THURSDAY, SEPTEMBER 2, 1909.

## THE USE AND MISUSE OF DRUGS.

*Drugs and the Drug Habit.* By Dr. Harrington Sainsbury. (New Library of Medicine.) Pp. xv+307. (London: Methuen and Co., n.d.) Price 7*s*. 6*d*. net.

THERE is required no little courage on the part of both writer and publisher to issue a book with this forbidding title, which to the layman brings memories of the nauseous ransom paid for release from disabilities, and to the medical man renews the griefs of "lectures on materia medica at 8 o'clock on a winter's morning" which Darwin found "something fearful to remember." To those who are not repelled by the exterior, however, we can promise much enjoyment from the perusal of this volume, which is written with the same distinction of literary style and with the same felicity of illustration as marked the author's "Principia Therapeutica." He writes for the educated layman, and, avoiding the technical pitfalls with which medicine is so bestrewn, gives a clear view of the principles on which treatment must rest. Commencing with a short historical review of the deductive medicine of the "systems," among which he places that curious survival homœopathy, he passes quickly to the definition of his subject, noting by the way the etymological connection between drug and dry, and then discusses the general aim of drug-giving and the grounds on which it is based. Here, as throughout, the author draws many analogies between familiar physical phenomena and the action of drugs, and attempts to dispel the mysticism in which therapeutics is still involved to the lay mind.

The mental effect of treatment as apart from the actual effects of the drug and the psychology of the placebo are dealt with at some length. Drugs are thrown into large classes as nutrients, incitants, depressants, alteratives, antiseptics, and sedatives, which may perhaps find justification in treatises of this kind, but which it is difficult to reconcile with the canons of modern pharmacology. The newer methods of therapeutics, such as organotherapeutics and serum therapeutics, are the subjects of an interesting chapter, in which they are claimed as the modern developments of ancient theory and in no way distinct in character from other forms of medication. Is not the theory underlying the use of thyroid extract the same as that which suggested the administration of pepsin, and is not the newest vaccine a direct descendant, even in terminology, from that of Jenner?

A third of the book is devoted to the habitual use or abuse of the narcotic and soporific drugs, such as opium, alcohol, and chloral, and to the treatment of the consequent "drug habits." Here the importance of re-building the character and self-control of the victim is insisted on, and no medicinal treatment is considered of value except in a purely subsidiary and incidental way. Some support is given to the mental treatment of these cases, but its difficulties and drawbacks are recognised. Improvement is often obtained when the environment of the subject is changed and the possibility of indulgence is restricted, so that the impulses to satisfy the craving are not aroused, while those tending to develop control gain ascendancy. The difficulty arises on return to the habitual environment, when the old impulses throng through the well-beaten paths, and the feeble, newly-acquired, guardian impulses are not aroused by the surroundings. One form of drug habit which finds no mention we should like to have seen discussed from the author's standpoint, the most prevalent, and not least pernicious of them all, that of self-drugging with all sorts of nostrums, which becomes an obsession with many of the laity, and is often accompanied by profound distrust of the medical profession.

The drink problem receives some attention, and the solution suggested is not legislation and restriction so much as education in self-control. The shortcomings of the recent Sale of Poisons Act are subjected to criticism; but should we have a Sale of Poisons Act at all? Might we not trust to education here also? On p. 231 it is stated that morphia may be detected in the urine in subjects of the opium habit, which is not in agreement with ordinary experience, and the direct inheritance of drug habits, as apart from the inheritance of the mental weakness which underlies these, requires further confirmation.

## EUCLID'S ELEMENTS IN ENGLISH.

*The Thirteen Books of Euclid's Elements.* Translated from the Text of Heiberg, with Introduction and Commentary, by T. L. Heath, C.B. 3 vols. Vol. i., Introduction and Books i.–ii., pp. x+424; Vol. ii., Books iii.–ix., pp. 436; Vol. iii., Books x.–xiii. and Appendix, pp. 554. (Cambridge: University Press, 1908.) Price 2*l*. 2*s*. net.

OUR island is the last home of ignorant Euclidolatry; *argal*, a German scholar has been allowed to edit, and a German firm to publish, the best and only critical text of Euclid's works. Our ancient universities maintain compulsory Greek; *argal*, Dr. Heath has thought it necessary to undertake, in addition to his commentary, an English translation of the text. Accepting these facts as part of the eternal fitness of things, those who can beg, borrow, or buy these three handsome volumes will be able to learn the actual contents of the "Elements," the history of their propagation and influence on mathematical study, and their relation to Greek science and philosophy in general. Dr. Heath has been so long a devoted student of Greek geometry that he is able to give his readers a very good idea of its developments and peculiar limitations; his commentary seems to cover every point of real interest, and he has rightly given, by way of comparison, some account of the modern theory of irrationals.

Naturally, the definitions and postulates receive a good deal of attention. The definition of a straight line is translated "a straight line is a line which lies evenly with the points on itself," and the same rendering of ἐξ ἴσου is given in the definition of a plane. Readers of the notes on these definitions will probably agree with the editor that ἐξ ἴσου, thus applied, was an obscure phrase even to a Greek, and that it was meant to refer to what we may call the indifferent distribution of points and lines on a line and a plane respectively. At any rate, Proclus's explanation is clearly wrong, though it very likely contains the substance of what a teacher would often say in commenting on Euclid's text.

Another very interesting note is that on the Greek notion of "angle." As this included curvilinear angles, it led to a variety of discussions and some paradoxes; for example, taking a semicircle AMB and a tangent BT at one end of the diameter AB, it was argued that the angle at B between the circle and diameter is less than the rectilineal angle ABT, because BT is outside the circle, while, on the other hand, any acute rectilineal angle can be proved less than the curvilinear angle in question. However (p. 176, bottom), there is some evidence of a way of looking at angles such as we should now express in terms of the differential calculus.

To the famous postulate 5 (generally referred to as the 11th axiom) eighteen pages are devoted. Here it must suffice to say that sufficient references are given to the principal authorities on the theory of parallels and non-Euclidean geometry, and that attention is properly directed to the work of the Jesuit Saccheri.

The books least familiar to students are, of course, the arithmetical books (vii.-ix.) and Book x. With regard to the former, Dr. Heath has given diagrams consisting of straight lines, just like those in Book v.; this is rather misleading, and it would surely have been better to give rows of dots, or, at any rate, graduated straight lines to the same scale. In this part of the work, Dr. Heath gives algebraical paraphrases of the less obvious propositions; these will be found very helpful to those not familiar with Greek methods of reasoning.

As De Morgan said long ago, the most remarkable (and in some ways most characteristically Greek) book of the "Elements" is the tenth. If we turn its propositions into algebra, we find that they contain an exhaustive classification of a certain set of irrationals (or irrational ratios), all, of course, constructible from a given line by means of rule and compass. Dr. Heath, in his introductory note, gives the irrationals in question in an algebraic form, which is perhaps the best suited for comparison with the propositions, but hardly so neat as De Morgan's in his article ("Penny Cyclopædia") on "Irrational Quantities," which is still worth careful reading.

It should be mentioned that there is an appendix containing the spurious Book xiv. (by Hypsicles), and a note on the so-called Book xv., two elaborate indexes, and a beautiful facsimile of a page of the Bodleian MS. D'Orville 301.

Those who are really interested in Greek geometry will be deeply grateful to Dr. Heath for putting together, in such an attractive form, such a large amount of historical information, and thus saving students from an immense amount of toilsome research. Finally, the excellence of the diagrams, especially in Book xiii., should not be overlooked.

G. B. M.

## HYPNOTISM AND OCCULTISM.

*Hynotism, including a Study of the Chief Points of Psycho-therapeutics and Occultism.* By Dr. Albert Moll. Translated from the fourth enlarged edition by A. F. Hopkirk. Pp. xvi+610. (London and Felling-on-Tyne: The Walter Scott Publishing Co., 1909.) Price 6s.

THIS book is a translation from the fourth edition of the original work. The author presents his readers with a survey of all that is most important in the whole province of hypnotism, and indeed has left little unsaid which could be of any value. An opening chapter on the history of hypnotism indicates the gradual progress of the science from the stage in which it was almost hopelessly mixed up with superstitious quackery, through periods of utter neglect on the part of the scientific world, to the era, which is even now only dawning, in which the subject is submitted to the strictest critical examination of physiologists and psychologists. The literature that has grown up round the subject is enormous, and its volume almost daily increases.

Dr. Moll's work is not one which is likely to appeal to the general reader, and, indeed, we are of opinion that this is one of its greatest merits. In this country hypnotism has too long been a subject in which a certain class of mind has taken an interest alternatively to spiritualism, "Christian Science," or other occult system of the day. To such, a carefully attentive and balanced criticism is positively repellent, and it is, unfortunately, in this class that hypnotism has hitherto had its vogue. Dr. Moll deals in very considerable detail with the symptoms of hypnosis. Indeed, by far the longest chapter of his book is devoted to a minute account of the psychological, physiological and even anatomical changes which may be noted during, or as a result of, hypnosis. The various explanations which have from time to time been put forward as to the hypnotic state are subjected to searching analysis, and the confident assertions of many of them are shown to have no foundation. What we know about mental processes is confined to a few concomitant phenomena, while the real nature of such processes appears for ever debarred us, and to our author the endeavours of some investigators to explain mental processes by means of our present knowledge of the central nervous system indicate a disquieting tendency to overestimate the gifts of physiology. It is, at any rate, plain that authorities take up diametrically opposite sides in their hypotheses as to the nature of the hypnotic phenomena.

The medical and legal aspects of hypnotism are dealt with at much length, and an important conclusion that emerges from the consideration of hypnotism from these points of view is that its practice should be confined to those who are properly trained in the diagnosis of the affections which it is sought to treat. To hypnotise those who should not be hypnotised, and to seek to cure disease which is not amenable to such treatment, is to bring hypnotism into disrepute and to wrong the sufferer. Dr. Moll winds up his work with a chapter upon occultism, not because there is any internal affinity between hypnotism and occultism, but because the two subjects are often mentioned together, a connection determined by their historic development. We wish that all enthusiastic investigators of the occult could be induced to peruse this part of the book. With a candid admission of the depth of our ignorance, our author asserts that we have no right flatly to refuse to recognise any domain of research. Yet if the conditions of research into the phenomena of occultism be severe, and if none of the assertions of occultists be accepted without proof, there is, according to Dr. Moll, no single series of experiments that carries with it a convincing proof of the reality of occultistic phenomena. We can warmly recommend this work to our readers as a thorough exposition of an abstruse subject.

## *ELECTRO-TECHNICS.*

*Einführung in die Elektrotechnik.* By Dr. C. Heinke. Pp. xviii+501. (Leipzig: S. Hirzel, 1909.) Price 13 marks.

AT a first glance this seems a very interesting book, but a closer study of its pages produces a feeling of mental fatigue, not to say impatience; and this is probably due to the fact that many obvious points are set out at great length whilst really important or difficult matters are passed over with tantalising brevity. Thus the author gives us many pages on the calculation of the current in a circuit containing inductance and resistance, or capacity and resistance, but the subject of single-phase commutator motors is dismissed in exactly two and one-third pages.

After a long-winded introduction, in which the author develops his ideas as to what should be taught at a technical high school and what constitutes the real difference between the mere technical man and the scientific engineer, we find a chapter which a less pedantic writer would have simply headed "mechanical analogies," but which bears the title "Conceptions of a Mechanical Nature to facilitate the Mental Connection between all Basic Electromagnetic Phenomena." The fifty-odd pages in which the author develops his analogies are very interesting, but they can only be read with advantage by persons who are already well acquainted with the subject. A beginner will find the analogy more difficult to understand than the electrical phenomenon itself.

Next follows a chapter entitled "Pressure Producers" (German Spannungserzeuger), and in this we find the old-fashioned frictional machine (but not the Wimshurst), Armstrong's experiment, primary batteries, thermopiles, and the dynamic generation of E.M.F. discussed. The latter leads to the following chapter, on "The Technical Production of Electrical Energy." This occupies some 200 pages, and contains a most bewildering collection of all possible things either directly or very remotely connected with dynamos. A few titles of the matters treated will suffice to show how varied is the character of the subjects collected under this head:—mechanical details of armature; commutator and field system; characteristics; interpoles; the Thury system as applied to the Moutiers-Lyon installation; relay for field regulation; mechanical analogy of alternating currents; a number of obsolete alternators; some modern alternators; form factor; oscillograms; vector diagrams; currents in branch circuits; resonance; stationary waves in the antenna of a wireless station; the Slaby-Arco-Braun system of wireless; Dobrovolsky's balancing transformer for three-wire system; electric bells; Rhumkorff interrupter; Wehnelt interrupter; selenium cell; buzzer; maximum cut-out; high-frequency arc; wireless telephony; power of electric currents.

The fifth chapter deals with the application of electricity, and here we get also a great variety, such as Geissler tubes, lifting magnets, bells, telephones, transformers, motors, the Kjellin furnace, a catalogue picture of a chain-welding machine, glow lamps, arc lamps, secondary batteries, a load diagram of a central station, switchboard diagrams for D.C. and A.C. stations. The final chapters deal with measuring instruments, cables and overhead lines, switches, and accessory apparatus. The grouping of all these different matters as adopted by the author may be logically right, but it is not convenient for the reader; it is also irritating to find a page or so of elementary mathematical treatment interleaved between catalogue pictures of some firm's apparatus, whilst the important features of the thing illustrated are hardly mentioned. The book has interesting parts, but to find them the reader must know a good deal of the subject; and even then the search will be rather troublesome, as there is no index.

GISBERT KAPP.

## *OUR BOOK SHELF.*

(1) *Leitfaden der Tierkunde für höhere Lehranstalten.* By Dr. K. Smalian. IV. and V. Teilen. (Leipzig: G. Freytag, 1909.) Price 1.80 marks each.

(2) *Naturwissenschaftliches Unterrichtswerk für höhere Mädchenschulen.* By Dr. K. Smalian and K. Bernan. I. Teil. Pp. 50; illustrated. (Leipzig: G. Freytag, 1909.) Price 1.20 marks.

(1) WITH the two parts referred to under the title of the work first quoted, Dr. Smalian brings to a conclusion his "Leitfaden," of which the earlier parts have been already noticed in NATURE. The fourth part, which is devoted to the Arthropoda, is stated to be for lower third form teaching (Lehrstoff der Untertertia), while the fifth part, dealing with the other invertebrates, is intended for upper third form instruction (Lehrstoff der Obertertia). Whether the parts intended for the higher forms are considered to comprise more difficult zoology than those for the lower grades is not very easy to decide. As regards style of treatment, the two parts before us seem to follow very much the lines of their predecessors, and contain a vast store of information, conveyed in a very condensed and concise manner, this technicality of the text being in some degree relieved by the coloured plates, the subjects of which are well selected, and illustrate the life-history of a number of species.

(2) This text-book has been written by the authors in accordance with the requirements of a new scheme of instruction authorised for higher grade girls' schools in Prussia, and in order to conform exactly

with these regulations Dr. Smalian has enlisted the services of a teacher in one of these schools at Halle. The first half of the part before us is devoted to the elements of botany, and the second to the rudiments of zoology, as exemplified by mammals and birds, the plan being to describe one particular species of plant and animal in considerable detail, and then to discuss some of its relatives. The illustrations, coloured and otherwise, are, if we mistake not, the same as those used in the "Leitfaden," although in certain instances reduced in size. This part is intended for the instruction of the seventh class (Lehrstoff der vii Klasse), so that in this case also the various sections of the work are to be read in consecutive order by the different classes. Both text-books appear well suited for their respective purposes.

*Die Photographie.* By W. Zimmermann. Pp. iv+164. (Leipzig: Quelle und Meyer, n.d.) Price 1.80 marks.

In twenty-three short chapters and an introduction the author has provided a general guide for beginners in photography similar to the numerous small guides that we have in English, but differing from them in being more fundamental and less detailed in the matter of manipulation and precautions. The difference may be due to the more general diffusion of elementary scientific knowledge in Germany than in this country. Formulæ for the preparation of various printing papers and plates are given, as well as instructions for their use, so that the volume is in no sense a mere collection of instructions for the manipulation of commercial products. This being as it is and the volume so small, it is interesting to note the selection that the author makes from the innumerable alternatives now available. The formulæ for developers are in the following order:—Ferrous oxalate, pyro-soda, pyrocatechin, pyrocatechin without sulphite, hydroquinone, and metol-hydroquinone.

In a chapter on "the chemical action of light and development" the ionic theory is employed, a commendable procedure if those for whom the book is written may be presumed to understand it. But the author evidently has his doubts, for he sets forth in detail the chief fundamental facts upon which the theory rests. In this case it appears to us that so far as the very little chemistry introduced is concerned, the explanations would have been more simple and still sufficient if the facts had been dealt with on the older plan, without reference to electric charges and their migrations. We notice a few old-fashioned errors with regard to actual products of certain chemical changes, but on the whole the text is trustworthy, interesting, clear, and very concise, and the illustrations are apt.

*Science in Modern Life.* Edited by Prof. J. R. Ainsworth Davis. Vol. iii. Pp. ix+187. (London: The Gresham Publishing Co., 1909.) Price 6*s.* net.

The two earlier volumes of this work—which is to be completed in six volumes—were noticed in Nature of March 4 (vol. lxxx., p. 1). The intention of the work is to give a broad outline of the principles of science and their relations to human progress and industry. The various departments of natural knowledge are surveyed by eleven different authors, each of whom is well qualified to deal with his particular subject. The present volume is devoted chiefly to light, sound, magnetism, electricity, and other branches of physics not dealt with in the second volume; and, in addition, about seventy pages are given to general biology and botany.

Mr. J. H. Shaxby's treatment of physics seems to us to be appropriate to the design of the work and calculated to create and foster interest in the subject. Attention is given to the studies of recent years, such as radiation pressure, radio-activity, Hertzian waves, and wireless telephony, and the style of description is both readable and attractive. Dr. H. J. Fleure deals with the difficult subject of the cell and nuclear division, and gives a general survey of simple forms of life. Neither this section, however, nor that by Mr. J. M. F. Drummond on botany which follows it, will be intelligible without preliminary knowledge of the subject, and will not appeal, therefore, to general readers.

A work on various subjects, written by several authors, is rarely uniform in character and scope, and the present series of volumes is no exception to the rule. In spite of this fact, we are glad to express the hope that the work will be the means of bringing problems and advances of modern science under the notice of a wide circle of readers.

*The Central Nervous System of Vertebrates.* By J. B. Johnston. Pp. 170. (Jena: G. Fischer, 1909.)

This interesting monograph appears in Dr. J. W. Spengel's "Ergebnisse und Fortschritte der Zoologie." It gives an excellent account of the structure and mechanism of the central nervous system founded on morphological and physiological facts, as these have been laboriously collected by the most modern methods by which the nervous elements have been examined. The author deals with the plan of reflex mechanisms, he describes the architecture and localisation of the central ganglia and nerve-roots, and his illustrations are drawn from morphological studies of the simpler types. One of the most important sections is No. vii., in which he discusses the functions of the great divisions of the nervous system. Nowhere have we seen a better discussion of the relations and functions of the cerebellum, or a more lucid account of the remarkable deep connections of the auditory nerves. The author has evidently received illumination from the researches and constructive criticism of Sherrington, while, as indicated by a good bibliography, he is acquainted with the literature of this vast subject. The work is a valuable contribution to human and comparative neurology.

John G. McKendrick.

*Vorlesungen über technische Mechanik.* By Dr. August Föppl. Vierter Band, Dynamik. Dritte, stark veränderte Auflage. Pp. viii+422. (Leipzig: B. G. Teubner, 1909.) Price 10 marks.

In this volume the vector equation of mass acceleration commonly known as Newton's laws is applied to the "law of areas," the problems of harmonic and oscillatory motion, the brachistochrone, motion of a rigid body, motion under no forces and motion of a top, vibrations of elastic bodies and equations of motion of hydrodynamics. The use of vector equations throughout and differences of notation and terminology make the treatment a little difficult for an English reader to follow; but it is clear that the author has fully realised the subject of his book to be *dynamics,* not the integration of differential equations. If exceptions exist, the most noticeable one is in the sections dealing with cycloidal motion, the whole problem of which can be solved, almost without writing down a single equation, by showing the geometric properties of the cycloid in a diagram where the author employs many formulæ. Among practical illustrations we notice the reference to Schlick's balancing of marine engines, while the reference to the Kegelbahn or skittle-ground takes our thoughts back to the Fatherland, with its pleasant afternoons spent in admiring the view, drinking beer, and listening to the heavy roll of the balls.

## 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.]*

### Beliefs and Customs of the Australian Aborigines.

IN May of last year (1908) I had the good fortune to meet the Bishop of North Queensland (Dr. Frodsham) at Liverpool, and he gave me in conversation some valuable information as to the native Australian beliefs and customs based on his personal knowledge of the aborigines. He told me that he had travelled among the Arunta as well as among various North Queensland tribes, and he asked me whether I was aware that the Australian aborigines do not believe children to be the fruit of the intercourse of the sexes. His Lordship informed me that this incredulity is not limited to the Arunta, but is shared by all the North Queensland tribes with which he is acquainted, and he added that it forms a fact which has to be reckoned with in the introduction of a higher standard of sexual morality among the aborigines, for they do not naturally accept the true explanation of conception and childbirth even after their admission into mission stations. The Bishop also referred to a form of communal or group marriage which he believes to be practised among aboriginal tribes he has visited on the western side of the Gulf of Carpentaria, but, unfortunately, I had not time to obtain particulars from him on this subject.

I pointed out to his Lordship the high scientific importance of the information which he had volunteered to me, and I requested that he would publish it in his own name. He assented; but as some time has passed without his finding leisure to draw up a full account, he has kindly authorised me to publish this brief statement, which has been submitted to him and approved by him as correct. I need not indicate to anthropologists the great interest and value of the Bishop's testimony as independently confirming and extending the observations of Messrs. Spencer and Gillen on the tribes of Central Australia. In the interest of science it is much to be desired that the Bishop, or those of his clergy who know the natives, would publish fuller information on these topics.

J. G. FRAZER.

Cambridge, August 23.

### A Question of Percentages.

IN NATURE of August 5 (p. 159) Mr. Cunningham asked a question as to the proper method of arriving at the mean percentage of marks obtained on papers of different values in an examination, and this has been very clearly answered by Mr. Whalley. The same question, however, arises in experimental work, particularly in agricultural and horticultural experiments, and there the answer is by no means so evident. An examiner may be supposed to have sufficient knowledge to weight his papers properly, but in an experiment no data may be available for the purpose.

Take a case where three sets of different varieties of trees are subjected to some particular treatment, and compared with three similar sets not so treated, and suppose, as an exaggerated example, that the actual measurements, say, of growth, are as follows:—

| | Treated | Untreated | Diff. per cent. A | Diff. per cent. B |
|---|---|---|---|---|
| I. ... ... | 240 ... | 120 ... | +100 ... | +100 |
| II. ... ... | 60 ... | 50 ... | + 20 ... | + 20 |
| III. ... ... | 4 ... | 8 ... | − 50 ... | −100 |
| Sum ... | 304 ... | 178 ... | — | — |
| Mean diff. | +71 ... | — ... | + 23 ... | + 7 |

If the numbers of trees in the various sets are not the same, the results may, of course, be easily weighted to correct for this; but there are other differences for which they cannot be weighted, namely, those dependent of the differences in nature of the different varieties and of attendant circumstances beyond the control of the experimenter. The mean deduced from working each result out separately (+23) ignores all such differences, and is clearly incorrect; but that deduced from the sums of the measurements (+71) is equally so, for it ignores the difference in habit of the different varieties, and gives undue weight to the results from that variety which happens to be the most rampant grower. This difficulty has been alluded to more than once in the reports of the Woburn Experimental Fruit Farm, and the only way out of it appears to be to take the mean of the means deduced in these two ways; at any rate, it is rarely safe to draw any conclusions as to the results of experiments unless these two means agree fairly with each other.

