THURSDAY, SEPTEMBER 11, 1913.

THE STATE MYCOLOGIST IN THE COLONIES.

Handbook of Fungus Diseases of the Potato in Australia and their Treatment. By D. McAlpine, Department of Agriculture, Victoria. Pp. iii+215+1 map. (Melbourne: J. Kemp, Government Printer, 1912.)

THIS handbook records matters of scientific interest to economic mycologists in all countries, while, at the same time, it is written with the aim (which is certainly achieved) of giving valuable information and assistance in practical matters to the Australian farmer. In the latter direction Dr. McAlpine has performed, indeed, a public service.

The book deals in detail with *Phytophthora infestans, Alternaria solani,* Rhizoctonia, "scab," *Fusarium solani,* and *Bacillus solanacearum,* and is illustrated with 158 excellent photographs. In a very instructive appendix are given the various legislative "orders" relating to potato diseases which have been put into force under the "Vegetation Diseases Acts" and "Quarantine Act" of Australia.

The author states that in Australia the annual value of the crop is more than one-and-a-half million pounds sterling. He rightly insists on the economic importance of all steps being taken by the State, and by the individual, to safeguard the health of the crop, and remarks:—"Some of the worst diseases with which the [potato] grower has to contend in Britain and elsewhere are not known here, simply because the fungi causing them have not been introduced, and with a Quarantine Act in existence they are not likely to be."

The *educational* value of the legislation against plant-diseases is shown by the following observation:—

"There is a widespread desire on the part of growers to know more about the diseases of the potato, for the ignorance of the past can no longer be tolerated, since there is a rigid system of inspection to prevent diseased tubers passing from one State to another."

With regard to all the diseases mentioned, a number of highly interesting experiments and observations are recorded. In the case of *Phytophthora infestans* it is shown that sporangia can infect the unbroken skin of healthy tubers; and that the mycelium can remain living in a dried-up tuber ("as dead as a mummy," to quote the author's words) for more than four months. Interesting facts are given as to the different characteristics the disease shows in Australia, as compared with Great Britain and particularly Ireland; while the tubers are attacked in Australia, there is, generally speaking—owing to the prevailing hotter weather—no sudden blackening and decay of the tops. This difference not unnaturally led potato-growers in Australia, particularly those who had had experience of the "blight" in Ireland, to the error of supposing that the two diseases were distinct. (The speculation naturally suggests itself whether a distinct form of *P. infestans* may not arise in Australia, specialised to these new conditions of temperature and humidity.) It is found that the "kangaroo apple" (*Solanum aviculare*) serves as a host-plant; while it is stated that *S. nigrum,* a common weed in Australia, appears to be immune. Another very interesting fact which has been ascertained is the undoubted connection of the severity of the disease with the rainfall in different districts.

A full discussion is given of the life-history of *P. infestans.* Here, as with so many common fungous diseases, there are important gaps in our knowledge waiting to be filled; it is necessary to take every opportunity to point out that in no department of botany is research needed more than in economic mycology. The author remarks: "probably the mycelium remains dormant in the [potato] stalks"; such statements are to be deprecated—it is better to admit the absence of definite knowledge.

A series of experiments showed that the Fusarium of the potato and tomato are transferable; excellent photomicrographs are given of the life-history and stages of development of this fungus.

E. S. S.

PHYSICAL TRAINING.

Ma Leçon-Type d'entraînement complet et utilitaire. By Lieut. G. Hébert. Pp. 208. (Paris: Librairie Vuibert, 1913.) Price 1.75 francs.

ON reading this book most people interested in physical training will agree that although the training it describes may be *utilitaire,* it is certainly not *complet.* The training aims at the acquirement of strength and endurance, as do all forms of physical training and many forms of sport. But M. Hébert's system lacks the completeness and variety of the Swedish system, which is the system now used in our Army and Navy and in an increasing number of schools in this country. There is nothing M. Hébert claims for his method which is not equally true of the Swedish method.

M. Hébert divides exercises into seven classes: (1) marching; (2) running; (3) jumping; (4) climbing; (5) lifting; (6) throwing; (7) exercises of defence—boxing and wrestling. What are called balance movements in the Swedish system, as well as abdominal movements, are included in a truly "utilitarian" manner in climbing exercises. So far as abdominal movements are concerned, this does not allow graduation of resistance, which is a matter of importance. There is little variety of limb exercises, and the group of back exercises, so valuable for their corrective effect in regard to the faulty positions assumed in most occupations, are entirely omitted. Head and neck exercises are also omitted, while lateral trunk movements are only used as applied movements, and are therefore not graduated.

The more definite grouping of movements in the Swedish system allows of a more graduated and equal training for each part of the muscular system, each group of muscles, and, therefore, of the body as a whole. Free trunk movements, lateral and otherwise, are obviously of value in acquiring strength, flexibility, and complete coordination. Free arm movements are of greater value for the normal development of the arms and chest than movements of lifting and throwing.

In a Swedish gymnastic lesson the hardest work is performed in the middle of the lesson, the amount of exertion being graduated throughout. M. Hébert also graduates the amount of work, but leaves the hardest work to be performed towards the end of the lesson. The effect of the former method on circulation and respiration, and on the distribution of blood in the body, is more desirable physiologically.

M. Hébert lays due stress on the importance of deep breathing, and of complete expiration as well as inspiration. On the other hand, he recommends as an exercise slow marching, breathing in during four to six steps, breathing out during four to six steps, either with or without an intermediate respiratory pause with expanded chest for one or two steps. This method of breathing would undoubtedly tend to produce over-expansion of the chest, with loss of elasticity and accompanying emphysema. Marching with the hands crossed behind the back (Fig. 18) also tends to fix the chest and hinder complete expiration, besides producing a bad position of the shoulders and often hollow back. Sun and air baths and gymnastics in the open air are recommended, the body being uncovered except for a short pair of drawers, unless the weather is specially inclement, or the sun very hot, when a protective covering for the head and back of the neck is added.

The illustrations as a whole are excellent, although some of those which illustrate jumping appear to show that the height of the jump is considered more than the correctness of the attitude in jumping (Fig. 94).

The book is arranged clearly, and is much more readable than the more severe and "complete" gymnastic treatises. MINA L. DOBBIE.

OUR BOOKSHELF.

L'Aviation. By Prof. P. Painlevé, Prof. E. Borel, and Ch. Maurain. Sixième edition. Pp. viii+298. (Paris: Librairie Félix Alcan, 1913.) Price 3 francs 50 centimes.

WRITTEN by well-known members of the French aeronautical professions, the book provides a simple and interesting account of the position of aviation at the present time. The text rarely deals quantitatively with the problems connected with the construction or motion of an aeroplane, calculations being left to notes at the end of the book; the notes occupy about one-third of the whole, and refer to investigations many of which are the original work of the authors.

Only a very short historical portion is provided as introduction, but it is interesting to find that the work of Sir George Cayley, as a pioneer in the development of the theory of the aeroplane, is prominently referred to. The "coming of the aeroplane" is dated from the autumn of 1908, when the flights of Farman, Delagrange, Blériot, and the striking achievements of the Wright Brothers began to attract attention; the subsequent rapid development of aviation and its causes are referred to and illustrated by means of a record of the best results obtained at successive stages.

A discussion of the flight of birds in winds, on the lines of Langley's "Internal work of the Wind," is used to illustrate the possibility of extracting energy from the wind, and so flying without the use of an engine.

Aeroplanes of various types, made in the period 1908–10, are illustrated, and the organs described in some detail. The use of the elevator and rudder in manœuvring is referred to under gliders, but stability, longitudinal and lateral, is referred to power-driven machines. Lateral stability is subdivided into "stability of gyration," that is, stability of direction relative to the wind and "lateral stability" in the limited sense of keeping an even keel. Bryan has shown that the subdivision may be misleading as the two are not independent, and should be treated together.

The book has the advantage of a very complete index, which makes reference a simple matter.

Household Bacteriology: For Students in Domestic Science. By Estelle D. Buchanan and Prof. R. E. Buchanan. Pp. xv+536. (New York: The Macmillan Company; London: Macmillan and Co., Ltd., 1913.) Price 10*s*. net.

"DOMESTIC science" is rightly coming to the fore, and in any course of instruction devoted to this subject, some amount of bacteriology, or "microbiology," as it may preferably be termed, must be included. The present volume practically covers the ground of such knowledge of micro-organisms as is desirable for the domestic science student. It is divided into five sections, dealing respectively with (*a*) morphology and classification; (*b*) cultivation and methods of investigation; (*c*) physiology; (*d*) fermentation; and (*e*) micro-organisms in relation to health and disease. The bacteria, yeasts, and moulds are considered at some length, and a chapter on parasitic Protozoa is included.

The chapters on general morphology and classification are particularly good, and a clear account is furnished of the distinguishing features of the various groups. In an appendix an illustrated key is given of the families and genera of the common moulds; this will be found most useful in the laboratory by other than domestic science students.

The brief description of the optics of the immersion system of lenses is correct, so far as it goes, but a paragraph should have been inserted pointing out that the great advantage of this system is the increased resoluion obtained thereby.

The chapter on food preservation is a useful summary of the subject, but might have been extended with advantage.

The chapters on the nitrogen cycle in nature, alcoholic and other fermentations, enzyme action, and the ripening of certain foods are all satisfactory and convey a considerable amount of coordinated information on these important aspects of microbiology.

Much space (140 pages) is devoted to a consideration of disease and disease-producing micro-organisms, vegetable and protozoal. We think this section could have been somewhat curtailed with advantage, having regard to the avowed limitation of the book; and the space so gained might then have been devoted to a fuller consideration of certain aspects of household microbiology. Such criticisms as have been offered are those of detail, but the book as a whole is an excellent one. It is profusely and well illustrated, and can be strongly recommended not only to the domestic science student, but to a wider public.

R. T. HEWLETT.

Aus Süd-Brasilien. Erinnerungen und Aufzeichnungen. By Dr. W. Breitenbach. Pp. xvi+251. (Brackwede i. W.: Dr. W. Breitenbach, 1913.) Price 3 marks.

THE author has lived in southern Brazil for several years in order to observe the land and the people, especially the German colonies, and beginning in the year 1884, he has since described his observations in more than thirty newspaper articles, essays, and separate pamphlets. Considering the changes which have come about in Brazil within the last thirty years, this book does not pretend to deal with present-day questions of commerce, industry, and general development, or with the colonisation scheme, which, of course, has undergone complete modifications. His personal experiences, narratives of various journeys, are also omitted, having been described elsewhere. It is, therefore, not very obvious why these "reminiscences and notes" should be published now.

The fragmentary chapters on minerals, fauna, and flora are poor. Others, written in the easy, fluent style of feuilletons or causeries, deal with the life in towns, chiefly Porto Alegre, the capital of South Brazilian Germans, whose customs, modes of assimilation, ideals, and successes are compared with those of the Brazilians.

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.*]

Branch Product in Actinium C.

IT is now well established that the atoms of radium C and thorium C can break up in two distinct ways, *i.e.*, with the expulsion of either an α or a β particle. It is to be expected from the close analogy between the C products of the various radio-active families that actinium C should also show abnormal disintegration, and, further, it might be anticipated that one of the branch products would emit an α particle with great velocity and corresponding long range. We have made experiments to test this point. A source of actinium active deposit was covered with a sheet of mica equivalent to about 5 cm. air in stopping power of α particles, and the whole placed in an exhausted chamber with a zinc sulphide screen about 2 cm. from the source. The numbers of scintillations appearing on the screen per minute for different pressures of the air inside the apparatus were counted, and thus the falling off of the α particles with "range" determined. The results showed that in addition to the α particles of actinium C with range 5·4 cm., a small number, about 1 in 600, can penetrate as far as about 6·45 cm. Special experiments showed that the long-range α particles could not be due to radium or thorium impurity, and they must therefore be attributed to the expected new branch product.

In connection with this question, it should be noticed that Mme. Blanquies, in 1910, inferred the existence of two α-ray products in the active deposit of actinium from the shape of the Bragg ionisation curve. The small fraction of long-range α particles found in our experiments, viz., 1 in 600, is, however, quite insufficient to be reconciled with her results. We are therefore repeating her experiments.

E. MARSDEN.
R. H. WILSON.

University of Manchester, September 3.

The Terrestrial Distribution of the Radio-elements and the Origin of the Earth.

IN NATURE of June 19 and August 7, 1913, Mr. Holmes, in two interesting letters, shows on the basis of the planetesimal hypothesis how a concentration of the radio-elements might possibly take place in the earth's crust with their absence at depth to satisfactorily account for the observed temperature gradient of the earth; and in his latest communication he indicates how the inhibition of radio-activity by pressure might bring about the same result. But the terrestrial distribution of these elements seems to be of further importance in that it may enable us to determine whether our earth had a stellar or a planetesimal origin.

On the stellar hypothesis, the earth would be partly developed by a process of oxidation—practically the same as that by which we manufacture steel from impure cast-iron at the present day—and whereby all the impurities would be removed to the surface to form the primal crust. The first of this primal crust would be the most acidic, and the last the most basic; and in the metallic core left, even the oxygen which had been the means of removing all the impurities would itself be undetectable. Thorium is but a higher member of the silicon (carbon) family as uranium is of the oxygen family, and the conditions which remove the lower members should be effective in removing the higher ones also. So, too, it seems only natural that the most acidic rocks—that is, the rocks containing the greatest proportions of silicon and of oxygen—should at the same time be associated with the greatest proportions of thorium and of uranium, which are but the highest members of the silicon and oxygen families.

On the planetesimal hypothesis a similar distribution of the radio-elements can hardly be imagined. To get a metallic core that shall be free from thorium and uranium, we have to imagine the planetesimals undergoing individually the oxidation process which has just been sketched, unless the planetesimals happened to be fragments of a preformed stellar mass. Provided all went well, when these planetesimals were piling themselves together to form the earth, the result would be a metallic core free from thorium and uranium, but surrounded by a crust in which these elements would be uniformly distributed. As the acidic rocks differ only in degree from the basic rocks, it would be impossible for the former to rise up through miles of a mixture of both to form an acidic layer, as happens in the case of a stellar earth. Subsequent aqueous action is relatively negligible in both cases.

It is probably the exigencies of the planetesimal hypothesis that constrain Mr. Holmes to state that there is "clearly a marked antipathy between the radio-elements and native iron, for in all the terrestrial examples which have been examined uranium and thorium are barely detectable." As a matter of fact, these elements alloy with iron and nickel, which are the constituents of native iron; and their marked absence is a proof that native iron had undergone an oxidation process at one time in its history, and so had its thorium and uranium removed.

95 Bath Street, Glasgow. GEORGE CRAIG.

THE INTERNATIONAL UNION FOR SOLAR RESEARCH.

THE fifth conference of the International Union for Cooperation in Solar Research was held at Bonn, by invitation of Prof. Kayser, from July 31 to August 5. The attendance was about 100, including delegates from nearly every country in Europe, and a large contingent from America. In the absence of Prof. Hale and Dr. Schuster, through sickness, the executive committee was ably represented by Prof. Turner. As on former occasions, the chief business at the general meetings was to receive and discuss the reports of the various committees appointed for the organisation of observations and methods of reduction.

An important part of the work of the union has been that relating to standards of wave-length. At the last meeting, held at Mount Wilson, California, in 1910, it was believed that a final set of standards was well within sight, but further investigations have revealed unexpected difficulties. It has, in fact, been found by Goos and others that the wave-lengths of many of the iron lines vary slightly with the length of the arc and the portion of the arc observed. Fortunately, most of the lines already adopted as secondary standards, from interferometer determinations, preserve their positions under a variety of circumstances, but they are not sufficient in number. Hence, it has become necessary to attempt the definition of a standard iron arc for determinations of further standards, and for the production of reference spectra to be used in the determination of wave-lengths by interpolation. The committee recommended the following specification for the iron arc: (1) Length of arc, 6 mm. (2) For lines of wave-length greater than 4000, current to be 6 amperes, and for lines of shorter wave-length 4 amperes, or possibly less. (3) Direct current, positive pole above negative, P.D. of 220 volts, electrodes being iron rods of 7 mm. diameter. (4) An axial portion of the arc, at its middle, about 2 mm. in length, to be used as the source of light. Cooperation in the determination of tertiary standards is desired from all who possess concave gratings, plane gratings, or prisms of sufficient dispersion and resolving power, and to this end it is recommended that additional secondary standards be determined with the interformeter, so that the interpolation method may be used with greater exactness.

At the Mount Wilson meeting a committee on the determination of the solar rotation by means of the displacements of lines was formally organised, and a programme of research agreed upon. Each cooperating observer undertook observations, at specified latitudes, in a definite region of the spectrum, in addition to a control region common to all the observers, and lines were to be selected so as to include elements of widely different atomic weights. It now appears that different observers may obtain results differing systematically by as much as 10 per cent. from one another, and that serious discrepancies have also been found in the results obtained by different observers from measurements of the same photographs. The committee accordingly recommended that, before proceeding further, investigations of these sources of error should be made by determinations of velocity at the solar equator by as many different methods as possible.

Satisfactory progress in work with the spectroheliograph was reported, but the Committee hoped that additional observatories would install instruments of high dispersion, in order that filaments and alignments in the upper atmosphere might be more completely recorded. As the result of representations made by Prof. Ricco and by the Royal Astronomical Society, the title of the spectroheliograph committee was changed to "Committee on Solar Atmosphere," so as to include and unify all the observations on the solar atmosphere, visual and photographic, except those associated with eclipses. The organisation directed by Prof. Ricco, and other observers of prominences, were thus given the opportunity of closer connection with the union. After some discussion, a subcommittee for visual observations of prominences and related phenomena was appointed, with Prof.

Ricco as chairman and Father Cortie as secretary. The chief recommendations subsequently made by the sub-committee were that the limiting height of prominences for general statistical purposes should be 30″, and that results should be expressed as profile areas, the conventional "prominence unit" being defined as the area covered by an arc of 1 degree along the sun's limb and 1 second of arc of the celestial sphere in height. It is hoped that by this means it may be possible to combine more successfully the records made at different observatories.

From the report presented by the committee on the spectra of sun spots, it appears that spots observed during the recent minimum did not differ appreciably from those observed at maximum, so far as could be determined with instruments of moderate dispersion. It is, however, considered desirable that the systematic visual observations initiated by the committee should be continued at least until 1916, so as to include a complete cycle of eleven years. The organisation of photographic investigations of spot spectra has been undertaken, and several observers have already agreed to cooperate in preparing a much-needed catalogue of lines affected in spots, and also in other investigations.

An important outcome of the Mount Wilson meeting was the extension of the scope of the union so as to include astrophysics in general. The committee then appointed to consider the possibility of securing uniformity in the classification of stellar spectra has collected opinions from a great number of workers, and reports that a provisional preference for the Draper classification is nearly unanimous. At the same time, the general feeling is opposed to immediate committal to any system, and an effort will be made to secure the material necessary for the establishment of a classification that can be recommended for permanent and universal adoption.

The formal proceedings were varied by addresses and papers on subjects coming within the scope of the union, and by several interesting social gatherings. The receptions by the municipalities of Bonn and Cologne, and by Prof. and Frau Küstner at the Bonn Observatory, will long remain a pleasant memory to those who were privileged to be present, as will also the excursion on the Rhine generously provided by Prof. Kayser. The success of the conference as a whole was largely due to the admirable arrangements made by Profs. Kayser and Pflüger.

The next conference will be held at Rome in 1916. A. FOWLER.

THE BRITISH ASSOCIATION.

ARRANGEMENTS FOR THE BIRMINGHAM MEETING.

