houlston and Sons, 1890.)

THE interior economy of the hive is known intimately to every bee-keeper; with the anatomy of its makers, rulers, citizens, not one in a hundred is familiar. The mass of facts accumulated during two centuries of discovery lies for the most part embalmed in the Proceedings of Societies, locked up in costly monographs, untranslated from foreign languages: for the first time it is here presented to English readers, in a form at once exhaustive and concise, by the most accomplished of modern apiarians. Careful compilation from former writers, enriched with much matter that is wholly new, conscientious exclusion of theories unverified by experiment, accuracy of illustration secured by direct drawing from photo-micrographs or microscopical preparations, justify, we think, with the deductions inseparable from a first attempt, Mr. Cowan's claim to have produced "the most perfect work of its kind."

From its pages bee-keepers will learn the extent to which the romantic stories current as to their favourites are borne out by scientific research or must be abandoned. They will find the most startling of all, parthenogenesis in the young hive-mother, to be an established fact; the unwedded queen lays eggs profusely, but all of them give birth to drones. No less certain is the derivation of queens and workers respectively from the different kinds of food administered to them in their larval state. The future queen is fed on "chyle-food," elaborated in the chyle-stomach of the nurses, until it assumes the chrysalis change, from which it emerges a perfect female. The future worker is weaned upon the fourth day, and fed henceforth on honey and digested pollen, with the result that its ovaries are rudimentary and sterile, while its further genital structure renders it incapable of mating. The fecundation of the queen takes place within a few days of her quitting the cell, and lasts for life; the millions of spermatozoa injected being imprisoned in a special receptacle, retaining their vitality throughout her lifetime, and escaping one by one to fertilize each egg as it is laid. Similar storage power is present in the digestive system. The food swallowed by the mouth passes, not into the stomach, but into a temporary reservoir, called the honey-sac, from whence it can at will be regurgitated into the cells, or passed into the stomach through a valvular orifice, called the stomach-mouth, a construction which "permits the bee to eat when, where, and how she pleases, without having to trouble her mouth in any way." And this strange power extends even to the respiratory process, one or two rapid breaths so charging the tracheæ, that for the next three minutes no further inhalation is required. One singularly interesting belief is shattered by Mr. Cowan—that the accuracy of comb structure is due to "mathematical instinct." The story of Maraldi's measurements and of Kœnig's miscalculations, of the error in a book of logarithms discovered by the exquisite precision of bee mensuration in constructing the lozenge-shaped plates which form the basis of the cells, has been popularized by many writers. Mr. Cowan's observations show that in no carefully measured piece of comb are all the diameters of the different cells or all the angles of the different plates identical, nor is their variation regular. Cells are sometimes square, sometimes acute-angled, sometimes roughly circular. The mechanical purpose of the bees is confined to the formation of a cylinder, but the mutual interference of the little architects, as in the case of impinging soap-bubbles or bottled peas, compels the hexagonal form.

Though shorn of its mathematical attributes, bee intelligence remains extraordinary. Their prescience, their power of exact mutual communication, the elaborate community of discipline and labour which characterize each colony, throw doubt on the familiar distinction which denies to lower animals the analytic faculty—accords perception to the bee or dog, conception to the human mind alone. Into this question Mr. Cowan does not enter; but we are not surprised to learn from him that the well-defined brain necessary to the possession of intelligence forms in the bee 1/174 part of the volume of the body, sinking in other insects as low as to the 1/4200 part.

The muscular power of the bee is as much in advance of man's as its intellectual power is inferior. A man's power of traction is far below the weight of his body; a bee can draw twenty times its own weight. Its flight exceeds twelve miles an hour, and it will go four miles in search of food. Its wings, braced together in flight by a row of hooklets, bear it forward or backward, with upward, downward, or suddenly arrested course, by a beautiful mechanical adaptation which the author admirably describes. Its voice-organs are three-fold, the vibrating wings, the vibrating rings of the abdomen, and a true vocal apparatus in the breathing aperture or spiracle: the first two produce the buzz; while the hum—surly, cheerful, or colloquially significant—is due to the vocal membrane. Some of its notes have been interpreted. "Huumm" is the cry of contentment; "Wuh-wuh-wuh" glorifies the incessant accouchements of the queen; "Shu-u-u" is the frolic note of young bees at play; "Ssss" means the muster of a swarm; "Brrr" the slaughter or expulsion of the drones; the "Tu-tu-tu" of newly-hatched young queens is answered by the "Qua-qua-qua" of the queens still imprisoned in their cells.

Enough has been said to indicate the interest and value of this compendium. For the description of the tongue, with its rod, spoon, tasting-hairs; of the pulvillus and its adhesive secretions; of the marvellous changes developed in the later metamorphosis of the imperfect insect; of the compound eyes with their mosaic vision, incapable, we suppose, through deficient muscular adjustment (though on this point Mr. Cowan is silent) of delineating to the bee mind a perceptual world resembling ours; of the sting and poison sac, of the wax pockets with their secreted scales, drawn out by the hind leg pincers, made malleable by mastication and saliva, kneaded into foundation walls and cells—the student must betake himself to the book. Full of matter as it is, it leaves much unsettled. The character of the sensory functions, the existence of gustatory and olfactory organs, the meaning of various unintelligible bodies in the antennæ, abdomen, labium, ligula, have not been determined—may perhaps never be understood; for "it is not impossible that insects may possess senses or sensations, of which we can no more form an idea than we should be able to conceive red or green if the human race were blind."

For the second edition of the book, which will doubtless be demanded, certain minor corrections or amendments may be suggested. If bees are *Hymenoptera*, the Greek word for a membrane (p. 2) may fairly be written hymen. In p. 12, ll. 15, 16; in p. 63, ll. 4–9; p. 102, l. 29, slight textual improvements are desirable. Near the bottom of p. 26 is an obvious printer's error. On p. 64, l. 11, the word "those" is not readily referable to the ganglia for which it presumably stands. The engraving on p. 25 is repeated on p. 96. The description of the brain, pp. 68, 69, is indistinct in places, and would gain if the illustrations, 31, 32, were lettered. So, again, the inversion of the male organ, pp. 129, 130, is not clear from Fig. 53. And is it impertinent to hope that the peroration may be re-written or struck out? By all means recognize formally, if it has not indeed been all along assumed, the benevolence and wisdom of a superintending Providence; but the pious platitudes of pp. 190–192 are an anti-climax to the clear reasoning and precise statement which are maintained throughout the scientific portion of the work, and leave on the critical palate an after-taste less sweet than honey to the mouth.

W. TUCKWELL.

ELECTRO-METALLURGY.

The Electro-plater's Hand-book. By G. E. Bonney. (London: Whittaker and Co., 1891.)

THIS book is designated as being a "practical manual for amateurs and young students in electro-metallurgy," and intended "to meet the wants of amateurs and young workmen desiring a practical manual in electroplating at a low price."

It contains a large amount of sound information respecting the details of preparing metals for electroplating; the processes of cleaning, polishing, scratch-brushing, &c.; a number of useful practical tables; numerous recipes for making plating solutions; descriptions of workshop appliances, tools, pieces of apparatus, &c., of immediate practical use to a working electroplater; and it is essentially an ordinary workman's book.

In accordance with the statement made in the preface, that "it supposes the workman to have an elementary acquaintance with electrical science," the book contains scarcely any information respecting the scientific principles of the art, and even Faraday's great law of definite electro-chemical action is not mentioned or explained. It will therefore be more readily purchased by the ordinary than by the superior workman, by those who prefer to work by "rule of thumb" than by those who wish to be guided by the light of science.

With regard to the statement on p. 1, that "the art of electro-metallurgy cannot be said to own an inventor," it is true that the complete art is not due to a single inventor, but it owns a series of inventors, viz. Wollaston (1801), Brugnatelli (1805), De la Rue (1836), Jacobi, Jordan, Spencer (1839), J. Wright (1840), J. Elkington (1865), &c., who were all of them prominent inventors of electro-metallurgical methods.

The rules given on p. 4 for selecting a suitable depositing solution are good, and are well known; and the remark on p. 7, that "the main consideration must be directed to current density," is a widely applicable and very useful one.

The statement on p. 8, that the current deposits the metals from solutions of alloys "in proportions determined rather by their electric equivalents than by the quantity of the metal in solution," is a very doubtful one; for instance, with copper and zinc in solution together, the electro-chemical (not "electric") equivalents of which are about equal, if the copper is in large proportion compared with the zinc, copper is deposited, and no zinc is usually deposited along with it, unless the current is of great density at the cathode.

The idea expressed on p. 12, that "the internal resistance of a battery may be reduced by using longer connecting wires between the cells," is not in accordance with the established conventional understanding that "internal resistance" is limited to the contents of the battery cells. The information given about the arrangement and construction of several kinds of voltaic batteries is full and explicit, and the same may be said about arranging their cells in series and in parallel.

With regard to the chapter on dynamos, it is a very useful one, and the longest in the book; and the construction and action of those machines are as fully described as any other part of the subject; but, whilst several good dynamos for electro-plating are mentioned and illustrated, some obsolete and wasteful ones are figured and described which might have been omitted.

Chapter vi., "Electro-plating with Silver," contains much information, both of general matters and of details useful to the working electro-plater; the statement, however, on p. 125, that the foreign salts which usually exist in a dissolved state in a cyanide of silver plating liquid "offer a resistance to the current," has never yet been demonstrated; in fact, they rather tend to diminish the resistance, and it is only when they give rise to a solid film upon the anode, and that is very rarely, when the liquid is deficient in free cyanide of potassium, that they obstruct the current.

The statement on p. 165, in the chapter on "Nickel-plating," that "very little was effected in a practical way until 1890, when Mr. Adams discovered that nickel could be deposited from a solution of the double sulphate of nickel and ammonia in a reguline condition," requires a ittle qualification, for both previous to and at that period the double chloride of nickel and ammonium (generally known as "Gore's solution") was rapidly extending in use in America, the early home of nickel-plating.

The chapter on "Electro-plating with Copper," is very brief. Whilst the one on "Dynamos" occupies 38 pages, and might in some respects have been abbreviated with advantage, that on "Copper" fills only 7 pages, and might have been extended.

A rather serious error in the chemistry of the subject occurs on p. 200, where the table, and the column of numbers which are really the atomic weights, are each headed "combining weights"; and the electro-chemical equivalents are headed "electric equivalents." Now, the atomic weights are in many cases very different from the combining weights, and the numbers which are there termed "electric equivalents" should have been called "electro-chemical equivalents."

Instead of employing the simple terms, made, making, stirring, &c., the author, throughout the book, continually uses the phraseology, "made up," "making up," "stirring up," "faking up" the solutions, batteries, apparatus, &c. These, however, are minor matters. The chief defect in the book is that "it supposes the workman to have an elementary acquaintance with electrical science," and, apparently on the basis of that usually fallacious assumption, omits nearly all information respecting the fundamental principles of the subject. Now we know that not only the working man, but the sons of persons in much higher grades of society, are often so incompetent to appreciate the great value to themselves of knowledge of important principles, and so anxious to obtain "quick returns" in the form of money or some easily perceived personal advantage, that they are apt to shirk learning anything which they think will not quickly yield them a profit, and this radical defect should not be encouraged by those who undertake to teach. An omission of all reference to the principles of his subject further indicates that the teacher himself either does not fully understand or adequately appreciate the foundation of the matter he professes to teach. Knowledge of the principles of his occupation is usually the point in which the workman is most deficient and most requires to be instructed.

Notwithstanding this drawback, the book is sound and good in nearly all matters of practical detail; it is also well illustrated, and may be used with advantage as supplementary to a more scientific one containing the principles of electro-metallurgy.

OUR BOOK SHELF.

Primitive Folk: Studies in Comparative Ethnology. By Élie Reclus. (London: Walter Scott, 1891.)

This volume belongs to the "Contemporary Science Series," edited by Mr. Havelock Ellis. It contains a popular account of the Eastern and Western Inoits, the Apaches, the Naïrs, the mountaineers of the Neilgherries, and the Kolarians of Bengal. The writer does not group his facts in accordance with any controlling idea, so that the book can hardly be said to have much continuity of interest. He writes, however, in a fresh and lively style, and has brought together many curious facts; and the work may serve as an attractive and useful introduction to the study of some aspects of ethnography. He has, of course, to describe many customs and modes of thought which, if judged from our point of view, would produce a strange impression; but these he puts in their right place as elements which mark definite stages of evolution. Too few references are given in the notes, but the information may be accepted as generally trustworthy, having been for the most part derived from the statements of travellers and missionaries during the first half of the present century.

Tongues in Trees and Sermons in Stones. By the Rev. W. Tuckwell. (London: George Allen, 1891.)

THE greater part of this volume is occupied with papers which bear the general heading "Tongues in Trees." They are interesting essays, written in a clear and pleasant style, and presenting in a fresh light a good many "facts in flower-lore" Among the subjects are tree worship, tree myths and superstitions, plant-names of persons, places, and seasons, plant monsters, gardens, and plant literature. The fact that there may be "sermons in stones" is illustrated by a short paper on sundials, reprinted from the *Gardener's Chronicle.*

Supplement to Euclid Revised. By R. C. J. Nixon, M.A. (Oxford: Clarendon Press, 1891.)

IN this little book the author has placed before the student a few theorems on the more modern geometry of the triangle. The points touched upon relate to the Lemoine and Brocard points, lines, and circles; and a study of these will serve as a good introduction to many of the more advanced theorems. The proofs are concise and complete, and a few exercises are added which are more or less depend ent on them.

LETTERS TO THE EDITOR.

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

The Alpine Flora.

FOR some years past we have endeavoured to make as large a collection as possible of the more interesting Alpine plants at Kew. And I am glad to take the opportunity of acknowledging the invaluable assistance which we have received from Mr. G. C. Churchill, who has spared no pains on our behalf in corresponding with collectors in almost every mountainous district of Europe.

The observation of the Kew collection has led me to a few conclusions which have some bearing on the questions raised by Mr. Cockerell and the Rev. George Henslow.

What are called Alpine plants appear to me to fall into two distinct classes: (1) those which have a permanent adaptive habit, and (2) those which have a habit due merely to the local conditions of the growth of the individual plant and not maintained by it if those conditions are altered.

The peculiarities of the former class I am disposed myself to attribute to natural selection. Anyone who supposed that Alpine plants were particularly "hardy," would find himself very rapidly disabused by attempting to cultivate them. I have no definite data on the subject; but I am inclined to think that for the most part they are intolerant of very low temperatures. They certainly are extremely impatient of humidity during the period—a comparatively long one—when they are not in active growth. We are for these reasons, paradoxical as it may seem, obliged at Kew to winter our large collection in frames under glass. The reason is that in Nature, except for a short time, Alpine plants are covered with snow, which keeps them dry and protects them from a very low temperature. Last summer, accompanied by the foreman of our herbaceous department, Mr. Dewar, I crossed the Zwischbergen Pass from the village of Simplon to Saas. This was late in July, and on the south side we crossed a wonderful area of Alpine vegetation from which the snow had only just melted; I suppose that by the end of September it would be covered up again. I presume the ground would remain permanently frozen at no great depth. The plants were therefore reduced to growing in the thin film of soil warmed during exposure by the sun's heat, and this would be, for the greater part of the year, sandwiched between deep overlying snow and the frozen subsoil. No plants which had other than a close dwarf habit would seem to me to have a chance of existing under such circumstances.

On the lower slopes of the valleys of the Alps there is a profusion of interesting plants, which, though not so dwarf as those of higher levels, still derive a good deal of their charm from the more or less compactness of their growth. It is a matter of general experience that, when many of these are transferred to a country such as England, they develop into a coarse weedy habit which deprives them of a great deal of their interest. I am not satisfied that a richer soil in cultivation affords the explanation of this. I am disposed to think that the conditions of solar illumination in the Alps are a much more important factor.

Every practical gardener knows that if you want to maintain a desirable stubby habit in a decorative plant, and to keep the vegetative structures in proper subordination to the flowers, you must expose your plants as freely as possible to sunlight. This is not, however, a question of promoting assimilation, but of preventing elongation and extension of parts already formed. There is probably much the same amount of material in the stubby and the lanky plant.

Now plants which grow at high levels in the Alps are *above* a great screen of aqueous vapour. They therefore get a larger share of the more refrangible rays of sunlight than those that grow nearer sea-level. And it is precisely these rays which it is known inhibit growth or rather extension in length.

But though, as I believe, this restraining and, if you like, dwarfing action has been going on for ages, it is not my experience that the effect is permanently impressed on the species. I wish it were.

I have met with one plant in which I thought that I had got a case of the transmission of an acquired habit. Mr. Churchill procured for us seeds of *Arabis anachoretica*, "a form of *Arabis alpina*, L., with thin tissue-papery leaves, growing in hollows of the rock where neither sun nor rain reach it, just as *Saxifraga arachnoidea*, as also *Heliospermum glutinosum*, and *Zahlbrucknera paradoxa*, all which have very thin tissue-paper leaves." Alas, on cultivation at Kew it reverted forthwith to commonplace *Arabis alpina*.

Mr. Henslow remarks that "the action of the environment on plants is a thing which can be tested," while the accumulation of many useful variations cannot. Hence he appears to infer that Darwinism is an *a priori* theory, while neo-Lamarckism is not. If he rests this conclusion on facts drawn from the study of the Alpine flora, all I can say is, that while horticulturists are obtaining dwarf varieties every day by selection, I am not aware of any one which can be demonstrably shown to be the product of the action of external conditions.

W. T. THISELTON-DYER.

Royal Gardens, Kew.

Neo-Lamarckism and Darwinism.

MR. COCKERELL'S reply to my remarks is interesting, as I cannot find anything in it with which I do not cordially agree. A few words, however, may clear up matters.

(1) I referred only to dwarfs on high Alpine and Arctic regions. *These* are due to cold (see Grisebach, "La Vég. du Globe," i. 49–51; Verlot, "Sur la Production et la Fixation des Variétés," p. 36).

(2) I am quite convinced of the hereditary character of "nanism," as also of "gigantism," as well as of other results of the direct action of the environment on plants; and am glad to find Mr. Cockerell has come to this conclusion from direct observation. As an example of hereditary hypertrophy, the parsnip called "The Student" was raised by Prof. Buckman from seeds of wild plants in 1847, and it is still sold by Messrs. Sutton as their "best variety." On the other hand, Mr. Cockerell is perfectly correct in admitting the, so to say, plasticity of dwarfs. Indeed, my own experiments with aquatic plants lead me to wonder why some species are, as a rule, so fixed as they generally appear to be.

(3) Tall plants are certainly liable to be injured by winds; as I added, "as occurs at lower altitudes"; but are there any such among the dwarfs *above the tree line?* If not, natural

selection has no tall varieties to work upon; or, if natural selection has eliminated them at that altitude, why has it not done so at a lower one? M. Bonnier's experiments go to show that they will not form there (see his figures and descriptions in "Rev. Gén. de Bot.," ii. p. 528). All I ask for is, that Mr. Cockerell's supposition should have some *facts* to rest upon.

(4) I did not question the fact that dwarf Alpine plants may have additional warmth from their close proximity to the soil. De Candolle observes: "Il en résulte que la couche superficielle du sol doit en général être plus chaude sur les montagnes que dans la plaine, la moyenne extérieure étant supposée semblable" ("Géog. Bot.," ii. p. 260). Indeed, I have grounds for suspecting that the prostrate condition, common in Alpine and other exposed plants, is generally, a result of the direct response of thermotropism. In support of this statement, I will give two out of several observations. *Malva sylvestris*, when growing freely, surrounded by a dry hard soil which is readily heated, develops a prostrate form; but it grows erect when in a damp soil and surrounded by other plants. Bluebells often lay their first leaves flat on the ground. The temperature *on* the soil and under such a leaf was 47°·5 at 10 o'clock this morning (April 15). The plant is growing in shade and in damp soil. The temperature of the air 3 inches above it was 44°·5. Lastly, the temperature *under* the flat leaves of a plantain, growing in short grass covered with dew, was 52°·5, while at 3 inches above it, it was 49°·5. What I questioned was the right of building an argument upon a statement, when the opposite condition of great cold arising from radiation at night is ignored. Thus, De Candolle observes:—"Plus on s'élève, moins les rayons solaires sont interceptés par l'atmosphère, plus aussi la déperdition de chaleur par le rayonnement nocturne et par l'évaporation est considérable" (*l.c.* ii. p. 259).

(5) Individual plants may be sheltered; but is not exposure the *prevailing* feature in high regions? Such is, at least, what I have observed.

(6) "Those plants which naturally grow quickly . . . . would survive." Quite true; all I maintain is that the primary cause is not *internal*, as "naturally" seems to imply, but *external* to the plant. That is, the total amount of heat and light received by the plant in a comparatively short time is what induces the rapid development of high Alpine and Arctic plants. But, on the other hand, the responsiveness of plants is not absolutely alike in all cases; therefore we have enough evidence to draw the deduction which I will express in Mr. Cockerell's words: "Given such variations [in the responsiveness to climate], is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short?" Certainly, as M. Verlot shows (*l.c.* p. 48), what horticulturists are familiar with, that precocious and late varieties are hereditary.

Mr. Cockerell's reply does not combat my sole contention, that it is unscientific to form theories of what "may be" and "might be," when *no, or insufficient, facts* are brought forward on which the supposition is based. There may not be a sufficiency to justify one in unhesitatingly accepting any particular theory; but all I ask for is something *positive*, or well-determined *facts*, upon which a hypothesis may be based. Science is not advanced, but hindered, by "may be's" and "might be's."

George Henslow.

Co-adaptation and Free Intercrossing.

As it appears to me, from his reply, that Prof. Meldola's views on the subject of "co-adaptation" are really the same as my own, I write once more in order to point out the identity.

In the first place, I of course agree that "every [considerable] variation, of whatever kind, must of necessity entail numerous structural and functional modifications"; and therefore "that there is not any 'fortuity' in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual." "But," as Prof. Meldola continues, "the case we have to consider is that in which the variations, A, B, C, D, &c., are not [thus] physically or physiologically correlated; but in which they are supposed to be 'independent variables.'" Now, this being the only case we have to consider, Prof. Meldola says, "The chances against such variations occurring in any one individual are, I concede most fully, 'infinity to one.'" And lower down he as fully concedes the consequence, that if it be supposed necessary for the utility of A that A should occur in association with B, C, D, &c., then the chances must be infinity to one against the perpetuated existence of A. But this is all that Broca's and Spencer's "difficulty" from co-adaptation alleges; and, as I said in my last letter, if it is to be met, it can only be so by denying that any cases do actually occur in nature where the utility of A is thus dependent upon the association of A with the other independent variables, B, C, D, &c. Accordingly, this is the way in which Prof. Meldola's reply does meet the "difficulty." For he concludes—and very properly—"if it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio*"—*i.e.*, to show cases where the utility both of A and of B is dependent on the association A + B. And he goes on to add, "that the particular case of co-adaptation quoted by Mr. Pascoe [*i.e.* that of the giraffe] may resolve itself into a 'confluence of adaptations.'" But, if so, *cadet quæstio* as regards this particular case. And similarly, he continues, as regards all other possible cases; for the next sentence is,—"In fact, I do not think it would be going too far to put forward the proposition that all cases of co-adaptation may be ultimately explained in the same way, *i.e.* that they arise from the coalescence (by intercrossing) of *n* modifications each *useful* (not useless) in itself, and acquired at successive periods in the phylogeny of the race."

Thus Prof. Meldola meets the "difficulty" in the way which I believe is the only way that it can be met, viz. by denying that, as a matter of fact, any cases of "co-adaptation" (as distinguished from a "confluence of adaptations") ever do occur in nature. This, however, raises a distinct question. The issue between him and those who advance the "difficulty" will now no longer be logical, but biological. And, as stated in my previous letter, I do not intend to go into this distinct question. My only object has been to show that, unless the supposed fact of co-adaptation be denied, any appeal to the analogy of artificial selection is illogical; while, if this supposed fact is denied, any appeal to the analogy—then "real" enough—is superfluous. For, as I said before, it is self-evident that if each of the independent variables is of utility *per se*, natural selection will, sooner or later, blend them all together through free intercrossing.

Thus, with regard to the matter of co-adaptation, it appears that we are in complete agreement. Unless any given case—such as that of the giraffe—is denied to be a real case of co-adaptation, Prof. Meldola "fully concedes" that the chances against its evolution by natural selection alone must be "infinity to one," and, therefore, that the analogy of artificial selection as regards that case would be false. I am sorry to observe, however, that such agreement does not appear to prevail with regard to the distinct and much more important "difficulty of the swamping effects of intercrossing." For in his last paragraph Prof. Meldola ranges himself on the side of the "neo-Darwinians" as regards this difficulty, remarking, on the one hand, that he does not believe it to be "real," and, on the other hand, that it has been "amply treated of by Mr. Wallace and others." The difficulty here alluded to is to conceive how it is so much as logically possible for natural selection *alone* to preserve and accumulate incipient variations in the face of free intercrossing. "Mr. Wallace and others" have attempted to show that this is possible by pointing to the simple and well-known fact that "no being on this earthly ball is like another all in all." They tell us that every specific character—be it of structure, colour, form, size, strength, fleetness, and so forth—is variable round the specific average or mean; so that, if it be desirable either to augment or to diminish any given specific character, there will always be one-half of the number of individuals composing the species which are varying in the required direction at the same time. Now this, of course, is sufficiently obvious; but what does it prove? It only proves what nobody, so far as I am aware, has ever dreamed of doubting—viz. that if it were to become a matter of importance in the struggle for existence that the human form, for example, should be increased as to height, short men and women would be eliminated by natural selection, while tall men and women would transmit their tallness, until in the course of many generations the race would develop into a race of giants. All this is what I would call "pure Darwinism"; and it is not apparent to me how its re-statement by "Mr. Wallace and others" has in any way "broadened" our "ideas" upon the subject. But what I have called "neo-Darwinism" is the Darwinism which fails to distinguish between such a case of the *transmutation* of a specific type in a single line of change, and the *differentiation* of a specific type in two or more lines of change. The race of giants is formed—if I may again quote the pre-Darwinian expressions of the Laureate —by successive generations progressively mounting to higher things on the steps supplied by their own "dead selves." But this ladder-like succession of species in time differs in many respects from a tree-like multiplication of species in space; and the respect in which it differs most has reference to "the difficulty of the swamping effects of intercrossing." The transmutation of species in a single line of change is self-evidently capable of being effected by natural selection alone, because here free intercrossing among the uneliminated "fittest" is the very means whereby the transmutation is effected. But the tree-like multiplication of species (or Darwin's "divergence of character") is no less self-evidently incapable of being effected by natural selection alone; it is not so much as logically possible that any arborescence of species can take place unless natural selection is assisted by some form of isolation at the origin and throughout the development of every branch. The race of giants is evolved by intercrossing being *permitted* between all the uneliminated individuals of each successive generation; but if at the same time a race of pygmies, another race of blacks, another of whites, &c., are to be formed, it is a plain necessity of the case that intercrossing must be *prevented* between all these different races; else natural selection could not so much as begin to differentiate them.

This all-important distinction between what Mr. Gulick has concisely termed "monotypic" and "polytypic" evolution is not observed by "Wallace and others," who are now said to have "amply dealt" with the "difficulty" in question. For they deal only with the case of monotypic evolution, with reference to which no one has been so foolish as to allege any difficulty; and thus their whole treatment of the subject is irrelevant to the only difficulty which has been alleged, viz. the abstract impossibility of natural selection having ever effected *polytypic* evolution, or *divergence* of character, without the co-operation, in some form or another, of segregate breeding by the prevention of free intercrossing. GEORGE J. ROMANES.

Oxford, April 17.

The Flying to Pieces of a Whirling Ring.

DR. LODGE appears to be still in error in making (p. 534), without qualification, the statement that "the dangerous tension will be set up in a straight portion of any endless band running in the direction of its length with the critical speed $\sqrt{\left(\frac{T}{\rho}\right)}$, by the agency of the curved portions which necessarily exist somewhere." For the curved portions of such a band might run *inside* smooth guides which would supply the necessary centripetal pressure and relieve the band from all tension. Such an arrangement was in my mind when I wrote (p. 463) that the band *need not* be stretched to its breaking strain. It is true that Dr. Lodge began his original letter (on p. 439) by specifying a ring "not radially sustained," but he will, I feel sure, allow me to call attention to the fact that without a repetition of this qualification his remark that I have quoted is incorrect: and I think he will see that his supposition that my remarks referred only to a straight bar with free ends, and not to an endless band, is contrary to the expressed terms of my letter. The fact remains that the motion of even an endless band has not necessarily anything to do with the stability of the straight part. Dr. Lodge apparently still wishes us to suppose otherwise.

Devonport, April 12. A. M. WORTHINGTON.

A Lecture Experiment illustrating the Magnetic Screening of Conducting Media.

A LECHER'S tube (*Wied. Ann.*, xli. p. 850) is put, by means of two cork rings, into another large (4 cm. diam.) glass tube, with a crane on one end. Holding the tube in one hand, and approaching it to a wire in which electrical waves are produced, a continuous lighting in the Lecher's tube is to be seen. This lighting remains even if the large tube, including the Lecher's tube, is filled with water. But if the tube is filled with dilute sulphuric acid the light disappears.

When we open the crane and the acid flows out, we perceive the light again, but only in that part of Lecher's tube which is not surrounded with the acid. The light in the Lecher's tube penetrates but very little below the level of the acid in the surrounding tube. J. J. BORGMAN.

St. Petersburg, University, April 14.

The Intelligence of the Thrush.

IT is, I think, well to record the following observations of the intelligence of the thrush. The first happened on June 28, 1865. I then saw, from the windows that look out on the little lawn north of my house, a thrush steadily "stepping westward" in front of the hedge that parts the lawn from the public road. The bird seemed to be intentionally making for a gravel path that, after passing almost close to the windows, bends to the north-west, toward the small gate of my front garden. It was bearing something in its bill. On coming to the path it attempted to break this on a stone. It did not succeed. It then tried another stone. This time it succeeded. Thereupon it flew away. On the spot I found a remarkably big stone embedded in the path, and round it were scattered bits of snail shell. The bird had eaten the snail.

The second of the observations I would note, and the more striking of the two, happened on June 5, 1890. I then was viewing the gravel path from the westernmost of the four windows. Just beneath me, standing on the path, was a female thrush. She had succeeded in breaking a snail shell. She had the snail in her bill. But, despite of vigorous efforts, she could not swallow it. Up hopped a male thrush. Standing before the female, he opened his bill. She dropped the snail into his bill. He chewed the snail. He dropped it back into the female's ready bill. She swallowed it. The pair blithely trotted off, side by side, toward the small gate. I saw them no more.

JOHN HOSKYNS-ABRAHALL.

Combe Vicarage, near Woodstock, Oxfordshire, April 16.

Cocks and Hens.

WHILST I was living at Highfield House in Nottinghamshire in 1879, a duck-wing game bantam injured her leg after she had been sitting for a fortnight, and could no longer remain on the eggs. The cock bird, however, took her place, and not only hatched the brood, but acted in all respects like a hen, brooding the young, setting his feathers up like a brood hen, and using the same peculiar cluck that a hen has for calling her chickens, also sleeping on the nest instead of his usual perch—being, in fact, a mother for the time. Years before (in 1841) one of my Dorking hens was accidentally killed, leaving a brood of chickens some five weeks old; the Dorking cock took to them and brought them up, but in this instance there was no change except the act of brooding. E. J. LOWE.

Shirenewton Hall, Chepstow, April 12.

Cackling of Hens.

IT is often difficult to recall an actual instance of what may be a matter of very common occurrence. Such is to a certain extent the case with the subject to which Prof. Romanes's query in NATURE of April 2 (p. 516) refers.

In a general way it is my impression that the cackling of jungle fowl is not very commonly heard in India, but I feel certain that I have heard it occasionally, and that I once did hear it upon a somewhat considerable scale is impressed very distinctly upon my memory by certain and special circumstances. My tent for a few days in April 1876 was pitched close to a perfectly impenetrable patch of thorny jungle in Orissa. This cover was full of jungle fowl, and I remember hearing the cackling of the hens, which reminded me of the familiar farmyard sounds of home. It is possible that in this case the safety of their retreat may have had something to do with their not fearing to cackle with unusual vigour. V. BALL.

Science and Art Museum, April 18.

THE PARADOX OF THE SUN-SPOT CYCLE IN METEOROLOGY.

IN 1872 and 1873, three writers, independently of each other and dealing with different branches of meteorological science, revived an old speculation of Sir William Herschel's, that the sun's heat probably varies with the visible changes of its photosphere, and that this variation is sensibly reflected in the meteorology of our planet. Mr. Chas. Meldrum reviewing the statistics of the cyclones of the Indian Ocean, Mr. Norman Lockyer those of the rainfall of Ceylon, Southern India, and Australia, and Dr. Köppen the recorded temperatures of numerous stations in various parts of the world, separately arrived at the conclusion that these three classes of phenomena severally afford evidence of a periodical increase and decrease coinciding with that of the solar spots. The speculation, thus started, was followed up with avidity by a large number of inquirers in different parts of the world. The intensity of the solar radiation, the barometric pressure, the levels of rivers, lakes, and inland seas, and even such more remote effects of meteorological conditions as are manifested in the prices of grain, the recurrence of exceptional vintages, of famines, and in the activity of trade, were all brought under investigation; and, for some years, this and other scientific journals contained frequent articles, bringing to notice some new instance or supposed instance of a recurrent variation conforming more or less accurately to the well-known solar period of eleven years. It must be admitted that some of these supposed coincidences were based on evidence that was far from convincing; and since the major part of the weather phenomena of the globe failed to show any distinct trace of the influence of the solar cycle, interest in the subject gradually declined, and for the last few years the discussion of the question has been comparatively in abeyance.

But anyone who considers carefully the bulk of the evidence brought to light in the course of the inquiry will, I think, be prepared to admit that, amid much that is doubtful and inconclusive, the general result has been to establish so many cases of the variation of certain meteorological elements, coinciding with that of photospheric activity, as to place the general truth of the hypothesis beyond all reasonable doubt; but that the amount of the variation is so small in comparison with the irregular vicissitudes of the weather, that it is for the most part obscured and unrecognizable, except fitfully, or in some few favoured regions where these irregularities are less prominent. This has long been the opinion of the present writer, and a similar view was expressed in no hesitating terms by Prof. Arthur Schuster in 1884, in a paper read before the meeting of the British Association at Montreal, and printed in their Reports. "There can," he says, "be no longer any doubt that during about four sun-spot periods (1810 to 1860) a most remarkable similarity [existed] between the curves representing sun-spot frequency and the curves of nearly every meteorological element which is related to temperature. This is not, in my opinion, a matter open to discussion: it is a fact. But it is equally certain that during the thirty or forty years previously to that time no such relationship existed, and that since 1860 the connection has again in some instances become less distinct."