Similar difficulties arise in interpreting the results of other experiments; with a number of analyses, for instance, in which different quantities of material were taken, the mean of the individual results assumes that none of the errors is proportional to the quantities taken, whereas a mean deduced from the sum of the quantities taken and found assumes that all the errors are directly proportional to these quantities, neither of which assumptions is correct, as a rule.

Another source of error in horticultural experiments is that the differences observed are not unfrequently of different signs, and since a *plus* difference of 50 per cent. has a very different value from a *minus* difference of 50 per cent., the algebraic sum of such differences is fallacious. This is evident from the values given above for I. and III., in which the proportions are exactly reversed, but which figure under A as differences of +100 and −50 respectively. A more correct way of calculating such differences is to take the lowest (or highest) value in each pair of plots as the standard of comparison, instead of the value in the check plot, and to affix a + or − sign to the difference, according to whether the plot under treatment has given a larger or smaller value than the untreated plot. Such differences are given under B, and correctly represent the ratios of the experimental measurements. It would be well if such a method of calculating percentage differences could receive some special designation, so that it might become recognised, for without this its use is likely to lead to misunderstanding.

SPENCER PICKERING.

### The Planar Arrangement of the Planetary System.

IN your issue of July 29 your reviewer devotes some space to my paper on the origin of the planetary system (*Astronomische Nachrichten*, No. 4308), and closes by asking, "Why, for instance, on the hypothesis of capture, are the vast majority of the orbits near the plane of the ecliptic and their motion direct?" This is because our system was formed by the unsymmetrical meeting of two streams of nebulosity or by the mere gravitational settling of a single nebula of curved and unsymmetrical figure, giving a rotating cosmical vortex, or spiral nebula, but without hydrostatic pressure as imagined by Laplace. In Lick Observatory Publications, vol. viii., Plate 38, you will find an illustration of H.V. 2 Virginis, a spiral nebula of unsymmetrical figure just beginning to coil up and form a system. What will happen in the later stages of this nebula is sufficiently shown in the Lick photographs of other nebulæ given in this volume. As the mass whirls and condenses under resistance, it will necessarily retain and draw down most of the nebulosity into the plane of motion. This is exactly what has given the planar arrangement of the bodies in the solar system. In *Astronomische Nachrichten*, Nos. 4341–2, your reviewer will find a fuller explanation of the method of capture, and other papers yet to come will make the theory so clear that it need not take up more of your valuable space at present.

T. J. J. SEE.

Naval Observatory, Mare Island, Cal., August 12.

### The Benham Top.

MY attention has been directed to a paper in the Transactions of the Ophthalmological Society, by Mr. A. S. Taylor, entitled "Colour Phenomena due to Intermittent Stimulation with Light: Note on the Colours of Benham's Top."

It is to the conclusions in the latter part of the paper that I desire to refer, as last year, in a paper before the Physical Society (see NATURE, June 18, 1908, p. 166), I endeavoured to explain this phenomenon in a somewhat different way. The main conclusion in both theories is the same, but Mr. Taylor explains the necessary thinness of the lines as due to irradiation, whereas it appears to me to be a contrast effect. The weak illumination of the disc does not seem to warrant an assumption of irradiation.

Stewart's curves for colour vision and Burch's work on "Artificial Temporary Colour Blindness" show that the colour sensations have different rates of growth and decay. When this difference has been admitted, it is easy to explain the colours of the Benham top by the sequence of white and black divisions on the disc, the thinness of the lines being necessary to reinforce by a contrast the weak colour effect. F. PEAKE SEXTON.

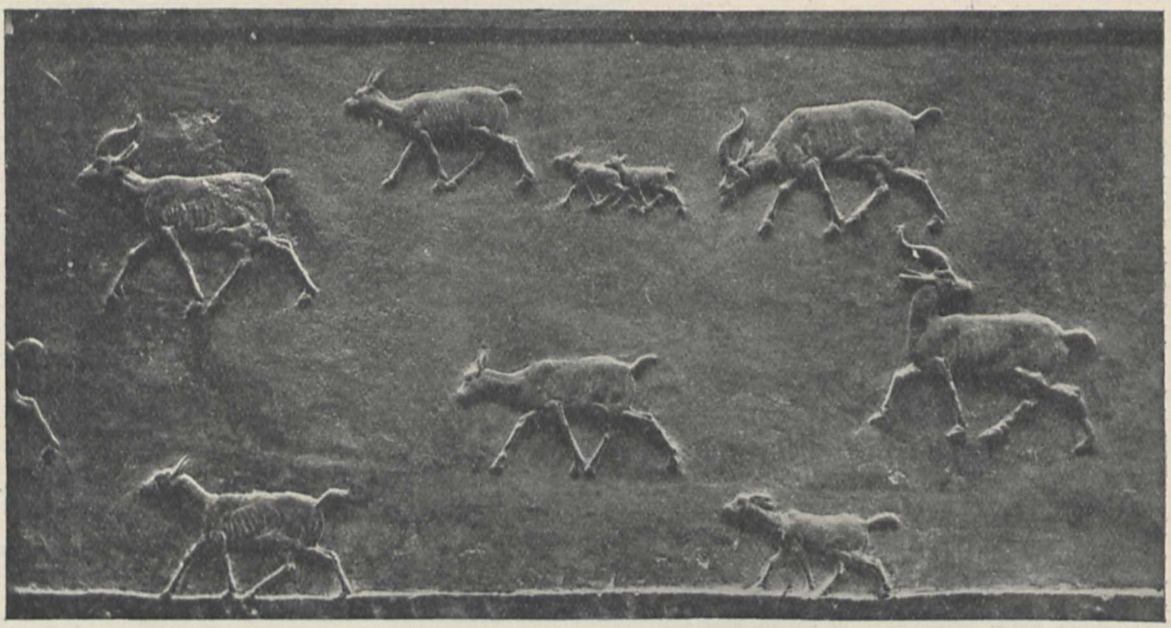
Hampton Wick.

## *MAN AND ANIMALS.*[1]

THIS volume, of which some chapters have already appeared in the *Contemporary Review,* will be eagerly read and studied by all interested in animal psychology and the treatment of the inferior animals by man. For, mixed up with a large amount of perhaps somewhat irrelevant matter relating to the history of religions, the author has collected a vast store of information relating to the estimation or otherwise in which animals have been held by the ancient nations from Egyptian, Bhuddistic, and Græco-Roman times to the Middle Ages; while the concluding chapter deals with modern ideas on the subject.

The objects, origin, and conception of sacrifice are each treated at considerable length, while folk-lore has been largely drawn upon to illustrate the subject from all points of view. Among numerous other instances of the wide origin of such legends, we may refer to the author's account of how the Welsh story of the wolf-hound Gellert, is paralleled in other countries. The Countess appears to be a firm believer in the doctrine that the intelligence of the lower animals differs only in degree from that of their masters; and the theory that any animals are mere automatons is persistently scouted. Moreover, if we read between the lines, the author appears to be convinced that animals have souls, and are therefore immortal, although we do not find this stated in so many words. That this was the idea of the Jews in biblical times the author considers to be proved by the doctrine that "the blood is the life." If this idea of animal immortality be true, no thoughtful person can read the volume without serious misgivings and heart-searchings.

Throughout, the book is written in an attractive style, and we have read it from preface to index with real pleasure. The attractive style of the text is rendered the more interesting by the numerous excellent plates with which the volume is illustrated. Some of these do not appear, however, to be referred to in the text; and it is a pity that the author did not consult a naturalist before deciding on their titles. We find, for instance, the plate facing p. 108 (herewith reproduced), taken from an Assyrian relief in the British Museum, lettered "wild goats and young"; while in the illustration facing p. 116 we have another Assyrian sculpture, representing a god carrying a horned animal, apparently furnished with a beard, which is designated an "antelope." That the animals in the first figure are not wild goats is evident from the character of the horns of the males, the lack of a beard in that sex, and the absence of both these appendages in the females; and we have a strong suspicion that they really represent one of the larger African gazelles, such as the addra.



Assyrian Relief in the British Museum showing a troop of (?) Gazelles. From the "Place of Animals in Human Thought," where the animals are termed "wild goats."

The work, moreover, sadly wanted revision by a well-educated proof-reader. "Worser," which occurs at least twice, is not English; neither is "Quartenary," also occurring twice, correct orthography; the latter remark also applying to "camel-leopard" (p. 345). A mungoose, moreover, is not a member of the weasel tribe (p. 308), neither is it correct to term a wolf a vulpine animal. If such blemishes occurred in many books we could mention, we should pass them over without notice; the pity of it is that they mar the pages of such a thoroughly interesting volume as the one of which we now take leave. R. L.

[1] "The Place of Animals in Human Thought." By the Countess Evelyn Martinengo Cesaresco. Pp. 376; illustrated. (London: T. Fisher Unwin, 1909.) Price 12*s.* 6*d.* net.

## STYLES OF THE CALENDAR.

FEW subjects are more full of pitfalls than that of the change of style and consequent alteration of dates, which therefore requires great care in its consideration. Many articles have appeared from time to time in *Notes and Queries*, and I would refer to two of my own in 9th S., vol. v., pp. 344, 461. Here I need only say that the dates given in Whitaker are correct, though it seems to me it would be better to call May 14 May Day by Julian style instead of old May Day. It is difficult always to remember that no alteration was made in the difference between the two styles in 1600, but was in 1700, 1800, and 1900. None will be made in 2000. There was an old saying about St. Barnabas's Day:—

"Barnaby, Barnaby, Barnaby bright,
The longest day and the shortest night."

Saints' days, &c., being kept on the same nominal day, June 11 was still St. Barnabas's Day, though what was that day in the old calendar became June 21 from 1582 to 1700, then June 22 from 1701 to 1800, June 23 from 1801 to 1900, and now June 24. But the saint's day remaining June 11, the above distich ceased to apply. George III.'s birth was announced as on May 24, but when the style was changed in 1752, his birthday became June 4, and was kept on that date during his reign. When Gregory XIII. altered the style in 1582, the count of days was changed by ten; when England adopted it in 1752 we had to change the days by eleven; in 1801 this became twelve, and in 1901 thirteen, so that we differ now from Russia and the Greek Church by that number in our dates.

The question of an alteration is again being discussed, and uniformity is so desirable that we hope Gregorian usage will be adopted, though it is not ideally perfect, and a better rule would have been to drop a leap-year at the end of each period of 128 years.[1] The late Prof. Newcomb thought that the alteration of the style was a mistake, on the ground that there was no particular object in keeping the same dates at the same seasons over very long intervals of time, and the hiatus made by the omission of days caused, as it was bound to do, great confusion. It is not always recollected that the reason why a change was so long advocated in the western Church, and at last carried out, was the supposed necessity of regulating Easter by the full moon following the vernal equinox, which was supposed to fall on March 21 at the time of the Council of Nicæa, A.D. 325, and really did so in the preceding century. To that epoch, then, everything was referred; otherwise it would have been more natural to have started when the alteration was made by which the mean calendar year was made nearly of the true length of the tropical year.

From 1500 to 1700 May Day was kept in England on the day corresponding by the season to what we should now call May 11; after 1700, until the alteration of the style in 1752, on what would be May 12 by the new reckoning. At that time, then, the dancing round the May-pole, so popular in this country, took place at nearly what is now the middle of the month. It is of interest to remember that the great May-pole in the Strand was removed, in 1717, at the instance of Sir Isaac Newton, to Wanstead, to support the Huyghens telescope of great focal length, which had been lent to Pound (the uncle of Bradley, afterwards Astronomer Royal), who made excellent use of it.

Christmas Day, of course, and all holy and saints' days, fell, after the alteration of the calendar, several days later, according to the season; ten days when Gregory XIII. decreed the change in 1582, and eleven when it was adopted in England in 1752. After 1900 the difference between the two styles became thirteen days, and old Christmas Day is marked in Whitaker and other almanacs on January 7. New Year's Day fell seven days afterwards, on the day we now call January 14; that is not marked in Whitaker, but the Russians and the Orientals generally keep it on that day, thirteen days after ours.

It is, of course, necessary to be very careful in comparing historic events (whether astronomical or otherwise) between 1582 and 1752, English dates being given in old style, and many (not all) of the Continental dates in new style.

It has been suggested to me that the following tables will be useful for reference in calendar questions.

The Gregorian calendar was arranged to start from the date of the Council of Nicæa in A.D. 325, so that the vernal equinox should henceforth be made to fall on March 21, as it was supposed to do then. Table I. gives the dates when the equinoxes and the solstices fell by the Julian style from A.D. 200 to the present century. Table II. gives the dates in Gregorian reckoning of the first day of May and the last day of October (All Hallows' Eve) for the same centuries.

TABLE I.

| A.D. | Vernal Equinox O.S. March | Summer Solstice O.S. June | Autumnal Equinox O.S. September | Winter Solstice O.S. December |
|---|---|---|---|---|
| 200 | 21 | 22 | 24 | 22 |
| 300 | 20 | 21 | 23 | 21 |
| 400 | 19 | 20 | 22 | 20 |
| 500 | 19 | 20 | 22 | 20 |
| 600 | 18 | 19 | 21 | 19 |
| 700 | 17 | 18 | 20 | 18 |
| 800 | 16 | 17 | 19 | 17 |
| 900 | 16 | 17 | 19 | 17 |
| 1000 | 15 | 16 | 18 | 16 |
| 1100 | 14 | 15 | 17 | 15 |
| 1200 | 13 | 14 | 16 | 14 |
| 1300 | 13 | 14 | 16 | 14 |
| 1400 | 12 | 13 | 15 | 13 |
| 1500 | 11 | 12 | 14 | 12 |
| 1600 | 10 | 11 | 13 | 11 |
| 1700 | 10 | 11 | 13 | 11 |
| 1800 | 9 | 10 | 12 | 10 |
| 1900 | 8 | 9 | 11 | 9 |

TABLE II.

| A.D. | Gregorian Dates May | Gregorian Dates October |
|---|---|---|
| 200 | 1 | 31 |
| | | November |
| 300 | 2 | 1 |
| 400 | 3 | 2 |
| 500 | 3 | 2 |
| 600 | 4 | 3 |
| 700 | 5 | 4 |
| 800 | 6 | 5 |
| 900 | 6 | 5 |
| 1000 | 7 | 6 |
| 1100 | 8 | 7 |
| 1200 | 9 | 8 |
| 1300 | 9 | 8 |
| 1400 | 10 | 9 |
| 1500 | 11 | 10 |
| 1600 | 12 | 11 |
| 1700 | 12 | 11 |
| 1800 | 13 | 12 |
| 1900 | 14 | 13 |

It will be noticed that, in the first table, as the exact times of the equinoxes and solstices vary in different years (according to the distance from leap-year), and also their local times vary in different places, the dates are for the mean, and usually apply to Europe.

W. T. LYNN.

[1] See the writer's "Celestial Motions," eleventh edition, p. 5.

## *AFRICAN ENTOMOLOGICAL RESEARCH COMMITTEE.*

THE subjoined official announcement of the appointment of an African Entomological Research Committee will be received with much satisfaction in all quarters where the importance of a scientific basis for administrative and other official action is duly recognised. Among the advances of biological science in the last few years, none has been more remarkable than the discovery that the cause of many diseases, the nature and origin of which had hitherto escaped detection, was to be sought in the presence of parasitic micro-organisms of various kinds and qualities in the tissues of animals and plants. The part played by insects and ticks in the dissemination of these morbific parasites is now known to be of immense importance, and great efforts have already been made, not without success, to restrict the occurrence of malarial and other disorders by the systematic destruction of the insect-carriers of the organisms concerned. For this purpose it is essential to distinguish with accuracy between various closely allied species; and it is here that the work of the skilled entomologist proves its necessity. It was well remarked by Mr. A. E. Shipley, in his recent presidential address at Winnipeg to the Zoological Section of the British Association, that "a few years ago no knowledge could seem so useless to the practical man, no research more futile than that which sought to distinguish between one species of a gnat or tick and another; yet to-day they knew that that knowledge had rendered it possible to open up Africa and to cut the Panama Canal." This witness is true; and it would be difficult to point to a more complete demonstration of the fact that natural knowledge pursued for its own sake, without any direct view to future utility, will often lead to results of the most unexpected kind, and of the very highest practical importance. It is this that justifies the demand that both governments and such private individuals as have the means should do all in their power to encourage the study and pursuit of science as science, without waiting for such applications as may prove to be of commercial or political value.

When the benefits to be derived from the scientific treatment of a subject are so manifest as in the present case, even the most indifferent of public bodies can hardly afford to stand aloof; and it is to be hoped that the activity of the Colonial Office in this direction, begun under the auspices of Mr. Chamberlain, and culminating for the present in the recent action of Lord Crewe, may be taken as an indication that the Government of this country is becoming increasingly alive to the desirability of securing the cooperation of scientific authorities in administrative measures. But beyond this, the movement will deserve a still greater welcome if it helps to emphasise the importance of encouraging the pursuit of genuine science, even when no immediate prospect is offered of material results. F. A. D.

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In view of the intimate relation which is recognised as existing between certain insects and the propagation of diseases of both man and animals in tropical Africa, and of the similar relation between insects and economic plants, which is becoming more evident as settlement in the continent progresses, Lord Crewe has appointed a Scientific Committee, the object of which will be to further the study of economic entomology with special reference to Africa.

This body will be known as the African Entomological Research Committee, and Lord Cromer has consented to act as chairman. The other members of the committee are:—Lieut.-Colonel A. W. Alcock, C.I.E., F.R.S., Mr. E. E. Austen, Dr. A. G. Bagshawe, Dr. J. Rose Bradford, F.R.S., Colonel Sir David Bruce, C.B., F.R.S., Dr. S. F. Harmer, F.R.S., Dr. R. Stewart MacDougall, Sir John Macfadyean, Sir Patrick Manson, K.C.M.G., F.R.S., Mr. R. Newstead, Prof. G. F. Nuttall, F.R.S., Prof. E. B. Poulton, F.R.S., Lieut.-Colonel D. Prain, C.I.E., F.R.S., Mr. H. J. Read, C.M.G., the Hon. N. C. Rothschild, Dr. D. Sharp, F.R.S., Dr. A. E. Shipley, F.R.S., Mr. S. Stockman, Mr. F. V. Theobald, and Mr. C. Warburton. Mr. A. C. C. Parkinson, of the Colonial Office, is acting as secretary to the committee, and Mr. Guy A. K. Marshall as scientific secretary.

Arrangements are being made to despatch a trained entomologist to the east side of tropical Africa and another to the west, for the purpose of stimulating official and other residents to collect and observe noxious insects, and of affording instruction in the use of scientific methods. By this means it is hoped to obtain throughout these territories an organised body of investigators who will communicate all their collections and observations to the committee. These collections will be classified by a number of British and in some instances Continental entomologists, and named specimens will be distributed to such institutions as may require them for purposes of instruction, both at home and in Africa. The committee will also keep in touch with the work which is being done in this branch of science in Egypt and the Sudan and in South Africa. The scientific results, including all observations and experiments made by the collectors, will be published from time to time in a journal or bulletin to be issued by the committee.

It is hoped that by such organised cooperation the knowledge of these matters will be materially increased, so as to render possible the application of effective remedial measures. Offers of cordial assistance have been received from such institutions as the British Museum (Natural History), the London and Liverpool Schools of Tropical Medicine, and the leading universities, in all of which valuable work has already been done in the same direction.

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## *THE BRITISH ASSOCIATION AT WINNIPEG.*

THE Winnipeg meeting of the British Association has been a complete success. Hundreds of citizens of Winnipeg, together with representatives of science in Canada, the United States, and Great Britain, filled the Walker Theatre when the presidential address was delivered by Sir Joseph Thomson on August 25. Mr. Francis Darwin, the retiring president, was unable to be present, but he sent a letter, which was read by Prof. Carey Foster, expressing the hope that the meeting would be worthy of its distinguished president. The addresses of presidents of sections were delivered on August 26 and 27, and we are able to print two of them this week, with a summary of the lecture delivered by Dr. A. E. H. Tutton on the former date. We have been forced, however, to omit parts of Prof. Armstrong's long address to the chemical section in order to find space this week for Dr. Smith Woodward's address to the section of geology.

The *Times* correspondent at Winnipeg reports that the proposal to create a separate section of agriculture, which at present is a subsection of botany, has been rejected. On Saturday, August 29, there were various excursions to places of interest. Nearly a hundred members visited Portage la Prairie, and were conveyed in motor-cars through the wheatfields, the trip covering a distance of thirty miles. The visitors were thus given the opportunity of seeing one of the finest wheat areas in the province, and of studying modern methods of garnering the harvest. The botanical section visited Winnipeg Beach, while the members of the engineering section inspected St. Andrew's lock, now being built by the Dominion Government in order to furnish uninterrupted navigation, *via* Red River, between Winnipeg and the lake bearing the same name. In Winnipeg itself a reception was given by Lord Strathcona at his former residence at Silver Heights, and was attended by about fifteen hundred people.

The *Times* also reports that one of the tangible results of the meeting of the Association in Canada has been the purchase by Dr. Gray, the Warden of Bradfield College, Berks, and president of the section of educational science, of a ranch of 2000 acres near Calgary. A competent Canadian has been appointed superintendent, and it is Dr. Gray's intention to afford an opportunity to Bradfield boys, on the completion of their school course, to acquire practical knowledge of the farming and ranching conditions of Alberta.

Dr. T. G. Bonney, F.R.S., will be president of the Association for the meeting to be held at Sheffield next year from August 31 to September 7.

## SECTION B.

### CHEMISTRY.

OPENING ADDRESS (ABRIDGED) BY PROF. H. E. ARMSTRONG, PH.D., LL.D., F.R.S., PRESIDENT OF THE SECTION.

AFTER an interval only a year short of a quarter of a century, it is my privilege again to occupy the chair of this section, and that, too, under conditions of special significance. The British Association has never before sought to carry the banner of science so far west into British Dominions—never before was it so clear that the progress of humanity is linked with the progress of science by an indissoluble bond: science defined in a word being *knowledge*, not mere work nor mere lip knowledge, but systematised established knowledge, not assumed knowledge—although hypothesis often serves to guide inquiry and truth is arrived at only gradually and slowly by a series of rough approximations. Moreover, science is true knowledge *of every kind*—there is too often a tendency to give a narrow interpretation of the word. One reason probably why the term does not produce any proper effect upon the average British ear is that it is not an English word but a mere adaptation from the Latin—a language which apparently cannot be engrafted upon our Saxon tissues, although, perhaps, it may be that we have so little feeling for it because we have been allowed to learn so little else in our higher schools; monotony of diet ever favours diminutive growth. Germans, I always feel, enjoy a great advantage over us in possessing the popular word *Wissenschaft*—in calling science the ***business of knowing***, the ***business of gaining wisdom***, of ***being wise***.

Naturally I am constrained on the present occasion to take stock of the position of our science, to draw a comparison between the condition of affairs chemical when we met in Aberdeen in 1885 and their present state. No like period of human history has been more fruitful of advance; at the same time, no period illustrates more clearly the difficulties that lie in the path of progress—because of the innate conservatism proper to human nature.