THE meeting, which began on September 10 in Birmingham under the presidency of Sir Oliver Lodge, F.R.S., promises to be a large and important one. The number, both of local and of visiting members and associates, amounts at the time of writing to about 2600.

Among the corresponding and foreign representatives are:—Sir H. Angst (Consul-General, Zurich); Prof. Svante Arrhenius (Stockholm); Prof. Bemmelen (Groningen); Prof. H. Braus (Heidelberg); Prof. Capitan (Paris); Prof. Chodat (Geneva); Madame Curie (Paris); Dr. Dollo (Brussels); Prof. Lorentz (Leiden); Prof. Reinke (Kiel); Prof. Keibel (Freiburg); Dr. Versluys (Giessen); Dr. Gregory (New York); Prof. Pringsheim (Breslau); Prof. Sörensen (Copenhagen); and Prof. R. W. Wood (Baltimore). It is hoped that others may be able to attend the meeting.

A new procedure has been adopted this year with a view to reducing the pressure on the reception room during the first day or two of the meeting. To each intending visitor a "Selection Circular" has been addressed, giving a list of the various functions (addresses, discourses, entertainments, and excursions), which have been arranged, with a request that the functions selected should be indicated on the returned half of the circular. The response to this appeal has been very gratifying, as over fifteen hundred replies have already been received. Nevertheless, the work of the reception room officials has been very heavy.

The usual business meetings were held on Wednesday last. At the general committee a deputation from the Corporation and University of Manchester invited the Association to visit that city in 1915. The representatives present were: the Deputy Lord Mayor of Manchester, the Town Clerk, the Vice-Chancellor, Prof. Horace Lamb, F.R.S., Prof. H. B. Dixon, F.R.S., and Mr. Maxwell Garnett. In the evening the President delivered his long-expected address, the contents of which will now have become known.

On Thursday evening (September 11) the Lord Mayor and Lady Mayoress of Birmingham are holding a reception in the Council House Buildings, in which the new art gallery and natural history museum are lodged. Special loan collections have been deposited in the museum during the association week.

The University Degree Congregation takes place on the afternoon of September 11, and a fuller account will be given in our next issue. The list of graduands has been limited to a few distinguished men of science from abroad, representative mainly of the chief European nations. The following is the list of recipients of honorary degrees:—Madame Curie (Sorbonne. Paris), Prof. H. A. Lorentz (Leyden), Prof. Keibel (Freiburg), Prof. R. W. Wood (Baltimore), Prof. Svante Arrhenius (Stockholm). In the unavoidable absence of the chancellor, Mr. Joseph Chamberlain, Vice-Chancellor Barling conferred the honorary degree of LL.D. on the graduands, the ceremony in each case being prefaced by a presentation and speech made by the principal (Sir Oliver Lodge). After the ceremony the various departments of the New University Buildings were visited.

Entertainments and Excursions.

With regard to the entertainments: at the garden party given by Messrs. Cadbury Brothers at Bourneville on Friday, September 12, there will be a masque, folk-dancing, and choral singing by village children and workpeople. On Saturday a long list of excursions fills the bill, and an "Excursion Guide" giving full information of the route followed in each case, together with a full description of the several places to be visited, has been compiled by the chairman and secretary of the Excursions sub-committee (Mr. John Humphreys and Mr. F. B. Andrews), and is obtainable at the reception room; Stratford-on-Avon and Warwick, Coventry and Kenilworth, Worcester; Malvern; Banbury and Compton Wynyates; Lichfield and Wall; Droitwich and Hartlebury Castle, and the Arden villages are some of the chief points. In addition to the general excursions, some of the sections are arranging special visits for their members, particulars of which can be obtained in the reception room. On Saturday evening there will be a military concert and torchlight tattoo in the Botanical Gardens, Edgbaston.

On Sunday (September 14) there will be special services at the principal places of worship in the city. The Bishop of Birmingham is to preach in the Pro-Cathedral.

On Monday evening there will be three entertainments given by the local committee. The Prince of Wales Theatre will present the opera *Orpheus*, under the management of Herr Denhof. The Repertory Theatre will give St. John Hankin's *Return of the Prodigal*, and the New Street Picture House will exhibit films of historical and scientific interest.

Lectures.

The evening discourses take place on September 12 and September 16. The first will be given by Sir Henry Cunynghame, of the Home Office, and will deal experimentally with "Coal Dust Explosions and the Means of Preventing Them." The second—by Dr. Smith Woodward, F.R.S., of the British Museum (Natural History)—will be an illustrated exposition of "Missing Links among Extinct Animals." Both of these lectures will be delivered in the new Central Hall.

This year five "Citizens' Lectures" are to be given to (mainly) working-class audiences at the Digbeth Institute, Birmingham. They are not intended for members, but form an altogether independent scheme, provided partly by the association and partly by the local committee, with a view of interesting those members of the community who cannot join the association. The first lecture will be given by Dr. A. C. Haddon, F.R.S., on the decorative art of Savages. The other lectures are "The Panama Canal," by Dr. Vaughan Cornish, F.R.G.S.; "Recent Work on Heredity, and its Application to Man," by Dr. Leonard Doncaster; "Metals under the Microscope," by Dr. Walter Rosenhain, F.R.S.; and "The Evolution of Matter," by Mr. F. Soddy, F.R.S. The arrangements for these lectures have been entrusted to a committee, on which the Workers' Educational Association, the Birmingham Trades Council, the City Council, and other kindred bodies are represented. The demand for tickets has been very encouraging.

Sectional Proceedings.

A general statement of the addresses and of the chief papers to be delivered during the meeting has already been made public, and it is now only necessary to mention a few of the main topics of interest.

Section A (Physics and Mathematics) is naturally very strong this year, both from the fact that the president is a leading physicist, and also from the support which has been given to the sectional president, Dr. Baker, by the presence of distinguished colleagues, both from this country and from abroad. Among these may be mentioned Lord Rayleigh, Sir J. J. Thomson, Sir Joseph Larmor, Prof. Rutherford, and Prof. Bragg. Attention will be devoted especially to the subjects of radiation, radio-active emanations, and the structure of the atom. Prof. H. H. Turner will demonstrate a seismograph (which was one of the late Dr. Milne's instruments) to be erected in the basement of the University (Mason College). There will be a joint meeting with the Geographical Section on geodetic problems to be held on Tuesday morning, September 16.

The Chemical Section (B) meets in the Technical School. In addition to the usual programme, there will be a discussion on coal and coal-fuels on Monday morning (September 15), and another on radio-active elements and the periodic law on Tuesday, September 16; whilst on Friday morning (September 12) the two divisions of the section will discuss respectively the significance of optical properties of substances and certain problems in metallurgy.

The Geological Section (C) has a full programme, both of papers and excursions dealing with local problems in coal-mining, and in stratigraphical and palæontological geology. Special interest is exhibited in the address by Prof. Lapworth on the geology of the country round Birmingham.

The Zoological Section (Section D) is devoting Friday morning to a discussion on mimicry, the inheritance of melanism, and other problems of especial interest to entomologists, whilst on Monday morning the subject of "Convergence in the Mammalia" will be attractive to geologists, as well as to those distinguished zoologists who are to take a foremost part in the discussion. Many other papers of interest are promised, and on Monday afternoon at 3 p.m. Prof. Minchin is to give a special address on some aspects of sleeping sickness. We may here mention what promises to be one of the most important demonstrations and papers, namely, Prof. Benjamin Moore's "Synthesis of Organic Matter by Inorganic Colloids." This subject will be given jointly to Section D, Section K (Botany), and Section I (Physiology) on Tuesday morning at 10.45. Prof. Braus (of Heidelberg) will give a kinematograph exhibition that morning in the Picture House, New Street, of the development of the heart, and on Thursday afternoon there will be a joint excursion by a limited number (twenty-five) of members of each of the sections of Zoology, Botany, and Agriculture, respectively, to the Burbage Experimental

Station, when Major Hurst will demonstrate his Mendelian experiments.

The Economic Section has a well-organised discussion on inland waterways, to be opened by Lord Shuttleworth on Friday, September 12, and another on prices and the cost of living, on the Monday following. The subject of the "Panama Canal" will be discussed from many points of view, both by this section in the Queen's College, Paradise Street, and by the Engineering Section in the Technical School.

A large number of photographs of the Canal will be shown in the latter meeting room.

The Engineering Section has a full programme. In the Mechanical Sub-section Prof. Burstall has a paper on fuels that should attract attention; in the Electrical one there are contributions to the study of wireless telegraphy, and a paper on electric cooking. The transport and settlement of sand and sand-bars, and the re-construction of Snow Hill Station, Birmingham, are among the topics for discussion by the Mechanical Engineers. A gyroscope will be exhibited by Mr. J. W. Gordon.

Anthropology (Section H) has a long and interesting programme for each day, including Wednesday, September 17. The subject-matter, indeed, is so large and varied that in addition to the large meeting room in the Temperance Hall, Temple Street, a sub-section has been arranged to meet in the University (Mason College), Edmund Street. For details of the work of this section the daily programme must be consulted, and mention here can only be made of the joint discussion with the Education Section on the value of museums, to the report by Dr. Fleure and Mr. T. C. Jones on the ethnology of Wales; the papers by Dr. Flinders Petrie on Egyptian exploration; and to the paper on Palæolithic cave-paintings by Dr. Capitan, of Paris.

Section I (Physiology) suffers to some extent from the meeting during the past week of the International Congress of Physiologists at Groningen; but though many prominent men will thereby be prevented from attending the Birmingham meeting, an interesting debate on the physiology of reproduction is assured, and the psychologists have such a strong programme that their work is to form the basis for an independent sub-section, the problems of which are so closely allied to those of education that the meeting room of the sub-section is placed close to that of Education in the University (Mason College). A joint meeting of their two bodies will discuss "Research in Education" on Monday morning, September 15.

In the Botanical Section there will be, in addition to the usual programme of single papers (which include such interesting topics as "The Preservation of the British Flora" and "The Colours and Pigments of Flowers"), one or more joint discussions. On Friday morning, September 12, there will be a conference with the Agricultural Section on barley production, and probably a second on the fruit industry. The agriculturists are offering a paper on German forestry methods, by Prof. Fraser Storey, that is sure to appeal equally strongly to the Botanical Section.

Meetings devoted to topics of general interest form a distinctive feature of the Education Section. "The Educational Use of Museums" is the title of a discussion, which is strongly supported by the leading directors of our museums, whilst "The Function of the Modern University" is the subject for Friday's discussion. As this is introduced by Sir Alfred Hopkinson, and supported by such speakers as Lord Kenyon, Sir H. Reichel, Sir George Kenrick, Sir James Yoxall, and Miss Burstall, there is sure to be a large attendance.

Lastly, Agriculture (Section M) has a paper of outstanding interest, "The Partial Sterilisation of Soil by means of Caustic Lime," by Dr. Hutchinson and Mr. M. MacLennan, and gives the results of some striking experiments recently carried out at the Rothamsted Laboratory, Harpenden. Contributions from the same laboratory are: "The Relations between Protozoa and Soil-Problems," by Mr. T. Goodey (protozoologist to the University of Birmingham), and "The Weeds of Arable Land," by Dr. Brenchley. Prof. Sörensen will give an account of his recent investigations on cereals, whilst the economic side of agriculture will be represented by Sir Richard Paget's address on the possibilities of partnership between landlord and tenant.

INAUGURAL ADDRESS BY SIR OLIVER J. LODGE, D.SC., LL.D., F.R.S., PRESIDENT.

Continuity.

Natura non vincitur nisi parendo.

FIRST let me lament the catastrophe which has led to my occupying the chair here in this city. Sir William White was a personal friend of many here present, and I would that the citizens of Birmingham could have become acquainted with his attractive personality, and heard at first hand of the strenuous work which he accomplished in carrying out the behests of the Empire in the construction of its first line of defence.

Although a British Association address is hardly an annual stocktaking, it would be improper to begin this year of office without referring to three more of our losses: one that cultured gentleman, amateur of science in the best sense, who was chosen to preside over our jubilee meeting at York thirty-two years ago. Sir John Lubbock, first Baron Avebury, cultivated science in a spirit of pure enjoyment, treating it almost as one of the arts; and he devoted social and political energy to the welfare of the multitude of his fellows less fortunately situated than himself.

Through the untimely death of Sir George Darwin the world has lost a mathematical astronomer whose work on the tides and allied phenomena is a monument of power and achievement. So recently as our visit to South Africa he occupied the presidential chair.

By the third of our major losses, I mean the death of that brilliant mathematician of a neighbouring nation who took so comprehensive and philosophic a grasp of the intricacies of physics, and whose eloquent though sceptical exposition of our laws and processes, and of the modifications entailed in them by recent advances, will be sure to attract still more widespread attention among all to whom the rather abstruse subject-matter is sufficiently familiar. I cannot say that I find myself in agreement with all that Henri Poincaré wrote or spoke in the domain of physics, but no physicist can help being interested in his mode of presentation, and I may have occasion to refer, in passing, to some of the topics with which he dealt.

And now, eliminating from our purview, as is always necessary, a great mass of human activity, and limiting ourselves to a scrutiny on the side of pure science alone, let us ask what, in the main, is the characteristic of the promising though perturbing period in which we live. Different persons would give different answers, but the answer I venture to give is—rapid progress, combined with fundamental scepticism.

Rapid progress was not characteristic of the latter half of the nineteenth century—at least, not in physics. Fine solid dynamical foundations were laid, and the edifice of knowledge was consolidated; but wholly fresh ground was not being opened up, and totally new buildings were not expected.

"In many cases the student was led to believe that the main facts of nature were all known, that the chances of any great discovery being made by experiment were vanishingly small, and that therefore the experimentalist's work consisted in deciding between rival theories, or in finding some small residual effect, which might add a more or less important detail to the theory."—*Schuster.*

With the realisation of predicted æther waves in 1888, the discovery of X-rays in 1895, spontaneous radio-activity in 1896, and the isolation of the electron in 1898, expectation of further achievement became vivid; and novelties, experimental, theoretical, and speculative, have been showered upon us ever since this century began. That is why I speak of rapid progress.

Of the progress I shall say little; there must always be some uncertainty as to which particular achievement permanently contributes to it; but I will speak about the fundamental scepticism.

Let me hasten to explain that I do not mean the well-worn and almost antique theme of theological scepticism: that controversy is practically in abeyance just now. At any rate, the major conflict is suspended; the forts behind which the enemy has retreated do not invite attack; the territory now occupied by him is little more than his legitimate province. It is the scientific allies, now, who are waging a more or less invigorating conflict among themselves, with philosophers joining in. Meanwhile the ancient foe is biding his time and hoping that from the struggle something will emerge of benefit to himself. Some positions, he feels, were too hastily abandoned and may perhaps be retrieved; or, to put it without metaphor, it seems possible that a few of the things prematurely denied, because asserted on inconclusive evidence, may after all, in some form or other, have really happened. Thus the old theological bitterness is mitigated, and a temporising policy is either advocated or instinctively adopted.

To illustrate the nature of the fundamental scientific or philosophic controversies to which I do refer, would require almost as many addresses as there are sections of the British Association, or, at any rate, as many as there are chief cities in Australia; and perhaps my successor in the chair will continue the theme; but, to exhibit my meaning very briefly, I may cite the kind of dominating controversies now extant, employing as far as possible only a single word in each case so as to emphasise the necessary brevity and insufficiency of the reference.

In physiology the conflict ranges round *Vitalism.* (My immediate predecessor dealt with the subject at Dundee.)

In chemistry the debate concerns *Atomic structure.* (My penultimate predecessor is well aware of pugnacity in that region.)

In biology the dispute is on the laws of *Inheritance.* (My nominated successor is likely to deal with this subject; probably in a way not deficient in liveliness.)

And besides these major controversies, debate is active in other sections:—

In education, *Curricula* generally are being overhauled or fundamentally criticised, and revolutionary ideas are promulgated concerning the advantages of freedom for infants.

In economic and political science, or sociology, what is there that is not under discussion? Not property alone, nor land alone, but everything—back to the garden of Eden and the inter-relations of men and women.

Lastly, in the vast group of mathematical and physical sciences, "slurred over rather than summed up as Section A," present-day scepticism concerns what, if I had to express it in one word, I should call *Continuity.* The full meaning of this term will hardly be intelligible without explanation, and I shall discuss it presently.

Still more fundamental and deep-rooted than any of these sectional debates, however, a critical examination of scientific foundations generally is going on; and a kind of philosophic scepticism is in the ascendant, resulting in a mistrust of purely intellectual processes and in a recognition of the limited scope of science.

For science is undoubtedly an affair of the intellect, it examines everything in the cold light of reason; and that is its strength. It is a commonplace to say that science must have no likes or dislikes, must aim only at truth; or as Bertrand Russell well puts it:—

"The kernel of the scientific outlook is the refusal to regard our own desires, tastes, and interests as affording a key to the understanding of the world."

This exclusive, single-eyed attitude of science is its strength; but, if pressed beyond the positive region of usefulness into a field of dogmatic negation and philosophising, it becomes also its weakness. For the nature of man is a large thing, and intellect is only a part of it: a recent part, too, which therefore necessarily, though not consciously, suffers from some of the defects of newness and crudity, and should refrain from imagining itself the whole—perhaps it is not even the best part—of human nature.

The fact is that some of the best things are, by abstraction, excluded from science, though not from literature and poetry; hence perhaps an ancient mistrust or dislike of science, typified by the Promethean legend. Science is systematised and *metrical* knowledge, and in regions where measurement cannot be applied it has small scope; or, as Mr. Balfour said the other day at the opening of a new wing of the National Physical Laboratory:—

"Science depends on measurement, and things not measurable are therefore excluded, or tend to be excluded, from its attention. But life and beauty and happiness are not measurable." And then characteristically he added: "If there could be a unit of happiness, politics might begin to be scientific."

Emotion and intuition and instinct are immensely older than science, and in a comprehensive survey of existence they cannot be ignored. Scientific men may rightly neglect them in order to do their proper work, but philosophers cannot.

So philosophers have begun to question some of the larger generalisations of science, and to ask whether in the effort to be universal and comprehensive we have not extended our laboratory inductions too far. The conservation of energy, for instance: is it always and everywhere valid; or may it under some conditions be disobeyed? It would seem as if the second law of thermodynamics must be somewhere disobeyed—at least, if the age of the universe is both ways infinite—else the final consummation would have already arrived.

Not by philosophers only, but by scientific men also, ancient postulates are being pulled up by the roots. Physicists and mathematicians are beginning to consider whether the long known and well established laws of mechanics hold true everywhere and always, or whether the Newtonian scheme must be replaced by something more modern, something to which Newton's laws of motion are but an approximation.

Indeed, a whole system of non-Newtonian mechanics has been devised, having as its foundation the recently discovered changes which must occur in bodies moving at speeds nearly comparable with that of light. It turns out, in fact, that both shape and mass are functions of velocity. As the speed increases the mass increases and the shape is distorted, though under ordinary conditions only to an infinitesimal extent.

So far I agree; I agree with the statement of fact; but I do not consider it so revolutionary as to overturn Newtonian mechanics. After all, a variation of mass is familiar enough, and it would be a great mistake to say that Newton's second law breaks down merely because mass is not constant. A raindrop is an example of variable mass; or the earth may be, by reason of meteoric dust; or the sun, by reason of radio-activity; or a locomotive, by reason of the emission of steam. In fact, variable masses are the commonest, for friction may abrade any moving body to a microscopic extent.