In so far as regards the temperature of the globe, these conclusions are almost identical with those enunciated by Prof. Köppen, in a paper published in the April–May Heft of the Austrian Meteorological *Zeitschrift* for 1881, and indeed the apparent discrepancy of the temperature and sun-spot curves after 1858 had been indicated by him in his former paper, published eight years earlier. But this discrepancy did not set in simultaneously in different parts of the globe, nor has it been such as to obliterate all trace of the sun-spot cycle in the temperature curves. During about four cycles previous to 1858, the temperature of the tropical zone rose and fell with somewhat striking regularity inversely as the spotted surface increased or diminished, so that the highest temperatures approximately coincided with the sun-spot minima, and the lowest with their maxima. A similar course of variation, but of less amplitude and more complicated with subordinate disturbances, characterized also the temperate zones during a great part of this period; but in the north temperate zone, after 1856, these subordinate disturbances gained in relative importance, and although well-marked minima still approximately coincided with the sun-spot maxima of 1860 and 1870, the intermediate period exhibits a series of oscillations some of which are of not less amplitude than that of the solar cycle. In the south temperate zone, on the other hand, the conformity of the temperature with the [inverted] sun-spot curve continued well marked up to 1870, but the secondary oscillation, which is also shown by the sun-spot curve, is more strongly reflected in the former; while in the tropics, as far as can be inferred from the evidence of the very few stations that furnish continuous registers for these years, the coincidence began to fail in 1858, and from 1870 to 1875 the course of the variation appears to have been entirely in disaccord with that of the solar spots.

Now it is in the tropics, and more especially the central equatorial portion of that zone, that we should expect to find evidence of the coincidence if it really exists. It is not only that the solar action is here most intense, but that the influx of the lower atmospheric currents, which are one of the principal agencies that affect and disturb the local "solar" climate, is here most regular, while the alternations of anticyclones and cyclones—the one bringing clear skies and free radiation, the other a cloud canopy and obstructed radiation—are of minor importance, and indeed in the equatorial zone almost evanescent as disturbing causes.

But, as has been already remarked, the stations in the tropics that furnished comparable and continuous registers before 1875 are very few. Up to 1860 they varied from three to twelve for the entire region, and even up to 1875 they were not greatly more numerous. But, since that date, the temperature of India has been recorded with far greater completeness, and it will be interesting, therefore, to inquire how far the ampler evidence thus furnished for at least one tropical country of great extent confirms or invalidates the conclusions drawn from the very defective data of previous years. If we find, as the result of this examination, that the temperature of India as a whole, since 1875, has followed in its variations from year to year the waxing and waning of the spotted areas of the solar surface, and that with even greater regularity than was shown by the scanty records at Prof. Köppen's disposal for the earlier period, we may then infer with some likelihood that some of the discrepancies shown by his curves really arise from the imperfect elimination of the mere local disturbances which all single registers exhibit, when compared with each other, in a more or less marked degree.

For several years past, the annual reports on the meteorology of India have contained a table showing the deviation of the temperature of the country, as a whole, from its normal or average value, in each successive year since 1875; but this has been computed yearly from the several averages of the stations as known up to date, and as these have been corrected yearly, the standards have varied throughout, and in the earlier years differed from those now accepted by amounts quite appreciable in an investigation of this kind. Moreover, the registers of all the existing stations could not be utilized in the earlier years, their average values being then unknown. In the following table, therefore, the mean annual anomalies have been recomputed from the published registers, taking as a common standard the mean temperature of each station given in the report for 1888, and including the registers of all stations in India, Burma, Ceylon, and the adjacent islands, that have furnished a complete or nearly complete register in each year. The results are given in the third column of the table; the fourth shows the annual increments or decrements, and the two right-hand columns Wolf's relative sun-spot numbers and their progressive variation.

Year.	Stations.	Temperature (° F.)		Sun-spot number.	
		Annual anomaly.	Prog. var.	Annual.	Prog. var.
1875	75	0	—	17·1	−27·5
1876	81	+0·21	+0·21	11·3	− 5·8
1877	90	+0·40	+0·19	12·3	+ 1·0
1878	102	+0·84	+0·44	3·4	− 8·9
1879	107	+0·02	−0·82	6·0	+ 2·6
1880	107	+0·37	+0·35	31·5	+25·5
1881	112	+0·29	−0·08	54·2	+22·7
1882	107	+0·10	−0·19	59·6	+ 5·4
1883	118	−0·30	−0·20	63·7	+ 4·1
1884	114	−0·56	−0·26	63·4	− 0·3
1885	120	−0·26	+0·30	52·2	−11·2
1886	120	+0·10	+0·36	25·7	−26·5
1887	127	−0·20	−0·30	13·1	−12·6
1888	127	+0·36	+0·56	6·7	− 6·4
1889[1]	29	+0·70	+0·34	5·8	− 0·9

From this table it appears that, except in the two anomalous years 1879 and 1887, the temperature of India, &c., as a whole, has followed with remarkable regularity the course of the sun-spot variation; the maximum temperature occurring in 1878, the year of the sun-spot minimum, and the minimum temperature in 1884, in which the spots were almost as numerous as in the preceding year of their maximum. If the figures of column 3 be smoothed by the same process as was applied by Köppen to those of the years antecedent to 1860, it will be found that when laid down graphically the resulting curve corresponds to the inverted sun-spot curve more closely than in any antecedent period. Thus treated they are as follows:—

1875 ...	° —	1882 ...	+ 0°05
1876 ...	+ 0·21	1883 ...	− 0·27
1877 ...	+ 0·46	1884 ...	− 0·42
1878 ...	+ 0·53	1885 ...	− 0·25
1879 ...	+ 0·31	1886 ...	− 0·06
1880 ...	+ 0·26	1887 ...	+ 0·13
1881 ...	+ 0·26	1888 ...	+ 0·30

The amount of the oscillation during this cycle, appears to have been about 1° F., and is about the same as the average of the four cycles antecedent to 1860 as shown by Prof. Köppen's figures.

So far, then, from there being any reason to conclude that the meteorology of our planet since 1860 has ceased to display that direct influence of the solar cyclical variation that was so marked during the preceding half century, it appears that when ample evidence is forthcoming, and in that region where the sun's effect is most direct and intense, being at the same time least exposed to casual disturbance, that influence is displayed as strongly as ever, and it seems highly probable that the apparent discrepancy in tropical regions adverted to by Profs. Köppen and Schuster is to be in great part attributed to the imperfection of the data at their command.

Of all atmospheric conditions, the temperature is the most direct and immediate effect of the sun's heat, and therefore the most likely to afford indication of any variation in the intensity of that heat. But the element which, in this connection, more than any other, has engaged the attention of meteorologists, is the rainfall; and it is their general failure to obtain any distinct confirmation of the influence of the solar cycle in the annual variation of the rainfall, that perhaps, more than aught else, has tended to bring the whole hypothesis into discredit. But there are very good reasons why such inquiries should fail. No meteorological element is more subject to purely local variation, or is more largely influenced by what, in our ignorance, we must term the casual and irregular movements of the atmosphere; and yet most meteorologists have been content to summarize the results of single stations or of a few stations widely scattered, and to accept them as representative of enormous areas. How erroneous and misleading such assumptions must necessarily be, may be proved by anyone who will take the trouble to discuss the annual distribution of the rainfall of any European country well furnished with rain-gauge stations, or, still better, that of any extensive region, such as the United States or the Indian Empire; and he will find that, in any given year, there is no uniform excess or deficiency throughout, but while some districts, states, or provinces have received more than an average amount, others are in defect of the average.

If, for the sake of argument, we may assume that the influence of the sun-spot cycle has been conclusively established in the case of the temperature of the tropics, it follows necessarily that it must also hold good, though, perhaps, with diminished intensity, in every other kind of meteorological condition and in all parts of the world; but it may well be that the effect is so small, that in the higher latitudes it can only be detected on the average of many cycles, and when the simultaneous condition of the whole temperate zone, for instance, can be brought under review. For the present we may restrict our attention to the tropics.

That the rainfall of the Indian Empire, as a whole, exhibited no regular periodic variation during the 22 years 1864–85, I have shown elsewhere. But to this the Carnatic province, comprising the whole of the great plain in the south-east of the peninsula, affords an apparent somewhat striking exception, to which attention was drawn in NATURE, vol. xxxvi. p. 227. It was the rainfall of this tract, or rather of its chief city, Madras, that furnished one of the instances adduced by Mr. Lockyer, in the paper already referred to at the beginning of this article. From the results of the twenty-two years, comprising, therefore, two complete solar cycles, it appeared that, with a general average of only 35 inches for the whole province, a variation of no less than 14 inches took place between the years of maximum and minimum on the mean of the two cycles, even when the amounts actually recorded had been reduced to the terms of a harmonic series. Whether this remarkable oscillation will be found to hold good in future cycles, I will not venture to speculate; but it must be confessed that it seems extremely disproportionate to the temperature-oscillation, the magnitude of which rests on a far more extensive basis.

The Carnatic is the most southern lowland of the Indian peninsula, and therefore perhaps more favourably situated for displaying the direct effect of any variation of the solar heat than most other parts of the Empire, but it is by no means beyond the influence of the dry winds, the unseasonable incursion of which, I have elsewhere shown to characterize years of drought, as is also the case in Northern India. Ceylon, the southernmost portion of which lies within the equatorial zone of comparatively uninterrupted rainfall, is still more advantageously situated; but undoubtedly the region that holds out the best promise of deciding the question of rainfall periodicity, in the present case, is the Eastern Archipelago, in which the network of rain-gauge stations established by the late Dr. Bergsma, has already furnished data for thirteen years, and should therefore in the course of a

[1] The temperature of this year is taken from a preliminary report, received from India since this article was written. 1889 appears to have been a year of maximum temperature probably, as it was also the year of sun-spot minimum.

few years more afford materials sufficient, at all events, for a preliminary inquiry into the subject.

Meanwhile, it will be asked "Why,—instead of seeking for evidence in these by-ways of atmospheric action and reaction, where, at best, we can but hope to detect some remote and distorted reflection of the varying phases of the primary agent—why do we not appeal at once to the fountain head, and put the question to that very radiant heat which is the prime motor, the law of whose variations we are endeavouring to discover?" The obvious reply is that such a course is at present impracticable. So great and at the same time so variable is the atmospheric absorption, that the attempts that have been made to determine the calorific intensity of the sun's rays on the exterior of our atmosphere—the quantity termed, as if in mockery, the solar constant—give values differing so greatly according to the state of the atmosphere, that it is hopeless to detect in them with any certainty a variation so small as that indicated by the temperature oscillation. The only continuous actinometric register as yet carried out is that of M. Crova at Montpellier since 1883. I have not seen any reduction of his results for the determination of the so-called solar constant in different years, but the variations of the mean measured intensity on clear days from year to year are very irregular, and the mean of 1883 exceeds that of 1887 by not less than 12 per cent.

The more popular instrument, the sun thermometer, is still more unsatisfactory. While it is affected by obscure atmospheric changes equally with the actinometer, it is also influenced by every change in the surrounding objects, and by the wind, and the instrument itself is so uncertain that it is hard to find two that read alike. Many of them also are subject to a gradual deterioration that renders their earlier and later registers no longer comparable. Only in the few rare cases, where one and the same invariable instrument has been observed on the same spot, with the same *entourage*, during many years in succession, can any valid comparison be made between the values of successive years, and owing to the great fragility of these thermometers and the difficulty of adequately protecting them, such records are very rare. One such register was obtained by the late Prof. S. A. Hill at Allahabad during the ten years 1876-85, and the results were very carefully discussed by him, and, as far as possible, corrected for the varying absorption of the atmosphere due to the several elements, dry air, water vapour, and suspended dust. The mean result, in terms of the corrected insolation readings (*i.e.* the excess of the equilibrium temperature of the instrument over the temperature of the surrounding atmosphere), was thus found to be as follows in each year:—

1876	82°·8	1881	81°·8
1877	85·1	1882	79·6
1878	85·2	1883	78·6
1879	83·6	1884	80·4
1880	82·7	1885	83·1

The cyclical variation shown by these figures is identical in character with that of the air temperature of India as a whole, the maximum being in 1878, the minimum in 1883, which were respectively the years of minimum and maximum sun-spots, but, as might be expected, the oscillation is much greater. Even when the range is somewhat reduced by the smoothing process already applied to the temperature records, it amounts to five and a half times as much as that of the air temperature in the same period. From the nature of the observations, however, no great stress can be laid on this fact.

We now come to what may be termed the paradox of the whole problem. We have seen that both the air temperature and that of insolation seem to testify unmistakably to the fact that the sun's heat is greatest when his surface is least spotted, and *vice versâ*. But the evidence of the spectroscope points in a diametrically opposite direction, and so also do Meldrum's and Poey's statistics of the frequency of tropical cyclones, and, as far as it goes, the more dubious evidence of the rainfall, since, in all cases in which any appearance of a periodical variation has been detected, the rainfall is most abundant about or shortly after the epoch of maximum sun-spots, and least about the years of minimum, implying therefore increased evaporation and an increased movement of the atmosphere at the former epoch. The variation of the barometric pressure which has been detected in the Indo-Malayan region on the one hand, and in Western Siberia and Russia on the other, also seems to show that in years of maximum sun-spots a larger portion of the tropical atmosphere is transferred to high latitudes in the winter hemisphere, which again implies an increased disturbance of atmospheric equilibrium at that epoch between the tropics and the circumpolar zone, and therefore an increased intensity of the disturbing agent.

I must content myself with pointing out this discrepancy without attempting to explain it. It does not necessarily invalidate the evidence of either class of facts, since there may possibly be causes at work, which, when known, will be found to reconcile the apparent inconsistency; but it should assuredly act as a stimulus to our efforts to extend our basis of facts, in full confidence that all inconsistency will eventually disappear.

Before concluding this summary, I must briefly notice a very important investigation of Prof. Hann's, which, at first sight, might seem to negative the whole hypothesis of a cyclical variation of the solar heat; though such a conclusion would be by no means legitimate, and, in point of fact, is not put forward by its author. It was shown by Lamont that, when the diurnal double oscillation of the barometer is analyzed into its two chief constituents, viz. a wave of diurnal and two of semi-diurnal period, while the former varies very greatly in character and magnitude with the geographical conditions of a place, the latter is almost unaffected by these conditions, except that its amplitude decreases the higher the latitude. Assuming that this semi-diurnal tide is a direct effect of the sun's rays absorbed by the higher strata of the atmosphere, Dr. Hann examined the registers of the hourly observations of the barometer at Bombay, Batavia, and Vienna from 1847 to 1862, to see whether the amplitude of this element of the oscillation showed any appreciable increase and decrease corresponding to the phases of the sun-spot cycle. The result was that no such variation was to be detected. The fundamental assumption here made, that the magnitude of this double oscillation should vary with the quantity of heat absorbed by the atmosphere, was verified in a subsequent elaborate memoir, in which it was shown that at the time of perihelion, when our planet receives one-fifteenth more heat than at aphelion, the amplitude of the semi-diurnal tide is, on an average, as much as one-tenth greater—an exaggeration of the effect which Dr. Hann attributes to secondary meteorological actions. In any case, the result satisfies the logical conditions of the inquiry.

Now, accepting Dr. Hann's conclusion that "the heat absorbed by the atmosphere does not vary considerably [*erheblich*] with sun-spot frequency," it remains to inquire whether, taking as our standard the amount of the temperature variation deduced by Köppen, and substantiated by the later Indian registers, the effect to be expected is of such magnitude as would be readily rendered evident in the amplitude of the semi-diurnal wave of pressure. There is no reason to suppose that it would bear a greater ratio to the total pressure effect than does the temperature increment during the sun-spot cycle to the total effect of the sun's rays on the temperature of our atmosphere. What the temperature of our earth would be, in the absence of the sun, we do not indeed know. But we shall probably be well within reasonable limits if we assume that it would be at least 100° below the zero of Fahrenheit's scale, or 180° below the actual mean temperature of the tropics. Of course, under such circumstances, the temperature of the tropics would be no higher than that of the Poles. Making this assumption, then, the oscillation of the temperature during the course of the sun-spot cycle is only 1/180 of the total effect. Now the amplitude of the semi-diurnal element of the barometric oscillation at Batavia (the most equatorial of the three stations) is 1·896 mm., or 0·0746 of an inch, and the 1/180 part of this is 0·0004 inch—a quantity that would be quite inappreciable in an investigation of this kind, masked as it must be by the much larger irregular variations shown by the register. It does not seem, then, that Dr. Hann's negative results really affect the validity of the positive evidence already afforded by other meteorological phenomena, and need not discourage us in our endeavours to obtain an explanation of the paradox indicated above, which, to my mind, is the most interesting feature of the problem. HENRY F. BLANFORD.

THE QUESTION OF THE ASTEROIDS.

IT has already been noted that the editors of the *Berliner Jahrbuch* have decided only to issue ephemerides for certain of the minor planets (NATURE, vol. xliii. p. 111). The Bureau des Longitudes has endeavoured to remedy the inconvenience that arises from this decision by an extension of one of its departments. The general discussion that led the Bureau to adopt this course, and the importance of observations of these comparatively small bodies, are well expounded by M. F. Tisserand in the *Annuaire* for 1891.

Kepler recognized the continuity of the mean distances of the planets from the sun, when he said: "Infra Martem et Jovem novum interposui planetam." The publication of Bode's empirical law in 1772 confirmed Kepler's ideas, and fixed the distance of the hypothetical planet as 2·8 times the mean distance of the earth from our luminary. But the existence of such a planet appeared still more probable when the calculations of Lexell and Laplace had shown that the magnitude of the orbit of the planet Uranus, discovered by Sir William Herschel in 1781, might have been predicted with accuracy from this relation between planetary distances. At a Congress held at Gotha in 1796, it was proposed to search for the unknown body, and twenty-four astronomers each undertook the examination of an hour of the zodiac. The discovery of Ceres by Piazzi on January 1, 1801, almost before the association of observers had got fairly to work, is a matter of common knowledge. Gauss's calculations showed that the mean distance of this planet from the sun is 2·77, which corresponds with that indicated by Bode's law; hence the gap appeared to be filled, but by a body of very modest size, for the measures made by Herschel only assign it a diameter of about 155 miles.

Olbers's discovery of a second planet, Pallas, moving round the sun at the same mean distance as Ceres, gave the question another aspect. It was proved by Gauss that the two bodies may pass very near to each other at two points situated on the line of intersection of the planes of their orbits. This led Olbers to believe that the new planets were portions of a larger body broken up by some internal disturbance, and he accordingly suggested that other fragments might be found near the points of intersection of their orbits. The hypothesis was supported by Harding's discovery of Juno in 1804, near one extremity of the line of intersection, and the discovery of Vesta by himself in 1807, close to the other extremity.

It was not until 1845 that a fifth planet was discovered by Encke, and this was even smaller than the four that preceded it. After this date the discoveries became more frequent, and now the number has reached 308. But the magnitudes of the newly-discovered bodies are decreasing, for, whilst the first four have magnitudes comprised between 6 and 8, the two discovered by Encke are only of the 9th magnitude, and those now found are rarely brighter than the 13th magnitude.

The hypothesis advanced by Olbers as to the origin of the *asteroids*—a designation due to Herschel—has not found much support. Newcomb, from an investigation of the orbits of the first forty asteroids, found that their planes are far from having a common line of intersection. It may be suggested that this geometrical condition was fulfilled at a certain epoch, and that the perturbations caused by Jupiter and Saturn have caused the present distribution. Calculations show, however, that the required condition never existed; hence Olbers's hypothesis must be abandoned.

With regard to the width of the zone which contains the orbits of the asteroids, we find that (149) moves round the sun at the shortest mean distance, viz. 2·13, and that (279), the asteroid most removed from our luminary, has a mean distance of 4·26. The periods of revolution of these two bodies are, respectively, 3·11 and 8·81 years. It will therefore be seen that the asteroids revolve in orbits much greater and less than that assigned by Bode's law.

When the eccentricities of the orbits are considered, it is found that (132) may approach to a distance of 1·61 from the sun, whilst (175) may get so far away as 4·73 times the earth's mean distance. The asteroids are, therefore, contained in a wide zone, and the whole of their positions form a kind of ring having a radius a little more than three times the distance of the earth from the sun.

If the asteroids are arranged into groups, of which the eccentricities are comprised between limits differing by 0·05, the following result is obtained:—

23	between 0·00 and 0·05		51	between 0·20 and 0·25	
54	„ 0·05 „ 0·10		12	„ 0·25 „ 0·30	
74	„ 0·10 „ 0·15		9	„ 0·30 „ 0·35	
62	„ 0·15 „ 0·20		1	„ 0·35 „ 0·40	

The mean eccentricity, 0·15, is much higher than the corresponding mean of the major planets—viz. 0·86. It appears, therefore, that some notable differences must have existed in the conditions of formation.

But the difference is still more striking in the case of the inclinations of the orbits. The mean inclination is 8°, which is slightly greater than that of Mercury and that of the sun's equator. Of 293 asteroids, 17 have inclinations greater than 20°. These are given in the following table, and also their eccentricities and mean distances:—

	Mean distance.	Inclination.	Eccentricity.
(290)	2·31	21·55	0·05
(273)	2·40	20·24	0·16
(25)	2·40	21·35	0·25
(132)	2·60	25·0	0·38
(164)	2·63	24·25	0·35
(185)	2·74	23·17	0·13
(247)	2·74	25·7	0·24
(71)	2·76	23·19	0·17
(2)	2·77	34·44	0·24
(148)	2·77	25·21	0·18
(183)	2·80	26·33	0·35
(130)	3·11	22·57	0·21
(276)	3·12	21·58	0·09
(31)	3·15	26·27	0·22
(176)	3·19	22·31	0·16
(154)	3·20	20·59	0·08
(225)	3·40	20·45	0·27

It will be seen that, with two exceptions, the orbits greatly inclined to the ecliptic have also considerable eccentricity. The converse of this is not true, however, for great eccentricity does not appear necessarily to carry with it a high inclination. A consideration of these eccentricities and inclinations naturally leads one to ask whether they were the same at the time of the formation of the bodies whose orbits they represent, or whether the values have been increased under the influence of perturbations. On this point Leverrier made the following remark:—

"There exists a region between Jupiter and the sun, such that, if a small mass could be placed there, in an orbit slightly inclined to that of Jupiter, this little mass would be able to move out of its primitive orbit, and to attain a great inclination to the plane of the orbit of this planet and to that of Saturn. It is remarkable that this position is found at very nearly double the distance of the earth from the sun—that is to say, at the interior limit of the zone where the minor planets are found."

This fact is very interesting in itself, but it is not sufficent to explain the large inclinations that are found at the distances 2·75 and 3·15. M. Tisserand found some years ago that the region of instability is at a distance of only 1·83 from the sun. We must therefore conclude that the perturbations caused by Jupiter and Saturn are insufficient to explain the considerable values of the eccentricities and inclinations of a great number of asteroids; these values are never very small, and consequently the conditions under which Laplace's hypothetical nebulosity existed were not the same at the time of formation of the older planets as at the creation of the asteroids. The question is therefore a very interesting one from a cosmographical point of view, and the accumulation of new discoveries of asteroids is the only thing that will facilitate its solution.

The distribution of asteroids according to their mean distances from the sun, or, what amounts to the same thing, according to their mean diurnal motion, brings out some interesting facts. If the number of asteroids be tabulated having mean movements comprised between 540″ and 550″, 550″ and 560″, and so on, with increments of 10″ up to 1140″, an accumulation is evident about 640″, 780″, and 815″, which movements correspond to the mean distances 3·13, 2·75, and 2·67. Two gaps are seen about 600″ and 900″, or at the distances 3·27 and 2·50. The mean diurnal motion of Jupiter is 299″·12, or nearly 300″. It will therefore be seen that the gaps correspond to two regions where the mean motion of the planet would be exactly double or triple that of Jupiter. There are other gaps less definitely marked, where the relation between the two diurnal motions, instead of being equal to 2 or 3, is represented by the fractions $\frac{5}{3}$, $\frac{7}{3}$, $\frac{5}{2}$, $\frac{7}{2}$, &c. Kirkwood developed this relation in 1866, and generalized it by saying that the parts of the zone of the asteroids in which there exists a simple commensurable relation between the time of revolution of a minor planet and that of Jupiter are represented by gaps similar to the intervals that separate the different rings of Saturn. It should be remarked, however, that the intervals are not so well defined as in the case of Saturn's ring, inasmuch as after a gap the number of asteroids does not increase sharply, but little by little, until it reaches the normal value.

Can these gaps be explained by the theory of perturbations? The illustrious Gauss, writing to Bessel in 1812, remarked: "The mean motions of Jupiter and Pallas are in the relation expressed by $\frac{7}{18}$, a value that ought to be realized more and more exactly under the influence of the attraction of Jupiter, as is also the case in the equality of the motions of revolution and rotation of the moon."

Newcomb in his researches into Saturn's system, found that, in the case of exactly commensurable motions, the perturbations could not increase beyond a certain limit. But this consequence is by no means necessary, for, in all probability, there would only be more or less irregular oscillations, and equilibrium would then be restored. The work of Gyldén and M. Tisserand himself tend to the same conclusion. It is therefore probable that if the gaps had not existed in the beginning, the ulterior perturbations by Jupiter would not have been sufficient to produce them; they must have existed immediately after the formation of the asteroids. This is another reason why the question is of interest from a cosmographical point of view.

The asteroids situated at the outer limit of the ring are not devoid of interest. Some of them have orbits very similar to certain comets of short period; thus the orbit of (175) is very similar to that of Tempel's periodic comet (1867 II.), as is shown by the following comparison:—

	Mean distance.	Eccentricity.	Longitude of ascending node.	Inclination.
			°	°
(175)	3·51	0·35	23·6	3·8
Tempel's comet	3·49	0·41	72·4	10·8

Other asteroids are important because of their near approach to Jupiter. Of these, (279), discovered by Palisa about two years ago, is of special interest, for in 1912 it will be at the same distance from Jupiter as the earth is from the sun. At this time the attraction of Jupiter on the asteroid will be more than $\frac{1}{50}$ that of the sun; thus the calculations of the perturbations promise to be interesting and difficult, and from them the mass of Jupiter may be determined with considerable precision.

The asteroids which occur at the inner limit of the ring are also useful, especially if their orbits are very eccentric and they approach the earth within 0·7 its mean distance. In such cases their parallax may be very accurately determined by observations made at two stations some distance apart. From the values obtained, the parallax of the sun may be deduced, and this is one of the best methods at our disposal for the determination of an element of fundamental importance in astronomy.

We have said that it is impossible to entertain the idea that all the asteroids were formed by the rupture of a single planet. But groups of two planets may be formed having strikingly analogous elements. The most interesting is formed by the asteroids (37) and (66); their orbits are almost equal ellipses, situated very nearly in the same plane, and differ only in the orientation of their major axes. The analogy is apparent from the following comparison of elements:—

	Mean distance.	Eccentricity.	Longitude of ascending node.	Inclination.
			°	°
(37)	2·644	0·176	8·21	3·7
(66)	2·645	0·175	8·17	3·6
(106)	3·17	0·18	63·2	4·6
(245)	3·10	0·20	62·2	5·6
(218)	2·67	0·12	170·8	15·2
(246)	2·69	0·10	162·6	15·6
(84)	2·36	0·24	327·5	9·4
(2)	2·38	0·22	334·7	9·7

This quasi-identity of elements cannot be accidental, and it would not require many facts of this nature to throw new light upon the origin and formation of the bodies possessing them.

Enough has been said to prove that the discovery of asteroids has brought out some important facts. For this and other reasons, M. Tisserand thinks that the search for them should be continued. The calculations required to furnish elements and ephemerides are certainly formidable, but they could be divided among several scientific establishments. The Bureau des Longitudes is willing to play an onerous part. We hope that sufficient resources will be accorded to it to allow this extremely useful work to be carried on. RICHARD A. GREGORY.

SOLUTIONS.[1]

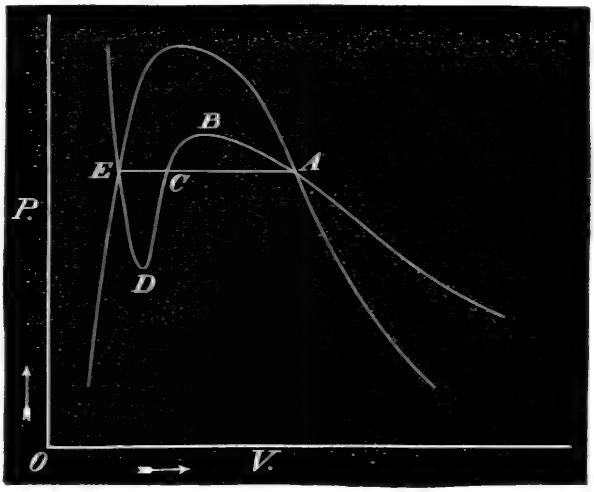
THE brilliant presidential address of Prof. Orme Masson at the Chemical Section of the Australasian Association for the Advancement of Science marks a distinct advance in our ideas of solution. The analogy between the behaviour of a liquid and its vapour in presence of each other and of a pair of solvents capable of mutual solution is so striking as to carry conviction. The resemblance of the liquid-vapour curve, with its apex at the critical point, to the solubility curve, with its apex at the critical solution point, appears to me to prove beyond cavil that the two phenomena are essentially of the same nature.

There are two other phenomena, which, it appears to me, are made clear by the ideas of Prof. Masson. The first of these has reference to supersaturated solutions. The curves (published in NATURE, February 12, p. 348) showing the analogy between liquid-gas and solution curves, are isobaric curves, or, more correctly, they represent the terminations of isobaric curves in the region of mixtures, where, on the one hand, a liquid exists in presence of its vapour, and, on the other, one solvent in presence of another (for both solvents play the part of dissolved substances, as well as of solvents). M. Alexéeff's data are not sufficient to permit of the construction of a curve representing a similar region mapped out by the termination of isothermal lines. But it is obvious that it would be possible to determine osmotic pressures of various mixtures by the freezing-point method, and so to construct isothermal curves for such mixtures of solvents. And there can be no reasonable doubt that, as the isobaric curves of liquid-gas and of solvent-solvent display so close an analogy, the isothermal curves would also closely resemble each other.

Granting, then, that this is the case, we may construct an imaginary isothermal curve on the model of the curve for alcohol published in the Phil. Trans. by Dr. Sydney Young and myself. Now, in one series of papers on the liquid-gas relations, we showed that with constant volume pressure is a linear function of temperature; and we were thus able to calculate approximately the pressures and volumes for any isothermal representing the continuous transition from the gaseous to the liquid state (see *Phil. Mag.*, 1887, vol. xxiii. p. 435). It would be interesting to ascertain whether, if concentration be kept constant, osmotic pressure would also show itself to be a linear function of temperature. But, this apart, it appears in the highest degree probable that there should also exist, in theory at least, a continuous transition from solvent to solvent, the representation of which would be a continuous curve. In such a case, on increasing the concentration of the solution by eliminating one solvent, the other solvent should not separate visibly, but the two should remain mixed until one solvent has been entirely removed. The accompanying diagram will make this clear. The sinuous curve ABCDE may represent either continuous change from gas to liquid along an isothermal on decrease of volume, or it may represent a similar continuous change from saturated solution to dissolved substance on increase of concentration.

Mr. Aitken's experiments on the cooling of air containing water-vapour have shown us that it is possible to realize a portion of the curve AB; the phenomenon of "boiling with bumping" constitutes a practical realization of a portion of the curve DE; and we may profitably inquire what conditions determine such unstable states with solvent and solvent.

Regarding the portion of the curve AB, I think that no reasonable doubt can be entertained. It precisely corresponds to the condition of supersaturation. In the liquid-gas curve, the volume is decreased at constant temperature without separation of liquid; in the solvent-solvent curve the concentration is increased without separation of the solvents. Dr. Nicol has shown that it is possible to dissolve dry sodium sulphate in a saturated solution of sodium sulphate to a very considerable extent without inducing crystallization; and here we have a realization of the unstable portion of the curve AB. In the gas-liquid curve pressure falls with formation of a shower of drops; in the solvent-solvent curve crystallization ensues, and the solvents separate. The phenomena are, however, not completely analogous; the complete analogy would be if the temperature were so low that the substance in the liquid-gas couple were to separate in the



solid, not in the liquid, state. This, so far as I am aware, has not been experimentally realized, but one sees no reason why it should not be possible.

I have some hesitation in offering speculations as to the state of matter at the portion of the continuous curve DE. It may be that it corresponds to a syrupy or viscous state. Cane-sugar at a moderate temperature dissolves water; indeed it is possible to obtain a solution of 1 per cent. of water in molten cane-sugar. And such a solution, if quickly cooled, remains a syrup. But it can be induced to crystallize by the presence of crystals. Thus, in such a mixture of sugar and water, a few grains of crystalline sugar cause the whole mass to crystallize, and water saturated with sugar and sugar separate into two layers. Here, again, a complete analogy fails us, for it is a solid which separates. As we know nothing of the osmotic pressure of a syrup, the analogy is a defective one; but it is probable that a dilute solution of sugar would pass continuously into a syrup of pure sugar by evaporation of the solvent, and analogy would lead to the supposition that the syrup coincides with the unstable state of the liquid. I would, therefore, offer the analogy between the syrupy and the supercooled states as a tentative one: it lacks foundation in both cases.

One point remains to be mentioned. I have for the

[1] "Some Suggestions regarding Solutions." By William Ramsay, Ph.D., F.R.S., Professor of Chemistry in University College, London. Read before the Royal Society on Thursday, March 5.

past nine months, in conjunction with Mr. Edgar Perman, been determining the adiabatic relations for liquid and gaseous ether: the rise of pressure and temperature when volume is decreased without escape of heat. It is obvious that similar relations are determinable for solutions, and probably with much greater facility. M. Alexéeff has made some measurements which might be utilized for this purpose; but they are far too few in number, and, moreover, the necessary data as regards osmotic pressure are wholly wanting. It would be possible, by a series of differential experiments, to ascertain the evolution of heat on increasing concentration, and so to arrive at a knowledge of the specific heats of the solution at constant osmotic pressure, corresponding to the idea of specific heats at constant pressure; and also of specific heats at constant concentration, corresponding to specific heats at constant volume. I do not know whether such researches would yield as accurate results as those we are at present carrying out, but they are at least well worthy of attention.

THE SCIENCE COLLECTIONS.

We give below the latest Parliamentary proceedings relating to this question:—

April 16.—Mr. Mundella asked the First Lord of the Treasury whether, in proposing to give the large plot of ground opposite the Normal School of Science at South Kensington for a Gallery of British Art, the Government had fully considered the difficulties of placing an art gallery under an independent management between portions of the Science School and the science collections of the Science and Art Department; and whether the Government would appoint a small committee to report on the matter.

Mr. W. H. Smith—In assigning as a site or a Gallery of British Art the plot of ground opposite the Royal College of Science, the Government have not overlooked the requirements, immediate or prospective, of the Science and Art Department, either in respect of its science collections or in respect of the additional accommodation required for the College of Science. For the science collections there will be available, part at once and part within a year or two, a continuous range of galleries, consisting of the present southern, western, and eastern galleries, and the cross gallery which is about to be constructed between the western and eastern galleries, thus affording more than the amount of accommodation which the committee of 1889 considered to be necessary. Over and above this accommodation, Government has at disposal more than three acres of vacant land facing the Imperial Institute, and considerable areas besides to the south of the present southern galleries. A portion of these vacant lands can be utilized for the extension of the College of Science and for future growth of the science collections. Additions to the College of Science must in any case take the form of a separate building, divided from the present building by Exhibition Road; and as access to the lands mentioned above from Exhibition Road will be secured by means of a corridor, the interposition of the Gallery of British Art need have no more serious effect than to increase by some 60 yards (which will be under cover) the distance between the two portions of the Science College. As the Art Gallery will be a distinct and separate building, the fact that it will be under different management need cause no greater difficulty than does the fact that the Natural History Museum is under a different management from that of the adjoining science galleries.

