THURSDAY, AUGUST 31, 1911.

THE FOUNDATIONS OF MATHEMATICS.

Principia Mathematica. By Dr. A. N. Whitehead, F.R.S., and B. Russell, F.R.S. Vol. i. Pp. xv+666. (Cambridge: University Press, 1910.) Price 25*s.* net.

THIS work contains some thousands of propositions, each, with its proof, expressed in a shorthand so concise that if they were all expanded into ordinary language, the room taken up would be ten times as large at least; space, time, and mass are not considered at all, and arithmetic is merely foreshadowed by the introduction of the symbols 0, 1, 2, and 2_r. How then, it may be asked, can the authors pretend to be writing about mathematics? The answer amounts to saying that for every branch of the tree of knowledge there is a corresponding root, and every advance in climbing seems to compel a similar advance in delving. Just as the discovery of non-Euclidean geometries led to the reconsideration of geometrical axioms, so Cantor's invention of transfinite numbers has reacted upon the theory of elementary arithmetic, and hence upon the whole of analysis and all its applications.

Besides this, there has grown up a school of mathematicians intensely interested in the logical side of their subject. Indeed, this was inevitable as soon as the primary distinction between ordinal and cardinal number was fully grasped, and the nature of the arithmetical continuum had been strictly defined. The inquirer was driven back and back to questions of order, and correspondence, and relations, and classes, until he felt bound to construct a symbolical logic fit to express the chain of deductions he found latent in the most familiar processes of arithmetic. This has led to an immense aggregation of what may be called mathematical prolegomena; and with this the first volume of the "Principia Mathematica" is almost exclusively concerned.

Thus the actual titles of its two parts are "Mathematical Logic" and "Prolegomena to Cardinal Arithmetic," and both are so elaborate that only a meagre account of them can be given in a review. The theory of deduction is based upon seven assumptions, called primitive propositions, and upon the notions of disjunction (p or q) and implication (either not-p or q). In about thirty pages the authors obtain the main results of the purely formal logic of propositions. This is followed by a very interesting section on "apparent variables," including the theory of propositions of different orders. A real advance seems to have been made here in the analysis of vicious-circle fallacies, and false generalisations, especially as they occur in mathematical reasoning. It is pointed out that such phrases as "all propositions" or "all properties of x" are strictly meaningless, and a legitimate use of such terms is based upon an axiom of reducibility (pp. 173–5) which is stated in the form: "Any function of one argument or of two is formally equivalent to a predicative function of the same argument or arguments," and its main use is at the beginning of the calculus of classes (p. 197). Whether this axiom is really simpler than the introduction of "class" as a primitive term seems debatable, but it does not matter much for practical purposes.

The next three sections deal with classes and relations, and introduce a large number of new symbols and a long series of propositions. Fortunately here, as elsewhere, each section is preceded by a summary, giving the principal theorems; and, in fact, the reader will find it helpful to go through all these summaries (after the introduction) before attacking the chapters in detail.

Coming now to the more directly mathematical part, we have first of all (p. 356) a discussion of unit classes, which illustrates the subtleties of this new calculus. Thus it is found necessary to construct a symbol for "the class of which the only member is x," as distinguished from x itself. At first this seems to be superfluous, but when we suppose x to be a class, we see that it is not. The next step is to define the cardinal number 1 as the class of all unit classes. Similarly the cardinal number 2 is defined as the class of all couples (x, y) such that (x, y) and (y, x) are equivalent; and the ordinal number 2_r as the class of ordered couples (x, y) such that (y, x) is different from (x, y). Besides these we have a symbol $\dot{2}$ for the class of all relations consisting of a single couple, including couples (x, x). Then we have a series of theorems on subclasses, relative types of classes, one-one and one-many relations, &c., leading up to the fundamental notion of similarity of classes which is the necessary basis of all arithmetic proper.

We next come to the difficult question of selections, from relations and from classes of classes respectively.

"If k is a class of classes, then μ is called a selected class of k when μ is formed by choosing one term out of each member of k."

(It would perhaps be more precise to say "a class selected from k," because μ, as a class, is not generally a member of k.) Now at first sight it looks as if a selected class could always be formed, but this is not really obvious when k is infinite, and, in fact, it has not been proved in general. If it could be, it would follow that every class can be well-ordered, and the difficulty of asserting this in general can be seen from a special case. Consider the aggregate of colours, merely as sensations of my own; how can I order them, without importing some additional foreign element, such as the time when I first became conscious of a particular one, or its analysis by a colour-box, or something of that sort? Besides this, there is the logical difficulty of making an assertion about "every" class, for one reason because assertions form a class.[1] Hence the section (p. 561) on the conditions for the existence of selections is one of special interest: its most important bearing on arithmetic is in the theory of multiplication.

The final section is on inductive relations, especially

[1] This is undeniable, because "assertions do not form a class" is itself an assertion, and only a formal, not a real contradiction of the above statement.

the ancestral relation, which, to avoid a vicious circle, is defined so as to apply to members of an infinite class. As the authors explain in a note, this section is mainly based upon Frege's work, and is used afterwards to deduce the properties of finite cardinals and the transfinite cardinal $\aleph_0$. Here we find the Peanesque notation in all its development, and must make up our minds to learn it thoroughly, or else to express its formulæ in an equally exact, but less unfamiliar symbolism. This leads us to the few critical remarks that we venture to offer on this admirable and elaborate work. Every communication of ideas from one mind to another is made by means of a conventional symbolism; no symbolism can be more *exact* than language, because language is, in the last resort, required to explain and define it. But it may be more *concise* than language, and this is the real virtue of the Peano notation and its derivatives. To show how easy it is to exaggerate the value of the notation as such, we may take an example from p. 16 of the present work. The authors say that "it is an obvious error, though one easy to commit," that "No A is B" is the contradictory of "every A is B," and proceed to add that the symbolism exposes the fallacy at once. Really it does nothing of the kind; truly the symbol $\sim \{(x).\phi(x)\}$, the contradictory of $(x).\phi(x)$, is different *in form* from $(x). \sim \phi(x)$, but how can we tell from looking at them that these last two symbols are not equivalent? Again, the authors profess to give a proof of the law of excluded middle; they assume it in defining the assertion symbol, for they practically say "if the proposition to which this sign is prefixed is false the book is in error," tacitly assuming (here) that all their propositions are significant. The law of excluded middle is surely axiomatic for a significant proposition, the only trouble is in being quite sure that our assertions are really significant, and in this it is reason that must guide us, not symbolism, though a proper choice of symbolism may conduce to economy of thought. As an illustration, we may refine a little on the paradox of Epimenides. Suppose a Frenchman or a German asserts "Every statement that has ever been made by an Englishman is false." This is a significant statement, and *as such* must be true or false. But suppose an Englishman says the same thing: the proposition ceases to be significant, unless he adds "except one," when it again becomes significant. Questions of this kind are not so trivial as they appear, and a really philosophical study of language might do a good deal towards making more definite the metaphysical basis of knowledge. G. B. M.

MOVEMENT AND ESCAPEMENT.

Le Mouvement. Mesures de l'étendue et mesures du temps. By Prof. J. Andrade. Pp. vi+328. (Paris: Librairie Félix Alcan, 1911.) Price 6 francs.

SOME literary effusions—for instance, the novel with a purpose—present to the reviewer an awkward problem, namely, whether to concentrate his attention on the novel as such, or on the purpose. The present work might almost be included in some such category, inasmuch as it may be regarded from the point of view of a mathematician pure and simple, of a more or less practical mechanic, or even of an astronomer, while all the time it apparently claims to be a philosophical treatise, and as such to appeal to what may be called the general reader. In some parts of the book the philosopher is much in evidence, and in many places the absence of diagrams, and the assumption that the reader will understand determinants, vectors, or even ordinary equations of motion without explanation, would certainly repel the ordinary reader. The mathematician will find perhaps little that is novel. The suggestions of non-Euclidean space, whether that of Lobatchewsky or of Riemann, are little more than suggestions, and can only give those to whom such ideas are new the kind of shock the earlier cyclists felt on first riding a free-wheel. On the other hand, a very good historical sketch, amply provided with diagrams, is given of the development of scientific clock-making with due respect to the great English horologists.

A brief sketch of the contents of the book will serve to indicate the scope of the author's endeavour, and it is difficult to conceive how, within the limits of such a volume, a perfectly satisfactory result could have been achieved. Perhaps only a fellow-countryman of the great French philosophers of the past would ever have attempted such a task. The first part treats of geometrical ideas of number and space, the author showing a decided preference for vectorial or polar coordinates, and for rotation as a means of translation. The finite straight line is elaborately discussed, and ordinary geometrical propositions regarded from the point of view that came into vogue about a quarter of a century ago, when Nixon's Euclid began to oust Todhunter's in some schools. Triangles and solids, plane and spherical areas, volumes, velocity, vectors, the theorems of Ampère and Stokes, moments, composition of vectors and vectorial quantities, bring us through trigonometry and statics to non-Euclidean geometry by a somewhat tortuous route.

The second part introduces force, one chapter being devoted to the notions of astronomy and celestial mechanics from Hipparchus to Newton, and another to the principles of dynamics, equilibrium, and the two fundamentals, which, in the author's view, are clock and orientation; a third dwells on the vital importance of a function of forces, on stability and conservative systems, on isolated systems, Painlevé's theorem and Laplace's invariable plane; and these are followed by simple and damped oscillations, spiral movement, elastic bodies, and fluids, and the bending of springs. The third part deals with optics more especially of the telescope, with a more special devotion to different methods of geodesy from Picard to the use of invar and the Jäderin wire, and the correction of the units of the metric system.

The fourth and last part deals with the chronometer in general, and escapements in particular, calling for continued experimental work on indicated lines, touches on chronographs and synchronisation of clocks, and gives extracts from the annual trial-numbers from the Greenwich volumes, showing the improvements makers have been able to secure in the last sixty odd years. W. W. B.

COSMICAL PHYSICS.

Researches on the Evolution of the Stellar Systems. By Prof. T. J. J. See. Vol. ii., "The Capture Theory of Cosmical Evolution, founded on Dynamical Principles and Illustrated by Phenomena Observed in the Spiral Nebulæ, the Planetary System, the Double and Multiple Stars and Clusters, and the Star-clouds of the Milky Way." Pp. viii+734. (Lynn, Mass.: T. P. Nichols and Sons; London: W. Wesley and Son, 1910.)

IT is with mingled feelings that, after reading through this immense volume of Dr. See's, the reviewer attempts to present it fairly to the readers of this journal. This book and the theory presented therein is "the culmination of continued labor extending over more than a quarter of a century." It calls therefore for a full and careful discussion. It is a great pity that the writer over and over again by loose dogmatic statements repels the critic, and that he so frequently makes claims as to the rigorousness of the methods he employs, claims which a careful examination quite fails to endorse. As an example of the former fault we choose the extraordinary statement on p. 152, which comes at the end of an account of some quite inconclusive mathematical work on the effect of a resisting medium. The italics are the author's.

"*Whatever doubt may arise as to the effect of the resisting medium in the present state of the solar system, there can be no possible doubt as to its power in our system at the epoch when the planets were formed. The observed roundness of the orbits of the planets is an everlasting witness to the presence of a resisting medium against which these bodies revolved for immeasurable ages. There is no other admissible explanation of this phenomenon, and as the resisting medium is a* vera causa, *on the secular effects of which all mathematicians are agreed, we may hold that it has as surely rounded up these orbits as if we had witnessed the transformation within the short period of human history covered by exact observations.*"

For the author's claims to mathematical rigorousness of treatment reference may be made to pp. 237, 259. The answer to these claims is twofold. On the one hand rigorous proof is from the nature of the case impossible in cosmogony. Too many uncertainties are necessarily involved in the premises for any amount of exact mathematical reasoning to lead to a rigorous proof, and even in this mathematical reasoning Dr. See is by no means perfect. It is a pity that his reply to the mathematical point raised by Mr. Brodetsky in the *Astronomische Nachrichten* (No. 4408) should be limited to the suggestion that "Mr. Brodetsky is unfortunate in writing from Cambridge." If Dr. See's views do not meet with the full attention that he desires for them (or to do them justice, that they deserve), the fault lies partly with his method of presenting them.

After these strictures we may turn to the more pleasant task of dwelling on the theory that Dr. See has built up. In his view the solar system has developed from a spiral nebula. Condensations round various nuclei have gradually developed into planets. The resistance of the medium through which these bodies have revolved about the central condensation or sun, has led to the gradual falling in and rounding of their orbits, and also to the capture by the planets of all their satellites. Thus the moon, which originally revolved outside the present orbit of Neptune, was captured by the earth while in the process of falling into the sun through the slow decrease of its orbit. The theory is supported by arguments adduced from many branches of astronomical research. Some of the material which Dr. See has brought together to support his views is of very decided interest, notably chapter ix. on the capture of comets, and chapter xxi. with its welcome extracts from the papers of Herschel. Dr. See has been very generous in the extracts he quotes from the work of other people. It is not obvious, however, why chapter xii., with its long extracts on lunar motion, should figure in this work. There is no independent criticism made of the controversies referred to, and a bare statement of the results arrived at, with references, should have amply sufficed. In some of his criticisms of other contributions to cosmogony, Dr. See is much happier than in his own constructive work. Thus some of his criticisms of Messrs. Chamberlain and Moulton on p. 106 are distinctly to the point; while independently of other workers in the same field, he has brought forward some cogent reasons against the nebular hypothesis of Laplace in its ordinary form; he also criticises some conclusions frequently drawn (though not always correctly) from the papers of Sir George Darwin.

Many points of detail offer themselves for criticism in the treatment accorded by Dr. See to all the problems discussed in his book. But we have said enough to give the general scope of the work. While not able to accept Dr. See's views as to the important part played by the resisting medium in the evolution of our system, we are prepared to find in this resisting medium a *vera causa* the effect of which has been frequently overlooked by other workers. This book is an exaggeration. It may serve to restore the true balance of forces.

It remains to be added that the book has been very well prepared for publication. It is produced in a manner that does credit to its author and publishers alike. The photographic reproductions are very good. The moon, nebulæ, and star-clouds are well represented in a series of very fine plates. It is a pity that the headings used were added to the fine photographs by Barnard. A useful summary of the whole book is added in chapter xxiv., which gives a formidable list of problems explained by the theory.

BOTANICAL MONOGRAPHS.

Das Pflanzenreich. Regni vegetabilis conspectus. Edited by A. Engler.

41. Heft. (iv. 56*a*) Garryaceæ; (iv. 220*a*) Nyssaceæ; (iv. 220*b*) Alangiaceæ; (iv. 229) Cornaceæ. By W. Wangerin. Pp. 18+20+25+110. Price 9.20 marks.

42. Heft. (iv. 147) Euphorbiaceæ-Jatropheæ. By F. Pax. Pp. 148. Price 7.40 marks.

43. Heft. (iv. 228) Umbelliferæ–Apioideæ–Bupleurum, Trinia et reliquæ Ammineæ heteroclitæ. By H. Wolff. Pp. 214. Price 10.80 marks.

46. Heft. (iv. 94) Menispermaceæ. By L. Diels. Pp. 345. Price 17.40 marks.

(Leipzig: Wilhelm Engelmann, 1910.)

THE four volumes of which the titles appear above are sufficiently diverse to indicate various general features of this elaborate series. In the first place, sufficient material for the first volume on the Euphorbiaceæ is supplied by the account of the tribe Jatropheæ, while Mr. H. Wolff includes no more than a portion of the subtribe Ammineæ in the first volume dealing with the Umbelliferæ. The termination Jatropheæ follows the official rules that tribe names shall end in -eæ, and the suffix, -ineæ, indicates that Ammineæ is a subtribe. The Menispermaceæ are amenable to treatment in a single volume, while the inclusion of four families under one cover is due to the circumstance that certain genera, formerly included in Cornaceæ—notably by Dr. Harms in the "Pflanzenfamilien"—are now severed from that family and placed in three distinct families. Of these, Garryaceæ and Alangiaceæ are both monogeneric, while Nyssaceæ comprises Nyssa, Camptotheca, and Davidia. Garrya has always been a puzzle, and even now Dr. Wangerin expresses himself somewhat dubiously as to the proposed location in the Amentales near Salicaceæ. Alangiaceæ and Nyssaceæ are referred to a relationship with the Combretaceæ in the order Myrtifloræ, the only doubtful point being the exact position of the monotypic genus Davidia. With regard to the Cornaceæ, the author has no hesitation in placing them at the very bottom of the Umbellifloræ, immediately anterior to the Caprifoliaceæ. Dr. Wangerin pays special attention to the various forms of inflorescence in the Cornaceæ, and submits an analytical key to the genera founded upon anatomical characters. The family consists of ten genera, of which Cornus is the largest with fifty species.

The tribe Jatropheæ comprises thirteen genera, including the well-known Hevea and two small new genera; Jatropha far outnumbers the other genera with 156 species, as Hevea approaches next with 17 species. Prof. F. Pax has worked out a phylogenetic arrangement of genera and species based upon a detailed study of the geographical distribution. Seven genera are wholly American, five are palæotropic, and Jatropha is tropically cosmopolitan. The chief centre of development is situated in Brazil, but other centres occur in Central America and East Africa, chiefly owing to the species of Jatropha found in those regions. Diagrams are provided to illustrate natural affinities of the genera and of different sections and subsections of the genus Jatropha.

The elucidation of the genus Bupleurum, which now extends to 100 species, is the chief feature of the Ammineæ-heteroclitæ. It is interesting to note that in a family showing such diverse fruit characters the most satisfactory characters for splitting up the genus are furnished by the leaves. The criticism of unnecessary diffuseness must be urged against the author; the extreme instance is supplied by the description of subspecies and varieties under *Bupleurum falcatum*, extending over thirteen pages.

As might be expected from a botanist of such wide experience, Prof. L. Diels has produced one of the most interesting monographs. The various sections in the general introduction of the Menispermaceæ are carefully elaborated. The family contains a very large number of lianes, not one of which climbs by means of tendrils; twining is the usual device; the extent of anomalous stem development is still a matter for investigation. The value of Miers's contributions to the classification of the family is generously emphasised, although the author finds it necessary to make a number of alterations in the constitution of the tribes. Much useful information is supplied in the notes inserted after the diagnoses of the genera. One new genus is proposed, and several new species are indicated which are scattered generally through the genera.

OUR BOOK SHELF.

The Animals and Man: an Elementary Text-book of Zoology and Human Physiology. By Prof. V. L. Kellogg. Pp. x+495. (New York: Henry Holt and Co., 1911.)

ALTHOUGH written from an American point of view, with American animals as the chief types, this well-illustrated volume may be confidently recommended to the English student on account of its lucid style, orderly arrangement, and the method of treatment of the various sections of its subjects. Based on two still more elementary text-books of zoology by Prof. Kellogg, the volume includes chapters on human structure and physiology by Miss McCracken, a fellow professor of the author at Stanford University; this lady's contribution forming eight chapters in the fourth part. The other chapters on physiology are by Prof. Kellogg.

The first six chapters are devoted to the constituent parts of animals and their respective functions; the subject being introduced by contrasting the organisation of a grasshopper—or rather a locust—with that of a snail; while in subsequent chapters a so-called sunfish (in reality a member of the perch group), a sparrow, a toad, a crayfish, and an amœba are made to serve as types of their respective classes. With part ii., containing three chapters, we have summaries of the life-histories of certain kinds of animals; mosquitoes and caterpillars, with their transformations, serving to illustrate insects, while frogs and birds are taken as examples of two vertebrate groups broadly distinguished by the great divergence in their development.

Systematic zoology and the classification and habits of animals form the subject of part iii., with eleven chapters; the author commencing his survey with the protozoa and concluding with mammals. The account of the former group is well up to date, and particular attention may be directed to the excellent description of the mode in which man becomes infected with malaria by means of mosquito bites. It is the clearest and most simple account of a complex and puzzling phenomenon which we have had occasion to read. Neither are domesticated animals left out; and in this section it may be mentioned that the author follows Prof. Keller in regarding humped cattle as descended from the bantin of the Malay countries. On pp. 270–1 Hainan is misprinted Hainau, and *cygnoides* rendered *cygmoides*, while an altogether misleading figure of a lamb is made to serve as the representative of the handsome wild sheep of Transcaspia. A brief account of fossil animals, or rather fossil vertebrates, concludes this section of the work, which is followed by the aforesaid chapters from the pen of Miss McCracken.

Chapters on the relation of micro-organisms and sanitation, on ancient and modern man, the struggle for existence, communal life, &c., conclude a very readable book on a very technical subject. R. L.

How to Enamel: being a Treatise on the Practical Enamelling of Jewellery with Hard Enamels. By H. M. Chapin. Pp. xii+70. (New York: J. Wiley and Sons; London: Chapman and Hall, Ltd., 1911.) Price 4*s*. 6*d*. net.

THIS is an unpretentious little book written by a practising enameller. It describes in plain language the simplest methods of enamelling on metals, and has the merit of avoiding all air of mystery pertaining to the craft. The writing has no claims to literary finish, and the Americanisms scattered up and down the pages will come upon an English reader with something of a shock. But the writer's gift for expressing his meaning plainly, and his practical hints as to helps and hindrances in the work, obviously the result of direct personal experience, will earn the gratitude of the reader who goes to him for instruction.

In so small a compass, of course, no more than the elements of the subject are treated, and the beginner is very properly warned that only experience can teach him his craft. By omitting some not very helpful pages on transferring photographs to enamel, room might have been found for illustrating and commenting on a few fine examples of enameller's work of former ages; or, perhaps more stimulating to craftsmen, some specimens of the handiwork of such modern masters as Lalique, Thesmar, Du Suau de la Croix, Fisher, and Dawson, would have set before the beginner something to remind him that the result of all his efforts will be worth nothing unless quickened by the breath of art.

History of Geology. By H. B. Woodward, F.R.S. Pp. vi+154. (The History of Science Series.) (London: Watts and Co., 1911.) Price 1*s*. net.

No more appropriate writer could have been found for this condensed history of geology than the author of the recently published "History of the Geological Society of London." The personal touches which abounded in that volume have of necessity been curtailed in the treatment of a wider theme; but we meet here pleasantly with Mary Anning (p. 63) and Etheldred Benett (p. 126), side by side with Humboldt and James Hall. The book is clear and interesting in all its chapters. Stratigraphy naturally assumes most importance, since it includes the succession of organisms on the earth, and this is the aspect of geology that appeals most directly to the mind of man. Perhaps there are almost too few references to the difficulty experienced by the early geologists in making headway in countries where adherence to a Jewish system of cosmogony was held to be an act of public morals. Those who begin with Mr. Woodward's present book may well pass on, guided by his fourth chapter, to the opening pages of Lyell's "Principles of Geology."

Petrology is treated less systematically, and few will agree with the statement (p. 143) that "the petrology of the Igneous rocks has the advantage of being a more exact science than that of Palæontology."

The pertinacity of Romé de l'Isle and the self-sacrificing life of Haüy receive only slight mention on p. 43. We should have liked some reference to the successful stand made by English-speaking geologists against the view that igneous rocks assumed a new facies with the passing of Mesozoic forms of life, and of the part played by Jull in this matter—since other living workers are mentioned—and in the development of the teaching of geology. The heading "Early Geological Maps" (p. 50) does not include William Smith or Macculloch, the maps of the former being described on p. 34, while Macculloch's Scotland has to wait until p. 80. "Progress in British Geology" occurs twice as a heading in chapter iii. These are small points of arrangement and are easy to correct. The portraits of geologists have been selected from good and thoroughly interesting originals. We feel that we must mention specially the early Lyell, the William Buckland expounding the tooth of a hippopotamus, and the thoughtful von Buch resting so naturally in the open air. G. A. J. C.

A Treatise on Wireless Telegraphy and Wireless Telephony. By Prof. T. Mizuno. Pp. ix+563+x+208 Figs. Written in Chinese characters. (Tokyo: The Maruzen-Kabushiki-Kaisha, 1911.) Price 4.50 yen, or 9*s*.

SEVEN years ago Prof. Toshinojo Mizuno, of the Imperial University of Kyoto, published a popular work on wireless telegraphy and telephony. At that time it was difficult to transmit messages more than two hundred miles. The present volume is in the main a theoretical consideration of the same subject, and is intended for the use of students at the university. With the exception of the numerous formulæ and equations which suggest a treatise on higher mathematics, the fact that the text is in Chinese idiographs, places this work beyond the reach of European students. The references to Maxwell and Hertz in the early chapters indicate that the author has started on good foundations. Following these, references are made to the work of many investigators in England, Germany, Italy, and other European countries.

The description of instruments, which are illustrated diagrammatically, concludes with a reference to the telephonic relay of Mr. S. Brown, which shows that the writer is well up to date in regard to modern inventions. The author says but little about his own work, or the contributions to improvements in practical wireless telegraphy made in his own country, but these exist. The whole work may be compared to a play of Shakespeare with actors in Eastern costume, but it also suggests that Japan is abreast with the abstruse researches of the West in connection with which she has made advances.

Les Machines à écrire. By J. Rousset. Pp. 177. "Encyclopédie Scientifique des Aide-Mémoire." (Paris: Gauthier-Villars and Masson et Cie., n.d.) Price 2.50 francs.

IN this little book the author dissects the typewriter of commerce, and in a series of chapters shows how in different machines each function is performed. There are fifty-eight figures. The descriptions and figures are clear, and the book should fulfil its purpose. It is a little difficult, however, to see what this purpose is, for ingenious as the mechanism of typewriters may be, it is all visible, and anyone with any sense of mechanics can see it all for himself and understand it, and, moreover, in the larger towns at any rate, there is no difficulty in finding all the better-known examples, and willing expositors in the shops in which they are sold. Still, it is well that the subject should be dealt with systematically.

Lissajous'sche Stimmgabelkurven in stereoskopischer Darstellung. By J. W. N. Le Heux. Pp. 8+18 plates [loose cards in case]. (Leipzig: Johann Ambrosius Barth, 1911.) Price 6 marks.

