NATURE

SATURDAY, OCTOBER 24, 1931.

CONTENTS.

Editorial and Publishing Offices:
MACMILLAN & CO., LTD.,
ST. MARTIN'S STREET, LONDON, W.C.2.

No. 3234, Vol. 128]

Native Administration in Africa.

DURING the past few years, incidents have occurred from time to time in certain parts of the British Empire which suggest that British administrators are losing their hold on the minds, if not the affections, of the indigenous races of the tropics. There can be no doubt that, if greater insight into the feelings and prejudices of the native races had been shown, recent difficulties in India, Burma, and East and West Africa would not have arisen, or would never have reached such an acute stage. There is no excuse for this state of affairs so far as our relations with the African Negro are concerned, since as a rule he started with a great admiration and friendliness for the European, while the workings of his mind are atune to ours and run to a large extent on parallel lines.

It is a truism to say that no people can be efficiently ruled and influenced without sympathy and that true sympathy cannot be given without a more than superficial knowledge of their religious beliefs and old customs. This point was emphasised by Lord Lugard in his recent lecture before Sections E and H of the British Association, an account of which appears elsewhere in this issue. Yet it is still insufficiently grasped by the British people as a whole, and even by those who are engaged in colonial administration. To this must be ascribed very largely the growing dissatisfaction and unrest which have been so evident of late years.

Take, for example, Nigeria, one of the greatest possessions of the British Crown and of immense economic value to the whole world, both from the number (some 20,000,000) and the industrious character of its population and from the great natural resources of the land. There is probably no country where a knowledge of the beliefs and customs of the people is of such paramount importance, and none where the study of these matters has received less consideration. The unhappy change which has there taken place in the feelings of the natives towards the administration, has been to a certain extent brought about by unavoidable causes, such as the world disturbances due to the War and the consequent trade depression; a misunderstanding of the phrase self-government; the departure of a large number of the older and more experienced officers and their replacement by young men (cadets now go out at the age of about twenty-two) of no experience and perhaps of greater inclination to hasty action; and the rapid spread of motor communications, which encourages officials to remain more in central towns and to confine their travelling to quick journeys in cars along the main roads, instead of, as in the old days, going slowly from place to place on foot or by bicycle, visiting even the smallest villages and getting to know the people thoroughly.

The chief cause of dissatisfaction in Southern Nigeria, however, has undoubtedly been the methods employed in the introduction of taxation, which has occupied so large a part of the officials' time since 1926. Unfortunately, the preliminaries for this momentous step were entrusted to officers with little experience of the country or acquaintance with the people. Those who had studied these matters and ventured to suggest the desirability of a longer preparation and further investigation were disregarded. The affair culminated, most regrettably, in the serious riots which took place at the end of 1929, and the old mutual confidence between the administrative officers and the mass of the people is now largely at an end.

It is true that, under the ægis of Sir Hugh Clifford, who was himself interested in native customs, a section on anthropology was included in the 1921 Census reports, and that, since then, two officers have been entirely engaged in anthropological studies ; little consideration, however, seems to have been given to this in the framing of government policy—though, on the Gold Coast, Capt. Rattray's illuminating investigations into the comparatively small but important Ashanti tribe met with considerable encouragement and attention.

During the last decade much stress has been laid on the necessity of learning native languages. This is all to the good, even in the southern provinces of Nigeria, where there are scores of languages and the main three are divided into so many dialects that it is difficult for an officer to become really proficient in any one of them, unless he is always stationed in the same place, which is rarely possible or desirable. The number of Europeans who could give a translation accurate enough for court purposes is very small indeed, while the African is such a remarkable linguist that, as a rule, he acquires a better mastery of English than the average European can hope to do of his tongue. English is being learned throughout Southern Nigeria so quickly that in a few years it will be the lingua franca of the whole country, a position which it has already attained on the main roads. Even so, it is very advantageous for an officer to acquire some knowledge of as many languages as possible so that he can to a certain extent check his interpreter, although this may be best achieved by care in the framing of questions ; it is generally easy to tell from the answers if these, or the statement, have been correctly translated. It is, however, of far greater importance to become conversant with the religion and customs of the people. It is not yet appreciated how largely the Negro is ruled by his religious feelings. His belief in the spiritual world is no ' accident ', to be thought of occasionally and then dismissed from the mind, but forms the essence of his being ; he has, for example, to take into consideration the dead as well as the living members of his family. This information can be gained if sought with sympathy and tact and if the few more or less technical words are learned and defined.

It is probably to the new situation created by the taxation troubles, which came to the knowledge of the Secretary of State, that we owe the recent change in the attitude of the Nigerian Government indicated by the publication of three substantial volumes of ethnographical investigations ; it is to be hoped that government efforts in this direction will not cease here, even in the present straitened financial circumstances. The stress laid by Lord Passfield on the importance of anthropology has at last made clear the necessity for the study of this science. As a result, officers are now being encouraged to inquire into the indigenous social organisation before any further steps are undertaken in the way of native administration schemes.

Here, however, another danger has arisen which should be guarded against, since the inquiries have generally been made by junior officers of little experience and training. This gives rise to mistakes, and it is essential that no further changes in administration should be introduced unless all such investigations are carefully checked by men with full knowledge of the country and people—particularly as regards the native courts. The native courts have been the means of providing a very fine system of justice, despite abuses in the way of bribes (partly a relic of the old custom of both sides giving presents to the judges) and pressure by court officials, especially of late years, during which they have not been so closely supervised.

The courts are composed of the chiefs of the various villages and are only an extension of the old indigenous gerontocracies. Fortunately, native law and custom were on the whole fair and reasonable and are still in force except in such matters as witchcraft, where they conflict with European ideas. There are no legal quibbles, and there is no doubt that the common people obtain a greater degree of justice than in those courts which are under indirect rule and the influence of powerful emirs and kings. Bribery was largely checked and a general improvement shown in one province where two reforms were instituted: first, every member is allowed to sit and give judgment at each session, instead of only three or four chiefs in rotation; and secondly, the members take an oath on the strongest local juju to give a just decision—though this would only have effect in areas where the belief in jujus is still strong.

The people themselves would, on the whole, prefer to have their cases tried by the administrative officers—especially in those parts where bribery is much practised—but this is rendered impossible by the immense number of cases, even if it were not desirable that they should gradually develop their institutions in the direction of self-government. A large amount of executive power is also entrusted to the native courts, and, as a matter of fact, the greater part of the administration of the southern provinces is carried on by their means.

In the present state of affairs, the absolute necessity of paying more attention to the study of anthropology cannot be too strongly emphasised. The tropical colonies of the British Empire are vital to its existence, and there is grave danger that these will pass out of its control unless science is brought to bear in their administration.

London and the British Association.

(1) *London and the Advancement of Science.* By Various Authors. Issued by the British Association for the Advancement of Science on the Occasion of its Centenary Meeting in London, 1931. Pp. iii + 321. 3*s*. 6*d*.

(2) *The British Association for the Advancement of Science: a Retrospect, 1831–1931.* By O. J. R. Howarth. Centenary (second) edition. Pp. vii + 330 + 19 plates. 3*s*. 6*d*.

(London: British Association, 1931.)

THE object of the British Association is, and always has been, the advancement of science. By means of its annual meetings at home and in the Dominions, by financial help towards scientific research, and by the printed reports of its yearly assemblies, it has acted as a leaven in the intellectual life of our nation. Now, in its centenary year, it has published two handbooks giving further justification for its honourable and useful existence. The pair are really companion books. The latter work traces the birth, growth, and struggle for recognition in a conservative age—and the subsequent triumph—of the British Association, which has roamed from county to county, and from continent to continent, dispensing its good offices in the promotion of science. This official "Retrospect" has been compiled by Mr. O. J. R. Howarth, the secretary of the Association, and the task of collecting the material and welding it into a coherent, readable mass of information has been well performed.

From this record of the nomadic life of the Association, the first edition of which was reviewed in Nature of Sept. 2, 1922, we turn, perhaps in undue haste, to the story of "London and the Advancement of Science", written by various authors, under the general editorship of Mr. Howarth.

The book originated in a suggestion made by Dr. Allan Ferguson, which was given effect by the Council of the British Association. It contains ten chapters dealing with the learned societies, the education of London, the development of medicine, government and scientific research, and such familiar features of the metropolis as the museums, Kew Gardens, and the Royal Observatory at Greenwich. A brief history of the scientific instrument makers of London concludes the book.

Dr. Ferguson's introductory survey of the outstanding names of London men of science who may be considered the most worthy of mention in such an extended scroll, provides a foretaste of good things to come. He relates the fact, not generally known, that Chaucer was an accomplished astronomer and the author of a treatise on the astrolabe, for a long time a standard work of reference. The other names selected are those of Henry Billingsley, the first translator of Euclid into English, a Lord Mayor who lived in Candlewick Ward—a modified designation of his residential quarter persisting in the name of Cannon Street; William Gilbert, the author of "De Magnete", of whose scientific spirit and achievements Galileo had the greatest esteem; William Harvey the physician, Francis Bacon, Christopher Wren, Henry Cavendish, and William Hyde Wollaston. One other figure is among those narrated in great detail, namely, John Hill, "botanist, apothecary, physician, hackwriter, actor, and playwright". A fairly good case is made out for this man's inclusion in such distinguished company, for, in spite of his mental vagaries and diffuse occupations, "his judgments on specific points were well in advance of his age". An idea of his contemporary renown may be gathered from the fact that when George III. and Dr. Samuel Johnson met in the Queen's library, His Majesty closely questioned the famous lexicographer on what he thought of Dr. Hill. Johnson answered that "he was an ingenious man, but had no veracity". Hill asserted that he had seen objects magnified to a much greater degree by using three or four microscopes at a time than by using one. "Now," added Dr. Johnson, "anyone acquainted with microscopes knows that the more of them he looks through the less of the object appears." "Why," replied the king, "this is not only telling an untruth, but telling it clumsily."

The next two chapters describe the learned societies of London. As a tribute to its eclecticism, a whole chapter is given to the Royal Society. Then follow brief but interesting accounts of the Royal Society of Arts, the Royal Institution, and of nearly sixty other societies severally connected with all the ramifications into which systematised knowledge leads men of science, covering in their entirety close on one hundred pages. The undergraduate might read these chapters with profit, before deciding on a future career, and a thesis might be evolved from them, correlating our economic and contemplative life with the growth of these numerous scientific societies. By the way, no mention is made in the book of the London Institution, at one time the rival of the Royal Institution.

The chapter devoted to government and scientific research describes the various departments that watch continually over the private and public needs of our nation, safeguarding and promoting our welfare in times of peace and defending us in the hour of war. No longer is it left to a Bishop Watson to improve the manufacture of gunpowder, or to undertake private afforestation to demonstrate the latent resources of a vacant countryside. Before Parliament would pay a Mrs. Stephens £5000 for divulging her remedy for stone, a Government analyst would detect the soap and snail shells contained in the nostrum. The problem of the resistance of water to moving vessels is nowadays investigated in the experimental tanks of the National Physical Laboratory and not, as it once was, in the vats of Capt. Beaufoy's brewery.

London's heritage of science is abundantly witnessed in her magnificent museums. They show no signs of diminution either numerically or in popularity. They are not curiosity shops on a large scale, but storehouses of information for the studious and pleasure haunts for the general public. Dr. Bather's record contains descriptions of more than three score of them, as well as an interesting foreword on many which have been absorbed or dispersed. Little did men like Tradescant, Lever, and Bullock dream that museums run for profit would be replaced by free institutions worthy of taking a place among the stateliest buildings of the city.

The volume closes with a brief account of the history of the London makers of scientific instruments, by Mr. R. S. Whipple. Besides being skilled craftsmen with a European reputation for the excellence of their work, many of these men have signed the charter book of the Royal Society. Graham's mercury pendulum still beats out the seconds, Ramsden's eyepiece and his dividing engine are well known to all physicists, and the name of Dollond is a household word. Room might have been found for a few more. The most serious omission, however, is the name of Edward Nairne, who lived in Cornhill. His business premises were the rendezvous of men like Canton, Henly, Ingenhousz, and even the shy, retiring Henry Cavendish. Nairne made an electrical machine for Priestley and was among the first to freeze water with the help of an air pump, the receiver of which contained strong sulphuric acid. He was a fellow of the Royal Society and instrument maker to the Royal Exchange.

Throughout the book, the part that London has played in the advancement of science is generously and entertainingly told. Of course, no single volume could fully encompass the scientific history of the City of London. Indeed, the task of the authors must at times have been one of exclusion rather than of inclusion. Much could be said of London's coffee-houses and inns. At "Rawthmell's" the Royal Society of Arts was founded. Wollaston and Thomas Thomson converted Davy to the atomic theory at the "Crown and Anchor". Think, too, of the experiments that have been carried out in the public parks: Faraday, Woodward, Smeaton, Stanhope, and Tyndall in Hyde Park; Benjamin Franklin on Clapham Common; Hawksbee at White Conduit Fields, and so on. What of the Halls of Delight, the Adelaide Galleries, and the Pantheon, that amused our grandparents with the latest inventions and scientific discoveries?

These and other miscellaneous features that come to mind might form another chapter, but doubtless the task which the Council set out to achieve has been satisfactorily accomplished, and the extensive literature on our wonderful city is the richer for this comprehensive volume. A few errors of orthography have been noted. In "The British Association for the Advancement of Science", page 13, Brewster's address should be Allerly and not Allerby. In "London and the Advancement of Science", page 110, Allan should be Allen; page 168, Dionysius is misspelt Dionysus; and page 259, Carlile should be Carlisle. These mistakes are, of course, repeated in the Index, where other errata occur; thus Sisson's Christian name is given as Johnathan; J. S. Smith should be J. E. Smith; J. Clark Maxwell is obviously incorrect, so is Humphrey Davy, and Leeuwerhoek should be Leeuwenhoek.

Insects and Climate.

The Transactions of the Entomological Society of London. Vol. 79, Part 1, April 24. Pp. 247. (London: Entomological Society of London, 1931.) 21*s*.

SOME two years since, Mr. B. P. Uvarov, well known as a leading authority on insect nutrition and metabolism, and more particularly for his work on the periodicity of locust invasions, undertook at the instance of the Dietetics Committee of the Committee of Civil Research, and under the auspices of the Empire Marketing Board, a survey of insects in relation to climate. This work was carried out by Mr. Uvarov with extraordinary diligence, and the result of his labours is embodied in an important contribution which forms a separate part of the *Transactions of the Entomological Society of London* for the year 1931.

The treatise is divided into two principal parts; in the first of which the physical factors of insect life, namely, heat, humidity, light, atmospheric pressure, and electricity, are considered with special reference to the part they play in insect physiology; while the second part is concerned with the practical bearing of the foregoing data on such insect activities as movements, feeding, reproduction, abundance, and distribution; the whole with an eye to problems of economic entomology in the widest sense. It will be seen that the scope of the work is extensive, and the labour of consulting and summarising the literature of the subject must have been great. The author states that out of 1300 books and papers examined, more than 1150, written in eleven languages, furnished material for his summary.

Throughout the work, Mr. Uvarov is careful to point out the necessity of obtaining, where possible, quantitative data both by experiment and observation; thus he shows that in spite of a well-grounded opinion that the swarming of certain insects is influenced by conditions of atmospheric pressure, no definite conclusion on the matter can be reached in the comparative absence of exact statistical data. The same applies to the alleged influence of the electric state of the atmosphere.

An interesting section is devoted to the consideration of seasonal cycles. Pictet's experiments, showing that in six generations a substantial change in the life-cycle of *Lasiocampa quercus* could be produced in adjustment to new conditions, suggest to the author the direct inheritance of acquired response to climatic influence. It may be remembered that a similar suggestion was made many years ago by Weismann in the case of *Heodes phlœas* and its modification *eleus*. But, as Mr. Uvarov reasonably allows, the subject awaits further experimental investigation. The experiments of Süffert with the well-known butterfly *Araschnia prorsa-levana* are spoken of in the same section as giving an entirely new aspect to the problem of seasonal dimorphism, but it may be questioned whether they have added anything material to what had already been established by Merrifield. The latter investigator, it is true, made no use of the term 'diapause' proposed by Henneguy and adopted by Süffert in the sense of a period of dormancy, but the fact itself, with its implications, appears and is recognised in the work of the older authority.

Attention is also directed to the careful experiments of Sir G. A. Marshall with tropical African butterflies. In these, definite results were obtained by the employment of varying conditions of temperature and humidity separately and in combination. The influence of each of these as a factor was clearly established, but it is not easy to see how the results harmonise, as is alleged by the author, with Süffert's "idea that the production of seasonal forms is connected with the type of development, *i.e.* with the presence or absence of a diapause". This dictum seems to call for expansion.

In the concluding section of the work, the data so assiduously collected are applied, under the comprehensive head of climatic influences, to the practical study of the problems of economic entomology. Various means of control of insect-caused injuries of plants and of insect-borne diseases of man are brought under review, and the importance of exact knowledge of climatal conditions for the destruction or checking of insect pests is duly emphasised. The whole leads up to a discussion of the possibility of forecasting the course of events in the life of insects, especially as to " the seasonal appearance of pests, the years of outbreaks, and the ability of an introduced pest to survive in a new country ". This calls for an intimate knowledge of many subjects as to which, as the author says, our information is limited and fragmentary.

Nothing is more noteworthy in this admirable treatise than the discrimination with which, while the existence in some cases of relevant and well-attested data is approved, their absence or insufficiency in others is admitted. We are frequently reminded, by the author's method, of Bacon's unsparing analysis of existing knowledge in his " Advancement of Learning ". But it is further to be remarked that although, in the course of investigation of causes, stress is constantly laid on the importance of quantitative estimates, the author is on his guard against " a blind faith in statistical methods ", which if used without due caution involve mainly a mechanical process instead of a course of reasoning prompted by the valuation on biological lines of the appropriate and relevant data. F. A. D.

Short Reviews.

International Research Council : International Astronomical Union (Union Astronomique Internationale). Report of Commission 3 : *Atlas céleste.* Par E. Delporte. Pp. 6 + 26 cartes. (Cambridge : At the University Press, 1930.) 5*s.* net.

The confusion caused by the rather indefinite and entirely irrational delimitation of boundaries between constellations has now been successfully attacked by the International Astronomical Union's commission on notation. No reform could be reasonably expected to ignore entirely the old traditional boundaries, or to transfer important stars into adjacent constellations, and this point has been fully recognised by the commission. The suggested scheme defines boundaries everywhere as portions either of hour circles or parallels of declination, but in such a way as to follow approximately the old boundaries. Nearly all stars with Bayer or Flamsteed designations, and all variables discovered before July 1929, remain in the constellation to which they were previously assigned.

A twofold importance attaches to this work, in that the maximum amount of rationalisation is introduced and also that the boundaries are finally standardised, since the approval of the scheme by the General Assembly of the Union makes it internationally authoritative. Delporte's excellent atlas covers the whole sky in 26 maps, showing stars down to the sixth magnitude with the new constellation boundaries. The printing is good, and the scale resembles that of Norton's atlas, although more stars are depicted than in Norton's or other similar atlases. Opposite each map is a table of all stars down to magnitude 4·5, as well as variables, double stars, and important nebulæ or clusters ; giving positions for 1875 and 1925, magnitudes, and spectral types. At the end are given two charts in which are inserted the exact right ascensions or declinations of the boundaries at all points. The atlas deserves to rank high amongst others of its size, quite apart from its indispensability for purposes of reference.

Health and Education in the Nursery. By Victoria E. M. Bennett and Susan Isaacs. Pp. xiv + 308. (London : George Routledge and Sons, Ltd., 1931.) 6*s.* net.

The scientific study of motherhood and of babyhood has made great headway in recent years, and has made possible the production of such a book as this, a book which combines simplicity of statement with accuracy of fact. Its contents were formerly published as two separate volumes, which are here expanded and united. The old-fashioned ' mother's manual ' was usually written by a doctor—of course, a man—who restricted his advice to that which concerned the welfare of the body. It is a sign of the times that this book is written by two women, and that it deals at equal length with the bodily and the mental aspects of healthy growth and development.

In a sense, Miss Bennett's task was the easier, because the facts and principles of physiology and hygiene belong to the realm of positive science. Mrs. Isaacs, on the other hand, sometimes has to handle matters of opinion, as, for example, the time and manner of sex instruction for children, if it is given at all. But readers of Mrs. Isaacs' previous publications on similar subjects will be prepared to find that she is always suggestive and never dogmatic.

The Concentric Method in the Diagnosis of Psychoneurotics. By Prof. M. Laignel-Lavastine. (International Library of Psychology, Philosophy and Scientific Method.) Pp. x + 217. (London : Kegan Paul and Co., Ltd. ; New York : Harcourt, Brace and Co., 1931.) 10*s.* 6*d.* net.

In the book before us, Laignel-Lavastine presents his methods of examination and treatment of the psycho-neurotic in a series of lectures which were originally delivered at La Pitié Hospital, Paris. His method consists of emphasising the manifold aspects of any case of mental disorder. It is not a case of whether this is a psychogenic or a physiogenic disorder, but how much of this disorder is psychogenic and how much is physiogenic. He describes four concentric zones with a " morbific kernel " at the centre. The zones from within, outwards, are the visceral, the endocrine, the nervous, and the psychic. In every case it is essential to investigate these zones and evaluate their respective values in etiology. His attitude towards psychotherapy is essentially that of the French school headed by Janet. For the teaching of Freud he has no great affection. To those who have confession as part of their religious creed there is no great need for Freud—so says Lavastine.

Maxwell and Modern Theoretical Physics.*

By Prof. Niels Bohr, For.Mem.R.S.

I FEEL greatly honoured in being given this opportunity of paying a tribute of reverence to the memory of James Clerk Maxwell, the creator of the electromagnetic theory, which is of such fundamental importance to the work of every physicist. In this celebration we have heard the Master of Trinity and Sir Joseph Larmor speak, with the greatest authority and charm, of Maxwell's wonderful discoveries and personality, and of the unbroken tradition upheld here in Cambridge connecting his life and his work with our time. Although I have had the great privilege, in the years of my early studies, of coming under the spell of Cambridge and the inspiration of the great English physicists, I fear that it may not be possible for me to add anything of sufficient interest in this respect, but it gives me very great pleasure indeed to be invited to say a few words about the relation between Maxwell's work and the subsequent development of atomic physics.

I shall not speak of Maxwell's fundamental contributions to the development of statistical mechanics and of the kinetic theory of gases, which Prof. Planck has already discussed, especially as regards Maxwell's fruitful co-operation with Boltzmann. It is only my intention to make a few remarks about the application of the electromagnetic theory to the problem of atomic constitution, where Maxwell's theory, besides being extremely fruitful in the interpretation of the phenomena, has yielded the utmost any theory can do, namely, to be instrumental in suggesting and guiding new developments beyond its original scope.

I must, of course, be very brief in commenting upon the application of Maxwell's ideas to atomic theory, which in itself constitutes a whole chapter of physics. I shall just recall how successfully the idea of the atomic nature of electricity was incorporated into Maxwell's theory by Lorentz and Larmor, and especially how it furnished an explanation of the dispersion phenomena, including the remarkable features of the Zeeman effect. I would also like to allude to the important contribution to the electron theory of magnetism made by Prof. Langevin, whom we much regret not to be able to hear to-day. But above all, I think in this connexion of the inspiration given by Maxwell's ideas to Sir Joseph Thomson in his pioneer work on the electronic constitution of matter, from his early introduction of the fundamental idea of the electromagnetic mass of the electron, to his famous method, valid to this day, of counting the electrons in the atom by means of the scattering of Röntgen rays.

The developments of the atomic theory brought us soon, as everybody knows, beyond the limit of direct and consistent application of Maxwell's theory. I wish to emphasise, however, that it was just the possibility of analysing the radiation phenomena provided by the electromagnetic theory of light which led to the recognition of an essentially new feature of the laws of Nature. Planck's fundamental discovery of the quantum of action has necessitated, indeed, a radical revision of all our concepts in natural philosophy. Still, in this situation, Maxwell's theory continued to provide indispensable guidance. Thus the relation between energy and momentum of radiation, which follows from the electromagnetic theory, has found application even in the explanation of the Compton effect, for which Einstein's idea of the photon has been so appropriate a means of accounting for the marked departure from the classical ideas. The use of Maxwell's theory as a guide did not fail either in the later stage of atomic theory. Although Lord Rutherford's fundamental discovery of the atomic nucleus, which brought our picture of the atom to such wonderful completion, showed most strikingly the limitation of ordinary mechanics and electrodynamics, the only way to progress in this field has been to maintain as close contact as possible with the classical ideas of Newton and Maxwell.

At first sight it might perhaps look as if some essential modification of Maxwell's theory was needed here, and it has even been suggested that new terms should be added to his famous equations for electromagnetic fields in free space. But Maxwell's theory has proved far too consistent, far too beautiful, to admit of a modification of this kind. There could only be a question, indeed, of a generalisation of the whole theory, or rather of a translation of it into a new physical language, suited to take into account the essential indivisibility of the elementary processes in such a way that every feature of Maxwell's theory finds a corresponding feature in the new formalism. In the last few years, this aim has actually been attained to a large extent by the wonderful development of the new quantum mechanics or quantum electrodynamics, connected with the names of de Broglie, Heisenberg, Schrödinger, and Dirac.

When one hears physicists talk nowadays about 'electron waves' and 'photons', it might perhaps appear that we have completely left the ground on which Newton and Maxwell built; but we all agree, I think, that such concepts, however fruitful, can never be more than a convenient means of stating characteristic consequences of the quantum theory which cannot be visualised in the ordinary sense. It must not be forgotten that only the classical ideas of material particles and electromagnetic waves have a field of unambiguous application, whereas the concepts of photons and electron waves have not. Their applicability is essentially limited to cases in which, on account of the existence of the quantum of action, it is not possible to consider the phenomena observed as independent of the apparatus utilised for their observation. I would like to mention, as an example, the most conspicuous application of Maxwell's ideas, namely,

* Address delivered on the occasion of the Maxwell Centenary Celebrations at Cambridge on Oct. 1.

the electromagnetic waves in wireless transmission. It is a purely formal matter to say that these waves consist of photons, since the conditions under which we control the emission and the reception of the radio waves preclude the possibility of determining the number of photons they should contain. In such a case we may say that all trace of the photon idea, which is essentially one of enumeration of elementary processes, has completely disappeared.

For the sake of illustration, let us imagine for a moment that the recent experimental discoveries of electron diffraction and photonic effects, which fall in so well with the quantum mechanical symbolism, were made before the work of Faraday and Maxwell. Of course, such a situation is unthinkable, since the interpretation of the experiments in question is essentially based on the concepts created by this work. But let us, nevertheless, take such a fanciful view and ask ourselves what the state of science would then be. I think it is not too much to say that we should be farther away from a consistent view of the properties of matter and light than Newton and Huygens were. We must, in fact, realise that the unambiguous interpretation of any measurement must be essentially framed in terms of the classical physical theories, and we may say that in this sense the language of Newton and Maxwell will remain the language of physicists for all time.

I do not think that this is a proper occasion to enter into further details regarding these problems, and to bring new views under discussion. In conclusion, however, I am glad to give expression to the great expectation with which the whole scientific world follows the exploration of an entirely new field of experimental physics, namely, the internal constitution of the nucleus, which is now carried on in Maxwell's laboratory, under the great leadership of the present Cavendish professor. In the fact that nobody here in Cambridge is likely to forget Newton's and Maxwell's work, we see perhaps the very best auguries for the continued success of these endeavours. Even if we must be prepared for a still further renunciation of ordinary visualisation, the basic concepts of physics which we owe to the great masters will certainly prove indispensable in this new field as well.

The Temples of Yucatan: Work of the Scientific Restorer.

ALL the great eastward-jutting promontory of Yucatan and Quintana Roo is covered with the ruins of stone temples, erected by architects of great skill, and often beautifully decorated with carvings and wall paintings. Lying between the Usumacinta River and the Caribbean, the region is a low tableland of limestone, scarcely raised from the sea, its general flatness relieved only by a few hills two or three hundred feet high ; no rivers run in all the promontory, the abundant rain falling between May and December soaking through the thin layer of soil—frequently no more than a few inches deep—to the natural limestone hollows that serve Yucatan as wells. Sub-tropical, genial, extremely productive where any soil exists, the country's wild covering is light woodland, with peculiar and beautiful vegetation, sheltering hosts of birds and beasts.

This is the background of one of the great puzzles of the history of mankind. For while the evidence is rich, and almost endless, of the high accomplishments of a brilliantly artistic people, the early origins of these people, the reason of their coming to Yucatan, and the causes for the abandonment of site after site, are still mysterious. The most plausible suggestions are no more than guesses, and none is even reasonably adequate to explain the undoubted fact that the builders of many a splendid complex of pyramids and temples appeared in the region, set to work to cut—with stone tools, for these superb architects and masons did not know the use of metal—huge masses of stone ; to transport these masses, by man-labour, since they never had the help of the wheel ; to set up and carve and paint great buildings ; to house and worship their gods, and to practise their astronomical skill ; and that after sometimes no more than a hundred years or so, priests and builders marched away, leaving the stately buildings to become the haunt of wild creatures, to be overgrown with trees and forgotten.

According to the calculations of modern archæologists, the erection by the Maya of stone temples (probably preceded by structures of less enduring materials, such as wood and thatch) began before the Christian era ; and certain temples in Yucatan were still in occupation, as great religious and civic centres, in the early part of the sixteenth century, when the first Spaniards arrived. From the time of the Spanish conquest, the Maya and their culture suffered eclipse. A sponge was passed over the long record of their scientific and artistic attainments. With a few exceptions, to which students of American archæology owe a debt, the Spanish conquerors felt no curiosity concerning the ideas and civilisation of the vanquished people of Yucatan. They were heathen, their gods were idols ; and the great pyramids merely served, when their uplifted temples were despoiled, as quarries for building material and sites for Christian churches.

The clan of learned men, priest-kings, adepts in astronomy, disappeared. They were not only the political leaders, but also the repositories of historical knowledge, experts in the art of recording. The Maya, alone among native American races, possessed a writing method, developed into an accurate system, concerned with historical and calendrical records. These records were carved in stone, and sometimes in wood, engraved in pottery, and painted upon parchment and upon paper made from the leaf of the maguey plant. The latter, collected sedulously by the Spanish missionaries, were burnt in heaps in the marketplace of Merida; so careful was the search for these pagan documents that to-day only three Maya manuscripts are known to survive. The art of writing, and of reading the manuscripts and glyphs carved in stone, was irretrievably lost—although, by the irony of fate, archæologists of to-day have puzzled out the meaning of about one-third of the writing by the aid of notes made by a Bishop of Yucatan, and by that aid alone.