That mass is constant is only an approximation. That mass is equal to ratio of force and acceleration is a definition, and can be absolutely accurate. It holds perfectly even for an electron with a speed near that of light; and it is by means of Newton's second law that the variation of mass with velocity has been experimentally observed and compared with theory.

I urge that we remain with, or go back to, Newton. I see no reason against retaining all Newton's laws, discarding nothing, but supplementing them in the light of further knowledge.

Even the laws of geometry have been overhauled, and Euclidean geometry is seen to be but a special case of more fundamental generalisations. How far they apply to existing space, and how far time is a reality or an illusion, and whether it can in any sense depend on the motion or the position of an observer: all these things in some form or other are discussed.

The conservation of matter also, that main-mast of nineteenth-century chemistry, and the existence of the æther of space, that sheet-anchor of nineteenth-century physics—do they not sometimes seem to be going by the board?

Prof. Schuster, in his American lectures, commented on the modern receptive attitude as follows:—

"The state of plasticity and flux—a healthy state, in my opinion—in which scientific thought of the present day adapts itself to almost any novelty, is illustrated by the complacency with which the most cherished tenets of our fathers are being abandoned. Though it was never an article of orthodox faith that chemical elements were immutable and would not some day be resolved into simpler constituents, yet the conservation of mass seemed to lie at the very foundation of creation. But nowadays the student finds little to disturb him, perhaps too little, in the idea that mass changes with velocity; and he does not always realise the full meaning of the consequences which are involved."

This readiness to accept and incorporate new facts into the scheme of physics may have led to perhaps an undue amount of scientific scepticism, in order to right the balance.

But a still deeper variety of comprehensive scepticism exists, and it is argued that all our laws of nature, so laboriously ascertained and carefully formulated, are but conventions after all, not truths; that we have no faculty for ascertaining real truth; that our intelligence was not evolved for any such academic purpose; that all we can do is to express things in a form convenient for present purposes and employ that mode of expression as a tentative and pragmatically useful explanation.

Even *explanation*, however, has been discarded as too ambitious by some men of science, who claim only the power to describe. They not only emphasise the *how* rather than the *why*—as is in some sort inevitable, since explanations are never ultimate—but are satisfied with very abstract propositions, and regard mathematical equations as preferable to, because safer than, mechanical analogies or models.

"To use an acute and familiar expression of Gustav Kirchhoff, it is the object of science to *describe* natural phenomena, not to *explain* them. When we have expressed by an equation the correct relationship between different natural phenomena we have gone as far as we safely can, and if we go beyond we are entering on purely speculative ground."

But the modes of statement preferred by those who distrust our power of going directly into detail are far from satisfactory. Prof. Schuster describes and comments on them thus:—

"Vagueness, which used to be recognised as our great enemy, is now being enshrined as an idol to be worshipped. We may never know what constitutes atoms, or what is the real structure of the æther; why trouble, therefore, it is said, to find out more about them. Is it not safer, on the contrary, to confine ourselves to a general talk on entropy, luminiferous vectors, and undefined symbols expressing vaguely certain physical relationships? What really lies at the bottom of the great fascination which these new doctrines exert on the present generation is sheer cowardice; the fear of having its errors brought home to it." . . .

"I believe this doctrine to be fatal to a healthy development of science. Granting the impossibility of penetrating beyond the most superficial layers of observed phenomena, I would put the distinction between the two attitudes of mind in this way: One glorifies our ignorance, while the other accepts it as a regrettable necessity."

In further illustration of the modern sceptical attitude, I quote from Poincaré:—

"Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value. . . .

"The fundamental propositions of geometry, for instance Euclid's postulate, are only conventions; and it is quite as unreasonable to ask if they are true or false as to ask if the metric system is true or false. Only, these conventions are convenient. . . .

"Whether the æther exists or not matters little—let us leave that to the metaphysicians; what is essential for us is that everything happens as if it existed, and that this hypothesis is found to be suitable for the explanation of phenomena. After all, have we any other reason for believing in the existence of material objects? That, too, is only a convenient hypothesis."

As an antidote against overpressing these utterances, I quote from Sir J. Larmor's preface:—

"There has been of late a growing trend of opinion, prompted in part by general philosophical views, in the direction that the theoretical constructions of physical science are largely factitious, that instead of presenting a valid image of the relations of things on which further progress can be based, they are still little better than a mirage. . . .

"The best method of abating this scepticism is to become acquainted with the real scope and modes of application of conceptions which, in the popular language of superficial exposition—and even in the unguarded and playful paradox of their authors, intended only for the instructed eye—often look bizarre enough."

One thing is very notable, that it is closer and more exact knowledge that has led to the kind of scientific scepticism now referred to; and that the simple laws on which we used to be working were thus simple and discoverable because the full complexity of existence was tempered to our ken by the roughness of our means of observation.

Kepler's laws are not accurately true, and if he had had before him all the data now available he could hardly have discovered them. A planet does not really move in an ellipse but in a kind of hypocycloid, and not accurately in that either.

So it is also with Boyle's law, and the other simple laws in physical chemistry. Even Van der Waals' generalisation of Boyle's law is only a further approximation.

In most parts of physics simplicity has sooner or later to give place to complexity; though certainly I urge that the simple laws were true, and are still true, as far as they go, their inaccuracy being only detected by further real discovery. The reason they are departed from becomes known to us; the law is not really disobeyed, but is modified through the action of a known additional cause. Hence it is all in the direction of progress.

It is only fair to quote Poincaré again, now that I am able in the main to agree with him:—

"Take, for instance, the laws of reflection. Fresnel established them by a simple and attractive theory which experiment seemed to confirm. Subsequently, more accurate researches have shown that this verification was but approximate; traces of elliptic polarisation were detected everywhere. But it is owing to the first approximation that the cause of these anomalies was found, in the existence of a transition layer; and all the essentials of Fresnel's theory have remained. We cannot help reflecting that all these relations would never have been noted if there had been doubt in the first place as to the complexity of the objects they connect. Long ago it was said: If Tycho had had instruments ten times as precise, we would never have had a Kepler, or a Newton, or astronomy. It is a misfortune for a science to be born too late, when the means of observation have become too perfect. That is what is happening at this moment with respect to physical chemistry; the founders are hampered in their general grasp by third and fourth decimal places; happily they are men of robust faith. As we get to know the properties of matter better we see that continuity reigns. . . . It would be difficult to justify [the belief in continuity] by apodeictic reasoning, but without [it] all science would be impossible."

Here he touches on my own theme, *Continuity*; for if we had to summarise the main trend of physical controversy at present, I feel inclined to urge that it largely turns on the question as to which way ultimate victory lies in the fight between continuity and discontinuity.

On the surface of nature at first we see discontinuity; objects detached and countable. Then we realise the air and other media, and so emphasise continuity and flowing quantities. Then we detect atoms and numerical properties, and discontinuity once more makes its appearance. Then we invent the æther and are impressed with continuity again. But this is not likely to be the end; and what the ultimate end will be, or whether there is an ultimate end, is a question difficult to answer.

The modern tendency is to emphasise the discontinuous or atomic character of everything. Matter has long been atomic, in the same sense as anthropology is atomic; the unit of matter is the atom, as the unit of humanity is the individual.[1] Whether men or women or children—they can be counted as so many "souls." And atoms of matter can be counted too.

Certainly, however, there is an illusion of continuity. We recognise it in the case of water. It appears to be a continuous medium, and yet it is certainly molecular. It is made continuous again, in a sense, by the æther postulated in its pores; for the æther is essentially continuous. Though Osborne Reynolds, it is true, invented a discontinuous or granular æther, on the analogy of the sea-shore. The sands of the sea, the hairs of the head, the descendants of a patriarch, are typical instances of numerable, or rather of innumerable things. The difficulty of enumerating them is not that there is nothing to count, but merely that the things to be counted are very numerous. So are the atoms in a drop of water—they outnumber the drops in an Atlantic Ocean—and, during the briefest time of stating their number, fifty millions or so may have evaporated; but they are as easy to count as the grains of sand on a shore.

The process of counting is evidently a process applicable to discontinuities, *i.e.*, to things with natural units; you can count apples and coins, and days and years, and people and atoms. To apply number to a continuum you must first cut it up into artificial units; and you are always left with incommensurable fractions. Thus only is it that you can deal numerically with such continuous phenomena as the warmth of a room, the speed of a bird, the pull of a rope, or the strength of a current.

But how, it may be asked, does discontinuity apply to number? The natural numbers, 1, 2, 3, &c., are discontinuous enough, but there are fractions to fill up the interstices; how do we know that they are not really connected by these fractions, and so made continuous again?

(By number I always mean commensurable number; incommensurables are not numbers: they are just what cannot be expressed in numbers. The square root of 2 is not a number, though it can be readily indicated by a length. Incommensurables are usual in physics and are frequent in geometry; the conceptions of geometry are essentially continuous. It is clear, as Poincaré says, that "if the points whose coordinates are commensurable were alone regarded as real, the in-circle of a square and the diagonal of the square would not intersect, since the coordinates of the points of intersection are incommensurable.")

I want to explain how commensurable fractions do not connect up numbers, nor remove their discontinuity in the least. The divisions on a foot rule, divided as closely as you please, represent commensurable fractions, but they represent none of the length. No matter how numerous they are, all the length lies between them; the divisions are mere partitions and have consumed none of it; nor do they connect up with each other, they are essentially discontinuous. The interspaces are infinitely more extensive than the barriers which partition them off from one another; they are like a row of compartments with infinitely thin walls. All the incommensurables lie in the inter-

[1] In his recent Canadian address, Lord Haldane emphasised the fact that though humanity is individually discontinuous it possesses a social and national continuity.

spaces; the compartments are full of them, and they are thus infinitely more numerous than the numerically expressible magnitudes. Take any point of the scale at random, that point will certainly lie in an interspace: it will not lie on a division, for the chances are infinity to 1 against it.

Accordingly incommensurable quantities are the rule in physics. Decimals do not in practice terminate or circulate; in other words, vulgar fractions do not accidentally occur in any measurements, for this would mean infinite accuracy. We proceed to as many places of decimals as correspond to the order of accuracy aimed at.

Whenever, then, a commensurable number is really associated with any natural phenomenon, there is necessarily a noteworthy circumstance involved in the fact, and it means something quite definite and ultimately ascertainable. Every discontinuity that can be detected and counted is an addition to knowledge. It not only means the discovery of natural units instead of being dependent on artificial ones, but it throws light also on the nature of phenomena themselves.

For instance:—

The ratio between the velocity of light and the inverted square root of the product of the electric and magnetic constants was discovered by Clerk Maxwell to be 1; and a new volume of physics was by that discovery opened.

Dalton found that chemical combination occurred between quantities of different substances specified by certain whole or fractional numbers; and the atomic theory of matter sprang into substantial though at first infantile existence.

The hypothesis of Prout, which in some modified form seems likely to be substantiated, is that all atomic weights are commensurable numbers; in which case there must be a natural fundamental unit underlying, and in definite groups composing, the atoms of every form of matter.

The small number of degrees of freedom of a molecule, and the subdivision of its total energy into equal parts corresponding thereto, is a theme not indeed without difficulty but full of importance. It is responsible for the suggestion that energy too may be atomic!

Mendelejeff's series again, or the detection of a natural grouping of atomic weights in families of seven, is another example of the significance of number.

Electricity was found by Faraday to be numerically connected with quantity of matter; and the atom of electricity began its hesitating but now brilliant career.

Electricity itself—*i.e.*, electric charge—strangely enough has proved itself to be atomic. There is a natural unit of electric charge, as suspected by Faraday and Maxwell and named by Johnstone Stoney. Some of the electron's visible effects were studied by Crookes in a vacuum; and its weighing and measuring by J. J. Thomson were announced to the British Association meeting at Dover in 1899—a fitting prelude to the twentieth century.

An electron is the natural unit of negative electricity, and it may not be long before the natural unit of positive electricity is found too. But concerning the nature of the positive unit there is at present some division into opposite camps. One school prefers to regard the unit of positive electricity as a homogeneous sphere, the size of an atom, in which electrons revolve in simple harmonic orbits and constitute nearly the whole effective mass. Another school, while appreciative of the simplicity and ingenuity and beauty of the details of this conception, and the skill with which it has been worked out, yet thinks the evidence more in favour of a minute central positive nucleus, or nucleus-group, of practically atomic mass; with electrons, larger—*i.e.* less concentrated—and therefore less massive than itself, revolving round it in astronomical orbits. While from yet another point of view it is insisted that positive and negative electrons can only differ skew-symmetrically, one being like the image of the other in a mirror, and that the mode in which they are grouped to form an atom remains for future discovery. But no one doubts that electricity is ultimately atomic.

Even magnetism has been suspected of being atomic, and its hypothetical unit has been named in advance the *magneton*; but I confess that here I have not been shaken out of the conservative view.

We may express all this as an invasion of number into unsuspected regions.

Biology may be said to be becoming atomic. It has long had natural units in the shape of cells and nuclei, and some discontinuity represented by body-boundaries and cell-walls; but now, in its laws of heredity as studied by Mendel, number and discontinuity are strikingly apparent among the reproductive cells, and the varieties of offspring admit of numerical specification and prediction to a surprising extent; while modification by continuous variation, which seemed to be of the essence of Darwinism, gives place to, or at least is accompanied by, mutation, with finite and considerable and in appearance discontinuous change.

So far from Nature not making jumps, it becomes doubtful if she does anything else. Her hitherto placid course, more closely examined, is beginning to look like a kind of steeplechase.

Yet undoubtedly continuity is the backbone of evolution, as taught by all biologists—no artificial boundaries or demarcations between species—a continuous chain of heredity from far below the amœba up to man. Actual continuity of undying germ-plasm, running through all generations, is taught likewise; though a strange discontinuity between this persistent element and its successive accessory body-plasms—a discontinuity which would convert individual organisms into mere temporary accretions or excretions, with no power of influencing or conveying experience to their generating cells—is advocated by one school.

Discontinuity does not fail to exercise fascination even in pure mathematics. Curves are invented which have no tangent or differential coefficient, curves which consist of a succession of dots or of twists; and the theory of commensurable numbers seems to be exerting a dominance over philosophic mathematical thought as well as over physical problems.

And not only these fairly accepted results are prominent, but some more difficult and unexpected theses in the same direction are being propounded, and the atomic character of *energy* is advocated. We had hoped to be honoured by the presence of Prof. Planck, whose theory of the *quantum*, or indivisible unit or atom of energy, excites the greatest interest, and by some is thought to hold the field.

Then again radiation is showing signs of becoming atomic or discontinuous. The corpuscular theory of radiation is by no means so dead as in my youth we thought it was. Some radiation is certainly corpuscular, and even the ethereal kind shows indications, which may be misleading, that it is spotty, or locally concentrated into points, as if the wave-front consisted of detached specks or patches; or as J. J. Thomson says, "the wave-front must be more analogous to bright specks on a dark ground than to a uniformly illuminated surface," thus suggesting that the æther may be fibrous in structure, and that a wave runs along lines of electric force; as the genius of Faraday surmised might be possible, in his "Thoughts on Ray Vibrations." Indeed, Newton guessed something of the same kind, I fancy, when he superposed ether-pulses on his corpuscles.

Whatever be the truth in this matter, a discussion on radiation, of extreme weight and interest, though likewise of great profundity and technicality, is expected on Friday in Section A. We welcome Prof. Lorentz, Dr. Arrhenius, Prof. Langevin, Prof. Pringsheim, Prof. R. W. Wood, and others, some of whom have been specially invited to England because of the important contributions which they have made to the subject-matter of this discussion.

Why is so much importance attached to radiation? Because it is the best-known and longest-studied link between matter and æther, and the only property we are acquainted with that affects the unmodified great mass of æther alone. Electricity and magnetism are associated with the modifications or singularities called electrons; most phenomena are connected still more directly with matter. Radiation, however, though excited by an accelerated electron, is subsequently let loose in the æther of space, and travels as a definite thing at a measurable and constant pace—a pace independent of everything so long as the æther is free, unmodified and unloaded by matter. Hence radiation has much to teach us, and we have much to learn concerning its nature.

How far can the analogy of granular, corpuscular, countable, atomic, or discontinuous things be pressed? There are those who think it can be pressed very far. But to avoid misunderstanding, let me state, for what it may be worth, that I myself am an upholder of *ultimate* Continuity, and a fervent believer in the æther of space.

We have already learnt something about the æther; and although there may be almost as many varieties of opinion as there are people qualified to form one, in my view we have learnt as follows:

The æther is the universal connecting medium which binds the universe together, and makes it a coherent whole instead of a chaotic collection of independent isolated fragments. It is the vehicle of transmission of all manner of force, from gravitation down to cohesion and chemical affinity; it is therefore the storehouse of potential energy.

Matter moves, but æther is strained.

What we call elasticity of matter is only the result of an alteration of configuration due to movement and readjustment of particles, but all the strain and stress are in the æther. The æther itself does not move, that is to say it does not move in the sense of locomotion, though it is probably in a violent state of rotational or turbulent motion in its smallest parts; and to that motion its exceeding rigidity is due.

As to its density, it must be far greater than that of any form of matter, millions of times denser than lead or platinum. Yet matter moves through it with perfect freedom, without any friction or viscosity. There is nothing paradoxical in this: viscosity is not a function of density; the two are not necessarily connected. When a solid moves through an alien fluid it is true that it acquires a spurious or apparent extra inertia from the fluid it displaces; but, in the case of matter and æther, not only is even the densest matter excessively porous and discontinuous, with vast interspaces in and among the atoms, but the constitution of matter is such that there appears to be no displacement in the ordinary sense at all; the æther is itself so modified as to *constitute* the matter in some way. Of course, that portion moves, its inertia is what we observe, and its amount depends on the potential energy in its associated electric field, but the motion is not like that of a foreign body, it is that of some inherent and merely individualised portion of the stuff itself. Certain it is that the æther exhibits no trace of viscosity.[2]

Matter in motion, æther under strain, constitute the fundamental concrete things we have to do with in physics. The first pair represent kinetic energy, the second potential energy; and all the activities of the material universe are represented by alternations from one of these forms to the other.

Whenever this transference and transformation of energy occur, work is done, and some effect is produced, but the energy is never diminished in quantity: it is merely passed on from one body to another, always from æther to matter, or *vice versâ*—except in the case of radiation, which simulates matter—and from one form to another.

The forms of energy can be classified as either a translation, a rotation, or a vibration of pieces of matter of different sizes, from stars and planets down to atoms and electrons; or else an æthereal strain which in various different ways is manifested by the behaviour of such masses of matter as appeal to our senses.[3]

Some of the facts responsible for the suggestion that energy is atomic seem to me to depend on the discontinuous nature of the structure of a material atom, and on the high velocity of its constituent particles. The apparently discontinuous emission of radiation is, I believe, due to features in the real discontinuity of matter. Disturbances inside an atom appear to be essentially catastrophic; a portion is liable to be ejected with violence. There appears to be a critical velocity below which ejection does not take place; and, when it does, there also occurs a sudden rearrangement of parts which is presumably responsible for some perceptible æthereal radiation. Hence it is, I suppose, that radiation comes off in gushes or bursts; and hence it appears to consist of indivisible units. The occasional phenomenon of new stars, as compared with the steady orbital motion of the millions of recognised bodies, may be suggested as an astronomical analogue.

The hypothesis of *quanta* was devised to reconcile the law that the energy of a group of colliding molecules must in the long run be equally shared among all their degrees of freedom, with the observed fact that the energy is really shared into only a small number of equal parts. For if vibration-possibilities have to be taken into account, the number of degrees of molecular freedom must be very large, and energy shared among them ought soon to be all frittered away; whereas it is not. Hence the idea is suggested that minor degrees of freedom are initially excluded from sharing the energy, because they cannot be supplied with less than one atom of it.

