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School and University Preparation for Productive Industry.

UNTIL recently there has been little attempt to bridge the gulf that separates the school life of a youth from his after-school life. This deficiency has been due in a large measure to a lack of sympathy and aloofness on the part of those responsible for his education and those who employ his services. Particularly has this been the case of education and industry. On one hand, educationists have tended to despise industry as sordid materialism, and, on the other, industrialists have had little use for the bookishness of education. Perhaps neither party has been to blame for its attitude, for the old-time habit of industry was to work for gain, and pure bookishness certainly was the characteristic of our educational system.

Fortunately, the outlook of both sides is altering and conceptions are widening. Educationists are agreed that " education is the art of making people at home in their environment," and industry is awakening to the consciousness that it has a greater responsibility to the community than the providing of necessities, conveniencies, and luxuries. Out of this mutual understanding is coming education for industry, so that the transition from school to work will in time be accomplished as easily as any natural development. Recently it has been suggested that a committee representative of the public schools and universities on one hand, and of industrialists on the other, should serve for the interchange of views on the subject of bringing the supply of the schools and the demands of industry nearer together. This would make for better and increased understanding between the two parties.

Whether it is best for a youth to enter industry from the public school or the university is largely dependent on the type of work he is to do. If it is essentially technical in character, as in engineering manufacture, it will usually be found that only a university can provide the sound technical education required. On the other hand, if trustworthiness, sound common sense, judgment, and the ability to turn experience to good account are the essentials, it will often be desirable to enter direct from the public school. But whether the recruits for industry are drawn from the public schools or universities, the fundamental requirements for the work remain the same, for it is the youth with marked personal qualities, in whom a broad outlook and keen, properly directed imagination is developed, who matures under the influence of experience into the best type of industrialist.

Engineering is one of the chief industries to absorb the product of the public schools and universities, and its needs as regards the training of its recruits can be taken as generally representative of what is required by all productive industries.

The primary aim of all schools conducted for general educational purposes should be to teach an organic body of principles that will serve the students as occasion requires, and in this respect the general education of an embryo engineer cannot be substantially different from that for other professions. Since engineering is an applied science, it is, of course, essential that there should be a sound training in the fundamentals of science, and too much stress cannot be laid upon the need for a thorough grounding in physics. Experience shows that as a foundation this is undoubtedly more vitally and fundamentally important than mathematics, mechanics, etc. School workshop education is not always on the right lines, and wide experience has taught the unwisdom of extending these activities. Their inclusion in school work is justified because they have helped to correct some of the faults connected with the bookishness from which our educational system has suffered, and also because of the close connexion that exists between motor activities and mental development. In this connexion there should be no misapprehension on the part of both students and authorities of schools and universities as to the purpose of this small amount of practical training, and though such training serves to illustrate the technical principles of engineering, it cannot replace or shorten that real practical training that is attainable only in the commercial atmosphere of a works.

In public schools to-day there exists a tendency to regard the man who plays hard as a hero and to despise the man who works hard. To be really successful a man must be able to work and to play, and from the point of view of the needs of industry it is desirable that an equilibrium should be maintained between work and play. The prejudice that exists against the youth who really gets down to work is one of the most lamentable features of public school life, and every effort should be made to break down this attitude. Too many public school boys have a superficial knowledge of many subjects but sound workable knowledge of none. This lack of application shows itself in their after-life in an inability to see a job through. Interest and enthusiasm more than inherent knowledge are the great factors of success in commerce and industry. At the present time there is a great need for industrial leaders who are sufficiently sympathetic to appreciate the aims and aspirations of organised labour and to take these into consideration. This sympathetic attitude cannot be developed so long as the present tendency exists to foster in public school and university men a sense of superiority over those who have not been so fortunately placed.

The function of the university training of engineers is to provide sound technical education in the principles underlying engineering practice and to develop a capacity for orderly, logical thinking and a facility in applying technical principles to the solution of practical problems. The university should specialise on fundamental principles, but this is not all; too many students lay too much stress on the purely technical side of their training. Technical and scientific knowledge can be considered only as a tool and does not in any sense take the place of those personal qualities that are necessary for success in every career. There is too great a tendency to overlook the development of such qualifications as the ability to express oneself with precision and clearness in speech and writing, the ability to carry a job through, and the development of a general outstanding personality. At the present day the great weakness of engineering students from the universities is the lack of educated common sense, which includes a sense of order, proportion, measure, correctness, and elasticity. This shows itself chiefly in the absence of a sense of proportion resulting in the inability to distinguish between what is technically practicable and what is practicable so far as shop equipment is concerned. There is a tendency to trust too much to imagination, an inability to appreciate the difference between cause and effect, and a lack of appreciation of cost.

A student being trained for manufacturing engineering work will eventually find himself in one of the four following branches: (1) Technical design of apparatus; (2) commercial engineering; (3) industrial administration and management; (4) research work. Suitability for the different divisions is determined largely by the inherent characteristics of the individual. All branches call for sound technical knowledge, but for design this must be linked with broad judgment; for commercial work, with a commercial instinct; for administration and management, with an innate capacity for understanding and controlling labour; and for research work, with a bent for investigation and an exceptional training in physics and mathematics. What young engineers frequently fail to realise is that, while at college, training is the first consideration, and that little more than effective assimilation of the instruction provided is expected of them. When, however, they enter industry, practical training, while of the utmost importance, is informal and must be conducted in a manner subservient to the primary object of an industrial organisation, namely, effectiveness in manufacture.