Broadly, it may be said that for three hundred years, between the time of the Spanish conquest and the overthrow of the authority of Ferdinand VII., Maya culture was not only crushed in its development, but also ignored. The great series of temples, extending from Copan in the present Republic of Honduras, through Guatemala, through British Honduras, and spreading all over the Yucatec promontory, remained lost in the silence of the sub-tropical woodland; trees grew in the temples and disintegrated the great stone stairways. Maya speech survived; the humble folk, conforming to Spanish ways near towns, nevertheless retained customs and a large proportion of racial purity, especially in outlying regions; and, underneath and side by side with the Christianity patiently taught by missionary priests of Spain, maintained the cult of the ancient Maya gods.

Independence in Spanish America meant the collapse of a fence guarded for three centuries, and travellers came from all parts of the world, and particularly from Europe, to visit the regions formerly forbidden. By the middle of the nineteenth century a few inquisitive and enthusiastic travellers had seen some of the Maya ruins of Central America and Yucatan, and their books had startled a world which up to then regarded Greece and Rome as the chief fountains of romantic antiquity. Most things archæological were measured by these or the Egyptian standard. Even to-day, when ethnology has become a recognised science, it is not always possible to induce the neophyte to consider early American culture as something indigenous, basic, springing from the soil of the New World. If he cannot read Asia into American art, he must have Atlantis as a link.

Nevertheless, John Lloyd Stephens' "Incidents of Travel in Central America and Yucatan" became in the eighteen-forties a 'best-seller'; the drawings of Catherwood, illustrating the genial pen of the author, render this a perennially delightful travel book. The names of the splendid ruins of Copán and Quiriguá, Palenque and Uxmal, became familiar to the English-speaking world. Nothing, however, was done at the time to restore the ruins, or to make scientific examinations; and the work of Maudslay, thirty years later, stands out as the first sustained effort to survey and record certain great sites.

The science of archæology, in fact, was only gradually attaining its stature. The serious worker in the field during the early years was hampered by heavy expenses as well as physical difficulties. The example of Lord Elgin in Greece and of Lord Carnarvon in Egypt was not for some time followed in the Americas by any investigator after Maudslay; but when public interest became extended to—for example—Crete, and Ur of the Chaldees, the time was ripe for enterprise, already engaged in the Old World, to be directed to the untilled fields of the New.

A great step forward was made when the Carnegie Institution of Washington obtained in 1923, from the Government of Mexico, a concession to work for ten years in the magnificent complex of ruins at Chichen Itzá in Yucatan (see Fig. 1).

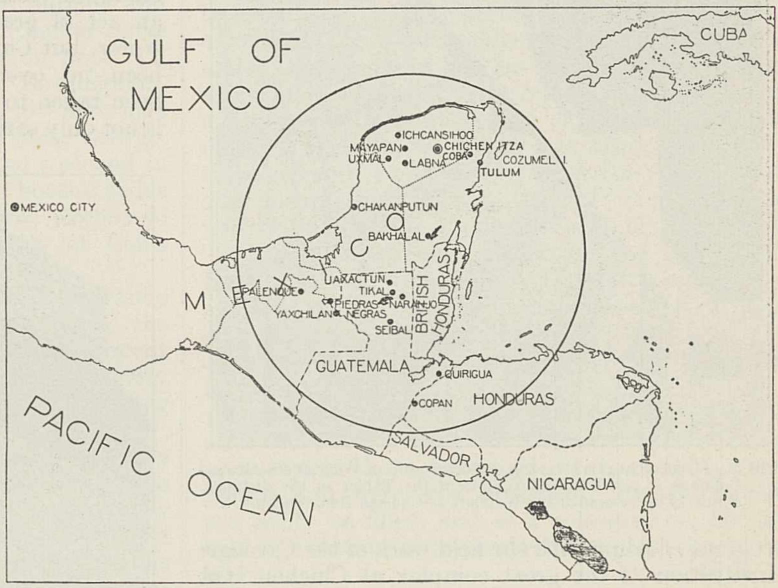


Fig. 1.—Maya region of Middle America.
From "The Temple of the Warriors at Chichen Itzá, Yucatan".

Work was commenced in 1925, after careful preparations for many branches of scientific study to be followed, as well as for the physical work of clearing and restoration of certain ruins. For the Carnegie Institution, well supplied with funds, studies biology and linguistics side by side with archæological work; scientific surveys and photography are the work of special departments, and the labour of restoring Maya architecture is performed only after careful study. Every tool, thus, is put into the hands of the staff; the Institution has organised one of the best-equipped enterprises that has ever had the good fortune to work in the fascinating field of archæology, east or west.

Not least of the Carnegie Institution's merits is the broad line upon which the department dealing with publications is operated. In all branches of the widely distributed activities of the body, care is taken to make, preserve, and issue records of progress, generously presented to other workers in similar fields, throughout the world. To this department of publications is due a recently issued book invaluable to Americanists, produced in two

FIG. 2.—Serpent head at the top of north ramp of Warrior's Stairway. Columns of sanctuary of Temple of the Tables in the distance. From "The Temple of the Warriors at Chichen Itzá, Yucatan".

volumes, dealing with the field work of the Carnegie Institution at the great complex at Chichen Itzá known as the 'Temple of the Warriors'.* The pages measure some 12 in. by 9 in., the first volume containing 485 pages of text and illustrations, while the second consists of plates, of which many are printed in colour. Three persons are responsible for this magnificent record, Earl H. Morris, M. Jean Charlot, and Ann Axtell Morris, and the colleagues are to be congratulated upon work shedding light upon an extraordinary period in the history of Yucatan. When the staff of the Chichen Itzá project began work in January 1925, the slopes of the Temple of the Warriors were densely clothed with bushes, a few axe-cut stumps showing that partial clearing had once been performed. "Presumably, this work was done by Maudslay in 1888. In thirty-seven years the forest would have had ample time to reach this stage of maturity," says Mr. Morris. Dilapidated stone carvings lay on the summit, the stairways were broken, and masses of debris were tangled with vegetation. The original design of the great complex of buildings could scarcely be perceived. To-day, as those fortunate enough to have seen Chichen Itzá lately can say, the whole series of majestic buildings, with fantastic and vivid sculptures in the round, striking reliefs, and mural decorations, stand clear from the surrounding bush. The plan of the series is complicated. From the top of the great main stairway one looks down upon acres of colonnades, while all about the white tops of other pyramids and temples rise from shrubby background under a brilliant blue sky, some still in ruin and others undergoing restoration or completed; the fine pyramid of the Castillo, for example, owes its present dignity to the work of Mexican archæologists commissioned by the Federal Government of Mexico.

Research, skill, and unlimited patience have made the restoration of the Temple of the Warriors an act of grace. One fears the hand of the restorer, but Chichen has been fortunate; there has been no over-restoration; scrupulous care has been taken to follow ancient lines; and the result is not only satisfying to the eye but also convincing.

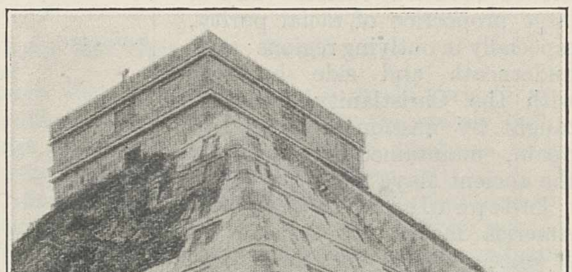


FIG. 3.—North-west corner of Castillo as repaired by Mexican Government. Panelled effect of zones of pyramid is same as finish of substructure of Temple of the Chac Mool. From "The Temple of the Warriors at Chichen Itzá, Yucatan".

The huge serpent columns, the Chac Mool figure, the atlantean altar, the mask panels of the façade, the sculptured bands that run along certain exterior walls, are beautiful examples of Maya art;

* Carnegie Institution of Washington. Publication No. 406: The Temple of the Warriors at Chichen Itzá, Yucatan. By Earl H. Morris, Jean Charlot, Ann Axtell Morris. Vol. 1. Pp. xix+485. Vol. 2. Pp. viii+170 plates. (Washington, D.C.: Carnegie Institution, 1931.) 20 dollars.

but no less interesting are the evidences of the Maya architect's methods of construction. Stairways and vaulted chambers, columns and platforms, and enormous substructures are eloquent of the skill of these vanished people.

The Chichen Itzá project of the Carnegie Institution had devoted four field seasons of some four and a half months each to work on the Warriors when the present volume was written. Work has been under the direction of Dr. Sylvanus G. Morley throughout, who not only planned but also directed operations at Chichen. During the last twenty years, he has devoted himself to the Maya problem, and valuable results have been exemplified in his publications, as well as in the standing monument of the field work in Yucatan.

Few regions equal this strange promontory of the New World in opportunity for archæological investigation. The surface has only been scratched. Individuals and institutions able to share in the unravelling of problems in Yucatan, a chapter of the history of mankind, are to be envied.

Obituary.

Prof. Percy Groom, M.B.E., F.R.S.

Through the death of Prof. Percy Groom, which occurred at Gerrard's Cross on Sept. 16, there has passed away a man with an outlook and interests somewhat exceptional in the senior ranks of British botanists. He was born on Sept. 12, 1865, and entered Mason College, Birmingham, as a student, from which he passed to Cambridge on being elected to an exhibition at Trinity College. He obtained a first class in Part I. and a second in Part II. of the Tripos in 1887, taking botany as his principal subject. Afterwards he was elected to a Frank Smart studentship in botany at Caius College.

During his student life Groom studied for a time at the University of Bonn, where he enjoyed the friendship of the botanists, headed by the eminent Prof. Strasburger, who were making that university the Mecca of so many of the English-speaking followers of the science. He owed much to A. F. W. Schimper, and a delightful appreciation of that gifted man appears as a foreword to the translation of Schimper's "Plant Geography", revised by Bayley Balfour and himself, which was published by the Clarendon Press in 1903. In 1889 he was appointed to the professorship of botany and arboriculture at the Imperial College of Whampoa, in China, and it was doubtless largely owing to this that he was led to turn his attention to the study of trees, and thence to their diseases and economic products, on which he largely concentrated his attention in after years.

On returning to England in 1892, Groom settled for some years in Oxford, migrating to Edinburgh in 1898 to take charge of the classes in plant-physiology under Prof. Bayley Balfour. His stay there was short, as he was appointed in the following year to the headship of the biological department in the Royal Indian Engineering College at Coopers Hill, in succession to Marshall Ward, and on the closing down of that College he came to London as head of the department of botany at the Northern Polytechnic. In 1908 he joined the staff at the Imperial College of Science and Technology, first as an assistant professor, and was promoted to the chair of the technology of woods and fibres in 1911, an appointment he held to the end of his life.

Groom was the author of several books, and he contributed a number of papers, mainly on the anatomy of flowering plants, to various scientific journals, but as time went on he became more and more identified with the 'applied side' of his subject, publishing many papers in various technical journals on timber and forestry topics, as well as on the various diseases which affect wood under the numerous and diverse conditions under which it is employed. He served as a member of many committees concerned with these various matters, and his assistance was often sought by those engaged in industry, as well as by various government departments. One of his last papers was written for the Public Record Office, entitled "A Preliminary Investigation on the Mildew of Book-covers in Relation to Humidity and Temperature of the Air", and this investigation was still going on when he died. His work was recognised by his election to the fellowship of the Royal Society in 1924.

Groom's lectures at the Imperial College, especially those on timber for engineers and others interested in the technical side of the subject, were valued and were well attended. His place will not easily be filled, and as a colleague he will be held in affectionate remembrance by all who knew him.

J. B. F.

Sir William Simpson, C.M.G.

By the death of Sir William Simpson on Sept. 20, after a few days' illness, a great exponent of tropical hygiene has passed away. William John Ritchie Simpson was born in Aberdeen, in 1855, where, after schooling in Jersey, he became a medical student, qualifying in 1876 and taking the M.D. degree four years later. He determined to choose public health as a career, and soon became the first medical officer of health of his native city and lecturer on hygiene in the University.

These posts were relinquished in 1886, when Simpson was appointed chief health officer in Calcutta, a position he held for twelve years, and thenceforward his life was devoted to the advancement of tropical hygiene and medicine. Returning to England, Simpson was elected, in 1898, professor of hygiene in King's College, London, and in 1899 was associated with the late Sir Patrick Manson and others in the foundation of the London School of Tropical Medicine, of which he became one of the lecturers.

From now onwards Simpson was almost continuously engaged in official inquiries into the health of various parts of the tropical dominions of the British Empire. In 1900–1 he was a member of a commission to inquire into the occurrence of dysentery and enteric fever in South Africa and was chief commissioner dealing with an outbreak of plague in Cape Colony, and in 1902 was asked to report upon the causes and continuance of plague in Hong Kong. In 1906 he studied the sanitary conditions of Singapore, in 1908 those of West Africa, of East Africa, Uganda, and Zanzibar in 1913–14, and of the mines and villages of the Gold Coast in 1924. As the outcome of extensive studies on plague, he delivered the Croonian Lectures on this subject at the Royal College of Physicians, London, in 1907.

Simpson was a prolific writer. In addition to many papers and addresses, he published a treatise on plague, a volume on the principles of hygiene in tropical and subtropical climates—the first authoritative work of its kind—and a smaller popular book on the maintenance of health in the tropics. When in India, he edited the *Indian Medical Gazette*, and afterwards was responsible for the *Journal of Tropical Medicine*. In his later years, Simpson was one of the founders of the Ross Institute for Tropical Diseases, in which he became director of tropical hygiene. Here his last work was done, and he died in its hospital.

The services of Simpson were recognised by the bestowal of numerous distinctions: the C.M.G. in 1909, the Order of St. Sava of Serbia, where he had served during the War, in 1918, and the honour of knighthood in 1923. A genial and kindly man, though a strong one when occasion required, he will be greatly missed by a large circle of fellow-workers. R. T. HEWLETT.

WE regret to announce the following deaths:—

Sir J. Hawtrey Benson, formerly president of the Royal College of Physicians of Ireland, and also medical adviser for Ireland for the Colonial Office, on Oct. 9, aged eighty-eight years.

Dr. Thomas A. Edison, world-renowned for his inventions connected with the phonograph, telegraphic and telephonic communication, and the incandescent electric lamp, on Oct. 18, aged eighty-four years.

Dr. Samuel W. Stratton, president of the Massachusetts Institute of Technology, Boston, Mass., who was Director of the U.S. Bureau of Standards from 1901 until 1923, on Oct. 19, aged seventy years.

News and Views.

IT is reported in the daily Press that Dr. J. Papish, assistant professor of chemical spectroscopy at Cornell University, has identified the element of atomic number 87 in samarskite. It is stated to occur to the extent of one part in two million. This element would appear to be the missing alkali following cæsium. The radioactive properties of the element, if any, should be interesting.

ALTHOUGH the recent agitation against the initiation of girls among the Kikuyu of Kenya has now subsided to a great extent, the causes which lay at its root are still operative. It affords an extremely instructive example of how, in dealing with native peoples, the best intentions may often prove positively harmful if not based on a thorough and sympathetic understanding of the full implication of native custom. For example, one point which was raised in the recent trouble by the missionaries was the excessive 'price' demanded in marriage by parents of native girls, which has led to a decrease in marriage and an increase in prostitution, with a related spread of venereal disease. It is, however, pointed out by Mr. L. S. B. Leakey, who discusses the whole situation in a communication in the *Journal of the Royal Anthropological Institute*, vol. 61, Jan.–June 1931, that to a great extent the missionaries have only themselves to blame, in consequence of the requirement that the parents of the girl should sign a certificate at marriage that the whole of the 'bride-price' has been paid. This departure from native custom of a payment on account at marriage with a subsequent settlement by instalments, instead of protecting the bridegroom from a parent's extortion, as it was intended, has deterred young men from marriage and has forced up the 'bride-price'. Mr. Leakey shows how misunderstanding of the character of the initiation customs and their implications, as well as mistranslation of passages in the Scriptures relating to the Virgin Mary arising out of this misunderstanding, brought about a situation which was politically dangerous and has resulted in the moral and physical deterioration of the native. Unfortunately, in present conditions, difficulties such as these are inevitable, yet they are due to causes which might easily be avoided: when the right course is pointed out by those who have the requisite knowledge, it is usually too late. Once there has been a break in tribal custom it is often impossible, as in the present case, to revert to the old law.

ON Oct. 29 occurs the centenary of the birth of the distinguished American geologist and palæontologist, Othniel Charles Marsh. Born at Lockport, New York, Marsh graduated from Yale College in 1860, and after studying geology and mineralogy at the Sheffield Scientific School, studied palæontology and anatomy in Berlin, Heidelberg, and Breslau, and travelled in various parts of Europe. Returning to America in 1866, he became professor of palæontology at Yale College and also Director of the Geological and Palæontological Department of the Natural History Museum founded by the philanthropist Peabody, who was Marsh's uncle. For many years he was connected with the United States Geological Survey; he organised many expeditions to the far west, then a somewhat inaccessible region, and in 1871 discovered the first pterodactyl remains in America, and afterwards brought to light many new genera and families, including the toothed birds and the flying and swimming reptiles. His great reputation was based on his numerous researches on the vertebrates of the western States. Both at home and abroad he received many scientific honours. He died at New Haven on March 18, 1899. His private collection of fossils was bequeathed to the Peabody Museum.

A LETTER from Faraday to William Jordan, of the *Literary Gazette*, now in the possession of Mrs. Charles A. Faraday, of Birmingham, whose late husband was a member of the Faraday family, recalls an incident in his life and explicitly refers to the knighthood which rumour has joined with his name. In the *Literary Gazette* of Aug. 12, 1837, an announcement appeared headed "Sir Michael Faraday" and expressing pleasure at the recent conferment of the honour. Faraday wrote to Jordan on Aug. 15 stating that although he is not sure whether the statement was made in jest or earnest, "it looks so like the latter that I thought I should be unkind in leaving you in error respecting my condition, which is what I trust it will always remain—plain *Mister*". He concludes by asking that no reference whatever should be made to the matter. The original letter was lent by Mrs. Charles Faraday to a member of the Rochdale Literary and Scientific Society and exhibited at a meeting of the Society, and is being reproduced as a facsimile in vol. 17 of the *Transactions* now about to be issued. Inquiries and search have been made, but so far have not succeeded in tracing any former publication of this letter.

AN exhibition to illustrate the development of glass technological research was opened by Sir Richard Gregory in the Science Museum, South Kensington, on Oct. 21. During his opening remarks, Sir Richard gave an interesting résumé of the history of glass-making. Beads of glass were discovered by Sir Flinders Petrie, in an Egyptian grave, which date so far back as 3400 B.C.; while two thousand years later various kinds of coloured glass were made in Egypt. Faraday carried out an extensive series of researches with the object of improving the manufacture of optical glass, and in his Bakerian lecture for 1829 he described glass containing boric oxide, the forerunner of the modern borosilicate glasses. Despite the antiquity of glass-making, however, systematic research is only a matter of the last sixteen years. Immediate attention to the problem of glass was clearly essential as soon as the War broke out, especially with reference to chemical glassware and optical glasses. The recognition of glass technology as a science may be dated to the founding, in 1915, of the Department of Glass Technology in the University of Sheffield. This led to the formation, in 1916, of the Society of Glass Technology, which has now become international in character. As a result of systematic research, our knowledge of the chemistry and physics of glasses has been greatly extended, resulting in a number of new types of glasses and an improvement of old ones. Mechanical development, too, has taken great strides. Such development in research is well, though not exhaustively, illustrated by the exhibition, which is divided into fourteen sections. There is, however, much of importance still to be learnt, especially about the physics and chemistry of glass. It is hoped to keep the exhibition open until the end of the year, and possibly longer.

THE Babirusa of Celebes, of which a pair has been recently acquired by the Zoological Society of London, has been always rare in captivity, and long unrepresented in the Society's collection. The beast is also interesting in itself in more ways than one; in addition to the curious upgrowth of the upper canines of the male through the skin of the face, it is remarkable for its white iris and practically hairless skin, though this nude condition is approached by the African wart-hog. The upgrowing tusks have the appearance of horns, and in fact the Malay name given above means pig-deer, while the horned boars mentioned by Calpurnius as shown in the Roman arena could not have been anything else but this species. Yet Celebes seems to have been outside the knowledge of ancient geographers, which points to the wide range of trade in the Roman imperial period, since the circus could exhibit animals from lands unknown. On the same occasion were exhibited mountain hares in their white coat, zebus, bisons, an elk, a hippopotamus, and seals "with contending bears". If bears were set on to seals in a flooded arena, these would surely have been polar bears, ice-drifted farther south than now, in a period when the reindeer ranged south as far as Germany.

OF late years attention has been directed to some very remarkable variations that have been appearing in the eggs of common fowls. Formerly only varying as a rule from white, through cream—the colour of those laid by the ancestral red jungle-fowl—to brown, they now display a great variety of spotting in dark brown shades on a creamy ground, ranging from fine speckling, like that seen on the eggs of the guinea-fowl, to the freckling of the turkey's egg, and even to heavily spotted and blotched types mimicking the eggs of rails and birds of prey. The fowls that lay such eggs are chiefly the Dutch breeds of Barnevelders and Welsummers, birds which from their appearance have much blood of birds of the Cochin type, such as is found in most of the popular breeds to-day. Even blue eggs, laid by Chilian fowls called Araucanos, have been reported recently, and Mr. C. A. Finsterbusch gives an account of these birds in the *Feathered World* for Aug. 28 (p. 263), illustrated by his own sketches. From these, and from his statements, it is clear that 'Araucanos' are not a fixed breed, but present a great variety of types, any of which apparently may produce eggs of such unusual tints as bluish, purplish, and greenish, the last sometimes spotted with brown, like the European eggs cited above.

THE South London Botanical Institute celebrated its coming of age at the annual meeting and conversazione of members on Oct. 16. Founded and endowed by the late Allan Octavian Hume in 1910, the Institute has done useful work as a centre of botanical interest in south-east London. The excellent library and extensive herbarium of British plants (which includes the herbaria of William Beeby and Frederick Townsend, in addition to Hume's own collection) are available for use by students daily from 2 until 9 P.M. Demonstrations and lectures by well-known botanists are given at fortnightly intervals, and Saturday rambles are conducted by the curator to places of botanical interest around London. A small garden and conservatory are stocked with a selection of British plants. The Institute is governed by a council of fellows, representing London botany, and elected representatives of the members, with Dr. A. B. Rendle as president. Mr. W. R. Sherrin is the residential curator and secretary. Membership is open to any person interested; there is no subscription beyond a contribution of 2*s.* 6*d.* to cover expenses of printing and postage. The address of the Institute is 323 Norwood Road, S.E., about five minutes' walk from Tulse Hill Station on the Southern Railway.

The ninth Annual Report of the Safety in Mines Research Board, for the year 1930, has just been published (London: H.M. Stationery Office. 2*s.* net), and contains a record of a very large number of researches which are being carried out under the ægis of the Board. There are no less than fourteen different subjects for research, the progress of which is discussed in the report, whilst detailed reports upon them are to be found in the appendices. It is also most gratifying to find that active progress is being made in co-operating with similar boards in other countries, Belgium, France, and the United States being specially mentioned; there is also a reference to co-operation with Germany in the matter of one research, namely, that on wire ropes, and it might well be desired that co-operation with the various German research boards should be extended. All these researches are of international rather than of national importance; they must be conducted by men exceptionally well qualified for the task, and such men are too scarce in all countries for any of their work to be wasted, as it necessarily would be if the overlapping of effort, which has marked the independent work of such boards in different countries, were to be continued. The subjects covered by this report are of the utmost importance to the miner, ranging from explosions through electrical working to explosives, safety lamps, mechanical appliances, and wire ropes, whilst the subject of falls of ground is beginning to receive the attention which its very grave influence on mining accidents unfortunately renders necessary. The report is published at a very low price, and deserves the careful study of all interested in the safety of miners.

At a meeting of the Newcomen Society held at the Science Museum on Oct. 14, three short papers were read. The first of these dealt with the work of Jolliffe and Banks, contractors, and was by Mr. H. W. Dickinson. Attention had been directed to the work of the firm by the notices of the centenary of the opening of the present London Bridge, which fell on Aug. 1 this year. London Bridge, Southwark Bridge, and Waterloo Bridge, all constructed within the period 1812–1831, were designed by John Rennie and carried out under the superintendence of him and his son, Sir John Rennie; but Jolliffe and Banks were the contractors for all three works. Little has hitherto been generally known of either partner, but Mr. Dickinson was able to show that they were the principal firm of contractors of their time, executing important harbour and drainage works in various parts of the country. These works included the construction of Sheerness dockyard. Edward Banks began life in the north of England and by the time he was thirty years of age had done much canal work, but his partner, William John Jolliffe, was at first a clergyman. Their partnership lasted about thirty years. They both died in 1835, at Tilgate Lodge, Sussex, and they are buried in adjoining parishes in Surrey, Jolliffe in Merstham Church and Banks in Chipstead Churchyard. Banks was knighted in 1822, being the first professional engineer in the country to receive that honour. There is an account of his life in the "Dictionary of National Biography", but it is meagre and unsatisfactory, and Mr. Dickinson was able to give much interesting information which has hitherto been unavailable.

In many of the schools which train the blind for various handicrafts and professions, the principles of electricity and magnetism are included in the standard curriculum. One of the difficulties in teaching the blind is to enable them to form mental pictures of the various phenomena. In the *Electrical Times* for Oct. 8, there is a description of Braille models invented by Dr. F. W. Alexander, of Teddington, which have proved a great help to teachers. Using simple materials like plywood and nails, he constructs models which show the positions of the lines of force between magnets in various positions, the current flow in the armature of a dynamo, the magnetic reactions in a simple motor, the amplitude variations of an alternating current, and similar theorems graphically by means of touch. This method of teaching the blind has much to recommend it, and the models are readily understood by those who have been born sightless. We understand that the method is novel and has not been in use before. Blind workmen are sometimes employed in making wireless sets, their affliction developing a marvellous sensitivity of touch.

On account of the present world-wide economic depression, the sixteenth International Geological Congress, to be held in the United States, has been postponed for one year—to June 1933. A revised circular has now been issued, accompanied by an excellent map showing the routes to be followed on the various excursions before, during, and after the meetings at Washington. As a result of responses to the announcements made in the first circular, the following list of topics for discussion is proposed: measurement of geological time; batholiths and related intrusives; zonal relations of metalliferous

(*Continued on p.* 723.)

Supplement to NATURE

No. 3234 OCTOBER 24, 1931

The Evolution of the Universe.

SUPERFICIALLY the most significant feature of the discussion at the British Association on the evolution of the universe, the contributions to which form our Supplement this week, was the question of the relation of the physical universe to life and mind. Criticisms of the physical arguments gathered so closely round the neglect of consciousness in the physical scheme that Sir James Jeans felt it necessary to explain at the end of the meeting that physicists were not necessarily ignorant of the existence or importance of consciousness in the universe, but, as members of Section A of the Association, they were not called upon to take it into account in considering their own problems.

Sir James's remark was timely and, in view of certain aspects of the discussion, was undoubtedly justified. At the same time, there was perhaps more cogency, implicit if not expressed, in the criticisms in question than was met by his reply. It was undoubtedly the physical universe, and the physical universe only, that was under examination, but that is a big thing and eternity is a long time. Before consciousness can be ignored in considering the indefinite future and past, it must be shown that, throughout the whole history and extent of this physical universe, consciousness can have no possible effect on it. Until that is established, such words as ' inevitable ' and ' certain ' must be left out of the discussion, and unfortunately they were not.

The universe on its first introduction to the unprejudiced inquirer is a collection of objects of various kinds—large, small, heavy, light, plain, coloured, moving, stationary, animate, inanimate, solid, liquid, gaseous, tangible, intangible, visible, invisible, and so on. There is no obvious reason why the distinction between animate and inanimate should differ in kind from that between any of the other diversities, but in the course of scientific investigation it has been found that all the others can be correlated with one another and expressed as functions of a very few fundamental concepts, while the peculiar characteristics of animate objects remain obstinately and completely outside. Two possible attitudes to this situation immediately suggest themselves: we may assume a fundamental duality between a ' physical world ' and a ' world of life ', or we may attribute the intractability of life merely to our ignorance of the full potentialities of matter.

Now, these attitudes are clearly incompatible, yet they are often unconsciously adopted by the same thinker. When, like Sir James Jeans (to whom his critics, however much they may disagree with him, must concede the great merit of embodying his ideas in definite phrases at which they can fling their darts), we speak of the evolution of the ' physical universe ' as a self-contained problem, we adopt the first; and when, like General Smuts, we speak of the ' origin of life from matter ', we adopt the second. It would be interesting to know to what extent the two assumptions were tacitly associated at the backs of the minds of the several speakers.

An attitude which, whether right or wrong, is consistent is the assumption of duality as a temporary expedient, coupled with the effort to interpret the comprehensive universe, starting independently from the two sides of matter and mind. This is what is in practice being done—at least in part. The approach from the side of matter constitutes generalised physics, and the approach from the side of mind constitutes psychology.

From the physical direction of approach it is natural to look for the emergence of life at some time in the past and to express it as a possible function of such physical concepts as time, space, mass, or their equivalents—' the origin of life from matter '. This is a laudable endeavour, although while we remain in ignorance of this mysterious function it must stop short of the final generalisation of a ' physical universe '. The psychological direction of approach, on the other hand, offers a diametrically opposite view. Space-time, mass, energy and the like give place to such concepts as will, purpose, desire; and matter becomes not the source of life but at most its tool and perhaps its creature. Mechanical action is no longer the ground out of which volition has sprung ; it is the ultimate point which volition can reach. Just as such mechanical human actions as breathing and blood-circulation are expressed as mental acts become automatic through age-long practice, so, as soon as psychology realises its cosmic significance, the orderly movements of the physical world must tend to be expressed as the functioning, grown habitual, of a universal consciousness which in its youth may have been as wayward as an irresponsible child and in its old age may exhibit the disturbing effects of 'repressions' associated with such waywardness.