THE author refers to the interest which Lissajous figures have in physics and mathematics, more especially when presented in their most attractive form so as to appear in stereoscopic relief. As is well known, pairs of figures otherwise identical but slightly different in phase appear when viewed in a stereoscope (or by accustomed eyes without a stereoscope) to blend together and form a single picture in three dimensions. The author discusses eighteen plates as follows, three of ratio 1 : 1, five of 1 : 2, two of 2 : 3, three of 3 : 4, two of 3 : 5, and two of 4 : 5 ratios. Some show a single line only, others give ten or more closely spaced lines. The plates are so clear that the stereoscopic effect is perfectly seen without a stereoscope.

Europe in Pictures. By H. Clive Barnard. Pp. 64. (London: A. and C. Black, 1911.) Price 1*s.* 6*d.*

THE pictures in this book will serve admirably to illustrate geography lessons in schools. The text is scarcely so suitable for school purposes; it is arranged unattractively and in such a manner that the plates often have little to do with the letterpress facing them.

LETTERS TO THE EDITOR.

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

A Pseudo-Aurora.

FOR some time I have been staying at the Kurhaus, St. Beatenberg, Switzerland, and my window commands a view of the Bernese Oberland from the Wetterhorn to the Balmhorn. The Eiger Mönch, Jungfrau, and Blümlisalp stand out most clearly above the lower mountains in front of them, of which the Faulhorn and Mesen are members. There has been a continuance of hot and dry weather for many weeks, and there have been occasional thunderstorms with both forked and sheet lightning. On the night of August 21, about ten o'clock, semicircular flashes of light shot up apparently behind the Mönch, quivered for a few seconds, and then disappeared. I counted twenty-eight in a minute. The light was sometimes intense at a central point, which was steady, and from this a quivering glow proceeded and lighted up from 15° to 20° of the horizon. The outline of the Jungfrau group could occasionally, but not always, be seen.

The appearance seemed to me very like an aurora borealis which I saw in Scotland in the 'fifties, but the centre of the light here was to the south-west of where I stood. I do not know how long the light had appeared before I saw it, but it continued to flash with great brilliancy for about twenty minutes. It then became less bright, and did not shoot so high into the sky, but extended laterally to the south for about 30° behind the Oberland chain. After half an hour more these died away, and on looking out two hours later nothing was to be seen. I am informed that a similar phenomenon was visible on the previous night, but was less brilliant. The resemblance to a true aurora was so great that I have thought it might be worth description. LAUDER BRUNTON.

Rainless Thunderstorms.

DURING the long-continued drought local storms have been reported here and there, and have been described as rainless. Will you or any of your readers explain this phenomenon? I have always imagined that raindrops played a large part in the manufacture of atmospheric electricity, but I suppose that there are electrical storms in rainless countries.

A. A. M.

Hove, August 16.

THE point raised in the foregoing letter is one of considerable interest in connection with the origin of the electrical phenomena of thunderstorms. The fact that thunderstorms are usually accompanied by clouds of a special character and heavy rain is common knowledge, and after Wilson's discovery of the difference in the effectiveness of the positive and negative ions as condensation nuclei it was generally assumed that condensation produced the necessary separation of the positive and negative electricity, and was an essential feature in thunderstorms. Simpson in his recent paper on the "Electricity of Rain and its Origin in Thunderstorms" makes splashing and breaking up of actual raindrops a necessary part of the mechanism of a thunderstorm.

Published accounts of rainless thunderstorms are not common, but one was contributed by Mr. E. J. Lowe to NATURE for September 7, 1893. He says, "On August 9 (at Shirenewton, near Chepstow) there was no rain but more lightning than I had seen since the memorable storm of August 9, 1843. It commenced at 9 p.m., and lasted five hours. From very frequent counting there could not have been less than 10,000 flashes."

More recently, Captain A. Simpson, of the s.s. *Moravian*, described a thunderstorm near Cape Verde lighthouse, when there was no rain nor even lower clouds. "For fully an hour the sky was one blaze of lightning, and the wire ropes, mast heads, yard arms, derrick ends, &c., were lighted up." See M.O. Pilot Chart of the North Atlantic and Mediterranean, April, 1903. E. G.

Meteorological Office, South Kensington, London, S.W., August 22.

Habits of Dogs.

CAN any of your readers inform me whether it is common for dogs to eat wasps, or if it is likely to prove injurious? A young bulldog of mine ("Billy") now finds his chief amusement in catching flying wasps with his mouth, and I think he must swallow them, as they generally vanish, though occasionally I have found the corpse on the floor. It seems evident from the dog's demeanour that the sting makes some impression; he shakes his head and licks his lips energetically, and occasionally runs to a corner and rolls on his back kicking. But the next moment he is off after another. That he is not invulnerable appears further from the fact that yesterday, after treading on a wasp, he lifted a paw and limped on three legs, until I applied ammonia. There was a tender spot where the skin had been grazed between the toes; possibly the sting lit there.

The same bulldog had another curious habit. When I was on the Cornish coast he spent much of his time in rolling boulders backwards along the beach or in shallow water. His method was to embrace the stone with his powerful fore-arms and fling it towards his hind paws, licking it well over at every pause. As he generally chose the biggest stone he could well move, it was laborious work; but he was tremendously enthusiastic about it. In the garden too, if not watched, he would drag the stones from the rockery across the lawn. This pursuit has now lapsed from lack of opportunity, though he occasionally practises on a stray brick or flower-pot.

It testifies to the hardiness of the national breed that "Billy" was undamaged by a "head-on" collision with a motor-car which he had charged. I saw him knocked forward, and then struck by the wheel, but at my cry of horror he came galloping back to me as cheerful as ever. The driver had doubtless put on the brake as soon as possible, for he kindly stopped a moment later to see if the dog had been hurt. A. EVERETT.

Woking, August 21.

THE KACHÁRIS OF ASSAM.[1]

THIS, the last volume included in the excellent series of monographs for which we are indebted to the Government of Eastern Bengal and Assam, differs from its predecessors in that it contains less pure ethnography and more of the charming personality of the author, of whom his old friend, Mr. J. D. Anderson, contributes an appreciative memoir. Mr. Sidney Endle worked as a missionary under the Society for the Propagation of the Gospel, and as chaplain to the tea-gardens of Upper Assam, from 1864 to 1907, when, exhausted by long and devoted labour among the people he loved so well, he died in a steamer on the Brahmaputra while on his way to Europe.

The Kacháris, to use the name given to them by the Hindus, are usually known to the Bengalis as Mech, from the Sanskrit Mleccha, meaning "barbarian," but call themselves Bodo or Baro. Their Hindu name seems to be connected with that of the powerful Koch empire, which once included, roughly speaking, the present British provinces of Eastern Bengal and Assam, the name now surviving in the small native State of Koch Behar.

The people now described form part of the northern group of a once widespread race, divided from the southern group by a line closely following the Brahmaputra valley. In the southern group the strongest tribes are the Garos and the people of Hill Tippera. The separation of the northern from the southern group is complete, as is shown by the absence of common tradition and intermarriage; and their languages, though possessing much in common, differ from each other nearly as much as Italian does from Spanish. How this once united people became divided history does not tell. But Mr. Endle, with much probability, suggests that it resulted from the invasion of the Ahoms, a Shan tribe from whom Assam has acquired its name, who early in the thirteenth century entered the province from the Upper Irawaddy valley.

The Bodo Kacháris, numbering about 272,000 souls, now occupy the Kachári Duárs or passes and the districts of West Darrang and North Kamrup. In stature they are much smaller and shorter than the races of North-west India, and bear some resemblance to the Nepalese. Their physical type—square-set faces, projecting cheek-bones, almond-shaped eyes, and the almost complete absence of beard and moustache—connects them with the Mongoloid peoples. Mentally they are much inferior to their Hindu neighbours, but what they succeed in learning they retain with much tenacity. They are intensely clannish and obstinate. Owing to their comparative isolation they have acquired few of the vices of civilisation, an occasional bout of indulgence in rice-beer being one of their most obvious failings. Their standard of female chastity is much higher than that of the neighbouring tribes.

They are a prosperous people, well skilled in agriculture, growing the valuable Eri silk, out of which they weave an excellent cloth. It is curious that Col. Gurdon, the director of the Ethnographical Survey, and the author are at issue on the question whether their subdivisions are endogamous or exogamous, a matter easily solved by local inquiry. In their manners and customs they much resemble the Garos, who are described in a monograph included in this series.

In religion they are in the animistic stage, with a pantheon containing groups of household and village deities. The leading members of the former group are Bathau, the tree spirit embodied in the *Euphorbia splendens*, found in nearly every house yard, and his consort Mainao, who is, as her name implies, the guardian goddess of the rice fields.

Two appendices, one describing some of the allied tribes, the other adding three additional folk tales collected by Mr. Anderson, increase the value of the book, which, if not the work of a trained anthropologist, gives a sympathetical account of an interesting people.

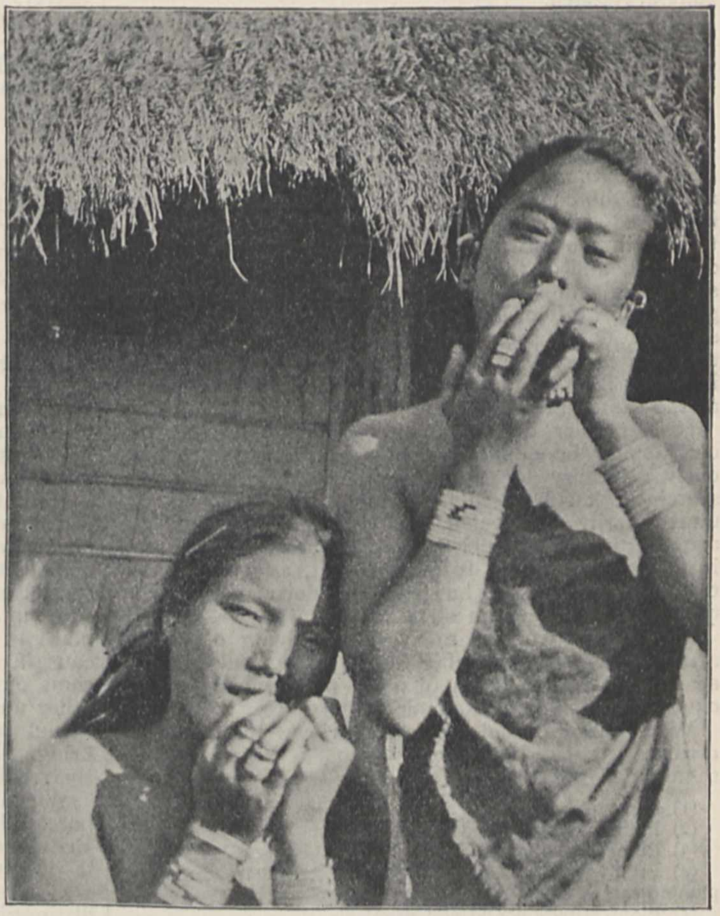

Photo.] [*Mrs. H. A. Colquhoun.*

Kachári Girls playing Jew's Harps (Gongina). From "The Kacháris."

[1] "The Kacháris." By the late Rev. Sidney Endle. With an Introduction by J. D. Anderson. Pp. xix+128. (London: Macmillan and Co., Ltd. 1911.) Price 8s. 6d. net.

THE PROMOTION OF AGRICULTURAL RESEARCH AND LOCAL INVESTIGATIONS.

THE Board of Agriculture and Fisheries has been in communication with the Development Commissioners with a view to the formulation of a scheme for the promotion of agricultural research and local investigations in England and Wales, and the Treasury, on the recommendaton of the commissioners, has now sanctioned the allocation of funds to be distributed by the Board in accordance with the general principles set out below. The total maximum sum which will be expended when the scheme is in full operation will be about 50,000*l*. per annum.

The scheme provides for—

(1) A system of agricultural research which will secure for each group of the problems affecting rural industry a share of attention roughly proportional to its economic importance.

(2) The concentration of the scientific work on each group at one institution or at institutions working in combination.

(3) Grants for special investigations for which provision may not otherwise be made.

(4) The grant of scholarships with a view to the increase of the number of men fully qualified to undertake agricultural research.

(5) The carrying out of investigations into problems of local importance, especially those involving the application of modern research to local practice, and the provision of scientific advice for farmers on important technical questions.

Subjects of Research.

In making arrangements for the separate investigation, so far as possible, of each group of allied subjects the commissioners and the Board have been impressed with the importance of securing continuity in work which is necessarily of considerable duration, and at the same time of providing staffs of specialists and experts who will be permanently engaged on work arising from the investigation of the same group of problems. By this means concentration and economy of effort will be better secured than it would be if a number of institutions were dealing at the same time with the same group of problems.

It is neither desirable nor possible to prevent all overlapping or duplication of work, but it is obviously necessary to proceed on a plan by which research work subsidised from public funds will not be unnecessarily duplicated. It is also desirable to arrange that each problem shall be undertaken by the institution best fitted to deal with it, and usually by the institution which has specially devoted its attention to problems of an allied nature.

It is also important to avoid the giving of undue attention to one part of the field of agricultural research, to the exclusion of other parts which are of equal scientific and economic importance.

With these considerations in view, it has been arranged that grants should be made for research in the following groups of subjects:—

(1) Plant physiology.
(2) Plant pathology and mycology.
(3) Plant breeding.
(4) Fruit growing, including the practical treatment of plant diseases.
(5) Plant nutrition and soil problems.
(6) Animal nutrition.
(7) Animal breeding.
(8) Animal pathology.
(9) Dairying.
(10) Agricultural zoology.
(11) Economics of agriculture.

Special Grants for Research.

A sum not exceeding 3000*l*. per annum will be available for assistance in respect of special investigations for which provision is not otherwise made.

Grants from this fund will be made on the recommendation of the Board's Advisory Committee on Agricultural Science, which will consider, not only whether the proposed investigation is desirable in itself, but whether it could not be better carried out at one of the special research institutions referred to above. The grants will be made from year to year, and will be for one year only in each case.

Scholarships.

In order to secure the services of a number of carefully trained men for work in connection with the scheme, the Board proposes in each of the years 1911, 1912, and 1913 to offer twelve scholarships of the value of 150*l*. per annum, tenable for three years.

It is proposed that candidates for scholarships should be selected by a special committee, representing the institutions in which the selected candidates will subsequently work. The award of twelve scholarships will be conditional on a sufficient number of thoroughly suitable candidates presenting themselves.

Local Advice and Investigations.

Grants will also be made to certain universities, university colleges, and agricultural colleges in England and Wales, for the purpose of enabling them to supply scientific advice to farmers on important technical questions, and to carry out investigations into problems of local interest, which can be more conveniently studied on the spot than at one of the research institutions.

By means of these grants it is hoped to provide an expert staff possessing both scientific and practical qualifications, the members of which will devote themselves to solving difficult local problems, and in other ways endeavour to secure the application of science to practice.

THE RECOGNITION OF PALÆOBOTANY.

IN *The Times* of August 24 a correspondent appeals for the adequate official recognition of palæobotany in Britain, and suggests that "some millionaire, anxious to be of service to his day and generation" might "do a unique and serviceable deed in endowing this neglected but important science." It is indeed strange, though true, that there is no professorship or lectureship in palæobotany in any of our universities, Cambridge alone having an ill-paid demonstratorship; and hitherto there has been no special curatorship of fossil plants even in the British Museum. This country only takes an honourable place in the promotion of the science at present because a few distinguished men of private means, and some enthusiastic students working in the midst of other duties, are devoted to it; and also because the actual occupants of the chairs of botany in Cambridge, London, and Manchester happen to make it their chief line of research.

When, however, *The Times* correspondent compares the general recognition of palæobotany in Britain and the British possessions with that which it receives in other countries, his statement is weakened by a tendency to special pleading. The distinguished professor of the Swedish State Museum, who is mentioned as "decorated with Royal Orders," is not merely a palæobotanist, but also a great geographer, the hero of several important Arctic expeditions. The United States Geological Survey may be well equipped for the study of fossil plants; but it should be added that no more important and fundamental contribution to palæobotany has emanated from America during recent years than that of the assistant curator of a university museum who pursues his researches in the intervals of many other duties. The Canadian Geological Survey may not have a palæobotanist on its permanent staff; but it does not fail to recognise the importance of fossil plants when necessity arises, and it is employing a professional palæobotanist on special service at the present time.

Finally, the statement that "our country, with all her colonies and dependencies, with their thousands of square miles of coal-bearing, fossil plant-bearing rocks, does nothing" to help the prospector, is made in forgetfulness of the exhaustive and well-illustrated "Catalogue of the Glossopteris Flora" which was published by the Trustees of the British Museum six years ago, partly with the view of helping the development of the coal-fields in India and the southern hemisphere, where the handbook is extensively used.

S. H. BURBURY, F.R.S.

BY the death of Mr. Samuel Hawksley Burbury, on August 18, at eighty years of age, we have lost an ardent worker in the domain of mathematical physics who did much to elucidate the mysteries of several problems in molecular dynamics. Mr. Burbury was the son of Mr. Samuel Burbury, of Leamington, and was born at Kenilworth in May, 1831. He was educated at Shrewsbury and St. John's College, Cambridge; he was Craven University scholar, Chancellor's medallist, Browne medallist, twice Porson prizeman, and fifteenth Wrangler and second in classical tripos, 1854. He was called to the Bar at Lincoln's Inn in 1858, but his new profession did not prevent him from continuing his mathematical studies, and he thus became one of the few workers in this country who have produced original mathematical investigations while engaged in duties other than that of a mathematical teacher.

Much of Mr. Burbury's work was done in collaboration with the late Rev. H. W. Watson, F.R.S., with whom he shared the joint authorship of treatises on "The Application of Generalised Coordinates to the Dynamics of a Material System" (1879) and "The Mathematical Theory of Electricity" (1883–5), in the latter of which the authors endeavoured to place electrostatics and electromagnetism on a more formal basis than had been done by Clerk Maxwell in his original treatise. It is perhaps a pity that this book appeared at a time when experimental developments were beginning to break down many of our preconceived electrical theories, and, so far as we can gather, Watson and Burbury's treatise is not studied so much as it deserves. It affords a fairly satisfactory representation of electrical phenomena as known at the time, but, of course, every mathematical or dynamical theory of physical phenomena can only be regarded as a scheme for coordinating results of experiments in the simplest form; and with the discovery of new facts every such scheme is liable to be found inadequate when the necessity arises of superseding it by a more comprehensive scheme. It is thus probable that at the present time there is no scheme which represents our existing knowledge of electrical phenomena much better than Watson and Burbury's represented our knowledge at the time it was written.

But, like his friend Watson, Burbury seems to have chosen as his favourite study that branch of molecular dynamics in which Boltzmann occupied a central position. Burbury and Boltzmann were certainly in constant correspondence with each other, and many of Boltzmann's papers were evidently the result of Burbury's criticisms. Perhaps Burbury's training as a barrister gave him special qualifications for playing the *rôle* of critic; at any rate, if there was a weak point in any argument Burbury would certainly find it out in a very short time. A great amount of time was spent in examining Boltzmann's "Minimum Theorem," according to which an assemblage of molecules representing on the kinetic theory a perfect gas, tends to assume the distribution commonly known as "Maxwell's Law." In the proof of this theorem the question of reversibility plays an important part, and it cannot be said that the introduction of probability considerations altogether overcame the difficulty of accounting for an irreversible phenomenon by means of a system the elements of which were subject to the equations of motion of reversible dynamics.

In his "Kinetic Theory of Gases" (1899), Burbury advanced a novel theory as to the distribution of velocities in a medium the molecules of which were too close together to satisfy the fundamental hypotheses involved in the proof of Maxwell's law. According to Burbury, the velocities of neighbouring molecules would become *correlated,* the probability factor involving, besides the usual exponential of the energy, the exponential of the vector product of the velocities of pairs of molecules. Burbury further offered, on this hypothesis, a tentative explanation of liquefaction. According to Burbury, Maxwell's law would thus become inapplicable to a dense gas. On the other hand, if applied to rare gases, it leads to the conclusion that helium and hydrogen cannot escape from the earth's atmosphere, a conclusion which the late Dr. Stoney stated was not in agreement with his observations. Thus the kinetic theory of gases affords another instance of a scheme which covered our knowledge of physical phenomena at one time but no longer does so. To overcome this difficulty we are now resuscitating kinetic theories, but employing them on a smaller scale than before—to electrons instead of molecules.

Another question which occupied Burbury, especially during recent years, was the loss of availability which occurs when two gases mix by diffusion of constant temperature. As Burbury argued, if this happens when different kinds of molecules mix by diffusion, the same thing should be true when molecules of the same kind mix by diffusion. Burbury's views on this subject were stated in *Science Progress* a few years ago. This problem again gave Burbury scope for his critical mind. It cannot be said, however, that he, or, indeed, anyone else, has succeeded in giving a reason *why* the total entropy of a litre of one gas and a litre of a second gas is equal to the entropy of the mixture when its volume is *one* litre, while if two litres of the *same* gas at the same pressure are allowed to mix, the sum of their entropies is equal to the entropy of the mixture when its volume is *two* litres. What Burbury really showed by his arguments was that the truth or otherwise of these statements can only be tested by experimental evidence.

At the present time the study of mathematical physics is rather out of fashion in this country, and it is not unusual to deprecate this study on the ground that it frequently fails to account for the results of observation. Is not this failure one of its most valuable features? Whenever a new physical phenomena is discovered, plenty of people are ready enough to invoke molecules, the æther or electrons, to account for it, and to talk about the motions of these which give rise to the observed phenomenon. The mathematical physicist comes along and says, "Very well; then let us write down the equations of motion and see if the reasoning works out correctly." He obtains a result which does not account for experimental conclusions. Which is wrong? Not the mathematician who has merely attempted to place the reasoning on an exact basis, but the *unmathematical physicist* who has endowed his ether molecules, his ether, or his electrons with properties that are incompatible with each other. In Mr. Burbury we have had a worker who never flinched from the task of following a line of argument up to its logical conclusions, however much these might run counter to orthodox views. He would drive his opponent from one stronghold to another, the controversy being conducted in the most friendly way the whole time. The contest would frequently end in a truce, both sides agreeing that there was much in Nature that we could never understand. But Burbury was rarely the first to give in. It is these passages of arms which very often enable men to appreciate each other's good qualities, and to realise the useful part which men like Burbury may play in evolving order out of chaos. G. H. BRYAN.

PROF. A. LADENBURG.

THE death occurred at Breslau, on August 15, of Dr. Albert Ladenburg, professor of chemistry in the University of Breslau. Dr. Ladenburg was born at Mannheim in 1842, and graduated as doctor of philosophy in 1863. In 1873 he accepted an invitation to take up a position as professor of chemistry and director of the laboratory at Kiel. In 1886 the honorary degree of doctor of medicine of Berne University was conferred on Dr. Ladenburg in recognition of his scientific investigations, and British and other societies, including the Pharmaceutical Society of Great Britain, also honoured him with honorary membership. He was also awarded the Hanbury gold medal for his services in the promotion of research on the chemistry of drugs. It was in 1889 that Dr. Ladenburg took up the post of professor of chemistry at Breslau, and he occupied the office with very great success.

Ladenburg's name is best known by his synthetic work on the production of homatropine. On splitting up atropine, tropic acid and tropine can be formed as derivatives; the latter Ladenburg combined with amygdalic acid to form a compound which is converted into oxy-toluyl-tropeine, or homatropine, an artificial alkaloid which, with its salts, has proved of the greatest service in ophthalmic surgery. His mathematical method of treating synthetic formulæ, and his prismatic benzene ring, place him in the first rank of chemists as a theorist; while as to his practical work, his list of communications to scientific societies and literature in this country and elsewhere includes articles on "The Valency of Nitrogen," on "Synthetic Alkaloids," on "The Relationship between Hyoscyamine and Atropine and the Conversion of the one Alkaloid into the other," on "Hyoscine," on "The Mydriatic Alkaloids occurring in Nature," on "The Synthesis of Coniine," and on "The History and Constitution of Atropine," in addition to the compilation with other collaborators of a dictionary ("Handwörterbuch der Chemie"), consisting of thirteen volumes dealing with inorganic and organic chemistry.

THE BRITISH ASSOCIATION AT PORTSMOUTH.

BY the time this issue reaches the readers of NATURE the eighty-first meeting of the British Association will have been inaugurated at Portsmouth, and, given fair weather conditions, we trust it will be a useful and enjoyable gathering. Judging from the number of distinguished men of science who have expressed their intention of being present, the meeting should be of importance as regards its scientific work, as well as successful from a social point of view.

The reception-room is the large Connaught Drill Hall, which appears to be ideal for that purpose. It gives under one roof a large reception hall with post office, telephone, &c., and a comfortably furnished reading and writing room for the members. In addition to this there is also a small room set apart for the use of ladies.

In point of view of numbers, the Portsmouth meeting may not reach that of Sheffield last year, but this is accounted for partly by the absence of any special industry attached to the town, and also may, to some extent, be due to the absence of any university or university college. Most of the accommodation available is, however, booked, and those who arrive late may have difficulty in finding quarters.

The meeting rooms are a little scattered, but this was unavoidable, and notices will be displayed making the routes to be taken to the various section-rooms easy to find.

In passing, mention may be made of a convenient plan for communication between members of the association. It is a box which will be placed in the reception-room, into which notes may be dropped addressed to other members. This box will be frequently cleared, and the notes delivered on request to those to whom they are written.

The pleasures of the meeting commence to-day (Thursday), when at 2.30 a party will be taken over the dockyard and battleships. A garden-party is to be given this afternoon by Sir John and Lady Brickwood at their beautiful residence in the town. In the evening the Mayor will give a reception at the South Parade Pier, which is the property of the Corporation.

On Friday afternoon there will be a special visit to the new filtration works of the Borough of Portsmouth Water Company, and Saturday will be entirely devoted to all-day excursions, including two to the Isle of Wight, and three drives in the South Downs, starting from Chichester, to which city there will be a special train. The drives are to (1) Kingly Vale, West Dean, and Goodwood; (2) Boxgrove Priory and Arundel Castle; (3) Bignor (with the Roman remains) and Parham Park.