April 20.—Sir H. Roscoe asked the First Lord of the Treasury whether he would have sketch plans prepared and placed in the Library, showing the relative positions of the existing science schools and science museums, the proposed buildings for the same purposes, and the proposed gallery of modern art, on the land at South Kensington.

The Chancellor of the Exchequer—As no definite scheme has yet been framed, it would be impossible to show exactly the appropriation of land now vacant, but I can promise the hon. member a ground plan, showing what land is available for further science buildings.

Sir H. Roscoe asked the right hon. gentleman whether he would give an assurance that the final step would not be taken in the appropriation of the land until after hon. members had had some opportunity of inspecting the plan.

The Chancellor of the Exchequer—The position of the matter is this. The House knows of the very generous offer that has been made of £80,000 to erect a building. A great deal of necessary and unavoidable delay has taken place, and I think it would be unwise to risk the failure of that generous gift by any further delay. Therefore, I am unwilling to pledge myself to any further delay, but I can assure my hon. friend that every security will be given to gentlemen interested in science that their wishes shall be met as far as possible.

There are a few more questions which might be asked, such as the following:—

1. Did the generous donor of the £80,000 know when the site was chosen that its employment for an art gallery interfered with its contemplated and natural use in relation to the buildings adjacent to it?

2. Are there no other sites, including one in Kensington Gardens, which would equally satisfy the donor, and not be liable to the objections so widely raised against the proposed one?

3. Has Mr. Goschen from first to last made any inquiry whatever as to whether there were any objections to the proposed employment of the site?

4. Has the Science and Art Department been asked its opinion, or have the professors of the College been consulted?

It would seem, indeed, that here we have another instance of the way in which Government arrangements in relation to science are made without knowledge.

NOTES.

The Council of the British Association for the Advancement of Science has resolved to nominate as President of the Association for next year Mr. Archibald Geikie, F.R.S., Director-General of the Geological Survey. The meeting will be held in Edinburgh.

On Tuesday next (April 28) Dr. E. E. Klein, F.R.S., will begin at the Royal Institution a course of three lectures on Bacteria: their nature and functions (the Tyndall Lectures); and Mr. H. Graham Harris will on Saturday (May 9) begin a course of three lectures on the artificial production of cold.

The Royal Naval Exhibition will be opened by the Prince of Wales, on behalf of the Queen, on Saturday, May 2.

The following cutting from the *Sydney Morning Herald* —date not stated—has been sent to us for publication:—"The barque *Killarney* had both a stormy and an extraordinary passage, in one portion of which she stood a good chance of being placed on the list of missing vessels. At 9 p.m. on October 15, 1890, when the barque was 50 miles east of Kent's Group, there was a sudden shift of wind during a heavy thunderstorm. In the midst of a heavy clap a bulky mass was heard to fall into the sea about 200 yards from the vessel. The roar of it coming through the air was quite distinct from that of the thunder, and spray was thrown fully 40 feet high on its reaching the water. The falling mass is believed to have been a meteor."

The Wolsingham Observatory Circular No. 31 states that a new variable star was found on March 2 at 4h. 26m. 4s. +65° 53'

('55). The observation has been confirmed at Harvard Observatory.

IN his report on educational work for the year ending February 23, 1891, the Principal of the Mason Science College records that in every department of the College (with the exception of one department which has laboured under special disadvantages through the death of an honoured professor) there is an increase in the number of students, a growing earnestness of purpose, and a creditable record of work accomplished. The analysis of attendances of students shows that the number of students attending in one department only is steadily decreasing, while the numbers attending in two or more departments, and preparing for University examinations, are increasing with equal persistence.

Science of April 3 gives a full account of the somewhat elaborate preparations which have been made for the work of the fourth session at the Marine Biological Laboratory of Wood's Holl, Massachusetts. The laboratory for investigators will be open from June 1 to August 29. It will be fully equipped with aquaria, glass ware, reagents, &c., but microscopes and microtomes will not be provided. In this department there are fourteen private laboratories supplied with aquaria, running water, &c., for the exclusive use of investigators, who are invited to carry on their researches here free of charge. Those who are prepared to begin original work, but require supervision, special suggestions, criticism, or extended instruction in technique, may occupy tables in the general laboratory for investigators, paying for the privilege a fee of fifty dollars. The number of such tables is limited to ten.

M. ROTY, Member of the Institute, has designed for the French Association for the Advancement of Science a new medal, a representation of which is given in *La Nature*. On the obverse are two figures, representing France and Science. France, dejected by the misfortunes of 1870-71, is being encouraged by Science, which points to the symbols of sunrise and recovered prosperity. On the reverse is a fine female figure, seated and reading, described by *La Nature* as "science, poetry, idealized thought."

IN a speech delivered the other day at Kimberley, Mr. Cecil Rhodes announced that he had received "enormous subscriptions" for the establishment of a teaching University in Cape Colony.

THE St. Petersburg correspondent of the *Times* says the Russians seem determined, if possible, to retrieve the failure of Captain Grombtchefsky in trying to get through Afghan Kafiristan. A telegram from Samarkand states that another captain, by name Bortchefsky, is about to start from Southern Bokhara at the head of a scientific expedition to the Pamir, and will endeavour to penetrate Kafiristan and reach the frontier of China.

THE *Kew Bulletin* for April opens with a note on Persian tobacco or tombak, establishing the botanical identity of the tobacco plant cultivated in Persia. Dr. J. Hornsey Casson lately sent to the Kew Gardens, from the Shiraz district, some specimens of fruit, flower, and seed. The material thus obtained "required a good deal of soaking and manipulation before it could be brought into a form in which it could be compared botanically. This, however, having been done, the conclusion was incontestable that the plant of the Shiraz *tunbaku* was nothing more, as had indeed been expected, than ordinary *Nicotiana Tabacum*." The same number contains some letters by the late George Woodruff and Harold Edmund Bartlett, who were sent from Kew in 1889 to take charge of the botanical stations established by the Royal Niger Company in the interior of the Niger Protectorate. The letters were written to former fellow-gardeners at Kew, and, as the *Bulletin* says, are "interesting as showing the type of men that the Royal Gardens turn out; the plucky way in which they face their difficulties, their loyalty to their employers, and the kindly feeling they entertain towards Kew." The gardeners at Kew are specially trained to fit them for such appointments as those accepted by Mr. Woodruff and Mr. Bartlett. "The Royal Gardens have, in fact, always been an advanced technical school. Each gardener is admitted for a two years' course, during which he has the opportunity of seeing every kind of cultivation carried on in the establishment, and, in addition, obtains systematic instruction in scientific subjects connected with his profession. The best men receive appointments as opportunity offers, and they are now to be found in every part of the world." In the April number there are also sections devoted to Aden Barilla, and to "Assam rubber for West Africa."

WRITING to the New York *Nation* from Keneh, Upper Egypt, on March 17, Mr. W. H. Goodyear describes an important and most interesting discovery made by Mr. Petrie at Maydoom. Mr. Petrie has there unearthed "the oldest known Egyptian temple and the only Pyramid temple ever found." Apart from the "Temple of the Sphinx" at Ghizeh, this building is also "the only temple of the Old Empire so far known." It was buried under about forty feet of rubbish. It lies directly at the centre of the eastern base of the Pyramid, on the side facing which it has two round-topped obelisks. "Obelisks and temple chambers so far entered," says Mr. Goodyear, "have the plain undecorated style of the Old Empire, as shown by the Temple of the Sphinx, but hieratic inscriptions in black paint found within fix the name of Seneferoo as builder, and confirm the supposition to this effect hitherto based on the fact that tombs near the Pyramid contain his cartouche. Seneferoo is the king connecting the third and four dynasties, and variously placed in either. According to computations of Mariette and Brugsch, the antiquity will be about 4000 B.C., or earlier." On Tuesday, March 10, Mr. Petrie's workmen reached a platform which appeared to be a causeway terminating with two obelisks at the base of the Pyramid. "In the forenoon of Wednesday," continues Mr. Goodyear, "a workman came to say that an opening had been found under the platform on the side next the Pyramid. This proved to be the top of a doorway choked by detritus, through which Mr. Petrie crawled into an interior of three chambers and discovered the inscriptions mentioned. I had the pleasure of following him. Mr. Petrie thought the apartments had not been previously entered for about three thousand years—that is to say, that the rubbish fallen from the Pyramid had choked the entrance about three thousand years after construction. A friend who was with me noticed on the floor some dried wisps of papyrus, a plant now extinct in Egypt. The chambers thus far found are so filled that one cannot stand erect in them, and a door at the end of the third chamber is blocked by large stones. Over all lies an enormous mass of detritus, whose removal by Arab diggers is now in progress. I had the pleasure next day of carrying the news of Mr. Petrie's find to the gentlemen of the Egypt Exploration Fund at Beni-Hassan, and of witnessing their unaffected delight over it."

PRINCE ALBERT of Monaco has begun the publication of the results of his scientific voyages in his yacht; they are intended to comprise navigation, hydrography, oceanographic physics, and zoology. The zoological portion will be published under the editorship of M. J. Guerne, assisted by numerous specialists. The first part (published by Masson, Paris), on the marine Mollusca of the Azores, by M. Dautzenberg, with four large plates, two of them coloured, has already been issued.

THE "Year-book of Australia for 1891" contains an article upon scientific progress in Australia during 1890. It is compiled from information supplied by the various Australian scientific associations. The compiler cannot be charged with having formed an extravagant estimate of the value of the work done. It may not, he says, have been "so fruitful in important results as have similar labours in older countries; nevertheless, it has not been without interest or utility."

So far as is at present known, the first person who kept a record of the weather was Walter Merle. He did so for the years 1337 to 1344, and his manuscript on the original vellum still exists. Thanks to the courtesy of the officials of the Bodleian Library, Mr. G. J. Symons has had this manuscript photographed, and reproductions of the ten large photographs, with a full translation (the original is in contracted Latin), some particulars as to Merle, and a list of the subscribers, are to be given in a handsomely printed volume. Mr. Symons wishes to call attention to the fact that no one will be able to obtain a copy who does not apply for one before May 1. Except ten copies reserved for subscribers too distant to apply before tha date, not a single copy in excess of those subscribed for will be printed.

In our issue of the 9th instant, p. 548, we published a formula for wind velocities as observed with Robinson's cup anemometer, which is likely to be practically used by engineers as well as meteorologists. The formula, as quoted by the *American Meteorological Journal* for February, contains an erroneous interpolation of v. The correct formula is

$$\log V = 0{\cdot}509 + 0{\cdot}9012 \log v;$$

where V is the wind's velocity, v the velocity of the cups in miles per hour. It should be noted that the constants of the formula apply only to anemometers of the pattern specified, the cups of which are 4 inches in diameter, and the arms, each 6·72 inches, measured from the axis to the centres of the cups. Prof. Marvin says nothing as to the weight of this gear set in motion by the wind, which ought also to be a constant quantity.

The American Hydrographic Office reports, in the Pilot Chart of the North Atlantic Ocean for April, that the weather was very abnormal over the Atlantic during March, the usual conditions of wind and pressure having been entirely reversed for a large part of the month. Unusually low barometric pressure and cyclonic wind circulation have prevailed very generally to the southward of the Azores, and between the Azores and Bermuda, whilst anticyclones have been very frequent and persistent along the 50th parallel. The result has therefore been that the north-east trades have actually been reversed for some time, and the westerly winds that usually prevail along the steamer routes have been often replaced by persistent easterly winds. Fog has been reported in great quantities on the Grand Banks and the coast to the westward as far south as Hatteras.

In the Proceedings of the Royal Prussian Meteorological Institute, vol. i. No. 2 (Berlin, 1890), Dr. Sprung gives an account of an elaborate series of experiments to show the effect of difference of exposure upon the readings of thermometers. The experiments were made at Gross-Lichterfelde, near Berlin, by assistants of the Prussian Meteorological Office, observations being taken six times daily with various screens, and with the "sling" thermometer, from June 1886 to March 1887 inclusive, under very favourable conditions. The readings at each of the hours of observation are tabulated for two representative months, together with the state of the sky and of the wind. The principal results likely to be of interest to English observers are: (1) that exposure outside windows, both with and without screens, if properly protected from radiation, gives readings which agree well together; (2) the readings of the various screens usually employed, which are necessarily exposed to the sun, give results which do not agree so well together; (3) on sunny summer days, the mean temperature in the ordinary screens is about 0°·7 higher than that obtained from the window exposure; (4) the range shown by the registering thermometers is considerably greater in the screens than that got from the window exposure; (5) the English (Stevenson) screen generally gives better results than the others, but the humidity observations in the evening are too high, as compared with those obtained by the window exposure and the French screen.

Mr. J. W. Fewkes calls attention in the *American Naturalist* to a peculiar gesture often used by the Zuñi and Navajo Indians. In indicating that a person or thing is far away, or where an event has happened or a person is at the time of speaking, these Indians, instead of turning the head that way or pointing with the finger, raise the head and project the lower jaw in the direction which they wish to indicate. Mr. Fewkes noticed that the same gesture was used several times for exactly the same purpose by a New England Indian with whom he was talking; and he now wishes to find out whether this method of suggesting distance or direction is characteristic of the American aborigines generally. "Those whom I have consulted," he says, "tell me that it is. If it is, we may well wonder why such an insignificant habit should be so tenacious in a tribe so long in contact with the whites, and so much affected by their civilization in much more important particulars, as the Passamaquoddies. It is conceivable that gestures like this, certainly spontaneous and in some respects involuntary, may furnish data of ethnological value."

A new illustrated volume intended for amateur astronomers, and written by Mr. Denning, of Bristol, will shortly be published by Messrs. Taylor and Francis, under the title of "Telescopic Work for Starlight Evenings." The book will include chapters on telescopes and observational matters, in addition to descriptions of the principal celestial objects.

A very useful little book, published by Messrs. Longmans, Green, and Co., and compiled and edited by the Rev. Isaac Warren, M.A., consists of a series of examination-papers in Euclid and trigonometry that have been proposed to students of the University of Dublin. There are also papers in plane trigonometry that have been set in the examinations for the National School teachers by the Board of National Education. In each case the answers are appended, and, in the latter, hints are inserted for their solution. To students of the University who are preparing for these examinations, it is needless to say that they will find the work most useful; while for others, the papers will afford a capital chance for testing their knowledge on the subjects in question.

A paper upon the chlorobromides of silicon is contributed by M. Besson to the current number of the *Comptes rendus*. There are three theoretically possible compounds of this nature derived from silicon tetrachloride, $SiCl_4$. They are $SiCl_3Br$, $SiCl_2Br_2$, and $SiClBr_3$ respectively. The two first of these were prepared some time ago by M. Friedel, by the action of bromine upon silicon chloroform, $SiHCl_3$, at 100° in sealed tubes. The first compound results from the substitution of bromine for hydrogen according to the equation $SiHCl_3 + 2Br = SiCl_3Br + HBr$. The second compound is then formed by the action of this hydrobromic acid upon the first compound, $SiCl_3Br + HBr = SiCl_2Br_2 + HCl$. M. Besson now describes how all three compounds, including the hitherto unisolated $SiClBr_3$, may be obtained by the action of hydrobromic acid gas upon silicon tetrachloride. Dry hydrobromic acid is without action at the ordinary temperature, but partial substitution of bromine for chlorine occurs at high temperatures, owing to the difference in the relative heats of formation of hydrochloric and hydrobromic acids, and the partial dissociation of the latter. The dry gas is passed, saturated with vapour of silicon tetrachloride, through a porcelain tube heated to redness. The product obtained in the receiver contains considerable quantities of unaltered tetrachloride, but, by repeating the process a few times, a product is obtained consisting largely of the first chlorobromide, $SiCl_3Br$. It is comparatively easy to separate, by fractional distillation, this compound, which boils at 80°, from the remaining tetrachloride, and the $SiCl_3Br$ thus obtained in a fairly pure state serves for the preparation of the second and third chlorobromides by passing its vapour, instead of the tetrachloride, together with hydrobromic acid through the hot porcelain tube. The second chlorobromide, $SiCl_2Br_2$, has been said to boil about 100°. M. Besson finds that his carefully purified specimen boils at 103°–105°. It was found impossible to separate the third compound, $SiClBr_3$, from this second one by fractional distillation, but by taking advantage of the fact that the second chlorobromide cannot be solidified at − 60°, while the third compound solidifies at − 39°, and afterwards distilling the solid obtained, the third compound, $SiClBr_3$, has been isolated as a liquid boiling at 126°–128°. This substance exhibits the property of superfusion in a very high degree; it may be cooled as low as − 50° without solidification ensuing, provided the liquid be maintained perfectly still. On agitation to even the slightest extent, however, it suddenly solidifies, the thermometer rising instantly to − 39°. All three chlorobromides combine directly with gaseous ammonia to form additive compounds, white amorphous solid bodies decomposed by water. In case of the first chlorobromide, $SiCl_3Br$, a similar compound has been obtained under pressure in a Cailletet tube with phosphoretted hydrogen, PH_3. The combination occurs at 0° under a pressure of 25 atmospheres, or at − 22° under 17 atmospheres, all the liquid being then transformed into a white solid, which persists when the pressure is removed, but which is again dissociated upon slightly warming the tube.

THE additions to the Zoological Society's Gardens during the past week include a Lesser Ourang-Outang (*Simia morio* ♂) from Sarawak, Borneo, presented by Commander Ernest Rason, R.N.; two Suricates (*Suricata tetradactyla*) from South Africa, presented by Mr. J. W. Munt; two Azara's Opossums (*Didelphys azaræ* ♂ ♀) from La Plata, presented by Mr. Edward C. Hawe; a Lion (*Felis leo* ♀), bred in Holland; a Nylghaie (*Boselaphus tragocamelus* ♂), bred in France, purchased; a Grey Parrot (*Psittacus erithacus*) from West Africa, deposited.

THE PRESENT METHODS OF TEACHING CHEMISTRY.[1]

IN their second Report, which was presented at the Newcastle-on-Tyne meeting, the Committee gave an account in some detail of the general lines which, in their opinion, an elementary course of instruction in physical science might most profitably follow. During the past year the Committee have been principally engaged in collecting and comparing the regulations, with respect to chemistry, which are issued by the more important of the examining bodies in the kingdom, in order to discover how far their requirements are in harmony with such a course of instruction as that suggested by the Committee. Since the information which has been collected is of general interest, the greater part of it is here printed. It consists of a brief outline of the noteworthy features in the regulations of the various Examination Boards, and, wherever it appeared necessary, of recent examination papers. The examinations about which information is now given are as follows:—

Oxford and Cambridge Schools Examination Board.
University of Cambridge Local Examinations.
University of Edinburgh Local Examinations.
University of Glasgow Local Examinations.
University of London Matriculation.
University of Durham Certificate for Proficiency in General Education.
Victoria University Preliminary Examination.
College of Preceptors—Professional Preliminary Examination.
Science and Art Department Examination in Chemistry.
Intermediate Education Board for Ireland.
Civil Service of India.
India Forest Service.
Royal Military Academy, Woolwich.
Cadetships, Royal Military College, Sandhurst.
Engineer Students, H.M. Dockyards.

With respect to the regulations which relate to these examinations, the Committee consider it desirable to direct especial attention to the following points.

It is of great importance that natural science should be sufficently represented on the Board which issues the regulations and is responsible for the proper conduct of the examination. It is remarkable that, although chemistry is an important subject in the Oxford and Cambridge Schools Examination, no representative of this science is appointed by either University to act on the Examination Board, whilst Oxford does not appoint a representative of any one branch of natural science.

The Committee note with satisfaction that in these examinations, most of which are held to test proficiency in general education, chemistry is generally included, in addition to one or more branches of experimental physics, and that in many cases the examination is in part a practical one. An important exception to this statement is found in the case of the University of Durham, which, although it grants a certificate of proficiency in general education, does not include among the subjects of this examination either chemistry or any branch of experimental science. Science is represented only by elementary mechanics, and even this is an optional and not a compulsory subject.

As regards the status occupied by chemistry and experimental physics in public examinations, the position of these subjects is still frequently lower than that of the other principal subjects of examination, and much yet remains to be done to secure the adequate recognition of the educational value of natural science. Attention may here be drawn to the position assigned to physical science by the Intermediate Education Board for Ireland, upon whom devolves the examination of most of the Irish public schools. According to the regulations at present enforced by this Board, natural philosophy and chemistry appear as optional subjects, each having a relative value represented by 500 marks, the value of Greek and Latin being assessed at 1200 marks each. It is to be hoped that the Commissioners may, before long, see their way to introduce elementary physical science as a compulsory subject of these examinations, and to increase the marks assigned to it beyond the present number of 500, which is less than one-half of that awarded to Greek or Latin (1200).

Another very anomalous case is that of one of the Civil Service examinations, viz. the examination for engineer students in H.M. Dockyards. In this examination, "very elementary physics and chemistry" are included as a single subject, to which is allotted 100 marks out of a total number of 1950! In the profession for which this is an entrance examination, applicable to boys who are about to leave a public school, not only is the possession of a scientific habit of mind of the highest moment, but a considerable knowledge of physics and chemistry is indispensable.

The Committee are strongly of opinion that some attempt should be made to remedy a conspicuous deficiency in nearly all existing examinational regulations. It is virtually impossible to ascertain, in the course of a single short examination, especially when the number of candidates is large, whether sufficient time has been devoted to the study of the elements of physical science to make it of permanent advantage to the student; neither is it possible to determine whether the character of the instruction has been in every respect satisfactory. Periodical inspection of the teaching by properly qualified inspectors, such as is now practised to some extent by more than one Government department, would seem to constitute the best method of dealing with this defect, the reports of the inspectors, as well as the students' own record of work testified to by the teacher, being taken into account in awarding prizes, certificates, and grants, in addition to the results of an examination.

With respect to the schedules and examination papers, typical specimens of which are here printed, it will be seen that for the most part they do not aim at an educational training of the kind suggested in the Committee's last report. Although nearly all the examinations included are intended to maintain a high stand-

[1] Third Report of the B.A. Committee, consisting of Prof. H. E. Armstrong, Prof. W. R. Dunstan (Secretary), Dr. J. H. Gladstone, Mr. A. G. Vernon Harcourt, Prof. H. McLeod, Prof. Meldola, Mr. Pattison Muir, Sir Henry E. Roscoe, Dr. W. J. Russell (Chairman), Mr. W. A. Shenstone, Prof. Smithells, and Mr. Stallard, appointed for the purpose of inquiring into and reporting upon the Present Methods of Teaching Chemistry. (Drawn up by Prof. Dunstan.) To which is appended a Paper, by Prof. Armstrong, on "Exercises in Elementary Experimental Science."

ard in general education, yet, as a rule, the schedule of work proposed and the questions set in the papers are more suitable for those who wish to make a special and detailed study of chemistry as a science. Insufficient attention is paid to problems, like those suggested in the Committee's last report, designed to develop the power of accurate observation and correct inference; few of the questions asked are adapted to test the mental power of students, which should have been strengthened and trained by the experimental study of physics and chemistry. The great majority of the questions asked involve an enumeration of the properties and modes of preparation of different chemical substances; but this by itself is a wholly unsatisfactory method of ascertaining whether a student has derived benefit from experimental work. The mere writing out by the student of methods of preparation of individual substances is no proof that he has learned chemistry. The Committee are of opinion that it is not advisable to ask young students to give purely formal definitions of chemical terms. A glance at the examination questions appended will show that definitions of such terms as *atomic weight*, *molecular weight*, *water of crystallization*, *acid*, *base*, *salt*, are often demanded. Such questions encourage many students to learn by rote certain forms of words without attempting to grasp the facts and generalizations which those words summarize. Moreover, as many, if not most, of the terms used in chemistry cannot be defined, the demand for definitions of these terms by examiners leads to a pernicious and unscientific way both of teaching and learning, by which an apparent accuracy in the use of phrases is substituted for a real acquaintance with facts and principles. Again, too much attention is often devoted to calculations which, while they furnish useful exercises, do not necessitate any special scientific knowledge. Another noteworthy feature of these examination schedules and papers is the very general exclusion of any reference to organic substances. There appears to be no reason, even in elementary examinations, why the questions should be exclusively confined to inorganic materials. Moreover, elementary organic chemistry can be made the basis of excellent training in scientific method, especially if the teaching does not follow the formal order or aim at the completeness which are usual in text-books, most of which are written for those who are studying chemistry as a special subject, and not chiefly for the sake of the educational benefit which may be derived from it. In general elementary teaching at any rate it is unnecessary even to make the conventional distinction between inorganic and organic chemistry.

The foregoing remarks apply not only to school examinations, but also to the various Civil Service examinations, where it is of the highest importance that candidates should have received a sound scientific training. Most of those selected will afterwards fill positions in which the scientific method of dealing with the various problems which will constantly be presented for solution cannot fail to be of the highest value.

It may perhaps be thought that a great deal of what has been said in criticism of the present examinational demands in physical science, might more properly have been urged against the teaching. But since the first report of this Committee was issued, in which attention was drawn to the defective character of much of the elementary teaching, it has been repeatedly represented by teachers in schools of every grade that the character of their instruction is necessarily governed by the requirements of examiners, and that if modifications were made by Examining Boards in the present regulations it would be possible at once to make the corresponding changes in the methods of teaching.

The obvious conclusion is that the necessary reforms can only be brought about by the active co-operation of examiners and teachers.

[Here follow, in the Report, a selection of examination papers.]

Appendix.

Exercises illustrative of an Elementary Course of Instruction in Experimental Science. By Prof. Armstrong.

The scheme put forward in the report presented last year by the Committee sufficed to indicate the kind of instruction likely to inculcate habits of observing correctly, of reasoning from observation, and of setting new questions and obtaining answers thereto by experiment and observation: habits which it is now generally admitted are of great consequence in the struggle for existence, and which cannot be acquired except through training in the methods of experimental science. Nevertheless, it has been felt that detailed directions how to proceed were necessary for the use of the less experienced teachers, and that even those who fully sympathize with the proposals already made would welcome the more complete display of the system. I have therefore obtained the permission of the Committee to append the following suggestions to their report, in amplification of certain parts of the scheme already published.

It is obviously impossible to sketch more than a small portion of a complete programme of instruction; the portion now offered is that appropriate to the earliest stage in which quantitative studies can be engaged in: its study can be commenced by children of fair intelligence when nine or ten years old. It is an essential feature of the scheme that it has reference to common things, the object being to lead children to engage in the rational study of the objects which are daily brought under their notice.

Time to be devoted to Experimental Studies and Mode of Teaching.—Frequently during the past year the question has been put to me, "How much time is to be devoted to such science teaching?" and complaint has been made of the difficulty of dealing with large classes of children, of keeping them employed, and of providing the requisite space and appliances.

The question as to time will ever continue to be put until the fundamental fallacy which hitherto has retarded the progress of experimental teaching in schools is discarded, viz. that sufficient training in a scientific subject can be imparted in the course of a term or two. This undoubtedly is the view entertained in the majority of schools—girls' schools in particular. It is well known, for example, that of the many hundred students who each year present themselves at the London University Matriculation Examination, the vast majority have had but a *few months' coaching* in chemistry, mechanics, or physics, although they have had lessons in arithmetic and like subjects during the *whole period* of their school career. It was long a superstition that to pass in chemistry all that was necessary was to have read some one of the small text-books, and a very large proportion of matriculants have doubtless had only such preparation. The fact is that our schools hitherto have been all but entirely in the hands of those who have had a purely classical or mathematical training, and who have gained their knowledge by reading; teachers thus trained cannot realize that the useful effect of science teaching is only attained when the instruction is carried out on entirely different lines; they cannot realize that *accurate experimenting* is the essential feature in the system; that knowledge gained by mere reading is and can be of little use, as in acquiring it the mental faculties which it is desired to exercise never become trained. It must be recognized by all who have charge of schools that, in order to secure the due development of those faculties which science teaching alone can affect, the instruction must be imparted *from the very beginning and during the entire period of the school career.*

If this be done, many of the difficulties hitherto encountered may disappear. Probably it will be found advantageous, at least in the earlier stages, rather than disadvantageous, to devote but a short time during any one lesson to actual experimental work. There is no doubt that far too much is usually attempted; that too many facts are brought under the student's notice in the course of the lesson, the result being a blurred mental picture destitute of sharp outlines. After considerable experience I am satisfied that it is difficult to proceed too gradually—it may almost be said too slowly.

The following two sets of instructions are given by way of illustration; it is not pretended that they are complete, nor is it suggested that the exercises should be worked through exactly in the order in which they are stated, or completed by all pupils; the teacher must determine which are suitable for the particular set under instruction.

Studies of Water and Common Liquids.

1. Make every effort to elicit from the pupils by question and answer all that *they* have noticed with regard to water. Induce them to take advantage of any opportunities the neighbourhood affords of observing water and its effects. Let them ascertain the area covered by the school-house roof and the amount of water which falls on it when it rains; institute systematic observations of rainfall, and embody the data in arithmetical exercises. Call attention to the different yearly rainfall of different parts of the country, and point out the influence of hills

and mountains: let outline maps be coloured, so as to indicate the different rainfall of different districts.

2. Call attention to the geographical distribution of water, &c.; also to the work which it does in Nature (cf. Geikie's "Physical Geography," Huxley's "Physiography," &c.), illustrating this part of the subject, especially at an inland school, by lantern photographic slides of ships, sea-coasts, Niagara Falls, &c.

3. Call attention to the disappearance of water, *i.e.* the drying up of rain, the drying of clothes, &c., and lead the pupils to notice that this takes place most quickly in hot weather and in warm places; then let them pour water into a clock glass placed either over a saucepan in which water is boiled by a gas-burner (or petroleum or spirit-lamp, if gas be not available), or in a small gas cooking-stove; they will see that the water evaporates, leaving a certain amount of *residue*. [At this stage experiment on the extent to which water evaporates out of doors and indoors under different conditions and at different times of the year by exposing water in weighed glass (crystallizing) dishes about 4 inches in diameter, and weighing at intervals. Also call attention to the fact that in certain states of the weather things become damp, and that moisture is sometimes deposited on the windows in cold weather; then let the condensation be noted of a liquid indistinguishable from water, which occurs, for instance, when a closed flask filled with water and ice is exposed in a room. Let some seaweed inclosed in a muslin bag be hung up out of doors where it cannot be wetted by rain, and have it weighed daily. At the same time have the temperature, direction of the wind, and character of the weather noted. Later on have the dry and wet bulb thermometer read daily. Have the changes in weight of the seaweed and the dry and wet bulb thermometer readings represented by curves. Lead the pupils to contrast and discuss the results.] The experiment should then be repeated with a known quantity of water and a weighed glass dish, so as to determine the amount of residue; the character of the residue should be noticed. Discuss the origin of the water, and point out whence the residual matter *may* have come. Next, if a well water was taken, let a local river or pond water be examined in a similar way, then rain water, and, if possible, sea water.

4. Let an ordinary 2-ounce narrow-mouth stoppered bottle, having a nick filed down the stopper, be filled with each of the waters and weighed, and let the operation be repeated several times with each water, so that the *experimental error* may be ascertained; it will be found that the different waters, sea-water excepted, have practically the same *density*. At this stage arithmetical exercises relating to the weight of known bulks, and *vice versâ*, of water, the quantities of dissolved solids present in given bulks of various waters, &c., may advantageously be set; these should be solved practically by actual measurement in as many cases as possible.

5. Next ask, "But what becomes of the water when driven off by heat?" If it have not been noticed that water collects (condenses) on some object near at hand, let a cold object be held over boiling water, then let water be boiled in a glass flask connected with a glass condenser. Afterwards have water distilled in larger quantity from a tin (2-gallon) can. The density of the distilled water should then be determined, and its behaviour on evaporation. Data would thus be accumulated, rendering it possible to explain the drying up of water under ordinary conditions, the origin of rain, the differences between waters from various sources, and the method of separating water from the associated foreign matters will have been brought home to the minds of the pupils.

6. As the water is heated to boiling in the flask, if attention be paid to all that occurs, it will probably be noticed that bubbles separate from the water, rising up through it and escaping at the surface; frequently the bubbles adhere for a time to the flask. Let the experiment be repeated in such a way that the something which escapes from the water can be collected and measured. For example, a 2-gallon tin can having been filled with water, insert into the neck a rubber cork through which a bent *delivery tube* is passed; place the can over a burner, introduce the upturned end of the delivery tube into a basin of water, and insert a small jar over it. Heat to boiling. An air-like substance will gradually be driven off, but it will be noticed that after the water has been boiling for some time it ceases to give off gas; let the amount of gas collected be measured, and have the experiment repeated several times. As the gas does not continue to come off on boiling the water, it would seem that it is not a part of the water—there is so little of it, but merely something dissolved in the water; it is like air, and the water had been in contact with air—may it not be air? Let the boiled water be poured out into a galvanized iron pan, and after it has been exposed to the air for several hours let it be again boiled. The water which previously no longer gave off gas, will now yield probably as much as before. It will thus be discovered that water dissolves air as well as the solid matters with which it comes into contact, and the presence of air in water will be recognized. This knowledge will be of value later on when the existence of animals and plants under water comes to be considered.

7. Attention having thus been directed to the solvent action of water, let special experiments be made on its solvent action, using salt, sugar, suet, washing soda, alum, tea and coffee, field or garden soil, clay, chalk or limestone, gypsum, &c.; known quantities of the filtered solutions should be evaporated to dryness, and the residues dried (conveniently in a small gas cooking-oven) and weighed. Opportunity will be afforded to call attention to the separation of some of the substances from solution in definite shapes, *i.e.* crystals; show these under the microscope as well as home-made cardboard models of some of them. Let larger crystals of alum be grown, and call attention to sugar crystals. Natural crystals of calcite, gypsum, pyrites, quartz, fluorspar, &c., would be appropriately shown at this stage. The question may then be put, Does the water which passes through the body dissolve anything? By evaporating urine and determining the amount of dried residue it would be found that a good deal of matter passes away from the body in solution.

8. Having directed attention to the different behaviour of different waters with soap, let determinations be made of the amount of alcoholic soap solution required to produce a lather in distilled and other waters. Directions for performing the soap test are easily obtained from a book on water analysis, and the operation is one of extreme simplicity.

9. Other liquids should now be compared with water, such as methylated spirit, turpentine, petroleum, salad oil, vinegar, and perhaps the common acids—muriatic, nitric, and sulphuric—also. The noticeable differences between these and water—appearance, odour, taste in dilute solution—having been registered, their relative densities should be determined; also their behaviour towards water and towards each other, their behaviour when heated on the water-bath in comparison with that of water, their behaviour when burnt, their behaviour when boiled together with water in a flask attached to a condenser, and their solvent action in comparison with that of water should be ascertained.

10. Having given an account of the origin, &c., of the various liquids examined, and having alluded to the presence of alcohol in beer and wine, demonstrate the separation of alcohol from beer by distillation; then describe the production of alcohol by fermentation and carry out the experiment, first with sugar and yeast, then with malt; explain that yeast is an *organism*, and show it under the microscope and lantern photographs of it. Make several mixtures of alcohol and water, and let the relative density of each be determined; then exhibit a table of relative density of spirit solutions of various strengths. Let a measured amount of beer be distilled, have the distillate made up with distilled water to the bulk of the beer taken, and let its density be determined; reference being then made to the table of relative densities, the strength of the alcoholic distillate would be ascertained, and thus the amount of alcohol in beer would be determined.