Before attempting to deal with any of the problems which concerned us at Aberdeen, I will first briefly pass the more salient features of advance in review. Few probably are aware how extraordinary is the command we now have of our subject. In 1885, in defending the tendency of chemists to devote themselves to the chemistry of carbon, I could speak of the great outcome of their labours as being the establishment of the doctrine of structure. Everything that has happened in the interval is in support of this contention. It is interesting that in a recent lecture[1] on the Physical Aspect of the Atomic Theory, the most prominent living exponent of physical theories has given a not unwelcome recognition of our right-mindedness in saying: "As time goes on it becomes increasingly difficult to resist the direct evidence for the simple view that, in many cases, chemical combination is not so much a fusion or intermingling of the combining atomic structures as rather an arrangement of them alongside one another under steady cohesive affinity, the properties of each being somewhat modified, though not essentially, by the attachment of the others; and that the space formulæ of chemistry have more than analogical significance." And again in the following passage, in which a far-reaching confession is made: "The aim of structural chemistry must go much deeper (than dynamical methods of treatment); and we have found it difficult, on the physical evidence, to gainsay the conclusion that the molecular architecture represented by stereo-chemical formulæ has a significance which passes beyond merely analogical representation and that our dynamical views must so far as possible be adapted to it." The remark made by Helmholtz in one of his letters, "that organic chemistry progresses steadily but in a manner which, from the physical standpoint, appears not to be quite rational," must be regarded as little more than a confession that he was out of his depth. When properly understood, nothing could be more rational and logical than the way in which our theory of structure has been gradually built up on an impregnable basis of fact, with the aid of the very simple conceptions of valency postulated by Frankland and Kekulé. Our security lies in the fact that the postulates of our theory have been tested in an almost infinite variety of cases and never found wanting; this is not to say they are applicable in all cases, but merely that whenever we are in a position to apply them we can do so without hesitation. Larmor refers to the habit of physicists of taking comfort in Helmholtz's remark; it will be well if instead they make themselves acquainted with our methods and with the results we have won, with a minimum of speculative effort, by the cultivation of an instinct or sense of feeling which experience shows to be an effective guide to action. Now that physical inquiry is largely chemical, now that physicists are regular excursionists into our territory, it is essential that our methods and our criteria should be understood by them. I make this remark advisedly, as it appears to me that of late years, while affecting almost to dictate a policy to us, physicists have taken less and less pains to make themselves acquainted with the subject-matter of chemistry, and especially with our methods of arriving at the root-conceptions of structure and of properties as conditioned by structure. It is a serious matter that chemistry should be so neglected by physicists and that the votaries of the two sciences should be brought so little into communion.

The central luminary of our system, let me insist, is the element carbon. The constancy of this element, the firmness of its affections and affinities, distinguishes it from all others. It is only when its attributes are understood that it is possible to frame any proper picture of the possibilities which lie before us, of the place of our science in the Cosmos. But, as Longfellow sings of the sea in his poem, "The Secret of the Sea," "Only those who brave its dangers comprehend its mystery"—only those who are truly conversant with the root-conceptions of organic chemistry are in a position to attempt the interpretation of the problems of our science as a whole or even to understand the framework upon which it is built up. And yet we continue to withhold the knowledge of the properties of carbon from students until a late period of their development; indeed, when I insisted recently that organic and inorganic chemistry should be taught as one subject to medical students,[1] I was told that it could not be; that the attempt had been made with disastrous consequences. I trust that ere long the futility of such an attitude will be generally realised.

It is remarkable how much our conceptions are now guided by geometrical considerations. The development by van 't Hoff of the Pasteur hypothesis of geometrical asymmetry has been attended with far-reaching consequences during the period under review, the completeness with which the fundamental properties of the carbon atom are symbolised by a regular tetrahedron being altogether astounding.

[1] The Wilde Lecture, 1908. By Prof. Larmor, Sec.R.S., Manchester Literary and Philosophical Society Memoirs.

[1] "The Reform of the Medical Curriculum" (*Science Progress*, January and April, 1907).

Our present conception is that the carbon atom has tetrahedral properties in the sense that it has four affinities which operate practically in the direction of the four radii proceeding from the centre towards the four solid angles of a regular tetrahedron.

More than analogical significance—to use Larmor's expression—must be accorded to this symbol on account of its remarkable accordance with the facts generally, whether derived from the study of asymmetric optically active substances or from observation of the activity of ring structures of various degrees of complexity. Nothing is more surprising than the completeness with which the vast array of facts included in organic chemistry may be ordered by reference to the tetrahedral model. In the future, when our civilisation is gone the way of all civilisations and strangers dig on the sites of our ruined cities for signs of our life, they will find the tetrahedron and the benzene hexagon among the mystic symbols which they have difficulty in interpreting; if, like the ancient Egyptians, we made our tombs records of our wisdom, such symbols would long since have acquired sacred significance and the public would probably have learnt to regard them with awe and to respect them as totems. Chemists might at least wear them on aprons in imitation of the Freemasons; perhaps no two other symbols have so great a significance—they reach into life itself.

It would seem that carbon has properties which are altogether special, the influence which it exercises upon other elements in depriving them of their activity is so remarkable. In their recent discussion of the relation of crystalline form to structure, in which valency is represented as a function of the volume sphere of influence exercised by an element, Barlow and Pope arrive at the remarkable conclusion that carbon is probably the only element the atom of which has a volume sphere of influence four times that of the hydrogen atom; although it combines with four atoms of hydrogen, silicon apparently has only half the volume sphere of influence of carbon. This may, in a measure, account for the very great dissimilarity in behaviour of the two elements, which is most pronounced in their oxides, the single atom of carbon all but dominating two atoms of oxygen in carbon dioxide (which is consequently gaseous), whilst the atom of silicon in silicon dioxide in no way eclipses the two atoms with which it is associated but leaves both charged with residual affinity which enables them to form complex collocations of remarkable fixity in the fire. At bottom the differences between organic and inorganic nature are to be regarded as very largely the expression of this difference. Ropes of sand are proverbially treacherous; yet without sand, if silica had been a gaseous substance, our world might have worn a strangely different aspect.[1]

The mineral world apparently owes its rigidity to the fact that the metals and certain other elements are so imperfectly capable of dominating oxygen that oxides generally polymerise with great readiness, giving rise to substances which do not even fuse easily. The organic, on the other hand, appears to be plastic by reason of the close approach to neutrality which is conditioned by association with carbon.

Nothing is more striking than the remarkable diversity of properties manifest both in the materials which at present we are content to call elements and in the compounds formed by their interaction; the range of variation met with in the case of the compounds of carbon with hydrogen and oxygen alone is almost infinite. We are almost compelled to attribute this diversity more to differences in the complexity and structure of the molecules than to differences in their material composition. The chemist, of necessity, must be a dreamer, knowing as he does that things are not as they seem to be. But this is not sufficiently remembered; indeed, students are systematically trained up in an atmosphere of pretence. The beginner is allowed to regard *elementary oxygen*, for example, as a colourless gas, which is generally harmless until things are presented to it in a more or less heated condition, whereat it takes umbrage and burns them up. He would regard elementary carbon as a soft black substance, which if smeared on the face of the white man makes him look like a nigger, were it not that he also learns that at times it is the hardest and whitest substance known; of organic chemistry, which alone can give him honest ideas of carbon, he is not allowed to hear, as I have said. The sting of awakening conscience is salved by the introduction of a long Greek word when he is told that the two substances, soot and diamond, are *allotropic* forms of the element carbon; nevertheless, he regards them both as elementary carbon. Gradually, perhaps, he awakens to a sense of the wrong that he has suffered at the hands of his teachers, as he realises that from no one substance can he gather what the properties of an element are, that after all the elementary substance is but an ideal—in other words, a mere concept. If appreciative, he then learns to think of the blandness of water, the sweetness of sugar, the sourness of vinegar, the causticity of soda, indeed every distinctive property of every known oxygen compound as more or less a property of, more or less conditioned by, the element oxygen; he is brought back, in fact, to the position from which Lavoisier started, as he realises that the oxygen *gas* which he inhales is not elementary oxygen; he can then perhaps appreciate the wonderful acumen which this greatest of chemical philosophers displayed when he wrote: "Nous avons donné à la base de la portion respirable de l'air le nom d'oxygène en le derivant de deux mots grecs ὀξύς, acide, γείνομαι, j'engendre, parce qu-en effet une des propriétés les plus générales de cette base est de former des acides en se combinant avec la plupart des substances. Nous appelerons donc gaz oxygène la reunion de cette base avec le calorique." We have allowed a century to pass without recognising the wonderfully accurate powers of prevision displayed by Lavoisier; what is worse, we have been so far led astray that instead of regarding oxygen as the characteristic and attractive elements in acids, hydrogen has been allowed to usurp the position: the extent to which the cult of the hydrogen ion now dominates the text-books is well known; in days to come, when the history of our times is written, it will be referred to as a remarkable example of chemical shortsightedness.

Names are needed for the elements which would serve to distinguish the ideal elementary substances from the forms in which they are known to us. No more appropriate name than oxygen could possibly be selected for the fundamental material; if the *gen* terminal could be applied to elementary materials generally, it would be an advantage; it would not be easy, however, if this were done, to devise an appropriate separate name applicable to the active constituent of air.[1]

[1] The solid model of silica which Barlow and Pope have constructed has very remarkable attributes, in that the oxygen atoms appear to be uniformly related and in intercommunication throughout its mass: so that a mass of silica, whatever its size, may almost be regarded as a single molecular complex. A similar view may be taken of the plastic metals such as those of the platinum group, gold, silver and copper. Whether when rendered brittle by association with small amounts of impurity these are resolved into simpler molecular complexes or whether the molecules merely become separated by substances which promote discontinuity and brittleness, it is impossible to say at present. The cause of hardness in mineral materials is, however, a question of no slight interest and importance. The property is strikingly exemplified in the diamond. It is difficult to understand the intense hardness of this material, on the assumption that the diamond is composed of paraffinoid carbon—that is to say, carbon with all its affinities satisfied. At present we appear to have no clue to the manner in which affinity acts in promoting the formation of such solids. But it is obvious that all solids are possessed of some degree of "surface affinity," as they not only grow when placed in solutions but determine the separation of solid from a solution at a degree of saturation which is often considerably below that at which the solution is actually saturated with the substance; and such surface affinity, moreover, is selective, as the determinative effect is exercised only upon the substance itself or substances isomorphous with it—although exception must be made in favour of water, which all surfaces appear to attract. Sir James Dewar's observations on the condensation of gases by charcoal at low temperatures afford most striking illustrations of surface affinity.

[1] In naming the inert gas in air, which he ultimately termed *azotic gas*, having proposed the name *azote* for the element, Lavoisier had in view as alternatives the terms *alcaligen* and *nitrogen*. As there was no proof that the element was a constituent of alkalies other than ammonia, he rejected the former name on the gronnd that it might convey too broad an impression; in course of time the latter is become the popular name, except in France, where motives of piety have prevailed; but the French practice has been justified by the universal use of the term *azo* in connection with many nitrogen derivatives.

Had Lavoisier realised that the alkalies and basic oxides generally owe their basicity to oxigen as much as acids and acidic oxides generally owe their acidity to oxygen—the one being oxygen tempered by metal, the other oxygen tempered by non-metal—as the number of basic oxides far outweighs the number of acidic oxides, he might well have chosen the name alcaligen rather than oxygen. The choice he made was a particularly happy one and striking evidence of his genius and sense of euphony—for oxygen is *par excellence* the acid-forming element and is most truly called *sour-stuff*, the stuff of which sour things are made—for whatever the properties of the

In 1885 I closed my address with a reference to the structure of the elements which implied that their behaviour was that of compound substances; the feeling that this is the case has long been general among chemists. Our present attitude towards this problem is a curious one and not altogether satisfactory—it is impossible to deny that we have somewhat lost sense of proportion, even if our methods have not savoured of the unscientific. The discovery of radium appears to have upset our balance—we have been carried away by the altogether mysterious and unprecedented behaviour of this weird and wondrous substance. But may we not ask: Is radium an element? Has it not been too generally, too hastily assumed that it is? Little as we know of it, does not its behaviour straightway outclass it as an element? Surely it does! Is not the established fact that an emanation proceeds from it, which in turn decomposes and gives rise to helium, a proof of its compound nature? Again, is the evidence of such a character as to justify us in asserting that uranium is the parent of radium? If it be such, must not uranium also disappear from the list of elements; must it not indeed be removed on the ground that it gives rise to uranium without any reference to its supposed relationship to radium?

The answers given to such questions must depend on our definition of an element. At present we seem to be without one.

The conception that the breakdown of radium is spontaneous and apart from all external impulse or control is also one which should be received with caution. There is reason to suppose that in all ordinary cases in which compounds undergo decomposition spontaneously, the decomposition is conditioned by an impurity; the effect, moreover, is usually cumulative. This is true of highly explosive substances, such as chloride of nitrogen and gun-cotton, for example. It might be supposed that something similar would happen in the case of radium—but apparently such is not the case; it is assumed that occasionally a molecule explodes spontaneously, not only without being incited thereto, but also without in any way affecting its neighbours.

The alternative explanation that radium in some way acts as a receiver, transforming energy from some external source to which ordinary substances fail to respond and being thereby stimulated to decompose, is at present out of favour, although perhaps more in accordance with its peculiar behaviour.[1]

The liberation of helium as a product of radio-active change is in itself a significant fact, in view of the possibility that helium may be an element of intense activity. Nothing in connection with the problem is more surprising, however, than the apparent production, in course of time, of a whole series of degradation products which differ greatly in stability—such behaviour is entirely without precedent and not at all becoming in elements.

No such remarkable and inspiring problem has ever before been offered for solution. We can only wonder at the results and admire the genius which some have displayed in interpreting them, Rutherford in particular. Yet outsiders may well hold judgment in suspense for the present: whilst it is permitted to workers to make use of hypothesis in every possible way in extending inquiry, the public are in no wise called upon to accept such hypothesis as fact.

But apart from the suggestion that elements may give rise to others spontaneously, we have been entertained of late with stories of elements being converted into others under the influence of the energy let loose by the breakdown of radium. There is reason, however, to suppose that the powers of radium may have been greatly overpainted; energy of almost any degree of intensity in the form of high-tension electricity is now at our disposal, and the effect which radium produces on living tissues, glass, &c., is of the same character as that effected by the Röntgen ray discharge, the only difference being that the effect is produced somewhat more rapidly; it is not to be imagined, therefore, that the discovery of radium has put any very novel intensity of power into our hands.

I pass to the consideration of the classification of the elements. The recognition of certain properties, the association of certain ideals with the several elements, is a necessary step in classifying the elements in accordance with Mendelejeff's great generalisation—or rather it may be said to be both involved in and an outcome of Mendelejeff's conception.

Until recently our difficulty was to understand the relationship of the metallic and the non-metallic elements; now we are confronted with another problem—that of the existence of inert "paraffinoid" elements. It is commonly assumed that these are monatomic, but the evidence on which this assumption is based is absolutely unconvincing, and would be generally admitted to be so were we in the habit of looking before we leapt to conclusions. Assuming that the elements are compounds, the formation of inert compounds does not appear to be out of place, in view of the existence of practically inert hydrocarbons. But, on the other hand, in view of the properties of nitrogen, which is one of the most active of substances in the monatomic state, although an inert gas in the diatomic condition, it may well be that the inertness of helium and the other members of the argon group is also simulated. Sir James Dewar's observations have shown that helium and charcoal have no inconsiderable affinity at the boiling point of the former, which is within five degrees of the absolute zero, the molecular heat of absorption (apart from that due to liquefaction) of helium at that temperature being apparently as high as about sixty calories. The proof he has also given that helium alone does not convey an electric discharge is also of significance since the passage of a discharge through it under ordinary conditions is an indication that it can be included with other substances in a conducting system. Such evidence as there is therefore points to the elements under discussion being different from the others only in the degree of stability of their molecules.

Of late years the difficulty of classifying the elements has been increased rather than diminished, not merely because of the discovery of the inert gases but also on account of the apparent impossibility of ordering the position of an element such as tellurium in accordance with its atomic weight. There appears to be little room left for doubt that the value cannot be far removed from that of iodine; it should be considerably lower. It may be pointed out that the accepted value of selenium is closer to that of bromine than would be expected if a relationship were maintained corresponding to that between chlorine and sulphur. It would seem that Mendelejeff's original conception of the elements as a simple series in which the properties are periodic functions of the atomic weights must be abandoned in favour of some more comprehensive scheme. From the chemist's point of view, it is impossible to abandon the guiding principle underlying the arrangement in family groups, which dates back to Dumas; perhaps insufficient attention has been paid in the past to the maintenance of this principle.

initial oxide of a series, as the proportion of oxygen is increased, the acidic qualities are invariably strengthened.

The choice of a terminal connoting the elementary radicles which would be applicable generally and also acceptable is very difficult. If usage do not forbid change, probably our ears will decline to allow us to be systematic. The terminal *gen* is not applicable to many present names. In the interest of euphony, exception may be taken to the adoption of *ion* as a final syllable. In English ears most of the words with this ending have an ugly sound if pronounced so as to make it significant; moreover, our object is to secure a term which is applicable to the elementary material, whatever its state; the term ion is suggestive of a particular state—a state of chemical activity; and at present there is no agreement as to the nature of an ion. The terms atom, radicle (simple and compound), ion and molecule now all have their separate meaning and value and are indispensable.

The only terminal which seems in any way likely to be generally satisfactory in use is the terminal *yl*, which is already applied to organic radicles; its use might well be extended to radicles generally.

[1] I may here put on record the opinion Lord Kelvin expressed on this question in a letter to me dated September 13, 1906:—

"Ever since, nearly four years ago, we heard of the hundred calories per hour given out by radium, I have had on my mind the question of some possible mechanism such as that which you suggest by which energy from surrounding matter (far or near) could automatically come into radium to supply the energy of the heat which it gives out. The more I think of the question the less I see of that possibility. At present I can see nothing else than that the energy given out is taken from a previously existing store of potential energy of repulsive force between separable constituents of radium.

"The 'disintegration of the radium atom' is wantonly nonsensical. It is nonsense very misleading and mystifying to the general public, because, if what is at present called radium can be broken into parts, it is not an atom.

"'Energy of an atom' implies a thorough misunderstanding of the meaning of the word energy, which is capacity for doing work.

"I admire most sincerely and highly the energy of the workers in Radio-activity and the splendid experimental results which they have already got by resourceful and inventive experimental skill and laborious devotion. I feel sure that as things are going on we shall rapidly learn more and more of the real truth about radium."

Taking into account this principle, it is impossible to arrange a long series of elements such as the rare earths continuously in order of atomic weight, as they would be brought into every family in the table by such a procedure: the difficulty has been got over by Brauner, who has proposed to arrange a large number of the rare earths in a single vertical series under barium. Biltz has made a similar proposal.

The principle had been advocated by me previously in an article written for the "Encyclopædia Britannica."[1]

In the arrangement I have proposed, it is not only assumed that there may be as many as sixteen vertical series of elements of which the elements from hydrogen to oxygen are initial terms, some series being at present unrepresented, it is also suggested that groups of elements occur in perhaps four of these series, numbers 4, 8, 12 and 16, the largest being that of the so-called rare earths in series 8.

The principle which is assumed to be in operation is that which is so clearly manifest in the case of hydrocarbons: successive vertical series of elements correspond to successive isologous series of homologous hydrocarbons. In the case of the hydrocarbons, the passage from one isologous series to another often takes place from a term several places removed from the origin of the series—for example, from benzene, $C_6H_6$, which may be regarded as primarily a derivative of hexane to naphthalene, $C_{10}H_8$, which is not an immediate derivative of benzene but of butylbenzene. It is conceivable that at the genesis of the elements a process was at work corresponding to that by which a hydrocarbon such as naphthalene is derived from benzene, and by which the former then serves in turn as the point of departure for more complex hydrocarbons of other series. There is no reason, from this point of view, why progression should not take place along a particular line and that terms should exist in a series through which this line passes but below it—for example, that antimony and iodine may bear a direct linear relationship, but that tellurium, instead of being the element in the progression series in the oxygen group, is a homologue of greater weight. The same view may be taken of selenium. In this way, it would be possible to maintain selenium and tellurium in the oxygen-sulphur series, from which they cannot well be separated, whilst retaining Mendelejeff's conception of a genetic relationship along the series. The only departure involved is in assuming that instead of forming a single linear series ascending regularly in spiral progression—a series which can, as it were, be strung on a single spirally wound cord—the elements closely simulate a series of homologous isologous hydrocarbons. From this point of view, it is easy also to understand that some vertical series are unrepresented.

In discussing the chief attributes of the elements none is so difficult to deal with as that of valency, using the term in the broadest possible sense, not merely as indicative of the number of units of affinity but as including the, at present, all but incomprehensible problems of residual affinity and elementary character. I discussed the subject somewhat fully in my former address, dwelling especially on the properties of negative elements and their power of acting as linking agents; this view has met with ample confirmation in the interval, and will, I believe, be found to be of wide application in the future. I have already referred to the manner in which it is exemplified by silica.

The greatest advance in the discussion of the problems of valency in recent years is that made by Barlow and Pope, as their method of treatment is one which applies to solid substances—the correlation of structure with crystalline form which it effects promises to be of far-reaching importance.

Apart from hydrogen, carbon is the one element of certain character, always acting as a tetrad—its affinities may be only incompletely satisfied but they are always exercised, it may be supposed, even in ethenoid and similar compounds; carbon monoxide apparently is the only exception to this rule, its relative inactivity being one of the most puzzling enigmas of our science, especially as the oxide becomes one of the most active of known substances when only two atoms of hydrogen are added to it. Most other elements (non-metallic) seem to vary in valency, the valency beyond a certain minimum being dependent on the nature of the association. Of late years, attention has been directed in particular to the quadrivalency of oxygen in many of its compounds.

[1] *Cf.* Roy. Soc. Proc., 1902, vol. lxx., pp. 86–94.

The quadrivalency of sulphur in substances such as trimethylsulphonium iodide, $Me_3SI$, having been proved to demonstration by the production of optically active compounds of this type (Pope and Peachey), it can no longer be supposed that in such cases we are dealing with compounds in which the negative constituents of the parent molecules are conjoined, *e.g.* MeI : $SMe_2$. And yet we must contemplate the existence of such compounds as possible—in the case of nitrogen, for example, as ammonia must be supposed to form the compound $NH_3 : OH_2$ in preference to the hydroxide $NH_4.OH$, the latter being only a very minor constituent, the former the major component of the aqueous solution of the gas; hydrogen chloride, on the other hand, appears only to afford one product with ammonia, viz. $NH_4.Cl$. The existence of such differences affords clear proof in the case of the non-metallic elements other than carbon that valency is not merely a variable but also a reciprocal or dependent function.

There is no reason to suppose that hydrogen ever acts otherwise than as a simple monad; and the behaviour of the alkalies and alkaline earths in salts would seem to justify the conclusion that they have no tendency to vary in valency, were it not for the existence of well-defined non-volatile hydrides of these metals which are clearly substances of some degree of molecular complexity. Such compounds are illustrations of the difficulties which surround the subject. It has long been clear that the exhibition of the higher valency by an element is a process of a different order from that manifest when it exerts only its lower proper valency measured in terms of positive radicles such as H or $C_nH_{2n+1}$ radicles. What that difference is we are not able at present to decide—carbon (together with silicon) differs from almost all other elements, especially in combining with hydrogen and analogous radicles to the extent of its maximum valency.

The proposition I made in 1888 (*Phil Mag.*, Series V., 25, 21) that the valency lines should, in some cases, be represented as passing through the atom, so that each is capable of acting in two directions, is the only consistent mode of expressing varying valency which has been devised, the only one, moreover, by which attention is directed to the great difference.

In many cases probably there has been a tendency to exaggerate the valency value—in the case of chlorine, for example, in assuming that it functions as a heptad in the perchlorates. In this and many other instances, it suffices to assume that the chlorine and oxygen atoms are united in a closed ring, the chlorine functioning as a triad. Some such explanation will doubtless be given of the structure of the metallic ammonias and similar compounds. The co-ordination values introduced by Werner serve only to establish certain empirical relationships and are useful for the purposes of classification. The perhaps more rational plan of dealing with such compounds suggested by Abegg has a similar value.

It is to the advantage of the hypothesis formulated by Barlow and Pope that the elements are represented as of constant valency in so far as their relative volume spheres of influence are concerned—the compound in which the higher valency is manifest being derived from that of lower valency by the opening out of the close packed arrangement and the insertion of certain new elements; but the fact that in such cases the volume is altered not in one direction alone in the crystalline structure but proportionately in all directions would seem to show that the volume sphere of atomic influence does actually change; the change is one, however, which affects all the atoms in the complex proportionately.