I should prefer to express the fact by saying that the ordinary encounters of molecules are not of a kind able to excite atomic vibrations, or in any way to disturb the æther. Spectroscopic or luminous vibrations of an atom are excited only by an exceptionally violent kind of collision, which may be spoken of as chemical clash; the ordinary molecular orbital encounters, always going on at the rate of millions a second, are ineffective in that respect, except in the case of phosphorescent or luminescent substances. That common molecular deflections *are* ineffective is certain, else all the energy would be dissipated or transferred from matter into the æther; and the reasonableness of their radiative inefficiency is not far to seek, when we consider the comparatively leisurely character of molecular movements, at speeds comparable with the velocity of sound. Admittedly, how-

2 For details of my experiment on this subject see Phil. Trans. Roy. Soc. for 1893 and 1897; or a very abbreviated reference to it, and to the other matters above-mentioned, in my small book "The Ether of Space."

3 See, in the Philosophical Magazine for 1879, my article on a classification of the forms of energy.

ever, the effective rigidity of molecules must be complete, otherwise the sharing of energy must ultimately occur. They do not seem able to be set vibrating by anything less than a certain minimum stimulus; and that is the basis for the theory of *quanta*.

Quantitative applications of Planck's theory, to elucidate the otherwise shaky stability of the astronomically constituted atom, have been made; and the agreement between results so calculated and those observed, including a determination of series of spectrum lines, is very remarkable. One of the latest contributions to this subject is a paper by Dr. Bohr in *The Philosophical Magazine* for July this year.

To show that I am not exaggerating the modern tendency towards discontinuity, I quote, from M. Poincaré's "Dernières Pensées," a proposition which he announces in italics as representing a form of Prof. Planck's view of which he apparently approves:—

"A physical system is susceptible of a finite number only of distinct conditions; it jumps from one of these conditions to another without passing through a continuous series of intermediate conditions."

Also this from Sir Joseph Larmor's preface to Poincaré's "Science and Hypothesis":—

"Still more recently it has been found that the good Bishop Berkeley's logical jibes against the Newtonian ideas of fluxions and limiting ratios cannot be adequately appeased in the rigorous mathematical conscience, until our apparent continuities are resolved mentally into discreet aggregates which we only partially apprehend. The irresistible impulse to atomise everything thus proves to be not merely a disease of the physicist: a deeper origin, in the nature of knowledge itself, is suggested."

One very valid excuse for the prevalent attitude is the astonishing progress that has been made in actually seeing or almost seeing the molecules, and studying their arrangement and distribution.

The laws of gases have been found to apply to emulsions and to fine powders in suspension, of which the Brownian movement has long been known. This movement is caused by the orthodox molecular bombardment, and its average amplitude exactly represents the theoretical mean free path calculated from the "molecular weight" of the relatively gigantic particles. The behaviour of these microscopically visible masses corresponds closely and quantitatively with what could be predicted for them as fearfully heavy atoms, on the kinetic theory of gases; they may indeed be said to constitute a gas with a gram-molecule as high as 200,000 tons; and, what is rather important as well as interesting, they tend visibly to verify the law of equipartition of energy even in so extreme a case, when that law is properly stated and applied.

Still more remarkable—the application of X-rays to display the arrangement of molecules in crystals, and ultimately the arrangement of atoms in molecules, as initiated by Prof. Laue with Drs. Friedrich and Knipping, and continued by Prof. Bragg and his son and by Dr. Tutton, constitute a series of researches of high interest and promise. By this means many of the theoretical anticipations of our countryman, Mr. William Barlow, and—working with him—Prof. Pope, as well as of those distinguished crystallographers von Groth and von Fedorow, have been confirmed in a striking way. These brilliant researches, which seem likely to constitute a branch of physics in themselves, and which are being continued by Messrs. Moseley and C. G. Darwin, and by Mr. Keene and others, may be called an apotheosis of the atomic theory of matter.

One other controversial topic I shall touch upon in the domain of physics, though I shall touch upon it lightly, for it is not a matter for easy reference as yet. If the *Principle of Relativity* in an extreme sense establishes itself, it seems as if even *time* would become discontinuous and be supplied in atoms, as money is doled out in pence or centimes instead of continuously; in which case our customary existence will turn out to be no more really continuous than the events on a kinematograph screen, while that great agent of continuity, the æther of space, will be relegated to the museum of historical curiosities.

In that case differential equations will cease to represent the facts of nature, they will have to be replaced by finite differences, and the most fundamental revolution since Newton will be inaugurated.

Now in all the debateable matters of which I have indicated possibilities I want to urge a conservative attitude. I accept the new experimental results on which some of these theories—such as the principle of relativity—are based, and am profoundly interested in them, but I do not feel that they are so revolutionary as their propounders think. I see a way to retain the old and yet embrace the new, and I urge moderation in the uprooting and removal of landmarks.

And of these the chief is Continuity. I cannot imagine the exertion of mechanical force across empty space, no matter how minute; a continuous medium seems to me essential. I cannot admit discontinuity in either space or time, nor can I imagine any sort of experiment which would justify such a hypothesis. For surely we must realise that we know nothing experimental of either space or time, we cannot modify them in any way. We make experiments on bodies, and only on bodies, using "body" as an exceedingly general term.

We have no reason to postulate anything but continuity for space and time. We cut them up into conventional units for convenience' sake, and those units we can count; but there is really nothing atomic or countable about the things themselves. We can count the rotations of the earth, or the revolutions of an electron, or the vibrations of a pendulum, or the waves of light. All these are concrete and tractable physical entities; but space and time are ultimate data, abstractions based on experience. We know them through motion, and through motion only, and motion is essentially continuous. We ought clearly to discriminate between things themselves and our mode of measuring them. Our measures and perceptions may be affected by all manner of incidental and trivial causes, and we may get confused or hampered by our own movement; but there need be no such complication in things themselves, any more than a landscape is distorted by looking at it through an irregular window-pane or from a travelling coach. It is an ancient and discarded fable that complications introduced by the motion of an observer are real complications belonging to the outer universe.

Very well, then, what about the æther, is that in the same predicament? Is that an abstraction, or a mere convention, or is it a concrete physical entity on which we can experiment?

Now it has to be freely admitted that it is exceedingly difficult to make experiments on the æther. It does not appeal to sense, and we know no means of getting hold of it. The one thing we know metrical about it is the velocity with which it can transmit transverse waves. That is clear and definite, and thereby to my judgment it proves itself a physical agent; not, indeed, tangible or sensible, but yet concretely real.

But it does elude our laboratory grasp. If we rapidly move matter through it, hoping to grip it and move it too, we fail; there is no mechanical connection. And even if we experiment on light, we fail too. So long as transparent matter is moving relatively to us, light can be affected inside that matter; but when matter is relatively stationary to matter nothing observable takes place, however fast things may be moving, so long as they move together.

Hence arises the idea that motion with respect to æther is meaningless; and the fact that only relative motion of pieces of matter with respect to each other has so far been observed is the foundation of the principle of relativity. It sounds simple enough as thus stated, but in its developments it is an ingenious and complicated doctrine embodying surprising consequences which have been worked out by Prof. Einstein and his disciples with consummate ingenuity.

What have I to urge against it? Well, in the first place, it is only in accordance with common sense that no effect of the first order can be observed without relative motion of matter. An æther-stream through our laboratories is optically and electrically undetectable, at least as regards first-order observation; this is clearly explained for general readers in my book, "The Ether of Space," chapter iv. But the principle of relativity says more than that; it says that no effect of any order of magnitude can ever be observed without the relative motion of matter.

The truth underlying this doctrine is that absolute motion without reference to *anything* is unmeaning. But the narrowing down of "anything" to mean any piece of matter is illegitimate. The nearest approach to absolute motion that we can physically imagine is motion through or with respect to the æther of space. It is natural to assume that the æther is on the whole stationary, and to use it as a standard of rest; in that sense motion with reference to it may be called absolute, but in no other sense.

The principle of relativity claims that we can never ascertain such motion: in other words it practically or pragmatically denies the existence of the æther. Every one of our scientifically observed motions, it says, are of the same nature as our popularly observed ones, viz. motion of pieces of matter relatively to each other; and that is all that we can ever know. Everything goes on—says the principle of relativity—as if the æther did not exist.

Now the facts are that no motion with reference to the æther alone has ever yet been observed: there are always curious compensating effects which just cancel out the movement-terms and destroy or effectively mask any phenomenon that might otherwise be expected. When matter moves past matter observation can be made; but, even so, no consequent locomotion of æther, outside the actually moving particles, can be detected.

(It is sometimes urged that rotation is a kind of absolute motion that can be detected, even in isolation. It can so be detected, as Newton pointed out; but in cases of rotation matter on one side the axis is moving in the opposite direction to matter on the other side of the axis; hence rotation involves relative material motion, and therefore can be observed.)

To detect motion through æther we must use an æthereal process. We may use radiation, and try to compare the speeds of light along or across the motion, or we might try to measure the speed, first with the motion and then against it. But how are we to make the comparison? If the time of emission from a distant source is given by a distant clock, that clock must be observed through a telescope, that is by a beam of light; which is plainly a compensating process. Or the light from a neighbouring source can be sent back to us by a distant mirror; when again there will be compensation. Or the starting of light from a distant terrestrial source may be telegraphed to us, either with a wire or without; but it is the æther that conveys the message in either case, so again there will be compensation. Electricity, magnetism, and light, are all effects of the æther.

Use cohesion, then; have a rod stretching from one place to another, and measure that. But cohesion is transmitted by the æther too, if, as believed, it is the universal binding medium. Compensation is likely; compensation can, on the electrical theory of matter, be predicted.

Use some action not dependent on æther, then. Very well, where shall we find it?

To illustrate the difficulty I will quote a sentence from Sir Joseph Larmor's paper before the International Congress of Mathematicians at Cambridge last year:—

"If it is correct to say with Maxwell that all radiation is an electrodynamic phenomenon, it is equally correct to say with him that all electrodynamic relations between material bodies are established by the operation, on the molecules of those bodies, of fields of force which are propagated in free space as radiation and in accordance with the laws of radiation, from one body to the other."

The fact is, we are living in an epoch of some very comprehensive generalisations. The physical discovery of the twentieth century, so far, is the electrical theory of matter. This is the great new theory of our time; it was referred to, in its philosophical aspect, by Mr. Balfour in his presidential address at Cambridge in 1904. We are too near it to be able to contemplate it properly; it has still to establish itself and to develop in detail, but I anticipate that in some form or other it will prove true.[4]

Here is a briefest possible summary of the first chapter (so to speak) of the electrical theory of matter:—

(1) Atoms of matter are composed of electrons—of positive and negative electric charges.

(2) Atoms are bound together into molecules by chemical affinity which is intense electrical attraction at ultra-minute distances.

(3) Molecules are held together by cohesion, which I for one regard as residual or differential chemical affinity over molecular distances.

(4) Magnetism is due to the locomotion of electrons. There is no magnetism without an electric current, atomic or otherwise. There is no electric current without a moving electron.

(5) Radiation is generated by every accelerated electron, in amount proportional to the square of its acceleration; and there is no other kind of radiation, except indeed a corpuscular kind; but this depends on the velocity of electrons and therefore again can only be generated by their acceleration.

The theory is bound to have curious consequences; and already it has contributed to some of the uprooting and uncertainty that I speak of. For, if it be true, every material interaction will be electrical, *i.e.* æthereal; and hence arises our difficulty. Every kind of force is transmitted by the æther, and hence, so long as all our apparatus is travelling together at one and the same pace, we have no chance of detecting the motion. That is the strength of the principle of relativity. The changes are not zero, but they cancel each other out of observation (NATURE, vol. xlvi., p. 165, 1892).

Many forms of statement of the famous Michelson-Morley experiment are misleading. It is said to prove that the time taken by light to go with the æther stream is the same as that taken to go against or across it. It does not show that. What it shows is that the time taken by light to travel to and fro

[4] For a general introductory account of the electrical theory of matter my Romanes lecture for 1903 (Clarendon Press) may be referred to.

on a measured interval fixed on a rigid block of matter is independent of the aspect of that block with respect to any motion of the earth through space. A definite and most interesting result: but it may be, and often is, interpreted loosely and too widely.

It is interpreted too widely, as I think, when Prof. Einstein goes on to assume that no non-relative motion of matter can be ever observed even when *light* is brought into consideration. The relation of light to matter is very curious. The wave front of a progressive wave simulates many of the properties of matter. It has energy, it has momentum, it exerts force, it sustains reaction. It has been described as a portion of the mass of a radiating body—which gives it a curiously and unexpectedly corpuscular "feel." But it has a definite velocity. Its velocity in space relative to the æther is an absolute constant independent of the motion of the source. This would not be true for corpuscular light.

Hence I hold that here is something with which our own motion may theoretically be compared; and I predict that our motion through the æther will some day be detected by help of this very fact—by comparing our speed with that of light: though the old astronomical aberration, which seemed to make the comparison easy, failed to do so quite simply, because it is complicated by the necessity of observing the position of a distant source, in relation to which the earth is moving. If the source and observer are moving together there is no possibility of observing aberration. Nevertheless, I maintain that when matter is moving near a beam of light we may be able to detect the motion. For the velocity of light in space is no function of the velocity of the source, nor of matter near it; it is quite unaffected by source or receiver. Once launched it travels in its own way. If we are travelling to meet it, it will be arriving at us more quickly; if we travel away from it, it will reach us with some lag. And observation of the acceleration or retardation is made by aid of Jupiter's satellites. We have there the dial of a clock, to or from which we advance or recede periodically. It gains while we approach it, it loses while we recede from it, it keeps right time when we are stationary or only moving across the line of sight.

But then, of course, it does not matter whether Jupiter is standing still and we are moving, or *vice versâ*: it is a case of relative motion of matter again. So it is if we observe a Doppler effect from the right- and left-hand limbs of the rotating sun. True, and if we are to permit no relative motion of matter we must use a terrestrial source, clamped to the earth as our receiver is. And now we shall observe nothing.

But not because there is nothing to observe. Lag must really occur if we are running away from the light, even though the source is running after us at the same pace; unless we make the assumption—true only for corpuscular light—that the velocity of light is not an absolute thing, but is dependent on the speed of the source. With corpuscular light there is nothing to observe; with wave light there is something, but we cannot observe it.

But if the whole solar system is moving through the æther I see no reason why the relative æther drift should not be observed by a differential residual effect in connection with Jupiter's satellites or the right and left limbs of the sun. The effect must be too small to observe without extreme precision, but theoretically it ought to be there. Inasmuch, however, as relative motion of matter with respect to the observer is involved in these effects, it may be held that the detection of a uniform drift of the solar system in this way is not contrary to the principle of relativity. It is contrary to some statements of that principle; and the cogency of those statements breaks down, I think, whenever they include the velocity of light; because there we really have something absolute (in the only sense in which the term can have a physical meaning) with which we can compare our own motions, when we have learnt how.

But in ordinary astronomical translation—translation as of the earth in its orbit—all our instruments, all our standards, the whole contents of our laboratory, are moving at the same rate in the same direction; under those conditions we cannot expect to observe anything. Clerk Maxwell went so far as to say that if every particle of matter simultaneously received a graduated blow so as to produce a given constant acceleration all in the same direction, we should be unaware of the fact. He did not then know all that we know about radiation. But apart from that, and limiting ourselves to comparatively slow changes of velocity, our standards will inevitably share whatever change occurs. So far as observation goes, everything will be practically as if no change had occurred at all; though that may not be the truth. All that experiment establishes is that there have so far always been compensations; so that the attempt to observe motion through the æther is being given up as hopeless.

Surely, however, the minute and curious compensations cannot be accidental, they must be necessary? Yes, they are necessary; and I want to say why. Suppose the case were one of measuring thermal expansion; and suppose everything had the same temperature and the same expansibility; our standards would contract or expand with everything else, and we could observe nothing; but expansion would occur nevertheless. That is obvious, but the following assertion is not so obvious. If everything in the Universe had the same temperature, no matter what that temperature was, nothing would be visible at all; the external world, so far as vision went, would not appear to exist. Visibility depends on radiation, on differential radiation. We must have differences to appeal to our senses, they are not constructed for uniformity.

It is the extreme omnipresence and uniformity and universal agency of the æther of space that makes it so difficult to observe. To observe anything you must have differences. If all actions at a distance are conducted at the same rate through the æther, the travel of none of them can be observed. Find something not conveyed by the æther, and there is a chance. But then every physical action is transmitted by the æther, and in every case by means of its transverse or radiation-like activity.

Except perhaps Gravitation. That may give us a clue some day, but at present we have not been able to detect its speed of transmission at all. No plan has been devised for measuring it. Nothing short of the creation or destruction of matter seems likely to serve: creation or destruction of the gravitational unit, whether it be an atom or an electron, or whatever it is. Most likely the unit of weight is an electron, just as the unit of mass is.

The so-called non-Newtonian Mechanics, with mass and shape a function of velocity, is an immediate consequence of the electrical theory of matter. The dependence of inertia and shape on speed is a genuine discovery, and, I believe, a physical fact. The Principle of Relativity would reduce it to a conventional fiction. It would seek to replace this real change in matter by imaginary changes in time. But surely we must admit that Space and Time are essentially unchangeable: they are not at the disposal even of mathematicians; though it is true that Pope Gregory, or a Daylight-saving Bill, can play with our units, can turn the 3rd of October in any one year into the

14th, or can make the sun South sometimes at eleven o'clock sometimes at twelve.[5]

But the changes of dimension and mass due to velocity are not conventions but realities; so I urge, on the basis of the electrical theory of matter. The Fitzgerald-Lorentz hypothesis I have an affection for. I was present at its birth. Indeed, I assisted at its birth; for it was in my study at 21 Waverley Road, Liverpool, with Fitzgerald in an armchair, and while I was enlarging on the difficulty of reconciling the then new Michelson experiment with the theory of astronomical aberration and with other known facts, that he made his brilliant surmise:—"Perhaps the stone slab was affected by the motion." I rejoined that it was a 45° shear that was needed. To which he replied, "Well, that's all right—a simple distortion." And very soon he said, "And I believe it occurs, and that the Michelson experiment demonstrates it." A shortening long-ways, or a lengthening cross-ways would do what was wanted.

And is such a hypothesis gratuitous? Not at all: in the light of the electrical theory of matter such an effect ought to occur. The amount required by the experiment, and given by the theory, is equivalent to a shrinkage of the earth's diameter by rather less than three inches, in the line of its orbital motion through the æther of space. An oblate spheroid with the proper eccentricity has all the simple geometrical properties of a stationary sphere; the eccentricity depends in a definite way on speed, and becomes considerable as the velocity of light is approached.

All this Profs. Lorentz and Larmor very soon after, and quite independently, perceived; though this is only one of the minor achievements in the electrical theory of matter which we owe to our distinguished visitor Prof. H. A. Lorentz.

