THURSDAY, OCTOBER 6, 1910.

THE MAMMALS OF MANITOBA.

Life-histories of Northern Animals: an Account of the Mammals of Manitoba. By Ernest Thompson Seton. Vol. i., Grass-Eaters. Pp. xxx+673. Vol. ii., Flesh-Eaters. Pp. xii+674–1267. (London: Constable and Co., Ltd., 1910.) Price £3 13*s*. 6*d*. net.

SO far as this country is concerned, it is a great pity that Mr. Seton did not include in his admirable life-histories the whole mammalian fauna of North America, as the restriction of the species to those inhabiting a particular area can scarcely fail to be faulty in the eyes of English readers, who will miss such well-known animals as the big-horn sheep, white goat, Columbian black-tailed deer, and the brown bears and caribou of Alaska. It is likewise a matter for regret that the whole of the table of contents is included in the first volume, instead of the portion relating to the Carnivora being reserved for the second. Except this very small modicum of fault-finding, I have nothing but commendation to bestow on these handsome volumes, which, it may be presumed, are an English edition of the work issued last year in America under the same title.

The mammals of Manitoba are fifty-nine in number, and of these the author describes the life-history in his own inimitable manner, and with a wealth of detail that leaves little, if anything, for future field naturalists to record. The great feature of the work is, of course, that it is the result of personal observation and first-hand knowledge, acquired during wanderings extending over a long series of years, and embracing a very large portion of the North American continent. The map of his travels shows, in fact, that Mr. Seton has explored the whole of the United States, from ocean to ocean, so that the red lines which mark his route form a perfect network. Southward he has touched the Mexican border, while northward he has visited Labrador on the east, and on the west has made a single traverse to the heart of the Mackenzie district. And it is these extensive journeys, with the knowledge thereby acquired, that intensifies our regret that he did not see fit to make his work cover the whole North American mammal fauna. In spite of the almost omniscient character of his personal knowledge of the habits and distribution of the animals of which he writes, the author has been at the pains to quote the names of observers by whom special traits were first recorded.

In addition to his well-known power of conveying important scientific information in most attractive and popular language, Mr. Seton enjoys the great advantage of being a skilled artist, so that he is able to present to his readers portraits of the animals in the particular pose which he considers most characteristic and interesting. The amount of labour he has expended on artistic work may be inferred from the fact that the whole of the 560 illustrations were drawn with his own pencil. Such sketches are, in my opinion, infinitely superior to photographs, which too frequently do not bring out the characteristic features which the describer desires to emphasise. That many of these illustrations, if I mistake not, have appeared in other publications, testifies to public appreciation of their artistic merit.

In the space at my disposal it is quite impossible to attempt anything in the way of a critical review of these two bulky quarto volumes, and I shall therefore content myself with noticing some of the author's observations relating to the larger species which appear of special interest.

By far the most characteristic American type of big game is undoubtedly the prongbuck, a species the distributional area of which has been reduced by about one-half, and the numbers of which were estimated by the author in 1900 not to exceed, at most, 100,000, of which half must be in Mexico. And yet from the accounts of travellers of no earlier date than 1868 it seems probable that these graceful antelopes, as they are locally called, actually outnumbered the bison in the days of its prime. Taking the number observed in one particular district as a basis, the author calculates that, on a low estimate, there must have been over 40 millions on the great plains. Since 1900 these antelopes, in spite of reported local increases, have probably suffered a further serious decrease, the number in Wyoming in 1905 being estimated at not more than one-fourth of what it was five years previously.

As regards the extermination of the bison, Mr. Seton, although as a naturalist regretting the event, takes what may be called the practical view of the subject, and declares it to have been absolutely inevitable. The plains were required by the advance of civilisation, and the supporting of vast herds of stupid bison, ready to stampede in an overwhelming mass on the slightest alarm, was not the best use to which they could be put. On the testimony of two independent observers, he asserts that blizzards, especially those of 1871–2 and 1880–1, had a share in the extermination of the bison, one of the two witnesses stating that the Dakota blizzard was more destructive to the herds than the Indians. Since, however, blizzards are only occasional events, Mr. Seton doubts if their destructiveness was equal to that of agencies working with greater regularity.

In concluding this brief notice of a first-class work, it may be noted that the author is a firm believer in the evolution of the mind of man from that of animals below him in the zoological scale.

R. L.

THE CARE OF TREES.

The Care of Trees in Lawn, Street, and Park. By Bernard E. Fernow. Pp. x+392. (New York: Henry Holt and Co., 1910.)

ALTHOUGH there is scarcely a garden or park of any pretensions in this country which does not contain within its boundaries one or more trees particularly valued for their interest, beauty, or associations, how rarely do their owners ever take any steps to keep them in health, and thus prolong their term of years. The care of trees, indeed, more especially those parts in it which may be described as surgical and antiseptic, is an art strangely recent in origin. Some of the practices of still living "tree-doctors" (one may instance the leaving of a stump "to draw the sap" when a branch or limb is sawn off) betray a simple faith curiously reminiscent of the methods of sixteenth-century practitioners on the human frame.

But owners of trees are, on the whole, content to let them run their ordinary course, although nothing is more certain than that by judicious treatment the span of years of many trees may be extended by tens, perhaps hundreds, of years. The value, of course, of many such trees is a purely sentimental one. Yet among individual living things trees seem to link the centuries together more effectually than anything else. What heart is not moved by the sight, and still more by the possession, of a tree under which it is known some famous man of old, or even one's own forbears, sat and mused.

The author of this work is well known to those in his own walk in life, as one of the most eminent and trustworthy authorities on the subject in the United States and Canada. And we may say at once that this book fully bears out his reputation. It is a pleasant change, after wading through much of the ignorant twaddle that is nowadays so plentifully offered to the tree-loving public, to come across a book the author of which has (what, indeed, the unsophisticated might regard as essential) an adequate knowledge of his subject.

Mr. Fernow's object is, briefly, to interest his readers in their trees, to give them some idea of how they grow and do their work, and to give directions for their cultivation and preservation. Much of what he writes is more particularly applicable to the north-eastern United States and the adjacent parts of Canada, where the climatic conditions, especially in relation to tree and shrub growth, are sufficiently dissimilar to our own as to render some modification of his recommendations necessary before they can be adopted here. But his treatise, on the whole, is very well worth study by those interested in the trees and shrubs of English parks and gardens.

We regret that the author thought it necessary to give a sort of recommendation—although certainly a half-hearted one—to the book on pruning written by A. des Cars, nearly fifty years ago. Des Cars' system of pruning is hopelessly discredited by now. As applied to trees grown solely for timber it was out of the question for reasons of cost, and as applied to the purely ornamental trees of gardens, the rigid formality he advocated was absolutelv at variance with the tastes of ninety-nine people out of a hundred.

The latter part of this book is taken up with the description of trees and with the consideration of their respective value and treatment. It is somewhat cursorily done, and is the least satisfactory part, in that it is far from free of errors. The horse chestnut, for instance, is not Chinese (p. 250), nor is *Prunus pissardii* Japanese (p. 304). The author has sadly confused the *Pyrus sinensis* of Lindley, a true pear, with the common *Cydonia japonica*, which is, of course, a quince (p. 297). It is a remarkable lapse to recommend rhododendrons for calcareous soils, which, with the exception of one or two species, they abhor (p. 372). The list of "shrubs fit for rock gardens" is strangely inadequate (p. 373). About a dozen plants are mentioned, half of which are absolutely unfitted for any ordinary rock garden, whilst the scores of dainty shrubs, mainly alpine, the neatness of habit and slow growth of which render them peculiarly fitted for such a position, are quite ignored. Errors in spelling, too, are numerous.

In view of the thoughtless and ignorant outcry which is usually set up in the daily Press whenever the removal or thinning, or even lopping, of trees in public places is done, it was a happy thought to quote a letter written by J. R. Lowell to the president of Harvard University in 1863. The following words will bear repeating:—

"Something ought to be done about the trees in the college yard. They remind me always of a young author's first volume of poems; there are too many of 'em and too many of one kind. If they were not planted in such formal rows, they would typify very well John Bull's notion of 'our democracy,' where every tree is its neighbour's enemy, and all turn out scrubs in the end, because none can develop fairly. . . . We want to learn that one fine tree is worth more than any mob of second-rate ones. Do pray take this matter into your own hands—for you know how to love a tree—and give us a modern instance of a wise saw."

THE MAKING OF BEET-SUGAR.

Beet Sugar Making and its Chemical Control. By Y. Nikaido. Pp. xii+354. (Easton, Pa.: The Chemical Publishing Co.; London: Williams and Norgate, Ltd., 1909.) Price 12*s*. 6*d*. net.

THE aim of this work, the author remarks, is to aid those who are starting on a career of beet-sugar manufacture, but who lack systematic training in the technique thereof.

In principle, the production of sugar from beetroots is a simple matter. The sugar and other soluble bodies are extracted from the sliced roots by diffusion in water; the juice thus obtained is purified from acids and other objectionable matter by "defecation" with lime, and after the excess of lime has been removed by treatment with carbonic acid, the liquor is concentrated by evaporation until the sugar crystallises out. Whilst, however, there is nothing complicated about the principle, successful and profitable production depends upon close attention to a number of points in respect of which the chemist's help is needed.

These points Mr. Nikaido describes and explains at length. The essential part of the book is contained in one chapter—the eighth. This is devoted to expounding the "practical operation," *i.e.* management, "of a beet-sugar house." It sets forth the various steps involved, from harvesting the beets to packing the sugar, and gives details of the chemical examinations necessary for the proper control of the processes. The descriptions bear the stamp of practicality, and the value of the book in actual work is enhanced by a series of useful tables. In the last chapter a number of "special analyses" are considered; they relate to methods of testing beetroot seed, to the examination of various materials used in sugar manufacture, and to particular cases of sugar analysis—*e.g.* the determination of raffinose.

The chief criticism suggested, on looking through the volume, is that a disproportionate amount of space is allotted to preliminary and incidental matters. Apart from an appendix of tabular material and the index, the book contains 294 pages. Of these only 120 are devoted to the real object of the work—beet-sugar making and its chemical control. The rest is taken up with accessory description, much of which is merely general elementary chemistry. This would be much better learned from an ordinary text-book. A "theory of the origin of limestone" (p. 40); a description of the metallurgy of iron (p 48), or the chemistry of lead (p. 54); a dissertation on the molecular structure of the hydrocarbons (pp. 61–2): all these are rather out of place in a book devoted to sugar; or, at least, such things should not get the lion's share of the space. Whilst it is legitimate enough to discuss the general chemistry of the sugars, and even perhaps the theory of the polariscope, the rest of the matter in question gives one the impression of being largely "padding."

This apart, the book deals lucidly with the everyday problems of beet-sugar production, and should prove very useful to those for whom it is written.

C. S.

METHODS OF ROCK-ANALYSIS.

Analyse der Silikat- und Karbonatgesteine. By W. F. Hillebrand; translated by E. Wilke-Dörfurt. Zweite Auflage. Pp. xvi+258. (Leipzig: W. Engelmann, 1910.) Price 6 marks.

The Analysis of Silicate and Carbonate Rocks. A revision of Bulletin 305. By W. F. Hillebrand. Bull. 422, U.S. Geol. Survey. Pp. 239. (Washington: 1910.)

AS a consequence of the modern developments of petrology, accurate chemical analyses of rocks, and of the component minerals of rocks, have become more than ever an urgent desideratum; and it is a fortunate coincidence that there has been at the same time a decided revival of mineral chemistry, so long overshadowed by that of the carbon compounds. Not only is the discovery of new rock-types continually providing fresh material, but also it has to be recognised that the older rock-analyses, admirable in their own time, no longer suffice for the requirements of the present day. The best modern analyses have the advantage of greatly improved methods of separation; and, further, they aim at a much greater thoroughness of treatment, often including estimations of twenty or more constituents, instead of the eight or nine which satisfied Bunsen and Delesse.

In this work a leading part has been taken by the American chemists, and particularly those of the United States Geological Survey. In the last thirteen years four bulletins have been issued giving complete analyses of many American rocks, conducted in the laboratory at Washington. In the first of these, some forty pages were devoted to a discussion of methods of analysis by Dr. Hillebrand. This part, considerably enlarged, was issued as a separate bulletin in 1900, a new edition appearing in 1907; and it is this last which has now been translated into German, with some revision and additions by the author. It has been closely followed by a revised edition of the original, so that the latest advances in this branch of practical chemistry are now accessible equally to German and English readers.

The methods which are here fully and clearly set forth are, in the main, those which the experience of the author and his colleagues has led him to prefer; but alternative methods are often given, especially when the first one requires complicated and costly apparatus. Many of the analytical methods described are, of course, familiar to the working chemist, but the author's matured judgment on their relative merits cannot fail to be of use; and, even apart from this critical discussion, it is a great convenience to have the scattered literature of the subject brought together and presented in systematic shape.

As an illustration of the author's treatment, we may select the estimation of ferrous iron, always a crux in rock-analyses (pp 154–71). First comes a section, added in the present edition, pointing out the important error introduced by oxidation of the material during the process of grinding, and the devices by which this error may at least be minimised. This is followed by a comparison between Mitscherlich's sealed tube method of estimation and the hydrofluoric acid methods; the former is in general to be avoided, on account of the reducing action of sulphides present in the rock. Since, however, Mitscherlich's method is probably the best in those cases where it can safely be used, it is described, with important modifications suggested by experience. The general principle of the hydrofluoric acid method is then set forth, with a discussion of the chief sources of error and of the influence of sulphides, vanadium, and carbonaceous matter on the determination of the ferrous iron. Finally, the author describes the method itself in its various modifications, as advocated by Cooke, Pratt, and Treadwell, respectively.

Twenty years ago the petrologist who did not perform his own chemical analyses felt that he was delegating part of his legitimate task to another. A more exacting standard has made a division of labour, as regards complete rock-analyses, almost inevitable, and Dr. Hillebrand's manual is accordingly addressed to the chemist rather than the petrologist. On the other hand, few of us are in the advantageous position of the United States Geological Survey, which can command the services of six or eight skilled specialists; and it is also to be remembered that one complete analysis demands as much time and labour as, perhaps, three of a less ambitious kind. Some petrologists will be of opinion that there is still a place for rock-analyses, conducted according to the best methods, but including estimations of only a moderate number of constituents.

If the petrologist cannot make his own analyses, he should, none the less, be competent to interpret them with judgment, and we should have been grateful to the author for some guidance in this matter. Everybody knows, for instance, that the silica is likely to be more correctly determined than the alumina, and so in a general way for other constituents; but a summary discussion by a skilled mineral analyst of the probable errors attaching to the several chief constituents of igneous rocks would be very welcome.

A. H.

NEW GEOGRAPHICAL BOOKS.

(1) *Distant Lands. An Elementary Study in Geography.* By H. J. Mackinder. Pp. xvi+296. (London: Geo. Philip and Son, Ltd., n.d.) Price 2*s.*

(2) *A First Book of Physical Geography.* By W. M. Carey. The First Books of Science Series. Pp. viii+150. (London: Macmillan and Co., Ltd., 1910.) Price 1*s.* 6*d.*

(3) *A Physiographical Introduction to Geography.* By Prof. A. J. Herbertson. The Oxford Geographies. Pp. 120. (Oxford: The Clarendon Press, 1910.) Price 1*s.* 6*d.*

(4) *Geology.* By Prof. J. W. Gregory. Dent's Scientific Primers. Pp. 140. (London: J. M. Dent and Sons, Ltd., n.d.) Price 1*s.* net.

(5) *An Economic Atlas.* By J. G. Bartholomew, with an introduction by Prof. L. W. Lyde. Pp. xii+64. (Oxford: The Clarendon Press, 1910.) Price 3*s.* 6*d.* net.

(6) *Devonshire.* By F. A. Knight and Louie M. (Knight) Dutton. Cambridge County Geographies. Pp. xii+245. (Cambridge: University Press, 1910.) Price 1*s.* 6*d.*

(7) *Dorset.* By A. L. Salmon. Cambridge County Geographies. Pp. ix+154. (Cambridge: University Press, 1910.) Price 1*s.* 6*d.*

(8) *Derbyshire.* By H. H. Arnold-Bemrose. Cambridge County Geographies. Pp. x+174. (Cambridge: University Press, 1910.) Price 1*s.* 6*d.*

(9) *A Systematic Geography of Asia.* By G. W. Webb. Pp. vi+100. (London: Methuen and Co., Ltd., 1910.) Price 1*s.*

MR. MACKINDER has now brought his studies in the teaching of geography by means of its correlation with history to a penultimate stage. Approximately half this book (1) deals with history in some form, either with the world-movements of peoples, such as the Magyars or Turks, or with the history of discovery connected with the names of Marco Polo or Cook. There is, as yet, little political geography, all of which is promised in the final volume of the series, and the treatment tends to ignore the possibilities of correlation with other subjects in the school curriculum. Most pupils learn something of the value of coordinates in relation to the fixing of the position of points in space, and provided the problems of latitude and longitude be postponed, their adequate treatment follows as a special case of this method of recording the positions of points; Mr. Mackinder approaches these problems by way of an account of the work of Eratosthenes and of the eclipse of the sun at the battle of Arbela. The book makes an interesting reader, and is profusely illustrated with maps and diagrams, some of which imply a geographical knowledge which the text does not call upon the pupils to utilise.

The beginner in any study should know the technical language in which the phenomena of that subject are described: hence the utility of the three books which represent the physiographic aspect of geography. Mr. Carey (2) brings to his explanation of the terms of physical geography, and of the "principles which underlie and control the development of the physical conditions" of any region, the methods of the successful teacher. He gives a series of practical exercises which familiarise the pupil with the matter to be considered; he then presents the facts in their usual setting, and elucidates the technical terms and the principles, and, finally, asks questions which force the pupil to realise the meaning of the matter studied. The references are usually to parts of the British Isles.

Prof. Herbertson (3) attempts a succinct summary of world geography. The text and the illustrations require the active cooperation of the teacher with the pupil at every step, and thus the work is much more difficult for the pupil than either of the two previously mentioned. For example, Mr. Carey makes the pupil draw an isotherm, and then discusses the interpretation of typical isotherms for the British Isles; Prof. Herbertson deals with world isotherms at once, assuming that the pupil knows how they are made and what they mean. Prof. Herbertson gives a useful concluding chapter on map nets, while there is an appendix containing many revision questions contributed by Miss Kirk.

Prof. Gregory (4) contributes an explanation of the technical terms employed in geology, which should serve as an excellent introductory primer, but there is lacking the apparatus of exercise and question for school use. Probably, of all subjects, geology requires the assistance of an expert who can suggest and advise as to the particular ways in which practical work in the field should be performed, and the beginner, whether school pupil or private student, would be greatly helped were this primer provided with guidance in this direction.

The "Economic Atlas" (5) is a re-issue of the "School Economic Atlas," with slight modifications. Prof. Lyde, in an introduction, limits economic geography to a study of the earth in relation to man, and provides a series of valuable hints as to the study of the maps which follow. In this introduction Prof. Lyde claims that the water-parting between the Atlantic and the Indo-Pacific Oceans divides the world into two fairly equal parts, and in an inset map the water-parting is shown by a black line on a map of the world, having the Pacific Ocean in the middle. There is no suggestion of the internal drainage systems of the continents, nor of the drainage into the Arctic Ocean. A consideration of the map and text, apart from a consideration of these other facts, would probably lead to erroneous conclusions. The introduction and the maps are distinctly valuable, but there are certain difficulties with regard to this method of presenting the facts of geography. In the first place, the statistics given should be averages for a series of years, and diagrams should show the proportions of their various parts; the diagrams relating to the production of cereals, &c., give no statement of the proportions in which the different areas contribute to the total production. Further, maps without some other form of data are apt to be misleading; for example, the traditional English idea of Canada is that it is a producer of wheat. The fact that Canada produces more oats than wheat does not appear from the maps in the atlas. Oats are, curiously enough, omitted from the economic map of the United States and Canada. Similarly, in regard to the maps of minerals, such as iron and coal, it would be distinctly useful if some indication were added as to the areas in which the mineral deposits are being worked; for example, coal and iron are shown in India, but there is no indication that practically the only mining carried on in India is for coal.

The three additions to the Cambridge County Geographies, "Devonshire" (6), "Dorset" (7), and "Derbyshire" (8), are of the already familiar type of descriptive geography which this series illustrates. In "Derbyshire," Mr. Arnold-Bemrose exemplifies one of the best features of the series. He tells the story of the rocks, and shows the relation between these and the surface features, the climate, and the occupations of the people. His facts range from the Derwent Valley water scheme to the life of early man in these islands as inferred from the deposits in the caverns. In the other volumes, the authors deal with the coastal features of the counties and with the changes in the outline of the coast due to sea encroachments. These books are storehouses of facts of many kinds, and will be useful as reference books in school and other libraries. It may be urged that both in the text and in the appendix the statistics should be average values for a series of recent years.

Mr. Webb's "Asia" (8) is systematic but not regional; for example, the existence of the Thar desert is explained out of all connection with the desert belt immediately to the west; again, in the case of Japan, the large proportion of the country which is forested is ignored, and emphasis is laid upon the growth of rice, "for which the climate is specially suitable," and tea.

B. C. W.

OUR BOOK SHELF.

Catálogo Sistemático y Descriptivo de las Aves de la República Argentina. By Roberto Dabbene. Tomo Primero. *Anales Museo Nacional de Buenos Aires,* serie 3, vol xi., pp xiv+513+map. (Buenos Aires: 1910.)

IN this work, of which the present portion occupies the whole of the serial volume in which it appears, the author proposes to do for the birds of Argentina that which Mr. Ridgway is accomplishing for those of North and Central America. To a great extent the two works will, when completed, cover the whole of the South American avifauna, for, owing to the imperfect information with regard to the zoology of the outlying provinces of the Argentine Republic, Dr. Dabbene has felt himself compelled to include in his catalogue the birds of the south of Brazil, Bolivia, and Paraguay, and of the frontier districts of Chile and Uruguay.

The author has in the main followed the classification adopted in the British Museum "Hand-list of Birds," although for the Passerines he had to rely on the Museum "Catalogue," as the last part of the former work did not reach him in time to be used. This is a pity, as Dr. Sharpe made certain amendments in the arrangement of the orders which might have been advantageously followed. The present volume commences with a general account of the structure of birds, illustrated with text-figures, which, although somewhat crude, serve their purpose well, the whole account being well up to date. Ending with a bibliography, this section is followed by one on the distribution of Argentine birds, which is perhaps the most important part of the whole volume; the geographical ranges of the various species being shown in tabular form. The volume concludes with a somewhat heavy list of addenda and corrigenda (in addition to an extensive list of errata in the preliminary portion), followed by several copious indexes. The systematic part of the work will, it may be presumed, commence in the next volume, and will afford a better criterion for testing the value of the undertaking than is afforded by the one in hand.

R. L.

Land and Fresh-water Mollusca of India, &c. By Lieut.-Col. H. H. Godwin-Austen. Vol. ii., part xi. Pp. 239–310; cxviii–cxxxii. plates. (London: Taylor and Francis, 1910.)

MALACOLOGISTS will gladly welcome a further instalment of this valuable work from the pen of that nestor of Indian conchology, Col. Godwin-Austen. Like the previous part (NATURE, vol. lxxvi., 1907, p. 244), this contains further descriptions of forms, some of them new, belonging to the families Zonitidæ and Endodontidæ.

In 1907, as we pointed out, the author transferred the genera Austenia, Girasia, and Cryptosoma from the Heliocarioninæ to the Macrochlamyinæ. Next year, in the "Fauna of India: Mollusca," and now again in the part under notice, these genera reappear in their old position. These changing views are not so much to be wondered at when the extreme difficulty of reducing this complex and puzzling group to order is taken into consideration. Their classification depends on anatomical differences which are by no means so well defined by nature as one could desire. The apparent introduction, however, of one genus into two subfamilies is due to an unfortunate misprint on p. 272, where *Austenia* appears for *Euaustenia.*

All the Endodontidæ described belong to the genus Pupisoma, comprising forms included by the early writers in the genus Pupa.

A reference to the Mauritian species of Macrochlamys (Proc. Malac. Soc., vol. vi., 1905, p. 320), which the author now refers to *M. renitens,* Morelet, concludes the number.

The plates, which are faithful reproductions of the author's vigorous and effective drawings, call as such for commendation.

Jack's Insects. By Edward Selous. With forty-four illustrations by J. A. Shepherd. Pp. xiii+379. (London: Methuen and Co., Ltd., 1910.) Price 6s.

ALL we need say about this book is that Jack and his sister fall asleep over a book of natural history, and dream that they are talking to the insects, &c., to which it relates.

LETTERS TO THE EDITOR.

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

The Fur Trade.

