THURSDAY, APRIL 4, 1895.

VERWORN ON GENERAL PHYSIOLOGY.

Allgemeine Physiologie. Ein Grundriss der Lehre vom Leben. Von Max Verworn. (Jena: Fischer, 1895.)

THIS handsome and well-illustrated volume of some six hundred large octavo pages, by a young German physiologist already favourably known by his special researches, is ambitious in design but praiseworthy in purpose. The author complains of the too narrow character of most of the physiological inquiries and writings of the present time; and not without some justice. A cursory reader of a modern text-book of physiology (of my own, for instance), might easily come to the conclusion that most of our current physiological doctrines had been arrived at by the exclusive study of the frog, the rabbit, and the dog, with occasional help from that of the horse, of a fish, of a bird, and of man. All the wealth of opportunity for observation and experiment offered by the innumerable other forms of life, seems to be neglected. And there follows naturally the inference that physiology would gain a healthier tone and broader grasp, by widening the field of its study. Years ago the great Johannes Müller, in his immortal work, took such a broad survey; later on, Carpenter followed the same course in his "Comparative Physiology," a work to which I, at least, owe much; and now Dr. Verworn attempts to present, in a general view, the light which the multitudinous special researches of more recent days have shed on the fundamental phenomena of life.

A very little reflection will show that when the meaning of the term physiology is carried beyond the bounds which academic conditions have fixed for it, it becomes difficult to say what parts of the knowledge of living beings are not embraced by it. A physiologist, withdrawing himself from the immediate demands of the schools, and brooding over the many aspects of his science, cannot help feeling that all inquiries into the phenomena of life end by taking the form of physiological inquiries. While morphological facts may in the first instance, and for a while, be regarded by him chiefly as helps towards the solution of special physiological problems, he cannot help believing that the ultimate interpretation of the phenomena of form must be based on the principles of that fundamental physiology, which, for want of a better word, we may call molecular. And, even at the present time, imperfect as physiology as yet is, some of us may think that some modern morphological speculations have gone astray, through heed not being given to what even a narrow imperfect physiology is already able to teach.

Hence, Dr. Verworn, taking as the title of his book "General Physiology," has naturally been led to dwell on many topics which are not to be found in a treatise of narrower scope. After a very brief historical sketch, and a chapter on the composition or nature of living matter, in which he discourses on the "cell" and its constituents, and on the physical and chemical properties of living matter, as well as on the differentials of living and lifeless matter, he proceeds to the consideration of the elementary phenomena of life. These he treats under the heads of change of substance (metabolism), change of form, in which such topics as heredity, adaptation, cell division, reproduction and the like are dealt with, and change of energy. Then follows a chapter on "The General Conditions of Life," in which, among other matters, he treats of the origin of life on the globe, and the history of death. "Stimuli and their effects" are next discussed, and the final chapter is headed the "Mechanism of Life."

Even six hundred goodly pages furnish all too small a room for such a treatment of each of the many and varied topics handled in them as would in each case appear adequate to those who had made them the objects of special study. Moreover, the author, as he states in the preface, has striven to write in such a way that the reader should not be easily wearied; and the practised writer knows that a simple way of not wearying the reader is by a light touch, so to brush away all difficulties as to lead the reader to think he grasps clearly that which, in reality, no one truly understands. The author further, as he also states, addresses himself not to physiologists, or even to biologists, exclusively, but also to the cultured general reader; and when a discussion about an abstruse physiological topic has to be so conducted as to please the ear of the cultured general reader, the special physiologist naturally finds the discussion spoiled by things which to him seem essential, being left out, and by things which to him seem unimportant, or even irrelevant, being dwelt upon at length. It would be ungracious, therefore, to find fault with the particular way in which the author handles that or that of the many themes with which he deals. For the same reason, hesitation may be felt in objecting to the prominence given in the work to definitions, some of which a captious critic might urge had something of the mark of barrenness, or to the frequently occurring judicial "summing up" in paragraphs emphasised by spaced type; or to the abundant "generalisations," some of which may be contested, while others appear to draw perilously near to platitudes. The work, as a whole, must commend itself highly to those morphologists who desire to learn the more general and fundamental results of recent physiological inquiry; for the author has woven into his work the very latest news from the physiological laboratory. On the other hand, the physiologist, whose activity has been too much limited to the study, by complicated instruments of precision, of the phenomena of the higher animals, who has moved too exclusively in what some take pleasure in hall-marking as "horological physiology," will largely profit by having his attention directed to the many physiological questions which arise in the course of morphological investigations, started by observations recorded, for the most part, in periodicals or works not falling within his ordinary reading. As especially interesting, perhaps, may be mentioned the chapter on stimuli and their effects, as illustrated by the study of the simplest forms of life, and the discussion on the part played by nuclei in the development of physiological phenomena, in both of which matters the author is able to appeal largely to his own observations. Very suggestive also is the exposition of the doctrine of assimilation and dissimulation and of its applications, which forms a large part of the chapter dealing with "the mechanism of life."

The author claims for his work, that it is essentially an exposition of "cellular physiology." I am myself inclined to think that he exaggerates the value of this point of view. The idea of the cell, important as it has been, and still is, when we deal with the cell as a morphological unit, seems to me of much less importance when we deal with it as a physiological unit. But I pass this over, in order to join issue with the author on two other matters. He speaks "of the impotence of the physiology of to-day, when brought to face the simplest processes of life," and writes as if all the knowledge which, especially during the last thirty years, has been gained by the application of exact physical and chemical methods to the study of the phenomena of life, simply led to the end of a blind alley. He urges that, while we are rapidly approaching perfection in these things, we yet are impotent in face of the deeper problems. He even goes very near repeating the taunt of the Philistine, "With all your boasted progress, you are still unable to tell us *what life is*," with, however, the difference that he uses the taunt in order to incite physiologists to adopt other methods of inquiry. I venture to think that in this the author is quite wrong. Not only has the progress of physiology, due to the use of exact methods, been remarkable during the last half-century or so, but also by those methods we have been drawn measurably nearer the inner and hidden mysteries. To take one instance among many, the application of exact physical methods to the study of muscles, so far from having brought us to a point beyond which we cannot go, seems just now to be opening up the way to fruitful conceptions of the intimate nature of muscular contraction—conceptions which would have been impossible in the absence of the knowledge gained by the graphic method. Two armies, from two different sides—one physical, the other chemical—are attacking that difficult—to some it seems impregnable—fortress. They have already gained many of the outworks; they are pressing on, drawing nearer to each other; and when they touch hands, they will do that which will put to shame all scoffs at their impotence.

Then again, the author finds fault with the preponderant attention given by physiologists to the study of the higher vertebrate animals; he blames them for their comparative neglect of the lower and, especially, of the lowest invertebrate forms. It is not for me, who in my rash youth had wild dreams of building up a new physiology by beginning with the study of the amœba, and working upwards, to say one word against the experimental investigation of the lower forms of life. But experience and reflection have shown me that, after all, the physiological world is wise in spending its strength on the study of the higher animals. And for the simple reason that in these, everything being so much more highly differentiated, the clues of the tangles come, so to speak, much more often to the surface, and may be picked up much more readily. Taking again, as an instance, the molecular processes which give rise to the movements of animals, and which appear under such forms as that of amœboid movement, and that of the contraction of a striated muscle, I venture to think that the very apparent simplicity of the former is an obstacle to our getting a real grasp of its inner nature, and that by our studies of the complex muscle, we are drawing nearer to such a grasp than we could ever have done by observations confined to the phenomena of the amœba itself. And so in many other instances. The study of the lower forms of life is, in reality, more difficult than that of the higher forms; and the latter naturally comes first. At the same time the author will have the sympathy of all, if his contention be limited to the assertion that the full fruition of physiological truth can only be reached by the careful study of all forms of life, whether high or low; this, indeed, his own special investigations have already shown, and in this sense the contribution to a "general physiology," which his present volume offers, is gladly welcomed by us all. M. FOSTER.

NERNST'S THEORETICAL CHEMISTRY.

Theoretical Chemistry, from the Standpoint of Avogadro's Rule, and Thermodynamics. By Prof. Walter Nernst, Ph.D. of the University of Göttingen. Translated by Prof. Charles Skeele Palmer, Ph.D. of the University of Colorado. Pp. xxv.–697. (London: Macmillan and Co. 1895.)

NO one can compare the knowledge we possess to-day of the conditions and general laws of chemical change with the state of our knowledge ten years ago, without being much impressed by the enormous advances made in the last decade. Accurate and generalised knowledge of physical chemistry did not exist ten or twelve years ago. What is practically a new science has arisen, based on the work of such men as Guldberg and Waage, van't Hoff, Ostwald, Arrhenius, Nernst, Raoult, Gibbs, and Thomsen, who built upon the foundations laid by Dalton, Avogadro, Berthollet, Faraday, Helmholtz, Thomson, and Clausius. The special mark of the new science is that it has been produced, to use the words of Nernst, "by the co-operation of two sciences which hitherto have been, on the whole, quite independent of each other." One hardly knows whether to speak of *physical chemistry* or *chemical physics*.

The text-book of this new science is Ostwald's "Lehrbuch der Allgemeinen Chemie." Notwithstanding the thoroughly satisfactory character of that work, there was room for another book which should treat the subject in less full detail, and which should pay especial attention to the data and the conceptions that have been systematised and applied in a general way. Prof. Nernst has written exactly the book that was wanted; and this book now appears in a form which brings it within the reach of all English-speaking students.

The *Theoretical Chemistry* of Prof. Nernst deals with (1) the universal properties of matter, (2) the atom and the molecule, (3) the transformations of matter, and (4) the transformations of energy. The first section treats of (1) the gaseous, (2) the liquid, (3) the solid, state of aggregation, (4) the physical mixture, and (5) dilute solutions. The second section is devoted to (1) the atomic theory, (2) the kinetic theory of the molecule, (3) the determination of molecular weight, (4) the constitution of the molecule, (5) physical properties and molecular structure, (6) the dissociation of gases,

(7) electrolytic dissociation, (8) the physical properties of salt solutions, (9) colloidal solutions, and (10) the absolute size of molecules. Sections three and four are concerned with the subject of affinity. The author treats that part of this subject which is connected with the transformations of matter under the headings: (1) the law of chemical mass-action, (2) chemical statics in (*a*) homogeneous, and (*b*) heterogeneous systems, (3) chemical equilibrium in salt solutions, and (4) chemical kinetics. In dealing with the energetics of chemical affinity, Prof. Nernst treats of (1) the applications of the first law of heat, (2) temperature and (*a*) complete, (*b*) incomplete, chemical equilibrium, (3) temperature and the velocity of reactions, (4) heat and chemical energy, (5) electrochemistry, (6) photochemistry. An appendix contains accounts of the more important developments of theoretical chemistry in the year 1893.

The ground covered by the book is evidently very large, and very different from that traversed by the books on theoretical chemistry published ten years ago.

Theoretical chemistry, as understood by Nernst, is based on Avogadro's law, and van't Hoff's extension of that law to dilute solutions, the law of mass-action, and the laws of thermodynamics. It would not be very far from the truth to say that the book is concerned with the meaning of the sign of equality in chemical equations, and that the connotation of that sign is elucidated by applications of the laws of Avogadro and van't Hoff, and the law of mass-action, bound together and lighted up by the laws of thermodynamics.

The essential characteristics of the book, so far as I can judge, are the exceeding clearness in the statement of each problem of theoretical chemistry, the cutting out of irrelevant issues, and then the binding together of the apparently detached discussions into an harmonious whole by the application of the general principles of the molecular-atomic theory, and of chemical energetics.

The treatment of *osmotic pressure* may be chosen as an illustration of these characteristics of Prof. Nernst's book. The meaning of the term *osmotic pressure* is made clear on pp. 118–119, by the description of a theoretical experiment on the diffusion of sugar in aqueous solution through a semi-permeable partition fitted with a movable piston. Unfortunately, the translator has rendered *Lösungsmittel* by "solvent material"; had he used the term "solvent" (as he does in some other passages), the meaning would have been better conveyed. Then follow lucid paragraphs on the methods of measuring osmotic pressure (I wish the translator had not allowed the phrase to pass, "ether containing considerable benzene"), admirably illustrating the bearings on this problem of determinations of vapour-pressures, boiling points, and freezing points. Having thus, by adhering strictly to the problem under discussion, arrived at a clear conception of osmotic pressure, the author proceeds, in a few brief and clear paragraphs, to consider the circumstances which condition the osmotic pressures of solutions, viz. the concentration of the solution, the temperature, the nature of the dissolved substance, and the nature of the solvent. The outcome of the matter is then summed up in the statement—*the lowering of the freezing point of a dilute solution is proportional to the number of molecules of the dissolved substance*. This summary at once suggests an inquiry into the range of applicability of the law of osmotic pressure, and the laws of solution in general; and the inquiry leads to a statement of van't Hoff's extension of the law of Avogadro. The experimental determination of molecular weights by the application of the van't Hoff generalisation is described (pp. 224–231) in that part of the book which deals with the determination of molecular weights. When the author comes to treat of dissociation he returns to the subject of osmotic pressure, and shows how an application of the hypothesis of dissociation to salts in dilute aqueous solutions leads to a far-reaching theory of chemical change, and he propounds this theory in a clear and practical manner. In one of the chapters dealing with thermochemistry (pp. 565–567), the author, following van't Hoff, brings the subjects of osmotic pressure and electrolytic dissociation within the range of thermodynamical methods, and, by a fundamental equation, connects the equilibrium of a chemical system with such conditions as temperature, pressure, and dissociation. Had Prof. Nernst been tempted to discuss such a side issue as the part played by the solvent in bringing about electrolytic dissociation, he could not have arrived at the very general results to which his strictly accurate and limited method of inquiry have led him. He does, indeed, devote a paragraph or two to this matter of the action of the solvent (*e.g.* pp. 232, 444). He is careful to note the great interest and importance of the question; at the same time he draws attention to the fact, often overlooked, that no definite answer can be given to questions regarding the existence of compounds of molecules of the solvent with molecules of the dissolved substance by the study of the osmotic pressures of dilute solutions, because the existence of such compounds would not affect the osmotic pressures of fairly dilute solutions.

Prof. Nernst dismisses the so-called *hydrate theory of solution* in a short and somewhat contemptuous paragraph (p. 444); he speaks of this conception as having no theoretical basis, as having led to no general laws, and as based on an uncritical examination of experimental data.

A very admirable feature of the book is the care taken to warn the student against drawing unsound and inapplicable deductions from sound generalisations. For instance, at the beginning of the chapter on the physical properties of salt solutions (p. 331), the statement is enunciated that *the properties of an aqueous solution of a salt are made up, additively, of the properties of the free ions*. And then the illegitimate and misleading inferences which may be, and some of which have been, deduced from this generalisation are noted in a few sentences, before the true meaning and applicability of the statement are developed.

I should like to deal at length with the author's treatment of the law of mass-action, and the many applications and developments of this law; but space forbids. Among the applications of the law of mass-action I would ask the student to note that which serves to explain the discrepancies among the values for the *strengths* of acids obtained by Ostwald by employing different methods of measurement (p. 438).

The volume literally abounds in suggestions; new light is thrown on almost every question of theoretical chemistry. When this book becomes generally known and studied by English chemists, it seems to me it will be impossible for any of them to refuse to acknowledge the marvellous advances which have been made in the science by the introduction of the conception of electrolytic dissociation.

I would recommend every student of advanced chemistry to study this work. Merely to glance through it is little use: it must be studied laboriously; and it will well repay the labour. Of course there are weak parts in the book. I think the treatment of *the constitution of the molecule* is too sketchy; and chapter vi. of Book iv., on *electrochemistry*, should, in my opinion, have been either expanded or omitted.

Of the translation it is difficult to speak advisedly. I think the translator has attempted an impossible task, the task, namely, of literally changing German into English. If the meaning of sentences in one language is to be conveyed in another language, it seems to me that a paraphrase, not a so-called literal translation, is needed. The task of translation must have been extremely difficult; the subject-matter is complicated, and German is not a language distinguished by its lucidity. The meaning of the original is conveyed on the whole; but the sentences read strangely. See, for instance, the most peculiar sentence near the bottom of page 591. There is an extraordinary sentence about plucking fruit from stepping-stones, on p. 354. Several cases of absolute mistranslation are to be found; for instance, the sentence in italics in the ninth and tenth lines from the bottom of p. 254, and the sentence at the beginning of Book iii., p. 353. *Beliebig* is sometimes translated "casual," sometimes "selected."

I admit the great difficulty of the task undertaken by the translator: as I have said, he has generally succeeded in conveying the meaning of the original; but I think the rendering into English might have been at once more accurate, more elegant, and more readable.

M. M. Pattison Muir.

OUR BOOK SHELF.

Bird Notes. By the late Jane Mary Hayward. Edited by Emma Hubbard. Pp. 181. (London: Longmans, Green, and Co. 1895.)

Catalogue of the Birds of Prey (Accipitres and Striges). By J. H. Gurney, F.Z.S. Pp. 56. (London: R. H. Porter, 1894.)

A dainty book is Miss Hayward's, the pretty little process-blocks, representing a number of our common birds, matching the short sketches of avian habits. The lamented author had a "deep-rooted love of the beauty of the world." She was a close and unwearied watcher of bird traits, and her notes possess the charm of all original observations. From a scientific point of view, the chief failing of many of the notes is that they endow the birds too largely with human consciousness. Mrs. Hubbard recognises the objection, and says something in favour of this "anthropomorphism"; but while such fancies are poetically attractive, and may be psychologically justifiable, they must always be of less value than the facts which give them birth.

The ornithological papers of the late Mr. J. H Gurney were both numerous and important, and in the volume under notice we have further evidence that the son worthily carries on his father's interest in the collection at Norwich Museum. All the birds of prey (hawks and owls) in the Museum were catalogued by Mr. J. Reeve, the veteran custodian, and from this MS. catalogue, and his father's "List of Diurnal Birds of Prey," Mr. Gurney has compiled the list of Accipitres and Striges. According to the list of the former order, the total number of existing species of diurnal birds of prey is now 470, of which at least 89 are only sub-species. The total number of existing species of owls is placed at 268, of which 87 appear to be only sub-species.

Before each bird's name, in the two lists, a letter is placed to mark the zoological region to which it belongs, on Mr. Sclater's classification. A striking testimony to the efficiency of this system is given by Mr. Gurney, in the following words: "The way in which these several divisions [Mr. Sclater's] are justified by the Birds of Prey, and especially by the Diurnal Birds of Prey, is remarkable, and if they were to be decided afresh by that class of birds alone they could not very well be improved upon. Seven-eighths of the Raptores are found in one region only—*i.e.* not in more than one; and the region which has the greatest number is the Neotropical or South-American region, which contains 181 Hawks and Owls."

The whole of the Raptorial collection of Norwich Museum is now being transferred to Norwich Castle; and the completeness of the collection can be judged from the fact that it comprises 403 out of the total of 470 accepted species and sub-species of Accipitres, and 195 out of 268 known species and sub-species of the order of Striges. Mr. Gurney may well be proud of the collection, and of the fine Castle Museum in which it is housed.

Prince Henry the Navigator. By C. Raymond Beazley, M.A., F.R.G.S. (Heroes of the Nations Series.) (Putnam's Sons).

In this most interesting and valuable book, Mr. Beazley shows us clearly the growth of geographical knowledge, carrying his researches back earlier even than 130 A.D., he tells us that the first maps and charts of the old world are due to Eratosthenes and Strabo. Ptolemy succeeded them, improved their work, and, where knowledge failed him, made errors himself; the author writes thus of his great chart: "Never was there a clearer outrunning of knowledge by theory, science by conjecture, than in Ptolemy's scheme of the world (*c.* A.D. 130)."