On Sunday the Bishop of Winchester is to preach at the Portsea parish church, and on Tuesday the Mayor will entertain the members at a garden-party. In addition, the naval authorities have organised a naval display in Stokes' Bay, consisting of an attack by torpedo-boat destroyers and submarines. Visitors should not neglect a visit to the old *Victory*, one of the most interesting "links with the past" in existence, and a full description of which, written by Mr. W. L. Wyllie, R.A., will be found in an interesting little handbook to Portsmouth which will be presented to members.

INAUGURAL ADDRESS BY PROF. SIR WILLIAM RAMSAY, K.C.B., PH.D., LL.D., D.SC., M.D., F.R.S., PRESIDENT.

IT is now eighty years since this Association first met at York, under the presidency of Earl Fitzwilliam. The object of the Association was then explicitly stated:—"To give a stronger impulse and a more systematic direction to scientific inquiry, to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers, to obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which impede its progress."

In 1831 the workers in the domain of science were relatively few. The Royal Society, which was founded by Dr. Willis, Dr. Wilkins, and others, under the name of the "Invisible, or Philosophical College," about the year 1645, and which was incorporated in December, 1660, with the approval of King Charles II., was almost the only meeting-place for those interested in the progress of science; and its Philosophical Transactions, begun in March, 1664-5, almost the only medium of publication. Its character was described in the following words of a contemporary poem:—

> "This noble learned Corporation
> Not for themselves are thus combined
> To prove all things by demonstration,
> But for the public good of the nation,
> And general benefit of mankind."

The first to hive off from the Royal Society was the Linnean Society for the promotion of botanical studies, founded in 1788 by Sir James Edward Smith, Sir Joseph Banks, and other Fellows of the Royal Society; in 1807 it was followed by the Geological Society; at a later date the Society of Antiquaries, the Chemical, the Zoological, the Physical, the Mathematical, and many other Societies were founded. And it was felt by those capable of forming a judgment that, as well expressed by Lord Playfair at Aberdeen in 1885, "Human progress is so identified with scientific thought, both in its conception and realisation, that it seems as if they were alternative terms in the history of civilisation." This is only an echo through the ages of an utterance of the great Englishman, Roger Bacon, who wrote in 1250 A.D.: "Experimental science has three great prerogatives over all other sciences: it verifies conclusions by direct experiment; it discovers truths which they could never reach; and it investigates the secrets of Nature, and opens to us a knowledge of the past and of the future."

The world has greatly changed since 1831; the spread of railways and the equipment of numerous lines of steamships have contributed to the peopling of countries at that time practically uninhabited. Moreover, not merely has travelling been made almost infinitely easier, but communication by post has been enormously expedited and cheapened; and the telegraph, the telephone, and wireless telegraphy have simplified as well as complicated human existence. Furthermore, the art of engineering has made such strides that the question "Can it be done?" hardly arises, but rather "Will it pay to do it?" In a word, the human race has been familiarised with the applications of science; and men are ready to believe almost anything if brought forward in its name.

Education, too, in the rudiments of science has been introduced into almost all schools; young children are taught the elements of physics and chemistry. The institution of a Section for Education in our Association (L) has had for its object the organising of such instruction, and much useful advice has been proffered. The problem is, indeed, largely an educational one; it is being solved abroad in various ways—in Germany and in most European States by elaborate Governmental schemes dealing with elementary and advanced instruction, literary, scientific, and technical; and in the United States and in Canada by the far-sightedness of the people: both employers and employees recognise the value of training and of originality, and on both sides sacrifices are made to ensure efficiency.

In England we have made technical education a local, not an Imperial, question; instead of half a dozen first-rate institutions of University rank, we have a hundred, in which the institutions are necessarily understaffed, in which the staffs are mostly overworked and underpaid; and the training given is that, not for captains of industry, but for workmen and foremen. "Efficient captains cannot be replaced by a large number of fairly good corporals." Moreover, to induce scholars to enter these institutions, they are bribed by scholarships, a form of pauperism practically unknown in every country but our own; and to crown the edifice, we test results by examinations of a kind not adapted to gauge originality and character (if, indeed, these can ever be tested by examination), instead of, as on the Continent and in America, trusting the teachers to form an honest estimate of the capacity and ability of each student, and awarding honours accordingly.

The remedy lies in our own hands. Let me suggest that we exact from all gainers of University scholarships an undertaking that, if and when circumstances permit, they will repay the sum which they have received as a scholarship, bursary, or fellowship. It would then be possible for an insurance company to advance a sum representing the capital value, viz. 7,464,931*l*., of the scholarships, reserving, say, twenty per cent. for non-payment, the result of mishap or death. In this way a sum of over six million pounds, of which the interest is now expended on scholarships, would be available for University purposes. This is about one-fourth of the sum of twenty-four millions stated by Sir Norman Lockyer at the Southport meeting as necessary to place our University education on a satisfactory basis. A large part of the income of this sum should be spent in increasing the emoluments of the chairs; for, unless the income of a professor is made in some degree commensurate with the earnings of a professional man who has succeeded in his profession, it is idle to suppose that the best brains will be attracted to the teaching profession. And it follows that unless the teachers occupy the first rank, the pupils will not be stimulated as they ought to be.

Again, having made the profession of a teacher so lucrative as to tempt the best intellects in the country to enter it, it is clear that such men are alone capable of testing their pupils. The modern system of "external examinations," known only in this country, and answerable for much of its lethargy, would disappear; schools of thought would arise in all subjects, and the intellectual as well as the industrial prosperity of our nation would be assured. As things are, can we wonder that as a nation we are not scientific? Let me recommend those of my hearers who are interested in the matter to read a recent report on Technical Education by the Science Guild.

I venture to think that, in spite of the remarkable progress of science and of its applications, there never was a time when missionary effort was more needed. Although most people have some knowledge of the results of scientific inquiry, few, very few, have entered into its spirit. We all live in hope that the world will grow better as the years roll on. Are we taking steps to secure the improvement of the race? I plead for recognition of the fact that progress in science does not only consist in accumulating information which may be put to practical use, but in developing a spirit of prevision, in taking thought for the morrow; in attempting to forecast the future, not by vague surmise, but by orderly marshalling of facts, and by deducing from them their logical outcome; and chiefly in endeavouring to control conditions which may be utilised for the lasting good of our people. We must cultivate a belief in the "application of trained intelligence to all forms of national activity."

The Council of the Association has had under consideration the formation of a Section of Agriculture. For some years this important branch of applied science, borrowing as it does from botany, from physics, from chemistry, and from economics, has in turn enjoyed the hospitality of each of these sections, itself having been made a sub-section of one of these more definite sciences. It is proposed this year to form an Agricultural Section. Here there is need of missionary effort; for our visits to our colonies have convinced many of us that much more is being done for the farmer in the newer parts of the British Empire than at home. Agriculture is, indeed, applied botany, chemistry, entomology, and economics, and has as much right to independent treatment as has engineering, which may be strictly regarded as applied physics.

The question has often been debated whether the present method of conducting our proceedings is the one best adapted to gain our ends. We exist professedly "to give a stronger impulse and a more systematic direction to scientific inquiry." The Council has had under consideration various plans framed with the object of facilitating our work, and the result of its deliberations will be brought under your attention at a later date. To my mind, the greatest benefit bestowed on science by our meetings is the opportunity which they offer for friendly and unrestrained intercourse, not merely between those following different branches of science, but also with persons who, though not following science professionally, are interested in its problems. Our meetings also afford an opportunity for younger men to make the acquaintance of older men. I am afraid that we who are no longer in the spring of our lifetime, perhaps from modesty, perhaps through carelessness, often do not sufficiently realise how stimulating to a young worker a little sympathy can be; a few words of encouragement go a long way. I have in my mind words which encouraged me as a young man, words spoken by the leaders of Associations now long past—by Playfair, by Williamson, by Frankland, by Kelvin, by Stokes, by Francis Galton, by Fitzgerald, and many others. Let me suggest to my older scientific colleagues that they should not let such pleasant opportunities slip.

Since our last meeting the Association has to mourn the loss by death of many distinguished members. Among these are:—

Dr. John Beddoe, who served on the Council from 1870 to 1875, has recently died at a ripe old age, after having achieved a world-wide reputation by his magnificent work in the domain of anthropology.

Sir Rubert Boyce, called away at a comparatively early age in the middle of his work, was for long a colleague of mine at University College, and was one of the staff of the Royal Commission on Sewage Disposal. The service he rendered science in combating tropical diseases is well known.

Sir Francis Galton died at the beginning of the year at the advanced age of eighty-nine. His influence on science has been characterised by Prof. Karl Pearson in his having maintained the idea that exact quantitative methods could—nay, must—be applied to many branches of science which had been held to be beyond the field of either mathematical or physical treatment. Sir Francis was General Secretary of this Association from 1863 to 1868; he was President of Section E in 1862, and again in 1872; he was President of Section H in 1885; but, although often asked to accept the office of President of the Association, his consent could never be obtained. Galton's name will always be associated with that of his friend and relative, Charles Darwin, as one of the most eminent and influential of English men of science.

Prof. Thomas Rupert Jones, also, like Galton, a member of this Association since 1860, and in 1891 President of the Geological Section, died in April last at the advanced age of ninety-one. Like Dr. Beddoe, he was a medical man with wide scientific interests. He became a distinguished geologist, and for many years edited the Quarterly Journal of the Geological Society.

Prof. Story Maskelyne, at one time a diligent frequenter of our meetings, and a member of the Council from 1874 to 1880, was a celebrated mineralogist and crystallographer. He died at the age of eighty-eight. The work which he did in the University of Oxford and at the British Museum is well known. In his later life he entered Parliament.

Dr. Johnstone Stoney, President of Section A in 1897, died on July 1, in his eighty-sixth year. He was one of the originators of the modern view of the nature of electricity, having given the name "electron" to its unit as far back as 1874. His investigations dealt with spectroscopy and allied subjects, and his philosophic mind led him to publish a scheme of ontology which, I venture to think, must be acknowledged to be the most important work which has ever been done on that difficult subject.

Among our corresponding members we have lost Prof. Bohr, of Copenhagen; Prof. Brühl, of Heidelberg; Hofrat Dr. Caro, of Berlin; Prof. Fittig, of Strassburg; and Prof. van 't Hoff, of Berlin. I cannot omit to mention that veteran of science Prof. Cannizzaro, of Rome, whose work in the middle of last century placed chemical science on the firm basis which it now occupies.

I knew all these men, some of them intimately; and, if I have not ventured on remarks as to their personal qualities, it is because it may be said of all of them that they fought a good fight and maintained the faith that only by patient and unceasing scientific work is human progress to be hoped for.

It has been the usual custom of my predecessors in office either to give a summary of the progress of science within the past year or to attempt to present in intelligible language some aspect of the science in which they have themselves been engaged. I possess no qualifications for the former course, and I therefore ask you to bear with me while I devote some minutes to the consideration of ancient and modern views regarding the chemical elements. To many in my audience part of my story will prove an oft-told tale; but I must ask those to excuse me, in order that it may be in some wise complete.

In the days of the early Greeks the word "element" was applied rather to denote a property of matter than one of its constituents. Thus, when a substance was said to contain fire, air, water, and earth (of which terms a childish game doubtless once played by all of us is a relic), it probably meant that they partook of the nature of the so-called elements. Inflammability showed the presence of concealed fire; the escape of "airs" when some substances are heated or when vegetable or animal matter is distilled no doubt led to the idea that these airs were imprisoned in the matters from which they escaped; hardness and permanence were ascribed to the presence of earth, while liquidity and fusibility were properties conveyed by the presence of concealed water. At a later date the "Spagyrics" added three "hypostatical principles" to the quadrilateral; these were "salt," "sulphur," and "mercury." The first conveyed solubility, and fixedness in fire; the second, inflammability; and the third, the power which some substances manifest of producing a liquid, generally termed "phlegm," on application of heat, or of themselves being converted into the liquid state by fusion.

It was Robert Boyle, in his "Skeptical Chymist," who first controverted these ancient and mediæval notions, and who gave to the word "element" the meaning that it now possesses—the constituent of a compound. But in the middle of the seventeenth century chemistry had not advanced far enough to make his definition useful, for he was unable to suggest any particular substance as elementary. And, indeed, the main tenet of the doctrine of "phlogiston," promulgated by Stahl in the eighteenth century, and widely accepted, was that all bodies capable of burning or of being converted into a "calx," or earthy powder, did so in virtue of the escape of a subtle fluid from their pores; this fluid could be restored to the "calces" by heating them with other substances rich in phlogiston, such as charcoal, oil, flour, and the like. Stahl, however false his theory, had at least the merit of having constructed a reversible chemical equation:—

$$\text{Metal} - \text{phlogiston} = \text{Calx}\,;\quad \text{Calx} + \text{phlogiston} = \text{Metal}.$$

It is difficult to say when the first element was known to be an element. After Lavoisier's overthrow of the phlogistic hypothesis, the part played by oxygen, then recently discovered by Priestley and Scheele, came prominently forward. Loss of phlogiston was identified with oxidation; gain of phlogiston with loss of oxygen. The scheme of nomenclature ("Méthode de Nomenclature chimique"), published by Lavoisier in conjunction with Guyton de Morveau, Berthollet, and Fourcroy, created a system of chemistry out of a wilderness of isolated facts and descriptions. Shortly after, in 1789, Lavoisier published his "Traité de Chimie," and in the preface the words occur: "If we mean by 'elements' the simple and indivisible molecules of which bodies consist, it is probable that we do not know them; if, on the other hand, we mean the last term in analysis, then every substance which we have not been able to decompose is for us an element; not that we can be certain that bodies which we regard as simple are not themselves composed of two or even a larger number of elements, but because these elements can never be separated, or rather, because we have no means of separating them, they act, so far as we can judge, as elements; and we cannot call them 'simple' until experiment and observation shall have furnished a proof that they are so."

The close connection between "crocus of Mars" and metallic iron, the former named by Lavoisier "oxyde de fer," and similar relations between metals and their oxides, made it likely that bodies which reacted as oxides in dissolving in acids and forming salts must also possess a metallic substratum. In October, 1807, Sir Humphry Davy proved the correctness of this view for soda and potash by his famous experiment of splitting these bodies by a powerful electric current into oxygen and hydrogen, on the one hand, and the metals sodium and potassium on the other. Calcium, barium, strontium, and magnesium were added to the list as constituents of the oxides, lime, barytes, strontia, and magnesia. Some years later Scheele's "dephlogisticated marine acid," obtained by heating pyrolusite with "spirit of salt," was identified by Davy as in all likelihood elementary. His words are: "All the conclusions which I have ventured to make respecting the undecompounded nature of oxymuriatic gas are, I conceive, entirely confirmed by these new facts." "It has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties, its colour, and to call it chlorine." The subsequent discovery of iodine by Courtois in 1812, and of bromine by Balard in 1826, led to the inevitable conclusion that fluorine, if isolated, should resemble the other halogens in properties, and much later, in the able hands of Moissan, this was shown to be true.

The modern conception of the elements was much strengthened by Dalton's revival of the Greek hypothesis of the atomic constitution of matter, and the assigning to each atom a definite weight. This momentous step for the progress of chemistry was taken in 1803; the first account of the theory was given to the public, with Dalton's consent, in the third edition of Thomas Thomson's "System of Chemistry" in 1807; it was subsequently elaborated in the first volume of Dalton's own "System of Chemical Philosophy," published in 1808. The notion that compounds consisted of aggregations of atoms of elements united in definite or multiple proportions, familiarised the world with the conception of elements as the bricks of which the Universe is built. Yet the more daring spirits of that day were not without hope that the elements themselves might prove decomposable. Davy, indeed, went so far as to write in 1811: "It is the duty of the chemist to be bold in pursuit; he must recollect how contrary knowledge is to what appears to be experience. . . . To inquire whether the elements be capable of being composed and decomposed is a grand object of true philosophy." And Faraday, his great pupil and successor, at a later date, 1815, was not behind Davy in his aspirations when he wrote: "To decompose the metals, to re-form them, and to realise the once absurd notion of transformation—these are the problems now given to the chemist for solution."

Indeed, the ancient idea of the unitary nature of matter was in those days held to be highly probable. For attempts were soon made to demonstrate that the atomic weights were themselves multiples of that of one of the elements. At first the suggestion was that oxygen was the common basis; and later, when this supposition turned out to be untenable, the claims of hydrogen were brought forward by Prout. The hypothesis was revived in 1842, when Liebig and Redtenbacher, and subsequently Dumas, carried out a revision of the atomic weights of some of the commoner elements, and showed that Berzelius was in error in attributing to carbon the atomic weight 12·25 instead of 12·00. Of recent years a great advance in the accuracy of the determinations of atomic weights has been made, chiefly owing to the work of Richards and his pupils, of Gray, and of Guye and his collaborators, and every year an international committee publishes a table in which the most probable numbers are given on the basis of the atomic weight of oxygen being taken as sixteen. In the table for 1911, of eighty-one elements, no fewer than forty-three have recorded atomic weights within one-tenth of a unit above or below an integral number. My mathematical colleague, Karl Pearson, assures me that the probability against such a condition being fortuitous is 20,000 millions to one.

The relation between the elements has, however, been approached from another point of view. After preliminary suggestions by Döbereiner, Dumas, and others, John Newlands in 1862 and the following years arranged the elements in the numerical order of their atomic weights, and published in *The Chemical News* of 1863 what he termed his law of octaves—that every eighth element, like the octave of a musical note, is in some measure a repetition of its forerunner. Thus, just as C on the third space is the octave of C below the line, so potassium, in 1863 the eighth known element numerically above sodium, repeats the characters of sodium, not only in its physical properties—colour, softness, ductility, malleability, &c.—but also in the properties of its compounds, which, indeed, resemble each other very closely. The same fundamental notion was reproduced at a later date, and independently, by Lothar Meyer and Dmitri Mendeléeff; and to accentuate the recurrence of such similar elements in *periods*, the expression "the periodic system of arranging the elements" was applied to Newlands' arrangement in octaves. As everyone knows, by help of this arrangement Mendeléeff predicted the existence of then unknown elements, under the names of eka-boron, eka-aluminium, and eka-silicon, since named *scandium*, *gallium*, and *germanium*, by their discoverers, Cleve, Lecoq de Boisbaudran, and Winckler.

It might have been supposed that our knowledge of the elements was practically complete; that perhaps a few more might be discovered to fill the outstanding gaps in the periodic table. True, a puzzle existed, and still exists, in the classification of the "rare earths," oxides of metals occurring in certain minerals; these metals have atomic weights between 139 and 180, and their properties preclude their arrangement in the columns of the periodic table. Besides these, the discovery of the inert gases of the atmosphere, of the existence of which Johnstone Stoney's spiral curve, published in 1888, pointed a forecast, joined the elements like sodium and potassium, strongly electronegative, to those like fluorine and chlorine, highly electropositive, by a series of bodies electrically as well as chemically inert, and neon, argon, krypton, and xenon formed links between fluorine and sodium, chlorine and potassium, bromine and rubidium, and iodine and cæsium.

Including the inactive gases, and adding the more recently discovered elements of the rare earths, and radium, of which I shall have more to say presently, there are eighty-four definite elements, all of which find places in the periodic table if merely numerical values be considered. Between lanthanum, with atomic weight 139, and tantalum, 181, there are in the periodic table seventeen spaces; and although it is impossible to admit, on account of their properties, that the elements of the rare earths can be distributed in successive columns (for they all resemble lanthanum in properties), yet there are now fourteen such elements; and it is not improbable that other three will be separated from the complex mixture of their oxides by further work. Assuming that the metals of the rare earths fill these seventeen spaces, how many still remain to be filled? We will take for granted that the atomic weight of uranium, 238·5, which is the highest known, forms an upper limit not likely to be surpassed. It is easy to count the gaps; there are eleven.

But we are confronted by an *embarras de richesse*. The discovery of radio-activity by Henri Becquerel, of radium by the Curies, and the theory of the disintegration of the radio-active elements, which we owe to Rutherford and Soddy, have indicated the existence of no fewer than twenty-six elements hitherto unknown. To what places in the periodic table can they be assigned?

But what proof have we that these substances are elementary? Let us take them in order.

Beginning with radium, its salts were first studied by Madame Curie; they closely resemble those of barium—sulphate, carbonate, and chromate insoluble; chloride and bromide similar in crystalline form to chloride and bromide of barium; metal, recently prepared by Madame Curie, white, attacked by water, and evidently of the type of barium. The atomic weight, too, falls into its place; as determined by Madame Curie and by Thorpe, it is 89·5 units higher than that of barium; in short, there can be no doubt that radium fits the periodic table, with an atomic weight of about 226·5. It is an undoubted element.

But it is a very curious one. For it is *unstable*. Now, stability was believed to be the essential characteristic of an element. Radium, however, disintegrates—that is, changes into other bodies, and at a constant rate. If 1 gram of radium is kept for 1760 years, only half a gram will be left at the end of that time; half of it will have given other products. What are they? We can answer that question. Rutherford and Soddy found that it gives a condensable gas, which they named "radium emanation"; and Soddy and I, in 1903, discovered that, in addition, it evolves helium, one of the inactive series of gases, like argon. Helium is an undoubted element, with a well-defined spectrum; it belongs to a well-defined series. And radium emanation, which was shown by Rutherford and Soddy to be incapable of chemical union, has been liquefied and solidified in the laboratory of University College, London; its spectrum has been measured, and its density determined. From the density the atomic weight can be calculated, and it corresponds with that of a congener of argon, the whole series being: helium, 4; neon, 20; argon, 40; krypton, 83; xenon, 130; unknown, about 178; and niton (the name proposed for the emanation to recall its connection with its congeners and its phosphorescent properties), about 222·4. The formation of niton from radium would therefore be represented by the equation: radium (226·4) = helium (4) + niton (222·4).

Niton, in its turn, disintegrates, or decomposes, and at a rate much more rapid than the rate of radium; half of it has changed in about four days. Its investigation, therefore, had to be carried out very rapidly, in order that its decomposition might not be appreciable while its properties were being determined. Its product of change was named by Rutherford "radium A," and it is undoubtedly deposited from niton as a metal, with simultaneous evolution of helium; the equation would therefore be:

niton (222·4) = helium (4) + radium A (218·4).

But it is impossible to investigate radium A chemically, for in three minutes it has half changed into another solid substance, radium B, again giving off helium. This change would be represented by the equation:

radium A (218·4) = helium (4) + radium B (214·4).

Radium B, again, can hardly be examined chemically, for in twenty-seven minutes it has half changed into radium C^1. In this case, however, no helium is evolved; only atoms of negative electricity, to which the name "electrons" has been given by Dr. Stoney, and these have minute weight which, although approximately ascertainable, at present has defied direct measurement. Radium C^1 has a half-life of 19·5 minutes, too short, again, for chemical investigation; but it changes into radium C^2, and in doing so each atom parts with a helium atom, hence the equation:

radium C^1 (214·4) = helium (4) + radium C^2 (210·4).

In 2·5 minutes radium C^2 is half gone, parting with electrons, forming radium D. Radium D gives the chemist a chance, for its half-life is no less than sixteen and a half years. Without parting with anything detectable, radium D passes into radium E, of which the half-life period is five days; and, lastly, radium E changes spontaneously into radium F, the substance to which Madame Curie gave the name "polonium," in allusion to her native country, Poland. Polonium, in its turn, is half changed in 140 days, with loss of an atom of helium, into an unknown metal, supposed to be possibly lead. If that be the case, the equation would run:

polonium (210·4) = helium (4) + lead (206·4).

But the atomic weight of lead is 207·1, and not 206·4; however, it is possible that the atomic weight of radium is 227·1, and not 226·4.

We have another method of approaching the same subject. It is practically certain that the progenitor of radium is uranium, and that the transformation of uranium into radium involves the loss of three α particles, that is, of three atoms of helium. The atomic weight of helium may be taken as one of the most certain; it is 3·994, as determined by Mr. Watson in my laboratories. Three atoms would therefore weigh 11·98, practically 12. There is, however, still some uncertainty in the atomic weight of uranium; Richards and Merigold make it 239·4, but the general mean, calculated by Clarke, is 239·0. Subtracting 12 from these numbers, we have the values 227·0, and 227·4 for the atomic weight of radium. It is as yet impossible to draw any certain conclusion.

The importance of the work, which will enable a definite and sure conclusion to be drawn, is this: For the first time, we have accurate knowledge as to the descent of some of the elements. Supposing the atomic weight of uranium to be certainly 239, it may be taken as proved that, in losing three atoms of helium, radium is produced, and, if the change consists solely in the loss of the three atoms of helium, the atomic weight of radium must necessarily be 227. But it is known that β rays, or electrons, are also parted with during this change; and electrons have weight. How many electrons are lost is unknown; therefore, although the weight of an electron is approximately known, it is impossible to say how much to allow for in estimating the atomic weight of radium. But it is possible to solve this question indirectly by determining exactly the atomic weights of radium and of uranium; the difference between the atomic weight of radium *plus* 12, *i.e.* plus the weight of three atoms of helium, and that of uranium, will give the weight of the number of electrons which escape. Taking the most probable numbers available, viz. 239·4 for uranium and 226·8 for radium, and adding 12 to the latter, the weight of the escaping electrons would be 0·6.

The correct solution of this problem would in great measure clear up the mystery of the irregularities in the periodic table, and would account for the deviations from Prout's Law, that the atomic weights are multiples of some common factor or factors. I also venture to suggest that it would throw light on allotropy, which in some cases, at least, may very well be due to the loss or gain of electrons, accompanied by a positive or negative heat-change. Incidentally, this suggestion would afford places in the periodic table for the somewhat overwhelming number of pseudo-elements the existence of which is made practically certain by the disintegration hypothesis. Of the twenty-six elements derived from uranium, thorium, and actinium, ten, which are formed by the emission of electrons alone, may be regarded as allotropes or pseudo-elements; this leaves sixteen, for which sixteen or seventeen gaps would appear to be available in the periodic table, provided the reasonable supposition be made that a second change in the length of the periods has taken place. It is, above all things, certain that it would be a fatal mistake to regard the existence of such elements as irreconcilable with the periodic arrangement, which has rendered to systematic chemistry such signal service in the past.