11. The behaviour of water when heated may now be further studied: attention having been called to the thermometer as an instrument which enables us to judge how hot or cold it is, water should be heated and the gradual rise of the mercury column noted, and the steady position which it assumes when the water boils. In the same way boiling water should be allowed to cool and the fall of the mercury column noted; further cooling should then be effected by means of ice, so that opportunity might be given for the stationary position to be observed which the column eventually takes up and maintains so long as unmelted ice is present. Having specially directed attention to these "fixed points," describe the construction of the thermometer. Next let a quantity of water be distilled from a flask or can having a thermometer in its neck, and let the steady position of the mercury throughout the distillation be observed. Also let water be frozen by means of a mixture of ice and salt; the "temperature" of the freezing mixture having been ascertained, the thermo-

meter bulb should be inserted into the water which is being frozen (in a test tube), so that the ice may form around its bulb: the temperature should be noted during freezing and also during the subsequent melting of the ice. Do this out of contact with the refrigerating mixture.

12. Let the relative density of ice be determined, *i.e.* after showing that although "lighter" than water ice is "heavier," than turps, let a cylinder partly filled with turpentine be counterpoised, and after the temperature has been lowered by immersing the cylinder in ice water, note the position of the turps, then introduce a few pieces of dried ice, note the rise of the turpentine —thereby determining the volume of the ice—and subsequently weigh in order to ascertain the weight of ice introduced. Have the result thus obtained checked by subsequent observation of the bulk of water which results when the ice melts. The expansion of water on freezing having thus been observed, the bursting of pipes in winter may be explained; and attention may also be directed to the destructive effects on rocks produced by the freezing of water; the extent to which ice floats may be discussed, and arithmetical problems may be set which will lead the pupils to realize the extent to which the volume changes when water changes its state.

13. Let the relative density of water and the other liquids be determined at 0° C. and at a higher temperature—that at 0° by weighing, and that at the higher temperature by observing the expansion of the liquids in bulbs with graduated stems of known capacity; let curves be constructed showing the relation between temperature and volume.

14. Let spirit, turpentine, petroleum, and vinegar be distilled; the temperature during distillation being observed, the gradual rise especially in the case of spirits and petroleum will be noted. Fractionally distil several times some quantity of spirit and of petroleum; let the relative density of each separate fraction be determined, and let the water separated from the spirit be characterized by freezing it and determining the melting-point of the ice and the boiling-point of the liquid which results when the ice melts.

15. Having directed attention to the fact that heat is "used up" in melting ice and boiling water, let determinations be made of the amounts, following Worthington's "Practical Physics," for example.

Studies of Chalk and other Common Solids.

1. Call attention to the use made of lime in building and its production from chalk or limestone; slake a lump of lime; exhibit specimens and pictures of chalk cliffs or quarries and lime-kilns—if not to be seen in the district. Point out on a geological map those parts of the country in which chalk occurs, and those where limestone is met with. Explain how chalk is supposed to have been formed, and show pictures of the forms which are present in it, and, if possible, microscopic slides. Explain that whitening, which is purchasable everywhere, is but lævigated chalk; describe its preparation, and let chalk and sand be separated by lævigation.

2. Let the conversion of chalk into lime be studied quantitatively. For this purpose three to five grams of dried whitening should be weighed out in a small platinum dish and heated to full redness in the *covered* dish during an hour over a Fletcher Argand Bunsen burner: the dish is then removed from the burner, and after about ten minutes, when cold, is weighed; it is then again heated, say for half an hour, &c.; usually there is no further loss. Several experiments should be made in this way, so that it may be noted that practically the same percentage of loss is incurred and the same amount of lime obtained in each case; and similar experiments should be made with chalks from different localities (Note A).

3. At the conclusion of each experiment, the residue should be carefully moistened with distilled water and the effect noticed; usually the lime slakes, becoming hot—some limes, however, slake very slowly, and the heating is imperceptible. The excess of water should then be driven off by heating in a water-oven until the weight no longer diminishes.

4. In comparing the solvent action of the various liquids previously studied, it will probably have been noticed that chalk is dissolved by acids—for example, vinegar or muriatic acid—with effervescence; such an acid may therefore be used, if necessary, in cleaning out the dish at the conclusion of the experiment if any of the solid adhere to it. Then, having made it clear that the effervescence is due to the escape of an air-like substance or gas, which is conveniently termed *chalk-gas*, let the amount of gas which is given off when the chalk is dissolved in acid be determined. For this purpose, the simple apparatus shown in Fig. 1 may conveniently be used. From 1·5 to 2 grams of the chalk is weighed out on a small square of tissue paper, which is then folded up at the sides and dropped into the bottle A, from which the tube B has been removed; a little water is then added (about 5 cubic centimetres), and the chalk is shaken out of the paper; about 5 cubic centimetres of nitric acid is now poured into the tube B, which is then carefully replaced in the bottle A. The cork having been inserted, connection is established by means of the flexible tube C with the bottle D. The side tube E having been so adjusted that the end *e* is on a level with the water in the bottle D, the measuring cylinder H is so placed that any water which runs from *e* may be collected in it, and the bottle A is then carefully tilted so that the acid may gradually run out of the tube B into A; gas is at once given off and expels water from D. As the water sinks in D, the side tube E is lowered so that its orifice remains about on a level with the water in D. The water is then measured. Several experiments should be made, and the results should be compared by calculating the volume of gas which would have been obtained, supposing, say, 100 grams of the chalk had been dissolved.

5. In this way it is ascertained that *chalk-stuff* is *characterized* by (1) yielding between 56 and 57 per cent. of lime, which in-

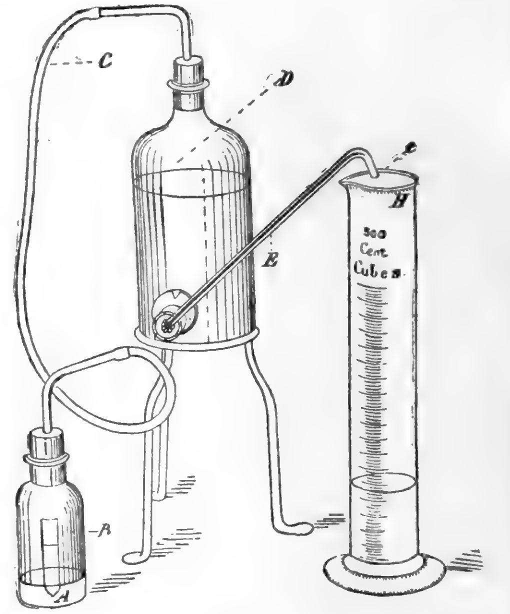


Fig. 1.

creases by about 33 per cent. when slaked; and (2) by yielding about 22,000 cubic centimetres of chalk-gas per 100 grams when dissolved in acid.

6. Comparing lime with chalk, it is found that if the chalk be thoroughly burnt no gas is evolved on dissolving the recently slaked lime in acid; this result serves at least to suggest that the gas which is given off when chalk is dissolved in acid is perhaps expelled during the conversion of chalk into lime. The loss in weight which occurs is therefore determined, and when it is ascertained that it is very nearly the same as when chalk is burnt, no room is left for doubt that the same substance is dispelled by heating and by dissolving the chalk in acid. The experiment is very easily carried out in a small bottle or conical flask provided with a tube to contain acid, and closed by a cork through which pass a narrow tube bent at a right angle and a small drying tube full of cotton-wool. The chalk is weighed out on thin paper and dropped into the flask, a little water is poured on to it, and the acid tube is then introduced, after which the cork is inserted. The bent tube is closed by a small stopper. On tilting the flask, acid escapes and attacks the chalk; the spray is prevented from escaping by the cotton-wool. When the action is at an end, air is sucked in through the narrow bent tube to displace the chalk-gas, and finally the

loss in weight is determined. Such an apparatus gives admirable results.

7. Marble may then be examined in a similar way; as it is found to behave both on heating and when dissolved in acid much as chalk does, it may be presumed to consist of chalk-stuff. Next, limestones should be taken; the result obtained with them may be lower owing to their containing clay, &c.; but this is to a large extent rendered evident by insoluble matter left on treating with acid. Let the percentage of chalk-stuff in the limestones be calculated from the results which they afford, assuming the results obtained with chalk to be practically those afforded by pure chalk-stuff. Lastly, direct attention to the occurrence of crystals (calcite) in limestone rocks, to stalactites, &c.; show specimens, and have them examined: the results will show that they also consist of chalk-stuff.

8. Having pointed out that chalk consists of shells, &c., of sea-animals, coral and shells of various kinds—oyster, cockle, limpet—should be given for examination; all these will be found to give results from which it may be inferred that for the most part they consist of chalk-stuff. Egg-shell and lobster or crab-shell in like manner will be found to yield lime when burnt, and to behave much as chalk does towards acid, but the presence of a certain amount of "animal" matter will be evidenced by the blackening on heating, and the insolubility of a certain proportion in acid.

9. Ordinary bone, gypsum, clay, and rocks other than chalk or limestone rocks are next given for study, in order that it may be discovered that the behaviour of chalk-stuff is peculiar and characteristic, and that there are many varieties of natural solids. Rough estimates of the amount of chalk in soil may be made by determining the amount of chalk-gas evolved on treating the soil with acid.

10. In a hard-water district the residue from the water will probably look more or less like chalk; its behaviour when treated with acid and when strongly heated should therefore be determined, and local boiler or kettle scale should then be studied as chalk was previously.

11. In this manner a large number of data will be accumulated which render it possible to discuss the origin of chalk; to explain the presence of chalk-stuff in water; and its withdrawal from water by animals; &c.

The study of chalk in the manner indicated would make it possible for the student (1) to comprehend the principle of the method followed by chemists in characterizing substances whereby they are led to discover distinct forms or species; (2) to realize not only that there are *compounds*, but also that such substances have a fixed composition; and (3) the entire difference in properties between a compound and its constituents would have been brought out most clearly by comparison of chalk-stuff with its constituents—lime and chalk-gas. The chalk studies, in fact, should serve to incite the student's curiosity, and should lead to further inquiries being undertaken as to the composition of other substances and the characters of their constituents, and as to the nature of other changes; and with regard to the method of undertaking inquiries into the composition of other substances, the important results obtained in the case of chalk by studying the *changes* which it undergoes would serve to illustrate the importance of studying change as a means of determining composition.

It cannot be denied that only well-informed, thoughtfu teachers could give useful instruction in accordance with the foregoing schemes; but this is scarcely an objection. The amount of special training required to carry out the experimental portion would not, however, be great; and there is no reason why such instruction should not be given in schools where there is no special science teacher engaged—although the services of such a teacher would undoubtedly be necessary if instruction in accordance with the more complete scheme embodied in the report presented last year by the Committee were carried out in its entirety.

The suggestion that probably it will be found advantageous, at least in the earlier stages, rather than disadvantageous, to devote but a short time during any one lesson to actual experimental work would be realized in practice if the experimental science lesson were associated with the measurement or practical arithmetic and drawing lessons; and it is difficult to imagine that this is not possible. Suppose a set of twenty-four pupils to be at the disposal of a teacher during an entire morning or afternoon in a room of sufficient size, properly appointed, and that they are set to work to carry out the experiments with chalk, described above. Several—say six—might be told off to weigh out in platinum dishes the necessary quantities of whitening, and having then placed the dishes on Fletcher burners or in a muffle, they would return to their places; at the end of an hour they would remove the dishes, and, after leaving them during ten minutes to cool, would weigh them. To determine whether any change took place on further heating, they would re-heat the dishes during, say, half an hour, at the expiration of which time they would, as soon as the dishes were cool, weigh them again. As soon as the first set of six had weighed out the chalk, a second set of six might be set to work in a precisely similar way if the necessary apparatus were available, or if not at some other exercise involving the use of the balance.

The nature of the experiments which each set were engaged in performing should be made known to the whole class, and all the data should be written up on a blackboard. Each pupil should write out an account of the experiments and of the results; opportunity would thus be given to compare the results of the six or twelve separate experiments. At the next lesson the two remaining sets of the class would carry out the same experiments. Each pupil would thus have the advantage of performing one or other of the experiments, and of knowing what results had been obtained by a number of fellow-students. If necessary, two pupils might be set to perform one experiment, care being taken that they took equal parts in it; and thus the whole class of twenty-four might complete the experiment or experiments in a lesson.

Those of the class who at any time were not actually engaged in carrying out the experiment might be occupied in other ways, *e.g.* in measuring distances, in drawing figures of stated dimensions, &c., in determining areas, in determining relative densities, in working out arithmetical problems, or in writing out notes and answers to questions. It would not be difficult as the class progressed to devise an infinite number of problems and exercises, the data for which were derived from experiments performed by the class.

If only one such lesson were given per week, a single teacher and an assistant might deal with 240 pupils, or with half that number if each class had two lessons per week—a much better course; and, working on a similar plan, much useful work might be done even in the course of two hours.

With regard to the appointments for such work, the schoolroom should be provided with simple working benches in addition to the ordinary desks and forms. A narrow table might be placed, preferably across one end of the room, on a raised platform, at which the teacher could sit and on which the balances could be placed; the teacher would then be able to supervise the weighing, and secure that due care were taken of the balances. A narrow bench (of deal, into which paraffin had been "ironed," so as to waterproof it) might be fixed against and along the wall at either side of the room. This should be fitted with simple cupboards and drawers for apparatus, and with gas taps if possible; and at a suitable distance from the wall and above the table there should be a bar carried by brackets affixed to the wall, from which various apparatus, small scales, &c., could be suspended. A simple draught arrangement should and might easily be fitted at each working place, so that no unpleasant or noxious fumes need escape into the room. At the other end of the room it would be desirable to have a demonstration table, and behind this, against the wall, a draft closet at one end of a bench at the other end of which was a capacious sink. It would be well also to have a sink within the closet, which could be made use of, for instance, in washing out a sulpuretted hydrogen apparatus. A muffle furnace at the side of the ordinary stove would be a most valuable adjunct.

The cost of carrying out experiments such as have been suggested remains to be considered.

The chief item is undoubtedly the balance. Useful work may be done at a very early stage of the measurement lessons with scales costing five or six shillings, as suggested by Prof. Worthington, but their use for quantitative chemical work, such as is comprehended in the foregoing scheme is entirely to be deprecated. The acquisition of the habit of weighing carefully and exactly is in itself a discipline of the utmost value, to which every boy and girl should be subjected. It is all-important, therefore, that a fairly good balance should be used, and that the utmost care in its use should be enjoined. When not in use the balance should be covered over with a cardboard box. Becker's No. 51 (Fig. 2) and No. 67 balances, to be had from Townson and Mercer, the English agents, are to be strongly recommended, the former being probably the more suitable, as the pans are carried by "bowed" wires, giving more room for manipulation, when, as in determining relative densities by the hydrostatic method, a bridge to carry a glassful of water is placed across the scale-pan. No. 51 costs £1 17*s.* 6*d.*; No. 67, £2 1*s.* A suitable set of weights (No. 31), from 500 grams downwards to centigrams, costs 18*s.* 4*d.* Even if six balances were provided—and such a number would suffice for a large class—the cost would be but £18.

A convenient size of platinum dish to use is one about $\frac{3}{4}$ inch deep and 2 inches wide, weighing, with a light cover, about 20 grams. At a normal price of platinum, such a dish would cost about 25*s.*, so that a considerable number might be provided for an outlay of £10. Such dishes not only last a long time when properly used, but are of value when damaged (Note A).

A water oven for drying would cost about £1; one of Fletcher's small air ovens for drying costs 17*s.* 6*d.*

Fletcher's Argand Bunsen burners, with tripod, are to be recommended as superior to the ordinary burners for school work. The smaller size costs 2*s.*; the larger, 3*s.* Suitable

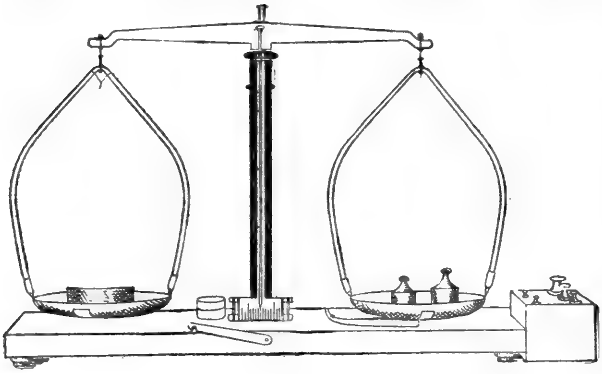


Fig. 2.

black rubber tubing for use with these burners, $\frac{3}{8}$ inch in diameter, costs about 9*d.* per foot. A pair of iron crucible tongs costs 1*s.*

The apparatus for measuring the gas evolved on dissolving chalk in acid would cost about 7*s.*, including a 500 cubic centimetre measuring cylinder.

Glass basins about 3 inches in diameter cost 4*d.* each; clock glasses, 6 inches in diameter, 5*s.* per dozen.

50 c.c. burettes cost 3*s.* 6*d.* each.

It is unnecessary to refer to the cost of the few remaining articles required for the suggested experiments, as they are well known. An expenditure of £50 would certainly cover the cost of apparatus required by a class of, say, twenty-four, and which would suffice for the use of several such classes.

Note A.—The unfortunate rise in the price of platinum, which makes the purchase of any number of platinum vessels for school use out of the question, has led me to make a number of experiments in the hope of substituting silver; but, as was to be expected, this has proved to be impossible. I find, however, that porcelain may be used, provided that the heating be effected in a muffle furnace. Small thin hemispherical porcelain capsules may be obtained from the dealers, about the size of the platinum dishes specified, which are more suitable than porcelain crucibles for the experiment. Such dishes may also be used in studying the effect of heat on organic substances, the char being burnt in the muffle furnace.

SCIENTIFIC SERIALS.

American Journal of Science, April.—On allotropic silver, by M. Carey Lea. This paper is in continuation of one contained in the March number of the *Journal*, in which the gold-coloured forms of allotropic silver were examined. The subject now considered is the relation existing between the allotropic forms of silver taken generally and silver as it exists in its compounds, and more especially in the silver haloids. The investigation leads to the conclusion that silver may exist in three forms:—(1) Allotropic silver, which is protean in its nature, may be soluble or insoluble in water, and have almost any colour; but in all its insoluble varieties always exhibits plasticity. It is chemically active. (2) The intermediate form, which may be yellow or green, but is never plastic, and is almost as indifferent chemically as white silver. (3) Ordinary silver. Allotropic silver is affected by all forms of energy, and the effects are strikingly analogous to those produced on silver haloids by the same agencies. It is, therefore, concluded that in the silver haloids silver may exist in the allotropic form.—The phenomena of rifting in granite, by Ralph S. Tarr.—The red-rock sandstone of Marion County, Iowa, by Charles R. Keyes.—The volumetric composition of water, by Edward W. Morley (continued from the March number of the *Journal*). The hydrogen used in the investigation was obtained by the electrolysis of dilute sulphuric acid. By this means it has been found possible to get hydrogen containing less than one-hundreth of a cubic centimetre of nitrogen in two litres of hydrogen, and containing no other impurity in amount large enough to be detected. An apparatus for the measurement of gases has been constructed, in which the mean error of measurement of the volume of hydrogen and oxygen used in the experiments has been less than one part in fifty thousand. With this, twenty experiments have been made, which give a maximum value for the composition of water 2·00047, a minimum value of 2·00005, and a mean value 2·00023. The composition of water may, therefore, be taken as 2·0002 volumes of hydrogen to one volume of oxygen.—On certain points in the estimation of barium as the sulphate, by F. W. Mar. Some experiments made by the author indicate that hydrochloric acid may be introduced freely and without detriment to quantitative exactness, in the precipitation of barium in the form of sulphate from pure solutions. Up to a determined point, the amount of hydrochloric acid employed accelerates the precipitation. The quantity of alkaline salts present is shown to have no very marked influence on the time of formation of the precipitate.—On halotrichite, or feather alum, from Pitkin County, Colorado, by E. H. S. Bailey. An analysis of the mineral shows that it is essentially a sulphate of alumina and ferrous oxide, with a part of the former replaced by ferric oxide, and a part of the ferrous oxide replaced by magnesia.—On a new serpent from Iowa, by R. Ellsworth Call.—On crystallized azurite from Arizona, by O. C. Farrington.—On the occurrence of xenotime as an accessory element in rocks, by Orville A. Derby.—On the magnetite ore districts of Jacupiranga and Ipanema, Sao Paulo, Brazil, by Orville A. Derby.—On pink grossularite from Mexico, by C. F. de Landero.—Restoration of *Triceratops*, by O. C. Marsh.—Development of the Brachiopods, Part 1, introduction, by Dr. Charles E. Beecher.

SOCIETIES AND ACADEMIES.

London.

Geological Society, April 8.—Dr. W. T. Blanford, F.R.S., Vice-President, in the chair.—The following communications were read:—The Cross Fell inlier, by Prof. H. A. Nicholson and J. E. Marr. The tract of Lower Palæozoic rocks lying between the Carboniferous rocks of the Cross Fell range and the new red sandstone of the Eden Valley is about sixteen miles in length, and little more than a mile in average breadth; the inlier extends in a general north-north-west and south-south-east direction, and the normal strike of the rocks is about north-west and south-east. The tract is divided along its entire length by a fault, which separates the Skiddaw slates (with the Ellergill beds of one of the authors, and the Millburn series of Mr. Goodchild) from higher beds on the west. A detailed classification of the Skiddaw slates is not attempted, but the authors describe the succession of the rocks in the faulted blocks of the western portion. Their classification is as follows:—

Coniston grits	= Ludlow.
Coniston flags (lower portion)	= Wenlock.
Stockdale shales	= Llandovery-Tarannon.
Ashgill shales *Staurocephalus* limestone Dufton shales and Keisley limestone *Corona* beds Rhyolitic group	= Bala.

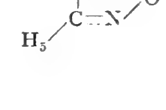
A brief comparison of these rocks with those of other regions is made by the authors. Two appendices are added: one, by Mr. Alfred Harker, contains petrographical notices of certain sedimentary and volcanic rocks in the Skiddaw slates, of the volcanic rocks of the Eycott and Rhyolitic groups, and of the principal varieties of intrusive rocks; the second, by Mr. A. H. Foord, contains a description of some Cephalopods from the rocks of the inlier. The reading of this paper was followed by a discussion, in which Prof. Boyd Dawkins, Dr. Hicks, Mr. Rutley, Mr. Hudleston, the Chairman, and Mr. Marr took part.—On the igneous rocks of the south of the Isle of Man, by Bernard Hobson. Omitting the Foxdale granite, the oldest igneous rocks of the island appear to be the diabase dykes of Langness, &c., intrusive in Lower Silurian slates. The Crosby microgranite dyke is also intrusive in these beds, and though its age is difficult to fix, it is probably newer than the Foxdale granite, which appears to be of post-Lower Silurian and pre-Carboniferous age. Next come the volcanic rocks of Lower Carboniferous age—an augite-porphyrite series consisting of tuff, breccia, agglomerate, bedded lava, and intrusive masses exposed in a narrow strip extending from Poolvash to Scarlet Point. A vent seems to have been opened during or after the deposition of the Poolvash limestone, from which fine volcanic ashes were ejected to form marine tuff. At intervals between the eruptions the Poolvash marble was deposited, and became interstratified with the tuff. The vent then probably became plugged up, and, a violent explosion following, supplied material for the agglomerate overlying the tuff. Lava then welled forth, and finally the volcano became extinct, and the intrusive mass of the Stack, regarded by the author as a volcanic neck, was exposed by denudation. It was probably at the close of volcanic activity that a melaphyre dyke was formed, resembling the porphyritic olivine-basalt of the Lion's Haunch, Edinburgh. At Poortown an intrusive mass occurs, provisionally termed augite-picrite-porphyrite, and considered by Mr. J. G. Cumming to be of post-Carboniferous age. Numerous dykes of ophitic olivine-dolerite occur between Bay-ny-Carrickey and Castletown Bay, at Langness, &c. They are post-Lower Carboniferous, and possibly of early Tertiary age. Full details with regard to the development and the macroscopic and microscopic characters of the various igneous rocks are supplied by the author, who acknowledges his indebtedness to Prof. Boyd Dawkins for the use of his geological map and notes. Prof. Boyd Dawkins said that the paper was the first instalment of the results of the geological mapping on the six-inch scale of the Isle of Man, which he had been carrying on for several years, and in which he had been assisted by the author. The igneous rocks of the island presented points of considerable difficulty. The author had, in his opinion, made a valuable addition to our knowledge. Replying to a question put by Mr. Rutley, Mr. Hobson explained that the igneous rocks at Scarlet Point belong to two distinct periods, the augite-porphyrites being of Lower Carboniferous age, while the olivine-dolerite dykes are certainly post-Lower Carboniferous, and perhaps of early Tertiary age.

Royal Meteorological Society, April 15.—Mr. A. Brewin, Vice-President, in the chair.—The following papers were read:—Some remarkable features in the winter of 1890–91, by Mr. F. J. Brodie. The author points out the peculiarities or special features of interest in the weather which prevailed over the British Isles during the past winter. In addition to the prolonged frost, which lasted from the close of November to about January 22, he finds that the barometric pressure for the whole winter was about a quarter of an inch above the average; and that, when the wind was not absolutely calm, there was an undue prevalence of breezes from some cold quarter. The percentage of winds from the southward did not amount to one-half of the average. The number of foggy days in London was no less than twice the average. The rainfall over the greater part of the British Isles was less than half the average. The author says that almost every element in the weather has been influenced to an abnormal degree by the remarkable prevalence of high barometrical pressure; and if we were called upon to define the season 1890–91, we should have little hesitation in giving it the name of the "anticyclonic" winter.—The rainfall of February 1891, by Mr. H. S. Wallis. This was one of the driest months upon record, the mean rainfall over England, excluding the Lake District, being only 0·066 inch, or about one-fortieth of the average.—On the variations of the rainfall at Cherra Poonjee, in the Khasi Hills, Assam, by Mr. H. F. Blanford, F.R.S. Cherra Poonjee has long been notorious as having a heavier rainfall than any other known place on the globe, the mean annual fall being frequently given as about 600 inches. Mr. Blanford has made a critical examination of the various records of rainfall kept at this place, and has come to the conclusion that the above amount is too high, and that the average annual rainfall is probably only a little over 500 inches.

Mineralogical Society, April 14.—Mr. R. H. Scott, President, in the chair.—The following papers were read:—On the occurrence of an aluminous serpentine (pseud-ophyte), with flint-like appearance, near Kynance Cove, by Howard Fox.—Note on the occurrence of melanterite in the Upper Eocene strata of the Thames basin, by the Rev. A. Irving.—Notes on various American minerals, including Bastnäsite, oligoclase, quartz, and sapphire, by G. F. Kunz.—On the devitrification of cracked and brecciated obsidian, by Prof. Grenville A. J. Cole.—Notes on crystallites, by F. Rutley.—On a method of observing the absorption spectra of minerals with the aid of the microscope, by Allan Dick.

PARIS.

Academy of Sciences, April 13.—M. Duchartre in the chair.—On the algebraical integration of differential equations, by M. H. Poincaré.—Description of an open manometer 300 metres high established at the Eiffel Tower, by M. L. Cailletet. The construction of the Eiffel Tower offered exceptional advantages for the installation of a large open manometer, and M. Cailletet appears to have carried out his design with great success. The length of the manometer is 300 metres, hence the pressure which it is possible to attain with the tube filled with mercury is 400 atmospheres. A glass tube would not sustain such an enormous pressure, so a steel tube 4·5 mm. in diameter has been employed. Specially devised stop-cocks are fitted on the tube at intervals of 3 metres. Each of these is connected with a vertical tube of glass about 3 metres high. When any pressure is required the stop-cock at the proper height is opened, and the hydraulic pump set working until the mercury appears in the glass tube that has been put in communication with the steel one. A manometer of this character is of prime importance, as it furnishes a means of accurately testing others, and must be invaluable to M. Cailletet in his researches on vapour tensions and the compressibility of gases.—Report on a memoir by M. de Sparre, entitled "On Foucault's Pendulum."—On the measurement of a new base for French triangulation, by General Derrécagaix. The base chosen is between Villejuif and Juvisy. Its length is 7226·792 metres at 19°·26 C., with a probable error not exceeding a centimetre.—Observations of the Barnard-Denning comet and some new asteroids, discovered by Borrelly and Palisa, made at Algiers Observatory with the telescope of 0·50 metre aperture, by MM. Rambaud and Sy. The comet's position is recorded from April 4–7, Borrelly's asteroid on April 4 and 6, and Palisa's on April 7.—On linear differential equations, by M. E. Vessiot.—On a class of complex numbers, by M. André Markoff.—Relation between the electro-magnetic and electro-static units, by M. H. Pellat. A series of twenty experiments gave the relation as $3{\cdot}0093 \times 10^{10}$, and another series of thirty-three experiments gave $3{\cdot}0091 \times 10^{10}$. It is pointed out that the number $3{\cdot}009 \times 10^{10}$ only differs by $\frac{1}{600}$ from Cornu's value for the velocity of light ($3{\cdot}004 \times 10^{10}$).—On the variation of fusion points with pressure, by M. B. C. Damien.—Transformation of cupreine into quinine, by MM. E. Grimaux and A. Arnaud. Heating to 100° in sealed tubes, for twelve hours, a mixture of molecular proportions of cupreine (the base of *Quina cuprea*) and methyl chloride with an atomic proportion of sodium (the whole dissolved in methyl alcohol), yields quinine. The sulphate possesses all the characteristics of the sulphate of natural quinine. The authors deduce from the experimental evidence given that quinine is the methoxide of cupreine.—On the action of hydrobromic acid upon silicon chloride, by M. A. Besson.—The calorimetric study of platinic chloride and its combinations, by M. L. Pigeon.—The estimation of rhodium by electrolyis, by MM. A. Joly and E. Leidié. The conditions under which this estimation can be accurately carried out are given in detail; the method cannot be employed in presence of nitric or oxalic acids.—On an amidoisoxazol, by M. Hanriot. The preparation and properties of methylethylamidoisoxazol are described. The formula



is allotted to the new compound. It

is an isomeride of the monoxim of propionylpropionitrile, $C_2H_5 . C(NOH) . CH(CH_3) . CN$, from which it is prepared.—The use of phenylhydrazine for the determination of sugars, by M. Maquenne.—New combinations obtained with certain metallic sulphites and aniline, by M. G. Denigès.—On a violet colouring matter derived from morphine, by M. P. Cazeneuve.—Note concerning aspergillin, a vegetable hæmatin, by M. Georges Linossier. The author controverts the statement of M. Phipson, that aspergillin is identical with palmellin. A comparison of the properties of the two bodies shows many analogies of aspergillin with hæmatin, but none with palmellin.—Note on the influence exerted by neutral mineral salts of potassium upon the solubility of potassium bitartrate, by M. Ch. Blarez.—On the characterization of fig-wine, by M. P. Carles.—A method of recognizing margarine in butter, by M. R. Lézé.—On the process of purification of an alcoholic liquor (from molasses) during rectification, by M. Ed. Mohler.—Artificial reproduction of daubreelite (schreibersite), by M. Stanislas Meunier. Daubreelite has been obtained by treating at a red heat with sulphuretted hydrogen (1) a mixture in the proper proportions of ferrous chloride and chromic chloride; (2) very finely powdered natural chrome iron ore; (3) an alloy of iron and chromium. The last method yields the best result.—On *Clusia* of the *Anandrogyne* section, by M. J. Vesque.—On the existence of the medullary liber in the root, by M. J. Hérail.

Stockholm.

Royal Academy of Sciences, April 8.—The Neuroptera of Scandinavia; Part 2, Neuroptera Trichoptera (*Phryganea*, L.), by the Rev. H. Wallengren.—Further remarks on the history of the vegetation of Greenland, by Prof. A. G. Nathorst.—Studies on enstatite, and the products of its metamorphoses, by K. Johansson.—Research on a group of long-periodical elementary links in the reduction of time, by Dr. K. G. Olsson.—*Tetrao bonasiotetrix*, Bogdanow, a hybrid between the black-cock and *Tetrao bonasia*, by Hr. G. Kolthoff.—Contributions to the knowledge of the Salices in the mountains of the south-west of Jemtland, by Hr. B. Floderns.—Contributions to the knowledge of *Bolocera*, a genus of the Actiniæ, by Hr. O. Carlgren.—On the rate of mortality within a determined age, by Dr. G. Eneström.—On a new sort of hygrometer, by Hr. C. Sondén.—On a new chronometer, which shows the thousandth of a second, exhibited by the inventor, Hr. W. Schmidt.

Amsterdam.

Royal Academy of Sciences, March 28.—Prof. van de Sande Bakhuyzen in the chair.—Mr. van der Waals dealt with the pressure of co-existent phases of a mixture, and particularly of solutions of salts and acids. He gives for this pressure a formula, into which enter two parameters, viz. the parameter c of electrolytic dissociation, and the parameter a of physical action; and points out that the solutions of salts and acids may be classified into two groups. For solutions belonging to the first group, for which $(a - 2) c > 1$, the relative diminution of pressure per molecule always exceeds 2, and possesses a maximum value. The second group, $(a - 2) c < 1$, shows for this diminution a minimum value and a maximum value. To the first group belongs the solution of KOH, to the second group the solution of SO_4H_2 in water.—Mr. Beyerinck spoke of the accumulation of atmospheric nitrogen in cultures of *Bacillus radicicola*. Whilst on a former occasion he had to state experiments with *Bacillus radicicola*, from which he deduced that, under the conditions thereby observed, an absorption of atmospheric nitrogen did not take place, he could now fix circumstances wherein such an accumulation may be obtained. To a decoction of stems and leaves of beans, 2 per cent. cane-sugar was added, and 100 c.cm. of this solution were filled into several Kjeldahl nitrogen balloons, and sterilized under cotton-enclosure. Some of these balloons were infected with very active material of *Bacillus radicicola* var. *Fabæ*, and exposed during eight weeks to a temperature of 5°–15° C. in a semi-obscure part of the laboratory. The matter for the infection was isolated in 1889, from tubercles of Windsor beans, and lately cultivated on a decoction of lucerne-stems with 2 per cent. cane-sugar and 8 per cent. gelatine, this being an extremely good soil for our *Bacillus*. Next to every infected balloon there was put a non-infected one for controlling the nitrogen absorption. A very profuse and interesting vegetation was soon perceived in the infected balloons. This vegetation contained rods, rapid-moving swarming individuals, bacteroids, and "stars." These stars are bacteroids, with many ramifications instead of three. The nitrogen was dosed after Kjeldahl's method, with iodometric determination of the ammoniac. The numbers would certainly have been found greater if the cultures had not been interrupted whilst the bacteria were still growing and swarming with great vigour, as was the case after eight weeks. In the following table the increase of nitrogen, albuminous matter, and bacteria corresponding with this increase—the latter being calculated as containing 75 per cent. water—from twelve experiments are inserted. The increase of nitrogen, 0·0011718 gr. per 100 c.cm., in the second experiment was found as the difference between 0·0061194 gr. in the infected, and 0·0049476 gr. in the non-infected balloon, &c.