New and Old Anthropology.

The Children of the Sun: a Study in the Early History of Civilisation. By W. J. Perry. Pp. xiv+551+16 maps. (London: Methuen and Co., Ltd., 1923.) 18*s.* net.

ANTHROPOLOGY is at present going through an acute crisis. A complete change in method, aim, and scope is demanded by a number of writers, while the rest, holding fast to the old principles, oppose the innovators and reject their results wholesale.

The traditional orthodox anthropology of the past, working by the comparative method, attempted to set up parallels and similarities of custom and belief in the various branches of mankind, savage, barbarous, and civilised. It rested on the assumption that such parallels are the result of the essential identity or at least uniformity of human nature. Its final aim was the establishment of evolutionary schemes in which the development of human customs, beliefs, and institutions had to be traced from their origins through various progressive stages until they reached the crowning form in modern civilisation.

Anthropologists of the new school dismiss altogether the idea of a typical uniform evolution, through which all sections of humanity had to pass. They are, as a rule, also hostile to psychological interpretations of culture, and most of them reject the principle of mental uniformity of all races and varieties of man. The appearance of the same forms of culture all over the globe they explain by the principle of diffusion from one or a limited number of centres. Their main interest lies, therefore, in the tracing of actual historical dependencies of the various civilisations and the establishment of historical links, migrations of peoples and movements of culture. In their opinion, devolution rather than progress plays the leading part in the foundation of the various customs actually found in barbarism and savagery.

The new historical school, the beginnings of which date so far back as the appearance of the first pioneer memoirs of the great German geographer and anthropologist, Friedrich Ratzel ("On the African Bow," 1887, etc.), had a new and dramatic revival in 1904 when Dr. Ankermann and Dr. Graebner challenged the old methods at a big meeting of the Berlin Anthropological Society. This school had been gradually making headway in Austria, in the excellent work of Pater W. Schmidt and of the "Anthropos" circle of scholars, and in America in the contributions of a number of anthropologists. But it was reserved to British science to produce the most brilliant and thorough-going achievements of the new anthropology and to produce them—in historical irony of their main tenet—by independent evolution of thought. At the Portsmouth meeting of the British Association in 1911, Dr. W. H. R. Rivers declared in his now famous presidential address to Section H (Anthropology) his independent rediscovery of the methods of the historical school, and he indicated the new lines of anthropological advance. At that time already a still more ambitious scheme and one destined to wield the greatest influence was reaching maturity in the solitary, at first even auto-didactic, researches of the great anatomist, Prof. Elliot Smith.

Prof. Elliot Smith soon found himself in collaboration with Dr. Rivers and with a younger worker, Mr. W. J. Perry, who, in a series of important memoirs and in a remarkable volume on "The Megalithic Culture of Indonesia," contributed some of the leading ideas to the new system. Mr. Perry's new book, "The Children of the Sun," is the first comprehensive and systematic exposition of Prof. Elliot Smith's theory. As the standard statement of the most extreme of the historical theories, as well as on its own merits, the new volume is bound to arouse a wide and lasting interest and to leave a permanent mark on the development of anthropological doctrine.

Like all the upholders of the new anthropology, the author rejects the doctrine of a repeated, independent evolution. Following Prof. Elliot Smith, he maintains that civilisation has developed only once, and that the original home of all human cultural achievement was Egypt. There the three essential factors needed for this almost miraculous birth of higher humanity happened to coincide: the unique climate, the finest race and the ideal conditions of isolation and geographical centrality, where culture could quietly develop and whence it could easily spread. The essential features of the archaic civilisation—agriculture with irrigation, megalithic buildings, mummification, a certain type of burial, sun-worship, dual organisation of society—can be traced as they evolved, or rather historically happened, once only in Egypt under its especially favoured conditions.

The archaic civilisation, once crystallised, began to expand and spread over the world. The most important of Mr. Perry's contributions to the new theories consists in the elaboration of a scheme of these expansions. The motives which led the archaic explorers all over the vast region where he finds their traces were the desire for gold, pearls, precious stones, conceived as magically endowed with life-giving virtues. The routes which they followed, the manner in which they advanced, the various areas which they occupied, and especially the changes and regressions of the archaic culture in each area, all this is described by the author and cannot be easily summarised here in a few words. It is given with astonishing erudition, width and daring of outlook, and a great ability for marshalling evidence in Mr. Perry's book, which no anthropologist can omit to read. It also bears so much on all associated branches of learning, such as history, archæology, geography, and sociology, that it is bound to influence modern humanism in general.

This very many-sidedness of the book, which is its greatest achievement, must needs be also the source of some of its weaknesses. Every one of the specialists in the various branches it covers, impressed as he must feel by the author's extraordinary versatility and the extent of his learning, will without doubt find certain mistakes, gaps, and inaccuracies. If those which the present reviewer can detect in the narrow area of his own field of work (Melanesian New Guinea) and in the subject of sociology of primitive peoples, can be taken as the measure of the average accuracy of the book, then a careful overhauling of evidence will serve to improve the work in those future elaborations which Mr. Perry promises.