We gather much information concerning Greek and Arabic geographers, of the early Christian pilgrims, and of the discoveries of the Norsemen. Throughout the book, we watch, as it were, the growth and improvements of the maps and charts. We see the expansion of geography due to the crusades and land travel. Finally we are brought to Prince Henry the Navigator himself. From youth upwards, retired and studious, he withdrew himself at the age of twenty-one to his Naval Arsenal at Sagres, and devoted the rest of his life to the accomplishment of his three chief objects—"to discover, to add to the greatness and wealth of Portugal, and to spread the Christian faith." We can but marvel at this great man, at the untiring energy with which he worked, but still more at the greatness of that work.

Mr. Beazley has treated the subject in a very thorough and interesting manner, and the numerous maps form a most important part of the book; they date from 130 A.D.–1492. No pains have been spared to make the subject quite clear to the student. All through the book we see how the dominion of the sea has been continuously enlarged by the perpetual application of science to the art of navigation.

W.

An Elementary Treatise on Theoretical Mechanics. Part iii. Kinetics. By Alexander Ziwet, Assistant Professor of Mathematics in the University of Michigan. (New York: Macmillan and Co., 1894.)

THE first two parts of this excellent treatise have already been noticed in these columns; this third part keeps up to the same excellence, and we look forward to a sequel, in the absence of any indication that the treatise is yet complete.

We think the author would have done well to have followed the opinion of his American colleague, Prof. T. W. Wright, and to have reserved absolute measurements to the Metric system of units, while using gravitation units only with the British foot and pound. These last units are too insular and provincial ever to be employed in cosmopolitan problems where results have to be translated into absolute measure; and James Thomson's word, *poundal*, is never likely to be of any practical use.

Lagrange's and Hamilton's general dynamical equations are expounded with clearness and elegance; the application of the principle of the Conservation of Areas to the paradoxical motion of a kitten, let fall by his feet a short distance above a table, has excited considerable discussion recently at the Paris Academy of Sciences; this problem would provide the author with an illustration of the methods of generalised coordinates.

The copious list of authorities at the end of the chapters is a valuable feature of the book. 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.*]

Destruction of the Seismological Observatory at Tokio, Japan.

I REGRET to say that a letter, which has just arrived from Tokio, informs me that Prof. John Milne has lost all his valuable seismographic instruments, with his library and many manuscripts, through a fire which has occurred at his house and observatory. Prof. Milne wishes me to announce that his address-book has been destroyed, but he will be able to forward vol. iv. of the *Seismological Journal* to those entitled to it, if they will send in their names to him, "care of the *Japan Mail* office, Yokohama." He further wishes me to state that he has 600 damaged copies of the Seismological Society's *Transactions*, and that from these he will be happy to complete sets. Applicants for the copies of the *Transactions* should address Prof. Milne, care of the Geological Society, Burlington House, London, W.

I am sure that scientific men all over the world will feel the deepest sympathy with Prof. Milne in his great and, indeed, irreparable loss. He was preparing to return to Europe when the fire occurred, and he wishes to appeal to all who can furnish him with separate copies of papers relating to earthquake phenomena, to replace, so far as is possible, those he has lost by the destruction of his library. JOHN W. JUDD.

April 1.

On Mersenne's Numbers.

IN 1644 the mathematician Mersenne asserted that out of the 56 primes not < 257, there were only 12 primes, viz.:—

$$q = 1, 2, 3, 5, 7, 13, 17, 19, 31, 67, 127, 257,$$

which, taken as *exponent* (q), make the number $N = (2^q - 1)$ also prime. No proof was published, and even up to now, this statement has only been partially verified: the verification is still one of the difficult problems of higher arithmetic. According to a paper by Mr. W. W. Rouse Ball, in the *Messenger of Mathematics*, vol. xx. p. 34, Mersenne's statement has been verified for the 18 prime values of $q < 60$, and for 14 higher values, and one additional number N has been shown to be prime by Prof. Seelhoff, viz. when $q = 61$. This left 23 cases unverified, viz. 3 supposed to be prime (when $q = 67, 127, 257$), and the remaining 20 supposed to be composite (when $q = 71, 89, 101, 103, 107, 109, 137, 139, 149, 157, 163, 167, 173, 181, 193, 197, 199, 227, 229, 241$).

I have recently discovered the verification of one of the latter, viz. that

$$(2^{197} - 1) \text{ is divisible by } 7487.$$

This can be readily verified directly by the method of Congruences.

It has also been verified by actual division by Mr. R. Tucker (Sec. London Mathematical Society), who has kindly sent me the quotient consisting of 56 figures. The mode of *discovery* of this factor has been communicated to the London Mathematical Society, and will be sent to one of the mathematical journals. ALLAN CUNNINGHAM.

March 23.

Tan-Spots over Dogs' Eyes.

I TRUST you will allow me to point out that the drift of my letter on the above subject in NATURE, vol. l. p. 572, has not been fully apprehended. Hitherto we seem to have no very clear cases in which we can actually trace the operation of "natural selection." I think, when examined, this will be found to be an instance.

The spots appear to have arisen in the dog as comparatively recent permanent markings—for protective purposes—after semi-domestication. As Mr. Worthington G. Smith says, they are not seen among wild animals allied to the dog.

They appear to have arisen since the original Red Dog—be he Dhole, Pariah, or Dingo—became pied, and at times *black*, through domestication. It is only on a black coat that the tan-spots would be conspicuous, and simulate eyes.

Perhaps Mr. A. R. Wallace may throw light on the matter. The spots seem to be the only really permanent marking among dogs, and are now being bred out. S. E. PEAL.

Sibsagar, Asam, February 19.

MR. PEAL'S suggestion appears to be a probable one, and is supported by Mr. Worthington Smith's observations (NATURE, vol. li. p. 57). The spots may have been protective to the animals during sleep, causing them to look as if awake. The reason that they do not occur in wild dogs may be that the latter conceal themselves when sleeping, which the half-domesticated animals were not able to do.

ALFRED R. WALLACE.

THE AGE OF THE EARTH.

SINCE physicists do not seem to be in complete accord on the question of the time which has elapsed since the earth first permanently crusted over, it may perhaps be as well to investigate the evidence to be obtained from a study of stratified deposits.

One of the first to raise a remonstrant voice against the philosophers who demanded practically unlimited time was Sir Archibald Geikie, whose original discussion of the data known regarding the present working of rivers gave us the fraction $\frac{1}{6000}$ as representing the annual rate at which the Mississippi is lowering its basin. The surprise with which this result was received is now almost forgotten, in an unquestioning acceptance. The question of the rate of deposition was next treated by Dr. Haughton, in the year 1880, with his usual mathematical severity. Dr. Haughton, however, preferred to take into consideration six other great rivers besides the Mississippi, and thus obtained the fraction $\frac{1}{3090}$ as representing the average thickness of rock which is annually worn away from the terrestrial surface by the denudation of rivers. But the proportion of sea-bottom to land surface is as 145 : 52, so that if the suspended sediment be spread evenly over the sea-floor, the average rate of accumulation will be $\frac{1}{8616}$ of a foot per annum. The maximum thickness of the stratified series was estimated by Dr. Haughton to be 177,000 feet, and thus if the rate of deposition in the past was on the whole uniform and the same as that of the present, this thickness of rock would have required a period of 1,526,750,000 years for its accumulation. Dr. Haughton is not a uniformitarian, consequently he divided this number by 10. Dr. Wallace next made what must be considered a great step in advance, by pointing out that the sediment which is carried into the sea is not deposited uniformly over the whole sea-floor, but, as the *Challenger* dredgings clearly showed, along a comparatively narrow marginal tract. Instead, therefore, of multiplying $\frac{1}{3090}$ (the yearly rate of denudation) by $\frac{52}{145}$, he divided it by $\frac{1}{19}$ (the proportion of the area of maximum deposition to the area of denudation), and thus obtained 28 millions as the number of years required for the accumulation of 177,000 feet of rock.

A further correction was next made by Mr. C. Davison, who showed that the fraction $\frac{1}{3090}$ is obtained by an error in arithmetic, and that the true value is $\frac{1}{2400}$. Introducing this fraction into Mr. Wallace's calculation, we obtain in round numbers 22 millions of years, a close approximation to the result, deduced from physical considerations, by Mr. Clarence King.

Of late years considerable additions have been made to our knowledge of the thickness of the systems of stratified rock, and I present the following table as representing the maximum thickness of all known formations down to the base of the Cambrian, a definite horizon marked, as is well known by the occurrence of fossil remains of most of the great subdivisions of the Invertebrata:—

System.	Thickness in feet.	First appearance of
Cambrian ...	... 16,000	
Ordovician	... 14,000	
Silurian ...	... 14,000	... Fish
Devonian ...	... 20,000	
Carboniferous	... 21,000	... Amphibians
Permian ...	... 12,000	... Reptiles
Trias ...	... 13,000	... Mammals
Jurassic ...	... 8,000	
Cretaceous	... 14,000	
Eocene ...	... 12,000	... Eutheria
Oligocene	... 12,000	
Miocene ...	... 6,000	
Pliocene ...	... 2,000	
	164,000	

The total thickness is 164,000 feet, lying in a fairly continuous series, and calculating by Mr. Wallace's method, this leads to the conclusion that, in round numbers, 21 millions of years have elapsed since the beginning of Cambrian times. The truth of Mr. Wallace's argument depends on the assumption that an area of maximum deposition retains a constant position during the existence of a geological system. This is no doubt approximately the case, but so far as it is not, the deviation from stability will render Mr. Wallace's estimate deficient. On the other hand, as Mr. Wallace himself recognised, the area of maximum deposition does not extend uniformly round the coast line, but is concentrated, if one may so speak, near the mouths of rivers: the effect of taking this into account will far more than compensate for any shifting of the area. It is unnecessary to do more than point out that deposits, where they attain their maximum thickness, are of a more or less deltaic nature, and were probably deposited near the mouth of large rivers, in seas more or less land-locked. From investigations in which I am now engaged, I am led to conclude that where systems attain their maximum thickness, accumulation may have proceeded at the rate of one foot in a century, or even more rapidly.

The question largely depends on the relative size of areas of denudation and deposition: an objector to my estimate may urge that accumulation at this rate involves the existence of areas of denudation of much larger dimensions than the map will find room for. It is worth while to inquire into this, and a single example will suffice. Let us consider the coal measures of the British Isles. Suppose they cover, to the depth of half a mile, a circular area 300 miles in radius, having its centre somewhere over Anglesey, their volume will thus be 141,372 cubic miles; add to this 15,876 cubic miles for the deposits of greater thickness occurring over the North of England, and South Wales and Somersetshire. This gives a total thickness of 157,248 cubic miles. But since the maximum thickness is 12,000 feet, these will have accumulated, according to our assumption of 1 foot in a century, in 1,200,000 years. The coexistent area of denudation affords $\frac{1}{2400}$ of a foot of sediment per annum, or ·00000008 cubic mile per square mile yearly. In 1,200,000 years this will amount to nearly $\frac{1}{10}$ cubic mile per square mile; and thus the 157,248 cubic miles of sediment in the coal measures will have required a land surface 1,572,480 square miles in area for their supply. This will be represented by a circular area with a radius of 707 miles, and that an area of land several times these dimensions may have existed north and west of the British Isles during carboniferous times, is an assertion which most geologists will be prepared to defend.

So far as I can at present see, the lapse of time since the beginning of the Cambrian system is probably less than seventeen millions of years, even when computed on an assumption of uniformity, which to me seems contradicted by the most salient facts of geology. Whatever additional time the calculations made on physical data can afford us, may go to the account of Pre-Cambrian deposits, of which at present we know too little to serve for an independent estimate.

No one can regard without satisfaction the introduction into Lord Kelvin's argument of well-ascertained data as regards the melting points and other properties of rocks. Dr. Joly finds the melting point of basalt to be even lower than that of diabase, viz. 815° C., a result in accordance with that found by other investigators. These facts, though of great assistance in supporting the short chronologists of the earth's age, may prove embarrassing when the question of the physical state of the interior of the earth is ready for reconsideration. Dr. Joly finds the value of dt/dp for basalt to be 0·006, and for diabase, according to Carl Barus, it is 0·021 at 1200° C.; in either case the temperature gradient gains on the melting point gradient rapidly enough to show that, at no great distance beneath the surface of the earth, the interior, if it consist of such rocks as these, is in a state of liquidity. Geologists in general would probably be glad to purchase an internal liquid shell at a cost of several millions of years. Would not, however, the admission of the existence of liquid shells in the interior of the earth, deprive the mathematical argument, as at present formulated, of all validity?

W. J. Sollas.

THE ANNIVERSARY OF THE CHEMICAL SOCIETY.

The anniversary meeting of the Chemical Society was held at the Society's rooms, on Wednesday March 27, when the following officers and Council were elected:—

President, Dr. A. G. Vernon Harcourt, F.R.S. Vice-Presidents (who have filled the office of President), Sir F. A. Abel, K.C.B., F.R.S., Dr. H. E. Armstrong, F.R.S., Dr. A. Crum Brown, F.R.S., W. Crookes, F.R.S., Dr. E. Frankland, F.R.S., Sir J. H. Gilbert, F.R.S., Dr. J. H. Gladstone, F.R.S., Dr. H. Müller, F.R.S., W. Odling, F.R.S., Dr. W. H. Perkin, F.R.S., Lord Playfair, K.C.B., F.R.S., Sir H. E. Roscoe, F.R.S., Dr. W. J. Russell, F.R.S., and Dr. A. W. Williamson, F.R.S. Vice-Presidents Dr. E. Atkinson, Horace T. Brown, F.R.S., Prof. F. R. Japp, F.R.S., Ludwig

Mond, F.R.S., C. O'Sullivan, F.R.S., and Prof. W. C. Roberts-Austen, C.B., F.R.S. Secretaries, J. Millar Thomson, and Wyndham Dunstan, F.R.S. Foreign Secretary, Prof. Raphael Meldola, F.R.S. Treasurer, Dr. T. E. Thorpe, F.R.S. Council, Dr. P. Phillips Bedson, Bennett Hooper Brough, Prof. Harold Dixon, F.R.S., Dr. Bernard Dyer, R. J. Friswell, Otto Hehner, Dr. F. Stanley Kipping, Herbert McLeod, F.R.S., W. A. Shenstone. Dr. Thomas Stevenson, Dr. W. P. Wynne, and Prof. Sydney Young, F.R.S.

Prof. Armstrong, F.R.S., the retiring President, delivered his address, in which he gave an account of the work of the Society during the past year. The Faraday Medal was presented to Lord Rayleigh for the distinguished services he has rendered to chemical science through the discovery of argon. Lord Rayleigh responded in a few words, sharing the honour bestowed on him with Prof. Ramsay. The President then called on Prof. Ramsay, who laid before the Society an account of the discovery of Helium in Clèveite, and on Mr. Crookes, who described it spectroscopically. (These two communications will be found in another column.) In the evening the Fellows and their guests dined together at the Hôtel Métropole. Among those present were :—

Dr. H. E. Armstrong, the Right Hon. Jas. Bryce, M.P., President of the Board of Trade ; the Right Hon. A. J. Balfour, M.P., Sir Henry Roscoe, M.P., Mr. A. Vernon Harcourt, Lord Rayleigh, Sir Walter Prideaux, Clerk of the Goldsmiths' Company ; Dr. W. J. Russell, Mr. T. F. Blackwell, Master of the Salters' Company ; Prof. Odling, Sir Owen Roberts, Mr. Carteighe, Mr. Lavers Smith, Dr. W. H. Perkin, Prof. Thorpe, Dr. J. H. Gladstone, Mr. W. Crookes, Dr. Stevenson, Mr. C. E. Groves, Prof. McLeod, Prof. Percy Frankland, Prof. Rücker, Prof. Dunstan, Dr. Atkinson, Prof. Ramsay, Prof. Emerson Reynolds, Prof. Dewar, Sir H. Gilbert, Prof. Smithells, Prof. Tilden, Dr. Wynne, Mr. Alex. Siemens, Prof. Thomson, Mr. Thiselton-Dyer, Dr. Hugo Müller, Mr. Norman Lockyer, Sir P. Magnus, Prof. Meldola, Dr. W. H. Symons.

A full report of the speeches made at the dinner appeared in the *Times* of the following day, and we are indebted to it for the following extracts :—

In proposing the toast of the Houses of Parliament, the President, Dr. H. E. Armstrong, remarked with reference to argon, that the discovery which Lord Rayleigh and Prof. Ramsay had made was not a chance discovery, but was the outcome of twelve years of hard work on the part of Lord Rayleigh in pursuit of the fourth decimal. The interest in the newly-discovered argon would undoubtedly be an abiding one, and it was believed that its development would be one of the most extraordinary discoveries that had been made in our time. He also referred to the discovery of Helium by Mr. Norman Lockyer, and its recent identification by Prof. Ramsay and Mr. Crookes.

Mr. Bryce, in responding to the toast, said : Physical science, and particularly chemical science, very frequently came in contact with the work of the administrators of the Government, and hardly a day passed by when they were not connected in some way with electricity or chemistry. He had been greatly impressed by the progress made in the quantity of science teaching, which had in some places almost ousted the literary side of teaching. In spite of that large quantity of science teaching, however, the Universities had not yet provided adequate means for the preparation of science teaching ; and although there were a great many men teaching scientific knowledge and teaching it well, enough had not been done to give them a systematic training and to make them not only scientific men, but skilful, finished, and experienced teachers of science as a special branch of instruction. A good deal remained to be done in that way.