At present, unfortunately, our methods of treating the problems of valency are such that we cannot in any way give expression to the energy side of the phenomena.

Of late there has been talk of electrons in this connection, but what is said is little more than superficial paraphrase, in the advanced scientific slang of the day, of the ideas which have long been current. When, following Odling, we represent valency by dashes written after the elementary symbol, we give clear expression by means of a simple convention to certain ideas that are well understood by all among us who are versed in the facts; to speak of electrons and use dots instead of dashes may serve to mislead the unwary, who hang on the lips of authority, into a belief that we have arrived at an explanation of the phenomena, but those who know that we have reached only the let-it-be-granted stage and who feel that the electron is possibly but a figment of the imagination [1] will remain satisfied with a symbolic system which has served us so long and so well as a means of giving simple expression to facts which we do not pretend to explain. Not a few of us who listened to the discussion of the nature of the atom at Leicester could not but feel that the physicists knew nothing of its structure and were wildly waving hands in the air in the endeavour to grasp at an interpretation which would permit of mathematical interpretation being given to the facts. Until the credentials of the electron are placed on a higher plane of practical politics, until they are placed on a practical plane, we may well rest content with our present condition and admit frankly that our knowledge is insufficient to enable us even to venture on an explanation of valency.

In 1885 and again in 1888, I ventured to call in question the interpretation of valency which Helmholtz had given in the Faraday lecture in 1881. On the present occasion, I would insist still more emphatically on the insufficiency of the atomic charge hypothesis; especially that it affords no satisfactory explanation of variable valency and of those fine shades of difference which are manifest, especially in the case of nitrogen, when the radicle attached to the dominant element is varied. In 1885 I discussed this question with reference to the nature of electrolytes and questioned the conclusion Helmholtz arrived at that electrolytes belong to the class of typical compounds the constituents of which are united by atomic affinities, not to the class of molecular aggregates. The opinion I then ventured to give was as follows:—

"The current belief among physicists would appear to be that primarily the dissolved electrolyte—the acid or the salt—is decomposed almost exclusively. We are commonly told that sulphuric acid is added to water to *make it conduct*, but the chemist desires to know why the solution becomes conducting. It may be that in all cases the 'typical compound' is the actual electrolyte—*i.e.* the body decomposed by the electric current—*but the action only takes place when the typical compounds are conjoined and form the molecular aggregate*, for it is an undoubted fact that HCl and $H_2SO_4$ dissolve in water, forming 'hydrates.' This production of an 'electrolytical system' from dielectrics is, I venture to think, *the* important question for chemists to consider. I do not believe that we shall be able to state the exact conditions under which chemical change will take place until a satisfactory solution has been found."

The position is not very different now. Although the propagation of the ionic dissociation cult has assumed the form of a fine art, we are still as far as ever from agreement as to the nature of chemical change; the speculation has not helped us in the least to clarify our ideas; at most we learn that interactions are between ions, and even these, as a rule, are supposed to remain apart until they enter into the solid state. Throughout all these years I have never varied my opinion that the dissociation hypothesis is incompatible with the facts. On more than one occasion I have stated definite reasons which induce me to deny its usefulness,[2] and these arguments have never been met; in fact, there has been little but a conspiracy of silence on the part of the upholders of the creed.

A large amount of work bearing on the subject has been done, chiefly by H. Brereton Baker. Strangely enough, no proper notice of his results has been taken outside England, and even there the importance of the observations has not been sufficiently appreciated. Perhaps the most remarkable feature in the situation is that Baker himself scarcely seems to be alive to the meaning of the evidence which he has supplied; the attitude which he has displayed in his recent Wilde lecture can only be described as halting. Baker has shown, in case after case, that the occurrence of change is dependent on the presence of moisture, his greatest feat perhaps being the observation that it is possible not only to prepare nitrous anhydride in the solid and liquid states but to volatilise it unchanged if only water be excluded.

I venture to think there is only one point of view from which the problem of chemical change can be approached, that, namely, which we owe to Faraday—to which hitherto justice has in no way been done—on which I dwelt persistently in my previous address: that the forces termed chemical affinity and electricity are one and the same. In every case of chemical change there is a coincident electrical change, an electric flux; on the other hand, every case of electrical change is accompanied by chemical change, some alteration in molecular configuration is effected; the force of chemical affinity is in some way disturbed by a momentary displacement of the molecules when a current passes through a conductor. Such being the case, the conditions determinative of chemical change can only be those which permit of an electric flux. Two substances in apposition do not give rise to a current; at least three are required to determine a slope of potential. Chemical change can only take place if one of the three be an electrolyte. In all cases apparently the chemical change supervenes upon the electrical, the electrolyte being resolved into its ions, one of which at least combines coincidently with the adjacent electrode. Apparently these considerations are applicable to changes generally. And it should be added that, according to this view, the catalyst actually determines the occurrence of change.

The only other criterion which it is necessary to apply in order to decide whether change be possible in any given case is to consider if the change contemplated be one involving development of energy. It is important to remember also that a change which could not otherwise take place becomes possible when a suitable depolariser is introduced into the circuit.

The evidence that similar considerations apply to the gaseous and the liquid states cannot well be gainsaid. Before framing a theory of chemical change it is therefore necessary to formulate a definition of an electrolyte. It is doubtful if any single substance be an electrolyte; the conductivity of fused salts may well be and probably is conditioned by some admixture. Aqueous solutions of alkalies, acids and salts without exception are electrolytes. *Everything points to the fact that in such solutions the solvent and solute act reciprocally; the contention that the solute alone is active cannot be justified.* As water is altogether peculiar in its activity as a solvent and is a solvent which gives rise to conducting solutions, an explanation of its efficiency must be sought in its own special and peculiar properties.

Since 1886 this conclusion has been impressed upon me with indisputable force, and I have frequently ascribed the effect produced by the one constituent upon the other in a solution to the residual affinity of the negative elements in the two compounds which act reciprocally. It was only recently, however, that I saw my way to postulate a complete theory which would serve to account for the properties of solutions and generally that I realised how the reciprocal effect might be produced.

I would substitute for the misleading conception that liquids are comparable in their behaviour with gases the idea that the liquid state is one in which the residual affinity of the negative elements in particular always comes into play and causes the formation of molecular aggregates of various degrees of complexity; moreover, that the alteration in the properties of any given solvent by the dissolution in it of another substance is largely, and, in some cases, mainly due to a disturbance of the equilibrium natural to the solvent by an alteration in the proportion in which the several aggregates are present. The alteration in some particular property produced in *a given mass of the solvent* may, from this point of view, be taken as the measure of the activity of a substance, just as the alteration in the pressure of a particular volume is taken as the measure of the alteration produced in a gas. In the case

[1] In my opinion the experimental evidence is in no way satisfactory. It appears to me to be desirable that in studying the phenomena of electric discharge in gases and especially in vapours of complex substances, the horrible pitfalls should be taken into account with which the field of work is studded; unless every precaution to secure purity—precautions such as Baker and Dewar have taught us to use—be taken at every step, the conclusions based on all such observations must be open to grave doubt.

[2] Compare Chem. Soc. Trans. 1895, 1122; Royal Soc. Proc. 1886, xl., 268; 1902, lxx., 99; 1903, lxxii., 258; 1904, lxxiii., 537; 1906, lxxviii., 264; 1907, lxxix., 586; 1908, lxxxi., 80; *Science Progress*, April, 1909.

of non-electrolytes, if only a small amount of the solvent be withdrawn by combination with the solute, the alterations may be regarded as almost entirely due to the "mechanical" interference of the substance introduced, opportunity being given for the simpler, more attractive molecules of the solvent to exist in greater proportion because of the diminution of the chance of reuniting which is conditioned by the presence of practically inert molecules of another kind; if a more or less considerable amount of the solvent become associated with the solute the conditions become more complex, but similar considerations apply. From such a point of view a liquid is rendered more active by the addition of any soluble substance. Its vapour pressure is therefore diminished; the internal "osmotic" stresses are raised; its freezing-point is lowered.

Although it is generally admitted that water is not a uniform substance but a mixture of units of different degrees of molecular complexity, the degree of complexity and the variety of forms is probably underestimated and little or no attention has been paid to the extent to which alterations produced by dissolving substances in it may be the outcome and expression of changes in the water itself. The attempt to extend the "laws" which are applicable to the gaseous state to liquids has led us away from the truth by narrowing our conceptions. If the contention be justifiable that the alterations attending dissolution are very largely alterations in the character of the water, attention has been directed of late far too exclusively to the dissolved substance.

To give emphasis to the view, I have advocated[1] the restriction of the name *water* to the liquid mixture and have proposed that the simple molecule represented by the symbol $OH_2$ be termed *Hydrone*. The generalised expression

$$(H_2O)_x \rightleftharpoons xOH_2$$

may be considered to be representative of the state of equilibrium in water—that is to say, of the character of the change which it undergoes when the conditions are varied either physically or by dissolving substances in it—in the sense that it pictures the resolution of the more complex into simpler forms and *vice versâ*, without taking into account the variety of molecular forms ($x$, $x^1$, $x^2$ . . . .) which are present.

It is probable that the agreement between "theory" and practice on which reliance has been placed, particularly in interpreting osmotic phenomena, is more often than not only apparent and fictitious, and but the outcome of counterbalancing effects which have been left out of account. We are too prone to believe in constants; we need to remember that, except perhaps in the case of the perfectly gaseous state, *constants are dependent variables*. To take an example, it is assumed that glucose and cane sugar produce like osmotic effects when used in equivalent proportions; indeed, it has been the fashion of late years to treat non-electrolytes as harmless neutrals: in point of fact they differ as much in behaviour as do electrolytes, and such a conclusion must be viewed with the gravest suspicion. Recently Dr. Eyre and I have been able to show that three substances so similar as methylic, ethylic and propylic alcohols produce effects in precipitating salts from solution which are markedly different, propylic alcohol being the most effective although the least soluble. It is clear that the precipitant does not act mainly by itself combining with and withdrawing water in direct competition with the salt; but that it promotes the *dissociation of water* by the mechanical interposition of its molecules; in fact, that the "dehydrating" powers *of the water* are enhanced owing to the increase in the proportion of simple molecules in the liquid conditioned by the presence of the solute.

The same effect is obvious when the reduction of the electric conductivity of a salt, such as potassium chloride, by equivalent quantities of the three alcohols is considered. This amounts to about 6 per cent. in the case of methylic, 12 in that of ethylic, and 17 in that of propylic alcohol; the reduction effected by glucose, however, amounts to about 27, and that effected by cane sugar to no less than 42 per cent. In these two latter cases the amount of water actually withdrawn from the solution by the sugar is probably considerable, and the "mechanical effect" of the solute is therefore exercised in a more concentrated solution—more concentrated, that is to say, than those in which the alcohols act. If, therefore, solutions of glucose and cane sugar of equivalent strength produce like osmotic effects, it is because unperceived compensating factors are at work in the solutions which in algebraic sum have the same aggregate influence.

To explain the effect produced by substances which give rise to conducting solutions when dissolved in water (acids, alkalies, and salts), it is necessary to consider the special nature of the changes which may be supposed to attend dissolution in such cases. Why, it may be asked, is an aqueous solution of hydrogen chloride a conductor, whilst that of alcohol is a non-conductor? I believe the answer to be that it is because, in the former case alone, the two components of the solution are *reciprocally distributed*; that it is because two correlative systems—

$$HCl\Big\langle{}^{H}_{OH} \quad \text{and} \quad H_2O\Big\langle{}^{H}_{Cl}$$

are produced which interact under the influence of the electric stress.[1] In the case of alcohol no such interchange takes place. It may be that the alcohol is hydrolated to some slight extent, but the hydrol must be less basic than hydronol; probably, like ammonia, alcohol exists in solution for the most part in the hydronated state:—

| $H_2O\Big\langle{}^{H}_{OH}$ | $EtHO\Big\langle{}^{H}_{OH}$ | $EtHO{=}OH_2$ |
|---|---|---|
| Hydronol. | Ethanol-hydrol. | Ethanol-hydrone. |

Much more must be learnt of the properties of solutions before definite decisions can be arrived at with regard to such delicate and refined issues.

I would apply the interpretation here given of the nature of conducting solutions generally to the explanation of all cases of chemical change: in other words, I assume that in all cases correlative systems are present which are formed by the reciprocal distribution of the interacting substances. From this point of view the solvent is no mere medium but an active participant in the series of interchanges of which, as a rule, only the final product is noticeable.

The solution thus offered of the complex problem discussed very fully in my Address in 1885, which has ever since occupied my thoughts, will, I trust, be found to be helpful, although by no means complete in all its details.

In effect, the doctrine makes no demand which the chemist should not be able to grant forthwith, as it is generally supposed that hydrols are easily formed—to give an example, in the case of the conversion of chloral, $CCl_3.COH$, into chloraldehydrol (chloral-hydrate), $CCl_3.CH(OH)_2$. The novelty of the conception lies in supposing that the occurrence of electrolysis involves the interaction of the hydrol and its correlative and the explanation which it affords of the difference between electrolytes and non-electrolytes.

It is essentially an association theory, although it involves the dissociation of the interacting substances but never the production of separated ions. In the case of aqueous solutions the amount of the distributed substances may be taken as the measure of the activity—of the degree of ionisation, so-called. A wrong view prevails that the so-called molecular conductivities are measures of activity; they are in reality only measures of the relative activities under corresponding conditions of the substances to which they refer. The molecular conductivity of an acid is at a maximum in its weakest solutions: presumably it is then present to the maximum extent in its simplest state and in the active hydrolated state; but as a hydrolytic agent its activity is at a maximum near to the opposite end of the scale. In other words, the hydrolytic activities of a series of acids are in the order of their molecular conductivities in solutions of comparable strength, but molecular-hydrolytic and molecular-electrolytic activity run in opposite directions; the maximum electrolytic conductivity of an acid solution, which is manifest at a particular

[1] Roy. Soc. Proc. 1908, xxxi., 80; *Science Progress*, January, 1909.

[1] I would repeat the plea I put forward in 1885 that the use of the term hydrochloric acid as applied to hydrogen chloride is undesirable if not unjustifiable; the solution of the gas may be said to contain *chlorhydric acid*, $HCl(OH_2)x$. From my point of view, oxygen *is* a constituent of all acids.

degree of concentration—presumably at the point at which the two forms of the distributed materials most nearly balance—is also in no way identical with maximum molecular hydrolytic activity. On these assumptions not a few of the deductions based on the ionic dissociation hypothesis are clearly fallacious.

It has been asserted that the association hypothesis does not admit of quantitative treatment, and that therefore it is at a disadvantage; but if the quantitative meaning given to various results in accordance with the tenets of the dissociation hypothesis be more often than not one which is inadmissible, little is gained by applying the speculation quantitatively. As already remarked, the only cases in which chemical and electrolytic activity can be compared by the methods proposed are those in which the comparison is made between solutions of comparable or equivalent strength—that is to say, between compounds arranged in vertical series in the order of their activity.[1] Electrolytes are comparable in most, if not in all, their properties when the comparison is made in this way; but order of activity is one thing, actual activity another. It is in this sense, and this sense only, that we may agree with Arrhenius in his statement, "L'activité électrolytique se confond avec l'activité chimique."

The ionic dissociation hypothesis is a beautiful mare's-nest, which fails apparently to fit the facts whenever it is examined. "And the moral of *that* is," to quote the words of the classical Duchess so well known to children, "we must not use the words *ion* and *ionisation* in any speculative sense but confine their application to cases such as were contemplated by Faraday when he introduced the term ion; the conception of activity, whether electrolytic or chemical, should alone be attached to such words; no idea of actual, separate, individual existence should enter into our minds in using them: the ion is to be thought of merely as the potentially active, transferable radicle in a compound, not as a separated particle enjoying independent existence." It is so easy to speak of dissociation when it is desired to give expression to the idea; the first thing the scientific speaker or writer should guard against is ambiguity.

The subject of gaseous interchanges must not be left out of account, although it is impossible to do justice to it. Mendelejeff's contention that gaseous interchanges are usually bimolecular has been defended by Dixon and Larmor of late. But the facts must be faced. The almost inconceivable frequency of the molecular impacts must not be forgotten. The extraordinary attractive power of the hydrone molecule is also to be remembered—this would tend to promote the formation of aggregates with which the necessary third substance would every now and then form a bimolecular system—which, however, would in reality be at least trimolecular. The proportion of hydrone molecules in a dried gas has probably been under-estimated —the density of hydrone being very low (9)—as no dehydrating agent can be supposed to remove all such molecules or even nearly all; the hydrated substance must have a certain pressure of dissociation. Sir James Dewar's appears to be the only method which is in any way deserving of the epithet absolute; the results he has obtained with helium in a radiometer are strongly in favour of my view. Lastly, the gradual growth in velocity of the explosive wave up to the point of detonation as the compression becomes greater is clear indication that reduction in volume and increase of opportunity for the formation of systems of the proper degree of complexity is a matter of great consequence. Even the behaviour of cordite is significant, particularly the projection of unburnt rodlets of the material from the gun: apparently it is not decomposed by shock intramolecularly but is decomposed by heat into gases, which interact explosively.

Having dealt with the subjects of chemical change and the nature of solutions, however inadequately, I must now endeavour to justify my opening reference to the importance of the organic side of our science.

The province of organic chemistry is so vast that it may appear to be difficult to distinguish the main lines of advance from the by-paths which intersect the field of inquiry in every direction. In reality this is not the case; certain salient features stand out which must attract attention if once attention be directed to them. The efforts of the chemist to elucidate structure and to correlate structure with function have been extraordinarily successful. In the first place, as already remarked, the student of the subject now has his attention concentrated on the tetrahedron as the symbol of the *functional activity* of carbon; however numerous the compounds, he knows that certain simple rules can be laid down as applicable to all. It is established beyond question that carbon atoms have a remarkable tendency to form ring systems. The affinities of the atom seem to act almost rigidly in certain directions, which appear to be those of lines drawn from the middle point of a regular tetrahedron to its angular points. Rings containing either five or six atoms of carbon are therefore those which are most readily formed and of maximum stability; carbon atoms may and do unite in pairs, threes and fours, but compounds of this order are far less permanent than those containing either five or six atoms, as the affinities appear to meet in such a manner that they do not satisfy each other and consequently the compounds enter somewhat readily into combination with other substances. When the number of carbon atoms exceeds six, not only is there less tendency to form a ring system but the stability of the system is slight; when the number is considerable, stability is attained by the formation of complex systems, consisting of several rings conjoined (camphor, naphthalene, anthracene, &c.).

The behaviour of carbon compounds generally, in so far as this may be regarded as dependent on the condition of the carbon, is extraordinarily simple, and may be summed up in the statement that it is either paraffinoid, ethenoid or benzenoid.[1] Paraffinoid carbon is incapable of combining with other substances and but slightly attractive, so that the hydrogen atoms are by no means easily displaced from paraffinoid compounds[2]; ethenoid carbon combines readily with various substances, forming compounds of paraffinoid type; in the benzenoid state, carbon appears to combine somewhat readily with a variety of substances, but the products enjoy only an ephemeral existence and usually escape notice, as they at once break down, giving rise to benzenoid substitution derivatives, so that in this last form carbon simulates the paraffinoid state but is more active.

In earlier years our attention was concentrated on benzene and the benzenoid compounds; much was done to elucidate the structure of these hydrocarbons and of their derivatives; meanwhile these latter have proved to be of extraordinary significance technically, notably as dyestuffs, but also on account of their medicinal value, as perfumes and in photography.

The structure of benzene has been the subject of much discussion during the period under consideration. I trust I shall not be accused of parental bias if I urge that the centric[3] formula is the best expression of the *functional activity* of the hydrocarbon benzene and its immediate derivatives; the attempts which have been made of late years to resuscitate the Kekulé oscillation hypothesis in one form or another appear to me to be devoid of practical significance. Any formula which represents benzene as an ethenoid must be regarded as contrary to fact. But in considering the properties of benzenoid compounds generally, it is necessary to make use of the Kekulé conception as well as the centric expression. The model of benzene devised by Barlow and Pope subserves a somewhat different and complementary purpose, being primarily of importance

[1] Solutions of acids and alkalies have maximum conducting power at certain relatively high degrees of concentration. Hydrolytic activity also increases steadily in the case of acids as the solution becomes more concentrated; whether it attains to a maximum and whether this coincides with the conductivity maximum is uncertain at present; it is very difficult to decide this point experimentally, as the rate of change is so rapid in strong solution; moreover, the action takes another course in strong solutions, as compounds are formed by the interaction of the hydrolyte and hydrolist, so that two changes are superposed which cannot well be followed separately.

[1] A fourth condition requiring recognition is that of the carbon in acetylene; at present the acetylene compounds are so few in number, however, that this form may be left out of account.

[2] The displacement of the hydrogen associated with carbon is in all probability a secondary phenomenon; it is likely that this is true generally and that hydrogen is never merely removed or attracted away but always has its place taken by a radicle which becomes temporarily attached to the multivalent atom with which the hydrogen is associated.

[3] I have discussed this matter somewhat fully in a recent essay, with reference to the nature of amorphous carbon, in connection with the remarkable work of Sir James Dewar on the absorption of gases by charcoal at low temperatures (*Journal of the Royal Institution*).

on the geometric side in discussing the relation of form to structure.[1]

The discovery of trimethylene by Freund and the subsequent introduction of synthetical methods of preparing polymethylenes by W. H. Perkin, jun., mark the onset of a new era, opening out as they did the possibility of understanding the structure of camphor and the terpenes and other constituents of the volatile oils from plants.

Chemist after chemist had attempted in vain to solve the riddle presented by camphor. Suddenly, in a moment of inspiration, a satisfactory solution of the problem was offered by Bredt. The acceptance of the bridged ring, the special feature of the Bredt formula of camphor, marks the introduction of a new moment into organic chemistry.

The recognition of similar rings in several hydrocarbons of the terpene class, mainly in consequence of the masterly work of von Baeyer, has contributed in no slight degree to an understanding of these compounds; nevertheless, much remains to be learnt and there are many and serious difficulties to be overcome before we shall be in a position to appreciate the genetic relationship of all the substances included in the group. When the account of the work is written it will form one of the most striking and fascinating chapters in the history of our science.

Among the many names of those who have contributed to its development the first to be mentioned is that of Wallach, to whose unwearied efforts, continued during a long series of years, so much is owing. The synthetic work carried out with brilliant success in recent years by W. H. Perkin may also be referred to as of extraordinary promise but of well-nigh inconceivable difficulty.

Before leaving this chapter, reference should be made to the almost protean character of camphor, as disclosed by the work of inquirers such as Kipping, Pope, Forster, Lapworth and Lowry; no other substance has lent itself to use in quite so many directions and with such fruitful results. Special mention may be made of the demonstration which Pope has given, with the aid of the camphor-sulphonic acids, that nitrogen, sulphur, selenium and tin give rise to optically active substances in all respects analogous to those furnished by carbon. The success with which Kipping's arduous labours have been crowned is also very noteworthy, taking into account the many difficulties he has overcome in preparing optically active silicon compounds. The extension of the Pasteur-van 't Hoff theory of asymmetry inferentially to all elements which are at least quadrivalent, now accomplished, is of superlative importance.

Lowry's refined observations on the conditions which determine the interconversion of isodynamic forms of some of the camphor derivatives may also be cited as of special value as a contribution to the study of metamerism and the conditions which determine chemical change generally.

Not the least interesting feature of camphor is the light thrown by its behaviour on the influence which oxygen exercises as an attractive element and on the part which spatial configuration may play in determining directions of change. It is clear that, whatever the agent, the attack is always delivered from the oxygen centre and that the direction in which the attack becomes effective depends on the position which the agent can take up relatively to the various sections of the molecule.[1]

It must be confessed that our efforts to penetrate behind the veil in the case of the higher carbohydrates—starch and cellulose in particular—have not been rewarded with success.