Attention has repeatedly been drawn to the enormous quantity of energy stored up in radium and its descendants. That, in its emanation, niton is such that if what it parts with as heat during its disintegration were available, it would be equal to three and a half million times the energy available by the explosion of an equal volume of detonating gas—a mixture of one volume of oxygen with two volumes of hydrogen. The major part of this energy comes, apparently, from the expulsion of particles (that is, of atoms of helium) with enormous velocity. It is easy to convey an idea of this magnitude in a form more realisable by giving it a somewhat mechanical turn. Suppose that the energy in a ton of radium could be utilised in thirty years, instead of being evolved at its invariable slow rate of 1760 years for half-disintegration, it would suffice to propel a ship of 15,000 tons, with engines of 15,000 horse-power, at the rate of 15 knots an hour for thirty years—practically the lifetime of the ship. To do this actually requires a million and a half tons of coal.

It is easily seen that the virtue of the energy of the radium consists in the small weight in which it is contained; in other words, the radium-energy is in an enormously concentrated form. I have attempted to apply the energy contained in niton to various purposes; it decomposes water, ammonia, hydrogen chloride, and carbon dioxide each into its constituents; further experiments on its action on salts of copper appeared to show that the metal copper was converted partially into lithium, a metal of the sodium column; and similar experiments, of which there is not time to speak, indicate that thorium, zirconium, titanium, and silicon are degraded into carbon; for solutions of compounds of these, mixed with niton, invariably generated carbon dioxide, while cerium, silver, mercury, and some other metals gave none. One can imagine the very atoms themselves, exposed to bombardment by enormously quickly moving helium atoms, failing to withstand the impacts. Indeed, the argument *à priori* is a strong one; if we know for certain that radium and its descendants decompose spontaneously, evolving energy, why should not other more stable elements decompose when subjected to enormous strains?

This leads to the speculation whether, if elements are capable of disintegration, the world may not have at its disposal a hitherto unsuspected source of energy. If radium were to evolve its stored-up energy at the same rate that gun-cotton does, we should have an undreamt-of explosive; could we control the rate we should have a useful and potent source of energy, provided always that a sufficient supply of radium were forthcoming. But the supply is certainly a very limited one; and it can be safely affirmed that the production will never surpass half an ounce a year. If, however, the elements which we have been used to consider as permanent are capable of changing with evolution of energy, if some form of catalyser could be discovered which would usefully increase their almost inconceivably slow rate of change, then it is not too much to say that the whole future of our race would be altered.

The whole progress of the human race has indeed been due to individual members discovering means of concentrating energy and of transforming one form into another. The carnivorous animals strike with their paws and crush with their teeth; the first man who aided his arm with a stick in striking a blow discovered how to concentrate his small supply of kinetic energy; the first man who used a spear found that its sharp point in motion represented a still more concentrated form; the arrow was a further advance, for the spear was then propelled by mechanical means; the bolt of the crossbow, the bullet shot forth by compressed hot gas, first derived from black powder, later from high explosives, all these represent progress. To take another sequence: the preparation of oxygen by Priestley applied energy to oxide of mercury in the form of heat; Davy improved on this when he concentrated electrical energy into the tip of a thin wire by aid of a powerful battery, and isolated potassium and sodium.

Great progress has been made during the past century in effecting the conversion of one form of energy into others with as little useless expenditure as possible. Let me illustrate by examples: A good steam engine converts about one-eighth of the potential energy of the fuel into useful work; seven-eighths are lost as unused heat and useless friction. A good gas engine utilises more than one-third of the total energy in the gaseous fuel; two-thirds are uneconomically expended. This is a universal proposition; in order to effect the conversion from one form of energy into another, some energy must be expended uneconomically. If A is the total energy which it is required to convert, if B is the energy into which it is desired to convert A, then a certain amount of energy, C, must be expended to effect the conversion. In short, $A=B+C$. It is eminently desirable to keep C, the useless expenditure, as small as possible; it can never equal zero, but it can be made small. The ratio of C to B (the economic coefficient) should therefore be as large as is attainable.

The middle of the nineteenth century will always be noted as the beginning of the golden age of science, the epoch when great generalisations were made, of the highest importance on all sides, philosophical, economic, and scientific. Carnot, Clausius, Helmholtz, Julius Robert Mayer abroad, and the Thomsons, Lord Kelvin and his brother James, Rankine, Tait, Joule, Clerk Maxwell, and many others at home, laid the foundations on which the splendid structure has been erected. That the latent energy of fuel can be converted into energy of motion by means of the steam engine is what we owe to Newcomen and Watt; that the kinetic energy of the fly-wheel can be transformed into electrical energy was due to Faraday, and to him, too, we are indebted for the re-conversion of electrical energy into mechanical work; and it is this power of work which gives us leisure, and which enables a small country like ours to support the population which inhabits it.

I suppose that it will be generally granted that the Commonwealth of Athens attained a high-water mark in literature and thought which has never yet been surpassed. The reason is not difficult to find; a large proportion of its people had ample leisure, due to ample means; they had time to think and time to discuss what they thought. How was this achieved? The answer is simple: each Greek Freeman had, on an average, at least five helots who did his bidding, who worked his mines, looked after his farm, and, in short, saved him from manual labour. Now we in Britain are much better off; the population of the British Isles is in round numbers 45 millions; there are consumed in our factories at least 50 million tons of coal annually, and "it is generally agreed that the consumption of coal per indicated horse-power per hour is, on an average, about 5 lb." (Royal Commission on Coal Supplies, Part I.). This gives seven million horse-power per year. How many man-power are equal to a horse-power? I have arrived at an estimate thus: A Bhutanese can carry 230 lb. *plus* his own weight, in all 400 lb., up a hill 4000 feet high in eight hours; this is equivalent to about one twenty-fifth of a horse-power; seven million horse-power are therefore about 175 million man-power. Taking a family as consisting, on the average, of five persons, our 45 millions would represent nine million families, and dividing the total man-power by the number of families, we must conclude that each British family has, on the average, nearly twenty "helots" doing his bidding, instead of the five of the Athenian family. We do not appear, however, to have gained more leisure thereby; but it is this that makes it possible for the British Isles to support the population which it does.

We have in this world of ours only a limited supply of stored-up energy, in the British Isles a very limited one—namely, our coalfields. The rate at which this supply is being exhausted has been increasing very steadily for the last forty years, as anyone can prove by mapping the data given on p. 27, table D, of the General Report of the Royal Commission on Coal Supplies (1906). In 1870 110 million tons were mined in Great Britain, and ever since the amount has increased by three and a third million tons a year. The available quantity of coal in the proved coalfields is very nearly 100,000 million tons; it is easy to calculate that if the rate of working increases as it is doing, our coal will be completely exhausted in 175 years. But, it will be replied, the rate of increase will slow down. Why? It has shown no sign whatever of slackening during the last forty years. Later, of course, it must slow down, when coal grows dearer owing to approaching exhaustion. It may also be said that 175 years is a long time; why, I myself have seen a man whose father fought in the '45 on the Pretender's side, nearly 170 years ago! In the life of a nation 175 years is a span.

This consumption is still proceeding at an accelerated rate. Between 1905 and 1907 the amount of coal raised in the United Kingdom increased from 236 to 268 million tons, equal to six tons per head of the population, against three and a half tons in Belgium, two and a half tons in Germany, and one ton in France. Our commercial supremacy and our power of competing with other European nations are obviously governed, so far as we can see, by the relative price of coal; and when our prices rise, owing to the approaching exhaustion of our supplies, we may look forward to the near approach of famine and misery.

Having been struck some years ago with the optimism of my non-scientific friends as regards our future, I suggested that a committee of the British Science Guild should be formed to investigate our available sources of energy. This Guild is an organisation, founded by Sir Norman Lockyer after his tenure of the Presidency of this Association, for the purpose of endeavouring to impress on our people and their Government the necessity of viewing problems affecting the race and the State from the standpoint of science; and the definition of science in this, as in other connections, is simply the acquisition of knowledge, and orderly reasoning on experience already gained and on experiments capable of being carried out, so as to forecast and control the course of events, and, if possible, to apply this knowledge to the benefit of the human race.

The Science Guild has enlisted the services of a number of men, each eminent in his own department, and each has now reported on the particular source of energy of which he has special knowledge.

Besides considering the uses of coal and its products, and how they may be more economically employed, in which branches the Hon. Sir Charles Parsons, Mr. Dugald Clerk, Sir Boverton Redwood, Dr. Beilby, Dr. Hele-Shaw, Prof. Vivian Lewes and others have furnished reports, the following sources of energy have been brought under review: the possibility of utilising the tides; the internal heat of the earth; the winds; solar heat; water-power; the extension of forests, and the use of wood and peat as fuels; and, lastly, the possibility of controlling the undoubted, but almost infinitely slow, disintegration of the elements, with the view of utilising their stored-up energy.

However interesting a detailed discussion of these possible sources of energy might be, time prevents my dwelling on them. Suffice it to say that the Hon. R. J. Strutt has shown that in this country, at least, it would be impracticable to attempt to utilise terrestrial heat from bore-holes; others have deduced that from the tides, the winds, and water-power small supplies of energy are no doubt obtainable, but that, in comparison with that derived from the combustion of coal, they are negligible; nothing is to be hoped for from the direct utilisation of solar heat in this temperate and uncertain climate, and it would be folly to consider seriously a possible supply of energy in a conceivable acceleration of the liberation of energy by atomic change. It looks utterly improbable, too, that we shall ever be able to utilise the energy due to the revolution of the earth on her axis, or to her proper motion round the sun.

Attention should undoubtedly be paid to forestry and to the utilisation of our stores of peat. On the Continent, the forests are largely the property of the State; it is unreasonable, especially in these latter days of uncertain tenure of property, to expect any private owner of land to invest money in schemes which would at best only benefit his descendants, but which, under our present trend of legislation, do not promise even that remote return. Our neighbours and rivals, Germany and France, spend annually 2,200,000*l.* on the conservation and utilisation of their forests; the net return is 6,000,000*l.* There is no doubt that we could imitate them with advantage. Moreover, an increase in our forests would bring with it an increase in our water-power, for without forest land rain rapidly reaches the sea, instead of distributing itself so as to keep the supply of water regular, and so more easily utilised.

Various schemes have been proposed for utilising our deposits of peat: I believe that in Germany the peat industry is moderately profitable; but our humid climate does not lend itself to natural evaporation of most of the large amount of water contained in peat, without which processes of distillation prove barely remunerative.

We must therefore rely chiefly on our coal reserve for our supply of energy, and for the means of supporting our population; and it is to the more economical use of coal that we must look in order that our life as a nation may be prolonged. We can economise in many ways: By the substitution of turbine engines for reciprocating engines, thereby reducing the coal required per horse-power from 4 to 5 lb. to $1\frac{1}{2}$ or 2 lb.; by the further replacement of turbines by gas engines, raising the economy to 30 per cent. of the total energy available in the coal, that is, lowering the coal consumption per horse-power to 1 or $1\frac{1}{4}$ lb.; by creating the power at the pit-mouth, and distributing it electrically, as is already done in the Tyne district. Economy can also be effected in replacing "beehive" coke ovens by recovery ovens; this is rapidly being done; and Dr. Beilby calculates that in 1909 nearly six million tons of coal, out of a total of sixteen to eighteen millions, were coked in recovery ovens, thus effecting a saving of two to three million tons of fuel annually. Progress is also being made in substituting gas for coal or coke in metallurgical, chemical, and other works. But it must be remembered that for economic use gaseous fuel must not be charged with the heavy costs of piping and distribution.

The domestic fire problem is also one which claims our instant attention. It is best grappled with from the point of view of smoke. Although the actual loss of thermal energy in the form of smoke is small—at most less than a half per cent. of the fuel consumed—still the presence of smoke is a sign of waste of fuel and careless stoking. In works, mechanical stokers, which ensure regularity of firing and complete combustion of fuel, are more and more widely replacing hand-firing. But we are still utterly wasteful in our consumption of fuel in domestic fires. There is probably no single remedy applicable; but the introduction of central heating, of gas fires, and of grates which permit of better utilisation of fuel will all play a part in economising our coal. It is open to argument whether it might not be wise to hasten the time when smoke is no more by imposing a sixpenny fine for each offence; an instantaneous photograph could easily prove the offence to have been committed, and the imposition of the fine might be delayed until three warnings had been given by the police.

Now I think that what I wish to convey will be best expressed by an allegory. A man of mature years, who has surmounted the troubles of childhood and adolescence without much disturbance to his physical and mental state, gradually becomes aware that he is suffering from loss of blood; his system is being drained of this essential to life and strength. What does he do? If he is sensible he calls in a doctor, or perhaps several, in consultation; they ascertain the seat of the disease, and diagnose the cause. They point out that while consumption of blood is necessary for healthy life, it will lead to a premature end if the constantly increasing drain is not stopped. They suggest certain precautionary measures; and if he adopts them he has a good chance of living at least as long as his contemporaries; if he neglects them his days are numbered.

That is our condition as a nation. We have had our consultation in 1903; the doctors were the members of the Coal Commission. They showed the gravity of our case, but we have turned a deaf ear.

It is true that the self-interest of coal consumers is slowly leading them to adopt more economical means of turning coal into energy. But I have noticed, and frequently publicly announced, a fact which cannot but strike even the most unobservant. It is this: When trade is good, as it appears to be at present, manufacturers are making money; they are overwhelmed with orders, and have no inclination to adopt economies which do not appear to them to be essential, and the introduction of which would take thought and time, and which would withdraw the attention of their employees from the chief object of the business—how to make the most of the present opportunities. Hence improvements are postponed. When bad times come, then there is no money to spend on improvements; they are again postponed until better times arrive.

What can be done?

I would answer: Do as other nations have done and are doing; take stock annually. The Americans have a permanent Commission initiated by Mr. Roosevelt, consisting of three representatives from each State, the sole object of which is to keep abreast with the diminution of the stores of natural energy, and to take steps to lessen its rate. This is a non-political undertaking, and one worthy of being initiated by the ruler of a great country. If the example is followed here the question will become a national one.

Two courses are open to us: first, the *laissez-faire* plan of leaving to self-interested competition the combating of waste; or second, initiating legislation which, in the interest of the whole nation, will endeavour to lessen the squandering of our national resources. This legislation may be of two kinds; penal, that is, imposing a penalty on wasteful expenditure of energy supplies; and helpful, that is, imparting information as to what can be done, advancing loans at an easy rate of interest to enable reforms to be carried out, and insisting on the greater prosperity which would result from the use of more efficient appliances.

This is not the place, nor is there the time, to enter into detail; the subject is a complicated one, and it will demand the combined efforts of experts and legislators for a generation; but if it be not considered with the definite intention of immediate action, we shall be held up to the deserved execration of our not very remote descendants.

The two great principles which I have alluded to in an earlier part of this address must not, however, be lost sight of; they should guide all our efforts to use energy economically. Concentration of energy in the form of electric current at high potential makes it possible to convey it for long distances through thin, and therefore comparatively inexpensive, wires; and the economic coefficient of the conversion of mechanical into electrical, and of electrical into mechanical, energy is a high one; the useless expenditure does not much exceed one-twentieth part of the energy which can be utilised. These considerations would point to the conversion at the pit-mouth of the energy of the fuel into electrical energy, using as an intermediary turbines, or preferably gas engines, and distributing the electrical energy to where it is wanted. The use of gas engines may, if desired, be accompanied by the production of half-distilled coal, a fuel which burns nearly without smoke, and one which is suitable for domestic fires, if it is found too difficult to displace them and to induce our population to adopt the more efficient and economical systems of domestic heating which are used in America and on the Continent. The increasing use of gas for factory, metallurgical, and chemical purposes points to the gradual concentration of works near the coal mines in order that the laying-down of expensive piping may be avoided.

An invention which would enable us to convert the energy of coal directly into electrical energy would revolutionise our ideas and methods, yet it is not unthinkable. The nearest practical approach to this is the Mond gas-battery, which, however, has not succeeded, owing to the imperfection of the machine.

In conclusion, I would put in a plea for the study of pure science, without regard to its applications. The discovery of radium and similar radio-active substances has widened the bounds of thought. While themselves, in all probability, incapable of industrial application, save in the domain of medicine, their study has shown us to what enormous advances in the concentration of energy it is permissible to look forward, with the hope of applying the knowledge thereby gained to the betterment of the whole human race. As charity begins at home, however, and as I am speaking to the *British* Association for the Advancement of Science, I would urge that our first duty is to strive for all which makes for the permanence of the British Commonweal, and which will enable us to transmit to our posterity a heritage not unworthy to be added to that which we have received from those who have gone before.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. H. H. TURNER, D.Sc., D.C.L., F.R.S., PRESIDENT OF THE SECTION.

The Characteristics of the Observational Sciences.

It will doubtless startle my audience to hear that this Section has only once in its history been addressed by an astronomical President upon an astronomical topic. I hasten to admit that I am not using the term astronomical in its widest sense. Huxley once declared that there were only two sciences, Astronomy and Biology, and it is recorded that "the company" (which happened to be that of the Royal Astronomical Society Club) "agreed with him." One may agree with the company in assenting to the proposition in the sense in which it is obviously intended without losing the right to use the name astronomy in a more restricted sense when necessary; and at present I use it in its classical sense. At Brighton, in 1872, Dr. De La Rue addressed Section A on Astronomical Photography in words which are still worthy of attention, though they are all but forty years old; and this is the only instance I can find in the annals of the Section. There have, of course, been occasional astronomical Presidents such as Airy, Lord Rosse, and Dr. Robinson, but these presided in early days before the Address existed, or when it was brief and formal; and the only allusions to astronomical matters were the statements, by Robinson and Airy, of what the Association had done in subsidising the reduction of Lalande's observations and the Greenwich lunar observations. In 1887 Sir Robert Ball occupied this chair, but he selected from his ample scientific wardrobe the costume of a geometer, and left his astronomical dress at home. A great man whose death was announced almost as I was writing these words, Dr. Johnstone Stoney, spoke (in 1879 at Sheffield) of the valuable training afforded by the study of mechanics and of chemistry, with that keen insight which made him so valuable a member of our Section. Other Presidents whom we have been glad to welcome as astronomers at certain times and seasons did not choose the occasion of their presidency for any very definite manifestation of astronomical sympathy.

The Addresses of Sir George Darwin (in 1886) and of Prof. Love (in 1907) on the past history of our earth certainly have an astronomical bearing, but if we distinguish between the classical astronomy and its modern expansions they would be assigned to the latter rather than to the former; and so do the few astronomical allusions in Prof. Schuster's Address at Edinburgh in 1892. Even if we include, instead of excluding, all doubtful cases, there will still appear a curious neglect of astronomy by Section A in the last half-century, all the more curious when it is remarked that the neglect does not extend to the Association itself, seeing that there have been three Astronomical Presidents of the Association who had not been previously chosen to fill this chair. The neglect is not confined to astronomy, but extends, as some of us recently pointed out, to the other sciences of observation; and we thought that, as a corollary, it would be better for the Section to divide, in order that these sciences might not continue the struggle for existence in an atmosphere to which they were apparently ill-suited. But the Section decided against the suggestion, and I have no intention of appealing against the decision. This explicit statement will, I trust, suffice to prevent misunderstanding if I proceed to examine the possible causes of neglect—for I cannot but regard the record as significant of some cause which it will be well to recognise even if we cannot remove it. Personally I think the cause is not far to seek, and my hope is to make it manifest; but as the statement of it involves something in the nature of an accusation, I will beg leave to make it as gently as possible by using the words of others, especially of those against whom the mild accusation is to be made.

Let me begin by quoting from the admirable Address—none the less admirable because it was only one-quarter of the length to which we have become accustomed—delivered by my late Oxford colleague, the Rev. Bartholomew Price, at Oxford in 1860, wherein he referred to the constitution of this Section as follows:—

"The area of scientific research which this Section covers is very large, larger perhaps than that of any other; and its subjects vary so much that while to some of those who frequent this room certain papers may appear dull, yet to others they will be full of interest. Some of them possess, probably in the highest degree attainable by the human intellect, the characteristics of perfect and necessary science; while others are at present little more than a conglomeration of observations, made indeed with infinite skill and perseverance, and of the greatest value: capable probably in time of greater perfection, nay, perhaps of the most perfect forms, but as yet in their infancy, scarcely indicating the process by which that maturity will be arrived at and containing hardly the barest outline of their ultimate laws."

A little later in the Address Prof. Price made it quite clear which were the sciences "in their infancy."

"And finally we come to the facts of meteorology and its kindred subjects, many of which are scarcely yet brought within any law at all."

There is here much that will command ready and universal assent; but is there not also a rather unnecessary social scale? The science of planetary movement had not yet been "brought within any law at all" (as we now use the term) in Tycho Brahé's time; but was the astronomy of Tycho Brahé socially inferior to that of Kepler? It is difficult to fix the eye on such a question without its being caught by the splendour of Newton towering so near; and the idea of a scale descending from that great height is almost irresistibly suggested. But in spite of this grave difficulty, I ask whether there is of necessity any drop whatever from the plane of Kepler, who realised the laws, to that of Tycho, who never reached any suspicion of the true laws, but had nevertheless such faith in their existence that he cheerfully devoted his life to labours of which he never reaped the fruits? Is it not a dangerous doctrine that the work done previous to the formulation of a law is in any way inferior? Take the case of a man like Stephen Groombridge, who made thousands of accurate observations of stars in the early part of last century. Fifty years later something of the value of his work began to emerge from a comparison with later observations which showed what stars had moved and how; but it was not until nearly a century had elapsed that something about the laws of stellar movement was extracted from his patient work, combined with a repetition of similar works at Greenwich. *Then*, with the skilful assistance of Mr. Dyson and Mr. Eddington, Groombridge at last came into the fruits of his labours; but had he been asked during his lifetime for credentials in the shape of laws, on pain of being classed as an inferior in the social scientific scale, he would have been lamentably unprepared. Or consider the case of M. Teisserenc de Bort, when he began sending up his balloons. "Show me your laws," cries the mathematician. "But they are just what I hope to find," replies M. de Bort. "Yes, but surely you have formulated some law you wish to test?" pursues the invigilator. "How am I to give you proper scientific rank unless you can produce at least a tentative law?" "On the other hand I wish to keep a perfectly open mind," maintains M. de Bort. "Then I fear I cannot admit you to our class at present; you must join the infants' class, and I can only give you my best wishes that you may reach maturity some day." Unperturbed, M. de Bort continues to send up his balloons, and almost immediately discovers the great fact about the isothermal region which will be a permanent factor in the meteorology of the future. The mathematician is now ready to admit him, as a worthy person who has found a law about the constitution of the atmosphere. But was not the merit in sending up the balloons, whatever came of it? Is it not sometimes more courageous to take risks of failure? The mathematician, safe in his stronghold which possesses "probably in the highest degree attainable by the human intellect the characteristics of perfect and necessary science," is like a man who has inherited a good old-established business, and he has a distaste for the methods of those who have to try new ventures. No doubt many who make such trials fail; but, on the other hand, great fortunes have been made in that way.

It may seem, however, that too much is being deduced from a single quoted opinion, which may easily have been personal and not representative. Let me, therefore, take another which presents a different aspect of the same matter. I take the opening words of Sir G. H. Darwin's Address to this Section at Birmingham in 1886.

"A mere catalogue of facts, however well arranged, has never led to any important scientific generalisation. For in any subjects the facts are so numerous and many-sided that they only lead us to a conclusion when they are marshalled by the light of some leading idea. A theory is then a necessity for the advance of science, and we may regard it as the branch of a living tree, of which facts are the nourishment."

Those who have read the letters of Charles Darwin will recognise that this opinion was also held by the father, and may have been adopted by the son. It is no part of my purpose to raise any question of originality: I mention the point merely to take the opportunity it gives me of showing that I do not approach lightly an opinion held by two such men. With the utmost respect, I wish to question whether the criterion indicated goes deep enough. Often have we had ocular demonstration of the value of a theory in stimulating the advance of science; but is advance wholly dependent on the existence of a theory? I have tried to indicate already a deeper motive power by such instances as the work of Tycho, who had no theory, but who perceived the need of observation. And I will now definitely formulate the view that the perception of the need for observations, the faith that something will come of them, and the skill and energy to act on that faith—that these qualities, all of which are possessed by any observer worthy the name, have at least as much to do with the advance of Science as the formulation of a theory, even of a correct theory. The work of the observer is often forgotten—it lies at the root of the plant; it is easier to notice the theories which blossom and ultimately produce the fruit. But without the patient work of the observer underground there would be neither blossom nor fruit. It is also easy to fix attention on the mechanical nature of much observation; but this is not the principal feature of observing any more than is numerical computation of mathematics. There are men like Adams who perform gigantic numerical computations faultlessly, but there are others who would take equal rank as mathematicians who cannot do three additions correctly; and, again, others who could compute well and quickly, but prefer to hand over that part of their work to someone else. Similarly some great observers themselves look through the telescope, and some merely direct others how to do so; the spark of divine fire is not dependent on this detail, but on the possession of the qualities above mentioned—perception, faith, skill, and energy.