Thus, for example, speaking of mother-right, Mr. Perry says (p. 246): "In British New Guinea, matrilineal institutions exist in a developed form in Bartle Bay only," on the authority of Prof. Seligman, to whose "Melanesians" he refers, mentioning four different places in the book. Yet in the very introduction to his book, Prof. Seligman says (on p. 9): "The most characteristic cultural feature of the Massim is the existence of a peculiar form of totemism with matrilineal descent." Now the Massim, who are more numerous and occupy a wider area than the other Melanesians of British New Guinea, have a social organisation directly dominated by mother-right, as can be easily verified by the study of their culture in Prof. Seligman's work (pp. 376-746) as well as in the publications of the present reviewer.

The sentence (p. 241), "Under matrilineal institutions, the daughter of the king is the lawful heir to the throne," is, I think, due to a slip or careless wording, but this can only partially excuse it. When in another place (pp. 95, 96), speaking of a group of languages, the identity of which has been established by Pater W. Schmidt, Mr. Perry calls them Austronesian instead of Austroasian, and thus mixes two technical terms, used by the discoverer in careful contradistinction—this makes on the pedantic reader the impression of the sources having been consulted somewhat cursorily.

In the chapter on the "Givers of Life," in which Mr. Perry develops the revolutionary and interesting theory of magic, on the lines indicated by Prof. Elliot Smith, we find the statement that "stones play the most conspicuous part in the magical beliefs and practices" of the Melanesians (p. 393). This is scarcely correct, for, though in Melanesia the nature of magical substances varies greatly with the particular district, yet vegetable matters everywhere preponderate and are used almost exclusively in most areas. It is also not accurate to say that "the Australians do not use stones for their magic" (p. 397). Again, the statement that "the magical practices of peoples of the lower culture in Indonesia, Melanesia, and elsewhere tend to centre round a few substances" (p. 385), will deeply astonish the anthropologist acquainted at first hand or through other sources with some or any of the areas. These are just a few samples of inaccuracies which, if space would allow, could be easily multiplied from almost every chapter containing anthropological evidence.

Now all this does not mean that the argument of these chapters, somewhat imperfectly documented as it is, should be considered as disposed of. Mr. Perry's book is the first systematic outline of a big and daring theory of human culture, and the first scheme of its birth, history, spread, and partial decay. In such a work, details do not matter so much as the truth of the outline, nor the formal, logical validity of the argument so much as the right intuition of the historical reality. But it was necessary to point it out in consideration of Mr. Perry's promise of pending future elaborations of the system. The present reviewer believes that, admirable as is the present achievement of Mr. Perry, it is capable of still further improvement, and that this improvement must lie, not so much in the widening of scope, perhaps, as in a deeper sociological analysis and in a closer scrutiny of the evidence.

The new volume is a great advance in two respects on the other works of the historical school, including the early statements of Prof. Elliot Smith and of Dr. Rivers. First, Mr. Perry and Prof. Elliot Smith, in his later works, admit fully the necessity of psychological interpretations of culture. In the second place, Mr. Perry, reversing the entirely unsatisfactory view of the German school that a culture complex must be regarded as a loose agglomeration of cultural items, emphasises at every opportunity the importance of regarding human civilisation as an organic whole, the necessity of studying every item and aspect of culture in its functional dependence upon the others.

With such sound methodological foundations, the conceptions of Prof. Elliot Smith and Mr. Perry are capable of being developed still further and of deepening our understanding of human culture and its history. Whether their work will dispose of all the principles of the old anthropology is another matter. The present reviewer has, through his field work among savages and through his early associations with uncultured, half-savage peasants of Eastern Europe, as well as through his slight acquaintance with civilised men, retained a firm belief in the uniformity of human nature, and all his theoretical bias makes him remain faithful to the old school. But certainly Mr. Perry's book and Prof. Elliot Smith's teachings are making a very strong case in all their positive claims, and they will force the old school to revise its position especially as regards the principle of evolution. But in the measure as the old school recognises the necessity of taking into consideration historically established facts such as taught by Prof. Elliot Smith and Mr. Perry, while the new school moves towards psychological interpretations and sociological analysis, both are bound to find some common ground for collaboration, and there is some hope that the war of extinction may end in a true peace treaty.

B. MALINOWSKI.

The Teaching of General Biology.

General Biology. By Prof. Leonas Lancelot Burlinghame, Prof. Harold Heath, Prof. Ernest Gale Martin, and Prof. George James Peirce. Pp. xxix + 568. (London: Jonathan Cape, 1923.) 21*s*. net.

LET us say at the outset that this introduction to "General Biology" is both original and admirable in plan, as well as adequate in execution. The English reader is tempted to ask why such books should be produced only in the United States. To one who knows the educational systems of the two countries the answer is plain. In a nutshell, it is that while in England biology is a subject for specialists, in America it is treated as part of a general education —and this in the universities as well as the schools.

Let me illustrate this by concrete examples. In the University of Oxford, with more than 4000 undergraduates, the number taking the Biological Preliminary is about 60. Some 90 per cent. of these are doing so because it is a requirement for a medical degree; the rest, almost without exception, as a foundation for honours in zoology, botany, or geology. In 1913 the reviewer visited, among various American universities, the State University of Texas. Here, out of 3000 undergraduates, more than 600 were taking elementary biology; and this proportion, though high, was not exceptional.