Moreover, though much has been done of late years to unravel the nature of the vegeto-alkaloids, substances such as quinine are still only partially deciphered and not one of the more complex alkaloids has been produced synthetically. In view of the fact that quinine is still the one effective and practically safe anti-malarial medicine, the disclosure of its constitution is much to be desired. The isolation of adrenaline from the suprarenal capsule and the discovery that this alkaloid—which is an extraordinarily active substance physiologically—plays a most important part in controlling vital processes is of supreme interest. Other glands—the pituitary gland, for example—appear to contain peculiar active substances, which are of particular consequence in regulating animal functions. The discovery of such substances affords clear proof that life is largely dependent on what may be termed chemical control.

In addition to indigo, the simpler yellow and red natural colouring-matters have now been thoroughly examined, but this class of substance still affords abundant opportunity to investigators. Kostanecki's comprehensive studies of the xanthone group may be referred to as of particular value.

Attention may be directed here to the investigation of brazilin and hæmatoxylin by W. H. Perkin and his various co-workers, not merely as being full of interest and importance as a contribution to our knowledge of the relation between colour and structure and as a brilliant example of technical skill but because of the illustration it affords of the extreme intricacy of such inquiries and of the vast amount of labour they entail. The general public probably has not the slightest conception of the difficulties which attend such research work and of its costliness.

As an investigator of vegetable colouring-matters, no one has been more assiduous or has displayed greater skill of late years than A. G. Perkin. His recent refined investigation of the colour-yielding constituents of the indigo plant is of exceptional value at the present time, although it is to be feared that it comes too late to save the situation in India. The work of the brothers Perkin, it may be pointed out, is of exceptional interest on the human side as well as from the scientific standpoint, as their enthusiasm and wonderful manipulative skill afford a striking and noteworthy example of hereditary genius.

Two substances of commanding interest which have long resisted attack—the red colouring-matter of the blood and leaf-green—are at last going the way of all things chemical, as the secret of their nature is being wrung from them. In Willstätter's skilful hands chlorophyll is proving to be by no means the fugitive material it was supposed to be; the complexity of the problem it offers, however, seems to be far beyond anything that could have been anticipated; so much greater will be the interest attaching to the final solution. The discovery that green chlorophyll is a magnesium salt is of special importance, as the first clear indication of the manner in which magnesium salts are of service to plants.

Apart from the special interest which attaches to the investigation of vegetable colouring-matters on account of their being coloured substances, such inquiries are of value as furnishing material for the discussion of the metabolic activity of plants.[2]

[1] The time is now approaching when it will be possible to extend the study of benzenoid compounds beyond the formal and superficial stage; hitherto we have been content to develop the methods of preparing such substances and to determine their number and their distinctive properties. Everything has to be learnt as to the exact character of the changes which attend their formation from the parent substance benzene and as to the exact nature of their inter-relationship. The impression produced by benzene, in my mind, is that of an eminently plastic system capable of responding to every slight change that may be impressed upon it. Nothing is more remarkable than the difference between benzene and its homologues, so obvious in the extraordinary increase in activity which attends the introduction of hydrocarbon radicles in place of one or more hydrogen atoms. But such plasticity is not characteristic of benzene only: if the properties of benzenesulphonic acid be contrasted with those of the various substituted benzenesulphonic acids, it is clear that every variation meets with some response from the sulphonic group; what is still more remarkable, if the hydrogen in the hydroxylic group in the phenolsulphonic acids be displaced by other radicles, not only does the oxygen atom to which the radicle is attached seem to respond to the change but the benzenoid system and the still more distant sulphonic system are also affected. It is well known that the physical constants are all variables in the case of benzenoid compounds. Perhaps the most remarkable confirmation of the view here advanced, however, is that afforded by the conclusion arrived at by Barlow and Pope that in the case of benzene derivatives, although the spheres of influence of the carbon and hydrogen atoms are relatively the same as in the parent compound, the spatial arrangement of the component spheres of atomic influence remaining practically unchanged, nevertheless the actual volumes of the spheres of influence of both carbon and hydrogen alter proportionally to the alteration in molecular volume. Thus they maintain that in the case of the conversion of benzene (molecular volume 77·4) into tetrabromobenzene (molecular volume 130·2), the volumes of the spheres of influence of both carbon and hydrogen expand in the ratio of 77·4 : 130·2. Such a conclusion is very noteworthy.

[1] *Cf.* Chem. Soc. Trans.

[2] But a note of sadness pervades the story. The effect of learning to understand Nature always appears to be that we at once brush her aside when we have wrested from her the secrets which she has so long preserved inviolate. No sooner did we learn the nature of the madder colouring-matters than we proceeded to prepare them artifically—thus putting an end to the cultivation of a valuable crop. Indigo is meeting with a like fate, a catastrophe which might well have been avoided had scientific assistance been called in at the proper time. Not content with making natural colouring-matters, we set to work to outrival the rainbow in our laboratories and the feminine world is decked with every variety of colour in consequence, although unfortunately our blends too often lack the beauty of those of truly natural origin, which rarely, if ever, offend the eye. We congratulate ourselves on our cleverness in thus imitating Nature but no idea of thrift

Even colloids are being brought into line. Studded as they are with active centres (oxygen or nitrogen atoms), they seem to be able to attract and retain hydrone molecules at their surfaces in ways which give them their peculiar glue-like attributes: as a consequence living tissue appears to be little short of animated water.

To the present generation of students, the organo-metallic compounds must have appeared to belong to the past; the discovery of methides of platinum and gold by Pope will not only serve to re-awaken interest in this group of compounds but is of primary importance as a contribution to our knowledge of the valency of these elements; the stability of the platinum derivatives is altogether astonishing.

The discovery announced in June last, at the International Congress of Chemistry, by Mond, of compounds of carbonic oxide with ruthenium and uranium is a striking and most welcome extension of his previous labours, which had placed us in possession of carbonyls of nickel, iron and cobalt. The metallic carbonyls possess altogether remarkable properties: at present, these defy explanation; nickel carbonyl in particular seems to be an exception to all rules. The complex iron carbonyls made known by Dewar and Jones also have most fascinating attributes, the variety of colours they display being specially interesting. The marked individuality of the members of the iron group as exemplified in their carbonyl derivatives is in striking contrast with the tendency they display to behave as related elements; the deeper problems of valency are clearly exposed for consideration in such peculiarities.

The discoveries of the special activity of magnesium as a synthetic agent and of the superior value of nickel as a catalyst in fixing hydrogen are other illustrations of the individuality of metallic elements. We are greatly indebted to the French chemists for the invaluable preparative methods they have based on the use of these two agents.

Although satisfactory progress has been made in almost every direction, many of the nitrogen compounds are still not properly understood. It is clear that we are as yet in no way seized with understanding of the attributes of this element as we are of those of oxygen and carbon, particularly in the case of mixed carbon-nitrogen compounds: we can make nothing of the physical data such substances afford. Nitrogen, in fact, is an extraordinary element, far more remarkable than any other; its "temper" appears to vary more than that of any other element according to the character of its associates—nothing could be more remarkable, for example, than the change in properties from ammonia, $NH_3$, through hydrazine, $NH_2.NH_2$, to azoimide, $N_3H$. No other element can be so poisonous, so immediately fatal to life. We lack a model symbolic of its functions—which means that we are unable to fathom its vagaries and reduce them to simple order.

The oximes and the diazo-compounds in particular have given rise to much dispute. Stereo-chemical formulæ have been assigned to these, but probably they have little relation with the truth; although they have been of service by supplying symbols which can be offered up at examinations, by confining attention they have served to sterilise inquiry. No better illustration could be given of the truth of the remark made by my friend the Professor that man is an idolater by nature, a fact that chemists should always bear in mind.

The compounds in question are difficult substances to handle, far too prone to undergo change without invitation—it is to be feared that many of the conclusions which have been arrived at are based on incomplete if not unsatisfactory evidence.[1] When I think of the state of our knowledge, I am reminded of the father of diazo-chemistry, Peter Griess, and of his marvellous experimental gifts; there is great need of such a man to re-investigate the whole subject.

If we inquire as to the general effect of the increase of knowledge of organic compounds, it is clear that the lessons which emerge from all modern inquiries are such as to justify Larmor's remark that our conceptions of structure must be granted more than analogical significance. Everything tends to show that function and structure are most closely connected—odour, taste, colour, physiological effect, are specific rather than general properties, each conditioned in its special variety by some special structure; we are approaching very closely to a time when it should be possible to discuss such properties with considerable confidence.

Still, it must not be forgotten that the problems they offer are all valency problems, and that the nature of valency eludes us entirely at present.

The greatest advance which chemists may pride themselves upon having made during the past decade or two remains to be considered. In 1885, I spoke as follows:—"The attention paid to the study of carbon compounds may be more than justified both by reference to the results obtained and to the nature of the work before us; the inorganic kingdom refuses any longer to yield up her secrets—new elements—except after severe compulsion; the organic kingdom, both animal and vegetable, stands ever ready before us. Little wonder, then, if problems directly bearing upon life prove the more attractive to the living. The physiologist complains that probably 95 per cent. of the solid matters of living structures are pure unknowns to us, and that the fundamental chemical changes which occur during life are entirely enshrouded in mystery. It is in order that this may no longer be the case that the study of carbon compounds is being so vigorously prosecuted. Our weapons—the knowledge of synthetical processes and of chemical function—are now rapidly being sharpened, but we are yet far from ready for the attack."

My forecast has been more than justified; indeed, the advance to be recorded is nothing short of marvellous: the great problems of vital chemistry appear now no longer to be unattainable to our intelligence—their cryptic character seems to have disappeared almost suddenly. Many have contributed in greater or less degree, but none in such measure as Emil Fischer, whose work both in the sugar group and in connection with the albuminoids must for ever rank as monumental.

It is difficult to appreciate the extent to which the practical genius of this chemist has carried us—difficult alike for those who understand the subject and those who do not; the significance of his labours is only apparent when the bearing of his results on the interpretation of vital phenomena is fully considered. In 1885 we were disputing as to the structure of substances such as glucose and galactose; now we not only are satisfied that they belong to the group of aldhexoses (aldoses) derived from normal hexane, but, taking into account the monumental discoveries of Pasteur, to which precision has been given by van 't Hoff's great generalisation, we are in a position to assign fully resolved structural formulæ not only to the natural products but to the nine other isomeric aldhexoses which Fischer has prepared artificially.

It is a striking fact that only three of the sixteen possible aldhexoses and but a single ketohexose (fructose), of which many are possible, are met with naturally. Nature is clearly most sparing, most economical, in her use of materials. And not only is this true of the hexoses, as very few of the possible lower and higher homologous carbohydrates occur in vegetable or animal materials and the condensed carbohydrates (cane sugar, starch, &c.) are all formed apparently from the hexoses and pentoses which occur naturally. The albuminoids, the alkaloids, the terpenes are also optically active substances; in other words, only a limited number of the possible forms are present. There is reason to suppose that the compounds of natural occurrence stand in close genetic connection and belong with few exceptions to the same series of enantiomorphs; in no other way is it possible to account for the occurrence of one only of the pair of enantiomorphous isomerides and for the relatively small number

possesses us: moreover, our attempts to imitate if not to undo her work are never direct but are always made with her aid, with Nature's product—coal; we are no longer content to ride on horseback but must rush through space and instead of watching the birds fly seek to emulate them but always with the aid of fuel won by Nature from the soil and air in days long past. Too much is being done in every direction to waste natural resources, too little to conserve them, too little to employ man in his proper place—as tiller of the soil. Here lies the chemist's opportunity. At no very distant date, perhaps, when petrol is exhausted, toll will be taken from the sun in the form of starch or sugar and this will be converted into alcohol.

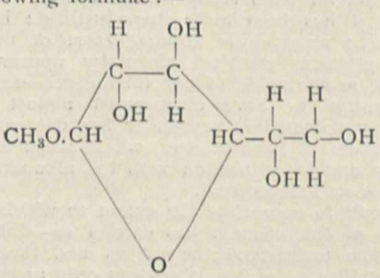
[1] Since this was written, Thiele's discovery of "Azomethane," MeN : NMe, has been announced. This is described as being, in the solid state, a distinctly coloured, very pale yellow substance. There can be little if any doubt, therefore, that, as Robertson and I have argued, the colourless so-called syn- and antidiazo-salts cannot possibly be compounds of the —N : N— or diazene type; such compounds would all be at least yellow in colour.

of compounds. Moreover, not only the sugars and most of the other products of the disintegration of the albuminoids but also the amino-acids, in like manner, are derivatives of compounds containing at most six atoms of carbon; the fats alone are of a considerably higher degree of complexity but they are probably collocations of the simpler units.

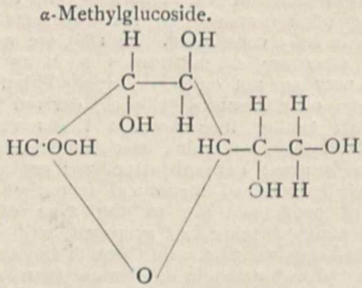
The terpenes and essential oils are mostly $C_{10}$ derivatives; the alkaloids have complex formulæ but the units of which they are composed are simple; as all of them are optically active, it is clear that only some of the possible enantiomorphous combinations are present.

We are bound, therefore, to assume that a large proportion of the changes which occur in living organisms—which constitute vital metabolism—are directed changes. What is the nature of the directive power? We are already able to go far in explaining this, although our knowledge is mainly of analytical changes, the nature of synthetic changes being, at present, only inferentially disclosed to us.

It has long been known that under natural conditions many complex compounds such as starch, cane sugar, and other similar substances are broken down hydrolytically, not by the unassisted action of water but by the cooperation of enzymes; the effect produced by these enzymes is precisely similar to that of acids, except that all acids produce the effect, acting only with different degrees of readiness, whilst enzymes are strictly selective, a given enzyme acting only, as a rule, either on a single substance or on a series of substances similar in structure. Indisputable evidence has been obtained that the enzymes which act on the carbohydrates are intimately related in structure to the compounds which they attack, fitting them—to use the apt simile introduced by E. Fischer—much as a key fits into a good lock: the slightest alteration in the structure of the carbohydrate is sufficient to throw the enzyme out of action. The closeness of the association is well illustrated by the case of the two methylglucosides, which differ merely in the manner displayed by the following formulæ:—



α-Methylglucoside.



β-Methylglucoside.

The relative positions of the single hydrogen atom and of the hydroxyl group attached to the carbon atom are merely interchanged, but this is sufficient to render the one (the α) proof against the action of emulsin, the enzyme of the almond; the other (the β) proof against that of maltase, the enzyme present in yeast.

The enzyme may be pictured as attaching itself to a surface of the molecule, and at the same time as associated with hydrone in such a manner that this is brought to bear at the junction which undergoes disruption. The action of acids, although similar, is simpler in that the attachment is not to the molecule as a whole but only at, or near to, the junction which is resolved.

In the case of the albuminoids, the action is probably more local in character, in so far as the resolution of their polypeptide section is concerned, the same enzyme being able to effect the resolution of a considerable number of compounds.

All the peptolytes have in common the junction C.CO.N; the peptoclasts by which such substances are gradually resolved probably fit this group alone; but other enzymes are of more complex organisation, akin to that of the sucroclasts—such as arginase, for example. In principle, however, the enzymes are to be regarded as all acting alike, each as fitting some particular asymmetric centre if not the whole of the molecule which undergoes hydrolytic disruption under its influence, the asymmetric centre being that at which the cleavage is effected.

In synthetic changes the operation is reversed. It may be supposed that the separation of hydrone is determined by the circumstance that water can be formed by the interaction of this hydrone as it separates with that which is attached to the hydrolated enzyme; the formation of water, in fact, plays a great part in such changes.

The action of oxydases may be regarded from a similar point of view. The early observations of Adrian Brown on the oxidising activity of *Bacterium Xylinum*, coupled with the later work of Bertrand, afford clear proof that these enzymes are possessed of selective powers. It is conceivable that such enzymes become attached to a molecule at some one centre, and that they then deliver their attack at some more or less distant point by presenting the oxygen with which they are loosely associated at this point. It is easy, on such an assumption, to understand how ethenoid linkages may be developed in various positions in the molecule of a fatty acid.

Rosenthaler's recent observations on the formation of optically active phenyl hydroxycyano methane,

$$PhCH(OH)CN,$$

from hydrogen cyanide and benzaldehyde on shaking the solution with emulsin are, however, among the most significant yet made. I have myself confirmed his statements. The ease with which the change takes place—the manner in which the change is accelerated by the enzyme—is altogether remarkable.

Although there can be little doubt, in the case of plants and animals, that the synthetic processes do not occur spontaneously and directly between the interagents, but are for the most part at some stage or other directed or controlled, it cannot well be supposed that every asymmetric compound is the direct outcome of a controlled process; nor is it necessary to assume that such is the case. Not a few asymmetric compounds are probably but secondary products formed by the breakdown of compounds which are the products of directed synthesis.

Ehrlich's observations on the formation of the amylalcohols from the isomeric aminocaproic acids (leucines) may be referred to in this connection. Taking into account the manner in which the vegetable organism is provided with conservative powers and its tendency to retain nitrogen, in view of the peculiar structure of the members of the terpene group—especially the presence of the isopropyl group and of methyl in association with the ring in such hydrocarbons—it is highly probable that the terpenes are derived from amino-acids. A molecule of leucine, a molecule of alanine and a molecule of formaldehyde obviously provide the materials for the production of methylisopropyldihydrobenzene; it is not difficult to picture the series of changes which would lead to the formation of the hydrocarbon from such a conjunction.

The general impression produced by facts such as have been referred to is that directive influences are the paramount influences at work in building up living tissues. These come into operation, it is to be supposed, at a very early stage in the case of the plant. The initial step probably involves the electrolysis of water under the influence of solar energy and the reduction by the hydrogen thus liberated of the carbon dioxide, which is eventually converted into formaldehyde, either directly or, it may be, through the intermediation of oxalic and formic acids. The part which chlorophyll plays in this process can only be surmised: it is not improbable that reduced chlorophyll is the active reducing-agent: that chlorophyll itself is active in conditioning the resolution of water under the influence of solar energy into reduced chlorophyll and oxygen or, more probably, a labile peroxide, from which oxygen is independently split off at a subsequent stage, it may be under the influence of a so-called catalase.

Whatever the process by which the plant acquires its initial store of carbonaceous material, the formaldehyde is apparently at once made use of and, in part at least, converted into starch. The view may be taken that glucose is the primary product of condensation—that the formaldehydrol molecules become ranged against a glucose template in series of sixes, which are soldered by enzymic influence into a single molecule by the interaction of contiguous hydrogen and hydroxyl radicles along the chain.

The glucose is thereafter carried a stage higher and converted into maltose or it may be that a maltose template is effective from the beginning and that the biose is the immediate product of condensation; the conversion of maltose into starch must take place in some similar manner. The recent observation that cellobiose is a β-glucoside enables us to realise that the formation of cellulose differs from that of starch in that the glucose molecule, instead of being converted into the β-glucoside maltose, becomes changed into the correlated β-glucoside, a membrane being thus secured which can resist the diastic enzymes by which starch is attacked.

The formation of the albuminoid substances may be regarded from a similar point of view. At present, however, there is no satisfactory evidence to show at what stage nitrogen is introduced into the molecule. As the plant takes up nitrogen in the form of nitrate, not as ammonia, it is probable that the nitrate is reduced to hydroxylamine and that this, rather than ammonia, is the active synthetic agent. Formaldehyde and hydroxylamine would yield formaldoxime, which would easily pass into methylamine on reduction; the interaction of formaldoxime and formaldehydrol might give rise to a higher aldoxime which would be easily convertible into amino-acetic acid (glycine). Higher glycines might be formed from glycine by syntheses similar to those Erlenmeyer has effected; but to account for the formation of asymmetric amino-acids it is necessary to assume that the action is controlled at this stage and that the glycine is formed against a template perhaps under the influence of an enzyme.

Another conceivable mode of formation is by the fermentative degradation of glucosamine.

Until we know more of the order in which the amino-acid radicles are united in the various albuminoids and of the character of the associations other than those which are characteristic of polypeptides, we can consider the formation of albuminoids only from a very general point of view; but taking into account the very different proportions in which amino-acids and other cleavage products are formed on hydrolysing substances of different origin, it is clear that the several sections of the molecule must be differently ordered in the different proteins; again, therefore, it is necessary to assume that the formation of such substances is directed. We may picture molecule after molecule as being "brought into line" against a template and the junctions which are required to bind the whole series together as being made through the agency of the enzymic dehydrating influence before referred to.

Attention has been directed to the relatively simple way in which the hydrocarbons are constructed, that even the paraffins are not to be visualised as so many ducks strung upon a ramrod, Münchausen fashion, but as forming curls, owing to the natural set of the affinities. This probably is true of complex substances such as the proteins.

Protoplasm, in fact, may be pictured as made up of large numbers of curls, like a judge's wig—all in intercommunication through some centre, connected here and there perhaps also by lateral bonds of union. If such a point of view be accepted, it is possible to account for the occurrence in some sections of the complex series of interchanges which involve work being done upon the substances brought into interaction, the necessary energy being drawn from some other part of the complex where the interchanges involve a development of energy.

The conclusions thus arrived at may be utilised in discussing the problem of heredity. The inheritance of parental qualities, the need to assume continuity of the germ plasm and the comparative unimportance from the standpoint of heredity of somatic qualities, as well as the non-inheritance of mere environmental effects (acquired characters), are all necessary consequences of the view I have advanced.

The general similarity of structure throughout organised creation may well be conditioned primarily by properties inherent in the materials of which all living things are composed—of carbon, of oxygen, of nitrogen, of hydrogen, of phosphorus, of sulphur. At some early period, however, the possibilities became limited and directed processes became the order of the day. From that time onward the chemistry prevailing in organic nature became a far simpler chemistry than that of the laboratory; the possibilities were diminished, the certainties of a definite line of action were increased. How this came about it is impossible to say; mere accident may have led to it. Thus we may assume that some relatively simple asymmetric substance was produced by the fortuitous occurrence of a change under conditions such as obtain in our laboratories and that consequently the enantiomorphous isomeric forms of equal opposite activity were produced in equal amount. We may suppose that a pool containing such material having been dried up dust of molecular fineness was dispersed; such dust falling into other similar pools near the crystallisation point may well have conditioned the separation of only one of the two isomeric forms present in the liquid. A separation having been once effected in this manner, assuming the substance to be one which could influence its own formation, one form rather than the other might have been produced. An active substance thus generated and selected out might then become the origin of a series of asymmetric syntheses. How the complicated series of changes which constitute life may have arisen we cannot even guess at present; but when we contemplate the inherent simplicity of chemical change and bear in mind that life seems but to depend on the simultaneous occurrence of a series of changes of a somewhat diverse order, it does not appear to be beyond the bounds of possibility to arrive at a broad understanding of the method of life. Nor are we likely to be misled into thinking that we can so arrange the conditions as to control and reproduce it; the series of lucky accidents which seem to be required for arrangements of such complexity to be entered upon is so infinitely great.

It is impossible to rate chemistry at too high a value in Canada. The maintenance of the fertility of your fields, the proper utilisation of your vast mineral wealth, the purity of your food supplies[1] will depend mainly on the watchful care and skill of chemists; but the educational value of the subject may also be set very high. If properly taught in your schools, it will afford a means superior to all others, I believe, of training faculties which in these days should be developed in every responsible citizen. No other subject lends itself so effectively as a means of developing the experimental attitude of mind—the attitude of working with a clearly conceived purpose to a desired end, which is so necessary to success in these days; and if care be taken to inculcate habits of neatness and precision and of absolute truthfulness, if care be taken to teach what constitutes evidence, the moral value of such work is incalculable. But to be effective it must be done under proper conditions, systematically; the time devoted to the work must be adequate; I would even advocate that the subject be allowed to come before conventional geography and history and other unpractical subjects, assuming that the training is given in a practical way and with practical objects in view, not in the form of mere lessons learnt by rote; if taught in the form of mere didactic lessons it is as worthless as any other subject as mental discipline. Let me add that I would confine the teaching to a narrow range of problems but make it very thorough with reference to these.