Mr. Balfour made the speech of the evening in proposing "Prosperity to the Chemical Society." In the course of his remarks, he said : In the last speech to which we listened with so much pleasure, the President of the Board of Trade reminded us that his department—one of the most important departments in the Government—was brought face to face and in the closest relation to science almost every day in connection with one or other of the great practical questions with which it has to deal. Undoubtedly that is so, but I think he will probably agree with me when I say that we politicians—he and I who are engaged in the work of every-day political conflict—cannot boast that we or those whom we represent are in the position of using science as the handmaid of great national purposes, or that we have the power to turn it and direct its great forces whither we will. For my own part, though the last thing I wish to do is to suggest that the work of practical politicians is other than a work which takes the highest qualities of a man, still I have to admit, on looking back on the history of civilisation, that if we want to isolate the causes which, more than any other, conduce to the movements of great civilised societies, you must not look to the politicians of the hour on whom, it may be, all eyes are fixed ; you must look to those who, often unknown by the multitude, whose work, it may be, is never properly realised by the men of their country till after they are dead—you must look on them and on their labours to find the great sources of social movements. It is to those who, very often with no special practical object in view, casting their eyes upon no other object than the abstract pure truth which it is their desire to elucidate, penetrate further and further into the secrets of nature and provide the practical man with the material upon which he works—these are the men to whom, if you analyse the social forces to their ultimate units, we owe most, and to produce such men, and to honour such men, and to educate such men, does the Society, whose health I am now proposing, devote its best energies. I do not think, so far as I am acquainted with scientific history, that Englishmen need fear that they have been behind the rest of the world in evolving those root ideas which are the sources of great discoveries, which are themselves great discoveries, and which are, too, the sources and roots of other great discoveries. It may be, however, that though, as a nation, we have been as productive as other nations—I put it modestly —in the men of genius who have made these fundamental discoveries, we have not, as a nation—and I do not think we have—sufficiently realised how great a bearing theory in these modern days must necessarily have upon practice if we are to keep pace with the rest of the world. We have produced great theorists—none greater. We have produced men of great practical genius—none greater. I am not sure, however, that at this moment we are not behind one at least of the great nations of the continent, perhaps more than one, in the art of combining theory and practice—in the art of so welding together in a great organic and self-supporting whole the man of genius, who at one end of the scale discovers a new law of nature, and the man of practice, on the other hand, whose business it is to turn these discoveries to account. I do not venture upon a subject upon which, after all, I am not wholly competent, and I will not develop this subject at greater length ; but I should like to do what I can to dispel the prejudice which certainly exists at this moment in certain influential quarters against technical education properly understood. Technical education properly understood, suffers greatly under technical education improperly understood, and there is so much nonsense talked upon this subject, there is so much money uselessly spent, there are so many things taught to persons who do not want to learn them, and which, if they did want to learn them, they could by no possibility turn to practical account, that it is no matter of astonishment that some persons are disposed to say that "technical education is only the last bit of political humbug, the last new scheme for turning out a brand new society ; it is worthless in itself, and not only is it worthless, but it is exceedingly expensive." While I think that those who object to technical education have their justification, it yet remains true that if you include, as you ought to include, within the term technical education, the really scientific instruction which would turn scientific discoveries to practical account—if that is what you mean, or what you ought to mean, by technical instruction, then there is nothing of which England has at this moment any greater need, and there is nothing which, if she, in her folly, determines to neglect it, will more conduce to the success of her rivals in the markets of the world, and to her inevitable abdication of the position of commercial supremacy which she has hitherto held. I do not deny that manufactures and commerce have received an immense amount of gain from theoretical investigations, and, as everybody will admit who has even the most cursory acquaintance with, let us say, the history of discoveries in electricity and magnetising power, science has been the means of great gain through industrial development. While both these things are true, I am the last person to deny that it is a poor end, a poor object for a man of science to look forward to of merely making money for himself and other people. After all, while the effect of science on the world is almost incalculable, that effect can only be gained in the future, as it has only been gained in the past, by men of science pursuing knowledge for the sake of knowledge and for the sake of knowledge alone; and if I thought that by anything that had droppe i from me to-night I had given ground for the idea that I looked on science from what is commonly called a strictly utilitarian point, that I measured its triumphs by the number of successful companies which it had succeeded in starting, or by the amount of dividends which it gave to capitalists, or even by the amount of additional comfort which it gave to the masses of population, I should greatly understate my thought; but I know that this great Society, while it has in view these useful objects, still puts first of all the pursuit of truth as its object and as the cause to which every man of science pays his devotion. Truth, not profit, must necessarily be the motto of every body of scientific men who desire to be remembered by posterity for their discoveries. These things can be done only from a disinterested motive, and it is because I believe that societies like the great Society I am addressing do more than any other organisation to attain that great object, because I think they bring together men engaged in congenial pursuits, because the stimulus of minds brought close to other minds with honourable motives, and the honourable rivalry of men engaged in the same great task, must lead to an enormous expansion of our knowledge of the secrets of nature, that I, as an outsider, not belonging to your body, but in the name of the public for which I venture to speak, wish you all success and all prosperity.

The President briefly responded to the toast.

Mr. Vernon Harcourt proposed "Learned Societies," coupled with the name of Lord Rayleigh, who briefly responded.

Sir Henry Roscoe proposed "The Visitors," and Sir Owen Roberts and Prof. Rücker responded.

Dr. W. J. Russell proposed "The President," who concluded with the toast of "The Secretaries," coupled with the name of Prof. Thomson.

SIR HENRY CRESWICKE RAWLINSON, BART.

HENRY CRESWICKE RAWLINSON was descended from the family of Rawlinsons who, in the last century, settled down at Chadlington, in the county of Oxford; he was born April 11, 1810, and in 1862 he married Louisa Caroline Harcourt, daughter of Henry Seymour, of Knoyle, Wilts, and he died on March 5 last. At the early age of seventeen he went out as cadet to India, and in a very short time made himself an excellent Persian scholar; in 1833 he was sent to Persia, his fine command of the language of that country, no doubt, influencing his selection by "John Company." For six years he served diligently, and filled many military posts in the great cities of Persia, and he succeeded in infusing something nearly akin to order in the forces of the "King of Kings." In 1839 the relations between England and Persia became "strained," and Rawlinson left the country for Afghanistan; in 1840 he was appointed Political Agent of the Indian Government in Kandahar, a post which he held until 1842. During these years he wielded the sword as often as the pen, and his courage and personal bravery in the field made him a terrible opponent of the wily Afghan. In 1844 he was sent to Bagdad as H.B.M.'s Consul for Turkish Arabia, and in 1851 he was made Consul-General, the importance of Bagdad being, thanks to Rawlinson's labours, fully recognised. In 1855 he was made Crown Director of the East India Company, and in 1856 he was promoted to the dignity of K.C.B.; two years later he was elected Member of the India Council, and in 1859 he was sent to Teheran as Minister Plenipotentiary. He represented in Parliament for a short time (1865–1868) the borough of Frome, but a Member's life offered no attractions to him.

The above brief statement of facts will indicate sufficiently the abilities of Rawlinson, who was a man equally able as a statesman, diplomat, and soldier; but there is yet another side of him of which nothing has been said, and it is that of the scholar. Before Rawlinson had been five years in India, he had read the greater part of the literature of Persia, and he was even at that time (1832) a skilled and fluent talker in Persian; long passages from the finest poets he had learned by heart, and his conversations were so full of extracts from them, that a native once described his talk as "a garden of pearls in metre." From modern Persian to the ancient language is, relatively, but a step, and when Rawlinson found himself in Persia in 1833, he turned his mind to the study of the remains of the kings who had cut their records in the rocks in the cuneiform characters. So far back as 1835, he copied the tablets at Hamadān, and without the help of books, or even any knowledge of the alphabet worked out by Grotefend in 1802, by making the same guesses as Grotefend, he identified correctly the names of Hystaspes, Darius, and Xerxes. In 1836 he collated the first paragraphs of the great Behistun inscription with the tablets at Elwend, and identified the old Persian forms of the names Arsames, Ariaramnes, Teispes, Achaemenes, and Persia; by this time he had made an alphabet of eighteen characters. Early in 1837 he had copied all the other paragraphs of the Behistun inscription, and in the winter of that year he sent to the Royal Asiatic Society his translation of the two first paragraphs which recorded the genealogy and titles of Darius Hystaspes. Without any desire to belittle the work of other investigators, we must say that these would have been inexplicable if the systems of transliteration followed by Grotefend and Saint Martin had been employed, and whatever else may be theirs, Rawlinson's discovery at this period of the phonetic values *kh*, *b*, *m* and *n* is beyond all doubt. About this time he decided that the translation of the Persian cuneiform texts could only be effected by a knowledge of Zend, and he set to work to master the contents of the work of Anquetil du Perron and M. Burnouf's commentary on the Yacna, which was, however, not in his hands until 1838; he obtained some assistance, too, from a priest of Yezd, who translated for him some Zend MSS. In 1838 Rawlinson discovered the phonetic values of *h*, *w*, *i*, *v*, *th* and *jh*, and in 1839 he had practically settled the alphabet, which in all essential points agreed with that of Lassen, published in his *Alt-persischen Keilinschriften*. Here must be noted the fact that Rawlinson never contested the priority of alphabetical discovery with Lassen, even though there is abundant proof that all he owed to Lassen was a single phonetic value; but what he did claim, and claim rightly, was the credit of having translated literally and grammatically two hundred lines of cuneiform writing which contained historical statements of the greatest value, *for the first time*, as early as 1839. In this year, while he was putting the final touches to his work, political necessity caused him to be sent from Persia to Afghanistan, and his studies were so much interrupted during the next five years, that he was unable to publish the result of his labours until 1847.[1]

Meanwhile the mound of Kouyunjik, which marks the site of the palaces of ancient Nineveh, was being explored by Mr. Layard, and the mound of Khorsabad, some few miles off, had begun to yield splendid results to its talented excavator, M. Botta. That Kouyunjik formed part of old Nineveh was always well known, and so far back as 1820 Mr. Rich picked up three fragments of clay tablets inscribed in cuneiform writing. As soon as Rawlinson could obtain copies of the inscriptions dug out by Mr. Layard, he devoted himself to the study of them, and the practical outcome of these labours were his publications:—"A Commentary on the Cuneiform Inscriptions of Babylonia and Assyria; including readings of the inscription on the Nimrud Obelisk, and

[1] In the tenth volume of the *Journal of the Royal Asiatic Society*. (London, 1847.)

a brief notice of the ancient kings of Nineveh and Babylon," London, 1850; and "Outline of the History of Assyria," London, 1852; and "Notes on the Early History of Babylonia," 1854. He had also in 1850 and 1851 revised the "Inscriptions in the Cuneiform Character from Assyrian Monuments," which Mr. Layard published in 1851. Curiously enough, though Rawlinson's translation of the Behistun inscription was accepted generally, there were many, and Sir G. C. Lewis was among them, who stated unhesitatingly that the cuneiform inscriptions had not yet been accurately deciphered, and we owe it to Mr. Fox Talbot that this view was proved to be erroneous. Rawlinson had undertaken to publish a series of cuneiform texts with English translations of the same for the Trustees of the British Museum, and Talbot, having obtained a set of the plates of the text of the great Tiglath Pileser inscription, began to work at an independent translation which, when finished, he sent in a sealed packet to the Council of the Royal Asiatic Society, pointing out that if his own translation and that of Rawlinson, when it appeared, should agree, a strong proof of the accuracy of Rawlinson's system would be established. The Council appointed a Committee consisting of Dean Milman, Whewell, Gardner Wilkinson, Grote, Cureton, and Prof. Wilson as examiners, and they asked Rawlinson, Oppert and Hincks to send in versions of the same inscription by a certain date. Talbot's arrived first, Rawlinson's next, Hincks's next, and Oppert's next; the last two scholars could not, however, translate all the inscription for want of time. The four independent translations[1] were carefully examined, and it was found that they agreed as to general sense in a marvellous manner, and the Committee rightly judged that the decipherment of the cuneiform inscriptions was a *fait accompli*; Rawlinson, however, translated the whole text, while his three competitors left passages here and there unrendered.

In the matter of publication, Rawlinson's greatest work was undoubtedly the "Cuneiform Inscriptions of Western Asia" in five vols. folio, which he prepared for the Trustees of the British Museum; here he supplied material for generations of workers, and gave the proofs of his knowledge and ability in cuneiform matters, which have justly earned for him the title of "Father of Assyriology." Between the years 1858 and 1875 he largely assisted his brother Prof. Rawlinson in his works on Oriental history, and a large share of whatever credit attaches to them is due to him. His last published translation was that of the cylinder of Cyrus, which recorded his conquest of Babylon; it appeared in the *Journal of the Royal Asiatic Society*, new series, vol. xii. (1880) p. 70. As was to be expected, honours were showered upon Rawlinson from all parts of the civilised world. He was elected to a Trusteeship of the British Museum—a much coveted honour—in 1876; Oxford, Cambridge, and Edinburgh gave him honorary degrees; Prussia awarded him the "Ordre pour le Mérite"; and the Academies of other countries elected him to Memberships. In summing up his labours, it is hard to say whether he did most for cuneiform scholarship, or to advance the interests and empire of Her Britannic Majesty in the East. Small-minded men, wishing to lessen Rawlinson's merits, have harped upon the fact that Lassen made his alphabet before Rawlinson; but this he freely admitted, only saying in reply that he never saw it until 1839. That Rawlinson was the first European who translated cuneiform inscriptions, is beyond all doubt, and from first to last, *i.e.* from 1835 to 1895, he exercised a wise and beneficent influence over cuneiform studies which cannot be overrated. His position in Persia gave him unrivalled opportunities, which he used to the best of his ability, and as Consul of Bagdad from 1844 to 1855, his strong but silent power was freely exerted on behalf of Mr. Layard while carrying on his work at Kouyunjik; beyond all doubt is it the fact that he has done more for cuneiform research and excavation than any other man living or dead. It must never be forgotten, too, that he was the only early excavator who had fully qualified himself to understand his work, and of them all he was the only one who could read cuneiform. He was a fine example of the English soldier, now only too rare, for to the bravery of the warrior he added the courtesy of the diplomat, and a wide knowledge of Oriental countries, languages, and history; his modesty was as great as his learning was deep. His ready wit and honest straightforwardness made him a favourite at every Oriental Court, and helped greatly to bring his plans to a successful issue; and his fearless bearing and manly love of warlike exercises attracted to him the admiration of the Indian and Persian soldiers who came in contact with him. His tolerance led men, both in the East and in the West, to confide in him, while his natural good-heartedness often made them like him as much as they trusted him. Other Englishmen have left behind them in the East fame for a certain exploit, or renown as great horsemen, &c., but no man was more feared and liked throughout the East than Henry Creswicke Rawlinson. He was a thorough Englishman, and with him English interests were paramount everywhere; he never forgot, too, in spite of his gracious bearing to all, that he was a trusted servant of the "Right Honourable John Company Sahib Bahadur." Wherever he went he impressed his personality in a marvellous degree, and an idea of the reputation which he left behind him in Bagdad may be gained from the remark which an old native of that city made to a British merchant some years ago. "Sahib, in the days of Rawlinson Sahib, may God lengthen his life, if you had put an English hat on the head of a dog and sent him through the bazaar, the Turks would have made way for him and bowed to him on the right and left as he passed, and now they spit on us as they pass us."

[1] See Rawlinson, "Inscription of Tiglath-Pileser I., King o Assyria, about 1150 B.C." (London, 1857.)

NOTES.

WE are glad to learn that Prof. Huxley is now rapidly recovering from his recent attack of influenza.

REFERENCE was made last week to the activity and great development of the Brooklyn Institute of Arts and Sciences. Among the most successful of its recent enterprises must be counted the establishment of a summer school of biology by the sea. The first session was held in July and August 1890, at Cold Spring Harbour, on the north shore of Long Island, in a building lent by the New York Fish Commissioners. The school annually increased in size and importance, and a specially designed laboratory, capable of accommodating fifty students, was last year opened for work in the same locality. The laboratory is well arranged for the purposes of lectures and practical instruction, and possesses private laboratories for investigators, a library, aquaria with running water, both salt and fresh, boats, microscopes—indeed everything needed to make profitable a summer at the sea-shore. It is impossible to over-estimate the advantages which marine laboratories afford for the purpose of biological instruction; and America may be congratulated upon the successful inauguration of an institution specially adapted to subserve this important end.

WE drew attention a short time ago (NATURE, vol. xlix. p. 597) to the fact that the late Sir W. Macleay had willed to his executors the sum of £12,000, for the foundation of a chair of Bacteriology in the Sydney University. The terms of the bequest provided for a six months' course on the subject, to be binding on every student before being admitted to a science or medical degree at the University. Pending the inability of the Senate to accept, within a month after notice of the legacy, the conditions contained in a memorandum annexed to the will, the legacy was to be void, and the sum given over to the Linnean Society of New South Wales, for the endowment of research in bacteriology. On receiving the money, the senators, the majority of whom are lawyers, have sought a judicial interpretation of the words "six months' course," and have secured the handsome endowment to themselves, on the ruling of Court, for disposal on their own terms. What they will do with it remains to be seen. Sir W. Macleay had been a member of the Senate, and his impression of its conduct may be gauged from the fact that in bequeathing this very legacy he took especial pains to protect his executors against that which he termed "its very uncertain views." In this he has failed! "Sharp practice" may be a conventional, but it is hardly a befitting term for the behaviour of the Senate, which thus expresses the gratitude of a colony for the devotion of a life and fortune to the advancement of science and scientific education.

We understand that the Macleay memorial volume is selling very slowly. Its promoters are considerably to the bad on the undertaking.

Dr. Doberck would be glad if astronomers who notice shooting stars having radiants south of the equator, will send their observations for discussion to the Hong Kong Observatory, where some attention is being paid to southern shooting stars.

General Sir George Chesney, to whom belongs the credit of originating and organising the Royal Indian Engineering College at Coopers Hill, died on Sunday.

Prof. Ludwig Schläfli, the well-known Swiss mathematician, has just died at Berne, at the age of eighty. In 1853 he was appointed professor of mathematics at the University of Berne, where he first acted as *privat-docent*, but some time ago he gave up his post on account of his advanced years.

Messrs. Sotheby, Wilkinson, and Hodge will include the original autograph manuscript of Gilbert White's "Natural History and Antiquities of Selborne" in their sale commencing April 22. Several passages in the manuscript have not been published, and the whole has never passed out of the possession of the lineal descendants of the author.

The Geologists' Association have arranged an excursion to the Tertiary Beds of the Isle of Wight, during Easter, under the direction of Mr. R. S. Herries and Mr. H. W. Monckton. The party will leave London next Thursday, and will return on the following Tuesday.

The Liverpool Marine Biology Committee have appointed Mr. J. C. Sumner, from the Biological Laboratory of the Royal College of Science, South Kensington, as Curator of the Port Erin Biological station.

Dr. Jentink, of Leyden, has recently drawn attention to the scantiness of trustworthy evidence concerning the distribution of the two species of Rhinoceros which inhabit the Malay archipelago. It is sometimes stated that both species (*R. sumatrensis* and *R. sondaicus*) are to be found in Borneo, Sumatra, and Java; but it appears that nothing can be maintained with absolute certainty at present, except that *R. sondaicus* inhabits Java, and *R. sunatrensis* both Sumatra and Borneo. It is clearly desirable that any authentic information bearing upon this point should be brought to light, if yet unpublished.

The Biological and Microscopical Section of the Academy of Natural Sciences of Philadelphia have just sustained a severe loss by the sudden death of Dr. George A. Rex. Dr. Rex was the highest authority an the Myxomycetes in the United States. He was the author of numerous species, which, owing to his extreme conservatism, will doubtless continue to bear his name. Many forms, new to him, remained in his collection unnamed for years, and were only published when he had thoroughly convinced himself that they were really new to science. Although he was interested principally in the Myxomycetes, he was an earnest student of the lower orders of fungi, and an ardent admirer of everything beautiful in microscopic nature.

We have also to record the deaths of a number of other scientific workers in the United States. Dr. Darwin G. Eaton died in the evening of March 17, aged seventy-two. He was prominent as an instructor in the Packer Institute and in the Long Island College Hospital. His works belong chiefly to chemistry and astronomy. He was twice a member of Government parties to observe eclipses of the sun. Dr. P. H. Van der Weyde died a few hours later, on the 18th ult. He was born at Wymeguen, Holland, in 1813, and removed to New York in 1849. He taught in the University of the City of New York, at Cooper Union, and Girard College. Dr. Henry Coppee, acting president of Lehigh University, died March 20, at Bethlehem, Pa., aged seventy-five years. He was elected Regent of the Smithsonian Institution in 1874. Mr. Isaac Sprague, well known as a botanist and artist, and illustrator of botanical works, died at Wellesley Mills, Mass., March 13.

We notice the announcement of the death of Mr. A. G. More, who for many years exercised an influence of a special kind on the zoologists and botanists of Ireland as an authority to whom they implicitly referred questions on the subject of the determination of the species of Irish birds and Irish plants. Under the modest title of "Contributions towards a Cybele Hibernica," in conjunction with the late Dr. David Moore, he published in 1866 an excellent account of the geographical distribution of plants in Ireland. Another good piece of work by him was his list of Irish birds, published in the year 1885, in connection with the collection in the Dublin Science and Art Museum, and in 1887 he published an excellent guide to the Natural History Department of the Museum. He succeeded Dr. Carte as Curator of that department, but illness compelled him to retire in 1887, after twenty years' service.