Doubtless the difference has its roots in the fact that in England specialisation is greater and standards higher for the B.A. degree than in the United States; and doubtless our system has many advantages. But it makes a cruel split between different branches of knowledge, and sees to it that a man reading for honours does *not* get a general education. It may be asserted that school is the place for general education; but, first, there is often early specialisation at school; secondly (and most important), for the great majority, the progress of their mental development combines with the university atmosphere to make the first undergraduate year the best time for discussion of general principles; and thirdly, in our particular case, biology is not usually taught to older boys at school.

What is the result? Again the example may point the tale. A question sticks in memory from undergraduate days. "What is Mendelism?" inquired the historian of the biologist; "I always thought it was some obscure vice, but I gather from you that it is something scientific." . . . Or, more recently, a candidate for election to a research fellowship at a certain college, on stating that his work had a bearing upon the cancer problem, was asked by one of the electors "if it would cure burns." . . .

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Since in America every university and college has a first-year course in general biology, which will be taken by anything from 10 to 25 per cent. of the undergraduates, a proportionately larger biological staff is required. Hence the greater proportion of biologists in the academic population, hence the vast volume of research in general biology (as opposed, say, to pure physiology) which issues from their country, and hence, finally, the growing up (ironically enough in the very land where one-sided education in the past has produced the extraordinary anti-evolution movement we are now witnessing) of an educated class to whom biological principles are as much part of their fundamental equipment as history or elementary mathematics.

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Some idea of the book under review may be best given by excerpts from the table of contents. § 1, Living Substance. § 2, The Rôle of Green Plants. § 3, The Maintenance of Life (including 4 chapters, on metabolism, transport systems in animals, digestive systems, and respiration). § 4, On the Adjustment of Organisms, ingeniously leading on, from plant adaptations, through animal movement, sense organs, nervous and chemical co-ordination, to diseases due to "Maladjustment." § 5, The Association of Organisms, with an introduction on colony formation, symbiosis, and parasitism, leads on to infectious disease—plant disease, animal disease, and the relation of insects to disease. § 6, Death and the Duration of Life. § 7, The Rôle of Micro-organisms, leading up to a discussion of soil fertility. § 8, Growth and Reproduction. § 9, Heredity (including an excellent elementary presentation of the essentials of neo-Mendelism). § 10, Evolution ("Evidences"—"Theories"—"Results"). § 11, Distribution. § 12, Man: 5 chapters, dealing with human evolution, inheritance (of physical and mental traits), man's place in Nature, and human progress.

The book is the result of the collaboration of four professors of Stanford University, from the departments of zoology, physiology, genetics, and plant physiology respectively; and embodies a first-year course of work which has actually been in successful operation for some years.

With such a wide range, it is inevitable that there occur sins both of omission and of commission. For example, when heredity and eugenics are treated at some length, it is a pity to omit all discussion of the sex-ratio and of the effects of inbreeding. The definition of a species (p. 412) is worse than most; the treatment of sexual selection feeble. There is no mention of the nerve-net or of the autonomic nervous system. In the discussion of man's place in Nature there is a curious timidity in taking the necessary plunge into psychology. Further, the book could have been rounded off by a chapter or two on the history of biological thought and on the aims of the science in the future, and, in another direction, by giving a list of books where the various topics could be pursued.

In spite of these details, and of a certain pedestrianism of treatment and style, the book is important by virtue of carrying out a new plan—by virtue (let us repeat it once more) of treating elementary biology as a proper and necessary part of general education, instead of as a door-mat for specialists.

. . . .

What is the moral for us in Great Britain? It is difficult to be precise. Biology is certainly being more generally taught in schools, and taught in the light of a wider vision; but it still plays Cinderella to its elder sisters the so-called "basic" (in many respects, more abstract) sciences. In the universities the problem is still more difficult. It really resolves itself into this: now that the scientific renaissance of the last three hundred years has embraced all the sciences and influenced all branches of thought, is it feasible to frame some scheme which will give the able young man coming up to the university, not only a sound specialist education, or a professional, or a technical one, but also the opportunity of becoming acquainted with the chief trends of thought in his own civilisation? This should not be impossible. If boys left school a little earlier, and the average time spent in reading for the B.A. degree were four instead of three years, the first year could be devoted to the task we have outlined—a task which it is surely a special duty of the universities to undertake. At any event, not until some such idea is realised in our educational system will it be possible to have any generally accepted intellectual basis for civilisation. All great civilisations in the past have always had some such underlying framework of general thought. Ours oscillates between incompatibles.

J. S. H.

Crystal Structure.

Kristalle und Röntgenstrahlen. Von Prof. Dr. P. P. Ewald. (Naturwissenschaftliche Monographien und Lehrbücher, herausgegeben von der Schriftleitung der *Naturwissenschaften.*) Pp. ix+327. (Berlin: Julius Springer, 1923.) 6 dollars.