	Increase of nitrogen per litre.	Increase of albuminous matter per litre (= N × 6·25).	Increase of bacteria therewith corresponding (= N × 6·25 × 4).
1st experiment ..	0·009114	0·0569625	0·227850
2nd ,, ...	0·011718	0·0731375	0·292550
3rd ,, ...	0·018228	0·1129140	0·451656
4th ,, ...	0·015624	0·0976500	0·390600
5th ,, ...	0·010416	0·0651000	0·260400
6th ,, ...	0·013020	0·0813750	0·325500

It is yet to be determined whether this increase is due to the fixation of free atmospheric nitrogen by the bacteria, or to the complete exhaustion of the environment of all nitrogen compounds by these organisms, and a thence caused "affluing" of the ammoniac or the other nitrogen compounds of the air into the culture-liquid.

CONTENTS.

THURSDAY, APRIL 30, 1891.

THE SCIENCE MUSEUM AND GALLERY OF BRITISH ART AT SOUTH KENSINGTON.

THE memorial which we print below, and which is still in course of signature, shows with no uncertain sound what is the general opinion of men of science with regard to the suggested allocation of the plot of ground opposite the Royal College of Science for an Art Gallery.

The memorialists, of course, are as appreciative as others of Mr. Tate's munificent offer, nor do they in the least object to the erection of art galleries; their only point is that the plot of ground in question finds its natural use in the additional buildings for the College, for which, indeed, it was understood plans have been for some time in course of preparation.

The reason for this is not far to seek. Not only is it convenient that the two halves of the College should be as near together as possible, but the professors in both use daily in their lectures the apparatus in the Science Museum; there is therefore necessarily the most intimate connection with the buildings in which the teaching is carried on, and those in which the materials for teaching are stored.

The suggested arrangement sunders them as far as possible from each other.

The treatment of the needs and claims of science by Ministers of all shades of party politics may probably account for the present dilemma. It may turn out that those who are responsible for having made the offer to Mr. Tate had not an idea that there was the slightest objection to the proposed action; and not only so, but it is quite possible, and we believe it is true, that the scientific authorities of the Science and Art Department were never consulted at all in the matter.

It is from this point of view that the Memorial, representing the opinion of Oxford, Cambridge, Edinburgh, and the other teaching centres in London, not to mention the President and Officers of the Royal and other scientific Societies, is of so great importance.

Men of science have waited patiently for nearly 20 years for the realization of the recommendation of the Duke of Devonshire's Commission that the pure and applied sciences, on which our national industries depend, should be treated like the other branches of human knowledge. Natural history, antiquities, literature, and art, find places in our Museum system. Why, then, not physical science?

During these years Committee after Committee has been appointed. They have all endorsed the recommendation to which we have referred; but the Government cares so little for these things, that some years ago, in 1877, they refused the offer of the ground on which the Imperial Institute is now being erected and £100,000 for a building, from the Royal Commissioners of the 1851 Exhibition.

Penultimately a Committee appointed by the Treasury reported that the claims of science were just and must be met. The Treasury then found £100,000 to buy the land they might have got for nothing more than ten years ago.

And now finally, as it would appear, lest they should be tempted to carry out their own intentions of a year ago, they hand over to an Art Gallery a large slice of the land bought for Science, and that slice the one which, in the opinion of everybody whose opinion is worth having, if thus occupied, will make the whole transaction ridiculous.

The Memorial, which is still in course of signature, runs as follows:—

Memorial to the Most Honourable the Marquis of Salisbury, K.G., F.R.S., Premier and Secretary of State for Foreign Affairs.

I. Whereas in 1890 Parliament voted £100,000 for the purchase of a site at South Kensington upon which to erect suitable buildings for the Science Museum of the Department of Science and Art and for the extension of its Science Schools; in accordance with the recommendations of the Royal Commission over which the Duke of Devonshire presided in 1874, as well as of various Committees and other high scientific authorities and of a Treasury Committee appointed in 1889,

II. And whereas it has recently been proposed to appropriate a considerable portion of this site nearest to those institutions fc the erection of a new Gallery of British Art,

III. And whereas it has been decided that this Gallery shall have no connection with the Science and Art Department; a building devoted to Art, under independent administration, being thus interpolated between the two parts of a Scientific Institution under that Department,

IV. And whereas it is obviously expedient that, whatever arrangements may be made, the various portions of the Science Museum and of the Science School should be as close as possible to, and in direct contiguity with, one another—for the reason that the Instruments and Museum specimens exhibited in the one have to be used by the Professors and students in the other—and that this will not be the case if the Science Collections are housed in the East and West Picture Galleries as proposed,

We desire most respectfully to express to your Lordship our strong opinion that the development and efficiency of the Science Museum and of the Science School and Laboratories of the Department of Science and Art will be seriously jeopardized by any arrangements that may be made for erecting a Gallery of British Art upon that portion of the land which was recently purchased by Parliament; and that this site is none too large, seeing the progress that is being made in the matter of Scientific and Technical Instruction, to provide for the future requirements of the Collections and Schools controlled by the Department of Science and Art; and that we would therefore earnestly entreat that another site should be found for the proposed Gallery of British Art.

Sir WILLIAM THOMSON, President of the Royal Society, Professor of Natural Philosophy, University of Glasgow.
MICHAEL FOSTER, F.R.S., Secretary Royal Society, Professor of Physiology, Cambridge.
Lord RAYLEIGH, F.R.S., Secretary Royal Society, Professor of Physics, Royal Institution.
Sir DOUGLAS GALTON, K.C.B., F.R.S., General Secretary British Association.
Sir JAMES PAGET, Bart., F.R.S., late President Royal College of Surgeons.
General STRACHEY, C.S.I., F.R.S., Chairman Meteorological Council.
ROBERT H. SCOTT, F.R.S., Secretary Meteorological Council.
W. BURDON SANDERSON, F.R.S., Professor of Physiology, Oxford.
Sir WILLIAM TURNER, F.R.S., Professor of Anatomy, University of Edinburgh.
T. MCKENNY HUGHES, F.R.S., Woodwardian Professor, Cambridge.
W. H. M. CHRISTIE, F.R.S., Astronomer-Royal, Past President Royal Astronomical Society.
ETTRICK W. CREAK, Commander R.N., F.R.S.
W. T. BLANFORD, F.R.S., late Director Meteorological Department, India.
JOHN RAE, F.R.S.
FRANCIS GALTON, F.R.S., President Kew Observatory.
Prof. W. H. FLOWER, C.B., F.R.S., Director Natural History Museum.

P. L. Sclater, F.R.S., Secretary Zoological Society.
Sir William Roberts, M.D., F.R.S.
Hugo Müller, F.R.S., Past President Chemical Society.
Dr. E. Frankland, F.R.S., Past President Chemical Society.
Dr. J. H. Gilbert, F.R.S., Past President Chemical Society.
Dr. W J. Russell, F.R.S., Past President Chemical Society.
Prof. Raphael Meldola, F.R.S., Foreign Secretary Chemical Society.
Shelford Bidwell, F.R.S.
Dr. J. H. Gladstone, F.R.S., Vice-Chairman School Board for London.
Sir Richard Quain, Bart., F.R.S., President Royal College of Physicians.
Prof. W. E. Ayrton, F.R.S., President Physical Society.
Prof. John Perry, F.R.S., Secretary Physical Society.
Prof. W. Grylls Adams, F.R.S., Past President Physical Society.
William Crookes, F.R.S., President Society of Telegraphic and Electrical Engineers.
T. G. Bonney, F.R.S., Professor of Geology, University College, London.
Sir William Bowman, Bart., F.R.S.
Lazarus Fletcher, M.A., F.R.S., Past President Mineralogical Society.
Sir Henry E. Roscoe, F.R.S., M.P., Past President British Association.
Sir John Lubbock, Bart., F.R.S., M.P., Chairman London County Council.
G. H. Darwin, F.R.S., Plumian Professor, Cambridge.
Sir G. G. Stokes, Bart., M.P., Past President of the Royal Society.
Sir Frederick Bramwell, F.R.S., Past President British Association.
Sir Bernhard Samuelson, Bart., F.R.S.
T. Archer Hirst, F.R.S., Past President Mathematical Society.
Bartholomew Price, F.R.S., Professor of Natural Philosophy, Oxford.
E. J. Stone, F.R.S., Radcliffe Observer, Oxford, Past President Royal Astronomical Society.
W. Odling, F.R.S., Professor of Chemistry, Oxford, Past President Chemical Society.
R. B. Clifton, F.R.S., Professor of Experimental Philosophy, Oxford, Past President Physical Society.
G. Carey Foster, F.R.S., Professor of Physics, University College, London, Past President of Physical Society and of Institute of Electrical Engineers.

COUNTY COUNCILS AND TECHNICAL EDUCATION.

THERE was one announcement in Mr. Goschen's Budget speech which came as welcome news to the friends of technical education. It was that the new grant out of the beer and spirit duties, which is at the disposal of County Councils with the power to use it for educational purposes, will not be diverted to other objects, but will be permanently renewed. In an announcement made in December last in reply to a question from Lord Hartington, the Chancellor of the Exchequer had held out the prospect of the permanence of the grant provided it were well used for the purpose for which it was granted. But none the less the definite language used on Thursday last is reassuring, and is likely to act as a useful stimulus to those few County Councils which have not yet definitely decided on the appropriation of the fund. It takes away the last shred of excuse from the obstructives who oppose entertaining any large scheme of educational organization on the ground that the grant should be treated as a windfall and not as income.

We have before us two small volumes recently issued by the National Association for the Promotion of Technical and Secondary Education, containing two series of selected reports of County Councils, and other typical schemes and proposals which have been drawn up in various districts for the utilization of the new fund. These publications, issued with the view of giving the various County Councils full and timely information as to what is being done and proposed elsewhere, bear welcome testimony to the energy and ability with which, as a whole, the County Councils are grappling with the unaccustomed task which the possession of the new grant entails upon them. It is highly creditable that only two County Councils in England (London and Middlesex) have as yet failed entirely to rise to the level of their new opportunities, and have yielded to the sordid temptation to sink the whole of the first year's revenue in the rates. There is already evidence that Middlesex means next year to reconsider its hastily-adopted policy, and a scheme is already being drafted for that county. Of the case of London we shall have more to say presently.

Of the remaining English Councils, 29 have devoted the whole, and 9 part of the fund to educational purposes. In 8 more counties the special Committees appointed to consider the matter have not yet finally reported, but in nearly every case their report is likely to be in favour of the application of the money to education. In Wales, which has a separate and more comprehensive Act of its own, which it is to be hoped will shortly be extended to England, almost the whole of the fund is being divided between the purposes of technical and intermediate education. In Scotland, unfortunately, very little has as yet been done, owing largely to the cumbrous and ineffective machinery of the Technical Schools Act which is in force in that country. The amendment of that Act is urgently required in order to prevent the whole or major part of the Scotch grant from falling into the rates.

Taking the country as a whole, there cannot be a doubt of the immense stride that has been taken during the last few months in the matter of national education, and the stirring up of interest in the subject all over the country is quite as valuable as the actual assistance given out of the fund. It is indeed almost necessary to warn County Councils against the temptation to move too fast, and to adopt schemes which will at once absorb the whole grant, and create vested interests hard to deal with in the future. The whole matter is in an experimental stage, the problem of the organization of technical education by counties being completely new. In attacking such a problem blunders are certain to be made, and money is certain to be wasted at first. Fresh legislation will probably be needed before County Councils possess the requisite powers to deal comprehensively with the whole question, and it will be well for them at least to be alive to the danger of creating future difficulties by tying up the whole grant at once. Up to a few months ago there was a danger lest the grant should cease to be applicable to education if unappropriated within the financial year. All doubt, however, on this point has now been removed by Sir Henry Roscoe's Act of the present session, under which full powers are possessed by County Councils of banking and accumulating any unappropriated balance.

It is fair to say that the schemes before us recognize for the most part the danger to which we have alluded, and proceed to work cautiously and experimentally. Variety and not uniformity is the leading note of the collection of schemes before us, and within a few months a series of most valuable educational experiments will have been worked out in various parts of the country—none the less valuable because a certain proportion of them may end in failure. Some of the most active counties are those in the west. Devonshire has a plan already in full swing for the delivery of courses of lectures throughout the rural districts on the sciences bearing on agriculture, under the auspices of the University Extension scheme. Gloucestershire has a system of cookery schools in the villages. Somersetshire has a carefully thought out scheme of distribution, under which the fund will be divided into three parts, one of which will be distributed to the various districts, in proportion to population; the second reserved in the hands of the County Council, to be administered for purposes common to the whole county; and the third granted to such Districts as are willing to levy a rate under the Technical Instruction Acts. Thus local activity is stimulated and assisted, without running the risk of frittering away the fund in small and comparatively useless doles. Here, as elsewhere, in the absence of regularly elected District Councils, it has been found necessary to divide the county into districts for administrative purposes, with specially constituted Committees for the disposition of the grant.

The county boroughs have not, on the whole, been backward to avail themselves of their new powers, though the delay that has taken place in the adjustment of the claims of counties and county boroughs has kept them in doubt for some time as to the exact amount which they might expect to receive. The energy, however, of the county boroughs as well as the counties has received lately a great impetus from the passage of the new Act empowering local authorities to contribute to institutions outside their district. This new power, which they did not before possess, makes it possible for a great institution like the Yorkshire College or the Durham College of Science to be made a centre for the instruction of the inhabitants of the surrounding counties, and to be supported by contributions from the whole of the area benefited.

To the general progress of the country London has hitherto presented a melancholy exception. The reasons are not far to seek. The London County Council is overworked, and being *in articulo mortis* is in a special degree afraid of its ratepaying constituents. The rates in the metropolis are high, and the requirements in all directions exceptionally large. All this forms no valid excuse for "grabbing" a fund ear-marked for educational purposes, but it explains the strength of the temptation, to which the Council have unfortunately yielded, to divert the fund to the relief of local taxation. It must, moreover, be confessed that the temptation was increased by the fact that the special Committee, appointed to inquire into the application of the fund, presented to the Council a very one-sided report, based on somewhat partial evidence, recommending the distribution of the balance for the first year almost exclusively among one class of institutions—the new polytechnics now being constituted out of the funds of the City parochial charities. It was felt that, useful as these polytechnics might be, they did not cover the whole ground, and representatives of older institutions were loud in their complaints. It is true that the special recommendations of the Committee were confined to the distribution of a balance of £23,000 which remained over for the first year, the bulk of the money received having been already thrown into the rates; and that the report contemplated the framing of a much more comprehensive scheme for the disposition of the funds to be received in future years. But a report like this is hastily read, and probably the one-sided appearance of the proposals had much to do with their rejection, and, unless a great effort is made, may yet prejudice the case for technical education in London for some time to come.

One thing, however, is certain. No decision of the present moribund Council as to the permanent appropriation of the fund to the relief of the rates can possibly bind its successor, and we may hope that before long the London Council will take steps to remove the stigma of being the only important Council in England which deliberately shirks its duty with respect to the education of the inhabitants. But if it still persists, some means of compulsion must be found, or the fund may have to be intrusted to the administration of some other Board, where it will not be subject to the depredations of the "ratepayers' friends."

PHILIP HENRY GOSSE.

The Life of Philip Henry Gosse, F.R.S. By his son, Edmund Gosse, Hon. M.A. of Trinity College, Cambridge. (London: Kegan Paul, Trench, Trübner, and Co., Ltd., 1890.)

THE second son of Thomas and Hannah Gosse, Philip Henry, was born in Worcester on April 6, 1810. A couple of years afterwards his parents went to reside at Poole, where Philip Henry's love for natural history appears to have been very early awakened. From his Aunt Bell he learned of the metamorphosis of insects, the name of our common red sea anemone, and she even suggested to the boy that he should try to keep sea anemones alive in vessels of fresh sea-water; this Aunt Bell was the mother of the well-known Prof. Thomas Bell, the the latter of whom was some eighteen years Gosse's senior. Thomas and Hannah Gosse belonged to two different orders of being. From a physical standpoint, we read of the father that he was a grey and withered man, who never smiled, who had no push in him, no ambition, and no energy; while the mother was a very handsome and powerfully built woman, with a pastoral richness of nature, like a Sicilian shepherdess of the olden times. Both parents did their duty, each after their light, to each other and to their offspring; but the artist father, the at one time pupil of Sir Joshua Reynolds, the man who had stored up in his brain masses of artistic and literary information, the man of pure conduct and pious habits, met with but little sympathy and but little appreciation from his splendid wife, and yet we read that he died in his eightieth year "entirely tranquil." This mother, too, was a good mother; she looked well after the education of her children, and the aptitude for learning which her second son, Philip Henry, showed, induced her to make peculiar sacrifices for his advantage, so that in 1823 she got him admitted into the Blandford School, where he acquired a fair knowledge of the rudiments of both Latin and Greek.

After a short sojourn in a large mercantile house, as a junior clerk, at Poole, Philip's mother accepted for him an appointment in the counting-house of the Messrs. Harrison and Co., at Carbonear, Newfoundland, where his elder brother, William, had for some time been. Here he met William St. John, whose portraiture is exceedingly well drawn for us in Philip's notes; here, too, he had large opportunities, of which he availed himself, for miscellaneous reading, some for falling in love, which he did not avail himself of; and here, too, he seems to have made up his mind to devote himself to the study of natural history. After five years in Newfoundland he returned on leave to Poole, and once more, after a brief six weeks' visit to his home, we find him back at Carbonear, which was finally left in June 1835 for Canada. Here farming was attempted, with rather miserable results. After a three years' trial, he found himself at twenty-eight years of age, not possessed, when all his property was told, of so many pounds. Through all this dreary time, the one thing that enabled him to overcome fatigue, and made him even forget his poverty, was his enthusiastic pursuit of the natural sciences, and amidst his ploughing, planting, and reaping, he made the observations and kept the notes which enabled him afterwards to publish his "Canadian Naturalist."

The next year of Gosse's life was spent in Alabama. On his way south he spent a few weeks in Philadelphia, where he made the acquaintance of Titian R. Peale, then preparing to start on the Wilkes Expedition to the South Seas, and of Prof. Nuttall, the botanist; and he occupied himself with visiting all those spots which were associated with the memory of America's greatest ornithologist, Alexander Wilson—"here was his residence; in yonder house he kept school; here were the very scenes described in his truly delightful volumes." Proceeding in the *White Oak*, a small schooner bound to the port of Mobile, he reached King's Landing, Alabama, after a somewhat disagreeable voyage of nearly a month's duration. He now became master of a school composed of the sons of a Mr. Justice Saffold and of other landed proprietors of the district. The school-house was situated in a very romantic spot, amidst a clearing of a great oak-forest hard by the village of Mount Pleasant. The teaching did not occupy the whole of Gosse's time. Birds and insects abounded; one little prairie knoll, about a mile from his own house, was a mass of blue larkspurs and orange milkweed, and was a marvellous haunt of butterflies.

"An eye," he writes, "accustomed to the small and generally inconspicuous butterflies of our own country, can hardly picture to itself the gaiety of the air here, where it swarms with large and brilliant-hued swallow-tails and other patrician tribes, some of which, in the extent and volumes of their wings, may be compared to large bats. These occur, too, not by straggling solitary individuals: in glancing over a blossomed field on my prairie knoll you may see hundreds, including, I think, more than a dozen species; besides other butterflies, moths, and flies."

Nor was larger game wanting—squirrels, opossums, and bears were often too numerous. The natural history surroundings were delightful; but, naturalist though he was, Gosse could not concentrate his thoughts on nature, and in dwelling on the conditions of the slaves, in witnessing the lawlessness of some of the slave-masters, his mind sickened, and he left the country and fled.

Five weeks—the first five of 1839—were occupied in the voyage to Liverpool. After eleven years of wandering, the traveller was back again. He had essayed many things, and none of them seemed to have succeeded; and yet all the while he had been "unconsciously serving an apprenticeship for the occupation for which he was fitted," and in which he was to spend the rest of his life. During the voyage home he commenced to write out his notes on Canadian natural history, and the rough manuscript of the "Canadian Naturalist" was finished ere the vessel entered the Mersey. His fortunes at this time seemed to have reached about their lowest ebb, when, on the recommendation of Prof. Bell, the amiable and now venerable publisher, Van Voorst, offered him one hundred guineas for the manuscript of the "Canadian Naturalist." Gosse's troubles were then, in great measure, over, and his career as an author had begun. There were, no doubt, still periods of poverty, borne with an admirable patience; but from 1840 to 1844 he published an "Introduction to Zoology," "The Ocean," contributed to some of the scientific journals; became a familiar visitor to the British Museum, and, towards the close of 1844, went on an exploring expedition to Jamaica. Nearly two years were spent most profitably in this lovely island; and in that most delightful of Gosse's books, the "Naturalist's Sojourn in Jamaica," the reader will find abundant proof of how well the time was spent in investigating the treasures of the place. As this was the last experience which Gosse had in studying the fauna or flora of other regions save those of the British Isles, we may remark, in passing, that he seems, in the tropics, to have passed over without much notice the forms of marine life: thus, while the mammals, birds, reptiles, and fish of Jamaica are carefully noted, and many new forms described, only a casual glance is bestowed on its corals and sea anemones. With his after-experience, had Jamaica been revisited, the glories of the "many small corals, apparently alive, of different species, some of which were very pretty," and of "the magnificent living corals forming great bushes at the bottom of the sea," would not have been left unsung.

Between 1846 and 1851 his volume on the "Birds of Jamaica" was published, and was well received; it was in octavo form, uniform with the afterwards published "Naturalist's Sojourn in Jamaica," but without illustrations; these were afterwards issued in a small folio volume, one of the most difficult nowadays to procure of all Gosse's works. Through an error in calculation there was a slight pecuniary loss on every copy subscribed for, and means were not resorted to to publish extra copies at an advanced price. Several popular works were written about this time for the Society for Promoting Christian Knowledge. On November 22, 1848, he married Emily, daughter of William Bowes, of Boston. In 1849 he commenced to pay particular attention to the lower forms of animal life, particularly the Rotifers; and he made the acquaintance of Quekett and Bowerbank. In 1849 his son Edmund was born. In 1850 the manuscript of the "Naturalist's Sojourn in Jamaica" was returned to him by Mr. John Murray, only to be accepted to their ultimate advantage by the Messrs. Longmans.

Towards the close of 1851 a complicated series of circumstances combined to drive Gosse away from his constant writing and drawing in London, where the monotony of his life was even more deleterious to his health than the severity of his labours, and his active work at the southern sea-shore of Great Britain now began. St. Marychurch, on the coast of Devonshire, was first of all selected.

"It lies open to the east, on a level with the tops of the cliffs, and enjoyed on clear days a refreshing view of the purple tors of Dartmoor away in the west. It was little in Philip Gosse's mind, when he first stepped up the reddish-white street of St. Marychurch, that in this village he would eventually spend more than thirty years of his life, and would close it there."

For the present his stay was transitory, but his investigations and discoveries supplied the material which enabled him to bring out "A Naturalist's Rambles on the Devonshire Coast." He also was busily working on the subject of marine vivaria, which he and Mr. Warington succeeded in bringing into notice and use, and which gradually from a toy have become a means of scientific research. "The Devonshire Coast" was published in 1853, and resulted in a fairly remunerative sale. At this time Gosse was asked to lecture: he had never attempted such a thing, but willingly promised to give a lecture on Sponges, accompanying his remarks with some life-like sketches on the black-board. One is not astonished to learn that the experiment was most successful, and that he for the future adopted lecturing as a branch of his labours. In 1854 "The Aquarium" was published. This little work had an immense success; the subject and the half-dozen coloured plates were attractive, but from a scientific standpoint the work will not bear comparison with the "Naturalist's Rambles on the Devonshire Coast"; still the profit on its sale reached the large sum of over £900. This volume was reviewed by Kingsley in the *North British Review* for November 1854, and Kingsley afterwards expanded this notice into a little volume called "Glaucus." In 1854, "Tenby," the last of this series of illustrated books treating of British marine zoology, was published, and also the "Manual of Marine Zoology." In 1856 he was elected a Fellow of the Royal Society; and his memoir on the "Manducatory Organs in the Class Rotifera" was published in this Society's Transactions.

In February 1857, Mr. Gosse had the sorrow of losing his first wife, and her death seems to have marked a crisis in his career; he became more than ever reserved; London became inexpressibly disagreeable to him, and towards the close of 1857 we find him settled at Sandhurst, hard by St. Marychurch, which now became his home.

Gosse was at this time forty-seven years of age; his life from his seventeenth year had been one succession of wanderings. The struggle for existence had never been very severe; his scientific work had merited and secured the esteem of many; his writings had been, all things considered, a success; and, in despite of some feebleness of bodily health, he had been enabled to work hard with pencil, pen, and tongue. His religious views were peculiar, and he gave them a quite peculiar prominence in many of his writings; still he never was subjected to any extreme or very unkindly criticism therefor. As a completely self-taught naturalist he had succeeded in training himself up to a comparatively high standard of knowledge, and at this period all his friends hoped that once time had worn away some of the sorrow from his heart, he would have returned to his studies with renewed zest; and so in time he did, but not before entering into a vague and unsatisfactory series of speculations on the origin and creation of life. Perhaps no work since the "Vestiges of Creation" was received with a greater tempest of adverse criticism than "Omphalos," published by him in 1857. The work of a serious biologist, its at-once-felt unreality, though charming in its way, was clearly not the object aimed at by its author: as a play of metaphysical subtlety, with its postulates true and its laws fairly deduced, it stands complete: Bishop Berkeley would have appreciated this volume, though even he would not have believed in its conclusions. Neither Gosse's friends nor foes seemed to have had any appreciation for it. Here, though we feel bound to make but a passing allusion to this work, we cannot refrain from an expression of regret that Kingsley's letter should have been published without the passage alluding to Newman having been first omitted. The book was a failure from a pecuniary point of view, and yet, though the fact is not alluded to in this memoir, its author contemplated "a sort of supplement, or rather complement, to it, examining the evidences of Scripture; not merely the six-day statements, but the whole tenor of revelation." This never appeared.

The "History of British Sea-Anemones" was published in twelve parts, the first of which appeared on March 1, 1858; it is an excellent monograph of our native forms, which will always be a work to refer to. The complete work appeared in January 1860, after which Gosse published his "Romance of Natural History," and married Miss Eliza Brightwen, who still survives him. Although we read that "the year 1861 was the last in which my father retained his old intellectual habits and interests unimpaired," yet the series of memoirs on the Rotifers published between 1861 and 1862, which culminated in his contributions to Dr. Hudson's well-known memoir, his papers on other natural history subjects, not to speak of his large correspondence, showed no impairment of intellect, and we should have thought that the latter years of his long life were among the most enjoyable of them all.

The end came on the morning of August 23, 1888, the immediate cause being an attack of senile bronchitis, brought on by exposure to cold while investigating some double stars. His remains lie buried in the Torquay Cemetery.

To all the numerous readers of Gosse's works and to his many correspondents, this biography by his only son will be most welcome. From a literary point of view, it leaves nothing to be desired; his chief object has been "to present his father as he was," for in so doing he felt sure of that father's approval. The task was, no doubt, a difficult one, for to write such a life from a perfect standpoint would have required, over and above literary skill, not only a most tolerant sympathy with the religious views and feelings of the man, but also a large acquaintance with the studies of his life, and neither of these qualifications does Mr. Edmund Gosse lay claim to. Still in the chapter of "General Characteristics" there is a fair summing up, though we cannot agree that "his letters were usually disappointing." In these columns we have not felt at liberty to allude, except in the most passing way, to the subject of the religious features of Gosse's life, and yet to the student of the man this aspect is one of great interest: for it we must refer the reader to his "Life."

The aim of this volume was to present the reader with a sketch of a man who has left his mark on the popular natural history of this century, and we believe that this object has been fully accomplished.

Here and there we have noticed, as we read, a few oversights, the most of which the biological reader will be able to correct as he reads. The words on p. 194, "the prints of Musignano," might not at first seem to indicate "the Prince of Musignano," afterwards known as the Prince of Canino; and in reference to the statement on p. 241, about Gosse's discovery of *Balanophyllia regia* on the coast of Devonshire, it is nowhere stated in the "Rambles on the Devonshire Coast" that the genus Balanophyllia was only known till then as containing fossil forms. Philip Gosse certainly knew otherwise.

E. P. W.

A CLASS-BOOK ON LIGHT.

A Class-book on Light. By R. E. Steel, M.A., Bradford Grammar School. (London: Methuen and Co., 1891.)

MR. STEEL'S book contains a curious mixture of good and bad. There is a great deal in it which may be most warmly commended. Many of the explanations are clear and concise; the figures are, on the whole, good, and the proofs sound and complete; the style is generally interesting, and the important points duly emphasized; but, with all this, there are some truly astounding statements, which detract immensely from the merits of the whole.

In the first chapter, Mr. Steel endeavours to explain how it is that we look upon light as a form of energy. He tells us (p. 4): "Thus, we can measure energy by the mass moved into the distance moved through against a known force, or by the mass moved multiplied by the increase of velocity produced"—two false statements which have not even the merit of being consistent. Or, again: "Light is a form of energy. . . ." "It is a vibration of the particles of ether. . . ." A form of energy cannot also be a vibration; the vibrating particles of ether possess energy, and if the vibrations be of a certain kind, this energy affects the eye as light. On p. 5 we have the following statement:—"We can pass electricity along substances such as carbon which offer great resistance, and thereby the electricity is converted into light. . . ."

It is, of course, difficult to explain, at once simply and in precise language, the relations between the various forms of energy; but this difficulty hardly affords sufficient excuse for the vague or erroneous expressions found in this part of the book; and it is to be hoped that, whenever another edition is called for, there may be some modifications introduced here. For, as has already been mentioned, the optical part is good, though some points are open to criticism. Thus, the proof given in § 12 does not really deduce the law of reflexion from the wave theory. This cannot be done without the principle of interference. It is said, p. 25, line 11, that, under certain circumstances, "DC must be the reflected wave"; but why? The point requires proof, and none is given. A similar criticism applies to the explanation of the law of refraction (§ 29). These points might with advantage have been further considered after § 80, which gives the explanation of the rectilinear propagation.

The first eight chapters deal mainly with geometrical optics, and contain nearly all the propositions which are ordinarily required. The formulæ for refraction through a lens proved in § 41 might also have been proved in a manner similar to that employed in § 22, when treating of reflexion at a mirror.

The earlier part of chapter vi., dealing with dispersion and the spectroscope, is rather meagre. A figure might with advantage have been introduced showing the path of a pencil of rays in a spectroscope.

Throughout the book a little more care might have been bestowed on the position of some of the figures. Thus, Fig. 72 is referred to on pp. 92 and 93, and not on 94. Had it been placed on p. 93, it could have been seen without turning over the leaf; it is, however, on p. 94, where it is not wanted. The same sort of thing is noticeable in other places; thus § 39 is headed "Concave Lenses"; after four lines of letterpress we have Fig. 61, but this is a figure of a convex lens referred to in the previous section. The figures for § 39 are over the page; of course the letterpress makes it clear which figures are meant, but still the arrangement is confusing to the student.

A good deal of matter—perhaps a little too much—is condensed into the last three chapters, dealing with interference, double refraction, and the interference of polarized light, but all that these chapters contain may be read with advantage. In the hands of a teacher who will correct the errors of the earlier part, the book may be used with profit.

THE PHILOSOPHY OF SURGERY.

Studies of Old Case-books. By Sir James Paget, Bart. Pp. ix. and 168. (London: Longmans, Green, and Co., 1891.)

IN this little book, Sir James Paget has digested some of the stores of knowledge which he has acquired in the course of a long and active professional life. Since he first became a medical student, Sir James tells us, he made it a practice to take notes of all the interesting cases which came under his notice, so that the quantity of material which he possesses in the way of unedited manuscript must be enormous, and he is thereby brought into the same class as John Hunter, though we may sincerely hope that, for the benefit of posterity, his records of cases will not share the same fate as befel Hunter's. Sir James is, however, rather sceptical as to the value of the observations which he has recorded at the cost of so much labour, for he says: "While looking through, and in some degree studying, the many thousand cases thus recorded, it has not been difficult to find why they would be now so nearly useless and uninteresting to anyone but myself." The changes which a progressive science like surgery necessarily undergoes in the course of sixty-one years enable the notes to be read by the light of our present knowledge, often with disheartening results when we reflect how difficult a thing it is to interpret a fact, even when it has been recorded. The use, however, of keeping up such a series of case-books is thus forcibly put forward by the author:—"The habit of recording facts is nearly essential to the habit of accurately observing and remembering them. That which we intend to record we are bound, and may be induced, to observe carefully, and the very act of carefully recording is nearly equivalent to a renewed observation." These studies, however, are not mere transcriptions from the old case-books, but they are a series of short essays upon rare or little-considered surgical cases which from time to time have come under the notice of the author. Each essay is full of that philosophy which can only be acquired by the most highly cultivated minds, and even by them only after a long and observant career. Each essay, too, is full of suggestion for further work—work which will not be carried out by Sir James Paget himself, but work which he may well live to see carried through by the younger generation of scientific surgeons, who look to him as a friend and a guide in those numerous difficulties which beset all earnest inquirers after truth.

The essays are seventeen in number, and, with a few exceptions, deal with subjects which are of interest only to the medical man. Two of the chapters are of interest to the general reader, however—that on errors in the chronometry of life is the most interesting, in which an attempt is made to show how many of the phenomena in health and disease may be explained by assuming that they are due to a disturbance of the time-rate at which the processes of life are discharged. For example, certain turtles lay their eggs in hollows made in the sand, leave them there to be hatched, and, at the time of hatching, return to them for the sake of their young. It might be asked, How can these creatures, and many others in similar cases, reckon the passage of time? Most probably they do not reckon it at all: but just as the timely-attained fitness of their organization for preparing and filling their nests impelled them to those acts, so some time-regulated organic processes, taking place in them after the laying of their eggs, bring about at length a new condition, of which a dim consciousness becomes an impulse to them to return to their nests. In the other essay of general interest, Sir James Paget discusses the use of the will for health.

OUR BOOK SHELF.

Euclid's Elements of Geometry. Books III. and IV. Edited by H. M. Taylor, M.A. (Cambridge: University Press, 1891.)

It is only of late years that the University of Cambridge has taken the wise step of giving greater scope to the teaching of geometry by not insisting on the actual proofs of propositions as presented in Euclid's text. All the definitions, axioms, and postulates, together with the sequence of propositions which he adopted, are retained, but in solving them "no proof will be accepted that assumes anything not proved in preceding propositions."

The work under consideration is published in the "Pitt Press Mathematical Series," and it will be found to contain some important changes, both with regard to the proofs and method of arrangement. In the first few propositions of Book III. the author has introduced several proofs which seem preferable to those generally adopted, while their order of sequence has been extensively changed. The alterations in the remaining propositions of this book have not been carried to any extent, but several outlines of alternative constructions have been inserted in cases where they may be most instructive.

Of the additional propositions introduced, one involves the principle of the rotation of a plane figure about a point in its plane, while following Proposition 37 are two others, the latter of which is commonly known as Ptolemy's theorem. Ten other additional propositions are at the end of this book, and they will serve as a good introduction to the many theorems on poles and polars, radical axes of circles, orthogonal circles, and the nine-point circle of a triangle. In the fourth book the propositions are as usual, no material alteration having been made. Throughout the work each proposition is accompanied by numerous exercises, while a capital set of miscellaneous problems terminates each book. We may safely say that the work of which this is an instalment will take the first place among the many books on the elements of geometry.

Zoological Articles contributed to the "Encyclopædia Britannica." By E. Ray Lankester, M.A., &c., and others. (London: Adam and Charles Black, 1891.)