We cannot escape from this possibility in its most general form. If we assert duality, we are confuted by the obvious interaction between mind and matter in living creatures. If, on the other hand, we pursue the study of matter in the hope that an interpretation of mind in physical terms will one day be reached, we cannot deny the right of the psychologist to pursue the study of mind in the hope that an interpretation of matter in mental terms will one day be reached. The 'physical universe' is a convenient fiction for the former pursuit, but we have no right to call it anything else. With appropriate reservations it may be likened to the carrot held before the donkey. It makes him run but it has nothing to do with what awaits him at the end of the road. From this point of view there is a world of meaning in Prof. de Sitter's claim, made on quite other grounds, that the physical universe is a hypothesis.

When we leave these rock-bottom considerations and contemplate the discussion in its purely physical aspect, we again see Prof. de Sitter's suggestion as one of the outstanding points. It is, indeed, so obvious as to be in grave danger of being overlooked : the closed universe has never been observed, but is assumed in order to correlate facts that are observed ; it is therefore a hypothesis. In this respect, as Prof. de Sitter says, it resembles the atom, but there appears to be an essential difference. There is no reason, so far as we know, why, if it is real, it should not be observed in the future if the rate of increase of telescopic power exceeds its rate of expansion. In that event it would cease to be a hypothesis and become a phenomenon. But the atom, by the requirements of its own nature, is inherently unobservable and can never be other than a hypothesis. If, therefore, it has properties which are impossible to phenomena, it has a legitimate defence. But if the universe, which may one day be observed, has such properties, then we can definitely say that it is a hypothesis which is wrong. Possibly "the velocity of change of the quantity . . . which . . . is interpreted as the 'radius of the universe' has nothing to do with the rate of evolutionary change of stars and stellar systems", but in that case we cannot rest content with the contradiction but must change the interpretation of that quantity, in the sure and certain hope that it has an interpretation in terms of familiar conceptions.

Even apart from this essential difference between the universe and the atom, however, the remarkable and in many ways mysterious parallelism between the details of the atomic hypothesis and direct experience must make us hesitate before concluding that the two types of time-scale in question are inherently irreconcilable. The second law of thermodynamics, the Michelson-Morley experiment, and—perhaps most striking of all—Sir Arthur Eddington's bombshell of a correspondence between the characteristics of the electron and those of the universe, afford sufficient examples of alternative interpretations which appear to be completely equivalent to make us doubt a final incompatibility, even if one were theoretically permissible.

The most profound significance of the discussion as a whole, however, lay in the fact that the various speakers not only had no common starting-point but also made no attempt to find one. Possibly this was inevitable under the conditions of the discussion, but if so, so much the worse for the conditions. We need not return to the basis of scientific thought to see this ; it is only too evident on the higher platform of purely physical ideas. It is an instructive, though exceedingly difficult, task to examine the various contributions in order to discover what was implicitly assumed ; and when we do this, however imperfectly, we find a diversity which not only makes us wonder, but also directs attention to the urgent need of defining what actually is the basis of modern physical theory.

Sir James Jeans paid homage to the second law of thermodynamics, which to Sir Oliver Lodge was an idol against which philosophers were to be warned. General Smuts accepted the law but claimed that life was the result of the odd chance which it allows — a remarkable claim, which implies that the chance of life persisting for a single moment longer is inconceivably small. The Bishop of Birmingham started from particular facts (or statements having the appearance of facts) rather than principles, but the arguments which followed seemed to spring more from independent beliefs and hopes than from the promised source. Those beliefs and hopes had no audible echo in the remarks of the other speakers, and the chief ' facts ' were treated with scant respect by Profs. de Sitter and Millikan respectively. Sir Arthur Eddington and the Abbé Lemaître built on the field equations of relativity, and the former also on the wave equation of an electron—grounds which were not openly challenged, but which were by no means universally shared. Prof. Milne, like Prof. Millikan, gave a very suggestive account of a particular process, but that process can scarcely be said to lie at the heart of the question. C'est magnifique, mais ce n'est pas l'univers. Severely local in time and space, Prof. Milne was perhaps justified in assuming the conservation of energy, but Sir James Jeans, taking a wider outlook, had previously spurned that principle. This difference might be reconciled by a slight change in the definition of the word ' energy ', but only the previous day Prof. Bohr had been telling Section A of the Association that the concept of energy seemed to be inapplicable to atomic phenomena, and that in the sun and stars the energy might come from nothing! What about the evolution of the universe in that case ?

The question that sounds insistently through all this confusion is : What is the fundamental basis of physical thought ? " Give me whereon to stand, and I will move the earth," said Archimedes. Where do we stand in order to move the law of conservation of energy ? It does not matter in the least whether Bohr's suggestion is upheld or not. The fact that it was seriously made shows that there is some foothold, beneath the law in question, which physicists were assumed to share, and that foothold is deeper than anything that was assumed at the discussion we are considering. It is idle to suppose that we can move the universe from a higher level than that necessary to move a physical theory, and the ordinary man, for whom the discussion was primarily intended, might well ask why a company of some of the world's leading thinkers, discussing the world's weightiest problem, should not at least start from common fundamental principles. As it stands at present, the chief value of the discussion lies in the incidental contributions to advanced physical thought which it elicited ; but if it leads ultimately—perhaps by way of an *ad hoc* discussion at some future meeting of the British Association—to a definite formulation of the basic principles of physics, it will perform a service far more in accordance with its original purpose. H. D.

Contributions to a British Association Discussion on the Evolution of the Universe.

By Sir James Jeans, F.R.S.

We are, of course, discussing only the physical universe. Here strict determinism reigns, because even if there is no determinism in the behaviour of individual atoms, there are so many atoms in even the tiniest bit of matter that we may take an average. The laws of probability provide something which is, for our present purpose, equivalent to a strict causal determinism.

It follows that the final state of the universe is inherent in the present state, just as this present state was inherent in the universe at its creation. The physical universe never has any choice—it must inevitably move along a single road to a predestined end. What we are calling evolution is like the rolling of a train along a single-track line, with no junctions of any kind.

The various possible lines of development for the universe are like an enormous number of single-track lines ; and as we do not know which track we are on, it is futile to discuss at what particular spot it ends. But the second law of thermodynamics makes it possible to discover in what kind of country it ends, and this is really the information we want.

Strictly speaking, this law does not deal in certainties, but in probabilities—although of a somewhat unusual kind. The number of particles in the universe is probably of the order of 10^{79}, and all the odds to which we are introduced by the second law involve very high powers of 10^{79}. We need not trouble to differentiate too closely between such long odds and certainties.

This law teaches that the universe will ' evolve ' through a succession of states of ever-increasing entropy, until it ends in the final state of maximum entropy. Beyond this it cannot go ; it must come to rest—not in the sense that every atom in it will have come to rest (for maximum entropy does not involve this), but rather in the sense that its general characteristics cannot change any more. Yet if someone asserts that this will not happen, and that the universe will move to a state of *lower* entropy than the present, we cannot prove him wrong. He is entitled to his opinion, either as a speculation or as a pious hope. All we can say is that the odds against his dream coming true involve a very high power of 10^{79}—in his disfavour.

The question of discovering the final state of the universe is merely that of discovering how far its entropy can increase without violating the physical laws which govern the motions of its smallest parts.

General considerations show that the universe still has a long way to go. It can increase its entropy by distributing its radiant energy more uniformly; at present this is still very far from being uniformly distributed. Out in the farthest depths of space the density of radiant energy corresponds to a temperature of less than one degree above absolute zero: in the interstellar spaces of the galactic system, three or four degrees only; near the earth's orbit about 280 degrees; at the sun's surface about 6000 degrees; at the sun's centre perhaps 40 or 50 million degrees. The entropy is increased by equalising these temperatures: that is why energy flows from the sun's hot centre to its cooler surface, and why it then streams out into space, past the earth's orbit into the cold and dark of interstellar and intergalactic space. There can be no end until all these regions are at the same temperature, with radiant energy diffused uniformly through space.

All radiant energy has its origin in atomic disturbances. Each atom is a sort of storehouse of energy or mass—we now believe the two to be identical—and at intervals, either spontaneously or through interference from outside, an atom may discharge some of its energy or mass into space in the form of radiation. This wanders through space like a sort of bullet of radiation travelling with the velocity of light; we call it a photon. Now the so-called 'cosmic radiation' which falls on to the earth from outer space appears, so far as we can tell, to consist of photons whose masses are comparable with those of complete atoms. Indeed, the two most massive types of photons so far detected in this radiation are found, to within errors of measurement, to have precisely the masses of the helium and hydrogen atom respectively. The simplest interpretation is that these photons originate out of the complete transformation of atoms of helium and hydrogen into radiation. But this is not the only interpretation, nor, I think, the most probable. All atoms consist of protons and electrons in equal numbers, so that their masses are (approximately) exact multiples of the mass of the hydrogen atom, which contains one of each. If an electron and a proton were to neutralise one another in any atom whatever, the atomic weight of the atom would be reduced by unity, and the atom would eject a photon of mass equal to the hydrogen atom. In the same way, if an α-particle were to neutralise itself by combining with two electrons in any atom whatever, the resulting photon would have a mass equal to the helium atom.

Although not everyone agrees, this seems to me the most plausible interpretation of the two most penetrating constituents of the cosmic radiation. I think they provide evidence that matter can be annihilated, and point to a general degradation of complex atoms in the direction of simplified structure and decreasing atomic weight.

On this view, electrons and protons must be regarded as concentrated stores of energy, which are capable of being set loose and dissipated in the form of radiation. The entropy of the universe is increased by this process, and the final state of maximum entropy is one in which every electron and proton which is capable of annihilation has been annihilated.

The question at once arises as to how many of the 10^{79} or so electrons and protons which form the universe are subject to annihilation. They may all be, in which case the final state of the universe will be one in which all matter is dissolved into radiation, and nothing remains but radiation traversing empty space. Or again, only special types of atoms may be liable to annihilation, just as only special types are liable to radioactive disintegration. In this case the final state will be one from which these atoms, like the radioactive atoms, will all have disappeared. There will be a universe of cold inert matter and of uniformly distributed radiation of very feeble intensity.

Amongst a mass of theoretical possibilities, the one certain fact is that if the atoms of our earth are undergoing annihilation, their rate of destruction must be exceedingly slow; their average life must be of the order of 10^{18} years at least, or else the earth would be markedly hotter than it is. This suggests the possibility that annihilation may only occur in types of atoms which are not found on earth—possibly atoms of higher atomic weight than terrestrial atoms.

Many considerations suggest that the stars produce their radiation by the annihilation of their substance. Perhaps the strongest, and certainly the most clear-cut, is that the stars which appear to be the youngest (such as binary stars describing close circular orbits) are statistically the most massive. It looks as though the stars lost a large part of their mass in the course of their lives, and this can only mean that a large number of their atoms, or a large part of these atoms, undergo annihilation. If so, our terrestrial atoms may well be a sort of indestructible ash, the relics of more complex atoms after all that can be annihilated has been—just as lead and helium are the relics of radioactive atoms of greater atomic weight.

There is, however, an alternative possibility. If matter is capable of annihilation, the general principles of the quantum theory show that the total amount of this annihilation must consist of two parts. The first is an annihilation which goes on steadily, regardless of external conditions of temperature and pressure, just as radioactive disintegration does. The second is an annihilation which is incited by high temperature, and whose rate increases with an increase of temperature. Calculation shows that this will only come into play when the temperature begins to approach a million million degrees.

Clearly if the cosmic radiation we receive on earth proceeds from annihilation, it must be annihilation of the first kind; for radiation which had been produced by annihilation of the second kind, and so at a very hot place, would all be absorbed by matter before it could climb down the very long temperature gradient and emerge into outer space. But it is possible that it is mainly radiation of this second kind which, after innumerable absorptions and re-emissions, or repeated softenings, appears as the ordinary light of the sun and stars. Thus many astronomers think that matter is annihilated in appreciable amount at high temperatures only, and that the annihilation at low temperatures is negligible. On their view, terrestrial atoms escape annihilation, not because they are specially indestructible, but because they are specially cool.

Yet this view is confronted by many difficulties. The cosmic radiation cannot be produced by this high temperature annihilation, and yet this is of vast amount—comparable at least with the radiation of all the stars. Also, for the heat of the stars' interiors to produce annihilation, their temperatures must approach a million million degrees. This requires what so far seems to be an impossibly high temperature-gradient inside the star. Again, such high temperatures, and such a mode of generation of energy, would, so far as we can see, make the stars unstable and, indeed, explosive structures. The best simple analogy to such a structure is a keg of gunpowder, with its centre raised to the flash-point of the powder. Nevertheless, it must be admitted that we are far from definite knowledge on these questions, which are still in the stage of very heated discussion.

A further possibility is that stellar light and heat do not result from annihilation of matter at all, but from some less fundamental change in atomic structure. If so, we are no longer compelled to postulate unduly high temperatures at the centres of the stars ; this difficulty disappears. If the atomic changes are spontaneous, like radioactive change, the difficulty as to stability also disappears as, indeed, it does whatever the origin of the energy. But these milder changes cannot provide enough energy for the long lives—millions of millions of years—through which the stars have, to all appearances, existed. The equipartition of energy in the motion of the stars in space, as well as in the internal motion of binary systems, and also the small masses of what appear to be the oldest stars, all point to extremely long stellar lives. To provide adequate total radiation, throughout these, we need complete annihilation of matter; nothing less drastic will serve. If we cannot have this, we must conclude that the universe of stars is still quite young, in spite of looking so old; its many appearances of great age must all be deceptive.

This seems far more possible than it did even a year or two ago. Indeed it derives much support from recent developments in the theory of relativity. These suggest, somewhat strongly, that the whole universe may be expanding, while recent astronomical observations, if they have been rightly interpreted, indicate that it actually is expanding, and this at so rapid a rate that it becomes a mere transitory and ephemeral structure compared with what we recently thought: the spectra of the great extra-galactic nebulæ seem to indicate that these bodies are running away from one another so fast that they cannot have been running for long. This reduces the whole life of the universe to a matter of hundreds of thousands of millions of years at most —and incidentally, in so doing, brings almost complete chaos into the already chaotic problem of stellar evolution.

Another possibility—nearly but not quite identical with the foregoing—is that the universe retains its size, while we and all material bodies shrink uniformly. The red shift we observe in the spectra of the nebulæ is then due to the fact that the atoms which emitted the light millions of years ago were larger then than the present-day atoms with which we measure the light—the shift is, of course, proportional to distance. The final end here is a universe in which all matter has shrunk to nothing.

On the other hand, if the universe is expanding, the stars are merely pouring out their radiation into a bottomless pit, since the space to be filled with radiation is for ever increasing in amount. The total energy of the universe is for ever decreasing in amount, because radiation does work in pressing out the boundaries of the universe—just as a gas loses energy and so cools, when it expands and presses back the boundaries of its ' universe '. Thus the mass of the stars is continually changing into energy, while this energy in turn changes into mere additions to the size of the universe. There is conservation neither of mass nor of energy. Nor, if the evidence of the cosmic radiation is to be trusted, is there any conservation of matter. Matter turns into energy and energy into mere bigness of space.

Suppose some infallible oracle offered to give a ' Yes ' or ' No ' answer to two scientific questions for each of us. Personally, I think I might choose as my two questions :

1. Does the main energy of stellar radiation come from the annihilation of matter ?

2. Is the universe expanding at about the rate indicated by the spectra of the nebulæ ?

By Abbé G. Lemaître, Observatory, Louvain.

I PROPOSE to give some answer to the two questions raised by Sir James Jeans, which so clearly summarise the present state of the problem of the evolution of the universe. I will begin with the second question, because I think that its solution may throw some light on the first one : " Is the universe expanding at about the rate indicated by the spectra of the nebulæ ", the atomic constants not being modified by some artificial change of gauge ? I add these words, because it is clear that any artificial expansion could be provided by arbitrarily varying the units of length, time, and mass. Expansion of the universe is in some sense relative : it is relative to the whole set of essential properties of matter being assumed to be constant.

The expansion of the universe is a matter of astronomical facts interpreted by the theory of relativity, with the help of assumptions as to the homogeneity of space, without which any theory seems to be impossible. I shall not discuss the legitimacy of this interpretation, as I do not know any definite objection made against it and this is not the place; and it is not necessary to give a new popular version of the leading principles of the theory of relativity. I shall rather try to show that the universe must be expanding, or rather that the most necessary processes of evolution are contradictory to the view that space is and has always been static.

It has been pointed out by Sir Arthur Eddington that a static universe is unstable, and he proposed the problem of finding the possible causes of its expansion. He suggested that such a cause might be the formation of condensations. I obtained recently a solution of this problem, and the main results are as follows :

When the expansion is already started, the effect of kinetic energy or pressure of radiation is quite negligible. On the contrary, pressure is the chief factor in the question of instability of a static universe. If the pressure were rigorously zero, the expansion could never appear. But, if the pressure (or kinetic energy) is not zero, any diminution of pressure must start the expansion. For example, a world full of radiation starts expanding as soon as the radiation can transform itself into matter.

When condensations exist or are formed, the problem is complicated by gravitational effects ; but it can be shown that the general expansion of the universe depends entirely on the density of kinetic energy or of pressure at the places where the gravitational influences of the condensations cancel one another. I call these places (for brevity) ' neutral zones '. Condensation in itself has no direct effect whatever on the stability of the universe : but condensations would necessarily induce a rarefaction at the neutral zone and so a diminution of the density of kinetic energy at the neutral zone ; and this must induce expansion.

We can conclude that any general process of condensation, occurring in a world where the kinetic energy does not vanish, must induce expansion. Therefore, practically, the expectation of Sir Arthur Eddington is fully confirmed. For example, formation of stars out of a primeval gas starts the expansion ; formation of extra-galactic nebulæ out of a uniform mass of gas or of stars starts the expansion. I think that these results add much weight to the fact that the actual velocity of expansion fixes a limit to the time scale of the evolution, as we must rule out of our speculations every process which would start a premature expansion of space.

Even if we had no experimental evidence of the expansion of space, considerations of stability would fix a limit to the time-scale of evolution. The reason is that, if the universe has existed for too long a time, any general process of condensation would be contradictory to the actual value of the density of matter. Although this quantity is not known with great accuracy, its value may give some idea of the maximum scale of evolution. I find that any general process of condensation, even of very moderate intensity, cannot have happened earlier than a few hundred thousand million years ago.

As stated by Sir James Jeans, this brings almost complete chaos into the already chaotic problem of stellar evolution. A complete revision of our cosmological hypothesis is necessary, the primary condition being the test of rapidity. We want a 'fireworks' theory of evolution. The last two thousand million years are slow evolution : they are ashes and smoke of bright but very rapid fireworks.

I think that the key of the problem is afforded by the discovery of the cosmic rays. Cosmic rays are of enormous energy. Their intensity is estimated to be about one-tenth of that of the light coming to us from the stars. If these rays are really cosmic, their energy is much bigger than that of the light of the stars, because it must be of equal intensity all over space. Simple computation shows that the energy of cosmic rays is comparable in amount to the whole energy of matter, being possibly one thousandth, and at least one hundred thousandth, of the total energy of matter.

If the cosmic rays originated chiefly before the actual expansion of space, their original energy was even bigger, and it has been reduced by the expansion in the ratio of the change of the radius of the universe during the time of their transmission through free space. We get photographs of nebulæ at a distance of about one hundred million light years ; light from these nebulæ travelled through space during about one hundredth of the time of expansion. It does not seem improbable that the cosmic rays have travelled around one hundred times longer and were really produced by the process of the formation of the stars. This may give the solution of the puzzle. The only energy we know which is comparable to the energy of the cosmic rays is the matter of the stars. Therefore it seems that the cosmic rays must have originated from the stars.

Now the stars are surrounded by an atmosphere, and an atmosphere would altogether prevent any escape of cosmic rays from the inside of a star. The explanation seems to be that the cosmic rays went off from the stars at a time when the stars had no atmosphere. The stars are born without atmosphere ; the atmosphere evolved after the escape of the cosmic rays.

We are thus led to the conclusion that the stars were born some ten thousand million years ago without atmospheres, and that the cosmic rays are outstanding features of the formation of a star.

How could we explain such a phenomenon as that ? Sir James Jeans has given strong reasons for admitting the existence of atoms of considerably higher atomic weight than our actual dead atoms. Cosmogony is atomic physics on a large scale—large scale of space and time—why not large scale of atomic weight ? Radioactive disintegration is a physical fact, cosmic rays are like the rays from radium. Have they not escaped from a big scale super-radioactive disintegration, the disintegration of an atomic star, the disintegration of an atom of weight comparable to the weight of a star.

The birth of a star would be an atom of weight somewhat greater than the actual weight of the star, and the star would be formed by the super-radioactive disintegration of its original atom. It is conceivable that the greater part of the products of disintegration would be kept back together by the gravitational attraction of such a massive atom, although a considerable part, say one thousandth, should be able to escape into free space at the beginning of the evolution, before the products of disintegration are numerous enough to form an atmosphere. Cosmic rays would be glimpses of the primeval fireworks of the formation of a star from an atom, coming to us after their long journey through free space.

The frequency of cosmic rays is, of course, very high ; nevertheless, it may be thought to be too low to be the by-product of such a tremendous disintegration of matter. However, it must be realised that the observed frequency of the cosmic rays is not the original frequency. The original frequency has been reduced in the ratio of the expansion of space, and was at least twenty times greater than the observed frequency.

I think that a possible test of the theory is that, if I am right, cosmic rays cannot be formed uniquely of photons, but must contain, like the radioactive rays, fast beta rays and alpha particles, and even new rays of greater masses and charges. I have shown that the momenta of such rays must be reduced by the expansion in about the same ratio as that of the photons.

The mass of a star should be determined by the weight of its original atom, and it is conceivable that stars might be born with different masses. If the mass of the original atom is too big, the star must be finally broken up by radiation pressure, and the original atom must give birth to a cluster of stars, chiefly formed by stars of maximum mass. If the star comes from an atom, both masses and luminosity are determined by the weight of the original atom. Thus this theory accounts for a mass-luminosity relation.

The actual theory does not completely bring order into the chaotic state of cosmogony imposed by the fact of the expansion of space. Explanation of the approximate equipartition of energy between the stars, or evolution with loss of mass along the Russell diagram, might be dismissed with regret. But I do not see any way to retain these processes of evolution, because they are altogether too slow.

If I had to ask a question of the infallible oracle alluded to by Sir James Jeans, I think I should choose this: "Has the universe ever been at rest, or did the expansion start from the beginning?" But, I think, I would ask the oracle not to give the answer, in order that a subsequent generation would not be deprived of the pleasure of searching for and of finding the solution.

If the total time of evolution did not exceed, say, ten times the age of the earth, it is quite possible to have a variation of the radius of the universe going on, expanding from zero to the actual value. I would picture the evolution as follows: At the origin, all the mass of the universe would exist in the form of a unique atom; the radius of the universe, although not strictly zero, being relatively very small. The whole universe would be produced by the disintegration of this primeval atom. It can be shown that the radius of space must increase. Some fragments retain their products of disintegration and form clusters of stars or individual stars of any mass. When the stars are formed, the process of formation of the extra-galactic nebulæ out of a gaseous material, proposed by Sir James Jeans, could be retained for the star-gas filling the space. The numerical test works out equally well for this case.

Whether this is wild imagination or physical hypothesis cannot be said at present, but we may hope that the question will not wait too long to be solved.

We want two things. First, a theory of nuclear structure sufficient to be applied to atoms of extreme weights. For these atoms, the problem cannot be separated into a nuclear problem and a problem of surrounding electrons; because it is easily seen that the *K* ring would merge into the nucleus. We must wait, but we may trust the physicists that we do not have to wait too long. The second thing we want is a better knowledge of the nature of the cosmic rays. The correlation of the theory of nuclear structure with further observations on the cosmic rays must answer yes or no to our question; and we shall prefer this answer, however incomplete it may be, to any answer of any infallible oracle.

By Prof. W. de Sitter, Observatory, Leyden.

I HAVE been asked to make a short contribution to this discussion about the evolution of the universe. I hope you will find it short, although its length will, I fear, exceed the diameter of the earth's orbit round the sun. This way of expressing a time in a unit of length is perhaps somewhat unusual. To have a distance expressed in a unit of time is, of course, very familiar. A light-year has become an accepted unit of distance.

I have purposely used the inverse method to direct attention to the fact that the corresponding units of time and of space are so very widely different in relation to actual phenomena. The diameter of the earth's orbit enormously exceeds all lengths that we ever come across in our daily life, whilst a quarter of an hour is considered a short interval. Similarly, a thousand million light-years is a long distance, in fact many times greater than any actual distance of which we have certain cognisance, but a thousand million years is a short time in the evolution of the universe. It is only a third or a quarter of the accepted age of the earth, and I do not think geophysicists will be ready to take off even one single zero. We believe, and so far as I can see on good grounds, that the ages of the sun and the majority of the stars are at least a thousand times longer. The time needed for the development of a double star, or even a quadruple or sextuple system, and that required for the evolution of a stellar system or spiral nebula are at least of the same order.

In the farthest galaxies that we can observe things appear to be very much the same as in our immediate neighbourhood. There seems to be no indication that these far-away systems are in an appreciably earlier state of evolution than our own.

Although, of course, the trustworthiness of this statement decreases very rapidly with the distance, still it seems to show that the lapse of time corresponding to the distance of these systems is only an entirely inappreciable fraction of their whole lifetime. All these considerations consistently lead to the conclusion that the time elapsed since the beginning of the evolution of the universe is to be measured in billions of years, or perhaps in thousands of billions. I do not propose to discuss the question whether there ever was a beginning. It suffices for my argument to define the ' beginning ' as that state of the universe and its constituent parts which we are with our present knowledge and theories content to use as a starting point, beyond which we do not wish, or are not able, to extend our investigations.

It is in accordance with this slowness of the evolution of the universe—or I should rather say of the constituent parts of the universe, its galaxies and stars—that the universe was, until very recently, generally assumed to be practically in a state of equilibrium. Accordingly a theory of the universe, in order to find popular favour, had to be static. Some may have felt inclined to disagree with this generally adopted attitude, but if they ventured to confess their dissenting views at all, they did so rather diffidently. A static solution of the fundamental equations of the theory of relativity was required. The choice was between two possible statical solutions, which I have been in the habit of calling the solutions *A* and *B*, but which may be better distinguished by the names of the ' static ' and the ' empty ' universe. Einstein's solution, the ' static ' universe, was a genuine statical solution. The other one was essentially non-static, but, as a consequence of its emptiness, it could parade in the garb of a static one. Both of these, however, failed to represent the observed facts in the actual universe, which is neither empty nor static, since it is full of galactic systems, which are all moving with large and systematic velocities. The way out of this dilemma has been shown by the Abbé Lemaître, whose brilliant solution, the ' expanding universe ', was discovered by the scientific world about a year and a half ago, three years after it had been published.

It is not necessary to explain this theory in detail here. It has become widely known by technical and popular expositions in many periodicals. The principal point of it is that it shows the observed radial motions of the spiral nebulæ to be in accordance with an isotropic, homogeneous, but non-static solution of the field equations of the general theory of relativity, or, we can say, to be a pure effect of the inertia of these bodies. The static solution, in which inertia leaves the relative distances of different material bodies (statistically) unchanged, is shown to be unstable. The actual universe must therefore be represented by one of the non-static solutions. In these the change at a certain moment may be either an expansion or a contraction. Observation shows that at the present moment we are on an expanding branch. The solutions are arranged in families. In some, the universe contracts to a finite minimum radius and then starts to expand again, in others this minimum is zero ; in other solutions again the radius oscillates between zero and a finite maximum value in a rather short period. These oscillating solutions, however, require a density exceeding what is indicated by our knowledge of the distribution of matter in our neighbourhood.

I must lay stress on the fact that in using the words ' universe ', ' radius ', ' expansion ', etc., we are really speaking metaphorically, putting an interpretation on the equations which is by no means imperative. There occurs in the equations a certain small quantity, which may be either positive, negative, or zero, and which is interpreted as the reciprocal of the square of the radius of curvature. But both this interpretation, and the assumption tacitly made that it is positive, are entirely gratuitous, and not demanded by the theory. I will, however, continue to use this convenient metaphorical speech.

Lemaître's theory not only gives a complete solution of the difficulties it was intended to solve, a solution of such simplicity as to make it appear self-evident, once it is known (like Columbus's famous solution of the problem of how to stand an egg on its small end)—it also incidentally contains the answer to some questions of long standing, such as what becomes of the energy which is continually poured out into space by the stars : it is in fact used up by the work done in the adiabatic expansion of the universe. There can be not the slightest doubt that Lemaître's theory is essentially true, and must be accepted as a very real and important step towards a better understanding of Nature.

Now if we adopt this theory of the expanding universe, it is very tempting to seek a connexion between this expansion and the evolution of the material bodies constituting the universe, and to identify the beginning of the expansion with the beginning of that evolution. But the time elapsed since the beginning of the expansion is only a few thousand million years—an interval that we have learned to consider as very short from the evolutionary point of view. There is no escape from this. We can, in fact, make the interval logarithmically infinite, but that is only a mathematical trick: we call zero minus infinity, but this does not make the interval during which anything really happens any longer. It is a consequence of Lemaître's equations that the time taken by the universe to increase its radius from anywhere near its minimum to its present value is of the order of magnitude of the radius itself, if we adopt corresponding units of space and time, for example, years for time and light-years for space.

The real origin of the difficulty is that the ratio of the natural units of space and time is determined by the velocity of light, whilst the velocities which determine the rate of progress of the evolutionary process, say in the case of a stellar system, are those of material bodies (stars), which are of the order of one ten-thousandth of that of light. Consequently, if we wish to construct a causal connexion between the commencement of the expansion and events which are supposed to have happened at a very early stage of the evolution of the stellar systems—such as the first formation of condensations, or the imprisoning of free energy inside matter called 'stagnation' by Lemaître—we unavoidably meet with the difficulty that the time elapsed since these two beginnings is some thousands of times longer in one chain of events than in the other. I do not think it will ever be found possible to reconcile the two time scales.