FEW single physical experiments can ever have opened up so large a field in so short a time as the discovery twelve years ago by Laue that crystals diffract X-rays. In addition to the new science of X-ray spectroscopy, which has so profound a significance as regards atomic structure, Laue made possible the study of crystals, which has proved of such importance in many unexpected ways. This study is essentially a borderline subject, which interests equally the chemist, physicist, crystallographer, and metallurgist. A writer in reviewing the research which has been done cannot assume that his reader will be well acquainted with every one of these sciences; and he must provide an introduction to crystallography for the physicist and chemist, and an introduction to atomic physics for the crystallographer. This has been admirably done by Prof. Ewald, and his book should make a wide appeal. He confines himself to the analysis of crystal structure, and to the physical and chemical significance of the crystal patterns. The book contains an excellent list of the crystals which have been analysed by X-rays, giving a brief account of the results and references to the original papers.

There is one chapter in the book (chapter xvii.) which most readers will find all too short. It deals with the relationship between the physical properties of a crystal and its structure. Now that we know how the atoms are arranged in crystals, it is the crystalline state and not the gaseous state of matter which is most amenable to mathematical treatment and to investigation of atomic forces. The heats of formation of compounds, the nature of valency forces, piezoelectricity, refractivity, specific heats, and elasticity can all be dealt with in quite a new way. There is here an immense field, not only for the physicist and physical chemist, but also for the applied mathematician. A new mathematical treatment must be developed to deal with the orderly array of atoms in a crystal, and link up the properties of the crystal as a whole with those of the atoms which build it. All work on these lines has so far been done on the Continent, and it is to be hoped that books like this will interest applied mathematicians in Great Britain in a fascinating field.

Prof. Ewald's book is excellently illustrated and clearly written. Every one interested in crystal structure will welcome it as a most useful addition to the literature on the subject.

Our Bookshelf.

L'Évolution des étoiles. Par Jean Bosler. (Recueil des Conférences-Rapports de Documentation sur la Physique ; Vol. 8, 1[re] Serie, Conférences, 19, 20 ; Édité par la Société *Journal de Physique.*) Pp. 103. (Paris : Les Presses universitaires de France, 1923.) 10 francs.

IN 1910, M. Bosler published "Théories modernes du soleil," in which he told the story of the sun, or rather as many different versions of the story of the sun as there had been investigators to propound them. He now increases the debt which astrophysics owes him by telling the story of the stars. It is told with the same charm as the earlier story, with the same skill in the piecing together of different researches to form a connected exposition, and with the same judgment in the selection of those details, either experimental or mathematical, which are of permanent interest. But in surveying the two books, even taking account of their different fields, one is struck with the enormously increased unanimity of view which characterises the researches of the last ten years. There is now a cohesive body of astrophysical theory, based largely on modern atomic theory, which is genuinely accepted.

M. Bosler takes us over the ground which is principally associated with the names of Russell, Eddington, Adams, and Saha. Almost the only criticism one would make is a criticism of the title. M. Bosler scarcely deals with stellar evolution—chiefly for the reason that practically nothing is known about it. He treats of the actual constitution of the stars—of their observed spectra, and of the picture of a stellar atmosphere which Saha's applications of the theory of spectra have taught us to formulate ; of their observed masses, densities, luminosities, and surface brightnesses, which are summed up in the Hertzsprung-Russell theory of giants and dwarfs ; and of stellar interiors, as explored by Eddington by an application of the fundamental laws of physics to the two just-mentioned sets of facts. In what way, or how quickly, the constitution of a star changes in time, we scarcely know. The solution is almost certainly bound up with the solution of the problems of the nucleus of the atom, and it is with the bearing of these on the source of stellar energy that M. Bosler concludes his summary.

A valuable list is given of all the leading references.

E. A. M.

Ergebnisse der exakten Naturwissenschaften. Herausgegeben von der Schriftleitung der *Naturwissenschaften.* Zweiter Band. Pp. iii + 252. (Berlin : Julius Springer, 1923.) 2 dollars.

THE second annual volume contains four papers on astronomical subjects, dealing with stellar movements, parallax, the Galaxy, and variations of latitude. Prof. F. Henning describes the production and measurement of low temperatures, giving an account of the work of Kamerlingh Onnes in Leyden, and of the forms of thermometer employed in this class of work. Prof. J. Franck contributes a very interesting article on the exchange of energy quanta when atoms and molecules collide ; a number of phenomena connected with the emission and absorption of light by gases, with dissociation, and with photochemistry, are explained in terms of the quantum theory and of Bohr's atomic model. Prof. W. Gerlach deals with magnetism and atomic structure, discussing the relation between the Weiss and the Bohr magneton, and space quantelation. Prof. A. Landé treats the theory of the Zeeman effect, Prof. F. Paneth describes the discovery of hafnium, and Dr. G. Masing and Dr. M. Polanyi contribute a long and well-illustrated article on cold working and strengthening of metals.

A Literary and Historical Atlas of Europe. By Dr. J. G. Bartholomew. (Everyman's Library, No. 496.) Pp. xiv + 254. (London and Toronto : J. M. Dent and Sons, Ltd. ; New York : E. P. Dutton and Co., 1923.) 2*s.* 6*d.* net.

THIS is a new edition of an atlas by Bartholomew that was first published in 1910. It contains 96 pages of coloured maps, historical and geographical, plates of coins, numerous plans of battles and sketch maps of districts of literary or historical associations, a useful gazetteer, and a full index. The political maps of Europe show the new boundaries ; but, except in the case of Switzerland and a few sectional maps of the British Isles, none of the maps has orographical colouring. In fact, relief of the surface is not well shown. Plate 7, which is termed ethnographical, really shows the languages rather than the races of Central Europe. The sketch maps and plans are as in the original edition and do not include any additions arising from the War. The same omission would appear to apply to the gazetteer, but the index has been revised. The technique and colour printing are excellent, and the book is good value at the price.