[1] I should like to take this opportunity of saying that it is impossible to over-rate the public value of the great work which Dr. Wiley has undertaken in the United States in endeavouring to secure the supply of food free from deleterious ingredients. At home we certainly need some one to preach a similar crusade and to free us from doctored infants' foods and the innumerable host of medicines by which even our fair fields are disfigured.

## SECTION C.

### GEOLOGY.

OPENING ADDRESS BY ARTHUR SMITH WOODWARD, LL.D., F.R.S., V.P.Z.S., Sec. G.S., KEEPER OF GEOLOGY IN THE BRITISH MUSEUM, PRESIDENT OF THE SECTION.

THE circumstances of the present meeting very clearly determine the subject of a general address to be expected from a student of extinct animals. The remarkable discoveries of fossil backboned animals made on the North American continent during the last fifty years suggest an estimate of the results achieved by the modern systematic methods of research; while the centenary celebration of the birth of Darwin makes it appropriate to consider the extent to which we may begin deducing the laws of organic evolution from the life of past ages as we now know it. Such an address must, of course, be primarily biological in character, and treat of some matters which are not ordinarily discussed by Section C. The subject, however, can only be appreciated fully by those who have some practical acquaintance with the limitations under which geologists pursue their researches, and especially by those who are accustomed to geological modes of thought.

There has been an unfortunate tendency during recent years for the majority of geologists to relinquish the study of fossils in absolute despair. More ample material for examination and more exact methods of research have altered many erroneous names which were originally used; while the admission to scientific publications of too many mere literary exercises on the so-called "law of priority" has now made it necessary to learn not one, but several names for some of the genera and species which are commonly met with. Even worse, the tentative arrangement of fossils in "genetic series" has led to the invention of a multitude of terms which often serve to give a semblance of scientific exactitude to the purest guesswork, and sometimes degenerate into a jargon which is naturally repellent to an educated mind. Nevertheless, I still hope to show that, with all these difficulties, there is so much of fundamental interest in the new work that it is worth while to make an effort to appreciate it. Geology and palæontology in the past have furnished some of the grandest possible contributions to our knowledge of the world of life; they have revealed hidden meanings which no study of the existing world could even suggest; and they have started lines of inquiry which the student of living animals and plants alone would scarcely have suspected to be profitable. The latest researches are the logical continuation of this pioneer work on a more extensive scale, and with greater precision; and I am convinced that they will continue to be as important a factor in the progress of post-Darwinian biology as were the older studies of fossils in the philosophy of Cuvier, Brongniart, and Owen.

In this connection it is necessary to combat the mistaken popular belief that the main object of studying fossils is to discover the "missing links" in the chain of life. We are told that the idea of organic evolution is not worthy of serious consideration until these links, precise in character, are forthcoming in all directions. Moreover, the critics who express this opinion are not satisfied to consider the simplest cases, such as are afforded by some of the lower grades of "shell-fish" which live together in immense numbers and have limited powers of locomotion. They demand long series of exact links between the most complex skeletal frames of the backboned animals, which have extreme powers of locomotion, are continually wandering, and are rarely preserved as complete individuals when they are buried in rock. They even expect continual discoveries of links among the rarest of all fossils, those of the higher apes and man. The geologist, on the other hand, knowing well that he must remain satisfied with a knowledge of a few scattered episodes in the history of life which are always revealed by the merest accident, marvels that the discovery of "missing links" is so constant a feature of his work. He is convinced that, if circumstances were more favourable, he would be able to satisfy the demand of the most exacting critic. He has found enough continuous series among the mollusca, for example, and so many suggestions of equally gradual series among the higher animals, that he does not hesitate to believe without further evidence in a process of descent with modification. The mere reader of books is often misled by the vagaries of nomenclature to suppose that the intervals between the links are greater than in reality; but for the actual student it is an everyday experience to find that fossils of slightly different ages which he once thought distinct are linked together by a series of forms in which it is difficult to discover the feeblest lines of demarcation. He is therefore justified in proceeding on the assumption that in all cases the life of one geological period has passed by a natural process of descent into that of the next succeeding period; and, avoiding genealogical guesswork which proves to be more and more futile, he strives to obtain a broad view of the series of changes which have occurred, to distinguish between those which denote progress and those which lead to stagnation or extinction. When the general features of organic evolution are determined in this manner, it will be much easier than it is at present to decide where missing links in any particular case are most likely to be found.

Among these general features which have been made clear by the latest systematic researches, I wish especially to emphasise the interest and significance of the persistent progress of life to a higher plane, which we observe during the successive geological periods. For I think palæontologists are now generally agreed that there is some principle underlying this progress much more fundamental than chance-variation or response to environment however much these phenomena may have contributed to certain minor adaptations. Consider the case of the backboned animals, for instance, which I happen to have had special opportunities of studying.

We are not likely ever to discover the actual ancestors of animals on the backboned plan, because they do not seem to have acquired any hard skeleton until the latter part of the Silurian period, when fossils prove them to have been typical and fully developed, though low in the backboned scale. The ingenious researches and reasoning of Dr. W. H. Gaskell, however, have suggested the possibility that these animals originated from some early relatives of the scorpions and crustaceans. It is therefore of great interest to observe that the Eurypterids and their allies, which occupy this zoological position, were most abundant during the Silurian period, were represented by species of the largest size immediately afterwards at the beginning of the Devonian, and then gradually dwindled into insignificance. In other words, there was a great outburst of Eurypterid life just at the time when backboned animals arose; and if some of the former were actually transformed into the latter, the phenomenon took place when their powers both of variation and of multiplication were at their maximum.

Fishes were already well established and distributed over perhaps the greater part of the northern hemisphere at the beginning of Devonian times; and then there began suddenly a remarkable impulse towards the production of lung-breathers, which is noticeable not only in Europe and North America, but also probably so far away as Australia. In the middle and latter part of the Devonian period, most of the true fishes had paddles, making them crawlers as much as swimmers; many of them differed from typical fishes, while agreeing with lung-breathers, in having the basis of the upper jaw fused with the skull, not suspended; and some of them exhibited both these features. Their few survivors at the present day (the Crossopterygians and Dipnoans) have also an air-bladder, which might readily become a lung. The characteristic fish-fauna of the Devonian period, therefore, made a nearer approach to the land animals than any group of fishes of later date: and it is noteworthy that in the Lower Carboniferous of Scotland—perhaps even in the Upper Devonian of North America, if footprints can be trusted—amphibians first appeared. In Upper Carboniferous times they became firmly established, and between that period and the Trias they seem to have spread all over the world; their remains having been found, indeed, in Europe, Spitsbergen, India, South Africa, North and South America, and Australia.

The Stegocephala or Labyrinthodonts, as these primitive amphibians are termed, were therefore a vigorous race; but the marsh-dwelling habits of the majority did not allow of much variation from the salamander-pattern. Only in Upper Carboniferous and Lower Permian times did some of their smaller representatives (the Microsauria) become lizard-like, or even snake-like in form and habit; and then there suddenly arose the true reptiles. Still, these reptiles did not immediately replace the Stegocephala in the economy of Nature; they remained quite secondary in importance at least until the Upper Permian, in most parts even until the dawn of the Triassic period. Then they began their flourishing career.

At this time the reptiles rapidly diverged in two directions. Some of them were almost exactly like the little Sphenodon, which still survives in some islands off New Zealand, only retaining more traces of their marsh-dwelling ancestors. The majority (the Anomodonts or Theromorphs) very quickly became so closely similar to the mammals that they can only be interpreted as indicating an intense struggle towards the attainment of the higher warm-blooded grade; and there is not much doubt that true mammals actually arose about the end of the Triassic period. Here, again, however, the new race did not immediately replace the old, or exterminate it by unequal competition. Reptiles held their own on all lands throughout the Jurassic and Cretaceous periods, and it was not until the Tertiary that mammals began to predominate.

As to the beginning of the birds, it can only be said that towards the end of the Triassic period there arose a race of small Dinosaurs of the lightest possible build, exhibiting many features suggestive of the avian skeleton; so it is probable that this higher group also originated from an intensely restless early community of reptiles, in which all the variations were more or less in the right direction for advancement.

In short, it is evident that the progress of the backboned land animals during the successive periods of geological time has not been uniform and gradual, but has proceeded in a rhythmic manner. There have been alternations of restless episodes which meant real advance, with periods of comparative stability, during which the predominant animals merely varied in response to their surroundings, or degenerated, or gradually grew to a large size. There was no transition, for instance, between the reptiles of the Cretaceous period and the mammals which immediately took their place in the succeeding Eocene period: those mammals, as we have seen, had actually originated long ages before, and had remained practically dormant in some region which we have not yet discovered, waiting to burst forth in due time. During this retirement of the higher race the reptiles themselves had enjoyed an extraordinary development and adaptation to every possible mode of life in nearly all parts of the globe. We do not understand the phenomenon—we cannot explain it; but it is as noticeable in the geological history of fishes as in that of the land animals just considered. It seems to have been first clearly observed by the distinguished American naturalist, the late Prof. Edward D. Cope, who termed the sudden fundamental advances "expression points" and saw in them a manifestation of some inscrutable inherent "bathmic force."

Perhaps the most striking feature to be noticed in each of these "expression points" is the definite establishment of some important structural character which had been imperfect or variable before, thus affording new and multiplied possibilities of adaptation to different modes of life. In the first lung-breathers (Stegocephala), for example, the indefinite paddle of the mud fishes became the definite five-toed limb; while the incomplete backbone reached completeness. Still, these animals must have been confined almost entirely to marshes, and they seem to have been all carnivorous. In the next grade, that of the reptiles, it became possible to leave the marshes; and some of them were soon adapted not only for life on hard ground or in forests, but even for flight in the air. Several also assumed a shape of body and limbs enabling them to live in the open sea. Nearly all were carnivorous at first, and most of them remained so to the end; but many of the Dinosaurs eventually became practically hoofed animals, with a sharp beak for cropping herbage, and with powerful grinding teeth. In none of these animals, however, were the toes reduced to less than three in number, and in none of them were the basal toe-bones fused together as they are in cattle and deer. It is also noteworthy that the brain in all of them remained very small and simple. In the final grade of backboned life, that of the mammals, each of the adaptive modifications just mentioned began to arise again in a more nearly perfected manner, and now survival depended not so much on an effective body as on a developing brain. The mammals began as little carnivorous or mixed-feeding animals with a small brain and five toes, and during the Tertiary period they gradually differentiated into the several familiar groups as we now known them, eventually culminating in man.

The demonstration by fossils that many animals of the same general shape and habit have originated two or three times, at two or three successive periods, from two or three continually higher grades of life, is very interesting. To have proved, for example, that flying reptiles did not pass into birds or bats, that hoofed Dinosaurs did not change into hoofed mammals, and that Ichthyosaurs did not become porpoises; and to have shown that all these later animals were mere mimics of their predecessors, originating independently from a higher yet generalised stock, is a remarkable achievement. Still more significant, however, is the discovery that towards the end of their career through geological time totally different races of animals repeatedly exhibit certain peculiar features, which can only be described as infallible marks of old age.

The growth to a relatively large size is one of these marks, as we observe in the giant Pterodactyls of the Cretaceous period, the colossal Dinosaurs of the Upper Jurassic and Cretaceous, and the large mammals of the Pleistocene and the present day. It is not, of course, all the members of a race that increase in size; some remain small until the end, and they generally survive long after the others are extinct; but it is nevertheless a common rule that the prosperous and typical representatives are successively larger and larger, as we see them in the familiar cases of the horses and elephants of the northern hemisphere, and the hoofed animals and armadillos of South America.

Another frequent mark of old age in races was first discussed and clearly pointed out by the late Prof. C. E. Beecher, of Yale. It is the tendency in all animals with skeletons to produce a superfluity of dead matter, which accumulates in the form of spines or bosses as soon as the race they represent has reached its prime and begins to be on the down-grade. Among familiar instances may be mentioned the curiously spiny Graptolites at the end of the Silurian period, the horned Pariasaurians at the beginning of the Trias, the armour-plated and horned Dinosaurs at the end of the Cretaceous, and the cattle or deer of modern Tertiary times. The latter case—that of the deer—is specially interesting, because fossils reveal practically all the stages in the gradual development of the horns or antlers, from the hornless condition of the Oligocene species, through the simply forked small antlers of the Miocene species, to the largest and most complex of all antlers seen in *Cervus sedgwicki* from the Upper Pliocene and the Irish deer (*C. giganteus*) of still later times. The growth of these excrescences, both in relative size and complication, was continual and persistent until the climax was reached and the extreme forms died out. At the same time, although the palæontologist must regard this as a natural and normal phenomenon not directly correlated with the habits of the race of animals in which it occurs, and although he does not agree with the oft-repeated statement that deer may have "perfected" their antlers through the survival of those individuals which could fight most effectively, there may nevertheless be some truth in the idea that the growths originally began where the head was subject to irritating impacts, and that they so happened to become of utility. Fossils merely prove that such skeletal outgrowths appear over and over again in the prime and approaching old age of races; they can suggest no reasons for the particular positions and shapes these outgrowths assume in each species of animal.

It appears, indeed, that when some part of an animal (whether an excrescence or a normal structure) began to grow relatively large in successive generations during geological time, it often acquired some mysterious impetus by which it continued to increase long after it had reached the serviceable limit. The unwieldy antlers of the extinct Sedgwick's deer and Irish deer just mentioned, for example, must have been impediments rather than useful weapons. The excessive enlargement of the upper canine teeth in the so-called sabre-toothed tigers (Machærodus and its allies) must also eventually have hindered rather than aided the capture and eating of prey. The curious gradual elongation of the face in the Oligocene and Miocene Mastodons, which has lately been described by Dr. Andrews, can only be regarded as another illustration of the same phenomenon. In successive generations of these animals the limbs seem to have grown continually longer, while the neck remained short, so that the head necessarily became more and more elongated to crop the vegetation on the ground. A limit of mechanical inefficiency was eventually reached, and then there survived only those members of the group in which the attenuated mandible became shortened up, leaving the modified face to act as a "proboscis." The elephants thus arose as a kind of after-thought from a group of quadrupeds that were rapidly approaching their doom.

The end of real progress in a developing race of backboned animals is also often marked by the loss of the teeth. A regular and complete set of teeth is always present at the commencement, but it frequently begins to lack successors in animals which have reached the limit of their evolution, and then it soon disappears. Tortoises, for instance, have been toothless since the Triassic period, when they had assumed all their essential features; and birds have been toothless since the end of Cretaceous times. The monotreme mammals of Australasia, which are really a survival from the Jurassic period, are also toothless. Some of the latest Ichthyosaurs and Pterodactyls were almost or quite toothless; and I have seen a jaw of an Upper Cretaceous carnivorous Dinosaur (Genyodectes) from Patagonia so completely destitute of successional teeth that it seems likely some of these land reptiles nearly arrived at the same condition.

Among fishes there is often observable still another sign of racial old age—namely, their degeneration into eel-shaped forms. The Dipnoan fishes afford a striking illustration, beginning with the normally shaped Dipterus in the Middle Devonian, and ending in the long-bodied Lepidosiren and Protopterus of the present day. The Palæozoic Acanthodian sharks, as they are traced upwards from their beginning in the Lower Devonian to their end in the Permian, also acquire a remarkable elongation of the body and a fringe-like extension of the fins. Among higher fishes, too, there are numerous instances of the same phenomenon, but in most of these the ancestors still remain undiscovered, and it would thus be tedious to discuss them.

Finally, in connection with these obvious symptoms of old age in races, it is interesting to refer to a few strange cases of the rapid disappearance of whole orders of animals, which had a practically world-wide distribution at the time when the end came. Local extinction, or the disappearance of a group of restricted geographical range, may be explained by accidents of many kinds; but contemporaneous universal extinction of widely spread groups, which are apparently not affected by any new competitors, is not so easily understood. The Dinosaurs, for instance, are known to have lived in nearly all lands until the close of the Cretaceous period; and, except perhaps in Patagonia, they were always accompanied until the end by a typically Mesozoic fauna. Their remains are abundant in the Wealden formation of Western Europe, the deposit of a river which must have drained a great continent at the beginning of the Cretaceous period; they have also been found in a corresponding formation which covers a large area in the State of Bahia, in Brazil. They occur in great numbers in the freshwater Upper Cretaceous Laramie deposits of Western North America, and also in a similar formation of equally late date in Transylvania, South-east Europe. In only two of these regions (South-east England and West North America) have any traces of mammals been found, and they are extremely rare fragments of animals as small as rats; so there is no reason to suppose that the Dinosaurs suffered in the least from any struggle with warm-blooded competitors. Even in Patagonia, where the associated mammal-remains belong to slightly larger and more modern animals, these fossils are also rare, and there is nothing to suggest competition. The race of Dinosaurs seems, therefore, to have died a natural death. The same may be said of the marine reptiles of the orders Ichthyosauria, Plesiosauria, and Mosasauria. They had a practically world-wide distribution in the seas of the Cretaceous period, and the Mosasauria especially must have been extremely abundant and flourishing. Nevertheless, at the end of Cretaceous times they disappeared everywhere, and there was absolutely nothing to take their place until the latter part of the Eocene period, when whales and porpoises began to play exactly the same part. So far as we know, the higher race never even came in contact with the lower race; the marine mammals found the seas vacant, except for a few turtles and for one curious Rhynchocephalian reptile (Champsosaurus), which did not long survive. Another illustration of the same phenomenon is probably afforded by the primitive Carnivora (the so-called Sparassodonta), which were numerous in South America in the Lower Tertiary periods. They were animals with a brain as small as that of the thylacines and dasyures which now live in Tasmania. They appear to have died out completely before they were replaced by the cats, sabre-toothed tigers, and dogs, which came down south from North America over the newly emerged isthmus of Panama at the close of the Pliocene period. At least, the remains of these old carnivores and their immigrant successors have never yet been found associated in any geological formation.

These various considerations lead me to think that there is also deep significance in the tendency towards fixity in the number and regularity (or symmetry) in the arrangement of their multiple parts, which we frequently observe in groups of animals as we trace them from their origin to their prime. It is well known that in certain of the highest and latest types of bony fishes the vertebræ and fin-rays are reduced to a fixed and practically invariable number for each family or genus, whereas there is no such fixity in the lower and earlier groups. In the earliest known Pycnodont fishes from the Lower Lias (Mesodon) the grinding teeth form an irregular cluster, while in most of the higher and later genera they are arranged in definite regular rows in a symmetrical manner. Many of the lower backboned animals have teeth with several cusps, and in some genera the number of teeth seems to be constant; but in the geological history of the successive classes the tooth-cusps never became fixed individual entities, readily traceable throughout whole groups, until the highest or mammalian grade had been attained. Moreover, it is only in the same latest grade or class that the teeth themselves can be treated as definite units, always the same in number (forty-four), except when modified by degeneration or special adaptation. In the earlier and lower land animals the number of vertebræ in the neck depends on the extent of this part, whereas in the mammal it is almost invariably seven, whatever the total length may be. Curiously constant, too, in the modern even-toed hoofed mammals is the number of nineteen vertebræ between the neck and the sacrum.

I am therefore still inclined to believe that the comparison of vital processes with certain purely physical phenomena is not altogether fanciful. Changes towards advancement and fixity which are so determinate in direction, and changes towards extinction which are so continually repeated, seem to denote some inherent property in living things, which is as definite as that of crystallisation in inorganic substances. The regular course of these changes is merely hindered and modified by a succession of checks from the environment and Natural Selection. Each separate chain of life, indeed, bears a striking resemblance to a crystal of some inorganic substance which has been disturbed by impurities during its growth, and has thus been fashioned with unequal faces, or even turned partly into a mere concretion. In the case of a crystal the inherent forces act solely on molecules of the crystalline substance itself, collecting them and striving, even in a disturbing environment, to arrange them in a fixed geometrical shape. In the case of a chain of life (or organic phylum) we may regard each successive animal as a temporary excrescence of colloid substance round the equally colloid germ-plasm which persists continuously from generation to generation. The inherent forces of this germ-plasm, therefore, act upon a consecutive series of excrescences (or animal bodies), struggling not for geometrically arranged boundaries, but towards various other symmetries, and a fixity in number of multiple parts. When the extreme has been reached, activities cease, and sooner or later the race is dead.

Such are some of the most important general results to which the study of fossils has led during recent years; and they are conclusions which every new discovery appears to make more certain. When we turn to details, however, it must be admitted that modern systematic researches are continually complicating rather than simplifying the problems we have to solve. Prof. Charles Depéret has lately written with scant respect of some of the pioneers who were content with generalities, and based their conclusions on the geological succession of certain anatomical structures rather than on a successive series of individuals and species obtained from the different layers of one geological section; but even now I do not think we can do much better than our predecessors in unravelling real genealogies. At least Prof. Depéret's genealogical table of the Lower Tertiary pig-like Anthracotheriidæ, which he publishes as an illustration of "évolution réelle," seems to me to be no more exact than several tables of other groups by previous authors which he criticises. His materials are all fragmentary, chiefly jaws and portions of skulls; they were obtained from several isolated lake-deposits, of which the relative age cannot be determined by observing the geological superposition; and they represent a group which is known to have lived over a large part of Europe, Asia, Northern Africa, and North America. There is therefore no certainty that the genera and species enumerated by Prof. Depéret actually originated one from the other in the region where he happened to find them; he has demonstrated the general trend of certain changes in the Anthracotheriidæ during geological time, but really nothing more.

Even when a group of animals seems to have been confined to one comparatively small region, where the series is not complicated by migration to and from other parts of the world, modern research still emphasises the difficulty of tracing real lines of descent. The primitive horned hoofed animals of the family Titanotheriidæ, for example, are only known from part of North America, and they seem to have originated and remained there until the end. As their fossil skeletons are abundant and well preserved, it ought to be easy to discover the exact connections of the several genera and species. Prof. Osborn has now proved, however, that the Titanotheres must have evolved in at least four distinct lines, adapted "for different local habitat, different modes of feeding, fighting, locomotion, &c., which took origin, in part at least, in the Middle or Upper Eocene." They exhibit "four distinct types in the shape and position of the horns, correlated with the structure of the nasals and frontals, and indicative of different modes of combat among the males." The ramifications of the group are indeed so numerous that the possibility of following chains of ancestors begins to appear nearly hopeless.

Among early reptiles the same difficulties are continually multiplied by the progress of discovery. About twenty years ago it began to appear likely that we should soon find the terrestrial ancestors of the Ichthyosauria in the Trias; and somewhat later a specimen from California raised hopes of obtaining them by systematic explorations in that region. During more recent years Prof. J. C. Merriam and his colleagues have actually made these explorations, and the result is that we now know from the Californian Trias a multitude of reptiles, which need more explanation than the Ichthyosauria themselves. Prof. Merriam has found some of the links predicted between Ichthyosaurs and primitive land reptiles, but he has by no means reached the beginning of the marine group; and while making these discoveries he has added greatly to the complication of the problem which he set out to solve.

Serious difficulties have also become apparent during recent years in determining exactly the origin of the mammals. For a long time after the discovery of the Anomodont or Theromorph reptiles in the Permian-Trias of South Africa, it seemed more and more probable that the mammals arose in that region. Even yet new reptiles from the Karoo formation are continually being described as making an astonishingly near approach to mammals; and, so far as the skeleton is concerned, the links between the two grades are now very numerous among South African fossils. Since these reptiles first attracted attention, however, they have gradually been found in the Permian and Trias of a large part of the world. Remains of them were first met with in India, then in North America, and next in Scotland, while during the last few years Prof. W. Amalitzky has disinterred so many nearly complete skeletons in the north of Russia that we are likely soon to learn more about them from this European country than from the South African area itself. Quite lately I have received numerous bones from a red marl in Rio Grande do Sul, Southern Brazil, which show that not merely Anomodonts, but also other characteristic Triassic land reptiles were likewise abundant in that region. We are therefore now embarrassed by the richness of the sources whence we may obtain the ancestors of mammals. Whereas some years ago it appeared sufficient to search South Africa for the solution of the problem, we are now uncertain in which direction to turn. We are still perhaps inclined to favour the South African source; but this is only because we know nothing of the Jurassic land animals of that part of the world, and we cherish a lingering hope that they may eventually prove to have included the early mammals for which we have so long sought in vain.

