THURSDAY, OCTOBER 1, 1908.

MATHEMATICAL ASPECTS OF ELECTRICITY AND MAGNETISM.

The Mathematical Theory of Electricity and Magnetism. By Prof. J. H. Jeans, F.R.S. Pp. viii+536. (Cambridge: University Press, 1908.) Price 15*s*. net.

ELECTRICITY and magnetism now form so vast a subject that their mathematical aspects cannot be all dealt with in a single volume even of the present size. Thus a choice has to be made by the author, and one's estimate of his success naturally depends to some extent on what one believes most appropriate for the type of reader whose wants he professes to supply. In his preface the author tells us that whilst his work covers much the same range as Maxwell's treatise, it is in many respects more elementary; that it is not, like Maxwell's great work, for the fully equipped mathematician, but more especially for the student and for the physicist of limited mathematical attainments.

The difficulties experienced by a good mathematician in Maxwell's treatise arise more from what it omits than from what it contains. The difficulty lies in following Maxwell's train of thought, and in seeing what exactly it is he is trying to prove. There is a substantial substratum of truth in the remark once made to the writer that it would have been an immense improvement to Maxwell's "Electricity" to have been written by Routh. Maxwell's treatise is a work of genius, but it never was a good text-book for students. The distinguished editors of the second and third editions have very naturally treated the treatise as a species of sacred writing, not to be lightly modified even in details, and though Prof. J. J. Thomson's "Recent Researches" appeared as a supplementary volume to the electrical part, it is not a complete treatise in itself. Thus there does appear room for a complete mathematical treatise in English, such as might be written by an accomplished mathematician who had the time, the knowledge, and the natural gifts necessary for clear exposition. The production of such a book, it may be added, need not be regarded as showing any lack of reverence for Maxwell's memory.

Now it seems to the present writer that while Prof. Jeans's eye when he started writing may have been focussed on the reader of "limited mathematical attainments," it gradually extended its range of vision until it viewed in the distance, though but dimly, the complete treatise hinted at above. The result is that the book seems not unlikely to reduce the self-esteem of any conscientious reader of limited mathematical attainments who has no one at hand to advise him what to omit, at least for a first reading.

Chapters i. to viii., pp. 5–294, *i.e.* more than half the book, are devoted to electrostatics. This apparent disproportion the author ascribes to space being given in chapter viii. to the explanation of the mathematics of spherical and ellipsoidal harmonics, conjugate functions, Schwarz's transformation, and similar matters. This chapter is a very long one, dealing also with inversion and the theory of images, and containing the solution of problems which illustrate the various methods. Chapters ix. and x. deal with currents, mostly steady currents; chapters xi. and xii. treat of "permanent" and "induced" magnetism, including a few pages on terrestrial magnetism. Chapter xiii. deals with the magnetic field produced by steady currents, chapter xiv. with the induction of currents in linear circuits, make and break currents and oscillatory discharges, and chapter xv. with the induction of currents in continuous media and current sheets. The three last chapters, xvi., xvii. and xviii., treat of Hamilton's principle, Lagrange's equations, the general electromagnetic equations, and the electromagnetic theory of light.

An important feature of the book is the insertion of examples for the student at the end of most of the chapters. In all, there are some 250 of these, varying much in difficulty, but mostly of the type characteristic of Cambridge college and university examinations. Another feature is the insertion of numerical results in the text illustrating the size of practical electrical units; these should reduce the risk of mistakes in applications of general formulæ. There are not infrequent references in the text to physical results calculated to warn the student against improper applications of the mathematical theory, but they do not always seem quite adequate. The statement, for instance, p. 400, that magnetic permeability in iron continually increases as temperature is raised up to the point of recalescence is too general. At the end of each chapter is a list of authorities. These lists are, however, mainly devoted to stating which precise part of a few English books (especially Maxwell's treatise and J. J. Thomson's "Elements") deals with the subject of the chapter. A single really good name-index would probably be more generally useful. In the absence of a name-index, the general index, pp. 532–6, seems hardly adequate. It does not contain, for instance, the names of Kelvin, Larmor, Lodge, Rayleigh or Thomson. Amongst the subjects to which little space is devoted are methods of measurement and comparison, the theory of instruments, problems relating to dynamos, electric lighting or traction, and electrical engineering generally, rapidly alternating currents, detailed theories of electrons or moving charges, atmospheric electricity and conduction in gases.

The printing and general appearance are what one expects of the Cambridge University Press. Even Cambridge proof-readers, however, must occasionally miss something, *e.g.* $\delta^2V/\delta x$, p. 59, and $\iiint dS$, p. 372.

The signs attached to the Gaussian constants B_{11}, B_{42} and B_{44} (Neumayer's values) in art. 456 appear to be wrong; but B_{11} is given the correct sign in art. 457. It seems curious, by the way, that, notwithstanding the great prominence given to Cambridge sources of information, there is no reference to J. C. Adams's great work on the Gaussian constants, or to Shelford Bidwell's article on magnetism in the *last* edition of the "Encyclopædia Britannica" (Prof. Jeans's references seem all to the ninth edition).

In a few cases there are slips which can hardly be assigned to the printer, *e.g.* in the analysis of art. 520, but few such have been noticed. In conclusion, it may be said that in the opinion of the present writer the type of reader for whom the book is best adapted is the student preparing for a mathematical examination, such as the Cambridge Tripos, in which theory plays the principal part. It should also, however, prove a good book of reference to the physicist of superior mathematical attainments. For either of these types of readers it seems likely to be a really useful book, so far as its scope extends.

C. CHREE.

PETRELS.

A Monograph of the Petrels (order Tubinares). Parts i., ii. and iii. By Dr. F. Du Cane Godman, F.R.S. With hand-coloured plates. (London: Witherby and Co., 1908.) Price 2*l*. 5*s*. per part.

WE welcome another instalment of the finely illustrated "monographs" in which ornithologists are gradually, if slowly, writing the history of the birds of the world. The latest of these monographs to be launched is founded on the synopsis of the order Tubinares, published by the late Mr. Salvin in the twenty-fifth volume of the "Catalogue of the Birds in the British Museum." It was Mr. Salvin's intention on the completion of that work to have issued a series of coloured illustrations representing all the species of petrels, shearwaters, fulmars, diving petrels, and albatrosses, which constitute the order Tubinares, and at the time of his death in 1897 many of the plates had been prepared. The present author has had the series of coloured plates completed, and he is now issuing them in the form of a monograph, adding such synonymy and accounts of the geographical distribution and habits of the species as Mr. Salvin originally intended, and bringing the work up to date.

Since the twenty-fifth volume of the British Museum Catalogue was issued in 1896, considerable additions to our knowledge of the Tubinares have been made. Some remarkable discoveries have been made by American ornithologists in the seas of California and the islands which lie off the south-western coast of North America, and Sir Walter Buller's supplement to the "Birds of New Zealand" has added to our knowledge of the group. Moreover, from the observations made by the naturalists to several recent Antarctic expeditions, we have learned a great deal about the breeding habits of certain well-known species, which, retiring to those inhospitable regions for the purpose of reproduction, had up to then managed to keep us very much in the dark as to the manner of their nesting. The material thus examined has enabled the author to undertake the revision of the Tubinares with some confidence. The order as at present known embraces more than one hundred species.

The first three parts of the work are now in our hands, and fully carry out so far as they go the objects set forth in the author's note. The first part deals with the smaller petrels of the following genera:—Procellaria, Halocyptena, Oceanodroma, Oceanites, Gorrodia, Pelagodroma, Pealea, and Cymodroma (in part). First on the list comes our own familiar stormy petrel—the petrel *par excellence*—of which a very good and concise history is given, including synonymy, geographical distribution, breeding, and general life habits, and a full description of the plumage of the adult and young; and this is the general plan of the work.

Twenty-four species are treated of in sixty-eight pages of letterpress, and there are twenty coloured plates. Part ii. concludes Cymodroma, and deals with twenty-four species of the large genus Puffinus, the shearwaters, in sixty-four pages, with twenty coloured plates. In this part may be noticed the great shearwater, of which, though the bird was known to Latham so long ago as 1785, and although it sometimes appears in large numbers off our own coasts, the breeding place is still unknown. It can scarcely, however, be doubted that this must be sought for in southern latitudes, and in our winter.

Part iii. concludes Puffinus, and treats of four restricted genera and twenty-three species of the extensive genus Œstrelata. Among the former we find the silvery-grey fulmar of the southern oceans, which in general appearance so much resembles the fulmar of the north that Latham and Gmelin described it as a variety of that species. Also the great dark-coloured petrel familiarly known to sailors as the Cape Hen. The casual occurrences of *Œstrelata haesitata*, *Œ. brevipes*, and *Œ. neglecta* in the British Islands are wonderful instances of the wandering habits of these small fulmars. The accounts given of the breeding and general life habits of these ocean wanderers, the gliding flight of which has so often beguiled the monotonous hours of the passengers on liners, are very interesting; and those who often have occasion to go on long voyages (in the southern seas especially), and take an interest in the birds they see, would do well to study the plates, at all events, in this fine work, and so have a chance of learning (roughly speaking) the names of the petrels which may on some days be seen from the deck in great numbers. But specimens of these birds are very rarely secured, and no opportunity should be lost of preserving any that by a lucky chance should come into the traveller's hands; for some species are known from single specimens only, and others from but little more.

The work will be completed in five quarterly parts. It is beautifully printed on rag paper, and we need only say of the plates that they are by Mr. Keulemans, and drawn and coloured under the most careful supervision. This means that they are as near perfection as it is possible for ornithological plates to be.

O. V. A.

OUR BOOK SHELF.

Das Weltgebäude, Eine gemeinverständliche, Himmelskunde. By M. Wilhelm Meyer. Zweite, umgearbeitete Auflage. Pp. xii+691. (Leipzig and Vienna: Bibliographisches Institut, 1908.) Price 16 marks.

TEN years ago we read the first edition of this work with considerable satisfaction, and the examination of the second edition has been interesting, since it shows how much astronomical science has advanced in the interval, and demonstrates the necessity of frequent revision of popular text-books of this character. In some respects the work has already fallen behind—inevitable from the time required to pass such a book through the press—and though an appendix is not a convenient form for supplying the most recent information, it might have been adopted here with advantage. If we may trust the index and a very careful scrutiny of the text, there is no reference to the eighth satellite of Jupiter. On the same grounds we think that justice has not been done to the energy and success with which Prof. Hale has pursued his investigations, and the references to Sir David Gill are meagre. Altogether the revision does not seem to have been made with sufficient thoroughness; there has been too great a reluctance to sacrifice the material prepared for the earlier edition, with the result that the author has retained references to earlier work which has been superseded by the employment of larger means and greater experience. But though one may regret that in some particulars the work might be improved, it still possesses very high claims to consideration. The main facts are presented in an admirably attractive manner, leaning perhaps, where opportunity offers, to the sensational side; but nevertheless the description is trustworthy, likely to captivate the amateur, and gain recruits to the study of astronomy.

The book is divided into two parts, the first devoted to purely descriptive astronomy, the other to explaining the motion of the heavenly bodies. In the first part, after an introduction explaining the optical principles of the telescope and its application to photography and spectrum analysis, we have a series of chapters describing the several members of the solar system, including comets and meteors, and finally the sun itself. Next follows a description of the stellar universe, separate chapters being assigned to the classification of stellar spectra; the nebulæ and star clusters; the Milky Way; double and variable stars. In this section the author's task is comparatively simple. No material fact must be omitted; but in the second part, that treating of motion, he has to exercise selection, both of the subject itself and its method of treatment. Mathematical completeness is impossible, and therefore the chapters on planetary motion, solar parallax, aberration and precession, are scarcely satisfactory. But readers for whom the book is intended will no doubt find the treatment adequate, and those who desire a more thorough discussion would not look for it here.

Practical Coal Mining. By Leading Experts in Mining and Engineering. Edited by W. S. Boulton. Divisional-volume v. Pp. viii+176. (London: The Gresham Publishing Company, n.d.) Price 6*s*. net.

IN NATURE of May 23, 1907, and of March 19, 1908, notices were published of previous instalments of this work, which, when completed in six volumes, is intended to cover the whole ground of modern coal-mining practice. Each section of the work is written by a different author, a division of responsibility that renders a certain want of harmony in the treatment of the subject-matter inevitable. This defect is less noticeable in the present volume than in the four preceding volumes. Mr. James Ashworth's contribution on lighting covers 56 pages, with 125 excellent illustrations, and gives a concise review of the history of the safety lamp, and of the safety lamps in use at the present day. The importance of lamp housing is urged, and useful rules are given for the use of safety lamps. Mr. W. Galloway devotes 70 pages with 30 illustrations to a masterly essay on colliery explosions and rescue appliances. This contribution is also issued separately by the publishers. The subjects of fire damp, blasting, coal dust and rescue appliances are fully discussed. Mr. Galloway's new departure in the method of explaining great colliery explosions whereby coal-dust is elevated to the rank of principal agent was received unfavourably and long rejected by many as illusory. Now, even the French engineers, after the Courrières explosion, have come into line with those of other countries, after having strenuously opposed the so-called coal-dust theory for thirty years. Mr. H. F. Bulman gives a brief account of mineral holdings, covering 20 pages. Lastly, there is the beginning of what promises to be a most useful section on mine surveying by Mr. L. H. Cooke.

LETTERS TO THE EDITOR.

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

Photographs of Comet *c* 1908 at the Royal Observatory, Greenwich.

LONG-EXPOSURE photographs of comet *c* 1908 have been obtained with the 30-inch reflector on September 6, 7, 8, 14, 17, 18, 21, 25, and 26, the exposure ranging from forty-five minutes to an hour (except on September 14 and 25, when exposures of twenty minutes were made). The comet possesses a bright tail, the structure of which is well shown in the photographs, to a distance of $1\frac{1}{2}°$ from the head. The appearance of the tail changes greatly from night to night, so that photographs taken at much shorter intervals than a day are desirable in order to trace the alterations in structure continuously. With this in view, on September 17 three photographs, with exposures of forty-five minutes, were taken at intervals of approximately an hour (reckoned from the middle of each exposure); decided change had occurred between the first and last photographs, and the middle photograph served to show how the transition had taken place. Efforts are being made to obtain a series of photographs at short intervals extending throughout a night, but so far the full moon and the weather have prevented this.

A photograph taken on September 21 with a portrait lens of 11 inches focus showed the tail extending to a length of 4°. W. H. M. CHRISTIE.

Royal Observatory, Greenwich, September 28.

Library Cooperation in Regard to Scientific Serials.

ABOUT two years and a half ago a short paper of mine on "Library Aids to Mathematical Research" was fortunate enough to attract a little passing attention, the object of it being to bring about friendly cooperation among the public libraries of a city or district with a view to prevent waste in the purchase of duplicates, and thereby to make a greater number of serials accessible to research students. Of course, it was intended that this cooperation should be accompanied by the publication of a hand-list giving the names of the serials, and showing workers in what libraries any given serial was certain to be found. In NATURE in particular the matter received sympathetic consideration (see vol. lxxiii., pp. 372, 413, 438, 464, 513), and from the eminence of the correspondents hopes were raised that something practically valuable would be the outcome.

In the matter of *voluntary cooperation* among the libraries, exceedingly little would seem to have been effected, and the place where the need is greatest, London, appears to be as far off as ever from possessing a reference-library in which could be consulted the whole of the literature indexed in the Royal Society's Catalogue of Scientific Papers and in the International Catalogue.

As regards the production of a *hand-list of serials*, however, a most important step has been taken, the Royal Society having decided to preface each volume of its great subject-index with such a hand-list, and having actually carried out its decision in the case of the first volume. In this volume, which has recently appeared, and in which the pure mathematics of the nineteenth century is the subject, the list extends to forty pages, and includes the titles of 700 serials, every serial's name being followed by initials indicating some library or libraries in London, Cambridge, Oxford, Dublin, Edinburgh, or Glasgow where the serial is to be seen. This list, like the index which it precedes, is an immense boon to mathematicians. All honour to those concerned in its preparation, and may the other volumes soon follow!

What now remains, in order to satisfy the reasonable demands of students of mathematics, is the preparation of a one-page supplement making the list complete up to the present day. There may be differences of opinion as to what such a supplement ought to include. My original proposal to the London Mathematical Society in 1904 was to take as a guide the list of journals published by the council of the International Catalogue, and to the extent of one subject Prof. Armstrong's ideal would thus be attained. A more thorough course would be to associate with this list the corresponding lists which form the bases of the *Jahrbuch über die Fortschritte der Mathematik* and the *Revue semestrielle des Publications mathématiques*. Doing this, I find that our supplement, to be exhaustive, would need to include between thirty and forty entries instead of four-and-twenty; as, however, a number of these would concern journals of a very elementary character, the most prudent course at the outset might be to select only those that are included in *all* the three lists. The number thus reached would be a dozen, and the following are their names:—*American Mathematical Monthly, L'Intermédiaire des Mathématiciens, Revue de Mathématiques spéciales, Zeitschrift f. math. u. naturw. Unterricht, Abhandl. zur Geschichte d. math. Wiss., Mathematikai és Phys. Lapok, Periodico de matematiche . . . , Supplemento al Periodico . . . , Il Pitagora, Boll. di Bibl. e Storia delle sci. mat., Tōkyō sugaku Butsurigaku Kwai Kiji, Proceedings of the Intern. Math. Congress.* Some even of these may not be very important, but surely so long as mathematicians are referred to them by the three standard annuals above mentioned it is eminently desirable that one should know where they can be consulted. Libraries, therefore, which possess sets of them should make themselves known at such a centre as the office of the International Catalogue, where possibly a suitable opportunity might present itself for placing the information at the disposal of the public. THOS. MUIR.

Cape Town, S.A., September 1.

Research Work on Natural Indigo.

My attention has been directed to a review of the report on indigo research work at Leeds University, recently made by Mr. Bloxam and others to the Government of India, which appeared in your issue of July 30. In the course of this review Prof. Meldola directs attention to the contention, which has been made by Mr. Bloxam, that, by means of new and improved methods of analysis, he has shown that there is yet scope for considerable improvement in the process of indigo manufacture. Prof. Meldola believes that a good case has been made out, and severely criticises the planters, and those who have advised them, for having neglected to take advantage of the possibilities which have been indicated by Mr. Bloxam's work, since he considers that therein lies the sole hope of the salvation of their industry.

As representing the planting community of Bihar, I feel bound to say a word in our defence and in that of our advisers. We cannot agree with Prof. Meldola that the only hope of the survival of our industry lies in a realisation of the possibilities which Mr. Bloxam believes to exist in the improvement of our process of manufacture. This process has, indeed, been considerably improved in recent years, and, thanks to this, to changes in our agricultural practices, and to the substitution of the Java for the Sumatrana plant, we are now in a position to turn out our indigo at half its former cost, and we have every reason to hope that, with a few seasons of favourable climatic conditions, we shall be able to compete with the synthetic product at the lowest price at which it is likely to be able to be produced. There is also, as Prof. Meldola points out, a biological side to our problem, and we anticipate that investigation from this aspect, which is yet in its infancy, will ultimately lead to considerable further benefit.

But it is nevertheless totally unjustifiable to describe our attitude towards Mr. Bloxam's work as a hostile one. It is perhaps true that we delayed calling in scientific aid to our industry too long, but investigations directed towards the improvement of indigo manufacture have now been in progress in India for ten years, and as a result of these investigations and by the aid of the new methods which have been introduced, our scientific advisers now tell us that nothing further can be done in improving the main processes. As practical business men we are inclined to accept this verdict rather than the opposed one, *not* because it mitigates our "past neglect," for surely it would not do so even if no improvement on our original process had been found possible (which is far from being the case), but because it seems to us more probable that investigators on the spot, who have been daily handling the fresh plant and the products of manufacture for a term of years, are more likely to be in a position to form a correct opinion than those who have dealt with preserved material for a comparatively short period. Further, it would seem that no motive other than an honest conviction could influence those who declare that their work has reached a conclusion.

We are, of course, totally unable to judge of the scientific arguments which have been advanced on either side in the recent controversy, but it is doing the gravest injustice to those who have advised us to imply, even remotely, that they have chosen to disregard the researches at Leeds lest they should disprove their own contentions. Far from having "deliberately brushed aside" the conclusions drawn by Mr. Bloxam and his colleagues, our advisers have devoted a great deal of time to close examination of the evidence whereon these conclusions are based. They have satisfied themselves that this evidence is erroneous, and continued investigations of the crucial points at issue have only served to confirm their original views. Nevertheless, they have repeatedly impressed upon us the enormous benefit which might accrue to our industry even if Mr. Bloxam's contentions were only partially correct and became realisable in practice, and they have urged us to use every effort to obtain an entirely independent opinion in the matter for this reason, and in spite of their settled conviction that such independent opinion is bound to confirm their own. It is solely owing to this urging on their part that we have taken steps to do this. T. R. FILGALT.

(General Secretary, Bihar Planters' Association.)

Mozufferpore, August 26.

I CAN assure the secretary of the Bihar Planters' Association that the comments upon their neglect of scientific method when they were first brought into competition with synthetic indigo which I felt bound to make were prompted solely in the interests of the native industry. It is practically conceded in the foregoing communication that there has been such neglect, and that the practical outcome of the revision of their processes has been the halving of the cost of production of the natural product in the course of a few years. This is satisfactory so far as it goes, but the main issue is still left very doubtful. In spite of the reduction of the cost of production by one-half, it appears that they are still in India at the mercy of climatic conditions, and even then, supposing these to be favourable for a few seasons, they have only "every reason to hope" that they will be able to compete with their coal-tar rival. Those who have at heart the welfare of our Indian Empire will cordially endorse the wish that their hope may be realised, but the point at issue between the report to the Indian Government and the Planters' Association is really whether finality has been reached in the way of improvement. According to the statement of the secretary, they have been advised that no further improvement in the "main processes" is possible. The results of the application of the newer methods of analysis indicate clearly enough that there is more indigotin in the plants than has hitherto been suspected. The advisers to the association certainly do "brush aside" this work done at Leeds if they authorise the secretary to state "they have satisfied themselves that this evidence is erroneous." Those who in common with myself have looked critically and, I may say, quite impartially into the evidence have come to the conclusion that the analytical methods are quite dependable. Others will no doubt corroborate this statement. After the publication of the report the Planters' Association held a meeting, at which they passed a resolution expressing confidence in, and practically endorsing the opinion of, their own advisers, in face of the new evidence offered from the Leeds laboratory. I gathered this information from a report of the meeting in one of the Indian papers, which was forwarded to me at the time. This attitude, which may fairly be described as one of hostility, would have been stiffened by the above letter were it not therein admitted that the "biological side" of the problem is still in its infancy, and that further development in this direction is anticipated. Also it is conceded that "an entirely independent opinion" in the matter (? of the manufacturing processes) is to be obtained. Thus all the contentions of those who felt the ignominy of this great Indian industry "taking its whipping in a crouching attitude" are likely to be met, and our best wishes are, it is needless to say, with the planters. If they are, by the inexorable laws of nature, beaten in the long run, it will at any rate redound to their credit that they did not succumb without a good fight.