We thus come, however reluctantly, to the conclusion that the expansion of the universe on one hand, and the evolution of stellar systems and stars on the other hand, are two different processes, taking place side by side, but without any apparent connexion between them. The expansion has only been going on during an interval of time which is as nothing compared with the duration of the evolution. Leaving the oscillating universes, and those that start from a zero radius, out of account, the universe may have been practically stationary at or very near its minimum size for an infinite time before starting to expand, or it may have contracted during an infinite time and, after passing through a minimum a few thousand million years ago, started to expand again. In both cases there appears to be no causal connexion between the change of size of the universe as a whole and the evolution of the systems which it contains.

It should not be forgotten that all this talk about the universe involves a tremendous extrapolation, which is a very dangerous operation. I have sometimes called the part of the universe of which we know anything with certainty 'our neighbourhood'. The limits of this neighbourhood have been enormously extended in the last ten or fifteen years by the results derived from observations with the large telescopes at Mount Wilson and elsewhere, but still it is presumably only a very small portion of the whole of the universe. All assertions regarding those portions of the universe which lie beyond our neighbourhood either in space or in time are pure extrapolations. In making a theory of the universe we must, however, adopt some extrapolation, and we can choose it so as to suit our philosophical taste. But we have no right to expect it to be confirmed by future observations extending to parts now outside our reach.

The extrapolation that is at the base of the theory of the expanding universe is that our neighbourhood is just an ordinary point, or small area, in the universe, not differing from any other small area in any essential property. This is, of course, a very natural hypothesis to make, and I do not see how we can avoid making it, but it remains a hypothesis and an extrapolation. It involves, of course, the assertion—the axiomatic truth of which can scarcely be doubted by any physicist—that the laws of Nature remain the same at all times and all places. The observed fact is that the spectral lines in the light which reaches us from bodies at a great distance are displaced towards the red, or in other words, that *light is reddened by age*. Now the only interpretation of a reddening of light consistent with the accepted laws of Nature in a homogeneous world is a receding 'velocity' of the source. This is the observational evidence of the 'expanding universe' in a nutshell.

The dilemma that we are in has a certain similarity to that by which atomic physics was confronted some twenty years ago when the necessity became apparent of ascribing to the atom properties which in a finite material body would be contradictory. The concept of the universe as a closed entity is, so far as I can see, a hypothesis, an arbitrary addition to, or extension beyond, the observed phenomena by our imagination. We must be prepared to allow this 'universe', as we have been forced to grant to the atom, the freedom to have contradictory properties, and in particular we must try to accustom ourselves to the idea that the velocity of change of the quantity of the dimension of a length which occurs in the equations, and is interpreted as the 'radius of the universe', has nothing to do with the rate of evolutionary change of stars and stellar systems.

It seems to me that the current interpretation, and the consequent models of the universe as an expanding hypersphere (or elliptical space), may in course of time be found to be too simplistic, and be replaced by one in which the apparent contradictions are more satisfactorily hidden from view. But this does not affect the theory, which will retain its value, independent of the interpretations put upon it.

By Sir ARTHUR EDDINGTON, F.R.S.

DISCUSSION of detailed theories of stellar evolution is overshadowed by the fact that the time-scale is once again in the melting-pot. I think it will be agreed that if Prof. de Sitter is right in regarding the facts as indicating a rapid expansion of the universe or scattering apart of the galaxies, the very long time-scale of billions of years which has been fashionable of late becomes exceedingly incongruous : we should have to accept an age of the order 10^{10} years for the galaxies and presumably also for the stars. But the theory of the expanding universe is in some respects so preposterous that we naturally hesitate before committing ourselves to it. It contains elements apparently so incredible that I feel almost an indignation that anyone should believe in it—except myself. I have had a special reason for believing it which I have referred to from time to time, but it was not until last month that I was able to put it into definite shape.

I believe that from pure physical theory we can not only predict that this phenomenon of expansion will occur but also predict the actual rate of expansion ; and the calculated result agrees with the observed recession of the nebulæ. This result comes out of the wave equation for an electron—the fundamental equation of modern quantum theory. When I adapt the wave equation to take account of the curvature of space, I find that it ought to contain a term $\sqrt{N}/R$, that is to say, the square root of the number of electrons in the universe divided by the radius of the universe in its equilibrium state.

I do not suppose that this is a new term to be inserted as a correction to the ordinary equation ; it is already in the equation in disguise. It is the term attributed to the mass of the electron and ordinarily written mc^2/e^2. Sir J. J. Thomson was the first person to measure the mass of an electron. I do not think he realised in 1897 that the thing he was after—the constant which was responsible for the effects in the vacuum tubes attributed to mass—was the square root of the number of electrons in the universe divided by the radius of the universe. Really he was poaching on astronomical preserves. He was finding the rate of recession of the spiral nebulæ, or at least a very little calculation will derive it from his measures.

I take the value of $\sqrt{N}/R$ (or as Sir J. J. Thomson mysteriously called it, mc^2/e^2) according to his measurements and those of his successors, and combine it with well-known formulæ of the relativity theory which Prof. de Sitter has described ; then I can find at once the principal data about the size of the universe. For example, its original radius was 1070 million light-years, before it started to expand. I find also $N = 1{\cdot}29 \times 10^{79}$; and, what is of special interest, the rate of recession of distant objects can be calculated ; the result in the usual units (km. per sec. per megaparsec) is 528. This is the whole expansion effect, which will be reduced a little by the attraction of the galaxies on one another, but the reduction is not likely to be large.

The value found from astronomical observation ranges from 430 to 550 according to various determinations.

Naturally this close accordance of theory and observation has made me believe that both are right and that the observed motions of the nebulæ are genuine ; so that we must accept this alarmingly rapid dispersal of the nebulæ with its important consequences in limiting the time available for evolution.

By Prof. ROBERT A. MILLIKAN, California Institute of Technology, Pasadena, Calif.

ANYONE who knows me is quite aware of the fact that I have no qualifications for participating in a discussion of the evolution of the universe, unless perhaps it be because of my interest and activity in the development of our knowledge of the cosmic radiation. Since, however, results in this field now seem destined to exert a profound, if not a determinative, influence upon all theories of stellar evolution, it may not be out of place for me to outline the present status of our experimental findings in it, and to do what I can to show whither they point.

I note first, however, that the opening up of this amazing new field of knowledge is the work solely of the experimentalist. Plentiful as theorists have always been, especially in astronomy, and confident as they have always been in their conclusions, not one of them, so far as I know, who speculated about the nature of the universe, ever predicted cosmic rays, or even dreamed of their existence—certainly not sufficiently definitely to suggest any experiments actually to bring them to light. Prior to 1910 not a trace of evidence had appeared that such rays existed. They had not even been seriously proposed. Apart from a passing suggestion by O. W. Richardson,[1] in 1906, that electroscope-discharge effects observed on earth might possibly have something to do with solar influences—a suggestion quickly negatived by the fact that these effects are as strong at night-time as in day-time—I can find no record of the existence anywhere up to 1910 of any ideas even remotely related to those of the cosmic rays. Indeed, all the work that had been done prior to 1910, even on rays capable of discharging electroscopes through metal walls centimetres, or even inches, thick (so-called penetrating rays), was generally interpreted in terms of earth-rays, or of radioactive emanations getting from the earth into the lower atmosphere, and these are, in fact, responsible for much the greater part of the then observed penetrating rays.

In 1909 all the work that had appeared in this field up to that date was reviewed by Kurz,[2] and careful consideration given to each one of the only three possible origins of the observed effects, namely: (1) the earth, (2) the atmosphere, and (3) the regions beyond the atmosphere. The last two were definitely discarded, and the conclusion drawn that there was not the slightest evidence for the existence of any penetrating rays other than those produced by radioactive substances in the earth—this with full knowledge, too, dwelt upon at length in this very article, that half a mile of the earth's atmosphere would absorb all such radioactive radiations.

When, therefore, in that same year the experimentalist, Gockel,[3] took an electroscope three different times in a balloon to heights which reached four and a half kilometres and found that its rate of discharge was there even higher than on the earth, he had discovered something new and important, namely, that although there are penetrating rays that do originate in the earth and are indeed abundantly given off from practically all the elements of the earth's crust, as Kurz and the other workers prior to 1910 had rightly concluded, yet there must be other rays, abundant at high altitudes, that come in from above, originating either in the remoter regions of the atmosphere or else coming in from outer space; in other words, that one or the other of the two alternatives which Kurz had explicitly considered and definitely discarded had been incorrectly set aside for at least some rays that actually exist. Which one of these two, namely, upper atmosphere or outer space, it took a great deal of work by Hess, by Kolhörster, by v. Schweidler, by Bowen, Otis, Cameron, myself, and others, from 1910 to 1925 definitely to determine, and even in 1927, at the Como conference, one of the most distinguished of living physicists declared himself still a believer in the theory of an upper atmospheric origin.

To-day, however, I think the cosmic origin has been generally conceded, and with that concession it follows from the measurements themselves, not only that in the particular portion of our galaxy immediately around us, the energy carried by the cosmic rays is at least a tenth [4] of that existing in the form of radiant heat and light, but also, since these latter radiations must be diminished greatly in intergalactic space, that the energy carried by the cosmic rays throughout the universe is of the same order of magnitude as, possibly greater than, all other radiant energies combined. In the light of this fact, when one reflects that the second law of thermodynamics, which has, strangely, been thought by some so determinative for theories of the origin and destiny of the universe, and which may be roughly said to be merely a generalisation of the fact always observed *here on earth* that all forms of energy tend to become converted into heat and then to be radiated away from the earth and hence lost to us, one sees *how prone we are to make sweeping generalisations upon insufficient knowledge*. This is why the experimentalist has played and always will play so important a rôle in the progress of science. From the very inception of the experimental method he has continually been bringing to light facts which were not within the theorist's ken even when that theorist had got observational phenomena pieced together, *as he thought*, into a beautifully consistent and 'necessarily related' scheme. With perhaps the largest source of radiant energy as yet unconsidered, may it not possibly be that the thermodynamic theorist has gone too far in his dicta about the origin and destiny of the universe? This is my excuse for forgetting at least for the moment all about theories and asking first: What are the experimental facts in the field of cosmic rays?

There are three main facts that now seem to be quite well established, though discussion is still rife about some of them. I hope, however, that some of the new data that I am now able to present will help to bring about better agreement.

(1) The first fact is the complete uniformity in distribution of the cosmic rays within the present limits of experimental error in the measurement of their intensity. This has been disputed by some, but I should like to show how I have been testing it this summer. I have the results of a continuous month of observation of the intensities of the cosmic rays taken through four daily six-hour periods as follows: 6 A.M. to noon, noon to 6 P.M., 6 P.M. to midnight, and midnight to 6 A.M. None of the means of intensities in these four periods differ by so much as one half of one per cent. The barometer which, as Cameron and I first showed in 1925,[5] influences markedly these intensities, was extraordinarily constant during this summer month in Pasadena, scarcely varying 3 mm. either way from the mean throughout twenty-five days; the barometer readings go through a small minimum each afternoon, due to currents set up by the sun's heat, and a small maximum early each morning. The cosmic rays, on the other hand, go through a small afternoon maximum and an early morning minimum, thus showing quite conclusively, I think, that the minute variations in cosmic ray intensities are not due to radiations from the sun, which is shining during both maxima and minima, but rather to slight variations in the atmospheric blanket induced by the alternate heating and cooling of the gaseous matter through which the rays reach the earth. Further evidence for the correctness of this conclusion is found in the fact that the period from midnight to morning, during which the atmosphere is more quiet than is the case in any of the other three periods, shows the smallest variations in the individual readings, thus appearing to indicate that these, too, are due to atmospheric disturbances. The conclusion is consonant, too, with the latest, very exact measurements of Hoffmann[6] at Halle, though the conditions in central Europe are less favourable for testing the especial points brought to light in my own work.

This independence of the cosmic ray intensities of the positions of the sun, the Milky Way, or other celestial objects is the most amazing property of these rays, since it seems to show quite definitely that those portions of the universe—better, those directions in the celestial dome—in which matter is most abundant, such as the directions of the sun or the Milky Way, have no advantage over those directions in which matter is very scarce. I can see no possible escape from the conclusion that the conditions existing in those portions of the universe where matter is abundant, that is, in and immediately about the stars, are inimical to the formation of cosmic rays. This in itself bars out the likelihood of their being due to the annihilation of protons, provided such annihilation is to be called upon, in accordance with the demands of most modern astronomers, for maintaining the temperatures of the stars. In making this statement I am eliminating as scientifically unacceptable the hypothesis that in bygone ages these rays were created by processes no longer existing and have since been wandering like lost souls about a universe from which there is for them no possibility of escape. Further evidence on this point will be presently given.

If, then, the cosmic rays are forming *now*, there is no place of origin left except *outside the stars*, in the interstellar spaces, where both temperature and pressure are exceedingly low. This is not so unlikely a place of origin as it was a few years ago, before Bowen[7] at the Norman Bridge Laboratory had solved the seventy-year-old riddle of the nebulium lines and proved that the common elements oxygen, nitrogen, carbon, and sulphur, as well as hydrogen and helium, exist out there, giving rise to these nebulium radiations *in regions which may be light-years away from the exciting stars.*

(2) The second most significant cosmic ray fact is that which, after some less precise tests by Cameron and myself in 1926, was brought to light most unambiguously just a year ago when, by taking very careful and very exact readings with the same sensitive electroscope, first at Pasadena, lat. 34° N., then at Churchill, Manitoba, lat. 59° N., the nearest settlement on earth to the north magnetic pole, I proved that the incoming cosmic rays are not influenced at all by the earth's magnetic field, and drew from that fact the inevitable conclusion that when these rays enter the earth's atmosphere they have not previously traversed any amount of matter which is comparable with the thickness of that atmosphere, or else they would of necessity be mixed with secondary beta rays which would be directed into the earth most abundantly along the earth's magnetic lines and therefore enter it in the neighbourhood of the magnetic poles. These experiments furnish another independent and, I think, very beautiful proof that the rays must originate in interstellar space, for if they came from even the remotest exteriors of stars, they would have to be appreciably mixed with magnetically deflectable beta rays. The fact that they are not so mixed seems to me to

hit the annihilation theory a second fatal blow, for there is no sort of reason for assuming that annihilation takes place *only* in interstellar space. Such an assumption would destroy the whole purpose of the annihilation hypothesis, which up to the present has been to furnish the requisite energies to the stars.

Before I leave this point (No. 2), let me combine the experimental fact of uniformity of distribution (No. 1) with this other fact that the rays enter the atmosphere unmixed with deflectable beta rays. The uniformity of distribution could indeed be reconciled with the annihilation hypothesis if it could be assumed that, in looking out from the earth into the celestial dome, one were always looking through a quantity of matter equivalent to, say, a hundred metres of water, which is a thickness sufficient to absorb 99 per cent of all incoming cosmic rays. That one would actually encounter one thousandth part as much matter as this in going out to infinity through interstellar space seems to be contrary to all the other astronomical evidence. But let us forget this and see what the cosmic rays alone have to say about this point. If annihilation is going on in all matter independently of temperature, then so far as the cosmic rays coming to the earth's atmosphere are concerned, every element of the celestial dome would be like every other element, even if the sun were in one of these elements. This is the only way the annihilation hypothesis of the origin of cosmic rays can be reconciled with their uniformity of distribution. But in this case the whole of the cosmic ray beam entering the earth would be a beam in complete equilibrium with its whole train of secondaries, including the deflectable beta rays, and these latter would of necessity spiral about the earth's magnetic lines and thus enter the earth only near the earth's magnetic poles, a behaviour which clashes with fact No. 2. Since, then, no trace of such an effect is actually found, the cosmic rays alone deny the validity of this hypothesis and with it of this form of the annihilation hypothesis as to their origin. It seems to me, therefore, that as an explanation of facts about cosmic rays the annihilation hypothesis fails at every point at which one can test it. I shall comment upon still another of its failures in the next section.

(3) The third well-established and most significant fact of cosmic rays is that *they have a banded structure*. Cameron and I first brought this fact sharply to light in 1925,[8] but I do not think the evidence is yet clearly understood or its significance fully appreciated. The evidence goes back to the high balloon flight [9] with self-recording electroscopes which Bowen and I made in 1922, a flight in which we reached an altitude of $15\frac{1}{2}$ km. and got 92 per cent of the distance to the top of the atmosphere as measured by the weight of the earth's atmosphere left beneath our instruments. *In other words, remembering that the atmosphere is the equivalent of* 10 *metres of water, we were within* 80 *centimetres of the top*. Now, 80 centimetres of water allows at least 3 per cent of rays as soft even as those of thorium C″ to pass through it. We made this high flight on purpose to determine whether the rate of increase of ionisation within a closed electroscope continued exponentially to the top of the atmosphere as Kolhörster's earlier balloon flight to a height of 9 km. had indicated was the case up to that altitude. Further, Cameron and I have since, with precision instruments, obtained quite as high ionisation readings up to 5 km. as those given by Kolhörster, thus checking sufficiently his exponential curve in the lower stretches of the atmosphere; but the $15\frac{1}{2}$ km. balloon flight not only failed to do this, but it showed definitely and altogether unambiguously, since all possible sources of error would have increased, not decreased, our readings, that with increasing altitude the ionisation fails to maintain its rate of increase but drops back toward lower values, just as it should do if a band of about 25,000,000-volt pure photons enters the atmosphere and requires a considerable distance of penetration into it before it gets into equilibrium with its secondaries. This furnishes a third bit of independent evidence that the cosmic rays do enter the atmosphere as practically pure photons, and hence that they originate in interstellar space.

The high balloon flight shows much more than this. It proves conclusively that neither gamma rays of energy 2,500,000 volts, like those from thorium C″, nor rays of intermediate penetrating power up to that of the softest cosmic rays, which pass through about five times as great a thickness of water and have an energy of about 25,000,000 volts, enter the earth's atmosphere in amounts appreciable in comparison with that of this softest cosmic ray band; for any such abundant rays would have very rapidly discharged our electroscope when it got within 80 cm. of water of the top of the atmosphere. In other words, there is a definite and strong cosmic ray band of penetrating power about five times (energy ten times) that of the hardest gamma rays of thorium. The analysis of this band on its other side, that is, on its short wave-length side, into much lower intensity bands but of energies of the order of 100,000,000 volts and 180,000,000 volts respectively, has been made by Cameron and me,[10] though these results are less certain. *The existence, however, of a very strong cosmic ray band at about* 25,000,000 *volts, fairly* *sharply limited on both the long wave-length side and the short wave-length side, is altogether definitely established by our experiments.* This band carries of the order of 90 per cent of the total cosmic ray energy entering the atmosphere.

Now, the experimental fact of the existence of such a band is only interpretable, so far as I can see, by the assumption of some kind of an energy-emitting atomic transformation of definite energy-releasing value taking place continuously throughout interstellar space. The other suggestions which have been made to account for a band having such energy can, as it seems to me, be quite definitely eliminated. There are but two of them. First, some kind of a cosmical electrical field has been postulated of sufficient total potential difference to impart the necessary 25,000,000 volts to electrons falling through it. There is indeed known to be a symmetrical field surrounding the earth, *but its value is definitely known to be about one million volts and its direction is such as to drive electrons out, not in; in other words, it has one twenty-fifth the requisite strength and is of the wrong sign.* No potential difference of 25,000,000 volts, symmetrical with respect to the earth and positive at its surface, can possibly be assumed without, as I think, colliding head on with other well-established facts; and even if such a field existed, it would produce a more or less continuous spectrum, like the general X-ray spectrum, instead of a well-marked band. I cannot see any way of making this suggestion reproduce even remotely the experimental situation.

The second effort has been to find a way by which rays due to the annihilation of protons (which are at least thirty-five times too energetic) could be softened down to the observed values. The most interesting of these suggestions has been made by Dr. Zwicky, who seizes upon the nebular red shift recently discovered by Hubble to suggest that if at the creation, which he estimates from the maximum possible amount of matter in interstellar space would have been about 10^{19} years ago, annihilation rays began suddenly to be formed, and if these same rays are still hurtling about in a spherical universe and are subject to the red shift, these rays, with all that have been created since, would now constitute a spectrum having a definite limit to its frequency on the long wave-length side, but falling off very slowly in intensity on the short wave-length side, where the limit would be 25,000,000 volts. In my opinion, this is the only conceivable mechanism by which annihilation rays could become reduced in energy to that observed on the long wave-length side of the band which carries nine-tenths of the energy of the cosmic rays; but the shape of the spectrum on the other side is completely wrong, since it represents a very slowly falling intensity with increasing frequency, instead of a fairly sharp, well-marked band such as experiment reveals.

No other known mechanism can soften an original beam of photons of the gamma ray type. If such a beam is in the least inhomogeneous, our universal experience is that it is hardened, not softened, by passage through matter. If, on the other hand, the original beam is monochromatic, it is also at first apparently hardened in the process of getting into equilibrium with its secondaries, and when that condition has been attained it has regained its original absorption coefficient. If annihilation rays maintain the temperatures of the stars at all, the mechanism by which they do it is not that of a continuous degradation of wave-length until the wave-lengths corresponding to heat and light waves are reached. The process is rather that the penetrating power of the beam of photons is maintained *until all of these photons have been picked off by Compton encounters* and complete absorption of the beam has thus been brought about, the temperature of the matter traversed having been thus slowly raised by this straight attrition process. Our third fact, then, of a banded structure in the particular region in which the chief cosmic ray band is found, appears to me to be the last arrow that pierces the heart of the already twice fatally wounded annihilation hypothesis of their origin.

Also, I should like to present one more reason why the hypothesis of cosmic electrical fields as an agency for directly imparting energies of from 25,000,000 volts to, say, 400,000,000 volts to electrically charged particles cannot be admitted. It is not only that the most fantastic assumptions would have to be made to make such fields produce cosmic ray bands of the type observed, but also, more than that, to make fields of any such intensities symmetrical with respect to the earth and thus account for the uniformity of distribution of cosmic rays would involve, as it seems to me, something very like a return to the geocentric theory of the universe—a return scarcely acceptable to any scientific worker who has lived since A.D. 1500.

With cosmic electrical fields and the annihilation of protons both completely unacceptable, what is then left to furnish the energy, first, of the great cosmic ray band which carries 90 per cent of the energy of these rays? The answer seems to me to be as follows:—If the Einstein equation $E = mc^2$, and the actual facts of isotopes, as accurately worked out first by Aston, are taken as guides, then this answer is unambiguous. But first, how dependable is this Einstein relation? Note first that it is a purely thermodynamic equation, stating merely energy relations. In using it, therefore, we are completely independent of any assumption as to the nature of the cosmic rays. Whether they are photons or high-speed charged particles is, so far as it is concerned, quite immaterial. Again, as to its dependability, I think that most physicists would say that it is about as safe a guide as any theoretical equation which we now have in physics. It not only rests on excellent theoretical foundations, but also it has predicted quantitative relations which have stood the tests of all the careful experiments which we have as yet been able to apply to it. *It states with entire definiteness that there is no atomic transformation whatever that can furnish the necessary energy except an atom-building process.*

I have thus far reached, merely by a process of exclusion, the conclusion that the cosmic rays are the wireless signals of the building in interstellar space of some at least of the heavier elements out of the lighter. The evidence, however, is very much better than that. It is reasonably quantitative in the case of the main cosmic ray band. Here again theory and experiment support one another. For, if atom-building takes place at all, there is one such atom-building act that is more fundamental than all others, and that also must take place more frequently than all others, namely, the formation of helium out of hydrogen, because we have abundant evidence that all the elements are actually built out of hydrogen and helium, and that helium is built out of four atoms of hydrogen, so that the hydrogen-to-helium transformation should take place much more frequently than any other. The energy of this transformation is computed from Einstein's equation and Aston's measurements at 25,000,000 volts: just what I stated above to be the energy of the large cosmic ray band that carries the great bulk of the cosmic ray energy entering the atmosphere. But the way in which I arrived at that figure requires some explanation. Most simply stated, the method used was to compare directly in the waters of high altitude lakes the penetrating power of the cosmic rays there found with the penetrating power of the hardest known monochromatic gamma rays, namely, those of thorium C″, which have an energy of about 2,500,000 volt-electrons. *This I have done directly, the observed ratio being between* 6 *and* 12.

The comparison cannot be made directly with great precision, because the cosmic rays are not homogeneous, but when the inhomogeneities are sifted out into bands (and this does not need to be done with great precision, almost any reasonable kind of sifting being satisfactory, since the softest band carries so large a fraction of the total energy) the penetrating power of this softest band comes out very close to five times that of thorium C″. Further, at altitudes between 6 km. and 9 km., where the harder components should exert almost no influence, according to the observations of both Hess and Kolhörster in their manned balloon flights, the directly measured penetrating power of the cosmic rays was six times that of the hardest gamma rays. This checks most satisfactorily with our analysis of our curve. The best formula we now have (the Klein-Nishina) connecting penetrating power and energy then makes this energy come out ten times that of thorium C″, or about 25,000,000 volts. This Klein-Nishina formula has been directly proved by Bowen and myself, as well as by others, to fit the facts up to rays of the hardness of those of thorium C″ reasonably well, and the extrapolation from there up to the softest cosmic ray band is not likely to be very badly in error. Indeed this whole procedure may be looked upon merely as the extrapolation, to a not unreasonable distance, of an *experimental* curve, and thus as largely independent of the Klein-Nishina formula or indeed of any theory. At any rate the foregoing is, in my judgment, the only quantitative test of the energy of any portion of the cosmic ray spectrum that has any sort of significance. For to use the Klein-Nishina formula to compute the energies of the rays, not five times but 200 times as penetrating as those of thorium C″, as some who have sought to make the cosmic rays proton-annihilation rays have done, seems to me to be extraordinarily rash. If my extrapolation is not significant, theirs must represent that lack of significance raised to a very considerable power.

Let me not, however, overstress the precision of this quantitative comparison. *It is scarcely necessary to do so, since there is no other energy-releasing act of this order of magnitude that can produce this soft component of the cosmic rays.* There are, to be sure, other *atom-building* acts which might produce as soft or softer rays, and some of these may well be taking place, such, for example, as the formation of carbon out of helium, but a consideration of the *abundance* of the elements involved shows that such rays would in general be of negligible intensity. The fact that there are but some five *abundant* elements is here a powerful guide.

So far I have dealt only with the formation of helium out of hydrogen. Cameron and I have shown that our complete cosmic ray curve is consistent with the existence of three other bands corresponding to the three other abundant groups of elements which we have called the oxygen group, the silicon group, and the iron group, but the evidence was here necessarily qualitative in view of two recently demonstrated cosmic ray facts. The first is that referred to above, namely, the evidence that the rays come into the earth's atmosphere as practically pure photons. For this means that we may not assume that the harder rays have reached a condition of equilibrium with their secondaries at the points at which we measure them.

The second is that interesting new fact discovered by Bothe and Kolhörster[11] a couple of years ago: that the penetrating power of the secondary beta rays may apparently, with increasing energy, rise up to, or possibly surpass, the penetrating power of the original photons. This seems to me to furnish the most simple explanation of the fact brought to light by Regener, as well as by Cameron and myself in our work at great depths in water, namely, that there is a very small component of the cosmic rays of intensity of the order of 1/500 of that of the whole cosmic ray beam as it enters the atmosphere, which has a penetrating power thirty-five or forty times that of the main cosmic ray band of which we have been speaking. The hardest rays of appreciable intensity that should be formed by the atomic building process are those of iron, and their energy should be about seventeen times that of the helium rays. There are indeed more penetrating atom-building rays that might be formed, but the abundance of the elements corresponding to them is so small that these rays should be exceedingly feeble—probably too feeble to make the formation of these heavy elements out of hydrogen a good theory as to the origin of the hardest component of the cosmic rays.

There is, however, already some little evidence, which we hope soon to complete and extend, that the penetrating power of these very hard rays increases more rapidly than does their energy, and in the present state of our knowledge this is the best working hypothesis to account for this one point which might raise doubts about the completeness of the atom-building explanation of the cosmic rays. If, however, it might be assumed that proton-annihilation does not take place in the stars, but does take place, *with* atom-building, in interstellar space, then proton-building might perhaps be called upon to account for this 1/500th part of the cosmic rays which possess this great penetrating power. In other words, if one prefers to go no further than to seek an answer to the question, What processes are able to take place in Nature that are appropriate for supplying the energy actually found in the cosmic rays? then the answer is, atom-building taking place in interstellar space for supplying more than 99·5 per cent of the rays and proton, or nucleus, annihilation for supplying the remainder. If we assume them both, then the energy-conditions represented in Einstein's equation can easily be satisfied.

The reasons, however, for such proton-annihilation in interstellar space are not so cogent as are those for atom-building. These latter reasons, apart from the direct experimental evidence, may be stated thus. We know that all the atoms are actually built up out of hydrogen, and it is therefore natural to assume, even apart from direct experimental evidence, that somewhere or other the process is going on now. Secondly, Bowen has shown that the reason so-called forbidden spectroscopic lines appear in the nebulæ and not on earth is that, given a long enough time free from collisions, an atomic change will take place that cannot take place where atomic collisions are frequent. It is not a dissimilar hypothesis, and one concordant with modern wave mechanical thinking, too, that a cluster of four hydrogen atoms *free from collisions for a long enough time* will jump over a potential wall and find themselves together in the nucleus. The reason this does not take place in the atmospheres of the stars, or even on earth, is presumably that temperature and density, that is, energy and frequency of collisions, destroy the clusters or prevent altogether their formation. But at low enough temperatures, under the influence of ordinary molecular forces, these clusters *must* form; for what is liquefaction other than the process of formation of such larger molecular groups? It is such clustering, combined with freedom from energetic collisions, which, according to the hypothesis herein presented, provides the necessary condition for occasional atom-building. It might conceivably take place in the very remote regions of our atmosphere, but in that case it should also take place in the remote regions of the sun's atmosphere; and the cosmic rays should then be much stronger during the day than at night, a result contrary to fact. Atom-building *in interstellar space* is therefore a natural enough hypothesis. Proton annihilation, on the other hand, must take place, if at all, either within the nucleus or through the rushing of an enormously energetic electron into the nucleus. These acts ought to be either independent of temperature or else facilitated by it. The first cannot be true if cosmic rays are to be explained by annihilation, else the sun would be enormously influential. The second may be true, and annihilation therefore confined to the interiors of stars as has been generally assumed. It is therefore very unnatural, if not quite impossible, to assume annihilation to take place in interstellar space and *not* in the atmospheres of the suns. This hypothesis, therefore, I should not wish to make until every other avenue of escape from our difficulties has been closed. If the ideas presented herewith prove correct, they will obviously influence strongly not only present theories but also all future theories of the origin and destiny of the universe.