The mystery of the origin of the marine mammals of the order Sirenia and Cetacea appears to have been diminished by the discoveries of the Geological Survey of Egypt, Dr. Andrews and Dr. Fraas in the Eocene and Oligocene deposits of the Mokattam Hills and the Fayum. It is now clear that the Sirenians are closely related to the small primitive ancestors of the elephants; while, so far as the skull and dentition are concerned, we know nearly all the links between the early toothed whales (or Zeuglodonts) and the primitive ancestors of the Carnivora (or Creodonts). The most primitive form of Sirenian skull hitherto discovered, however, is not from Egypt, but from the other side of the world, Jamaica; and exactly the same Zeuglodonts, even with an associated sea-snake, occur so far away from Egypt as Alabama, U.S.A. The problem of the precise origin of these marine mammals is therefore not so simple as it would have appeared to be had we known only the Egyptian fossils. The progress of discovery, while revealing many most important generalities, has made it impossible to vouch for the accuracy of the details in any "genealogical tree."

Another difficulty resulting from the latest systematic researches is suggested by the extinct hoofed mammals of South America. The llamas, deer, and peccaries existing in South America at the present time are all immigrants from the northern continent; but during the greater part of the Tertiary period there lived in that country a large number of indigenous hoofed mammals, which originated quite independently of those in other regions. They seem to have begun in early Eocene times much in the same manner as those of the northern hemisphere; but as they became gradually adapted for life on hard ground, they formed groups which are very different from those with which we are familiar in our part of the world. Some of them (Proterotheriidæ) were one-toed mimics of the horses, but without the advanced type of brain, the deepened grinding teeth, the mobile neck, or the really effective wrist and ankle. Others (Toxodontidæ) made some approach towards rhinoceroses in shape and habit, even with a trace of a horn on the nose. Until their independent origin was demonstrated, these curious animals could not be understood; and it is probable that there are innumerable similar cases of parallel development of groups, by which in our ignorance we are often misled.

It would be easy to multiply instances, but I think I have now said enough to show that every advance in the study of fossils reveals more problems than it solves. During the last two decades the progress in our knowledge of the extinct backboned animals has been truly astonishing, thanks especially to the great explorations in North America, Patagonia, Egypt, Madagascar, and South Africa. Whole groups have been traced a long way towards their origin; but with them have been found a number of previously unknown groups which complicate all questions of evolution to an almost bewildering extent. Animals formerly known only by fragments are now represented by nearly complete skeletons, and several which appeared to have a restricted geographical range have now been found over a much wider area; but while this progress has been made, numerous questions have arisen as to the changing connections of certain lands and seas which previously seemed to have been almost settled. The outlook both of zoology and of geology has, therefore, been immensely widened, but the only real contribution to philosophy has been one of generalities. Some of the broad principles to which I have referred are now so clearly established that we can often predict what will be the main result of any given exploration, should it be successful in recovering skeletons. We are no longer bold enough to restore an entirely unknown extinct animal from a single bone or tooth, like the trustful Cuvierian school; but there are many kinds of bones and teeth of which we can determine the approximate geological age and probable associates, even if we have no exact knowledge of the animals to which they belong. A subject which began by providing material for wonder-books has thus been reduced to a science sufficiently precise to be of fundamental importance both to zoology and to geology; and its exactitude must necessarily increase with greater and greater rapidity as our systematic researches are more clearly guided by the experience we have already gained.

## *NOTES.*

THE special report agreed upon by the Select Committee on the Daylight Saving Bill contains the following conclusion:—"Having regard to the great diversity of opinion existing upon the proposals of the Bill and to the grave doubts which have been expressed as to whether the objects of the measure can be attained by legislation without giving rise, in cases involving important interests, to serious inconvenience, your committee recommend that the Bill be not further proceeded with." We are glad the committee has arrived at this conclusion, which embodies the views expressed in these columns on several occasions. Most people are in favour of the principle of making the best and fullest use of daylight hours, but the compulsory alteration of the system of time-reckoning for several months of the year is quite a different matter. As we have pointed out, in engineering, building, agricultural and other industries in which it is difficult to carry on work by artificial light, the hours of labour are already adapted to the daylight hours in different seasons. Here we have the voluntary adoption of the principle of daylight saving, and we are in complete sympathy with any movement to encourage the extension of the custom to other industrial or commercial circles where earlier hours of commencing work during certain months are practicable or desirable. This can be accomplished, however, without legislation, and the committee has acted wisely in recommending that the Bill, which would make a seasonal change of time compulsory, be not carried further.

A BILL to promote the economic development of the United Kingdom and the improvement of the roads therein was introduced in the House of Commons on August 26 by the Chancellor of the Exchequer. In the explanatory memorandum of this Bill it is stated that Part i. enables the Treasury to make free grants and loans for the purpose of aiding and developing forestry, agriculture, and rural industries, the reclamation and drainage of land, the improvement of rural transport (other than roads), the construction and improvement of harbours and canals, and the development and improvement of fisheries, and for any other purpose calculated to promote the economic development of the United Kingdom. A grant or loan must be made either to or through a Government department, and all applications for grants or loans have to be referred to an advisory committee, and the recommendations of the committee considered before the grant or loan is made; but the responsibility of making the grant or loan will rest with the Treasury, who will not be bound by the recommendations of the committee. All grants and loans will be made out of a separate fund, which will be fed by (1) sums annually voted by Parliament; (2) a sum of 2,500,000*l*. charged on the Consolidated Fund and payable in five annual instalments of 500,000*l*. each in 1911, 1912, 1913, 1914, and 1915; (3) sums received by way of interest on and repayment of loans and the profits made as the result of a grant or loan in cases where the repayment of such profits is made a condition of the grant or loan. Power is given to the Board of Agriculture and Fisheries and the Department of Agriculture and Technical Instruction for Ireland to acquire land (compulsorily if necessary) for any purpose for which a grant is made to them. As the *Times* points out, this part of the Bill will permit the expenditure of money on scientific research and experimental work of a kind likely to be beneficial to agriculture. Part ii. of the Bill constitutes a Road Board for the purpose of improving the facilities for motor traffic. In addition to the power of acquiring land for the purposes of new roads proposed to be constructed by the Road Board, the Board is given power to acquire land in rural districts on either side of any such proposed road to the extent of 220 yards in depth.

DR. ALEXANDER RUSSELL has been appointed principal of Faraday House in succession to the late Mr. H. E. Harrison.

DR. T. H. BRYCE, lecturer on anatomy at Queen Margaret College, Glasgow, has been appointed Regius professor of anatomy in the University of Glasgow in succession to Prof. J. Cleland.

WE regret to see the announcement of the death, at sixty-seven years of age, of Prof. E. C. Hansen, head of the physiological department of the Carlsberg Laboratory, Copenhagen, for studies of chemistry and plant physiology, with particular reference to fermentation.

THE death is announced, at sixty-four years of age, of Dr. Radcliffe Crocker, distinguished particularly by his work on diseases of the skin. Dr. Crocker was the first president of the dermatological section of the Royal Society of Medicine, and made many valuable contributions to the literature of dermatology, among them being his "Treatise on Diseases of the Skin" and "Atlas of Diseases of the Skin."

THE twentieth annual general meeting of the Institution of Mining Engineers will be held at Newcastle-upon-Tyne on September 15. The following are among the papers to be read or discussed:—fire-damp caps and the detection of fire-damp in mines by means of safety-lamps, E. B. Whalley and W. M. Tweedie; equipment for the study of flame-caps and for miscellaneous experiments on safety-lamps, Prof. G. R. Thompson; electricity in coal mines, R. Nelson.

THE Budapest correspondent of the *Times* reports that the sixteenth International Congress of Medicine was officially opened there on August 29 by the Archduke Joseph in the name of the King of Hungary and Emperor of Austria. More than 4300 members have enrolled their names in the list, and they include a large number of eminent authorities on medicine from all parts of the world. We hope to give, in a future issue, an account of subjects of wide scientific interest and importance brought before the congress.

THE next International Congress of Mining, Metallurgy, Applied Mechanics, and Practical Geology will be held at Düsseldorf during the last week of June, 1910, under the auspices of the Rhenish-Westphalian Mining Industry. An influential committee of organisation has been formed which is charged with the making of the arrangements for the reading and discussion of papers, visits to places of technical interest, and social entertainments. Further information can be obtained in due course on application to the secretary of the Iron and Steel Institute or to the committee of organisation, Jacobistrasse, 3–5, Düsseldorf.

ON August 28 a West Indian hurricane struck the coast of north-eastern Mexico, and thence travelled inland over the States of Tamaulipas and Nuevo Leon. According to a *Times* correspondent, a deluge of rain fell for ninety-six hours, registering in all 17½ inches, and the rivers, overflowing their banks, inundated territory estimated at 300 miles by 400 miles in area. Monterey, the capital town of Nuevo Leon, appears to have suffered most. The overflowing river swept through the lower parts of the town as a torrent half a mile, destroying life and property in its course.

IN accordance with previous announcements, arrangements have been made to hold the autumn meeting of the Iron and Steel Institute in London on September 27–30 and October 1. The programme includes the following papers:—the determination of the power consumption of reversing rolling-mills, C. A. Ablett; comparative tests of cast iron, E. Adamson; artificial magnetic oxide of iron, F. J. R. Carulla; action of air and steam on pure iron, Dr. J. Newton Friend; corrosion of iron, Dr. J. Newton Friend; uniform moisture in blast, Greville Jones; the refining of steel by electricity, Disponent E. J. Ljungberg; the fuel economy of dry blast, as indicated by calculations from empirical data, R. S. Moore; the "growth" of cast irons after repeated heatings, Prof. H. F. Rugan and Dr. H. C. H. Carpenter; the maintenance and renewal of permanent way, R. Price-Williams; the constitution of carbon-tungsten steels, T. Swinden.

THE past summer, comprised by the three months June, July, and August, was of a generally unsettled character over the whole of the British Islands; rain was of very frequent occurrence, and the days were, as a rule, decidedly cool. At Greenwich there were in all forty-one days with the temperature above 70°, of which twenty occurred in August, and there were six days with the thermometer above 80°, all of which occurred in August. Of recent years, 1903 and 1907 are the only summers as cold. The mean temperature for the whole period of three months was rather more than 2° below the average; the mean for the respective months was 55° in June, 61° in July, and 63° in August. The aggregate rainfall for the three months was 8·65 inches, of which 3·69 inches fell in June, 3·16 inches in July, and 1·80 inches in August; the total for the summer was 1·87 inches more than the average. Rain fell on forty-eight days, of which eighteen occurred in June, nineteen in July, and eleven in August. The total duration of sunshine was 502 hours, of which 106 occurred in June, 179 in July, and 217 in August. The deficiency of sunshine at Greenwich for the three months was 141 hours.

THE council of the National Museum of Wales is prepared to receive designs for a new museum at Cardiff, at a cost for the completed building of 250,000*l.*, inclusive of carving, but exclusive of decorative sculpture. A copy of a detailed statement of conditions and instructions to competing architects has reached us from Dr. W. E. Hoyle, director of the museum, and the prospect it presents is very pleasing. The museum is to afford the accommodation known to be necessary in all the various departments of a national museum, and will include the following exhibition galleries:—history and antiquities; geology and mineralogy; Welsh natural history; zoology and botany general; industries; art; children's room; aquarium. There will also be special rooms for study and reserve collections for each department. The circular of instructions states that the purpose of the building is to preserve and display articles of various kinds, not only with satisfaction to the connoisseur in each variety, but with a taste and artistic refinement likely to waken the interest of the public generally. The exhibition cases will, as a rule, stand free in the rooms and not against the walls. Designs for the building must be sent in (carriage paid) addressed to the director, National Museum of Wales, City Hall, Cardiff, on or before January 31, 1910.

THE aviation week at Rheims ended on Saturday, August 28, when several remarkable flights and achievements were accomplished. The meeting has shown that aëroplanes of various designs are able to remain in the air for two or three hours, and to attain speeds of about fifty miles an hour. The Grand Prize for the longest flight was won by Mr. Farman, with a flight of nearly 112 miles in 3h. 4m. 56·4s.; the Gordon-Bennett Cup for speed by Mr. Curtiss, who flew the two-lap twenty-kilometre course in 15m. 50·6s.; the speed prize for swiftest flight over thirty kilometres by Mr. Curtiss, who did the distance in 26m. 40·2s.; the passenger prize by Mr. Farman, who flew six miles with two passengers in 10m. 39s.; and the altitude prize by Mr. Latham, for reaching the greatest height of 155 metres. The *Morning Post* of August 28 gives the following interesting table showing the successive stages of distances achieved in aëroplane flights since the commencement of public flights in Europe:—

| Date | Aëroplanist | Place | Time | Distance |
|---|---|---|---|---|
| | | | h. m. s. | Metres |
| Oct. 14, 1897 | Ader ... ... | Satory ... | — | 300 |
| Dec. 17, 1903 | O. Wright... | Dayton ... | 0 0 59 | 260 |
| | | | | Kilom. |
| Dec. 17, 1904 | O. Wright... | Dayton ... | — | 4·5 |
| Sept. 26, 1905 | O. Wright... | Dayton ... | 0 18 9 | 17·9 |
| Sept. 29, 1905 | O. Wright... | Dayton ... | 0 19 55 | 19·5 |
| Oct. 3, 1905 | O. Wright... | Dayton ... | 0 25 5 | 24·5 |
| Oct. 4, 1905 | O. Wright... | Dayton ... | 0 33 17 | 33·4 |
| Oct. 5, 1905 | O. Wright... | Dayton ... | 0 38 3 | 38·9 |
| Sept. 14, 1906 | S. Dumont.. | Bagatelle ... | 0 0 8 | — |
| | | | | Metres |
| Oct. 24, 1906 | S. Dumont.. | Bagatelle ... | 0 0 8 | 50 |
| Nov. 13, 1906 | S. Dumont.. | Bagatelle ... | 0 0 8 | 60 |
| Nov. 13, 1906 | S. Dumont.. | Bagatelle ... | 0 0 8 | 82·60 |
| Nov. 13, 1906 | S. Dumont.. | Bagatelle ... | 0 0 21$\frac{1}{5}$ | 220 |
| Oct. 15, 1907 | H. Farman.. | Issy ... ... | 0 0 21 | 285 |
| Oct. 26, 1907 | H. Farman . | Issy ... ... | 0 0 27 | 363 |
| Oct. 26, 1907 | H. Farman.. | Issy ... ... | 0 0 31$\frac{3}{5}$ | 403 |
| Oct. 25, 1907 | H. Farman.. | Issy ... ... | 0 0 52$\frac{3}{5}$ | 771 |
| Nov. 9, 1907 | H. Farman.. | Issy ... ... | 0 1 14 | — |
| Jan. 11, 1908 | H. Farman.. | Issy ... ... | 0 1 45 | — |
| | | | | Kilom. |
| Jan. 13, 1908 | H. Farman.. | Issy ... ... | 0 1 28 | 1·5 |
| Mar. 21, 1908 | H. Farman.. | Issy ... ... | 0 3 31 | 2 |
| April 10, 1908 | Delagrange. | Issy ... ... | — | 2·5 |
| April 11, 1908 | Delagrange. | Issy ... ... | 0 6 30 | 3·9 |
| May 27, 1908 | Delagrange. | Rome ... ... | 0 15 25 | 9 |
| May 30, 1908 | Delagrange. | Rome ... ... | 0 15 26$\frac{1}{5}$ | 12·7 |
| June 22, 1908 | Delagrange. | Milan ... ... | 0 16 30 | 17 |
| July 6, 1908 | H. Farman.. | Issy ... ... | 0 20 19$\frac{3}{5}$ | 19·7 |
| Sept. 6, 1908 | Delagrange. | Issy ... ... | 0 29 53$\frac{4}{5}$ | 24·7 |
| Sept. 9, 1908 | O. Wright... | Fort Myer... | 0 57 31 | — |
| Sept. 9, 1908 | O. Wright... | Fort Myer... | 1 3 15 | — |
| Sept. 10, 1908 | O. Wright... | Fort Myer... | 1 5 52 | — |
| Sept. 11, 1908 | O. Wright... | Fort Myer... | 1 10 50 | — |
| Sept. 12, 1908 | O. Wright... | Fort Myer... | 1 15 20 | — |
| Sept. 21, 1908 | W. Wright.. | Auvours ... | 1 31 25$\frac{4}{5}$ | 66·6 |
| Dec. 18, 1908 | W. Wright.. | Auvours ... | 1 54 53$\frac{3}{5}$ | 99·8 |
| Dec. 31, 1908 | W. Wright.. | Auvours ... | 2 20 23$\frac{1}{5}$ | 124·7 |
| Aug. 7, 1909 | Sommer ... | Châlons ... | 2 27 15 | — |
| Aug. 25, 1909 | Paulhan ... | Bétheny ... | 2 43 24$\frac{4}{5}$ | 133·6 |
| Aug. 26, 1909 | Latham ... | Bétheny ... | 2 17 21$\frac{3}{5}$ | 154·6 |
| Aug. 27, 1909 | Farman ... | Bétheny ... | 3 4 56$\frac{3}{5}$ | 180 |

SEVERAL remarkable pictures illustrate Lieut. Shackleton's account of his Antarctic expedition which he commences in the September number of *Pearson's Magazine*. The article is an interesting narrative of the origin and early work of the expedition, and may be regarded as an earnest of the detailed account to be published this autumn. We notice a few of the noteworthy points. Pack ice was sighted about 1500 miles from Lyttelton, and the journey was then continued southwards along the 178th meridian west. After passing through hundreds of tabular icebergs by means of narrow lanes, the Ross Sea was reached, and the *Nimrod* was taken along the edge of the Great Ice Barrier, the enormous cliff of ice towering high above the vessel's crow's nest. The pack ice barred the way to King Edward VII. Land, where it had been intended to winter, so the vessel steamed to McMurdo Sound, and winter quarters were established at a spot twenty miles north of the point at which the *Discovery* expedition wintered. The party which ascended Mount Erebus made valuable observations of the volcano. In the old crater, above the southern end of which rises the active cone, a number of curious mounds were seen which proved to be fumeroles. The steam from the fumeroles is converted into ice as soon as it reaches the surface of the snow plain, and the result is the production of ice mounds. The active crater of Erebus was found to be between 800 feet and 900 feet deep, with a maximum width of half a mile, and at the bottom were seen three well-like openings from which the steam is projected. The height of the volcano was found to be 13,350 feet, and immense moraines ascend the western slope to a height of fully 1000 feet above sea-level. As the adjacent sea is at least 300 fathoms deep, this indicates that when at its maximum development the ice sheet must have had a thickness of not less than 2800 feet.

IN Publication No. 60 of the Hull Museum Mr. T. Sheppard figures a fine skull of *Bison priscus* recently obtained from gravel at Kelsey Hill, Yorks.

WE have to acknowledge the receipt from Mr. B. B. Woodward of a copy of his presidential address to the Malacological Society upon the subject of Darwinism and malacology.

IN the Proceedings of the Academy of Natural Sciences of Philadelphia for May Mr. J. P. Moore describes part of a collection of polychætous annelids dredged in 1904 off southern California; a large number of new species are included in the collection.

IN the Journal of the Royal Microscopical Society Messrs. Heron-Allen and Earland continue their account of the Foraminifera found in the sand at Selsey Bill, Sussex. These include, not only recent, but also many extinct forms from various geological horizons.

IN No. 191 of the Proceedings of the American Philosophical Society are included addresses on Darwin and Darwinism delivered at a commemorative meeting held at Philadelphia on April 23. In the first of these Mr. Bryce, British Ambassador at Washington, gives interesting personal reminiscences of Darwin and of the reception accorded to the "Origin of Species" on its first appearance. Mr. G. L. Goodale and Mr. G. S. Fullerton follow on with addresses respectively devoted to the influence of Darwin on the natural and on the mental and moral sciences, while Mr. E. G. Conklin winds up with the world's debt to Darwin.

IN February last Dr. N. Annandale obtained on the Orissa coast of India a number of small more or less nearly globular organisms in the tide-wash. When placed in water their shape changed from globular to conical, and indicated that they were evidently pelagic sea-anemones, although devoid of tentacles. The mouth is conspicuous, forming a relatively long, narrow slit expanded at one end, and the whole organism presents a milky appearance, which conceals the internal organs. Externally a vinous tinge, deepening into brown at the aboral pole (which is perforated by a pore) was noticeable. As these actinians, which are apparently adult, although no gonads are visible, evidently indicate a new generic and specific type, Dr. Annandale has described them under the name *Anactinia pelagica*.

*Investigator sicarius* is the name proposed by Captain F. H. Stewart in vol. i., No. 4, of Memoirs of the Indian Museum for a new type of gephyrean worm of which specimens were obtained by the s.s. *Investigator* in the Gulf of Manaar and off the Arakan coast. This annelid is regarded as indicating, not only a new genus and species, but likewise a new order, which it is proposed to designate by the uncouth title of "Investigatoroidea." This order is defined as gephyreans with an anterior terminal mouth and posterior subterminal vent, and a nervous system composed of dorsal cerebral ganglia and two lateral nerve-cords. The replacement of the median ventral nerve-cord by a pair of ventro-lateral cords is not considered sufficient to bar the inclusion of the new organism in the Gephyrea.

CONSIDERABLE interest attaches to an article in the August number of the *American Naturalist*, by Prof. D. H. Campbell, on the new flora of Krakatau (Krakatoa). When the island was first visited by scientific men two months after the great eruption of 1883, the whole surface was buried under a layer of ashes and pumice averaging 90 feet in depth, and in some places reaching double this depth. A clean sweep of living organisms was thus made. Except phosphorus and nitrogen, the elements necessary for plant-life existed in the ashes. By 1886, when the island was visited by Dr. Treub from Java, a number of plants had already established themselves, slimy, blue-green algæ being of special importance in preparing the soil for higher plants. The plants in the interior were found to be quite different from those near the coast, and the preponderance of ferns was remarkable. Since 1897, when another visit was paid, the progress of the new flora has been rapid, this being especially noticeable in 1906 in the case of the forest trees, which make it evident that the island will ere long be as densely afforested as ever. Nitrogen-forming bacteria have played an important part in rendering the soil fitted for vegetation. As regards distributional agencies, there seems no doubt that the earliest plant-immigrants, such as bacteria, blue-green algæ, ferns, and mosses, were wind-borne, and the same is probably true of the first phanerogams, Compositæ, and grasses to reach the island. On the other hand, ocean-currents have probably been the chief agents in transporting seeds and fruits, those of the strand-plants being almost certainly water-borne.

AN article on the American Forest Service contributed by Mr. T. S. Woolsey to the *Indian Forester* (June and July) for the information of foresters in British India will certainly interest readers who follow the development of scientific forestry. It is mentioned that the actual administration of national forests in America by trained foresters only dates back four years, and in that time opposition has been overcome and public opinion educated by the publications which have been liberally distributed. The general nature of arrangements for timber sales is described in detail, and the outlines for working plans on national forests are indicated. Research at the experiment stations is concerned with tree and stand studies, run-off and ground-water measurements, and meteorological observations.

IN consequence of the observation that lumps of gum are occasionally exuded by the bromeliad, *Guzmannia zahnii*, an investigation of other plants belonging to the same family was undertaken by Mr. K. Boresch. It was then found that several allied plants, notably *Aechmea pineliana*, showed the same phenomenon. The gum passages are situated in the cortex of the stem; they usually originate lysigenously and the adjoining parenchymatous cells frequently grow in like thylloses; although not definitely ascertained, it seems probable that the process is pathological. Incidentally, the author traced a meristematic zone which gives rise to a thickening of the stem, as in Ruscus. The paper appears in the *Sitzungsberichte der kaiserlichen Akademie der Wissenschaften*, Vienna (vol. cxvii., part viii.).