Under the above title, Prof. E. Ray Lankester has republished a series of articles communicated by himself and others to the recently published edition of the "Encyclopædia Britannica," the whole forming a very admirable and well-illustrated treatise on some of the more important groups of the animal kingdom.

The memoirs on the Protozoa, Hydrozoa, Mollusca, Polyzoa, and Vertebrata are by Prof. Lankester; that on the Sponges is by Prof. Sollas; that on the Planarians is by Prof. von Graff; that on the Nemertines is by Prof. Hubrecht; that on the Rotifers is by Prof. Bourne; and that on the Tunicates is by Prof. Herdman.

While not professing to be a complete hand-book to zoology, this volume will be a very useful one to the student, as it will give him an excellent summary of the most recent views of the above specialists on the various groups treated of.

A few new figures have been added to the original text, and a few errors—due to previous oversight—have been corrected. Some quite recent discoveries are alluded to in footnotes, while in the preface Prof. Lankester points out one or two important alterations in classification and nomenclature which it was not possible to incorporate in the text.

The illustrations are refreshingly new, and they are as attractive as instructive. This volume is one we can very strongly recommend to all working students.

The British Empire: the Colonies and Dependencies. By W. G. Baker. (London: Blackie and Son.)

This volume is one of the series known as "Blackie's Geographical Manuals," and forms the second part of a book on the British Empire, the first part having dealt with "the home countries." The author sets to work systematically, beginning with the British possessions in Europe, then advancing to those in Asia, Africa, America, and Australasia. The method he adopts in describing the various countries included in the British Empire has the great merit of being natural, simple, and clear. The position and extent of each country are first noted; next he explains the form of surface, describes the climate, and traces the coast-line. Details as to population, industries, and commerce come afterwards, and these are followed by "such facts concerning the social and political condition of the people as are usually included in a course of geography for children: the form of government, and descriptions of the political divisions and chief towns." Mr. Baker has selected his facts with much discretion, and presents them in a way which is likely to interest young readers and to exercise the intelligence as well as the memory. There are many very good illustrations and several coloured maps.

LETTERS TO THE EDITOR.

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

The Rôle of Quaternions in the Algebra of Vectors.

PROF. GIBBS seems to forget that his pamphlet was specially announced as *Arranged for the use of Students in Physics.* When I wrote the remark which has called forth his recent letter, I was discussing the reasons for the comparatively slow progress of Quaternion Analysis in recent times. And, as it is precisely to students of Physics that I think we must look for such progress, anything which is calculated to divert their attention from Quaternions, or to confuse them in their use of Quaternion symbols, must be regarded as tending to retard the progress of the method. The Vector-Analysis supported by Prof. Gibbs involves a serious departure from the usage of ordinary Algebra, inasmuch as $\alpha\beta$ is not regarded as a product, but merely as a "kind of product." This is specially likely to confuse an ordinary student, and is undoubtedly artificial in the highest degree:—while one of the chief recommendations of Quaternions is their naturalness, *i.e.* the utter absence of artifice in their fundamental rules.

Some of Prof. Gibbs' suggested notations are very ingenious, and well calculated to furnish short cuts to various classes of results *already obtained*. But they do not seem to have led to any important extensions of such results; and must therefore be regarded as having claims to consideration very much inferior to those of the novelties of notation suggested by Mr. M'Aulay; for these have undoubtedly shown their value as instruments of research, while they do not depart from algebraic usage in a matter so fundamental as is the notation for ordinary multiplication.

It is singular that one of Prof. Gibbs' objections to Quaternions should be precisely what I have always considered (after perfect inartificiality) their chief merit:—viz. that they are "*uniquely adapted to Euclidian space*, and therefore specially useful in some of the most important branches of physical science." What have students of physics, as such, to do with space of more than three dimensions?

I do not agree with Prof. Gibbs in his statements as to the superiority of his notation over that of Quaternions, so far at least as can be judged from the examples he cites. Surely the simple equality

$$S\sigma\phi\rho = S\rho\phi'\sigma = \Sigma(S\alpha\sigma S\lambda\rho)$$

shows, at least as plainly as does

$$\sigma.(\alpha\lambda + \beta\mu + \gamma\nu).\rho = \rho.(\lambda\alpha + \mu\beta + \nu\gamma).\sigma,$$

that the common value is "exactly the same kind of function of σ that it is of ρ." As to $S\sigma\phi$, it is simply meaningless, and I cannot imagine anyone writing such an expression.

But I have a positive objection of another kind to Prof. Gibbs' proposed notation. To save complexity a great number of *indispensable* brackets, or their equivalents, are omitted. If all the really necessary signs of this kind were inserted, the translations of moderately complex quaternion formulæ into his language would be frightful even to look at. To take a very simple case:—so far as I understand him Prof. Gibbs would write

$$(\alpha \times \beta) \times (\gamma \times \delta) \times (\epsilon \times \zeta)$$

as the equivalent of

$$VV(V\alpha\beta V\gamma\delta)V\epsilon\zeta.$$

But, if so, how would he write, without additional parentheses,

$$VV\alpha\beta VV\gamma\delta V\epsilon\zeta, \quad V.\alpha\beta VV\gamma\delta V\epsilon\zeta, \quad V.V\alpha\beta V\gamma\delta V\epsilon\zeta, \text{ \&c.?}$$

Even the first of the four just written has but *one* pair of brackets, besides the five Vs; while Prof. Gibbs' equivalent has *three* pairs of brackets, besides the five ×s. And, so far as I can see, Prof. Gibbs must invent a new notation entirely for the last two formulæ written above, since the product of three vectors is nowhere provided for in his system, important as it is and of constant recurrence in physical problems.

St. Andrews, April 25. P. G. TAIT.

P.S.—As I see that the correspondence in the *Athenæum* regarding the notice of Hamilton in the *Dictionary of National Biography*, has just been summarily closed, I would remark that the patent error of that notice is the confusion of Hamilton's *Varying Action* with his *Quaternions*. The consequence is that Hamilton gets no credit whatever for his absolutely invaluable contribution to Theoretical Dynamics!

The Influenza.

THE influenza—or the peculiar fever we know by that name—seems to be again making its appearance amongst us; at present in scattered cases in the towns, villages, and country districts of the west of England, and with some severity in Yorkshire. How is it that having had the disease once does not give immunity against future attacks, as is more or less generally the case with diseases of this kind, *i.e.* those caused by microbes, as this is now held to be? It would be interesting if some statistics could be arrived at with regard to the present outbreak, showing the proportion of cases of those who had already had influenza, and of those, who, having escaped before, have now taken the disease. E. ARMITAGE.

April 22.

Antipathy [?] of Birds for Colour.

YOUR correspondent "M. H. M." asks the question in your issue of April 16 (p. 558), "Has the sparrow an aversion to yellow?" because of its habit of destroying, as if in wantonness, the petals of crocuses of that particular colour. From some observations I made on the subject, when it was broached about four years ago, an account of which appeared in your columns (vol. xxxvi. p. 174), it is evident that the birds show a decided *fondness* for the colour, instead of an aversion to it. In the case I described, the birds used laburnum sprays only for their nests. It may interest your correspondent to know of such an incident, and to refer to the other correspondence on the subject alluded to on the occasion. WILLIAM WHITE.

The Ruskin Museum, Sheffield, April 22.

A Meteor.

SEEING the note in your issue of the 16th inst. (p. 566) as to a meteor observed by a lady correspondent at Ruanlanihorn, Cornwall, on March 26 last, I send you the following extract from my note-book in reference to a meteor observed by me at Martock, Somerset, on the same evening, and which there would seem to be little doubt was the same as that referred to in your note:—

"March 26.—At Martock, Somerset, saw a very fine meteor in the southern sky; time 7.16 p.m. It was travelling from west to east at a comparatively slow speed; the meteor was of a white hue, and left a visible train, in length about equal to the apparent diameter of the moon; the train was a deeper yellow tint and was broken up towards its end. The meteor was visible fully 5 seconds."

Martock is about 110 miles from Ruanlanihorn.

Addiscombe Road, Croydon, April 20. W. BUDGEN.

ON TIDAL PREDICTION.[1]

THE moon and the earth revolve in an orbit about their centre of inertia, and the centrifugal force due to the revolution exercises an unequal repulsion on the various parts of the earth; the moon also attracts the nearer parts of the earth more strongly than the remote parts. The differential action arising from these two forces is the cause of the tides.

This is explained in many elementary books, and although the exposition is often obscure rather than lucid, I will not stop to go over the demonstration, but will merely refer to Fig. 1, which shows the direction and relative intensity of the tide-generating force at various parts of the surface of a planet.

It is obvious, from the figure, that if the system were at rest the ocean would be elongated towards and away from the satellite, and equally depressed all round the sides. This result is expressed in technical language by saying that the figure of equilibrium is a spheroid with its axis of symmetry pointed at the satellite.

Newton, the founder of the equilibrium theory of the tides, was well aware that this theory would give no approximation to the truth when the system is endowed with motion, and when the ocean is interrupted by land. But whilst he recognized that the theory was useless as directly furnishing a method of tidal prediction, he was clearly aware that it was convenient to retain the conception of the figure which an ocean would assume if the whole system were at rest. In this view he was right, and all that can be attained in tidal science rests on the equilibrium theory. It is true that the word theory is not a good one to express what is meant; but if it had been called "the equilibrium specification of tidal forces," the objections which have been raised against it would have been obviated.

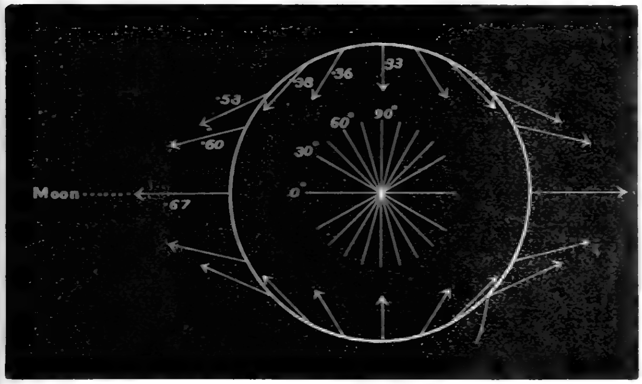


FIG. 1.—Diagram of tide-generating forces.]

The meaning of this is that, if the ocean covered the whole earth, and if all the motions were very slow, the tides would be such as are determined by the equilibrium theory. Since the intensity and direction of tide-generating forces are independent of the velocities of the earth's rotation and of the moon's motion, and also of the shape and size of the oceans, the so-called equilibrium tide serves to specify the tidal forces in the actual case. It is accordingly convenient to speak of the equilibrium tide as of a real phenomenon, and to make the transition to the actual oscillations of the sea at a later stage of the discussion.

We have only as yet spoken of the lunar equilibrium tidal spheroid, but for similar reasons there must also be a solar spheroid, of a smaller degree of elongation or ellipticity. The superposition of these two spheroids specifies the total resultant tidal force.

Since the major axes of the lunar and solar spheroids point to their respective luminaries, and since the earth rotates, the motion of the resultant spheroid relatively to the earth's surface, and the law of variation of its ellipticity, are very complex; further analysis of the two spheroids becomes, therefore, necessary. It is, accordingly, usual to regard the lunar spheroid as being built up out of three others, as follows: first, a prolate spheroid with its major axis in the equator in the same meridian as the moon; secondly, a prolate spheroid with its major axis in latitude 45°, also in the same meridian as the moon, the latitude being north or south, according as the moon is north or south of the equator; thirdly, an oblate spheroid with its axis coincident with the polar axis of the earth.

The axes of the first and second of these spheroids always follow the moon's meridian, and the ellipticities of all three vary slowly as the moon changes her linear distance from the earth and her angular distance from the equator. The ellipticity of the first or equatorial spheroid oscillates about a mean value; but the ellipticity of the second kind of spheroid is greatest when the moon is furthest north, diminishes to nothing (so that the spheroid becomes a sphere) when the moon is on the equator, and its axis leaps from 45° N. latitude to 45° S. latitude at the moment that the moon crosses from north to south, when the ellipticity begins to increase again.

The third kind of spheroid is of comparatively little importance in tidal theory, and I shall not make further reference to it.

Each of the two lunar spheroids moves slowly in space as it follows the moon's meridian; but to an observer, who is carried rapidly round by the earth's diurnal motion, the oscillations of the ocean due to the two spheroids would have different characters. In the case of the first sort of spheroid, he would pass through *both* protuberances in the course of one rotation of the earth, whilst he would only meet one protuberance of the second sort of spheroid, on account of its unsymmetrical position with regard to the earth's axis. Thus the first sort would by itself give him two high waters *per diem*, but the second sort would only give him one high water. The first may therefore be called the lunar semi-diurnal spheroid, and the second the lunar diurnal spheroid.

A similar analysis of the total solar spheroid gives a solar semi-diurnal and a solar diurnal spheroid; and the third sort of solar spheroid may again be omitted from consideration.

The axes of the two lunar and of the two solar spheroids follow the moon's and sun's meridians respectively, and their ellipticities vary with the linear distances of the two bodies from the earth, and with their angular distances from the equator. Taking these spheroids as specifications of tidal force, we see that the lunar and solar tidal influences are not yet analyzed into forces which are regularly periodic, either in intensity or in period. Accordingly, the next step is to break up each of the four variable spheroids into a number of new ones, which have invariable ellipticity and which move with uniform velocity relatively to the earth's surface. It is theoretically necessary to take an infinite number of these new spheroids to build up each of the former ones, but about five new spheroids suffice to represent any one of the old ones so closely that the error is negligible.

The result of this analysis of tidal forces is, then, to get some twenty equilibrium spheroids, which do not any longer follow the moon's or sun's meridian, but which do move with uniform velocity over the earth, and which are invariable in shape. These spheroids are, like the former ones, divisible into two groups—a semi-diurnal group,

[1] A paper or lecture delivered to the Cambridge Philosophical Society on February 23, 1891. A few passages in it have already appeared in NATURE, in the account of the Bakerian Lecture delivered to the Royal Society on January 29, 1891. (See Phil. Trans. Roy. Soc., 1891.)

each of which would, by itself, make high water twice in a period not much different from a day, and a diurnal group, giving high water about once a day.

These tidal spheroids are merely specifications of the tidal forces, and may be used in the actual case with the same confidence as if the sea covered the whole earth, and as if the system were at rest. The spheroids, in fact, indicate that if the globe were covered with water, and rotated very slowly, there would be regularly periodic rises and falls of water equal to those given by each of the ideal equilibrium spheroids. But the determination of the oscillations of a sea of variable depth and irregular outline on a rotating planet quite surpasses the power of mathematical analysis, even when the generating forces are regularly periodic. It can only be asserted that at each place there will actually occur a regular rise and fall of the sea with unknown amplitude, exactly co-periodic with the passage of the equilibrium spheroid, and that high water will follow the crest of the ideal equilibrium spheroid by some unknown but constant interval. It may, however, be certainly concluded that if two spheroids of the same class—both semi-diurnal, or both diurnal—move with nearly equal speed over the earth, then the heights of the corresponding high waters will be nearly proportional to the ellipticities of the respective spheroids, and the intervals of retardation will be nearly equal to one another. It follows from this law that an equilibrium spheroid, which varies slowly in ellipticity, and which moves nearly uniformly over the earth, will correspond at any place with a real oscillation of the sea, of amplitude nearly proportional to the varying ellipticity, and with nearly constant retardation of high water behind the passage of the crest of the equilibrium tide. The more rapidly the ellipticity varies, and the more irregular the motion of the spheroid, the less accurate does this law become.

Now this brings us to consider two methods of treating tidal observation and prediction—namely, the synthetic and the analytic methods.

In the synthetic method all the semi-diurnal spheroids are added together to form a resultant luni-solar semi-diurnal spheroid, and a similar recomposition of the other group gives a resultant luni-solar diurnal spheroid. These two resultant spheroids are found to move over the earth with some approach to uniformity, and their ellipticities vary with moderate slowness. It is, then, assumed that at any port the heights of the semi-diurnal and diurnal tide-waves will vary *pari passu* with the ellipticities of the two spheroids, and that the retardations in time will be nearly constant.

In the analytic method as many tidal spheroids are considered as are wanted to give a sufficiently accurate representation of the tidal forces. Each one of these constituent spheroids is then known to give rise to a mode of oscillation of the sea, which at any port is constant in amplitude and in retardation.

In both plans astronomical considerations are essential: in the synthetic method the laws of the variation of the ellipticity and of the speed of motion are dependent on the positions of the moon and sun; and in the analytic method we have to determine how many different spheroids of constant ellipticity are wanted to build up the resultant tidal spheroid with sufficient accuracy, and what are their various uniform velocities over the earth's surface.

In the synthetic method there is a single semi-diurnal and a single diurnal spheroid, and the singleness of the spheroids is the important consideration.

In the analytic method the number of spheroids is immaterial, whilst the constancy of their ellipticities and the uniformity of their motions are the objects aimed at.

On account of this difference of aim, it is necessary to fix the positions of the moon and sun in different ways in the two methods. In the synthetic method the positions are determined by angles which bear a simple relationship to the earth's surface; in the analytic method by angles which increase at a uniform rate.

A point on the earth is fixed by its longitude and latitude, but when this system is applied to a celestial object, its longitude, measured from the meridian of the place under consideration, is called the local hour angle, and its latitude is called the declination. Accordingly, in the synthetic method the semi-diurnal and diurnal spheroids have their ellipticities and velocities specified by means of the hour angles, declinations, and linear distances (or horizontal parallaxes) of the moon and sun. There are apparently six variables, but as both the sun's declination and its parallax depend on the time of year, they are equivalent to only five variables.

The principal variations of the total semi-diurnal spheroid depend on the two hour angles, but there are also subordinate changes due to variations in the two declinations and the two parallaxes.

In the case of the diurnal spheroid we must regard not only the two hour angles but also the moon's declination as principal variables, whilst the changes due to the sun's declination and to the two parallaxes remain subordinate.

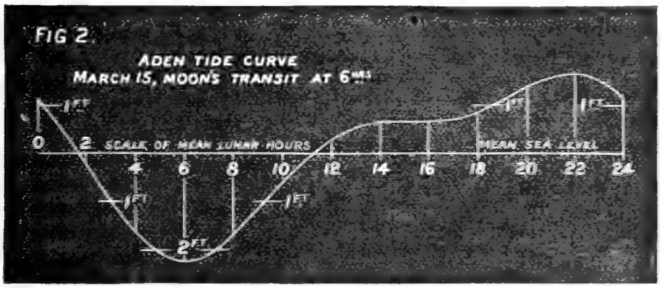
It happens that on the eastern coasts of the North Atlantic the diurnal spheroid gives rise to very little oscillation of sea-level, and therefore in Europe the two hour angles may be regarded as principal variables, and the two declinations and parallaxes as subordinate ones. The apparent time of the moon's transit being only another name for the excess of the sun's hour angle over the moon's, it is easy to regard the time of moon's transit and the interval thereafter as the two principal variables, instead of the hour angles themselves. The passage of the luni-solar equilibrium semi-diurnal spheroid over the meridian of any place bears an intimate relationship to the time of moon's transit at that place; accordingly, in the synthetic method, the interval after moon's transit and the height of rise are given as specifying the tide. The interval and height are afterwards corrected according to the declinations and parallaxes of the two bodies.

At places where the diurnal tide is small, the synthetic method possesses a great advantage—namely, that the inequalities in the height and interval are so regular in each half lunation, that a single table arranged according to the hour of moon's transit, is sufficient; there are, of course, also tables of corrections arranged according to the declinations and parallaxes of the moon and sun.

We have seen, however, that as concerns the diurnal spheroid, the moon's declination should rank as a principal variable, and the application of the synthetic method to non-European ports has been an attempt to keep the moon's declination down to its supposed proper position as a subordinate variable. In this attempt the corrections have shown an ever-increasing tendency to complication, and there have been constructed correctional tables depending not only on the two declinations and two parallaxes, but also on the rates of change of the moon's right ascension, declination, and parallax. Even with all this care the results are not easily made satisfactory for places with a large diurnal tide. As late as 1840, Whewell had not realized that European tides are abnormal in their simplicity. In a later memoir he was evidently surprised at the importance of the diurnal tide in other parts of the world, but he seems always to have regarded it as a matter to be treated by means of corrections.

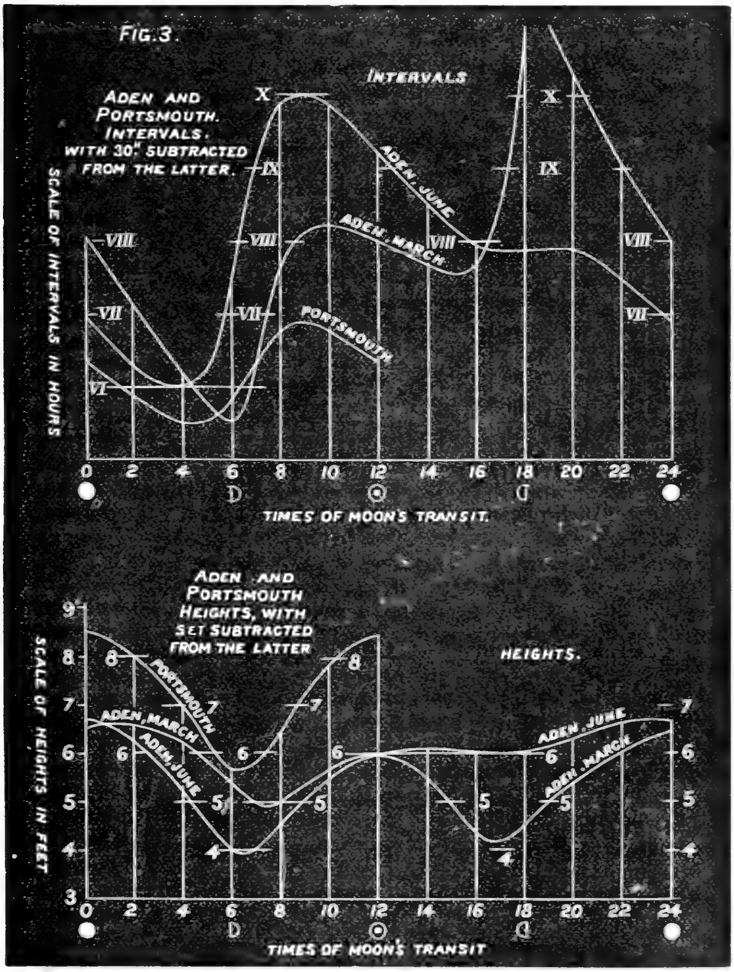
In order to prove how futile corrections must be in such cases, I refer to Fig. 2, which exhibits the rise and fall of water during one day at Aden when the moon crosses the meridian at 6 p.m. in the middle of March. In the synthetic method we should have to suppose as a first approximation that there were two equal high waters and two equal low waters in the day. In this tide curve traces of the second high water and of the second low water are seen in the lingering of the water a little above mean water-level during about four hours—namely, from 12½h. to 16½h.

The same thing is shown in even a more striking way



by Fig. 3, which gives the fortnightly inequality in the height and interval during any half lunation at Portsmouth, and also shows the same inequalities during two different whole lunations at Aden.

In these figures the horizontal line is a scale of the



times of the moon's meridian passage. When the transit is at 0h. or noon, it is change of moon; when at 12h. or midnight, it is full moon; and transits at 6h. and 18h. are the half moons, waxing and waning. In the curves of intervals the ordinates are the number of hours after moon's transit to high water; and in the curves of heights the ordinates give the number of feet of rise at high water.

The Aden curves only show the height and interval for the high water which follows the moon's upper or visible transit, and the same curves would have to be repeated with their second halves in the first place, and with their first halves in the last place, in order to give the height and interval for the high water following the moon's lower or invisible transit.

If the synthetic method were applied to Aden, we must as a first approximation regard the March curve and the June curve as identical, and the second halves of each as a repetition of the first halves. It is obvious that it is impossible to work with any pretence to accuracy on such an hypothesis.

It will be noticed that the March curve of intervals is interrupted about the time when the moon's transit is at 18h.; this means that the corresponding high water is missing. Fig. 2, in fact, has exhibited that one of the two high waters is missing. The missing or evanescent high water is the one which ought to have followed moon's transit at 18h., and the existent one follows moon's transit at 6h.[1] Great as is the difference between the two halves of the March curve of intervals, it is less conspicuous than the contrast between the two halves of the June curve.

Finally, whilst the greatest interval exceeds the least by about an hour and a half at Portsmouth, it exceeds by nearly six hours at Aden.

The contrast between the March and June curves of heights for Aden is not so striking as in the case of the intervals, but is still very great.

Enough has now been said to show the necessity for a new departure in the really normal case of a considerable diurnal tide, and the impulse was given by Sir William Thomson when he proposed the harmonic analysis of tidal observations. This is, in fact, the analytic method the principles of which have been already sketched.

In the harmonic method, about twenty equilibrium spheroids are taken as giving a sufficiently accurate representation of the tidal forces. It has already been shown that constancy of ellipticity and uniformity of velocity are the leading considerations, and that the speeds of the spheroids are to be expressed by means of angles or quantities which increase at a uniform rate. Accordingly, the speeds are now expressed in terms of the mean solar time, and of the mean longitudes of the moon, of the sun, of the lunar perigee, of the lunar node, and of the solar perigee—all of which increase at a uniform rate. Since the longitude of the solar perigee is to all intents a constant, there are five variables instead of the six of the synthetic method.

Only three of these variables—namely, the time and the mean longitudes of the moon and sun—are required to express the speeds of the spheroids of the largest ellipticity; whilst the speeds of the less important

[1] The order of these two high waters is apparently inverted in the figure, because the first ordinate corresponds to a time 7h. 54m. after moon's transit, being the moment of "mean lunar semi-diurnal high water." The high water which follows the transit at 6h., has just passed when the figure begins.

spheroids require also the longitudes of the moon's perigee and node.[1] There are, therefore, three principal variables and two subordinate ones, but no advantage is taken of this subordination, each spheroid being treated by itself.

Each spheroid, in its passage over the earth corresponds with regularly alternating tidal forces, which generate regular oscillations of the sea of unknown amplitudes and retardations. Observation of the tides at any port affords the means of determining the amplitude and retardation of each mode of motion, and, accordingly, there are two tidal constants corresponding to each equilibrium spheroid.

The separation of tidal oscillations into some fifteen or twenty distinct simple oscillations is admirable as a method of analysis and registry, but its advantages for the purpose of prediction are less conspicuous. Since high and low water are single events arising from a number of separate causes, synthesis is essential to prediction, and the computation of a tide-table involves the determination of nearly four maxima and minima per diem of an algebraic expression of fifteen or twenty terms. Dr. Börgen, of Wilhelmshaven, has attacked the problem with courage and success, but the work remains, and always must remain, very laborious. In fact, prediction in the harmonic system labours under one great disadvantage—it is very expensive.

Sir William Thomson was so conscious of this disadvantage, that, shortly after his initiation of the harmonic method, he suggested that the obstacle might be turned by the mechanical synthesis of the separate oscillations. It was in 1872, I believe, that he laid down the principles upon which a synthetic machine might be constructed. Mr. Edward Roberts, of the *Nautical Almanac* Office, bore a very important part in the realization of the idea, and the tide-predicting instrument, now in the Indian Store Department at Lambeth, was constructed by Légé, under his direction. A paper by Sir William Thomson, in the sixty-fifth volume of the Transactions of the Institution of Civil Engineers, and the subsequent discussion, contain details of the respective parts played by Sir William Thomson, Mr. Roberts, and Messrs. Légé in the realization of the idea.

The machine cost several thousands of pounds,[2] and it is the only one of its kind. It requires skill and care in manipulation, and it has been ably worked by Mr. Roberts for the production of tide-tables for the Indian Government, ever since its completion. Tables for thirty-one ports in the Indian Ocean are now being mechanically computed and published annually.

A whole lecture would be required for a full description of the instrument, and I will confine myself to a diagrammatic illustration of the principles involved.

A cord passes over and under a succession of pulleys, being fixed at one end and having at the other end a pen which touches a revolving drum. If all the pulleys but one be fixed, and if that one executes a simple harmonic motion up and down, the pen will execute the same motion with half amplitude. If a second pulley be now given an harmonic motion, the pen takes it up also with half amplitude. The same is true if all the pulleys are in harmonic motion. Thus the pen sums them all up, and leaves a trace on the revolving drum. When the drum and pulleys are so geared that the angular motion of the drum is proportional to mean solar time whilst the harmonic motions of the pulleys correspond in range and phase to all the important lunar and solar tides, the trace on the drum is a tide curve, from which a tide-table may be constructed. The harmonic motion of the pulley is given by an arrangement indicated only in the case of the lower pulley in Fig. 4. The pulley frame has attached to its vertical portion a horizontal slot, in which slides a pin fixed to a wheel. If, whilst the drum turns through one mean solar hour, the wheel turns through two mean lunar hours, the pulley executes lunar semi-diurnal oscillations. When the throw of the pin and its angular position are adjusted so as to correspond with the range and phase of the observed lunar semi-diurnal tide, the oscillation of the pulley remains rigorously accurate for that tide for all future time, if the gearing be rigorously accurate, and with all needful accuracy for some ten years of tide with gearing as practically constructed. The upper pulleys have to be carefully counterpoised, as indicated. It has not been found that any appreciable disturbance is caused by the inertia of the moving parts, even when the speed of working is high. It requires about four hours to run off a year's tides. The Indian instrument combines twenty different harmonic motions.

But, notwithstanding its admirable construction, the tide-predicting instrument mitigates rather than annihilates the disadvantages of the harmonic method. For, besides the large initial capital expenditure, the expenses of running off the curve, of the measurement of maxima and minima, of verification and of printing, are so considerable that the instrument cannot, or at least will not, be used for minor ports in remote places. It is, besides, not impossible that national pride may deter the naval authorities of other nations from sending to London for their predictions.

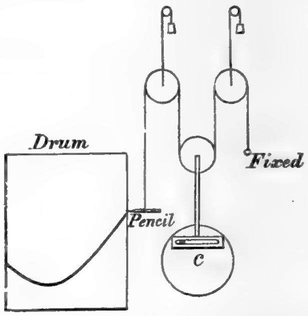


FIG. 4.—Diagram of tide-predicting machine.

There is, then, a want of other methods of forming tide-tables, if they can be devised. Now, a tide-table for any port is not necessarily of the kind produced by the machine, for such tables may be divided into two classes, which may be called the general and the special. In a special table the times and heights of high and low water are specially predicted for each day of each future year. This is the kind of table furnished by the machine, but although it is the table which a sailor likes best, it is too expensive for universal use. A general tide-table, on the other hand, merely states the law of the tides in such a form that, by reference to the *Nautical Almanac* and a little simple arithmetic, a few tides may be quickly computed for the days required.

Such tables may be computed once for all, so that the expense of producing them is of little importance; they may be printed and published along with sailing directions, and they serve for all future time.

General tide-tables have for years past been given for places where there is no diurnal tide, and where the synthetic method is easily applicable. In such a case the sailor finds the apparent time of moon's transit, and looks out in the tide-table the height and interval corresponding to that time. He then adds the interval to the mean time of moon's transit, and he has thus obtained the approximate time and height of high water. Tables of

[1] In actual use the method is partially synthetic, because the ellipticities and speeds of all the spheroids of lunar origin are taken as varying to a very small extent with the longitude of the moon's node, which completes its circuit in about nineteen years.

[2] Sir W. Thomson tells me that with a different sort of gearing the expense of construction may be much diminished.

corrections according to the moon's declination, moon's parallax, and the time of year furnish him with the means of obtaining the correct time and height.

I do not think, however, that any general tide-tables, worthy of the name, have hitherto been made for ports with a large diurnal tide, and these are the majority of all ports. It is the object, then, of this paper to show how such tables may be formed. The tables are, unfortunately, not nearly so short as when the tides are simply semi-diurnal, and the expense of computing them will be not inconsiderable, but it will be an expense incurred once for all for each port.[1] The system proposed will not render the tide-predicting instrument less useful, but it will fulfil other requirements, and will, I think, obviate the disadvantage which seemed inherent to the analytic or harmonic method.

A sketch of the manner in which it is proposed to compute these tables, and of their form when completed, will now be given.

In the complete harmonic analysis of the tides there are three principal variables—namely, the mean solar time and the mean longitudes of the moon and sun; and two subordinate ones—namely, the mean longitudes of the lunar perigee and node. In order to effect a partial synthesis of the harmonic spheroids, I introduce the conception of a fictitious satellite which moves along the equator in exactly the same way as the moon moves along the ecliptic, and which coincides with the moon at the moment that she is in the equator.[2] The time is counted from the moment at which the fictitious satellite crosses the meridian of the place of observation, so that, for each high water, time is counted from a different epoch; thus, mean solar time is replaced by the interval since fictitious transit: this is the first of the principal variables. The second variable is the moon's phase counted in degrees from $0°$ to $360°$, or, what amounts to the same thing, the excess of the moon's true longitude above the sun's. The third variable is the time of year, or, speaking exactly, the sun's true longitude. For the subordinate variables, the longitude of the moon's node is retained as one, whilst the excess of the moon's parallax above or its defect below its mean value is taken as the other.

By means of this change of variables, the fifteen or twenty true harmonic spheroids may be replaced by three semi-diurnal and three diurnal spheroids, whose ellipticities vary within very narrow limits, and whose speeds over the earth's surface are nearly uniform. Four of these six spheroids are subject to small corrections for the variability of the moon's parallax and for the longitude of the moon's node; and the two remaining spheroids should, in strictness, be subject to corrections for the sun's parallax, which are, however, so small that they may perhaps be neglected.

The transition from the equilibrium spheroids to the actual oscillations of the sea is made by means of the harmonic tidal constants, which are supposed to have been found by the reduction of tidal observations.

Then, for any given time of year I compute the interval from fictitious transit to high water, and the height, for all phases of the moon; certain factors are also computed, by means of which the nodal and parallactic corrections to the time and height may be found.

If these computations were made at frequent intervals of the year and of the moon's phase, we should have the desired general tide-table, but it would not be given in a very convenient form, because the time of fictitious transit is not given explicitly in the *Nautical Almanac*.

The next step is, therefore, to find formulæ for the interval of time between a fictitious transit and a moon's real transit, and for the moon's phase in terms of the time of moon's transit. By means of these formulæ the tables are transformed, so that we get a table of intervals after moon's transit arranged according to the hour of transit. The table resembles that in use in the synthetic method, save that we have not to consider the moon's or sun's declinations, and that there are only two correctional tables; it is necessary, however, to compute for all times of year.

I had hoped that short tables computed at intervals of a month would have sufficed for practical purposes, but when the diurnal tide is large, the changes in the height and interval are so abrupt and so large that a skeleton table would give very inaccurate results, unless used with elaborate interpolation. We can clearly not expect sailors to use tables of double entry, in which interpolation to second and even third differences is required, and indeed all interpolation is objectionable. I therefore propose that the tables shall be made so full as to obviate interpolation; tables for about every ten days, and for every 20m. of moon's transit, seem to satisfy the requirements of the case.

The computation would run to a great length if the calculation had to be made in full, but it is fortunately possible to reduce the work to a great extent.

It appears that the height, interval, and corrections for any tide occurring after a moon's transit at any hour and at any time of year, are nearly identical with the same quantities for the tide occurring after a moon's transit twelve hours later and six months later. Thus the tables need only be formed for the half instead of for the whole year. Further, it is found to give an adequate insight into the law of tide, if the computations are actually made for each month and for each hour of transit. Since there are six months and twenty-four hours of transit, the computations are made for 144 heights, intervals, and correctional factors. There are other artifices by which the work is abridged, but into which I will not enter.