Mnemic Psychology. By Richard Semon. Translated from the German by Bella Duffy. Pp. 343. (London : G. Allen and Unwin, Ltd., 1923.) 14*s.* net.

THIS translation of a valuable scientific work on a problem of present biological interest is unfortunately sponsored by an editor who seems to have completely misunderstood the author's theory. Vernon Lee, in a lengthy introduction divided into chapters, tells us that she has altered the author's title "Mnemic Sensations" (*mnemische Empfindungen*) to "Mnemic Psychology." The only reason we can discover is that she is confused in her mind as to what mnemic sensations can mean. Apparently she thinks there is a contradiction in terms, and she attributes her confusion to the author. We recommend readers to omit the introduction and restore the author's title.

The Life of the Scorpion. By J. Henri Fabre. Translated by Alexander Teixeira de Mattos and Bernard Miall. Pp. v + 344. (London : Hodder and Stoughton, Ltd., 1923.) 8*s.* 6*d.* net.

FABRE'S delightful style is well retained by the translators of this well-known work. It should be mentioned that about half of the book relates to the Languedocian scorpion, while the remainder is concerned with various Hemiptera, Pentatomæ (misprinted in "Contents" as Peutalomæ), Reduvius, and various aphids. It is almost presumption to say that the matter is excellent throughout.

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.*]

The Transmission of Human Malaria.

THE history of the transmission of human malaria by means of Anopheline mosquitoes has occasioned many polemics, especially between Sir Ronald Ross and myself. Ross has now thought it proper to reopen the question in his "Memoirs," published last year.[1] As I cannot accept his opinions, I may be allowed to reply to him in the columns of NATURE; but I shall limit myself to a statement of the facts, without heeding the language in which I have been vilified, and carefully avoiding personalities.

In 1890 I investigated the modes of transmission of malaria. As it had been suggested that malaria was propagated by mosquitoes, I made experiments in my laboratory at Catania with the common gnat (*Culex pipiens*), and on failing to obtain the expected results, I dismissed this hypothesis. Judging from the structure of the malarial parasites, I affirmed that they could not live in the external world in the form in which they appear in the blood; and, as it was commonly believed that they developed from the ground, I ventured the *hypothesis* that in their independent life they might be represented by an Amœba, which they so much resemble. My hypothesis did not prove true, but the observation on which it was founded was afterwards confirmed. I demonstrated, moreover, that the so-called "flagella" never appear in the blood while circulating, but only after it has been shed; and arguing by analogy, I supposed that these forms were a product of degeneration. This hypothesis also was refuted; but the fact which I had observed was recognised to be exact. The above will suffice to rectify what Ross has written on this subject.

I did not interest myself further in this matter until 1898, when the hypothesis that mosquitoes transmit malaria had been again suggested by Laveran, Manson, Bignami, and Koch. In a discussion with Bignami, Dionisi, and Koch, I then formulated the following dilemma: either the hypothesis was baseless, or special blood-sucking insects must exist in malarious countries. In order to establish which of the two propositions was correct, towards the middle of July—*i.e.* after the end of the session—I started on a trip through malarial and non-malarial localities.

It is not necessary to enter into minute particulars here: I shall say only that I reached my goal by following a way I had especially elected. In my search for the intermediate hosts of worms, I saw that searching at random was an uncertain method which required protracted and extensive inquiries. I conceived, therefore, and recommended in my paper (1889) the method of limiting the search for intermediate hosts by a preliminary detection of suspected forms. To explain my meaning by a similitude, if in a village of a thousand inhabitants a theft has been committed, it will be very difficult to discover the thief, unless it is first established, by appropriate investigations, which persons fall under suspicion. Once in possession of the list of suspects, it is much easier to find the culprit, as every detective knows full well. Likewise, by comparing many places in northern and central Italy (and not only "near Rome," as Ross writes on p. 404), where malaria is endemic, with others where it is not, I came to the conclusion that three marsh mosquitoes were particularly suspect (*Culex penicillaris, Culex vexans, Anopheles claviger*), and that the common *Culex pipiens*, which is not a marsh form, was to be considered innocuous.

As soon as my suspicions fell on these three forms, I began my experiments. At my request, and with the consent of Bignami, a perfectly healthy man—the famous Sola of Bignami, who had willingly allowed himself to be repeatedly, and always in vain, bitten by *Culex pipiens* reared in the room where he was confined—was now subjected to the bites of the three suspected forms of mosquitoes, which were collected by my order at Maccarese. Even before this experiment could give any result, however, my collector, who was bitten in a malarial locality by these three species only, contracted æstivo-autumnal fever. Some time later Sola also fell sick (I communicated this experiment with Sola to the R. Accademia dei Lincei on November 6, 1898), and it was thus ascertained that one at least of these three forms was to be incriminated. It remained to prove which of them was the true culprit. In this I succeeded more easily than I had expected, because at the end of autumn malaria was still raging in the Agro Romano, while the two Culex had disappeared. In an addition to our note of December 4, 1898, we (Bastianelli, Bignami, and myself) were already able to state that we had obtained a "double tertian" infection, by means of the bites of *Anopheles claviger* (=*maculipennis*) alone, in a man certainly not previously infected with malaria, and confined in a non-malarial place.