[1] Richardson, NATURE, **73**, 607; **74**, 55; 1906.
[2] Kurz, *Phys. Zeit.*, **10**, 836; 1909: see also Millikan, NATURE, **126**, 14; 1930, for historical studies.
[3] Gockel, *Phys. Zeit.*, **11**, 280; 1910: also **12**, 597; 1911.
[4] Millikan and Cameron, *Phys. Rev.*, **31**, 930; 1928.
[5] Millikan and Cameron, *Phys. Rev.*, **28**, 856; 1926.
[6] Hoffmann, *Zeit. für Physik*, **69**, 703; 1931.
[7] Bowen, *Astroph. Jour.*, **67**, 1; 1928.
[8] Millikan and Cameron, *Phys. Rev.*, **28**, 851; 1926: see also *Phys. Rev.*, **37**, 244; 1931.
[9] Millikan and Bowen, *Carnegie Institution Year-book*, **21**, 385; 1922: also *Phys. Rev.*, **22**, 198; 1923, and **27**, 353; 1926.
[10] Millikan and Cameron, *Phys. Rev.*, **37**, 235; 1931.
[11] Bothe and Kolhörster, *Zeit. für Physik*, **56**, 751; 1929.

By Prof. E. A. Milne, F.R.S.

It has sometimes been asked whether the universe is evolving at all. Certainly its future evolution is a matter of speculation, and its past evolution a matter of inference. But that it is evolving at this present time is not a matter of speculation at all—it is a matter of observation. We actually see stars undergoing drastic changes—changes of organisation so fundamental that they can almost be described as mutations. I refer to those outbursts which we call ‘ novæ ’.

From time to time in the heavens a faint star is seen to blaze up for a few hours or days, increasing in brightness perhaps some twenty-five thousand times, then irregularly fading and after a few years returning to its pre-outburst brightness. Such outbursts are by no means uncommon. The first quarter of the current century has witnessed five novæ visible to the naked eye. Many will remember Nova Aquilæ of 1918, which was visible even before dark; and Nova Cygni of 1920. These from their brightness were comparatively near-by stars—Nova Aquilæ, the more distant, was only some three hundred parsecs away. But many more have been observed in the most distant regions of space. Up to 1929 more than eighty novæ had been recorded in the great nebula of Andromeda, and were used by Hubble to determine its distance. Thus novæ are distributed throughout space. They are also distributed fairly frequently in time. Hubble has estimated that some thirty novæ occur each year in the Andromeda nebula. It has also been estimated that in our own galactic system there is at least one nova per year. Such estimates are conflicting, but, as many have pointed out, they imply that every star ' becomes a nova ' at least once in its life-time. We thus have in each nova an evolutionary event characteristic of the stars as a whole. Let us examine the event more closely.

The spectrum of a nova shows that the outburst is accompanied by the emission of streams or jets of gas, in various directions, at the large velocity of some 1500 km. a second. There is little doubt that this gas is a portion of the atmosphere driven off by the greatly increased radiation pressure. Some years ago when I was engaged in discussing the sun's chromosphere, I found that the hotter stars would have difficulty in retaining certain elements such as calcium, since the pressure of the outflowing radiation, greedily absorbed by the atoms in their spectral lines, would be strong enough not only to balance gravity at the star's surface, but further to repel these atoms in a kind of upward rain. Later, I examined the probable limiting velocity that such accelerated atoms would attain, and I found that it was of the order observed for the gases expelled from novæ. Thus one of the characteristics of a nova outburst is that the huge increase in brightness blasts off, almost explosively, portions of the star's atmosphere, and that these clouds of gas travel outwards through space. We shall see the evolutionary significance of this later.

This driving off of atoms probably has its maximum effect when the star is at its brightest. What of the subsequent events as the star fades ? The atmospheric layers, or some of them, continue their journey outwards, but what of the mother star itself ? Let us re-examine such a star a few years after the outburst. We find that it has returned to the same undistinguished brightness as it possessed before the outburst—but with a difference. Its spectrum is now of what is called type *O* or Wolf-Rayet, indicating a very high surface temperature or surface brightness. From this we can make a most important deduction. If the *total* brightness of the star is the same as before the outburst, but the *surface* brightness—the brightness per square mile—is much greater than before, the total area of the star must be much smaller than before. If the area is smaller, so must be the radius and the volume. The reduction in radius as observed may be as much as 10 : 1 or even more. Thus after the outburst the star is found to have shrunk. We must resist the temptation to describe it as the shadow of its former self. It is smaller in dimensions but much brighter per square mile, and as bright as before in the aggregate. We have the paradoxical situation that the outer parts expand, forming an advancing outward-moving cloud, but the inner parts contract.

We can only see the shrunken star when the expanding clouds have moved out of the way. During the outburst itself the expanding atmospheric gases obscure our vision of what is happening. We can only conjecture what goes on behind the expanding screen. But there can be little doubt as to what is actually happening. The expulsion of the atmospheric clouds is a consequence of the brightening, but the shrinking of the rest of the star is not just an accompaniment of the brightening—it is the actual origin of the brightening. When the star contracts it must lose gravitational energy, which is set free as heat and light. Somehow it must get rid of this energy. If it got rid of it slowly there would be little visible effect. Actually we see the star, in its period of maximum brightness, getting rid of an enormous amount of energy very quickly indeed.

Since the rate of brightening is very rapid, we infer that the process of shrinkage is very rapid—in fact cataclysmic. The process of shrinkage is a veritable collapse. In a nova outburst the star is seen to be collapsing on itself ; and the suddenness of the collapse, and the resulting enormous amount of gravitational energy that must be got rid of in the short time available, conspire to produce the huge brightening of the star as observed. This sudden liberation of energy produces enormously increased radiation, which in turn expels the outer layers of gas. Such is the probable explanation of the origin of novæ, or ' new stars '.

These outer layers thus expelled constitute an exceedingly small proportion of the whole mass of the star. Thus the mass of the star, after the outburst, is practically the same as before, yet it occupies a much smaller volume. Hence its mean density must be much larger than before. The increase in density as observed varies from one-hundredfold to ten-thousandfold. The gases expelled from the star during the outburst are chips from the old block ; but the star itself does not remain an old block ; it becomes very much of a new block—a very dense block.

We can now link up this widely spread phenomenon with our general views on stellar evolution. A star may be supposed to fade, very slowly but quite definitely, as it ages. At some particular luminosity it collapses, giving rise to the appearance of a nova. This must be due to some structural weakness in the star, some insecurity in its foundations. It falls on itself like a house of cards. Samson in the Old Testament story lost his strength when he was shorn of his locks. The star also loses its strength when it is shorn of its locks. But the shearing of the stellar locks—the liberation of wisps of atmospheric gases—is a consequence, not the cause, of the internal weakening.

We know other stars which are very dense—the stars which are known as ' white dwarfs ' and the nuclei of the planetary nebulæ. Planetary nebulæ are probably exceptionally large ex-novæ in which the expelled gases linger round the star as a permanently accompanying nebula. It is reasonable to assume that the white dwarfs have also undergone the process of collapse—that every white dwarf has been at one time a nova. Though few white dwarfs are known, all those with which we are acquainted are close to the sun, whence it is concluded that they are very common in Nature. This is in general agreement with the ideas I am describing, since if novæ are common the heavens must be littered with ex-novæ, and these remain in the form of white dwarfs. We must now endeavour to explain the origin of the weakening which causes the cataclysm. In my investigations of stellar structure I have encountered circumstances in which this weakening occurs. There is considerable discussion at the moment as to whether stars are built of perfect gas or not. It is true that the mathematician can construct in imagination configurations of perfect gas which in some ways resemble stars, but these are not the only configurations that he can construct. We are also acquainted with another phase which gaseous matter can assume—what is called the degenerate state, a state which gases take up when the density becomes disproportionately large compared with a certain function of the temperature. Our knowledge of this state is due to Fermi and Dirac, and it was shown by R. H. Fowler that this must be the state of a very dense star such as a white dwarf.

The mathematician's task in this connexion is to construct all the configurations in which a given mass of material can occur. Similar situations arise elsewhere in physics. For example, if the mathematician is dealing with a specimen of water in an enclosure, he will find configurations in which it may be entirely steam (in a super-cooled state) or partly in the form of steam and partly in the form of water. Some of his systems will be one-phase systems and some two-phase systems. Similarly, the astrophysicist can construct configurations, of the stellar order of magnitude, in some of which the material is entirely in the form of perfect gas, in others of which it is partly in the form of perfect gas and partly in the form of degenerate gas. There are probably other phases yet unknown to us, but we can at least construct the outer parts of possible configurations. When this is done, it is clear that the two-phase configurations, having very dense cores, have enormously less energy than the one-phase configurations, and consequently that the one-phase configurations, if disturbed, will undergo a transition to the two-phase configuration. The purely gaseous configuration is analogous to the super-cooled steam, which if suitably disturbed condenses partly into drops of water. Again, it is analogous to the excited state of a Bohr or Schrödinger atom which when disturbed executes a transition to a state of lower energy, giving up the difference as radiation. In each case the excess of energy is given up suddenly. Similarly, a gaseous stellar configuration if disturbed would collapse and give up energy rapidly as it passes to a configuration of lower energy. We infer that stellar gaseous configurations are in general unstable and cannot exist in Nature. The stars in Nature must have small dense cores—a sort of foundation stone.

As the luminosity of such a star decreases, the core will vary in size. The behaviour of the core is very complicated, and I have not yet been able to trace it in full detail. But it appears that at a certain critical value of the luminosity the core increases *suddenly* in radius, due to collapse of the gaseous envelope. This is a consequence of the very low opacity of degenerate matter.

Degenerate matter obstructs the passage of radiation far less strongly than gaseous matter, and so experiences a smaller radiation pressure ; accordingly, at a certain luminosity, as soon as more degenerate matter is deposited on the core, radiation pressure weakens still further, further collapse occurs, more degenerate matter is deposited, and so on. The star has a canker at the core. Or, to put it another way, degeneracy is an internal disease which propagates itself with startling rapidity under favourable stellar conditions. The outward and visible symptom of the disease is the nova-phenomenon, and, as is not unknown in medicine, a high surface temperature is developed. The existence of a discontinuity in core-radius can be established mathematically, but I only provisionally identify this particular discontinuity with the observed discontinuity in Nature. We at least obtain a hint as to the character of one of the most important events in the evolutionary history of a star.

Collapse alone is not, however, the only possibility. If the collapsing star is in a state of rotation, the increased angular velocity consequent on collapse may cause the star to divide into two, in the manner described by Jeans. This may be the origin of the great frequency of occurrence of double stars in Nature. Something of this kind has been observed in the case of Nova Pictoris. The two fragments so produced may remain collapsed (and thus very dense), or, in certain circumstances, provided as they are with an internal source of energy, they may re-expand and re-form even giant stars. In the fable the mountains gave birth to a mouse. In the heavens the dwarf may give birth to giants.

To summarise, we see a twofold evolutionary process at work amongst the stars, both parts of the process occurring in this same phenomenon of the nova. On one hand we observe the stars to be collapsing and giving birth to planetary nebulæ, white dwarfs, and even double stars. On the other hand, we see nebulous clouds driven off to space, there to reinforce the already existing cosmic cloud. Reinforce ? Possibly we have struck the real origin * of the cosmic cloud—possibly the cosmic cloud is the totality of the debris of the expelled atmospheres of novæ and other hot stars. The universe is simultaneously condensing into hard blocks and evaporating to form a tenuous nebula. I repeat, this is what we observe. Sir James Jeans has suggested that the universe began as a widely-extended nebula, which condensed into stars in virtue of gravitational instability. The picture I have attempted to draw is that of a concourse of stars, on one hand condensing into denser stars in virtue of instability under a waning light pressure, and on the other hand simultaneously expelling their atmospheres and generating a nebula.

* This idea has been suggested also by Vorontsov-Velyaminov, *Observatory*, Aug. 1931.

By General the Right Hon. J. C. Smuts, P.C., C.H., F.R.S.

In a sense my presidential address dealt with some aspects of the evolution of the universe, but a good deal more could be said, if there were time, about the philosophical issues which are raised by this subject.

I do not agree with those who say that the recent advances in physics have no important value for philosophy. The most creative philosophical thinkers in the past have as a rule been saturated with the science of their time, which gave substance and body to their philosophy ; and it is only to be expected that the recent revolutionary advances of physics are bound to have the most profound effect on our world-view and on our philosophical outlook. How, for example, could philosophy, which has for thousands of years speculated on the nature of time and space, be unaffected by the fruitful integration of the two which has been effected by the physicists in our time ? Again, the concept of the quantum, with its peculiar behaviour, its holism, its indeterminacy, is bound to be far-reaching for philosophy no less than for physics. Of course, it is extremely difficult at this early stage, when we are at the beginnings of these changes and physical theory is still in a state of flux, to say what exactly will be the philosophical outcome of the new physics. But I have little doubt that the revolution in physics will yet be followed by a revolution in philosophy, and that in their joining of forces a new era will open up for both science and philosophy.

So far as philosophy is concerned, one is at present perhaps more impressed with the difficulties and perplexities, arising from the new concepts in science, than with its solid results. Thus one is inclined, from the point of view of relativity, to attach the greatest importance to the new space-time concept. If the old forces of Nature, like gravitation, and perhaps even electromagnetism, are (as Einstein teaches) nothing but curvatures of space-time, if matter itself is really only such a curvature, one feels inclined to look upon space-time as the basic structure of the world, and as no mere mathematical symbolism. Space-time becomes something like the old ether, a substratum or matrix from which all the physical differentiations have taken place. To such a view one is inevitably led by the relativity theory and its results. Yet the next moment, when one considers the behaviour of the ultimate physical units, especially the electron and the quantum, one meets with phenomena in flagrant contradiction with the idea of space-time, as if, for the electron and quantum, space and time really do not exist, as if space-time is a macroscopic result rather than an ultimate basic feature of the cosmos. If space-time is merely a statistical macroscopic result, there seems to be some flaw in the fundamental relativity treatment which makes it basic to everything in the world of existence. We seem to be making for a real clash between the relativity and the quantum concepts, unless we have to admit that both are still provisional, and that a wider, truer unification or reconciliation is still to come.

I could refer to other difficulties and perplexities arising from the new physics, but I pass on to another point which has perhaps a closer bearing on our topic for discussion. I wish to refer to the peculiar character of the ultimate physical units and their bearing on the evolution of the universe. Many physicists, including even a profound thinker like Sir Arthur Eddington, maintain the view that exact science, and physics in particular, is confined to the metrical aspects of the world and has nothing to say as to the nature of the universe. If this is really so, then how could Sir James Jeans say, in a recent book of his, that the universe as viewed by the new physics is more like a great thought than a great machine ? This seems to go far beyond the metrical characters of the world. If physics were confined simply and solely to the metrical aspects of the world, then how can Sir Arthur Eddington himself find intimations of freewill in the law of indeterminacy ? Truth is a whole, and the truth of physics will be found to link on and to be but part of that larger truth which is the nature and the character of the universe.

When we speak of the evolution of the cosmos we are faced with a series of questions in regard to the ultimate physical constituents of the world and their interplay, the routes they follow, the structures they form, and the resulting characters, size, mass, and properties of the universe as a whole. These are problems for physics. But we find that that evolution also comprises the emergence of life and mind, of the human soul and human personality, and a whole new world of values of all sorts. Truth, beauty, goodness, and love are as much structures of the evolutionary universe as the sun and the earth and the moon. But when we come to scrutinise the ultimate foundations of this vast superstructure, we naturally look for the characters which would in the long run render such enormous developments possible. This would be in particular the attitude of the philosopher, with his wider outlook over the realms of existence ; and it is for him that from this point of view the new quantum physics has a peculiar fascination. For he finds that the physicists, working merely with their own tools and their own incomparable technique, and looking for no more than the metrical units which subtend this universe, have indeed, like the man who looked for asses and found a king, found much more than they have looked for. The units they have discovered will constitute not only a world of physics but, in the end and at far removes, also a world of life and spirit.

These units, particularly the electron and the quantum, have an almost metaphysical aspect : they are physics infected with thought ; they are not in space-time, they behave as wholes, they are indeterminate in their behaviour, so that the law of chance rather than the ordinary causal law applies to them. Of course, it is possible that this ambiguous character is due to infection from the mind of the research worker, as if the mental instrument of research has affected the result. But it is far more likely that in this dual character of the physical units we reach an ultimate basic fact, that we transcend the mind-matter distinction and reach the bedrock where no such differentiation has yet arisen. Thus it comes that the ultimate units are not purely physical or material, but point to an undifferentiated primitive world matrix, which includes both the physical and the thought characters of the world.

When from these units we start our process of world building, we find at first what is apparently merely a physical universe. But gradually the suppressed vital and mental elements, inherent in the universe from the start, begin to emerge. Cosmic evolution is thus found to include organic evolution, and this again gives rise to the evolution of spirit, of social and spiritual values, which form our own human phase of this cosmic process.

The origin of life from matter, of mind and personality from both, has always seeemed an unresolvable mystery. The philosophy of emergence which has recently arisen, and purports to explain the creative rise in evolution, the coming of life and mind and spirit—this philosophy has appeared to be based on a miracle or a series of miracles. The quantum physics has come to the relief of this school, which surely teaches a significant truth. According to quantum doctrine, the roots of life and mind lie imbedded deep down in the ultimate structure of this universe, and they are not mere singular apparitions of an unaccountable character, arising accidentally in the later phases of evolution.

To me the significance of the new physics for philosophy seems to lie particularly in this linking of the deep-down beginnings with the most advanced achievements of the universe. The apparent huge gaps in evolution, requiring a miracle of leaps at more than one stage, are thus shown to be a gross exaggeration. We see the universe gradually pushing to the front certain features and characters which have been there all the time from the very beginning, although in most primitive and scarcely recognisable form.

One concluding remark. We cannot say that the universe has been built up from these units or any ultimate units in the course of its evolution. We can only say that these are the ultimate elements which we find on analysis. The universe may for ever have been a complex affair, a closely knit structure of some sort or other, as the law of entropy would seem to indicate. It may even have been one vast quantum, as the Abbé Lemaître suggests. But in the evolution of this mass there has been relative movement of parts or features; some have pushed more to the fore with time, and there is no doubt that life and mind are features that have thus emerged from the mass, with the increase of entropy.

By the Right Rev. E. W. Barnes, F.R.S., Bishop of Birmingham.

I WILL begin by briefly recapitulating the theory of the evolution of the universe in the form in which, as I understand, it at present exists.

In the beginning, a large unbounded finite three-dimensional universe with space of very small positive curvature was filled with highly diffused matter of very small density. The matter began to aggregate into masses of approximately equal size, spread fairly uniformly throughout the space; and the whole space began, at some epoch or another, to expand. The masses attracted neighbouring matter and somehow acquired velocities of rotation which increased as the matter in them condensed. Finally, incredibly vast bun-shaped aggregates, spinning too quickly for the stability of their outer edges, began to throw off drops, as it were. The drops congealed into suns, and ultimately the aggregates became the spiral nebulæ which now exist. Each of them, apart from possible central regions of diffuse matter, consists of thousands of millions of stars. Our sun is a star of no particular importance, belonging to a spiral nebula or group of nebulæ called the Galactic universe. That universe came into existence some five million million years ago.

Since they first existed, stars in the various universes have moved aimlessly under the influence of their initial velocities and mutual attractions. Periodically, but rarely, at intervals of tens or hundreds of millions of years, collisions between suns in the various universes have taken place and planetary systems have been born. So the earth came into existence some two or four thousand million years ago. Thus ours is quite possibly one of the youngest planetary systems in *our* universe. On the cooling earth primitive forms of life appeared at least a thousand million years ago; and gradually, by a slow evolution, more highly developed living organisms arose. Finally, about a million years ago sub-men emerged from a group of anthropoid apes.

If I personally am critical of this picture, I plead that we must not confuse speculative possibility with satisfactory demonstration. I am concerned that we do not give arguments to obscurantists, who claim that the scientific theories of one generation are usually repudiated by the next. So I would begin by emphasising that in the group of possibilities and probabilities just outlined there is much less certainty than, say, in the facts upon which Darwin rested his conclusions when the "Origin of Species" was published seventy-two years ago.

I need not refer to the prejudiced opposition by which Darwin was assailed. Of course, his triumph has been signally complete. But others before him had put forward theories of the evolution of terrestrial animals. What Darwin did was so to accumulate and arrange biological facts, that experts were convinced that evolution by the mechanism of natural selection had produced from primal organisms the vast range of living things upon the earth, including man himself. Since Darwin wrote, further investigation and discovery have confirmed his insight. Some of his subordinate opinions were erroneous ; but his main scheme stands intact because his facts were correct. All fresh geological and embryological investigation confirms the conclusions on which he rested. The scanty remains of primitive man that are discovered from time to time accord with expectation ; and such statistical investigation as those in R. A. Fisher's recent volume are a triumphant vindication of the potency of natural selection.

It is worth while recalling these facts when we consider the picture of the evolution of the universe which has emerged from recent work. I personally doubt whether the time has yet come for an astrophysicist of genius to write a book which shall in its own sphere rival the "Origin of Species". We can point to a few new facts and to considerably more new (and occasionally discordant) theories. Out of them there has emerged the present picture, immensely exciting, but by no means certain.

What of it can we regard as certain ? First of all, there is no reason to doubt the existence of island universes. Such form that vast, fairly regular distribution of spiral nebulæ through the depths of space which is revealed by photographs taken in the great telescopes. We can say with fair certainty that our own galactic universe is either a single spiral system or an aggregate of several such, each analogous to millions of others with which space is strewn.

Secondly, I would say that it is fairly certain that our space is finite, though unbounded. Infinite space is simply a scandal to human thought ; and, though we must not assume that the universe was made that man might understand it, the alternatives to finite space are incredible. We cannot accept the idea of island universes succeeding one another indefinitely as we pass in imagination through the depths of space. Such a distribution does not accord with a Euclidean-Newtonian gravitational scheme, for it would lead to infinite gravitational potentials. Neither can we with equanimity think of a vast finite group of universes forming a sort of island in empty space. Ultimately such a group ought, one would surmise, to aggregate into a single mass. But in Riemannian spherical space we can have a finite and uniform distribution of universes, inasmuch as such space is unbounded so that every point is related to the whole as is every other point. Finally, there is no fact of observation to set against the belief that space has a very small positive curvature.

Thirdly, I think we must accept as highly probable Jeans's hypothesis that in the stars matter is actually destroyed as protons and electrons unite to form radiation. To this conclusion, however, we are driven by failure to find any satisfactory alternative explanation of the vast output of energy by the stars. If, however, their lives were not to be measured by so long a period of time as millions of millions of years, the necessity for assuming the actual destruction of matter would not be so great.

Consider now some of the uncertainties and difficulties which belong to the present scheme. First of all, of course, there is the insoluble difficulty of infinite time. No man of science will postulate a supernatural intervention, a stirring of the uniformly distributed matter filling space with which in imagination the present scheme begins. Yet in default of such a beginning, we must imagine an infinite regress, a never-ending sequence of alternate periods of world-building and world-destruction, the rise and fall of universes without end. In comparison with such a past the future is perhaps less perplexing, though it is not very satisfying, because the second law of thermodynamics seems to necessitate an end when all energy will have so run down that nothing happens anywhere to break a deadly uniformity. Of course, we can escape from the difficulties caused by infinite time if we accept the opinion entertained by some philosophers that time is not real. With them we may hold that the notion of time is due to the nature of our perception. If we accept such an idea, we can assert that consciousness in its passage through the space-time continuum meets but does not cause events. It then follows, however, that all our fancied activities are an illusion. Against any such belief we must put what surely is our constant and invariable experience. We *have* a measure of freedom of will. Labour and struggle *are* real. We *can* get nearer truth and overcome evil as we strive for goodness. Thus I am forced to conclude that the time-process must be real. Yet, unless time is an illusion, or unless alternatively the cosmos had a beginning in time, any picture of the evolution of the universe must fail to satisfy.

Let us take it, however, that the primal mist that filled all space in the beginning aggregated into masses of roughly equal size in a finite universe and that these masses slowly began to condense and revolve. Whence came their rotation ? To this question I can find no satisfying answer. Let us ignore the difficulty. Out of condensation and rotation came the spiral nebulæ, the universes of thousands of millions of stars with which space is strewn. Obviously, with relatively few exceptions, the great nebulæ are built to a common pattern. They are results of rotation, and the picture of vast rotating lenticular masses throwing off stars, like drops at the edge of a fly-wheel, satisfies the imagination. But why are the arms of the spirals so short ? We should expect arms which curl repeatedly round the central nucleus. None such exist ; and yet there is apparently no ejection into space of early-born stars to account for the disappearance of the coils of stars that ought to be visible.

The different chains of reasoning by which Jeans has been led to assign a period of millions of millions of years for the age of our universe seem to me to demand respect. I doubt whether they are conclusive. But I wish that there were certain knowledge of the development and decay of typical stars. Theories abound. Some are magnificent in the ingenuity and in the intellectual power which have gone to their making. But the final theory of stellar evolution has not emerged. I confess, moreover, that I am by no means happy with regard to the expanding universe; and I doubt whether Doppler's principle is rightly applied to measure the velocities of recession of the great nebulæ. It is not improbable that the reddening of their light is due to other causes. If the great nebulæ are moving away as fast as is suggested, we are lucky to be living at an epoch when we are able with our telescopes to see them at all. Moreover, the Friedman-Lemaître equations give contraction as an equally possible alternative to expansion. May it not be that a velocity of approach is masked by some other effect? A universe that was continuously contracting would have a snug end. Steady and continued inflation, either of a currency or of a universe, is disquieting.

It is, however, when we come to the formation of planetary systems that I feel especially uneasy. The current theory is, as I have said, that the earth and its associated planets arose from the encounter of our sun with a wanderer which came so near as to disrupt it some few thousand million years ago. If the theory be true, planetary systems must be rare and therefore consciousness, as men know and possess it, is rare. In fact, the existence of consciousness, when it occurs, will be the by-product of an accident. We are then apparently forced to conclude that the universe was not created with the primary object of producing beings in whom mind should lead to spiritual excellence.

There is, of course, no reason why consciousness should be associated with animals such as ourselves who represent transformations of carbon compounds at temperatures between the boiling and freezing points of water. It might, for all we know to the contrary, be associated with changes in the ionisation of atoms or with the disintegration of their nuclei at temperatures of hundreds of millions of degrees. But of any such bases for the appearance of mind we have no knowledge. What we do know with certainty is that throughout the universe the raw material of which it is made is fairly uniform. The matter in distant stars is the same as that which exists in our own sun. We must then assume that there are planetary systems in distant island nebulæ, and that on some of them conditions resemble those which exist on our earth. So life, as we know it, must be distributed throughout the universe; but, if the collision theory of planetary origins is correct, the distribution is astonishingly sparse.

I do not, of course, suggest that there are human beings on other planets. The direction of the physical and physiological evolution of living things upon our earth would seem, if we can judge by the geological record, to have been somewhat erratic. Particular mutations coincided with particular conditions of environment to determine the direction of change at any instant. But, throughout the known geological process there has been large-scale progress, a possibly unsteady but quite definite development of mind. In the possibly very different living organisms of other planets there will have been a progressive development of mind. Our physical structure matters little in comparison with the kind of consciousness which it carries.

We can then, as it seems to me, assume the existence throughout the universe of conscious beings. If it be true that our earth and all planetary systems similar to our own originated in a chance collision of suns, life elsewhere must be as a rule unimaginably more developed than with ourselves. Also planets carrying living organisms must be incredibly rare. After a life history of five million million years our galactic universe will have but one sun in a hundred thousand with satellites which can carry life. Such extravagant world-building for such meagre results leaves one dubious as to whether the theory is correct. In defence of such doubts as are forced upon me I might point out that the origin of our moon, with its exceptional density and massiveness, has not been finally settled. The theory that it was broken from the earth when the latter was mainly liquid owing to a chance 'resonance' phenomenon Jeffreys, in a recent paper, deems untrue. Even present estimates of the age of the earth, in so far as they depend on the rate of disintegration of uranium, puzzle us because we do not know why there should have been any uranium in the earth at its birth. Thus I personally should not be surprised if new facts were forthcoming to give some other explanation of the existence of planetary systems. I suspect that such systems are much more numerous than is at present believed.

I need scarcely emphasise that the issue raised by the relative frequency of planetary systems is of great philosophical importance. From Judæa, through the Christian church, has come a belief in ethical theism which has been a strong and ennobling influence in European civilisation. To-day such belief rests upon the conviction that we can only explain the universe by assuming that it is due to creative thought and will, associated with purpose and plan. Such purpose appears most clearly on earth in the progressive development of mind, which has ended in the recognition of moral values by humanity; and the religious outlook of many of us is determined by our belief that God has thus created man for His service. But, if consciousness should be proved to be but a rare accident of a vast, otherwise aimless, universe, such belief in God would be encumbered by a new perplexity. I may add that the belief would not disappear, since we should still have to explain why man has been created with the conviction that he must be loyal to goodness and truth.

Such philosophico-religious speculation is, however, premature. We need more facts and, that we may obtain them, we need new instruments of greater power and precision. The interferometer, we may hope, points the way to instrumental triumphs in the future. If only an instrument could be invented which should enable us to determine whether stars, within, say, a hundred light-years' distance, have planetary systems attached to them! We should then know whether any of the few thousand stars near the sun have planets on which life may conceivably exist. If even one such system were found, the present theory of planetary origins would collapse. Failing any such invention of a super-telescope, there remains the possibility of wireless communication. As I have already indicated, I have no doubt that there are many other inhabited worlds, and that on some of them beings exist who are immeasurably beyond our mental level. We should be rash to deny that they can use radiation so penetrating as to convey messages to the earth. Probably such messages now come. When they are first made intelligible a new era in the history of humanity will begin. At the beginning of the era the opposition between those who welcome the new knowledge and those who deem it dangerously subversive will doubtless lead to a world war. But the survivors, when they extricate themselves from the economic consequences of the peace treaty, will begin what we may correctly term a strenuous correspondence course. I should like to be living then. We might get a true understanding of the evolution of the universe.

By Sir Oliver Lodge, F.R.S.