The annual banquet of the Institution of Civil Engineers took place on Wednesday, March 27, a distinguished company being present. Sir Robert Rawlinson, K.C.B. (the President) occupied the chair, and among the guests were the Duke of Cambridge, the Duke of Teck, the American Ambassador, the Marquis of Salisbury, Sir John Fowler, Sir Frederick Bramwell, Sir Redvers Buller, Mr. C. T. Ritchie, Sir F. A. Abel, Sir George Stokes, Sir Courtenay Boyle, Sir John Donnelly, Mr. Henry Kimber, M.P. (Master of the Merchant Taylors' Company), Dr. Anderson, Sir B. Baker, Mr. C. Barry, Prof. Forbes, Dr. Frankland, Mr. Hawksley, Dr. Kennedy, Sir G. L. Molesworth, Mr. Preece, Mr. Rennie, Prof. Roberts-Austen, Sir David Salomons, Prof. Unwin, Mr. Walmisley, and Mr. Yarrow. The Marquis of Salisbury, in proposing the toast of "The Institution of Civil Engineers," dwelt upon the system of sanitary military reform set on foot by Sir Robert Rawlinson, and which has had such benefical results. He continued:—The system has yet some advance to make, and I hope some day the civil engineers, if the military engineers do not do it, will remove the reproach of typhoid fever from our barracks. But an enormous distance has been traversed by the genius of civil engineers, and by none more than by the President who occupies your chair; and I think it has reacted on civil society. We never really took in hand the sanitation of our civil population until we had been taught by necessity, by the evils attacking our military forces, and the remedies that were adopted, to confront those evils; and the result of the last forty years of sanitary exertion on the part of civil engineers, is one of the most splendid triumphs in the history of any profession which has adorned this country. You have done what many a continental nation wishes it could imitate you in doing. You have driven cholera from your shores. It can no longer make any impression upon us, and year by year the health of the population grows, the sanitary reforms increase, and you have converted all those who once doubted to believe in the sanitary character of the maxims which you inculcate and the methods which you propose.

MAJOR CARDEW's report on the circumstances attending the accidents which have lately occurred in London, in connection with electric light mains, has been published in a Parliamentary paper. The accidents were of two kinds—those due to severe electric shocks caused by strong electrification of the surface of the ground, and those resulting from explosions in the street boxes through which the electric light mains run. In the City cases investigated by Major Cardew, the immediate causes of the accidents were: the breaking down of the insulation of a high pressure main in a street box, causing the formation of a powerful electric arc between the outer conductor of this main and the end of an iron pipe, forming a conduit for the mains, which projected into this box, and the consequent charging of this pipe to a dangerous extent; and the presence of gas in the Electric Light Company's pipes and street boxes to an amount which formed a highly explosive mixture afterwards fired by the electric arc. In the case of explosions in the St. Pancras district, it was suggested that the explosive gas was derived from the sewers, and also that it was hydrogen generated by electrolytic action. Major Cardew could find, however, no trace of such action, and, after careful consideration of the evidence and his own investigations, he concludes that the explosions were caused by the firing of a mixture of coal-gas and air by an electric spark. No time should, therefore, be lost in removing these two existing causes of danger, namely, the possibility of an accumulation of coal-gas in the conduits and that of the occurrence of an electric spark at the mains, whether the mains be insulated or of bare copper strip, such as is used by the St. Pancras Vestry. This Vestry has previously had their attention drawn to the necessity of efficient ventilation, but have failed to remedy the defects. The inquiry elicited the fact that there are only eleven ventilating pipes and three ventilating covers to street boxes for a total length of six and a half miles of conduit, a proportion quite inadequate for the immediate escape of gas from the conduits. Major Cardew recommends that accumulation of gas should be prevented by thorough ventilation, by making the sides and bottoms of street boxes impervious to gas, and by filling up the boxes as far as practicable with incombustible material. It is to be hoped that the Board of Trade will see that his recommendations on this and other sources of danger are acted upon.

THE New York Academy of Sciences, on Wednesday, March 13, repeated the experiment, begun last year, of giving an annual exhibition, with very gratifying results. Numerous scientific instruments were exhibited; the apparatus which excited most general interest being one for testing and photographing the voice.

A MEETING of the Institution of Mechanical Engineers will be held on Wednesday evening, April 24, and Friday evening, April 26, at 25 Great George Street, Westminster. The discussion will be resumed, on the Wednesday evening, upon the governing of steam engines by throttling and by variable expansion, by Captain H. Riall Sankey. The third report to the Alloys Research Committee will be presented by Prof. W. C. Roberts-Austen, C.B., F.R.S., and also appendices to it on the elimination of impurities during the process of making "best selected" copper, by Mr. Allan Gibb, and on the pyrometric examination of the alloys of copper and tin, by Mr. Alfred Stansfield. The anniversary dinner will take place on Thursday evening, April 25.

THE New York signal service office, a branch of the United States weather bureau, has just been removed from the Equitable building, No. 120 Broadway, which has been its headquarters for twenty-four years, to the new Manhattan Life Insurance Company's building, No. 66 Broadway, which is now one of the highest buildings in the world. The bureau will occupy the twenty-first, twenty-second, and twenty-third stories in the tower, and the observations will be made at a height of 356 feet above the street, or 380 feet above tide water. The old quarters were only 185 feet from the street. The equipment will all be new, and will include electric signal lights, which can be seen as far as Asbury Park and Fire Island. When the weather bureau was established in 1870, it occupied the top floor of a building in Wall Street for a few months. On the completion of the Equitable building, that was selected as then the highest in the city, but the rapid growth of tall office buildings all around has made it undesirable as an observing station. Even the present lofty quarters will soon be overtopped by the building of the American Surety Company, twenty-seven stories high, on the corner of Broadway and Pine Streets, between the Equitable and the Manhattan buildings.

EXTENSIVE geological researches have been made by Russian mining officers, as well as by professors of the Tomsk university, in connection with the Siberian railway, which, as is known, has already been completed as far as the Irtysh, opposite Omsk. Numerous layers of coal have been found in the basin of the Irtysh, but none of them have any industrial value. Rich palæontological collections, which very well characterise the age of the coal-bearing deposits of the Kirghiz Steppe, have, however, been made out at Bez-tyube. The best coal was found at Kuu-cheku, 40 miles from Karaganda, but it is too far from the railway (375 miles) to have an immediate practical value. The great difficulty which the builders of the railway have to contend with is the total absence of building stone in these lowlands, covered with clays and sands. Thus, for the building of the bridge over the Irtysh, near Omsk, new carlies had to be opened higher up the river, but the cost of extraction and transport was so great that most of the stone required for the bridge is brought by rail from Chelyabinsk, a distance of nearly 500 miles.

THE Society of Naturalists at the St. Petersburg University which celebrated last year its twenty-fifth anniversary, has issued an excellent volume containing a review of its five-and-twenty years' activity. Each branch (zoology, physiology, botany, and geology) is treated by a specialist, and the reviews show at a glance what has been done in each of these divisions of science; while the work of botanical, zoological, &c., exploration of the Russian Empire which has been accomplished by members of the Society, is represented on two maps. It is sufficient to say that the White Sea, the Caspian Sea, the Aral-Caspian and the Altai expeditions, which have so often been mentioned in these columns, the Crimea and the Neva Permanent Committees, and the Solovetsky biological stations, are the work of the Society. The volume is adorned by a good portrait of the late Prof. K. Th. Kessler, the founder of the Congresses of the Russian Naturalists and Doctors, and also of the Societies of Naturalists at all the

Russian Universities. The Society, which began twenty-five years ago with 74 members only, has now 374 active and 25 honorary members.

THE Kazan Society of Naturalists at the Kazan University, which was founded in 1869, has also issued a similar anniversary volume, in which the review of work done in botany is especially valuable, as it has been written on the above plan. The volume contains also an index of all papers published in the twenty-seven volumes of its *Bulletin* and *Memoirs*, the titles of the papers being given both in Russian and German.

AN old estimate of the frequency of earthquakes was that no a day passed without a shock being felt somewhere on the earth. In a new determination (*Comptes rendus*, vol. cxx. pp. 577–579), M. de Montessus de Ballore obtains a much higher figure. Dividing all the registers we possess into historical, seismologica and seismographical, and assuming the latter to be perfect, he finds, by comparing the different classes for the same region, that in the first 96·24 per cent., and in the second 84·48 per cent., of the total number of shocks escape record. In a group of well-studied earthquake districts, with a combined area of 11,691,000 sq. km., the average yearly numbers of shocks for the three classes are 341·35, 878·57, and 2222·24 respectively. Hence, multiplying by the proper factors for the first two classes, it would appear that the total number of shocks actually occurring in the above area must be estimated at 16,957 a year, or one in every half-hour.

IN several papers of great interest (*Atti della R. Acc. dei Fisiocritici*, Siena, vol. v., 1894), Prof. G. Vicentini describes his new seismometrograph erected at Siena, and the seismic record obtained with it from February to July, 1894. The instrument consists of a heavy pendulum, 1½ m. in length and 50 kg. in mass, the base of which is connected with one end of a very light vertical magnifying lever. The other end of this lever is connected with the ends of two light horizontal levers placed at right angles to one another. These magnify the original movement of the ground seventy times, and trace fine lines on two strips of smoked paper moving at the rate of 2 mm. a minute. With this arrangement the heavy mass, it is found, remains stationary during vibrations of the ground, and the trace ceases simultaneously with the removal of the disturbing cause. For seismic purposes, the instrument possesses several advantages, especially the small cost of working and the rapid movement of the paper bands. In the plates accompanying the papers, copies are given of the traces produced by passing carriages, by strong gusts of wind, and by several earthquakes, the most beautiful being those of the Japanese earthquake of March 22, the Greek earthquake of April 27, and the Constantinople earthquake of July 10.

SURGEON-CAPTAIN R. H. ELLIOT, of the Indian Medical Service, has recently reinvestigated the value of strychnine as an antidote against snake poison in the most thorough manner. He experimented chiefly with cobra poison, but also with the venom of Russell's viper and the krait, using frogs, lizards, ducks, fowls, hares, guinea-pigs, dogs, goats, pigs, and monkeys as test animals. He confirms the results of Drs. D. D. Cunningham and A. A. Kanthack, that strychnine is not an antidote against snake poison.

THAT the German Cholera Commission has worked most energetically is shown by the voluminous documents now appearing in the *Arbeiten aus dem Kaiserlichen Gesundheitsamte.* A detailed inquiry has been held into all the various outbreaks of cholera which occurred in Germany between the latter part of the years 1892 and 1893, respectively, and the mass of minute incidents thus collected have been brought together, and cover considerably over 600 quarto pages. The Hamburg inquiry not unnaturally occupies the largest volume, and is extensively illustrated with diagrams, &c. The Commission has succeeded in producing a most important and valuable contribution to the study of epidemiology, and their labours on cholera must long be regarded as the standard work on the subject. We only wish that influenza, which is now far more constantly with us, and claims such an increasing number of victims, might be subjected to the same crucial inquiry; possibly then, as in the case of the later outbreaks of cholera in Germany, we might be in a position to take some steps to effectually check its apparently capricious career.

A NEW contrivance for reading the position of the pointer in sensitive balances is described by W. H. F. Kuhlmann in the *Zeitschrift für Instrumentenkunde.* It has often been attempted to save time by accurate readings with the aid of a telescope or microscope, but none of those methods have been found to answer the purpose of the practical chemist or physicist. Herr Kuhlmann's contrivance is at once simple and effective. The pointer moves *behind* the divided scale, and the face of the latter is turned towards a concave cylindrical mirror attached to the central column of the balance. This mirror can be adjusted to face the observer, who sees in it a magnified image of the pointer rapidly moving across the magnified scale. It is thus made possible to graduate the scale much more finely than heretofore. The pointer must be very fine at the end, but any danger of its being damaged on that account is obviated by the fact of its moving between the scale and the mirror. Another improvement, described by Dr. Classen in the same number, is an arrangement for exchanging objects and weights without opening the balance case. This is accomplished by making a portion of the scale pans detachable. Each scale pan consists of a set of bent rods. One of these, bent into an irregular shape, can be lifted out and conveyed on a circular rail to the other scale pan, the latter undergoing this operation at the same time. A double weighing is no more troublesome than a single weighing, and the advantages of the method, especially when absolute weights are required, are obvious.

THE phenomenon of polyembryony, or the development of two or more embryos in a single seed, has been the subject of several investigations. It has been shown that it may be due to the division of the nucellus, or to the fusion of two ovules, or to the presence of several embryo-sacs in one ovule. Further, Strasburger has found that it is often to be accounted for by the ingrowth of some cells of the nucellus into the embryo-sac, which there develop into adventitious embryos; in other cases he ascertained that two egg-cells are normally present in the embryo-sac, which on fertilisation give rise to two embryos. Finally, Dodel and Overton showed that it was possible for the synergidæ to develop into embryos. More recently, M. Tretjakow, in a short but interesting paper (*Ber. d. Deutsch. Bot. Gesell.*, February 1895) describes yet another cause of polyembryony. In *Allium odorum*, in addition to the normal embryo formed from the egg-cell, not infrequently the antipodal cells also give origin to embryos. Sometimes only one of these develops, but M. Tretjakow has observed all three antipodal cells start into growth and give rise to three embryos. These antipodal embryos commence their development immediately after the fertilisation of the egg-cell, and the cell-divisions, at least in the earlier stages, correspond exactly with those in the embryo formed from that cell. The Russian author, accepting the view that the antipodal cells are homologous to the vegetative cells of the prothallia of ferns, compares these antipodal embryos with those arising by apogamy on fern prothallia.

IN the course of a few remarks made by Prof. D. E. Hughes, F.R.S., at the recent banquet given by the staff of the National Telephone Company, some points in connection with the early history of telephony were mentioned. The text of the speech is published in the *Electrical Engineer* for March 22, and the following note from it is interesting. Prof. Hughes said:—The earliest record of a perfect theoretical electric telephone, was contained in Du Moncel's "Exposée des Applications," Paris, 1854; when M. Charles Bourseul, a French telegraphist. conceived a plan of conveying sounds and speech by electricity. Suppose, he explained, "that a man speaks near a movable disc sufficiently flexible to lose none of the vibrations of the voice, that this disc alternately makes and breaks the current from a battery; you may have at a distance another disc which will simultaneously execute the same vibrations." Unfortunately M. Bourseul did not work out his idea to a practical end, but these few words contain the shortest possible explanation of the theory of the present telephones.

IT is now exactly thirty years since Prof. Hughes first experimented with a working telephone. In 1865, being at St. Petersburg in order to fulfil his contract with the Russian Government for the establishment of his printing telegraph instrument upon all their important lines, he was invited by his Majesty the Emperor Alexandre II. to give a lecture before his Majesty, the Empress, and Court at Czarskoi Zelo, which he did; but as he wished to present to his Majesty not only his own telegraph instrument, but all the latest novelties, Prof. Philipp Reis, of Friedericksdorf, Frankfort-on-Maine, sent to Russia his new telephone, with which Prof. Hughes was enabled to transmit and receive perfectly all musical sounds, and also a few spoken words, though these were rather uncertain, for at moments a word could be clearly heard, and then from some unexplained cause no words were possible. This wonderful instrument was based upon the true theory of telephony, and it contained all the necessary organs to make it a practicable success. Its unfortunate inventor died in 1874, almost unknown, poor, and neglected; but the German Government have since tried to make reparation by acknowledging his claims as the first inventor, and erecting a monument to his memory in the cemetery at Friedericksdorf.

SINCE the enunciation by Virchow, in 1858, of his theory of cellular pathology, much attention has been given to the study of this unit. Nearly all the unsolved problems of medical science involve, in one way or another, the consideration of some one of the functions of the cell. At the Philadelphia Academy of Natural Sciences, on February 5, Dr. C. L. Leonard directed attention to a new method of studying one of these functions. The method consists in making a consecutive series of instantaneous photomicrographs of the same microscopic field, taken at definite intervals, so that a comparative study of the series can afterwards be made. The results obtained by this method are the elimination to a greater extent of the personal equation of the observer, the procuring of incontestable proof of phenomena observed, the extension of the observations over any length of time, and the possibility of studying the changes occurring over the entire field at any one moment. The method also enables the student to study the condition of a fresh, living, unstained specimen for any length of time, in fields taken at definite intervals. So far Dr. Leonard has confined the greater part of his study to cell motion as exemplified in the movements of the red and white blood corpuscles. He exhibited to the Academy a number of photomicrographs illustrating the amœboid motion of the white blood corpuscle, and also showing motion in the red blood corpuscle. Some of the photographs seem to show that diapedesis is not a filtration due to pressure, but is due to a truly amœboid motion and power of the red blood corpuscles. Further photographs illustrated the position of the corpuscles within the capillaries, and showed the presence of nuclei in the red corpuscles of the frog while in the living tissues.

AT a recent meeting of the Paris Academy, M. Désiré Korda read a paper on a "thermo-chemical carbon cell." The author finds that if barium peroxide is heated to redness in contact with a carbon plate, the oxide becomes reduced to baryta, and a difference of potential of about one volt is produced, the carbon plate being negative. A similar result was obtained with cupric oxide as soon as a layer of potassium carbonate was placed between the oxide and the carbon, the difference of potential in this case amounting to 1·1 volts. The experiments were in each case performed by connecting a plate of gas-retort carbon by means of a platinum wire to one terminal of a Richard voltmeter, and placing on the carbon a few c.c. of the salt. A platinum wire dipping in the salt served to complete the circuit. On heating the carbon to a dull red heat in a Bunsen flame, a violent effervescence takes place, carbon dioxide being evolved, and the voltmeter shows a deflection corresponding to about 1 volt. This deflection remains constant as long as any of the higher oxide is left.

READERS of Mr. Edward Step's books and magazine articles on popular botanical subjects, will be pleased to learn that he has written a pocket-guide to British wild flowers, to be published by Messrs. Frederick Warne and Co. The title of the book will be "Wayside and Woodland Blossoms."

THE fourth part of "Dissections Illustrated" (Whittaker and Co.), by Mr. C. Gordon Brodie, containing sixteen magnificent plates drawn and lithographed by Mr. Percy Highley, has just appeared. Students of human anatomy could hardly desire a handbook in which typical dissections are more clearly displayed than they are in Mr. Brodie's work, which has now been completed. The whole work contains seventy-three coloured plates, drawn to two-thirds natural size.

THE Zoological Society of France has just published a new edition of the "Rules of Nomenclature of Organised Beings adopted by the International Congresses of Zoology (Paris, 1889; Moscow, 1892)." A copy will be sent gratis to every professor of zoology or comparative anatomy, director of a museum, library or laboratory, or assistant in the same, also to every learned society, upon application to the general secretary, M. le Dr. R. Blanchard, 7 Rue des Grands Augustins, Paris.

THE valuable work in zoology, carried on in H.M. Indian Marine surveying steamer *Investigator*, under Commanders A. Carpenter and R. F. Hoskyn, is already known to many biologists, and Part I. of the illustrations referring to it, now obtainable through Mr. Bernard Quaritch, should make it known to more. The Part contains seven splendid photo-etchings representing twenty-six remarkable fishes, and five illustrative of Crustacea. The former were prepared under the direction of Mr. A. Alcock, and the latter under the direction of Mr. J. Wood-Mason.