IT is recorded in the "Life of Professor Owen" that on one occasion Lord John Russell sent him a bone, requesting that it might be identified. Owen replied that it was an ordinary ham-bone, pertaining to the genus Sus, and was informed that Lord John had received from President Grant what purported to be a bear's ham, concerning which he had his doubts, hence the inquiry.

It is not improbable that naturalists are occasionally appealed to in a similar manner for judgments on the identity of furs. In these circumstances, they may well be excused if they lack Prof. Owen's assurance, or are found entirely wrong. The mysteries of the fur trade are surely beyond the understanding of even a skilled zoologist; but there is an American publication in which they are revealed to the astonished reader. This is the retail catalogue of Albrecht and Son, of Minnesota. This firm does a very large business, and has no wish to deceive its customers as to the nature of their purchases, and yet cannot get away from the current trade names. Consequently, it publishes a detailed synonymy, some of which has not yet found its way into zoological works. I venture to extract a few items for the instruction and entertainment of those who have not access to the catalogue.

Alaska Bear.—"The best Minnesota Raccoon, coloured a dark brown."

Adelaide Chinchilla.—Australian Opossum.

French Ermine.—"Made from selected skins of the French Coney."

Baltic (White) Fox.—"The fur of the large Hare of northern Europe."

Iceland White Fox.—"White Thibet Lamb, combed until the hair is straight."

Kamtschatka Fox.—"Trade name of the northern Timber Wolf."

Baltic Lynx.—"This fur is secured from the large Belgian Hare of northern Europe, dyed a jet black."

Finland Lynx.—Australian Wallaby.

Natural Black Marten.—"From the black Marten (commonly known as skunk)."

Russian Marten.—American Opossum.

Russian Mink.—Mongolian Marmot.

Siberian Pony.—Russian Calf.

Inland Seal.—French Coney.

Coast Seal.—"Albrecht coast seal is made from the skins of the French Coney."

But the catalogue itself should be in every zoological library. It contains other items of interest; thus it appears that one may spend the trifling sum of 625 dollars on a muff made of Russian sable. Single skins of this animal are valued as high as 500 dollars when of the best quality.

T. D. A. COCKERELL.

University of Colorado, September 11.

An Attempt to Determine the Supposed Change in Weight Accompanying the Radio-active Disintegration of Radium.

SOME time ago Mr. Grant and I designed and constructed a micro-balance (Proc. Royal Society, A, vol. lxxxii., p. 580) for the purpose of demonstrating, if possible, the occurrence of a change in weight accompanying the radio-active disintegration of radium.

We have recently been attempting to do this, using 7·5 milligrams of pure radium bromide. It was necessary to have two balances of great sensitiveness in order to carry out our proposed experiment, and the two balances which we have used have each a sensitiveness of $2 \cdot 5 \times 10^{-9}$ grams, and for both of them the resting point has not varied by more than 5×10^{-9} gram during several periods of observation varying in length from a week to a month.

The plan of our proposed experiment was as follows. The two balances were mounted side by side in the same balance case in such a way that the images of a fine illuminated slit could be reflected on to the same scale from the mirrors on the two balances.

One of the balances was to serve as a check on the second, which was to be used to determine the weight of the active deposit from the radium. This second balance was of the type B figured in our paper (*loc. cit.*), and was provided with a fine fibre attachment and quartz hook from which could be suspended a piece of fine platinum wire.

A piece of stout platinum wire was passed through the wall of the balance case, and well insulated from it, and by a mechanical contrivance this insulated wire could be brought, when desired, into contact with the wire suspended from one end of the balance beam. By means of this second wire the suspended wire could be raised to a negative potential of five hundred volts.

Through the wall of the balance case were also passed two glass tubes leading to an air-circulating apparatus and to a bubbler containing the solution of radium bromide.

By means of this circulating apparatus the air contained in the balance case could be bubbled continuously through the radium bromide solution, thus keeping it charged with the radium emanation. As this decayed into radium A, the latter in the electric field should be driven to the charged and counterpoised platinum wire and accumulated there, and so its weight determined.

The behaviour of the balance was investigated with the greatest care, and after many disturbing effects had been discovered and eliminated one at a time, it was finally shown that the resting point of both balances was constant for at least a month, when every condition was the same as for the actual experiment with the single exception that the bubbler contained water instead of the radium bromide solution. Thus the following changes in conditions did not affect the resting point by more than $5 \cdot 0 \times 10^{-9}$ gram:—(*a*) Releasing and arresting the balance beam; (*b*) touching and jerking the suspended platinum wire; (*c*) starting and stopping the circulation of the air; (*d*) charging and discharging the platinum wire.

The experiment was then started by replacing the water in the bubbler by the solution of radium bromide and the behaviour of the balance again tested, when it was found that the resting point was still unaffected by the operations (*a*), (*b*), and (*c*).

The wire was then charged, and it was expected that this would cause an increase in weight of the order of 10×10^{-9} gram per day, but instead of this an increase of weight of about 30×10^{-7} gram has been found to occur.

Whilst it is not impossible that this increase in weight is due to radio-active changes, it is so much greater than that calculated on the basis of the electrical evidence that the conditions of the experiment require to be re-examined with the greatest care. An explanation that suggests itself to us is the following. With the balance case used by us and described in our paper (*loc. cit.*) the ground joints and flange are necessarily made vacuum tight by some form of lubricant. We have used the so-called rubber grease, which probably distributes vapour of heavy molecular weight throughout the balance case, although its vapour pressure is far too small to be detected by ordinary methods.

It is possible that the ions produced by the emanation and its radio-active products form nuclei for the condensation of this vapour, and that these condensed aggregates are driven with the charged nuclei to the charged and counterpoised platinum wire, there accumulated, and weighed.

The balance case and other apparatus employed is being modified so as to exclude all possibility of condensation of vapour, and it is hoped, when the necessary preliminary study of the behaviour of the instruments has been carried out, to determine whether the large change in weight that has been observed is an accompaniment of the radio-active process or an adventitious effect.

BERTRAM D. STEELE.

The University, Melbourne.

The Habits and Distribution of Scutigera in India.

BEYOND the bare fact of its occurrence, but little else seems to have been recorded concerning either the habits or distribution of this remarkable myriapod in India. During the last three years I have come across it on many occasions, and it proves to be very widely distributed in the United Provinces. It occurs from a few hundred feet above sea-level in the plains, at Allahabad, up to nearly 11,000 feet in the Himalayas, having thus a considerable vertical range. I have found it plentiful at Bhowali, among the Himalayan foot-hills of Kumaon, at 5700 feet, and at many other localities in the same district. It occurs at Dhakuri and Dhwali at an elevation of 9000 feet, and also at Phurkia at about 10,300 feet—about five miles below the Pindari glacier. In the neighbouring district of Garhwal I have met with it this year under stones between Badrinath and the village of Mana, situated at the entrance of the pass of the same name into Tibet. The elevation was approximately 10,800 feet, or possibly a little more. How many species are included within this area of distribution I am unable to say, as the Indian forms are at present being worked out by Prof. Silvestri.

Scutigera exhibits a strong dislike to expose itself to daylight and sunshine, and lurks during the daytime in dark places. The commonest situation for finding it is under matting covering the floors of bungalows, or on walls in dark corners, or under stones, &c., out of doors. Sinclair, in vol. v. of the "Cambridge Natural History," states that in Malta Scutigera darts about in the hot sunshine after its prey. In India, so far as my experience goes, it is a nocturnal animal. It is difficult to secure perfect examples, for Scutigera never seems loth to part with one or more of its extremely long and fragile limbs, which are dismembered with extreme readiness. After separation from the body the legs exhibit active muscular contractions for about twenty or thirty seconds. One of its chief enemies in the plains seem to be scorpions, which inhabit very much the same situations. On one occasion a large Buthus was observed to seize a Scutigera by several of its legs with one of its pedipalps. The Scutigera departed in great haste, and made good its escape, leaving several legs behind in the possession of the scorpion. From this occurrence one is led to suggest that the extreme length of the legs of Scutigera may perhaps be an adaptive modification for defensive purposes. When an animal is encompassed by such an armature of appendages it makes it difficult for an enemy to seize it by a more vital part of the body.

From watching the living animal, there seems no doubt that the extremely long and antenna-like hind pair of legs function as sense-organs. Individuals in captivity kept these appendages frequently uplifted and on the move after the same manner as antennæ. A large spider was placed in a vessel along with a Scutigera, and the latter manifested great alarm. The long posterior appendages were kept in constant agitation, apparently to guard against a rear attack, while the antennæ were behaving in the same manner at the opposite end of the body.

A. D. IMMS.

Biological Laboratory, Muir College, University of Allahabad, September 5.

Fire Tests with Textiles.

I SHALL be glad if you will insert the following letter in reference to fire test with textiles, as the conclusions at which I have arrived, after repeated experiments, are so different from those reached by the British Fire Prevention Committee that I feel I cannot allow the assertions of that committee to pass unchallenged. I have over and over again shown publicly and privately, including a demonstration to the members of the Home Office Committee referred to below, that "Non-flam" flannelette is only non-inflammable if it is washed in a certain way. If washed in the manner usually employed by the ordinary housewife, *i.e.* washed with soap and water, and finally wrung out of clear water until all trace of soap has gone, it burns as readily as ordinary cheap flannelette. This fact was testified to by many witnesses besides myself before the Home Office Committee. The manufacturers of "Non-flam" acknowledge this in a letter to me (a letter which was added as a footnote to my evidence before the Departmental Committee on Coroner's Law, second report, p. 42), from which the following are extracts:—"If a piece of Non-flam is washed . . . with plenty of soap to form a good lather, and then rinsed in water, but not beyond the point at which the water on wringing runs a little milky, showing that a little soapy water still remains in the cloth, it will be found to have lost scarcely any of its fire-proof qualities even after repeated washings. . . . You may perhaps ask why we have not issued instructions as to the method of washing to be adopted with Non-flam. . . . It has never been done for two reasons. One is that to issue such instructions would create suspicion. . . . The other reason is that, upon inquiry, we are satisfied that ninety-nine times out of a hundred the method which is followed in the domestic wash could hardly be improved upon. . . . The clothes, after washing, are seldom, if ever, rinsed until no soap is left in them." My contention is that this method of washing is *not* the one employed by the ordinary woman who washes at home. My school nurse made inquiries for me of a dozen mothers as to their method of washing their children's flannelette garments, and, without exception, they all said they finally wrung out of fresh water to get rid of all traces of soap. (The reason for this is, I believe, that if any soap is left in it makes the clothes nasty and sticky.)

I am confident that the 1400 lives annually lost by burning and scalding in this country will only be substantially diminished when fire-guards are compulsory. Eighty-five per cent. of these deaths were proved to be due to the want of a guard. Section 15 of the Children Act, excellent as it is, needs strengthening. It only renders it a penal offence if a child is burnt to death or seriously injured owing to the want of a fire-guard. It does not compel a parent to take reasonable precautions, by the provision of a guard, to prevent this burning.

I shall be most interested to see what conclusions are arrived at by Mr. Gladstone's committee after having heard the evidence of the various witnesses. The final report may be expected shortly.

LEONARD PARRY.

83 Church Road, Hove, September 28.

Customs at Holy Wells.

IT has been suggested that the following information is of sufficient interest to justify insertion in NATURE.

Some of your readers doubtless know the Well of St. Cubert, near Crantock, Newquay, and have read the guide-book description of the miraculous cures of infants which used to take place there in old times.

The book on ancient and holy wells in Cornwall, by Mr. Quiller-Couch, gives a full account of the ceremonies which must be observed, and Sir Norman Lockyer quotes them in the chapter on holy wells in "Stonehenge and other Stone Circles."

These authorities seem, however, to be unaware that the help of St. Cubert was sought as late as the latter half of the nineteenth century. When we were at Newquay in 1886, my parents' man-servant, a native of Mawgan, near Newquay, told the other servants that he as a delicate infant had been passed through the hole in the rock of St. Cubert's Well "for luck."

Whether all the proper ceremonies had been observed I cannot say. The servant must have been about eighteen years old in 1886, and he was alive and well in 1898, since which time we have lost sight of him.

ZORAH GODDEN.

Littlewood, Weybridge, September 27.

A Meteorological Phenomenon.

ON Monday, September 26, travelling between Etaples and Breteuil, Chemins de Fer du Nord, *en route* to Paris, my family and myself and a friend (five in all) observed the following meteorological phenomenon.

The sun was hidden, but traceable, behind a white mist from 3.30 p.m. until 4.55 p.m. (French time), and on the north and south at a considerable distance through the mist there appeared two arcs of a circular halo, showing on the north colours from red to green, and on the south colours from red to yellow, only the other spectrum colours were absent.

About 4.30 p.m. a dark grey cloud hid the whole phenomenon, but about 4.45 p.m. until the end of the display the grey cloud passed, and the sight was both beautiful and brilliant.

The colour-bands were very wide; the green was the widest and palest, and I only observed it on the northern arc.

R. ASHINGTON BULLEN.

Bordeaux, September 28.

RADIUM STANDARDS AND NOMENCLATURE.

THE International Congress of Radiology and Electricity, held at Brussels, September 12th to 15th, afforded an excellent opportunity of discussing several important questions of general interest to workers in radio-activity. The need of a definite radium standard, in which all results should be expressed, has been growing more acute with the increase of accuracy of radio-active measurements. At the present time, scientific results are expressed in many cases in terms of arbitrary radium standards kept in each laboratory, and it has been difficult to be certain of the accuracy or relative value of such standards. Mr. C. E. S. Phillips several years ago pointed out to the Röntgen Society the desirability of adopting a fixed radium standard, and arranged for the preparation of several small radium standards which were compared with the working standard adopted by Rutherford and Boltwood. Duplicates of the latter standard have been used for several years by a number of English, American, and Continental workers.

At the opening meeting, Prof. Rutherford read a report on the desirability of establishing an international radium standard. He pointed out that he had compared by the γ ray method the radium standards used by several important European laboratories, and had found that there was a considerable difference amongst them, amounting in some cases to 20 per cent. It is now possible to measure with considerable precision a number of magnitudes connected with radium; for example, the volume of the emanation, the heating effect, the rate of production of helium, and the rate of emission of α and β particles. The values of each of these quantities is dependent on the accuracy of the radium standard in which the results are expressed. For the comparison of results obtained by workers in different laboratories, it is necessary that they should all be expressed in terms of the same standard. For example, at the present time it is not possible to compare the results obtained on the heat emission of radium by various observers until the radium standards employed have been accurately compared. When once a standard has been adopted, it is relatively a simple matter to determine the radium contents of substandards by the γ ray method or modification of it, without opening the tube containing the radium.

A special international committee was appointed to report to the congress on the best means to be adopted to fix an international radium standard. This committee comprises the following workers in radio-activity representative of a number of countries: Mme. Curie, Debierne, Rutherford, Soddy, Hahn, Geitel, Meyer, Schweidler, Eve, and Boltwood. No doubt representatives of other countries who are prepared to assist in the work will be added later. This committee reported to the congress at its final meeting and their suggestions were adopted by the congress. As a member of the committee, Mme. Curie agreed to prepare a radium standard containing about 20 milligrammes of radium enclosed in a suitable sealed tube. This standard is somewhat large, but the amount was considered necessary on account of the difficulty of weighing small quantities of radium salt with the requisite accuracy. The thanks of all workers in this subject are due to Mme. Curie in undertaking the full responsibility of preparation of a standard, and for the large expenditure of time and labour its preparation will involve. The committee agreed to reimburse Mme. Curie for the cost of the radium and its preparation, after which the standard becomes the property, and is under the control, of the international committee. It was suggested that the standard should be suitably preserved in Paris. The initial cost of preparation of this standard will be somewhat heavy (about 500*l.*), but it is hoped that scientific societies and Governments of various countries will assist in defraying the expenses.

As soon as the primary standard has been prepared, it is proposed to approach through the committee the various national laboratories to ask them to acquire a radium standard accurately determined in terms of the primary standard. In this way it was thought that any Government interested in the question could acquire an accurate radium standard to be used as a basis for standardisation of quantities of radium in use in scientific laboratories, or to be sold commercially. As the primary standard is somewhat large for use in ordinary laboratories, the committee propose to investigate the question of the best method of comparing accurately in terms of the primary standard smaller substandards containing one or two milligrams of radium.

The committee also has under consideration the question of the preparation of very small substandards to be used for the determination of minute quantities of radium and of radium emanation. It is proposed that special investigations be made by the committee to determine the most suitable method of preparation and preservation of such standards. There is at present some uncertainty of how far radium solutions are affected by time in consequence of the tendency of radium to be precipitated out of the solution. No doubt before long it should be possible to secure accurate standard solutions to distribute amongst scientific workers.

In the course of the congress it was suggested that the name Curie, in honour of the late Prof. Curie, should, if possible, be employed for a quantity of radium or of the emanation. This matter was left for the consideration of the standards committee; the latter suggested that the name Curie should be used as a new unit to express the quantity or mass of radium emanation in equilibrium with one gram of radium (element). For example, the amount of emanation in equilibrium with one milligram of radium would be called 1/1000 Curie or one millicurie. The adoption of this unit will avoid much circumlocution, and will prove useful since the radium emanation is now so widely used in all kinds of experiments.

The committee has under consideration the question whether special names should be given to a very small quantity of radium, and also to the emanation in equilibrium with it. For example, the quantity 10^{-12} gram radium seems a natural unit for expression of the radium content of rocks and soils. At the same time, the large amount of investigation on the emanation content of springs and waters may make it desirable to adopt a convenient unit for expression of such quantities.

The committee pointed out that its recommendations were tentative, as all the members of the standards committee were not present at the congress, and had no opportunity of expressing their opinions. It is intended that the preparation of the radium standard should be proceeded with as soon as possible, and it is hoped that the standardisation of substandards will be possible before a year has elapsed. Prof. Stefan Meyer, of the University of Vienna, was appointed secretary of the international committee, and all communications relative to standards should be addressed to him.

The question of the nomenclature of radio-active products was informally discussed at the congress. There was a general consensus of opinion that it was not desirable to alter materially the present system of nomenclature, although it was recognised that it is far from perfect. It was felt that the gain to be obtained by a possibly more systematic nomenclature was more than counterbalanced by the confusion that would arise in consequence of a change of names. It was pointed out that the present system of nomenclature was capable of extension to include possible new products. For example, if future investigation should disclose that the product radium C consists of several products these could be named radium C_1, radium C_2, radium C_3, &c., but the term radium C would be used generally to represent the group of products as they normally always occur together. Reference was made to the undesirability of individual workers assuming the right to give new and fancy names to well-known substances.

A number of suggestions in regard to general nomenclature in radio-activity and ionisation were also made to the congress. For example, it is proposed that the term "half-value period" should be used in all cases to represent the term required for a substance to be transformed to half its original value. It is suggested that the terms "induced" and "excited" activity should be abandoned and the term "active deposit" employed in its stead, as reference is usually made to the radio-active matter itself and not to its radiations. There was a good deal of informal discussion amongst members as to the exact use of a number of scientific terms arising in radio-activity and allied subjects. Such discussions are of great importance in preventing unnecessary confusion in nomenclature due to the development of a rapidly growing subject.

A more general account of the meetings and deliberations of the congress, prepared by Dr. Makower, will appear in another issue of NATURE.

E. RUTHERFORD.

HEREDITY AT THE CHURCH CONGRESS.

THE discussion on heredity and social responsibility at the meeting of the Church Congress at Cambridge showed clearly the growing appreciation of the importance of biological principles in the study of social phenomena. The debate was opened by a paper by Dr. G. E. Shuttleworth, who dealt with the subject of the feeble-minded, chiefly from the medical point of view. After tracing the history of the different methods of treatment, he pointed out that in the case of most of the feeble-minded "there existed morbid heredity of a strongly transmissive character," and that the only sound process of attacking the problem was to be found in segregating the rising generation of the feeble-minded in industrial colonies, apart from the general community, for in that way alone could the propagation of the evil be prevented by means in harmony with our feelings of humanity.

Mrs. Pinsent, of Birmingham, a member of the Royal Commission on the Feeble-Minded, gave a brilliant address, which was clearly the chief feature of the meeting. She produced the histories of several mentally defective families, in which disease, mental defect, and crime appeared generation after generation. She pointed out the cost of such families to the community, and the appalling waste of social effort involved in their supervision and maintenance. Good and useful families, themselves often with narrow means, were being taxed to support these degenerate folk, until the more efficient restricted their families under the growing economic pressure, and reduced expenditure on maintenance and education. Thus the unfit replaced the fit within our own civilisation and under our own eyes.

The crowded audience was clearly in sympathy with Mrs. Pinsent's view of the situation, and realised the dangers of ignoring any longer the increased chances of reproduction and survival which our modern humanitarian legislation and social action had given to the degenerate classes.

The Bishop of Ripon spoke on the declining birth-rate, and said that, had it appeared in the less worthy elements of society it would have been welcome, but that, as it chiefly affected the better stocks of our race, it was deeply to be deplored. Especially was it disastrous from the point of view of the Empire, which could not hope to stand against other peoples, and especially against the increasing birth-rate, the growing numbers, and the improving organisation of the Eastern nations, unless our empty spaces in the Colonies were filled with men of British race. Marriage ought to be discouraged among the unfit, while the growth of the fit should be encouraged by a higher sense of duty in the homes and an imperial ideal of national life.

Mr. W. C. D. Whetham traced the part played by religion in the sociological development of society, and pointed out that it alone could give a motive strong enough to lead the mass of mankind to prefer the ultimate good of humanity to the immediate advantage of the individual. Hence religion possessed a real biological survival value, as Kidd showed in his book on "Social Evolution." It followed that the National Church had a very great responsibility towards the race. To play its proper part, it must maintain its hold on the efficient families of the nation, and preach the duty of encouraging the rapid reproduction of the good stocks, while limiting the output of those defective in mind or body. The future belongs to those nations whose religious teachers realise this responsibility.

In the general discussion which followed, there was an almost unbroken agreement with the main point of view of the readers of papers. While one speaker thought that only 50 per cent. of mental defect could be traced to heredity, and another emphasised the importance of alcoholism, there was a general consensus of opinion that the country must be awakened to the need of encouraging the growth of good stocks, and that the reproduction of the feeble-minded must be prevented by legislative action.

The general effect of the meeting on the mind of the listener was to produce the belief that the Church Congress, at all events, was ready, in matters of social action, to "think biologically"—surely an encouraging sign.

COCOS-KEELING ATOLL.[1]

THE author, who was for fifteen months medical officer to the cable station at Cocos-Keeling, presents us with an interesting book on that atoll made classical by the researches of Darwin during the voyage of the *Beagle*. The account of the formation and history of the colony is a romance vividly portrayed, but the main interest of the book lies in the author's observations on coral-life and on the processes in operation which can shape an atoll.

The true coral animal (Madreporaria) is a colonial sea-anemone, which continually deposits under itself carbonate of lime, thus raising its seat higher and higher above the bottom. It sits on the surface of the dried coral, such as it is commonly known to us, and in no way presents the features of an Alcyonacean, such as is represented in Fig. 6 of the book. It is peculiarly unfortunate that this figure should have been inserted, since the skeleton of reef corals, with which the author is dealing, consists entirely of dead material. It exposes the writer to the suspicion that he is unacquainted with the real nature of the coral skeleton, and hence largely throws doubt on his really admirable observations on the growth of corals in relation to their environment. These are in no way scientific, but consist of the notes of a painstaking naturalist.

The extraordinary variability in coral skeletons is well known to zoologists, and may aptly be compared to the growth shown by our forest trees in different environments. Reef corals, too, resemble trees in that they are largely dependent for their food on chlorophyll, which is present in minute algæ, living in their digestive cavities. The coloration of most reef corals is largely due to these algæ, and their mode of growth is sympathetic to them in that the coral skeleton is deposited so as to expose the polyps to the maximum amount of light. Such appear to us the ordinary views of zoologists, but our author regards sediment as the main factor to account for the variability of corals; that it is an important subsidiary factor cannot, of course, be denied. Some corals, such as Cœnopsammia, have no algæ, but pigments in granules in their cells; they, of course, are unaffected by light. Yet others probably have similar pigment together with algæ, but our author does not follow out what should be a most profitable line of research.

The statement that corals know "no natural death," does not rest on observation, and is contrary to the few facts we have. No zoologist would consider the rate of growth of corals slow. The observations on the forms of growth of corals are not convincing, since our author does not appear to have examined the zooids to see whether he is really dealing in any genus with one or more species. He is hence not justified in stating that the distribution of atoll corals "is a distribution of types and *not of species*." The observations on the effect of silt suggest research, such as has for some years been undertaken by Wayland Vaughan at the Tortugas; they are not definitive enough to be of much value. Vaughan, by the way, found no great difficulty in transplanting corals.

FIG. 1.—The Lagoon Shore of Pulu Tikus, to show the Sand-piling by a westerly wind. From "Coral and Atolls."