There is one point in the foregoing letter which appears of considerable importance, and to which I should like to take the present opportunity of directing attention. The evidence of the advisers to the association is accepted because it appears that they are on the spot and dealing with the fresh plant, while the Leeds chemists have been investigating "preserved material." Now if the Leeds results by the isatin method are correct—and I repeat that I see no reason to doubt them—it follows that "preservation" leads to an increased development of indican. May not this hint be worth following up practically? In thanking the secretary of the association for his communication, I should like, in conclusion, to repeat what I said during the discussion before the Society of Chemical Industry last autumn. The results given by the newer methods of analysis may be unrealisable in practice; it does not follow that because a certain percentage of indican is present in an Indigofera leaf the corresponding quantity of indigotin, or anything approaching that quantity, can be got out of it in the factory. All that is contended is that at the present juncture the indications furnished by a scientific quantitative method render it imperative that every resource should be strained to save the native industry. Further developments will be anxiously waited for in this country.

R. MELDOLA.

I ENTIRELY agree with the opinion expressed by Prof. Meldola in his article, entitled "A Contribution to the Indigo Question," which recently appeared in NATURE (p. 296), that the case had "at one period assumed a polemical aspect most detrimental to the real cause at issue," and I write this with no desire to discuss the responsibility for this regrettable state of affairs, or to revive it. My object is to record some results recently obtained by Mr. Briggs and myself, which we had not intended publishing, but which may prove of interest in the light of Prof. Meldola's article.

Anhydrous indican was prepared according to the method of Perkin and Bloxam (Journ. Chem. Soc., vol. xci., p. 1715); its melting point was, as stated by these authors, 176–178°. A gram of this substance was dissolved in 500 c.c. of water. Two 100 c.c. samples were withdrawn from this solution, and analysed by the isatin method of Orchardson, Wood, and Bloxam (Journ. Soc. Chem. Ind., vol. xxvi., pp. 8 and 1178), and two by the persulphate method of Bergtheil and Briggs (Journ. Soc. Chem. Ind., vol. xxv., p. 734, and vol. xxvi., p. 1173). This was repeated three times with two distinct preparations of indican. The following results, expressed as the amount of indigotin (in grams) to be derived from 100 c.c. of the solutions, were obtained. The figures are means of the duplicate experiments, which agreed very closely.

	Isatin method	Persulphate method	Theory
i.	0·0841	0·0840	
ii.	0·0855	0·0845	0·0888
iii.	0·0852	0·0845	

The indirubin obtained by the isatin method was analysed by titration with titanium chloride (Knecht) in each case, and found to be 98 per cent. pure (average of six samples); the titanium chloride solution was standardised on pure iron, and also on pure indigotin obtained by sublimation under reduced pressure (Bloxam).

The indican employed was evidently not pure, the analyses indicating a purity of 94·6 per cent. in the first case, and 95·6 per cent. in the second and third, but this degree of purity is sufficiently high for the purpose of comparing the methods. The comparisons indicate that almost identical results are obtained, the mean difference being 0·7 per cent.

Another point which these figures seem to establish is the accuracy of our method of determining indigotin (at any rate, so far as the factor for the relationship between indigotin and permanganate is concerned), for it is extremely improbable that, were an error involved in this method, it would be so exactly counterbalanced by errors in the other direction in the precipitation of indigotin by persulphate as to bring the results into such close approximation with those obtained by the isatin method.

If these two points are conceded, then the main grounds on which the contention is based, that "the older methods have overestimated the indigotin content of the dried cake, and have underestimated the amount of indican in the leaf," disappear.

C. BERGTHEIL.

Sirsiah, September 2.

An Alleged Excretion of Toxic Substances by Plant Roots.

SINCE the communication entitled "An Alleged Excretion of Toxic Substances by Plant Roots" appeared in NATURE (August 27, p. 402), it seems desirable to state the exact position taken by the Bureau of Soils on the question of deleterious substances in soils and root excretions.

Abundant evidence has already been presented to the effect that substances deleterious to plant growth do exist in many soils, and are mainly responsible for the infertility therein observed,[1] and toxic substances, to wit, picoline carboxylic acid and dihydroxystearic acid, have actually been isolated and identified. In carefully controlled experiments these toxic conditions have been shown to arise as the result of the continuous growth of the same sort of plants upon the soil. In addition, it has been shown that plants like wheat excrete substances which set up toxic conditions in the medium. Toxic conditions may also arise from the presence of the decomposition products of vegetable matter in the soil. Indeed, it has been shown that very many substances naturally occurring in plants are toxic in quite small amounts. When plants containing these substances are incorporated with the soil, they may play an important *rôle* as soil constituents.

Regarding the criteria of growth, it may be said that not transpiration alone, as implied in the article referred to, but several standards of growth were employed in the investigations of this bureau, viz. weight of green tops, dry weight, transpiration, turgidity and colour of roots, chemotropic response of the roots. All these criteria are employed in determining the physiological effect of substances on plants, but no one is regarded as absolute.

The statements made in Bulletin No. 48 of this bureau were based, as was said in the note referred to, upon many thousands of pot experiments, and the conclusions seem justified by the results of that work. It is obviously possible to choose figures from any table which are apparently discordant. A comparison of the paraffin pot method of testing soils with the results of continuous plot experiments in this country has shown good agreement.[2]

HOWARD S. REED.

Agricultural Experiment Station, Blacksburg, Va.

[1] Bulletins Nos. 28, 36, 40 and 41 Bureau of Soils; Rept. Hawaii Agr. Exp. Sta., 1906, p. 37; Journ. Biol. Chem., iii., Proc. 38 (1907); Journ. Amer. Chem. Soc., xxx., 1295 (1908); *Science*, xxvii., 190, 295, 328, 329 (1908).

[2] Bull. 109, Rhode Island Agr. Exp. Sta.

SURVEYING FOR ARCHÆOLOGISTS.[1]

IV.

Simple Instruments for Measuring both Magnetic Azimuth and Altitude.

ALTHOUGH undoubtedly for final observations at any monument a theodolite must be employed, using the sun or a star in order to obtain astronomical or true bearings and so avoid all magnetic difficulties, and reversing the telescope to secure the correct altitude of the horizon; for rapid surveys there are many handy forms of instrument by means of which preliminary information can be gathered, both with regard to azimuth and, what is equally important, the angular height of the horizon.

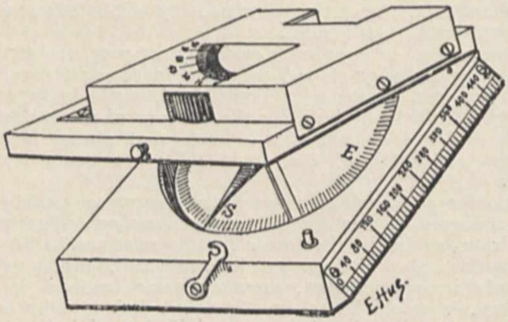

Fig. 13.—M. Hue's combined compass and clinometer.

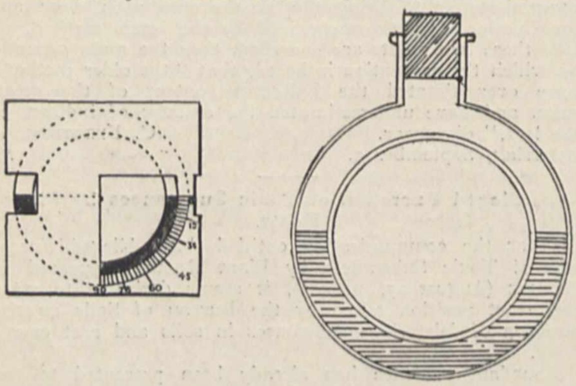

Fig. 14.—Details of the water-level clinometer.

It is quite certain that the use of the prismatic compass, in spite of its great convenience, must give way to other instruments which enable us to determine approximately the altitude of the horizon as well as the azimuth of any object the bearing of which we wish to obtain.

As a matter of fact there are now several such instruments available. They consist in the main of an azimuth compass, with an addition generally called a clinometer, enabling angles to be measured in a vertical plane. For this addition the first requisite, of course, is to be able to determine the true horizontal plane at the place of observation. This can be done by using a water level, a pendulum, or a properly adjusted bubble. I will give a brief description of three instruments which are based upon these various methods.

For the angular measurement of elevation, including, therefore, the angular height of the horizon as seen from any monument, the archæologist may use a very simple and convenient addition to the compass devised by M. Hue, a distinguished French archæologist. He uses the water-level principle. The method employed can be readily gathered from the accompanying woodcuts, obligingly sent to me by the publishers of the "Manual of Prehistoric Researches," published by the Société préhistorique de France; a book which shows us, by the way, that the French archæologists are much more thorough and philosophical in their inquiries than their British brethren. It is not a question of the spade *versus* the theodolite, but of the spade *and* the theodolite, and as full instructions are given about one as about the other.

It is quite refreshing to read the chapter "Indications pour faire un levé de Terrain à la Boussole," and then the instructions given relating to subsequent work with the large-scale maps published by the French Government.

In Barker's instrument, called for short a clino-compass, we find the pendulum method employed. The altitude zero of the instrument is shown when the pendulum hangs vertically at rest. This is an addition to the azimuth compass, and can be used when the azimuth measures have been made by making the plane of the instrument vertical. The figure will show the method of use. The degrees of elevation can be read under the prism as well as by the pointer at the bottom.

In a reconnaissance lately among the Aberdeen circles I employed a clino-compass of Barker's pattern; it weighs only a few ounces and is carried in a sling over the shoulders; even a tripod can be dispensed with, though it is much better to have one; the lightest form is that supplied by the Kodak Company for their cameras, to which must be added an adapter at the top to fit the base and allow the instrument to be used horizontally and vertically. In this form, especially in the case of the altitudes, the mean of several observations should be taken. In my opinion, a desideratum for such work is a simple small instrument with level and reversible telescope for small

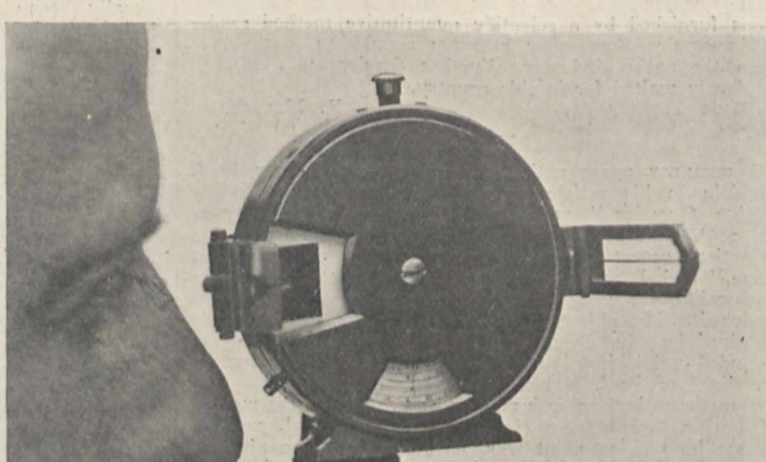

Fig. 15.—Vertical readings with Barker's clino-compass. An elevation of 3° indicated.

[1] Continued from p. 511.

altitudes only—a miniature dumpy level, fitting on to the same tripod which carries a full-sized azimuth compass, reading to half-degrees.

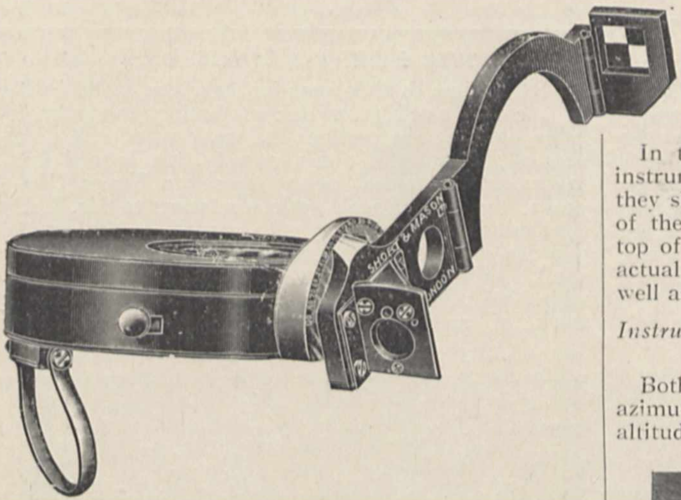

FIG. 16.—Verschoyle's pocket transit, showing the side arm for measurement of altitudes.

Azimuth and altitude are also provided for in the so-called Verschoyle pocket transit. In this the horizontal plane is provided for by adjusting a bubble in a short spirit level.

The altitude arrangement is on the side of an azi-

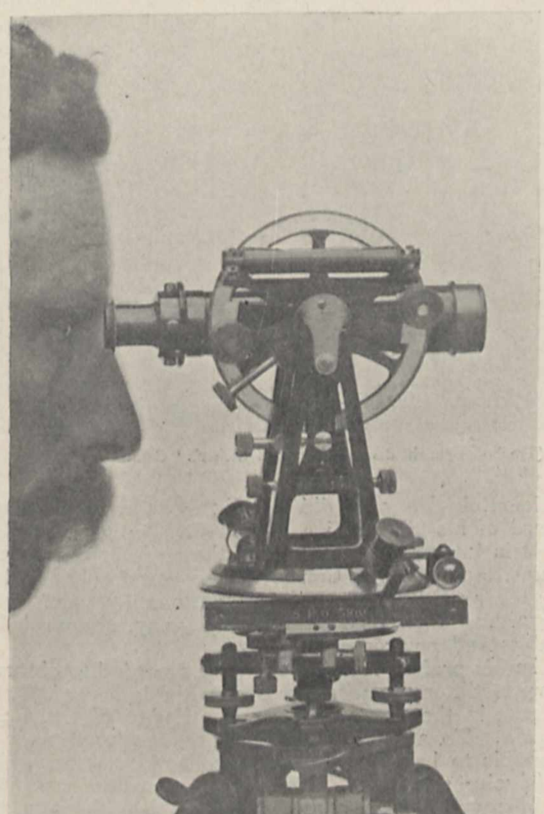

FIG. 17.—Observing an azimuth with a small theodolite.

muth compass, the graduations of which are read from the side by a right-angled prism, the graduations being cut on a bevelled edge.

Observations of magnetic azimuth and altitude can also be made by more complicated instruments, such as theodolites, miners' dials, &c., if, as is generally the case, a magnetic needle is provided for determining the magnetic north point.

The chief point about the theodolite in all its forms is that, whether provided with a needle to give magnetic north or not, observations of sun and stars can be made so that the true or astronomical north can be found.

In the readings of altitude made by any of these instruments, where trees, houses, &c., top the horizon, they should, of course, be neglected, and the elevation of the ground level at that spot taken. Should the top of the azimuth mark (stone, &c.) show above the actual horizon, its elevation should be recorded, as well as that of the horizon.

Instruments for determining Astronomical Azimuth and Altitude.

Both for the determination of astronomical or true azimuths directly, and for accurate observations of altitude, a theodolite is essential. It is true, as has

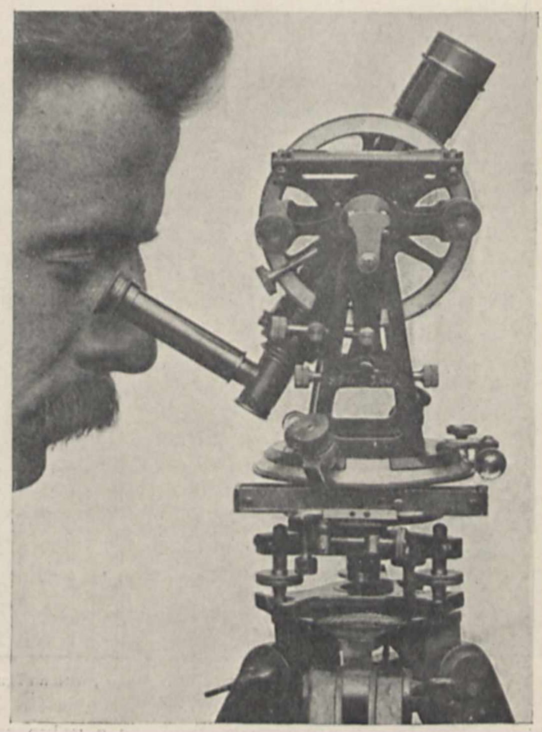

FIG. 18.—Eyepiece attachment to a theodolite to enable observation of the sun, or a star at a high altitude such as Polaris, to be made.

been stated, that a theodolite armed with a magnetic needle can provide us with magnetic bearings, but if the best use is to be made of it the needle should be discarded altogether.

A theodolite is a very complicated instrument, and really little can be learned by a perusal of a description, however long or detailed. The best way of learning how to use it is to get a friend to give you instructions while you, *yourself*, take each part out of its box, set it up, and then proceed to make some observations with it.

In using a theodolite the various alignments required are referred to some fixed point on the horizon, or at all events some distance away, and the angles determined; the true azimuth of the fixed point is then found by observations of the sun or a star.

(1) If only an approximate azimuth is required, the best means of determining it is by fixing the direction of the sun or a star when it has the greatest altitude. This direction, of course, defines the astronomical meridian, as all heavenly bodies cross it when they are at their greatest altitude.

By using stars of both high and low altitudes a greater exactness can be obtained, but, after all, the method only gives a first approximation, as its weakness lies in the very slow change of altitude as the meridian is approached.

(2) A much more accurate method is that of observing the azimuth of a star when at the same altitude east and west of the meridian. If the mean of the two readings given by the azimuth circle be taken, the resulting reading indicates the direction of the meridian.

(3) To find the meridian line by means of the pole star is a simple and accurate method, as a value can be obtained at *any* time at night by a simple altitude, provided the time of observation is known.

Should there not be sufficient time to take the necessary observations, the true bearing of the sun and also some star can be obtained by inspection from Birdwood's azimuth tables.

If we employ the sun in place of a star, its change of declination during the interval between the observations must be taken into account.

It is not alone with regard to azimuth that the results obtained by a theodolite far surpass all others in accuracy, as all magnetic difficulties are overcome, and larger circles give us closer and more accurate readings.

In altitude observations the fact that the observing telescope can be reversed and swung round so that all sources of errors of the horizontal plane of the instrument can be eliminated is a matter of equal importance, for in such work, if accuracy is required, as it should be, one setting and one reading are of little use.

NORMAN LOCKYER.

THE HORNED DINOSAURS.[1]

FROM time to time mention has been made in the columns of NATURE of contributions to our knowledge of what are perhaps the most wonderful members of a wonderful order, namely, the horned dinosaurs, or Ceratopsia, of the American Upper Cretaceous, the last of such notices relating to Mr. Lull's conclusions to be drawn with regard to the cranial muscles of the typical forms from the study of the skull. In the present sumptuously illustrated volume, which has a melancholy interest as being mainly the work of an exceedingly talented and promising palæontologist who did not live to earn the full reward of his labours, we have a full description of all that is known with regard to the osteology, relationships, and classification of these wondrous reptiles, together with notes and speculations (by Mr. Lull) regarding their distribution, phylogeny, and probable habits and environment.

Restoration of a Horned Dinosaur (Triceratops), with an Iguanodont (Trachodon) in the distance. From Hatcher's "Ceratopsia."

In his preface Mr. Hatcher, we are glad to observe, bore testimony to what we owe to the late Prof. O. C. Marsh in the matter of our knowledge of the Ceratopsia. To a large extent his generosity "made it possible to bring together the collections upon which this volume is based. Nor did his contributions to the subject end here, for, as appears on the title-page, the present memoir was based on his preliminary studies, and although he left no manuscript aside from his

[1] "The Ceratopsia." By J. B. Hatcher. Edited and completed by R. S. Lull. Pp. xxx+300; plates i-lii. (Washington: U.S. Geol. Survey, Monograph xlix., 1907.)

published papers on the Ceratopsia, he provided a fund of information in the shape of finished and unfinished drawings."

Mr. Hatcher considered the horned dinosaurs to be probably an exclusively American group, none of the European dinosaurs tentatively placed therein having any definite claim to such a position. In the case of a Wealden bone, described as a ceratopsian horn, the opinion is expressed that it is really a much weathered ungual phalange of a member of the sauropod group.

Leaving Mr. Hatcher's osteological section, the remainder of this notice may be devoted to brief mention of some of the interesting facts and speculations brought together in Mr. Lull's supplement.

The earliest known Ceratopsia occur in the Judith River beds, but of the ancestors of these latter we have no knowledge, possibly for the reason that they were inhabitants of dry land, instead of, like their successors, frequenters of swamps. The members of the group living at the Laramie epoch exhibit advance over their predecessors in the matter of bodily size, the preponderance of the supraorbital pair of horns over the single nasal one, the fuller development of the wonderful flange-like neck-shield of the skull, and the perfection of a peculiar type of dentition. Several attempts have been made to reproduce the external form of the horned dinosaurs, the most successful, in the opinion of Mr. Lull, being a painting and a statuette by Mr. C. R. Knight of Triceratops, the former of which is copied as a frontispiece to the volume before us, and is herewith reproduced.

That the horned dinosaurs were herbivorous is perfectly manifest; and it is suggested that while the edentulous, and doubtless horny, beak served for cropping succulent leaves and shoots, the teeth in the sides of the jaws chopped the food into short fragments, as they were not adapted for mastication. Swamps seem to have been the home of these rhinoceros-like dinosaurs; and this, it is suggested, may negative the idea that they were exterminated by the attacks of small predaceous mammals, since it has been considered that the latter were arboreal. If, however, mammals are derived from the theriodont reptiles, the theory that all the early forms were arboreal seems to require reconsideration.

Be this as it may, a more probable factor leading to the wane of the Ceratopsia was "changing climatic conditions and a contracting and draining of the swamp and delta regions caused by the orographic upheavals which occurred towards the close of the Cretaceous. The Ceratopsidæ and their nearest allies, the Trachodontidæ, both highly specialised plant-feeders, were unable to adapt themselves to a profoundly changed environment because of this very specialisation, and, as a consequence, perished."

The volume reflects the highest credit on all concerned in its production, and is an admirable example of the modern style of palæontological investigation, so intrinsically different in its picturesque speculation from the long series of dry details which alone formed the contents of works of this nature published a quarter of a century ago.

R. L.

NOTES.