It is well known that a physical theory which ignores some of the elements of the problem is incomplete, and is therefore liable to break down when confronted with the facts. A physical theory cannot take the whole universe into account; but if it is to be complete enough to be satisfactory, and to make trustworthy predictions, it must take all relevant factors into account.

Sir James Jeans began his discourse by saying that in Section A we were concerned only with the physical universe—that is, with material bodies and the forces that act upon them. I suppose that is true. But the fact that it is true seems to make it impossible for this Section to enter upon a philosophical discussion of such a subject as the universe as a whole, and to decide its fate upon purely deterministic lines. For the universe certainly contains more than we deal with in this Section. We must remember that there are Sections D and K and I and J—that is, we must realise that the universe is not solely inorganic. Some of the matter is animated; and although it is still obedient to the laws of physics and chemistry, an animated body behaves in a spontaneous manner not predictable by those laws. When a thing behaves as if it were alive, physics loses interest in it and hands it over to another section; for it is incompetent to deal with motions attributable to spontaneity and free will. Wherever life has entered in, the predictions of physicists and astronomers and mathematicians are spoilt. Laplace's calculator might reckon the behaviour of every particle in the universe so long as it was not interfered with by life and mind.

I have looked sometimes at the ripples coming over the sand on a sea beach and leaving a deposit of foam. I have thought whether a mathematician, given sufficient data, could predict every ripple and every line of foam. Yes, he could, theoretically, provided there were no boats, nor any fish. The splash of a fish, the ripples of a boat, would put his calculations out. Given even a spark of free will, there are no data that can be supplied. It may be said that our sense of free will is an illusion. Well, that is a philosophical question that can be raised. But it cannot be settled in this Section. So I venture to think that before we can philosophise upon such a theme as the ultimate fate of the universe, we must be able to take everything into account, and philosophise with a very wide and comprehensive knowledge of reality.

Maxwell showed how the effect of mind could be introduced into the scheme of physics without contravening any of the laws of energy, except the purely statistical second law of thermodynamics. His 'demon', by dealing with the particles individually and selectively directing them, could interfere with and neutralise the consequences supposed to be deduced from that law. I claim as a physicist that too much attention has been paid to this second law of thermodynamics, and that the final and inevitable increase of entropy to a maximum is a bugbear, an idol, to which philosophers need not bow the knee.

It is doubtless instructive to learn from high and competent authorities what the unadulterated or rather unvivified laws of physics applied to the universe will lead to. We are faced with a steady running down or degradation of energy to a predetermined end: without hope of novelty introduced at any stage of the process, all settled and dull, events just going through the hollow form of taking place. But it is all on the assumption that there is nothing or no one to wind it up or to guide it to some nobler end. Guidance has only recently intruded itself into the scheme of physics; but already there are guiding waves which determine the path of a particle of matter. What the significance of those guiding waves may be, whether they have any connexion with the observed phenomena we know as life and mind, is at present an unanswered question.

To philosophise from a restricted point of view is interesting enough, but it is not conclusive. It does not fully account for the state of the world to-day, nor can it be depended on to formulate its course to-morrow.

Printed in Great Britain by R. & R. Clark, Limited, *Edinburgh.*

deposits; major divisions of the Palæozoic; geomorphogenic processes in arid regions; fossil man and contemporary faunas; and orogenesis. It is also hoped to make provision for the presentation of papers not falling under any of these headings. The responses received, together with those from the first circular, will form the basis of the mailing list for the next circular. This will be issued in 1932 and will contain registration forms. The present circular may be obtained from the General Secretary, Sixteenth International Geological Congress, Washington, D.C., to whom all inquiries should be addressed.

Sir Sidney Harmer, lately Director of the Natural History Departments of the British Museum, has been elected a foreign member of the Royal Swedish Academy of Science.

At the ordinary meeting of the Institution of Electrical Engineers to be held on Nov. 5, an oil painting of the late Dr. S. Z. de Ferranti, an honorary member and a past president of the Institution, will be presented to the Institution by Mr. H. Marryat. The portrait is by Mr. David Jagger, R.A.

The following have been elected officers of the Philosophical Society of the University of Durham: president, Prof. R. A. Sampson; vice-presidents, Sir Westcott Abell, Prof. G. R. Goldsbrough, Prof. T. H. Havelock, Prof. P. J. Heawood, Sir William Marris, Prof. J. Irvine Masson; honorary general secretary, Mr. W. M. Madgin; honorary treasurer, Mr. J. W. Bullerwell; editor, Prof. G. W. Todd; and honorary librarian, Dr. F. Bradshaw.

On Oct. 13, the first public lecture under the Estlin Carpenter Educational Trust was given at Oxford by Sir Peter Chalmers Mitchell, on the subject of "Life and Matter". The chances, he pointed out, are enormously against the existence of requisite conditions for life elsewhere than in very minute parts of the universe. In support of the view that life is not a distinct principle apart from physical and chemical conditions, Sir Peter laid stress on the facts now known concerning the properties of crystals, of colloids, and of 'Bütschli's foams'. Speaking of the production of organic substances in the laboratory, he instanced the formation of starch and sugar from formaldehyde under the influence of ultra-violet rays. Much advance has been made in this direction since the first artificial production of urea.

An autumn expedition to Churchill, on Hudson Bay, for the purpose of photographing the aurora borealis is announced by the press bureau of the Canadian National Railways. Churchill has been chosen because it is in the direct line between the auroral and the magnetic poles and on the path of the maximum light frequency of auroral waves. The expedition, which is now at work, will remain for about six weeks, and hopes not only to photograph the aurora but also to measure its height. In order to record the complete range of colour in the aurora, special cameras have been designed.

The Ministry of Health desires to continue the investigations, commenced in 1926, into the characteristics of acute disease of the nervous system (occurring in the form of meningitis, encephalitis, or polio-encephalitis) following in the wake of vaccination or of acute infections, such as measles, chickenpox, influenza, and the like. It is therefore requested that medical practitioners who may be called in to attend cases of this nature will immediately forward information to The Senior Medical Officer, Med. i., Ministry of Health, Whitehall, London, S.W.1. Attention is also directed to the fact that an anti-vaccinal serum has been prepared by the Lister Institute, doses of which can be supplied by the Ministry on application, for the treatment of post-vaccinal encephalitis.

The twenty-second annual exhibition of electrical, optical, and other physical apparatus is to be held by the Physical Society and the Optical Society on Jan. 5–7, 1932, at the Imperial College of Science and Technology, South Kensington. The Trade Section will comprise the exhibits of manufacturing firms. The Research and Experimental Section will be arranged in two groups: (*a*) exhibits illustrating the results of recent physical research; (*b*) lecture experiments in physics. The Exhibition Committee invites offers, from research laboratories and institutions and from individual research workers, of exhibits suitable for inclusion in either of the above groups. Offers of exhibits, giving particulars of space and other facilities required, should be communicated immediately, and in any case not later than Nov. 9, to the Secretary, Exhibition Committee, 1 Lowther Gardens, Exhibition Road, London, S.W.7. The Section for Apprentices and Learners has for its object the encouragement of craftsmanship and draughtsmanship in the scientific instrument trades. Apprentices and learners may exhibit, in competition, specimens of their work, providing they are in the regular employ of a firm which is exhibiting at the next annual exhibition, or has exhibited once during the past three years. Printed particulars of this Section will be sent on application to the Secretary.

A short list (S.S. No. 2) of second-hand scientific journals, transactions of learned societies, and books relating to science has reached us from Messrs. Oppenheim and Co. (Rare Books), Ltd., 317A Fulham Road, S.W.10.

Applications are invited for the following appointments, on or before the dates mentioned:—A graduate assistant master for mathematics, science, etc., in the Devonport High School—The Secretary for Education, Rowe Street, Plymouth (Oct. 31). An engineering assistant in the water department of the City of Sheffield—The General Manager and Engineer, Town Hall, Sheffield (Oct. 31). A graduate assistant in the mathematics department of the Coventry Municipal Technical College—The Director of Education, Council House, Coventry (Nov. 12). A science master at Sir Thomas Rich's School, Gloucester—The Headmaster, School House, Gloucester. A senior mathematical master in the Newport, Isle of Wight, Secondary School—The Director of Education, County Hall, Newport, I.W.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Age of the Oldoway Bone Beds, Tanganyika Territory.

As readers of NATURE are aware, there has been a certain amount of controversy over the Oldoway skeleton found by one of us (Prof. Hans Reck) at Oldoway, in Tanganyika Territory, in 1913.

In order to clear up this question if possible, and to try to find some data by means of which the Oldoway bone beds could be definitely correlated with those in Kenya Colony, it was decided that the East African Archæological Expedition should spend a part of the 1931–32 season at Oldoway, and Prof. Reck (of Berlin) and Mr. Hopwood, of the British Museum (Natural History), were invited to be members of the expedition during that part of the season's programme.

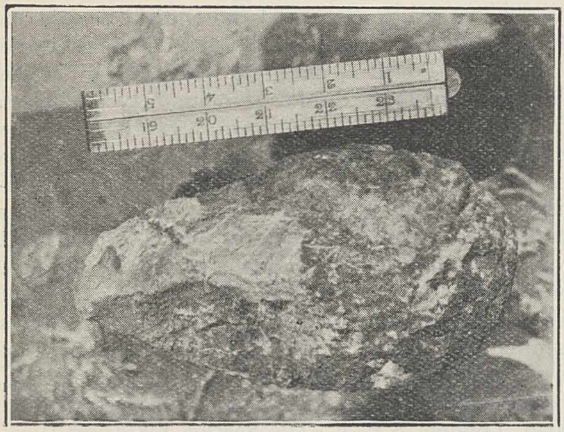


FIG. 1.—One of the coups de poing found *in situ* in Bed B at Oldoway.

FIG. 2.—Prof. Hans Reck and Mr. A. T. Hopwood sitting on the exact spot where Prof. Reck dug out his Oldoway man in 1913.

After a very careful examination of all the evidence on the spot, and the collection of certain new data, we have come to the following conclusions:

1. There is no possible doubt that the human skeleton found in 1913 came from Bed No. 2, and not from a pocket of Bed No. 4, as was suggested by one of us (Leakey) in "The Stone Age Cultures of Kenya".

2. Beds Nos. 3 and 4, which overlie Bed No. 2 conformably, have both yielded unrolled coups de poing *in situ*, and these tools are absolutely typical of the late Kenya Chellean and of the Kenya Acheulean from the upper part of the Kamasian series at Kariandusi River, as described in "The Stone Age Cultures of Kenya" by Leakey. Bed No. 2 has so far yielded two flakes with definite bulbs of percussion.

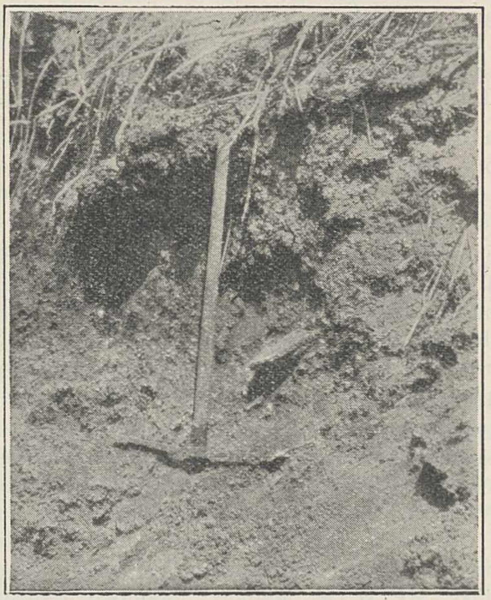


FIG. 3.—The same coup de poing as Fig. 1 but *in situ*.

3. Although in Kenya the deposits of Kamasian age have yielded as yet no identifiable fossils, it thus seems to us clear that the Oldoway bone beds are the equivalent of the Upper Kamasian, and this correlation is borne out by the fact that the Oldoway beds, like the typical Kamasian series in Kenya, are older than the period of volcanic activity and rift faulting, which was responsible in Kenya for the Gilgil and Kikuyu Escarpments and the volcanic mountains, such as Longonot, Suswa, etc., and in Tanganyika Territory for the Ngorongoro volcanic highlands and the escarpment which cuts them to the east.

4. The fact that the Oldoway bone beds prove to be of Upper Kamasian age and not Gamblian is absolutely in harmony with the palæontological evidence, since the Oldoway beds contain a fauna in which more than 50 per cent of the animals are extinct genera and species.

5. From all the available evidence, it thus seems to us that the Oldoway skeleton represents a type of *Homo sapiens* living during the Upper Kamasian period, from which up to the present the only lithic cultures known are the late Kenya Chellean and the Kenya Acheulean.

L. S. B. LEAKEY.
ARTHUR T. HOPWOOD.
HANS RECK.
East African Archæological Expedition,
P.O. Box 658, Nairobi, Oct. 5.

The Mercury Band Spectrum in Fluorescence.

MUCH has been written on the various band-series and isolated bands in the spectrum of mercury, and their relation to the atomic excited states: but it cannot be said that the subject has been satisfactorily cleared up.

I wish here to record an observation which will help to decide between the various alternatives which have found favour. Mercury vapour saturated at a pressure of 5 mm. was excited to fluorescence by a cooled mercury vacuum arc, the light of the latter being filtered by a dilute solution of thiophene in alcohol, which completely cuts off wave-lengths of 2460 or less. Under these conditions the fluorescence is excited exclusively by the core of the atomic resonance line. I have called such fluorescence the 'core effect'. The spectrogram shows the first three members of a series of bands originally found in absorption by R. W. Wood. These are at 2345, 2338, and 2334. Being of much shorter wave-length than any line from the thiophene-filtered source, the bands appear on a clear background free from false light. I do not know of any equally striking case of the violation of Stokes's law.

As regards interpretation, I limit myself at present to remarking that the experiment suggests a relation of Wood's bands to a ^3P and not to the ^1P state of the molecule. It would be easy to say a good deal more: but there has perhaps been a tendency to over-facile theorising on this subject, and further discussion is reserved.

RAYLEIGH.

Terling Place, Chelmsford,
Oct. 12.

Constitution of Thallium and Uranium.

THE preparation by Dr. v. Grosse of suitable volatile compounds of thallium and uranium has enabled me to obtain their mass-spectra. The lines of thallium were obtained by means of its triethyl compound. As was expected, it consists of two isotopes, 203, 205. The latter predominates to an amount in good agreement with the chemical atomic weight (204·39).

Results with uranium hexafluoride indicate that uranium is probably simple to at least 2 or 3 per cent. Its line 238 was photographed in satisfactory strength, followed by a series, 257 UF, 276 UF_2 . . . etc. The last line visible in this series, 333, corresponding to UF_5, is the heaviest mass definitely recorded on a mass-spectrum. It has not yet been possible to determine the packing fraction of U 238.

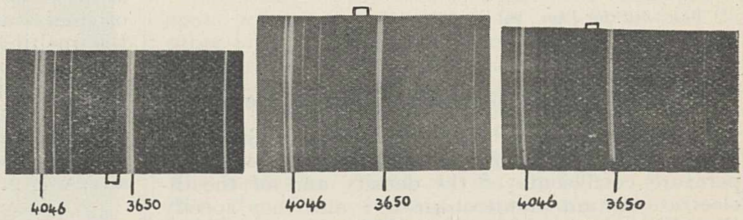
F. W. ASTON.

Cavendish Laboratory,
Cambridge,
Oct. 8.

Raman Lines due to Co-ordination Bond.

FOLLOWING J. J. Thomson, Kossel, and others, it can be shown that a large number of observed facts about co-ordination compounds can be explained on the assumption that these groups are held by purely electrostatic forces to the central atom. Grimm has been able to calculate the heat of association per molecule of NH_3 surrounding the cation of a simple salt, from a knowledge of the heat of formation of the ammines and the lattice energy of the simple salt. He has shown that for a monovalent cation with an octet shell the heat of association is 14 calories per gram molecule, 28 calories for a divalent ion, and so on. Also, the maximum co-ordination number of an atom can be shown to depend on the relative size of the latter to those of the co-ordination groups.

Other properties of such compounds, namely, the diamagnetism of certain complex radicles with a paramagnetic central atom, the change of valency of the radicle when one of the co-ordinated group is replaced by another, for example, by the replacement of a NH_3 group by Cl, find an easy interpretation on the theory proposed by Sidgwick. According to this theory, the co-ordinated groups are bound to the central atom by homopolar bonds, in which both the electrons are supplied by the former. It has been pointed out by one of us [1] that from magnetic evidence alone it can be shown that even in the case of co-ordination compounds of the iron group of elements there is not always a sharing of electrons as contemplated in Sidgwick's theory. According to our point of view, a true co-ordination bond occurs when the spin moment of one of the uncoupled electrons in the third shell of the central atom is neutralised by the spin moment of one of the two electrons supplied by the co-ordination group. This leads either to a complete neutralisation of the magnetic moment of the central atom or to a large reduction in its value.



Mercury arc. $[Co(NH_3)_6]Cl_2$. $[Co(NH_3)_6]Cl_3$.

FIG. 1.

From this point of view it is interesting to compare the magnetic moments of $[Co(NH_3)_6]Cl_3$ and $[Co(NH_3)_6]Cl_2$ with that of the simple salt $CoCl_2$. The first one is diamagnetic, and in it the NH_3 groups are replaceable by Cl with a lowering of the valency of the radicle, while the second compound has a magnetic moment almost the same as that of the simple salt, namely, 25 Weiss's magnetons, while that calculated according to Sidgwick's theory is 8·6.

Thus we see that the first one is a true co-ordination compound and the second is an associated compound. According to the view proposed here, since a co-ordination bond is a weak valency bond between the central atom and one of the co-ordinated group, the presence of such a bond ought to manifest itself in the Raman spectra. We have photographed the spectra of the radiation scattered by solutions of these two hexammine compounds, and have found definite evidence of the presence of modified lines in the case of the cobaltic compound, but not in the case of the other (Fig. 1). The mercury lines used by us were:

	λ.	ν.	Intensity.
A	3650·2 A.	27,388 cm.$^{-1}$	10
B	3654·8	27,354	6
C	3663·2	27,291	10

In some cases, by using a filter of Wood's glass (transmission 390-320 mμ), we have cut off all the other strong lines of mercury except those given above. With the cobaltic solution we obtained the following modified lines:

	λ.	ν.	Δν.	Origin.	Δν.	Origin.
a	3715·8 A.	26,905 cm.$^{-1}$	483	*A* – *a*	575	*A* – *b*
b	3728·5	26,813 }	478	*c* – *b*	562	*C* – *c*
c	3740·2	26,729 }				

It is supposed that the modified lines are due to the two strong mercury lines denoted by *A* and *C*, and the middle one represents two lines not resolved. That the absence of modified lines in the radiation scattered by the cobaltous compound is not due to the difference in the absorbing powers of the two solutions in this region is supported by the fact that while $[Co(NH_3)_6]Cl_2$ has an absorption band only at 519 mμ, $[Co(NH_3)_6]Cl_3$ has two, one at 515 mμ and another at 349 mμ. Similar modified lines also appear with a solution of $[Co(NH_3)_5Cl]Cl_2$.

D. M. BOSE.
S. DATTA.
University College of Science,
Calcutta, Aug. 15.

[1] Bose, *Zeit. für Phys.*, vol. 65, 677; 1930.

Melting Points of Nitrobenzene and of Benzene.

IN recent issues of NATURE, Messrs. Wolfke and Mazur have recorded discontinuities in the temperature coefficients of the density and of the dielectric constant of nitrobenzene; and they specify the melting point of their purified material as 5·5°.[1] It appears that this is taken by them to be the true melting point of nitrobenzene of really high purity.

Eight years ago, Roberts and Bury[2] reviewed the very various melting points—ranging from 3° to 9°—which are recorded for this substance; and by purifying commercial nitrobenzene they obtained constant fractions which melted 0·175° higher (by a Beckmann thermometer) than did purified dry benzene. Accordingly, accepting for the melting point of the latter compound the value 5·493° given by T. W. Richards, Carver, and Schumb,[3] they stated the melting point of nitrobenzene as 5·668° ± 0·010°. They also noted and measured the marked depression of melting point caused by traces of moisture, atmospheric or other. These figures, 5·493° for benzene and 5·67° for nitrobenzene, are quoted in the International Critical Tables as suitable datum-marks in precise thermometry.

It will be seen that the figure given by Roberts and Bury is nearly 0·2° above that of Mazur and Wolfke; this is not easy to explain, unless the latter's material were, after all, not as pure as the former authors', or unless the American figures for the melting point of benzene were too high.

Recently I needed some really pure nitrobenzene, and made it by the following method. Benzene, bought from Messrs. The British Drug Houses as "Extra pure, for molecular weight determination", and melting near 5·5°, was fractionally frozen between 5·58° and 5·38° in a slow current of pure dry hydrogen, in such a way that it took eleven hours to freeze out 210 gm.—two-thirds of the original—as a clear icy tube. This solid melted steadily at 5·58°, and the same figure was obtained a month later when phosphorus pentoxide was added to a 40 c.c. sample.

Of this purified benzene, 2 gram-molecules were mononitrated by hand for five hours at 45° (finally 60°) with a slight excess of a pure mixed acid of molar fractions H_2SO_4 (0·33), HNO_3 (0·18), H_2O (0·49), which acid I had found, in other work, not to dinitrate nitrobenzene. The product (98·5 per cent yield) was washed, dried, and distilled from phosphorus pentoxide below 64° (1·0–1·5 mm.) in an all-glass apparatus; the middle fraction was then put through exactly the same process of fractional freezing (5·80°–5·50°) as that described for the benzene. The solid fraction (four-fifths) melted at 5·84°, and this was repeated with a sample containing phosphorus pentoxide a month later. The liquid fraction, after being redistilled *in vacuo*, melted steadily at 5·83°. Exposure to air, or in rubber-stoppered vessels, lowered the melting point by about 0·1°. The thermometers were standardised by the National Physical Laboratory immediately after the end of these tests, and the temperatures given are true to the nearest 0·05°.

It thus appears that: (1) The melting point of benzene suggested by T. W. Richards as 5·493° is about 0·1° too low and should not be taken as a fixed point in precise thermometry. The value 5·58° agrees with the result obtained by Sydney Young. (2) The melting point of dry nitrobenzene is close to 5·85°. Judged by this, as also by the original standard of Roberts and Bury's materials, the melting point given by Mazur and Wolfke is decidedly low. I should not regard either my benzene or my nitrobenzene as being of the highest attainable purity; but the constancy of their arrest-temperatures for an hour or more, during the transition from liquid to solid block, indicates that the impurity, if any, was in each case extremely small.

IRVINE MASSON.
Durham, Oct. 2.

[1] Wolfke and Mazur, NATURE, **127**, 741; 1931. Mazur, *ibid.*, 893, 993. Wolfke and Mazur, *ibid.*, **128**, 548; 1931.
[2] Roberts and Bury, *J. Chem. Soc.*, **123**, 2037; 1923.
[3] Richards, Carver, and Schumb, *Jour. Amer. Soc.*, **41**, 2019; 1919.

Spectrum of Glow-worm.

It is well known that the luminous organs of insects like the fire-fly represent one of the most efficient sources of light, in that almost all the energy is concentrated in a narrow band in that part of the spectrum to which the human eye is most sensitive. In the case of the fire-fly, the spectrum photographed by Mr. Ives[1] shows that this band extends from 5230 to 6550 A.

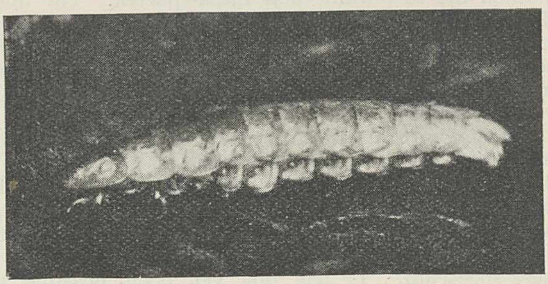


FIG. 1.

We had recently an opportunity of photographing the spectrum of a glow-worm which belongs probably to one of the species of Lampyridæ. Figs. 1 and 2 illustrate the worm in two positions and magnified about one and a half times. The portion indicated by an arrow in Fig. 2 shows one of the two luminous organs of the worm. The spectrum of the light emitted was photographed on an Ilford panchromatic plate with a night-sky spectrograph. An exposure of about fifteen minutes was found sufficient when a fairly narrow slit was used. The spectrum consisted of a single band extending from 5290 to 5860 A., that is, only half as broad as the

FIG. 2.

band in the spectrum of the fire-fly. Fig. 3 is an enlarged reproduction of the spectrum.

The glow-worm was not seen to emit any light by day even when observed in a dark room. Even at night the emission is not continuous. The worm can apparently do this at will. Slight irritation or shaking seems to make it well disposed to glow very brilliantly.

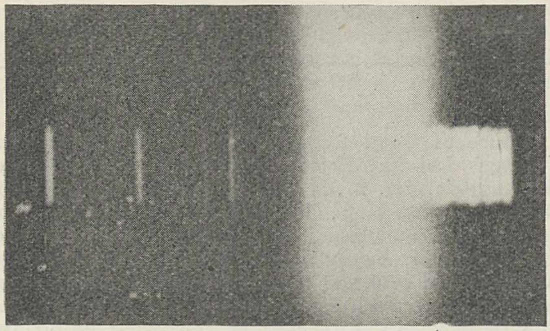


FIG. 3.—Spectrum of light from a glow-worm with a comparison spectrum (neon).

L. A. RAMDAS.
L. P. VENKITESHWARAN.

[1] H. E. Ives, "The Fire-fly as an Illuminant", *Journal of the Franklin Institute*, vol. 194, p. 213; 1922.

The Viscosity of Liquids.

IN NATURE for April 12, 1930, Prof. E. N. da C. Andrade states his theory of the viscosity of liquids as based upon "a temporary union of molecules in contiguous layers". This theory has been proposed in order to account for the temperature variation of the coefficient of viscosity in liquids, which is opposite in sign to that of gases. The nature of viscosity must be importantly different in a liquid and a gas.

It is the purpose of this brief letter to direct attention to the agreement of the theory of Prof. Andrade with the conception of the nature of the liquid state which arises from X-ray diffraction studies of various liquids. It is common for physicists to regard a liquid from the point of view of the nature of a gas, although in practically every kind of physical measurement except fluidity the liquid is more like the crystalline than the gaseous state, and even this exception is more apparent than real. The X-ray experiments have shown that there is coherent diffraction in liquids analogous to that in a crystal. Instead of sharply defined periodicities of structure in liquids there are found bands of periodicities. There is molecular arrangement caused by the molecular fields which cannot be accounted for by the existence of that portion of the field which has the effect of a shape or volume. This somewhat orderly arrangement of molecules is unstable; the distance of molecular separation is a matter of probability. At any instant there are spots throughout the liquid where the molecules have a more orderly arrangement which fades off into one of much less regularity. These spots or groups are not merely instantaneous, for periodicity occurs frequently in more than one direction—indeed, sometimes it is detectable in three, but there is also a constant change occurring in the constituents of a periodic region. This structure of a liquid is not crystalline. A word is needed, and I have suggested 'cybotaxis' as indicative of space arrangement.

Now the theory of Prof. Andrade postulates a duration of the 'temporary union' of molecules which does not exceed the very brief time for the molecules to acquire a common velocity of translation. Obviously the cybotactic condition is not described precisely in this manner, but the theory of Andrade may be said to represent one aspect, and may therefore be said to be to this extent in harmony with the cybotactic conception of liquid structure.

According to my own view, in these well-ordered spots or groups the slippage of one layer past the other is hindered by the molecular fields, and hence the larger or more perfect these groups the greater the viscosity of the liquid. This view is borne out by an examination of 22 octyl alcohols. Here a definite correlation has been found between the perfection of the orderly arrangement and the viscosity. The results are shortly to be published. It is clear that the effect of rising temperature would be to reduce the extent of a well-ordered group, and hence to decrease the viscosity.

It would seem therefore that X-ray diffraction experiments indicate strongly that Andrade's theory is in the correct direction, though not yet developed in accordance with my interpretation of the periodicities clearly evidenced by X-rays.

G. W. STEWART.

University of Iowa,
Iowa City, Iowa.

Nature of Viruses.

READING General Smuts's presidential address to the British Association and listening to a sectional discussion on the nature of viruses, I have been wondering whether the following will satisfy those who find great difficulty in believing that things so small as the agent of foot-and-mouth disease or the bacteriophage can be live organisms. General Smuts says that the world is constructed on a biological plan, that it is made up of events, and that matter, life, and mind are three grades of the same thing. In common thought, inorganic phenomena are quantitatively related to the matter with which they are associated. Mind, on the other hand, has no perceptible quantitative relations. Life would be generally considered to be quantitatively related either (by the mechanist) to living matter or (by the organicist) to living organisms. But need this always be so? Cannot life sometimes or partly be more like mind, so that the events of life are quantitatively out of proportion to the perceptible matter involved? A. E. BOYCOTT.

Radlett, Oct. 4.

Spin of Light Quanta.

[BY RADIOGRAM.]

THE possession by photons of intrinsic angular momentum is further strikingly confirmed by investigations of light scattering in liquids. The spin theory indicates diminution of rotational scattering to half the value given by classical considerations. Spectroscopic measurements of the depolarisation of scattered light, using broad and fine slits, furnish a convenient test. Observations by Venkateswaran with carbon disulphide and benzene liquids quantitatively confirm the predicted results.

C. V. RAMAN.
S. BHAGAVANTAM.

210 Bowbazar Street,
Calcutta, Oct. 12.

Research Items.

Were-Tigers in India.—In *Man* for October, Mr. L. A. Cammiade points out that in an article appearing recently in the *Illustrated London News*, and describing the killing of a man-eating tiger which had been responsible for the deaths of more than thirty people, there are indications that, all unknown to the hunter, Mr. L. Handley, the tiger he was hunting was, in the opinion of the jungle people, the Gonds, a man who by magical means had assumed that form for nefarious ends. Thus, although the Gonds are normally quite unafraid of tigers, they deserted Mr. Handley when he was approaching the place in the jungle where the body of a woman was being devoured. They would not follow the blood trail except in a crowd and with the beating of drums and letting off of fireworks. This would have frustrated his object had the tiger been an ordinary beast; but is intelligible as against witchcraft. In one case the body was placed in a tree after recovery; but this is a practice of the Koi and Konda Reddis, neighbouring jungle tribes, in similar cases. For some unknown reason the body must not rest on the ground. Mr. Handley would not be aware of the Gonds' belief, as to voice their suspicion above a whisper, even among themselves, might be followed by murder. Mr. Cammiade also points out that the deities "who have to be propitiated after the killing of the tiger", according to Mr. Handley's account, judging from their appearance, are not tiger deities but more probably the images of notable ancestors to whom an offering of thanksgiving was made for delivery from the were-tiger. Tiger deities would have to be propitiated, and consent obtained before any action was taken; while it is customary to ask the ancestors for protection on a journey through the jungle and for success in disposing of merchandise at its end.