The third part of the book deals with the Cocos-Keeling atoll and its problems, concluding with chapters on the formation of atolls in general. We agree with the author that "it is almost impossible to judge of the method of formation of any atoll not actually visited and examined." The lagoon of Cocos-Keeling is filling up, we are told, both by organic

[1] "Coral and Atolls." By F. Wood-Jones. Pp. xxiii+392. (London: Lovell, Reeve and Co., Ltd., 1910.) Price 24*s*. net.

growth within it and by material washed into it over the barrier. It may be so, but it does not justify the statement that "atoll lagoons tend, as a rule, to become smaller and shallower," and there is no attempt by reference to other atolls to justify it. Solution and material swept out by the tides are said to have nothing to do with the formation of the lagoons of atolls. The picture of a high island crumbling to pieces within the calm of an encircling barrier reef appears to our author to be contrary to all natural laws. On what view does he explain Agassiz's wonderful series of photographs of Fijian islands within barrier reefs? "In this (his own) description," he states, "it is assumed throughout that the lagoon is a slightly submerged reef"; why this assumption without evidence? The encircling reef is said to be "a mosaic inlay of coral fragments, cemented together into a solid platform," but there is no evidence that it was ever really examined. It is supposed to have grown up as a platform, and many of its constituent organisms must surely have remained in their growth-positions. A similar platform is found at 13 feet above mean tide level; it is stated that such a platform can only be formed below this level, and its existence is explained as due to elevation.

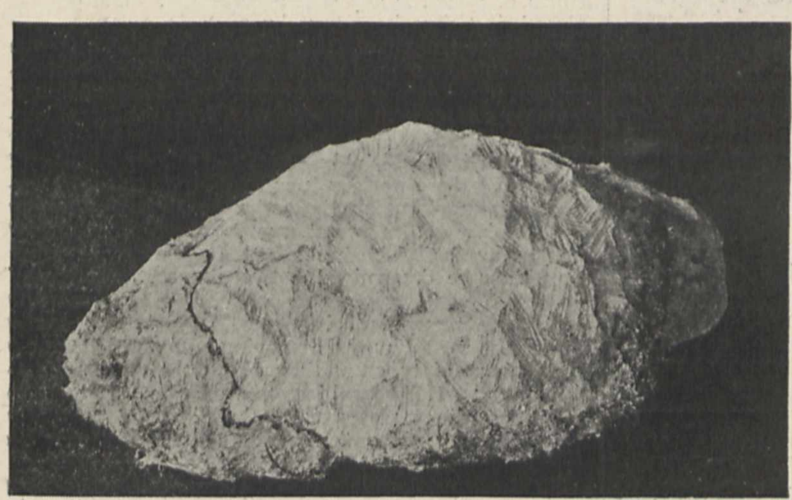

FIG. 2.—Photograph of a Boulder of Alga-covered Dead Coral Rock, to show the bites of a fish of the genus Scarus. The black line marks the edge of the alga covering not bitten away by the fish. From "Coral and Atolls."

Our author does good service in directing attention to the important effects of sedimentation. Sedimentation banks largely form the foundations of reefs, but "it matters not what the base may be so long as its platform comes within the wind-stirred area." "Any elevation which rises to this plane (the *limiting line of sedimentation*) will furnish the corals with a suitable basis." The depth of this *line* varies. It is entirely a supposititious *line*, and, so far as we can understand, may lie at any depth. Direct investigation on the processes of sedimentation in the ocean is certainly needed.

In conclusion, it cannot be said that Dr. Wood-Jones has much new to tell us. His volume is, however, a very readable one, and most suggestive of lines of research on corals, which might profitably be pursued by more precise methods. His range of investigation and reading were obviously too restricted to enable him to draw conclusions as to the formation of coral reefs in general. The account of the fauna and flora is very good, and the note on Scarus as a coral-feeder interesting. The illustrations of corals and other organisms are good, but a specialist should have been consulted, so that the names of the coral genera might have been inserted. An obvious Actinian (p. 161) would not then have been labelled as an Alcyonarian.

RESEARCHES IN STELLAR PARALLAX.[1]

THE Observatory of Yale College has acquired a deservedly high reputation for the zeal with which the staff has prosecuted the inquiry into stellar parallax and the standard of accuracy consistently maintained. This latest contribution to the subject cannot but enhance that reputation for accuracy, for the results sought do not aim so much at applying the method to fresh instances, as to the re-examination of previous investigations with the view of improving their trustworthiness. Of the stars, the distances of which are here discussed, two-thirds have already been the subject of inquiry at Yale or elsewhere, but on various grounds the results have been regarded with a degree of suspicion that made the repetition of the measures desirable.

The new material falls into two classes, one containing stars having a larger annual proper motion than about 0·4″; the other, selected stars in the Pleiades the observation of which might afford evidence as to the distance of the group as a whole. As the results derived from these Pleiades stars are not regarded as conclusive, and do not enter into the final catalogue, they may be dismissed here. One star gave the value zero, indicating that the Pleiades group is at the same distance as the star; the measures of another assigned the small negative parallax of −0·3″, "a value that would give a possible limit of systematic error"; while the third series, resulting in the value +0·6″, suggests that the star does not belong to the group at all, but is nearer to our system, "and this result would seem to be fairly assured." The approximate distance of the Pleiades group still remains a matter of conjecture.

Naturally in a work so long and laborious, difficulties arose in connection with the instrumental and optical equipment, necessitating interruptions in the continuance of the sequences. The most formidable of these was a tendency for the field lens of the eyepiece to work loose, to which inconvenience it is not necessary to refer further, than to express our assurance that the skill and experience of the observers would succeed in effectually removing any traces of systematic error arising from this untoward accident. To show that this confidence is warranted, we may give the final results obtained by the three observers in the case of the Arcturus determination, a star the measures of which have been most scrupulously examined, since for a star of such brilliancy and large proper motion the earlier values of parallax were so suspiciously small, as to suggest that some inherent quality in the star itself, such as colour, or some peculiarity in the observers' method of measuring, had influenced the result. With regard to the detection of

[1] Transactions of the Astronomical Observatory of Yale University Vol. ii., part ii. Parallax Investigations on thirty-five selected stars by Frederic L. Chase, Mason F. Smith, and William L. Elkin (Director). (New Haven: Published by the University, 1910.)

a colour effect in the observations, to which a reference is made in NATURE (vol. lxxv., p. 234), further examination has failed to disclose any systematic error attributable to that cause, while the arrangement and discussion of the several series of measures—Dr. Elkin himself made no fewer than seven—exclude the possibility of any personal peculiarity or habit escaping detection. The adopted parallax values for each of the three observers are as follows:—

Observer.	Parallax.	Probable Error.	Probable Error of Single Obn.	No. of Observation.
Elkin ...	+0″·051 ...	±0″·013 ...	±0″·240 ...	126
Chase ...	+0·085 ...	±0·007 ...	±0·127 ...	154
Smith ...	+0·050 ...	±0·011 ...	±0·126 ...	123

After an elaborate system of "weighting," for details of which we must refer to the original paper, the finally adopted value of the parallax of Arcturus is 0·066″ ± 0·006″.

The interesting stars 61 Cygni and Groombridge 1830, notwithstanding the repeated attempts that have been made to determine the parallax, are among those stars of which it has been thought desirable to repeat the measures. Without entering into further particulars, it may be said that the results are equally consistent as those obtained in the case of Arcturus, and command equal confidence.

The grand result of the work at Yale, which has occupied the three observers for some years, is to assign a parallax to 200 stars, with an accuracy that we believe has not been attained elsewhere. The three stars in the Pleiades not being included, we have here a catalogue of 197 stars, which Dr. Elkin forms into groups depending on magnitude and proper motion. This table is so small that it can be conveniently given here. It may seem but a modest outcome for so many years of careful and anxious work, but those who appreciate it most will be warmest in their congratulations to Dr. Elkin and his able colleagues on the satisfactory completion of a task of no common difficulty. The table into which so much work is compressed is as follows:—

Proper Motion	0″·00 – 0″·34	0″·41 – 0″·54	0″·55 – 0″·65	0″·66 – 0″·96	1″·01 – 7″·07
Mag. 0·0 – 2·5	+0″·031 (13)	+0″·100 (2)	+0″·113 (3)	... ... (0)	+0″·200 (2)
,, 3·0 – 5·0	+0·026 (9)	+0·024 (7)	+0·114 (5)	+0·091 (8)	+0·162 (6)
,, 5·1 – 7·0	−0·010 (7)	+0·034 (14)	+0·064 (16)	+0·036 (20)	+0·111 (8)
,, 7·1 – 9·0	... ... (0)	+0·040 (23)	+0·032 (23)	+0·018 (19)	+0·128 (12)

The number in brackets after the parallax signifies the number of stars in each group. As Dr. Elkin remarks, "There is, with slight exception, manifest a very decided sequence of values, both with respect to magnitude and size of proper motion, such as one might expect."

W. E. P.

THE PERFILOGRAPH.

THE perfilograph is an ingenious instrument for recording graphically the undulations of the bottom of a channel in depths up to about six or seven fathoms. It is the invention of Augustus Mercau, an Argentine engineer, by whom a paper was read at Buenos Aires before the Naval Section at the recent meeting of the International American Scientific Congress, in the course of which the instrument was fully described. The principles on which its construction depends and the practical results obtained from its use appear to present some points of interest.

A heavy weight of from 150 to 200 lb. being slowly dragged along the bottom by a wire rope attached to the stern of a steam launch, it is obvious that as the depth changes the inclination of the wire will vary. By an ingenious mechanism, the sine of the angle made by the wire with the horizontal plane is registered graphically in parallel ordinates on a roll of paper, which is slowly unwound by means of clockwork at a rate proportionate to that of the vessel. The lengths of the ordinates, being proportional to the sines of the varying angles, represent the undulations of the bottom referred to the horizontal plane, and are registered on a convenient scale on the paper by means of a system of levers.

The instrument, mounted in the stern of the launch, is placed accurately at a height of 5 feet above the water-line, and the depths are measured from a zero line drawn by a pencil pressing against the roll of paper as it is unwound. The system of levers by which the sines of the angles are registered is connected to a length of tubing of small diameter, through which a thin guiding wire passes. One end of this wire, which is about 50 feet in length, is secured to a spiral spring attached to the weight on the bottom; the other end is led underneath the base of the instrument and secured. The spiral spring yielding to a strain of about 150 lb., the tension on the guiding wire is automatically relieved by throwing the excess of strain on to the towing cable, the length of which is adjusted accordingly. The guiding wire, being thus kept taut by a strain not exceeding 150 lb., the length of tubing through which the wire passes takes up an inclination to the horizontal dependent on the depth of water, and in so doing it actuates the system of levers with which it is connected.

The speed at which the weight can be towed should not exceed three or four knots. There is an arrangement for marking on the paper the instant at which the position of the vessel may be fixed by means of sextant angles. The horizontal scale of the diagram may thus be determined between successive "fixes" by actual observation, independently of the rate at which the paper is being unrolled or the speed at which the vessel may be moving. Unless these two factors remain constant throughout the run, the horizontal scale will be subject to variation. It is desirable therefore to reconstruct the diagram, so far as its horizontal components are concerned, from the data afforded by the "fixes."

The horizontal scale of the diagram is roughly about 1/1000 or about 70 inches to the nautical mile; the vertical scale is about ¼ inch to a foot. The instrument has been adopted by the Argentine Hydrographic Service, and has been in constant use for some time past. It is understood that the results are considered quite satisfactory.

An opportunity of witnessing a practical trial was courteously afforded to the present writer by the Argentine naval authorities. The instrument was readily set up and adjusted, and several sectional lines were run across the dredged channel leading to the docks at Buenos Aires. The resulting diagrams over the same section run in opposite directions agreed accurately, and no difficulty whatever was experienced during the trials.

The following advantages have been claimed for the instrument:—(1) Continuity of the section traced; (2) accuracy of results; (3) rapidity as compared with ordinary methods; (4) economy of skilled labour; (5) facility of manipulation; (6) capability of use in circumstances of sea and weather when accurate soundings by the lead could not be obtained.

The trials were carried out in open water with its surface considerably disturbed by a fresh breeze, and afforded a very fair test of the capabilities of the instrument. There is certainly a source of error owing to the motion of the vessel. The section as shown on the diagram is necessarily a combination of effects due to the pitching of the vessel and the actual form of the bottom.

In several of the diagrams the rapid scending of the vessel was noticeable in oscillations to the extent of 2 or even 3 feet; but it was not difficult to draw a mean line which would eliminate the motion with a fair degree of accuracy. Practically the speed of the launch scarcely exceeded that at which she might have proceeded with two leadsmen sounding in the ordinary manner, and the same number of hands are required, although they need not necessarily be skilled leadsmen. Two officers are necessary for fixing the vessel, as they would be in ordinary circumstances.

On a rocky bottom, where dredging has been carried out, the weight would be constantly liable to catch in the inequalities of the bottom, and bring up the boat, thus causing delays and possibly breakage of gear. In such cases, moreover, the usual method of sweeping with an iron bar could not safely be dispensed with, however accurately each separate section might be obtained.

On the whole, it may be said that the advantages to be derived from the invention do not at present seem so clearly pronounced as to make it likely that it would be adopted for use under the conditions usually prevailing in the examination of dredged channels. There might, however, be special circumstances in which it could be used advantageously.

A. M. F.

NOTES.

A monument to Gregor Mendel, the naturalist, who was born at Amsterdam in 1822 and died at Brünn in 1884, was unveiled at the latter place on October 2.

The summer season, comprised by the six months from April to September, can in no sense be considered ideal, although from a meteorological point of view it has not differed very widely from the average. At Greenwich the mean temperature for the six months was 57·0°, which is 0·9° below the average of the past sixty years, but is 0·9° higher than for the corresponding six months in 1909. The warmest month was August, with a mean temperature of 62·2°, and this was followed by a mean of 61·5° in June. May and June were the only two months with the mean temperature in excess of the average. The highest shade temperature during the summer was 82·3°, in June, and there was no other month with a temperature of 80°. The only years since 1841 with a slightly lower absolute maximum summer temperature are 1853, 1862, 1879, and 1882. There were in all only fifty-one days with a temperature of 70° or above, and the only summers with so few warm days are 1860, 1879, and 1888. The most conspicuous month for the absence of warm days was July, when there were only six days with a temperature of 70° or above; this is the smallest number of such warm days in July since the establishment of trustworthy records in 1841. August had twenty days with a temperature of 70° or above, and June follows with seventeen; in September there were only two. The only instances of frost in the shade in the six summer months are two in April and one in May. The aggregate rainfall at Greenwich was 13·60 inches, which is 1·22 inches more than the average of the past sixty years, but is 0·44 inch less than for the corresponding period in 1909. The rainfall was in excess of the average in each month, with the exception of September, when there was a deficiency of 1·47 inches. The wettest summer month was July, with a total measurement of 3·55 inches, which is 1·15 inches more than the normal. In all, rain fell on ninety-two days, but only on three days in September, when the aggregate measurement was 0·72 inch, and on one day the fall was 0·66 inch. The duration of bright sunshine was 966 hours, which is 165 hours fewer than the average, and May is the only month with an excess of sunshine. The sunniest month was May, with a total duration of 219 hours, and the least sunny month was July, with 112 hours' duration, which is 124 hours fewer than the normal. The finest month of the six was undoubtedly September.

Mr. Marconi has informed the Marconi Wireless Telegraph Company that wireless telegraphic messages have been successfully transmitted between Clifden (Galway) and Buenos Aires, a distance of about six thousand miles, without the employment of an intervening relay station. We learn from a note in the *Engineer* for September 30 that the tramp steamer *Nonsuch*, whilst on her voyage from Bombay to Hull and Middlesbrough, was heard at the wireless station at the North Foreland calling her name when she was fifteen miles south of Cape de Gaa, at the south-east corner of Spain, a position distant 940 nautical miles from the North Foreland. This distance, across the obstacle of the whole of France and Spain, and the Pyrenees, is a remarkable range for wireless signals from a ship. The owners received a message through the station at Ushant, sent from the ship off Cape Roca, near Lisbon, no less than 610 nautical miles from Ushant. The *Nonsuch* is the first tramp steamer to be fitted with wireless telegraph.

The council of the Institution of Civil Engineers has made the following awards in respect of papers published in Section ii. of the Proceedings for the session 1909–10:—A Telford gold medal to Major W. W. Harts, U.S. Army (Nashville, Tenn.); a Watt gold medal to Mr. A. Trewby (London); a Crampton prize to Prof. A. H. Gibson and Mr. A. Ryan (Manchester); and Telford premiums to Messrs. W. R. Baldwin-Wiseman (Southampton), O. W. Griffith (London), Dr. W. E. Lilley (Dublin), W. Corin (Sydney), J. A. Saner (Northwich), and F. O. Blackwell (New York). The council has awarded the Indian premium for 1910 to Mr. C. W. Lloyd-Jones (Secunderabad).

The Paris correspondent of the *Times* has reported the death, in his sixty-ninth year, of Prof. Fulgence Raymond, clinical professor of diseases of the nervous system in the University of Paris, and superintendent of the Salpêtrière. Prof. Raymond became known first by a special study on "L'Hémichorée, L'Hémianasthésie, et Les Tremblements Symptomatiques." On the death of his friend and teacher Charcot, in 1894, Raymond was appointed to succeed to the chair of clinical professor of nervous diseases. He was the author of many works on nervous and kindred diseases, and was celebrated for the laboratories of pathological anatomy and physiological psychology which he instituted and superintended at the Salpêtrière. Prof. Raymond was a member of many of the scientific societies of Europe.

The ordinary meetings of the Royal Geographical Society for the winter session will begin on November 7, when Major P. Molesworth Sykes will lecture on his further journeys in Persia. Subsequent meetings have been provisionally arranged as follows:—November 21: some results of the Duke of the Abruzzi's Karakoram expedition, Dr. Filippo de Filippi; December 5: the new geography and its aims, Mr. H. J. Mackinder, M.P.; December 19: the French Antarctic Expedition, 1909–10, Dr. J. B. Charcot; January 16, 1911: the *Michael Sars* North Atlantic deep sea expedition, Sir John Murray and Dr. Hjort. A selection from the following papers may be expected during the session:—Recent explorations in Dutch New Guinea, Dr. H. A. Lorentz; the development of British Central Africa, Sir Alfred Sharpe; recent boundary work in Bolivia, Major P. H. Fawcett; the peoples of the Sudan, Dr. C. G. Seligmann; the geographical conditions affecting the development of Canada, Prof. W. L. Grant; economic geography of the Tyne, Mr. A. J. Sargent; distribution of cotton culture within the British Empire, Mr. J. Howard Reed; researches in the Himalayas, Dr. Arthur Neve; explorations in western and northern Australia, Mr. A. W. Canning.

MR. C. G. THORP, 182 St. George's Terrace, Perth, Western Australia, writes:—"I am endeavouring to prove the origin of obsidianites; it has been stated that Mr. Dunn's bubble hypothesis is impossible on account of the occurrence of dumb-bells. I wish to endeavour to make a dumb-bell by the union of the drops of two bubbles. Perhaps one of your readers would help me to the formation of a very viscid fluid that would dry." The inquiry was submitted to Prof. C. V. Boys, who has kindly sent the following reply:—"The best mixture for blowing bubbles that will solidify, not exactly dry, is resin containing one-tenth, more or less, of beeswax, melted and blown when fluid. Possibly the addition of Canada balsam would make the mixture more adhesive. No soap and glycerine mixture will make a bubble that will dry."

IN a letter entitled "An Undescribed Feather-element," which appeared in NATURE of September 15, Mr. F. J. Stubbs described a remarkable feature of the structure of the primary feathers of certain birds, which he said "seems to be hitherto undescribed." Two correspondents have written to point out that the peculiarity in question has been described before. Mr. W. P. Pycraft states that he published an account of the structure seventeen years ago in the pages of *Natural Science* (vol. iii., 1893, p. 197). Prof. R. v. Lendenfeld, of Prague, informs us that "these structures have been studied in my laboratory and carefully described and figured by one of my students, Dr. E. Mascha, in his paper 'Ueber die Schwungfedern' (*Zeitschrift für wissenschaftliche Zoologie*, vol. lxxvii., 1904, pp. 606–51, nine text figures, Plates 29–31), on p. 614 ff, an English version of which appeared in the Smithsonian Miscellaneous Collections (vol. xlviii., 1905, 30 pp., 15 plates) under the title 'The Structure of Wing Feathers.'"

IN *Man* for September Mr. W. E. Hardenburg, in an account of the Indian tribes of the Putumayo River, one of the principal tributaries of the Upper Amazon, describes what he terms a system of "wireless telegraphy" in use among this race. It consists of two logs of hard wood pierced by narrow holes of longitudinal section, burnt out by heated stones. One log is always thicker than the other, producing two grave notes, while the smaller trunk gives out two which are acute. They are hung from the roof timbers, and are beaten with a club tipped with rubber. A code is arranged based upon the differences of tone and the length and number of the blows, so that messages can be exchanged, on a clear day the sound reaching a distance of from 12 to 15 kilometres.

A SERIES of experiments by Messrs. R. M. Yerkes and D. Bloomfield is described in the *Psychological Bulletin* for August, planned to answer the question, Do kittens kill mice instinctively? The experiments decide in the affirmative. They show that "the instinct to kill may manifest itself in the kitten before the end of the first month of life, while the animal is yet feeble and barely able to eat a young mouse." The instinctive reaction, though somewhat variable with individual kittens, is fairly definite in character. It appears quite suddenly, and is aroused by the movement of the mouse, and, after the first reaction, by the smell of the mouse. Usually it develops during the second month of the kitten's life, and does not completely wane during the following two or three months, but "it apparently becomes increasingly difficult to evoke. The practical inference is: allow a kitten to exercise its killing instinct when young if a good mouser is desired." The authors admit the great value of imitation and experience for the killing reactions of kittens, and for the modification and development of these reactions. But the prime object of their experiments is to show that kittens, reared in solitude, seize the mouse, even in the first kill, so that they cannot be bitten by it, and that the visual experience of movement is the primary condition for the initiation of the instinct.

IN the Bulletin of the Johns Hopkins Hospital for September Dr. C. L. Minor, of Asheville, N.C., has a paper on the use of the X-ray in the diagnosis of pulmonary tuberculosis. This subject was discussed at the recent annual meeting of the British Medical Association in London (see NATURE, August 4). Dr. Minor gives full directions as to the most suitable arrangement of the apparatus and of the dark-room, and as to the positions in which patients should be examined. His paper is chiefly interesting as representing the point of view of a physician in general practice in contrast to that of the X-ray specialist. In his opinion the general practitioner should confine himself to the use of the fluorescent screen, and leave the taking and the interpretation of photographic plates, with the great amount of detail they contain, to the X-ray specialist. He enters fully into the controversy regarding the comparative value of the clinical and the radioscopic methods of diagnosis of phthisis in its earliest stages. Many X-ray specialists, he says, insist that signs of early phthisis are shown by the Röntgen rays before the physician is able to demonstrate them by the ordinary methods of physical diagnosis. In Dr. Minor's opinion this is due to the fact that many clinical physicians are not really expert in the method of physical diagnosis, and that they frequently overlook signs which a more trustworthy diagnostician would have discovered. Dr. Minor's own experience goes to show that there are few cases in which, although distinct shadows are shown by the X-rays, no physical signs can be elicited in the chest. The present writer, however, has frequently had patients sent for examination by thoroughly competent physicians with the statement that no physical signs of lung disease were present. He has then demonstrated to the physician the exact position and extent of the pulmonary lesion, and the physician has thereupon been able, by a careful examination, to detect definite signs at this spot. Dr. Minor directs special attention to the frequency of the discovery of enlarged lymphatic glands in the chest, and he describes the positions in which they are to be found. His conclusions lend further confirmation to the view, now generally accepted as true, that the X-ray examination of the lungs is a very valuable aid to the early diagnosis of pulmonary phthisis.

DR. P. L. SCLATER has contributed to the "Handbook of Jamaica for 1910" a revised list of the birds of Jamaica, based on the one by Messrs. A. and E. Newton in the same publication for 1881. The new list, which is printed separately in pamphlet form, and can be obtained in London of Messrs. H. Sotheran and Co., contains notes on the distribution of the various groups.

THE European hedgehog has been hitherto considered to differ from other species of its genus in the peculiar form and single root of the third upper incisor and upper canine, while it has also been supposed to be characterised by the constant presence and relatively large size of the second upper premolar. From an examination of a large number of specimens, Mr. E. Hollis (*Zoologist* for September) finds, however, that two of these characters are by no means constant. Thus, out of eleven British examples, in only one case was the upper canine single-rooted, while in six instances it was double-rooted, and in the remaining four in a kind of intermediate condition. In the same series the second upper premolar was absent in three instances, rudimentary in one case, and normal in the remainder. From this it is inferred that *Erinaceus europaeus* is in a state of unstable equilibrium in regard to the latter tooth, thereby resembling the Asiatic *E. micropus* and *E. pictus*. Somewhat similar variations were observed in the collection of skulls of the European species in the British Museum, but in no instance was the third upper incisor observed to be double-rooted. A single root to this incisor may therefore still be characteristic of *E. europaeus*.