PROF. S. H. VINES' "Students' Text-book of Botany" (Swan Sonnenschein and Co.), the first half of which was reviewed in these columns in October last (vol. l. p. 613), has just been published in its complete form. In addition to the sections mentioned in our notice, the work now contains descriptions of the Phanerogams, and the part on the physiology of plants. The whole volume makes a comprehensive text-book of botany possessing many excellent features, and of the usefulness of which there can be no question. It is a pity that so very many literal errors should have been overlooked while the work was passing through the press. The page of errata which precedes the contents is not the sort of thing one looks for in a new book.

THE first *Bulletin* of the Bohemian Academy of Sciences, founded in 1890 by the Emperor of Austria, has just been issued. It contains no less than twenty-three separate memoirs many of which are beautifully illustrated with coloured and other plates, amongst which we may specially mention the twelve successfully-executed photographs illustrating some bio-chemical studies by MM. Kruis and Rayman. There are French, German and Italian communications, so that the *Bulletin* may with justice be called "International." Science is very variously represented, and we find contributions in the departments of mathematics, biology, chemistry, geology, physics, physiology, and bacteriology. The committee of publication is to be congratulated, not only on the high standard of the original work here brought together, but also on the successful manner in which they have produced this journal. In addition to the plates, the printing and paper are both excellent.

THE Report of the Council of the Scottish Meteorological Society, on March 27, shows that the work of the Society is extending. A new station has been established in connection with the Society at Kingussie, Inverness-shire, the instruments for which were supplied chiefly by Mr. John Anderson. The station is under the management of Dr. De Wattville, who commenced the observations on January 1. The work at the two Ben Nevis observatories, made both with the eye and continuously recording instruments, has been carried on with the same zeal and success as in previous years. Much work has been done in the offices in Edinburgh and Fort-William in recopying, on daily sheets, the hourly observations of the two observatories, in connection with an examination of a comparison of the two sets of observations in their bearings on the storms and weather of North-Western Europe. This examination has been recently commenced by Dr. Buchan. The subject is divided into these several parts—cyclones; anti-cyclones; small differences of temperature between top and bottom, including inversions of temperature; very large differences of temperature; great dryness of air at the top; marked differences of wind at top and bottom, both as regards direction and force; relations to reported storms at the lighthouses; conditions under which very diverse readings of the two barometers occur. In each of these cases the weather charts of Europe at the time are thoroughly examined from various points of view. Several of the points examined have already been investigated to some extent; but what is now attempted to be done is an inquiry into their relations to each other. The importance of the inquiry consists in the fact that the high-level station dealt with is situated right in the general path of the cyclones of North-Western Europe, whereas the other high-level stations of Europe that have been used in similar investigations are altogether outside that path. Dr. Buchan has a stupendous piece of work under way, and we trust that it may soon be brought to a successful termination.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii*, ♀) from South Africa, presented by Mr. H. W. Weguelin; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. W. H. Hayner; a Pardine Genet (*Genetta pardina*) from West Africa, presented by Mr. George Danes; a Palm Squirrel (*Sciurus palmarum*) from India, presented by Mrs. Henry Jones; a Short-tailed Wallaby (*Halmaturus brachyurus*, ♀) from Queensland, presented by Mrs. L. Thompson; a Vulpine Phalanger (*Phalangista vulpina*, ♀) from Australia, presented by Master John Simonds; a Bronze-winged Pigeon (*Phaps chalcoptera*, ♂) from Australia, presented by Lady Buchan Hepburn; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, presented by Mr. Rowland Ward; an Egyptian Jerboa (*Dipus ægyptius*) from North Africa, an Oak Dormouse (*Myoxus dryas*), South European, presented by Dr. G. L. Johnson; a Cape Viper (*Causus rhombeatus*) from South Africa, presented by Mr. J. E. Matcham; a Hoolock Gibbon (*Hylobates hoolock*, ♂) from Assam, two Gazelles (*Gazella dorcas*, ♂ ♀) from Nubia, an Oak Dormouse (*Myoxus dryas*), South European, deposited; a Brazilian Three-banded Armadillo (*Tolypeutes tricinctus*, ♂) from Brazil, a Variegated Bittern (*Ardetta involucris*), a White-spotted Rail (*Rallus maculatus*), a Sooty Rail (*Rallus rythyrhynchus*), a Rosy-billed Duck (*Metopiana peposaca*), four Burrowing Owls (*Speotyto cunicularia*) from South America, purchased.

OUR ASTRONOMICAL COLUMN.

COMET *e* 1894 (SWIFT).—The general resemblance of the orbit of this comet to that of De Vico's, 1844 I, was noticed very soon after its first appearance (NATURE, vol. li. pp. 132, 160). Mr. Barnard was, fortunately, able to determine the place of the comet on five nights at the end of January, when it was "most excessively faint and difficult, about 10″ to 15″ in diameter," as seen with the 36-inch refractor. These observations have enabled Dr. Chandler to revise the elements of the orbit, and to undertake a discussion of the possible identity with the comet of 1844 (*Astronomical Journal*, No. 338). Dr. Chandler points out that in view of the numerous close family resemblances among the periodic comets, we can distinguish between similarity or identity in the present case only by actual calculation of the principal planetary perturbations. He has accordingly calculated the perturbations, and he finds that "both in direction and approximate amount these changes are uniformly of the character required to reconcile the differences between the observed orbits of the comets 1844 I and *e* 1894." Some of the results are shown in the following table, the elements for the 1844 comet being those of Brünnow :—

	1844 I	*e* 1894	
		Before perturb.	Observed.
Longitude of perihelion ...	$278^\circ\ 48'.6$ ...	$283^\circ\ 7'.5$ ...	$296^\circ\ 34'.1$
,, ,, node	64 20 ...	60 24 ...	48 40·7
Inclination	2 54·9 ...	2 53·1 ...	2 57·9
Eccentricity	0·61765 ...	0·60282 ...	0·5719
Period	5·466 years ...	5·615 years ...	5·863 years.

Dr. Chandler considers this to be "sufficiently demonstrative of the high probability of identity to justify a more refined calculation at a proper future time." As to future observations of the comet, he is not very hopeful. "The present perihelion distance will probably be changed by Jupiter in 1897 to one considerably beyond the orbit of Mars, so that unless a favourable reversion of the change of brilliancy which apparently took place between 1844 and 1894 should occur, it will in all likelihood hereafter be invisible; at least until, at some future approach to the critical point of disturbance near longitude 165°, simultaneously with Jupiter, it shall be thrown into a path in which, near perihelion, it will be again in reach of our telescopes."

A POSSIBLE NEW SATELLITE OF NEPTUNE.—In the course of a series of micrometric measures of the satellites of Uranus and Neptune, Prof. Schaeberle observed a suspicious object near to Neptune on September 24, 1892, when the seeing was exceptionally fine (*Astronomical Journal*, No. 340). The star or satellite was so faint that it was near the limit of vision of the 36-inch refractor of the Lick Observatory. During an hour and forty minutes, the total change of position angle was 2° greater than could be accounted for by the geocentric motion of the planet with reference to a fixed star, and this strengthens the idea that the object observed may have been a second satellite. At the time of observation the distance from the planet's centre was 24"·4. Prof. Schaeberle now somewhat reluctantly publishes these facts in connection with his measures of the known satellite, as he has not on any subsequent occasion been able to detect any object in the neighbourhood of Neptune in apparent orbital motion about the planet. The unusual clearness and steadiness of the night of September 24, 1892, however, is not considered to have been equalled in the later observations.

PROFESSOR MENDELÉEFF ON ARGON.

AT the meeting of the Russian Chemical Society, on March 14, Prof. Mendeléeff made some interesting remarks on the relations of argon to the periodic system. His views are summed up as follows in a proof-issue of the *Proceedings* of the Society:—

"As regards argon we must consider, first, whether it is a chemical individual, or a mixture, and then, whether it is a simple or a compound body. The supposition that it be a mixture, lies beyond all probabilities; it is contradicted by the researches of Olszewski into the liquefaction and solidification of argon. The supposition that it may be a compound has also little in its favour. The remarkable inactivity of argon testifies in favour of its being a simple body, although there are, of course, some compounds, also endowed with the same property to some extent. The spectrum of argon, too, is characteristic of a simple body.

"Taking it as a simple body, we must then consider its possible atomic weight, the weight of its molecule being near to 40 (although, probably, a little over 40, because of a slight mixture of nitrogen with the argon). The atomic weight of argon evidently depends upon the number of atoms which its molecule contains. We must, therefore, consider the series of possible molecular formulæ: A, A_2, A_3, . . . A_n.

"Upon the first supposition, A, the atomic weight of argon would be about 40, and, like cadmium and mercury, it would be a monatomic gas.

"In favour of this supposition we have the specific heat ratio at constant volumes and pressures, K, found by Rayleigh and Ramsay to be near to 1·66, *i.e.* to the value which is considered as characteristic for monovalent gases. It must, however, be borne in mind that K varies for compound molecules, even when these last contain the same numbers of atoms; thus, for most bivalent gases (nitrogen, oxygen, &c.) K is near to 1·4, while for chlorine it is 1·3. This last figure makes one think that K depends not only upon the number of atoms in the molecule, but also upon chemical energy, that is, upon the stock of internal motion which determines the chemical activity of a body, and the quantity of which must be relatively great with chlorine. If, with the chemically-active chlorine, K is notably less than 1·4, we may admit that for the inactive argon it is much more than 1·4, even though the molecule of argon may contain two or more atoms.

"If we admit that the molecule of argon contains but one atom, there is no room for it in the periodic system; because, even if we suppose that its density is much below 20 (although this is very unlikely to be the case, and the contrary could rather be surmised), and that the atomic weight of argon should fall between the atomic weights of chlorine and potassium, the new body ought to be placed in the eighth group of the third series; but the existence of an eighth group in this series could hardly be admitted. In fact, an eighth group is characteristic of the large periods; and it establishes a link between the metallic elements of the seventh groups of the even series, with the metallic elements akin to them, of the first groups of the uneven series. It appears, therefore, very unlikely that the atomic weight of argon might be about 40.

"Upon the second supposition (A_2), its atomic weight would be about 20, and in such a case argon would find its place in the eighth group of the second series, *i.e.* after fluorine. But the same objections as above could then be raised. Fluorine and sodium are, moreover, strikingly unlike to each other. However, it must be said in favour of this hypothesis that it would have the advantages of analogy, by giving a new eighth group to an even series. If we take also into consideration that the typical series are possessed of several peculiarities, we may be justified, to some extent, in supposing that the atomic weight of argon is 20, this hypothesis being already much more probable than the former (A = 40).

"If we suppose, further, that the molecule of argon contains three atoms, its atomic weight would be about 14, and in such case we might consider argon as condensed nitrogen, N_3. There is much to be said in favour of this last hypothesis. First of all, the concurrent existence of nitrogen and argon in nature; then, the fact that many of the bright lines of the two spectra are very near to each other. Then, again, the inactivity of argon would be easily explained, if it originates from nitrogen, N_2, with giving up heat. And finally, the fact of its having been obtained, though in a relatively small quantity, from artificially obtained nitrogen. The supposition of Rayleigh and Ramsay, according to which argon has been disengaged in this last case from water, is very probable, but at any rate it is not yet proved. The hypothesis of argon being condensed nitrogen might be tested by means of introducing boron, or titanium, into an atmosphere of argon, strongly heated, and through which electric sparks would be passed.

"If we suppose, next, that the molecule of argon contains four or five atoms, its atomic weight will be 10, or 8, and in such case there is no room for argon in the periodic system.

"And finally, if we admit that its molecule contains six atoms, and that its atomic weight is 6·5, we must place it in the first series. In such case, it would probably take its place in the fifth group. Accordingly, the suppositions that argon is condensed nitrogen, N_3, or that, containing six atoms in the molecule, its place is in the first series of the system, appear to be the more probable ones, if it is a pure simple chemical body.

"From a letter received by D. I. Mendeléeff from Prof. Ramsay, it appears that the investigation of argon is being continued, and that the body finds its place in the periodic system; but the ultimate results of the researches of the two authors, who have brought before chemistry such an important new problem, and given it such an exemplary investigation, are not yet known."

TERRESTRIAL HELIUM (?).

WE referred last week to Prof. Ramsay's discovery of another new gas obtained from cleveite. The following papers, by Prof. Ramsay and Mr. Crookes, on this subject were communicated to the Chemical Society at its anniversary meeting.

Prof. Ramsay's paper was as follows:—

In seeking a clue to compounds of argon, I was led to repeat experiments of Hillebrand on clèveite, which, as is known, when boiled with weak sulphuric acid, gives off a gas hitherto supposed to be nitrogen. This gas proved to be almost free from nitrogen; its spectrum in a Plücker's tube showed all the prominent argon lines, and, in addition, a brilliant line close to, but not coinciding with, the D lines of sodium. There are, moreover, a number of other lines, of which one in the green-blue is especially prominent. Atmospheric argon shows, besides, three lines in the violet which are not to be seen, or, if present, are excessively feeble, in the spectrum of the gas from clèveite. This suggests that atmospheric argon contains, besides argon, some other gas which has as yet not been separated, and which may possibly account for the anomalous position of argon in its numerical relations with other elements.

Not having a spectroscope with which accurate measurements can be made, I sent a tube of the gas to Mr. Crookes, who has identified the yellow line with that of the solar element to which the name "Helium" has been given. He has kindly undertaken to make an exhaustive study of its spectrum.

I have obtained a considerable quantity of this mixture, and hope soon to be able to report concerning its properties. A determination of its density promises to be of great interest.

The spectrum of the gas was next discussed by Mr. Crookes, who said

By the kindness of Prof. Ramsay I have been enabled to examine spectroscopically two Plücker tubes filled with some of the gas obtained from the rare mineral clèveite.[1] The nitrogen had been removed by "sparking." On looking at the spectrum, by far the most prominent line was seen to be a brilliant yellow one apparently occupying the position of the sodium lines.

[1] Clèveite is a variety of uraninite, chiefly a uranate of uranyle, lead, and the rare earths. It contains about 13 per cent. of the rare earths, and about 2·5 per cent. of a gas said to be nitrogen.

Examination with high powers showed, however, that the line remained rigorously single when the sodium lines would be widely separated. On throwing sodium light into the spectroscope simultaneously with that from the new gas, the spectrum of the latter was seen to consist almost entirely of a bright yellow line, a little to the more refrangible side of the sodium lines, and separated from them by a space a little wider than twice that separating the two sodium components from one another. It appeared as bright and as sharp as D_1 and D_2. Careful measurements gave its wave-length 587·45; the wave-lengths of the sodium lines being D_1 589·51, and D_2 588·91. The differences are therefore—

	Wave-lengths.	Differences.
D_1	589·51	
		0·60
D_2	588·91	
		1·46
New line	587·45	

The spectrum of the gas is, therefore, that of the hypothetical element Helium, or D_3, the wave-length of which is given by Ångström as 587·49, and by Cornu as 587·46.

Besides the Helium line, traces of the more prominent lines of argon were seen.

Comparing the visible spectrum of the new gas with the band and line spectrum of nitrogen, they are almost identical at the red and blue end, but there is a broad space in the green where they differ entirely. The Helium tube shows lines in the following positions:—

	Wave-length.	
(*a*) D_3, yellow... ...	587·45	Very strong. Sharp.
(*b*) Yellowish green ...	568·05	Faint. Sharp.
(*c*) Yellowish green ...	566·41	Very faint. Sharp.
(*d*) Green	516·12	Faint. Sharp.
(*e*) Greenish blue ...	500·81	Faint. Sharp.
(*f*) Blue	480·63	Faint. Sharp.

I have taken photographs of the spectrum given by the Helium tube. At first glance the ultra-violet part of the spectrum looks like the band spectrum of nitrogen, but closer examination shows considerable differences. Some of the lines and bands in the nitrogen spectrum are absent in that from the Helium tube, whilst there are many fine lines in the latter which are absent in nitrogen. Accurate measurements of these lines are being taken.

ISOLATION OF FREE HYDRAZINE, N_2H_4.

M. LOBRY DE BRUYN contributes a memoir of special interest to the current issue of the *Recueil des Travaux Chimiques des Pays-Bas*. It is not long since the distinguished Amsterdam chemist succeeded in preparing for the first time free anhydrous hydroxylamine, and now he announces that he has likewise been successful in isolating free hydrazine by a similar method. Eight years ago the important discovery of hydrazine was made by Prof. Curtius, and since that time the amount of knowledge which has been accumulated concerning the base and its compounds by Prof. Curtius and his assistants is so large that a separate volume might well be devoted to it. Nevertheless, the free anhydrous base itself has not hitherto been satisfactorily prepared; indeed it would now appear, in the light of M. de Bruyn's remarkable work, that it has not hitherto been in any way isolated. The hydrate of the base has been obtained in the pure state and fully described by Prof. Curtius, but in his later communications he has expressed the view that the free base is so unstable that most probably it is incapable of separate existence. The hydrate only is produced when the salts are decomposed by a caustic alkali, and even digestion in a sealed tube at 170° with anhydrous baryta, has failed to detach the water molecule from its combination with hydrazine. It appeared, however, to M. de Bruyn that the nature of the salts and other compounds of hydrazine rendered it scarcely probable that the base was less stable than hydroxylamine, and he considered it not unreasonable to hope that it might therefore be isolated in an analogous manner to the latter base, namely, by reacting upon the chloride with sodium methylate in methyl alcohol solution. The experiments made in this direction are only preliminary, but their result is so interesting that an account of them is at once published.

The salt employed was the chloride N_2H_4.HCl, prepared as described by Prof. Curtius. Ten grams of this salt in powder were added to 200 c.c. of pure methyl alcohol, and a solution of the calculated quantity of sodium methylate CH_3ONa in methyl alcohol were subsequently added. Common salt was immediately precipitated without any perceptible rise of temperature. The mixture was consequently boiled for half an hour in a flask fitted with an upright condenser. After cooling the sodium chloride was removed by filtration, and the solution submitted to distillation. At first mainly methyl alcohol passed over, but after a time the distillate began to contain augmenting quantities of hydrazine; the pressure was then reduced, and four further quantities separately collected. The temperature of ebullition rose to 55°, although the pressure was materially reduced. The last 20 c.c. contained the greater portion of the base. This last fraction was then again distilled at the ordinary pressure, until a residue was left which contained 73 per cent. of hydrazine. The hydrate of hydrazine only contains 64 per cent. of the base, hence it was evident that some free hydrazine had been obtained, and that hydrazine is a comparatively stable substance boiling at a temperature higher than that of methyl alcohol.

In a second experiment 42 grams of chloride of hydrazine were treated in a similar manner with methyl alcohol and sodium methylate. This larger quantity evinced some rise of temperature after the admixture, and the heat caused by the reaction of the hydrazine chloride, which at first had not all dissolved, was just sufficient to keep the liquid boiling for several minutes, when once it had been brought to the boiling point by extraneous heating. After the conclusion of the reaction the contents were cooled, avoiding the access of moisture and carbon dioxide, the sodium chloride filtered off as before, and distillation proceeded with, at first in an ordinary distillation apparatus and afterwards with the aid of a Le Bel-Henninger apparatus. A residual 40 c.c. was again fractionated under reduced pressure and six portions collected. The sixth friction contained no less than 82·6 per cent. of N_2H_4. A smaller quantity passing over after removing the sixth fraction contained over 84 per cent. The fifth and sixth portions solidified when cooled by a freezing mixture of ice and salt. The crystals melted about 4°. Although the crystals when exposed to the air exhaled dense white fumes, owing to the attraction of moisture, a number of them were quickly pressed between cooled blotting paper, weighed, and volumetrically analysed. The analytical numbers corresponded to 92 per cent. of N_2H_4, a result which, considering the extremely hygroscopic nature of the crystals, would appear to indicate that they consisted of practically pure N_2H_4. The dried crystals melted at $-1°$ to $-2°$.