The method I adopted was, therefore, first, to select the suspected forms; secondly, to establish which of them was to be inculpated, by subjecting healthy individuals to their bites.

Ross, on the contrary, followed a different plan. At the beginning (1896) he also subjected men to the bites of mosquitoes (which ?) fed on malarial blood; but afterwards he abandoned this method and chose another—examining every blood-sucking insect he could lay his hands upon, and trying to cultivate malarial parasites in it. For Ross "there was only one method of solution, that of incessant trial and exclusion. But this meant enormous labour—and I had already spent two years over the quest" ("Memoirs," p. 211).

My first note, entitled "Relations between malaria and particular insects — Big and little marsh mosquitoes" ("Rapporti tra la malaria e peculiari insetti—zanzaroni e zanzare palustri," which last words Ross mistranslates "gnats and marsh mosquitoes"—"Memoirs," p. 406), was published by the R. Accademia dei Lincei, among the "Notes received by the Academy before October 2, 1898," and reprints of it were already distributed on September 29. A second edition was published in the *Policlinico* (Sezione Medica), in the number dated October 1, but certainly issued several days after October 2. The editor of the *Policlinico* did not allow me to indicate that this was the second edition. I mentioned it as such, however, in my paper "Recenti scoperte sulla malaria esposte in forma popolare" (*Rivista di Scienze biologiche*, July 1899, p. 21). At any rate, that the edition in the *Policlinico* was the second is clear from the additions which I introduced in it—especially from the very important one regarding my collector, who, bitten only by the three species suspected, contracted malaria, as noted above. I insist upon these particulars, because Nuttall, having mistranslated "Communicazioni pervenute all' Accademia prima del 2 Ottobre" (that is, notes received by the

[1] "Memoirs," by Ronald Ross. (London: John Murray, 1923.)

Academy before October 2) by the words " read on the next day [October 2] at the Accademia dei Lincei," and believing erroneously that the *Policlinico* was really issued on October 1, considered the second edition as the first and the first as the second. In the second edition, which thus becomes the first for Nuttall, I showed that I was acquainted with the latest work of Ross; and as in the other edition the same work is not even mentioned, the reader may suppose that I repented of having referred to it at all! The truth is quite the contrary: I resolved to publish the second edition (in the *Policlinico*) chiefly because I had been in the meantime informed by Dionisi of Ross's discovery concerning Proteosoma, and I was sorry not to have been able to do it justice in the first edition.

Before I published my " Nota preliminare " of September 29, Ross had discovered in the " grey mosquito " the cycle of development of a parasite of sparrows (*Proteosoma grassii* Labbé), which I had previously shown to be very similar to the parasite of human malaria, and he had illustrated the subject in an extensive memoir. The indication of the sporozoites in the salivary glands was alone wanting; but even this was added by him a few days after the issue of my " Nota preliminare " (*i.e.* on October 11, 1898).

Naturally this fundamental discovery of Ross simplified the second part of my labour, which consisted in finding out how the parasites of human malaria behave in Anopheles. But here also I proceeded a little differently from Ross. To attain my object, two ways lay open before me: to examine the Anopheles in the chambers where the sick were confined, or else, as Ross had done with Proteosoma, to rear the parasites in the Anopheles. I experimented with both methods simultaneously and obtained complete success. With Ross's drawings, though imperfect, the task presented no real difficulty, and I had already detected Ross's bodies in two *Anopheles claviger*, caught in a chamber where four malarious people were confined (" Memoirs," p. 348), when Charles, unasked, showed me a preparation of a grey mosquito he had received from Ross (Charles's letter to Ross of November 25, " Memoirs," p. 344). At first, attempts at cultivation of the malarial parasites in Anopheles—undertaken in collaboration with my colleagues Bignami and Bastianelli—proved a failure. But then, considering that the Anopheles are cold-blooded animals and that we were in winter, I set things right by rearing the insects in a heated place (see Charles's letter reported in the " Memoirs," pp. 344-345).

By following my own way I discovered—partly in collaboration with Bignami and Bastianelli—that Anopheles transmits human malaria; and I at once expressed the opinion that Ross's experiment with the " grey mosquito " was not conclusive, and that this insect might have infected itself by biting a sparrow instead of a man—because Ross's " grey mosquito " appeared so similar to our *Culex pipiens*.

Ross does not believe it possible that, with all my comparative investigations in Italy, I should have achieved in a few months what he was not able to accomplish in three years of toil in India. Fortunately, to make sure of the genuineness of my observations, one has only to visit some of the places mentioned in my first note: the striking difference between the mosquito fauna of the malarious districts and that of the healthy ones—as pointed out by me in 1898—is always, even now, distinctly recognisable.

That Ross had not until then attained any definitive result so far as *human* malaria was concerned, appears from what Manson, who was minutely informed by Charles of all that was done at Rome, wrote to him on October 30, 1898 (" Memoirs," p. 334): " I hear Koch has failed with the mosquito in Italy so you have time to grab the discovery for England." Afterwards, Ross, who, through Charles, was regularly acquainted with our attempts, also undertook experiments with three species of Anopheles, using two cases of tertian fever and three with crescents,—but the results were negative!