THE fourth part of the Flora of Glamorgan, dealing with the division Incompletæ of the Dicotyledons, has been recently published. The flora is being prepared under the editorship of Prof. A. H. Trow by a committee, for whom Dr. and Miss Vachell act as secretaries. It is noted that several critical and polymorphic species require more detailed observation and study.

A SECOND report on the Hymeniales of Connecticut, constituting Bulletin No. 15 of the State of Connecticut Geological and Natural History Survey, requires to be taken in conjunction with the preliminary report published as Bulletin No. 3, in which the keys to the genera were given. Analytical keys for the species are supplied in the present bulletin, and are accompanied by excellent illustrations of selected types. Prof. A. E. White, who is responsible for both reports, also provides a good account of some edible species of Agaricaceæ; most of the species are similar to those recommended by British fungologists, but the author is bolder than some authorities in recommending *Lactarius piperatus* and *Cantharellus aurantiacus*.

THE plant formations of East Bolivia, which were briefly outlined and illustrated by Dr. Th. Herzog in a number of the *Vegetationsbilder*, are described at greater length in Engler's *Botanische Jahrbücher* (vol. xliv., part iii.). A well-defined formation is that of the Pantanales, developed near Carumba, in the extreme south-east, which is a fertile virgin forest, consisting largely of deciduous trees with a wealth of lianes. *Piptadenia macrocarpa* is a dominant tree, valued for its timber and astringent bark; *Tecoma Ipe* is another magnificent tree, yielding a durable timber, and *Calcophyllum multiflorum* is also characteristic of the formation. The lianes include *Urvillea laevis*, *Bignonia unguis cati*, and species of Serjania. The author presents a graphic description of the magnificent palm, *Orbignya phalerata*, *Physocalymma scaberrima* (Lythraceæ), and various species of Tecoma, that inhabit the highlands of Velasco, and extols the beauty of the flowering shrubs found on the Cordilleras.

DR. FELIX OSWALD, so well known for his geological map and description of Armenia, has published an account of the tectonic development of the Armenian highlands in *Petermann's Mitteilungen* for 1910 (pp. 8, 70, and 126). The movements that affected the pre-Devonian rocks are obscured by the potency of the Hercynian and Alpine movements. The Alpine uplift was foreshadowed in Armenia by considerable folding in Upper Eocene times, but the resulting land-surface was reduced almost to a peneplain before the great transgression of the Miocene sea. This sea even spread over land that had remained unsubmerged since the opening of the Mesozoic era. Its invasion, however, was soon checked, since in the Upper Miocene epoch the Alpine folding set in, accompanied by great intrusions of ultrabasic rocks. This folding continued long enough to involve the Sarmatian deposits on the south flank of the Caucasian chain. Dr. Oswald, with the aid of a map, explains the grouping of the main lines of folding, and discusses the breaking up of the highlands into elevated plateaus and regions of depression. He regards the volcanic phenomena as manifested along planes of fracture. When these, in Upper Miocene times, reached down to the ultrabasic regions, peridotites rose among the folded rocks. Regions of decreasing basicity have since been drawn on, and only the higher zones are now penetrated, as is evidenced by the recent rhyolitic lavas of the crater of Nimrud.

AN interesting lecture on "Vegetation and Rainfall," recently delivered at Perth, Western Australia, by Dr. A. Morrison, is reported in the *West Australian* of August 4. The author does not deal specially with the distribution of rainfall in the colony, but with the importance of a plentiful water supply and the warmth of the sun on vegetation generally, and on the influence of the latter in preserving the moisture of the soil. On hill-sides, without the protection of vegetation, the natural streams become choked with débris, causing floods and devastation in the country below, but vegetation gives time for water to percolate the soil without displacing it. The cosmic causes of rainfall must be carefully distinguished from local causes, which only modify the precipitation brought from distant parts. Irrigation during the dry season would do much in maintaining the desired moist condition of air and soil, and the plantation of trees would help to make it permanent; but when the settler has cleared the bush for cultivation he sometimes finds that a heavy fall of rain will run off the surface without having time to sink into the ground. The author considers that a large proportion of the country in Western Australia should be left in its original state, clothed with forests.

WE have received a separate copy of Prof. L. Palazzo's paper on his magnetic survey of Sardinia, which appeared last year in *Terrestrial Magnetism and Atmospheric Electricity*. While the eastern side of the island appears to be normal, the western shows great abnormalities which are not due to surface rocks. While the equal vertical force lines run nearly east and west across the island, the equal horizontal force lines, which have the general direction north-east to south-west, are disturbed at points near the middle and north end of the west coast. The isogonic and isoclinic lines show irregularities near the same points. The annual secular changes are:—in west declination, $-4{\cdot}6'$; in inclination, $-1{\cdot}5'$; in horizontal force, $+0{\cdot}00020$ dyne per unit pole.

THE *South African Journal of Science* for June contains an important paper by Dr. D. M. Tomory on modern methods of water purification. The Modder River water, from which Bloemfontein draws its supplies, refuses to settle clear, and cannot be effectively purified by sand filters, which, however, are very soon choked by the suspended clay. A remarkable improvement was effected by precipitating with lime and permanganate, and filtering rapidly through a mechanical filter, the deaths from enteric fever falling from 83 per 10,000 in 1896 and 20 per 10,000 in 1904 to 2·75 per 10,000 in 1908. In view of the necessity of increasing the plant, a tour was made to inspect the chief purification plants both in Europe and in America. England and the northern part of the States do not show many examples of river-waters of the Modder type, and appear to be admirably served by the "fool-proof" method of sand-filtration. But in Egypt and in the southern States the author found many analogous cases in which extraordinary results were achieved by chemical precipitation followed by rapid mechanical filtration. He concludes that the extraordinary rapidity of the mechanical filtration is accompanied by no special risks of pollution when it is used intelligently in conjunction with a chemical precipitation process, and that in the case of non-settling waters, which can only be purified by such a precipitation, the rapid mechanical filtration is decidedly to be preferred on the ground of efficiency combined with economy. The paper will be read with interest by many who have been compelled to limit their observations of water purification to the somewhat uniform conditions which prevail in the water supplies of Great Britain.

THE *American Journal of Science* for September contains an account of an important series of researches upon the complexity of tellurium, by Mr. W. R. Flint, of Yale University. The material used amounted to 500 grams of the redistilled metal; a series of five conversions of the basic nitrate $2TeO_2.HNO_3$ into the dioxide gave an atomic weight 127·45, with a maximum error 0·04. The material was fractionated by repeatedly precipitating the dioxide from solutions of the chloride by the addition of a large excess of water. Four precipitations reduced the atomic weight to 126·59, whilst ten precipitations gave the value 124·32 for a fraction amounting to 23 grams. This figure does not necessarily represent the lowest possible value for the atomic weight, but it agrees well with Mendeléeff's prediction that the true value would be 123 to 126, and "is apparently the nearest approach which has yet been made to the true atomic weight of the element tellurium." The fractions less easily hydrolysed by water were also worked up, and yellow to green substances were isolated; these gave negative results when tested for the commoner elements, but gave nearly all the reactions of tellurium, and many perhaps contain an unknown homologue.

THE trials of H.M. second-class cruiser *Bristol*, completed on September 27, form the subject of an illustrated article in *Engineering* for September 30. The *Bristol* is the first British ship tried with other than the Parsons type of turbine, her propelling machinery consisting of Curtis turbines of a special character designed by the builders, Messrs. John Brown and Co., of Clydebank. This firm has taken up the Curtis turbine on account of the following reasons:—(1) the potential advantages of acquiring experience with a type of marine turbine capable of using superheated steam, as in land installations; (2) the attainment of economy at low powers, without the disadvantage of very close-fitting parts, and the extremely fine adjustments entailed thereby; and (3) the simplification of the connections and general engine-room arrangement, and also the expectation of attaining higher efficiency by an increase in size of the individual propellers. With characteristic thoroughness, the firm first constructed experimental plant and conducted a lengthy research on several modifications of the Curtis turbine, the results of which are now embodied in the machinery of the *Bristol*. It is a pleasure to record that the result of this policy is that the *Bristol* on her official trials has secured the same mileage per unit of water consumption as has been obtained in the four ships of her class which preceded her, these having Parsons turbines, embodying the inventor's latest improvements at the date of their construction. As this is the first Brown-Curtis installation, even better results may be looked for as the experience of the makers extends.

THE syndics of the University Press, Cambridge, have entered into an agreement with the directors of the Chicago University Press to undertake the publication and sale in England and in the British colonies of books issued by the Chicago University Press. This will apply to all future publications and, subject to certain existing arrangements, also to books already published.

THE October issue of the quarterly list of second-hand instruments which he has for sale or hire has been published by Mr. C. Baker, of 244 High Holborn, London. The catalogue contains details of 1635 pieces of apparatus, and is concerned with microscopes, surveying and drawing instruments, telescopes, spectroscopic apparatus, as well as instruments for use in the study of most other branches of physics. Messrs. H. F. Angus and Co., 83 Wigmore Street, London, have also sent a copy of their first catalogue of second-hand scientific apparatus and accessories. This department has been added to the business but recently, but the list shows that workers in science will find already a good selection of instruments likely to provide the apparatus of which they may be in search.

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES IN OCTOBER:—

October 7. 5h. 6m. to 6h. 23m. Moon occults δ Scorpii (mag. 2·5).
,, 10. 11h. 7m. Minimum of Algol (β Persei).
,, 11. Mercury at greatest elongation west and visible as a morning star.
,, 13. 7h. 56m. Minimum of Algol (β Persei).
,, 18–22. Epoch of the October shooting stars (*Orionids*, radiant at 92° + 15°).
,, 26. 14h. 44m. to 15h. 51m. Moon occults η Leonis (mag. 3·6).
,, 27. 22h. 52m. Venus and Jupiter in conjunction, Venus 0° 11′ N.
,, 29. 13h. 16m. Mercury and Jupiter in conjunction, Mercury 0° 21′ N.

A BRIGHT METEOR.—From a correspondent we have received the following particulars of a bright meteor seen at South Kensington during Monday night, October 3:—Time, 12h. 50m.; approximate path, from 310°, +35°, to 294°, +35°; size, about one-third diameter of moon; duration of flight, about three seconds; no trail; colour, whitish-yellow.

REDISCOVERY OF BROOKS'S PERIODICAL COMET (1889 V.), 1910*d*.—A telegram from the Kiel Centralstelle announces that Brooks's periodical comet was rediscovered by Messrs. Aitken and Wilson, at the Lick Observatory, on September 28. Its position at 9h. 17·3m. (Lick M.T.) was R.A. = 19h. 47m. 51s., dec. = 28° 8′ 39″ S., which agrees closely with the ephemeris by Prof. Bauschinger mentioned in these columns on September 8. This position lies in Sagittarius about 1½° south-west of the fifth-magnitude star ω, and is on the meridian about 7.30 p.m.

As the magnitude of the comet is given as 13·0, it is unlikely that observations in these latitudes will be possible for some time, but the southern declination is decreasing, and perihelion passage is not due until January 8, 1911. According to the ephemeris, the comet was at its nearest point to the earth early in August, and its present distance is about 155 million miles.

THE LUMINOSITY OF COMETS.—As a reprint from *Science*, N.S., vol. xxxii., No. 817, we have received a paper in which Mr. W. L. Dudley discusses the causes which produce luminosity in cometary bodies. According to the author's reasoning, luminous comets are simply masses of gaseous matter illuminated by the heavy electric discharges constantly flowing from the sun to each of the planets. The "ionic breeze" thus produced renders the comet luminous, and at the same time brushes matter along with it to form the tail. Should the comet get into such a position as to be under the influence of two planets (kathodes) at the same time, more than one tail is produced by the streams of ions from the sun (anode). The author also offers explanations on this hypothesis for the production of multiple heads, the change of direction which sometimes takes place in the tail, and the polarisation of cometary light.

COLOURED STARS BETWEEN THE POLE AND 60° N. DECLINATION.—In continuation of previous lists, Herr Krüger now publishes, in No. 4441 of the *Astronomische Nachrichten*, a list of coloured stars which lie between the North Pole and 60° N. declination. The list includes 191 stars, and for each object gives the catalogue numbers, the colour on the Potsdam scale, Herr Krüger's observed colour, on a scale where 0° = white, 10° = red, the magnitude and the spectral type. Ninety-three of the stars lie within the colour-limits WG–G–, and are of the second and third types.

OBSERVATIONS OF THE COMPANION OF SIRIUS.—The observations of the companion of Sirius, made with the 40-inch refractor by Prof. Barnard during November 30, 1909, to March 15, are recorded in No. 617 of the *Astronomical Journal*. The values for position-angle and distance, for the mean epoch (1910·106), are 89·09° and 9·07″ respectively.

THE PERSEID SHOWER, 1910.—In a note appearing in No. 617 of the *Astronomical Journal* Mr. E. F. Sawyer describes the meteor observations made by him at North Weymouth, Mass., on August 9, 11, 12, and 13. The hourly rates for one observer, covering one-sixth of the visible sky with the centre in Perseus, were 15, 15, 8, and 6 on these respective dates. The meteors generally were bright, and left streaks for one or two seconds; the radiant was found to be at 38·5°, +55·5°.

A MODIFIED METHOD FOR NADIR OBSERVATIONS.—In making the ordinary nadir observations, where a bright thread is made to coincide with its reflected image, there is always some little doubt as to when exact coincidence occurs. To obviate this difficulty, Mr. R. M. Stewart suggests a method where the reflected bright image is obliterated by a dark thread. Experiments made with the Ottawa meridian circle show that this method is simple, and gives greater accuracy than the older one. In making the observation, the bright reflected image and the illuminated thread are brought nearly to coincidence as usual, but the eye-piece, or attached plane mirror, is then slightly rotated so that the field becomes dark and the thread invisible, although the reflected image remains bright. A slight motion then causes the dark thread to occult the bright image, and it is stated that this operation can be performed with much greater certainty than can the bringing into exact coincidence two bright lines (Journal R.A.S. Canada, vol. iv., No. 4).

A NEW MICROMETER.—Dr. Doberck describes a new micrometer in No. 4432 of the *Astronomische Nachrichten* made to his specifications by Messrs. Cooke and Son. In this instrument the frame carrying the wires is the only readily movable part, so that there is practically no fear of dragging, and the whole is built very rigidly and strongly, thus obviating any likelihood of derangement.

The errors of the screw, as shown by an investigation involving 4000 settings, are extremely small, the corrections being considerably below the probable errors of setting on stars. The instrument is called the Elizabeth Thompson micrometer.

THE MEAN PARALLAX OF TENTH-MAGNITUDE STARS.—From a number of measures, made during 1907–9, of the Engelhardt companions of Bradley stars, Dr. H. E. Lau finds that the mean parallax of tenth-magnitude stars, taking Campbell's value for the sun's velocity, is 0·0010″; this is smaller than the value found by Comstock. The value derived by Kapteyn's method should be 0·0011″, so that it would appear that these faint stars are not abnormal in constitution, nor is there indicated any marked absorption in space (*Astronomische Nachrichten*, No. 4430).

HALLEY METEORS.

IN a communication to Dr. W. J. S. Lockyer, Prof. David Todd, of the Amherst College Observatory, writes as follows:—

"So well established is the Schiaparelli-Newton theory of the connection between comets and meteors, that it is highly important to obtain observations from every possible radiant, in order to ascertain whether there may be any that do not conform to the general law.

"Particularly is it desirable to observe the meteors of the Halley stream. Prof. W. H. Pickering directed attention to the possibility of observing such meteors early last May, when the earth was at its nearest, not to the comet, but to the comet's orbit. Many astronomers kept an outlook for these meteors; and on the night of May 5–6 I made a balloon ascension for this purpose, with a certainty of clear skies, as the spring season had been especially cloudy. However, hardly more meteors than usual on a May night were seen, and none of them appeared traceable to the Halley radiant. Indeed, I had very little expectation of seeing Halley meteors on that occasion, as the region of the cometary orbit then nearest to us was that through which the comet had passed seventy-five years ago.

"Quite different will be the conditions next month. On October 18 the earth is closest to that region of Halley's orbit where the comet passed on March 13–15, 1910, and from which, therefore, it may be expected that many meteors will enter the earth's atmosphere, as the comet has visited that region so recently. Very unfortunately, the moon fulls at just that time, so that all the faintest shooting stars would be lost.

"Probably it will be well to begin the watch as early as October 15, as evidently the meteoric matter in this case is widely scattered along the comet's path."

According to the *Daily Mail* of October 4, a very brilliant meteor is recorded to have been observed at Johannesburg on October 3. Perhaps this may be a forerunner of the swarm referred to by Prof. Todd. The account of this large meteor is as follows:—

"JOHANNESBURG, *Monday*.

"Johannesburg was startled at 8.50 this evening by the largest shooting star or meteor ever seen in the district.

"Its light was equal to that of a naval searchlight at fifty yards' range. The sky was illumined for three minutes, and the streets were as light as if it were day. Natives were terrified, believing that the end of the world was at hand.

"The astronomical observers at the local observatory were nearly blinded. The director of the observatory estimated that the meteor was only 150 miles from the earth. The head of the meteor, he says, was pear-shaped and half the size of the moon in diameter. The tail was straight at first, but afterwards turned towards the south."

THE AUTUMN MEETING OF THE IRON AND STEEL INSTITUTE.

WHEN Buxton was selected as the locality for the autumn meeting of the Iron and Steel Institute doubts were freely expressed as to the suitability of the choice. These doubts proved to be groundless, for the attendance of members was larger than usual, and visits to the Midland Railway works at Derby, the London and North-Western works at Crewe, and to the Staveley Iron Works afforded opportunities for instruction, while the beautiful weather conditions caused the Duke of Devonshire's garden-party at Chatsworth, and other excursions in Derbyshire, to be very successful and enjoyable functions.

From a scientific point of view, the paper which attracted the most attention was that on the theory of hardening carbon steels, by C. A. Edwards, of Manchester. As is well known, metallurgists have long been divided into two camps, the "carbonists" and the "allotropists," and at times much heat has been introduced into the discussions which have taken place. The position advocated by Mr. Edwards is to an extent an intermediate one, as it is based on the assumption of the existence of three allotropic forms of iron, known as α, β, and γ respectively. At the same time, the absolute necessity of carbon for true hardening is maintained. After a clear explanation of the elementary facts connected with the phase rule in its application to alloys in general, and particularly to the iron carbon series, the author concludes that the hardness of carbon steel is due to the retention, by quenching, of the solid solution of carbon, or carbide of iron, in γ iron, and that the β-iron theory, as applied to the explanation of the increased hardness of steel, is untenable. The solid solution of carbon or carbide in γ iron decomposes with slow rates of cooling, and some force must be applied to prevent inversion taking place. The force is mechanical, and is caused by the contraction of the outer shell. There is no constitutional difference between austenite and martensite, the apparent difference being due to the twinning of the γ solid solution as a result of the mechanical pressure.

In the discussion which followed the reading of this paper, Prof. Arnold warmly congratulated the author on his contribution, but contended that more facts were required before generalisations were accepted, and pointed out that the cooling curves as given by himself, and confirmed at Charlottenburg, did not agree with those published by Dr. Carpenter. The latter stated that the difference was not one of observation, but of methods of recording and of interpretation. Prof. Turner asked for evidence of twinning, and suggested that twinning in crystalline rocks or in the brasses was the result of annealing after pressure, but that in the hardening of steels there was no such annealing.

Two papers which also led to an interesting discussion were taken together, and dealt, *inter alia*, with the changes on the length of cast-iron bars when cooling in a sand mould. These papers were entitled "The influence of Silicon on the Properties of Pure Cast Iron," by A. Hague and Prof. T. Turner, and "Manganese in Cast Iron, and the Volume Changes during Cooling," by H. I. Coe. It was pointed out that in Prof. Turner's original papers on silicon in cast iron, published in 1885, the materials used were relatively impure, and though the results have been confirmed by very extended practical application, it was thought well to start with the purest available materials and to observe the temperature and contraction changes and the microstructure, which had not been examined in the earlier tests. White iron, when free from elements other than carbon, shows only two slight arrests in the rate of contraction, and these correspond with the eutectic and the pearlite points respectively. On adding silicon or manganese, the iron, though still white, expands during and immediately after solidification. With more silicon the carbon is thrown out of solution, and a marked additional expansion occurs. Though much manganese tends to make iron white, about 0·5 per cent. of manganese, in presence of silicon, produces more secondary graphite, and thus lowers the combined carbon. In the manganese series of white iron the expansions form a regular curve with the percentages of manganese, and minima are found corresponding with the existence of four definite carbides. In the grey-iron series the expansions were relatively large, and the pearlite point disappeared suddenly with about 3·5 per cent. of manganese.

In the discussion references were made to the great detail involved in such an inquiry, and to the need of further work and generalisation. In a paper by S. Hilpert and E. Colver-Glauert, sulphurous acid was recommended as an etching agent for metallographic work. The acid is used as a saturated solution of sulphurous acid in water. It should be free from sulphuric acid, and should be diluted with water to about 3 or 4 per cent. of such acid. The time taken is said to vary from seven seconds to one minute. S. Hilpert, of Berlin, submitted a useful note on the preparation of magnetic oxides of iron from aqueous solutions, and stated that the production of Fe_3O_4 from aqueous solution is only possible through the precipitated FeO dissolving in the ammoniacal residue. The true magnetic oxide is Fe_2O_3 in the form of ferric ferrate, and the magnetic properties of Fe_3O_4 have their source in the acid properties of Fe_2O_3.

The remaining papers of a varied programme dealt with briquetting iron ores, electric power and electric steel refining, the Hanyang iron works in China, the production of rolled **H** beams, and experiments on fatigue in metals.

THE GEOLOGICAL CONGRESS AT STOCKHOLM.

THE eleventh International Geological Congress in Stockholm from August 17–25 has been generally pronounced by the members to have been one of the most successful yet held. There was an attendance of about 900, including representatives of all European countries except Portugal, of Australia, China, and Japan, and a distinguished contingent from America. The excursions at this congress have been unusually various and instructive, and they were heartily enjoyed, thanks to their skilful organisation and management. Before the meeting there were excursions to Spitsbergen, Lapland, and central Sweden; during it to the Archæan areas and glacial deposits around Stockholm and Upsala, and to the classical Silurian sections at Gothland; and after it to the chief iron fields and areas of geological interest in southern Sweden. The library of guide-books issued for the excursions forms an invaluable summary of the field geology of Sweden. The Swedes as a people are characterised by the thoroughness of their work and the charm of their manners; the foreign visitors return impressed by the excellence of Swedish contributions to geology and with pleasant memories of the hospitable reception from all classes, from the gracious courtesy with which the King and Queen received us in the palace to the smiling welcome of the peasants in the field.

So much work was done at the congress that no adequate account of it can be given in a short notice. Five sections and various commissions and committees met simultaneously.[1] The discussions were sometimes not influential, for they often followed the reading of several disconnected papers, and many of the speeches were rather further contributions to the subject-matter than discussions of the papers that had been submitted.

The first formal meeting of the congress was held on August 18, when the honorary president, H.R.H. Prince Gustave Adolphe, welcomed the congress in a graceful speech, referring to the dependence of mining on geology and the increasing importance of science now that it is devoting more attention to practical questions. The King of Sweden then declared the congress open. Prof. de Geer was installed as president, and gave a lecture on "The Geochronology of the last 12,000 Years." He remarked the complete failure of previous attempts to measure geological time in years, and described his determination of the length of post-glacial time in the Stockholm district. He noticed there that the marine post-glacial clays

[1] For notes on some of the meetings the writer is indebted to Prof. Hobbs, Prof. Cole, and Dr. J. W. Evans.

had been deposited in regular layers, which differ in colour and composition; the same succession is repeated time after time, with layers of varying thickness. He early suspected that each cycle in this series might represent one year's deposition, the layers laid down in the summer being thicker, as the floods then carried most mineral matter to the sea, and being brown owing to oxidisation, the autumnal layers being thinner and also blacker, owing to the higher percentage of organic matter. Near Stockholm there are many small linear moraines, each line of which he thought might be the terminal moraine deposited in one year. After thirty years' work he has discovered, by the correlation of the evidence of the seasonally banded clays, of the northward advance of their successive layers as they followed the receding ice front, of the annual moraines, and of the annual delta deposits laid down at the mouths of glacial rivers, that the retreat of the ice from Scandinavia was more rapid and less ancient than had been thought. The ice covered the site of Stockholm only a few thousand years ago, and withdrew at the rate of 200 metres a year. The latter part of the lecture was abridged, but Prof. de Geer announced that at Ragunda he had found a section which showed the full sequence of clays, from a layer formed in 1796, through a succession of lake clays and a fjord clay, to the seasonally banded clays deposited along the front of the receding ice-sheet; and, according to his determination, the ice receded from Ragunda only 7000 years ago. Prof. de Geer concluded his lecture with expressions of hope that the application of his method would allow of positive proof whether the glaciations of Scandinavia, the British Isles, and North America were synchronous.