THE death is announced of M. D. Clos, director of the Jardin des Plantes at Toulouse, and correspondant of the section of botany of the Paris Academy of Sciences.

PROF. L. H. BAILEY, of Cornell University, has accepted the chairmanship of the commission appointed by President Roosevelt to report upon the social and economic conditions of agricultural life.

A REUTER message from Berlin announces that the Academy of Sciences there has received a legacy of 30,000,000 marks (1,500,000*l.*), being the entire fortune of a millionaire named Samson, who recently died childless at Brussels.

AT Le Mans on Monday, Mr. Wilbur Wright travelled in his flying machine a distance of 48·12 kilometres in 1h. 7m. 11·4s. He afterwards performed a flight lasting 11m. 35·4s., with a passenger, at a speed of nearly one kilometre a minute.

THE Graham medal of the Royal Philosophical Society of Glasgow (awarded for original research in any branch of chemical science) is now open to competition. All information respecting the conditions of the award may be obtained from the secretary of the society.

THE quinquennial Riberi prize, of the value of 800*l.*, according to the *Athenæum*, has been awarded by the Academy of Turin to Prof. Bosco, of Turin, for his discovery of biological reaction, *i.e.* of a peculiar growth of mould on substances containing arsenic, tellurium, or selenium.

THE eighth International Congress of Hydrology, Climatology, Geology, and Physical Therapeutics is to be held at Algiers on April 4 to 10 next. All papers to be read at the meeting should be sent by, at latest, January 31. Full particulars of the congress can be obtained from M. Raynaud, 7 Place de la République, Algiers.

DR. SVEN HEDIN, in delivering a private lecture at Simla on his discoveries in Tibet, stated that although little is left in that country in the way of geographical discovery, in geology much remains to be done. Dr. Hedin is of opinion that from two to three years will be required to work up the mass of information collected by him relating to tracts hitherto unknown to Europeans.

THE Dove Marine Laboratory at Cullercoats was opened on Tuesday by the Duke of Northumberland. A polished granite tablet near the entrance bears the inscription:—"Erected A.D. 1908 by Wilfred H. Hudleston, M.A., F.R.S., for the furtherance of Marine Biology and as a Memorial of his Ancestress Eleanor Dove." The new building, which stands on the site of the old baths, contains an aquarium 30 feet by 23 feet, and there are eleven fish tanks. There is also a private aquarium, and provision is made in thirty-six tanks for the storing of materials for experimental work. A concrete tank holding 15,000 gallons of salt water will give a continual flow through the various tanks. The laboratory is in connection with Armstrong College, Newcastle-on-Tyne.

A RETURN issued by the Government of India shows that the total mortality amongst human beings reported to be due to snake-bite was 21,419 in 1907. The treatment of snake-bite by incision and application of permanganate of potash, as recommended by Sir Lauder Brunton (see NATURE, June 9, 1904, p. 141), continues, and lancets are distributed for this purpose, but the value of the results is discounted by the absence of identification of the snake that inflicted the bite. In Burmah nearly all the deaths occurred in paddy tracts where Russell's viper is particularly prevalent. Steps are being taken in that province to ensure a wide distribution of the Brunton lancets. It is reported that in the Pegu district six men and one buffalo bitten by Russell's vipers were operated on by headmen to whom lancets had been issued, and that all recovered but one man, who was unconscious before being treated. Eight cases are reported from the United Provinces of the successful use of Dr. Calmette's antivenene. In two of these cases the permanganate of potash treatment was also employed.

THE new session of the Royal Geographical Society will open on November 2, when Mr. D. G. Hogarth will give the first of a series of papers on the unexplored world, his subject being Western Asia. This will be followed by the undermentioned communications:—Some aspects of the River Paranà and its watershed, by Mr. W. S. Barclay; an account of his investigations on the Panama Canal, by Dr. Vaughan Cornish; a paper on his two expeditions into Bhutan, by Mr. J. Claude White; the Western Pacific, by Sir E. F. im Thurn; on a recent journey in North Central Arabia, by Captain S. S. Butler; South America and its Antarctic relations, by Prof. G. F. Scott Elliot; earthquakes and geography, by Mr. R. D. Oldham; and a paper on Australia, by Prof. J. W. Gregory, being the second of a series of lectures on the geographical conditions affecting the development of the British Empire; while Prof. W. M. Davis, of Harvard University, is to lecture in March on the Colorado Canyon and some of its lessons. Lectures may also be expected from Dr. Sven Hedin, Dr. M. A. Stein, Col. R. G. T. Bright (on the results of his expedition for the delimitation of the boundary between Uganda and the Congo Free State in the region of Mount Ruwenzori, Lakes Albert and Albert Edward, and the Semliki Valley), Lieut. A. Trolle (on the Danish expedition to north-east Greenland). On December 14 will be commemorated the jubilee of Speke's discovery of the Victoria Nyanza, and on that occasion Sir William Garstin is to read a paper on fifty years of Nile exploration and some of its results.

MR. GEORGE NICHOLSON, formerly curator of the Royal Gardens, Kew, whose death occurred on September 20 at Richmond, Surrey, as announced last week, was the son of a Ripon nurseryman. He was born in 1847, and after gaining experience in English and French nurseries was appointed a member of the Kew staff in 1873 as assistant to the curator, the second John Smith. He succeeded Smith as curator in 1886, and remained in this office until 1901, when he retired through enfeebled health. Nicholson was a prolific writer on horticultural matters, and his monographs of Quercus and Acer, published in the *Gardeners' Chronicle*, are testimonies to his conscientious and capable work. His memory, however, will live in the "Illustrated Dictionary of Gardening," published in four volumes, which he edited more than twenty years ago. Of all modern books, Nicholson's "Dictionary" has exerted the most influence in spreading horticultural knowledge. Nicholson was a first-rate gardener, an enthusiastic and critical British botanist, and a man who had studied chemistry very closely. He was greatly interested in natural history, particularly in insects, and contributed many papers to the *Kew Bulletin* on the fauna of the Kew Gardens. Nicholson was a Veitch medallist, and was amongst the first sixty who were awarded the medal of honour in horticulture by the Royal Horticultural Society in 1897. He was also an elected associate of the Linnean Society. It may be added that in 1901, when his retirement from the curatorship took place, Nicholson presented his excellent collection of British plants to the Aberdeen University.

No. 10 of vol. iv. of Circulars and Agricultural Journal of the Royal Botanic Gardens, Ceylon, is devoted to an account of the life-history and ravages of white ants, which are closely connected with botany through the cultivation by these insects of "fungus-gardens."

No. 10 of the "Fauna of New England" (Papers Boston Soc. Nat. Hist., vol. vii.) is devoted to the Pseudoscorpionida, Acarina, &c. New genera and species of local Thysanoptera form the subject of article 2, vol. viii., of the Bulletin of the Illinois State Laboratory of Natural History.

WE have to acknowledge the receipt of a copy of a suggestive historical address (published by G. Fischer, of Jena) entitled "Alte und Neue Naturgeschichte," delivered on July 30 by Prof. Ernst Haeckel at the meeting to celebrate the opening of a "Phyletische Museum" at Jena on the occasion of the 350th anniversary of the University.

JAPANESE locusts, by Messrs. S. Matsumura and T. Shiraki, and the earlier stages in the development of the vascular system of Ammocœtes, by Mr. S. Halta, form the subjects of vol. iii., part i., of the Journal of the College of Agriculture of Tohoku Imperial University, Sapporo, Japan.

A PRELIMINARY catalogue of the birds of Missouri, by Mr. Otto Widmann, forms the subject of vol. xvii., No. 1, of the Transactions of the Academy of Science of St. Louis. The subject is treated in considerable detail, with a preliminary account of the physiography of the district, but is, of course, mainly of local interest.

THE report of the Natal meeting (1907) of the South African Association has just reached us. In issuing the volume, the editorial committee regrets that it has been compelled to reduce many of the contributions to an abstract or a mere title, the reason being the lowness of the association's funds occasioned by the non-payment of their subscriptions by many of the members.

THE annual report of the acting superintendent of the Indian Museum, Industrial Section, Calcutta, for the year 1907–8 has reached us, from which we learn that the work of analysing samples has increased from about 100 samples per annum in 1897 to 340 in the period under review, which analyses are distributed as under:—natural exudations, 89; oils and oil-seeds, 66; dyes and tans, 35; fibres, 55; medicinal products, 25; food-stuffs, 58; minerals, 12.

A FINE example of an adult female *Mesoplodon bidens*, Sowerby, fully 16 feet long, was stranded on the West Sands, St. Andrews, on May 28. As one flipper seemed to be paralysed, it is possible that it had been struck by a spent shot or had collided with the ram or other part of a vessel of the fleet which was then in St. Andrews Bay. The specimen was at once secured by Dr. Tosh for the University Museum. Teeth were only visible in the mandible after maceration. The skeleton will be described by Sir William Turner.

THE valuable deodar-forests of the Simla area are suffering from a severe attack of four bark-boring beetles, two of which belong to the genus Scolytus, while a third, *Polygraphus minor*, is a species usually restricting its attentions to the blue pine, and the fourth is a buprestid. The last-named is new to Mr. E. P. Stebbing, who has contributed an illustrated account of the visitation to the Indian Forest Zoology Series.

IN the chief article in the August issue (part iv.) of the Journal of the Royal Microscopical Society, Mr. W. Wesché discusses the microscope as an aid to the study of biology in entomology, with special reference to the food of insects. According to the author, in the case of the large majority of insects the life-histories still remain unstudied, and the habits of many well-known species are based on conjecture. Details of the methods of work followed and some of the results obtained are given in the course of the paper.

THE littoral holothurians collected by the survey-ship *Investigator* form the subject of a memoir of Messrs. R. Koehler and C. Vaney in "Echinoderma of the Indian Museum." Out of forty-one species collected, fifteen are described as new. In the second number of Memoirs of the Indian Museum Captain R. E. Lloyd describes the anatomy of the huge phyllopod crustacean *Bathynomus giganteus*, of which the first known example was obtained in 1878 by the survey-ship *Blake*. The species is compared with the American representative of the genus, from which it is shown to differ in the number of basal plates.

To vol. ii., part i., of the Records of the Indian Museum Captain R. E. Lloyd contributes a paper on variation in an Indian species of the marine crustacean genus Squilla. The variation occurs in the number of "teeth" borne by the raptorial claw, and occurs in one particular race (or ? species) inhabiting a certain region of the Indian seas, in apparently deep water. The number of teeth may be as many as fifteen or eighteen, whereas the normal number is six or seven. In the opinion of the author, the facts in this case do not seem to be in favour of the "theory of gradual change" in organisms, and are put on record as a contribution to the study of animal variation available for comparison with other cases of a like nature.

THE Cretaceous fishes of Ceara, in Brazil, form the subject of a paper by Messrs. D. S. Jordan and J. C. Branner in vol. v., part i., of the quarterly issue of Smithsonian Miscellaneous Collections. The existence of fossil fishes in the Cretaceous deposits of this district has been known since the year 1841, when a collection was brought home by Mr. G. Gardner and submitted to Prof. L. Agassiz, by whom some were named and described. Since that date they have formed the subject of several papers, among these one by Dr. Smith Woodward in the Zoological Society's Proceedings for 1887 (quoted more than once in the article before us without any reference to the volume). In addition to the forms recognised by previous writers, Messrs. Jordan and Branner describe a new genus and species of Leptolepididæ, under the name of *Tharrhias araripis*, and likewise three other new species, referable to as many genera.

AN investigation into the germinative capacity of seeds of *Hevea brasiliensis*, carried out in Ceylon by Messrs. H. F. Macmillan and T. Petch, yielded the very definite result that in a few weeks the seeds lose their power of germinating. Seeds from untapped trees failed to germinate after being kept for three or four weeks. Seeds from trees that had been tapped for rubber showed a higher percentage in germination, and somewhat better keeping properties; otherwise they were smaller than the seeds from untapped trees, and would presumably yield less oil if they were crushed or extracted for that purpose. The experiments are described in the circulars (vol. iv., No. 11) issued from the Royal Botanic Gardens, Ceylon.

BOTANISTS who are interested in the "Kryptogamen Flora von Schlesien," originally edited by Dr. F. Cohn, will be glad to know that a final part, consisting of an index to the fungi, has just been issued. Dr. J. Schroeter undertook the portion dealing with the fungi, but died shortly before the last descriptive part was published. It was then intended to incorporate some of his notes in the next part; this project was, however, abandoned, and the index was compiled by Dr. A. Lingelsheim in its present form. Apart from the rest of the work, the index is useful, as it provides a list of host plants, cryptogamic as well as phanerogamic, upon which the fungi have been taken.

THE Rev. G. Henslow contributes one of his interesting historical accounts in connection with the cabbage group of plants to the new volume (xxxiv.) of the Royal Horticultural Society. It is remarked that Cato sang the praises of the cabbage and distinguished three kinds, while Pliny recognised six varieties. The author endeavours to trace modern races from the figures of the coleworts, forms of *Brassica oleracea*, inscribed in Gerard's Herbal dated 1597. Gerard refers the stock generally to *Brassica marina anglica* or English sea colewort. The kales are nearest to the suggested original type from which the true cabbage is derived by shedding of the lower stem leaves and aggregation of the leaves at the top; special development of buds in the lower leaves has led to the variety now represented by Brussels sprouts.

WE have been favoured with the report (No. 16) of the Danish Biological Station to the Board of Agriculture, in which Dr. C. H. Ostenfeld presents an account of the growth and distribution of the wrack-grass, *Zostera marina*, in Danish waters. The conditions necessary for the growth of Zostera are a salinity varying from ½ per cent. to 3 per cent., and a sufficiency of light such as can be obtained to a depth of six fathoms. The most luxurious development is attained on muddy or sandy bottoms in the sheltered waters of the fjords. The author subscribes to the opinion that the Zostera vegetation provides an important breeding ground for the nourishment of food-fishes.

IT is fitting that an early volume (part ii.) of the newly established *Indian Forest Records*, published by the Government of India, should be devoted to the consideration of compiling forest statistics with reference to the growth and increment of timber trees. Mr. A. M. F. Caccia, the officer in charge of forest working plans at the Imperial Research Institute, Dehra Dun, has collated existing data for the "sal" tree, *Shorea robusta*, to indicate how incomplete are present records, and what additional measurements are necessary to make the statistics as full as those compiled by forest research bureaux in Europe. With regard to the requirements of the "sal" plant, it is noted that it grows at elevations ranging from 150 feet to 6000 feet, where the annual rainfall varies between 40 inches and 180 inches, and demands, in addition, a loose, well-drained soil.

NOTIFICATION is given by the Board of Agriculture and Fisheries that the potato disease known as wart disease, cauliflower disease, or black scab, has been scheduled under the Destructive Insects and Pests Order of 1908. All occupiers of land on which the disease appears have to report the fact to the Board, from the secretary of which a leaflet describing the disease and suggesting preventive measures can be obtained on application. The Board of Agriculture and Fisheries has also issued a leaflet respecting grain weevils, in which brief descriptions are given of the nature of the harm done, the life-history and habits of the insect and its grub, and the known remedial measures. Copies can be obtained from the secretary.

THE Royal Commission on Sewage Disposal has recently issued its fifth report, and deals mainly with the relative merits of the various methods which are available for the purification of the sewage of towns. The work contains a number of appendices, and the general conclusion of the commissioners on the main subject is as follows:—"We are satisfied that it is practicable to purify the sewage of towns to any degree required, either by land treatment or by artificial filters, and that there is no essential difference between the two processes, for in each case the purification, so far as it is not mechanical, is chiefly effected by means of micro-organisms. The two main questions, therefore, to be considered in the case of a town proposing to adopt a system of sewage purification are, first, what degree of purification is required in the circumstances of that town and of the river or stream into which its liquid refuse is to be discharged; and, second, how the degree of purification can, in the particular case, be most economically obtained. The choice of a scheme must depend on a number of considerations which will be discussed later, but we may here state that we know of no case where the admixture of trade refuse with the sewage makes it impracticable to purify the sewage either upon land or by means of artificial processes, although in certain extreme cases special processes of preliminary treatment may be necessary."

IN the *Popular Science Monthly* for September, Mr. N. H. Winchell discusses the ethnology and traditions of the American Indian tribes, and specially of the so-called "mound-builders" of Minnesota. Modern research has established the enormous antiquity of these tribes in their present habitat. Thus the great variety of dialects, none of which can be connected with those of Europe or Asia, indicates either that the present population is the result of a number of successive migrations, or, which seems more probable, is the outcome of their long occupation of American soil. One fact is quite certain, that the mound-builders were the ancestors of some of the existing tribes. From a mass of confused legend it may be gathered that the practice of mound-building was confined to two stocks—the Algonquian and the Siouan—the former spreading over the north-eastern part of the United States and Canada, but with no representatives on the south-east Atlantic coast, the latter mainly confined to the great plains west of the Mississippi, this river apparently forming the boundary line between these two stocks. Whatever may be the value of Mr. Winchell's speculations regarding the post-Glacial movements of these races, he seems to be right as regards the sequence of tribal occupation in Minnesota. It begins with that of the Algonquian stock, a small area to the south-west being also held by the Ohio mound-builders. These were followed by Sioux fugitives from Ohio, to whom the majority of the mounds are attributed, to be followed, again, by an Ojibwa Algonquian incursion from the region of Lake Superior, these people dividing the State with the Sioux. This was the condition of things when the European appeared upon the stage. The value of this contribution to American ethnology would be greater if the essay had been accompanied by fuller reference to the authorities upon which it is based.

THE position of meteorology at the recent meeting of the British Association is again referred to in the September number of *Symons's Meteorological Magazine* (see NATURE, vol. lxxvi., p. 448). The writer states that from a meteorological point of view the meeting was the best since that of Southport in 1903, but, despite the efforts of the president of Section A (Dr. W. N. Shaw), no meeting ever showed more plainly the inferior position of meteorology as compared with other observational sciences. In the subsection of cosmical physics, meteorology and astronomy were mixed together in a way that was satisfactory to neither, and it was sometimes impossible to know at what hour the subsections would meet. The writer considers that a radical reform is necessary in the constitution of the association if it is to regain the high position it formerly held, and that meteorology will not be properly respected, or its true position understood, unless it is made at least a separate subsection, with a chairman of its own, and the hour of commencement of the meetings definitely announced.

WE have received from M. Charles Féry a pamphlet of 100 pages describing his exhibit of scientific apparatus at the Franco-British Exhibition. It is not by any means a maker's catalogue, but appears to consist of a collection of reprints of the original descriptions of the apparatus in the Proceedings of the Académie des Sciences, Société française de Physique, &c., some of which have been noticed in these columns. It will prove useful to those who are concerned with the scientific as distinct from the mere mechanical use of the apparatus, and it raises the question whether the publication of pamphlets of this type might not with advantage be taken up more extensively by instrument makers in this country.

IN order to assist makers of volumetric apparatus in establishing standards and perfecting methods of construction, the Bureau of Standards at Washington has for the last four years issued a circular containing specifications for and regulations for testing such apparatus. The third edition of this circular is embodied in an article in the May issue of the Bulletin of the bureau, on the testing of glass volumetric apparatus, by Messrs. N. S. Osborne and B. H. Veazey. In it the various specifications and rules for manipulation of the apparatus are discussed, and much information is given which bears directly on the construction, use, and testing of volumetric apparatus in general. As an example of the degree of accuracy expected, we note that for a litre flask the error should not exceed 0·3 cubic centimetre.

IN an article in the *Physikalische Zeitschrift* for September 1, Prof. H. A. Lorentz expresses with some reserve the belief that a satisfactory deduction of the law of radiation of a black body based on the electron theory, without the introduction of the so-called law of equipartition of energy, is impossible without serious modifications of the fundamental ideas of the theory itself. We must therefore accept Prof. Planck's theory of radiation as the only one tenable at the present time, and must wait for an explanation of the reason why the resonators of the theory do not appear to come within the province of the statistical mechanics of Gibbs. Prof. Lorentz points out a further difficulty of the electron theory of conduction of heat and electricity in metals as developed by Drude. According to this theory the oscillations of the free electrons within the metal account for the radiation of long waves by the metal, but are incapable of giving the short-wave radiation correctly, and no satisfactory explanation of this inconsistency has yet been given.

AN interesting contribution to the study of differences in the physiological behaviour of the right- and left-handed forms of optically active substances is contained in a short note by G. Bruni in the *Gazzetta Chimica Italiana* (vol. xxxviii., ii., p. 1). It is well known that the lower organisms, such as moulds, often show a striking preference for one of the two forms, assimilating or destroying it, whilst the other form is rejected. Whilst the same seems to be true in the case of the higher animals, the experiments as yet made are somewhat uncertain. In the present paper Mr. Bruni deals with the differences shown by the right- and left-handed forms of camphor. The latter was found in a large number of experiments to be about thirteen times as poisonous as the former when injected into the circulation of rabbits or guinea-pigs. The lævo form also differs strikingly from the dextro form in being practically tasteless; a somewhat similar difference has already been recorded for the two asparagines, the dextro form being sweet, the lævo form tasteless.

THE *Revue scientifique* for September 19 contains a lecture delivered by Sir William Ramsay to the French Association for the Advancement of Science during the meeting at Clermont-Ferrand. In this lecture a full account of the discovery of the inactive gases of the air is given in popular language. The relation of the radium emanation to these gases is also dealt with, allusion being made to the production of helium and neon from the emanation. A review of the periodic table shows that in two of the groups (6 and 7) inactive elements of the argon group with higher atomic weights than xenon may be expected, and details are given of the recent attempts made by Sir William Ramsay, in conjunction with Prof. Moore, to isolate these two elements. The less volatile residues arising from the fractional distillation of more than 100 tons of liquid air were placed at the disposal of the lecturer by M. Claude. Oxygen, nitrogen, hydrogen, hydrocarbons, carbon monoxide, and water vapour were removed in succession by the usual means, and the residue of inert gases cooled to $-185°$ C., and submitted to systematic fractionation. The result of this series of operations was the relatively enormous quantity of 300 c.c. of xenon. This was liquefied at $-130°$ C., and again submitted to a methodical fractionation. No trace of any foreign substance could be detected in this xenon, the spectrum of the last third of a cubic centimetre being absolutely identical with that of the bulk. This failure to isolate these heavy gaseous elements from the air may be due to their lack of stability; they may constitute the emanations of radium, thorium, and actinium.