The interpolations necessary for the completion of the tables for every 20m. of moon's transit, and for every ten days, may be made with sufficient accuracy by means of curves. Although this work is tedious, it is better to throw it on the professional computer, who does it once for all, rather than on the sailor, who would do it piecemeal and many times over on various occasions. It has occurred to me that the most succinct form in which the final results could be published would be by means of these curves without tabulation, but ten or twenty pages of a book convey the information in a much more portable form than an atlas of curves, so that I am doubtful of the advisability of the graphical form of tabulation. I may here remark that although the general tide-table is primarily intended for use in the general form, it may also be used for the computation of a special tide-table without heavy expense.

Such a piece of work as this can only be deemed complete when an example has been worked out to test the accuracy of the tidal prediction, and when rules have been drawn up for the arithmetical processes, forming a complete code of instructions to the computer.

I chose the port of Aden for the example, because its tides are more complex and apparently irregular than those of any other place which, as far as I know, has been thoroughly treated. The tidal constants for Aden are well determined, and the annual tide-tables of the Indian Government afford the means of comparison between my predictions and those of the tide-predicting instrument. The arithmetic of the example was long, and the plan of marshalling the work was rearranged many times. An ordinary computer is said to work best when he is ignorant of the meaning of his work, but in this kind of tentative work a satisfactory arrangement cannot be attained without a full comprehension of the reason of the methods. I was, therefore, very fortunate in securing the enthusiastic assistance of my friend Mr.

[1] I believe that it may cost about £50 to compute my table, but it is impossible as yet to give any precise estimate.

[2] The rigorous definition of the fictitious satellite is that its right ascension is equal to the moon's true longitude measured in her orbit.

Allnutt, and I owe him my warm thanks for the laborious computations he carried out. After computing fully half of the original tables, he made a comparison for the whole of 1889 between our predictions and those of the Indian Government.

The theory of the several methods of tidal prediction has hitherto taken up all our attention, and the reader will naturally be interested to know what degree of accuracy is attained by all this labour.

Captain Wharton has kindly supplied me with a comparison between the predictions, made by the synthetic method, for Portsmouth, and the curves registered by the tide gauge for the month of August 1886.

I find that the errors are as follows:—

Time: Magnitude of error. m. m.	No. of cases.	Height: Magnitude of error in inches.	No. of cases.
0 to 5	25	0, 1, 2, 3	16
5 to 10	21	4, 5, 6	16
10 to 15	10	7, 8, 9	13
17	1	10, 11	8
32	1	13, 14	3
		16	1
		18	1
	58		58

The table of errors in the height, however, gives much too favourable an impression of the success of the prediction, for the prediction is so generally higher than the actuality, that the mean of all the predictions is 5½ inches higher than the truth.

I find that the probable error in the time is 4m.; that is to say, half the actualities fall within 4m. on each side of the prediction. Again, allowing 6 inches for the systematic error in the heights, I find that the probable error in the heights is 3½ inches. The average amplitude of oscillation of sea-level at spring tide (or the spring rise) is 13 feet 6 inches, and at neap tide (or the neap range) is 7 feet 9 inches. The errors in height are therefore not considerable compared with the total changes.

Let us next consider a case where the predictions were made by the tide-predicting machine. Colonel Baird sent me some time ago a comparison, made in the office of the Survey of India, between the Aden predictions and actualities for the year 1884.[1]

Taking the month of January as probably a fair sample, I find that the errors of the 57 high water predictions of that month were as follows:—

Time: Magnitude of error. m. m.	No. of Cases.	Height: Magnitude of error. in.	No. of cases.
0 to 5	24	0	8
5 to 10	24	1	17
10 to 15	7	2	9
19	1	3	11
21	1	4	7
		5	5
	57		57

Mr. Allnutt determined the probable errors in time and height of the 698 high water and 698 low water predictions for the whole year, and found ± 4m. in time, and ± 2 inches in height, for both high and low waters.

On account of the magnitude of the diurnal tide at Aden, it is useless to state the spring rise of water, but about spring tide the range from high water to low water often amounts to 6 feet 6 inches or 7 feet. Hence, we may consider that an error of 1 inch at Aden has about the same relative importance as 2 inches at Portsmouth. Although the tidal constants of Aden were not so well determined in 1884 as they are now, I think the Aden predictions are better than those at Portsmouth—at least if the month of August 1886 is a fair sample of the latter port. It is fair, however, to state that the disturbance due to the irregular winds of our latitudes is very considerable at Portsmouth;[1] whereas the effects of the regular day and night breezes which occur at such a place as Aden are included in the tide-table.

It is a remarkable fact, at once creditable to the Indian Government and discreditable to all others, that the tides round the enormous range of coast of the Indian Ocean are better known than over any other area in the world.

Finally, let us consider our general tide-table, which has as yet only been worked out for high water at Aden. For the purpose of comparison, I computed a tide-table from March 10 to April 9, and from November 12 to December 12, 1884. In these periods there are 117 actual high waters, and one evanescent high water. Where a tide is evanescent—that is to say, when there is no rise of water—the general tide-table necessarily assigns a definite time for high water, although the rise of water may be *nil*. The following table classifies the errors of the (retrospective) predictions, without reference to signs:—

Time: Magnitude of error. m. m.	No. of cases.	Height: Magnitude of error. in.	No. of cases.
0 to 5	35	0	22
5 to 10	34	1	42
10 to 15	23	2	26
15 to 20	10	3	17
20 to 25	7	4	3
25 to 30	3	5	5
39 to 46	4	6	1
108	1	7	1
Evanescent	1	Evanescent	1
	118		118

Omitting the cases of evanescence and of 108m. (which will be shown to be justifiable), the probable errors are 8m in time,[2] and 1·4 inches in height.

When the rise from the higher low water to the lower high water is *nil*, there is evanescence, and when that rise is small there is approximate evanescence.

I have accordingly examined the 24 cases in which the time-error is greater than 15m. (there being two cases of 15m. error exactly), and the following table gives the results:—

Rise from L.W. to H.W.	Time errors.
Nil	(Evanescence)
2 in.	108m.
6 in. to 11 in.	22, 22, 39, 44, 46m.
14 in. to 18 in.	16, 17, 22, 28, 29, 40m.
19 in. to 29 in.	16, 18, 18, 18, 19, 19, 20, 21, 21, 22, 22m.
3 ft. 6 in.	25m.

When we consider that the rise often amounts to 7 feet, all these tides, excepting the last, must be considered as approximating to evanescence. I suspect that the last of these errors must have been due to wind.

We have seen in Fig. 2 the sort of hang there is in the water in the case of an evanescent tide, and, accordingly,

[1] I have recently learnt that the method of comparison in use at that time was such that there was a bias in favour of the prediction. It is not easy to read the time of high water on a tide-curve with accuracy, and previous knowledge of the predicted time gives the observer a bias in favour of a particular reading. This faulty method of testing the predictions has long since been superseded.

[1] I have no space to discuss another aspect of tidal prediction—namely, the influences of wind and weather on the tides; this is in itself a subject of considerable magnitude, and one which is not yet completely understood.

[2] There are circumstances connected with the alleged time-actuals for November 1884, which appeared to demand further inquiry, and I wrote to India accordingly. Colonel Hill, R.E., has now sent me a re-measurement of the tide-curves, but it is not possible to complete the examination of the results in time for this paper. I may state, however, that the comparison in the text leads to a substantially correct estimate.—*March* 20, 1891.

in approximate evanescence an error of half an hour, or even an hour, is quite insignificant.

If these 24 cases be subtracted, the probable error in the time falls to 7m.

These retrospective calculations are distinctly better as regards height than those of the Indian Government, but are worse in the times. It is proper, however, to remark that better values of the tidal constants were used than were accessible in 1884.

The comparison between our predictions and the Indian ones made for the whole of 1889 by Mr. Allnutt, led to conclusions very similar to those just explained.

It may be concluded from these comparisons that highly satisfactory tide-tables may be made from our general tide-tables, when the tidal constants are accurately known.

If the general tide-table were computed in the full form, both for high water and for low water, it would cover 36 pages of rather small type. But at many places it may not be thought worth while to proceed to the full degree of accuracy. If the computation were made for every half-hour of moon's transit, and for every fortnight, the table would be reduced to 16 pages; and if high waters only were given, to 8 pages.

I believe, however, that this is not the direction in which abridgments may be best made, but that it would be more advantageous to drop some of the corrections. Thus the correction to the height, which depends on the longitude of the moon's node, and a certain small correction to the time, which depends on a portion of the equation of time (of which no mention has been made), might be dropped without much loss of accuracy. If the tables are still supposed to be half-hourly and fortnightly, these omissions might reduce them to 6 pages. At some ports a rough high water tide-table might even be contained in 2 or 3 pages, but in this case it might be necessary to add a warning of the possibility of sensible error in the times and heights of the lower high water.

The question of how far to go in each case will depend on a variety of circumstances. The most important consideration is, I fear, likely to be the amount of money which can be spent on computation and printing; after this will come the degree of desirability of a trustworthy tide-table, and the accuracy with which the tidal constants are known—for it is obviously useless to form a nominally accurate tide-table from inaccurate values.

My aim has been to reduce the tables to a simple form, and if, as I imagine, the mathematical capacity of an ordinary ship's captain will suffice for the use of the tables, whether in full or abridged, the principal object in view has been attained. G. H. Darwin.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

SUFFICIENT time has elapsed since the dismissal of the International Congress to permit a deliberate view to be taken of the results of the meeting, and to determine how far the prospects of the photographic chart have been improved by the resolutions which the Committee has entered on its minutes. But there is the preliminary consideration, that it is now four years since the scheme was definitely conceived and resolved upon. Have those four years been profitably employed? Doubtless the initiatory steps, such as procuring the necessary instruments, and acquiring facility in their use, as well as a knowledge of their capacities, have occupied a longer time than was originally anticipated, but seen in the light of a fuller experience the completion of so many details in a shorter time was not warranted. It is a matter of legitimate congratulation that all the participating Observatories are adequately and similarly equipped, and that a work undertaken in hope, rather than founded on experience, is moving surely and certainly to a successful issue, and strictly on the lines originally contemplated. The early resolutions of the Committee were not guided by trial and practice, and it is fortunate that so little has had to be altered, and that the scheme in its integrity remains practically as it was conceived. The words with which Admiral Mouchez closed the Conference on the fourth day of debate happily described the situation, and the attitude of the various members of the Committee. "You have had to treat difficult questions," said he; "you have treated them without reference to any other thought than that of truth, and the progress of science. Divergences of views have exhibited themselves as to the means to be employed, never as to the end to be attained. These divergences were inevitable; they were even necessary; they have only made us see the questions in a clearer light, and have never disturbed the cordiality of your relations. The unanimity which has marked your final decisions is a certain pledge of ultimate success. It is in this conviction that I declare the Conference of 1891 closed, and the work of the photographic chart of the heavens commenced."

The keynote of one of these "divergences of views" was struck by the Admiral himself in his opening address, and the subsequent debate led to the exhibition of considerable excitement on the part of individual members of the Committee, which has been commented on elsewhere. It is well known that under the auspices of the International Committee it is proposed to effect two objects, each very desirable in itself. One of these, the one which really called the Committee into existence, is the photographic representation of the present condition of the heavens, in which shall be delineated stars which, under a certain definition, are called the fourteenth magnitude. The other, a more recent thought, is the preparation of a catalogue in which shall be exhibited the co-ordinates of stars down to the eleventh magnitude, on the scale of Argelander's zones. The question arose, as Admiral Mouchez pointed out, whether these two distinct and separate objects should be proceeded with *pari passu*, or whether, since a shorter time would be sufficient to furnish the plates from which the catalogue would be deduced, it was desirable that the data for the catalogue should be supplied before the more arduous undertaking of the chart plates should be commenced. Admiral Mouchez, in his opening address, clearly indicated the advisability of the latter method of proceeding, and adduced very excellent arguments for such a course. A catalogue, as he remarked, is immediately useful, and is a want among astronomers of the present day, whereas the chart is really intended for the benefit of future astronomers, who will value it the more in proportion to its completeness. Further, there are reasons to believe that the duration of exposure necessary to secure, on the negative, a black measurable image of a star of the fourteenth magnitude, will be much longer than has hitherto been anticipated, and that consequently a much greater expenditure of time will be necessary for the production of the plates than has generally been imagined. If, then, the two series of plates are obtained simultaneously, the shorter exposure plates will be injudiciously delayed, and the manufacture of the catalogue cannot be proceeded with till the chart plates of long exposure are completed—a course manifestly to the detriment of the present condition of astronomical science. On the other hand, if by co-operation the series of negatives of short exposure be secured in about two years, which is quite possible, the hands of astronomers will be greatly strengthened by the welcome which will be accorded to the catalogue, and they will be encouraged to undertake the more onerous part of the work. Additional experiments and information could be collected, and it is not too much to hope that some improvement in the preparation of photographic films might secure additional sensitiveness, and so facilitate and abridge the operations.

It is evident that here the lines of cleavage are saliently marked. It was inevitable that some members would feel themselves definitely pledged to the chart, and nothing but the chart; while others, looking to the applicability of photography to the rapid registration of stars, and the accuracy with which their places can be determined, would see greater immediate advantages to be drawn from the compilation of a catalogue, and would endeavour to divert the energies of the Committee into that particular channel.

The debate which followed was interesting, but turned mainly on the points indicated above. The Astronomer-Royal, who had not heard, but was probably acquainted with, the views of Admiral Mouchez, maintained the principle of the chart in its integrity, and this he could do the more boldly since he advocated but a very moderate exposure as sufficient to secure the impression of stars of the fourteenth magnitude. He admitted that, on nights when the sky was not wholly fine, it might be advantageous to take plates of short exposure, suitable for the catalogue, but that the general principle to be observed was, to take the short and long exposed plates, the one directly after the other, in order to avoid the frequent adjustment of the equatorial to the particular part of the sky to be photographed. Moreover, the Astronomer-Royal urged, and the force of the argument cannot be denied, that the Government funds had been granted with the special view of procuring the chart, properly so called, and that a breach of faith would be involved in urging forward any other work with instrumental means so provided, till that for which the grant was allowed was completed.

The proposal in favour of immediately proceeding with the catalogue plates found an able advocate in Dr. Gill, who, however, urged his entire devotion and loyalty to the completion of the chart as originally conceived. He reiterated the points that Admiral Mouchez had so clearly put in his opening address; and in addition, that it was undesirable to embark upon a work of unknown magnitude and extent, seeing that one ready to the hand, which equally involved the construction of a chart, called imperatively for their attention. Captain Abney had previously pointed out a method by which he believed the sensitiveness of the emulsion and the measurability of the images could both be improved, and Dr. Gill seized this point to urge on the Committee, with the weight due to his experience in this department of astronomical photography, the necessity of longer delay and further experiments. Other members of the Committee, with more or less originality, repeated these arguments, and in the hopelessness of arriving at a decision the President (M. Bakhuyzen) adjourned the debate, in the hope that reflection and informal interchange of opinions might disclose a *modus vivendi*.

The method pursued in this discussion has been dealt with at length because it is typical of the procedure that was adopted whenever it seemed impossible to arrive at a unanimous vote. In this case the expectation of the President was not disappointed. An informal meeting of several members of the Committee was held before the next *séance*, and the following resolution was drafted, which was carried without discussion and without dissent:—

"The work undertaken by the Congress of 1887 comprises two series of negatives made with different exposure: the permanent Committee, whilst recommending to observers to urge forward, with the greatest possible diligence, the execution of the negatives of the second category (negatives destined for the construction of a catalogue) is of opinion that they ought also to profit by the greatest possible number of fine nights to take negatives of long exposure for the first series (those of the chart)."

The resolution thus carried happily combines all sections of the Committee in a common work, and though the execution of either the chart or the catalogue was never really jeopardised, it is certain that both will go forward with greater alacrity and harmony on the part of all concerned.

In the early days of the scheme, members talked very glibly of the fourteenth magnitude, though not recklessly, for they were cautious to couple it with a definition, the accuracy of which may be now impugned, but which at the time sufficiently limited the acceptation. It was assumed, though possibly on insufficient evidence, that the exposure of a plate two and a half times that of a given exposure would insure the impression of one additional magnitude. This point has been submitted to further experiment, and it was shown at the Congress, principally by Dr. Scheiner, of Potsdam, that this law did not hold. The time required for the impression of stars brighter than the ninth magnitude is so short that there is a very great difficulty in determining it accurately. Dr. Scheiner had therefore, with a view of settling this question, carried out a series of experiments under various conditions—

(1) By employing plates of low sensibility.

(2) By covering the object-glass with a gauze screen having a very fine mesh.

(3) By reducing the aperture of the object-glass to nine centimetres, and employing both rapid and slow plates.

The result at which he has arrived after these experiments, the most complete that have been made, is that by multiplying the time of exposure by two and a half, a whole magnitude is never gained, but only a half or seven-tenths of a magnitude, and that consequently to obtain the fourteenth magnitude an exposure of *seven hours* would be necessary. Some objections may be raised to the method in which the magnitudes were determined by Dr. Scheiner, particularly to the result deduced from counting the stars. On a plate which he had exposed for eight hours he found impressed only 5689 stars, whereas it might have been anticipated that there would have been 10,566 stars. The argument drawn from the numbers counted might be valid if applied to a large portion of the sky, but on the small district covered by one plate various causes might operate to invalidate the law. Taking Dr. Scheiner's results, however, as they stand, the chart of the fourteenth magnitude is rendered impossible, and the Committee felt it necessary to adopt a method of proceeding which would render the chart not only possible, but should secure uniformity, and also permit of the scheme being carried out within a reasonable time.

By the kindness of the Paris authorities in forwarding early proof-sheets of the *procès-verbaux*, we are enabled to put before our readers a rigorous translation of the resolutions which the Congress has adopted, with unanimity, to this end.

"With the view of enabling observers to pass in a uniform and certain manner from the ninth magnitude of Argelander to the eleventh, which it is sought to obtain on the negatives destined for the catalogue, a Commission (consisting of MM. Christie, Henry *frères*, Pritchard, and Vogel) will distribute among the participating Observatories, screens, having a metallic webbing, absolutely identical for all the Observatories. These screens, placed in front of the objective of the photographic telescope, will diminish the brilliancy of a star by two magnitudes, and in the determination of the diminution of magnitude, the Commission will adopt the coefficient 2·512 for the ratio between two consecutive magnitudes."

Since there is no difficulty by various photometrical methods in determining the magnitude of a number of stars of the ninth magnitude, every Observatory will, with strict uniformity, be able to insure on its plates the impression of stars of the eleventh magnitude. And, in order to insure a reasonable limit of exposure for the chart plates, and to avoid any ambiguity which the employment of such a term as the fourteenth magnitude might produce, the Committee has further passed the following resolution :—

"The Committee indicates forty minutes as the duration of exposure for the negatives destined to form the chart, in the mean atmospheric conditions of Paris, and with plates prepared by the firm of Lumière, which are at present in use at Paris.

"The Commission mentioned above will forward to MM. Henry a screen, by means of which they will determine the time (t), expressed in minutes, required to form impressions of stars of the eleventh magnitude, starting from the ninth magnitude of Argelander. Then, for all observers who will be furnished with an identical screen, the ratio $\frac{40}{t}$ will be the factor by which the time of exposure necessary to give the eleventh magnitude is to be multiplied, in order to obtain the stars of the feeblest magnitude for the chart."

If the Congress had done nothing more than adopt these two resolutions, the meeting would have been justified. They not only render possible, but really insure, the execution of both the chart and the catalogue. In addition, however, much good work was done, both as concerns the method of measurement of the plates and the arrangements for effecting the meridian observations of the "*étoiles de repère.*" There is, too, left on their minutes this further important resolution :—

"Concerning the method of reproduction of the stars for the chart, only purely photographical processes shall be employed, to the exclusion of all other processes which demand the intervention of manual labour."

The names of Captain Abney and M. Cornu are added to those of a previously existing Committee to settle the details of the process.

It is anticipated that the metallic screens will be distributed among the various Observatories in less than two months. In the meanwhile each Observatory is at liberty to proceed at once with the catalogue plates, care being taken that stars of the eleventh magnitude are impressed. In this sense, Admiral Mouchez's words are to be understood when he declared the work of the photographic chart of the sky commenced.

MAGNETIC ANOMALIES IN FRANCE.[1]

BY M. MASCART.

[*Translation.*]

THE first magnetic observations made in France by M. Moureaux, in 1884 and 1885, at not more than 80 stations, only enabled us to sketch the magnetic characteristics of the country in their broad features. Nevertheless, these first results were sufficient to detect certain anomalies, which are independent of the well-known irregularities which occur near rocks of volcanic origin, and which show no trace of the uniformity which is generally supposed to exist elsewhere. It was therefore necessary to multiply the stations in order to obtain a representation of the facts, which, if not absolutely correct, should be at least substantially accurate.

We therefore propose to cover France with a much closer network, which shall include about 600 stations properly distributed. Up to the present the north and the north-west, or, rather, the districts penetrated by the Chemins de Fer du Nord et de l'Ouest, are nearly completed, together with the region which extends southwards from Paris to the Loire. The number of stations is now over 200. The following is a description of the first results of these new investigations.

The map of the isogonal lines, in the district under consideration, discloses two principal anomalies, the one in Brittany, the other in the immediate neighbourhood of Paris. The first of these, discovered by M. Moureaux in the year 1886, has not as yet been fully studied. It is still necessary to observe at some points between Pontivy and Morlaix, and along the coast line between the mouth of the Loire and the bay of Douarnenez. Nevertheless, it is beyond question that a centre of disturbance exists near Pontivy, where the declination, which has been determined twice, with the interval of a year between the two observations, is less than at Ploërmel, which is 90 kilometres to the east. To the west of Pontivy, the isogonals are bent towards the north-west, and are drawn together near Morlaix. They next run south, or even south-south-east, thus roughly imitating the form of the coast-line of Brittany. It is possible that when the survey of this district is completed, the primitive rocks which occur in it will serve to explain the peculiarities which observation has brought to light. It is therefore unnecessary to discuss it fully at present.

The second anomalous region, however, is of special interest, on account of the nature of the soil. The attention of M. Moureaux was called to it by the results which he obtained at Chartres on June 22, 1889. Other observations, made both in this town itself and at various other places in the district, have confirmed the accuracy of the first measurements, and have led the observer to study the phenomena in detail in order to discover the magnitude and extent of the anomaly.

According to the provisional map of the true isogonals, the direction of the lines of equal declination only becomes approximately uniform towards the northern boundaries of France. Irregularities can be detected even in the latitude of Amiens, which extend as far as the Ardennes.

The isogonal of 15° 20′, which passed through Paris on January 1, 1890, instead of extending towards Orleans, bends to the south-south-east as far as Gien, then doubles suddenly on itself, running north-west to Houdan, which is west of Paris, and finally resumes a southerly course on the geographical meridian of Chartres. The isogonals, drawn for every 10′ of declination, have all the same shape, from the English Channel as far south as the network of stations at present extends—that is, to near Cosne; and this conclusion is justified by a large number of concordant observations.

If the map of the true isogonals is compared with that in which the isogonals are assumed to be regular in form, we can deduce for every station the difference between observation and theory; and if the points where these quantities have the same value are connected, we obtain a map of the isanomalous curves of the declination.

A zone, corresponding to an excess of declination, extends on this map from the shores of the Channel (Dieppe) as far as the Loire (Cosne), and increases in intensity towards the south. The excess is 14′ at Neufchâtel-en-Bray, 19′ at Nantes, 24′ at Chevreuse, 30′ at Gien, and 36′ at Cosne. The northern extremity of this zone extends towards the east to the neighbourhood of Laon, where the excess is still 7′. Immediately to the west of this zone there is another, almost symmetrical with it, which corresponds, however, to declinations of less than normal value. Like the first, it increases in intensity towards the south. The discordance is −6′ near the mouth of the Seine, −8′ at Evreux, −10′ at Dreux, −13′ at Epernon, −18′ at Orleans. Thus, contrary to all expectation, the declination is less at Orleans than at Montargis and Gien, less at Epernon than at Paris.

The facts are consistent with the view that the north pole of the needle is attracted from both sides towards a line which passes through or near Fécamp, Elbeuf, Ram-

[1] This article is a *résumé* of a communication made by M. Mascart to the British Association at Leeds.

bouillet, and Châteauneuf-sur-Loire, making an angle of from 25° to 30° with the geographical meridian. The maps of the other elements also show effects along this line, viz. an increase in the dip, and a decrease in the horizontal force.

On comparing this anomaly with a geological map, we find that it extends exclusively over calcareous and cretaceous soils. In the region under consideration, the isogonals also present a regular deformation, which is not found over rocks of a kind to produce local effects on the compass. In the latter case the results are discordant, and it is seldom possible to represent them by curves. The totally unexpected phenomenon brought to light by this first series of observations seems due to some more general cause, the nature of which has yet to be determined. Before attempting an explanation, it will, no doubt, be necessary to study it more completely, and to extend the network of stations southwards. The irregularities in the isomagnetic lines which Messrs. Rücker and Thorpe have discovered in the south of England seem to be connected with those which M. Moureaux has observed in France.

On a Possible Connection between the Ridge Lines of Magnetic Disturbance in England and France. By A. W. Rücker.

The possible connection between the French and English surveys, referred to by M. Mascart, can only be appreciated in view of facts which have been brought to light in the magnetic survey of the United Kingdom. It may therefore be desirable that a few lines should be added as to the phenomena observed on this side of the Channel.

The most salient feature in the magnetic constitution of the south of England is the existence of a ridge line (*i.e.* a line towards which the north end of the needle is attracted) the direction of which coincides generally with that of the Palæozoic axis. It runs through South Wales, passes thence to the valley of the Thames, and deviates to the south through Kent.

Near Reading the disturbance reaches a local maximum, and from this point a spur is thrown off, which passes due south to the Channel. In the maps which illustrate the magnetic survey of Dr. Thorpe and myself, it is shown entering the sea near to, but a little to the west of, Selsey Bill.

If the direction of the ridge line described by M. Mascart as passing through Rambouillet, Elbeuf, and Fécamp, be prolonged, it cuts the English coast near Portsmouth.

Putting aside, therefore, all other considerations, we may at the very least conclude that as the directions of the English and French ridge lines intersect in the Channel, they may possibly be connected. There are, however, various arguments which increase the probability of the connection.

We have traced in the United Kingdom two main ridge lines which run through districts covered by sedimentary rocks, and where, therefore, the source of attraction, if due to rocks, must be deep seated. In both cases the general directions of the lines remain unchanged for long distances.

A line, about 150 miles long, drawn from St. Bride's Bay to Kew would fairly represent the direction of the main magnetic ridge in the south.

Two lines, each 75 miles long, drawn from Wainfleet to Market Weighton, and from Market Weighton to Ribblehead, would fairly represent the main features of the ridge line which runs from the Wash to South-east Yorkshire, and thence to Craven. They display, in fact, the same kind of continuity of direction as is shown by many mountain ranges. From Châteauneuf to Fécamp the ridge line follows an easy curve for 150 miles. The three most northerly stations lie nearly on a straight line 100 miles long, and it is quite in accord with what has been observed elsewhere that this general direction should be preserved for another 80 miles across the Channel. There are very similar magnetic indications that the Palæozoic axis extends across the Irish Channel, where it is 60 miles wide.

If this be so the direction of the French ridge alters when it approaches the English coast.

This might be expected. M. Moureaux finds that the magnetic disturbance diminishes as the latitude increases, and the disturbances in South Sussex and Hampshire are at all events not greater than those he finds in the north of France.

The declination disturbances at Neufchâtel-en-Bray and near the mouth of the Seine are $+14'$ and $-6'$ respectively; at Worthing and Ryde they are $+8'$ and $-10'$. When the line of the North Downs is passed, the phenomena are complicated by the meeting of the two lines of disturbance which run east and west and north and south respectively. That a locus of centres of attraction of diminishing intensity should be diverted by approach to another well-marked ridge line is *a priori* probable.

If, however, we take a further step, and attempt to seek a physical cause for the facts, it is noticeable that the English and French lines of disturbance meet a little to the east of the Isle of Wight, and almost in the prolongation of the great fault which traverses that island.

On the whole, there appears to be good reason to believe that the ridge line which is thrown off from the Palæozoic axis at Reading crosses the Channel, and is continued for 150 miles and for an unknown distance beyond into the heart of France. The magnitude of the declination disturbance at Cosne (the most southerly point to which it has been followed) is 36′. This is greater than any disturbance hitherto observed in England, and is surpassed only by those obtained in Scotland and Ireland relatively near to basaltic rocks. The largest similar disturbances in England were found at Melton Mowbray and Loughborough, on good ground, but not far from the igneous rocks of Charnwood Forest. M. Moureaux's future investigations will, therefore, be followed with interest on this side of the Channel. Valuable evidence may be gained as to the causes of these disturbances when the clue he is following southwards has been followed to the end.

NOTES.

At the next evening meeting of the Zoological Society, on Tuesday, May 5, a special conference on the fauna of British Central Africa will be held, in order to encourage the new Commissioner, Mr. H. H. Johnston, C.B., and his associates in their exploration of that country. The subject will be introduced by the Secretary, but many other members are expected to take part in the discussion. Mr. Johnston has already left for Nyassaland, but his naturalist, Mr. Alexander Whyte, and his Chief of the staff, Mr. B. L. Sclater, R.E., are expected to be present at the meeting.

An important addition has been made to the Zoological Society's living collection in the shape of a fine adult specimen of the Lesser Orang (*Simia morio*) which has been received as a present from Commander Ernest Rason, R.N. The Lesser Orang was discriminated from the larger and more common form by Sir R. Owen as long ago as 1836, but this is the first example of it that has been acquired alive. It inhabits the swamps at the mouth of the Sarawak River in Borneo, where it is known to the natives as "*Mias Kassar.*" An interesting account of its habits was sent home by the late Sir James Brooke in 1836, and was published in the Zoological Society's Proceedings for that year. The Lesser Orang is distinguishable from the larger Orang by its smaller size and by the absence of callosities on the face. The present specimen has been lodged in the Ant-eater's House, next door to the compartment occupied by "Sally," the bald-headed Chimpanzee, which has now lived seven years and six months in the Society's Gardens.

THE annual meeting of the Royal Society of Canada opens at Montreal on Wednesday, May 27, the sessions being held in the buildings of the McGill University. There will be the usual local excursions, receptions, and entertainments, in addition to the more serious work of the week. The Allan Line issues return tickets at £20 to £30, the Dominion Line at £16 to £30, and the Beaver Line at £16 to £18. The Committee are engaged in the preparation of a hand-book for gratuitous circulation among intending visitors, which will include an historical account of the Society, together with other interesting scientific and local information.

THE hand of Fate has been heavy of late upon the secretariat of the Reale Istituto Veneto. The Secretary has been seriously ill, the Vice-Secretary absent from heavy domestic calamity, and now Signor G. Berchet, who has been nominated by the President to undertake the secretarial work *pro tempore*, communicates to the Correspondents of the Istitute the death of Prof. Giovanni Bizio, an eminent member, who was formerly Secretary, and held that office for seventeen years.

THIS evening, a *conversazione* will be given by the Royal Microscopical Society.

THE Council of the City and Guilds of London Institute has issued its Report for the year 1890 to the Governors. It is to the electrical department that the greatest number of students have been attracted both at the Central Institution and at the Technical College, Finsbury. At both Colleges this department is overcrowded, and the Council is able to state that all those who completed their course at the end of last session have obtained employment. Unfortunately, the success of the electrical department has been to some extent achieved at the expense of the chemical department. "The great impetus which has been given of late years to the use or application of electricity," says the Council, "has made this branch of industry particularly attractive to young men in choosing their profession. There is, however, as great a want and as great scope for well-trained technical chemists, and the Council trusts that this important department may receive a larger share of new students in the future."

MR. G. J. ROMANES, F.R.S., is one of three gentlemen who were elected last week by the Committee of the Athenæum Club, in accordance with the rule which empowers the annual election by the Committee of nine persons of distinguished eminence in science, literature, or the arts, or for public services.

A REPORT from New York says that Lieutenant Robert Peary, of the United States Navy, who has obtained 18 months' leave, is making preparations for an Arctic expedition to start about the end of May. It is being sent by the Academy of Natural Sciences, and the duty of making observations will be confided to six persons. The explorers will start from St. John's Land, on Whale Sound, and scale the glaciers near the coast, up to a high altitude where the hard snowy plains will enable them to observe the shore formation until the spring, when they will start for the North Pole.

THE Geologists' Association has made arrangements for three geological excursions—which, if the weather is favourable, ought to be very pleasant—in the month of May. The first is an excursion to Guildford, and will take place on May 2. The next will last from May 16 to 19, when the members will have an opportunity of exploring the neighbourhood of Northampton. On May 30 there will be an excursion to West Surrey.

A COURSE of six lectures on photography is to be delivered at the Central Institution, Exhibition Road, by Mr. H. Chapman Jones. The lectures will be given on Wednesday evenings at 7.30, and will be begun on May 6. The subject will be treated both from a practical and from a scientific point of view.

A COURSE of six lectures (in connection with the London Society for the Extension of University Teaching) will be delivered in the lecture-room in the Gardens of the Zoological Society, Regent's Park, on Thursdays, at 5 p.m., beginning Thursday, May 23, by Mr. F. E. Beddard, Prosector to the Society and Davis Lecturer. The subject will be "The Animals living in the Zoological Society's Gardens." The lectures will be illustrated by diagrams, as well as by specimens.

ACCORDING to *Modern Light and Heat*, of Boston, something like an exodus of electricians from the United States to Europe is now going on. Many representatives of the leading American electrical companies are here already, and "many more are to follow." "It takes Americans," says our contemporary, "to stir up business of an electrical character in Europe, as is evidenced by the big shipments now being made from this side."

IN the current number of *Cosmos* there is a good reproduction of the interesting old portrait of Columbus, recently found at Como.

THE Rugby School Natural History Society has issued its report for 1890. The number of members and associates is now larger than it ever was, and the work during the year seems to have been generally most satisfactory. The editors complain, however, that there has been a lack of interest in geology. "In the early years of the Society," they say, "this section was the most popular of all; but later it led a lingering existence, and for some years has been practically extinct. We have a valuable geological collection, accumulated in the neighbourhood; and with the increased numbers on our lists we feel justified in calling on volunteers to continue this work of our predecessors."

THE new Quarterly Statement of the Palestine Exploration Fund contains an interesting paper, by the Rev. George E. Post, on land tenure and agriculture in Syria and Palestine, and on the physical, mental, and moral characteristics of the people. In the same number, besides various other contributions, there is a valuable comparison, by Mr. James Glaisher, F.R.S., of the highest and lowest temperatures of the air, and range of temperature, in Palestine and in England in the ten years ending 1889.

MM. GAUTHIER-VILLARS ET FILS have just issued "Les Théories Modernes de l'Électricité," a translation, by M. E. Meylan, of Prof. Oliver Lodge's well-known "Modern Views on Electricity." The same publishers, with M. Léon de Thier, Liége, issue a second edition of "Leçons sur l'Électricité," by M. Eric Gerard, the first edition of which was reviewed in NATURE. A second edition of M. J. Joubert's "Traité Élémentaire d'Électricité" has been issued by M. G. Masson, Paris.