Oceanic Bird Wanderers to Ceylon.—Under this title W. E. Wait describes twenty species of birds which appear casually in the neighbourhood of Ceylon (*Spolia Zeylanica*, vol. 16, p. 181, Aug. 1931). They comprise three species of terns, three frigate-birds, one booby, tropic bird, shearwater, and stormy-petrel, all of which may be said to frequent the tropical waters of the Indian Ocean; two gulls which are casual wanderers; and rare stragglers, including a shearwater and booby from the temperate South Indian Ocean, two skuas and a petrel from the Antarctic, and a petrel, a stormy-petrel, and probably a skua from the Northern Pacific Ocean. It seems likely that the location of Ceylon at the focus of many important steamer routes may have some bearing upon the appearance of these wandering birds, since it is known that many oceanic birds follow ships for long distances at sea.

A Supposed Primitive Gill-less Nudibranch.—A new family (Okadaiidæ) has been created by Kikutaro Baba for a gill-less holohepatic nudibranch, *Okadaia elegans*, described by him a year ago (*Annot. Zool. Japon.*, vol. 13, p. 63; 1931.) The new description deals in detail with the internal organisation of the sole species of the genus, which has been found at several localities on the Japanese coast. Until now, the only known gill-less dorid, of the tribe Holohepatica, has been *Doridoxa*, and while the main characters of *Okadaia* show that it belongs to the Holohepatica, there are others which recall in some degree the Cladohepatic series. The most striking structural characteristic described is the absence of a heart, for the sections revealed no trace of a proper pulsatory centre or the heart and pericardium comparable with those in other nudibranchs. Sir Charles Eliot was of opinion that the absence of gills had little effect on the rest of the organisation; this unique case of the correlated disappearance of branchiæ and the proper circulatory centre is regarded by the author as "the most primitive or retrogressive form" (surely not the same thing!) amongst the dorids.

Nature of Golgi Substance.—In Papers from the Tortugas Laboratory of the Carnegie Institution of Washington, vol. 27, is an interesting publication by D. H. Tennant, M. S. Gardiner, and D. E. Smith, on a cytological and biochemical study of the ovaries of the sea urchin, *Echinometra lucunter*. A new method is introduced into the study of cell inclusions by examining them both as structures and as substances. The tissues were subjected to the usual fixing and staining techniques, and at the same time the tissues were analysed and separated into their component substances. These substances were then subjected to the same fixation and staining techniques as the tissues themselves. As a result, the authors come to the conclusion that neither Golgi bodies nor chondriosomes are constant structural elements in the cellular architecture, but that both are the chemical products of physiological processes. It would seem that fatty acids of the oleic series meet the requirements of a Golgi substance in their reaction with osmium tetroxide. In the same report, observations are given on the formation of the egg in *Echinometra lucunter*, by R. A. Miller and H. B. Smith.

Rust Diseases of Cereal Crops.—A very useful summary of the present state of our knowledge of the commoner rust diseases of cereals is contained in the presidential address by Sir R. H. Biffen to the British Mycological Society (*Trans. Brit. Mycol. Soc.*, vol. 16, pt. 1, pp. 19-37). The strains and biologic forms of *Puccinia graminis* and other rusts are discussed in concise, though masterly fashion, and the recent work by Stakman and his colleagues on the crossing of forms is reviewed. Methods of control are dealt with at great length and include a historical as well as a modern presentation. Many factors which affect the incidence of epidemics are described, and the value of the removal of the ascidial hosts of such species as have them is discussed. The most valuable part, however, is the collection of recent results of work on the breeding of wheat resistant to the rust. The academic mycologist and the applied biologist will both find matter of interest in this account.

Geological Map of Finland.—Nearly twenty years have elapsed since the publication of the last edition of a general map of the ancient rocks of Finland. A new edition was prepared in 1925 for inclusion in the "Atlas of Finland", and this, with a few minor changes, is now published with *Bull.* 91 of the Commission géologique de Finlande. The Bulletin is a beautifully illustrated general account of the Pre-Quaternary rocks by Prof. J. J. Sederholm. The Pre-Cambrian rocks formerly known as Jatulian, Kalevian, and Ladogian are provisionally united under the common name *Karelian*. The levelled-down mountain chains which originated when these formations were folded are called *Karelides*. Their geotectonic study has already shown that they possess a structure analogous to that of the Alps. Mountain chains of a still more remote period are found from middle Sweden through southern Finland and these are designated *Sveco-Fennides*. It is shown that during the Pre-Cambrian, periods of erosion and quiet sedimentation were several times interrupted by epochs of orogenic folding and intrusion of granite, and it is thought probable that in these early ages, as since, mountain-building has been restricted to narrow belts of the earth's crust.

Absorption Spectra of Complex Salts.—A paper by Dr. R. Samuel in a recent issue of the *Zeitschrift für Physik* (vol. 70, No. 1) contains much new information on the absorption spectra of complex salts of iron, cobalt, nickel, palladium, and platinum. The measurements have been made both in the visible region and well into the ultra-violet (2300 A.), the progress towards shorter wave-lengths being due to an improvement in technique, involving the use of the continuous spectrum of hydrogen as a light source. Graphs are given showing the variation of absorption coefficient with wave-length for twenty-one substances. The chief interest in these experiments is in their possible bearing on the structure of the complex radicles. Dr. Samuel records a number of apparent regularities, such as the existence of three similarly situated bands in the spectra of the diamagnetic cyanides and the appearance of a fourth band at greater wave-length with paramagnetic cyanides. His work, although extensive, is, however, definitely of a preliminary nature. The physics department of the Muslim University of Aligarh, of which Dr. Samuel is the head, is being reorganised and equipped to provide facilities for research in molecular physics, and he is continuing his work there.

Explosions in Closed Cylinders.—There is a pronounced increase in the speed of propagation of flame in a gas mixture in a tube open at both ends when a number of restricting brass rings are arranged within the tube. This effect, observed by Chapman and Wheeler in 1926, has been shown by Kirkby and Wheeler, in the *Journal of the Chemical Society* for September, to apply also to a closed tube. In explanation of the general effect of restrictions, it has been shown that during the early stages of an explosion in a tube the unburnt mixture ahead of the flame front moves as a current and the speed of the flame depends on the speed of the current. When the restricting rings are close enough together, the tongue of flame issuing from the first ring passes through the second and initiates combustion of the gas beyond while the portion between the two rings is still burning. The combustion of a comparatively large volume of gas is thus almost instantaneous. The effect occurs also in closed tubes, the sudden expansion resulting from the almost simultaneous combustion of the gas in each compartment producing a shock wave.

Rhenium Compounds.—The preparation of rhenium tetrachloride, and the rhenichlorides of potassium and silver, is described by Briscoe, Robinson, and Stoddart in the September number of the *Journal of the Chemical Society*. Precipitates of insoluble rhenichlorides were also obtained with mercurous and thallous salts. The primary product of treating the metal in chlorine is the tetrachloride, $ReCl_4$, which has not previously been observed. No evidence of the existence of the reported hexa- and hepta-chlorides was obtained, although the black crystalline tetrachloride was always accompanied by traces of a brown, well-crystallised substance, melting sharply at about 21°, obtained in quantities insufficient for analysis. No evidence of the formation of a lower chloride was obtained when rhenium was treated in dry hydrogen chloride up to 900°, the metal being unattacked. Potassium rhenichloride, K_2ReCl_6, was obtained in green crystals by heating a mixture of rhenium and potassium chloride in chlorine and crystallising from cold water. The silver salt, Ag_2ReCl_6, was obtained as an orange precipitate on adding a slight excess of silver nitrate to an aqueous solution of potassium rhenichloride. It is not obviously crystalline. Preliminary experiments on the behaviour of metallic rhenium when heated with bromine and iodine confirmed the observations of Noddack: the products are regarded as tetrahalides.

Astronomical Topics.

Meteoric Hypothesis for the Origin of the Solar System.—The Abbé Moreux contributes articles on cosmogony to *Scientia* for September and October. Instead of invoking the approach of another star to the sun, he supposes the sun to have traversed a region of space full of dust or meteoric matter, such as are indicated by the dark 'Horse's Head' in Orion and a number of similar dark patches. The orbits of such particles about the sun would in general be hyperbolic, but he suggests that by collisions and other mutual interference a considerable part of the matter might be captured by the sun. He traces its subsequent behaviour in some detail, and attempts an explanation of Bode's law of planetary distances. He notes that the planes of the giant planets are alternately on opposite sides of the invariable plane of the system; he draws an argument from this in support of his system. But the planes are in a state of constant change, and it would seem that proof is required that the relation now existing is permanent before any cosmogonic argument can be drawn from it. In his discussion of Neptune's system he assumes that Neptune's rotation is retrograde, like the motion of the satellite. The observations of J. H. Moore and D. H. Menzel at the Lick Observatory in 1928 showed that the rotation is direct. In such a difficult subject as cosmogony, it is well that all possible suggestions should be examined. It is unlikely that any one of the suggested systems is completely true.

Repetition of the Michelson-Morley Experiment.—Six years ago, Prof. D. C. Miller announced the results of a series of experiments that he made on Mt. Wilson; these seemed to indicate a variation of some 9 km./sec. in the course of the year, which he ascribed to a drift of the solar system in a direction nearly normal to the ecliptic. In the "Encyclopædia Britannica" (14th edition, vol. 15, p. 418) are described three series of experiments, by Kennedy and Illingworth, by Piccard and Stahel, and by Michelson, Pease, and Pearson, all made between 1927 and 1929, and all giving a zero effect like the original experiment.

The *Scientific American* for October gives details of a later series of experiments carried out by Prof. G. Joos, of the University of Jena, using apparatus constructed by the Zeiss works. The path of the light in the apparatus is brought up to 70 feet by repeated reflections. The results are recorded photographically and they are stated to preclude any ether drift exceeding one mile per second, so that it may be assumed to be zero. The large cross of the apparatus is constructed of quartz, for the double reason of its low coefficient of expansion and its freedom from magnetic effects. The cross is suspended by 700 springs of piano-wire, so as to support every part equally and prevent torsion. There is thus a great majority of experiments that indicate a zero effect of ether drift.

Africa in Transition.*

DURING the last fifty years, Africa has passed from the age of the pioneer and of geographical exploration, and has become a mosaic of more or less organised territories, controlled by European Powers. But although Africa has made such an advance that Nairobi, where forty years ago no vestige of human life was to be seen, is now a modern city with hotels and cinemas, in the less accessible parts, off the rail and main roads, tribes still exist as we knew them long ago: men and women nude, the only instrument of agriculture a pointed stick, and money in the form of coin still unknown. What has been the effect of the impact of the twentieth century on these relics of the stone age? This is the field of the new exploration of Africa, the field in which we need the assistance of the practical and constructive anthropologist.

As contact with modern civilisation must have a profoundly disruptive effect on the social organisation of a primitive society, and this effect is a continuing process, it is, therefore, essential for the anthropologist, administrator, and educator, in endeavouring to build foundations capable of bearing a permanent superstructure, to remember that they are dealing with a people in a period of social and intellectual transition. In other words, scientific research, in so far as it is concerned with practical work, must be directed to the African of the future rather than the African of the past or even as he is to-day, though an apprehension of both is essential for an understanding of the tendencies of the future.

Great changes have been brought about by the introduction of law and order and the suppression of slave-raiding. The concentration of the use of force into the hands of a central authority has *pro tanto* undermined the power and authority of the chief, and deprived the youth of Africa of its normal occupation of fighting and military service; while the suppression of war and slave-raiding has broken down the isolation of communities, so that the individual is able to offer his services as a wage labourer hundreds of miles away from his home. He has become acquainted with the conditions of life in an urban native quarter, and has acquired the command of money. This has also assisted in the spread of disease.

These are the inevitable effects of the white man's rule. When we come to the methods of applying that rule, there are two principles, generally known as assimilation and association.

The theory of assimilation would assign age-long tradition, customs, and beliefs to the scrap heap and substitute for them the modern forms of democracy, making the African a black European or black American. This system has been adopted by France in West Africa, but a growing body of opinion in France doubts the wisdom of this policy. An analogous system, though stopping short of miscegenation and conscription, known as 'direct rule', has been adopted widely in British dependencies. It is the system of which the results are now to be seen in India.

The alternative system of association works by devolution to native communities of the management of their own affairs, subject to the supreme authority of the governor and the law of the land. It recognises native institutions as the basis on which to build, and aims at their progressive improvement and adaptation to the changing conditions of Africa. Which of the two systems shall Africa in transition follow? The answer brooks no delay. Great Britain must choose between the two systems, and on that choice the future of Africa depends. One thing is certain, that with the spread of education the African will not remain inarticulate.

Contact with the white man brings the African face to face at once with the fundamental conflict of conceptions between the communal outlook, upon which his life and actions have been framed, and the individualism taught by his dealing with the white man. His employer tells him his wages depend upon his individual exertions; the missionary tells him to obey the laws of God if he would save his own soul, and that tribal expiation will avail him nothing. These are new conceptions; for his dominant characteristics are identification with his clan and belief in the presence and potency of the ancestral spirits of the tribe. Missionaries of long African experience believe that it is possible to engraft pagan rites on Christianity as they were in the early Church, while the Standing Advisory Committee of the Colonial Office has declared that what is good in the old sanctions and beliefs should be strengthened and retained. It is a problem that cannot be ignored. The new policy, too, in secular education will have a profound effect on the future of Africa.

The economic development of the resources of Africa is having an effect on African life, which is receiving intensive study by the International Institute of African Languages and Cultures. The access of wealth which has followed the opening up of Africa by road and rail, making possible the export of additional material produced by the extension and improvement of methods in agriculture, has greatly raised the standard of life.

It lies with us to see that the conclusions of science are rightly applied, and that the transition of the African from a lower to a higher standard of life and civilisation may be guided by a well-considered policy and scientific study, not only of the African as he used to be, but as he is to-day, and more especially of the tendencies which will operate to change him still more to-morrow.

* Substance of a lecture by Lord Lugard, delivered on Sept. 24 at a joint meeting of Sections E (Geography) and H (Anthropology) of the British Association.

Control of Finance.

AT the recent meeting of the British Association, Mr. P. Barratt Whale read a paper to Section F (Economic Science and Statistics) on the Macmillan Committee's Report on Finance and Industry. This Report, he said, is as important to-day as when first printed, despite the crisis that has since occurred. The country no doubt will come through this crisis, and once more it will be necessary to operate the gold standard system, though it is desirable that this should be done on lines more satisfactory than in the immediate past. In the future, therefore, the Macmillan Report will be looked to for guidance.

The keynote of that Report is that currencies should be managed from the point of view of stabilisation of prices, and this is in accordance with the general trend of modern ideas in monetary science. It is, however, in marked contrast with the views which prevailed during the nineteenth century, when it was generally held that the gold value of the currency should remain unchanged. Now the main aim is to secure that the currency represents a definite standard of value in terms of its purchasing power, so that the price level can be stabilised. Stabilisation, however, may mean one of two things, and it will therefore be necessary to decide on the particular kind of stabilisation which is most desirable. Either the level of commodity prices as measured by the wholesale index number can be stabilised, or allowances can be made for changes in productive conditions which may imply variations in general prices not needed to be corrected by a process of stabilisation.

The first type of arrangement aims at complete stability in commodity prices, while the second would allow some variation in prices, so far as that variation should be attributed to general changes in productive conditions. Mr. Whale considers that the Macmillan Committee was justified in shirking a choice between these two methods, but a choice will have to be made and it is important for economists to determine what kind of stabilisation is most desirable. Currency systems will require to be managed by the central banks with much greater freedom than hitherto.

Restrictions which have existed in the past have proved harmful, since they have generally operated to lock up gold. It is not enough for the central banks to control the domestic situation only, as against the influences of the other banks in the country ; the gold standard also implies that the currencies of the different countries should move together and have a closely related balance. There should thus be common aims and a common policy for all the central banks of the world. The Report took the view that the existing unequal distribution of the world's gold supply is not a cause but rather the result of causes—namely, the definite intention of France and the United States to accumulate gold. But there is another view which the Committee did not put forward, and that is that the accumulations of gold might not have been deliberately acquired by these countries but might have flowed as a result of the balance of international payments.

On this view the fundamental cause was the unbalanced state of indebtedness. The remedy, if this be the cause, is that France and the United States should either lend more abroad or should import more goods. Mr. Whale considers that the Committee has failed to give sufficient emphasis to the importance of price adjustments in balancing indebtedness. Both France and the United States have refused to allow a rise in their internal price levels, but if this had been permitted it would have facilitated imports into these countries from the rest of the world.

The Macmillan Report represents a combination of the ideas of the currency school with those of the banking school. The latter insists on the need for elasticity in currency arrangements, while the former emphasises the necessity of strict regulation of currency. Control, however, should be exercised by the central banks, so as to secure stability in the price level. The Report is on the right lines, but it is clear that in many respects the ideas of the Committee require to be worked out in detail, and some of its fundamental assumptions might be challenged.

The Unit of Atomic Weight.

DISCUSSING the unit of atomic weight before Section A (Physics) at the meeting of the British Association in London on Sept. 28, Dr. F. W. Aston contrasted the point of view of the physicist and the chemist. The painstaking research in recent years to determine whether the atomic weights of complex elements vary with their origin and to effect the separation of isotopes has proved conclusively that for practical purposes variation in Nature is negligible, and has justified the decision of the Committee of the International Union of Chemistry to retain the old relative meaning of the words ' element ' and ' atomic weight '. There is little reason to alter the present unit of atomic weight, $O = 16$, which has figured so long in chemical literature.

The physicist, on the other hand, is interested in the actual weights of the atoms, and it is only in the last twenty years that any comparison of the actual weights of the individual atoms has been possible. The original parabola method of Sir J. J. Thomson was followed by the first mass-spectrograph, in 1919, with a resolving power of 1 in 130 and an accuracy of about 0·1 per cent. With few exceptions, atomic weights deduced from mass-spectra agreed well with those generally accepted. The second mass-spectrograph, built in 1925, with a resolving power of 1 in 500 and an accuracy attaining 0·01 per cent, permitted the calculation of the percentage deviation of weight of an atom from a whole number on the scale of $O^{16} = 16$. During the last three years a valuable check on chemical calculations has been provided in a photometric method of calculating the relative abundance of isotopes from the intensity of their lines on mass spectra, which has been used to determine the mean atomic weights of complex elements.

After discussing Giauque and Johnston's announcement that observations on oxygen band spectra indicate the presence of O^{17} and O^{18}, requiring alternatives to oxygen as our standard, Dr. Aston stated that he is in favour of retaining the present chemical scale unaltered. For most practical operations the chemist only requires a list of numbers, accepted internationally and guaranteed to within 0·1 per cent of the true atomic weights. For the fundamental requirements of physics the neutral atom of oxygen 16 appears to be the best standard, being the most convenient for work with band spectra and mass spectra. The 0·01 to 0·02 per cent difference in the two scales is not serious, and the meaning underlying the chemical scale differs so completely from that underlying the physical that confusion can easily be avoided by speaking of ' the atomic weight of chlorine ' on one hand and ' the weight of the atom of chlorine 35 ' on the other.

The Greenland Cod.

IN the recently published report of the North-western Area Committee of the International Council for the Exploration of the Sea,[1] much interesting and valuable information is brought together on the natural history and biology of the Greenland cod (*Gadus callarias*). This fish appears every year in Greenland waters, but its numbers are subject to most violent and sudden fluctuations.

About the year 1820, the date of the earliest available records, cod were present in enormous numbers in the Julianehaab district, and even so far north as Disco Bay. Thereafter they disappeared from all but certain fiords and sounds in the vicinity of Sarfânguak and Fiskenaesset in the south-west for roughly a quarter of a century. During the period 1845–49 they were back again in large numbers, but by the end of 1850 scarcely a fish remained. During the second half of the nineteenth century and the first two decades of the present century, cod continued to be scarce in Greenland waters. More recently, however, they have once more reappeared in enormous numbers, and in the year 1929 were again numerous so far north as Disco Bay.

Besides this recent enormous increase in the stock of cod, other changes also have taken place. Previously, in their lean years, the cod approached the coast only in the summer and autumn months, whereas now they come inshore and enter the fiords as early as March or April.

This great change in habit and increase in the stock of cod in recent years is not the only indication of fundamental changes in the animal life of the waters of south-west Greenland. Shoals of coalfish (*Gadus virens*) have also frequently been seen with the cod in various districts since 1924. The herring, too, a fish practically unknown around Greenland in normal times, has recently appeared in large numbers; and even haddock, not previously recorded from its waters, have been taken.

The climate and other natural conditions at south-west Greenland have also been quite out of the ordinary. The winters have been unusually mild, and the western ice has had a much more restricted distribution than formerly. The causes underlying the changes in the animal life of the area must probably be sought in the abnormal conditions in their environment, but sufficient data are not yet available for linking up cause and effect. There is of course every reason to expect a swing-back to normal conditions at any time; but if this reversion does not take place too rapidly, there is every prospect of a good cod fishery continuing off south-west Greenland at any rate until 1936. G. A. S.

[1] "Rapports et Procès-Verbaux des Réunions," vol. 72, April, 1931.

The Fortin Barometer.

WHILE a barometer of the Fortin pattern is generally regarded as a better laboratory instrument than one of the Kew pattern, many observers prefer the latter, owing to the difficulty found in setting the mercury accurately to the pointer in the cistern of the Fortin barometer. If the illumination is good and the mercury clean, this setting provides no difficulty; but barometers are often of necessity set up in positions of poor illumination and the mercury surface becomes dull after a few years of use, so that accurate setting of the mercury to the pointer is rendered difficult.

Messrs. C. F. Casella and Co., Ltd., of Regent House, Fitzroy Square, London, W.1, have recently brought out a new pattern of Fortin barometer designed to overcome these difficulties. The object aimed at appears to have been achieved in a most satisfactory manner. The usual glass cistern is replaced by a cast-iron one, having a horizontal extension at one side, in which a stainless steel pointer is rigidly fixed. In the front of the extension is a small telescope directed down at an angle towards the pointer, and on the back a plane glass window. Behind the window a white illuminated surface is provided. With clean mercury the view through the telescope shows a clear-cut impression, on a magnified scale, of the pointer facing its image, and the two can be brought into contact by using the adjusting screw, which raises the level of the mercury in the cistern of the barometer, with great confidence and precision. In order that the mercury surface may not become contaminated, all air entering or leaving the cistern has to pass through a small filter, which is said to be effective even in the atmosphere of a chemical laboratory. This does not end the novel features of the instrument, as the horizontal chamber terminates in a glass window held in position by a screwed ring. If the mercury surface is lowered and this ring with the window removed, the inside of the cistern can be cleaned and, if desired, the mercury pumped out of the cistern and fresh mercury inserted.

Owing to the pointer being at some distance from the vertical axis of the barometer, it is of special importance that the instrument should be set up truly vertical. The makers point out that verticality is easily ensured by rotating the barometer in its mounting and noting that the pointer remains in contact with the mercury surface in all positions. The design undoubtedly marks an important step forward in the construction of Fortin barometers.

University and Educational Intelligence.

CAMBRIDGE.—The following have been elected to fellowships at Trinity College: Prof. G. H. Hardy, Sadleirian professor of pure mathematics in the University (professorial fellowship); Mr. L. C. Young; and Mr. H. S. M. Coxeter.

Mr. A. H. Chapman (St. Catharine's) has been appointed official secretary to the Department of Engineering.

The Regent House has passed a grace to the effect that the title of Professor Emeritus be conferred on Sir R. H. Biffen upon his retirement from the professorship of agricultural botany.

The electors to the Frank Smart studentship in botany give notice that the studentship is now vacant. Any graduate of the University is eligible, provided that not more than fourteen complete terms have elapsed after his first term of residence. Women are also eligible. The successful candidate must devote himself to research in botany under the direction of the professor of botany. The value of the studentship is £200 per annum. Applications must reach Prof. A. C. Seward, at the Botany School, on or before Oct. 24.

FOR the forthcoming general parliamentary election, the Universities of Cambridge, Oxford, and Belfast, and the Scottish Universities, are returning unopposed members, as follows: Cambridge, Sir John Withers and Mr. G. H. A. Wilson; Oxford, the Right Hon. Lord Hugh Cecil and Sir Charles Oman; Belfast, Col. T. Sinclair; Scottish Universities, Mr. John Buchan, Dr. D. M. Cowan, and Mr. A. Noel Skelton. On the other hand, the Universities of London and Wales and the Combined English Universities are to have a poll. The following are the candidates for election: London (1 member), Sir E. Graham-Little and Major A. G. Church; Wales (1 member), Mr. E. Evans and Mr. S. Lewis; Combined English Universities (2 members), Miss E. Rathbone, Sir R. Craddock, Mr. H. G. Williams, Sir William Jowitt, and Mr. H. Nicolson.

FROM the United States Office of Education we have received two bulletins of more than ordinary interest. The first, "National Ministries of Education" (pp. 158, price 25 cents), is a description, supported by statistical tables and a bibliography, of the status, functions, and activities of the ministries of education in fifty-five of the seventy-three main political divisions of the world : the remaining eighteen, including Germany, the U.S.S.R., the United States, India, Brazil, Australia, and Canada, are excluded because their ministries of education, where they have any, are less than national in their legal and functional characteristics. Under the heading " General Characteristics " are dissertations on the internal organisation of the ministry in Italy, Belgium, Persia, and Uruguay, and organisations advisory to the ministry in Hungary, Belgium, Spain, England, New Zealand, and Bulgaria. In a chapter on the relations of the ministry to secondary education, the systems in force in Spain, Portugal, and Rumania are described as typical of the twenty-five Latin language countries studied, those of Yugoslavia, Denmark, and Hungary for the Germanic type, and those of the Irish Free State, New Zealand, and England and Wales for the English language countries. Special chapters are devoted to the Board of Education of England and Wales and the ministries of France, Belgium, and Mexico. The other bulletin, on " Accredited Higher Institutions ", is a compilation of the lists of institutions of college grade accredited or approved or classified by recognised State or voluntary agencies, including national professional organisations, with descriptions of standard criteria used.

Birthdays and Research Centres.

Oct. 26, 1874.—Prof. T. M. Lowry, C.B.E., F.R.S., professor of physical chemistry in the University of Cambridge.

I am interested in problems of valency, especially in the compounds of nitrogen, phosphorus, sulphur, and tellurium, and in the study of molecular structure by physical methods. Optical and spectroscopic methods have been used extensively for this purpose ; but researches on photochemistry, the diffraction of electrons, the scattering of molecular rays, electrode potentials, electrolytic conductivity, and lubrication are also in progress in the Laboratory of Physical Chemistry at Cambridge. In polarimetry, the study of optical rotatory power has been extended to vapours, and measurements are now being made of the form of the dispersion curves in the region of absorption within which the Cotton phenomenon is observed.

Oct. 27, 1856.—Prof. E. W. Hobson, F.R.S., formerly Sadleirian professor of pure mathematics in the University of Cambridge.

It is very improbable that, at my age, I shall be able to take up investigation of any new subject. I hope, however, to be able to fill up some gaps in subjects such as the theory of integral equations, and possibly in the calculus of variations ; subjects at which I have worked in past years.

Oct. 27, 1894.—Prof. Lennard-Jones, Melville Wills professor of theoretical physics in the University of Bristol.

The theoretical researches in the Wills Physical Laboratory, Bristol, centre round problems of cohesion and molecular structure. The main investigations, in which we are now engaged, aim at (1) a detailed knowledge of the electronic structure of certain atoms and molecules, (2) a calculation of the cohesive forces between atoms in a molecule, (3) a correlation of certain properties of gases and solids with the electronic structure of the atoms and molecules of which they are composed.

Societies and Academies.

London.

Society of Public Analysts, Oct. 7—**J. Cecil Maby**: The identification of wood and wood charcoal fragments. The economic and forensic importance of distinguishing between different woods by their microscopical structure is discussed, and the value of the method in archæological investigations is illustrated by various examples, such as the identification of the nature of the wood in the piles of lake dwellings, and of the charcoal from ancient furnaces.—**T. Callan** and **N. Strafford** : The examination of dyed leathers in cases of alleged dermatitis. The possibility of applying to dyed leather the tests used by Cox for the detection of diamines and allied bodies in fur has been investigated. The tannins in leather may interfere with many of these tests, but four of them will enable definite conclusions to be drawn, provided that control tests are applied to portions of the extract from the leather, after the addition of very small amounts of a meta- and para-diamine respectively.—**W. L. Davies** : The determination of chlorides in dairy products and biological material. The advantages of a wet (nitric acid) method are described, and suggestions for obtaining a sharper end-point in the titration of the excess of silver nitrate are made.

Physical Society, Oct. 16.—**G. A. Wedgwood**: Young's modulus for steel in two directions in a bar. Experiments showed that the elastic constant E of the various steels from which the hollow cylinders used were made is the same in two directions at right angles, one direction being along the axis of the original bar and the other across a diameter.—**N. W. McLachlan**: On the effective mass of flexible discs and conical diaphragms used for sound reproduction. The effective mass of a circular aluminum disc vibrating in air is zero at the centre-stationary and centre-moving modes. At a centre-stationary mode the effective mass attains a positive maximum before the zero value and a negative maximum thereafter. From the shape of the curves for a disc it is possible to interpret those obtained for conical diaphragms. In the latter case the curves depend upon the apical angle of the cone.—**A. T. McKay**: Further study of diffusion for the infinite plane sheet. A method is developed whereby the diffusivity and surface constants can be evaluated from experimental data. In order to facilitate the practical application of the methods propounded to this and similar diffusion problems, tables have been specially calculated giving the first four roots of each of the four equations $\left.\begin{matrix}\tan\\ \cot\end{matrix}\right\} x = \pm x . \left.\begin{matrix}\tan\\ \cot\end{matrix}\right\} \lambda$.