FROM Messrs. Carl Zeiss, Jena, we have received a copy of the third edition of their "Astro. 8" catalogue, a handsomely illustrated volume which should be seen by all those desiring to purchase any kind of instrument or fitting for astronomical use. The present edition, which can be obtained gratis and post-free upon application, has been prepared with the view of meeting all the likely requirements of the scientific amateur astronomer, and includes telescopes and accessories up to an objective clear aperture of 7⅞ inches (200 mm.); for larger or special instruments the firm furnishes special estimates. There are several new constructions appearing for the first time in this edition. Among these we notice an ingenious relief system of the hour and declination axes, a changing appliance permitting any accessory to be fitted instantly to the breach of the telescope tube without screwing, and a new sun prism devised by Father A. Colzi, and consisting of a Herschel reflector and a Pickering double-prism; the second prism contains a fluid the depth of colour of which may be chosen to give an agreeable brightness of image. The astro-Tessar and U.V. objectives are also illustrated and quoted, and the catalogue concludes with illustrated specifications of variously sized domes. The London address of the firm is 29 Margaret Street, Regent Street, W.

MR. JOHN MURRAY has published a cheap edition (price 2*s*. 6*d*. net) in cloth of Darwin's "Insectivorous Plants."

MESSRS. JOHN WHELDON AND CO., of Great Queen Street, W.C., have just issued a useful catalogue of books dealing with physical sciences.

A NEW edition of their useful illustrated price-list of balances, scales and weights has been issued by Messrs. F. E. Becker and Co., of Hatton Wall, London, E.C. Every form of weighing instrument seems to be represented, and the catalogue includes particulars of a great variety of weights and accessories for use with balances.

MESSRS. JOHN J. GRIFFIN AND SONS, LTD., are prepared to send post free to chemists, teachers of chemistry and others applying for it, a very complete price-list of organic and inorganic chemicals and volumetric solutions manufactured by Mr. C. A. F. Kahlbaum, of Berlin. All the chemical preparations described are included in Messrs. Griffins' London stock.

IN the Journal of the Franklin Institute (vol. clxvi., No. 2) Mr. J. S. Hepburn gives the results of tests of the numerous modifications of the Kjeldahl method for the quantitative determination of nitrogen. The nitrogen content of antipyrin was determined, but in no case was the theoretical percentage of nitrogen obtained. The absolute method of Dumas, however, may be applied to antipyrin with success.

THE report of the Felsted School Scientific Society for the year 1907 provides abundant evidence that good work continues to be done in this school, by the masters and others, to encourage and maintain among the older boys a practical interest in the study of science. The work accomplished during the year is chronicled under botanical, chemical, geographical, and zoological sections. The report of the geographical section includes a fairly complete weather record for the year with which the report deals.

A NEW volume of "The Fauna of British India" has just been published by Messrs. Taylor and Francis. The volume deals with the families Testacellidæ and Zonitidæ of the Indian land mollusca. The late Dr. W. T. Blanford, F.R.S., left a short manuscript in which the shells were dealt with, chiefly from the conchological side, and this formed the foundation of the volume. Lieut.-Colonel Godwin-Austen, F.R.S., who has been responsible for the malacological part, and whose name appears with that of Dr. Blanford upon the title-page, contributes an introduction which should be the means of creating interest in the two important families of Indian land-shells described.

OUR ASTRONOMICAL COLUMN.

COMET MOREHOUSE.—The following set of elements and an ephemeris for comet 1908*c* appear in No. 138 of the Lick Observatory Bulletins. They have been computed by Messrs. Einarsson and Meyer, of the Berkeley Astronomical Department, from observations made at the Yerkes and Lick observatories, and subsequent observations show a satisfactory agreement between the observed and calculated positions. It will be noted that, according to these elements, perihelion will not take place until January 5, whilst the computed increase in brightness is not so rapid as given previously.

Elements.

T = 1909 January 5·702 G.M.T.

$$\left.\begin{array}{l}\omega = 152°\ 4'\cdot0\\ \Omega = 90°\ 20'\cdot5\\ i = 135°\ 56'\cdot2\end{array}\right\}\ 1908\cdot0$$

$q = 1\cdot1680$

Ephemeris for Greenwich Mean Midnight.

1908	α (true) h. m.	δ (true) ° ′	log Δ	Brightness
Oct. 1·5	21 32·4	+72 58·4	0·1249	2·7
2·5	21 18·8	+72 5·0		
3·5	21 6·4	+71 6·9	0·1170	2·9

Large Group of Sun-spots.—The large group of sun-spots referred to in these columns on August 13, and again on September 10, has again been brought into view by the sun's rotation, this making the third rotation during which the same group has been seen. Its persistent activity is evidenced by the fact that it is once more visible to the naked eye, although the separate spots seem to be somewhat smaller and more scattered.

The Orbit of ζ Cancri C.—The measures of ζ Cancri made since 1756 are brought together by Prof. Doberck in No. 4273 of the *Astronomische Nachrichten* (September 14), and are supplemented by a few brief notes concerning the orbit of the smaller component (C) of the primary pair.

It will be remembered that this system was the first for which the existence of three components was established, the duplicate character of the larger star of the primary pair being discovered by Herschel in 1781. Subsequent observations showed that the motion of C is very irregular, and led to the suspicion that this star is accompanied by a dark companion. Independent evidence of the existence of this invisible companion is deduced by Prof. Doberck from measures made by Profs. Burnham and Barnard between 1891 and 1905.

He also finds that the star C moves round the centre of gravity of C and D (the dark body) in a circle of 0″·158 radius, the period being 17·43 years. Assuming the combined mass of A and B to be equal to that of the sun, it follows that the relative masses of A, B, C, and D are 0·5, 0·5, 0·62, and 0·43 respectively.

Search-ephemeris for Comet Tempel$_3$-Swift.—A continuation of the ephemeris, published by M. E. Maubant in No. 4269 of the *Astronomische Nachrichten*, for the comet discovered by Tempel in 1869 is given below:—

Ephemeris 12h. M.T. Paris.

1908	α h. m.	δ ° ′	log r	log Δ
Sept. 30	7 3·4	+31 3·5	0·0619	9·8396
Oct. 4	7 20·9	+30 16·9	0·0622	9·8411
8	7 37·4	+29 23·7	0·0634	9·8431
12	7 52·8	+28 25·4	0·0655	9·8454
16	8 7·2	+27 23·4	0·0683	9·8478

Three ephemerides are given, that from which the above is taken being computed for the mean date (September 30·88) for perihelion passage. According to the above, the comet should be some 2° S. of Castor on October 6, and about 45′ N. of Pollux on October 9.

The Manora Observatory.—According to a note published in No. 400 of the *Observatory* (p. 362, September) the Manora Observatory, the instruments of which were recently announced for sale, has been purchased by an anonymous person, who invites observers of all nations to observe with the equatorial.

A Nebulous Field in Taurus.—In the September number of the *Bulletin de la Société astronomique de France* (p. 400) Prof. Barnard has an interesting discussion of an extensive nebulosity, which he has photographed, in the constellation Taurus.

A splendid reproduction accompanies the note, and shows the peculiarities discussed. These consist of long dark lanes, in an otherwise nebulous region crowded with stars, apparently devoid of both stars and nebular matter, and Prof. Barnard discusses the hypothesis that their appearance is caused by the interposition of absorbing material between the background of nebula and stars and the earth. He finds this explanation difficult to embrace, but so far is unable to offer a more reasonable one. The field covered by the photograph lies between R.A. 4h. 0m. to 4h. 34m., and dec. +24° to +28°·5.

THE ISOTHERMAL LAYER OF THE ATMOSPHERE.

The important discussion of which we give here a detailed account was organised by the committee of Section A of the British Association, and took place at the recent meeting.

It was intended that M. L. Teisserenc de Bort should open the discussion, but he was unable to be present, and sent the following communication:—

Permit me to open the discussion on the isothermal layer and the inversions of temperature which are found there by recalling in a few words the results obtained during the past twelve years. Our experiments at Trappes have shown, in the first place, that the temperature ceased to diminish at a certain height after having passed through a point of maximum rate of decrease about 3000 metres lower down.

The altitude at which the diminution ceases changes with the character of the weather; it may descend as low as 8 kilometres at Paris during a cyclone, while it rises as high as 13 or 14 kilometres in high-pressure areas and in front of large cyclones.

I indicated these peculiarities for the first time in October, 1901, in a communication to the Luftschiffart Verein at Berlin, then in a communication to the Meteorological Society of France in March, 1902, and I developed these conclusions in a note to the Académie des Sciences in April, 1902.

A short time after, in the early part of May, 1902, Prof. Assmann showed from the ascents of six rubber balloons that not only was there a cessation of the decrease of temperature, but also an inversion. This inversion had also been very marked in the first ascents by Hermite and Besançon, but Prof. Assmann sought to explain it as being due to the effect of solar radiation on the thermometer, while the ventilation produced by the rapid ascent of the balloon showed that it could not be referred to such an error in the thermometer record.

Having once demonstrated the existence of this isothermal layer for places in the neighbourhood of Paris, we sought to find the evidence of it in other regions in order to show that it was a general phenomenon. Ascents made by us and our assistants in the winter of 1900–1, by M. de Quervain in Russia, by Mr. Eggenberger at Bath in England in 1902, have made it evident that the phenomenon was a general one. On referring to the results of the international ascents made in different countries, it is seen that the cessation of the temperature decrease is found in the case of all the balloons sent up, and that it is impossible to refer it to insufficient ventilation, since the phenomenon was well marked in ascents made during the night. Since this time, ascents made on board the *Princesse Alice* by Prof. Hergesell in 1905 have furnished evidence of the existence of the layer near the Azores; ascents made in the United States by Mr. A. L. Rotch have furnished evidence of its existence there with the peculiarities I have indicated, *i.e.* high up over high-pressure areas and low down over low-pressure areas.

The expeditions of the *Otaria*, organised in conjunction with my friend Mr. Rotch, have proved the existence of the zone in the tropics, and have shown that it is further from the earth near the equatorial regions where the trade winds meet.

Finally, the ascents made at the end of the winters of 1907 and 1908 by the French-Swedish expedition organised by the Observatory of Trappes, with the support of Prof. Hildebrandson, have shown that near the Arctic Circle, at Kiruna, the layer exists and possesses general characteristics analogous with those found in these regions.

The results of series of daily ascents for eight, ten, or more days in succession in February, 1901, March, 1903, and May, 1904, have proved that the change of altitude of the point where the temperature ceases to fall is accompanied by changes of temperature of 10° C., 15° C., 20° C. in an interval of a day or two at heights between 9 and 13 kilometres, variations great enough to be felt near the surface during the same time.

Thus the equalisation of temperature in the course of the year, which had been supposed to be nearly complete at 8 or 9 kilometres altitude, does not exist, but, on the contrary, sudden changes of temperature occur with the passage of cyclones and anticyclones which would furnish to an observer in those regions the chief evidence of the changes occurring at the surface.

Causes of the Isothermal Layer.—The summary of the observed phenomena has led me to this conclusion, that the cessation of the temperature diminution is due to the fact that there is at these heights no considerable vertical convection.

The fact that one meets with layers of air thousands of metres thick where the temperature increases and decreases rapidly, and others where it is stationary, is incompatible with the existence of motion of the air accompanied by pressure variations, which always tend to produce a vertical temperature gradient more or less near that for the adiabatic state. It does not follow that the movement in the isothermal layer must be horizontal, but that it takes place along the isobars without crossing these surfaces nearly in the manner in which a body rolls on an inclined plane.

These ideas have been developed in several communications, in particular at the Conférence d'Aérostation scientifique at St. Petersburg in September, 1904.

Dr. Shaw, in the absence of M. L. Teisserenc de Bort, opened the discussion. He explained what was the main feature of the phenomenon, and showed how it had been corroborated by *ballons-sondes* ascents made in England. The temperature of the air decreases in the lower layers on the average at 5° C. or 6° C. per kilometre up to a height of about 10 kilometres. Above this height the temperature ceases to fall rapidly and falls very slowly indeed, or remains constant or in some cases increases. It had been suggested that the phenomenon might be due to a change in the composition of the air at great heights. M. L. Teisserenc de Bort had succeeded in sending up balloons carrying vacuum tubes, which were opened and re-sealed electrically at a height of 14 kilometres. The samples of air so obtained were examined spectroscopically, and the examination showed that there was no change in the composition of the air sufficient to account for the cessation of temperature diminution.

Mr. Rotch said the only *ballons-sondes* which have been sent up in America were those dispatched by him. Since 1904 seventy-six rubber balloons have been launched from St. Louis, and all but one have been recovered. The majority of those which rose higher than eight miles (12,870 metres) entered the stratum of relatively high temperature.

All the ascents occurred after sunset, so that there can be no question as to the effect of solar radiation. The instruments used were of M. Teisserenc de Bort's construction, and were verified for low pressures and temperatures before and after the ascents. The warm stratum, which was not isothermal, but became warmer with increased height, was at its lowest level in summer, having a mean minimum temperature of −54°·6 C. at 12,000 metres. During the autumn of 1907 the warm stratum of temperature was penetrated eight times, the mean minimum temperature of −60°·5 C. occurring at 12,370 metres.

The changes in the level of the minimum temperature from day to day were large. Thus the minimum of −67°·1 C. at a height of 14,500 metres, on October 8, was followed two days later by a descent of the minimum of −62°·2 C. to 12,000 metres. In the first case, the temperature at the highest point reached, viz. 16,500 metres, was −58°·1 C., and in the second case, when 15,000 metres was attained, −56°·0 C. On November 6 the minimum temperature of −52°·2 C. occurred at 9700 metres, but the place of occurrence of the minimum of −63°·1 C. had risen to 14,250 metres on November 8. The temperatures at the highest points reached were −50°·5 C. at 10,000 metres and −60°·2 C. at 15,380 metres respectively.

These observations, made near latitude 35° N., show the warm stratum to be at a distinctly higher level than in northern Europe, whereas the results obtained by the expedition sent jointly by M. Teisserenc de Bort and the author to explore the atmosphere over the tropical Atlantic in 1906–7 show that it was there considerably higher. In fact, the observations obtained over the equator up to 15,000 metres show no reversal of temperature, and a lower temperature than exists at a corresponding height in northern latitudes.

Mr. Cave said that during the last week in July he was able, by means of theodolites, to follow four balloons into the isothermal layer. From these observations it appeared that the wind velocity increased to a maximum just below the isothermal zone, and decreased rapidly above. The wind velocities were very high, and most of the balloons went out to sea; one, sent up on July 28, was recovered. From the record of the meteorograph it appears that the isothermal layer was entered at 11,500 metres; the theodolite observations indicated that this was the height of the maximum wind velocity; above this the velocity dropped to eight miles per hour at 13,000 metres.

Mr. W. H. Dines said that he knew there had been some doubt expressed about the existence of the isothermal layer, and possibly there were still some who thought that the results obtained were due to instrumental errors. Such a view was now quite untenable, for about seventy ascents had been made in the British Isles during the last eighteen months, and the results entirely confirmed those previously made on the Continent and in America, although the instruments used for recording the temperature were of a totally different pattern. These ascents had mostly been made at about the time of sunset, so that no possibility of solar influence might be present, but in every case (about sixty), when sufficient height had been reached, the temperature gradient had become negligible or of opposite sign. After calibrating many instruments he was convinced that the temperatures recorded were, with but few exceptions, trustworthy within two or three degrees centigrade.

The results, however, were most remarkable, and it was not surprising that doubts about their accuracy were expressed. It had been found that over places only a few hundred miles apart, and at the same time, the temperatures might be widely different, and within the same week and over the comparatively small area of the British Isles differences of 30° C. had been recorded, namely, −40° C. at 15,000 metres, at Limerick on July 27, −60° C., at Pyrton Hill, Oxon., on the same date, and −69° C., at Pyrton Hill on July 29 and again on July 30. Very similar differences between Manchester, Ditcham Park, and Pyrton Hill had been noted on previous occasions.

The absence of any temperature gradient in the air is definite proof of the absence of any vertical circulation, but this alone did not present any difficulty. He (Mr. Dines) had always thought that the vertical circulation was chiefly due to the heat set free when aqueous vapour was condensed to water, and since it was known that the relative humidity was small at great heights, it might well be that above 10 or 12 kilometres there was no aqueous vapour, and therefore no vertical circulation. The difficulty was how large temperature differences could exist at small distances apart without producing convection currents. In a mass of gas at rest under a conservative system of forces the isobaric or isothermal surfaces must be coincident. In this case the temperature observations led to two contradictory results—they showed that there was no circulation and also that the isobaric and isothermal surfaces were not identical. At a height of 15 kilometres a very small change of pressure would produce a large adiabatic change of temperature, but it was difficult to see how, with so small a mass of air left above, changes of pressure could be produced. The accelerations produced by curvilinear motion of the air particles and by the effect of the earth's rotation an a moving body appeared to be far too small for the purpose. Was it possible that the upper air could carry a sufficiently strong electric current to be influenced by the earth's magnetic field, and so produce forces comparable with gravity? Prof. Schuster had suggested some such origin for the daily variation of the magnetic declination.

Mr. Gold said that any explanation of the existence of the isothermal layer must take into consideration the effect of atmospheric radiation. On the assumption that the radiation per unit area from a layer of gas was proportional to the mass of gas in the layer, and that the absorption followed the same law, he had worked out some results for the earth's atmosphere. If the atmosphere were of uniform constitution, so that the absorption by a layer of air of given mass was the same at whatever height the layer was taken, then the state of convective equilibrium could not exist to heights greater than those corresponding to a pressure equal to half the surface pressure. He found that for greater heights than this the radiation absorbed from the earth and the rest of the atmosphere alone was greater than that emitted at a temperature corresponding to the state of convective equilibrium. In consequence of this the temperature of the air in the upper layers would rise, and there would be a further increase owing to the absorbed solar radiation. In the actual case, the absorbing power of the atmosphere diminishes with increasing height owing to the diminution in the proportional amount of water vapour present. The absorbing power was therefore taken to be equal to $\alpha/(q-p)$, where α and q are constants. Two values were taken for q, for one of which the diminution in absorbing power was quicker, in the other slower, than the diminution in the proportion of water vapour present. The value of α was deduced from the observations of Langley, Paschen, and others.

The conclusions arrived at were:—

(1) If the temperature gradient in the lower layers of the atmosphere is such that $T \propto p^{\frac{1}{4}}$, *i.e.* is approximately adiabatic, and if the upper layer is isothermal, then the state $T \propto p^{\frac{1}{4}}$ must extend to a height greater than that for which $p = p_0/2$, and in general less than that for which $p = p_0/4$, where p_0 is the surface pressure.

(2) The temperature in the lower layers cannot be maintained by absorption of terrestrial and solar radiation; these layers tend to grow cooler, and their temperature is kept up by the supply of heat through convection from the earth's surface and by condensation of water vapour in the atmosphere.

(3) The lowest possible temperature in the atmosphere over a place at temperature 300° A. must be greater than 150° A. or 210° A., according as the atmosphere radiates and absorbs throughout the spectrum or transmits freely 25 per cent. of the earth's radiation.

Prof. Turner said that whereas meteorologists were perhaps primarily concerned with the facts themselves, and physicists with the causes of them, astronomers were interested in the effects of the existence of this isothermal layer, especially in the phenomena of atmospheric refraction. It had been usual to make certain assumptions about the upper air for the calculation of refraction, and these assumptions were now shown to be wrong. Were the refractions calculated on such assumptions wrong? The answer seemed to be that very rough assumptions were sufficient for astronomers; he had found, for instance, that the assumption of two homogeneous shells of air would give empirical results corresponding closely to the facts observed.

Further, no very great improvement was found by adding a third shell—the chief step came in taking two instead of one. Possibly this fact (that two shells were absolutely necessary, but a third was not so much needed) was in some way connected with the existence of two principal regions in the atmosphere.

Prof. J. J. Thomson asked if there was any indication of the thickness of the layer, and remarked that the ionisation in the atmosphere was a maximum at a layer considerably below this layer.

Dr. Walker stated that the Indian peasants were so ignorant that he had not yet ventured on sending up *ballons-sondes* there, the chances of recovering them being so remote.

THIRD INTERNATIONAL CONGRESS FOR THE HISTORY OF RELIGIONS.

OXFORD has good reason to be proud of the success of the congress, which was held there from September 15 to September 18; not only was the general level of the papers high, but the attendance of members—nearly 600—was so large that the Transactions will contain, besides the presidential addresses, some of the more important papers in full, with an abstract of the remainder. The total number of papers was well over 100, hence the need for limitations.

At an Oxford congress of religions it was natural that a part should be played by the Father of Anthropology, and the enthusiasm with which Dr. Tylor was greeted when he introduced the president, Sir A. C. Lyall, was as flattering a tribute to his greatness as he could desire. The subject of Sir A. C. Lyall's address was religious conflicts and the conditions under which one religion attained predominance over its competitors; he held that State recognition has been indispensable to religious consolidation, and ascribed to the absence of State regulation the freedom characteristic of Hindu theology.

The congress was divided into nine sections, besides a general one for papers of wider import, and in each section a presidential address was delivered; Sir John Rhys dealt with Celtic religion, and pointed out that our evidence was precarious, and our knowledge inferential only; Prof. Giles said that the Chinese had a sky-god, T'ien, who received, however, neither respect nor sacrifice; eventually this power became an abstraction; Mr. Hartland discussed, among other things, magic, a subject also dealt with by Dr. Jevons; Prof. Petrie discussed Egyptian religion, and pointed out that the prominence of the funerary cult in it was accidental and due to the rise of the bed of the Nile, which had covered up the Egypt of the living; in the life of the ordinary man, the local sacred animal or totem figured largely; the murder of a cat would have set Alexandria in flames, even down to Roman times.

Of the other papers, some were sensational, like that of Prof. Haupt, who maintained the non-Semitic descent of Christ; he argued that Galilee was denuded of Jews in 164 B.C., and that when the Jewish religion was reintroduced fifty years later, it was imposed on Assyrian colonists introduced by Tiglath Pileser; an effective criticism on this view was made by Dr. Gaster, who pointed out that the Jews would have been ready enough to seize on a much less valid ground for denying Christ's descent from David.

Dr. J. G. Frazer also dealt with Jewish beliefs, but his notes on them were the wonderful collections of parallel instances from all parts of the world which we expect from him; he traced the silent widow, for example, in North America, Madagascar, and Australia, where a two years' ban rests upon them, and has been perhaps a potent cause in the development of gesture language.

Dr. A. J. Evans read a paper on the cults of Minoan Crete, and pointed out that recent discoveries corroborated the views which he put forward in 1900; Minoan cults were predominantly aniconic, though images were also found; the cult objects were trees and pillars, and the double axe; the principal divinity was a nature goddess. As a pendant to this paper may be mentioned Miss Harrison's discussion of bird and pillar cults, in which she argued that the change from the "matriarchal" to the "patriarchal" stage caused a change of sex in the most important divinity.