By way of bringing out more fully the nature of the assertion made by Sir George Darwin, let me beg your attention to a striking incident in recent astronomical history. We all know how the great astronomer we lost last year, Sir William Huggins (one of those already mentioned as having occupied the presidential chair of the Association without having filled that of Section A), initiated the determination of velocities of the heavenly bodies in the line of sight by means of the spectroscope. We know, further, how the accuracy of these determinations was improved by the application of photography, so that it has recently become possible to measure the velocity of the earth in its orbit (as it alternately approaches and recedes from a given star) with a precision which matches that of other known methods. Now Mr. W. W. Campbell, on his appointment as Director of the Lick Observatory in 1900, perceived the desirability of observing the line-of-sight velocities of as many stars as possible, believed that that outcome would be in some way for the advancement of science, and resolutely acted on that belief, so that for many years the resources of his great establishment have been devoted to this work. He has not turned aside from it even to publish provisional results, and has thereby incurred some adverse criticism. But, having now accumulated a large mass of observation, he is proceeding to let them tell their own tale, and a wonderful story it is. We have, unfortunately, not time to listen to more than a fraction of it at the moment; but that fraction is well worthy of our attention. When the stars are grouped in classes according to their spectral type, their average velocities differ; and if the spectral types are arranged in that particular order which for quite independent reasons we believe to be that of development of the stars, there is a steady increase in the velocities. To put the matter in a nutshell, the older a star is the quicker it moves. There are no doubt several assumptions made in reducing the matter to this simple statement, but I venture to think that they do not affect the point I now wish to make, which is as follows. There is no doubt whatever that the catalogue of facts accumulated by Mr. Campbell, when arranged in an obvious order, has led to a most important scientific generalisation—a direct negative at this date of Sir George Darwin's opening sentence, however true it may have been when he wrote it. If we read on, his next sentence doubtless entitles him to say that it was the marshalling of the facts which led to the conclusion. It is not altogether clear to me in what way this marshalling differs from the permitted "arrangement" of the catalogue; but the third sentence seems to imply that the distinction lies in the existence of a theory. But certainly Mr. Campbell had no theory; so far is he from having had a theory that he finds it extremely difficult, if not at present actually impossible, to formulate one which will satisfactorily account for the extraordinary fact brought to light by the simple arrangement of his catalogue.

Witness his words in Lick Observatory "Bulletin," No. 196, dated April 20 last:—

"The correct interpretation of the observed facts referred to in this 'Bulletin' seems not easy of accomplishment, and the brief comments which follow make no pretensions to the status of a solution.

"That stellar velocities should be functions of spectral types is one of the surprising results of recent studies in stellar motions, for we naturally think of all matter as equally old gravitationally. Why should not the materials composing a nebula or a Class B star have been acted upon as long and as effectively as the materials in a Class M star? . . . The established fact of increasing stellar velocities with increasing ages suggest the questions: Are stellar materials in the ante-stellar state subject to Newton's law of gravitation? Do these materials exist in forms so finely divided that repulsion under radiation pressure more or less closely balances gravitational attraction? Does gravity become effective only after the processes of combination are well under way?"

Mr. Campbell is far from being helpless in the situation he has created; he is ready with suggestions, though he modestly puts them as questions; but they are obviously consequent, and not antecedent, to the advance which he has made. Even if the like has never happened before, *this* scientific advance is at any rate due to little more than the accumulation of facts which arranged themselves, as Bacon hoped would naturally happen. But does it detract from the merits of this fine piece of observational work that it was suggested by no leading theory? And I will ask even further: Would its merits have been less if no such immediate induction had presented itself? To this second question I can scarcely expect a general answer in the affirmative; it is so natural to judge by results, and so difficult to look beyond them to the merits of the work itself, that I shall not easily carry others with me in claiming that the merits of the observer shall be assessed independently of his results. And yet I affirm unhesitatingly that until this attitude is reached, we cannot do justice to the observer. I believe it will be reached in the future, and I shall endeavour to give reasons for this forecast; but I admit frankly that our habit of judging by results will be hard to break. It extends even to the observer himself, and leads to the withholding of his observations from publication, so that he may himself extract the results from them. In the pure interests of the advance of knowledge, it would be far better to publish the material, so that many brains rather than one might work upon it. But the observer knows that by this course he risks losing almost the whole value of his patient work, which would pass as unearned increment to the particular person who was lucky enough to make the induction. Hence arise quarrels such as those between Flamsteed and Newton; the former refusing to publish his observations until he had himself had an opportunity of discussing them, while Newton and Halley exerted their powerful influence in the contrary sense. This situation by no means belongs to a bygone age; it may and does arise to-day, and will continue to arise so long as the recognition of the observer's work is inadequate. It was mentioned a few minutes ago that Mr. Campbell had incurred adverse criticism by accumulating a considerable mass of unpublished observations. Let me be careful not to suggest that his primary motive was the desire to have the first use of them, for I happen to know that there was at least one other good and sufficient reason for his action in the difficulty of finding funds for publication, a difficulty with which observers are only too familiar. But, whatever the reason, there were those who regretted the delay in publication as hindering the advance of science. The whole question is a delicate one, and might have been better left unraised at the moment but for a most curious sequel, which puts clearly in evidence the importance of the observer and the desirability of allowing him to discuss his own work. To make this clear, a small digression is necessary.

During the last half-dozen years astronomers have been startled on several occasions by pieces of news of a particular kind, indicating the association of large, widely scattered groups of stars in a common movement. The discussion of these movements is to occupy the special attention of this Section at one of our meetings, which is an additional reason for brevity in the present allusion. Possibly, also, most members of the Section have already heard of Prof. Kapteyn's division of the great mass of bright stars into two distinct groups flying one through the other; and, again, of the discovery by Prof. Boss of a special cluster of stars in the constellation Taurus, moving in parallel lines like a flock of migrating birds. The fascination of this latter discovery, and of one or two others like it, is that when the information supplied by the spectroscope is combined with that furnished by the long watching of patient observers, we can determine the distance of the cluster and its shape and dimensions. We realise, for instance, that there is a large flat cluster migrating just over our heads, so that one member of it (Sirius) is close to our Sun—that is to say, only three or four light-years from him. "Close" is a relative term; and the distance travelled by light in three years is from some standpoints by no means despicable. But it is small in comparison with the dimensions of the cluster, which is about one hundred light-years from end to end. The study of these clusters will doubtless occupy our close attention in the immediate future; and it is very natural that the discovery of one should lead to the search for others. Accordingly, we heard last autumn with the deepest interest, but with modified surprise, the announcement of common movement in a class of stars of a particular spectral type. The announcement rested to some extent on the work done at the Lick Observatory, much of which has been published in an abbreviated form. But Mr. Campbell, in the Lick Observatory "Bulletin" already quoted, gives reasons why he cannot accept the conclusion, which is vitiated, in his opinion, by the existence of a systematic error in the observations. Now on such a point as this the observer himself is at any rate entitled to a hearing, and is often the best judge. To take proper precautions against systematic errors is the business of the observer, and his efficiency may very well be estimated by his success in this direction—this would be a far safer guide than to judge by results. But sometimes such errors, which are very elusive, do not suggest themselves until the observations have been completed, and must be detected from the observations themselves. This, again, is rightly the business of the observer, and the desire to free his observations from such error is a perfectly sound and scientific reason for withholding publication. In the present instance the error is a peculiarly insidious one; and, indeed, we are not even certain that it is an error. It is a possible alternative interpretation of the facts that the stars with Class B spectrum are in general moving outwards from the Sun, and the additional fact that there is a comparatively large volume of space round the Sun at present empty of B stars would seem to favour this alternative. But, as already mentioned, the observer himself prefers rather to credit his observations with systematic error, which gives a spurious velocity of 5 km. per second to stars of this type. Now it will readily be understood how an error of this kind may appear doubled: two vehicles travelling in opposite directions approach or recede from one another with double the speed of either, and if one were erroneously supposed to be at rest, the other would be judged to travel twice as fast. In this way the B stars in a particular portion of the sky were judged to be travelling with a common motion of 10 km. per second, which would have been a discovery of far-reaching importance if true, but which the observer relegates to the category of systematic errors.

The illustration will suffice to remind us that the work of the observer is far from being merely mechanical: it demands also skill and judgment—skill in defeating systematic error, and a fine judgment, born of experience, of the success attained. All this is independent of the generalisations which may or may not be arrived at. Bradley's skill as an observer enabled him to discover the Aberration of Light and the Nutation of the Earth's Axis; it was enhanced rather than lessened when he went on to make further observations which, had he lived, would have conducted him to the discovery of the Variation of Latitude. After his death the world waited more than a century for this discovery to be made; but Mr. Chandler, who played a leading part in it, has declared that Bradley was almost certainly on its track. It would almost seem that an observer is only properly appreciated by another observer. There are doubtless many who, assisted by the knowledge that Bradley's skill had twice previously conducted him to a discovery, would be ready to admit the value of his later work, although he did not live to crown it; but how many of these could properly appreciate Bradley without such assistance?

I venture to think that the great brilliance of Newton has dazzled our vision so that we do not see some things quite clearly.

"Had it not been for Newton," writes De Morgan in his "Budget of Paradoxes," p. 56, "the whole dynasty of Greenwich astronomers, from Flamsteed of happy memory, to Airy, whom Heaven preserve, might have worked away at nightly observation and daily reduction without any remarkable result: looking forward, as to a millennium, to the time when any man of moderate intelligence was to see the whole explanation. What are large collections of facts for? To make theories *from*, says Bacon; to try ready-made theories *by*, says the history of discovery; it's all the same, says the idolater; nonsense, say we!"

But nothing of this will fit in with what we know of

Bradley's work; he discovered aberration, not by any help from Newton, but by accumulating a mass of observations. He had no ready-made hypothesis, or rather he had a wrong one, viz. that the stars would show displacement due to parallax; and after this was proved wrong, as it was at the very outset, he had nothing in the way of a theory to guide him, and found great difficulty in devising one *after* he had collected his facts, which spoke for themselves so far as to reveal plainly the essential features of the phenomenon in question.

"Modern discoveries" (on the preceding page of the "B. of P.") "have not been made by large collections of facts, with subsequent discussion, separation, and resulting deduction of a truth thus rendered perceptible."

To this I venture to oppose not only such work as that of Bradley, but much in the recent history of astronomy; the discoveries about systematic proper motions, about moving clusters, about the growth of velocity with life-history, and so forth.

"There is an attempt at induction going on, which has yielded little or no fruit, the observations made in the meteorological observatories. The attempt is carried on in a manner which would have caused Bacon to dance for joy. . . . And what has come of it? Nothing, says M. Biot, and nothing will ever come of it: the veteran mathematician and experimental philosopher declares, as does Mr. Ellis, that no single branch of science has ever been fruitfully explored in this way."

De Morgan was a mathematician, and I have noticed that mathematicians are apt to be crisp in their statements: but he is a bold man who says "nothing will ever come of it." Perhaps an equally crisp statement on the other side may be pardoned. I adventure the remark that if nothing has hitherto come of such observations, it is because observers have been misled by the very teaching of De Morgan and others who share his views: they have been told that they will do no good without a theory until they have come to believe it; whereas the truth probably lies in a quite different direction. To present my reasons for this proposition I must ask you first to consider in some detail the method of discussing meteorological observations suggested some years ago by Prof. Schuster. He gave an account of it to the Department of Cosmical Physics over which he presided in 1902, so that I must face some repetition of what he said; but the matter is so important that I trust this may be pardoned.

Let us compare the records produced on a gramophone disc by the playing of a single instrument and by that of an orchestra. The first will be comparatively simple, and when suitably magnified will show a series of waves which in certain parts of the record form sequences of great regularity. These represent occasions when the single instrument played a long sustained note, the pitch of which is indicated by the frequency of the wave. If the instrument plays more loudly, while still keeping to the same note, the heights of the waves will increase, though their frequency will not be altered. The exact shape of each wave will represent the quality of tone which characterises the instrument: and if another instrument were to play the same note it would be different. But so long as we keep to the same instrument, whenever the same note recurred we should find, generally speaking, the same shape of wave: and we could resolve it into its constituents, one being the main wave and others harmonics of different intensities. The analysis of such a record would thus be a comparatively simple matter, on which we need scarcely dwell further. Very different is the case of the orchestral record. There are numerous instruments, playing notes of different pitch, intensity, and character, each of which, if playing alone, would produce its own peculiar record. But when they play together the records are all combined into one. The needle can only make one record, but it is a true sum of all the individuals; for when the instrument is set to reproduce the playing of the orchestra, a trained ear can perceive the playing of the separate instruments—when the strings are playing alone, and when the wind joins them: when the horn comes in, and whether there are two players or only one: nay, even that one of the second violins is playing somewhat flat! This could not happen unless the individual performances were essentially and truly existent in the combined record; and yet this consists of only one single wavy line. The waves are, however, now of great complexity, and it seems at first sight hopeless to analyse them. The mathematician knows, however, that such analysis is possible, and is quite simple in conception, though it may be laborious in execution. Selecting a note of any given pitch, a simple calculation devised by Fourier will reveal when and how loudly that particular note was being played. This being so, it is only necessary to repeat the process for notes of different pitch. But though this can be stated so simply, the carrying out in practice may involve immense labour, by reason of the number of separate notes to be investigated. It is not merely that these will extend from low growls by the double bass to high squeaks by the fiddles, but that their variety within these wide limits will be so great. The series is really infinite. We might, indeed, prescribe a certain scale of finite intervals for the main notes, as in a piano: but the harmonics of the main tones would refuse to obey this artificial arrangement, and would form intermediate pitches, which must be properly investigated if our analysis is to be complete. Moreover, the orchestral instruments will not keep to any such prescribed intervals, but will insist on departing from them more or less, according to the skill of the performer. There is a story told of an accompanist who vainly tried to adjust the key of his accompaniment to the erratic voice of a singer. At length, in exasperation, he addressed him as follows: "Sir, I have tried you on the white notes, and I have tried you on the black notes, and I have tried you on white and black mixed: you are singing on the cracks!" Some instruments will almost certainly "sing on the cracks," so that we shall not easily escape from the examination of a very large number of possibilities indeed—we may well call them *all* the possibilities within the limits of audibility. The illustration is already sufficiently developed for provisional use. My suggestion is that science has only dealt so far with the easy records, and that the genuine hard work is to come. If we can imagine a number of deaf persons turned loose among a miscellaneous collection of gramophone records, with instructions to make what they could of them, we can readily imagine that they would pick out those of single instruments first. We must make the researchers deaf, so that they may not use the beautiful mechanism of the human ear, which has as yet no analogue in scientific work. Possibly something corresponding to this wonderful and still mysterious mechanism may ultimately be devised, and then the course of scientific research may be fundamentally altered: but for the present we must regard ourselves as deaf, and as condemned to work by patient analysis of the records. It is perfectly natural, and even desirable, to begin with the easy ones; and the finding of an easy one would no doubt in our hypothetical case be a sensational event, reflecting credit on the lucky discoverer, who would be hailed as having detected a new law, *i.e.* a new simple case. But sooner or later these will be used up, and we must attack the more complex orchestral records in earnest. Shall we find that the best music is still to come, as our illustration suggests?

But we must return to Prof. Schuster's suggested plan of work. It is closely similar to that already sketched for dealing with a complex gramophone record. Let us consider the record of any meteorological element, such as temperature or rainfall. When these records are put in the form of a diagram in the familiar way we get a wavy line, which has much in common with that traced by a gramophone needle on a smaller scale. The sight of the complexities is almost paralysing, especially when those who would otherwise attack the problem are deterred by the emphatic assertion that it is useless to do so without the equipment of some guiding hypothesis. Most of the obvious hypotheses have, of course, already been tried, and the majority of them have failed. It is to Prof. Schuster that we owe the vitally important advice to disregard hypotheses and make a complete analysis of the record. Of course the labour is great, but the genuine observer is not afraid of labour: he has a right to ask, of course, that it shall not be interminable; and when we are told that we must examine an almost infinite series of possibilities, there would seem to be some danger of this. But in practice the work always resolves itself into a series of finite steps, owing to the finite extent of the observations. A definite illustration will make this clear. Suppose we have ninety years of rainfall, and we test the record for a frequency of nine years, which would run through its period ten times; we must certainly test independently for a frequency of ten years, which would only run through its period nine times, and thus lose one whole period on the former wave; and so, also, for a possible frequency of nine years and a half, and of nine years and a quarter. But a frequency of nine years and one day would not be distinguishable from that of nine years, for the phase would only change 1° in the whole available period of observation. Indeed, the same might be said of all frequencies between nine years and nine years and one month: for the extreme difference of phase would not exceed 40°. But in course of time, when the series of ninety years' observations become 900 years, the differences of phase will approach or exceed a complete cycle, and we must accordingly narrow the intervals between frequencies chosen for examination.

The length of the series of observations is thus an important factor in our procedure, for which Prof. Schuster has indicated a beautiful analogy. Our illustrations hitherto have been provided by the science of sound, but we may also gather them from that of optics. Testing a series of rainfall observations for a periodicity is like examining a source of light for a definite bright line. The process of computation indicated by Fourier gives us what corresponds to the measured brilliance of the bright line, and the complete process of analysis corresponds to the determination of the complete spectrum of the source of light, which may consist of bright lines superimposed on a continuous spectrum. And the length of the series of observations corresponds simply to the resolving power of the optical apparatus. The only point in which the analogy breaks down is unfortunately that of ease and simplicity. In the optical analogy, an optical instrument performs for us with completeness and despatch the analysis, which in its counterpart must be performed by ourselves with much numerical labour.

Let us consider how we should most conveniently proceed to the complete delineation of a spectrum. We should ultimately need an apparatus of the greatest possible resolving power, but it might not be advisable to begin with it; on the contrary, a small instrument which enabled us to glance through the whole spectrum might save much time. Suppose, for instance, that there was a bright line in the yellow; our small instrument might suffice to show us that it was due either to sodium or helium, but no more: the decision between these alternatives must be reserved for the larger instrument. On the other hand, if no line is seen in the yellow at all, we have ruled out both possibilities at once, and so economised labour. Hence it is natural to use first an instrument of low resolving power, and afterwards one of higher.

Now in the work for which this serves as an analogy this procedure is actually imposed upon us by the march of events. It has been pointed out that the resolving power of the optical apparatus corresponds exactly to the length of our series of observations. Hence our resolving power is continually increasing. Quite naturally we begin with a short series of observations, which shows us our lines blurred and confused: to define and resolve them we have but one resource—" wait and see "; wait and accumulate more observations, to lengthen the series. But the lengthening must be in geometrical progression; we must double our series to increase the resolving power in a definite ratio, and double it again. We begin to get a glimpse of the important part to be played by the observer in the future, and of his increase in numbers.

Let us glance at a few illustrations of the use of this method. Prof. Schuster has applied it, for instance, to the observations of sun-spots. Now it may fairly be said that the general law of sun-spots was thought to be known; the variation in a cycle of about $11\frac{1}{8}$ years has long been considered to represent the facts: it catches the eye at once in a diagram, and though there are also obvious anomalies, they had not been deemed worthy of any particular attention (with one exception presently to be mentioned) until Prof. Schuster undertook his analysis. To his surprise, when he calculated the periodogram of sun-spots, he found two entirely new facts:—

Firstly, that there were other distinct periodicities, notably of about four, eight, and fourteen years.

Secondly, that the eleven-year cycle had not been continuously in action, but that during the eighteenth century it had been much less marked than the eight-year and fourteen-year cycles.

A further most interesting fact seems to emerge, viz. that several of the periodicities are harmonics of a major period of some thirty-three years or more, and it seems just possible that a connection may ultimately be established with the Leonid meteor-swarm, which revolves in this period. But it would take us too far from our main point to follow these most interesting corollaries; the point well worthy of our special attention is this, that we have here an undoubted advance in knowledge resulting, not from observations made with regard to any particular theory, but from the simple collection of facts and the arrangement of them in all possible ways, the very method which has been despised and condemned. Let us contrast with this the method hitherto adopted, which has been to hunt for some particular possible cause which will give the eleven-year period. Thus Prof. E. W. Brown suggested[1] in 1900 that the eleven-year cycle was due to the tidal action of Jupiter, altered periodically by two causes:—

	Period	Mag. of force
By Jupiter's eccentricity ...	11·86 years ...	0·33
By the motion of Saturn ...	9·93 ,, ...	0·11

and he suggests his contention by an ingenious and striking diagram, which seems to explain not only the main cycle, but its anomalies. (This Paper is, in fact, the exception above referred to.) But if his contention is correct, the periodogram should show bright lines at 11·86 and 9·93 years, which it does not. This is worth noting, since it is sometimes said that there is nothing new in Prof. Schuster's method, which is true enough in one sense, since it is simply the analysis of Fourier. The novelty consists, *firstly*, in calling attention to the necessity of applying the analysis in all cases, a necessity which I venture to think was overlooked in this instance by so able a mathematician as Prof. Brown; and, *secondly*, in the insistence on the examination of *all* periods, irrespective of any particular theory or preconception. And in this second character the method seems to me to cut at the root of the canons of procedure which have found favour hitherto.

As a second instance I present with much more diffidence a few results which seem to emerge from a very laborious analysis of the rainfall at three or four stations, for which Prof. Schuster and myself are jointly responsible. There is some evidence for a cycle of 600 days in the Greenwich rainfall, to which a further cycle in the quarter period (150 days) lends support. On analysing the Padua records it is found that these cycles do not exist; but it seems quite possible that there are cycles of rather shorter period, viz. 594 days and $148\frac{1}{2}$ days, the relation of four to one being maintained. The separate links in this chain are none of them very strong, but they seem to hang together, and there is certainly a case for further investigation. But would this case have been likely to present itself in any other way than by the examination of the whole periodogram? I find it very difficult to think, even now the periods are suggested, of any theoretical cause; to let the facts speak for themselves took much time and labour, but I venture to think that we might have waited far longer, and cudgelled our brains much more, before we got the clue by formulating hypotheses of causation.

A new method is not adopted widely all at once. Prof. Whittaker has, I am glad to say, begun to apply the method to variable star observations, and is already hopeful of having obtained valuable information in the case of the star SS Cygni. Possibly we may hear something from him at this meeting. Meanwhile, I take the opportunity to remark that the history of variable star observation affords us many lessons as to the desirability of simply accumulating observations and letting them speak for themselves, instead of being guided by a theory on hypothesis. Let me give an instance. One of the fathers

[1] Monthly Notices R.A.S., lx., p. 600.

of variable star-observing, the late N. R. Pogson, made a series of excellent observations of the star R Ursæ Majoris in the years 1853 to 1860. He then seems to have formulated a particularly unfortunate hypothesis, viz. that he knew all about the variation; and he accordingly only made sporadic observations in succeeding years. Now this star, along with many others, varies in a manner which may be illustrated from the occurrence of sunrise. The average interval between two sunrises is exactly twenty-four hours; but this is only the average. In March the sun is rising two minutes earlier every day, and the interval is therefore two minutes short of twenty-four hours; as the year advances the daily gain slackens, and at midsummer the interval is exactly twenty-four hours; then the sun begins to rise *later* each day, and the interval exceeds twenty-four hours, and so on, so that there is a regular yearly swing backwards and forwards through a mean value, and, as in the case of all such swings, there is a sensible halt at the extreme values. Now when Pogson made his observations of R Ursæ Majoris in 1853–60 it was time of halt at an extreme; the period remained stationary, and the variation repeated itself eleven times in closely similar fashion, so that Pogson concluded it would continue in the same way. How many instances suffice for an induction? Many inductions have been based on fewer than eleven. Unfortunately, the period was just beginning to change sensibly, and we lost much valuable information, for no one else repaired Pogson's neglect adequately; and the whole swing of period occupies about forty years, so that the opportunity of studying the changes he missed has only quite recently returned. We are thus reminded how disastrous may be a break in the record. It should be one of the articles of faith with an observer that the record is sacred, and must not be broken. Most of them, indeed, act on that principle already; but there are heretics, and it pained us to find even Prof. Schuster himself tinged with heresy. On the very occasion when he did so much for the observer by presenting his beautiful method, he suggested that it might even be advisable to drop observing for a time in order to apply the method to accumulated observations. He may possibly be right, but the observer had better believe him wrong. There ought to be an "observer's promise," like the promise of the boy scout; and one part of it should be not to interrupt the record, and another should be to publish the observations regularly, and never to let them accumulate beyond five years.

The method of Prof. Schuster is not the only one that has been recently proposed for dealing with large masses of observations. We have also the methods of Prof. Karl Pearson. These have been far more widely adopted for use than the periodogram, and they have also been more adversely criticised. As regards criticism, I think it is fair to say that it has chiefly been directed towards the nature of the material on which Prof. Pearson has used his process than on the process itself, and at present we need not be concerned with it. The processes themselves are sound enough; one of them, for instance, is much the same as the old method of least squares in a simple form. But if the same criticism is made as has been made on the method of the periodogram, viz. that it is not new, we can reply in almost the same words in the two cases: the mathematical calculus may not be new; the novelty is the insistence on the application of it, and the application to all possible cases. Prof. Pearson ceases to look for one principal factor only, and examines all possible factors, just as Prof. Schuster examines all possible frequencies. Let us recur for a moment to the words of Sir George Darwin previously quoted.

"A mere catalogue of facts, however well arranged, has never led to any important scientific generalisation. For in any subject the facts are so numerous and many-sided that they only lead us to a conclusion when they are marshalled by the light of some leading idea."

Let us take, for instance, a catalogue of variable stars such as those of Mr. Chandler. Particulars for each star are given in separate columns, exclusive of the name and number. We might wait long for a leading idea to guide us in marshalling the facts, and, so far as I know, we have waited till now without any such idea occurring to anyone. But Prof. Pearson insists on the plain duty of determining the correlation between each and every pair of these columns, and any others we may be able to add. Anybody could have made the suggestion, and there was plenty of elementary mathematical machinery in existence for carrying it out; but, so far as I know, nobody did, any more than the critics of Columbus suggested how to stand up an egg. But the suggestion having been made by Prof. Pearson, it was so clearly sound that I did what lay in my power to follow it up, with the result that certain correlations were at once indicated which at least pave the way for further inquiry. If we cannot say more than this, it is simply because the catalogue of facts was not large enough. So far from the observers having wasted their energies by observing without any theory to guide them, more work of the same kind would have been welcome, for it would have reduced the probable error of the correlations indicated. As an example, I may quote the following. It has already been mentioned that a variable star maximum, though it may recur after a more or less definite period, on the average, is subject to a swing to and fro like the time of sunrise. Let us call the average interval *the day* of the star and the period of swing *the year*, without implying anything more by these names than appears in the analogy. Then I found[1] that the day and the year were correlated, the value of the coefficient being

$$r=0{\cdot}56 \pm 0{\cdot}08.$$

Having obtained this clue, it was interesting to use it for the elucidation of individual problems. The *days* of many stars are by this time pretty well known, but their *years* are very uncertain. In nine or ten cases the assessment of the vaguely known *year* was under revision, and in all, without exception, the revised assessment tended in the direction of the formula. In one case (S Serpentis) the formula suggested the solution of a long-standing puzzle.[2] Finally, the inquiry is suggested whether our own sun may be treated as a variable star with a period or *day* of eleven years, in which case its time of swing a *year* should be about seventy-five years if the formula is strictly linear. There are found to be indications of a swing of this order of magnitude, though the time given by the periodogram method is fifty-four years.[3] If the relation between *year* and *day* is not strictly linear, these figures could easily be reconciled for a case lying so far outside the limits within which the formula was deduced. But the ultimate successful establishment of the connection is of less importance for our present purpose than to notice the fruitfulness of the method of suggestion, which is as mechanical as Bacon himself could have wished.