A drop of the liquid obtained by fusion of the crystals did not explode when heated with a naked flame; a yellow flame was produced, however, accompanied by a hissing noise. The liquid base is heavier than water and considerable heat is evolved upon mixing it with a small quantity of the latter liquid. Dry oxygen slowly oxidises it. When paper is moistened with a drop of hydrazine and exposed to the air, it becomes hot spontaneously and fumes strongly. Crystals of sulphur dissolve promptly in the liquid base with considerable rise of temperature and formation of a reddish-brown liquid whose odour reminds one of ammonium sulphide; upon the addition of water to this liquid, sulphur is precipitated. The halogens react very violently with hydrazine, producing their acids, and liberating nitrogen. Iodine disappears instantly, and a quantitative experiment showed that the reaction proceeded in accordance with the equation $N_2H_4 + 2I_2 = N_2 + 4HI$. Potassium permanganate or bichromate act with great violence upon the liquid base, but the reaction is unaccompanied by either incandescence or explosion. The liquid appears to possess the further property of readily dissolving many salts, such as the potassium salts of the halogen acids, and nitre.

It would thus appear that, instead of being a gas, as at first supposed by Prof. Curtius, free hydrazine is at the ordinary temperature a liquid, which, however, solidifies at a temperature in the neighbourhood of that of the freezing-point of water, to colourless crystals. The base is, moreover, endowed with a very much higher degree of stability than was supposed. M. de Bruyn is now engaged in preparing it upon a very much larger scale, in order more completely to study its properties.

A. E. TUTTON.

SCIENCE IN THE MAGAZINES.

MR. CHAS. DIXON has discovered a new law of geographical dispersal of species, and he expounds its capabilities in the *Fortnightly*. Here is a statement of his conclusions:—"Species in the northern hemisphere never increase their range in a southern direction; they may do so north, north-east, or north-west, east or west. Species in the southern hemisphere never increase their range in a northern direction; they may do so south, south-east, or south-west, east or west. The tendency of life is to spread in the direction of the poles. Among the six corollaries which I have drawn from this law, mention may be made of the following. By the fourth corollary, species never retreat from adverse conditions. If overtaken by such they perish, or such portion of the species that may be exposed to them. By the fifth corollary, extension of range is only undertaken to increase breeding area. By the sixth corollary, contraction of range is only produced by extermination among sedentary species, and probably also by extermination (through inability to rear offspring) among migratory species that are neither inter-polar nor inter-hemisphere. . . . If this law of geographical distribution be true, polar dispersal of species—in other words, from the direction of the poles towards the equator—is a myth." Mr. Dixon brings forward a number of facts in support of his theory, which will no doubt be given the consideration it deserves.

An address by Mr. Leslie Stephen, on the choice of books, appears in the *National*; but, to prevent misconception, it is just as well to state at once that scientific literature is altogether ignored. Yet it is difficult to understand why this should be, for writings of men of science are apparently included in the definition stated by Mr. Stephen himself. "Literature, in short," he writes, "is one utterance of Matthew Arnold's *Zeitgeist*—the vague but real entity which is a summary of all the sympathies and modes of thought and feeling characteristic of the best minds at a given stage of human progress." A few natural history notes will be found in the *National*, in an account, by Miss Balfour, of a journey through the British South Africa Company's territory, in 1894.

Among other popular articles on natural history in the magazines received by us, we notice "Nestlings," by the Rev. Theodore Wood, in the *Sunday Magazine*, and "Snake-Taming" in *Chambers's Journal*. This periodical also contains a very readable elementary description of the great Indian Trigonometrical Survey. Mr. L. N. Badenoch describes a number of species of Plasmidæ in *Good Words*. In the same magazine Sir Robert Ball writes on the life and works of Copernicus. Under the title, "Tesla's Oscillator and other Inventions," a good account of some of Mr. Tesla's recent electrical work is given in the *Century*, by Mr. T. C. Martin. The article "discloses a few of the more important results he has attained, some of the methods and apparatus which he employs, and one or two of the theories to which he resorts for an explanation of what is accomplished." It is illustrated with fifteen figures, all of which possess points of interest. Mention must be made here of a short biographical sketch of Helmholtz, contributed by Mr. Martin to the March number of the *Century*, but overlooked at the time. The sketch is illustrated by a fine engraving from a photograph of Helmholtz, taken in 1893. A brief note in *Cassell's Family Magazine* describes some curious tubular dwellings constructed against the side of a small aquarium by the species *Amphithoæ littorina*. The tubes are semicircular, and composed of sand and small pieces of seaweed, cemented together with a glutinous matter secreted by these shrimps.

The practicability of constructing a railway from the Mediterranean to India is discussed by Mr. C. E. D. Black in the *Contemporary*. Over India proper there are 18,500 miles of lines open to traffic. But westward these lines break off at Peshawur, Chaman, and Kurrachee. It is proposed that a line should be constructed from Port Said, through Northern Arabia, along the edge of the Persian Gulf, to Kurrachee—a distance estimated at 2400 miles.

In addition to the magazines mentioned in the foregoing, the *Humanitarian*, *Scribner*, and *Longman's Magazine* have been received. A portrait of Prof. Bonney accompanies an article on "Science and Faith" in the first of these magazines.

PRECIOUS STONES, AND HOW TO DISTINGUISH THEM.[1]

AMONG the duties which fall to the lot of an official in the Mineral Department of the British Museum, in his otherwise unromantic and sternly studious life, is one which is not altogether devoid of human interest. It may happen, for example, that a lady having inherited a priceless heirloom in the shape of a large emerald, travels from the Antipodes in order to sell it in England for its true value, and desiring to display its charms brings it to the Museum. To inform such a person that the stone is but green bottle glass cannot be a pleasant task.

Only within the last few months came an Afghan prince who had sold his worldly goods, travelled to the coast of India, and worked his passage to England, having secreted about his person a stone which he supposed to be of enormous value. His story was that as he slept upon the hillside, Mahomet had appeared to him and told him that he would find a rare jewel under his hand. The poor man could not be convinced that a stone with this celestial guarantee could be anything common; for, as he said, "Mahomet cannot lie." Be this as it may, the stone was quartz, and its princely owner could only be advised to repair his fallen fortunes in some Oriental fashion at Constantinople—Kensington.

It is curious that the stones brought by such people are always, in the opinion of their owners, gems of the greatest value and rarity. Could they but have consulted some competent expert nearer home, they would have been saved time and money and bitter disappointment.

But after such interviews, I have always been very forcibly impressed by the fact that even the experts do not seem in the least aware of the simple and certain methods which have been placed at their disposal by recent mineralogical research. There is, perhaps, no subject in which experts have been so slow to take advantage of practical methods supplied by science as in the manipulation and discrimination of precious stones.

The stones brought by these chance visitors have often been bought and sold over and over again under totally false names. There is, I suspect, scarcely a collection, public or private, in which some of the jewels are not wrongly described.

Mistakes are constantly made; and these are sometimes of considerable commercial importance. It may be remembered, for example, that a few years ago much excitement was caused by the discovery of rubies in the Macdonell Range in Southern Australia. Much time and money was wasted in their extraction before it was discovered that, like the so-called Cape rubies, they were merely garnets.

I should be the last person to underrate the great value of that knowledge which results from long experience, or to deny that in ninety-nine cases out of a hundred an expert may be absolutely right. Every one must admire the confidence with which a practised eye can even pick out from several packets of diamonds those which came from a certain mine.

Such a professional expert may in five seconds pronounce a judgment which it might require half an hour to establish by scientific methods, and one which may be equally correct.

But there is a vast difference between "may be" and "is," and scientific men are not satisfied with that sort of judgment, but require actual proof.

One ought to distinguish between two sorts of expert knowledge—that which results from long experience and the training of eye and hand, and that which results from familiarity with scientific methods. To have confidence in the non-scientific expert, one must place reliance upon his personal character and the soundness of his senses, and be sure that his actual experience has included problems similar to the one submitted to him, and even then he may fail in that hundredth case.

But the scientific tests cannot err; moreover, they furnish a proof which carries conviction to all who see it. The opinion of the expert need *convince* none but himself.

An exact parallel is to be found in medical practice. It is no doubt often possible for a doctor of experience to diagnose diphtheria and phthisis by their symptoms. But in recent years new methods have been made available by the discoveries relating to bacteria, and at the present time no diagnosis of diphtheria or of the early stages of consumption would be con-

[1] A lecture delivered at the Imperial Institute, by Mr. H. A. Miers.

sidered complete which did not include the bacteriological evidence ; that is to say, the isolation and microscopic examination in each case of the specific bacillus. What is more, such evidence is proof positive of the existence of the disease.

Now the only characters at all generally employed by persons connected with the trade in precious stones are two—namely, the hardness and the specific gravity or weightiness.

If a stone scratches quartz, and is scratched by topaz, it is said to have a hardness between that of quartz and that of topaz ; if it scratches topaz, but is scratched by sapphire, it is said to have a hardness between that of topaz and that of sapphire. All minerals, including the gem-stones, have been tabulated according to their hardness with reference to ten standard stones, of which the diamond, the hardest of all known substances, heads the list. If, for example, a red stone, supposed to be a ruby, is found to be only about as hard as topaz, it cannot be a true ruby, but must be a spinel ruby, which is quite a different thing ; or if it is sufficiently soft to be scratched by rock-crystal, it is probably a red garnet.

This test is obviously a very rude one in more senses than one. Not only does everything depend upon the nature of the scratching part, whether it is a sharp corner or a curved surface, and upon the direction in which the scratch is made ; but, to say the least, the surface of a gem is certainly not improved by scratching.

The second test—that of the weightiness—is a really accurate and scientific one, provided that it be made by means of a delicate chemical balance. A stone which is, bulk for bulk, three times as heavy as water, is said to have a specific gravity of 3 ; one such as topaz, which is three and a half times as heavy as water, is said to have a specific gravity of 3·5. The ordinary method is to weigh the stone, suspended by a thread, first in air, and then immersed in water. The difference is exactly the weight of the water displaced by the stone, and so the specific gravity is easily found.

The objections to this method are, firstly, that it is too laborious ; and secondly, that it is not applicable when the stone is very small, because it is then impossible to weigh it with accuracy under water. I should not rely upon the specific gravity of a stone under two carats in weight as determined by this method. A method which I shall presently describe is perfectly free from both these objections.

Incredible as it may seem, the estimation of hardness and the specific gravity are the only attempts at anything like scientific measurement ever made in the ordinary course of business applied to stones ; and even then the weightiness is usually estimated merely by poising the stone in the hand. For the rest they are identified by their colour, their fire or sparkle, their lustre and their general appearance.

In a lecture delivered to the Society of Arts in 1881, Prof. Church drew attention to the necessity of scientific methods for this purpose, and has more than once, on subsequent occasions, reiterated his plea. I propose to dwell more particularly on improvements which have been introduced since the date of his lecture, and to indicate how one may, by simple practical tests, which require little special knowledge, distinguish with certainty all gem-stones without in any way injuring them.

Chemical analysis is, by the very nature of the problem, out of the question, for in order to make an analysis, or to apply the simplest chemical test, it is necessary to destroy a part of the material ; and this cannot be done, at any rate in the case of a cut stone.

We can begin by dismissing the hardness as a character which it is really unnecessary to determine, except to identify diamond or to distinguish a real stone from paste ; here, I know, I shall earn a rebuke from the orthodox mineralogist who, in order to pursue the study of what should be a peaceful science, arms himself with a knife, and proceeds to scratch everything which he comes across.

The weapons which I would recommend are of a milder nature : the microscope, the spectroscope, the goniometer, and the dichroscope.

Among the available characters of gems, first and foremost are the optical properties ; that is to say, the appearances seen when we look at them, or through them, in various ways.

The extent to which a ray of light is refracted on entering and leaving a transparent stone, is a characteristic property most useful for determination. As everyone knows, a stick half immersed in water appears bent owing to the refraction of light on passing out of the water ; if it is immersed in a more highly refractive liquid, it appears more bent.

To ascertain the refractive power of any transparent substance like glass, a prism-shaped piece is cut from it, and the extent to which a ray of light is refracted on passing through the prism is measured by the goniometer, an instrument found in every physical laboratory.

I have not seen this recommended as a method to be practically used, because it is commonly supposed that a special prism must be cut from the stone for the purpose. For the benefit of those who possess a goniometer, I may say that it is a method which I constantly apply, and find most useful for unmounted cut stones. It is always possible to find two of the facets which form a convenient angle, and, after inking over the remainder of the stone, to trace the ray passing through these two facets, and so to measure with absolute accuracy not only the refraction but the double refraction of the stone ; moreover, this method is applicable to any stone, however great its refractive power.

Another simple plan which can be used by any one, but unfortunately only for stones of comparatively low refractive power, has been invented during the last few years. This delightfully simple little instrument, known as the reflectometer, consists of a hemispherical glass lens viewed by an eye-piece containing a graduated scale ; it need only be pressed against the plane surface of a cut stone previously touched with a drop of liquid of higher refractive power than the stone itself. On looking into the eye-piece a shade is seen over half the field of view, and its edge crosses the scale at a point which gives the exact refractive index of the stone. The best available liquid is monobromo naphthalene, which has a refractive power higher than that of topaz, and enables one at a glance to distinguish a cut topaz or any less brilliant gem-stone.

Most useful, again, are the so-called interference figures—the appearances seen on looking through a transparent stone by means of a polarising microscope, such as is used by every geologist. There is, of course, nothing new in these figures ; they are now employed by geologists in the study of rocks, and even sometimes by those whose business it is to distinguish precious stones.

Without endeavouring to explain the nature of these figures, except to say that they are due to the double refraction of the crystal, it is easy to show that by looking at a stone through a microscope, one may see something very characteristic.

(The interference figures of several minerals were thrown upon the screen by means of a projection apparatus lent by Prof. Ayrton ; sapphire, tourmaline, and emerald were shown to give coloured circular rings intersected by a black cross ; sphene and chrysoberyl, coloured oval rings intersected by a hyperbola ; and quartz, coloured circular rings with a black cross having a tinted centre.)

This beautiful method is not employed nearly so largely as it deserves, because most people find it difficult. In order to see the figure it is necessary to look through any given crystal in one certain direction. (The stones used for projection were plates appropriately cut for the purpose). Now it may happen that a facetted stone is so cut that to look along the required direction would be to look through an angular corner ; and every one knows that it is not possible to look through a pointed corner, owing to the refraction of the light. For this reason when an unmounted cut jewel is held under the polarising microscope, and yields no interference figure when turned about into various positions, it is usually given up as hopeless. But obviously we have only to immerse the stone in some liquid having nearly the same refractive power as itself, in order to eliminate the difficulty due to refraction. I find that if the stone be placed in a small tube filled with oil or glycerine, and held in various positions, the interference figure can always be seen. Little more than a year ago, a small facetted stone of peculiar appearance was sent to me, which had deceived the experts to whom it had been shown, although agreeing in some respects with quartz, and was supposed to be a new stone. But by immersing it in oil in a hollow glass sphere, I was able to see the characteristic interference figure of quartz. When a stone has the refraction, the double refraction, the specific gravity, and the characteristic interference figure of quartz—it is quartz and nothing else.

Other optical characters of great value are those resulting from the absorption of the light in its passage through a crystal;

some of the colours contained in ordinary daylight are more absorbed than others, and the light emerges more or less coloured; in consequence of differences of absorption, some gem-stones appear differently coloured according to the direction in which one looks through them. I need not dwell upon this curious property because the instrument used to observe it is the one piece of scientific apparatus sometimes, but by no means generally, used by gem experts—I mean the instrument known as the dichroscope. (A diagram, kindly lent by Prof. Judd, illustrated the appearance seen with this instrument.) Far less familiar is the method of studying the absorption by means of the spectroscope, although the value of this extremely simple method was pointed out many years ago by Prof. Church. Every one knows the colours of the spectrum seen by daylight through a glass prism, and it is also well known that if light transmitted through various vapours be appropriately observed through such a prism by means of the spectroscope, certain black lines are seen in the spectrum, indicating that the vapour has absorbed light of a certain colour; in this way astronomers are able, by merely looking at the sun and stars, to ascertain many of the elements which they contain.

But it is not commonly known that a precisely similar effect is produced by many transparent minerals. It is only necessary to look through a pocket spectroscope in a bright light at any transparent mineral containing the rare element didymium, and certain black bands characteristic of that element are at once seen in the spectrum.

(A diagram of the spectrum of the phosphorescent light emitted by ruby when made to glow in the electric discharge in a vacuum tube, lent by Prof. Crookes, though not a picture exactly of what is here described, served to illustrate the appearance of the black bands in the spectrum of a red mineral.)

Now, there are two gem-stones which give very characteristic black bands when looked at through a spectroscope, namely, the jargoon or jacynth, and the variety of garnet known as almandine, commonly called carbuncle. When a stone, say one set in a ring, is looked at in this way, and gives the characteristic spectrum of zircon, it is at once known to be a jargoon without further trouble.

When one remembers how many pocket spectroscopes are bought by people who wish to see the rain-band and predict the weather, it is surprising that it has not also come into use for the examination of gems.

To pass from optical to other characters, there is a very remarkable property possessed pre-eminently by one mineral which has not, so far as I know, been previously recommended as a practical test.

A crystal of tourmaline while being warmed or cooled becomes electrified; one end becomes charged with positive, the other end with negative electricity. The fact has long been known. But a few years ago an extremely pretty and ingenious way of showing the electrification was devised by Prof. Kundt. If a mixture of powdered red lead and sulphur be shaken or blown through a sieve, the particles become electrified by mutual friction, and if it then be dusted upon a crystal of tourmaline which is being warmed or cooled, the positively electrified end of the crystal attracts the negatively electrified yellow sulphur, and the other end attracts the positively electrified red lead; one end of the crystal becomes red, and the other end yellow; and so the difference of electrification is made visible. Now every crystal of tourmaline behaves in this way, and I find it perfectly easy to show the property in an ordinary small jewel, even when mounted in a setting. All that is necessary is to warm the stone, and then, while it is cooling, to dust it with the mixture; at once one part of the stone becomes red, and another part yellow.

(A facetted stone treated in this way was shown upon the screen by reflected light.)

The last character which I have to mention is the one to which I alluded at the beginning, namely, the heaviness or specific gravity. The use of the balance is, as I said, too laborious; but within the last few years an entirely different method has been introduced.

Cork and wood float in water because, bulk for bulk, they are lighter; stone and iron sink because, bulk for bulk, they are heavier than water. But find some substance whose density is exactly that of water, and it will neither rise nor sink, but will remain poised in the water like a balloon in mid-air.

Several liquids have been discovered which are more than three and a half times as dense as water, in which, therefore, amethyst, beryl, and other light stones will actually float. Prof. Church strongly recommended mercuric and potassium iodide; but a still more convenient liquid is now available, namely, methylene iodide. This liquid has a specific gravity of 3·3, so that tourmaline readily floats in it; further, it is not corrosive or in any way dangerous, which is more than can be said for several of the other liquids which have been recommended.