Anthropologists are far from being agreed as to the definition of religion, and, not unnaturally, there was an attempt to define it in the section devoted to religions of the lower culture. Mr. Marrett held that Tylor's animism was far wider than religion, though it did not embrace all religion; the real criteria were two—first, the presence of *mana*, magico-religious force, and, secondly, the negative rites set up by a belief in *mana*, and commonly known as tabu; when the personal element became prominent in religion, animism came in; but it is really a primitive philosophy far wider than the supernatural.

Special interest attached to Dr. Seligmann's account of the Veddahs, from whom he has just returned; with them, as with many other races, fear was the main emotion, and at death they deserted the cave, leaving the body without food or fire; the cult of the dead was almost the central feature of the psychical life of the Veddahs. Funerary customs were also dealt with by Mr. T. C. Hodson in a paper on the Assam hill tribes, and by Mr. N. W. Thomas; the latter summarised Schmidt's views, as yet unpublished, as to the three strata in the population of Australia—old and new Australian and (?) Papuan—and pointed out that the burial customs largely followed the linguistic lines; in the south and west of Australia fear of the dead was found, and disposal of the body once for all; in the north and east the flesh was removed from the bones, and only with the burial of the latter was the spirit supposed to be dismissed to its own place; in the south the grave was the abode of the spirit.

Mr. W. W. Skeat's paper dealt with traces of totemism in the Malay Peninsula; totemism implies a group name, a belief in group kinship, and respect for "the blood," and of these the second is the primary one from which the others have sprung; but he was inclined to hold the view that totemism was originally independent of the notion of kinship; the Semang have not, as contended by Mr. Gomme, plant totemism, for plant names are far from general.

Among other papers may be mentioned one by Mr. Hollis on the Nandi, which suggests that their religion is a cross between Bantu ancestor cult and the Masai sky-god cult.

The social side of the congress was well looked after, and receptions were given by Prof. Gardner and Dr. Evans at the Ashmolean, Mr. Marrett and Dr. Farnell at Exeter, Profs. Driver and Sanday at Christ Church, Prof. Carpenter at Manchester College, and by the Mayor and Mayoress.

THE BRITISH ASSOCIATION.

SECTION I.

PHYSIOLOGY.

OPENING ADDRESS BY J. S. HALDANE, M.D., F.R.S., FELLOW OF NEW COLLEGE AND READER IN PHYSIOLOGY IN THE UNIVERSITY OF OXFORD, PRESIDENT OF THE SECTION.

The Relation of Physiology to Physics and Chemistry.

IN choosing to address you on the relation of Physiology to Physics and Chemistry, I am aware that I have selected a subject which has already been treated from this chair by more than one distinguished predecessor. My excuse for returning to it again is that it not only possesses deep scientific interest for us all, but that a great deal remains to be said about it.

The majority of physiologists in recent times have expressed more or less clearly the opinion that Physiology is the application to living organisms of the methods and modes of explanation of Physics and Chemistry. It is, in short, Physics and Chemistry applied to the activities of living organisms; so that the only explanations aimed at in Physiology are, or ought to be, physical and chemical explanations. A minority, which is at present a growing one, I think, have either definitely dissented from this view, or have remained unconvinced of its truth. As one of this minority I should like to place before you as shortly as possible what seem to me to be the main reasons of our dissent. Let me add that I have carefully pondered over these reasons during many years of active physiological work.

When we look back on the history of Physiology is seems perfectly evident that physiological progress has been dependent on the progress of Physics and Chemistry. On this point there is no room for doubt. To take only one example, where should we be in the investigation of animal metabolism but for the ideas and experimental methods furnished to us by Physics and Chemistry? We should know next to nothing about respiration, animal heat, nutrition, or muscular and other work. Physiology depends at every turn on Physics and Chemistry, and its future progress will certainly be equally dependent on advances in physical and chemical knowledge. This consideration has, I imagine, weighed very heavily in the minds of those physiologists who have concluded that Physiology is nothing but applied Physics and Chemistry. A further fact which weighs equally heavily is that in spite of diligent search no fact contradicting the fundamental laws of conservation of matter and energy has been discovered in connection with living organisms.

When, however, we ask what progress has been made towards the physico-chemical explanation of physiological processes, we at once enter upon controversy. We may point to advances in some directions, but they are accompanied by the appearance of unforeseen difficulties in other directions. Again, to take animal metabolism as a typical instance, the investigations of the last hundred and twenty years have enabled us to assign ultimate physical and chemical sources to the energy and material leaving the body in various forms. We can assign to such sources the energy of animal heat, muscular work, glandular, nervous, and other activity: also the carbon dioxide, urea, salts, and many other substances which leave the body or are formed within it. All of this new knowledge may be regarded as progress towards a physico-chemical explanation of life.

But there is another aspect to be considered; for side by side with what I have just referred to there has been a different kind of increase of knowledge with regard to animal metabolism. This growth of knowledge relates to the manner in which the passage of energy and material through the body is regulated in accordance with what is required for the maintenance of the normal structure and activities of the body. In Liebig's time, for instance, it was believed that the rate of respiratory exchange was regulated simply by the supply to the body of oxygen and food-material. If one breathed faster, or if the barometric pressure or percentage of oxygen in the air increased, the respiratory exchange was assumed to be also increased, just as ordinary combustion outside the body would be increased by an increased supply of oxygen. If, again, one took in more food it was supposed that the excess went to increase the rate of combustion in the blood (*luxus consumption*), just as a fire is increased when more fuel is supplied. We now know that these assumptions were wholly mistaken, and that the respiratory movements, respiratory exchange, and corresponding consumption of food material in the body are regulated with astounding exactitude in accordance with bodily requirements. If, for instance, the body consumes more proteid, it economises a quantity of fat or carbohydrate equivalent in energy value to the proteid; and from day to day the amount of energy liberated in the body is very steady. With regard to the excretion of material by the kidneys a similar growth in knowledge can be traced. It is scarcely a century since the urine was regarded as equivalent more or less to the liquid part of the blood separated from the corpuscles, which were unable to pass through the very fine capillary tubules supposed to exist in the kidney substance. Gradually, however, we have learnt how extraordinarily delicate is the selective action which occurs in the kidney substance, and how efficiently this selective action maintains the normal composition of the blood. Scarcely a remnant is now left of the old filtration theories. Our ideas of tissue nutrition and growth have undergone a similar change; and it is hard to realise that only about seventy years ago Schwann could put forward the theory that cell formation and growth is a process of crystallisation.

One can multiply instances like these almost indefinitely; but I have, perhaps, said enough to show that if in some ways the advance of Physiology seems to have taken us nearer to a physico-chemical explanation of life, in other ways it seems to have taken us further away. On the one hand we have accumulating knowledge as to the physical and chemical sources and the ultimate destiny of the material and energy passing through the body; on the other hand an equally rapidly accumulating knowledge of an apparent teleological ordering of this material and energy; and for this teleological ordering we are at a loss for physico-chemical explanations. There was a time, about fifty years ago, when the rising generation of physiologists in their enthusiasm for the first kind of knowledge closed their eyes to the second. That time is past, and we must once more face the old problem of life.

Let us first look at the answer given to this problem by many of the older physiologists. Roughly speaking, they carried physical and chemical explanation of physiological processes as far as they could, and for the rest assumed that at some point or other the physical and chemical factors are interfered with and ordered in a teleological direction by something peculiar to living organisms—the "vital principle" or "vital force." This theory, if one can call it a theory, had the negative merit that it did not lead physiologists to ignore facts which they could not explain. But in practice the "vital force" became simply a convenient resting-place for these facts. It was assumed that the vital force could do anything and everything, and that it acts "from the blue" on physical and chemical processes. Yet its action was admittedly dependent on physical and chemical conditions, such as warmth, the presence of oxygen, &c. In fact, no consistent definition was given to the conception of "vital force." It consequently never could become a working hypothesis of any value. Chiefly on this account, I think, it practically disappeared from Physiology last century. Yet the class of fact which led to the theory of "vital force" is now more prominent than ever; and what du Bois Reymond called the "spectre" of Vitalism meets us at every turn, thinly disguised under such names as "cell autonomy," "vital processes," &c. It is useless to shut our eyes and deny the existence of this "spectre." We must fairly face and examine it.

However difficult it may be to imagine physico-chemical explanations of such processes as respiratory exchange, secretion, muscular activity, &c., there is nothing in the known facts relating to each process taken by itself to preclude the possibility of such explanations. Let us then follow the Euclidean method and assume provisionally that they are nothing but physico-chemical processes. This assumption evidently implies that each of the living cells concerned has a very complex and definite structure varying according to its functions. To take an example, a secreting cell in the kidney may be assumed to have a structure which responds to the stimulus of a certain percentage of urea or sodium chloride in the blood, and reacts in such a manner that energy derived from oxidation is so directed as to perform the work of taking up urea or sodium chloride from the blood and transferring it against varying osmotic pressures from one end of the cell to the other. This mechanism must also be assumed to have the property of maintaining itself in working order, and probably also of reproducing itself under appropriate stimuli, besides also performing various other functions. Its physico-chemical structure must thus be very definite and complex—to an extent which the older physico-chemical theories took no account of. If we look to the cells in other parts of the body we are met with the same necessity for assuming complexities of structure which seem to grow in extent with every advance in physiological knowledge, every discovery of new substances present within or around the cells, every discovery of new physiological reactions.

Let us not lose courage, however, but continue to follow the direction in which our assumption leads. In assuming that the body is an enormously complex physico-chemical structure we have only begun to face the difficulties of our hypothesis: for we have still to consider how this structure can have originated in accordance with the physico-chemical theory of life. The adult organism develops from a single cell, the fertilised ovum. It is certain that this cell does not contain in a preformed condition the structure of an adult organism. The conditions of environment in which any particular ovum develops itself are doubtless indefinitely complex from the physico-chemical standpoint, as indeed is the environment of any particular portion of matter existing anywhere. But these conditions also vary almost indefinitely in the case of different ova, whereas the adult organism to which the ovum gives rise reproduces in minute detail the enormously complex characters of the parent organism. We are thus driven to the assumption that the ovum contains within itself a structure which, given certain relatively simple conditions in the environment, reacts in such a way as to build up step by step, from materials in the environment, the structure of the adult organism. To effect this the germ-cell must have a structure almost infinitely more definite and complex than that of any cell in the adult organism. Difficult as it may be to form any conception of the mechanism of a secreting cell, it is infinitely more difficult to form the remotest idea of that of a germ-cell.

But we are still only at the beginning of the difficulty. The assumed tremendous mechanism of the germ-cell has been developed, together with the whole of the rest of the parent organism and countless other germ-cells, from a previous germ-cell. What must the "mechanism" of this cell have been? And that of its endless predecessors? We have reached the Euclidean *reductio ad absurdum*.

I might strengthen my argument by referring to the further difficulty over any physico-chemical conception of what occurs in the sexual fusion of the male and female cell, or in the process of partial reproduction after injury, or in the facts established by Driesch and others with regard to the extraordinary reproductive powers of each cell in developing embryos. But I have purposely confined my references to more simple and well-known facts; for the more simply the argument can be put, the better. I confess that as a physiologist I am struck with amazement at the manner in which heredity is often discussed by contemporary writers who endeavour to treat the subject from a mechanistic standpoint. Sometimes, indeed, the germ-cell is acknowledged to be a complicated structure, but at other times it is treated as a "plasma," which can be mixed with other "plasma," divided, or added to, as if for all the world it were so much treacle! I have tried to place clearly before you the assumptions in connection with heredity which to my mind make the physico-chemical theory of life unthinkable, even if it be tenaciously clung to in connection with those ordinary physiological phenomena where, as already explained, it has proved so disappointing.

Our aim as physiologists is to render physiological phenomena intelligible—in other words, to obtain general conceptions as to their nature. The point now reached is that the conceptions of Physics and Chemistry are insufficient to enable us to understand physiological phenomena. But if so, we need not sit down in despair, for we can look for other working conceptions. Are we justified in doing this? I think we are.

There is a prevalent popular idea that the world as presented to us under the conceptions of Physics and Chemistry is more than our own imperfect conception of reality, and corresponds completely with reality itself. Philosophy has shown us, however, that this idea must be erroneous; for if it were correct, knowledge of such a world would be impossible. This was first clearly pointed out almost two hundred years ago in this city by one of the greatest of Irishmen, George Berkeley, at that time a Fellow of Trinity College.[1] The lesson taught by Berkeley, Hume, and their successors is not that Physical Science is of less value than it appears to be, but that its fundamental hypotheses are only working hypotheses, applicable only so far as they successfully fulfil their purpose. Each different science is thus free to employ whatever working hypotheses may prove most useful in interpreting the order of phenomena with which it deals. We are thus perfectly justified in seeking to find a conception of life which will serve as a better working hypothesis than that of life as a physico-chemical process.

I venture to think that the conception we are in search of lies very near to hand and is indeed in common use, though in a form which has hitherto been too ill-defined for deliberate scientific employment. It is simply the conception of the living organism, which stands, or ought to stand, in the same relation to Biology as the conceptions of matter and energy to Physics, or of the atom to Chemistry. Let me try to give more definition to this conception. A living organism is distinguished by the fact that in it what we recognise as specific structure is inseparably associated with what we recognise as specific activity. Its activity expresses itself in the development and maintenance of its structure, which is nothing but the expression of this activity. Its identity as an organism is not physical identity, since from the physical standpoint the material and energy passing through it may be rapidly changing. In recognising it as an organism we are applying an elementary conception which goes deeper than the conceptions of matter and energy, since the apparent matter and energy contained in, or passing through, or reacting with, the organism are treated as only the sensuous expression of its existence. Even the environment is regarded as in organic relation with the organism, and not as a mere physico-chemical environment. It follows that for Biology we must clearly and boldly claim a higher place than the purely physical sciences can claim in the hierarchy of the sciences—higher because Biology is dealing with a deeper aspect of reality. It must also be the aim of Biology gradually to penetrate behind the sensuous veil of matter and energy which at present seems to permeate the organic world at all points.

[1] "Treatise concerning the Principles of Human Knowledge," 1710.

Let us now see how the conception just defined can be used as a scientific working hypothesis. In accordance with it any form of physiological activity is presumably related essentially, and not accidentally, to the other details of activity and structure in the same organism. Stated generally, therefore, the problem of Physiology is not to obtain piecemeal physico-chemical explanations of physiological processes, but to discover by observation and experiment the relatedness to one another of all the details of structure and activity in each organism as expressions of its nature as an organism.

The first step in physiological or morphological discovery is to observe the bare sensuous fact of some detail of physical or chemical change, or of composition or structure, in connection with an organism. It is only, however, when we find that this detail is not accidental that it becomes of biological interest. We can observe its constancy or otherwise in the same organism or similar organisms—that is to say, the constancy of its relations to other details of structure and activity. Or we can by experiment search for the element of constancy when it is at first sight hidden from our view. In so far as we find this, it seems to me that we reach physiological or biological explanation; but evidently the process of reaching it is at any stage in knowledge only imperfectly realised, since new details of activity and structure are constantly being revealed.

Concrete examples will make the matter clearer, and I shall first take as an example the progress of knowledge in relation to animal heat. It was of course common knowledge from early times that in the higher animals a certain amount of warmth in the body is present during life. With the invention of the thermometer the body-temperature could be measured, and its extraordinary constancy observed. When Lavoisier measured the heat-production of an animal, and compared the output of heat with the output of carbon dioxide and disappearance of oxygen in respiration, an immense step forward was taken. This step was in two distinct respects a very great one. In the first place it revealed an element of identity between organic and inorganic phenomena, since heat-production in an animal was shown to be accompanied by chemical changes quantitatively identical with those accompanying heat-production by oxidation outside the body. In the second place, and from the distinctively physiological point of view, it revealed a fundamental relation between heat-production, respiratory exchange, and the consumption of food.

As regards the first of these points I should like to say definitely that I, for one, firmly believe that could we only understand them fully we could bring organic and inorganic phenomena under the same general conceptions. Lavoisier's discovery, like that of Mayer in relation to the sources of muscular energy, was a great advance in this direction. But this is a very different thing from an advance in the direction of rendering life intelligible in terms of physico-chemical conceptions as we commonly understand them. Lavoisier's discoveries did nothing in the direction of reducing to physico-chemical terms the apparent teleological or, as I should prefer to say, "physiological" element in the phenomena of animal heat.

It is to the second point that I wish to direct special attention at present. Lavoisier's discovery rapidly brought the phenomena of animal heat into direct relation, not only with respiration but with nutrition, circulation of blood, excretion, and other processes; and it was gradually discovered that the maintenance of a constant body-temperature renders physiologically intelligible a large number of phenomena in connection with different bodily activities—for instance, increased metabolism with fall of external temperature, sweating or increased circulation through the skin with muscular work, the relative constancy of metabolism during starvation, and the physiological equivalence of proteid, carbohydrate, and fat in proportion to their energy values. These phenomena are intelligible on the assumption that warm-blooded animals actively maintain a certain body-temperature, just as they maintain a certain bodily structure and composition. This mode of explanation is not a physico-chemical one, but I venture very confidently to assert that it is a physiological one, and in fact the only kind of explanation which really interests and appeals to a true physiologist. The thread of identity which has been traced through the phenomena just referred to seems to me to have proved a real scientific clue.

As another example I may perhaps be allowed to refer shortly to the regulation of breathing, as this is a subject on which I have recently been working. Current accounts of the clock-like action of the respiratory centre during normal breathing, with the expansion and contraction of the lungs acting as a sort of governor through the vagus nerves, always filled me with suspicion, as it seemed to me that such a regulation was altogether unlike a physiological one. This led me to investigate the matter further, along with Mr. Priestley; and we had the satisfaction of being able to prove that the ventilation of the lungs is actually regulated with exquisite exactness, in such a way as to keep the partial pressure of carbon dioxide in the alveolar air and presumably, therefore, in the arterial blood, constant. In reality, therefore, the lung ventilation is regulated in accordance with the requirements of respiratory exchange; and what seems to be true physiological explanation has been advanced a short stage.

The advance of knowledge with regard to the circulation might be made the text of a similar discourse. By a process of abstraction the circulation of the blood may be regarded as a mere mechanical process, connected only by the accidents of physical structure with other physiological processes. Under the influence of mechanistic theories the blood-pressure and rate of blood-flow through different organs were indeed for long supposed to be the primary determining cause of the physiological activities of these organs, just as the rate and depth of breathing were supposed to determine the consumption of oxygen by the body. Evidence is, however, accumulating on all hands that the blood-supply to various parts, like the air-supply to the lungs, is in reality determined by physiological requirements. In other words, it is a direct expression of the nature of the organism, just as the common-sense idea of life would lead us to expect.

I may pass next to a branch of physiological knowledge which is still in its early infancy. Under the influence of mechanistic ideas Physiology has for long left completely out of account investigation into the formation and maintenance of organic structure. For mechanistic explanations structure had to be assumed, and as a consequence anatomy was left high and dry in a position of helpless isolation. If, however, the real aims of Physiology are those which I have tried to indicate, the separation between Physiology and Anatomy must tend to disappear: for the structure no less than the activity of each part must be determined by its relations to the structure and activities of other parts in the organic whole of the living organism. We can investigate these relations, just as we investigate the connection of secretion with respiratory exchange, circulation, or the composition of the blood; and they must evidently be physiological relations. Our aim is not the hopeless one of giving a physico-chemical explanation of the development and maintenance of organic structure, but simply discover the physiological relations which determine the structure of each part and its maintenance. Many facts bearing on this subject have recently been brought to light by the application of experimental methods to embryology, and by the study of reproduction of lost or injured parts, and of grafting: also by the study of so-called "internal secretion" in connection with various organs. It seems clear, however, that we are only at the beginning of a vast development of knowledge in this direction, and that for this development far more refined methods of dealing with the chemistry of the body will be required.

It was in connection with the facts of reproduction and heredity that the difficulties of the mechanistic theory of life were found finally to culminate. For the distinctively biological theory of life, to which I have endeavoured to give some definition, these difficulties do not exist. They are, it is true, not solved; but they are set aside as being due to wrong initial assumptions and therefore purely artificial. The difficulty remains of reconciling the fundamental conceptions of Biology with those of Physics and Chemistry. This is, however, a matter of which the discussion must be handed over to Philosophy, which has many similar matters to deal with. If it is a fundamental axiom that an organism actively asserts or maintains a specific structure and specific activities, it is clear that nutrition itself is only a constant process of reproduction: for the material of the organism is constantly changing. Not only is there constant molecular change, but the living cells are constantly being cast off and reproduced. It is only a step from this to the reproduction of lost parts which occurs so readily among lower organisms; and a not much greater step to the development of a complete organism from a single one of the constituent cells of an embryo in its early stages. In all these facts we have simply manifestations of the fundamental characters of the living organism. The reproduction of the parent organism from a single one of its constituent cells separated from the body seems to me only another such manifestation. Heredity, or, as it is sometimes metaphorically expressed, organic memory, is for Biology an axiom and not a problem. The problem is why death occurs, what it really is, and why only certain parts of the body are capable of reproducing the whole. These questions carry us, at least in part, beyond the present boundary lines of Biology. They involve those ultimate questions which, as has just been pointed out, it is the province of Philosophy to deal with.

To turn to another set of questions, the distinctively biological standpoint in Biology involves a change in what has in recent times become the ordinary attitude towards organic evolution. Since our conception of an organism is different in kind, and not merely in degree, from our conception of a material aggregate, it is clear that in tracing back life to primitive forms we are getting no nearer to what is called abiogenesis. The result of investigation in this direction can only be to extend further the domain of Biology and widen biological ideas. Our aim must be, in short, not to reduce organic to inorganic phenomena, but to bring inorganic phenomena into the domain of Biology.

I am well aware that it will be strongly maintained that the change of front which I have urged as necessary involves the giving up of all real attempt at scientific explanation in Biology. As already explained, this is a philosophical question, and I shall not attempt to deal further with it here. What immediately concerns us as biologists is whether the change of front will further or hinder biological advance, particularly in Physiology. Now the first requisite of a working hypothesis is that it should work, and I have tried to point out that as a matter of fact the physico-chemical theory of life has not worked in the past and can never work. As soon as we pass beyond the most superficial details of physiological activity it becomes unsatisfactory; and it breaks down completely when applied to fundamental physiological problems, such as that of reproduction. Those who aim at physico-chemical explanations of life are simply running their heads at a stone wall, and can only expect sore heads as a consequence. It seems to me that the proposed change of front is only the conscious adoption of a common-sense idea which is somewhat vaguely, perhaps, present in the minds of all men, and which has in reality guided biological advance in the past. This idea, as I have tried to show, is a working hypothesis which actually works, and affords clear guidance for future advance.