Prof. Van Hise then delivered a lecture on "The Influence of Applied Geology and the Mining Industry upon the Economic Development of the World." He confined his lecture to the conservation of natural resources, and considered mainly the cases of iron and coal, as the failure of other metals would involve only minor readjustments. The working of coal and iron on a large scale introduced the industrial revolution of the nineteenth century, and gave commercial supremacy to countries endowed with both minerals. A civilisation can exist without iron; but a man with a wooden plough could till only one-tenth as much as with an iron plough, and hence the exhaustion of iron would mean that countries would support much smaller populations. The supply of high-grade iron ores will not last long in the chief iron-producing countries, but the quantities of low-grade ores are so immense that the total failure of iron ores is practically out of the question. Moreover, much of the iron extracted is available for all time. The coal question is more serious, as fuel when burnt is gone for ever, and the supply is so limited that it cannot last indefinitely. At the present rates of consumption, the coal in Britain and Germany may last from 500 to 1000 years, and the United States has sufficient for 6000 years; but if the consumption continues to increase at its recent rate all the coal in seams that can be worked under existing conditions will be used in 150 years; within 100 years rising prices will force men to turn to other sources of power—natural gas, petroleum, tides, and the sea; these, though all possible sources of power, are too expensive. The only perennial source of cheap power is water. The industrial future lies with countries rich in iron ores and water-power. Scandinavia has both, and it is especially favoured, as its recent glaciation has left so many lake basins, which provide easy water storage and the uniform discharge most suitable for the production of power.

Prof. van Hise made an earnest appeal to men of science to ask how long our natural resources can last, and to protest against needless waste. Primitive man and any philosopher at the beginning of the nineteenth century would have felt confident that natural resources would last indefinitely. But it is now manifest that new principles must apply to the conservation of our mineral supplies, and it is our manifest duty to leave our descendants a fair share, so that they may enjoy the comfort and leisure necessary for the intellectual development by which they can attain the godlike destiny of man.

Iron Ore Supplies.

The question of the iron ore supplies of the world was subsequently considered in a conference opened by the Prime Minister of Sweden, M. Lindman, who declared the conservation of iron ores to be more necessary than of coal, as water supply offers a permanent source of power and heat. He stated the measures adopted by Sweden to limit the export of its high-grade ores; they appear to amount to the future nationalisation of the chief iron mines.

Prof. Sjögren regarded the iron ore reserves as practically inexhaustible, and he added some fresh data to those announced in the report on the iron ore reserves of the world. Estimates received from Mr. Inouye, of Japan, show that the reports as to the unlimited iron ores in China are without adequate foundation. According to Prof. Sjögren, the best idea as to the amount of ore available in the less known regions of the world can be learnt by multiplying the area by a factor obtained by dividing the ore reserves, actual and potential, of Europe, the United States, and Japan, by their total area. On that assumption the ore supply available is 425,000 million tons.

Prof. Beyschlag defended the estimates of German ore supplies prepared for the congress from some recent criticisms, and proposed a commission to secure official evidence as to the ore reserves of the United States and the chief iron-producing countries.

M. de Launay, on the other hand, issued a warning against a serious possible source of loss, which is often disregarded by the advocates of conservation. There are in Europe vast quantities of low-grade ores, distant from supplies of fuel or power, that could not be worked in competition with the high-grade ores of many countries not yet iron-producing. If the European low-grade ores are not used now, in fifty years' time they will probably be useless. M. de Launay claimed, therefore, that under such conditions the sound policy is to accelerate by all means the production of these ores.

Prof. J. F. Kemp also repudiated the fears of an iron famine. He predicted a diminished demand on iron ores, as we are now passing from the age of steel to the age of cement, and also further discoveries of ores, such as that in Cuba, which will probably lead to the establishment of large iron works on the Atlantic coast of America. He insisted that the critical point with iron is not the supply of ore, but the exhaustion of the coking coals. Even if all the heat be supplied by electricity, half a ton of coking coal will still be required for the reduction of a ton of ore.

The only speaker in the discussion, Prof. J. W. Richards, of Lehigh, also agreed that the danger is with the coking coal, and he suggested a commission on the supplies of this material.

Glacial Erosion.

The first sectional discussion was on glacial erosion, under the chairmanship of M. de Margerie. Papers were contributed by Profs. Högbom, Penck, Davis and Reusch, and Dr. Nordenskjold, and in the discussion speeches were made by Profs. Wahnschaffe, Baltzer, Heim and Salomon, Dr. G. F. Becker, and Dr. Sederholm. Prof. Penck, in an eloquent summary of his paper, explained the evidence which has led him to attribute the main work in the formation of Alpine valleys to the action of ice. Prof. Davis insisted on the importance of the physiographic study of the question, and the comparison of never glaciated mountains, taken as the "norm" of mountain form, with those that have been glaciated; he advocated the formation of cirques by the "plucking" away of the rocks at the head of a valley, until the whole mountain ridge at the head of the valley may be torn away. Prof. Högbom, while advocating the erosive power of ice, remarked the difficulty of explaining some Swedish valleys that had been filled with ice, which had not removed their soft, pre-glacial deposits. Prof. Wahnschaffe referred to cases where ice had covered soft deposits, and had not even shifted boulders lying on them.

Prof. Reusch described the glaciated valleys near Christiania, which he thought were pre-glacial, and contrasted the effect of low-level ice in deepening and moulding valleys with the planing effect of high-level ice. The powerful influence of pre-glacial structures in determining the course and character of ice-worn valleys was also maintained by Dr. Becker, who attributed the Yosemite and other valleys in the Sierra Nevada to the existence of a vast system of joints, the decomposed rocks along which have been removed by ice. Dr. Nordenskjold insisted that long straight valleys like fjords can only be due to ice erosion.

The adjourned discussion, with Prof. Wahnschaffe in the chair, was opened by Prof. Salomon, of Heidelberg, who remarked that erosion must take place where a glacier presses firmly on the ground; but we must wait for the ice to withdraw before we can study its effects, just as we have to wait for the dissection of a volcano before we can see what has been going on in the depths. He had seen cause to change his mind, and to accept the potency of glaciers as eroding agents, especially where the rock-structures lend themselves to "plucking." Joints in igneous rocks are not always evenly distributed, and thus one part of the same mass may show erosion while another resists. The suggestion of the action of freezing water in the rock-joints under a glacier deserves full consideration. Dr. von Dechy, from his studies in the Caucasus, urged that much seeming erosion was due to the clearing out of previously filled valley floors and of lake basins by glaciers, and by catastrophic glacier-slides. Prof. Wahnschaffe remarked that the Caucasian area was not comparable with that north of the Alps, since no great Piedmont glacier had formed north of the Caucasus. Prof. Heim, in a vigorous speech, said the rock-surfaces were palimpsests of river action and glacier action, and the work of each was thus obscured. While stream action concentrates itself in a portion of the valley floor, a glacier spreads too widely to compare with it in erosive power. So-called "plucked" masses had often merely fallen from above on to the ice, and had come out below. Glaciers have overriden Alpine landslides, but even then without carrying many blocks away. The broad, rounded form of glacier valley floors may even be due to the wandering of a previous stream from side to side within its valley walls. Then comes a glacier, and gives a final touch to the form, overriding the taluses of a previous age at either side.

Prof. Högbom, of Upsala, regarded the great chalk masses, said to have been moved in northern Germany and elsewhere by glacial erosion, as having been prepared by fractures. He compared a great glacier to the overthrust mass in mountain-building, and the ground moraine to the breccia along the thrust plane. Erosion must be greatest under the vertical nose of an advancing glacier, and not much under the glacier as a whole. Prof. E. Stolley, of Brunswick, said the German chalk masses represented genuine plucking and pushing forward. Lakes due to glacial erosion occur even in the North German plain. Prof. Reusch confessed, like Salomon, to having changed his mind. He answered an objection in Heim's speech by showing how a glacier must leave some upstanding masses in its floor, and cannot be expected to plane all equally away. Prof. Penck, on closing the discussion, accepted excavating action of subglacial streams, especially along valley sides, and urged that the only differences between Prof. Heim and himself were now really quantitative.

The Pre-Cambrian Fauna.

The discussion on the sudden appearance of the varied Cambrian fauna showed the firm belief in the evidence of pre-Cambrian life as contended by Prof. Barrois from the graphite of Brittany, by Dr. Sederholm from traces of pre-Cambrian fossils in Finland, and by Prof. Rothpletz from the oolitic pebbles and organic traces in the pre-Cambrian conglomerates of Sweden. The discussion showed a general agreement as to the influence of the absence of carnivorous organisms from the pre-Cambrian seas. Thus, according to Dr. J. W. Evans, creatures then had no need of defensive structures, and according to Dr. R. A. Daly there was, for the same reason, an accumulation of decomposing organic matter in the early seas, and the resultant ammonium carbonate led to the precipitation of the pre-Cambrian limestones; Profs. Sollas and Steinmann both thought that the early organisms had no hard parts, which developed as the organisms became more complex. Prof. Walther suggested that the pre-Cambrian sea consisted of isolated basins, the waters of which differed in chemical composition, and that organisms living in water rich in silica secreted siliceous skeletons, those in water rich in carbonate of lime formed calcareous shells; the phosphatic skeletons of trilobites and some brachiopods were due to life in a sea rich in phosphate, and chitinous shells were developed in fresh-water basins.

In the section on general and regional geology, Dr. Evans exhibited an elaborate and ingenious model to illustrate the movements along the line of the San Andreas fault during the recent Californian earthquake. It is constructed of two sets of flexible wooden strips held together by strings at their common edge; the one part was subjected to slow lateral stresses, and suddenly released from the strain by cutting the strings. Vibrations were thus set up, the amplitudes of which were greatest at the adjacent edges, and a musical note was produced through the friction of metal attachments. Dr. Evans believed the earthquake stresses to be of slow accumulation, the larger vibratory movements after release causing the sensible shocks, the frictional small tremors the sounds.

Prof. Hobbs gave a lecture on "The Fracture Systems of the Earth's Crust," and urged their international investigation, owing to their importance in relation to land sculpture, the course of rivers, the discovery of obscure faults, and earthquake disturbances. Prof. H. F. Reid discussed the results of a recent paper on the California earthquake, and exhibited a model similar in principle, but not in construction, to that of Dr. Evans. Dissent from his views as to the cause of the earthquake was expressed by Prof. Rothpletz, Dr. Oldham, and the chairman (Prof. Hobbs).

An important paper by Prof. Tarr on the advance of glaciers in Alaska as a result of earthquake shaking indicated how the sudden advance and equally sudden subsequent stagnation of many Alaskan glaciers might be accounted for by masses of snow being shaken from the névé regions during the heavy earthquake of 1899. In discussing this paper, Prof. Frech showed how the explanation offered would account for the hitherto inexplicable sudden advances of the glaciers of the Alps.

Dr. H. Stille described the earth movements in the later rocks of north-western Germany, and showed the influence of the Palæozoic areas of the Rhine and the Harz.

Pre-Cambrian Geology.

The petrographic section met on Saturday, in the morning under President van Hise, to consider the principles of pre-Cambrian geology and the cause of regional metamorphism, and in the afternoon under M. Barrois, to discuss pre-Cambrian stratigraphic classification. There were fourteen papers and many speeches. The general result of the morning's discussion was summarised at the close by Prof. Cole as showing the great advance in recent years of the views of Michel-Levy and Barrois as to the formation of crystalline schists by intense granitic injections, which in recent years had been supported by Sederholm in Finland and his own work in Donegal. These views were clearly expressed by a statement of the evidence from Brittany by Prof. Barrois. Prof. Adams opened the discussion by an account of the constant association with the crystalline schists of vast granitic batholites, to which he attributed the metamorphosis. Dr. Sederholm exhibited a map of a Finnish islet on the scale of one-twentieth of natural size, and he described the granitisation of the pre-Cambrian sediments by injection with granite when the adjacent rocks were half melted and plastic. On the other hand, attention was directed to intrusive gneisses elsewhere which had a less metamorphic effect. Thus Prof. U. Grubenmann, of Zurich, contrasted the actions of the gneisses of Scandinavia and Finland with those of the Alps, which had done less in melting the adjacent rocks, but had a greater pneumatolitic effect.

Prof. Coleman described the alteration of conglomerates at Sudbury, Ontario, into rocks that had been mapped as

Laurentian gneisses, and contrasted the slight metamorphism of the Lower Huronian conglomerates of Cobalt with their alteration into gneiss at Michipicoten by infolding with Keewatin batholites.

Dr. Lane stated the three possible sources of the gneissic rocks known as the Laurentian, and from a comparison of the size of their constituents with those of the adjacent rocks concluded that the Laurentians must be due to the ascent of deep-seated fluid material.

In the afternoon meeting various subdivisions of the pre-Cambrian rocks were advocated. Mr. W. G. Miller explained the classification used by the Geological Survey of Ontario, which adopts three main divisions: the Keweenawan for the upper sandstones, the Huronian for the underlying schists, quartzites, &c., and the Laurentian-Keewatin for the basal complex. Prof. Coleman objected to the retention of Laurentian except as a temporary convenience, since the Laurentian are intrusive rocks of various ages. Dr. Sederholm explained the classification he had adopted for Finland and Scandinavia, where the pre-Cambrian system is broken up by great unconformities into divisions, each of which he thought from its thickness must correspond to the groups, and not to the systems, of the post-Cambrian rocks. He objected to the terms previously used, and proposed to call the pre-Cambrian rocks the Progonozoic, and to divide them into three divisions, the Archeo-, Meso-, and Næo-progonozoic.

Another case of supposed Palæozoic schists proving to be pre-Cambrian was described by Prof. J. F. Kemp from evidence displayed during recent work for the New York water supply.

President van Hise supported the threefold division of the pre-Cambrians, and Mr. Fermor the twofold division found more convenient in India, and referred to Sir Thomas Holland's term Purana for the non-foliated pre-Cambrian sediments. Miss Raisin directed attention to the analogous case in the English Midlands, and to Lapworth's term Uriconian for the comparatively unaltered pre-Cambrian volcanic series.

The petrographic section, under the presidency of Dr. Teall, devoted a morning to discussion of the principles of rock classification. Prof. Adams exhibited photographs of the structures he had produced in rocks, including the formation of flaser gabbro or augen gneiss by pressure at temperatures of 450° F. No fresh minerals were produced, but by mechanical movements the material of a massive diabase was rearranged as a gneiss.

Prof. Vogt urged the claim of eutectics as a factor in rock classification. Dr. A. L. Day explained the aims and methods of the researches on mineral formation and stability conducted in the Carnegie Institute, and expressed confidence that their methods could in time be applied to even such complex mixtures as ordinary rocks.

Dr. Whitman Cross defended the quantitative system of rock classification from recent criticisms, and said that the other systems were only less arbitrary in the degree that they were less definite. He referred to Becke's petrographic types—the Atlantic and Pacific—as based on distinctions that could not be sharply defined. Dr. Evans repeated his criticisms on the quantitative system, and the general discussion was continued by Dr. Benett, Prof. Kœnigsberger, and Prof. Tschirwinsky.

Meetings of the other sections were devoted to tectonic geology, especially of Switzerland, to the causes of the Ice age, to polar geology, applied geology, stratigraphy, and palæontology.

At the final meeting it was decided that the next meeting, in 1913, should be in Canada, and the hope expressed that the meeting in 1916 should be in Belgium.

THE THOMAS YOUNG ORATION.

PROF. R. W. WOOD, in delivering the Thomas Young Oration at the Optical Society on Thursday, September 29, described some apparatus with which he has been experimenting recently. The first of these, which he calls the echelette grating, is an instrument occupying a position between the echelon and the ordinary diffraction grating. It is a grating ruled with a crystal of carborundum on gold deposited on copper; the carborundum has the advantage over a diamond point of having perfectly straight sides meeting at an angle of 120°. The spacing is about ten times as coarse as usual. No metal is removed in ruling, but the gold is compressed so as to form ridges and hollows. The sides of these ridges are highly polished and almost optically flat. Such a grating may have various faults, such as having a flat or irregular top to the ridges, or the sides of one groove may be deformed in ruling the next; tests to determine whether the grating is free from faults were described.

A variety of gratings is obtained by altering the position of the crystal in ruling; thus some gratings have their two sides equally inclined to the surface of the plate, and in others there are inequalities in the inclinations of various magnitudes. The gratings thus obtained, with a known form of groove, have been used to determine the causes which throw the greater part of the light of a definite wave-length into one particular spectrum. These gratings bear the same relation to heat waves that the ordinary diffraction grating bears to light waves; thus they are specially suitable for use in investigations into radiant energy, being many times more efficient than prisms of rock salt. Diagrams were shown which demonstrated the greater resolving power of the grating compared with the rock-salt prism. A number of gratings were exhibited, and some of the tests for detecting faults were shown. A demonstration was also given of the ability of these gratings to concentrate the light of a definite colour into a particular spectrum.

Prof. Wood next described his mercury telescope, in which the mirror is a vessel containing mercury, the surface of which is made to assume a steady parabolic form by rotation under gravity. The practical difficulties to be overcome in preventing ripples on the mercury surface due to vibration or to a very slight obliquity in the axis of rotation were described. The mercury vessel is mounted on an axis with two conical bearings, and the whole mount is placed on a stand with levelling screws. To avoid the excessive friction due to conical bearings, the greater part of the weight is taken by a steel ball under the centre of the objective. A magnetic drive was first attempted, but was abandoned in favour of a mechanical connection consisting of half a dozen fine threads of pure elastic, thus any vibrations in the motor are absorbed by the elastic threads. Some star trails taken with the instrument in and out of adjustment were described.

Finally, some photographs of landscapes taken with infra-red light were shown.

THE POLAR ESKIMOS.[1]

ANTHROPOLOGISTS are now beginning to realise the necessity of supplementing the methods of a general ethnographic survey by a more intensive study of smaller groups within limited areas. A good example of this class of investigation is provided by the account of the Polar Eskimos by Dr. H. P. Steensby, who was a member of the expedition commissioned by the Danish Missionary Society in 1909 to establish a station in Greenland.

The tribe known as the Polar Eskimos occupies the west side of the Hayes Peninsula, extending from north-west Greenland towards the west between the Kane Basin in the north and the Melville Sound in the south. At present they number about two hundred souls. Compared with the people of the more southerly west Greenland, they appear to be a different race, the Mongolian type prominent in the latter region being here replaced by that called by Dr. Steensby the Indian. The so-called Mongolian racial characters, the low nose, oblique eyes, flat face, broad and large cheek-bones, are more prominent in the women than in the men. The skull is of the dolichocephalic class. The skin has always a yellowish ground-colour, and the so-called "Mongolian spot" is present in the sacro-lumbar region of children.

Much of the existing culture of the tribe seems to be due to the emigration of a body of their kinsmen from the coasts of North Devon and Ellesmere Land in the early 'sixties, and they present the almost unique condi-

[1] "Contributions to the Ethnology and Anthropogeography of the Polar Eskimos." By Dr. H. P. Steensby. Pp. 253—406. (Copenhagen: Bianco Luno, 1910.)

tion that during the comparatively short period since they came under European observation they have risen from practically the lowest to a comparatively high stage of culture. Kane, who in the early 'fifties first described them, found that they possessed little iron or wood, using sledge-runners of bone and pieces of barrel-hoops as knives. They did not hunt the reindeer, and were ignorant of the use of the bow and arrow; they could not catch salmon, and did not use the kayak. These cultural deficiencies were certainly survivals of their primitive social condition. During the 'sixties, however, they learned from emigrants from the American side of Smith's Sound the art of reindeer hunting, the use of the bow and arrow, skill in salmon catching, and the mode of building kayaks and hunting from them. The leader of this party of foreigners, Kridlarssuark, has now become the legendary culture hero. Finally, in 1891, Peary began his intercourse with them, which enabled them to obtain in exchange for their fox and bear skins the finest American weapons, with the result that the rapid destruction of game will probably soon destroy their main source of livelihood. Even up to the time of Peary's first visit stone knives and axes were in use, and they used to make rude implements with cutting edges of meteoric iron, the source of which was discovered by Peary during a later expedition in 1894. Even now they make their harpoon points of iron with a head-piece of bone, and they work iron with much skill with the files they used for the older material.

A similar course of evolution may be traced in the construction of their houses. In their original home they must have used whale-ribs for the support of the roof. Wood of sufficient span being now not procurable, they have, while retaining the primitive plan, adopted a new device for supporting the roof, planned on the model of the cantilever.

With this modern and fairly advanced culture the Polar Eskimo combines many savage characteristics. He is, says Dr. Steensby, "a confirmed egoist, who knows nothing of disinterestedness. Towards his enemies he is crafty and deceitful; he does not attack them openly, but indulges in back-biting; he will not meet his deadly enemy face to face, but will shoot or harpoon him from behind." They practise a rude form of justice. One man, because he was a notorious liar, was summarily killed by two chiefs, one of whom annexed the wife of the deceased.

We have said enough to show the interest and value of this account of a little known tribe. It is illustrated by characteristic sketches, the work of an Eskimo woman, which in style closely resemble the Bushman drawings recently published under the editorship of Mr. H. Balfour.

THE BRITISH ASSOCIATION AT SHEFFIELD.

SECTION I.

PHYSIOLOGY.

OPENING ADDRESS BY PROF. A. B. MACALLUM, M.A., M.B., PH.D., SC.D., LL.D., F.R.S., PRESIDENT OF THE SECTION.

THE record of investigation of the phenomena of the life of animal and vegetable cells for the last eighty years constitutes a body of knowledge which is of imposing magnitude and of surpassing interest to all who are concerned in the studies that bear on the organic world. The results won during that period will always constitute, as they do now, a worthy memorial of the intense enthusiasm of the scientific spirit which has been a distinguishing feature of the last six decades of the nineteenth century. We are to-day, in consequence of that activity, at a point of view the attainment of which could not have been predicted half a century ago.

This body of knowledge, this lore which we call cytology, is still with all this achievement in one respect an undeveloped science. It is chiefly—nay, almost wholly—concerned with the structural or morphological side of the cell, while of the functional phenomena our knowledge is only of the most general kind, and the reason is not far to seek. What little we know of the physiological side of the cell—as, for example, of cellular secretion, absorption, and nutrition—has only to a very limited extent been the outcome of observations directed to that end. It is in very great part the result of all the inferences and generalisations drawn from the data of morphological research. This knowledge is not the less valuable or the less certain because it has been so won, but simply because of its source and of the method by which we have gained it; it is of a fragmentary character, and therefore less satisfactory in our estimation.

This state of our knowledge has affected—or, to express it more explicitly, has fashioned—our concept of living matter. When we think of the cell it is idealised as a morphological element only. The functional aspect is not ignored; but we know very little about it, and we veil our ignorance by classing its manifestations as vital phenomena.

It is true that in the last twenty years, and more particularly in the last ten, we have gathered something from biochemical research. We know much concerning ferment or catalytic action, of the physical characters of colloids, of the constitution of proteins, and their synthesis in the laboratory promises to be an achievement of the near future. We are also in a position to understand a little more clearly what happens in proteins when, on decomposition in the cell, they yield the waste products, urea, and other metabolites, with carbon dioxide and water. Further, fats can be formed in the laboratory from glycerine and fatty acids, a large number of which have also been synthesised, and a very large majority of the sugars of the aldohexose type have been built up from simpler compounds. These facts indicate that some of the results of the activity of animal and vegetable cells may be paralleled in the laboratory, but that is as far as the resemblance extends. The methods of the laboratory are not as yet those of nature. In the formation of carbohydrates, for example, the chlorophyll-holding cell makes use of processes of the most speedy and effective character, but nothing of these is known to us except that they are quite unlike the processes the laboratory employs in the artificial synthesis of carbohydrates. Nature works unerringly, unfalteringly, with an amazing economy of material and energy, while "our laboratory syntheses are but roundabout ways to the waste sink."

In consequence, it is customary to regard living matter as unique—*sui generis*, as it were, without an analogue or parallel in the inorganic world—and the secrets involved in its actions and activities as insoluble enigmas. Impelled by this view, there are those, also, who postulate as an explanation for all these manifestations the intervention in so-called living matter of a force otherwise and elsewhere unknown, biotic or vital, the action of which is directed, according to the character of the structure through which it operates, to the production of the phenomena in question. Living protoplasm is, in this view, but a mask and a medium for action of the unknown force.