TWO Annual Reports of the Board of Regents of the Smithsonian Institution have lately been issued. One is for 1887-88, the other for 1888-89—counting the year from July to July. In each case Dr. Langley, the Secretary, is able to give a very satisfactory account of work done—work of which any institution might be proud. After the Report, each volume includes, as usual, a number of valuable scientific papers. In the volume for 1888 there are summaries of progress in various branches of science, a series of miscellaneous papers, and several biographical memoirs. In the volume for 1889 the best papers are reprints of writings by British and German men of science.

The Report of the U.S. National Museum, under the direction of the Smithsonian Institution, for the year ending June 30, 1888, has also been issued. It is presented in a massive volume, which takes in, besides the report of the assistant secretary in charge of the Museum, reports of the curators, papers describing and illustrating the collections, a bibliography of the institution during the year, and a list of accessions. The papers relating to the collections belong chiefly to the department of anthropology, and are of more than usual interest. They are carefully illustrated. In one of them Ensign A. P. Niblack, of the U.S. Navy, gives an elaborate account of the Coast Indians of Southern Alaska and Northern British Columbia. Mr. Walter Hough deals with the fire-making apparatus in the Museum, and Mr. P. L. Jouy with Corean mortuary pottery. Mr. Thomas Wilson contributes a study of prehistoric anthropology, and an essay on ancient Indian matting. He also presents the results of an inquiry as to the existence of man in North America during the Palæolithic period of the Stone Age.

Among the contents of the current number of the Journal of the Straits Branch of the Royal Asiatic Society, is a paper on the Sphingidæ, or hawk moths, of Singapore, by Lieut. H. L. Kelsall, R.A. Mr. H. N. Ridley contributes papers on the Burmanniaceæ of the Malay Peninsula; on the so-called tiger's milk, "Susu Rimau," of the Malays; and on the habits of the red ant commonly called the Caringa. These ants, although very ferocious, are remarkably intelligent; and Mr. Ridley gives a striking account of the way in which they make leaf nests. They have also great courage, and do not scruple to attack any insect, however large. Mr. Ridley once saw a fight between an army of Caringas, who tenanted the upper part of a fig-tree, and an advancing crowd of a much larger kind of black ants. The field of battle was a horizontal bough about 5 feet from the ground. The Caringas, standing alert on their tall legs, were arranged in masses awaiting the onset of the enemy. The black ants charged singly at any isolated Caringa, and tried to bite it in two with their powerful jaws. If the attack was successful, the Caringa was borne off to the nest at the foot of the tree. The red ant, on the other hand, attempted always to seize the black ant and hold on to it, so that its formic acid might take effect in the body of its enemy. If it got a hold on the black ant, the latter soon succumbed, and was borne off to the nest in the top of the tree. Eventually the Caringas retreated to their nest. The last to go had lost one leg and the abdomen in the fight; nevertheless, Mr. Ridley saw it alone charge and repulse three black ants one after the other, before it left the field.

The Meteorological Council have just published an atlas of cyclone tracks in the South Indian Ocean, from information collected by Dr. Meldrum, of Mauritius, during a period of thirty-eight years, from 1848 to 1885 inclusive, with the exception of three years for which no reports of cyclones were received. The tracks are represented in two sets of charts, one set showing the distribution in each year, and the other grouping the storms according to months, excepting for August and September, in which months no cyclones were recorded. In dealing with these cyclones, Dr. Meldrum has divided them into progressive and stationary. It is admitted, however, that some of the latter may have moved, but that their progress may not have been detected from lack of observations. The relative frequency of both classes of storms for the whole period is very small, varying from 1 in 18 years for July, to 5 in 3 years during February and March; but, although the number of storms is so small, it does not appear likely that many have been missed, considering the untiring persistence with which Dr. Meldrum has pursued his investigations. The tracks of the several cyclones will afford much valuable information, and lead to a better knowledge of the latitude in which the recurvature of the storms in that ocean takes place. A cursory examination shows that the range of latitude over which the points of recurvature extend varies considerably, being from about 15° to 25° S.

The Meteorological Department of the Government of India has published Part 3 of "Cyclone Memoirs," containing an elaborate discussion of the two most important storms in the Bay of Bengal during the year 1888—viz. those of September 13–20 and of October 27–31—and also of the cyclone in the Arabian Sea of November 6–9, 1888, accompanied by tables of observations during and before the storms and by 29 plates. The following is a very brief *résumé* of some of the more important conclusions arrived at by Mr. Eliot with regard to these storms, and with regard to cyclones generally in India: (1) that the difference of intensity in different quadrants is chiefly due to the fact that the humid winds which keep up the circulation enter mainly in one quadrant; (2) that the ascensional movement is usually most vigorous in the advancing quadrant, a little distance in front of the centre; (3) in consequence of this, and of rainfall taking place most vigorously in front of the cyclone, the isobars are oval in form, and the longest diameter coincides approximately with the direction of the path of the centre; this is not in the middle of the diameter, but at some distance behind; (4) that the cyclonic circulation cannot be resolved into the translation of a rotating disk or mass of air, and that its motion is somewhat analogous to the transmission of a wave; (5) that the direction of advance of these storms is mainly determined by rainfall distribution, and there is a marked tendency for storms to form in and run along the south-west monsoon trough of low pressure; (6) the lie of this trough depends upon the relative strengths and extension of the two currents.

At the Iowa Experiment Station great efforts were made in the spring of last year to find out the best way of preventing the striped squirrels from taking corn. According to Mr. C. P. Gillette, some notes by whom are printed in *Insect Life* (U.S. Department of Agriculture), the corn was treated in the following ways:—Smoked with meat in an ordinary smoke-house until the kernels were black; smoked in a barrel with tobacco dust; smoked over night in strong decoctions of tobacco and of quassia chips; soaked in a dilute carbolic acid mixture, in strong alum water, in salt water, and in kerosene. The squirrels would take the corn treated in any of these ways, though the carbolic acid treatment and the smoking with tobacco made the corn distasteful, and when in the vicinity of other grain would be left till the last. The best remedy seems to be to harrow the ground immediately after planting to cover the planter tracks, and then to scatter corn about the border of the fields and in the vicinity of the squirrel holes as soon as the corn begins to come up.

We have received the first number of the *London and Middlesex Note-book* (Elliot Stock). It is edited by Mr. W. P. W. Phillimore, and will be published quarterly. The periodical is likely to be very welcome to all students of the local history and antiquities of the cities of London and Westminster and the county of Middlesex.

During May, the lectures at the Royal Victoria Hall will be as follows:—5th, Dr. W. D. Halliburton, skin and bones; 12th, Mr. F. H. Blandford, silk and silkworms; 19th, Rev. J. Freeston, Galileo; 26th, Mr. W. North, the physiology of a dinner.

A paper upon the salts of the sub-oxide of silver is contributed by M. Güntz to the current number of the *Comptes rendus*. The question of the existence of these salts has been much discussed of late years, but very little trustworthy experimental evidence has been hitherto forthcoming. M. Güntz now claims to have prepared the sub-fluoride Ag_2F, sub-chloride Ag_2Cl, sub-iodide Ag_2I, and sub-sulphide Ag_4S. The sub-fluoride, Ag_2F, was described by M. Güntz about a year ago. It was obtained in the form of a crystalline powder, resembling bronze filings in appearance, by the electrolysis of a saturated solution of silver fluoride by means of a very strong electric current. These crystals of the sub-fluoride are unalterable in dry air, but are more or less rapidly decomposed by moisture. Water itself effects the decomposition at once with evolution of heat, one equivalent of silver being precipitated, and one molecule of ordinary silver fluoride passing into solution. This well-defined crystalline salt serves for the preparation of the others. When dry hydrochloric acid gas is led over the sub-fluoride the latter rapidly changes, becoming coloured a deep violet tint, and gradually becoming converted into the sub-chloride, Ag_2Cl. The vapours of the chlorides of carbon, silicon, and phosphorus act very much better than hydrochloric acid, fluorides of carbon, silicon, or phosphorus, and comparatively pure sub-chloride of silver being produced. Similarly, by leading a current of gaseous hydriodic acid over the sub-fluoride, the sub-iodide, Ag_2I, is obtained, the reaction in this case being accompanied by a large rise of temperature. The sub-sulphide, Ag_4S, is also obtained in like manner by passing sulphuretted hydrogen over the sub-fluoride. It is of considerable interest, moreover, that if the sulphuretted hydrogen is replaced by water vapour, and the tube containing the sub-fluoride is heated to 160°, the sub-oxide itself, Ag_4O, is obtained as the product of the reaction. As regards the initial preparation of the sub-fluoride, it is somewhat important to note that if a weak current is employed, and the temperature of the saturated solution of silver fluoride prevented from rising, only metallic silver is deposited at the negative pole. But if, on the contrary, a very strong current is employed, such as will cause considerable heating effect, the bronze-like crystals of the sub-fluoride make their appearance. M. Güntz announces that he now proposes to compare his sub-salts thus prepared with the products of the action of light upon the normal salts, a comparison which can scarcely fail to lead to results of interest from a photographic standpoint.

THE additions to the Zoological Society's Gardens during the past week include a Sooty Mangabey (*Cercocebus fuliginosus* ♂) from West Africa, presented by Mr. F. J. Bennett; a Brown Howler (*Mycetes fuscus*) from Brazil, presented by Mr. E. Luxmore Marshall; an Azara's Agouti (*Dasyprocta azaræ*) from Brazil, presented by Lord Hebrand Russell, F.Z.S.; two Wild Swine (*Sus scrofa*) from Spain, presented by Mr. Alex. Williams; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, a Rock-hopper Penguin (*Eudyptes chrysocome*) from the Falkland Islands, presented by Mr. H. B. Bingham; two Ring-necked Parrakeets (*Palæornis torquatus*) from India, presented by Miss E. Ogilvie; a Common Barn Owl (*Strix flammea*), British, presented by Mr. H. Bendelack Hewetson; a Common Fox (*Canis vulpes*), two White Pelicans (*Pelecanus onocrotalus*) from Southern Europe, deposited; an Egyptian Cat (*Felis chaus*) from North Africa, a Nankeen Night Heron (*Nycticorax caledonicus*) from Australia, a Great-billed Tern (*Phaethusa magnirostris*) from South America, purchased.

OUR PRESENT KNOWLEDGE OF THE HIMALAYAS.

THIS was the subject of an able paper read at Monday's meeting of the Royal Geographical Society, by Colonel H. C. B. Tanner (Indian Staff Corps), who for many years has been one of the officers of the Indian Survey, most of his time having been spent in various parts of the Himalayas from north-west to south-east. The paper was illustrated by a large number of admirable drawings by the author, which afforded an excellent idea of the physical and picturesque aspects of this great mountain system.

With regard to avalanches, Colonel Tanner stated that they play a great part in the conformation of the topography—a greater part, indeed, than is generally supposed, and this factor has not received the attention it deserves at the hands of geologists.

"I became acquainted," he said, "with four distinct kinds of avalanche, which, perhaps, are called by distinctive names by mountaineers, though I have been unable to ascertain them. The first, and the most common, is the precipitation of a mass of new snow from slopes which, from their steepness, are unable to retain more than a limited quantity of snow on them. They occur generally in winter and in early spring, and are the cause of the results just described. The second kind of avalanche is a descent of *old* snow, which is loosened by the heat of the sun. They may be heard throughout the summer and autumn, and are dangerous from the unexpected and irregular manner in which they slide off. The sportsman and traveller should guard against them by intelligently placing his camp in some sheltered spot out of their reach. This class is not usually of any great extent or weight, but such avalanches are of constant occurrence. The third kind can only be seen when the mountains are of peculiar formation or structure, and are really ice and not snow avalanches. They are of very constant occurrence in some localities, more particularly where small glaciers are situated high up on the crest of mountains, and are gradually pushed over the edge. In Lahaul, in the company of a friend, we watched the face of the well-known Gondla cliffs from the right bank of the Chandra River, and saw a number of these ice-falls, which came down every few minutes, filling the air with the noise of the loosened rocks and ice-blocks. The fourth kind of avalanche is one that I have only once seen, and have never known described. It is very curious, being the movements of billions of snowballs, which, in a stream a mile or half a mile long, I saw slowly wind down the upper part of an elevated valley in the Gilgit-Darel mountains. I was after ibex at the time of the occurrence, and was watching a herd of these animals, when I became aware of a low but distinct and unusual sound, produced by a great snake-like mass of snow winding down one of the valleys in my front. It occasionally stopped for a moment, and then proceeded again, and finally came to rest below me. I found this curious movement of snow was produced by countless numbers of snowballs, about the size of one's head, rolling over and over each other. The torrent-bed was full of them, an accumulation formed by numerous similar freaks of nature. I am quite unable to account for such an avalanche as the one now described. How does it originate, or by what process is the snow rolled up into these innumerable balls?"

Colonel Tanner made some interesting remarks on the subject of the line of perpetual snow. "Various authorities," he stated, "lay down such a line with great assurance, but for myself I find that circumstances of position, of climate, and of latitude, play so great a part in the position of this line that I am unable to define it even approximately. No sooner in one locality, or during one particular season, have I settled, to my own satisfaction, the line of perpetual snow, than I presently have been obliged completely to modify my views on the subject. On p. 154 of the 'English Cyclopædia,' vol. v., I read that snow lies 4000 feet higher in the northern than in the southern side of the Himalayas. On p. 281, vol. x., of the same work, it is stated that the snow-line on the northern slope is at 19,000 feet, which I should have been inclined to say is 1500 or 2000 feet too high. In Gilgit, during the end of summer, I found masses and fields of snow at 17,200 feet, and they extended down the northern slope certainly 2000 feet or even more below that altitude. In Kulu, which has many degrees of latitude less than that of Gilgit, avalanche snow lies in valleys above 8000 feet throughout the year after a good winter snowfall, but during the past spring, following a very mild winter, I found no snow at all at 8000 feet. There had been no avalanches, and even in June, at 14,000 feet, snow lay only in patches. I think that, in determining the snow-line with greater precision than has been done hitherto, scientific men should ascertain those altitudes on which perpetual snow lies on flat places in the position where it first falls, and should neglect the occurrence of a snow-field where it may have been protected from the sun's rays by its occurrence on the north face of a mountain. From memory I can state that there are a considerable number of typical localities which would help out such an inquiry. There is a peak (without name) about thirty miles north of Gilgit, with rounded summit, which, though only 17,500 feet high, is covered with a cap of perpetual snow."

Speaking of the Himalayan glaciers, Colonel Tanner stated that the most extensive and the most picturesque he has seen are in the Sat valley, which drains the southern face of Rakaposhi mountain in Gilgit. Three great glaciers come down into this valley, and dispute with the hardy mountaineers for the possession of the scanty area of the soil. Here may be seen forests, fields, orchards, and inhabited houses all scattered about near the ice heaps. The only passable route to the upper villages in this valley crosses the nose of the greatest of the three glaciers, and threads its way over its frozen surface. This glacier is cut up into fantastic needles of pure green ice, some of which bear on their summits immense boulders. About half a mile from its lower end or nose, Colonel Tanner found an island bearing trees and bushes, and at one place above this a very considerable tarn of deep blue-green water. The glacier had two moraines parallel with each other, and both bearing pine-trees; and from the highest point Colonel Tanner reached he fancied he saw the ice emerging from the *névé* at its source, far away up the slopes of Rakaposhi. In this glacier the pinnacles, wedges, blocks, and needles of ice were of the most extraordinary appearance, and the whole formed a weird and impressive view which he can never forget. Though the largest glacier Colonel Tanner has ever approached, it is very small indeed when compared with those described by Colonel Godwin-Austen in a locality not very far from the Sat valley. Insignificant though it is, it was more than Colonel Tanner could take in during his visit of two days' duration. It struck him at the time of his inspection that the peculiar stratified appearance of the ice needles, which in the case of the Sat glacier is very strongly marked, must have been caused by the different falls of avalanche snow on the bed of *névé* at the source of the glacier.

The lowest glacier Colonel Tanner has seen in the Himalayas is one that reaches the foot of the range near Chaprot Fort in lat. 35½°, in Gilgit. It is formed of beautiful clear ice and has no dirt. In Kulu and Labaul, lat. 32°, glaciers do not come down below 12,000 or 13,000 feet, and all are very dirty; and in Sikkim, lat. 28° or 29°, without having visited the glacier region himself, Colonel Tanner would say that the lowest limit reached by the Kinchinjanga group must be considerably higher, perhaps by 2000 feet or even more. The smallest mountain he has ever met with capable of giving rise to a glacier, is one on the Gilgit-Dareyl range, whose height is 17,200 feet, and in this case the mass of ice formed is of very inconsiderable size. Of the glaciers round Mount Everest and its great neighbours we know next to nothing, and the little we have learnt is derived from the itineraries of native explorers, who, of all classes of travellers, seem the least capable of furnishing trustworthy information regarding any subject lying at all outside their actual angular and distance measurements. But with his telescope, when employed on the survey of the Nipal boundary, Colonel Tanner has gazed long and earnestly at the icy region at the foot of Everest, and Peak No. XIII., where the glaciers extend over a very large area.

With regard to our actual knowledge of the Himalayas, Colonel Tanner thinks that perhaps our botanical knowledge is far ahead of other branches of science. Many eminent botanists have been at work for a long time past, and of late Dr. Duthie has been allowed to travel on duty into tracts not before visited by anyone possessing the requisite knowledge. It is likely that Dr. Duthie's museum at Saharunpur will, within a moderately short time, become an almost complete depository of the chief vegetable products of the Himalayas. The geologists, Messrs. Blandford, Edwin Austen, Richard Strachey, Stolitzka, and Lydekker, have been pretty well over those tracts open to Europeans, and are now well acquainted with all the leading features of their branch of science presented by the mountains of Kashmir, Kumaon, Kangra, and Sikkim. Ornithology has found many votaries, and the birds of these mountains are now probably all or nearly all known, though the late Captain Harman, only a few years back, discovered a new and handsome pheasant in the extreme eastern end, either of Bhutan or Tibet. The mammals, Colonel Tanner supposes, are all known, though one, at least, the Shaô, or great stag of Tibet, has not yet even been seen by any European, and the famous *Ovis poli* has been shot by not more than two or three sportsmen.

With regard to the work of the Survey, Colonel Tanner stated that the maps of Kashmir and Gilgit, without being free from error, are of the greatest use to a large class of officials. Incomplete though they may be, they were not brought up to their present state without taxing to the utmost the endurance of a hardy set of men. Adjoining Kashmir to the eastwards comes Kangra, with its subdivisions of Kulu, Lahaul, and Spiti. Kangra had once been roughly surveyed prior to the arrival there of Colonel Tanner's party, who are now at work on a very elaborate contoured map, which will take a long time to complete, owing to the intricacy of the detail demanded. Between Kangra and Kumaon occur various native States whose territories are being surveyed on the scale of 2 inches to 1 mile, also contoured work, resulting in very elaborate and trustworthy, though somewhat expensive, maps. Eastward of Kumaon, Nipal stretches along our border for some 500 miles till Sikkim is reached, and eastward again of Sikkim comes Bhutan, and various little-known semi-independent states which lie on the right bank of the Sanpo River. Nipal marches with the Kumaon border for many miles, and advantage was taken of the existence of the trigonometrical stations on the Kumaon hills to extend our knowledge of the adjacent topography of Nipal, and this was done about four years ago with some little result. The more prominent peaks in Nipal within a distance of about 100 miles were fixed trigonometrically, and some slight topographical sketching was done. From the trigonometrical stations near the foot of the lower hills, both in the North-West Provinces and in Bengal, trigonometrical points have lately been fixed, and some distant sketching done in Nipal, for 500 miles between Kumaon on the western and Sikkim on the eastern extremity of this kingdom; and, again, from the trigonometrical hill stations along the western boundary of Sikkim more points and hazy topography of Nipal was secured. This very meagre topography, sketched from very great distances, comprises all the geography of Nipal other than the sparse work collected by Colonel Montgomerie's explorers, or by explorers trained to his system who have worked since his death. All the existing data, whether trigonometrical, distant sketching, or native explorers' routes, are now being combined, as far as the often conflicting and contradictory materials admit. The resulting map of the country, though at most little better than none, is all we have to expect until some of the strictures on travelling in Nipal are lessened by the Nipal Government.

The whole of the Nipalese border, which marches with British territory for some 800 miles, is jealously guarded, and no European is allowed to cross it, except when the Resident of Kashmir or his own personal friends are permitted to proceed by a certain and particular route, between the military station of Segowli and Katmandu. Sikkim flanks the eastern boundary of Nipal, and the, until lately, indefinite western boundary of Shutan. British Sikkim is a small tract, which has twice been surveyed on suitably large scales. Independent Sikkim, which contains Kinchinjangee, one of the highest mountains, and some famous passes—the Donkhya, visited by Sir Joseph Hooker and a few others; and the Jelap, where our forces, under General Graham, have lately been employed, was surveyed in reconnaissance style by Mr. Robert, an energetic and hardy assistant of the Survey of India Department. The sketch map obtained by this gentleman is complete, and similar in character to that of Gilgit by Colonel Tanner, and to that of Nari Khorsam and Hundes by Mr. Ryall. It does not pretend to any exhaustive detail.

Our knowledge of Bhutan, or, rather, our ignorance of it, is about on a par with that of Nipal, but in Bhutan we have the valuable information left by Captain Pemberton, who forty-three years ago traversed the greater portion of the country from west to east. Besides Pemberton's work, Colonel Godwin-Austen, while he accompanied Sir Ashley Eden's mission to the court of the Deb Raja in the year 1863, executed a route survey in Western Bhutan. The engineer officers who were attached to the military force at Pewangiri also did some little topographical sketching, and beyond this we have distant sketching and trigonometrical work, as in Nipal, which, also, has yet to be combined with the route surveys of native explorers, some rather recent, and some of greater date. The difficulties which are presented to further researches in the direction of Bhutan geography seem unlikely to diminish. Our knowledge, then, of Bhutan is as unsatisfactory as that of Nipal. Eastward of Bhutan occur those numerous semi-independent hill States who sometimes, when necessity presses, own allegiance to Tibet, and at others assert their complete freedom from control. Colonel

Tanner himself has sent in two maps of this region derived from native sources, and both upset maps previously accepted, and it is highly improbable that we have any but the most rudimentary and vague knowledge of the course of the Sanpo below Gyala Sindong, and not even that of the course or limits drained by the Dibong. Colonel Tanner then referred in some detail to the great rivers that have their sources in the Himalayas, and concluded by giving some advice to tourists as to the best routes to take.

SCIENTIFIC SERIALS.

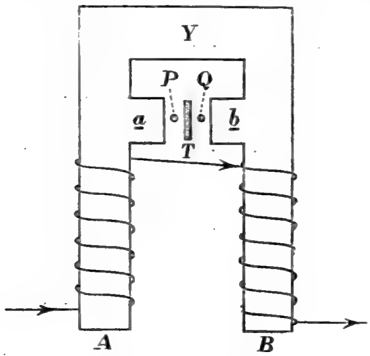
THE numbers of the *Journal of Botany* for February, March, and April, do not contain many articles of interest to those not concerned with systematic botany. Miss Barton has an interesting note on the occurrence of structures which have the appearance of galls on a common red seaweed, *Rhodymenia palmata.* Very few examples of this phenomenon have hitherto been described in connection with Algæ. Messrs. Britten and Boulger's biographical index of British and Irish (*sic*) botanists is now nearly completed, and we are glad to see that the editor of the *Journal* has not yet abandoned the idea of reprinting it in a separate form.

IN the *Botanical Gazette* for February, Dr. D. H. Campbell continues his useful series of papers on the histology of the higher Cryptogams, with a note on the apical growth of *Osmunda* and *Botrychium*, in which he shows the large range of variation on this point within the Osmundaceæ.—In the March number, Mr. G. F. Atkinson discourses on the black rust of cotton, a disease due to the attacks of several parasitic fungi, of which the most destructive is *Cercospora Gossypina.*

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, April 17.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were made:—On a property of magnetic shunts, by Prof. S. P. Thompson. After referring to a few instances in which magnetic shunts are employed, as, for example, the ordinary magnetic medical coil, and Trotter's constant current dynamo, he said that the particular property he wished to speak of was the time taken for such a shunt to lose its magnetism, as compared with the other branch of the magnetic circuit. If these times were very different, unexpected results might be produced, and these might be regarded as being due to a kind of magnetic time-constants. Short pieces, as was well known, demagnetize much more rapidly than long ones, particularly if the latter



form, or be part of, a closed magnetic circuit. Hence, in alternators, such as Kingdon's keeper dynamo, in which both magnets and armature are stationary, it was important that the revolving keepers should be short. The most important application of a magnetic shunt occurred, he said, in D'Arlincourt's relay, described in vol. iv. of the Journal of the Society of Telegraph Engineers and Electricians, and shown diagrammatically in the figure. In this relay the polarized tongue T plays between two projections, *a* and *b*, near the yoke Y, and it is claimed to have a quicker action than ordinary kinds. The reason of this the author explained as follows. When a current flows through the coils, the greater part of the magnetic lines pass through the yoke, but a few leak across from *a* to *b*, and move the tongue against the contact P. On stopping the current, the magnetism in the extremities A and B dies away much more rapidly than in the yoke; consequently, the direction of the field between *a* and *b* is reversed, and the tongue T thrown back against the stop Q. Prof. Perry asked if any experiments had been made to test whether the throwing back actually occurred. He also inquired whether such action would be augmented, or otherwise, by having a thick copper tube round the yoke, or by laminating the iron. Mr. Blakesley asked if placing a yoke across A B would not improve the action. The President said he would be glad to know whether the relay was any more sensitive than an ordinary one as regards the ampere-turns or the watts required to actuate the instrument. In India, he remembered that they used inductive coils shunting the ordinary relays, in order to expedite the action and to avoid confused signals arising from the electrostatic capacity of long lines. In reply, Prof. Thompson said he had tried an experiment on a horseshoe electro-magnet, and found evidence of throwing back when working near the bend or yoke. Putting a yoke across A B would, he thought, tend to neutralize the effect desired.—An alternating current influence machine, by James Wimshurst. This machine consists of a varnished glass disk, with or without metallic sectors, mounted on an axis, and rotating within a square wooden frame fixed in the plane of the disk. The frame carries four square glass plates, each of which has one corner cut away so as to clear the boss of the disk. These plates are placed one at each corner of the frame, alternately on the two sides, and the disk revolves between them. There are thus two plates on one side of the disk situated at opposite ends of a diagonal of the frame, and two on the other side of the disk, at opposite ends of the other diagonal. Tin-foil sectors fixed to the outer sides of the plates act as inductors, and wire brushes connected with them touch the disk about 90° behind the centre of the inductors. The peculiarity of the machine is that, although sparks can be readily obtained from it, a Leyden jar cannot be charged by bringing it to one of the terminals. From this the author concluded that the electricity produced was alternately of positive and negative sign, and this he showed to be the case by means of an electroscope. The alternations, he said, occurred about every three-quarters of a revolution, the suspended paper disks which he used as an electroscope remaining apart for that period and collapsing during the next quarter of a turn. Using disks with various numbers and sizes of sectors, the author finds that the smaller the sectors and the fewer the number the greater the quantity of electricity produced. Plain varnished glass is the best in this respect. Such a disk, however, does not excite itself quite so freely as one having numerous metallic sectors. By removing two of the inductors and placing an insulating rod carrying collecting brushes at its ends, across a diameter of the disk, the machine was used to produce direct currents. Numerous disks and various shaped inductors accompanied the machine, by means of which a Holtz, Voss, or ordinary influence machine could be imitated. Prof. S. P. Thompson congratulated the author on the most interesting and puzzling machine he had brought before the Society. He inquired if the machine would work if the direction of rotation was reversed, or if two of the inductors were removed, and also whether all the four inductors are electrically of the same sign at the same instant. In reply, Mr. Wimshurst said the machine would not excite if the direction of rotation be changed without also changing the direction of the brush arm, but it would work as a direct current machine when two inductors were removed.—On erecting prisms for the optical lantern, and on a new form of erecting prism made by Mr. Ahrens, by Prof. S. P. Thompson. The ordinary form of erecting prism, viz. a right-angled isosceles one, was, the author pointed out, open to the objection that the top halves of the faces inclosing the right angle were nearly useless, for only the light which, after the first refraction is totally reflected by the hypotenuse face, can be utilized. The fraction of the side which is useful varies with the refractive index, being 0·46 when $\mu = 1·5$, and 0·525 when $\mu = 1·65$. To increase these proportions, prisms with angles of 105° and 126° have been used by Wright and others, and in some cases the prisms have been truncated. With such large angles, much light is lost by reflection. Bertin employed two truncated right-angled prisms placed base to base with an air-film between them. Nachet has also made erecting prisms for microscopes in which internal reflection occurs from faces inclined at an angle of 81° to each other. This form of prism suggested to Mr. Ahrens the new form now shown, which may be described as a long right-angled prism whose ends are cut off so as to be parallel to each other and inclined at 45° to the hypotenuse face. The longitudinal acute angles not being required, are truncated. Light falling parallel to the axis, on one end of the prism is refracted, and after internal reflections, emerges parallel, but perverted. It is claimed that this form of prism gives, weight for weight, a larger angular field than any previously made. The performance of the new form of prism, and also of the ordinary forms, was tested before the Society.—At the close of the meeting, Dr. Atkinson, who had taken the chair, announced that the next meeting would be held at Cambridge on May 9, instead of May 8, as previously intended.

PARIS.

Academy of Sciences, April 20.—M. Duchartre in the chair.—On some calorimetric data, by M. Berthelot. (1) Aspartic acid and its mixed function. This acid is a bibasic acid in which the heat of neutralization for the first addition of a molecule of NaOH is about four times that for the second; this unequal heat liberation is connected up by the author with the presence in the molecule of the (NH_2) group. (2) Malonic chloride. (3) The formation of isomeric soluble and insoluble tartrates.—On the crystalline form and optical properties of M. Engel's new variety of sulphur, by M. C. Friedel.—An excursion from the Arago Laboratory to Rosas, Spain, by M. de Lacaze-Duthiers. An account is given of a zoological excursion into Spain taken by the pupils of the Arago Laboratory during the Easter holidays.—On the endothelium of the peritoneum, and some modifications which it undergoes during experimental inflammation; how we must understand the recovery of wounds by immediate healing, by M. L. Ranvier.—New nebulæ discovered at Paris Observatory, by M. G. Bigourdan. The list now given is in continuation of previous ones, and includes thirty-five newly-discovered objects between 16 hours and 24 hours of right ascension.—On the deformation of spiral surfaces, by M. L. Raffy.—On the theory of light, by M. C. Raveau.—Dissociation of amylene hydrobromide under reduced pressures, by M. Georges Lemoine. The conclusion is drawn that in the case of this body "dissociation is facilitated by a diminution of pressure." The curves given show the relationship between dissociation at $\frac{1}{10}$ atmosphere and that at the ordinary atmospheric pressure.—On the preparation and the reaction of ammoniacal chlorides of mercury, by M. G. André.—On the sub-salts of silver, by M. Güntz. Starting from the perfectly definite and crystallized sub-fluoride, Ag_2F, previously prepared by the author, the salts Ag_2Cl, Ag_2I, Ag_4S, have now been obtained, and their composition established by the analyses given.—On the sulphide of boron, by M. Paul Sabatier. The heat of formation of this body (prepared by the method of Sainte-Claire Deville and Wöhler) is found to be 82·6 calories; less than that of the oxide or chloride.—On boron hydride, by M. Paul Sabatier.—On two new forms of sulphur, by M. Engel.—A strong solution of sodium thiosulphate is decomposed by strong HCl; the filtrate on standing becomes yellow; extracted with $CHCl_3$ and the exract evaporated, orange-yellow crystals of the rhombohedral ystem are obtained. A variety of sulphur soluble in water is also described.—Action of urea on sulphanilic acid, by M. J. Ville. The following equation sums up the results obtained—

$$C_6H_4\left\langle\begin{matrix}NH_2\\SO_3H\end{matrix}\right. + CO\left\langle\begin{matrix}NH_2\\NH_2\end{matrix}\right. = NH_3 + C_6H_4\left\langle\begin{matrix}NH{-}CONH_2\\SO_3H\end{matrix}\right. .$$

—New combinations of metallic sulphites with the aromatic amines, by M. G. Denigès.—Estimation of acetone in methylated alcohols, by M. Léo Vignon.—On the purification of industrial waters and sewage, by MM. A. and P. Buisine. The authors propose the use of ferric sulphate as a purifying agent.—Contribution to the history of fecundation, by M. Hermann Fol. The changes which two corpuscles of the ovule undergo after fecundation are described.—On the gustatory organs of the sea-devil (*L. piscatorius*), by M. Frédéric Guitel.—On the innervation of the proboscis of Glycera or Rhynchobolus, by M. E. Jourdan.—On an artificial melanine, by M. Georges Pouchet. The author has prepared a body which has the general properies of the black matter in the blood known as melanine.—New researches on olfactometry, by M. Charles Henry.—On the assimilation of lichens, by M. Henri Jumelle. Numerous experiments lead the author to the conclusion that when certain favourable conditions of light, humidity, and season are realized, all lichens are capable of decomposing the carbon dioxide in the air very energetically, notwithstanding their respiration of carbon dioxide. Lichens thus increase in carbon. It is also shown that, *cæteris paribus*, direct sunlight is preferable to diffused light.—Influence of salinity on the quantity of starch contained in the vegetable organs of *Lepidium sativum*, by M. Pierre Lesage. The experiments indicate that when plants are watered with solutions containing from 12 to 15 grams of salt per litre the starch disappears completely. The diminution of starch is not proportional to the increase of salinity.—On some rye possessing peculiar poisonous properties, by M. Prillieux.—On the discovery of a spring at the bottom of the Annecy Lake, by MM. A. Delebecque and L. Legay.—On the soundings executed in the Pas-de-Calais in 1890, by M. J. Renaud.—On the metamorphic rocks of the Savoy Alps, by M. P. Termier.—Contribution to the study of the culture of colza, by MM. E. Louise and E. Picard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

An Introduction to the Study of Mammals: W. H. Flower and R. Lydekker (Black).—Forty Years in a Moorland Parish: Rev. J. C. Atkinson (Macmillan).—Elementary Text-book of Botany: E. Aitken (Longmans).—Proceedings of the Seventh Annual Convention of the Association of Official Agricultural Chemists (Washington).—The Missouri Botanical Garden (St. Louis).—Die Organisation der Turbellaria Acocla: Dr. L. von Graff (Leipzig, Engelmann).—A History of Chemistry: Dr. E. von Meyer; translated (Macmillan).—The Best Books: W. S. Sonnenschein; 2nd Edition (Sonnenschein.—General Report on the Operations of the Survey of India Department, 1888–89 (Calcutta).—Grundzüge der Geologie und Physikalischen Geographie von Nord-Syrien: Dr. M. Blanckenhorn (Berlin, Friedländer).—Ueber den Schädel eines fossilen dipnoërs Ceradotus Sturii, nov. sp.: F. Teller (Wien, Hölder).—Coal, and what we get from it: R. Meldola (S.P.C.K.)—Eighteen Years of University Extension: R. D. Roberts (Cambridge University Press).—The Monist, vol. i., No. 3 (Watts).—Willing's British and Irish Press Guide (Willing).

CONTENTS.

✓ 571/3 ✓ 535 ✓ 608 434 461/3