Let us admit frankly that there is an appearance of brutality about such methods. Is our method of search to be merely the old and prosaic one of leaving no stone unturned? We have been led to believe that there should be more of inspiration in it: that a true man of science should have some of the qualities of that fascinating hero of fiction, Mr. Sherlock Holmes, who picks up his clue and follows it unerringly to the triumphant conclusion. Such qualities will do the man of science no possible harm: indeed, they will be of the utmost value to him. The point to which I am now calling attention is the change in nature of the opportunities for using them, which are becoming every day more confused. Sir Conan Doyle, in the exercise of his art, keeps our attention fixed on a single trail: he conceals from us by mere omission the numerous trails which cross it. We admire the skill of the Indian who pursues an enemy through the trackless forest; but his success depends on the simplicity brought by this very tracklessness, and would be imperilled if there were numerous tracks. It may be remarked, however, that there is a still higher sagacity—that of the hound who, even among a number of tracks, can pick out the right one by scent. Let us imagine for a moment that the scientific man can be endowed in the future, by training or by some new invention, with a faculty of this kind, so that he may unerringly pursue a single trail even when it is crossed and recrossed by others. Then, in the terms of this metaphor, I draw attention to the fact that he has still to determine which is the right trail, and that in general he can only do so by pursuing each in turn to

[1] Monthly Notices R.A.S., lxviii., p. 544. [2] *Ibid.*, p. 156. [3] *Ibid.*, p. 659

the end. To take an example from recent scientific anecdote: I relate the story as I was told it, and, even if incorrect in detail, it will serve its purpose as a parable. The Röntgen rays were discovered originally by their photographic action, but afterwards it was found that they would render a screen of calcium tungstate phosphorescent. I was told that this discovery had been made in this wise: Mr. Edison had a large collection of different chemicals and a number of assistants; he set his assistants busily to work to try each substance in turn until the right one was found. Now this is not only a genuine scientific process, but it is *the fundamental process*. Let it be frankly admitted that our instincts are against it. We should much prefer to hear that some *hypothesis* had pointed the way, even a false hypothesis such as actually led to the discovery of the possibility of achromatism in lenses. Or if *memory* had played a part: The other day Prof. Fowler identified the spectrum of a comet's tail with one taken in his laboratory, of which he had some recollection, and our human sympathies fasten at once on this idea of recollection as a praiseworthy element in the discovery. Nay, even mere *accident* appeals to us more than brutal industry; if Mr. Edison had wandered into his laboratory, picked up a bottle at random, and found it answer his purpose, I venture to say that we should have instinctively awarded him more merit; there would have been just a chance that he was inspired. Let us by all means welcome hypothesis, memory, inspiration, and accident whenever and wherever they will help us; but they may fail, and then our only resource is to help ourselves by the unfailing method of examining all possibilities. The aid of the others is adventitious, and comes, like that of the gods, most readily to those who help themselves.

The maxim of "leaving no stone unturned" was enunciated from a rather different point of view some dozen years ago by an American geologist, Prof. T. C. Chamberlin, of Chicago, in a short paper for students entitled "The Method of Multiple Working Hypotheses."[1] After recalling how much the march of science in early days was retarded by the tyranny of a theory formulated too hastily, and how in later times attempts have been made to remedy this evil by holding the theory, provisionally only, as a working hypothesis, Prof. Chamberlin points out that even the working hypothesis has serious disadvantages:—

"Instinctively there is a special searching-out of phenomena that support it, for the mind is led by its desires. . . . From an unduly favoured child it readily grows to be a master and leads its author whithersoever it will. . . . Unless the theory happens perchance to be the true one, all hope of the best results is gone. To be sure truth may be brought forth by an investigator dominated by a false ruling idea. His very errors may indeed stimulate investigation on the part of others. But the condition is scarcely the less unfortunate.

. . . .

"To avoid this grave danger the method of multiple working hypotheses is urged. It differs from the simple working hypothesis in that it distributes the effort and divides the affections. . . . In developing the multiple hypotheses, the effort is to bring up into view every rational explanation of the phenomenon in hand and to develop every tenable hypothesis as to its nature, cause or origin, and to give all of these as impartially as possible a working form and a due place in the investigation. The investigator thus becomes the parent of a family of hypotheses: and by his parental relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted."

For the further elucidation of Prof. Chamberlin's proposals I must refer my audience to his original paper, which is well worthy of careful attention. He does not shirk consideration of the drawbacks—"No good thing is without its drawbacks," he writes. And it may be added that no good thing is entirely new or entirely old. Perhaps it is better to say that it is generally both new and old. The Method of Multiple Hypotheses is new because

[1] University of Chicago Press, 1897.

it is still necessary to remind scientific workers of all kinds that, so long as they restrict themselves to the examination of one hypothesis only, they can never reach complete logical proof: they can only attain a high measure of probability. What is often called verification[1] is not complete proof, but only increase in probability; for complete proof it is necessary to show that no other hypothesis will suit the facts equally well, and thus we are bound to consider other possible hypotheses even in the direct establishment of one.

But the method is also old in that it has long been adopted in practice, however partially and unconsciously, by scientific workers of all kinds. When, as a boy at school, I began to make physical measurements under Mr. J. G. MacGregor (now Professor of Physics at Edinburgh), I learnt from him one golden rule: "Reverse everything that can be reversed." The crisp form of the rule may be new to many who have long used it in their work: and its use is simply that of "multiple hypotheses." For when the current in a wire is reversed, the hypothesis is tacitly made that the effect observed may be due to the direction of the current: and when a measured spectrum photograph is turned round and remeasured, it is an admission of the hypothesis that the direction of measurement may be partly responsible for the observed displacements of the spectrum lines. By the various reversals we endeavour, in Prof. Chamberlin's words, "to bring up into view every rational explanation of the phenomenon in hand" which can be brought up into view in this way. But truly "no good thing is without its drawbacks," and one drawback to the recognition of this principle is that, by a process of mental confusion, it seems sometimes to be regarded as a distinct merit in a piece of apparatus that it can be reversed in a large number of ways. It must be remembered that the hypotheses thus examined and ruled out are chiefly instrumental ones superadded to those of Nature: and the latter are already sufficiently numerous, without our ingenious additions.

The view which I have endeavoured to put before you of the inevitable course of scientific work is that it will depend more and more on the patient process of "leaving no stone unturned." It may not be an inspiring view, but it should be at least encouraging, for it follows that no good honest work is thrown away. And it is just this encouragement of which the observer, as opposed to the worker in the laboratory and the mathematician, stands sometimes in sore need. The worker in the laboratory can often clear away his hypothesis on the spot: he can reverse his current then and there; but this is often impossible for the observer, who can and does reverse his spectrum plate for measurement, but to reverse the motion of the earth which affected the lines must wait six months, and to reverse also the motion of the star may have to wait six years, or sixty, or sixty thousand. In many cases he must leave the reversal to others, and thus not only can he not test all his hypotheses, but he may not even be able to formulate them. His aim cannot therefore be to establish within his lifetime some new law, and his work is not therefore to be appreciated or condemned by his success or failure in this respect. There are truer aims and surer methods of judgment. Something is inevitably lost when we endeavour to express these aims in the concrete; but, for the sake of illustration, we may say that the true observer is always endeavouring to reach the next decimal place, and is ever on the alert for some new event. Of the pursuit of the next decimal place it is needless to say more: the aim is as familiar in the labora-

[1] To show that the facts agree with the consequences of our hypothesis is not to prove it true. To show that is often called *verification*; and to mistake verification for proof is to commit the fallacy of the consequent, the fallacy of thinking that, because the hypothesis were true, certain facts would follow, therefore, since those facts are found, the hypothesis is true. . . . A theory whose consequences conflict with the facts cannot be true; but so long as there may be more than one giving the same consequences, the agreement of the facts with one of them furnishes no ground for choosing between it and the others. Nevertheless, in practice, we often have to be content with verification; or to take our inability to find any other equally satisfactory theory as equivalent to there being none other. In such matters we must consider what is called the weight of the evidence for a theory which is not rigorously proved. But no one has shown how weight of evidence can be mechanically estimated; the wisest men, and best acquainted with the matter in hand, are oftenest right.—"An Introduction to Logic," by H. W. B. Joseph, Fellow and Tutor of New College, Oxford, Clarendon Press, 1906, p. 480.

tory as in the observatory. But I often think that the recognition of new events is scarcely given its proper place in the annals of science if we have due regard to the consequences. I have protested that in much of his work the observer cannot be judged by the fruits of his labour, though there is an instinctive tendency to judge in this way; but here is a case where he might well be content to be so judged, and yet the consistent award is withheld. Think for a moment of the very considerable additions to our knowledge which have accrued from the discovery by Prof. W. H. Pickering of an Eighth Satellite to Saturn. The discovery led directly to the recognition of the retrograde motion; and to explain this we were led to revise completely our views of the past history of the Solar system. Incidentally, it stimulated the search for other new satellites, resulting in the discovery of a curious pair to Jupiter, and next of the extraordinary Eighth Satellite; while it was the investigation of the orbit of this curiosity which suggested an eminently successful method of work on Cometary orbits. If we judge scientific work by its results, we must take into account all this subsequent history in our appreciation of Prof. Pickering's achievement. But whether we do so or not is probably a matter of indifference to him, for the true observer is, above all things, an amateur, using the word in that splendid sense to which Prof. Hale recently introduced us. There have been many attempts to define an amateur. One was given by Prof. Schuster in his eloquent address to this Section at Edinburgh in 1892:—

"We may perhaps best define an amateur as one who learns his science as he wants it and when he wants it. I should call Faraday an amateur."

We need not quarrel with his definition, and certainly not with the noble instance with which he points it. But after all I prefer the definition of Prof. Hale[1]:—

"According to my view, the amateur is the man who works in astronomy because he cannot help it, because he would rather do such work than anything else in the world, and who therefore cares little for hampering traditions or for difficulties of any kind."

The wholly satisfactory nature of this view is that it provides not only a definition, but an ambition and a criterion. We feel at once the ambition to become amateurs, for I deny stoutly that the distinction is conferred at birth: it comes with work of the right kind. And we may know what is work of the right kind by this, if by nothing else: that by diligently performing it we shall become amateurs who find it impossible to stop: "who work in astronomy because we cannot help it." Before an army of such men even the vast hordes of dusky possibilities of which we are beginning to catch glimpses must yield. The fight may seem, and no doubt is, without end; and the opportunities for glorious deeds by which outlying whole troops of the enemy are demolished at once are becoming rarer. We are confronted with the necessity of attacking each possibility singly which threatens the stopping of the conflict through sheer weariness. Clearly the army of amateurs is the right one for the work; weariness cannot touch them; they will go on fighting automatically because "they cannot help it."

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. J. WALKER, D.Sc., F.R.S., PRESIDENT OF THE SECTION.

Theories of Solutions.

TWENTY-ONE years ago the Chemistry Section of the British Association at its meeting in Leeds was the scene of a great discussion on the nature of solutions. It was my first experience of a British Association meeting, and I well remember the stimulating effect of the lively discussion on all who took part in it. To-day, speaking from the honourable position of President of the Section, I conceive I can do no better than indicate the position of the question at the present time. And this appears to me the more appropriate as our science has had this year to mourn the departure of van 't Hoff, the founder of the modern theory of solution, whose name will remain one of the greatest in theoretical chemistry—in time to come, it will, I think, be considered almost the greatest. He had expressed the hope that he might attend this meeting as he did that twenty-one years ago. The hope is not fulfilled: his activity is merged in the final equilibrium of death. But his ideas are part and parcel of the chemical equipment of every one of us, and we know that whatever form the fundamental conceptions of chemistry may assume, the quantitative idea of osmotic pressure will be to the theory of solution what the quantitative idea of the atom is to chemical composition and properties. For I must emphasise the fact that chemistry is essentially a quantitative science, and no chemical theory, no partial chemical theory even, can be successful unless its character is quantitative. To quote the words of Lord Kelvin: "I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of science."

A general theory of solutions must be applicable to all solutions—to those in which solvent and solute exist in practically mere intermixture, as well as to those in which solute and solvent are bound together in what we cannot sharply distinguish from ordinary chemical union. Between these extremes all grades of binding between solvent and solute exist, and it may be well to give a few examples illustrating the various types of solution.

Where no affinity exists between solvent and solute, the solution is practically of the same type as a mixture of two gases which are without chemical action on each other. The solute is merely diluted by the solvent and retains its properties unchanged. An example of this type of solution may be found in the solution of one saturated hydrocarbon in another, say of pentane in hexane. On mixing the two liquids there is no evidence of union between them, the volume of the mixture is practically the sum of the volume of the components, the heat of solution is practically *nil*, the vapour pressure of each constituent is reduced merely as if by dilution with the other constituent, and so on. That there is some action between the two components even in this extreme case must be admitted, but it may be referred entirely to action of a physical kind, such as one finds on mixing one gas with another at considerable pressures. Action of a chemical nature is absent. If it be said that even saturated hydrocarbons have some chemical affinity for each other, recourse may still be had for examples to mixtures of two inactive elements, say liquid argon and liquid krypton, where chemical affinity is non-existent.

At the other extreme we have such solutions as those of sulphuric acid and water. Here there is every physical evidence of chemical union. The volume of the mixture is by no means the sum of the volumes of the components, the amount of heat evolved on mixing is very great, the separate liquids, which are practically non-conductors, yield on mixing a solution which is a good conductor, and so on. There is obviously here a great influence of the solvent water on the solute sulphuric acid, and this influence we can only account for by assuming that it is essentially chemical in character.

As the influence in such a case is necessarily reciprocal, then if even one of the constituents of the solution is inactive chemically there can plainly be no action of a chemical nature on mixing. Thus, no matter what solvent we take, it can exercise no action other than that of a physical kind on argon, say, which has been dissolved in it; and, again, if liquid argon is chosen as solvent no substance dissolved in it can be affected by it chemically, and we thus obtain only the properties of a physical mixture. It is convenient therefore to classify liquid solvents according to their chemical activity. The saturated hydrocarbons, which are chemically very inert, and, as their name paraffin implies, little disposed to chemical action of any kind, may be taken as typically inactive solvents, analogous to liquid argon. Water, on the other hand, as its numerous compounds (hydrates) with all kinds of substances testify, may be taken

[1] Monthly Notices R.A.S., lxviii., p. 64.

as a typically active solvent. The ordinary organic solvents exhibit intermediate degrees of activity.

For the purpose of illustrating the effect of solvents on a dissolved substance one may conveniently take a coloured substance in a series of colourless solvents. If the substance is unaffected by the solvent, we might reasonably expect the colour of the solution to be the same as the colour of the vapour of the substance at equal concentration. Iodine, for instance, gives rise to the familiar violet vapour. Its solution in carbon disulphide has a colour practically similar, but its solution in alcohol or water is of a brown tint quite different from the other. In the indifferent hydrocarbons and in chloroform the colour is like that in carbon disulphide, in methyl or ethyl alcohol it is brown. We conclude therefore roughly that iodine dissolved in saturated hydrocarbons, in chloroform, carbon tetrachloride and carbon disulphide is little affected by the solvent, whereas in water and the alcohols it is greatly affected, probably by way of combination, since in all the solvents two atoms of iodine seem to be associated in the molecule. That combination between the iodine and the active solvents has really occurred receives confirmation from the behaviour of iodine in dilute solution in glacial acetic acid. If the colour of this solution is observed in the cold it is seen to be brown, resembling in colour the aqueous solution. If the solution be now heated to the boiling-point, the colour changes to pink, which may be taken to indicate that the compound of iodine and acetic acid which is stable at the ordinary temperature becomes to a large extent dissociated at 100°.

Now, as I have said, a general theory of solution must be applicable to all classes of solution, and herein lies the importance of van 't Hoff's osmotic pressure theory. It applies equally to mixtures of gases, to mixtures of inert liquids, and to mixtures such as those of sulphuric acid and water; and it has the further advantage that so long as the solutions considered are dilute there are simple relations connecting the osmotic pressure with other easily measurable properties of the solutions. It has been unfortunately the custom to oppose the osmotic pressure theory of solution to the hydrate, or more generally the solvate, theory, in which combination between solute and solvent is assumed. The solvate theory is, in the first place, not a general theory, and in the second place it is perfectly compatible with the osmotic pressure theory. It is in fact with regard to a general theory of solutions on the same plane as the electrolytic dissociation theory of Arrhenius. This theory of ionisation applies to a certain class of solutions, those, namely, which conduct electricity, and is a welcome and necessary adjunct in accounting for the numerical values of the osmotic pressure found in such solutions. Similarly the hydrate, or more generally the solvate, theory is applicable only to those solutions in which combination between solvent and solute occurs, and will no doubt in time afford valuable information with regard to the osmotic pressure, especially of concentrated solutions in which the affinity between solvent and solute is most evident. It can tell us nothing about solutions in which one, or both, components is inactive, just as the electrolytic dissociation theory can tell us nothing about solutions which do not conduct electricity.

The great practical advantage bequeathed to chemists by the genius of van 't Hoff is the assimilation of substances in dilute solution to substances in the gaseous state. Here all substances obey the same physical laws, and a secure basis is offered for calculation connecting measurable physical magnitudes, irrespective of the chemical nature of the substances and of the solvents in which they are dissolved, provided only that the solutions are non-electrolytes. If the solutions are electrolytes, the dissociation theory of Arrhenius, developed independently of the osmotic pressure theory of van 't Hoff, gives the necessary complement, and for aqueous solutions offers a simple basis for calculation. Van 't Hoff has given to science the numerically definable conception of osmotic pressure; Arrhenius has contributed the numerically definable conception of coefficient of activity of electrolytes in aqueous solution, or what is now called the degree of ionisation.

Of late there has been a tendency in some thermodynamical quarters to belittle the importance of the conception of osmotic pressure. It is quite true that from the mathematical thermodynamical point of view it may be relegated to a second place, and even dispensed with altogether, for it is thermodynamically related to other magnitudes which can be substituted for it. But it may be questioned if without the conception the cultivators of the thermodynamic method would ever have arrived at the results obtained by van 't Hoff through osmotic pressure. Van 't Hoff was only an amateur of thermodynamics, but the results achieved by him in that field are of lasting importance, and his work and the conception of osmotic pressure have given a great stimulus to the cultivation of thermodynamics to chemistry.

And here we trench on a question on which a certain confusion of thought often exists. To the investigator it is open to choose that one of several equivalent methods or conceptions which best suits his personal idiosyncrasy. To the teacher such a choice is not open. He must choose the method or conception which is most clearly intelligible to students, and is at the same time least likely to lead to misconception. Osmotic pressure is a conception which the chemical student of mediocre mathematical attainments can grasp, and it is not difficult to teach the general elementary theory of dilute solutions by means of it and of reversible cycles without liability to radical error or misconception. I should be sorry on the other hand to try to teach the theory of solutions to ordinary chemical students by means of any thermodynamic function. The two methods are thermodynamically equivalent, and the second is mathematically more elegant and in a way simpler, but it affords less opportunity than the first for the student to submit his methods to any practical check or test, and in nine cases out of ten would lead to error and confusion. The difficulty of the student is not the mathematical one; with the excellent teaching of mathematics now afforded to students of physics and chemistry the mathematical difficulty has practically disappeared—the difficulty lies in critically scrutinising the conditions under which each equation used is applicable.

Of the mechanism of osmotic pressure we still know nothing, but with the practical measurement of osmotic pressure great advances have been made in recent years. In particular the admirable work of Morse and Frazer is of the first importance in establishing for solutions up to normal concentration the relationship between osmotic pressure and composition, and its variation with the temperature. Much may be anticipated from the continuation of these accurate and valuable researches, the experimental difficulties of which are enormous.

We are indebted to America not only for these researches, and for the voluminous material of H. C. Jones and his collaborators dealing with hydrates in solution, but also to A. A. Noyes and his school for accurate experimental work and for systematic treatment of solutions on the theoretical side. They, and also van Laar, have shown how solutions not coming within the ordinary range of dilute solutions to which van 't Hoff's simple law is applicable, may in some cases at least be made amenable to mathematical treatment. Van 't Hoff chose one simplification of the general theory by considering only very dilute solutions, for which very simple laws hold good, just as they do for dilute gases. Even a single gas in the concentrated or compressed form diverges widely from the simple gas laws; much more then may concentrated solutions diverge from the simple osmotic pressure law. The other simplification is to consider solutions of which the components are miscible in all proportions and are without action on each other; and this method has been developed with marked success from the point of view of osmotic pressure and other colligative properties.

The outstanding practical problem in the domain of electrolytic solutions is to show why the strong electrolytes are not subservient to the same laws as govern weak electrolytes. If we apply the general mass-action law of chemistry to the electrically active and inactive parts of a dissolved substance (the ions and un-ionised molecules) as deduced from the conductivities by the rule of Arrhenius, we find that for a binary substance a certain formula connecting concentration and ionisation should be followed, a formula which we know by the name of Ostwald's dilution law. This law seems to be strictly applicable to solutions of feeble electrolytes, but to solutions of strong electrolytes it is altogether without application. Wherein lies the fundamental difference between these two classes of solutions? Two kinds of explanation may be put forward. First, the ionised proportion may not be given accurately for strong electrolytes by the rule of Arrhenius; or second, the strong electrolytes do not obey the otherwise general law of active mass, which states that the activity of a substance is proportional to its concentration. The first mode of explanation has been practically abandoned, for other methods of determining ionisation give values for strong electrolytes in sufficient agreement with the values obtained by the method of Arrhenius. The other explanation is that for some reason the law of active mass is, apparently or in reality, not obeyed by some or all of the substances in a solution of a strong electrolyte. An apparent disobedience to the law of mass-action would, for example, be caused by the formation of complexes such as Na_2Cl_2, or Na_2Cl^+ or $NaCl_2^-$ in a solution of sodium chloride. Mere hydration, *e.g.* the formation of a complex $NaCl_1 \ 2H_2O$, would not affect the mass-action law in dilute solution, and the electrolyte would obey the dilution law in solutions of the concentration usually considered. A somewhat similar explanation, which takes into account the properties of the solvent, is that the ionising power of the solvent water undergoes a noticeable change when the concentration of the ions in it increases beyond a certain limit.

I should wish now to draw attention to a point of view which has not, so far as I am aware, been fully considered. To begin with we may put to ourselves the question: Is it the ions in the solution which are abnormal or is it the non-ionised substance? A simple consideration would point at once to it being the non-ionised portion. We have, for example, in acetic acid a substance which behaves normally, so that the ions H^+ and Ac^- as well as the undissociated molecule HAc are normal. Similarly in ammonium hydroxide the ions NH_4^+ and OH^- as well as the non-ionised NH_3 and NH_4OH all behave normally. When we mix the two solutions there is produced a substance, ammonium acetate, which behaves abnormally. Now, on the assumption that the equilibrium we are now dealing with is

$$NH_4^+ + Ac^- \rightleftharpoons NH_4Ac,$$

which of these molecular species is abnormal in the relation between its concentration and its activity? Probably not the ions NH_4^+ and Ac^-, because these were found to act normally in the solutions of acetic acid and ammonia. The presumption is rather that the abnormal substance is the undissociated ammonium acetate, for this occurs only in the abnormal acetate solution, and not in the normal acetic acid and ammonia. This view, that it is the non-ionised portion of the electrolyte which exhibits abnormal behaviour, and not the ions, has been reached on other grounds by Noyes and others, and I hope in what follows to deduce reasons in its support.

One is apt, because the ions are in general the active constituents of an electrolyte, to lay too much stress on their behaviour in considering the equilibrium in an electrolytic solution. We are justified in attributing the fact that acetic acid is a weak acid, whilst trichloracetic acid is a powerful one, rather to the properties of the un-ionised substances than to the properties of the ions. The divergence of trichloracetic acid from the simple dilution law may similarly be due to an inherent property of the un-ionised acid, a single cause being not improbably at the bottom of both, the great tendency to split into ions in water and also the abnormal behaviour towards dilution.