THE first portion of a study of various morphological features in the Umbelliferæ is contributed by Dr. K. Domin to the *Bulletin International* (1908), published by the Académie des Sciences de l'Empereur François Joseph I. Various interesting points are noted regarding the seedlings, *e.g.* the tubular shape of the cotyledon stalks in species of Ferula, the occurrence of so-called monocotyledonous embryos as in Bunium, and three cotyledons observed in various species of Eryngium. The formation of the tuber in such genera as Smyrnium and Bunium is traced to the hypocotyl, which also produces roots above the tuber. The most important statement refers to the stipules, which are stated to be universally present and free in all species of Hydrocotyle; Schizeilema and Huanaca possess adnate stipules, while species of Bowlesia show various modifications from an ochrea to laciniate appendages.

THE chief of the U.S. Meteorological Service decided to make some important changes in the scope and character of the *Monthly Weather Review*, beginning with July 1. The data are now grouped according to natural topographic districts; for this purpose the United States has been divided into twelve climatological districts conforming to its principal drainage areas, each being under the supervision of a selected division director. The Review is to be devoted almost exclusively to the publication and discussion of climatological, river, and forecast data; special articles of a scientific nature, but not strictly climatological, will be published in the Bulletin of the Mount Weather Observatory or in separate form. The editors (Messrs. Abbe and Abbe, jun.) will prepare, as hitherto, brief notes on the progress of meteorological science throughout the world, so that the Review may still mark the development of the science, without publishing extensively the details of meteorological papers.

WE have received a reprint of an article on "The Tides: their Causes and Representation," published by Mr. R. A. Harris in the June number of the *Popular Science Monthly*. The article is illustrated by useful charts showing the co-tidal lines in the different oceans.

A NEW rainfall map of the Balkan peninsula, compiled by Herr Franz Trzebitzky, appears in the August number of *Petermann's Mitteilungen*. The data are obtained from 380 stations, 95 in Croatia and Dalmatia, 93 in Bosnia and Herzegovina, 50 in Servia, 105 in Bulgaria, 1 in Montenegro, 12 in Turkey, and 24 in Greece. The averages are reduced to the period 1894–1905.

THE *Zeitschrift* of the Berlin Gesellschaft für Erdkunde (1909, p. 361) publishes a lecture delivered before the society by Prof. Hecker on the determination of the value of gravity and its application to the problem of the distribution of mass in the earth's crust. A general outline of modern methods is given, and a summary of the most important results obtained from observation.

THE *Mitteilungen* of the Vienna Geographical Society (1908, p. 150) contains a very interesting and suggestive paper by Dr. Max Müller on the graphical solution of a number of problems in "astronomical" geography. Some ten important problems are fully discussed, and methods of solution with the help of rule and compasses alone described. A final example shows how to determine the approximate latitude and longitude of a place, from two observations of the sun's altitude, using a large globe.

PROF. HAMMER, discussing a paper on the forty-ninth parallel by Dr. Klotz (Journal of the Royal Astronomical Society of Canada, 1908, p. 282), deals with the determination of the boundary line between Canada and the United States west of the Lake of the Woods in a note in *Petermann's Mitteilungen* (viii., p. 188). The extraordinary difficulties inherent in the accurate laying down of a parallel of latitude are dwelt upon with great emphasis.

THE director-general of the Egyptian Survey Department has issued a reprint of a technical lecture—one of a series of such lectures—by Mr. J. I. Craig, on map projections. The general principles of the subject are outlined, and examples given of projections employed for different kinds of maps. The construction of a network for a special purpose is illustrated in an extremely interesting manner by a "Mecca azimuthal" projection, which is designed to give the true bearing of Mecca from every point of the map. As the author remarks, "its usefulness in finding the direction of the *Qibla* is evident."

THE June number of the *National Geographic Magazine* contains an article by Mr. Charles E. Fay on "The World's Highest Altitudes and First Ascents," which is accompanied by some remarkably beautiful illustrations. Mr. Milnor Roberts describes the Mount Rainier National Park in an article entitled "A Wonderland of Glaciers and Snow," and an interesting table, compiled by Mr. N. H. Darton, gives the highest point in each of the States. Mr. Hugh M. Smith, deputy commissioner of the U.S. Bureau of Fisheries, contributes a graphic account of "Brittany, the Land of the Sardine."

THE *Journal de Physique* for August contains the address delivered by Prof. Paul Janet to the Société française de Physique in April, on the history and the present position of the question of the fundamental electrical units. After giving a brief history of the various commissions which have dealt with the subject, he gives in detail the decisions arrived at under the headings ohm, ampere, volt, and states to what extent these decisions have been adopted officially by the various Governments. He is of opinion that the action of many of these Governments in adopting particular numbers as official has been rather precipitate, and would prefer them to wait until the institution of an international electrical laboratory has permitted comparisons to be made between the electrical standards kept at the various national laboratories.

THE second of a series of papers dealing with the phenomena exhibited by electric arcs between metal electrodes appears in the *American Journal of Science* for August. It is by Messrs. W. G. Cady and G. W. Vinal, and treats of the oscillations which, under certain conditions, can be produced. If an electromotive force of several hundred volts is connected through a variable resistance to an air-gap having, say, a copper kathode and an anode of any conducting material, and an arc is started, on increasing the resistance until the current falls to about 0·4 ampere the arc begins to oscillate with a frequency which in general lies between 1000 and 50,000 per second, and finally, as the resistance in series is increased, becomes a glow discharge or is extinguished. The authors explain with the aid of the volt-ampere curve the conditions which control the production of these oscillations, and give a provisional theory which covers the phenomena observed up to the present time.

ENGINEERING units of measurement form the subject of a pamphlet which has just been produced by Mr. J. Ramsay, of the Glasgow and West of Scotland Technical College. The greater part of the thirty-six pages consists of definitions of the quantities and symbols which more commonly occur in engineering, and in each case the author gives both British and metrical units, together with the connection between them. Several useful tables are given at the end. The value of the pamphlet will be appreciated when it is remembered that students of engineering in this country are compelled to use both British and metrical systems of measurement, a condition which tends to produce much mental confusion and hinders progress. While most of the author's explanations are good, we do not think that his remarks on pp. 8 and 9 regarding weight, mass, and gravitational and dynamical units of force are sufficiently clear; but few writers have succeeded in producing absolutely convincing statements when they take, as the present author does, the British gravitation unit of force as the force with which the earth attracts a pound weight at the sea-level at Greenwich, and also a unit of mass of 32·2 lb. The publishers of the pamphlet are Messrs. John Smith and Son, Glasgow, and the price is 1*s*. net.

## OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES IN SEPTEMBER:—

Sept. 1. 16h. 0m. Mars in conjunction with Moon (Mars 1° 4′ S.).
2. 17h. 0m. Saturn in conjunction with Moon (Saturn 1° 14′ N.).
3. 9h. 2m. to 9h. 50m. Moon occults $\xi'$ Ceti (mag. 4·5).
7. Maximum of Mira Ceti (mag. 3·3–8·5).
10. Saturn. Major axis of outer ring = 45·31″, Minor axis = 10·00″.
15. 12h. 5m. Minimum of Algol ($\beta$ Persei).
16. 22h. 0m. Mercury at greatest elongation (26° 34′ E.).
18. 8h. 54m. Minimum of Algol ($\beta$ Persei).
23. 22h. 0m. Mars at opposition to the Sun.
,, 15h. 3m. Uranus in conjunction with Moon (Uranus 2° 35′ N.).
,, 3h. 39m. to 4h. 53m. Moon occults $\sigma$ Sagittarii (mag. 2·1).
28. 15h. 0m. Mars in conjunction with Moon (Mars 0° 9′ N.).
30. 0h. 0m. Saturn in conjunction with Moon (Saturn 1° 8′ N.).

THE SOUTH POLAR SPOT ON MARS.—With regard to the recent observation by M. Jonckheere, Dr. Lohse records in No. 4348 of the *Astronomische Nachrichten* (p. 61) that he observed the bright patch which has detached itself from the polar snow-cap on August 8. The position of the spot, in areographical coordinates, was:—longitude, 304·5°; latitude, −74·5°. A measure of the south polar spot gave a diameter of about 30°.

COMET 1909*b* (PERRINE, 1896 VII.).—The position of comet 1909*b*, according to the ephemeris given in No. 4348 of the *Astronomische Nachrichten*, on September 3 will be $\alpha$ (1910·0) = 1h. 12·9m., $\delta = +46° \ 24·8'$, whilst that on September 15 will be $\alpha$ = 2h. 0m., $\delta = +51° \ 32·9'$. Thus we see the comet is passing from Andromeda to Perseus, and on September 9 will pass about half a degree north of $\nu$ Persei; at the same time it is approaching both the earth and the sun, and is now about one magnitude brighter than when re-discovered.

THE ORBITS OF CERTAIN SPECTROSCOPIC BINARIES.—Nos. 15 and 17, vol. i., of the Publications of the Allegheny Observatory deal, respectively, with the orbits of the spectroscopic binaries $\pi^4$ Orionis and $\zeta^1$ Lyræ. The former is discussed by Mr. R. H. Baker on the basis of thirty-six spectrograms obtained with the single-prism Mellon spectrograph. The orbit is nearly circular, the eccentricity, in the final elements, being given as 0·027 ± 0·013, and the length of the semi-major axis is 3,393,000 km.; the amplitude of the velocity-variation is 51·8 km., and the period is 9·5 days. The spectrum is of the helium type, but does not show the spectra of both components. Mr. F. C. Jordan finds, from the discussion of sixty-four plates, that the orbit of $\zeta^1$ Lyræ is circular and the period is 4·29991 days; the amplitude of the variation is 102·48 km.

Mr. Jordan has also observed four of the stars in Taurus which Prof. Boss suggested belonged to a group having a common movement. He finds (Publication No. 16) that two of the four stars, Piazzi 234 and Bradley 716, give results in accordance with the idea that they belong to a cluster; the other two, 51 Tauri and $\iota$ Tauri, appear to have variable velocities.

THE BOLIDE OF APRIL 20 AS OBSERVED IN FRANCE.—The August number of the *Bulletin de la Société astronomique de France* contains a number of drawings and descriptions (pp. 357–61) of the remarkable meteor seen on April 20 at about 10 p.m. This meteor traversed Ursa Major, leaving behind it a train which lasted for about two minutes as a naked-eye object, according to M. Quénisset, and could be seen for five minutes with a prismatic binocular. The train moved in an east-and-west direction, and developed a condensation, which is shown by some observers as being at one side of a break in the train, and by others, M. Quénisset among them, as a bright loop. The brightness of the meteor was about equal to that of Venus at its brightest, whilst that of the train was comparable with the brightness of the Milky Way.

## MATERIA MEDICA AMONG THE ZULUS.

IN the July number of the Annals of the Natal Government Museum, Father A. T. Bryant, a competent observer of native life and author of a valuable dictionary of the tribal language, has for the first time collected materials for the study of Zulu materia medica and the methods of the local medicine-man. He records some 240 Zulu plants used in medicine, giving what the people believe to be their properties and the modes in which they are administered to the patient. Here, as among other savage races, the medicine-man was a personage originally distinct from the diviner or so-called witch-doctor; but their functions tend occasionally to overlap, the medicine-man dealing largely in magic and charms, while the witch-doctor makes himself familiar with curative herbs, though his real business is to indicate or "smell out" the agency which is supposed to have caused the illness.

The Kafir medical man has no knowledge of pathology. He knows as much of anatomy as can be learned from cutting up cattle for food; but the nervous system is a complete mystery to him, and though he has observed that the blood runs through the body, he does not associate its circulation with the beating of the heart. He works by the examination of symptoms, though he is ignorant of their cause, treating paraplegia, for instance, by local applications, and not connecting its occurrence with any brain disease. His occasional successes seem to be generally due to the influence of suggestion, by exciting the feeling of confidence or imagination which summons into action the remarkable recuperative powers of the patient. In his profession medicine and magic constitute a single art, and he is called upon to combat, not only the disease which has actually shown itself in the system, but also the machinations and forms of the black art which are believed to have induced it.

Like most savages, the Zulu is unusually susceptible to new diseases, though he is hardened against those which are old. Father Bryant gives interesting details of the more common diseases and their popular treatment. He records a form of disease, believed not to be known to medical science, resulting from an intestinal parasite developing into a species of beetle. The local form of phthisis seems to be different from that of Europe, the former setting in at the bottom, the latter at the top, of the lung. The medicine-man deals largely in blood-letting, poulticing, the use of ointments, the clyster and the emetic. He knows many of our standard remedies, while of others equally accessible to him he is ignorant. Thus he uses indigenous species of Nephrodium for the relief of tape-worm, and croton as a purgative, but it is apparently from the white man that he has learned that the *Ipomoea purpurea* has qualities analogous to jalap, and though castor-oil is used for dressing hides he is not aware of its medicinal value. But he undoubtedly is acquainted with a great number of simples, mostly vegetable; and Father Bryant believes that the further investigation, and in particular the chemical examination, of many of the drugs which he names will in all probability add valuable remedies to our pharmacopœia. Dr. E. Warren, curator of the museum, promises that his department will provide all possible assistance in material and information to any competent chemist who is prepared to undertake such an inquiry.

## AMERICAN INVERTEBRATES.

BULLETIN No. 63 of the United States National Museum is devoted to a monographic revision, by Mr. F. E. Blaisdell, of the beetles of the Eleodine section of the family Tenebrionidæ inhabiting the United States, Lower California, and the adjacent islands. The memoir includes 534 pages of text and thirteen plates.

In No. 2 of the Leland Stanford Junior Publications of the University of California Prof. F. M. Macfarland describes in considerable detail the anatomy of the opisthobranchiate molluscs obtained during the Brauner-Agassiz expedition to Brazil in 1899. The collection, although small, adds seven to the list of Brazilian species of the group; and since little was previously known with regard to the structure of the opisthobranchs of the district, the opportunity was taken of studying this as minutely as the amount of material permitted. A number of diagrammatic figures of the radula in different genera is given.

The feather-stars, or ophiurids, of the San Diego region form the subject of vol. vi., No. 3, of the University of California Publications in Zoology. The author, Mr. J. F. McClendon, began his investigation in the hope that a taxonomic and biological study of the local members of the group might facilitate work in which it was important to know the breeding-seasons and habitats of different species, but, unfortunately, he could not remain long enough to obtain all the data desired. It is believed, however, that the height of the breeding-season for most of the species is in the spring, although individuals full of apparently unripe eggs were taken in spring.

A number of new fossil echinoderms from the Cretaceous and Tertiary Ripley beds of Mississippi are described and figured by Mr. A. W. Slocum in vol. iv., No. 1, of the Geological Publications of the Field Museum, Chicago.

The re-arrangement of the large collection of graptolites which for many years has been in course of formation in the U.S. National Museum has afforded to Mr. R. S. Bassler the opportunity of revising the species of the dendroid group from the Niagaran Dolomite of Hamilton, Ontario, and the results of his studies are published, with a large number of illustrations, in Bulletin No. 65 of the museum.

## THE SEVEN STYLES OF CRYSTAL ARCHITECTURE.[1]

THE proverbial importance of the number seven is once more illustrated in regard to the systems of symmetry exhibited by solid matter in its most perfectly organised form, the crystalline. For there are seven such systems or styles of architecture of crystals, just as there are seven distinct notes in the musical octave, and seven chemical elements in the octave or period of Newlands and Mendeléeff, the eighth or octaval note or element being but a repetition on a higher scale of the first.

A crystal appeals to us in two distinct ways, first compelling our admiration for its beautifully regular exterior shape, and next impressing us with the fact of its internal homogeneity, expressed in the cases of transparent crystals by its perfect limpidity, and the obvious similarity throughout its internal structure. As it is with human nature at its best, the external appearance is but the expression of the internal character.

The purpose of this discourse is not so much to dilate upon the seven geometrical systems of crystals as to show how they are occasioned by differences in the internal structure, and to demonstrate this internal structure in an ocular manner, unfolding at the same time some interesting phases of recent investigation.

To the Greeks, whose wonderfully perfect knowledge of geometry we are ever admiring, the cube was the emblem of perfection, for like the Holy City, lying "foursquare," described in the inimitable language of the book of Revelation, "The length and the breadth and the height of it are equal." Moreover, even when we have added that all the angles are right angles, these are not the only perfections of the cube, for they carry with them, when the internal structure is developed to its highest possibility, no fewer than twenty-two elements (thirteen axes and nine planes) of symmetry.

At the other extreme is the seventh, the triclinic, system, in which the symmetry is at its minimum, neither planes nor axes of symmetry being developed, but merely parallelism of faces, sometimes described as symmetry about a centre, and in which there are no right angles and there is no equality among adjacent edges. Between these two extremes of maximum and minimum symmetry we have the five systems known as the hexagonal, tetragonal, trigonal, rhombic, and monoclinic, possessing, respectively, 14, 10, 8, 6, and 2 elements of symmetry. All crystals do not possess the full symmetry of their system, each system being subdivisible into classes possessing a definite number of the possible elements. Altogether there are thirty-two such classes, and their definite recognition we owe to the genius of von Lang and Story Maskelyne.

The characteristic property possessed in common by all crystals is that the exterior form consists of and is defined by truly plane faces, inclined, in accordance with one of the thirty-two classes of symmetry, at specific angles which are characteristic of the substance. This has only been proved to be an absolute fact within the last few years, although asserted by Haüy so long ago as the year 1783; for the numerous cases of so-called "isomorphous" salts, the first of which were discovered by Mitscherlich in the year 1820, were for long believed to be exceptions, and until the year 1890 no actual evidence one way or the other was forthcoming. But it was eventually shown that the crystals of the members of an isomorphous series did differ, both in their angles and in all their other crystallographic and physical properties, although in the cases of the angles the differences were very small. Moreover, the differences were shown to obey a simple but very interesting law, namely, that they were functions of the atomic weight of the chemical elements of the same family group the interchange of which gives rise to the series.

All crystals possess one other obvious property, that of homogeneity, and we now know that it is the character of the homogeneous substance which determines the external form. There are no fewer than 230 different kinds of homogeneous structures, neither more nor less, the elucidation of which we owe to the independent recent labours of Schönflies, von Fedorow, and Barlow; and it is a significant fact that the whole of them fall naturally into the thirty-two classes of crystals, leaving no class unaccounted for. Of these 230 modes of regular repetition in space fourteen are the space-lattices long ago revealed to us by Bravais, and all recent investigation concurs in indicating two facts, first, that it is the space-lattice which determines the crystal system, and second that it is the arrangement of the chemical molecules which is represented by the space-lattice. Each cell of the space-lattice corresponds to a molecule. The structure is certainly not solid throughout, however, part only being matter, and the rest æther-filled space, the relative proportions and the shape of the material portion being as yet unknown. We limit ourselves, therefore, to considering each molecule as a point, and we draw the lattice as a network of three systems of parallel lines, parallel to the directions of the three principal crystal edges, analogous, according to the system of symmetry, to

[1] Summary of evening discourse delivered before the British Association at Winnipeg on August 26 by Dr. A. E. H. Tutton, F.R.S.

those of the cube. The points of intersection we consider as those representing the molecules, inasmuch as any point within the limits of the cell may equally well be taken to represent the cell and the molecule, provided the choice is analogously made throughout the structure.

It has recently been found possible to determine the relative dimensions of these molecular cells, the distances of separation of the points of the space-lattice, in those cases where we know that the structure is similar, as in isomorphous salts; and the interesting discovery has been made that the "molecular distance ratios," as these space-dimensions are called, are functions of the atomic weights of the interchangeable members of the family of chemical elements constituting the series, just as the crystal angles have been shown to be.

We are now able, moreover, to take yet one further step, for the chemical molecules are composed of atoms, and it has been indubitably shown that the atoms occupy definite positions in the crystal. For when we replace, say, the alkali metal in a sulphate or selenate by another, we observe a marked alteration in the crystal angles and the molecular distance ratio along a particular direction, this direction being the same whichever metals of the group are interchanged; whereas if we replace the sulphur by selenium, a similar kind of alteration occurs, but along a totally different direction. Now we know that the atoms are arranged in the chemical molecule in what is known to chemists as their stereometric arrangement, depending on the maximum satisfaction of their chemical affinities. Hence this important experimental fact of the occupation by the atoms of definite positions in the crystal proves, first, the homogeneous similarity of arrangement of the molecules, and, secondly, explains why we have classes or subdivisions within the systems. For it is the arrangement of the atoms within the molecule which causes the variations of the degree of symmetry, within the limits prescribed by the system and space-lattice; in other words, which determines the class.

Now obviously any one of the atoms in the molecule may be chosen to represent the latter, and the points thus chosen analogously throughout the structure will constitute the molecular space-lattice. Hence the whole structure may be considered as made up of as many interpenetrating similar space-lattices as there are atoms in the molecule. The crystal structure will thus be dependent on two factors, the space-lattice and the scheme of interpenetration of the space-lattices, the former dominating the style of architecture, the crystal system, and the latter the vagaries of the style, the crystal class. Sohncke has shown that there are sixty-five such vagaries possible, which he terms regular point systems, and these coincide with sixty-five of the 230 possible modes of partitioning space.

These are the broad, simple facts, now proved up to the hilt, which explain the majority of crystal structures, all, in fact, but a very few, of the more complicated classes of the thirty-two. For the remaining 165 ways of appropriating space all fall into a very small number of crystal classes. They are of very great interest, however, and involve an entirely new principle, that of "reflective" or "mirror-image" symmetry, enantiomorphism as it is technically termed, and include those crystals which possess the remarkable property of rotating the plane of polarised light. These are the cases the geometrical possibility of which has been accounted for by the simultaneously independent work of Schönflies, von Fedorow, and Barlow, and to which we were experimentally introduced by the discovery of the right- and left-handed varieties of tartaric acid by Pasteur. The latter has since been followed by the revelation of many similar cases of two forms of the same chemical substance, related crystallographically and structurally like a right hand to a left-hand glove, and optically differing by the direction in which they rotate a beam of plane polarised light.

With their discovery and explanation the elucidation of the seven styles of crystal architecture and their thirty-two subdivisions becomes *un fait accompli*, and although many difficult problems still confront the crystallographer, problems of vast importance to chemistry, the groundwork is now securely laid, the memorable achievement of the last twenty years. The results, moreover, are in entire accordance with the now well-proved fact that the chemical atom is composed of electronic-corpuscles. For the definite orientation of the atom and its sphere of influence within the molecule and the crystal is thereby accounted for, the motion in the solid state so frequently hitherto attributed to the atom being a myth, such motion relating, in fact, to the corpuscles within the atom.

## SOCIETIES AND ACADEMIES.

PARIS.

**Academy of Sciences,** August 23.—M. Bouquet de la Grye in the chair.—The calculation of the roots of numerical equations: M. **Lémeray.** Referring to a recent paper by M. de Montessus on this subject, the author points out that he published a method on the same principle in December, 1898.—Movements from the vertical due to the attraction of the moon and sun, the earth being supposed absolutely rigid: Ch. **Lallemand.**—A poison elaborated by yeast: A. **Fernbach.** It has been recently shown by F. Hayduck that there exists in the yeast cell a substance which is toxic to yeast. The present paper contains some new observations on the same subject. A solution of the toxic substance is shown, not only to be antiseptic to yeast cells, but also to bacteria such as *B. coli* and Staphylococcus, although it has no effect on moulds. The most remarkable property of this substance is that it is volatile under reduced pressure at a temperature not above 40° C. The distillate contains no formaldehyde; further researches on the nature of this substance are in progress.—The development of the eggs of *Philine aperta* exposed to the action of radium: Jan **Tur.**

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