This is an old doctrine, but it has again made headway in recent years owing to the reaction from the enthusiasm which came from the belief that the application of the known laws of physics and chemistry in the study of living matter would explain all its mysteries. A quarter of a century ago hopes were high that the solution of these problems would soon be found in a more profound comprehension of the laws of the physical world. Since then there has been an extraordinary increase in our knowledge of the structure and of the products of the activity of living matter without a corresponding increase in knowledge of the processes involved. The obscurity still involving the latter appears all the greater because of the high lights thrown on the former. Despair, in consequence, has taken the place of hope with some, and the action of a mysterious force is invoked to explain a mystery.

It may be admitted that our methods of investigation are very inadequate, and that our knowledge of the laws of matter, seemingly comprehensive, is not at present profound enough to enable us to solve all the problems involved in the vital phenomena. The greatest factor in the difficulty of their solution, however, has been the fact that there has been a great lack of investigators specially trained, not only in biology, but also in physics and chemistry, for the very purpose of attacking intelligently such problems. The biologists, for want of such a wide training, have emphasised the morphological aspect and the readily observable phenomena of living matter; while the physicist and chemist, knowing little of the morphology of the cell and of its vital manifestations, have been unable to apply satisfactorily the principles of their sciences to an understanding of its processes. The high degree of specialism which certain departments of biology has in recent years developed has made that difficulty greater than it was.

It must also be said that in some instances in which the physicist and chemist attempted to aid in the solution of biological problems the result, on the whole, has not been quite satisfactory. In, for example, the phenomena of osmosis, the application of Arrhenius's theory of ionisation and van 't Hoff's gas theory of solutions promised at first to explain all the processes and the results of diffusion through animal membranes. These theories were supported by such an array of facts from the side of physics and physical chemistry that there appeared to be no question whatever regarding their universal validity, and their application in the study of biological phenomena was urged with acclaim by physical chemists and eagerly welcomed by physiologists. The result in all cases was not what was expected. Diffusion of solutes, according to the theories, should, if the membrane is permeable to them, always be from the fluid where their concentration is high to that in which it is low. This appears to happen in a number of instances in the case of living membranes—or, at least, we may assume that it occurs—but in one signal instance, at least, the very reverse normally obtains. In the kidney, membranes formed of cells constituting the lining of the glomeruli and the renal tubules separate the urine, as it is being formed, from the blood plasma and the lymph circulating through the kidney. Though the excreted fluid is derived from the plasma and lymph, it is usually of much greater osmotic concentration than the latter.

It may be urged that this and other discrepancies are explained by the distribution (or partition) coefficient of the solutes responsible for the greater concentration of the product of excretion, these solutes being more soluble in the excreted medium than in the blood plasma, and distributing or diffusing themselves accordingly. If such a principle is applicable here as an explanation, it may be quite as much so in other physiological cases in which the results are supposedly due only to the forces postulated in the theories of van 't Hoff and Arrhenius. Whether this be so or not, the central fact remains that the enthusiastic hopes with which the theories were applied by physiologists and biologists in the explanation of certain vital phenomena have not been wholly realised.

The result has been a reaction amongst physiologists and biologists which has not been the least contributory of all the causes that have led to the present revival of vitalism.

Another difficulty in accounting for the vital phenomena has been due, until recently, to a lack of knowledge of the physical and chemical properties of colloids and colloidal "solutions." The importance of this knowledge consists in the fact that protoplasm, "the physical basis" of life, consists mainly of colloids and water. Until eleven years ago, what was known regarding colloids was derived chiefly from the researches of Graham (1851–62), Ljubavin (1889), Barus and Schneider (1891), and Linder and Picton (1892–7), who were the pioneers in this line. In 1899 were published the observations of Hardy, through whose investigations very great progress in our knowledge of colloids was made. In 1903 came the invention of the ultramicroscope by Siedentopf and Zsigmondy, by which the suspension character of colloid material in its so-called "solutions" was visually demonstrated. During the last seven years a host of workers have by their investigations greatly extended our knowledge of the physical and chemical properties of colloids, and now the science of Collochemistry bids fair, the more it develops, to play a very important part in all studies bearing on the constitution and properties of living matter.

Then, also, there are the phenomena of surface tension. This force, the nature of which was first indicated by Segner in 1751, and described with more detail by Young in 1804 and Laplace in 1806 in the expositions of their theories of capillarity, was first in 1869 only casually suggested as a factor in vital processes by Engelmann. Since the latter date and until 1892, when Bütschli published his observations on protoplasmic movement, no serious effort was made to utilise the principle of this force in the explanation of vital phenomena. Even to-day, when we know more of the laws of surface tension, it is only introduced as an incidental factor in speculations regarding the origin of protoplasmic movement and muscular contraction, and yet it is, as I shall maintain later on in this address, the most powerful, the most important of all the forces concerned in the life of animal and vegetable cells.

It may be gathered from all that I have advanced here that the chief defect in biological research has been, and is, the failure to apply thoroughly the laws of the physical world in the explanation of vital phenomena. Because of this too much emphasis is placed on the division that is made between the biological and the physical sciences. This division is very largely an artificial one, and it will in all probability be maintained eventually only as a convenience in the classification of the sciences. The biologist and physiologist have to deal with problems in which a wide range of knowledge is necessary for their adequate treatment; and, if the individual investigator has not a very extensive training in the physical sciences, it is impossible for him to have at his command all the facts bearing on the subject of his research, unless the problem involved be a very narrow one. The lack of this wide knowledge of the physical sciences tends to specialism, and, as the specialism is ever growing, it will produce a serious situation eventually, for it will develop a condition in the scientific world in which coordination of effort and a broad outlook will be much more difficult than is the case now.

This growing defect in the biological sciences can only be lessened by the insistence of those in charge of advanced courses in biological and physiological laboratories that only those whose training is of a very wide character should be allowed to take up research. It is, perhaps, futile to expect that such a rule will ever be enforced, for in the keen competition between universities for young teachers who have made some reputation for original investigation there may not be too close a scrutiny of the qualifications of those who offer themselves for post-graduate courses. There is, further, the difficulty that the heads of scientific departments are not desirous of limiting the output of new knowledge from their laboratories by insisting on the wider training for the men of science who are in the process of developing as students of research.

It is perhaps true, also, that there still remains a great deal unobserved or unrecorded in the fields of biology, physiology, and biochemistry, in the investigation of all of which a broad training is not specially required to give good service; and that, further, this condition will obtain for one or two decades still. It is quite as certain, however, that the returns from such service will tend to diminish in number and value, and, if the coming generation of workers is not recruited from a systematically and broadly trained class of students, a period of comparative sterility may supervene.

As it is to-day, there are few who devote themselves to the direct study of the chemical and physical properties of the cell, the fundamental unit of living matter. There are, of course, many who are concerned with the morphology of the cell, and who employ in their studies the methods of hardening and staining which have been of very great service in revealing the structural as well as the superficial chemical properties of the cell. On the facts so gained views are based which deal with the chemistry of the cell, and which are more or less widely accepted, but the results and generalisations drawn from them give us but little insight into the chemical constitution of the cell. We recognise in the morphologists' chromatin a substance which has only in a most general way an individuality, while the inclusions in the nucleus and the cytoplasm, on the distinction by staining of which great emphasis is laid, can only in a most superficial way be classified chemically.

The results of digestion experiments on the cell structures are also open to objection. The action of pepsin and hydrochloric acid must depend very largely on the accessibility of the material the character of which is to be determined. If there are membranes protecting cellular elements, pepsin, which is a colloid, if it diffuses at all, must in some cases, at least, penetrate them with difficulty. In Spirogyra, for example, the external membrane, formed of a thick layer of cellulose, is impermeable to pepsin, but not to the acid, and, in consequence, the changes which occur in it during peptic digestion are due to the acid alone. Even in the cell the periphery of which is not protected by a membrane, the insoluble colloid material at the surface serves as a barrier to the free entrance of the pepsin. It is, however, more particularly in the action on the nucleus and its contents that peptic digestion fails to give results which can be regarded as free from objection. Here is a membrane which during life serves to keep out of the nucleus, not only all inorganic salts, but also all organic compounds, except chiefly those of the class of nucleo-proteins. That such a membrane may, when the organism is dead, be permeable to pepsin is at least open to question, and in consequence what we see in the nucleus after the cell has been acted on by pepsin and hydrochloric acid cannot be adduced as evidence of its chemical or even of its morphological character.

The results of digestive experiments on cells are, therefore, misleading. What may from them appear as nucleoprotein may be anything but that, while, if the pepsin penetrates as readily as the acid, there should be left, not nucleo-protein, but pure nucleic acid, which should not stain at all.

The objections which I now urge against the conclusions drawn from the results of digestion experiments have developed out of my own observations on yeast cells, diatoms, Spirogyra, and especially the blue-green algæ. The latter are, as is Spirogyra, encased in a membrane which is an effective barrier to all colloids. When, therefore, threads of Oscillaria are subjected to the action of artificial gastric juice, a certain diminution in volume is observed owing to the dissolving power of the hydrochloric acid, and an alteration of the staining power of certain structures is found to obtain; but the pepsin has nothing to do with these, as may be determined by examination of control preparations treated with a solution of hydrochloric acid alone.

It is thus seen how slender is our knowledge of the chemistry of cells derived from staining methods and from digestion experiments. That, however, has not been the worst result of our confidence in our methods. It has led cytologists to rely on these methods alone, to leave undeveloped others which might have thrown great light on the chemical constitution of the cell, and which might have enabled us to understand a little more clearly the causation of some of the vital phenomena.

It was the futility of some of the old methods that led me, twenty years ago, to attack the chemistry of the cell from what appeared to me a correctly chemical point of view. It seemed to me then, and it appears as true now, that a diligent search for decisive chemical reactions would yield results of the very greatest importance. In the interval I have been able to accomplish only a small fraction of what I hoped to do, but I think the results have justified the view that, if there had been many investigators in this line instead of only a very few, the science of Cytochemistry would play a larger part in the solution of the problems of cell physiology than it does now.

The methods and the results are, as I have said, meagre, but they show distinctly indeed that the inorganic salts are not diffused uniformly throughout the cell, that in vegetable cells they are rigidly localised, while in animal cells, except those devoted to absorption and excretion, they are confined to specified areas in the cell. Their localisation, except in the case of inorganic salts of iron, is not due to the formation of precipitates, but rather to a condition which is the result of the action of surface tension. This seems to me to be the only explanation for the remarkable distribution, for example, of potash salts in vegetable cells. We know that, except in the chloroplatinate of potassium and in the hexanitrite of potassium, sodium and cobalt, potassium salts form no precipitates; and yet, in the cytoplasm of vegetable cells, the potassium is so localised at a few points as to appear at first as if it were in the form of a precipitate. In normal active cells of Spirogyra it is massed along the edge of the chromotophor, while in the mesophylic cells of leaves it is condensed in masses of the cytoplasm, which are by no means conspicuous in ordinary preparations of these cells.

This effect of surface tension in localising the distribution of inorganic salts at points in the cytoplasm would explain the distribution of potassium in motor structures. In striated muscle the element is abundant in amount, and is confined to the dim bands in the normal conditions. In Vorticella, apart from a minute quantity present at a point in the cytoplasm, it is found in very noticeable amounts in the contractile stalk, while in the holotrichate infusoria (Paramæcium) it is in very intimate association with the basal elements of the cilia in the ectosarc. This, indeed, would seem to indicate that the distribution of the potassium is closely associated with contraction, and, therefore, with the production of energy in contractile tissues. The condensation of potassium at a point may, of course, be a result of a combination with portions of the cytoplasm, but we have no knowledge of the occurrence of such compounds; and, further, the presence of such does not explain anything or account for the liberation of energy in motor contraction. On the other hand, the action of surface tension would explain, not only the localisation of the potassium, but also the liberation of the energy.

In vessels holding fluids, the latter, in relation to surface tension, have two surfaces, one free, in contact with the air, and known as the air-water surface, the other that in contact with the wall of the containing vessel (glass). In the latter the tension is lower than in the former. When an inorganic compound—a salt, for example—is dissolved in the fluid it increases the tension at the air-water surface, but its dilution is much greater here than in any other part of the fluid, while at the other surface its concentration is greatest. In the latter case the condition is of the nature of adsorption. The condensation on that portion of the surface where the tension is least is responsible for what we find when a solution of a coloured salt, as, *e.g.*, potassium permanganate, is driven through a layer of dry sand. If the latter is of some considerable thickness, the fluid as it passes out is colourless. The air-solution surface tension is higher than the tension of each of the solution-sand surfaces, on which, therefore, the permanganate condenses or is adsorbed. The same phenomenon is observed when a long strip of filter paper is allowed to hang with its lower end in contact with a moderately dilute solution of a copper salt. The solution is imbibed by the filter paper, and it ascends a certain distance in a couple of minutes, when it may be found that the uppermost portion of the moist area is free from even a trace of copper salt.

If, on the other hand, an organic compound—as, for instance, one of the bile salts—instead of an inorganic compound is dissolved in the fluid, the surface tension of the air-water surface is reduced, and in consequence the bile salt is concentrated at that surface, while in the remainder of the fluid, and particularly in that portion of it in contact with the wall of the vessel, the concentration is reduced.

The distribution of a salt in such a fluid, whether it lowers surface tension or increases it, is due to the action of a law which may be expressed in words to the effect that the concentration in a system is so adjusted as to reduce the energy at any point to a minimum.

Our knowledge of this action of inorganic and organic substances on the surface tension in a fluid, and of the differences in their concentrations throughout the latter, was contained in the results of the observations on gas mixtures by J. Willard Gibbs, published in 1878. The principle as applied to solutions was independently discovered by J. J. Thomson in 1887. It is known as the Gibbs' principle, although the current enunciations of it contain the more extended observations of Thomson. As formulated usually it is more briefly given, and its essential points may be rendered in the statement *that when a substance on solution in a fluid lowers the surface tension of the latter, the concentration of the solute is greater in the surface layer than elsewhere in the solution; but when the substance dissolved raises the surface tension of the fluid, the concentration of the solute is least in the surface layers of the solution.*

It is thus seen how in a system like that of a drop of water with different contact surfaces the surface tension is affected, and how this alters the distribution of solutes. It is further to be noted that for most organic solutes the action in this respect is the very reverse of that of inorganic salts. Consequently, in a living cell which contains both inorganic and organic solutes, and in which there are portions of different composition and density, the equilibrium may be subject to disturbance constantly through an alteration of the surface tension at any point. Such a disturbance may be found in a drop of an emulsion of olive oil and potassium carbonate in the well-known experiments of Bütschli. When the emulsion is appropriately prepared, a minute drop of it, after it is surrounded with water, will creep under the cover-glass in an amœboid fashion for hours, and the movement will be more marked and rapid when the temperature is raised to 40° to 50° C. All the phenomena manifested are due to a lowering of the surface tension at a point on the surface, as a result of which there is protrusion there of the contents of the drop, accompanied, Bütschli holds, by streaming cyclic currents in the remainder of the mass.

Surface tension also, according to J. Traube, is all-important in osmosis, and he holds that it is the solution pressure (*Haftdruck*) of a substance which determines the velocity of the osmotic movement and the direction and force of the osmotic pressure. The solution pressure of a substance is measured by the effect that substance exercises when dissolved on the surface tension of its solution, or, to put it in Traube's own way, the more a substance lowers or raises the surface tension of a solvent (water), the less or greater is the solution pressure (*Haftdruck*) of that substance. This solution pressure, Traube further holds, is the only force controlling osmosis through a membrane, and he rejects completely the bombardment effect on the septum postulated in the van 't Hoff theory of osmosis.

The question as to the nature of the factors concerned in osmosis must remain undecided until the facts have been more fully studied from the physiological point of view, but enough is now known to indicate that surface tension plays at least a part in it, and the omission of all consideration of it as a factor is not by any means a negligible defect in the van 't Hoff theory of osmosis.

The occurrence of variations in surface tension in the individual cells of an organ or tissue is difficult to demonstrate directly. We have no methods for that purpose, and, in consequence, one must depend on indirect ways to reveal whether such variations exist. The most effective of these is to determine the distribution of organic solutes and of inorganic salts in the cell. The demonstration of the former is at present difficult, or even in some cases impossible. The occurrence of soaps, which are amongst the most effective agents in lowering surface tension, may be revealed without difficulty microchemically, as may also neutral fats, but we have as yet no delicate microchemical tests for sugars, urea, and other nitrogenous metabolites, and in consequence the part they play, if any, in altering the surface tension in different kinds of cells, is unknown. Further research may, however, result in discovering methods of revealing their occurrence microchemically in the cell. We are in a like difficulty with regard to sodium, the distribution of which we can determine microchemically in its chief compounds, the chloride and phosphate, only after the exclusion of potassium, calcium, and magnesium. We have, on the other hand, very sensitive reactions for potassium, iron, calcium, haloid chlorine, and phosphoric acid, and with methods based on these reactions it is possible to localise the majority of the inorganic elements which occur in the living cell.

By the use of these methods we can indirectly determine the occurrence of differences in surface tension in a cell. This determination is based on the deduction from the Gibbs-Thomson principle that, where in a cell an inorganic element or compound is concentrated, the surface tension at the point is lower than it is elsewhere in the cell. If, for example, it is concentrated on one wall of a cell, the surface tension there is less than on the remaining surfaces or walls of the cell. The thickness of this layer must vary with the osmotic concentration in the cell, with the specific composition of the colloid material of the cytoplasm and with the activity of the cell, but it should not exceed a few hundredths of a milimetre (0·02–0·04 mm.), while it might be very much less in an animal cell the greatest diameter of which does not exceed 20 μ.

Numerous examples of such localisation may be observed in the confervoid protophyta. In Ulothrix, ordinarily, there is usually a remarkable condensation of the potassium at the ends of the cell on each transverse wall. The surface tension, on the basis of the deduction from the Gibbs-Thomson principle, should be, in all these cases, high on the lateral walls and low on those surfaces adjoining the transverse septa.

The use of this deduction may be extended. There are in cells various inclusions the composition of which gives them a different surface tension from that prevailing in the external limiting area of the cell. Further, the limiting portion of the cytoplasm in contact with these inclusions must have surface tension also. When, therefore, we find by microchemical means that a condensation of an inorganic element or compound obtains immediately within or without an inclusion, we may conclude that there, as compared with the external surface of the cell, the surface tension is low. It may be urged that the condensation is due to adsorption only; but this objection cannot hold, for in the Gibbs-Thomson phenomena the localisation of the solute at a part of the surface as the result of high tension elsewhere of the solution is, in all probability, due to adsorption, and is indeed so regarded.[1]

It is in this way that we can explain the remarkable localisation of potassium in the cytoplasm at the margins of the chromatophor in Spirogyra, and also the extraordinary quantities of potassium held in or on the inclusions in the mesophyllic cells of leaves. In infusoria (Vorticella, Paramœcium) the potassium present, apart from that in the stalk or ectosarc, is confined to one or more small granules or masses in the cytoplasm.

How important a factor this is in clearing the active portion of the cytoplasm of compounds which might hamper its action, a little consideration will show. In plants, very large quantities of salts are carried to the leaves by the sap from the roots, and among these salts those of potassium are the most abundant as a rule. Reaching the leaves, these salts do not return, and in consequence during the functional life of the leaves they accumulate in the mesophyllic cells in very large quantities, which, if they were not localised as described in the cell, would affect the whole cytoplasm and alter its action.

Enough has been advanced here to indicate that surface tension is not a minor feature in cell life. I would go even farther than this, and venture to say that the energy evolved in muscular contraction, that also involved in secretion and excretion, the force concerned in the phenomena of nuclear and cell division, and that force also engaged by the nerve cell in the production of a nerve impulse, are but manifestations of surface tension. On this view the living cell is but a machine, an engine, for transforming potential into kinetic and other forms of energy through or by changes in its surface energy.

To present an ample defence of all the parts of the thesis just advanced is more than I propose to do in this address. That would take more time than is customarily allowed on such an occasion, and I have, in consequence, decided to confine my observations to outlines of the points as specified.

It is not a new view that surface tension is the source of the muscular contraction. As already stated, the first to apply the explanation of this force as a factor in cellular movement was Engelmann in 1869, who advanced the view that those changes in shape of cells which are classed as contractile are all due to that force which is concerned in the rounding of a drop of fluid. The same view was expressed by Rindfleisch in 1880, and by Berthold

[1] See Freundlich, "Kapillarchemie," p. 50, 1909.

in 1886, who explained the protoplasmic streaming in cells as arising in local changes of surface tension between the fluid plasma and the cell sap, but he held that the movement and streaming of Amœbæ and Plasmodiæ are not to be referred to the same causes as operate in the protoplasmic streaming in plant cells. Quincke in 1888 applied the principle of surface tension in explaining all protoplasmic movement. In his view the force operates, as in the distribution of a drop of oil on water, in spreading protoplasm, which contains oils and soaps, over surfaces in which the tension is greater, and as soap is constantly being formed, the layer containing it, having a low tension on the surface in contact with water, will as constantly keep moving, and as a result pull the protoplasm with it. The movement of the latter thus generated will be continuous, and constitute protoplasmic streaming. In a similar way Bütschli explains the movement of a drop of soap emulsion, the layer of soap at a point on the surface of the spherule dissolving in the water and causing there a low tension and a streaming of the water from that point over the surface of the drop. This produces a corresponding movement in the drop at its periphery and a return central or axial stream directed to the point on the surface where the solution of the soap occurred and where now a protrusion of the mass takes place resembling a pseudopodium. In this manner, Bütschli holds, the contractile movements of Amœbæ are brought about. In these the chylema or fluid of the foam-like structure in the protoplasm is alkaline, it contains fatty acids, and, in consequence, soaps are present which, through rupture of the superficial vesicles of the foam-like structure at a point, are discharged on the free surface and produce there the diminution of surface tension that calls forth currents, internal and external, like those which occur in the case of the drop of oil emulsion.

The first to suggest that surface tension is a factor in muscular contraction was D'Arsonval, but it was Imbert who, in 1897, directly applied the principle in explanation of the contractility of smooth and striated muscle fibre. In his view the primary conditions are different in the former from what obtain in the latter. In smooth muscle fibre the extension is determined, not by any force inside it, but by external force such as may distend the organ (intestine, bladder, and arteries) in the wall of which it is found. The "stimulus" which causes the contraction increases the surface tension between the surface of the fibre and the surrounding fluid, and this of itself has the effect of making the fibre tend to become more spherical or shorter and thicker, which change in shape does occur during contraction. He did not, however, explain how the excitation altered the surface tension, except to say that its effect on surface tension is like that of electricity, with which the nerve impulse presents some analogy. In striated fibre, on the other hand, the discs constituting the light and dim bands have each a longitudinal diameter which is an effect of its surface tension, and this causes extension of the fibre during rest. When a nerve impulse reaches the fibre the surface tension of the discs is altered, and there results a deformation of each involving a shortening of its longitudinal axis, and thus a shortening of the whole fibre.

According to Bernstein, in both smooth and striated muscle fibre there is, in addition to surface tension, an elastic force residing in the material composing the fibre which, according to the conditions, sometimes opposes and sometimes assists the surface tension. The result is that in the muscle fibre at rest the surface must exceed somewhat that of the fibre in contraction. In both conditions the sum of the two forces, surface tension and elasticity, must be zero. In contraction the surface tension increases, and with it the elasticity also. Taken as a whole, this would not explain the large force generated in contraction, for the energy liberated would be the product of the surface tension and the amount representing the diminution of the surface due to the contraction. As the latter is very small the product is much below the amount of energy in the form of work done actually manifested. To get over this difficulty, Bernstein postulates that in muscle fibres, whether smooth or striated, there are fibrils surrounded by sarcoplasma, and that each fibril is formed of a number of cylinders or biaxial ellipsoids singly disposed in the course of the fibril, but separated from each other by elastic material and surrounded by sarcoplasma. Between the ellipsoids and the sarcoplasma there is considerable surface tension which prevents mixture of the substances constituting both. The excitation through the nerve impulse causes an increase of surface tension in these ellipsoids, and they become more spherical. In consequence, the decrease in surface of all the ellipsoids constituting a fibril is much greater than if the fibril were to be affected as an individual unit only by an increase of surface tension, and thus the surface energy developed would be correspondingly greater. The ellipsoids, Bernstein explains, are not to be confused with the discs, singly and doubly refractive in striated fibre; for these, he holds, are not concerned in the generation of the contraction, but with the processes that make for rapidity of contraction. The extension of a muscle after contraction is due to the elastic reaction of the substance between the ellipsoids in the fibrils. Bernstein further holds that fibrils of this character occur in the protoplasm of Amœbæ, in the stalk of Vorticella, and in the ectoplasma of Stentor, and this explains their contractility.

It may be said in criticism of Bernstein's view that his ellipsoids are from their very nature non-demonstrable structures, and, therefore, must always remain as postulated elements only. Further, it may be pointed out that he attributes too small a part to surface tension in the lengthening of the fibre after contraction, and that the elasticity which muscle appears to possess is, in the last analysis, but a result of its surface tension.