Now it is scarcely possible to prepare a number of liquids, each having the specific gravity of one gem-stone, in order to identify each stone, but methylene iodide is easily diluted by adding benzene to it; each drop of benzene added makes the liquid less dense, and so it may be used to separate tourmaline and all the lighter gem-stones from each other. Nothing can be easier or more satisfactory than this method; no matter how minute the stone may be, it can be identified by its density in a few moments. Suppose it be doubtful whether a certain gem is aquamarine or chrysoberyl, all that is necessary is to place it in a tube of the liquid, together with a small fragment of true aquamarine to serve as an index; if it be a chrysoberyl, which has a specific gravity of 3·6, it will sink like lead; if it be an aquamarine, which has a specific gravity of 2·7, it will float; and if the liquid be then stirred and diluted until the index fragment is exactly suspended, the gem also will neither float nor sink, but will remain poised beside it.

The delicacy and simplicity of the method is marvellous; the only reason why it has not been more generally adopted is that, unfortunately, the greater number of gem-stones are heavier than methylene iodide. What is the use of employing such liquids when they cannot float jargoon, carbuncle, sapphire, ruby, chrysoberyl, spinel, topaz, peridote, and diamond, to mention only those stones whose names are familiar?

But this objection is now entirely removed, thanks to a discovery made quite recently by the distinguished Dutch mineralogist, Retgers. He has found a colourless solid compound which melts, at a temperature far below that of boiling water, to a clear liquid five times as dense as water; and therefore sufficiently dense to float any known precious stone.

This compound is the double nitrate of silver and thallium, and it further possesses a most remarkable property; it will mix in any desired proportion with warm water, so that by dilution the specific gravity may be easily reduced. The fused mass may be reduced in density by adding water drop by drop so as to suspend in succession jargoon, carbuncle, sapphire and ruby, chrysoberyl, and spinel.

This wonderful compound should certainly be employed by all who wish to distinguish gems with ease and certainty.

Let me now remind you how one could apply the methods which I have been describing, to identify with absolute certainty some gem-stone. Take, for example, a cut tourmaline. Dropped into methylene iodide it would just float, and, when the liquid is diluted, it would remain suspended beside an index fragment of tourmaline, and no other gem-stone. Examined with the dichroscope it would show two coloured images, indicating remarkable differences of absorption characteristic of tourmaline, and no other mineral; the absence of absorption bands, when it is viewed through the spectroscope, would show that it is neither garnet nor jargoon; in the polarising microscope it would show the interference figure of tourmaline.

Even if the stone were mounted in a setting so that these tests could not be applied, it could be examined with the reflectometer, the boundary of the shade would cross the scale at a point exactly corresponding to the refractive power of tourmaline; and lastly, it could be warmed and dusted with red lead and sulphur, when the two coloured patches would betray the electrical properties of tourmaline. There is enough evidence here to satisfy any one but an English jury hearing expert witnesses, and everything can be done without inflicting even a scratch upon the stone.

Another mineral character of great value in distinguishing gem-stones in the rough I have not alluded to, because it can only be made use of when they are more or less well crystallised; I mean the shape of the crystals. (This feature was illustrated by some very beautiful photographs of gem-stones and other minerals in their natural state, which were taken from specimens in the British Museum by the distinguished photographic expert, Mr. Hepworth.)

It might be asked, with some show of reason, why should we require all these scientific tests which I have described, when the varieties of precious stones are so few in number? In reality, however, gem-stones are far more numerous than is commonly supposed, although they often pass muster under erroneous names. Tourmaline is sold as ruby, cinnamon stone as jacynth, white jargoon and phenacite as diamond, while green garnets are universally known in the trade as olivine or peridote.

That the varieties of available gem-stones are not far more numerous, is due mainly to the prejudice of purchasers, who ring the changes on diamonds, rubies, sapphires, and emeralds, and have heard of nothing else; estimating the stones, as the public estimates popular actors or authors, not by their real excellence, but by their names.

In the mineral gallery of the British Museum are many examples of cut stones which have rarely or never been employed in jewellery, but should certainly win favour on their own merits.

One very curious example is a little gem cut from a crystal of the ordinary tin-stone, the same ore which is worked for tin in the Cornish mines. This is a stone which, when cut from a sufficiently transparent crystal, possesses a most beautiful lustre and colour.

As another example, I may mention a stone which, I suspect from its appearance, would make a very beautiful gem. It was sent with some other fragments from the ruby mines of Burmah; it is only a single rough fragment, and has completely puzzled every one to whom I have shown it. By means of the very tests which I have been describing, and without sacrificing more than a pin's point of the stone, I have been able to identify it as the boro-silicate of lime known as Danburite. This mineral, if it has ever been used in jewellery. which is most unlikely, has certainly never been rightly named.

(A number of facetted stones lent by Mr. Gregory, who has made many interesting experiments in this direction, were thrown upon the screen by reflected light; among these were several of the less familiar gems, such as tourmaline, chrysoberyl, phenacite, felspar, andalusite, axinite, spodumene, sphene, and idocrase.)

I do not know whether the final impression produced by what I have said, is that the determination of stones is an easy or a difficult thing. The impression which I wished to convey, is that where these scientific tests can be applied, it is an absolutely certain thing; and where they cannot be applied, there is no such certainty.

The crystals from which these gems are cut are changeless and imperishable, their beauty has been enhanced by the art of man, but they have lost none of their wonderful properties in the process; in fact, it is only by utilising these very properties that the lapidary converts them from dull stones to flashing jewels, and it is by these properties that we have to recognise them.

The ruby formed countless ages ago in the heart of Burmah, is the same thing in all essentials as the ruby formed to-day in a Paris laboratory.

It is curious to reflect that the diamond which to-day glitters in a London ball-room, may have adorned the crown of some Oriental monarch centuries ago—may have been picked from the shores of an Indian stream in the dawn of civilisation—may have been the silent witness of the growth and decay of empires—but by its own unchanging existence has always borne steadfast evidence to the everlasting laws of nature.

H. A. MIERS.

THE OBSERVATION OF EARTH-WAVES AND VIBRATIONS.

THE object of this communication is to call attention to the apparently high velocity with which motion is transmitted from an earthquake centre to places distant from it a quarter of the earth's circumference, and to the importance of instituting an extended systematic observation of these movements.

During the last few years Dr. E. von Rebeur-Paschwitz and other observers in Europe have recorded earth movements which had their origin in Japan or in other distant countries. Beyond a radius of a few hundred miles from their origin these disturbances are often too feeble to be sensible or to be recorded by ordinary seismographs. Their presence is, however, made known by the use of specially contrived nearly horizontal pendulums, and by these and other instruments we find that they usually have a duration of from ten to thirty minutes, though now and then they last one or two hours. On June 3, 1893, the writer obtained a record lasting 5 hrs. 24 min. In Europe what was probably the same disturbance indicated a movement which continued for about fifteen hours. From observations hitherto made, it seems extremely likely, as Dr. E. von Rebeur-Paschwitz has suggested, that these earth-waves could be recorded at almost any point upon the surface of our globe, while the phenomena they present are such that it is probable that their extended study would throw light, not only upon the manner in which motion is transmitted through the superficial portions of the earth, but also across its interior.

As illustrative of the results to which these records lead, I take those derived from diagrams of several seismographs in Tokyo, and from that of a long pendulum seismograph at Rocca di Papa in Italy, which on March 22, 1894, together with many other instruments in Europe, exhibited considerable motion. The origin of the disturbance was off the N.E. coast of Yezo (Lat. 42° N., Long. 146° E.).

From observations made in Tokyo, distant about 600 miles from the epicentrum, not only upon the initial disturbance, but also four after-shocks, it seems that motion was propagated at an average rate of about 2·3 km. per second. Inasmuch as the instruments from which these records were obtained, are not capable of recording movements of small amplitude, probably this velocity was that of the pronounced vibrations of the quasi-elastic nature characteristic of most earthquakes. There are reasons for believing that such waves outside an epifocal area are practically confined to the surface of the earth. A movement which from the manner in which it slowly affected ordinary or horizontal pendulum seismographs, had probably a similar character, travelled from Japan to Italy with a velocity of from 2·7 to 3 km. per second, the larger waves travelling at the slower rate.

Preceding these decided motions, minute tremors were observed, which, if they originated at the epicentrum and travelled on the surface of the earth, must have done so at a rate of 11·5 km. per second, while if they were created by the transformation of the energy of the partially elastic undulations as they passed from medium to medium, then their velocity of propagation must have been still more abnormal. If it is assumed that they reached Italy by direct radiation through the earth, or that in consequence of refraction they followed curvilinear paths, the observations indicate a velocity of 9 or 10 km. per second.

Considering the influence of gravity upon the propagation of surface undulations, the observed velocities may possibly be a little lower than what might have been expected. The minute tremors, however, seem to have a velocity which is roughly twice that for a condensational rarefactional wave in glass.

Observations upon other earthquakes, although none of them can claim any great degree of accuracy, point to the same general results.

At present, the diversity of instruments employed in Europe, and the various degrees of sensibility given to the few instruments which are approximately similar, apparently results in the recording of different phases of motion, and it is not likely that our knowledge will be increased or made more accurate until there is greater uniformity in the methods of observation.

Now to determine whether the disturbances created by large earthquakes are propagated to distant localities in the manner suggested, much might be learned by establishing twelve or fourteen similar instruments at an equal number of selected stations round the northern hemisphere. It is yet premature to indicate the class of instruments to be employed, but if their chief function is to record the time of arrival and the different phases of these wide-spreading movements, it is the writer's experience that many difficulties may be avoided in installation, adjustment and management, by using a type that is not too sensitive to extremely minute changes of level, such as accompany fluctuations in temperature and changes in atmospheric conditions. All of them should admit of adjustment to a similar degree of sensibility, and so far as possible be attached to similar foundations in localities or places where the effects of tremor storms, which often eclipse the effects due to earthquakes, are not likely to be pronounced. Photographic surfaces on which records are received, should move at a rate of *not less* than two inches per hour, which will enable an observer to determine time intervals to within 30 seconds.

It would seem advisable that the first attempt to make a seismic survey of the world should be tentative. Having obtained the co-operation of observers in selected localities, each of these should be furnished with a similar instrument, and if possible receive personal instruction as to its installation and working. If this is done, then an inexpensive type of apparatus may be employed, which in an ordinary foundation will yield results not much inferior to those obtained from more elaborate arrangements, which subsequently it may be thought desirable to establish.

Although it may only be possible to minimise the effects of tremors, the records of these over extended areas may perhaps present new features. Other movements which are likely to be noted, but which will not influence the recording of movements resulting from distant earthquakes, will be diurnal and other periodic displacements of the pendulums. The records of these, together with those of local earthquakes, could hardly fail in adding to the knowledge we possess about earth movements.

The principal object of the proposal made in the foregoing remarks, is to determine the velocity with which earthquake motion is propagated over the surface of the earth, and possibly through its interior. If it is established that vibratory motion is transmitted with a measurable velocity *through* the earth, it will be difficult to over-estimate the value of the knowledge we shall have gained. The rigid scrutiny of the records bearing on this latter point have to be left to European observers.

At present the writer is engaged in drawing up a report upon the state of our knowledge respecting the velocity with which earth vibrations and waves are transmitted through rock and earth, and in making experiments to determine a form of simple instrument which shall be not only sensible to slight changes of level, but which is also capable of recording vibrations of small amplitude.

Since writing the foregoing, which was printed for circulation amongst a few of my friends interested in this branch of earth physics, I have received NATURE of December 27, 1894, in which, on p. 208 Dr. E. von Rebeur-Paschwitz gives a description of the remarkable disturbance of June 3, 1893, to which I have already briefly referred. In July of that year I sent photographs of this record to various acquaintances in Europe. One of these, together with a description of the same, because it was illustrative of a great number of unfelt earthquakes which I had recorded, was sent with the fourteenth report to the British Association on seismological work in Japan during 1893–1894. This report is, I believe, now in the press. The object in calling attention to this matter is to show that this disturbance, wherever it originated, was also pronounced at places far distant from Strassburg, Nicolaiew and Birmingham.

Mr. C. Davison, who writes the introduction to the description of the European records of this earthquake, makes a brief reference to the desirability of having a few well-chosen stations in various parts of the world where earth pulsations might be recorded—a matter on which I have had considerable correspondence with my friend Dr. E. von Rebeur-Paschwitz. At present Mr. Davison considers that Europe is fairly well provided with instruments. Instruments are certainly fairly numerous, but at the same time in many instances they vary in their sensibilities and also in their objects. Judging from the report of the Committee on Earth Tremors to the British Association in 1893, p. 294, it would seem that the instruments in Birmingham and Edinburgh are arranged to be unaffected by rapid tremors, and register "slow earth tilts only," while the tromometers of Italy are, I presume, constructed to record what this name implies. Whether they are able to record "elastic" tremors is a debatable matter. A very good illustration of what the heterogeneity of the instruments at present employed in Europe leads to, is given by Mr. Davison in the records of two earthquakes, contained in the report of the above-mentioned Committee for 1894, which may have had velocities of propagation varying between 1 and 12 km. per second, according to the type of instrument from which records were obtained. The apparent explanation of these anomalies is that different instruments have recorded different phases of motion, and for this reason I have been led to say that it is not likely that our present knowledge will be increased until there is greater uniformity in the methods of observation.

Mr. Davison's remark that "in Japan Prof. Milne's tromometer (as described in British Association Report, 1892, pp. 207–209) leaves little to be desired," requires qualification. As a "tremor" recorder it is excellent, but even as such I have improved it by reducing its length to 30 mm. and its total weight to 0·39 grms. Unfortunately however, because it is a tremor recorder, its movements are such that even on a photographic film only one metre distant, it may during a severe "tremor storm" give a trace which appears as a band two inches in breadth, which eclipses any effects due to distant earthquakes, for the recording of which it is therefore useless. At various times I have experimented with at least a dozen of such contrivances, which from the nature of their construction have necessarily short periods of vibration, and are therefore not sensible to slight tilting. Although I condemn these instruments for the recording of distant earthquakes, I must admit that I am indebted to a pair of them for having first made visible to me the diurnal wave.

In the *Seismological Journal*, vol. iii. p. 60, a sketch is given of a horizontal pendulum, such as I have used to record the daily wave and unfelt earthquakes since November 1893. One of these, which has a boom 5 feet in length, has usually a period of about 50 or 55 seconds. Altogether I have six sets of such apparatus with photographic recording surfaces, together with one or two others, which are read once per day.

These have been installed at and given records from eighteen localities—in caves, in the solid rock, in an underground chamber, and on substantial columns rising from the natural soil.

Although these instruments are exceedingly bad forms for tromometers, by these experiments—each of which continued over several months, during which time continuous records were obtained—much was learned about the localities where "tremor" effects might be avoided.

To solve the problem under consideration it does not seem necessary to have an instrument sensible to less tilting than 1″, and it is certainly undesirable to have one sensible to the phenomena called earth tremors or microseismic disturbances, which appear to have the character of earth pulsations. What is required is an instrument which is susceptible to the tilting produced by the undulatory slow travelling quasi-elastic disturbances of an earthquake, and at the same time is sensible to the minute, and possibly truly elastic vibrations which, both in Japan and Europe, outrace the more pronounced movements. The first object, and to some extent the second, is certainly attained by many instruments in Europe. From the records given by the long pendulum of Dr. Agamennone, of which I have not had an opportunity to see a full description or drawings, I take it that for the preliminary vibrations, the pendulum acts as a steady point, relatively to which the movements are magnified by means of a pointer arranged like that of a seismograph. The larger movements, which have periods of about 16 seconds, may be due to the slow heeling over of the pendulum following the motion of the supporting tower. About these latter movements we already know a great deal, and their velocities of propagation, as determined by the most accurate methods in Japan, do not materially differ from the rate at which the same disturbances continue on their journey to Europe.

What we most require is an investigation of the velocities of propagation of the elastic movements which apparently go from Japan to Europe in 15 or 20 minutes.

If such phenomena exist, and if the European records are correct, their existence is a reality, the instruments to record their repetition must be sensible to small but rapid vibrations; and for the results to be comparable, these instruments must not only be similar in construction, but they must be similarly adjusted and similarly installed.

Within a few days the writer will have completed two instruments differing only in their size, which may be described as conical pendulum seismographs in which the multiplication relatively to their centres of oscillation will be adjustable from about 20 to 40. The registration will as usual be photographic. It is hoped that because the multiplication is large, and because everything is as light as possible, the preliminary tremors (elastic vibrations) may be recorded; while because the booms are long, it is not unlikely that the sensitiveness to tilting will be sufficient to record the slower waves. They will be tested upon a foundation the diurnal tilting of which is known, and where it is also known that tremors (earth pulsations) are seldom met with.

JOHN MILNE.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 21.—"A possible Explanation of the two fold Spectra of Oxygen and Nitrogen." By E. C. C. Baly.

The two spectra of oxygen are shown to be of a different nature. They behave differently, and reasons are given for their being in all probability the spectra of different gases. They may either be two spectra produced by different vibrations of the oxygen molecule, or they may be the spectra of two different modifications of oxygen, or the spectra of two distinct gases resulting from a dissociation of oxygen, a combination of which is called oxygen.

It appeared worth while to undertake experiments with a view of testing the last of these. Oxygen was sparked in an apparatus similar to that used by Prof. J. J. Thomson in his experiments on the electrolysis of steam. Hollow platinum electrodes were used, each one of which was connected with a Sprengel mercury pump. In the first experiments, the distance between the electrodes was 35 mm., and the highest pressure compatible with the appearance of the two spectra was made the starting point of the experiments. In these first experiments it was 380 mm. The density of the oxygen before sparking was determined, and taken as a test of its purity. The fractions obtained from the anode and cathode were weighed, and the results are given. They follow the lines of J. J. Thomson's results, inasmuch as with long sparks a lighter fraction was obtained at the cathode, and with short sparks a heavier fraction. The fractions from the anode were not so definite as from the cathode, though the difference was in the right direction. The probable maximum error of weighing was 0·0001 gram. This meant exactly one in the second decimal place of the density obtained. The general accuracy of the results may be gauged from the densities of unsparked oxygen obtained.

Density of cathode fraction with long sparks.		Density of oxygen unsparked.		Density of cathode fraction with short sparks.
15·78	...	15·88	...	16·00
15·79	...	15·87	...	16·01
15·80	...	15·89	...	16·02
15·79	...	15·88	...	16·04
		15·88	...	16·06
				16·05

Mean of results of other observers = 15·887.

Density of cathode fraction from oxygen, previously for three days fractionated with short sparks, 15·75.

The experiments are still in progress.