I would fain add a few words as to the relation of Physiology to Psychology and Ethics: for this is a subject of deep human interest. We know that at any rate the higher organisms are conscious and intelligent. This fact brings Physiology into touch with a new element in the behaviour of organisms. The subject is far too great a one for me to attempt to discuss here, but I should like to say that it appears to me very clear that just as Biology is something more than Physics and Chemistry, so Psychology is something more than Physiology, with the added assumption that consciousness is tacked on to certain physiological processes, if such a crude conception has any definite meaning. We can, it is true, by a process of abstraction treat sensation from the purely physiological side, as in investigating the physiology of the sense-organs; but this is Physiology and nothing else; for we are leaving out of account the distinctive elements of consciousness. At our present stage of knowledge life is not intelligence, and men or animals as intelligent individuals involve a deeper aspect of reality than Biology deals with. Our fundamental physiological working hypothesis cannot be successfully applied to the phenomena of intelligence, and the sooner and more definitely this is realised the better for Physiology.

In conclusion, let me endeavour to state shortly the main contention which I have endeavoured to place before you. It is that in Physiology, and Biology generally, we are dealing with phenomena which, so far as our present knowledge goes, not only differ in complexity, but differ in kind from physical and chemical phenomena; and that the fundamental working hypothesis of Physiology must differ correspondingly from those of Physics and Chemistry.

That a meeting-point between Biology and Physical Science may at some time be found, there is no reason for doubting. But we may confidently predict that if that meeting-point is found, and one of the two sciences is swallowed up, that one will not be Biology.

SECTON K.

BOTANY.

OPENING ADDRESS BY F. F. BLACKMAN, M.A., D.Sc., F.R.S., PESIDENT OF THE SECTION.

The Manifestations of the Principles of Chemical Mechanics in the Living Plant.

THE UNIFORMITY OF NATURE.

AMONG the phenomena of nature Man finds himself to be one of medium magnitude, for while his dimensions are about a billion times as great as those of the smallest atoms that compose him they are also about one-billionth part of his distance from the centre of his solar system.

From the vantage point of this medium magnitude the man of science scans eagerly the whole range of natural phenomena accessible to him with a strenuous desire for unity and simplification.

By the unwearying study of special sections of this long front of natural phenomena special guiding principles have been detected at work locally. No sooner has this been accomplished than, in obedience to this desire for continuity throughout, such principles have been freely extended on either side from the point of discovery.

Thus, the theory of probability, which dealt at first with so limited an occupation as drawing white and black balls out of an opaque bag, now is known as the only determinable factor in such remote things as the distribution of the duration of human lives and the effect of concentration of the colliding molecules in a solution upon the rate of their chemical change. Again, the principle of evolution discovered among living things has been extended, until to speak of the evolution of societies, of solar systems, or of chemical elements is now but commonplace.

The biologist, with all his special difficulties, has at least the limitation that he is only concerned with the middle range of the interminable hostile front of natural phenomena, and that for him is ordained the stubborn direct attack, leaving the brilliant attempts at outflanking movements to the astronomers on the one wing and the workers at corpuscular emanations on the other.

The atoms and molecules that the biologist has to deal with do not differ from those passing by the same names in the laboratories of chemistry and physics (at least no one suggests this), and their study may therefore be left to others. At the other end of the scale, with astronomical magnitudes we have not to deal, unless indeed we yield to the popular clamour to take over the canals on Mars as phenomena necessarily of biological causation.

In the study of that particular range of phenomena which is the special allotment of the physiologists, animal and vegetable, we have had ever before us the problem of whether there is not here some discontinuity in nature; whether the play of molecular and atomic forces occurring outside the living organism can ever account for the whole of the complexity and correlation of chemical and physical interactions demonstrable within the living structure.

As yet we are of course far from any answer to this question, and no one in a scientific assembly like this will call upon us for prophecies. Yet the subject to which I shall devote my Address has a bearing upon this question. I propose to consider a particular aspect of the relation of chemical changes in a test-tube to those taking place in a living growing plant, and this in the spirit of one who craves for continuity throughout natural phenomena.

The point of view from which the chemist regards the reaction taking place in his test-tube has undergone a change in the last twenty years, a change bringing it more into uniformity with that of the biologist. No longer content with an equation as a final and full expression of a given reaction, the chemist now studies with minutest detail and with quantitative accuracy the progressive stages of development of the reaction[1] and the effect upon it of varied external conditions, of light, temperature, dilution, and the presence of traces of foreign substances.

Perhaps it is too much to believe that this, as it were physiological, study of each reaction is the effect of some benign irradiation from the biological laboratory. At least, however, it is true that it is the modern study of "slow" chemical reactions which has made all this possible, and the living organism consists almost entirely of slow reactions. The earliest studied chemical reactions, those between substances which interact so quickly that no intermediate investigation can be made, did not of course lend themselves to this work, but nowadays whole classes of reactions are known which are only completed hours or days after the substances are initially mixed. To the slow reactions belong all the hydrolytic and dehydration changes of carbohydrates, fats, and proteids that bulk so largely in the metabolism of plants and animals, together with other fermentation changes such as are brought about by oxidases, zymases, and enzymes in general. This precise quantitative study of chemical reactions has been developing with remarkable acceleration for some twenty-five years, until it is grown almost into an independent branch of science, physical chemistry. This is sometimes called "general chemistry" because its subject is really the fundamental universal laws of the rate of chemical change, and these laws hold through all the families, genera, and species of chemical compounds, just as the same physiological laws apply to all the different types of plants.

Now if these laws are fundamental with all kinds of chemical change they must be at work in the living metabolic changes. If the chemical changes associated with *protoplasm* have any important factor or condition *quite different* from the state of things which holds when molecules react in aqueous solution in a test-tube, then it might happen that the operation of these principles of physical chemistry would be obscured and not very significant, though it is inconceivable that they should be really inoperative.

My present intention, then, is to examine the general phenomena of metabolism in an attempt to see whether the operations of these quantitative principles are traceable, and if so how far they are instrumental in giving a clearer insight into vital complexity.

THE DOMINANCE OF IRRITABILITY IN PHYSIOLOGY.

I think that certain manifestations of these principles are indeed quite clear, though not generally recognised, and that this neglect is largely due to the dominance of what our German colleagues call "Reizphysiologie"—the notion that *every* change in which protoplasm takes part is a case of the "*reaction*" *of an* "*irritable*" *living substance to a* "*stimulus*." Now this general conception of protoplasmic irritability, of stimuli and reactions was, of course, a splendid advance, the early development and extension of which we owe largely to our veteran physiologist Prof. Pfeffer, of Leipzig. Great as is the service it has rendered to many departments of botany, yet in one direction, I think, it has overflowed its legitimate bounds and swamped the development of the physical-chemical concepts which I shall indicate later on. The great merit of the "stimulus and reaction" conception is that it supplies a very elastic general formula for the sort of causal connection that we find occurring in all departments of biology; a formula which allows the phenomena to be grouped, investigated, and formally expounded, whether they be the temporary turgor-movements of "sensitive" plants, the permanent growth movements of tropistic curvatures, or the complex changes of plant-form and development that result from present and past variations of external conditions.

The strength and the weakness of the conception lie in its extraordinary *lack of particularity*. When an irritable cell responds to a stimulus by a reaction nothing is implied about the mechanism connecting the *cause and the effect*, and nothing even about the relative magnitudes of these, but all this is left for special research on the case under consideration. The one natural chain of cause and effect that is recognised to be outside this comprehensive category is that rather uncommon one in which a definite amount of energy of one kind is turned into an equivalent definite amount of energy of another. Here we have a direct "equation of energy," whereas in a reaction to a stimulus we are said to have typically an "unloosing" effect—a liberation of potential energy by a small incidence of outside energy, as in the classical analogies, drawn from completely comprehended non-living things, of a cartridge exploded by a blow, or the liberation into action of a head of water by the turning of a tap.

So elastic a conception may be easily stretched to fit almost any sequence of phenomena with the apparent closeness that argues a bespoken garment. We must therefore be critically on our guard against cases of such sartorial illusion.

THE PRINCIPLES OF CHEMICAL MECHANICS.

That my consideration of particular cases may be intelligible it seems necessary that I devote a few minutes to outlining the four quantitative mechanical principles which govern every single chemical reaction, though much that I have to say has been drawn from elementary books on physical chemistry.

These four principles are concerned with (1) the nature of the reaction in question; (2) the amount of reacting substances that happen to be present; (3) the temperature at which the reaction is taking place; and (4) the influence of catalysts upon the reaction.

For the moment we will confine ourselves to the first two matters, and assume that catalysts are absent and the substances at constant temperature.

(1) The first principle that we have to consider is that which declares that no chemical reaction is really instantaneous, though the interaction of substances is often so fast that a direct measurement of its rate cannot be made; and, further, that every reaction has its own *specific reaction-velocity* which distinguishes it from other reactions. This is expressed by giving to each particular reaction a numerical *velocity-coefficient* which is low or high proportionally as the reaction is slow or quick.

(2) This coefficient only expresses the actual experimental velocity when the reacting substances are present in *unit* concentration, because difference of concentration is just the most important factor controlling the actual reaction-velocity.

If a solution of a substance A of unit concentration is undergoing change, then to keep this reaction going at its present rate fresh amounts of A must be added continually just to equal the amount removed by the reaction and so keep the substance up to unit concentration. The amount of A that had to be added thus per unit time would give an exact measure of the amount being decomposed, *i.e.*, of the specific velocity of this reaction.

If the reaction were started with A at double unit concentration, then twice as much A would have to be added *per* unit time to keep the reaction velocity constant at the double rate it would have started at.

And with higher concentrations proportionally more A would have to be added. It is therefore shown that the amount of chemical change going on in unit time is proportional to the concentration. This is a most fundamental principle of chemical mechanics, known as the *law of mass*, and it may be stated thus: *the amount of chemical change taking place at any time is always proportional to the amount of actively reacting substance (or substances) present.*

To carry out experiments by the procedure given above is in practice very difficult, and the velocities of reactions are never measured by the chemist in this way. In a living organism this continual bringing up of new supplies of

[1] Modern research has made it clear that reactions conventionally represented by complex equations of many interacting molecules really take place in a succession of simple stages, in each of which, perhaps, only two molecules interact.

material to maintain a constant rate of change is the ordinary way of life, but in the chemical laboratory procedure is different. There, definite amounts of substances are initially mixed in a vessel, and the reaction is allowed to progress by itself without further additions. In this case there is a continual falling off of the concentration of the substance, and so a corresponding diminution of the actual reaction-velocity.

In this procedure the diminution of the initial amount of substance can be actually measured by withdrawing small samples at intervals of time and analysing them. Let us consider a definite example. Cane-sugar can be hydrolysed, under various conditions, to give two molecules of hexose, according to the equation

$$C_{12}H_{22}O_{11} + H_2O = 2C_6H_{12}O_6.$$

This reaction goes on, though extremely slowly, when an aqueous solution of cane-sugar is kept very hot in a beaker. Suppose we started with, say, 128 grams dissolved in a litre of water and traced the diminution of this amount down towards zero by withdrawing samples at intervals of time and analysing them. If we plotted the sugar-content of these successive samples against the times when they were taken we should get the curve given in Fig. 1. If we call n minutes the time taken for the sugar to diminish from 128 grams to 64 grams, we should find that in the second n minutes the sugar had fallen to 32 grams, after $3n$ minutes to 16 grams, and so on, the amount halving itself every n minutes. Thus the amounts of cane-sugar hydrolysed in successive equal intervals are 64, 32, 16, 8, 4, 2, 1 grams, amounts in each case just exactly proportional to the quantity of cane-sugar then remaining in solution, thus exemplifying the law of mass.

Such a curve as A in Fig. 1, which changes by a constant multiple for successive units of time (here halving itself every n minutes) is known as a logarithmic curve; the velocity of reaction at any moment is exactly indicated by the steepness of the curve at that moment; the velocity is greatest at first, and it declines to almost zero as the curve approaches the horizontal at the end of the reaction.

When instead of the decomposition of a single substance we deal with two dissolved substances, A and B, reacting together, then as *both* of them go on being thus used up, the amount of change must be ever proportional to tne mass or amount of A present multiplied by the mass of B present.

There is a special important case when the amount of, say, B is in very great excess of that amount required to unite with the whole of A. Then all through the slow progress of the reaction the amount of B never becomes reduced enough to make appreciable difference to its mass, and it may be considered as practically constant all along. In such a case the rate of the reaction is found to be proportional simply to the amount of A present, and we get again the curve A, Fig. 1. Here the amount of A may be considered as a limiting factor to the amount of reaction; B being in such great excess never falls low enough to take a practical part in determining the velocity.

The case of the hydrolysis of cane-sugar in aqueous solution is just such a case. The water itself enters into the reaction, but so little is used up in relation to the enormous excess present that the amount remains practically constant, and the rate of hydrolysis of the cane-sugar is determined only by the amount of the cane-sugar present at any moment.[1]

(3) We have now shown how the actual amount of chemical change going on in a solution is determined by the combined effect of (1) the specific reaction velocity, and (2) the law of mass. We have next to point out that the specific reaction coefficient is not the same in all circumstances, but is affected by variations of external conditions, always by temperature, and generally by the presence of traces of so-called catalysts.

The relation to temperature we will postpone, and proceed to consider our third principle, the acceleration of reaction velocity by *catalytic agents*.

It has long been known that small additions of various foreign substances may have a great effect in increasing the rate at which a reaction is proceeding. Thus this hydrolysis of cane-sugar, so slow with pure water, goes at a fair velocity if a few drops of a mineral acid are added to the solution, while the addition of a trace of a particular enzyme (invertase from plant or animal) enormously increases the rate of change, so that the whole 128 grams of cane-sugar are soon hydrolysed to hexose. The reaction progresses quantitatively in the same sort of way as before, giving a logarithmic curve of sugar-content. Indeed the same graphic curve, Fig. 1, A, would represent the facts if the value of n were reduced from many hundred minutes to quite a few.

The most striking point about this new state of things is that the added body is not used up by its action, but the acid or enzyme is still present in undiminished amount when the reaction is completed.

Such actions were at first styled "contact" actions, but are now known as catalytic actions, because we have learned that the catalyst does not work just by contact

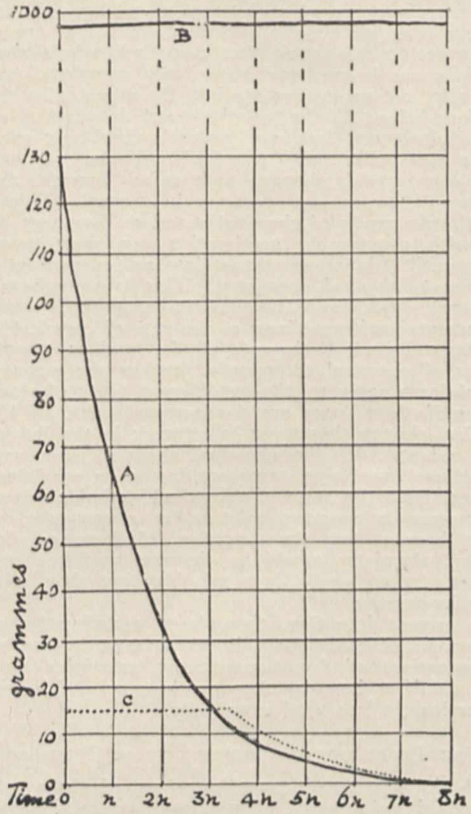

FIG. 1.

but by combining with the sugar to form an intermediate addition compound, and that this compound is then split up by the water liberating the catalyst again, but freeing the sugar part, not as cane-sugar, but combined with the water to form two molecules of hexose.

On many chemical reactions, finely divided metals such as platinum and gold have a very powerful catalytic action. Thus platinum will cause gaseous hydrogen and oxygen to unite at ordinary temperatures, and will split up hydrogen dioxide with the formation of oxygen. The intermediate stages in this catalytic decomposition may be summarily simplified to this—

$$H_2O_2 + Pt = PtO + H_2O \text{ and } PtO + H_2O_2 = Pt + O_2 + H_2O.$$

Thus the reaction goes on and on by the aid of the appearing and disappearing "intermediate compound" PtO until at the end the H_2O_2 is all decomposed and the platinum is still present unaffected.

The enzymes are the most powerful catalytic agents known, and most of them are specifically constituted to

[1] 128 grams cane sugar unite with 6·7 grams water in hydrolysis, and in our experiment nearly 1000 grams of water are present.

effect the hydrolysis, oxidation, reduction, or splitting of some definite organic compound or group of compounds containing similar radicals.

Innumerable enzymes have in late years been isolated from the plant-body, so that it would seem that there is one present catalytically to accelerate each of the slow single changes that in the aggregate make up the complex metabolism of the plant.

The law of mass applies with equal cogency to *catalytic* reactions. If twice the amount of acid is added to a solution of cane-sugar (or twice the amount of enzyme) then the reaction-velocity is doubled, and hydrolysis proceeds twice as fast. As the catalyst is not destroyed by its action, but is continually being set free again, the concentration of the catalyst remains the same throughout the reaction; while, on the contrary, the amount of cane-sugar continually decreases.

If the catalyst be present in great excess the amount of hydrolysis will be limited by the amount of cane-sugar present, and as this is used up so the reaction will progress by a logarithmic curve as in Fig. 1, A. In this case B may represent the amount of catalyst. If, on the contrary, there is a large amount of sugar and very little acid or enzyme present, so that the catalyst becomes the limiting factor, then we happen upon a novel state of things; for by the law of mass the rate of hydrolysis will now remain constant for some time until the excess of sugar is so far reduced that it in turn becomes a limiting factor to the rate of change. In this case the velocity curve would consist of a first phase with a straight horizontal line of uniform reaction-velocity leading into the second phase of a typical falling logarithmic curve (see Fig. 1, C). These conditions have been experimentally examined by Horace Brown and Glendinning, and fully explained and expounded by E. F. Armstrong in part ii. of the critical "Studies in Enzyme Action."[1]

Having now outlined the three fundamental principles of reaction-velocity, the law of mass, and the catalytic acceleration of reaction-velocity, we are in a position to consider the broad phenomena of metabolism or chemical change in the living organism from the point of view of these principles of chemical mechanics.

THE METABOLISM OF THE PLANT CONSIDERED AS A CATALYTIC REACTION.

Plants of all grades of morphological complexity, from bacteria to dicotyledons, have this in common, that throughout their active life they are continually growing. Putting aside the *qualitative* distribution of growth that determines the morphological form, as a stratum of phenomena above the fundamental one that we are about to discuss, we find that this growth consists in the assimilation of dead food-constituents by the protoplasm with a resulting increase in the living protoplasm accompanied with the continual new formation of dead constituents, gaseous CO_2, liquid water, solid cellulose, and what not. This continual flux of anabolism and katabolism is the essential character of metabolism, but withal the protoplasm increases in amount by the excess of anabolism over katabolism.

Protoplasm has essentially the same chemical composition everywhere, and in the whole range of green plants the same food-materials seem to be required; the six elements of which proteids are built are obviously essential in quantity as building material, but in addition small amounts of Fe, Ca, K, Mg, Na, Cl, and Si are in some other way equally essential. What part these secondary elements play is still largely a matter of hypothesis.

Regarding metabolism thus crudely as if it were merely a congeries of slow chemical reactions, let us see how far it conforms to the laws of chemical mechanics we have outlined.

If the supply of any one of these essential elements comes to an end, growth simply ceases and the plant remains stationary, half-developed. If a Tropæolum in a pot be watered with dilute salt-solution, its stomata soon close permanently, and no CO_2 can diffuse in to supply the carbon for further growth of the plant. In such a condition the plant may remain for weeks looking quite healthy, but its growth may be quite in abeyance.

[1] Proc. Roy. Soc., vol. lxxiii., 1904, p. 511.

In agricultural experience, in manuring the soil with nitrogen and the essential secondary elements, the same phenomenon is observed when there is a shortage of any single element. If a continuous though inadequate supply of some one element is available, then the crop development is limited to the amount of growth corresponding to this supply. Agriculturists have formulated the "law of the minimum," which states that the crop developed is limited by the element which is minimal, *i.e.*, most in deficit. Development arrested by "nitrogen-hunger" is perhaps the commonest form of this. All this is of course in accordance with expectation on physical-chemical principles. The quantity of anabolic reaction taking place should be proportional to the amount of actively reacting substances present, and if any one essential substance is quite absent the whole reaction must cease. It therefore seems clouding a simple issue and misleading to say of a plant which, from the arrested development of nitrogen-hunger, starts growth again when newly supplied with nitrogen that this new growth is a response to a "*nitrogen stimulus*." It would appear rather to be only the removal of a limiting condition.

Let us now move on a stage. Suppose a growing plant be liberally supplied with all the thirteen elements that it requires, what, then, will limit its rate of growth? Fairy bean-stalks that grow to the heavens in a night elude the modern investigator, though some hope soon to bring back that golden age with overhead electric wires and underground bacterial inoculations. If everything is supplied, the metabolism should now go on at its highest level, and quantities of carbon, nitrogen, hydrogen, and oxygen supplied as CO_2, nitrates, and water will interact so that these elements become converted into proteid, cellulose, &c. Now this complex reaction of metabolism only takes place in the presence of protoplasm, and a small amount of protoplasm is capable of carrying out a considerable amount of metabolic change, remaining itself undestroyed. We are thus led to formulate the idea that metabolism is essentially a catalytic process. In support of this we know that many of the inherent parts of the protoplasmic complex are catalytic enzymes, for these can be separated out of the protoplasm, often simply by high mechanical pressure. We know, too, nowadays that the same enzymes that accelerate katabolic processes also accelerate the reverse anabolic processes.

In time a small mass of protoplasm will, while remaining itself unchanged, convert many times its own weight of carbon from, let us say, the formaldehyde (HCHO) of photosynthesis to the carbon dioxide (CO_2) of respiration.

If metabolism is a complex of up-grade and down-grade changes catalysed by protoplasm we must expect the amount of metabolism to obey the law of mass and to be proportional to the masses of substances entering into the reaction. The case when any one essential element is a limiting factor we have already considered. When all are in excess, then the *amount of the catalyst present* becomes in its turn the limiting factor. Transferring this point of view to the growing plant, we expect to find the limited mass of protoplasm and its constituent catalysts setting a limit to the rate of metabolic change in the extreme case where all the materials entering into the reaction are in excess. When once this supply is available further increase in supplies cannot be expected to accelerate the rate of growth and metabolism beyond the limit set by the mass of protoplasm. This, of course, is in accordance with common experience. The clearest experimental evidence is in connection with respiration and the supply of carbohydrates—this, no doubt, because the carbohydrate material oxidised in respiration is normally stored inside plant-cells in quantity and can be estimated. When the supplies for an internal process have to be obtained from outside, then we have the complications of absorption and translocation to obscure the issue, especially in the case of a higher plant.