However that may be, I think the following reasoning goes far to show that the non-ionised portion of the electrolyte is that which is primarily abnormal in its behaviour, the ions acting in every way as normal. The dilution formulæ of Ostwald or of van 't Hoff is essentially equilibrium formulæ. One side of the equilibrium represents the interaction of the ions to form the non-ionised substance, the other side represents the splitting up of the non-ionised substance into ions. In order to fix our ideas, we may consider a salt which obeys the empirical dilution-formula of van 't Hoff. If c_u represents the molar concentration of the un-ionised portion, and c_i the molar concentration of each ion, then according to van 't Hoff's empirical formula,

$$\frac{c_i^3}{c_u^2} = \text{const.}$$

If the law of mass-action were obeyed we should have, on the other hand, Ostwald's dilution formula,

$$\frac{c_i^2}{c_u} = \text{const.}$$

According to this last formula, the activity of each substance concerned varies directly as its molar concentration, and a normal result is obtained on dilution. According to van 't Hoff's formula as stated above, the activity of none of the substances concerned varies directly as its concentration; but since the constancy of the expression is the only test of its accuracy, there are obviously other methods of stating the relation which will throw the abnormal behaviour either on the ions or on the non-ionised substance. Thus, if we write the equivalent form

$$\sqrt{\frac{c_i^3}{c_u^2}} = \text{const., or } \frac{c_i^{1.5}}{c_u} = \text{const},$$

the un-ionised substance is here represented as behaving normally, and the ions abnormally; whilst if we write the formula in the form

$$\frac{c_i^2}{c_u^{1.33}} = \text{const.}$$

the ions are represented as behaving normally, and the non-ionised substance abnormally. Now it is very important that a choice should be made amongst these three expressions, all equivalent amongst themselves so far as the mere constancy of the expression is concerned, as tested by measurements of electrolytic conductivity. Looked at from the kinetic point of view we have in the first form,

$$\frac{dx}{dt} = kc_i^3$$

$$-\frac{dx}{dt} = k'c_u^2,$$

both direct and reverse actions abnormal. In the second form, we have

$$\frac{dx}{dt} = kc_i^{1.5}$$

$$-\frac{dx}{dt} = k'c_u,$$

the ionisation being normal, the recombination abnormal. And in the third form we have

$$\frac{dx}{dt} = kc_i^2$$

$$-\frac{dx}{dt} = k'c_u^{1.33},$$

the ionisation being abnormal and the recombination normal.

Now, if it were possible to measure directly the velocity of either ionisation or recombination, we should at once be able to select the equilibrium formula which was really applicable. Unfortunately such velocities are so high as to be beyond our powers of measurement. Yet it seems possible to seek and obtain an answer from reaction velocities which are measurable. One assumption must be made, but it seems to me so inherently probable that few will hesitate to make it. It is this, if a substance in a given solution has normal activity with respect to one reaction, it has normal activity with respect to all reactions in which it can take part in that given solution. Similarly, if a substance in a given solution exhibits abnormal activity with respect to one reaction, it will exhibit abnormal activity with respect to all.

Granting this assumption, we have then to find a reaction in which either the ionised or un-ionised portion of an abnormal electrolyte is converted into a third substance with measurable velocity. Such a reaction exists in the transformation of ammonium cyanate into urea in aqueous and aqueous-alcoholic solutions, which was investigated some years ago by myself and my collaborators, and found to proceed at rates which could easily be followed experimentally. First of all comes the question: Is the urea formed directly from the ions or from the un-ionised cyanate? As Wegscheider pointed out, it is impossible from reaction-velocity alone to determine which portion passes directly into urea, if the velocities of ionisation and recombination are infinitely greater than that of the urea-formation, as is undoubtedly the case. Other circumstances make it highly probable that the ions are the active participants in the transformation, but we may leave the question open, and discuss the results on both assumptions.

Suppose, first, that the un-ionised cyanate is transformed directly into urea. Then we have the successive reactions

$$NH_4^{\cdot} + CNO' \rightleftharpoons NH_4CNO \longrightarrow CO(NH_2)_2.$$

The slight reverse transformation of urea into cyanate may for the present purpose be neglected, as it in no way influences the reasoning to be employed.

If the un-ionised substance behaves normally, then the conversion of the ammonium cyanate into urea, when referred to the un-ionised substance, will appear unimolecular and obey the law of mass-action: when referred to the ionised substance it will not appear to be bimolecular and will not obey the law of mass-action.

Suppose, now, that the direct formation of the urea is from the ions. Then we are dealing with the actions

$$NH_4CNO \rightleftharpoons NH_4^{\cdot} + CNO' \longrightarrow CO(NH_2)_2.$$

Again, let us assume the un-ionised substance to be normal. Once more, if the transformation is referred to the non-ionised substance it will appear as monomolecular; when referred to the ionised substance it will not appear as bimolecular, as it should if the mass-action law were obeyed.

It is a matter of indifference, then, so far as the point with which we are dealing is concerned, whether the ionised or the non-ionised cyanate is transformed directly into urea. If the non-ionised cyanate behaves normally the action when referred to it will in either case appear to be strictly monomolecular.

If the ionised cyanate, on the other hand, behaves normally, the reaction when referred to it will be bimolecular and normal; when referred to the non-ionised cyanate it will not be monomolecular, and therefore will be abnormal.

The actual experiments show that whether water or a mixture of water and alcohol be taken as solvent, the reaction when referred to the ions is strictly bimolecular; when referred to the non-ionised substance it is not monomolecular, *i.e.*, proportional to c_u, but rather proportional to a power of c_u other than the first, namely, $c_u^{-1.4}$.

This is, to my mind, a very strong piece of evidence that in the case of the abnormal electrolyte, ammonium cyanate, the abnormality of the ionisation equilibrium is to be attributed entirely to the non-ionised portion. But ammonium cyanate differs in no respect, with regard to its electrolytic conductivity, from the hundreds of other abnormal binary electrolytes with univalent ions; and I am therefore disposed to conclude that it is to the non-ionised portion in general of these electrolytes that the abnormality is to be attributed.

As I have already indicated, this conclusion is not altogether novel, but in my opinion it has not been sufficiently emphasised. Even in discussions where it is formally admitted that the divergence from the dilution law may be due to the non-ionised portion, yet the argument is almost invariably conducted so as to throw the whole responsibility on the ions. The point which ought to be made clear is whether the constant k of the equation

$$\frac{dx}{dt} = kc_i^2,$$

or the constant k' of the reverse equation

$$-\frac{dx}{dt} = k'c_u,$$

is really constant. If the former, then the ions are truly normal, and primary explanations of the abnormality of the strong electrolytes can scarcely be sought in high total ionic concentrations and the like, though a connection between the two no doubt exists, both being determined by the same cause.

In my illustration I have assumed that there holds good a dilution law of the kind given by Storch, of which van 't Hoff's dilution law is a particular case. Here the active mass is represented as a power of the concentration other than the first power. The argument I have used is altogether independent of this special assumption; the active mass of the abnormal substance may be any function of its concentration, and the same conclusion will be reached.

Nernst's principle of the constant ionic solubility product affords additional evidence that the ions act normally in solution. In deducing this principle it is generally assumed that it is the constant solubility of the non-ionised salt that determines the final equilibrium. This assumption, though convenient, is not necessary. The equilibrium is a closed one, thus:—

$$\begin{tikzcd} & \text{Ions} \arrow[dl, shift left] \arrow[dr, shift left] & \\ \text{Solid Salt} \arrow[ur, shift left] \arrow[rr, shift left] & & \text{Non-ionised Salt} \arrow[ul, shift left] \arrow[ll, shift left] \end{tikzcd}$$

The solid is not only in equilibrium with the non-ionised salt but also with the ions. Now, in the deduction of the change of solubility caused by the addition of a substance having one ion in common with the original electrolyte the mass-action law for ionisation is assumed. This is of course justified when we deal with feeble electrolytes, but in the case of salts and strong acids which do not follow the mass-action law the experiments are found still to be in harmony with the theoretical deductions. This is not only so when the two substances in solution are both abnormal, but also when one is abnormal and the other normal, no matter which is used to produce the saturated solution. In fact, the principle of the constant ionic solubility product may be employed with equal success to calculate the effect on the solubility of one electrolyte of the addition of another electrolyte with a common ion, whether both electrolytes are normal, both abnormal, or whether one is normal and the other abnormal. At first sight, this apparent obedience of abnormal electrolytes to the mass-action law seems strange, but a little consideration shows that if it is only the non-ionised portion of a salt that is truly abnormal, the theoretical result is to be expected. Suppose that the ions do behave normally in the ionisation, then they must also act with normal active mass with reference to the solid, with which they may be regarded as in direct equilibrium according to the closed scheme referred to above. A change, then, in the concentration of any one of the ions, brought about by the addition of a foreign salt with that ion, will necessarily bring about the change in solubility of the salt calculated from the mass-action law, so far at least as experiment can tell us, for any variation from theory is caused by the change in the nature of the solvent due to the addition of the foreign substance. We ought, then, on the assumption that the ions behave normally, to expect that the principle of the constant solubility product would yield results of the same degree of accuracy in dilute solutions whether the electrolytes considered were normal or abnormal. This, as I have said, is actually the case.

To put the whole matter briefly, in the equilibrium between electrolytes agreement will be obtained between theory and experiment whether we use the mass-action law, or an empirical law such as van 't Hoff's dilution formula, provided only that we attribute the abnormality to the non-ionised portion of the electrolyte. Thus we can deduce the ordinary formulæ for hydrolysis or for isohydric solutions as readily for abnormal as for normal electrolytes, and find the most satisfactory agreement with experiment in both cases.

By this one simple assumption, then, for which I have offered some direct justification, it is possible to find a basis for calculation with abnormal electrolytes. The problem of *why* certain electrolytes should be normal and others abnormal is, of course, in no way touched by this assumption. That is a matter for further investigation and research.

Another great desideratum of the theory of solutions is to find a general basis for the calculation of hydrates. The present position of the theory of hydrates in solution may perhaps most aptly be compared to the theory of electrolytic dissociation for solvents other than water. That hydrates exist in some aqueous solutions is undoubted, but no general rule or method exists for determining what the hydrates are and in what proportions they exist. Similarly the theory of electrolytic dissociation applied to other than aqueous solutions affords no general means of determining what the ions are and how great is the degree of ionisation. It is only for aqueous solutions that Arrhenius was able to give a practically realisable definition of degree of ionisation, and it is on this definition that the whole effective work on aqueous electrolytes is based; and until some general practically applicable principle of a similar character is attained for hydrates, the work done on that subject, however interesting and important it may be in itself, must necessarily be of an isolated character.

Arrhenius did not originate the doctrine of electrolytic dissociation or free ions: that was enunciated in 1857 by Clausius, and remained relatively barren. What he did was to introduce measurable quantities into the doctrine, and to show its simple quantitative applicability to aqueous solutions; immediately it became fertile. And as soon as a simple quantitative principle is developed for hydrates in solution, that doctrine will become fertile also.

It is surely now time that all the irrelevant and intemperate things that have been said and written by supporters of the osmotic pressure and electrolytic dissociation theories on the one hand, and by those of the hydrate theory on the other, should be forgotten. Far from being irreconcilable, the theories are complementary, and workers may, each according to his proclivity, pursue a useful course in following either. One type of mind finds satisfaction in using a handy tool to obtain practical results; another delights only in probing the ultimate nature of the material with which he works. For the progress of science both types are necessary—the man who determines exact atomic weights as well as the man who speculates upon the nature of the atoms. That the want of knowledge as to what the exact nature and mechanism of osmotic pressure is, should prevent accurate experimental work being done on it, or interfere with its use in theoretical reasoning, is equally ridiculous with the proposition that because in the theory of osmotic pressure we have a good quantitative tool for the investigation of solutions, therefore we should abandon altogether the problem of its nature.

The fundamental ideas of a science are the gift to that science of the few great masters; the many journeymen investigators may be trusted to utilise them according to their abilities. Having once given his great principles to the world, van 't Hoff remained practically a spectator of their development; but by his single act he provided generations of chemists with useful and profitable fields for their labour.

NOTES.

SIR ARCHIBALD GEIKIE, K.C.B., president of the Royal Society, Prof. Svante A. Arrhenius, and Prof. Elias Metchnikoff have been elected honorary members of the Vienna Academy of Sciences.

WE regret to announce the death, on August 23, at sixty-three years of age, of the Rev. F. J. Jervis-Smith, F.R.S., late university lecturer in mechanics and Millard lecturer in experimental mechanics and engineering, Trinity College, Oxford.

THE death is announced, at the age of eighty-two years, of Dr. G. F. Blandford. Dr. Blandford early devoted himself to the study of insanity, and was for many years lecturer on psychological medicine at St. George's Hospital. His chief work, "Insanity and its Treatment," was published first in 1871, and passed through several editions. In 1895 he delivered the Lumleian lectures on "The Diagnosis, Prognosis, and Prophylaxis of Insanity" before the Royal College of Physicians. In 1877 he was president of the Medico-psychological Association, and his address for the year was on "Lunacy Legislation." In 1887 he delivered an address before the International Congress of Medicine at Washington on "The Treatment of Recent Cases of Insanity in Private and in Asylums."

THE death is announced of Prof. Georges Dieulafoy, in his seventy-second year. Prof. Dieulafoy was well known in French medical circles, and was professor of clinics in the Necker Hospital, succeeding Prof. Trousseau in 1896. He was known as an author by the six volumes of his "Leçons cliniques de l'Hôtel-Dieu" and his "Manuel de pathologie interne," which has reached a seventeenth edition. He was a member of the Paris Academy of Medicine and a Commander of the Legion of Honour.

WE regret to record the death of Mr. J. R. Mortimer, of Driffield, on August 20, in his eighty-seventh year. Mr. Mortimer was one of the few remaining antiquaries of the old type, and during the past half-century he thoroughly investigated the archæological treasures of the East Riding of Yorkshire, in an area adjoining the field of Canon Greenwell's investigations. Mr. Mortimer excavated more than 300 Bronze-age burial mounds, and a number of Anglo-Saxon cemeteries, and also carefully mapped the elaborate series of prehistoric earthworks which occur on the Yorkshire Wolds in all directions. To a smaller extent he excavated Roman and later sites, and also made extensive collections from the chalk and other secondary formations in his district. These he transferred to a special building at Driffield, which has long been the rendezvous of antiquaries and geologists interested in East Yorkshire. This museum also contains a very large collection of stone and bronze axes, flint arrows, spears, &c., which number tens of thousands, all of which have been obtained in the vicinity. Mr. Mortimer was the author of numerous papers and memoirs, a complete list of which will be found in *The Naturalist* for May last. His principal work, however, is the massive volume "Forty Years' Researches in the British and Saxon Burial Mounds of East Yorkshire," which was published a few years ago by Browns. Besides elaborate tables of measurements of crania, &c., and hundreds of plans and sections of the barrows and earthworks, this volume has illustrations of more than a thousand Bronze-age drinking cups, food vessels, cinerary urns, and bronze and stone implements, from beautiful drawings made by his daughter, Miss Agnes Mortimer. This book, and his magnificent museum, will ever remain monuments to his memory.

THE death is announced of Mr. John Griffiths, the well-known fossil collector of Folkestone. He rendered important service to Mr. F. H. Hilton Price and Mr. J. Starkie Gardner in their researches on the Gault and associated formations, and he discovered a large proportion of the most important Gault fossils now in the British Museum and the Museum of Practical Geology.

THE Scunthorpe Urban District Council has appointed Mr. T. Sheppard, of the Hull Municipal Museums, as expert adviser to the new public museum at Scunthorpe.

IT is announced that on Saturday, September 9, an aërial postal service will be started between Hendon and Windsor. This scheme, which has the sanction of the Postmaster-General, was conceived by Mr. D. Lewis Poole and Captain W. G. Windham, and a contract has now been made with the Grahame-White Company for the carriage of mails by suitable pilots. Post-cards and envelopes, bearing a design of Windsor Castle, have been prepared, and will be on sale, at the price of 6½*d*. and 1*s*. 1*d*. respectively, in the establishments of a number of large firms in London. The letters must be posted in special boxes provided in these establishments, from which they will be collected and conveyed daily to Hendon by motor-van. The mail-bags will be also flown daily, weather permitting, to Windsor from the Hendon ground, and the correspondence distributed from there through the ordinary postal channels. All proceeds from the sale of the post-cards and envelopes will be devoted to charity. Captain Windham, the most active mover in the present scheme, inaugurated the first aërial post in India last February.

A SEISMOGRAPH has recently been installed in the Tunnel Colliery, Nuneaton, the object being to ascertain if the apparently inexplicable falls of coal and roof in mines have any relation with the occurrence of earthquakes. Whether or no the problem admits of solution in this direction, there can be no doubt that the comparison of earthquake records obtained on the surface and in mines will lead to interesting results.

IN the July number of *The Cairo Scientific Journal*, Mr. J. Craig discusses some results derived from the anthropometrical material which he had previously investigated and published in *Biometrika*. The measurements dealt with 9000 prisoners of Egyptian nationality, and were taken by the Anthropometric Bureau of the Ministry of the Interior from about 1902 to 1908. The relations of the Copts to the Moslem population, of the urban to the rural population, and of the people of Lower Nubia to the rest of Egypt, were studied, and though the data were not sufficient to support definite conclusions, indications were found of a differentiation into eastern, centre, and western delta districts; Girga and Qena provinces stood apart from Lower Nubia and Aswan on one hand, and from the rest of Upper Egypt on the other. The recent census is utilised to show the amount of migration of males from one province to another.

IT is satisfactory to learn from the report of the Maidstone Museum, Library, and Art Gallery for the period comprised between November, 1908, and October, 1910, that investigations have been undertaken in relation to the origin and purpose of the megalithic monuments of the district, more especially the one at Coldrum. Excavations undertaken beneath the dolmen occupying the centre of the stone circle at that spot revealed evidence of human interments, but nothing indicative of systematic burial or of the date when the interments were made. The skulls have been submitted to an expert, whose opinion as to their age and race had not been received when the report went to press. A model of the Coldrum structures has been presented to the museum by Mr. F. J. Bennett.

A FINE skull of the horned Dinosaur, *Triceratops prorsus*, has just been added to the gallery of fossil reptiles in the British Museum (Natural History). The specimen was discovered in the Laramie formation (Upper Cretaceous) of Wyoming, U.S.A., by Mr. Charles H. Sternberg, who undertook a special expedition for the purpose of making this addition to the museum collection. It is nearly complete, only the middle of the occipital crest, the left horn-core, and the left quadrate bone being restored in plaster. The skull proper measures 3½ feet in length, while the crest extends backwards for another 3 feet, and rises above the level of the tips of the horns. The brain-cavity has been carefully cleaned by the preparator, Mr. Frank O. Barlow, and a cast has been made in plaster. The total length of the cavity is only 10 inches, and its extreme width across the cerebral hemispheres is 2½ inches. For some time the museum has possessed a plaster copy of the restored skeleton of Triceratops as mounted in the National Museum at Washington, but the new specimen is the first actual skull of this remarkable reptile which has been exhibited in Europe. Portions of two skulls of the same genus, also discovered by Mr. Sternberg, have lately been acquired by the Senckenberg Museum, Frankfurt, where they are now being prepared.

The Times of August 28 gives an interesting and very graphic account of a remarkable hailstorm in the Pyrenees on August 16. The narrative of the storm is written by Mr. D. W. Wheeler, who, with his wife and Mr. O. P. Tidman, were encamped in the valley of Arayas, by the side of the Ordesa River. The valley lies on the Spanish side of the Pyrenees, in the Provincia de Huesca, five or six miles directly south of the Cirque of Gavarnie, and has an altitude of 4400 feet. Two storms were experienced, the first shortly after 11 a.m., and this was preceded by a clouding in of the sky and darkness. Mr. Wheeler says that midway in the darkness was the clear-cut straight line of cloud which invariably tells of hail. At first a few isolated hailstones were experienced, but soon the air suddenly became full of hailstones, and in a few minutes the storm had passed. The average size of the hailstones in the first storm is described as that of a marble, but mixed with these was a scattering of much larger stones, almost as large as golf-balls. Another storm, which followed fairly quickly on the first, was more severe. At first marble-sized hail fell and lightning blazed. Suddenly the whole land was bombarded by great hailstones as large as lawn-tennis balls. The violence of the hail is described as surpassing anything that had been previously heard of. All the mountains around were white with the covering of stones, which lay over everything like a sheet, so that in an hour summer had become winter. The smaller branches of trees had fallen as if they had been clipped by hedgers. The open grassland is described as pitted with holes, some of them a couple of inches in depth, and of about the same diameter. Testing the weight of the stones in two instances, in one six stones went to the kilogram, in the other five, which gives 5 and 7 ounces respectively. The size was that of a tennis-ball, and almost uniform. The storm wrought much destruction in the Pyrenean valleys. Seventy sheep were said to have been killed on the heights immediately above the position occupied by the writer of the narrative. Above the village of El Plan, thirty-five cows and some mules were killed. The size of the hailstones is said to have varied in different parts, according to different peasants' accounts, from "hen's eggs" to that of the closed fist. The Paris *Bulletin International* gives no indication of any atmospheric disturbance over the Spanish Peninsula. Mr. Wheeler directs attention to a hailstorm which occurred in Moravia in 1889, which is described in "Chambers's Encyclopædia," where the hailstones are said to be the size of a man's fist, and weighing 3 lb. Mr. Wheeler suggests that this should be three to the lb. The Hon. Rollo Russell, in his work on "Hail," describes a storm in the Orkneys in October, 1890, in which stones fell the size of a goose's egg, and the weight of the largest stones was estimated at 8 oz., and some penetrated the ground to the depth of 4 inches, whilst the depth of the hail in the open fields was 9 inches.

THE drought of July in this country and the serious drought in India are referred to in *Symons's Meteorological Magazine* for August. A map shows the parts of the United Kingdom in which more than ten consecutive days without rain occurred; in some districts in England, Wales, and the south of Ireland the rainfall was under 5 per cent. of the average, but was rather above the average in the west of Scotland. The duration of twenty-five days of drought was general to the west of a line drawn from the Solent to Dunstable (Bedfordshire); several places had no rain for the whole month, and the same district suffered droughts in May and June, and again in August. In India the feebleness of the south-west monsoon has caused great anxiety. Reports in *The Times* and other papers showed that up to July 28 the area most affected was west of a line drawn from Bombay through Jubbulpore to Darbhanga: nearly half of the whole country. A report dated August 21 showed, however, that conditions had greatly improved in several provinces, although famine appears to be certain in Kathiawar and Gujarat. In parts of the United Provinces and the Punjab the canals and rivers were also well supplied with water from the melting of the snow of last spring in the Himalayas.

THE Australian Monthly Weather Report for July, 1910 (lately received), contains an interesting article by Mr. E. T. Quayle on the amount of dust suspended in the atmosphere at Melbourne. One of Dr. Aitken's dust counters was in regular use at the Weather Bureau from March to July, 1909; one observation was usually made in the morning and another in the afternoon of each day. The average number of particles per cubic inch during this period was 674,000; the number steadily increased from an average of 460,000 during the first fortnight to 909,000 in the last, indicating a tendency to a winter maximum. This increase appeared to be seasonal, probably due to the diminished power of the sun in causing convectional movements of the air, and to greater relative humidity, rather than to smoke from chimneys. Taking only those days on which the wind direction remained the same, the morning observations gave an average of 638,000, and the afternoon 536,000. Wind direction had a considerable effect, the north wind being least dusty, and the north-east the most so, calms being by far the most dusty. The fact that the northerly winds in Melbourne are the least dusty is entirely contrary to the general impression on this subject. The greatest number of dust particles obtained in any one observation was 1,902,000 on a calm morning during a dense fog; the lowest recorded was 128,000, on a wet day.

IN conformity with the new regulations introduced by the Indian Museum Act of 1910, the report of the superintendent of the zoological and anthropological section of the museum at Calcutta for 1910–11 is issued in two divisions, one dealing with the progress and general condition of the establishment as a whole, and the other with the aforesaid section. In the former, after reference to the complete reorganisation of the various sections of the museum and the progress on a new wing, it is stated that the most unsatisfactory feature is the relatively small number of Europeans by whom the galleries are visited, the ratio being 15,485 Europeans to 786,519 natives. In the sectional portion of the report it is mentioned that the augmentation of the scientific staff has rendered it possible to provide instruction in the methods of zoological research, and likewise to institute inquiries relating to the fishing industries of the country. The report also contains two letters relating to the volumes on invertebrates in the "Fauna of British India," with reference to the advisability of engaging for this work the services of naturalists personally acquainted with India, and also whether the work could be best done in Calcutta or London.

IN the Transactions of the Lincolnshire Naturalists' Union for 1910 attention is directed by the president to the excellent manner in which the Lincoln Museum is discharging its proper and legitimate function as an exposition of everything connected with the natural history and antiquities of the county, strictly limiting its scope to the productions of that area. To this issue Mr. G. W. Mason contributes the fourth part of his catalogue of Lincolnshire Lepidoptera.

IN *The Field* of August 19 Mr. Pocock expresses the opinion that the animal commonly known in this country as the chita (*Cynaelurus jubatus*) is closely allied to the more typical cats, the puma, and the lynx, whereas lions, tigers, leopards, and jaguars are as markedly different. This conclusion is largely based on the fact that in the former group the hyoid apparatus is intimately connected with the skull, and that these animals purr instead of roaring. In the second group, on the contrary, the hyoid is suspended to the skull by means of a pair of long elastic cartilages, this structure being apparently connected with the power of roaring. The partial retractility of the claws of the chita is regarded as an adaptive feature connected with speed, for which this animal is specially built. It may be mentioned that in India the name chita (meaning spotted) is applied indifferently to the leopard and to *Cynaelurus jubatus*, for which reason "hunting leopard" is a preferable designation for the latter.

IN *The Zoologist* for August Captain Stanley Flower records his impressions of zoological gardens, museums, and aquariums in various parts of Europe visited by himself during the last three years, these including Birmingham, Brighton, Brunn-am-Gebirge (Austria), Cologne, Halifax, Lyons, Marseilles, Munich, Naples, Paris, Southampton, Stuttgart, and Vienna.

THE first of a series of "Behaviour Monographs" (New York: Hy. Holt and Co., 1911) in connection with *The Journal of Animal Behaviour* is by Mr. F. S. Breed, and deals with the development of certain instincts and habits in chicks. The results of a great number of experiments are tabulated and plotted. The author considers that the initial accuracy of the instinctive pecking has been exaggerated, and regards the improvement observed in the early days of life as due rather to the maturing of the organic mechanism than to the effects of habit. The efficiency of pecking has reached nearly 60 per cent. of accuracy by the beginning of the third day, about 80 per cent. on the eleventh day, after which it rises to a limit of about 85 per cent. The experiments on the rate of learning to respond differentially to objects of different brightness, colour, and size are carefully devised. The results of tests with differences of form were, however, negative. There was no conclusive evidence that the previous establishment of differential response to different colours facilitated that of responding to different sizes.