The key of the position, to my mind, is the nature of cohesion. I regard cohesion as residual chemical affinity, a balance of electrical attraction over repulsion between groups of alternately charged molecules. Lateral electrical attraction is diminished by motion; so is lateral electric repulsion. In cohesion both are active, and they nearly balance. At anything but molecular distance they quite balance, but at molecular distance attraction predominates. It is the diminution of the predominant partner that will be felt. Hence while longitudinal cohesion, or cohesion in the direction of motion, remains unchanged, lateral cohesion is less; so there will be distortion, and a unit cube x, y, z moving along x with velocity u becomes a parallelopiped with sides $1/k^2, k, k$; where $1/k^6 = 1 - u^2/v^2$.[6]

The electrical theory of matter is a positive achievement, and has positive results. By its aid we make experiments which throw light upon the relation between matter and the æther of space. The principle of relativity, which seeks to replace it, is a principle of negation, a negative proposition, a statement that observation of certain facts can never be made, a denial of any relation between matter and æther, a virtual denial that the æther exists. Whereas if we admit the real changes that go on by reason of rapid motion, a whole field is open for discovery; it is even possible to investigate the changes in shape of an electron—appallingly minute though it is—as it approaches the speed of light; and properties belonging to the æther of space, evasive though it be, cannot lag far behind.

Speaking as a physicist, I must claim the æther as peculiarly our own domain. The study of molecules we share with the chemist, and matter in its various forms is investigated by all men of science, but a study of the æther of space belongs to physics only. I am not alone in feeling the fascination of this portentous entity. Its curiously elusive and intangible character, combined with its universal and unifying permeance, its apparently infinite extent, its definite and perfect properties, make the æther the most interesting, as it is by far the largest and most fundamental, ingredient in the material cosmos.

As Sir J. J. Thomson said at Winnipeg—

"The æther is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe. . . . The study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist."

Matter it is not, but material it is; it belongs to the material universe, and is to be investigated by ordinary methods. But to say this is by no means to deny that it may have mental and spiritual functions to subserve in some other order of existence, as matter has in this.

The æther of space is at least the great engine of continuity. It may be much more, for without it there could hardly be a material universe at all. Certainly, however, it is essential to continuity; it is the one all-permeating substance that binds the whole of the particles of matter together. It is the uniting and binding medium without which, if matter could exist at all, it could exist only as chaotic and isolated fragments: and it is the universal medium of communication between worlds and particles. And yet it is possible for people to deny its existence, because it is unrelated to any of our senses, except sight—and to that only in an indirect and not easily recognised fashion.

To illustrate the thorough way in which we may be unable to detect what is around us unless it has some link or bond which enables it to make appeal, let me make another quotation from Sir J. J. Thomson's address at Winnipeg in 1909. He is leading up to the fact that even single atoms, provided they are fully electrified with the proper atomic charge, can be detected by certain delicate instruments—their field of force bringing them within our ken—whereas a whole crowd of unelectrified ones would escape observation.

"The smallest quantity of unelectrified matter ever detected is probably that of neon, one of the inert gases of the atmosphere. Prof. Strutt has shown that the amount of neon in 1/20 of a cubic centimetre of the air at ordinary pressures can be detected by the spectroscope; Sir William Ramsay estimates that the neon in the air only amounts to one part of neon in 100,000 parts of air, so that the neon in 1/20 of a cubic centimetre of air would only occupy at atmospheric pressure a volume of half a millionth of a cubic centimetre. When stated in this form the quantity seems exceedingly small, but in this small volume there are about ten million million molecules. Now the population of the earth is estimated at about fifteen hundred millions, so that the smallest number of molecules of neon we can identify is about 7000 times the population of the earth. In other words, if we had no better test for the existence of a man than we have for that of an unelectrified molecule we should come to the conclusion that the earth is uninhabited."

The parable is a striking one, for on these lines it might legitimately be contended that we have no right to say positively that even space is uninhabited. All we can safely say is that we have no means of

[5] In the historical case of governmental interference with the calendar, no wonder the populace rebelled. Surely someone might have explained to the authorities that dropping leap year for the greater part of a century would do all that was wanted, and that the horrible inconvenience of upsetting all engagements and shortening a single year by eleven days could be avoided.

[6] Different modes of estimating the change give slightly different results; some involve a compression as well as a distortion—in fact the strain associated with the name of Thomas Young; the details are rather complicated and this is not the place to discuss them. A pure shear, of magnitude specified in the text, is simplest, it is in accord with all the experimental facts—including some careful measurements by Bucherer—and I rather expect it to survive.

detecting the existence of non-planetary immaterial dwellers, and that unless they have some link or bond with the material they must always be physically beyond our ken. We may therefore for practical purposes legitimately treat them as non-existent until such link is discovered, but we should not dogmatise about them. True agnosticism is legitimate, but not the dogmatic and positive and gnostic variety.

But I hold that science is incompetent to make comprehensive denials, even about the æther, and that it goes wrong when it makes the attempt. Science should not deal in negations: it is strong in affirmations, but nothing based on abstraction ought to presume to deny outside its own region. It often happens that things abstracted from and ignored by one branch of science may be taken into consideration by another:—

Thus, chemists ignore the æther.

Mathematicians may ignore experimental difficulties.

Physicists ignore and exclude live things.

Biologists exclude mind and design.

Psychologists may ignore human origin and human destiny.

Folk-lore students and comparative mythologists need not trouble about what modicum of truth there may be in the legends which they are collecting and systematising.

And microscopists may ignore the stars.

Yet none of these ignored things should be denied.

Denial is no more infallible than assertion. There are cheap and easy kinds of scepticism, just as there are cheap and easy kinds of dogmatism; in fact, scepticism can become viciously dogmatic, and science has to be as much on its guard against personal predilection in the negative as in the positive direction. An attitude of universal denial may be very superficial.

"To doubt everything or to believe everything are two equally convenient solutions; both dispense with the necessity of reflection."

All intellectual processes are based on abstraction. For instance, history must ignore a great multitude of facts in order to treat any intelligently: it selects. So does art; and that is why a drawing is clearer than reality. Science makes a diagram of reality, displaying the works, like a skeleton clock. Anatomists dissect out the nervous system, the blood vessels, and the muscles, and depict them separately —there must be discrimination for intellectual grasp —but in life they are all merged and co-operating together; they do not really work separately, though they may be studied separately. A scalpel discriminates: a dagger or a bullet crashes through everything. That is life—or rather death. The laws of nature are a diagrammatic framework, analysed or abstracted out of the full comprehensiveness of reality.

Hence it is that science has no authority in denials. To deny effectively needs much more comprehensive knowledge than to assert. And abstraction is essentially not comprehensive: one cannot have it both ways. Science employs the methods of abstraction, and thereby makes its discoveries.

The reason why some physiologists insist so strenuously on the validity and self-sufficiency of the laws of physics and chemistry, and resist the temptation to appeal to unknown causes—even though the guiding influence and spontaneity of living things are occasionally conspicuous as well as inexplicable—is that they are keen to do their proper work; and their proper work is to pursue the laws of ordinary physical energy into the intricacies of "colloidal electrolytic structures of great chemical complexity" and to study its behaviour there.

What we have clearly to grasp, on their testimony, is that for all the terrestrial manifestations of life the ordinary physical and chemical processes have to serve. There are not new laws for living matter, and old laws for non-living, the laws are the same; or if ever they differ, the burden of proof rests on him who sustains the difference. The conservation of energy, the laws of chemical combination, the laws of electric currents, of radiation, &c., &c.—all the laws of chemistry and physics—may be applied without hesitation in the organic domain. Whether they are sufficient is open to question, but as far as they go they are necessary; and it is the business of the physiologist to seek out and demonstrate the action of those laws in every vital action.

This is clearly recognised by the leaders, and in the definition of physiology by Burdon Sanderson he definitely limited it to the study of "ascertainable characters of a chemical and physical type." In his address to the Subsection of Anatomy and Physiology at York in 1881 he spoke as follows:—

"It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of 'vitalism.' Before that time, even the greatest biologists—*e.g.* J. Müller—recognised that the knowledge biologists possessed both of vital and physical phenomena was insufficient to refer both to a common measure. The method, therefore, was to study the processes of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. The most efficient cause [conducing to the change] was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the conservation of energy. . . .

"Investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness."

Prof. Gotch, whose recent loss we have to deplore, puts it more strongly:—

"It is essentially unscientific," he says, "to say that any physiological phenomenon is caused by vital force."

I observe that by some critics I have been called a vitalist, and in a sense I am; but I am not a vitalist if vitalism means an appeal to an undefined "vital force" (an objectionable term I have never thought of using) as against the laws of chemistry and physics. Those laws must be supplemented, but need by no means be superseded. The business of science is to trace out their mode of action everywhere, as far and as fully as possible; and it is a true instinct which resents the mediæval practice of freely introducing spiritual and unknown causes into working science. In science an appeal to occult qualities must be illegitimate, and be a barrier to experiment and research generally; as, when anything is called an act of God —and when no more is said. The occurrence is left unexplained. As an ultimate statement such a phrase may be not only true but universal in its application. But there are always proximate explanations which may be looked for and discovered with patience. So, lightning, earthquakes, and other potents are reduced to natural causes. No ultimate explanation is ever attained by science: proximate explanations only. They are what it exists for; and it is the business of scientific men to seek them.

To attribute the rise of sap to vital force would be absurd, it would be giving up the problem and stating nothing at all. The way in which osmosis acts to produce the remarkable and surprising effect is discoverable and has been discovered.

So it is always in science, and its progress began when unknown causes were eliminated and treated as non-existent. Those causes, so far as they exist, must establish their footing by direct investigation and research; carried on in the first instance apart from the long-recognised branches of science, until the time when they too have become sufficiently definite to be entitled to be called scientific. Outlandish territories may in time be incorporated as states, but they must make their claim good and become civilised first.

It is well for people to understand this definite limitation of scope quite clearly, else they wrest the splendid work of biologists to their own confusion—helped it is true by a few of the more robust or less responsible theorisers, among those who should be better informed and more carefully critical in their philosophising utterances.

But, as is well known, there are more than a few biologists who, when taking a broad survey of their subject, clearly perceive and teach that, before all the actions of live things are fully explained, some hitherto excluded causes must be postulated. Ever since the time of J. R. Mayer it has been becoming more and more certain that, as regards performance of work, a living thing obeys the laws of physics, like everything else; but undoubtedly it initiates processes and produces results that without it could not have occurred—from a bird's nest to a honeycomb, from a deal box to a warship. The behaviour of a ship firing shot and shell is explicable in terms of energy, but the discrimination which it exercises between friend and foe is not so explicable. There is plenty of physics and chemistry and mechanics about every vital action, but for a complete understanding of it something beyond physics and chemistry is needed.

And life introduces an incalculable element. The vagaries of a fire or a cyclone could all be predicted by Laplace's calculator, given the initial positions, velocities, and the law of acceleration of the molecules; but no mathematician could calculate the orbit of a common house-fly. A physicist into whose galvanometer a spider had crept would be liable to get phenomena of a kind quite inexplicable, until he discovered the supernatural, *i.e.*, literally superphysical, cause. I will risk the assertion that life introduces something incalculable and purposeful amid the laws of physics; it thus distinctly supplements those laws, though it leaves them otherwise precisely as they were and obeys them all.

We see only its effect, we do not see life itself. Conversion of inorganic into organic is effected always by living organisms. The conversion under those conditions certainly occurs, and the process may be studied. Life appears necessary to the conversion; which clearly takes place under the guidance of life, though in itself it is a physical and chemical process. Many laboratory conversions take place under the guidance of life, and, but for the experimenter, would not have occurred.

Again, putrefaction, and fermentation, and purification of rivers, and disease, are not purely and solely chemical processes. Chemical processes they are, but they are initiated and conducted by living organisms. Just when medicine is becoming biological, and when the hope of making the tropical belt of the earth healthily habitable by energetic races is attracting the attention of people of power, philosophising biologists should not attempt to give their science away to chemistry and physics. Sections D and H and I and K are not really subservient to A and B. Biology is an independent science, and it is served, not dominated, by chemistry and physics.

Scientific men are hostile to superstition, and rightly so, for a great many popular superstitions are both annoying and contemptible; yet occasionally the term may be wrongly applied to practices of which the theory is unknown. To a superficial observer some of the practices of biologists themselves must appear grossly superstitious. To combat malaria Sir Ronald Ross does not indeed erect an altar; no, he oils a pond—making libation to its presiding genii. What can be more ludicrous than the curious and evidently savage ritual, insisted on by United States officers, at that hygienically splendid achievement, the Panama Canal—the ritual of punching a hole in every discarded tin, with the object of keeping off disease! What more absurd, again—in superficial appearance—than the practice of burning or poisoning a soil to make it extra fertile!

Biologists in their proper field are splendid, and their work arouses keen interest and enthusiasm in all whom they guide into their domain. Most of them do their work by intense concentration, by narrowing down their scope, not by taking a wide survey or a comprehensive grasp. Suggestions of broader views and outlying fields of knowledge seem foreign to the intense worker, and he resents them. For his own purpose he wishes to ignore them, and practically he may be quite right. The folly of negation is not his, but belongs to those who misinterpret or misapply his utterances, and take him as a guide in a region where, for the time at least, he is a stranger. Not by such aid is the universe in its broader aspects to be apprehended. If people in general were better acquainted with science they would not make these mistakes. They would realise both the learning and the limitations, make use of the one and allow for the other, and not take the recipe of a practical worker for a formula wherewith to interpret the universe.

What appears to be quite certain is that there can be no terrestrial manifestation of life without matter. Hence naturally they say, or they approve such sayings as, "I discern in matter the promise and potency of all forms of life." Of all terrestrial manifestations of life, certainly. How else could it manifest itself save through matter? "I detect nothing in the organism but the laws of chemistry and physics," it is said. Very well: naturally enough. That is what they are after; they are studying the physical and chemical aspects or manifestations of life. But life itself—life and mind and consciousness—they are not studying, and they exclude them from their purview. Matter is what appeals to our senses here and now; materialism is appropriate to the material world; not as a philosophy but as a working creed, as a proximate and immediate formula for guiding research. Everything beyond that belongs to another region, and must be reached by other methods. To explain the psychical in terms of physics and chemistry is simply impossible; hence there is a tendency to deny its existence, save as an epiphenomenon. But all such philosophising is unjustified, and is really bad metaphysics.

So if ever in their enthusiasm scientific workers go too far and say that the things they exclude from study have no existence in the universe, we must appeal against them to direct experience. We ourselves are alive, we possess life and mind and consciousness, we have first-hand experience of these things, quite apart from laboratory experiments. They belong to the common knowledge of the race. Births, deaths, and marriages are not affairs of the biologist, but of humanity; they went on before a single one of them was understood, before a vestige of science existed. We ourselves are the laboratory in which men of science, psychologists, and others make experiments. They can formulate our processes of digestion, and the material concomitants of willing, of sensation, of thinking; but the hidden guiding entities they do not touch.

So also if any philosopher tells you that you do not exist, or that the external world does not exist, or that you are an automaton without free will, that all your actions are determined by outside causes, and that you are not responsible—or that a body cannot move out of its place, or that Achilles cannot catch a tortoise; then in all those cases appeal must be made to twelve average men, unsophisticated by special studies. There is always a danger of error in interpreting experience, or in drawing inferences from it; but in a matter of bare fact, based on our own first-hand experience, we are able to give a verdict. We may be mistaken as to the nature of what we see. Stars may look to us like bright specks in a dome, but the fact that we see them admits of no doubt. So also consciousness and will are realities of which we are directly aware, just as directly as we are of motion and force, just as clearly as we apprehend the philosophising utterances of an Agnostic. The process of seeing, the plain man does not understand; he does not recognise that it is a method of æthereal telegraphy; he knows nothing of the æther and its ripples, nor of the retina and its rods and cones, nor of nerve and brain processes; but he sees and he hears and he touches, and he wills and he thinks and is conscious. This is not an appeal to the mob as against the philosopher, it is appeal to the experience of untold ages as against the studies of a generation.

How consciousness became associated with matter, how life exerts guidance over chemical and physical forces, how mechanical motions are translated into sensations—all these things are puzzling and demand long study. But the fact that these things are so admits of no doubt; and difficulty of explanation is no argument against them. The blind man restored to sight had no opinion as to how he was healed, nor could he vouch for the moral character of the Healer, but he plainly knew that whereas he was blind now he saw. About that fact he was the best possible judge. So it is also with "this main miracle that thou art thou, With power on thine own act and on the world."

But although life and mind may be excluded from physiology, they are not excluded from science. Of course not. It is not reasonable to say that things necessarily elude investigation merely because we do not knock against them. Yet the mistake is sometimes made. The æther makes no appeal to sense, therefore some are beginning to say that it does not exist. Mind is occasionally put into the same predicament. Life is not detected in the laboratory, save in its physical and chemical manifestations; but we may have to admit that it guides processes nevertheless. It may be called a catalytic agent.

To understand the action of life itself, the simplest plan is not to think of a microscopic organism, or any unfamiliar animal, but to make use of our own experience as living beings. Any positive instance serves to stem a comprehensive denial; and if the reality of mind and guidance and plan is denied because they make no appeal to sense, then think how the world would appear to an observer to whom the existence of men was unknown and undiscoverable, while yet all the laws and activities of nature went on as they do now.

Suppose, then, that *man* made no appeal to the senses of an observer of this planet. Suppose an outside observer could see all the events occurring in the world, save only that he could not see animals or men. He would describe what he saw much as we have to describe the activities initiated by life.

If he looked at the Firth of Forth, for instance, he would see piers arising in the water, beginning to sprout, reaching across in strange manner till they actually join or are joined by pieces attracted up from below to complete the circuit (a solid circuit round the current). He would see a sort of bridge or filament thus constructed, from one shore to the other, and across this bridge insect-like things crawling and returning for no very obvious reason.

Or let him look at the Nile, and recognise the meritorious character of that river in promoting the growth of vegetation in the desert. Then let him see a kind of untoward crystallisation growing across and beginning to dam the beneficent stream. Blocks fly to their places by some kind of polar forces; "we cannot doubt" that it is by helio- or other tropism. There is no need to go outside the laws of mechanics and physics, there is no difficulty about supply of energy—none whatever—materials in tin cans are consumed which amply account for all the energy; and all the laws of physics are obeyed. The absence of any design, too, is manifest; for the effect of the structure is to flood an area up-stream which might have been useful, and to submerge a structure of some beauty; while down stream its effect is likely to be worse, for it would block the course of the river and waste it on the desert, were it not that fortunately some leaks develop and a sufficient supply still goes down—goes down, in fact, more equably than before: so that the ultimate result is beneficial to vegetation, and simulates intention.

If told concerning either of these structures that an engineer, a designer in London, called Benjamin Baker, had anything to do with it, the idea would be preposterous. One conclusive argument is final against such a superstitious hypothesis—he is not there, and a thing plainly cannot act where it is not. But although we, with our greater advantages, perceive that the right solution for such an observer would be the recognition of some unknown agency or agent, it must be admitted that an explanation in terms of a vague entity called vital force would be useless, and might be so worded as to be misleading; whereas a statement in terms of mechanics and physics could be clear and definite and true as far as it went, though it must necessarily be incomplete.

And note that what we observe, in such understood cases, is an *interaction* of mind and matter; not parallelism nor epiphenomenalism nor anything strained or difficult, but a straightforward utilisation of the properties of matter and energy for purposes conceived in the mind, and executed by muscles guided by acts of will.

But, it will be said, this is unfair, for we *know* that there is design in the Forth Bridge or the Nile Dam, we have seen the plans and understand the agencies at work: we know that it was conceived and guided by life and mind, it is unfair to quote this as though it could simulate an automatic process.

Not at all, say the extreme school of biologists whom I am criticising, or ought to say if they were consistent, there is nothing but chemistry and physics at work anywhere; and the mental activity apparently demonstrated by those structures is only an illusion, an epiphenomenon; the laws of chemistry and physics are supreme, and they are sufficient to account for everything!