Physical Society, March 22.—Mr. R. T. Glazebrook, F.R.S., in the chair.—On the objective reality of combination tones, by Prof. A. W. Rücker and Mr. E. Edser. The question as to the objective or subjective nature of combination tones has excited much keen controversy, and the authors have devised some experiments to elucidate this point. These experiments, some of which were exhibited before the Society, show that under certain conditions difference and summation tones are produced which are capable of disturbing resonating bodies. As resonator they have, in the first instance, employed a tuning-fork. A piece of thin wood, about 5 in. square, is attached to one of the prongs of this fork, while a silvered glass mirror is attached to the other, and the pitch of the fork is very accurately adjusted to sixty-four complete vibrations per second. In order to detect any movement due to resonance set up in this fork, the mirror carried by the prong forms part of a system of mirrors for producing Michelson's interference bands. By this means a movement of the prongs of the fork of 1/80,000 of an inch (half a wave-length of light) is shown by the disappearance of the interference bands. As a source of sound a siren was employed, this being one of the instruments which Helmholtz recommends as giving the best results. The pitch of the notes given by the siren was adjusted by noting the disappearance of the beats produced by one of the notes with a bowed fork, or by a stroboscopic method. A large wooden cone, placed between the siren and the resonating fork, served to concentrate the sound on the wooden disc attached to this latter. The sensitiveness of the arrangement is such, that when a large Köenig standard fork, giving sixty-four vibrations per second, is struck so lightly that an observer, with his ear close to the fork, cannot detect the fundamental note, the bands instantly disappear. The apparatus, however, is unaffected by any other note, except one of sixty-four vibrations per second. A number of experiments have been made, using various rows of holes on the siren, and in every case when the summation or difference tone corresponded to sixty-four vibrations per second, the interference bands vanished, showing that under the conditions of the experiment these tones have an objective existence. An experiment has also been made to determine whether Köenig's lower beat tone when the interval is greater than an octave is objective. In this case, however, the authors entirely failed to get any evidence of such an objective existence. A number of experiments have been made with a view to elucidating the cause of the production of the summation tone, which tend to show that it is not the difference tone of the partials of the fundamental notes. In addition to using a tuning-fork to detect the combination tones, the authors have made use of an instrument originally devised by Lord Rayleigh. A light mirror is suspended by means of a fine quartz fibre, and hangs in the neck of a resonator, tuned to the given note, and when at rest is inclined at 45° to the axis of the resonator. Under these circumstances, when the resonator responds the mirror tends to turn and set itself at right angles to the direction of motion of the air in the resonator. The results obtained with this instrument are in complete accord with those obtained by the first method. Up to the present the authors have failed to obtain any evidence of the objective reality of the combination tones produced by organ-pipes and tuning-forks (see p. 474). The discussion on this paper was postponed till after the reading of the next paper.—Some acoustical experiments, by Dr. C. V. Burton. (1) On the subjective lowering of pitch of a note. The author has noticed that if a tuning-fork, mounted on a resonator, is strongly bowed, then if the ear is placed near the opening of the resonator the pitch of the note heard appears lower than when the fork is bowed very gently or is held at some distance. This subjective lowering of pitch is most marked with forks of low pitch; and in the case of a fork giving a note of 128 complete vibrations per second, amounts to about a minor third. The author suggests an explanation depending on the supposition that the basilar membrane of the ear behaves as if it consisted of a number of stretched strings of various lengths, each resounding to a given note; and that the appreciation of the pitch of a note depends on the localisation of the part of the basilar membrane which resounds most strongly. Further, he shows that in the case of a stretched string, for finite displacements, the string which most strongly resounds to any note will have a "natural" period longer than the period of the disturbance; the greater the disturbance the longer will be the natural period of the strings most strongly affected. Hence when the intensity of a note increases, the tract of the basilar membrane most strongly affected is displaced in the direction which corresponds to the perception of lower notes. (2) Objective demonstration of combination tones. Where two organ-pipes are sounded and alternately separated and brought close together, an observer, at some distance, hears the difference tone much more clearly when the pipes are close together than he does when they are separate. As the position of the pipes with reference to his ear does not appreciably change, the change in the intensity of the combination tone indicates that it has a real objective existence. The author mentioned that he had sounded his two pipes, which give a difference tone of 64 vibrations per second, before the collector of Prof. Rücker and Mr. Edser's apparatus, but without obtaining any motion of the interference bands, and that he was therefore less confident of the correctness of his deductions than he had been before. Mr. Edser mentioned that Dr. Burton had suggested an explanation of the production of objective tones in the case of the siren, which depends on the production of the tones in the wind-chest of the instrument itself when two rows of holes are simultaneously opened. They had made an experiment which seemed to show that the above explanation was incorrect, for on connecting together the wind-chests of two sirens, fixed on the same spindle, by means of a short length of wide metal tubing, no effect was observed on the bands when the two notes were produced on different instruments having what was practically a common wind-chest.—Prof. Everett (communicated) said he considered the experiments described in the paper proved conclusively the objective existence of the summation tones as distinguished from supposed beat tones. He had lately been investigating the pitch of the loudest combination tone obtained when two notes having frequencies as 3 to 5 are sounded. Is the frequency of this tone 2, *i.e.* the first difference tone, or is it 1, which corresponds to the first term of the Fourier series for the periodic disturbance? In the chords 2 to 3, 3 to 4, 4 to 5, &c., the difference of the integers being unity, the first difference tone is identical with the first Fourier tone. When the difference of the two integers which express the chord is not unity, then the writer considers that experiments he has made with strings and pipes, show that the first Fourier term is usually the only combination tone that is audible. Prof. S. P. Thompson considered that care should be taken to define what we mean by the subjective or objective existence of a note. There are two very delicate methods which have already been employed for detecting the existence of a given note in the air. (1) The formation of ripples on a soap-film stretched over the opening of a resonator tuned to the required pitch (Sedley Taylor); (2) the sounds produced in a telephone connected to a microphone placed on a thin elastic membrane stretched over the neck of the resonator (Lummer). It was very important to limit our acceptance of the demonstration of the objectivity of combination tones, given by the authors of the paper, to the case actually proved, *i.e.* to tones produced by the polyphonic siren. It did not necessarily follow that if pure tones produced by tuning-forks were used, the same results would be obtained. A number of experiments had been made by Zantedeschi in 1857, in which two notes were sounded, and skilled musicians were asked to record their impression of the third tone present. In 75 per cent. of the cases the note recorded was the difference tone; in the remaining 25 per cent. it corresponded to Köenig's beat tone. Köenig himself had never heard the summation tone in the case of lightly-bowed forks. Voigt in a theoretical paper has shown that if there are two disturbances whose mean kinetic energy differ, the Helmholtz tones will be produced; but that if the mean kinetic energy of the two disturbances are equal, the Helmholtz effects soon die out, and you get beat tones or beats. He (Prof. Thompson) considered that Dr. Burton had allowed his perception of tone to be governed by the quality of the note, and that the apparent lowering of pitch was due to the variation in the intensity of the overtones present. In reply to Prof. Thompson, Dr. Burton said he did not merely perceive a lowering of pitch, but he was able to estimate the change in pitch, and say at what instant, as the vibrations of the fork died out, the lowering amounted to a tone or half a tone, &c. Mr. Boys said he found that by careful attention he could apparently persuade himself that the note in Dr. Burton's experiment was lowered or raised in pitch, or that it remained unaltered. A similar effect in the case of the eye could be obtained with a stereoscopic picture. The Chairman considered that, while Helmholtz' explanation of the production of combination tones might be real, it did not follow that this explanation gave the sole cause of their formation. In particular, Helmholtz does not explain why the tones should only be produced by some sources of sound. Prof. Rücker, in his reply, said he did not deny the existence of Köenig's beat tones; in fact, he had heard them. They did not lay much stress on the negative result of the experiment they had made to test the objective existence of these beat tones.

Zoological Society, March 19.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—Lt.-Col. H. H. Godwin-Austen, F.R.S., presented a paper on behalf of Mr. Walter E. Collinge, and himself, "On the Structure and Affinities of some new Species of Molluscs from Borneo." Three new species were described, viz. *Damayantia smithi*, *Microparmarion pollonerai*, and *M. simrothi*. Details were given of their structure and comparisons instituted with other members of the genera and allied Indian genera. One of the most interesting features, perhaps, was the similarity they show anatomically to shell-bearing molluscs of Borneo. That these slug-like forms of Borneo have the same close relationship to the shell-bearing mollusca among whom they are now found living, as the Indian forms bear to *Macrochlamys* and allied shell-bearing genera, there can be little doubt, and any true attempt at classification must be based on these lines, and would place a wide gulf between *Girasia* and *Austenia* on the one side, and *Parmarion* and *Microparmarion* on the other.—Mr. F. E. Beddard, F.R.S., read a preliminary account of new species of earthworms belonging to the Hamburg Museum. These worms belong chiefly to the genera *Acanthodrilus* and *Microscolex*, and had been collected in South America.—Prof. F. Jeffrey Bell communicated, on behalf of Prof. Alphonse Milne-Edwards, Jardin des Plantes, Paris, the description of a new species of crab of the genus *Hyastenus*, obtained near the Straits of Magellan during the *Challenger* Expedition, and proposed to be described as *H. consobrinus*.—Dr. A. G. Butler gave an account of two collections of Lepidoptera received by the British Museum. One from Zomba, made by Mr. J. McClounie, remarkable for the number of specimens of the fine Butterfly genus *Charaxes* it contained. The other made at Fwambo, Lake Tanganyika, by Mr. Alexander Carson, interesting as including not only rare species previously only received from Zomba and Lake Mweru, but several novelties, the finest of which was *Junonia pavonina*, a new form allied to *J. artaxia*.—Mr. P. Chalmers Mitchell read a paper in which he gave a description of the proventricular crypts he had found in a specimen of the African Tantalus (*Pseudotantalus ibis*) recently living in the Society's Gardens.

Entomological Society, March 20.—Prof. Raphael Meldola, F.R.S., President, in the chair.—Mr. H. St. John Donisthorpe exhibited a living female of *Dytiscus marginalis*, with elytra resembling those of the male insect. Dr. Sharp, F.R.S., said he had seen this form before, but that it was very rare in this country, though abundant in some other parts of the palæarctic region. Prof. Stewart asked if the genitalia had been examined. Mr. Champion stated that Mr. J. J. Walker had collected several females of an allied species (*Dytiscus circumflexus*) at Gibraltar with elytra resembling those of the male.—Dr. Sharp exhibited specimens of *Brenthus anchorago*, from Mexico, showing extreme variation in size. He remarked that the males varied from $10\frac{1}{3}$ mill. in length to 51 mill.; the female from $9\frac{1}{2}$ mill. to 27 mill. In the male the width varied from $1\frac{1}{3}$ mill. to 4 mill. The length therefore varied from about 5 to 1, and the width from 3 to 1 in the male.—Mr. Blandford commented on the difficulty of mounting minute Lepidoptera, Diptera, Neuroptera, &c., and exhibited samples of strips of material which he had found most suitable for the purpose of staging minute insects. He said his attention had been called to this method of mounting by the receipt of specimens from Dr. Frič, of Prague. On examination of the material he found it to be a fungus, *Polyporus betulinus*. He stated that Lord Walsingham had expressed his satisfaction with this material, and had sent him specimens, similarly mounted, from Zeller's collection. Mr. McLachlan, F.R.S., remarked that he thought the material exhibited preferable to artichoke pith, which had been used for a similar purpose.—Mr. Goss exhibited a species of a Mantid, *Pseudocreobotra Wahlbergi*, Stål, received from Captain Montgomery, J.P., of Mid-Ilovu, Natal.—Mr. Frederick A. A. Skuse communicated a paper, entitled "On a colour variety of *Heteronympha merope*, Fab., from New South Wales," and sent coloured drawings of the typical form and the variety for exhibition.—Mr. Oswald H. Latter read a paper, entitled "Further notes on the secretion of potassium hydroxide by *Dicranura vinula* (imago) and similar phenomena in other Lepidoptera." The paper was illustrated by the oxy-hydrogen lantern. Prof. Meldola congratulated Mr. Latter on the thorough way in which he had worked out his experiments, and said that in view of the small quantity of material at his disposal, the concordance in the results was remarkable. He added that Mr. Latter had, for the first time, proved the secretion of free potassium hydroxide in the animal kingdom. Mr. Blandford, Mr. Merrifield, and Dr. Dixey continued the discussion.—Mr. Merrifield read a paper, entitled "The results of experiments made last season on *Vanessa C-album* and *Limenitis sibylla*." This was illustrated by an exhibition of specimens of *L. sibylla*, and a long series of *V. C-album*, to show the effects of temperature in producing abnormal forms. Dr. Dixey said that many of the forms of *V. C-album* exhibited reminded him of *V. C-aureum*, a Chinese species, which he believed to be one of the oldest forms of the genus. He thought that much of the variation shown in this series of specimens was due to atavism, and was not directly attributable to the effect of temperature. Mr. Barrett said he was interested to find that one of the forced forms of *L. sibylla* was similar to a specimen he had seen which had emerged from the pupa during a thunderstorm. In connection with Mr. Merrifield's paper, Mr. F. W. Frohawk exhibited a series of 200 specimens of *V. C-album*, bred from one female taken in Herefordshire, in April 1894. The series consisted of 105 males and 95 females, and included 41 specimens of the light form, and 159 of the dark form. Prof. Meldola, in proposing a vote of thanks to Mr. Merrifield, Dr. Dixey, and Mr. Frohawk, said that he was glad to think that the subject of seasonal dimorphism, which had been first investigated systematically by Weismann, was receiving so much attention in this country. He was of opinion that the results hitherto arrived at were quite in harmony with Weismann's theory of reversion to the glacial form, and all the evidence recently accumulated by the excellent observations of Mr. Merrifield and others went to confirm this view as opposed to that of the direct action of temperature as a modifying influence.

Mathematical Society, March 14.—Major P. A. MacMahon, R.A., F.R.S., President, in the chair.—The President announced that he had written letters of condolence to Lady Cockle and Mrs. Cayley, and had received their acknowledgments of receipt of the same, which he communicated to the meeting.—Prof. Hill, F.R.S., communicated a paper, by Mr. F. H. Jackson, entitled "Certain Π Functions," and the President (Mr. A. B. Kempe, F.R.S., in the chair) read a paper on the perpetuant invariants of binary quantics.—Lieut.-Col. Allan Cunningham, R.E., gave a proof that $2^{197}-1$ is divisible by 7487.—The President read a letter from the Rev. T. C. Simmons, announcing what the writer believed to be a "new theorem in Probability."

PARIS.

Academy of Sciences, March 25.—M. Marey in the chair.—On the theory of surfaces and of algebraical groups, by M. Émile Picard.—New researches by Prof. Ramsay on argon and on Helium, communicated by M. Berthelot. A letter from Prof. Ramsay was read describing the spectrum of argon obtained from clèveite and the discovery of Helium.—Remarks on the spectra of argon and of the aurora borealis, by M. Berthelot. During the author's recent experiments on the condensation of argon with benzene vapour under the influence of the silent discharge, a magnificent greenish-yellow fluorescence was observed. Its spectrum consisted of a series of lines and remarkable bands. So far as the experimental conditions allowed of comparison, this spectrum recalled that of the aurora borealis. It is suggested that this phenomenon may possibly be due to the formation of some fluorescent combination of argon in the upper regions of the atmosphere under electrical influence.—Researches on the metals of Cerite, by M. P. Schützenberger. The preparation of cerium sulphate in a state of such purity as to admit of accurate determinations of the atomic weight of cerium is described. The value 139·45 is obtained for this constant by a special process of estimating the sulphuric acid in this sulphate. It is shown that other methods of obtaining the atomic weight give unreliable results. Taking various fractions of the sulphate on recrystallisation, the later fractions give a much less value for the atomic weight of cerium than the earlier ones.—Observations of Charlois' planet BU, made at Toulouse observatory, by MM. B. Baillaud and Rossard.—Observations of Wolf's planet BT (March 16, 1895), made at Besançon observatory, by M. H. Petit.—A general property of axoids, by M. A. Mannheim.—On lines of curvature, by M. Thomas Craig.—On the theory of equations to the derived partials, by M. Wladimir de Tannenberg.—On linear equations to the derived partials, by M. Emile Borel.—On the movement of projectiles in the air, by M. Chapel. A series of four equations are given which supply a complete solution to the ballistic problem for speeds ranging between 300 m. and 1100 m.—On the extension to magnesia of a method of synthesising fluorides and silicates, by M. A. Duboin. The compounds $MgF_2 . KF$, $MgF_2 . 2KF$, and $MgO . K_2O . 3SiO_2$ are described and their optical and chemical properties given.—On a new method for the preparation of chloroplatinous acid and its salts, by M. Léon Pigeon. Reduction of chloroplatinic acid in accordance with the equation,

$$BaPtCl_6 + BaS_2O_6 + 2H_2O = H_2PtCl_4 + 2HCl + 2BaSO_4,$$

is employed. Following the method given in detail, the yield is almost theoretical—Heat of formation of calcium acetylide, by M. de Forcrand. The heat of formation of solid C_2Ca from diamond and solid Ca is −7·25 Cal., substituting amorphous carbon it is −0·65 Cal., for gaseous carbon it is +76·95 Cal.—Action of ortho-amidobenzoic acid on benzoquinone, by MM. J. Ville and Ch. Astre.—Alterations in saccharine matters during the germination of barley, by M. P. Petit. The conclusions are drawn: (1) There is a relation between the quantities of reducing sugars and of saccharose existing in barley during its germination. (2) The formation of saccharose commences even during the damping, whereas reducing sugars remain nearly constant during the same period. (3) The variation of reducing power represents the activity of respiration.—Chemical process for the purification of water, by MM. F. Bordas and Ch. Girard. Calcium permanganate is employed to oxidise the organic matters, and the excess of this salt is removed by treatment with lower oxides of manganese. The treatment recommended removes organic matter also by physically precipitating it from the water, which, after treatment, contains no micro-organisms, and very little calcium carbonate.—On the wheat produced on a saliferous soil in Algeria, by MM. Berthault and Crochetelle.—On the abnormal fronds of ferns, by M. Ernest Olivier.—Origin and division of granular nuclei in large sarcomatous cells, by MM. O Van der Stricht and P. Walton.—A note, by M. Delaurier, concerning an easy method of obtaining a perfect vacuum without mechanism, deals with the production of a vacuum by absorption, as with oxygen and iron at a red heat.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Birds, Beasts, and Fishes of the Norfolk Broadland: P. H. Emerson (Nutt).—Balistique des Nouvelles Poudres: E. Vallier (Paris, Gauthier-Villars).—La Théorie des Procédés Photographiques: A. de la Baume Pluvinel (Paris, Gauthier-Villars).—La Distillation: E. Sorel (Paris, Gauthier-Villars).—Dissections Illustrated: C. G. Brodie, Part 4 (Whittaker).—Methodisches Lehrbuch der Elementar Mathematik: Dr. G. Holzmüller, Dritter Teil (Leipzig, Teubner).—A Primer of Evolution: E. Clodd (Longmans).—Geometrical Conics: F. S. Macaulay (Cambridge University Press).—Standard Dictionary, Vol. 2 (Funk and Wagnalls).—Outlines of Zoology: J. A. Thomson, 2nd edition (Pentland).—The Book of the Dead. The Papyrus of Ani in the British Museum: Dr. E. A. W. Budge (British Museum).

PAMPHLETS.—Report of the Meteorological Council to the Royal Society for the Year ending March 31, 1894 (Eyre and Spottiswoode).—Stonyhurst College Observatory. Results of Meteorological and Magnetical Observations, 1894: Rev. W. Sidgreaves (Clitheroe).—18th Report of the State Entomologist on the Noxious and Beneficial Insects of the State of Illinois (Springfield, Ill.).

SERIALS.—Cassell's Magazine, April (Cassell).—Chambers's Journal, April (Chambers).—Century Magazine, April (Unwin).—Natural Science, April (Rait).—Zeitschrift für Wissenschaftliche Zoologie. lix. Band, 1 Heft (Leipzig, Engelmann).—Gazzetta Chimica Italiana, 1895, Fasc. 2 (Roma).—Humanitarian, April (Hutchinson).—Contemporary Review, April (Isbister).—National Review, April (Arnold).—Fortnightly Review, April (Chapman). Bulletin de la Académie Royale des Sciences, &c., de Belgique, tome 29, No. 2 (Bruxelles). Geographical Journal, April (Stanford).—Journal of the Royal Agricultural Society of England (third series), Vol. vi. Part 1 (Murray).

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