Paris.

Academy of Sciences, Sept. 7.—**André Blondel**: New graphical solutions of calculations for electric cables transmitting at high voltage.—**E. Bataillon** and **Tchou Su** : The three types of mitosis characteristic of the first development in the egg of *Bombyx*, fertilised or parthenogenetic.—**R. Tremblot**: The application of (optical) interference to some problems of flow at high velocities. By means of the apparatus described in an earlier communication the author has measured the distribution of the densities in a Laval tuyère in order to decide between the conclusions of Prandtl, Steichen, and Stodola (rigorously adiabatic flow) and those of Müller (at constant heat). The curves obtained agree with the first hypothesis within one per cent.—**Jean Peltier**: The search for want of symmetry and faults in ferromagnetic test pieces. In the differential arrangement described and figured, any want of homogeneity in the test pieces is shown by the production of a note in a loud speaker.—**Maurice Billy** and **Félix Trombe**: The preparation of pure cerium. In various experiments the crucible holding the fused salt was carbon, porcelain, fused quartz, and fluorspar porcelain (Damiens). The best results were obtained with an apparatus consisting of a carbon crucible as anode, at the bottom of which was a quartz or fluorspar crucible. The cathode was a molybdenum cylinder. The metal contained 0·08 per cent of silicon, and no other impurity could be detected.—**P. Fallot**: The palæozoic massif of Talambot (Spanish Riff).—**Michel Polonovski** and **René Hazard**: Some physiological effects of chlorotropane. The esterification of tropanol by hydrochloric acid increases the toxic power of this alcohol: it reinforces the paralysing action of the latter on the cardiac vagus, and gives to the action of the molecule on the intestine a resemblance to that produced by nicotine.—**P. Cappe de Baillon**: The experimental bipartition of the odd pieces of the exoskeleton in *Tenebrio molitor*.—**René Wurmser** and **Louis Rapkine**: A method of quantitative micro-injection.

Geneva.

Society of Physics and Natural History, June 18.—**Paul Rossier**: (1) An astrophysical formula. A classical relation between the absolute magnitude, the radius, and the temperature of a star necessitates a term based on a simplifying hypothesis concerning the spectral sensibility of the eye. Replacing this by a much more probable supposition, the author gives this term a closer approximation.—(2) The calculation of the apparent diameter of a star. The author replaces an old relation between the apparent magnitude, the apparent diameter, and the temperature of a star, an equation holding for an integral receptor only, by a new formula including the magnitude furnished by any receiver whatever. Although the numerical constants are furnished by the sun, the application to giant stars, the diameters of which have been measured with the interferometer, gives results agreeing with observation.—**W. H. Schopfer**: The study of the influence of yeast extracts and concentrates of B vitamins on the sexuality of a fungus. Concentrated extracts of yeast were added to a rigorously synthetic fungal medium. A clear action has been observed on the development and on the sexuality of the fungus utilised.—**A. Amstutz**: The petrographic character of Banks Island in Melanesia. The author has defined the petrographic character of Banks Islands, which form one of the numerous archipelagos scattered in the Pacific and are attached politically to the Franco-English condominium of the New Hebrides. These islands are essentially volcanic in origin and are constituted of augite labradorites.—**E. Joukowsky**: A levigator with a motionless liquid medium. The author describes an apparatus designed for the study of fine sediments. This allows the classification of the sediments into a considerable number of grades, according to the differences in the velocity of fall in water. The ultra-fine elements which remain in suspension are obtained without loss.—**A. Schidlof**: The application of wave mechanics to nuclear physics. With reference to a recent work of M. Terroux on the energy of the β-rays emitted by radium E, the author remarks that, contrary to the conclusions drawn by M. Terroux, the results of his experiments furnish a striking confirmation of the quantum theory of radioactivity.—**Georges Tiercy**: The systematic use of the rotation of the transit instrument. The idea of turning the transit instrument for all stars has been sometimes attributed to the German astronomer Schnauder (1891). The author shows that Plantamour, director of the Geneva Observatory, had applied this idea so far back as 1868; the credit of using this innovation systematically in the simplest possible manner appears to belong to the Padua Observatory (1878). Hence Schnauder was anticipated.

July 2.—**A. Borloz**: Liquation phenomena in a coinage bronze. The author has taken several samples from different points of an ingot of coinage bronze. The analyses show nearly perfect homogeneity, except for the upper portion, the jet, where there is some liquation, as shown by a concentration in the copper and a reduction in the proportion of tin.—**A. L. Perier**: Contribution to the study of the maxillofacial correlations. Is the alveolo-palatal sagittal development related to the upper facial height? Observations on two cranial series, Bushmen and Alpine, give a negative answer. In fact, the alveolo-palatal region is modelled by the dental arch, the potential of growth of which is quite independent of the other facial diameters.—**E. Galopin**: The use of the polarising microscope in the determination of organic substances. Organic substances generally, possessing very high double refraction, can only be studied optically in very minute crystals of the order of 0·01 mm. The author has recognised that special operations are necessary for the production of such crystals; he (with Marc Cramer) has studied the special media for microscopical preparation. The measurement of the double refraction is made by a Berek compensator with convergent light on plates fixed parallel to the principal sections of the mineral studied.—**J. J. Pittard**: The bitumen of the Urgonian limestone and the molasse of Pyrimont (Ain). The author shows by numerous analyses that the proportion of volatile products of the bitumen is much higher in the Urgonian limestone (69 per cent) than in the molassic grits superposed (55 per cent). This suggests impregnation *per ascensum*, the hydrocarbons coming from deep layers and showing increasing oxidation towards the top.

Rome.

Royal National Academy of the Lincei, April 12.—**A. Lo Surdo**: Thermionic valves with fall of potential across the grid. A thermionic valve is described in which the grid consists of a conductor of suitable form for passing a current through it. By adjusting the intensity and sense of this current the non-uniformity of behaviour of the various parts of the filament grid space may be diminished or enhanced. In this way it seems possible to increase markedly the amplifying power of thermionic valves.—**Q. Majorana**: Certain new facts ascertainable by means of ordinary photoelectric cells. Further observations on the negative electrification of the photoelectric metal of an ordinary photoelectric cell when red light impinges thereon are described.—**M. La Rosa**: New proof of the influence of motion of the source on the velocity of light; ballistic explanation of Miss Leavitt's law (2).—**A. Del Chiaro**: Homogeneous functions.—**F. Tricomi**: Further observations on the distribution of the baricentres of the plane sections of a body.—**G. Palozzi**: A characteristic property of Darboux's tangents.—**Giacinta Andruetto**: The intrinsic equations of elastic equilibrium. Using the methods of absolute differential calculus, Tonolo has established the intrinsic equations of anisotropic or isotropic elastic bodies, including the case when the elastic body is immersed in a space of constant curvature. All these equations may be derived by rapid calculations by applying modern vectorial methods to the known vectorial equations for the equilibrium of elastic media.—R. Calapso : Surfaces of the third degree (of Cayley's type) united to the point of a given surface.—R. Caccioppoli : The united elements of functional transformations : an observation on the problems of limiting values.—R. Serini : The integrals of the equations of propagation in one dimension.—G. M. Pugno : Contribution to the treatment of doubly hyperstatic elastic systems.—B. Novàkovà : Microphotometric measurements of the line H_a at the centre and at the edge of the sun.—R. Zoja : The distribution of the tensions in a solid with rectilinear axis and with rectangular transverse section of variable height.—C. Antoniani : The behaviour in an electric field of colloidal humic-mineral complexes. A colloidal complex, extracted from soil by means of 10 per cent sodium hydroxide solution and afterwards purified to remove clayey constituents, and containing 82·75 per cent of organic matter and small proportions of inorganic matter, was found to be electronegative and to have its isoelectric point at pH 7·4. When the complex was flocculated at the isoelectric point and brought back to the disperse state by addition of sodium hydroxide, it exhibited anodic migration under the influence of an electric field. As the concentration of the hydroxyl ions is increased, the velocity of this migration increases to a maximum and then gradually decreases to zero, cathodic migration ultimately ensuing.—G. Roberti : Hydrogenation of the nitrogen compounds contained in primary tar. At 350° C., under a pressure of 100 atmospheres, and in presence of cobalt sulphide as catalyst, the bases of primary tar give rise to hydrocarbons, the nitrogen being liberated as ammonia. Aniline yields cyclohexane, cyclohexene, and small amounts of hydrogenated diphenyls. Pyridine gives pentane, amylene, hexane, hexene, heptane, etc., whilst quinoline is converted into propylcyclohexane, propylcyclohexene, and other hydrocarbons.—M. Airoldi : Fossil Corallinaceæ from the Canaries (2). *Porolithon inaspectum* n. sp. and other forms are described.—G. Cannicci : Bacterial flora of certain fish. Bacteriological examination of cod, pilchards, and mullet caught in the Gulf of Naples shows that the cœlom contains only bacteria ; the gills contain mostly motile bacteria, with moulds and chromogenic micro-organisms in some cases. The pilchard is the richest in *B. proteus* and the cod in fusiform bacteria which liquefy gelatine. *B. vulgare* Hauser, which is able to degrade proteins with formation of toxic products, occurs most abundantly in the gills during the summer, and *B. proteus* is found only in small number during the autumn and winter.—Amelia Tonon : Structure of mulberry buds and their development (2).—R. Perotti : The struggle of cereals against *stretta*. Incorporation of finely divided wood or bone charcoal with the layer of soil pervaded by the root systems of chlorophytes of various families, including the Graminaceæ, results in diminished dispersion of water from the soil, the food reserves of which are utilised more efficiently.

Official Publications Received.

BRITISH.

Proceedings of the Royal Society of Edinburgh, Session 1930–1931. Vol. 51, Part 2, No. 17: The Ionizing Efficiency of Electronic Impacts in Air. By Dr. John Thomson. Pp. 127-141. 1s. 3d. Vol. 51, Part 2, No. 18: Relation between the Composition of Retortable Carbonaceous Minerals and their Yield of Crude Oil. By Prof. Henry Briggs. Pp. 142-149. 9d. (Edinburgh : Robert Grant and Son ; London : Williams and Norgate, Ltd.)

Transactions of the Royal Society of Edinburgh. Vol. 57, Part 1, No. 3: The Life-History and Cytology of *Didymium migripes* Fr. By Dr. Elsie J. Cadman. Pp. 93-142+5 plates. (Edinburgh: Robert Grant and Son ; London : Williams and Norgate, Ltd.) 8s.

Air Ministry : Aeronautical Research Committee: Reports and Memoranda. No. 1374 (E. 46—I.C.E. 789): Oxidation Characteristics of Fuel Vapours with regard to Engine Detonation. By Dr. E. Mardles. Pp. 27 +14 plates. (London : H.M. Stationery Office.) 1s. 6d. net.

The Royal Technical College, Glasgow. Calendar for the One Hundred and Thirty-eighth Session, 1931-1932. Pp. 448+xxx. (Glasgow.)

Transactions of the Leicester Literary and Philosophical Society, together with the Council's Report and the Reports of the Sections, 1930-31. Vol. 32. Pp. 78+9 plates. (Leicester.)

University of Manchester: Faculty of Technology. Prospectus of University Courses in the Municipal College of Technology, Manchester, Session 1931-32. Pp. 360. (Manchester.)

British Museum (Natural History). Picture Post-Cards. Set F39: British Orchids, Series No. 7. 5 cards in colour. 1s. Set F40: British Orchids, Series No. 8. 5 cards in colour. 1s. Set F41 : British Orchids, Series No. 9. 5 cards in colour. 1s. Set F42 : British Flowering Plants. Series No. 13. 5 cards in colour. 1s. Set F43: British Flowering Plants, Series No. 14. 5 cards in colour. 1s. (London : British Museum (Natural History).)

Journal of the Indian Institute of Science. Vol. 14A, Part 3 : i. Cantharidin from *Mylabris pustulata* Fb., India, by B. H. Iyer and P. C. Guha ; ii. The Fatty Acids from Oil of Cantharis (*Mylabris pustulata* Fb., India), by B. H. Iyer and P. Ramaswami Ayyar. Pp. 31-45. 1 rupee. Vol. 14A, Part 4: i. Amylase from Rice; ii. Dialysis of some Cereal Amylases. By Vinayak Narayan Patwardhan and Dattatreya Vishnu Karmarkar. Pp. 47-57. 12 annas. (Bangalore.)

Department of Agriculture, Straits Settlements and Federated Malay States. General Series, No. 5: Report and Proceedings of the Second Inter-Departmental Agricultural Conference held at Kuala Lumpur, Federated Malay States, 27th October to 1st November 1930. Pp x+79. 1 dollar. Scientific Series, No. 6: Stem-Rot of the Oil Palm in Malaya. By A. Thompson. Pp. iii+23+7 plates. 50 cents. Scientific Series, No. 7: Two Citrus Fruit Borers, by H. T. Pagden ; The "Green Scale" of Coffee *Coccus* (*Lecanium*) *viridis* Green, by N. C. E. Miller. Pp. 29. 50 cents. (Kuala Lumpur.)

Hydrographic Publication H.D. 302: Report on the Tides, Currents and Tidal Streams in the Southern Part of Torres Strait. Prepared under the direction of Rear-Admiral H. P. Douglas. Pp. 16. (London : H.M. Stationery Office ; J. D. Potter.)

The Journal of the Institution of Electrical Engineers. Edited by P. F. Rowell. Vol. 69, No. 417, September. Pp. 1045-1188+xxvi. (London : E. and F. N. Spon, Ltd.) 10s. 6d.

India : Meteorological Department. Scientific Notes, Vol. 3, No. 26 : Some Statistical Relations of Temperature and Pressure in the Upper Atmosphere over Agra (1926-29) and Batavia (1910-15). By S. Gopal Rao. Pp. 83-88. 4 annas ; 5d. Scientific Notes, Vol. 3, No. 28 : Horizontal Gradients of Pressure and Temperature in the Upper Atmosphere over India calculated from Pilot Balloon Winds. By A. Narayanan. Pp. 115-120. 6 annas ; 8d. (Calcutta : Government of India Central Publication Branch.)

FOREIGN.

The Science Reports of the Tôhoku Imperial University, Sendai, Japan. First Series (Mathematics, Physics, Chemistry), Vol. 20, No. 3, July. Pp. 323-488. (Tokyo and Sendai : Maruzen Co., Ltd.)

Proceedings of the United States National Museum. Vol. 79, Art. 16 : Descriptions of a New Genus and Eight New Species of Ichneumon-Flies, with Taxonomic Notes. By C. F. Muesebeck. (No. 2882.) Pp. 16. (Washington, D.C. : Government Printing Office.)

Conseil International de Recherches : Union Géodésique et Géophysique Internationale : Section d'Hydrologie Scientifique. Bulletin N. 16 : Note e comunicazioni della Sezione nazionale italiana. Pp. 55. Bulletin N. 17 : Notes et comunications. Pp. 44+14 planches. Bulletin N. 18: Réunion plénière de la Section (Stockholm, Août 1930). Pp. 32. (Venezia.)

Bulletin of the American Museum of Natural History. Vol. 61, Art. 8 : Notes on Amphibians from Fukien, Hainan and other Parts of China. By Clifford H. Pope. Pp. 397-611+plates 13-22. (New York City.)

U.S. Department of Agriculture. Circular No. 176: Observations on the Satin Moth and its Natural Enemies in Central Europe. By R. C. Brown. Pp. 20. (Washington, D.C. : Government Printing Office.) 5 cents.

U.S. Department of Commerce : Bureau of Standards. Bureau of Standards Journal of Research. Vol. 7, No. 2, August, R.P. Nos. 339-347. (Washington, D.C. : Government Printing Office.)

Publications of the Allegheny Observatory of the University of Pittsburgh. Vol. 8, No. 3 : Wave Lengths in the Spectra of the Vacuum Copper Arc. By Keivin Burns and Prof. Francis M. Walters, Jr. Pp. 37-38. Vol. 8, No. 4: Wave Lengths in the Spectra of the Vacuum Iron Arc. By Keivin Burns and Prof. Francis M. Walters, Jr. Pp. 39-64. (Pittsburgh.)

U.S. Department of Commerce : Bureau of Standards. Research Paper No. 327 : Special Refractories for use at High Temperature. By Wm. H. Swanger and Frank R. Caldwell. Pp. 1131-1143. (Washington, D.C. : Government Printing Office.) 10 cents.

Smithsonian Miscellaneous Collections. Vol. 85, No. 4 : Mexican Mosses collected by Brother Arsène Brouard, III. By I. Thériot. (Publication 3122.) Pp. 44. (Washington, D.C. : Smithsonian Institution.)

Proceedings of the United States National Museum. Vol. 79, Art. 13 : Notes on and Descriptions of some American Moths. By Carl Heinrich. (No. 2879.) Pp. 16+7 plates. (Washington, D.C. : Government Printing Office.)

Transactions of the San Diego Society of Natural History. Vol. 6, No. 25 : Age of the Orbitoid-Bearing Eocene Limestone and Turritella Variata Zone of the Western Santa Ynez Range, California. By W. P. Woodring. Pp. 373-387. Vol. 6, No. 26 : A New Subspecies of Peromyscus from the Gulf Coast of Lower California, Mexico. By Laurence M. Huey. Pp. 389-390. (San Diego, Calif.)

Instituts scientifiques du Buitenzorg : "'s Lands Plantentuin ". Treubia : recueil de travaux zoologiques, hydrobiologiques et océanographiques. Vol. 13, Livraison 2, Août. Pp. 169-292. (Buitenzorg : Archipel Drukkerij.) 2.50 f.

Svenska Hydrografisk - Biologiska Kommissionens Fyrskeppsundersökning år 1930. Pp. 46. (Göteborg : Elanders Boktryckeri A.-B.)

Airgraphics. By Alexander McAdie. Pp. iii+37+7 plates. (Cambridge, Mass. : Blue Hill Observatory.)

Diary of Societies.

FRIDAY, OCTOBER 23.

ROYAL SOCIETY OF MEDICINE (Disease in Children Section), at 5.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir Arthur Keith: Demonstration of Specimens illustrating the Nature and Extent of the Changes which have Affected the Jaws of English People in Recent Centuries.

SOCIETY OF CHEMICAL INDUSTRY (Liverpool Section) (at Liverpool University), at 6.—Dr. W. Trantom: The Barium Industry.

INSTITUTION OF MECHANICAL ENGINEERS, at 6.—Lt.-Col. E. Kitson Clark: Presidential Address.

SOCIETY OF CHEMICAL INDUSTRY (Newcastle-upon-Tyne Section) (at Armstrong College, Newcastle-upon-Tyne), at 7.30.—Dr. J. T. Dunn: Chairman's Inaugural Address.

JUNIOR INSTITUTION OF ENGINEERS, at 7.30.—I. Fagelston: Some Instruments used in connexion with Power Plant.

ROYAL SOCIETY OF MEDICINE (Epidemiology and State Medicine Section), at 8.—Prof. Major Greenwood: The General Register Office (Presidential Address).

ROYAL AERONAUTICAL SOCIETY (at Merchant Taylors' School).—C. R. Fairey: Lecture.

MONDAY, OCTOBER 26.

ROYAL SOCIETY OF EDINBURGH, at 4.30.—Statutory Meeting.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—C. P. G. Wakeley: Demonstration of Specimens of Primary Bone Tumours.

INSTITUTION OF MECHANICAL ENGINEERS (Graduates' Section, London), at 6.45.—W. W. S. Robertson: The Manufacture of Wire.

INSTITUTION OF ELECTRICAL ENGINEERS (Informal Meeting), at 7.—Discussion: Is the Engineer given his Due Share in Management?

INSTITUTION OF ELECTRICAL ENGINEERS (North-Eastern Centre) (at Armstrong College, Newcastle-upon-Tyne), at 7.—A. G. Shearer: Chairman's Address.

INSTITUTION OF AUTOMOBILE ENGINEERS (Glasgow Centre) (at 39 Elmbank Crescent, Glasgow), at 7.30.—W. A. Tookey: The Internal Combustion Engine and its Performances (Presidential Address).

SOCIETY OF CHEMICAL INDUSTRY (Edinburgh and East of Scotland Section) (jointly with Institute of Chemistry—Edinburgh and East of Scotland Section) (at North British Station Hotel, Edinburgh), at 7.30. —W. R. Guy: Housing the Industrial Chemist.

ROYAL SOCIETY OF MEDICINE (Odontology Section), at 8.—E. B. Dowsett: Operative Procedure for Cysts of the Jaws.—J. G. Turner: (*a*) Backward Movement of Lower First Molars; (*b*) Difficulties of Extracting Retained and Unerupted Teeth.

TUESDAY, OCTOBER 27.

ELECTRICAL ASSOCIATION FOR WOMEN (at Lighting Service Bureau, 15 Savoy Street), at 3.—Mrs. Pender Chalmers: By Air to Baghdad, Babylon, and Ur (Lecture).

LONDON NATURAL HISTORY SOCIETY (at London School of Hygiene and Tropical Medicine) (Annual General Meetings of Sections).—Archæology, at 6; Botany, at 6.30; Entomology, at 7; Plant Galls, at 7.30; Ramblers, at 7.45; Ornithology, at 8.15.

INSTITUTION OF ELECTRICAL ENGINEERS (East Midland Sub-Centre) (at College of Technology, Leicester), at 6.45.—Dr. A. B. Everest: The Use of Nickel in the Electrical Industry.

INSTITUTION OF ELECTRICAL ENGINEERS (North Midland Centre) (at Hotel Metropole, Leeds), at 7.—R. G. Ward: Chairman's Address.

ROYAL PHOTOGRAPHIC SOCIETY, at 7.—Dr. E. Goldberg: Photoelectric Cells (Traill-Taylor Memorial Lecture).

QUEKETT MICROSCOPICAL CLUB, at 7.—Gossip Meeting.

INSTITUTION OF AUTOMOBILE ENGINEERS (Leeds Centre) (at Hotel Metropole, Leeds), at 7.15.—W. A. Tookey: The Internal Combustion Engine and its Performances (Presidential Address).

WEDNESDAY, OCTOBER 28.

BRITISH ASTRONOMICAL ASSOCIATION (at Sion College), at 5.

NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS (Graduate Section) (at Bolbec Hall, Newcastle-upon-Tyne), at 7.15.—L. C. Burrill and E. W. Fraser-Smith: Addresses.

INSTITUTION OF AUTOMOBILE ENGINEERS (Manchester Centre) (at Engineers' Club, Manchester), at 7.30.—W. A. Tookey: The Internal Combustion Engine and its Performances (Presidential Address).

BRITISH PSYCHOLOGICAL SOCIETY (Medical and Industrial Sections) (at Medical Society of London), at 8.30.—Papers on Accidents by Prof. Major Greenwood, Eric Farmer, and Dr. G. Groddeck.

INSTITUTION OF CHEMICAL ENGINEERS (Graduates' and Students' Section). —C. A. R. Stead: Crushing and Grinding with special reference to Ball Mills.

THURSDAY, OCTOBER 29.

ROYAL AERONAUTICAL SOCIETY (at Royal Society of Arts), at 6.30.—Capt. A. G. Lamplugh: Accidents in Civil Aviation.

BRITISH ASSOCIATION OF CHEMISTS (Notts and Derby Section) (at Technical College, Derby), at 7.—F. Scholefield: Vat Dyestuffs—their Application and Properties.

NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS (Tees-Side Branch—Graduate Section) (at Cleveland Scientific and Technical Institution, Middlesbrough), at 7.30.—F. S. Fletcher: Chairman's Address.

INSTITUTION OF ELECTRICAL ENGINEERS (Irish Centre—Dublin) (at Trinity College, Dublin), at 7.45.—Lt.-Col. H. E. O'Brien: Chairman's Address.

FRIDAY, OCTOBER 30.

ROYAL ASTRONOMICAL SOCIETY (Geophysical Discussion), at 4.30.—Isostasy. Speakers: Sir G. P. Lenox-Conyngham (who will communicate a paper by Dr. De Graaf Hunter), Prof. A. M. Davies, Dr. Harold Jeffreys, H. L. P. Jolly, Prof. O. T. Jones. Chairman: Admiral H. P. Douglas.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir Arthur Keith: Demonstration of Specimens illustrating the Anatomy of the Pyloric Sphincter in Cases of Hypertrophic Enlargement.

NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS (in Mining Institute, Newcastle-upon-Tyne), at 6.—Mrs. E. M. Smith-Keary: The Effect of Immersion on Propellers.

INSTITUTION OF ELECTRICAL ENGINEERS (London Students' Section), at 6.15.—G. J. Shaw and G. A. M. Hyde: The Business of Electrical Apparatus Manufacture.

INSTITUTION OF CHEMICAL ENGINEERS (at Institution of Civil Engineers), at 6.30.—Dr. E. F. Armstrong: Hydrogenation (Lecture).

ILLUMINATING ENGINEERING SOCIETY (at Royal Society of Arts), at 6.30.—P. Good and others: Discussion on Some Impressions of the I.I.C. Floodlighting.

INSTITUTION OF MECHANICAL ENGINEERS (East Midland Branch) (at University College, Nottingham), at 6.30.—Prof. C. H. Bulleid: The Importance of Metallurgy to the Engineer.

INSTITUTION OF MECHANICAL ENGINEERS (Informal Meeting), at 7.—R. Borlase Matthews and others: Discussion on Engineering in Agriculture.

IRON AND STEEL INSTITUTE (jointly with West of Scotland Iron and Steel Institute) (at Royal Technical College, Glasgow), at 7.15.—J. H. Andrew, W. R. Maddocks, D. Howat, and E. A. Fowler: The Equilibrium of Certain Non-Metallic Systems.—B. Matuschka: The Solidification and Crystallisation of Steel Ingots: the Influence of the Casting Temperature and the Undercooling Capacity of the Steel.

HULL ASSOCIATION OF ENGINEERS (at Municipal Technical College, Hull), at 7.15.—J. N. Waite: Combustion and Boiler-house Efficiency.

SOCIETY OF CHEMICAL INDUSTRY (South Wales Section) (at Technical College, Cardiff), at 7.30.—R. D. Owen: Chemistry of Dairy Products.

JUNIOR INSTITUTION OF ENGINEERS, at 7.30.—S. J. Clifton: Automatic Combustion Control.

ROYAL AERONAUTICAL SOCIETY (at Eton College).—The Master of Sempill: Lecture.

PUBLIC LECTURES.

SATURDAY, OCTOBER 24.

ROYAL INSTITUTE OF BRITISH ARCHITECTS, at 3.—A. S. G. Butler: Tendencies in Modern Buildings.

HORNIMAN MUSEUM (Forest Hill), at 3.30.—F. A. Rudd: The Legacy of Antiquity.

MONDAY, OCTOBER 26.

UNIVERSITY COLLEGE, at 5.30.—Prof. G. M. Trevelyan: The Call and Claims of Natural Beauty (Rickman Godlee Lecture).—K. de B. Codrington: Indian Sculpture.

UNIVERSITY OF LEEDS, at 8.—Prof. B. A. McSwiney: Our Daily Diet.

TUESDAY, OCTOBER 27.

UNIVERSITY OF BRISTOL, at 5.—Dr. A. L. Flemming: The Partnership between Anæsthesia and Surgery (Long Fox Lecture).

LONDON SCHOOL OF ECONOMICS, at 5.—Prof. E. Cammaerts: The Development of Belgian Culture.

UNIVERSITY COLLEGE HOSPITAL MEDICAL SCHOOL, at 5.15.—Prof. C. R. Harington: The Chemistry and Functions of the Thyroid Gland. (Succeeding Lecture on Nov. 3.)

KING'S COLLEGE, LONDON, at 5.30.—Sir Bernard Pares: Russian History to 1861: Break-up at Kiev: Migrations; the Tartars.

UNIVERSITY COLLEGE, at 6.30.—S. J. F. Philpott: Fluctuations in Human Output. (Succeeding Lectures on Nov. 3 and 10.)

WEDNESDAY, OCTOBER 28.

ROYAL INSTITUTE OF PUBLIC HEALTH, at 4.—Prof. F. J. Browne: The Health of the Expectant Mother.

THURSDAY, OCTOBER 29.

UNIVERSITY COLLEGE, at 2.30.—Miss Margaret A. Murray: Egyptian Sculpture.

SCIENCE MUSEUM, SOUTH KENSINGTON (in connexion with the Exhibition of Modern Technical and Artistic Glasses), at 4.45.—Dr. W. M. Hampton: Recent Progress in Optical Glass.

B.M.A., at 5.15.—Dr. F. J. McCann: The Prevention of Cancer (Chadwick Lecture).

KING'S COLLEGE, LONDON, at 5.30.—Dr. Eveline Martin: West Africa of To-day: Northern Nigeria; the Moslem Walled Towns of Kano, Zaria, and Katsina; Camels, Traders, and Travellers.

SATURDAY, OCTOBER 31.

ROYAL INSTITUTE OF BRITISH ARCHITECTS, at 3.—H. B. Creswell: The Uses of an Architect.

HORNIMAN MUSEUM (Forest Hill), at 3.30.—J. E. S. Dallas: A Naturalist in Western Cornwall.

ANNUAL MEETING.

OCTOBER 31 TO NOVEMBER 2.

INTERNATIONAL SOCIETY OF MEDICAL HYDROLOGY (at American Hotel, Amsterdam).—Discussions on the Influence of Chill in the Causation of Disease, opened by Profs. Schade and van Loghem, and on Factors in Marine Treatment, opened by Prof. Moll and Dr. Häberlin.

CONFERENCE.

NOVEMBER 16 TO 21.

INTERNATIONAL CONFERENCE ON BITUMINOUS COAL (at Carnegie Institute of Technology, Pittsburgh, U.S.A.).—Subjects for discussion:—Coal as Locomotive and Steamship Fuel; Coal in the Electric and Gas Industries; Competition of Coal with other Fuels in Heating Buildings; Coal in the Metallurgical Industries; Coal and its By-products in the Chemical Industries; Problems of Coking; Origin and Classification of Coals; Cleaning and Preparation; Mechanism of Combustion; Pulverised Coal; Relationships of Coal, Oil, and Natural Gas; Coal versus Water Power; Hydrogenation and Liquefaction of Coal; Fertilisers from Coal; Economics of Low Temperature Carbonisation; Storage and Weathering of Coal; Smoke Abatement; Stream Pollution.