As regards Quincke's explanation of protoplasmic movement and streaming, as well as of muscular contraction, Bütschli has shown that it is based on a mistaken view of the structure of the cell in Chara and other plant forms in which protoplasmic streaming occurs. Bütschli's own hypothesis, however, is defective in that it postulates a current in the fluid medium just outside the Amœba and backward over its surface, the existence of which Berthold denies, and Bütschli himself has been unable to demonstrate, even with the aid of fine carmine powder in the fluid. He did, indeed, observe a streaming in the water about a creeping Pelomyxa, but the current was in the opposite direction to that demanded by his hypothesis. Further, his failure to demonstrate the occurrence of the postulated backflow in the water about the contracting or moving mass of an Amœba or a Pelomyxa makes it difficult to accept the hypothesis he advanced to explain that backflow, namely, that rupture of peripheral vesicles (*Waben*) of the protoplasm occurs with a consequent discharge of their contents (proteins, oils, and soaps) into the surrounding fluid. Surface tension, further, on this hypothesis, would be an uncertain and wasteful factor in the life of the cell. On *a priori* grounds, also, it would seem improbable that this force should be generated outside instead of inside the cell.

One common defect of all these views is that they made only a limited application of the principle of surface tension. This was because some of its phenomena were unknown, and especially those illustrating the Gibbs-Thomson principle. With its aid and with the knowledge of the distribution of inorganic constituents in animal and vegetable cells that microchemistry gives us we can make a more extended application of surface tension as a factor in cellular life than was possible ten years ago.

In regard to muscle fibre this is particularly true, and microchemistry has been of considerable service here. From the analyses of the inorganic constituents of striated muscle in vertebrates made by J. Katz and others we know that potassium is extraordinarily abundant therein, ranging from three and a half in the dog to more than fourteen times in the pike the amount of sodium present. How the potassium salt is distributed in the fibre was unknown before 1904, in which year, by the use of a method, which I had discovered, of demonstrating the potassium microchemically, the element was found localised in the dim bands. Later and more extended observations suggested that in the dim band itself, when the muscle fibre is at rest, the potassium is not uniformly distributed, and it was found to be the case in the wing muscles of certain of the Insecta—as, for example, the scavenger beetles—in which the bands are broad and conspicuous enough to permit ready observation on this score. In these the potassium salt was found to be localised in the zones of each dim band adjacent to each light band. Subsequently Miss M. L. Menten, working in my laboratory and using the same microchemical method, found the potassium similarly limited in its distribution in the muscle fibres of a number of other insects. She determined, also, that the chlorides and phosphates have a like distribution in these structures, and it is consequently probable that sodium, calcium, and magnesium have the same localisation.

Macdonald has also made investigations on the distribution of potassium in the muscle fibre of the frog, crab, and lobster, using for this purpose the hexanitrite reagent. He holds, as a result of his observations, that the element in the uncontracted fibril is limited to the sarcoplasm in the immediate neighbourhood of the singly refractive substance, while it is abundantly present in the central portion of each sarcomere of the contracted fibril—that is, in the doubly refractive material. I am not inclined to question the former point, as I have not investigated the microchemistry of the muscle in the crab and lobster, and my only criticism would be directed against placing too great reliance on the results obtained in the case of frog's muscle. The latter is only very slowly penetrated by the hexanitrite reagent, and, apparently because of this, alterations in the distribution of the salts occur; and, as I have observed, the potassium may be limited to the dim bands of one part of the contracted fibre and may be found in the light bands of another part of the same. In the wing muscles of insects in the uncontracted condition such disconcerting results are not so readily obtained, owing, it would seem, to the readiness with which the fibrils may be isolated and the almost immediate penetration of them by the reagent. Here there is no doubt about the occurrence of the element in the zones of the dim band immediately adjacent to the light bands.

Whether the potassium in the resting fibre is in the sarcoplasm or in the sarcostyle I would hesitate to say. It may be as Macdonald claims; but I find it difficult to apply in microchemical studies of muscle fibre the concepts of its more minute structure gained from merely stained preparations. Because of this difficulty I have refrained from using here, as localising designations, other expressions than "light bands" and "dim bands." The latter undoubtedly include some sarcoplasm, but in the case of the resting fibre I am certain only of the presence of potassium, as described, in the dim band regarded as an individual part, and not as a composite structure.

Now, on applying the Gibbs-Thomson principle enunciated above, this distribution would seem to indicate that in the dim band of a fibril the surface tension is greatest on its lateral walls, in consequence of which the potassium salts are concentrated in the vicinity of the remaining surfaces, *i.e.* those limiting the light bands. This explanation would seem to be confirmed by the observations I made on the contracted fibrils of the wing muscles of a scavenger beetle. In these the potassium was found uniformly distributed throughout each dim band, which, instead of being cylindrical in shape as in the resting element, is provided with a convexly curved lateral wall, and therefore with a smaller surface than the mass of the dim band has when at rest. This contour suggests that the surface tension on the lateral wall is lessened to an amount below that of either terminal surface, followed by a redistribution of the potassium salt to restore the equilibrium thus disturbed. The consequent shortening of the dim bands of the fibrils would account for the contraction of the muscle.

How the surface tension of the lateral wall of the dim band is lessened in contraction is a question which can only be answered after much more is known of the nature of the nerve impulse as it reaches the muscle fibril, and of the part played by the energy set free in the combustion process in the dim bands. It may be that electrical polarisation, as a result of the arrival of the nerve impulse, develops on the surface of the lateral wall, and as a consequence of which its surface tension is diminished. The energy so lost appears as work, and it is replaced by energy, one may suppose, derived from the combustion of the material in the dim band. In this case the disturbance of surface tension would be primary, while the combustion process would be secondary, in the order of time. In support of this explanation may be cited the fact that the current of action in muscle precedes in time the contraction itself—that is, the electrical response of the stimulus occurs in the latent period and immediately before the contraction begins.

It may, however, be postulated, on the other hand, that the chemical changes occur in those parts of the dim band immediately adjacent to the light bands, and as a result the tension of the terminal surfaces may be increased, this resulting in the shortening of the longitudinal axis of the dim band and the displacement laterally of the contents. This would imply that the energy of muscle contraction comes primarily from that set free in the combustion process, and not indirectly as involved in the former explanation.

Whatever may be the cause of the alteration in surface tension, there would seem to be no question of the latter. The very alteration in shape of the dim band in contraction makes it imperative to believe that surface tension is concerned. The redistribution of the potassium which takes place as described in the contracting fibrils of the wing muscles of the scavenger beetle can be explained in no other way than through the alteration of surface tension.

In the smooth muscle fibre potassium is also present and in close association throughout with the membrane. When a fresh preparation of smooth muscle is treated so as to demonstrate the presence of potassium, the latter is shown in the form of a granular precipitate of hexanitrite of sodium, potassium, and cobalt in the cement substance between the membranes of the fibres. In the smooth muscle fibres in the walls of the arteries in the frog the precipitate in the cement material is abundant, and its disposition suggests that it plays some part in the *rôle* of contraction. Inside of the membrane potassium occurs, but in very minute quantities, which, with the cobalt sulphide method, gives a just perceptible dark shade to the cytoplasm as a whole. Microchemical tests for the chlorides and phosphates indicate that the cytoplasm is almost wholly free from them, and consequently there is very little inorganic material inside of the fibre. Chlorides and phosphates, but more particularly the former, are abundant in the cement material, and their localisation here would seem to indicate that the potassium of the same distribution is combined chiefly as chloride.

In smooth muscle fibre, then, the potassium is distributed very differently from what it is in striated fibre, and on first thought it seemed difficult to postulate that the contraction could be due to alterations of surface tension. This, however, would appear to be the most feasible explanation, for the potassium salts in the cement substance might be supposed to shift their position under the influence of electrical force so as to reach the interior of the membranes of the fibres, in which case the surface tension of the latter would be immediately increased, and the fibre itself would in consequence at once begin to contract. The slowness with which this shifting into, or absorption by, the membrane of the potassium salts would take place would also account for the long latent period of contraction in smooth muscle.

It is of interest here to note that the potassium ions have the highest ionic mobility (transport number) of all the elements of the kationic class, except hydrogen, which are found to occur in connection with living matter. Its value in this respect is half again as great as that of sodium, one-eighth greater than that of calcium, and one-seventh greater than that of magnesium. This high migration velocity of potassium ions would make the element of special service in rapid changes of surface tension.

Loew has pointed out that potassium in the condensation processes of the synthesis of organic compounds has a catalytic value different from that of sodium. For example, ethyl aldehyde is condensed with potassium salts to aldol, with sodium salts to crotonic aldehyde (Kopf and Michael). Potassium is, but sodium is not, effective in the condensation of carbon monoxide. When phenol is fused with potassium salts condensation products like diphenol are produced, but when sodium salts are used the products are dioxybenzol and phloroglucin (Barth). It is, therefore, not improbable that potassium, along with those properties which come from its ionic mobility, has a special value in the metabolism of the dim bands of striated muscle fibre and in the condensation synthesis which characterise the chromatophors of protophyta (Spirogyra, Zygnema).

With the use of this method of determining differences in surface tension in cells it is possible, in some cases at least, to ascertain whether this force plays a part in both secretion and excretion, and evidence in favour of this view can be found in the pancreatic cells of the rabbit, guinea-pig, and in the renal cells of the frog. In the pancreatic cells there is an extraordinary condensation of potassium salts in the cytoplasm of each cell adjacent to the lumen of the tubule, and during all the phases of activity—except, it would appear, that of the co-called "resting stage"—potassium salts occur in, and are wholly confined to, this part of each cell. It is difficult to say whether they pass into the lumen with the secretion and their place taken by more from the blood-stream and lymph, but the important point is that the condensation of potassium salts immediately adjacent to the lumen seems to indicate a lessened surface tension on the lumen surface of the cell.

According to Stoklasa,[1] the pancreas of the pig is much richer in potassium than in sodium, the dried material containing 2·09 per cent. of potassium and 0·28 per cent. of sodium, while the values for the dried material of ox muscle are, as he determined them, 1·82 and 0·26 per cent. respectively. It is significant that in the pancreas this large amount of potassium should be localised as described.

In the renal cells of vertebrates there is usually a considerable amount of potassium salts distributed throughout the cytoplasm. These cells are always active in the elimination of the element from the blood, and it is in consequence not possible to determine whether there are differences in surface tension in them. Under certain conditions, however, these can be demonstrated. In the frogs which have been kept in the laboratory tanks throughout the winter, and in the blood of which the inorganic salts have been, because of the long period of inanition, reduced to almost hypotonic proportions, the renal cells are very largely free from potassium. When it is present it is usually diffused throughout the cytoplasm. If now a few cubic centimetres of a decinormal solution of potassium chloride be injected into the dorsal lymph sacs of one of these frogs, and after twenty minutes the animal is killed, appropriate treatment, with the cobalt reagent, of a thin section of the fresh kidney made by the carbon dioxide freezing method, reveals in the cells of certain of the tubules a condensation of potassium salts in the cytoplasm immediately adjacent to the wall of the lumen. There is also a very slight diffuse reaction throughout the remainder of the cytoplasm, except in that part immediately adjacent to the external boundary of the tubule. In these cells the potassium injected into the lymph circulation is being excreted, and the condensation of the element at or near the surface of the lumen is evidence that there the tension is less than at the other extremity of the cell.

These facts are in their significance in line with some observations that I have made on the absorption of soluble salts by the intestinal mucosa in the guinea-pig. When the "peptonate" of iron was administered in the food of the animal it was not unusual to find that in the epithelial cells of the villi the iron salt was distributed through the cytoplasm, but its concentration, as a rule, was greatest in the cytoplasm adjacent to the inner surface of the cell, from which it diffused into the underlying tissue. Here also, inferentially, surface tension is lower than elsewhere in the cell.

It would perhaps be unwise to form final conclusions at this stage in the progress of the investigation of the subject, but the results so far gained tempt one to adopt as a working hypothesis *that in the secreting or the excreting cell lower surface tension exists at its secreting or excreting surface than at any other point on the cell surface.* How this low surface tension is caused or maintained it is impossible to say, but, whatever the solution of the question may be, it is important to note that we must postulate the participation of this force in renal excretion in order to explain the formation of urines of high concentration. These have a high osmotic pressure, as measured by the depression of the freezing point, while the osmotic pressure of the blood plasma determined in the same way is low. On the principle of osmosis alone, as it is currently understood, this result is inexplicable, for the kinetic energy, as required in the gas theory of solutions, should not be greater, though it might be less, in the urine than in the blood. It is manifest that in the formation of concentrated urines energy is expended. We know also from the investigations of Barcroft and Brodie that the kidney during diuresis absorbs much more oxygen per gram weight than the body generally, and that, assuming it is used in the combustion of a proteid, a very large amount of energy is set free, very much more, indeed, than is necessary. It has also been observed that a portion of the energy set free is found in a higher temperature in the excretion than obtains in the blood itself circulating through the kidney. This large expenditure of energy is, probably, a result of the physiological adaptation of the principle of the "factor of safety," which, as Meltzer has pointed out, occurs in other organs of the body.

In cell and nuclear division surface tension operates as a force, the action of which cannot be completely understood until we know more of the part played by the centrosomes and centrosphere. That this force takes part in cell reproduction has already been suggested by Brailsford Robertson. He has devised an ingenious experiment to illustrate its action. If a thread moistened with a solution of a base is laid across a drop of oil in which is dissolved some free fatty acid, the drop divides along the line of the thread. When the latter is moistened with soap the drop divides in the same way and in the same plane. The soap formed in one case and present in the other, it is explained, lowers the surface tension in the equatorial plane of the drop, and this diminution results in streaming movement away from that plane which bring about the division. He suggests that in cell division there is a liberation of soaps in the plane of division which set up streaming movements from that plane towards the poles, and terminating in the division of the cytoplasm of the cell.

I have observed in the cells of Zygnema about to divide a remarkable condensation of potassium in the plane of division. In the "resting" cell of this Alga the potassium is, as a rule, more abundant in the cytoplasm near the transverse walls of the thread, and only traces of the element are to be found along the line of future division of the cell. But immediately after division has taken place the potassium is concentrated in the plane of division. This would seem to indicate that surface tension in the plane of division is, as postulated by the deduction from the Gibbs-Thomson principle, lower than it is on the longitudinal surface, and lower, especially, than it is on the previously formed transverse septa of the thread.

One must not, however, draw from this the conclusion that in all dividing cells surface tension is lower in the plane of division than it is elsewhere on the surface of the dividing structure. All that it means is that in the dividing cell of Zygnema the condition already exists along the plane of division, which subsequently makes for low surface tension in the cell membrane immediately adjacent to each transverse septum in the confervoid thread. If the evidence of low surface tension vanished immediately after division was complete, then it might be held that it determined the division. As it is, the low surface tension in this case is the result, and not the cause, of the division.

This conclusion is corroborated by the results of observations on the cells of the ovules of Lilium and Tulipa. The potassium salts in these are found condensed in minute masses throughout the cytoplasm. When division is about to begin the salts are shifted to the peripheral zone of the cytoplasm, and when the nuclear membrane disappears not a trace of potassium is now

[1] Stoklasa gave the values in K_2O and Na_2O.

found in the neighbourhood of the free chromosomes, a condition which continues until after nuclear division is complete. The absence of potassium, the most abundant basic element in the cytoplasm, would indicate that soaps are not present, and appropriate treatment of such cells, hardened in formaline only, with scarlet red demonstrates that fats, including lecithins, are absent also. This would seem to show that high instead of low surface tension prevails about the nucleus during division. During the "resting" condition of the nucleus this high tension is maintained, for, except in very rare cases, and these of doubtful character, there is no condensation of inorganic salts in the neighbourhood or on the surface of the nuclear membrane. It is also to be noted that the nucleus, with exceptions, the majority of which are found in the Protozoa, is of spherical shape, which also postulates that high surface tension obtains either in the cytoplasmic layer about the nucleus or in the nuclear membrane itself. It may also be suggested that high surface tension, and not the physical impermeability of the nuclear membrane, is the reason why the nucleus is, as I have often stated, wholly free from inorganic constituents.

It does not follow from all this that surface tension has nothing to do with cell division. If, as Brailsford Robertson holds, surface tension is lowered in the plane of division, then the internal streaming movement of the cytoplasm of each half of the cell should be towards that plane, and, in consequence, not separation, but fusion of the two halves would result. The lipoids and soaps would, indeed, spread superficially on the two parts from the equatorial plane towards the two poles, and, according to the Gibbs-Thomson principle, they would not distribute themselves through the cytoplasm in the plane of division, except as a result of the formation of a septum in that plane. In other words, the septum has first to exist in order to allow the soaps and lipoids to distribute themselves in a streaming movement over its two faces. In Brailsford Robertson's experiment this septum is provided in the thread. If, on the other hand, surface tension is higher about the nucleus in and immediately adjacent to the future plane of division, then constriction of the nucleus in that plane will take place accompanied or preceded by an internal streaming movement in each half towards its pole, and a consequent traction effect on the chromosomes which are thus removed from the equatorial plane. When nuclear division is complete, then a higher surface tension on the cell itself limited to the plane of division would bring about there a separation of the two halves, a consequent condensation on each side of that plane of the substances producing the low tension elsewhere, and thereby also the formation of the two membranes in that plane.

In support of this explanation of the action of surface tension as a factor in division I have endeavoured to ascertain if, as a result of the Gibbs-Thomson principle, there is a condensation of potassium salts in the cytoplasm at the poles of a dividing cell, that is, where surface tension, according to my view, is low. The difficulty one meets here is that, in the higher plant forms, cells preparing to divide appear to be much less rich in potassium than those in the "resting" stage, and under this condition it is not easy to get unambiguous results, while in animal cells potassium may even in the resting cell be very minute in quantity, as, for example, in Vorticella, in which, apart from the contractile stalk, it is limited to one or two minute flecks in the cytoplasm. Instances of potassium-holding cells undergoing division are, however, found in the spermatogonia of higher vertebrates (rabbit, guinea-pig), and in these the potassium is gathered in the form of a minute and thin cap-like layer at each pole of the dividing cell.

This of itself would appear to show that surface tension is less in the neighbourhood of the poles than at the equator of the dividing cell; but I am not inclined to regard the fact as conclusive, and a very large number of observations to that end must be made before certainty can be attained. I am, nevertheless, convinced that it is only in this way that we can finally determine whether differences of surface tension in dividing cells account, as I believe they do, for all the phenomena of cell division. The difficulties to be encountered in such an investigation are, as experience has shown me, much greater than are to be overcome in efforts to study surface tension in cells under other conditions, but I am in hopes that what I am now advancing will influence a number of workers to take up research in microchemistry along this line.

I must now discuss surface tension in nerve cells and nerve fibres. I have stated earlier in this address that I hold that the force concerned in the production of the nerve impulse by the nerve cell is surface tension. The very fact that in the repair of a divided nerve fibre the renewal of the peripheral portion of the axon occurs through a movement—a flowing outward, as it were—of the soft colloid material from the central portion of the divided fibre is, in itself, a strong indication that surface tension is low here and high on the cell body itself. This fact does not stand alone. I pointed out six years ago that potassium salt is abundant along the course of the axon and apparently on its exterior surface, while it is present but in traces in the nerve cell itself. In the latter chlorides also are present only in traces, and therefore sodium, if present, is there in more minute quantities, while haloid chlorine is abundant in the axon. Macdonald has also made observations as to the occurrence of potassium along the course of the axon, and has in the main confirmed mine. We differ only as to mode of the distribution of the element in the axon, and the manner in which it is held in the substance of the latter; but, whichever of the two views may be correct, it does not affect what I am now advancing. Extensive condensation or adsorption of potassium salts in or along the course of the axon, while the nerve cell itself is very largely free from them, can have but one explanation on the basis of the Gibbs-Thomson principle, and that explanation is that surface tension on the nerve cell itself must be high while it is low on or in its axon.

The conclusions that follow from this are not far to seek. We know that an electrical displacement or disturbance of ever so slight a character occurring at a point on the surface of a drop lowers correspondingly the surface tension at that point. What a nerve impulse fundamentally involves we are not certain, but we do know that it is always accompanied by, if not constituted of, a change of electrical potential, which is as rapidly transmitted as is the impulse. When this change of potential is transmitted along an axon through its synaptic terminals to another cell, the surface tension of the latter must be lowered to a degree corresponding to the magnitude of the electrical disturbance produced, and, in consequence, a slight displacement of the potassium ions would occur at each point in succession along the course of its axon. This displacement of the ions as it proceeded would produce a change of electrical potential, and thus account for the current of action. The displacement of the ions in the axon would last as long as the alteration of surface tension which gave rise to it, and this would comprehend not more than a very minute fraction of a second. Consequently, many such variations in the surface tension of the body of the nerve cell would occur in a second; and, as the physical change concerned would involve only the very surface layer of the cell, a minimum of fatigue would result in the cell, while little or none would develop in the axon.

It may be pointed out that in medullated nerve fibres the lipoid-holding sheath, in close contact as it is with the axon, must of necessity maintain on the course of the latter a surface tension low as compared with that on the nerve cell itself, which, as the synaptic relations of other nerve cells with it postulate, is not closely invested with an enveloping membrane. In non-medullated nerve fibres the simple enveloping sheath may function in the same manner, and probably, if it is not rich in lipoid material, in a less marked degree.

What further is involved in all this, what other conclusions follow from these observations, I must leave unexplained. It suffices that I have indicated the main points of the subject, the philosophical significance of which will appear to those who will pursue it beyond the point where I leave it.

In bringing this address to a close, I am well aware of the fact that my treatment of the subjects discussed has not been as adequate as their character would warrant.

The position which I occupy imposes limits, and there enters also the personal factor to account in part for the failure to achieve the result at which I aimed. But there is, besides, the idea that in applying the laws of surface tension in the explanation of vital phenomena I am proceeding along a path into the unknown which has been as yet only in a most general way marked out by pioneer investigators, and in consequence, to avoid mistakes, I have been constrained to exercise caution, and to repress the desire to make larger ventures from the imperfectly beaten main road. Perhaps, after all, I may have fallen into error, and I must therefore be prepared to recall or to revise some of the views which I have advanced here should they ultimately be found wanting. That, however, as I reassure myself, is the true attitude to take. It is a far cry to certainty. As Duclaux has aptly put it, the reason why Science advances is that it is never sure of anything. Thus I justify my effort of to-day.

Notwithstanding this inadequate treatment of the subject of surface tension in relation to cellular processes, I hope I have made it in some measure clear that the same force which shapes the raindrop is an all-important factor in the causation of vital phenomena. Some of the latter may not thereby be explained. We do not as yet know all that is concerned in the physical state of solutions. The fact, ascertained by Rona and Michaelis, that certain sugars, which neither lower nor appreciably raise surface tension in their solutions, condense or are adsorbed on the surface of a solution system, is an indication that there are at least some problems with a bearing on vital phenomena yet to solve. Nevertheless, what we have gained from our knowledge of the laws of surface tension constitutes a distinct step in advance, and a more extended application of the Gibbs-Thomson principle may throw light on the causation of other vital phenomena. To that end a greatly developed science of microchemistry is necessary. This should supply the stimulus to enthusiasm in the search for reactions that will enable us to locate with great precision in the living cell the constituents, inorganic and organic, which affect its physical state and thereby influence its activity.

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SECTION K.

BOTANY.

OPENING ADDRESS BY PROF. JAMES W. H. TRAIL, M.A., M.D., F.R.S., PRESIDENT OF THE SECTION.

THE honour conferred in the election to be President for the year of the Botanical Section of the British Association imposes the duty of preparing an address. I trust that my selection of a subject will not be attributed by anyone to a want of appreciation of the worth and importance of certain sides of botanical research to which I shall have less occasion to refer. These have been eloquently supported by former Presidents, and I take this opportunity to express the thanks I owe for the benefit received from their contributions to the advancement of the science of botany. They have told us of the advance in departments of which they could speak as leaders in research, and I do not venture to follow in their steps. My subject is from a field in which I have often experienced the hindrances of which I shall have to speak, both in personal work and still more as a teacher of students, familiar with the many difficulties that impede the path of those who would gladly give of their best, but find the difficulties for a time almost insurmountable, and who are too frequently unable to spare the time or labour to allow of their undertaking scientific investigations that they might well accomplish, and in which they would find keen pleasure under other conditions. Those whose tastes lie in the direction of studying plants in the field rather than in the laboratory are apt to find themselves hampered seriously if they seek to become acquainted with the plants of their own vicinity; and, if they wish to undertake investigations in the hope of doing what they can to advance botanical science, they may find it scarcely possible to ascertain what has been already done and recorded by others.