Well, they account for things up to a point; they account in part for the colour of a sunset, for the majesty of a mountain peak, for the glory of animate existence. But do they account for everything completely? Do they account for our own feeling of joy and exaltation, for our sense of beauty, for the manifest beauty existing throughout nature? Do not these things suggest something higher and nobler and more joyous, something for the sake of which all the struggle for existence goes on?

Surely there must be a deeper meaning involved in natural objects. Orthodox explanations are only partial, though true as far as they go. When we examine each parti-coloured pinnule in a peacock's tail, or hair in a zebra's hide, and realise that the varying shades on each are so placed as to contribute to the general design and pattern, it becomes exceedingly difficult to explain how this organised co-operation of parts, this harmonious distribution of pigment cells, has come about on merely mechanical principles. It would be as easy to explain the sprouting of the cantilevers of the Forth Bridge from its piers, or the flocking of the stones of the Nile Dam by chemiotaxis. Flowers attract insects for fertilisation; and fruit tempts animals to eat it in order to carry seeds. But these explanations cannot be final. We have still to explain the insects. So much beauty cannot be necessary merely to attract their attention. We have further to explain this competitive striving towards life. Why do things struggle to exist? Surely the effort must have some significance, the development some aim. We thus reach the problem of existence itself, and the meaning of evolution.

The mechanism whereby existence entrenches itself is manifest, or at least has been to a large extent discovered. Natural selection is a *vera causa,* so far as it goes; but if so much beauty is necessary for insects, what about the beauty of a landscape or of clouds? What utilitarian object do those subserve? Beauty in general is not taken into account by science. Very well, that may be all right, but it exists nevertheless. It is not my function to discuss it. No; but it is my function to remind you and myself that that our studies do not exhaust the universe, and that if we dogmatise in a negative direction, and say that we can reduce everything to physics and chemistry, we gibbet ourselves as ludicrously narrow pedants, and are falling far short of the richness and fullness of our human birthright. How far preferable is the reverent attitude of the Eastern poet:—

"The world with eyes bent upon thy feet stands in awe with all its silent stars."

Superficially and physically we are very limited. Our sense organs are adapted to the observation of matter; and nothing else directly appeals to us. Our nerve-muscle-system is adapted to the production of motion in matter, in desired ways; and nothing else in the material world can we accomplish. Our brain and nerve systems connect us with the rest of the physical world. Our senses give us information about the movements and arrangements of matter. Our muscles enable us to produce changes in those distributions. That is our equipment for human life; and human history is a record of what we have done with these parsimonious privileges.

Our brain, which by some means yet to be discovered connects us with the rest of the material world, has been thought partially to disconnect us from the mental and spiritual realm, to which we really belong, but from which for a time and for practical purposes we are isolated. Our common or social association with matter gives us certain opportunities and facilities, combined with obstacles and difficulties which are themselves opportunities for struggle and effort.

Through matter we become aware of each other, and can communicate with those of our fellows who have ideas sufficiently like our own for them to be stimulated into activity by a merely physical process set in action by ourselves. By a time succession of vibratory movements (as in speech and music), or by a static distribution of materials (as in writing, painting, and sculpture), we can carry on intelligent intercourse with our fellows; and we get so used to these ingenious and roundabout methods, that we are apt to think of them and their like as not only the natural but as the only possible modes of communication, and that anything more direct would disarrange the whole fabric of science.

It is clearly true that our bodies constitute the normal means of manifesting ourselves to each other while on the planet; and that if the physiological mechanism whereby we accomplish material acts is injured, the conveyance of our meaning and the display of our personality inevitably and correspondingly suffer.

So conspicuously is this the case that it has been possible to suppose that the communicating mechanism, formed and worked by us, is the whole of our existence: and that we are essentially nothing but the machinery by which we are known. We find the machinery utilising nothing but well-known forms of energy, and subject to all the laws of chemistry and physics—it would be strange if it were not so—and from that fact we try to draw valid deductions as to our nature, and as to the impossibility of our existing apart from and independent of these temporary modes of material activity and manifestation. We so uniformly employ them, in our present circumstances, that we should be on our guard against deception due to this very uniformity. Material bodies are all that we have any control over, are all that we are experimentally aware of; anything that we can do with these is open to us; any conclusions we can draw about them may be legitimate and true. But to step outside their province and to deny the existence of any other region because we have no sense organ for its appreciation, or because (like the æther) it is too uniformly omnipresent for our ken, is to wrest our advantages and privileges from their proper use and apply them to our own misdirection.

But if we have learnt from science that evolution is real, we have learnt a great deal. I must not venture to philosophise, but certainly from the point of view of science evolution is a great reality. Surely evolution is not an illusion; surely the universe progresses in time. Time and space and matter are abstractions, but are none the less real: they are data given by experience; and time is the keystone of evolution. "Thy centuries follow each other, perfecting a small wild flower."

We abstract from living, moving reality a certain static aspect, and we call it matter; we abstract the element of progressiveness, and we call it time. When these two abstractions combine, co-operate, interact, we get reality again. It is like Poynting's theorem.

The only way to refute or confuse the theory of evolution is to introduce the subjectivity of time. That theory involves the reality of time, and it is in this sense that Prof. Bergson uses the great phrase "creative evolution."

I see the whole of material existence as a steady passage from past to future, only the single instant which we call the present being actual. The past is not non-existent, however, it is stored in our memories, there is a record of it in matter, and the present is based upon it; the future is the outcome of the present, and is the product of evolution.

Existence is like the output from a loom. The pattern, the design for the weaving, is in some sort "there" already; but whereas our looms are mere machines, once the guiding cards have been fed into them, the loom of time is complicated by a multitude of free agents who can modify the web, making the product more beautiful or more ugly according as they are in harmony or disharmony with the general scheme. I venture to maintain that manifest imperfections are thus accounted for, and that *freedom* could be given on no other terms, nor at any less cost.

The ability thus to work for weal or woe is no illusion, it is a reality, a responsible power which conscious agents possess; wherefore the resulting fabric is not something preordained and inexorable, though by wide knowledge of character it may be inferred. Nothing is inexorable except the uniform progress of time; the cloth must be woven, but the pattern is not wholly fixed and mechanically calculable.

Where inorganic matter alone is concerned, there everything is determined. Wherever full consciousness has entered, new powers arise, and the faculties and desires of the conscious parts of the scheme have an effect upon the whole. It is not guided from outside, but from within; and the guiding power is immanent at every instant. Of this guiding power we are a small but not wholly insignificant portion.

That evolutionary progress is real is a doctrine of profound significance, and our efforts at social betterment are justified because we are a part of the scheme, a part that has become conscious, a part that realises, however dimly, what it is doing and what it is aiming at. Planning and aiming are therefore not absent from the whole, for we are a part of the whole, and are conscious of them in ourselves.

Either we are immortal beings or we are not. We may not know our destiny, but we must have a destiny of some sort. Those who make denials are just as likely to be wrong as those who make assertions: in fact, denials are assertions thrown into negative form. Scientific men are looked up to as authorities, and should be careful not to mislead. Science may not be able to reveal human destiny, but it certainly should not obscure it. Things are as they are, whether we find them out or not; and if we make rash and false statements, posterity will detect us—if posterity ever troubles its head about us. I am one of those who think that the methods of science are not so limited in their scope as has been thought: that they can be applied much more widely, and that the psychic region can be studied and brought under law too. Allow us anyhow to make the attempt. Give us a fair field. Let those who prefer the materialistic hypothesis by all means develop their thesis as far as they can; but let us try what we can do in the psychical region, and see which wins. Our methods are really the same as theirs—the subject-matter differs. Neither should abuse the other for making the attempt.

Whether such things as intuition and revelation ever occur is an open question. There are some who have reason to say that they do. They are at any rate not to be denied off-hand. In fact, it is always extremely difficult to deny *anything* of a general character, since evidence in its favour may be only hidden and not forthcoming, especially not forthcoming at any particular age of the world's history, or at any particular stage of individual mental development. Mysticism must have its place, though its relation to science has so far not been found. They have appeared disparate and disconnected, but there need be no hostility between them. Every kind of reality must be ascertained and dealt with by proper methods. If the voices of Socrates and of Joan of Arc represent real psychical experiences, they must belong to the intelligible universe.

Although I am speaking *ex cathedra*, as one of the representatives of orthodox science, I will not shrink from a personal note summarising the result on my own mind of thirty years' experience of psychical research, begun without predilection—indeed, with the usual hostile prejudice. This is not the place to enter into details or to discuss facts scorned by orthodox science, but I cannot help remembering that an utterance from this chair is no ephemeral production, for it remains to be criticised by generations yet unborn, whose knowledge must inevitably be fuller and wider than our own. Your President, therefore, should not be completely bound by the shackles of present-day orthodoxy, nor limited to beliefs fashionable at the time. In justice to myself and my co-workers, I must risk annoying my present hearers, not only by leaving on record our conviction that occurrences now regarded as occult can be examined and reduced to order by the methods of science carefully and persistently applied, but by going further and saying, with the utmost brevity, that already the facts so examined have convinced me that memory and affection are not limited to that association with matter by which alone they can manifest themselves here and now, and that personality persists beyond bodily death. The evidence, to my mind, goes to prove that discarnate intelligence, under certain conditions, may interact with us on the material side, thus indirectly coming within our scientific ken; and that gradually we may hope to attain some understanding of the nature of a larger, perhaps ætherial, existence, and of the conditions regulating intercourse across the chasm. A body of responsible investigators has even now landed on the treacherous but promising shores of a new continent.

Yes, and there is more to say than that. The methods of science are not the only way, though they are our way, of being piloted to truth. "*Uno itinere non potest perveniri ad tam grande secretum.*"

Many scientific men still feel in pugnacious mood towards theology, because of the exaggerated dogmatism which our predecessors encountered and overcame in the past. They had to struggle for freedom to find truth in their own way; but the struggle was a miserable necessity, and has left some evil effects. And one of them is this lack of sympathy, this occasional hostility, to other more spiritual forms of truth. We cannot really and seriously suppose that truth began to arrive on this planet a few centuries ago. The pre-scientific insight of genius—of poets and prophets and saints—was of supreme value, and the access of those inspired seers to the heart of the universe was profound. But the camp followers, the scribes and pharisees, by whatever name they may be called, had no such insight, only a vicious or a foolish obstinacy; and the prophets of a new era were stoned.

Now at last we of the new era have been victorious, and the stones are in our hands; but for us to imitate the old ecclesiastical attitude would be folly. Let us not fall into the old mistake of thinking that ours is the only way of exploring the multifarious depths of the universe, and that all others are worthless and mistaken. The universe is a larger thing than we have any conception of, and no one method of search will exhaust its treasures.

Men and brethren, we are trustees of the truth of the physical universe as scientifically explored: let us be faithful to our trust.

Genuine religion has its roots deep down in the heart of humanity and in the reality of things. It is not surprising that by our methods we fail to grasp it: the actions of the Deity make no appeal to any special sense, only a universal appeal; and our methods are, as we know, incompetent to detect complete uniformity. There is a principle of relativity here, and unless we encounter flaw or jar or change, nothing in us responds; we are deaf and blind, therefore, to the Immanent Grandeur unless we have insight enough to recognise in the woven fabric of existence, flowing steadily from the loom in an infinite progress towards perfection, the ever-growing garment of a transcendant God.

SUMMARY OF THE ARGUMENT.

A marked feature of the present scientific era is the discovery of, and interest in, various kinds of atomism; so that continuity seems in danger of being lost sight of.

Another tendency is toward comprehensive negative generalisations from a limited point of view.

Another is to take refuge in rather vague forms of statement, and to shrink from closer examination of the puzzling and the obscure.

Another is to deny the existence of anything which makes no appeal to organs of sense, and no ready response to laboratory experiment.

Against these tendencies the author contends. He urges a belief in ultimate continuity as essential to science; he regards scientific concentration as an inadequate basis for philosophic generalisation; he believes that obscure phenomena may be expressed simply if properly faced; and he points out that the non-appearance of anything perfectly uniform and omnipresent is only what should be expected, and is no argument against its real substantial existence.

NOTES.

IN view of the meeting of the British Association, a "Handbook for Birmingham and the Neighbourhood" has been issued (under the editorship of Dr. G. A. Auden) at the price of 3s. 6d. net by Messrs. Cornish Bros., Ltd., Birmingham. The volume is of an encyclopædic character, and should be of great service not only to members of the British Association, but to all who are interested in things pertaining to Birmingham. The work is divided into five main divisions—historical, municipal, educational, industrial, and scientific. In the latter, which of course appeals more especially to our readers, botany is dealt with by Prof. West and Messrs. Grove, Humphreys, Cleminshaw, and Duncan; Midland reafforesting by P. E. Martineau; the ornithology of the district by R. W. Chase; insects by H. W. Ellis; mammalia, amphibia, reptilia, and pisces by H. E. Forrest; microscopic aquatic fauna by H. W. H. Darlaston; meteorology by A. Cresswell; and the geology and physiography of the Birmingham country by Prof C. Lapworth, F.R.S. The last-named contribution is supplemented by very clear geological and topographical maps executed by Messrs. John Bartholomew and Co., of Edinburgh. Besides these maps, there are a number of illustrations in the text. Altogether the volume is an admirable production, and worthy of the occasion for which it has been prepared.

Apropos of the British Association meeting, a recent number of *The Westminster Gazette* contains an article on the Lunar Society, the members of which used to meet monthly in Birmingham in the eighteenth century, as nearly as possible at the time of full moon that they might have the benefit of its light in returning home—hence the name of the society. Each member was permitted to bring a friend, and some very distinguished men from time to time enjoyed the society's hospitality. Thus we find that among such guests were Sir Joseph Banks, Sir William Herschel, John Smeaton, of lighthouse fame, Josiah Wedgwood, Prof. Hugh Blair, Afzelius, the Swedish botanist, Daniel Solander, the naturalist, and Andre de Luc, the geologist. Among the members of the society or club were James Watt, Joseph Priestley, and Erasmus Darwin. The Priestley riots dealt a blow to the little society from which it never recovered, and it is therefore now no more than a memory.

PROF. MILNE bequeathed to the British Association his books, albums, and scientific instruments relating to seismology, and, subject to his wife's interest, the sum of 1000*l*. to the chairman of the seismology committee of the British Association for the furtherance of the study of terrestrial physics and its attendant subjects.

A FURTHER grant of 5000*l*., making 10,000*l*. in all, has been made by the Federal Government of the Commonwealth of Australia towards completing the work of the Mawson Antarctic Expedition and bringing the explorers back.

THE sum of 90,000 francs has been bequeathed to the Pasteur Institute at Paris for the founding of a prize for the best original work in the treatment of meningitis.

WE record, with regret, the death on Saturday last of Dr. Hugh Marshall, F.R.S., professor of chemistry in University College, Dundee.

THE death occurred on September 2, at Abo, Finland, at the age of sixty-three years, of Dr. O. M. Reuter, emeritus professor of zoology in the University of Helsingfors.

A BRONZE statue of the late Dr. Ludwig Mond, erected by Messrs. Brunner, Mond and Co., Ltd., on the lawn opposite Winnington Hall, near Northwich, Dr. Mond's residence, is to be unveiled on Saturday next by Sir John Brunner.

A MEMORIAL of the Russian explorer, Baron E. von Toll, in the form of a bronze portrait tablet, is to be set up on Kotelnyi Island, in the New Siberia group, by the leader of the German Taimyrland Expedition.

PAPERS dealing with various problems of heating and lighting are to be read by Prof. Bone, F.R.S., Prof. Vivian B. Lewes, Mr. L. Gaster, and Mr. T. Thorne Baker at a conference which is to take place on October 29 in connection with the National Gas Congress and Exhibition.

THE plumage prohibition clause in the United States Tariff Bill having been sanctioned by the Senate the importation into the United States of the plumage of wild birds, either raw or manufactured, for purposes other than scientific or educational, is prohibited.

THE first installation of wireless telephony in a coal mine in Great Britain has just been fitted up at Dinnington Main Colliery, South Yorkshire, with, it is said, satisfactory results. The system is the invention of Mr. J. H. Reinecke, of Bochum, Westphalia, and is in use in German collieries. According to *The Times* each instrument is connected by two wires with a piece of metal buried in the ground. The wires can also be attached to ordinary tramway rails, water-pipes, &c. At Dinnington instruments have been placed at two points—one in the transformer house near the pit bottom and the other 1000 yards "in-bye," and conversation has been carried on between these points just as through an ordinary telephone with the use of only about 20 yards of wire. The system also admits of the use of portable instruments weighing about 20 lb. each by means of which it is possible to communicate with the fixed stations from any part of the mine. All that is necessary is for the operator to attach the two wires of the instrument to any metallic substance at hand. Thus in the event of a disaster in a pit miners entombed by falls would be able to open up communication with other parts of the colliery. In ordinary working the portable instruments should be very useful in the case of a breakdown of the signalling apparatus, and coal turning could be carried on while the repairs were being done. The portable instrument can also be used in the cage while ascending or descending the shaft.

THE Italian archæological mission to Crete, under the leadership of Prof. Halbherr, announces the discovery at Cortina of a temple dedicated to Egyptian deities, bearing a dedication by Flavia Philyra, the foundress. In the inner cella were found images of Jupiter, Serapis, Isis, and Mercury, with fragments of a colossal statue, supposed to be that of the foundress. A little flight of steps leads down to a subterranean chamber in which ceremonies of purification were performed.

THE excavation of the numerous prehistoric sites in the island of Malta is being actively prosecuted under the direction of Prof. T. Zammit. The most important discovery is that of a series of well tombs of the Punic type at the Kallilia plateau, north-west of Rabat. A large number of skeletons, with pottery, lamps, spindle-whorls, and a circular bronze mirror, has been unearthed. A partial exploration of the Ghar Dalam cave, conducted by Prof. Tagliaferro and Mr. C. Rizzo, produced bones of a hippopotamus and a deer, above which lay a quantity of prehistoric sherds. The museum, by the bequest of the late Mr. Parnis, has received a large collection of books about Malta and numerous antique objects. The *Malta Herald,* in recording the progress of excavation, very reasonably urges that means should be taken to protect the sites partially examined from spoliation by the villagers.

LESS than 300 miles to the north of Rio de Janeiro, on the coast range of Minas Geraes, live the Uti-krag, a tribe of Botocudos still retaining some of their old customs, but rapidly succumbing to the fostering care of the recently established Board of Protection of the Native Indians. Mr. W. Knocke paid them a very short visit in the month of October, 1912, and he describes his observations in a pamphlet entitled "Algunas Indicaciones sobre los Uti-krag del Rio Doce," issued separately from the *Revista de Historia y Geografia* (vol. v., 1913). The name of Botocudos, given them by the Portuguese, refers to the plugs with which the men distend their ear lobes, the women also the lower lip; this is now becoming unfashionable. When in their wilds the women are stark naked. They are the ugly and less intelligent sex, with a considerably darker colour than the decidedly intelligent men. Their household goods seem to be restricted to bows and arrows, plaited bags, and bamboo water-vessels; consequently they cannot cook, but only roast their food. They are clay-eaters. The nasal flute is disappearing. They are able to count up to five, have three kinds of dances, and bury their dead. There is the following curious parallelism between these Uti-krag, which in their idiom means Tortoise-Sierra, and the Mimba of New Guinea (*cf.* Pilhofer, *Petermann's Mittheil.,* September, 1912):—They construct stockades by putting numbers of sharpened sticks, 4 to 5 in. in length, into the ground, covered with leaves. As the enemy, when treading on these spikes, is sure to stumble, he falls upon a second line of larger sharpened sticks, also concealed. The author thinks that this little tribe is not so much a sample of the vigorous primitive savage as rather ethically impoverished through life in the forest. There are eight photographs of the people, their arms, and the stockade spikes.