Let us first take a case where the carbohydrate supply is in excess and the amount of catalytic protoplasm is small and increasing. Thus it is in seeds germinating in the dark: respiration increases day by day for a time, though carbohydrate reserves are steadily decreasing. Palladine[1] has investigated germinating wheat by analysing

[1] "Revue gén. de Botanique," tome viii., 1896.

the seedlings and determining the increase of the essential (non-digestible) proteids day by day. The amount of these proteids he regards as a measure of the amount of actual protoplasm present. Assuming this to be so, he finds an approximately constant ratio between the amount of protoplasm at any stage and the respiration.

As germination progresses in the dark the supplies of reserve carbohydrate presently fail, and then the respiration no longer increases in spite of the abundant protoplasm. According to our thesis the catalyst is now in excess and the CO_2 production is limited by the shortage of respirable material.

This second type of case was more completely investigated by Miss Matthæi and myself in working on the respiration of cut leaves of cherry-laurel kept starved in the dark. For a time the CO_2 production of these non-growing structures remains uniform, and then it begins to fall off in a logarithmic curve, so that the course of respiration is just like C in Fig. 1. We interpret both phenomena in the same way: in the initial level phase the respirable material in the leaf is in excess, and the amount of catalytic protoplasm limits the respiration to the normal biological level; in the second falling phase some supply of material is being exhausted, and we get a logarithmic curve controlled by the law of mass, as much, it would seem, as when cane-sugar is hydrolysed in aqueous solution.

After these two illustrations of the action of the law of mass from the more simple case of respiration we return to the consideration of the totality of metabolic reactions as exemplified in growth.

What should we expect to be the ideal course of growth, that is, the increase of the mass of the plant regarded as a complex of reactions catalysed by protoplasm? Let us consider, first, the simplest possible case, that of a bacterium growing normally in a rich culture solution. When its mass has increased by anabolism of the food material of the culture medium to a certain amount it divides into two. As all the individuals are alike, counting them would take the place of weighing their mass. The simplest expectation would be that, under uniform conditions, growth and division would succeed each other with monotonous regularity, and so the number or mass of bacteria present would double itself every *n* minutes. This may be accepted as the ideal condition.

The following actual experiment may be quoted to show that for a time the ideal rate of growth is maintained, and that at the end of every *n* minutes there is a doubled amount of protoplasm capable of catalysing a doubled amount of chemical change and carrying on a doubled growth and development.

From a culture of *Bacillus typhosus* in broth at 37° C. five small samples were withdrawn at intervals of an hour, and the number of bacteria per unit volume determined by the usual procedure. The number of organisms per drop increased in the following series: 6·7, 14·4, 33·1, 70·1, 161·0.[1] This shows a doubling of the mass of bacteria in every fifty-four minutes and is the case actually represented in the strictly logarithmic curve of Fig. 2.

We may quote some observations made by E. Buchner[2] of the rate at which bacteria increase in culture media. *Bacillus coli communis* was grown at 37° C. for two to five hours, and by comparison of the initial and final numbers of bacteria the time required for doubling the mass was calculated. Out of twenty-seven similar experiments a few were erratic, but in twenty cases the time for doubling was between 19·4 and 24·8 minutes, giving a mean of 22 minutes. This produces an increase from 170 to 288,000 in four hours. No possible culture medium will provide for prolonged multiplication of bacteria at these rates.

Cohn[3] states that if division takes place every sixteen minutes, then in twenty-four hours a single bacterium 1μ long will be represented by a multitude so large that it requires twenty-eight figures to express it, and placed end to end they would stretch so far that a ray of light to travel from one end to the other would take 100,000 years.

[1] For this unpublished experiment on bacterial growth I am indebted to Miss Lane-Claypon, of the Lister Institute of Preventive Medicine.
[2] Buchner, "Zuwachsgrossen u. Wachsthumsgeschwindigkeiten" (Leipzig, 1901).
[3] Cohn, "Die Pflanze," p. 438 (Breslau, 1882).

The potentialities of protoplasmic catalysis are thus made clear, but the actualities are speedily cut short by limiting factors.

For a while, however, this ideal rate of growth is maintained. At the end of every *n* minutes there is a doubled amount of protoplasm present, and this will be capable of catalysing twice the amount of chemical change and carrying on a doubled amount of growth and development. This is what common sense and the law of mass alike indicate, and is exactly what this logarithmic curve in Fig. 2 expresses.

This increase of the amount of catalytic protoplasm by its own catalytic activity is an interesting phenomenon. In Section K we call it growth, attribute it to a specific power of protoplasm for assimilation (in the strict sense), and leave it alone as a fundamental phenomenon, but are much concerned as to the distribution of the new growth in innumerable specifically distinct forms. In the Chemical Section they call this class of phenomenon "autocatalysis," and a number of cases of it are known. In these a chemical reaction gives rise to some substance which happens to catalyse the particular reaction itself, so that it goes on and on with ever-increasing velocity. Thus, we said that free acid was a catalyst to the hydrolysis of cane-sugar; suppose now that free acid were one of the products of the hydrolysis of sugar, then the catalyst would continually increase in amount in the test-tube, and the reaction would go faster and faster. Under certain conditions this actually happens. Again, when methyl acetate is hydrolysed we normally get methyl alcohol and free acetic acid. This free acid acts as a catalyst to the hydrolysis, and the rate of change continually accelerates. Here, if the supply of methyl acetate were kept up by constant additions, the reaction would go faster and faster with a logarithmic acceleration, giving a curve of velocity identical with Fig. 2, A.

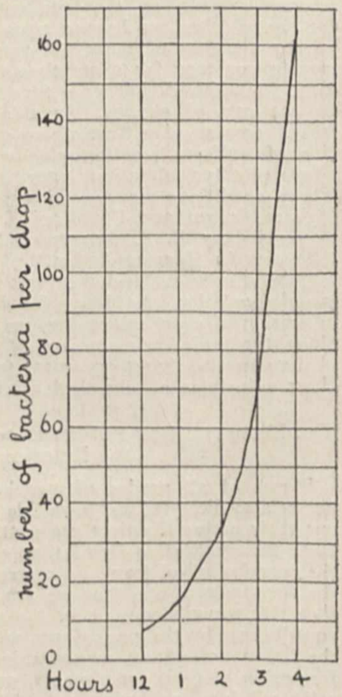

FIG. 2.

For a clear manifestation of this autocatalytic increase in the plant it is, of course, essential that the supply of food materials to the protoplasm be adequate.

Another case where we might look for a simple form of this autocatalytic increase in the rate of conversion of food materials to anabolites would be in the growth of a filamentous alga, like Spirogyra. Here, as in the bacterium, all the cells are still capable of growth. In this case the food-material needed in greatest bulk is carbon, which has to be obtained by photosynthesis. Some experiments have been started in the Cambridge Laboratory on the rate of growth of Spirogyra in large tubs of water kept at different temperatures and with varying facilities for photosynthesis and metabolism. Under rather depressing conditions the Spirogyra took several days to double its weight—a rate of metabolism out of all comparison slower than that of bacteria. Experiments on these lines, with the different food materials as limiting factors, should give instructive results.

We turn now to consider the growth of a flowering plant. Here conditions are more complex, and we know that at the flowering stage or end of the season the growth diminishes considerably. This difference from a simple alga or bacterium we can only regard as a secondary acquisition in relation to the external conditions—either a reaction to a present external stimulus or to the memory of past stimuli. In a flowering plant, too, all the cells do not continue to grow; many cells differentiate and cease to grow, and also some of the groups of meristem remain dormant in axillary buds. Clearly the growth curve cannot continue to accelerate logarithmically, and in later phases it must tail off; the "grand period" which growth is said to exhibit is another way of stating this. It will, however, be of great interest to us to see what will be the form of the curve of growth during the early period of development.

The importance of this class of work has been realised in Geneva, and detailed work is now being done under the inspiration of Prof. Chodat[1] in which the curve not only of growth (fresh weight) but of the uptake of all the separate important elements in selected plants is being carefully followed.

With plants grown in the open, climatic disturbances must occur. We shall therefore figure a curve for the fresh weight of a maize plant grown in water-culture. This is prior to the Geneva work, and due to Mlle. Stefanowska,[2] who has studied also the growth-curves of small animals. The first phase of the curve, lasting some fifty days, shows strictly uniform acceleration, doubling the weight of the

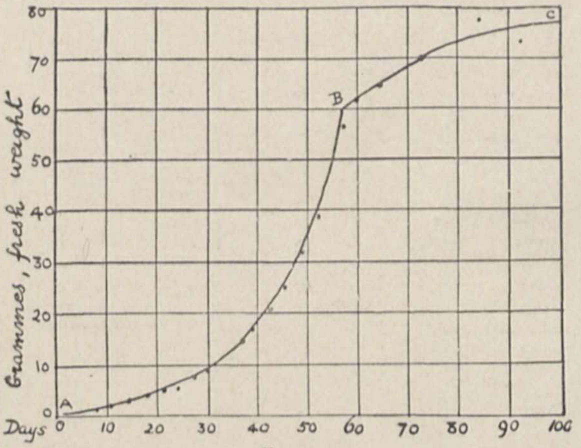

FIG. 3.

plant every ten days (Fig. 3). The precise external conditions are not stated.

In spite of the morphological complexity the autocatalytic reaction of growth is apparently not checked by inadequate supplies before the plant enters rather suddenly upon the second phase. Here, from the present point of view, we consider that the progress of growth is interrupted, not by the primary physical-chemical causes, but by secondary causes, presumably to be classed in the category of stimulus and reaction.

The numerous curves for the accumulation of different organic and mineral constituents worked out for barley and buckwheat at Geneva are of similar form, but do not keep up the uniform rate of doubling so well as does the curve of total fresh weight.

In this connection the tall and dwarf forms of the same plant present an interesting problem, and some experiments have been started on sweet peas at Cambridge. At the time of germination the seedlings weigh about the same, whereas at the end of the season the weight of a tall plant is many times that of a dwarf "cupid" growing alongside under similar conditions. Is the difference due to a less vigorous autocatalysis in the dwarf form, so that throughout its growth it takes a greater number of days to double its weight? Construction of the curves of growth

[1] A. Monnier, "Les Matières minérales et la Loi d'Accroissement des Végétaux," Geneva, 1905: N. Déléano, "Le Rôle et la Fonction des Sels minéraux dans la Vie de la Plante," Geneva, 1907. See also the independent work of Tribot, *Comptes rendus de l'Acad. des Sciences*, October 14, 1907.

[2] Stefanowska, *Comptes rendus de l'Acad. des Sciences*, February 1, 1904.

through the season will show whether it is this or some other alteration in the form of the curve.

I now propose to say a few words about one last point in connection with growth considered as a phenomenon of catalysis before passing on to deal with the effects of temperature.

Of the metallic elements that are essential for the growth of plants some occur in such minute quantities that one can only imagine their function is catalytic. If iron, for instance, played any part in metabolism which involved its being used up in any building material or by-product of metabolism, then a larger amount than actually suffices should be advantageous. If its function is catalytic the iron would go on acting indefinitely without being consumed, and so a minute trace might serve to carry out some essential, and even considerable, subsection of metabolism.

Elements like manganese, magnesium, and iron are often associated with non-vital catalytic action, and a preparation of iron has recently been quantitatively investigated which seems to have literally all the properties of an organic oxydase from plant tissues.[1]

So long ago as 1869 Raulin observed that traces of unessential salts, in particular those of zinc, added to the culture medium in which he grew the fungus Sterigmatocystis caused a rapid acceleration of the growth rate. The time that the mycelium took to double its weight was now reduced to a half or even a third. This continued enormous effect of so small a trace of substance is possibly to be regarded as an added catalyst to the normal protoplasmic apparatus. This sort of effect is currently labelled "chemical stimulation," and has been interpreted as an

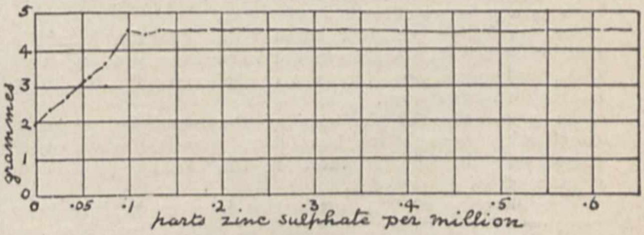

FIG. 4.

attempt of the fungus to grow away from an unpleasant environment. To me it looks as if such chemical stimulation were really another example of the injudicious extension of the concept of stimulus and reaction.

This effect of zinc upon the growth of mycelium has recently been verified and extended by Javillier,[2] who has made comparative cultures with increasing doses of zinc salt. He grew Sterigmatocystis for four days at 34° C. in media with graded additions of zinc salts. As the graphic representation shows, he finds a continuous regular increase of the number of grams of final dry weight with doses up to 0.00001 per cent., and then no greater but an equal effect up to 100 times as large a dose.

This form of curve with uniform rise at first, abruptly changing to a level top, suggests, as I have pointed out elsewhere,[3] the cutting-off of the primary rising effect by a limiting factor. In this case presumably the limit set by some other subsection of the metabolism has been attained.

ACCELERATION OF REACTION-VELOCITY BY TEMPERATURE.

We now turn to consider the fourth and last of the principles of chemical mechanics which we might expect to find manifested in metabolism.

It is a universal rule that rise of temperature quickens the rate at which a chemical reaction proceeds. Of course, in some rare conditions this may not be obvious, but be obscured by superposed secondary causes; but almost always this effect is very clearly marked.

Further, the nature of the acceleration is a peculiar one.

[1] J. Wolff, "Des Péroxydiastases artificielles," *Comptes rendus de l'Acad. des Sciences*, June 9, 1908.

[2] *Comptes rendus de l'Acad. des Sciences*, December, 1907.

[3] "Optima and Limiting Factors," *Annals of Botany*, vol. xix., April, 1905.

Rise of temperature affects nearly all physical and chemical properties, but none of these is so greatly affected by temperature as is the velocity of chemical reaction. For a rise of 10° C. the rate of a reaction is generally increased two or three fold, and this has been generalised into a rule by van't Hoff. As this increase is repeated for each successive rise of 10° C. either by the same factor or a somewhat smaller one, the acceleration of reaction-velocity by temperature is logarithmic in nature, and the curve representing it rises ever more and more steeply. Thus keeping within the vital range of temperature a reaction with a temperature factor of ×2 per 10° C. will go sixteen times as fast at 40° C. as at 0° C., while one with a factor of ×3 will go eighty-one times as fast.

This general law of the acceleration of reactions by temperature holds equally for reactions which are being accelerated by the presence of catalysts. As we regard the catalyst as merely providing for the particular reaction it catalyses, a quick way round to the final stage by passing through the intermediate stage of forming a temporary addition-compound with the catalyst itself, so we should expect rise of temperature to accelerate similarly these substituted chemical reactions.

If this acceleration is a fundamental principle of chemical mechanics it is quite impossible to see how vital chemistry can fail to exhibit it also.

ACCELERATION OF VITAL PROCESSES BY TEMPERATURE.

At present we have but a small number of available data among plants to consider critically from this point of view. But all the serious data with which I am acquainted, which deal with vital processes that are to be considered as part of the protoplasmic catalytic congeries, do exhibit this acceleration of reaction-velocity by temperature as a primary effect.[1]

Let us briefly consider these data. On the katabolic side of metabolism we have the respiratory production of CO_2, and opposed to it on the anabolic side the intake of carbon in assimilation.

As a measure of the rate of the metabolic processes constituting growth we have data upon the division of flagellates; and finally there is the obscure process of circulation of protoplasm.

The intensity of CO_2 production is often held to be a measure of the general intensity of metabolism, but any relation between growth-rate and respiration has yet to be clearly established. Our science is not yet in the stage when quantitative work in relation to conditions is at all abundant; we are but just emerging from the stage that chemistry was in before the dawn of physical chemistry.

Taken by itself the CO_2-production of an ordinary green plant shows a very close relation with temperature. In the case of the cherry-laurel worked out by Miss Matthaei and myself the respiration of cut leaves rises by a factor of 2·1 for every 10° C. (See Fig. 5, Resp.) This has been investigated over the range of temperatures from 16° C. to 45° C. At this higher temperature the leaves can only survive ten hours in the dark, and their respiration is affected in quite a short time, but in the initial phases the CO_2 output has the value of 0·0210 gr. per hour and unit weight of leaf, while at 16°·2 C. the amount is only 0·0025 gr. CO_2. Thus the respiration increases over a range of ten-fold with perfect regularity solely by increase of temperature. No reaction in a test-tube could show less autonomy. At temperatures above 45° C. the temperature still sooner proves fatal unless the leaf is illuminated so as to carry out a certain amount of photosynthesis and compensate for the loss of carbon in respiration. Thus, with rising temperature, there is at no time any sign of an optimum or of a decrease of the intensity of the *initial* stage of respiration.

Here, then, on the katabolic side of metabolism we have no grounds for assuming that "temperature-stimuli" are at work regulating the intensity of protoplasmic respiration, but we find what I can only regard as a purely physical-chemical effect. The numbers obtained by Clausen[2] for the respiration of seedlings and buds at different temperatures indicate a temperature coefficient of about 2·5 for a rise of 10° C.

To this final process of katabolism there could be no greater contrast than the first step of anabolism, the assimilation of carbon by the protoplasm as a result of photosynthesis. We must therefore next inquire what is the relation of this process to temperature.

This question is not so simple, as leaves cannot satisfactorily maintain the high rate of assimilation that high temperatures allow. The facts of the case were clearly worked out by Miss Matthaei,[1] the rate of assimilation by cherry-laurel leaves being measured from −6° C. to +42° C. Up to 37° C. the curve rose at first gently and then more and more steeply, but on calculating out the values it is found that the acceleration for successive rises of 10° C. becomes less and less. Between 9° C. and 19° C. the increase is 2·1 times, the highest coefficient measured, and exactly the same coefficient as for respiration in this plant, which in itself is a striking point, seeing how different the processes are. (See Fig. 5, Assim.) The decrease of the coefficient with successive rises is a state of things which is quite general among non-vital reactions. A critical consideration of the matter leads one to the conclusion, however, that this failure to keep up the temperature acceleration is really due to secondary

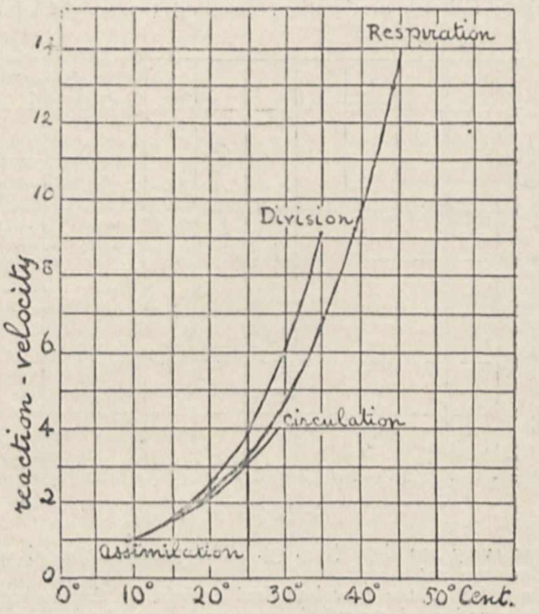

FIG. 5.

causes, as is also the appearance of an optimum at about 38° C. Some of these causes have been discussed by me elsewhere,[2] and I hope to bring a new aspect of the matter before the Section in a separate communication. The conclusion formerly come to was that probably in its initial stages assimilation at these very high temperatures started at the full value indicated by a theoretically constant coefficient, but that the protoplasm was unable to keep up the velocity, and the rate declined. It must be borne in mind here that quite probably no chloroplast since the first appearance of green cells upon the earth had ever been called upon for anything like such a gastronomic effort as these cherry-laurel leaves in question. It is not to be wondered that their capacities speedily declined at such a banquet, and that the velocity-reaction of anabolic synthesis traces a falling curve in spite of the keeping up of all the factors concerned, to wit, temperature, illumination, and supply of CO_2. This decline is not permanent, but after a period of darkening the power of assimilation returns. Physical-chemical parallels can easily be found among cases where the accumulation of the products of a

[1] A collection of twenty cases, mostly from animal physiology, by Kanitz (*Zeits. für Elektrochemie*, 1907, p. 707), exhibits coefficients ranging from 1·7 to 3·3.

[2] *Landwirtschaftliche Jahrbücher*, Bd. xix., 1890.

[1] Phil. Trans Roy. Soc., Ser. B, vol. cxcvii., 1904.

[2] "Optima and Limiting Factors," *Annals of Botany*, vol. xix., April, 1905.

reaction delays the apparent velocity of the reaction, but this complicated case may be left for further research.

In relation to assimilation, then, we must say that owing to secondary causes the case is not so clear over the whole range of temperature as that of respiration, but that at medium temperatures we have exactly the same relation between reaction-velocity and temperature.

We may consider now some data upon the combined net result of anabolic and katabolic processes. Such total effects are seen in their clearest form among unicellular saprophytic organisms for which we have a few data. Mlle. Maltaux and Prof. Massart[1] have published a very interesting study of the rate of division of the colourless flagellate *Chilomonas paramoecium* and of the agents which they say stimulate its cell-division, in particular alcohol and heat.

They observed under the microscope the time that the actual process of division into two took at different temperatures. From twenty-nine minutes at 15° C. the time diminished to twelve minutes at 25° C., and further to five minutes at 35° C. The velocities of the procedure at the three temperatures 10° C. apart will therefore be in the ratio of 1 : 2·4 : 5·76, which gives a factor of 2·4 for each rise of 10° C. (See Fig. 5, Division.)

Now we are told by the investigators that at 35° C. Chilomonas is on the point of succumbing to the heat, so that the division rate increases right up to the death point, with no sign of an optimum effect. Below 14° C. no observations are recorded.

Here, then, we have throughout the whole range exactly the same primary temperature relation exhibited by the protoplasmic procedure that we should expect for a chemical reaction in a test-tube.

This division phase is only a part of the life-cycle of the flagellate, and between division it swims about anabolising the food material of the medium and growing to its full size ready for the next division. One wishes at once to know what is the effect of the temperature upon the length of the life-cycle. Is the whole rate of metabolism quickened in the same way as the particular section concerned with actual division? Of course a motile flagellate cannot be followed and its life-cycle directly timed, but the information was obtained by estimating carefully what percentage of individuals was in a state of actual division at each temperature. It was found that always 4 per cent. were dividing, whatever the temperature. This proves that the whole life-cycle is shortened in exactly the same proportion as the process of division at each temperature, and that it is just twenty-five times as long. Therefore the life-cycle is 125 mins. at 35° C. and 725 mins. at 15° C., so that here, again, we have the physical-chemical relation with a factor of 2·4 for each rise of 10° C.