For a time the knowledge of plants was too much confined to the ability to name them according to the system in vogue and to a knowledge of their uses, real or imagined. The undue importance attached to this side of the study, even by so great a leader as Linnæus, naturally led to a reaction as the value of other aspects of botany came to be realised, and as improvements in the instruments and methods of research opened up new fields of study. The science has gained much by the reaction; but there is danger of swinging to the other extreme and of failing to recognise the need to become well acquainted with plants in their natural surroundings. The opportunities for study in the laboratory are so great and so much more under control, and the materials are so abundant and of so much interest, that there is for many botanists a temptation to limit themselves to such work, or at least to regard work in the field as subordinate to it and of little value. It is scarcely necessary to point out that each side is insufficient alone. Yet some find more pleasure in the one side, and do well to make it their chief study; while they should recognise the value of the other also, and learn from it.

It is especially on behalf of the work in the field that I now wish to plead. There are few paths more likely to prove attractive to most students. The study of the plants in their natural environments will lead to an understanding of their nature as living beings, of their relations to one another and to other environments, of the stimuli to which they respond, and of the struggle for existence that results in the survival of certain forms and the disappearance of others. In this way also will be gained a conception of the true meaning and place of classification as an indispensable instrument for accurate determination and record, and not as an end in itself. To one that has once gained a true insight into the pleasure and worth of such studies, collections made for the sake of mere possession or lists of species discovered in a locality will not suffice. Many questions will arise which will prove a constant source of new interest. From such studies a deep and growing love for botany has in not a few cases arisen.

The British flora has interested me for upwards of forty years, and has occupied much of my attention during that time—not only as desirous to aid by my own efforts to extend our knowledge of it, but also, as a teacher, seeking to assist my students to become able to do their parts also, and making use of the materials within reach to enable me to help them. Thus our present knowledge of the plants of our own country has become known to me, and the difficulties of acquiring that knowledge have also become known through both my own experience and those of my students. The nature of the hindrances and difficulties that at present bar the way has also become familiar, as well as the steps to be taken to clear some of them away and to make the path less difficult to those who come after us; and I have also gained a fairly good acquaintance with the means at the command of students of the floras of other countries, so as to have a standard for comparison in the estimate to be formed of the condition of matters in our own country.

In how far is the present provision for the study of the flora of the British Islands sufficient and satisfactory?

I venture to hope that the subject will be regarded as among those for the consideration of which the British Association was formed, and that a favourable view will be taken of the conclusions which I take this opportunity to lay before you. What, then, is the present provision for the study of our plants? Since the days of Morrison and Ray there have been many workers, especially during the past century; and an extensive literature has grown up, in the form both of books and of papers, the latter more or less comprehensive, in the scientific journals and in the transactions of societies. These papers contain much that is of great value; but, owing to the absence of any classified index, most of the information in it is beyond the reach of anyone, except at the expenditure of much time and labour. The constantly increasing accumulation of new publications makes the need for a classified index always more urgent; for the mass of literature is at present one of the greatest obstacles to the undertaking of new investigations, because of the uncertainty whether they may not have been already undertaken and overlooked through want of time or opportunity to search the mass exhaustively.

While the early writers of descriptive floras sought to include every species of plant known to occur in Britain, this has not been attempted during the past seventy or eighty years, and instead of one great work we now have monographs of the greater groups, such as Babington's "Manual" and Hooker's "Student's Flora" of the vascular plants, Braithwaite's "Mossflora," &c. Local floras still, in a good many cases, aim at including all plants known to grow apparently wild in the districts to which they refer; but they are often little more than lists of species and varieties and of localities in which these have been found. In some, however, there are descriptions of new forms and notes of general value, which are apt to be overlooked because of the place in which they appear.

The early works were necessarily not critical in their treatment of closely allied species and varieties, but they are valuable as giving evidence of what plants were supposed to be native in England when they were published. Even the works that were issued after Linnæus had established the binominal nomenclature for a time related almost wholly to England. Sibbald in "Scotia Illustrata" (1684) enumerated the plants believed by him to be native in Scotland, and of those then cultivated. Between his book and Lightfoot's "Flora Scotia," published in 1777, very little relating to the flora of Scotland appeared. Irish plants were still later in being carefully studied.

The floras of Hudson, Withering, Lightfoot, and Smith, all of which include all species of known British plants, follow the Linnæan classification and nomenclature in so far as the authors were able to identify the Linnæan species in the British flora. "English Botany," begun in 1795, with plates by Sowerby and text by Smith, was a work of the first rank in its aim of figuring all British plants and in the excellence of the plates; but it shared the defect of certain other great floras in the plates being prepared and issued as the plants could be procured, and thus being without order. Its cost also necessarily put it beyond the reach of most botanists, except those that had the advantage of access to it in some large library. A second edition, issued at a lower price, and with the plants arranged on the Linnæan system, was inferior to the first, in the plates being only partially coloured and in having the text much curtailed. The so-called third edition of the "English Botany," issued 1868–86, is a new work so far as the text is concerned, that being the work of Dr. Boswell Syme, who made it worthily representative of its subject; but the plates, with few exceptions, are reissues of those of the first edition, less perfect as impressions and far less carefully coloured; and this applies with still greater force to a reissue of the third edition a few years ago. This edition, moreover, included only the vascular plants and Characeæ. As this is the only large and fully illustrated British flora that has been attempted, it is almost needless to add that in this respect provision for the study of the flora of our islands is far behind that of certain other countries, and very notably behind that made in the "Flora danica."

Turning next to the provision of less costly aids to the study of British plants, we have manuals of most of the larger groups. The vascular plants are treated of in numerous works, including a considerable number of illustrated books in recent years, inexpensive but insufficient for any but the most elementary students. Fitch's outline illustrations to Bentham's "Handbook to the British Flora," supplemented by W. G. Smith, were issued in a separate volume in 1887, which is still the best for use in the inexpensive works of this kind. Babington's "Manual," on its first appearance in 1843, was gladly welcomed as embodying the result of careful and continued researches by its author into the relations of British plants to their nearest relatives on the Continent of Europe; and each successive issue up to the eighth in 1881 received the careful revision of the author, and contained additions and modifications. In 1904 a ninth edition was edited, after the author's death, by H. and J. Groves; but, though the editors included notes left by Prof. Babington prepared for a new edition, they were "unable to make alterations in the treatment of some of the critical genera which might perhaps have been desirable." The "Student's Flora of the British Islands," by Sir J. D. Hooker, issued in 1870, took the place of the well-known "British Flora" (1830, and in subsequent editions until the eighth in 1860, the last three being issued in collaboration by Sir W. J. Hooker and Prof. Walker-Arnott). The third edition of the "Student's Flora" appeared in 1884, and there has been none since. Mr. F. N. Williams's "Prodromus Floræ Britannicæ," begun in 1901, of which less than one-half has yet appeared, though a work of much value and authority, is scarcely calculated for the assistance of the ordinary student; and Mr. Druce's new edition of Hayward's "Botanist's Pocket Book" "is intended merely to enable the botanist in the field to name his specimens approximately, and to refresh the memory of the more advanced worker." In all the books that are intended for the use of British botanists, apart from one or two recently issued local floras, the classification is still that in use in the middle of last century, even to the extent in the most of them of retaining Coniferæ as a division of Dicotyledones. Apart from this, the critical study of British plants has led to the detection of numerous previously unobserved and unnamed forms, which find no place in the "Student's Flora," and are only in part noticed in the recent edition of the "Manual."

The "Lists" of vascular plants of the British flora that have recently been issued by Messrs. Rendle and Britten, by Mr. Druce, and as the tenth edition of the "London Catalogue of British Plants," are all important documents for the study of the British flora; but they illustrate very forcibly certain of the difficulties that beset the path of the student eager to gain a knowledge of the plants of his native land. In these lists he finds it scarcely possible to gain a clear idea of how far the species and varieties of the one correspond with those of the other, owing to the diversities of the names employed. It would be a great boon to others, as well as to students, were a full synonymic list prepared to show clearly the equivalence of the names where those for the same species or variety differ in the different lists and manuals. Probably in time an agreement will be generally arrived at regarding the names to be accepted, but that desirable consummation seems hardly yet in sight. Meantime, the most useful step seems to be to show in how far there is agreement in fact under the different names.

Among the Cryptogams certain groups have fared better than the higher plants as regards both their later treatment and their more adequate illustration by modern methods and standards. Several works of great value have dealt with the mosses, the latest being Braithwaite's "British Moss-flora," completed in 1899. The Sphagna were also treated by Braithwaite in 1880, and are to be the subject of a monograph in the Ray Society's series. The liverworts have been the subject also of several monographs, of which Pearson's is the fullest.

Among the Thallophyta, certain groups have been more satisfactorily treated than others—*e.g.* the Discomycetes, the Uredineæ and Ustilagineæ, the Myxomycetes, and certain others among the fungi, and the Desmidiaceæ among the algæ; but the Thallophyta as a whole are much in need of thorough revision to place them on a footing either satisfactory or comparable to their treatment in other countries.

Of the Thallophyta, many more of the smaller species will probably be discovered within our islands when close search is made, if we may judge by the much more numerous forms already recorded in certain groups abroad, and which almost certainly exist here also; but among the higher plants it is not likely that many additional species will be discovered as native, yet even among these some will probably be found. It is, however, rather in the direction of fuller investigation of the distribution and tendencies to variation within our islands that results of interest are likely to be obtained.

The labours of H. C. Watson gave a very great stimulus to the study of the distribution of the flora in England and Scotland, and the work he set on foot has been taken up and much extended by numerous botanists in all parts of the British Islands. It is largely owing to such work and to the critical study of the flora necessary for its prosecution that so many additions have been made to the forms previously known as British. Many local works have been issued in recent years, often of a very high standard of excellence. Besides these larger works, scientific periodicals and transactions of field clubs and other societies teem with records, some of them very brief, while others are of such size and compass that they might have been issued as separate books. A few of both the books and papers are little more than mere lists of names of species and varieties observed in a locality during a brief visit; but usually there is an attempt at least to distinguish the native or well-established aliens from the mere casuals, if these are mentioned at all. In respect of aliens or plants that owe their presence in a district to man's aid, intentional or involuntary, their treatment is on no settled basis. Every flora admits without question species that are certainly of alien origin, even such weeds of cultivated ground as disappear when cultivation is given up, as may be verified in too many localities in some parts of our country. Yet other species are not admitted, though they may be met with here and there well established, and at least as likely to perpetuate their species in the new home as are some native species.

Comparatively few writers seek to analyse the floras of the districts treated of with a view to determine whence each species came and how, its relation to man, whether assisted by him in its arrival directly or indirectly, whether favoured or harmfully affected by him, its relations to its environment—especially to other species of plants and to animals, and other questions that suggest themselves when such inquiries are entered on. It is very desirable that a careful and exhaustive revision of the British flora should be made on these and similar lines. In such a revision it is not less desirable that each species should be represented by a good series of specimens, and that these should be compared with similar series from other localities within our islands, and from those countries from which it is believed that the species originally was sprung. Such careful comparison would probably supply important evidence of forms being evolved in the new environments, differing to a recognisable degree from the ancestral types, and tending to become more marked in the more distant and longer isolated localities. An excellent example of this is afforded by the productive results of the very careful investigation of the Shetland flora by the late Mr. W. H. Beeby.

Within recent years excellent work has been done in the study of plant associations, but the reports on these studies are dispersed in various journals (often not botanical), and are apt to be overlooked by, or to remain unknown to, many to whom they would be helpful. The same is true in large measure of the very valuable reports of work done on plant-remains from peat-mosses, from lake deposits, and from other recent geological formations, researches that have cast such light on the past history of many species as British plants, and have proved their long abode in this country. Mr. Clement Reid's "Origin of the British Flora," though published in 1899, has already (by the work of himself and others) been largely added to, and the rate of progress is likely to become still more rapid. Among the fruits and seeds recorded from inter-glacial and even from pre-glacial deposits are some the presence of which could scarcely have been anticipated, *e.g.* *Hypecoum procumbens*, in Suffolk. Some of the colonists, or aliens now almost confined to ground under cultivation, have been recorded from deposits that suggest an early immigration into the British Islands. While much remains to be discovered, it is desirable that what is already established should find a place in the manuals of British botany.

Apart from the descriptive and topographical works and papers on our flora, there is a serious lack of information gained from the study of our British plants. Although a few types have received fuller study, we have little to compare with the work done in other countries on the structure and histology of our plants, on the effects of environment, on their relations to other species and to animals, and on other aspects of the science to which attention should be directed. On these matters, as on a good many others, we gain most of what information can be had, not from British sources, but from the literature of other countries, though it is not wise to assume that what is true elsewhere is equally true here. It is as well, perhaps, that for the present such subjects should find scanty reference in the manuals in ordinary use; but, when trustworthy information has been gained within the British Islands, under the conditions prevailing here, these topics should certainly not be passed over in silence. Students of the British flora have as yet no such works of reference as Raunkjaer's book on the Monocotyledons of Denmark or the admirable "Lebensgeschichte der Blütenpflanzen Mitteleuropas," at present being issued by Drs. Kirchner, Loew, and Schröter.

In a complete survey of the British botany there must be included the successive floras of the earlier geological formations, though they cannot as yet be brought into correlation with the recent or existing floras. In the brilliant progress made recently in this field of study our country and the British Association are worthily represented.

The present provision for the study of the British flora and the means that should be made use of for its extension appear to be these:—

Much excellent work has already been accomplished and put on record towards the investigation of the flora, but much of that store of information is in danger of being overlooked and forgotten or lost, owing to the absence of means to direct attention to where it may be found. A careful revision of what has been done and a systematic subject-index to its stores are urgently required.

The systematic works treating of the flora are in great part not fully representative of the knowledge already possessed, and require to be brought up to date or to be replaced by others.

Great difficulty is caused by the absence of an authoritative synonymic list that would show so far as possible the equivalence of the names employed in the various manuals and lists. There is much reason to wish that uniformity in the use of names of species and varieties should be arrived at, and a representative committee might assist to that end; but, in the meantime, a good synonymic list would be a most helpful step towards relieving a very pressing obstacle to progress.

There is need for a careful analysis of the flora with the view of determining those species that owe their presence here to man's aid, intentional or unconscious; and the inquiry should be directed to ascertain the periods and methods of introduction, any tendencies to become modified in their new homes, their subsequent relations with man, and their influence on the native flora, whether direct or by modifying habitats, as shown by *Lupinus nootkatensis* in the valleys of rivers in Scotland.

Those species that there is reason to regard as not having been introduced by man should be investigated as regards their probable origins and the periods and methods of immigration, evidence from fossil deposits of the period during which they have existed in this country, their constancy or liability to show change during this period, their resemblance to or differences from the types in the countries from which they are believed to have been derived, or the likelihood of their having originated by mutation or by slow change within the British Islands, and their relation to man's influence on them (usually harmful, but occasionally helpful) as affecting their distribution and permanence.

The topographical distribution, though so much has been done in this field during the past sixty or seventy years, still requires careful investigation to determine, not merely that species have been observed in certain districts, but their relative frequency, their relations to man (natives of one part of our country are often aliens in other parts), whether increasing or diminishing, altitudes, habitats, &c. From such a careful topographical survey much should be learned of the conditions that favour or hinder the success of species, of the evolution of new forms and their relation to parent types in distribution, especially in the more isolated districts and islands, and of other biological problems of great interest. A most useful aid towards the preparation of topographical records would be afforded by the issue at a small price of outline maps, so as to allow of a separate map being employed for recording the distribution of each form.

A careful study of the flora is also required from the point of view of structure and development, with comparison of the results obtained here with those of workers in other countries where the same or closely allied species and varieties occur. It is also needed in respect of the relations between the plants and animals of our islands, both as observed here and in comparison with the already extensive records of a similar kind in other countries. On such topics as pollination, distribution of seeds, and injuries inflicted by animals and galls produced by animals or plants we have still to make use very largely of the information gained abroad; and the same holds good with regard to the diseases of plants.

While "English Botany" in its first edition was deservedly regarded as a work of the first rank among floras, it has long been defective as representing our present knowledge of British plants, and it has not been succeeded by any work of nearly equal rank, while other countries now have their great floras of a type in advance of it. There is need for a great work worthy of our country, amply illustrated so as to show, not only the habit of the species and varieties, but also the distinctive characters and the more important biological features of each. Such a flora would probably require to be in the form of monographs by specialists, issued as each could be prepared, but as part of a well-planned whole. It should give for each plant far more than is contained in even the best of our existing British floras. Means of identification must be provided in the description, with emphasised diagnostic characters; but there should also be the necessary synonymy, a summary of topographical distribution, notes on man's influence upon distribution, abundance, &c., on any biological or other point of interest in structure or relations to habitat, environment, associated animals or plants, diseases, &c. Local names, uses, and folklore should also be included; and for this the need is all the greater, because much of such old lore is rapidly being forgotten and tends to be lost. In a national flora there should be included an account of the successive floras of former periods, and, so far as possible, the changes that can be traced in the existing flora from its earliest records to the time of issue should be recorded.

A flora of this kind would not only afford the fullest possible information with regard to the plant world of the British Islands at the date of issue, but would form a standard with which it could be compared at later periods, so as to permit of changes in it being recognised and measured. In the meanwhile, the production of such a flora can be regarded only as an aim towards which to press on, but which cannot be attained until much has been done. But while the fulfilment must be left to others, we can do something to help it on by trying to remove difficulties from the way, and to bring together materials that may be used in its construction.

I have sought to direct attention to the difficulties that I have experienced and to directions in which progress could be made at once, and to provision which should be made for the advancement of the study of the British flora with as little delay as possible. There is, I feel assured, the means of making far more rapid and satisfactory progress towards the goal than has yet been accomplished. Many persons are interested in the subject, and would gladly give their aid if they knew in what way to employ it to the best purpose. As a nation we are apt to trust to individual rather than to combined efforts, and to waste much time and labour in consequence, with discouragement of many who would gladly share the labour in a scheme in which definite parts of the work could be undertaken by them.

I believe that a well-organised botanical survey of the British Islands would give results of great scientific value, and that there is need for it. I believe, also, that means exist to permit of its being carried through. There is no ground to expect that it will be undertaken on the same terms as the Geological Survey. A biological survey must be accomplished by voluntary effort, with possibly some help towards meeting necessary expenses of equipment from funds which are available for assistance in scientific research. Is such a survey not an object fully in accord with the objects for which the British Association exists? In the belief that it is so, I ask you to consider whether such a survey should not be undertaken; and, if you approve the proposal, I further ask that a committee be appointed to report on what steps should be taken towards organising such a survey, and preparing materials for a national flora of the British Islands.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—On Saturday last, October 1, Dr. Mason, the outgoing Vice-Chancellor, announced in his valedictory address to the Senate two munificent benefactions which have recently been offered to the University. The Drapers' Company, which has already done so much for the Agricultural School, has offered a sum of 22,000*l.* towards the cost of erecting a new physiological laboratory on the Downing site, and a further sum of 1000*l.* for fittings. The proposed new laboratory for psychophysics, the cost of which has been collected by Dr. C. S. Myers, will, it is hoped, be erected in the close neighbourhood of the proposed building for physiology.

Since the foundation of the Schröder chair for German, the Cambridge Association has been turning its attention to the further endowment of the teaching of English. Through the instrumentality of Lord Esher, one of their members, Sir Harold Harmsworth became interested in the project, and he has very generously offered to endow a chair of English language and literature by presenting the University with 20,000*l.* The professor is to be called the King Edward VII. professor, and will be elected by the Crown.

The next combined examination for sixty-seven entrance scholarships and a large number of exhibitions at Pembroke, Gonville and Caius, King's, Jesus, Christ's, St. John's, and Emmanuel Colleges will be held on Tuesday, December 6, and following days. Mathematics, classics, and natural sciences will be the subjects of examination at all the above-mentioned colleges. Forms of application for admission to the examination may be obtained at the respective colleges.

THE Child Study Society has arranged a number of lectures and discussions on the recreational activities of children, to be delivered at the Royal Sanitary Institute between now and Christmas. The programme includes the following subjects:—October 13: Some first results of an investigation into the play interests of English elementary-school children, Miss Alice Ravenhill; October 27: games and toys for children under eight, Miss Clara E. Grant; November 3: story of some children's games, Mrs. Lawrence Gomme; November 17: the origin of certain games and toys, Dr. A. C. Haddon, F.R.S.; November 24: philosophy of boys' games, Mr. Felix Clay; December 1: the child's inheritance, Dr. C. W. Saleeby.

THE sixty-second session of the Bedford College for Women begins to-day. The college was founded in 1849 by Mrs. Elizabeth Jesser Reid, with the intention of offering to women the opportunity of a liberal education in the higher branches of knowledge. The number of students has increased steadily. We notice from the current calendar of the college that in 1889 the number of students was 145, in 1899 226, and in 1909 357. It will be remembered that the institution is now one of the constituent colleges of the University of London, and prepares its students for degrees in arts, science, and medicine. It is hoped that the new buildings of the college at York Gate, Regent's Park—which will provide accommodation for from 400 to 500 students, with residence for about a quarter of the number—will be ready for occupation in 1912.

THE new chemical and physiological laboratories for the University of Bristol are now complete, and were opened for students this week. The formal opening will take place on November 15 by Lord Winterstoke, Chancellor of the University. The new chemical department consists of thirty rooms and laboratories, and contains working places for two hundred students. The main laboratory is capable of accommodating eighty students working at one time. The laboratories have been wired for electrical experiments and so on, and heavy currents from the city supply are available for electro-metallurgical and physico-chemical investigations. Smaller laboratories, specially designed and equipped for physical, organic, biological, and photographic chemistry, have been provided. The department of physiology is arranged to accommodate fifty students. The main lecture theatre has seats for about 120, and is served by a preparation room, store, and museum, all on the same floor. Chemical physiology is taught in a special laboratory. Optical work, photography, and gas analysis are allotted a fine room, to which is attached a well-ventilated dark-room of ample size. Experimental physiology has its own laboratory, and histology is housed in one of the finest rooms in the building, with north light, weaving-shed roof, and a gallery over. There is also a demonstration theatre, built on the model of the operating theatre of a hospital. Research is amply provided for; there is a room for the preparation of electrometer and other records by photography, and a fine suite of rooms apart from the teaching laboratories. Incubator room, constant temperature room, and cold store are also provided. Altogether, between twenty and thirty rooms are comprised in the department, and they are thoroughly convenient and up-to-date. It is noteworthy that nearly 50,000*l*. has been expended on these additions.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 26.—M. Bouchard in the chair.—The president announced the death of Mme. Pasteur.—M. **Darboux** presented vol. xiv. of the *Travaux et Mémoires du Bureau international*, containing a full account of the measurements of the exact volume of the kilogram of water. Three different methods have been employed, and the mean of the closely concordant results gives 1·000027 cubic decimetres as the volume of the kilogram of water at 4° C. and under normal atmospheric pressure.—A. **Laveran**: The treatment of different trypanosomiases by arsenic and antimony emetic. The compound used was obtained in large crystals by crystallising together under certain conditions aniline-arsenyl-tartrate and aniline-antimonyl-tartrate. Details of the methods and dosage are given. Fifteen guinea-pigs infected with *T. evansi*, *T. gambiense*, *T. dimorphon*, or by *T. congolense* were cured. In four of these cases there was a relapse, which was cured by a second treatment. The possibilities of application to the human subject are discussed.—R. **Bourgeois**: The comparison of two astronomical pendulums with the aid of electrical signals transmitted by a submarine cable of great length. A Thomson siphon recorder was modified in a manner to suit this work. The method will be used to determine the difference of time between Brest and Dakar, a distance of about 4500 kilometres.—A. **Demoulin**: The families of Lamé composed of surfaces possessing singular points.—Gaston **Darboux**: Remarks on the preceding communication.—Carl **Störmer**: The canonical forms of the general equations of motion of a particle in a magnetic field and an electric field superposed.—H. **Truc** and C. **Fleig**: Experimental ocular action of the dust on tarred roads. Dust from tarred roads is shown experimentally to be capable of provoking much more serious eye troubles than dust from untreated roads, and the smaller the lapse of time since the road has been tarred the more serious are the lesions produced. —H. **Guillemard** and G. **Regnier**: Observations on animal calorimetry made on Mt. Blanc. Increase of altitude has no sensible effect on the body temperature, but there is a marked increase in the amount of heat evolved by the body as the altitude increases, amounting to more than 30 per cent. between Chamonix and the summit of Mt. Blanc. A discussion of the results leads to the conclusion that protection against the cold is the best way of combating mountain sickness.—Charles **Nicolle** and E. **Conseil**: Properties of the serum of convalescents and animals cured of exanthematic typhus. Serum collected from the ninth to the fourteenth day of convalescence has well-marked preventive and curative properties against the disease. The curative effects disappear if the serum is collected later.—Joseph **Roussel**: The mode of formation of tricalcium phosphate in Algeria and Tunis.

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