THE excellence of the work being done by French physical anthropologists is exemplified by the elaborate descriptive memoir by Prof. M. R. Anthony on the fossil skull of La Quina, contributed to *Bulletins et Mémoires de la Société d'Anthropologie* (No. 2, for 1913). The writer identifies it with the Neanderthal group, including the Spy, La Chapelle, and Gibraltar skulls. This comprehensive, well-illustrated memoir is an important contribution to our knowledge of palæolithic man.

THE seventh annual report on the results of the chemical and bacteriological examination of London waters for the twelve months ending March 31, 1913, by Dr. Houston, has been issued by the Metropolitan Water Board. After a summary of the condition of the raw water and the effects of storage and filtration, Dr. Houston's final conclusion is that the "quality policy" of the Metropolitan Water Board should be directed towards securing an "epidemiologically sterile" water (*i.e.*, a water containing none of the microbes associated with water-borne disease) *antecedent* to filtration, by means of storage (sedimentation, devitalisation, and equalisation), aided, if need be, by the occasional employment of supplementary processes of water purification.

THE final report of the Luangwa Sleeping Sickness Commission, by Dr. Kinghorn, Dr. Yorke, and Mr. Lloyd, published in a recent number of the Annals of Tropical Medicine and Parasitology (vol. vii., No. 2) with many illustrations, is a very important contribution to the study of trypanosomiasis in man and animals. Especially valuable are the observations on the human trypanosome, *T. rhodesiense,* its wide distribution in south Central Africa, its occurrence in wild game and domestic stock, and its transmission by *Glossina morsitans.* The authors affirm emphatically that the fly does not become infective until the trypanosome has invaded its salivary glands, an event which is the second and final act of a developmental cycle that begins in the gut of the fly. It was found that the first portion of this cycle could proceed at lower temperatures (60°–70° F.), but that for its completion higher temperatures (75°–85° F.) are essential. The parasites can, however, persist in the fly at an incomplete stage of their development for at least sixty days under unfavourable climatic conditions. Several species of trypanosomes, some old, some new, are described from wild game or domestic stock; remarkable among the new species is a large form of the ingens-type, to which the name *T. tragelaphi* is given, found in the blood of the sitatunga, *Limnotragus spekei.*

THE problems connected with tsetse-flies and the parasites of man and animals which they unwittingly transmit are perhaps the most important questions with which European administrators of African territories have to deal at the present time, and these troublesome insects continue to receive an amount of attention which their purely scientific interest would never have aroused. In the Annals of Tropical Medicine and Parasitology (vol. vii., Part 2), Prof. Newstead describes a new species of tsetse-fly from the Congo Free State under the name *Glossina severini*; and in the same number Mr. Llewellyn Lloyd publishes records and photographs of the breeding-places of *G. morsitans* at Ngoa, on the Congo-Zambesi watershed. The pupæ of *G. morsitans* were always found either in association with trees or in holes in the ground; in the former case the trees were always abnormal or injured. The pupæ were never found at the base of normal trees or under bushes, and they are always deposited in such positions that they are not likely to be scratched up by game-birds. In the Bulletin of Entomological Research (vol. iv., Part 1), Dr. Scott Macfie discusses, with the aid of many photographs, the distribution of tsetse-flies in the Ilorin province of northern Nigeria, and describes a new variety of *G. palpalis,* with an excellent coloured illustration. In the same journal Dr. J. O. Shircore describes two new varieties of *G. morsitans* from Nyasaland.

THE proceedings of the Orchid Conference held by the Royal Horticultural Society in November last are reported at length in the Society's *Journal* (vol. xxxviii., Part 3). They include four papers read at the Conference, in the first of which Prof. F. Keeble discussed the physiology of fertilisation, with special reference to recent investigations by Lutz, Fitting, and others, and pointed out that pollination may bring about three types of events: (1) fertilisation, (2) changes due to contact of pollen with the stigmatic surface, and (3) results which may be described as intoxications or responses to chemical stimulation. In a paper on the application of genetics to orchid breeding, Major C. C. Hurst recapitulated the first principles of genetics, and pointed out that as regards any one heritable character represented by a factor, there are three distinct kinds of individual plants—homozygous or pure, heterozygous or impure, and zerozygous or wanting. He also dealt with the identification of individual "stud" plants, colour and albinism, self-sterility in orchids, &c.

IT requires an altogether special equipment, not only of exact zoological and historical knowledge but also of sympathetic insight into the conditions which prevailed in the past, to compose such a delightful lecture as that which Prof. F. J. Cole, of Reading, has published in the Transactions of the Liverpool Biological Society (February 14, 1913). In his crisp and epigrammatic treatment of a series of well-chosen incidents he has admirably brought before us "the early days of comparative anatomy," and the feeling of most readers, and especially those who from their personal experience realise how difficult such knowledge is to acquire, will be to emulate Oliver Twist's example and ask for more. It is quite impossible to summarise a report so crowded with curious information, witty comment, and historical insight, illuminating the whole development of the science of comparative anatomy. From the knowledge acquired as a collector of old "anatomies," Dr. Cole has been able to explain the loose methods of publication in the seventeenth and eighteenth centuries, which opened the way for those glaring instances of unscrupulous plagiarism that have ever been a source of amazement to us who live in such vastly different circumstances. But no part of the discourse excels in piquancy and common sense the opening "apology" for the study of the history of biology.

A RECENT number of the *Centralblatt für Bakteriologie, Parasitenkunde,* &c. (Zweite Abt., Band 38) contains a detailed account by O. Schneider-Orelli of investigations on the life-history and habits of *Xyleborus dispar,* one of the bark-beetles (Scolytidæ). This species, notorious for the injuries it inflicts on fruit-trees, is remarkable for its symbiosis with a fungus, *Monilia candida,* Hartig. The female beetles, fertilised in the autumn, hibernate in their burrows through the winter and swarm out in the following April and May. Each female then becomes the foundress of a new colony; she bores into a tree and makes a system of burrows, the walls of which become lined with a growth of the fungus, forming a dense white mass, the so-called "ambrosia." The mother-beetle lays her eggs in the burrows and the larvæ feed on the ambrosia-fungus, not on the wood of the tree. Living cells or spores of the fungus are not to be found in the digestive tract of the larva, pupa, or newly hatched adult beetle, but the female beetles appear to take up the fungus from the walls of the burrow in which they have been bred, and the stomachs of the mother-beetles always contain a store of the fungus, capable of germinating. The culture is started in the new burrows by regurgitation of the fungus from the stomach, and is continued by the beetle plucking off clumps of the young culture and planting them further along in the burrow. If disturbed in her agricultural operations, the mother-beetle hastily swallows as much as she can of the fungus.

In a paper on the psychology of insects, read before the General Malarial Committee at Madras in November, 1912, Prof. Howlett, after giving an account of experiments carried out by him on the response of insects to stimuli, comes to the conclusion that insects are to be regarded "not as intelligent beings consciously shaping a path through life, but as being in a sort of active hypnotic trance." It is claimed that this view of insect-psychology opens up great possibilities in the study of insect carriers of disease, since "it is no intelligent foe we have to fight, but a mere battalion of somnambulists." If we discover the stimuli or particular conditions which determine the actions of an insect, we can apply them to its undoing. It was found, for example, that the females of the fruit-fly, a serious pest in some parts of India, emitted an odour resembling ordinary citronella, and that the males could be caught in very large numbers by baiting traps with citronella, since they came to the traps and remained there apparently under a blind impulse to follow the scent of the female. In this way they had succeeded in checking largely the incidence of the fruit-fly pest.

In reference to a recent paragraph in our notes columns on a large dinosaurian limb-bone from Bushman's River, S. Africa, we have received a letter from Dr. R. Broom pointing out that Owen was incorrect in stating that Anthodon came from that locality, and that (as mentioned in *Brit. Mus. Cat. Foss. Reptilia*) its real place of origin was Stylkrantz. It is added that Anthodon is not a dinosaur, but a pariasaurian, and is thus rightly classified in the work just quoted. Dr. Broom appears to forget that in 1895 (*Rec. Albany Mus.*, vol. i., p. 277) he himself stated in reference to Anthodon that there "seems a strong probability that the three original specimens were got by Bain at Bushman's River." Later on he observed that "by Owen Anthodon was believed to be a dinosaur; by Lydekker and others it has been believed to be allied to Pareiasaurus. . . . The teeth are unlike those of Pareiasaurus, and strikingly like those of dinosaurs, and it seems possible that Owen may ultimately prove to be right." Basing our remarks on these statements, our one error was the assertion that Anthodon is *known* to be a dinosaur. As the Stylkrantz beds are Permian, and those of Bushman's River Cretaceous, there can, of course, be no community between their faunas.

The September number of *The Selborne Magazine* contains a list of lectures delivered before the Selborne Society during the past few years, and the names of the lecturers. Any of these discourses, which cover a great range of subjects, and are profusely illustrated with lantern-slides, the respective lecturers are prepared to repeat, either singly or in series, to local natural history societies or schools in return for their expenses, or moderate fees.

To the August number of *The Irish Naturalist* Dr. R. F. Scharff contributes a note on the Belmullet whaling station, based on a paper by Mr. Burfield in the British Association report for 1912. The number of whales taken by the Blacksod Whaling Company in 1911 was sixty-three, against fifty-five the previous year. The catch of the other company is not given.

An interesting article by Mr. C. H. Eshleman on the "Climatic Effect of the Great Lakes as Typified at Grand Haven, Mich.," on the east of the lake, is published in the meteorological chart for September issued by the U.S. Weather Bureau. Few stations are more favourably placed for this purpose; it has a broad expanse of eighty-five miles of water to the westward, and the shore is comparatively regular and almost straight to the north and south. Its temperature is compared with that of Milwaukee on the west shore, and with several inland stations lying to the westward in the same latitude. The tables show, *inter alia,* that the annual means are practically the same; the monthly maxima along the lake are strikingly modified in spring and summer, but only slightly in the other seasons; the minima are greatly modified in autumn and winter. The lake acts as a barrier against the extreme cold from the far north-west; the temperature at Grand Haven is often 20° higher than at Milwaukee, but with easterly winds it is almost as cold at Grand Haven as away from the lake. All the other climatic features are modified, but the effect on yearly or monthly precipitation is not striking. We notice with regret that the publication of these valuable meteorological charts, including those of the great oceans, has now ceased.

The after-shocks of the Messina earthquake of December 28, 1908, have been referred to in several of our Notes. On the last occasion (vol. xci., p. 93) a summary was given of observations made at Messina during the year 1909. From these it appeared that the distribution in time of the after-shocks did not follow Omori's law, $y=h/(k+x)$, where h and k are constants and y the number of after-shocks during a given interval at time x from the earthquake. These observations, however, referred to all the shocks felt at Messina, and not only to the true after-shocks of the great earthquake. The latter are distinguished in the valuable notices of earthquakes observed in Italy during 1909, of which we have received the last three numbers for the year. From these it is seen that the decline in frequency of the true after-shocks, though exhibiting the usual fluctuations, does not depart widely from Omori's well-known law.

Part 14 of the *Verhandlungen* of the German Physical Society contains further details of the method used and the results obtained by Drs. A. Eucken and F. Schwers, of the University of Berlin, in their measurements of specific heats of substances at very low temperatures. A cylindrical block of the material to be investigated had a constantan heating wire of 2 mm. diameter and 200 ohms resistance wound round it, electrical insulation and adequate thermal contact being secured by varnish. The temperature attained was determined from the resistance of a lead wire wound round the cylinder in the same way. The electrical heating was carried out in a vacuum vessel at temperatures between 16° and 92° on the absolute scale. Over this range the specific heats of fluorspar and iron pyrites crystals vary as the third power of the absolute temperature in agreement with the theory put forward by Debye in the first instance for monatomic substances.

"THE Coal Resources of the World," to which allusion was made in NATURE of September 4, is being brought out in this country by the American Book Supply Co., Ltd., 149 Strand.

OUR ASTRONOMICAL COLUMN.

A NEW COMET.—A telegram from the Centralstelle at Kiel, dated September 3, announced the discovery of a comet of magnitude 10·0 by Dr. Metcalf, of Winchester, Massachusetts, on September 1, at 8h. 42·0m. M.T. Winchester. Its position was given as RA 6h. 50m., and declination 57° 0′ N., and was stated to have a slow motion to the north. It was suggested that it possibly might be Westphal's comet. A second telegram, dated September 4, stated that Prof. Antoniazzi had seen the same object on September 3 at 15h. 39·4m. M.T. Padua. The position he gave was RA 6h. 48m. 12s., and declination 57° 25′ 34″ N., the daily motion in RA being −1m. 16s., and in declination +34′. According to a writer in *The Times* (September 6), Prof. Antoniazzi's observation indicates that the comet cannot be that of Westphal, as the daily motion is diminishing instead of increasing.

The new comet is in the constellation of the Lynx, and therefore to be observed any time during the night, but best visible after midnight.

The following elements and ephemeris of comet *b* (Metcalf) have been communicated from Kiel, based on the observations of September 2, 3, and 4:—

$$\begin{aligned} T &= 1913 \text{ July } 20.1129 \text{ Berlin.} \\ \omega &= 51^\circ\ 31{\cdot}47' \\ \Omega &= 136^\circ\ 9{\cdot}99' \\ i &= 142^\circ\ 49{\cdot}23' \\ \log q &= 0{\cdot}20954 \end{aligned} \quad \left.\begin{matrix}\\ \\ \\ \end{matrix}\right\} 1913{\cdot}0.$$

12h. M.T. Berlin.

		R.A. h. m. s		Dec. ° ′
Sept. 11	...	6 32 54	...	+62 21·6
13	...	27 1	...	63 44·7
15	...	6 19 53		65 10 5

The magnitude is given as 10·5 on September 5 and 10·3 on September 15.

ANOTHER COMET.—Another telegram from Kiel, dated September 7, states that Dr. K. Graff discovered on September 6, at 15h. 9·1m. M.T. Bergedorf, a comet of the 11th magnitude. Its position is given as R.A. 23h. 48m. 1·8s., and declination 0° 27′ 44″ S. It is thus situated in the constellation of Pisces, and should be best seen about midnight.

THE PROTECTION OF SILVERED MIRRORS FROM TARNISHING.—Everyone who uses mirrors for astronomical purposes would welcome a satisfactory method of coating them with some material for preventing tarnishing. M. Perot some years ago published an account of a process he employed successfully for treating mirrors at the Meudon Observatory. It consisted of a thin coating of collodion being applied to the surface of the mirror, the film being obtained by pouring over the mirror a solution of collodion in amyl acetate. The mirrors used at the Helwan Observatory are subject to becoming spotted a few weeks after silvering, and attempts have been made to protect them. Mr. S. H. Trimen, of the Survey Department Laboratories, made the various trials (Khedivial Observatory, Helwan, Bulletin No. 10), but after repeated attempts with solutions of varying percentages he had at length to abandon the process. Blurred images of the stars and curious flares on the photographs of the bright stars were always the result of the application of the film. It is suggested that the problem may possibly be solved by using a solution possessing a lower viscosity, and it is to be hoped that such an attempt will be made.

RESEARCHES ON THE SUN.—The last two numbers of the *Astrophysical Journal* (June and July) have contained several important researches relating to solar physics. Prof. George E. Hale, in the July number, publishes his most valuable paper on the "Preliminary Results of an Attempt to Detect the General Magnetic Field of the Sun," a summary of which, based on an advanced proof, having been given in a previous number of this journal (June 17, p. 505). In connection with this research, Mr. Frederick H. Seares gives in the same number a paper entitled, "The Displacement-curve of the Sun's General Magnetic Field." The spectrum lines observed with the 75-ft. spectrographs and the polarising apparatus of the 150-ft. tower telescope showed displacements that apparently could not be attributed to any other cause than a general magnetic field of the sun, and the object of his research was to provide a more rigorous control of the results and their interpretation. Thus in the paper he compares the observed displacements with the theoretical displacement-curve derived on the assumption that the sun is a magnetised sphere, and further he provides formulæ for determining the position of the magnetic axis relative to the axis of rotation.

In the June number Mr. Charles E. St. John deals with the remarkable discovery by Mr. Evershed of the displacement of the Fraunhofer lines in the penumbræ of sunspots. The paper is entitled, "The Distribution of Velocities in the Solar Vortex," and the observations recorded in this research are in entire accord with Mr. Evershed's hypothesis that the displacements considered are due to a movement of the solar vapours tangential to the solar surface and radial to the axis of the spot vortex. Previous reference has already been made of Prof. Slocum's second paper on the circulation in the solar atmosphere as indicated by prominences.

THE INSTITUTE OF METALS.

THE autumn meeting of the Institute of Metals took place at Ghent on August 28 and 29 last. This was the first occasion on which this institute has held a meeting abroad, and the gathering may be described as a complete success. The attendance of members (about seventy-five) was particularly satisfactory on account of the representative character of those present, the foreign members of the institute, including members from Russia, Germany, Belgium, and America, being particularly well represented. The mornings of August 28 and 29 were devoted to the discussion of a long and interesting list of papers, while the afternoons were utilised for visits to works, the inspection of the exhibition, and the antiquities of the city. The social functions included a reception by the Burgomestre of Ghent in the ancient and beautiful Hotel de Ville.

The foremost place in the work of the meeting was taken by the reading and discussion of the second report to the Corrosion Committee, presented by Dr. G. Bengough and R. M. Jones. This report deals with the examination of a considerable number of examples of corroded tubes from service, of the investigation of the mechanism of corrosion in sea-water, both at the ordinary temperature and at higher temperatures by means of small laboratory experiments, and the systematic study of corrosion in condenser tubes of various composition under conditions as nearly like those of practical service as possible in an experimental condenser plant set up by the committee at Liverpool University. The results as stated in the report are remarkable, and diverge widely from the views generally accepted hitherto. The examination of material from service did not lead to any conclusive results, but the data furnished by the experimental plant—so far as they yet go—confirm the results of the small laboratory experiments. The report deals entirely with the process of corrosion by dezincification, which the authors have found to be the most common form of corrosion, although it appeared in the discussion that they admitted that pitting does occur in the absence of dezincification. They find, however, that none of the alloys tested by them undergo selective corrosion or dezincification when exposed to sea-water at the ordinary temperature, but they are all subject to it at higher temperatures, the process becoming vigorous towards 40° C. The action of dezincification is connected by the authors with the formation of a basic zinc chloride, or of zinc hydroxide, which is found attached to the surface of the tube, and under these patches the metal is dezincified. The action of this basic salt is described as regenerative and dependent upon the presence of dissolved oxygen in the water. The experiments of the authors dealt with four alloys, viz. brass, containing copper 70 per cent., zinc 30 per cent.; Muntz metal, containing copper 61 per cent., zinc 39 per cent.; "Admiralty" brass, containing copper 70 per cent., zinc 29 per cent., and tin 1 per cent.; and a special alloy, containing copper 70 per cent., zinc 28 per cent., and lead 2 per cent. As regards resistance to dezincification at 40° and 50° C. the last-named proved superior to the others, the Muntz metal and 70/30 brass being the least resistant. Another interesting and surprising result obtained by the authors is that dezincification does not appear to be due to electrolytic action; they find—contrary to what has been generally believed—that the presence of particles of carbon, or similar materials, even including a plug of pure copper screwed into the tube, does not give rise to local action of this kind. The report contains a detailed account of the experiments upon which these conclusions are based, and thus constitutes a record of work which must be of fundamental importance in the future study of the whole question of corrosion in copper alloys.