IN a paper on the birds inhabiting the bush (forest) districts of New Zealand, published in *The Emu* for July, Mr. J. C. M'Lean states that the number of such species is much less than in Australia. Originally there were only about a score of birds with strictly arboreal habits to be found in the whole of the North Island, and of these many which were once common are now rare, while one or two may possibly be extinct. An impression has, indeed, prevailed among ornithologists that nearly all the New Zealand bush-birds are *in extremis*; but this, it is satisfactory to learn, is not shared by Mr. M'Lean, who is of opinion that, although many are retreating before the axe and forest-fires, yet they are still to be met with in considerable numbers in the higher and more remote bush-country, where, it may be hoped, steps will be taken for their preservation.

IN vol. lvi., No. 21, of the Smithsonian Miscellaneous Collections Mr. E. W. Nelson describes a humming-bird from an elevation of about 3000 feet on Cerro Azul, in the Chepo district of Panama, which is referred to a new genus and species, *Goldmania violiceps*. The new bird is allied to the members of the large genus Saucerottea, but distinguished by certain peculiarities of the under tail-coverts and primary quills, and the short feathers of the tarsus, which leave the outer side of that segment exposed.

With the exception of the crown, forehead, and lores, which are iridescent violet, the general colour of the upper parts is metallic-green.

Paramaecium aurelia and *P. caudatum* are the subject of several observations by L. Woodruff (*Journ. of Morphol.*, June; *Journ. Exper. Zoology*, July; Proc. Soc. Exper. Biology, viii.). He finds that these protozoa grow well in a medium of beef extract, that the rate of reproduction in hay infusion is influenced by the volume of the culture medium, and that the organisms excrete substances which are toxic to themselves. The great majority of individuals of *aurelia* and *caudatum* can be distinguished both by shape and by size, but the power of reproduction of the two is practically identical. The macronucleus is subject to such variation that it affords no diagnostic feature; the micronuclear apparatus, on the other hand, affords crucial diagnostic characters.

A COLLECTION of six short papers is published in vol. xiii., part x., of the Contributions from the United States National Herbarium. A lichen contribution by Mr. A. W. C. Herre deals with the family Gyrophoraceæ in California; the single species *Umbilicaria semitensis*, and twelve species of Gyrophora, are described. Some interesting facts are noted by Mr. W. H. Brown as the result of observations upon plant life in four shallow lakes in North Carolina. Lake Ellis, the most fertile, is three miles long with a maximum depth of 2 to 3 feet. Three zones of vegetation are clearly distinguishable, dependent primarily on substratum, not on depth of water, which is fairly uniform. The central zone, underlaid by sand, carries a sparse growth of *Eriocaulon compressum* mingled with Eleocharis in shallower spots. The substratum becomes muddier in the intermediate zone, where *Philotria minor*, *Panicum hemitomon*, and *Eleocharis interstincta* are conspicuous, and in the thicker sediment of the marginal zone *Eleocharis mutata*, *Castalia odorata*, grasses, and sedges are the most prominent plants.

As the conditions of the various regions of the United States cause marked vegetation differences, there is a corresponding variety about the problems attacked by the various agricultural experiment stations. Both at the Connecticut and Wisconsin stations, as well as elsewhere in the old settled regions, the problem is to restore the lost fertility of the soil consequent on many years of pioneer agriculture. Especially are some of the Wisconsin clay soils deficient in phosphates, so that small dressings of rock phosphate are producing unexpectedly large returns. The management of sandy soils and of peat soils is also receiving attention, and under Prof. Whitson's direction some very useful work on soil improvement is being carried out. Another direction is given to the work at Connecticut. Considerable attention is being paid to dairying and fruit-growing, both of which systems tend to make the most of the soil and at the same time maintain its fertility. A batch of bulletins is recently to hand dealing with the renovation of old orchards and the planting of new ones, and with various dairy problems of practical importance.

THE July number of *The Cairo Scientific Journal* contains a preliminary note by Dr. W. Bean on the soils of the Gezira. A large tract on the left bank of the Blue Nile from Wad Medani to Kamlin is to be irrigated, and very wisely a thorough examination of the soil has been undertaken, samples being collected down to a depth of more than a metre along several lines across the area included in the scheme. The soils, both the more and the less fertile as the natives classify them, are well supplied with potash and phosphates, but, like most Egyptian and Sudan soils, are markedly deficient in organic matter and nitrogen, so that the results which may be obtained from their cultivation will largely depend on their treatment with respect to this deficiency. Rotation with a leguminous crop has not been used on most Sudan soils, but the experiment when made on a small scale near Khartoum gave results exceeding all expectations.

THE report of the Chemical Laboratories of the Survey Department of Egypt is this year published separately. Mr. A. Lucas, in discussing the work for 1910, reviews the various stages by which the work has developed since its commencement in 1899. Almost all the work is done for Government departments, and as approximately 40 per cent. of the materials offered or delivered are found to be adulterated or of very inferior quality, the laboratories furnish a valuable safeguard on expenditure made. In spite of this the importance of systematic analysis of supplies and accurately drawn up specifications is only being slowly realised, and the samples of material examined still bear but a small proportion to the number which would represent an adequate control of the supplies purchased.

IN continuation of an earlier contribution dealing with the Allioniaceæ of the United States, Mr. P. C. Standley has prepared a synopsis of the Mexican and Central American species of the family ordinarily known as the Nyctaginaceæ, that is published in the Contributions from the United States National Herbarium (vol. xiii., part xi.). Twenty-two genera are differentiated, but some of these are changes of name that are not sufficiently explained. Among the new species are additions to Allionia, Boerhaavia, Neea, and Pisonia. An interesting ubiquitous species is *Pisonia aculeata*, that owes its distribution to the viscid glands on the fruit.

THE prospects of viticulture in Rio Grande do Sul, the southernmost State of Brazil, are discussed by Senor A. de Azambuja in a pamphlet reprinted from articles in the *Gazeta do Commercio*, published at Porto Allegre. The first vine, introduced about sixty years ago, was the American variety Isabella. Other American and some European varieties were tried later, and the cultivation was taken up by Italian colonists. Different varieties have been grown with the object of discovering those suitable for producing table fruit, wine, and currant grapes. In recommending the Isabella and other American varieties of the *labrusca* species, the author claims that first-class wines can be produced when the details of manufacture are improved.

A SUMMARY of Dr. Kienitz's important investigations into the shapes and types of *Pinus sylvestris* is provided by Mr. B. Ribbentrop in the Transactions of the Royal Scottish Arboricultural Society (vol. xxiv., part ii.). The two extreme types are represented by the strong-branched, broad-crowned tree common in Scotland, and the slender pyramidal shape characteristic of the Baltic pine. The chief result of Dr. Kienitz's researches and experiments has been to demonstrate the heredity of special forms or races, even when transferred to different conditions of climate and soil, and thereby to prove the necessity for getting the best and most suitable seed. It is noted that the much-branched tree is better fitted to hold its own in the struggle for light, and is the prevailing form in milder localities, whereas the slender form is developed under more rigorous conditions, where heavy snowfalls constitute a primary source of danger.

THE volume of *Mitteilungen* of the Berne Scientific Society for 1910 contains an interesting attempt by P. Gruner to render the principle of relativity intelligible to the less mathematical reader. The twenty-one pages of "elementary" presentation still offer a formidable array of complex arguments, many of which are by no means easy to follow. Gruner imagines the inhabitants of earth and Mars as engaged in an attempt to unify and connect their respective time and space scales without the aid of astronomical observations of any other bodies, but with the free use of wireless telegraphy for mutual communications. He shows how, owing to relative motion, the scales must differ, and deduces Einstein's transforming equations in a simple manner. The scheme and argument could, no doubt, be still further simplified, and the simpler the better. Even this simplification tends to bring out the essential weakness of the theory, which assumes that successive light waves from a moving source are not concentric, and at the same time postulates, on the basis of Michelson's experiment, that this eccentricity cannot be discovered. Everything would be so much simpler if the speed of the body were added to the speed of the light it emitted, a supposition which, indeed, does not appear to contradict any astronomical observations.

ALTHOUGH the "exploring electrode" method of determining the distribution of electrical potential in the kathode dark space of a vacuum tube through which an electric discharge is passing has been suspected for some time, and has recently been superseded by the measurement of the deflection of a beam of kathode rays shot transversely through the discharge, it is important that the reason for the divergent results obtained by the former method should be ascertained. Prof. Wehnelt shows in a paper in the *Verhandlungen der Deutschen Physikalischen Gesellschaft* for July 30 that any small obstruction placed in the kathode dark space acquires a positive charge, and its potential is therefore higher than that of the point at which it is placed. Between the kathode and the obstruction the rise of potential is linear, but between the obstruction and the kathode glow it is curved, showing that electric charges are present in this portion of the discharge.

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES FOR SEPTEMBER:—

Sept. 2. 22h. 0m. Saturn stationary.
4. 4h. 37m. Uranus in conjunction with the Moon (Uranus 4° 35′ N.).
9. 3h. 0m. Mercury in inferior conjunction with the Sun.
13. 1h. 48m. Saturn in conjunction with the Moon (Saturn 4° 22′ S.).
14. 0h. 39m. Mars in conjunction with the Moon (Mars 4° 32′ S.).
15. 0h. 0m. Venus in inferior conjunction with the Sun.
17. 9h. 28m. Neptune in conjunction with the Moon (Neptune 5° 46′ S.).
20. 23h. 11m. Venus in conjunction with the Moon (Venus 13° 14′ S.).
23. 16h. 18m. Sun enters sign of Libra.
25. 2h. 0m. Mercury at greatest elongation W. of the Sun (17° 52′).
,, 12h. 0m. Mercury in perihelion.
,, 16h. 5m. Jupiter in conjunction with the Moon (Jupiter 2° 11′ N.).

BROOKS'S COMET, 1911*c*.—During several of the clear evenings which obtained at the latter end of last week, Brooks's comet was faintly visible to the naked eye of an observer who knew where to look for it. Ordinary opera-glasses showed it as a distinct nebulosity, and in the field of a 3¼-inch refractor it was a really brilliant object, some 5′ or 6′ in diameter, having a distinct nucleus. On Sunday night, at Gunnersbury, Mr. W. E. Rolston found the comet, as seen with opera-glasses, to be no less conspicuous than ω^1 Cygni (mag. 5·6), which it immediately preceded.

ENCKE'S COMET, 1911*d*.—Observations of Encke's comet, made by Dr. Gonnessiat at the Algiers Observatory, are recorded in No. 4518 of the *Astronomische Nachrichten*. On August 1, under excellent atmospheric conditions, the comet was seen before ninth-magnitude stars which were rising at the same time, and if seen in a dark sky would probably have equalled in brightness stars of the seventh or eighth magnitude.

Dr. Backlund briefly discusses the recent observations, and gives an ephemeris extending to September 21. At present the comet is apparently about 2° south-east of ν Leonis, and is travelling south of, and almost parallel to, the ecliptic, down through Virgo towards Libra; on September 14 it will be some 5° south of Spica.

THE ASPECT OF NOVA LACERTÆ.—On a photograph taken with fifty minutes' exposure on August 11, Herr Kostinsky found that the image of Nova Lacertæ was surrounded by a well-defined luminous aureole (black on the negative) similar to that which surrounded the images of Nova Persei in 1901. This aureole is not to be seen on similar negatives secured in January and February; therefore Herr Kostinsky deduces it may be taken as an indication that the nova has now become a gaseous nebula in the spectrum of which only bright radiations of hydrogen and the nebula lines are represented. The photographic magnitude of the nova on August 11 was about 10·5 (*Astronomische Nachrichten*, No. 4518).

KIESS'S COMET, 1911*b*.—An improved set of elements and an ephemeris are given for comet 1911*b*, by Dr. Kobold, in No. 4518 of the *Astronomische Nachrichten*. The comet reached its most southerly point on August 24, and is now travelling northwards slowly. For the next fortnight its apparent path lies through the constellation Telescopium. This comet was discovered independently by Herr Raimond Moravansky in Moravia on August 5, and the observation sent to Kiel; but this was nearly a month later than the discovery by Mr. Kiess.

THE EARLY VISIBILITY OF THE NEW MOON.—From calculations based on the data given by Mr. Horner for his remarkably early detection of the new moon, on February 10, 1910, Mr. Whitmell finds, after correcting for parallax, &c., that the difference in altitude between sun and moon at the moment of observation was only 3° 16′, the moon being 1° 46′ above, and the sun 1° 30′ below, the horizon. The corrected azimuth difference was only 9° 8′, and the moon's age sixteen hours, so that this observation is probably unique in its detection of the crescent so soon after "new moon" (*The Observatory*, No. 438).

VARIABLE STARS.—Observers of variable stars will find part ii., vol. lv., of the Annals of the Harvard College Observatory, prepared by Miss Cannon, useful. It contains a table in which are set out the maxima and minima of a large number of variable stars. For each variable the elements and the dates of observed maxima and minima are tabulated, with a special column showing the differences between the observed and calculated dates.

In No. 4515 of the *Astronomische Nachrichten* Herr Max Mündler publishes the results of a number of observations of variable stars made by him, with a 6-inch comet seeker, at Mundenheim during 1909–10.

WATER SUPPLY IN THE UNITED STATES.

TO its excellent series of pamphlets on water supply the United States Geological Survey has just added three papers, one (No. 270) descriptive of the hydrographical features of the Great Basin, an immense tract of country 208,000 square miles in area (just as large as Germany), and extending over parts of the States of Utah, Nevada, Idaho, Oregon, and California; the other two, practical manuals entitled, respectively, "Underground Waters" (Paper No. 258) and "Well-Drilling Methods" (Paper No. 257).

Of the first pamphlet, it is only necessary to remark that it follows on the same lines as those adopted for similar reports, recently reviewed in these columns, on other of the dozen districts into which the United States has been divided by the Geological Survey for the purpose of hydrographical research, and that it is equally excellent in compilation and treatment.

The two papers on well waters contain much useful information on the means of finding and securing for domestic consumption a satisfactory supply of water from underground sources. The pamphlet on well-drilling is especially practical in its description of the outfit and appliances required for the purpose, and of the methods to be followed according to the exigencies of particular localities, exigencies which, it is to be observed, are frequently of an exceptional nature. The literature on the subject of well-sinking is by no means extensive, and Mr. Bowman has exploited a field of his own, comprising those features of American practice which are associated with pioneer work in districts where many of the ordinary resources of highly developed communities are not readily available. The scope of the manual is not limited to water wells—all classes of borings for oil, gas, and water are treated, though naturally the hydraulic aspect of the subject is that which receives most prominent consideration. The interaction of borings undertaken for different ends is noted, and the flooding of oil wells by carelessly constructed and abandoned water shafts is made the subject of very necessary advice and caution. The text is freely, yet judiciously, illustrated by diagrams and photographs; and though one of the former, otherwise complete, rather amusingly indicates a platform carrying a couple of men in mid-air without any visible means of support, yet where misconception is unlikely it would be ungracious to cavil at so slight a defect. The manual is one deserving of cordial commendation.

MAGNETIC OBSERVATIONS.

THE magnetic observations made during 1910 at the Khedivial Observatory, Helwan, are included in a small pamphlet of seven pages, which gives for each month and the year the mean values of the magnetic elements, the diurnal variations in declination, horizontal force and vertical force, and particulars of the ranges of these elements on the eight most disturbed days of the year. In the diurnal variation tables values are given for both midnights, the aperiodic element not being eliminated. This is rather unusual. These tables go to 0·1′ in declination and to 1 γ (1×10^{-5} C.G.S.) in the force components. As the days tabulated in the month average twenty-eight, the expediency of going to 0·01′ and to 0·1 γ seems worthy of consideration, especially as the diurnal ranges are small. Disturbances at Helwan, at least in 1910, seem to rule small. The largest ranges in the selected disturbed days were only 13′ in declination, 187 γ in horizontal force, and 58 γ in vertical force. So far as internal evidence enables one to judge, its magnetic work does increasing credit to the Egyptian Survey Department, and it is to be hoped that it will continue to be prosecuted under favourable conditions.

In Blatt 3 of the Royal Observatory of Wilhelmshaven the assistant director, Prof. Bidlingmaier, continues the discussion of the magnetic disturbance character of the year 1910, initiated in Blatt 1 and 2, already noticed (March 16, p. 90). His method, it will be remembered, extends to individual hours the international scheme which assigns a magnetic character to individual days. Dr. Bidlingmaier's original view seems to have been that the disturbance character of a magnetic element might be based on the extent of its departure at the hour concerned from the corresponding mean monthly value on quiet days. He now regards this view as unsatisfactory, owing to its disregarding the influence of previous disturbance and making no allowance for Chree's discovery that the regular diurnal inequality varies according to the magnetic character of the day. His present estimate of hourly disturbance character seems based on the size of the maximum departure of the element during the hour from its mean value for the hour. He arrives at a numerical estimate of what he terms "Erdmagnetische Aktivitat" for the months of 1910, and compares it with Wolfer's sun-spot frequency. The hourly character of the first six months of 1911 is shown graphically in the manner applied in Blatt 1 and 2 to 1910.

The results of observations made at the U.S. Coast and Geodetic Survey's magnetic observatories at Cheltenham (Maryland), Sitka (Alaska), and Honolulu during 1907 and 1908 are published in volumes similar to those of previous biennial periods. Besides hourly readings and diurnal inequalities for the two years in question, the Cheltenham volume gives particulars of the mean annual values of the elements since the observatory began operations in 1901. A list is given of the fifty-eight principal magnetic disturbances of 1907 and 1908, with the times of their beginning and ending, and the curves obtained on nineteen of these occasions are reproduced, the times shown being G.M.T. The time scale adopted, 15 mm. to the hour, is more open than in previous years. The largest storms of the period were those of September 11 and 28, 1908. A list of earthquakes recorded by a Bosch-Omori seismograph is also given. The Sitka volume contains the usual tables of hourly readings and diurnal inequalities, and mean monthly values for the two years. A list is given of the sixty-two principal magnetic storms of the period, and the curves for sixteen of these are reproduced on a scale of 15 mm. to the hour. Sitka is a highly disturbed station, and during the principal storms there is at times considerable loss of trace; also the traces from the several elements being on one sheet—the usual practice with Eschenhagen magnetographs—there is a good deal of intercrossing of the traces. During the early part of 1908 there was also some loss of trace owing to defects in the driving clock, and a new one had to be substituted. Particulars are also given of the earthquake records obtained with a Bosch-Omori seismograph. Besides the ordinary tables of hourly readings, diurnal inequalities, &c., the Honolulu volume contains a list of the principal magnetic storms of the two years, and reproduces the curves for a number of these. Honolulu is a relatively quiet station, and loss of trace seems rare. The volume also contains a register of earthquakes recorded by a Milne seismograph.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE death is announced of Prof. J. P. Schweitzer, professor of chemistry in the University of Missouri from 1872 until 1910, when he became professor emeritus. Prof. Schweitzer was born in Berlin in 1840, and went to the United States in 1865. He was known for his work in analytical and agricultural chemistry.

MR. F. PULLINGER, chief inspector of the Technological Branch of the Board of Education; Mr. W. R. Davies, assistant secretary of the Technological Branch of the Board of Education; Prof. John Perry, F.R.S., professor of mechanics and mathematics in the Imperial College of Science and Technology; and Mr. W. Gannon, principal of the Woolwich Polytechnic, have been appointed members, for a period of three years, of the Examinations Board of its Department of Technology by the City and Guilds of London Institute. They succeed Mr. C. A. Buckmaster, Prof. W. Gowland, F.R.S., Mr. J. H. Reynolds, and Prof. W. Ripper, whose terms of office have expired.

IT is announced in *The Pioneer Mail* of August 4 that Rao Saheb Vasanji Trikamji has generously placed at the disposal of the Governor of Bombay a sum of two and a quarter lakhs of rupees for the foundation of a scientific library in connection with the institute of science now being erected in Bombay. The conditions attached to this donation are that the science institute library shall be called Vasanaji Trikamji Mulji Library. A marble bust of Vasanji Trikamji Mulji and two marble tablets mentioning the amount of the donation and other particulars are to be placed in suitable positions. The Governor has publicly thanked Rao Saheb Vasanji Trikamji for his benefaction, which will enable provision to be made for the formation of an adequate scientific library in Bombay in connection with the institute of science.

WITH the adoption of the Budget for 1909–10, a system of automatic increases in salaries was inaugurated at the University of California. We learn from *Science* that an instructor's salary is to be increased automatically 20*l.* per year from 200*l.* to 300*l.*, and the salaries of assistant professors 20*l.* a year from 320*l.* up to 400*l.* The automatic increases are not to apply to members of the faculty below the rank of instructor, nor above the rank of assistant professor, and there is to be no automatic increase after instructors have arrived at a salary of 300*l.*, and after assistant professors have arrived at a salary of 400*l.* Increases are not automatic in salaries of members of the faculty who are on part time only, nor in the case of instructors and assistant professors for a year of absence on leave. Increases of salary may, of course, be given in the cases cited above, in which no automatic increase is due as of right. Larger increases than of 20*l.* are sometimes made at the discretion of the president, with the approval of the regents.

The calendar for the session 1911–12 of the Glasgow and West of Scotland Technical College shows that the whole building now comprises more than seven acres of floor space. To quote the calendar, it "forms the largest structure in Great Britain devoted to education." It has cost, with the equipment, about 400,000*l.* The plan of confining each department to one floor has been followed in nearly every case, with the result that the internal arrangements generally are well adapted to promote efficiency in working. County secondary education committees in Scotland are authorised by the Education (Scotland) Act of 1908 to grant bursaries tenable at this college to students resident within their districts. It may be noticed, too, that a large number of firms in the area in which the college is situated have expressed their willingness to allow a selected number of their apprentices facilities for carrying out a scheme of college study conjoined with practical work. The courses of study in engineering are held during the winter session of the college, and thus student-apprentices are left free to spend the intervening summers in works. Some of these firms are willing to recognise, wholly or partially, the time spent in college as part of the apprenticeship period, but such recognition will be contingent upon satisfactory reports being received from the college in each case.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, August 21.—M. Armand Gautier in the chair.—The president announced the death of Albert Ladenburg, correspondant in the section of chemistry, and gave a short account of his work.—H. **Deslandres** and L. **d'Azambuja**: The velocities of rotation of the black filaments (flocculi) in the upper layer of the solar atmosphere. A historical sketch of the work done on the dark flocculi since their discovery by Hale and Ellermann in 1903, with special reference to the work done at the Meudon Observatory since 1908. Five diagrams are given showing successive positions of a filament on different dates, and four tables analysing various negatives.—J. **Boussinesq**: The spontaneous vibrations of a free bar, cooling by contact at its extremities and by radiation or convection at its lateral surface.—Kr. **Birkeland**: The sun and its spots. A description of experiments made with a magnetic globe as a kathode in a large discharging vessel, and a discussion of the possible bearing of these experiments on the theory of the sun. Seven photographs of the luminous phenomena observed are reproduced, and the author concludes that in the evolution of the solar system, electrical and magnetic forces must be regarded as playing a part comparable with gravitation.—A. de la Baume **Pluvinel** and F. **Baldet**: The spectrum of the Kiess comet (1911*b*). The Kiess comet was sufficiently bright during the second fortnight in July to allow of the photography of its spectrum by the prism-objective method. The wave-lengths and aspect of the bands measured are given in a table. The comet gave no continuous spectrum. Portions of the Swan spectrum and cyanogen spectrum are identified, and comparisons are made with the Johannesburg (1910*a*) and Morehouse (1908*c*) comets.—Michel **Fekete**: Some generalisations of a theorem of Weierstrass.—Georges **de Bothezat**: A method for the experimental study of the deadening of the oscillations of certain systems in motion in a fluid.—Em. **Bourquelot**: The glucoside from the leaves of the pear tree, its presence in the leaves of several varieties, its presence in the trunk and root. The existence of a true arbutine in the leaves, branches, and roots of the pear has been proved.—E. L. **Trouessart** and E. G. **Dehaut**: The wild and domesticated pigs of Sardinia and Corsica.—Édouard **Chatton**: Some parasites of marine copepods observed by M. Apstein.—E. **Roubaud**: New biological researches on the solitary wasps of Africa; evolution, variations, disturbances of the maternal instinct under the influence of hunger.—C. **Schlegel**: The development of *Maïa squinado.*—Maurice **Arthus**: The intoxications produced by snake venom.—J. **Basset**: The determining cause of "typhoid fever of the horse" (influenza, grippe, pasteurellosis, pferdestaupe, pink eye).—Maurice **Piettre**: A mode of resorption of fatty reserves.

FORTHCOMING CONGRESSES.

August 31–September 6.—British Association. Portsmouth. President: Sir William Ramsay, K.C.B., F.R.S. Address for inquiries: General Secretaries, Burlington House, W.

September 4–6.—Centenary of the University of Christiania. President of Festival Committee: Prof. Brögger.

September 9–20.—International Congress of the Applications of Electricity. Turin. President of the Committee of Honour: H.R.H. the Duke of the Abruzzi. Honorary Secretary of the Committee: Signor Guido Semenza, Via S. Paolo 10, Milano. International Secretary: Col. R. E. Crompton, C.B., R.E., Crompton Laboratory, Kensington Court, W.

September 12–15.—Celebration of the Five-hundredth Anniversary of the University of St. Andrews.

September 18–23.—International Conference of Genetics. Paris. President: Dr. Viger. Secretary: M. Philippe de Vilmorin.

September 25–29.—German Naturalists and Physicians, Karlsruhe.

October 2–7.—Third International Congress of Hygiene. Dresden. General Secretary: Dr. Hopf, Reichsstrasse 4, Dresden.

October 12–18.—Italian Society for the Advancement of Science. Rome. President: Prof. G. Ciamician. General Secretary: Prof. V. Reina, Via del Collegio Romano 26, Roma.

October 15–22.—Tenth International Geographical Congress. Rome. President: Marquis Raffaele Cappelli. General Secretary: Commander Giovanni Roncagli, Italian Geographical Society, Rome.

December 27.—American Association for the Advancement of Science. President: Dr. C. E. Bessey, University of Nebraska. Permanent Secretary: Dr. L. O. Howard, Smithsonian Institution, Washington, D.C.

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