In this paper of Maltaux and Massart these relations are not considered as the manifestation of physical-chemical principles, but are regarded as reactions to stimuli; and the paper contains a number of experiments upon the effect of sudden changes of temperature upon the occurrence of division. So far as one can make out from inspection of the scattered literature, it does seem established that sudden changes of temperature act as stimuli in the strict sense of the word. In many investigations one finds it stated that a quick change of temperature produced a certain reaction which a slow change of temperature failed to evoke. Usually all the phenomena are treated in terms of stimulation, and the absence of reaction with slow change of temperature is regarded as secondary. Were it not for the specific stimulatory effects of quick change, which are not difficult to comprehend as a phenomenon *sui generis*, I hardly think so general a tacit acquiescence would have been extended by botanists to the view that all enduring changes of velocity of metabolism brought about by lasting changes of temperature are stimulatory in nature.

No determination of the rate of development of bacteria through a very wide range of temperature seems to have been made. There are various incidental experiments which indicate values about 2 for the coefficient of increase of metabolism for a rise of 10° C.

I am not acquainted with any data for the growth-rate of whole flowering plants at different temperatures. Of course the case of growth most usually measured in the laboratory, namely, where one part of a plant extends at the expense of the reserves stored in another part and there is a decrease, not an increase, of total dry weight, is not the type of growth we have to deal with. Even for simple elongation of a shoot at different temperatures we have but few data. Those of Koppen (1870) generally quoted are wildly irregular, and in many cases it is clear that the growth-extension of complex structures is a process which proceeds by spasms rather than smoothly.

The rate of movement of circulating protoplasm increases rapidly with temperature, but Velten's numbers do not give an obvious logarithmic curve. If we confine our attention to the values for 29° C. and 9° C. we do find, however, that the velocity increases about two-fold for each rise of 10° C., being 10 mm. at 9° C. and 40 mm. at 29° C.

Taken altogether these various data clearly support the hypothesis that temperature accelerates vital processes in the same way as it does non-vital chemical reactions, that is, logarithmically by an approximately constant factor for each rise of 10° C.; and, further, it accelerates them to the same extent; that is, that the factor in question has values clustering about 2–3.[1]

To make these similarities more significant I ought to point out that no other properties of matter are accelerated to anything like this extent by rise of temperature. Most reactions increase in velocity by no less than 10 per cent. per degree rise of temperature; a most marked effect, and yet there is no generally accepted explanation of this almost universal phenomenon. By the kinetic theory of gases each rise of a degree in temperature increases the movements of the gas-molecules, so that the number of collisions between them is greater, but only about one-sixth per cent. greater. With rise in temperature, too, the viscosity of a solution diminishes, so that there is less resistance to internal changes; but this only to the extent of 2 per cent. per degree. The degree of ionisation also increases, but only extremely little, so that no change of known physical properties will explain the phenomenon. Various hypotheses which need not detain us have been put forward.

Unexplained though it may be, yet the quantitative treatment of the subject is clear enough and, I think, as cogent in the living organism as in the test-tube. If so, we may consider ourselves now justified in separating off from the realm of stimulation yet a third class of causal connection, namely, that between temperature and the general intensity of vital processes.

Conclusion.

In this attempt to assert the inevitableness of the action of physical-chemical principles in the cell, I have not ventured upon even the rudiments of mathematical form, which would be required for a more precise inquiry. Biochemistry is indeed becoming added to the ever-increasing number of branches of knowledge of which Lord Bacon wrote: "Many parts of nature can neither be invented with sufficient subtilty, nor demonstrated with sufficient perspicuity, nor accommodated unto use with sufficient dexterity, without the aid and intervening of the mathematics."

In this sketch which I have had the honour of outlining before you I have critically considered but few points. I have rather endeavoured to distribute imperfect data in the perspective in which they appear from the point of view of one who seeks to simplify phenomena by extending the principles of chemical mechanics so far as possible into the domain of vital metabolism. Much critical quantitative work has yet to be done before the whole becomes an intelligible picture.

To me it seems impossible to avoid regarding the funda-

[1] Maltaux and Massart, *Recueil de l'Institut botanique Bruxelles*, tome vi., 1906.

[1] It has been proposed to use the size of the temperature coefficient to settle whether a process like the conduction of an impulse along a nerve is a chemical or a physical process. See Keith Lucas, *Journal of Physiology*, vol. xxxvii., June, 1908, p. 112.

mental processes of anabolism, katabolism, and growth as slow chemical reactions catalytically accelerated by protoplasm and inevitably accelerated by temperature. This soon follows if we once admit that the atoms and molecules concerned possess the same essential properties during their brief sojourn in the living nexus as they do before and after.

Perhaps the more real question is rather as to the importance and significance of this point of view. Protoplasmic activity might be something so much *per se*, and the other factors of the nature of stimuli might be superposed so thickly upon that substratum which should be dominated by simple principles of chemical mechanics that for practical purposes the operations of the latter would be so overlaid and masked as to be negligible. A survey of this field, however, seems to show that this is not so, and that the broad action of the law of mass and the acceleration of reaction-velocity by temperature are obviously responsible for wide ranges of phenomena.

Now the conception at the bottom of these principles is that of reaction-velocity, and the conclusion of the whole matter is that the physiologist must frankly take over from physical chemistry this fundamental conception.[1] Under definite conditions of supply of material and temperature there is a definite reaction-velocity for a given protoplasm, and the main factors that alter the rate of metabolism, viz., heat, nutrition, and traces of impurities are exactly the factors which affect the velocity of reactions *in vitro*.

Working on this basis we no longer need the vague unquantitative terminology of stimulation for the most fundamental of the observed "responses" to external conditions. Three sets of phenomena we have observed which, though usually treated in the category of stimulation, draw a clearer interpretation from the conception of reaction-velocity. These were: (1) the relation of development to the absence or deficit of single essential food constituents; (2) the occasional striking effect of minute traces of added foreign substances upon the whole rate of growth and metabolism; and (3) the general doubling of the activity of vital processes by a rise of 10° C.

The next higher stratum of principles should be the complications introduced by limiting factors which interrupt the extent of the manifestations of these principles and by various correlations, as, for example, that by which the reaction-velocity of one catabolic process might withdraw the supply of material needed for full activity of another different process. To this sort of relation may be attributed that phenomenon so characteristic of the more complex vital processes and quite unknown in the inorganic world, namely, the optimum.

Finally, superposed upon all this comes the first category of phenomena that we are content still to regard as stimulatory. From the point of view of metabolism and reaction-velocity many of these appear very trivial, though their biological importance may be immense. Think how little the tropistic curvatures of stems and roots affect our quantitative survey; yet a little re-arrangement of the distribution of growth on the two sides of an organ may make the difference between success and failure, between life and death.

From our present point of view vision does not extend to the misty conceptions of stimulation upon our horizon. We may therefore postpone speculation upon the mechanical principles governing them and await the time when by scientific operations we shall have reduced to law and order the intervening region, which we may entitle the chemical substratum of life. This done we may venture to pitch our laboratory a march nearer to the phenomena of protoplasmic irritability and make direct attack upon this dominating conception, the first formidable bulwark of vital territory.

[1] No general treatment of the physiology of plants has yet been attempted in terms of reaction-velocity. Czapek, however, in the introduction to his stupendous "Biochemie der Pflanzen," vol. i., 1905, does direct attention to the conception of "reaction-velocity" and refer to the standard literature on this subject and on catalysis, though direct application is not made to the plant. Cohen ("Physical Chemistry for Physicians and Biologists," English edition, 1903) considers in detail some biological applications of the acceleration of reactions by temperature.

THE SCIENTIFIC STUDY OF PLAGUE.

THE fourth extra number of the *Journal of Hygiene*, containing the work of the Plague Commission, has appeared lately.[1] Chapter xxvi.—the first of this number—is a translation of a St. Petersburg thesis (1904) by Dr. Verjbitski, which has not been published before. The Russian worker arrived independently at conclusions, with regard to the transmission of plague by blood-sucking parasites, which tally well with those of the Indian workers. The common rat flea of Cronstadt, however, is *Typhlopsylla musculi*, and appears not to attack man. Experiments with bugs gave results similar to those with fleas.

Chapter xxvii. is the substance of a report submitted to the Indian Government by Lieut.-Colonel Bannerman and R. J. Kápadiâ in 1904. It shows that domestic animals (pigs, calves, fowls, turkeys, geese, and ducks) are not susceptible to a general infection with *B. pestis*, though local abscesses were sometimes produced by inoculation.

Chapter xxviii. gives some experiments on septicæmia in human plague, with others on the infectivity of excreta, supplementary to work detailed in an earlier number of these reports.

The most interesting portion of this number is contained in chapters xxix. to xxxi., dealing with the bionomics of fleas, the mechanism by means of which the flea clears itself of plague bacilli, and the seasonal prevalence of plague.

Simple and ingenious are the methods of carrying out flea experiments here described. The results, too, are interesting. It is found that fleas do not remain constantly on their host, but hop off on to the floor or into the nest of the rat. Here the eggs are laid, and, when the fleas seek food again, it is likely that a different rat will supply the meal. In this way the same flea may bite several rats in the course of the day, and forms a very efficient means of spreading infection. Not only this, but the experiments prove that, where many rat fleas are present (*P. cheopis*), some of them will readily attack man, though rats are at hand.

The developmental stages of the flea are passed through in three weeks in favourable circumstances. Temperature above 80° F. has a retarding influence, which becomes very marked between 85° F. and 90° F. At these temperatures fewer eggs are laid, and their development is slower than at lower temperatures such as 70° F.

Passing to the consideration of the seasonal prevalence of plague, we find that though climatic conditions go for something, yet they leave much to be explained. Charts are given which show the recurrent plague epidemics in six widely different localities, along with temperature and humidity curves. Humidity appears to have little importance. With regard to temperature, the following conclusions are drawn:—

(1) A plague epidemic is checked when the mean daily temperature passes above 80° F., and especially when it reaches 85° F. or 90° F.

(2) A mean temperature above 80° F. affects the conditions to which the plague bacillus is subjected in the stomach of the flea. At high temperatures, about 90° F., the plague bacilli disappear from the stomach of the flea much more quickly than at lower temperatures, namely, between 70° F. and 80° F.

(3) A plague epidemic may, however, come to an end when the temperature is most suitable. Other factors must therefore be present in these cases.

Reading further, we find these "other factors," tending to check an epidemic, are a diminution in the number of rat fleas, a diminution in the total number of rats, and an increase in the proportion of immune to susceptible rats. Perhaps the first of these factors is the least important; chart vii. shows that in Bombay, in 1907, the epizootics (in both *M. decumanus* and *M. rattus*), and even the epidemic, began to decline a month before the flea infestation showed any decrease.

The last two factors are consequences of the outbreak

[1] Reports on Plague Investigations in India. *Journal of Hygiene*, vol. viii., No. 2. Pp. 148; Charts vii. Cambridge: University Press, May, 1908.) Price 6*s*.

of plague itself, so that an epizootic has only to reach a certain intensity in order to bring about its own decline. It is difficult to estimate the decrease in rat population caused by an epizootic, but the systematic trapping carried out in the Punjab villages of Kasel and Dhand gave results which seem to indicate that this decrease may be considerable. An increase in the proportion of immune rats has a double action. First, it obviously connotes a decrease in the available number of susceptible rats; secondly, these immune rats actually protect their susceptible companions. For consider an infected flea which has just left a rat dead of plague. Such a flea is dangerous only so long as he carries living plague bacilli in his stomach. But the Commission has shown that the destruction of plague bacilli within the flea's stomach is largely effected by the activity of the rat's leucocytes, taken in at each fresh meal. But efficient phagocytosis depends on efficient opsonisation, so that if the infective flea chances to take a meal from an immune rat, the opsonic power of the blood of which is generally above normal, the phagocytic process will be hastened, and the flea will be less dangerous to his next susceptible host. This deduction was tested in an experiment in which fleas were first infected and then fed for twenty-four hours, one series on immune rats and another series on susceptible rats. The two lots of fleas were then allowed to feed on normal guinea-pigs, of which the immune-fed fleas infected only four out of eleven, while the others infected eight out of eleven. But we are led to expect further experiments on this interesting topic.

The number concludes with some brief remarks of the differential diagnosis of *B. pestis*. L. NOON.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

LONDON.—A course of eight lectures on "Algal Flagellates and the Lines of Algal Descent" will be begun by Dr. F. E. Fritsch at University College on October 26. During the second term a course of eight lectures on "Physical Chemistry and its Bearing on Biology" will be delivered by Dr. J. C. Philip, and in the third term a course of eight lectures on "Recent Advances in the Study of Heredity" will be delivered by Mr. A. D. Darbishire. A course of four lectures on "The Geological Structure of the Area of the Vosges" will be delivered at Bedford College by Miss C. A. Raisin, beginning on November 16, and in the second and third terms courses will be given at University College by Dr. A. Smith Woodward, F.R.S., and Prof. E. J. Garwood on, respectively, "The Use of Fossil Vertebrata in Stratigraphical Geology" and "The Geology and Physiography of Arctic Europe." Beginning on January 22, Dr. W. N. Shaw, F.R.S., will give a course of lectures on "The Climates of the British Possessions." On October 20 Dr. F. S. Locke will deliver, in the physiological laboratory of the University, a course of lectures on "Some Problems of General Physiology, more Particularly those Associated with Muscle," and in the second and third terms courses will be given by Prof. A. D. Waller, F.R.S., and Dr. A. Harden on, respectively, "General Physiology of Nerve" and "Chemical Biology of the Yeast Cell." On February 2 Dr. L. C. Parkes will begin, at University College, a course of four lectures on "The Medical Aspects of Recent Advances in Hygiene as Connected with Sewering." Prof. E. A. Minchin will in the third term give, at the Lister Institute, a course of lectures on protozoology, and in the first term Mr. R. Lydekker, F.R.S., will deliver three lectures on "The Living and Extinct Faunas of Africa and South America." Details as to the time and place of the delivery of the last-named course will be announced later. All the lectures referred to will be addressed to advanced students, and no charge will be made for admission.

University College.—The delivery of the following introductory public science lectures has been arranged for:—October 6, "Davy and Graham," by Sir William Ramsay, K.C.B., F.R.S.; October 8, "Personal Religion in Egypt," by Prof. W. M. Flinders Petrie, F.R.S.; October 8, "Gleanings in the Babylonian East," by Dr. T. G. Pinches; October 9, "Recent Developments in Philosophic Thought," by Prof. G. Dawes Hicks; October 9, "School Hygiene," by Prof. H. R. Kenwood; October 14, "The Scientific Principles of Radio-telegraphy," by Prof. J. A. Fleming, F.R.S.

Bedford College.—A course of lectures and demonstrations for teachers, and persons qualifying to be teachers, will be given on "The Hygiene of Common Life," by Dr. J. S. Edkins. The opening lecture (the admission to which will be free) will be delivered on October 13.

OXFORD.—In a Convocation to be held on October 8 it will be proposed to confer the degree of Doctor of Science, *honoris causa*, upon Dr. Svante August Arrhenius and Dr. A. G. Vernon Harcourt, F.R.S.

MR. MATTHEW MONIE has been appointed lecturer on geology at the Glasgow Agricultural College.

DR. H. BYRON HEYWOOD has been appointed assistant lecturer in the mathematical department of the East London College.

THE general prospectus of the day and evening classes to be held at the Battersea Polytechnic during the session which has just begun provides careful guidance for intending students. New classes have been arranged for advanced students in hygiene, geology, and bacteriology, and new trade classes in wheelwrights' work and gas-fitting have been inaugurated. It is satisfactory also to find that coordinated courses have been drawn up in engineering, chemistry, physics, mathematics, and other main branches of work. A building grant from the London County Council has made it possible to set about extending the laboratories for mechanical and electrical engineering, and to undertake extensive alterations and additions in the chemistry department.

THE Board of Education has issued the following list of candidates successful in the competition for the Whitworth scholarships and exhibitions, 1908:—*I. Scholarships* (125*l*. a year each, tenable for three years): W. H. Mead, Southsea; W. White, Portsmouth; W. H. Stock, Swindon; E. Bate, London. *II. Exhibitions* (50*l*., tenable for one year): A. H. Gabb, Swindon; A. McKenzie, Devonport; R. Bassett, Devonport; S. L. Dawe, Devonport; A. J. Triggs, Devonport; A. C. Lowe, Harrogate; J. R. Pike, Portsmouth; H. R. Allison, Gillingham; A. E. Beal, Sheerness; C. R. Kemp, London; H. L. Guy, Penarth, Glamorgan; H. G. Stephens, Leicester; F. E. Rowett, Chatham; C. E. Haddy, Torpoint, R.S.O., Cornwall; W. E. Tong, Gosport; G. W. Bird, Plymouth; C. W. Limbourne, Plumstead; W. G. Pitt, Plumstead; E. J. Cox, Gosport; G. H. Reid, Stonehouse, Devon; D. Watson, Swindon; J. E. Burkhardt, Newcastle-on-Tyne; P. R. Higson, London; A. J. Sear, Portsmouth; E. O. Hale, Stantonbury, Bucks; F. C. Ham, Plumstead; A. R. C. Winn, Hornchurch, Essex; J. Scobie, London; F. Bray, Devonport; C. P. T. Lipscomb, Plumstead.

THE second section of the new buildings of the Glasgow and West of Scotland Technical College was used for the first time on Tuesday, September 22, on the occasion of admitting to the associateship of the college the students who had gained the college diploma at the close of last session. Dr. G. T. Beilby, F.R.S., chairman of the governors, presided at the meeting held in the examination hall, and in the course of an address described the relations of the college to the reform in methods of coal consumption. The college was the first institution in the United Kingdom to establish special laboratories for the teaching and study of everything connected with fuel and combustion. The most recent knowledge on these subjects shows that in the great majority of cases smoke and dust are quite unnecessary concomitants of industry. The inquiries of the recent Royal Commission on Coal Supplies have made it abundantly clear that the present inefficient consumption of coal in Great Britain not only leads to the waste of from forty to sixty million tons per annum, but that this inefficiency is also responsible for the greater proportion of the smoke and dirt from which the nation suffers. It has been estimated that on the total British consumption 30 per cent. might be saved if the best-known means of consumption for each purpose were employed. The college has as its most obvious duty the education of specialists for the particular branches of industry which prevail in the district; but at the foundation of these industries there is one fundamental factor which affects each and all of them—they all depend ultimately on the combustion of coal for the production of light, heat, and power. Since the special laboratories were opened in connection with the Young chair of chemistry, Prof. Thomas Gray has carried out systematic instruction on the methods used for the scientific control of the combustion of coal and the economical utilisation of heat in factories. Not only are the regular students of chemistry, metallurgy, and engineering instructed in these methods as a necessary part of their curriculum, but similar instruction has been sought for and obtained by the members of the staff of a number of leading industrial concerns in the district.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 4.—"The Viscosity of Ice." By R. M. **Deeley**. Communicated by Henry Woodward, F.R.S.

The rate of motion of a number of glaciers has been determined by Tyndall. From his figures and estimates of their thickness and slope it is possible to calculate with some degree of accuracy the viscosity of several glaciers. Stated in dynes per square centimetre $\times 10^{12}$, the results are roughly as follow:—the Mer de Glace, 27; Morteratsch, 143; Lower Grindelwald, 3; and Great Aletsch, 126. It seems probable that these differences arise mainly from differences in the actual viscosity of the glacier ice due to its varying granular structure. It is shown that the viscous flow of a glacier exercises a drag on the floor upon which it rests amounting in the case of the Great Aletsch to about $2\frac{1}{2}$ tons per square foot, and that owing to the ability of the ice to transmit thrust, this force may be greatly exceeded at points where much resistance to motion is caused by inequalities in the floor upon which the ice rests.

McConnel made a number of experiments on the shearing motion which can be produced in ice crystals in directions at right angles to the optic axis. It is shown that this shear obeys the laws of viscous motion, and that the viscosity may be expressed by the following equation:—

$$\log_{10} \mu = 0{\cdot}301 + {\cdot}153t - 0{\cdot}00231t^2,$$

where μ is the viscosity in dynes per square centimetre $\times 10^{10}$, and t is the temperature below zero C. (considered positive). McConnel showed that when the load was taken off a bar of ice which had been yielding viscously, there was a slow partial recovery of the original form. Experiments with highly brittle pitch also showed that when the load was taken off a weighted bar there was an immediate elastic recovery, and also an additional slow recovery. This feature has also been described by Trouton.

The viscosity of ice at right angles to the optic axis is about 6250 times less than that of a glacier; the optic axis of glacier grains being at all angles, they lock each other. The motion of a glacier is due in a large measure to changes in the sizes and shapes of the glacier grains due to their growth and decay.

PARIS.

Academy of Sciences, September 21.—M. Bouchard in the chair.—The determination of the triple orthogonal systems which comprise a family of cyclids, and, more generally, a family of surfaces with lines of curvature plane in the two systems: Gaston **Darboux**.—The use of tartar emetic in the treatment of trypanosomiasis: A. **Laveran**. Guinea-pigs infected with *T. evansi*, *T. gambiense*, and the trypanosome of Togo, were treated with hypodermic injections of a solution of sodium antimonyl tartrate in 2 per cent. solution. The results were generally favourable, especially when the antimony salt was used in conjunction with atoxyl.—The impossibility of demonstrating the existence of an appreciable dispersion of light in interstellar space by the Nordmann-Tikhoff method: Pierre **Lebedew**. The ratio of the dispersion values found by Nordmann and by Tikhoff is 30:1, and the author concludes that a method of measurement which gives such different values for the same physical constant must be false in principle.—The spectra of the large planets photographed in 1907 at the Flagstaff Observatory: Percival **Lowell**. The principal lines and bands observed for Jupiter, Saturn, Uranus, and Neptune are tabulated. The presence of water vapour in the atmospheres of Jupiter and Saturn is proved, and also free hydrogen in Uranus and Neptune.—Reciprocal differences: E. **Nörlund**.—A lecture experiment concerning the rotation of the earth: Louis **Maillard**.—A particular form to which the differential equations of the trajectories of electrified corpuscles in a magnetic field can be reduced: C. **Störmer**.—The origin of the Brownian motion: Jean **Perrin**. Further experiments are described confirming the hypothesis that molecular agitation is the cause of the movement.—The thermoelectricity of cobalt: H. **Pécheux**. A study of a copper-cobalt thermocouple shows that molecular transformations occur in cobalt at 280° C. and 550° C.—Oleuropine, a new principle of glucoside nature extracted from the olive (*Olea europea*): Em. **Bourquelot** and J. **Vintilesco**.—The function of the nervous system in the changes of colour in the frog: E. **Sollaud**.—The supposed action of tobacco in producing abortion: R. **Robinson**. Evidence is adduced negativing this supposed action.—The cause of magnetic storms: K. **Birkeland**.

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