Of considerable general interest also was the paper on the intercrystalline cohesion of metals by Dr. W. Rosenhain, F.R.S., and Mr. D. Ewen, of the National Physical Laboratory. The authors of this paper elaborate their theory that the crystals of a metal, forming a crystalline aggregate, are held together by a thin layer of the same metal in the amorphous or undercooled liquid condition. The experimental evidence offered in their first paper they now supplement by evidence obtained from an entirely different direction. In the present paper the relative mechanical properties of the crystals and of the amorphous cement are discussed, and the authors indicate that while at ordinary temperatures the cement would be much harder and stronger than the crystals, at temperatures near the melting point this relation must be reversed, since the undercooled liquid cement must pass into the ordinary fluid condition in a gradual and continuous manner while the crystals will soften suddenly on melting. Just below the melting point, therefore, the theory indicates that the crystals could be pulled apart from one another without undergoing any distortion, thus giving a perfectly brittle fracture—the brittleness being entirely intercrystalline. The authors show such brittle intercrystalline fractures in the case of the purest lead, tin, aluminium, and bismuth, the lead fractures being particularly striking in appearance. At the meeting these data were supplemented by a similar brittle fracture obtained in a bar of the purest gold which had been prepared for the authors by Dr. T. K. Rose, of the Royal Mint. The discussion showed that the theory of an amorphous cement is still received with some reserve, but it was admitted that the accumulation of experimental evidence has considerably strengthened the position of the theory.

The list of papers dealt with at the meeting included nine others, among which those of Dr. T. K. Rose on the annealing of gold, of Dr. W. M. Guertler on the specific volume and constitution of alloys, of Prof. S. L. Hoyt on the constitution of the copper-rich kalchoids, or alloys of copper, zinc, and tin, of Mr. J. H. Chamberlain on volume changes in alloys, and of H. Garland on the metallographic study of some Egyptian antiquities, were of considerable interest. The discussions were in all cases vigorous and full of interest, and the meeting marks a decided advance in the development of the Institute of Metals.

THE PAST SUMMER.

BROADLY speaking, the past summer was essentially dry, generally cool, and particularly sunless considering the small amount of rain.

The reports issued by the Meteorological Office show that for the whole period of thirteen weeks which comprise the summer the mean temperature was below the average in all parts of the United Kingdom except in the north of Scotland, the deficiency being greatest over the east and south-east of England. The mean temperature was higher than in 1912 over the entire kingdom with the exception of the east of England, but it was everywhere much cooler than in the abnormally hot summer of 1911, the difference being greatest in the midland, southern, and eastern districts of England.

The sunshine was deficient over the eastern portion of the kingdom, but generally in excess in the western districts. The hours of bright sunshine were everywhere more numerous than in 1912, but far fewer than in 1911. Taking the British Isles as a whole the total hours of sunshine were 492 in 1913, 373 in 1912, and 679 in 1911.

The average rainfall for the whole of the British Isles for the three summer months—June, July, and August—was 4·47 in. this year, 12·92 in. last year, and 6·27 in. in 1911; the average number of rainy days are thirty-five this summer, sixty-one last year, and thirty-seven in 1911. The rainfall this summer was less in all districts of the United Kingdom than in the dry summer of 1911. In the south-west of England the aggregate summer rainfall was only 39 per cent. of the average, in the north-east of England 44 per cent., and in the midland counties and the south-east of England 45 per cent. The wettest districts were the north of Ireland, 68 per cent. of the normal, north-west of England, 67 per cent., and north of Scotland, 66 per cent.

At Greenwich, which fairly represents England, the weather at the end of May was persistently hotter than at any time during the summer. On six consecutive days the sheltered thermometer rose to 80° or above. Throughout the summer, from June to August, there were only four days with 80° or above, the average number of such warm days for the summer is fourteen, and in 1911 there were thirty-seven days as warm.

The following are the chief meteorological results at Greenwich:—

1913	Temperature							Rainfall			Sunshine	
	Mean highest day	Diff. from average	Mean lowest night	Diff. from average	Mean	Diff. from average	Days above average	Rainy days	Total	Diff. from average	Hours	Diff. from average
	°	°	°	°	°	°			In.	In.		
June ...	71	0	50	0	61	0	12	8	0·61	−1·33	204	+22
July ...	68	−6	52	−1	60	−4	1	12	2·01	+0·12	95	−91
August ...	71	−2	52	−1	62	−1	9	11	2·07	−0·27	143	−34
Summer...	70	−3	51	−1	61	−2	22	31	4·69	−1·45	442	−103

It is seen that June was very dry, but in other respects fairly normal. July had a fairly normal rainfall, but only about one-half of the average sunshine, whilst the temperature was exceptionally low. August was very dry until quite the close of the month, when exceptionally heavy rains fell over the south-eastern portion of England; at Greenwich the total for the last three days of the month was 1·22 in.; both sunshine and temperature were deficient.

CHAS. HARDING.

A MEMOIR ON THE ARTHROPOD EYE.[1]

CONSIDERABLE progress has been made of late towards the elucidation of the structure of the arthropod eye. Prof. G. H. Parker, of Harvard, was the first seriously to attack the more intricate problems of its structure, and his insight was such that most of his work stands unchanged even after the more recent elaborate researches of Hesse, Schneider, and others. In the present paper Dr. Trojan has, in addition to his own researches, verified or corrected the observations of these later investigators, whose work had summarised and illuminated the results of earlier writers.

It will be of profit to mention briefly the interesting points that Dr. Trojan has been able to add to our knowledge. The corneal cells are not "tile-shaped," but they are broader distally than proximally, so that they appear triangular in transverse section. The author is unable to support Schneider's observation that they are four in number, but agrees with Parker and Hesse that there are only two. The structure of the crystalline cells (cone cells) differs in one respect from that described by other writers; the upper part, the "Zapfen," is abruptly cone-shaped distally, and passes between the corneal cells to the facet. The general structure of the retinular cells and rhabdome is as Parker and later writers have described, but Dr. Trojan supports Hesse's opinion (and differs from Parker and Schneider) that there is no "zwischensubstanz" (matrix) between the "stiftchen" (fine rods) composing each half-plate of the rhabdome. The innervation of the rhabdome is effected as Hesse described, the nerve fibrillæ passing up the outer side of the retinular cell, round the nucleus, and terminating on "knöpfchen" as the base of the "stiftchen" composing the lamellæ of the rhabdome. Three optic ganglia are described.

The most important part of the paper is devoted to a study of the pigment of the eye in darkness and light; this, however, is best consulted in the original. There are only two pigment-bearing cells which form a continuous tubular sheath enclosing the whole ommatidium from the crystalline cones to the bundles of nerve fibrillæ under the basement membrane. The author's observations on the movements of the pigments of these cells, and also of the non-pigmented tapetum cells are of considerable interest.

In the course of the paper Dr. Trojan deals successively, under separate headings, with the different elements of the eye, giving an exhaustive anatomical and historical account of each, an arrangement which is lucid and very easy to follow. The plates illustrating the paper are very fine—we wish we could believe they could have been as exquisitely reproduced in this country—though we should have preferred to have had more of the author's own drawings in place of the photographs, beautiful as they are.

Dr. Trojan's paper is an important contribution to the literature of the arthropod eye, not only for the original matter it contains, but also as a critical review of the work of previous observers.

H. G. J.

[1] "Das Auge von Palaemon Squilla." By Dr. E. Trojan. Denk. d. Kais. Akad. d. Wiss. math-naturw. Klasse. Bd. 88. Wien, 1912. 54 pp.+6 pl.

BIOLOGY OF AQUATIC PLANTS.

DR. W. H. BROWN has contributed an important paper on the biology of aquatic flowering plants to *The Philippine Journal of Science* (vol. viii., pp. 1–20), under the title, "The Relation of the Substratum to the Growth of Elodea." He confirms the statements of previous observers that in this and in other submerged plants there is a "transpiration current" of water up the vessels of the stem, but his experiments lead to the conclusions that this current is simply a necessary consequence of the physical construction of the plant, and that the passage of water through a submerged plant does not show that the movement is of advantage to the plant by causing condensation of nutrient salts or that the roots are of advantage as absorbing organs.

Dr. Brown gives tables showing the relative growth of Elodea with and without addition of carbon dioxide to the water, in tap water, and in Knop's solution, with and without soil, rooted in and simply anchored over soil or sand, &c., and summarises the results as follows. Sufficient carbon dioxide to keep Elodea growing or even alive does not diffuse from the air into the water during winter and spring; the substratum probably serves as an important source of this gas. Elodea is not dependent on its roots for absorption of mineral salts, the chief function of the roots being to anchor the plant to the soil, which is advantageous when the soil contains organic matter and gives off carbon dioxide; plants rooted in good soil grow better than those anchored over the same soil. When carbon dioxide was supplied by a generator instead of by the soil, rooted and anchored plants grew about equally well; with similar soils, and no external supply of carbon dioxide, floating plants grew better than rooted ones, the air being in this case the source of carbon dioxide.

The author's work is of great interest with reference to the relation between the growth and abundance of plankton organisms (which form a large proportion of the food of fishes) and of larger water plants, and has obvious economic bearings; for instance, his experiments would seem to show that the larger submerged plants compete with the plankton algæ for both carbon dioxide and mineral salts, and must therefore be detrimental to their growth.

F. C.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

LONDON.—A course of twelve post-graduate lectures on "Conductors for the Electrical Transmission of Energy" will be delivered at University College by Prof. J. A. Fleming, F.R.S., beginning on October 29. The course, which is intended for post-graduate students and for telegraphic and electrical engineers, engaged in practical work, will be divided into two parts, which may be attended separately. Part i. will be devoted to "Telegraph and Telephone Conductors," and part ii. to "Electric Light and Power Conductors."

THE Maharaja of Jaipur has made a contribution of three lakhs towards the establishment of a Women's Medical College at Delhi.

AMONG recent appointments at American universities we notice the following:—Dr. A. H. Ryan to the chair of physiology in the medical department of the University of Alabama; Dr. J. A. Bullitt to the chair of pathology in the University of North Carolina.

MR. D. C. MATHESON, at present on the veterinary staff of the Board of Agriculture and Fisheries, and formerly connected with the veterinary school of the University of Liverpool, has been appointed to the chair of pathology and bacteriology in the Royal (Dick) Veterinary College, Edinburgh.

IT is stated in *The Lancet* that of the scholarships to be founded at Aberdeen University by the bequest of Mr. W. Robbie (briefly referred to in our issue of August 21) one is to be for chemistry. The principal of the sum left to the University by Mr. Robbie is to be kept intact, and the interest used in providing perpetual scholarships.

AN alarming outbreak of fire took place on Friday morning last at Dulwich College, damage being done to the extent of about 300*l*. The fire appears to have been the work of Suffragettes. The scene of the outrage was one of the chemical laboratories on the first floor of a block of buildings devoted partly to the engineering section of the college and partly to chemistry. Before the fire could be extinguished a lecture platform was destroyed, the floor of the room badly damaged, and the windows broken by the heat.

THE recently published reports for 1911–12 from those universities and university colleges in Great Britain which are in receipt of grants from the Board of Education show that, in the twenty-five institutions of higher education concerned, there were 22,895 students, excluding 238 who were preparing for matriculation. In English colleges there were 7827 full-time students, 3370 part-time day students, and 7295 part-time evening students. Of this number it appears that 1596 were engaged upon post-graduate work. In Wales there were 1377 full-time students and 343 part-time students, none of them attending in the evening. Of the total number of full-time students admitted during the session 1911–12 to English colleges, 4·6 per cent. were under seventeen years of age, 11·9 per cent. between seventeen and eighteen years of age, 27·6 between eighteen and nineteen years of age, and 55·9 per cent. more than nineteen years old. In Wales, 33·7 per cent. of the students were between eighteen and nineteen, and 54·1 per cent. more than nineteen years of age.

A COPY of the calendar of the day and evening classes to be held at the Battersea Polytechnic during the session which begins on September 16 has been received. Courses have been arranged both during the day and in the evening in preparation for degrees in science, engineering, and music at the University of London. In the day technical college, full-time courses are arranged in mechanical, civil, electrical, and motor engineering, architecture, and building, and chemical engineering, each covering a period of three years, at the end of which time students passing the necessary examinations are awarded the polytechnic diploma. There are also courses in mathematics, physics, chemistry, and botany. The training department of domestic science offers two, three, or four year courses in preparation for the teachers' diplomas in domestic subjects. In the evening, classes have been arranged to meet the needs of every class of student. Science, technology, commerce, art, and literature are all to be taught in a thoroughly practical manner, and the social and physical education of the students is not neglected.

THE new session of the Sir John Cass Technical Institute, Aldgate, E.C., will commence on September 22. The syllabus of classes, which has reached us, shows that the educational needs of the district are being cared for admirably. In connection with the higher technological work, several new departures are being made for the coming session. The curriculum in connection with the fermentation industries has been much developed, and now includes courses of instruction on brewing and malting, bottling and cellar management, brewery plant, and on the microbiology of the fermentation industries. A connected series of lectures dealing with the supply and control of power has also been arranged to meet the requirements of those engaged in works connected with chemical, electrical, and the fermentation industries. These will comprise a course of lectures on the supply and control of liquid, gaseous, and solid fuel, a course on electrical supply and control, and a course on the transmission of power. The courses in the metallurgical and other departments will be of the same character as in previous years, and the object will be to meet the needs of the industries in the districts served by the institute.

THE ninth annual report of the Education Committee of the County Council of the West Riding of Yorkshire is an excellent account of a good year's educational work. From the section dealing with higher technical education we learn that in consideration of the grants received from the County Council the Universities of Leeds and Sheffield have been engaged in the organisation and supervision of classes in coal mining, the Leeds University in the area of the West Yorkshire Coalfield, the Sheffield University in the area of the South Yorkshire Coalfield; and each University has made provision for the training in mine gas testing, of persons selected by the Education Committee as prospective teachers of this subject. The Joint Agricultural Council of the three Ridings of Yorkshire have continued the work connected with education and instruction in agricultural subjects, acting through the agricultural department of Leeds University, on the same lines as before. The two outstanding features of the year's work in the technical and evening schools which call for special mention have been, first, a successful summer meeting of teachers in evening schools, and secondly, a considerable development in regard to the provision of evening classes for adults in non-vocational subjects.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 25.—M. A. Chauveau in the chair.—Kr. **Birkeland**: Remarks on the attempts made by Hale to determine the general magnetism of the sun. The results recently published by Hale are at variance with the author's views, if the general magnetism of the sun is similar to that of the earth. The objection made by Hale to the theory of local vortices is discussed. According to the author's researches, the magnetic moment of the sun is of the order of 10^{28} C.G.S. units, and the magnetisation is directed in a sense contrary to that of the earth.—

Georges **Claude**: The maintenance of temperatures about $-211°$ C. by the use of liquid nitrogen. An admission of priority to Sir James Dewar.—R. **Swyngedauw**: The integration of the equation giving the distribution of the density of an alternating current in cylindrical conductors.—P. Th. **Muller** and R. **Romann**: The dissociation of good electrolytes and the law of mass action.—J. **Bougault**: The isomerisation of the α-hydroxy $\beta\gamma$-unsaturated acids into γ-ketonic acids. The views of Fittig, Thiele, and Erlenmeyer on this well-known isomeric change are discussed. The author puts forward experimental evidence in favour of the following simplified scheme:—

$$R.CH=CH.CH(OH).CO_2H \longrightarrow R.CH(OH).CH=CH.CO_2H \longrightarrow R.CO.CH_2.CH_2.CO_2H.$$

—Em. **Bourquelot** and M. **Bridel**: The biochemical synthesis of glucosides of polyvalent alcohols; the α-glucosides of glycerol and glycol.—Stanislas **Meunier**: An unrecognised point in the fossilisation of organic débris.

BOOKS RECEIVED.

Research in China. (In three volumes and Atlas.) Vol. iii.: The Cambrian Faunas of China, by C. D. Walcott. A Report on Ordovician Fossils collected in Eastern Asia in 1903–04, by S. Weller. A Report on Upper Paleozoic Fossils collected in China in 1903–04, by G. H. Girty. Pp. vii+375. (Washington, U.S.A.: Carnegie Institution.)

The Infinitive in Anglo-Saxon. By Prof. M. Callaway, Jr. Pp. xiii+339. (Washington, U.S.A.: Carnegie Institution.)

Botanical Features of the Algerian Sahara. By W. A. Cannon. Pp. vi+81+36 plates. (Washington, U.S.A.: Carnegie Institution.)

The Diffusion of Gases through Liquids and Allied Experiments. By Prof. C. Barus. Pp. vii+88. (Washington, U.S.A.: Carnegie Institution.)

The Fermentation of Cacao. Edited by H. H. Smith. Pp. lvi+318. (London: J. Bale, Sons, and Danielsson, Ltd.) 10*s.* net.

The Poisonous Terrestrial Snakes of our British Indian Dominions (including Ceylon), and How to Recognise Them, with Symptoms of Snake Poisoning and Treatment. By Major F. Wall. Pp. xiv+149+iv. Third edition. (Bombay: Bombay Natural History Society; London: Dulau and Co., Ltd.) 3 rupees.

A Handbook for Birmingham and the Neighbourhood. Prepared for the eighty-third Annual Meeting of the British Association for the Advancement of Science. Edited by Dr. G. A. Auden. Pp. vii+637. (Birmingham: Cornish Bros., Ltd.) 3*s.* 6*d.* net.

Gruppenweise-Artbildung unter spezieller Berüchsichtigung der Gattung Oenothera. By Prof. Hugo de Vries. Pp. viii+365+22 plates. (Berlin: Gebrüder Borntraeger.) 22 marks.

Chemical Technology and Analysis of Oils, Fats, and Waxes. By Dr. J. Lewkowitsch. Fifth edition, entirely rewritten and enlarged. Vol. i. Pp. xxiii+668. (London: Macmillan and Co., Ltd.) 25*s.* net.

Vectorial Mechanics. By Dr. L. Silberstein. Pp. viii+197. (London: Macmillan and Co., Ltd.) 7*s.* 6*d.* net.

Principles of Thermodynamics. By Prof. G. A. Goodenough. Second edition, revised. Pp. xiv+327. (London: Constable and Co., Ltd.) 14*s.* net.

Sinopsis de los Ascaláfidos (Ins. Neur). By R. P. Longinos Navás, S.J. Pp. 99+2 plates. (Barcelona: Institut d'Estudis Catalans.)

Abhandlungen der K.K. Geologischen Reichsanstalt. Band 16. Heft 4. Beiträge zur Kenntnis der Schichten von Heiligenkreuz (Abteital, Südtirol). By Dr. E. Koken. Pp. 43+6 Taf. (Vienna.) 12 kronen.

Handbuch der Morphologie der Wirbellosen Tiere. Edited by A. Lang. Erster Band. Protozoa. Lief. 2. Pp. 161–320. (Jena: G. Fischer.) 5 marks.

Minds in Distress. By Dr. A. E. Bridger. Pp. xii+181. (London: Methuen and Co., Ltd.) 2*s.* 6*d.* net.

Pheasants and Covert Shooting. By Capt. A. Maxwell. Pp. ix+332+16 plates. (London: A. and C. Black.) 7*s.* 6*d.* net.

Annals of the Astrophysical Observatory of the Smithsonian Institution. Vol. iii. Pp. xi+241. (Washington.)

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