On December 31, 1898 (*i.e.* before leaving India) Ross wrote to the *Annales de l'Institut Pasteur* a note[2] which was issued in February 1899, and in which he summarised his discoveries. (Ross remained in India until February 16, but without obtaining better results.) Regarding human malaria, he does not speak in this note of Anopheles—with which his experiments in Calcutta were a failure—but simply of " two mosquitoes of a new species," in which he had obtained (in August, 1897) the pigmented cells from the crescents; but he adds: " unfortunately circumstances prevented me from immediately continuing my investigations. This year (1898) special opportunities were afforded me by the Indian Government for the prosecution of these researches. For various reasons I resolved to experiment with the parasites of birds. . . ." Ross then mentions briefly his first discoveries in the " grey mosquito," and then continues: "Much was still left to be done.—It was clear that henceforth inquiries would have to be conducted in two directions.—First, it was necessary to establish step by step the development of Proteosoma in the mosquito, so as to determine the type of development for all these parasites, and to obtain a guide for the discovery of the general laws of the spread of malaria. Secondly, it seemed proper to try to become acquainted in an exact manner with the hosts of the human parasites and their habitat. This last plan of study was peculiarly attractive and promised interesting discoveries, but I chose the first, as being really the more important. To pursue both plans at once was impossible for a single person."

After describing all the discoveries he had made concerning Proteosoma in the " grey mosquito," Ross then writes: " Grassi, working quite independently of us, has recently made patient epidemiological investigations which have led him to suspect a species of mosquito, *Anopheles claviger* Fab., of being the agent of marsh fever in Italy. After seeing some specimens of my pigmented cells which Manson sent them, Grassi, Bastianelli, and Bignami have succeeded in producing these bodies by feeding this species of mosquito on men infected with crescents."

Elsewhere in this note Ross says that the " black spores " are " evidently durable forms, capable of living outside living organisms, in the external world "; and he says also that he agrees with Manson and Laveran that " it is quite probable that the parasite of malaria finds in man only an accidental host, and that it must reproduce itself in an external medium (probably as a parasite of the mosquito), its passage into the blood of man not being indispensable "; while he concludes: " I consider it probable that malaria is communicated to man only by the bites of mosquitoes and perhaps of other insects."

Yet before Ross had published this note we (Grassi, Bignami, and Bastianelli) had already announced (on December 22, 1898) that we had observed for the human malarial parasites in *Anopheles claviger* the same cycle which Ross had discovered for Proteosoma in *Culex fatigans*—including the passage of the sporozoites into the salivary glands! And before any further publications by Ross had appeared we had demonstrated that the " black spores " are degenerated malarial parasites, and that the malarial parasites are incapable of passing directly from

[2] The original is in French—the passages here cited being English translations.

mosquito to mosquito. I may note here, moreover, that by means of a long series of experiments—made partly with the collaborators mentioned above, and partly by myself alone—I had already demonstrated in 1899 that only species of the genus Anopheles are capable of transmitting malaria, and that all the Italian species of this genus are able to do so.

This, very briefly, is the true history of my contribution to the discovery of the transmission of human malaria, and every fact can be easily checked by reading my paper, "Documenti riguardanti la storia della scoperta del modo di trasmissione della malaria umana" (Milano, 1903), which I will gladly send to persons who are interested in the question. It is thus clear what my share in the work was, and what was done by Ross. But Ross, after having repeatedly and uselessly experimented in Calcutta with Anopheles (as noted above), when he came back to England suggested that all that I had discovered was already essentially included—although neither himself nor Manson had noticed it before—in his earlier papers, and especially in his two notes in the *British Medical Journal* of December 18, 1897 and of February 26, 1898. On the contrary, I have always believed, and still believe, that these two notes—of great historic importance, because they foreshadowed the new discoveries—are of little value as far as the transmission of *human* malaria is concerned.

From these two notes it appears that Ross had discovered some pigmented cells, which he then supposed and afterwards proved to be developmental stages of malarial parasites, in three "dappled-winged mosquitoes" fed on crescent-containing blood and in a "grey mosquito" fed on benign tertian blood. Even Ross has been obliged to admit that this second observation was erroneous and that the pigmented cells which he had discovered in the grey mosquito belonged to a malarial parasite of the sparrow and not of man. Ross holds now that he did not attach a conclusive value to this observation; but surely neither he nor Manson, before my publications, had even so much as suspected that there was an error in Ross's statement.

The experiments with the "dappled-winged mosquitoes" are also inconclusive, because these mosquitoes were not reared from the larvæ and therefore might have become infected previously by biting birds or mammals. Ross, who understands the value of this objection, has since asseverated and still asseverates that his mosquitoes were reared from the larvæ. His own note of December 1897 seems, however, to contradict this assertion. In this paper he speaks "of the more slender and white, but allied, species already referred to" (the species was "allied" to that in which he had obtained the development of the pigmented cells), and he adds parenthetically: "I have failed in finding their grubs also." By "also" he clearly implies that he had likewise failed to find the "grubs" of the allied species. If that be so, how could he have reared the adults, used for the experiments, from the larvæ?

In his "Memoirs," moreover, on p. 223, Ross gives a rough drawing of an egg, found by him in a "dappled-winged mosquito," which he says he reared from the larva, fed on blood containing crescents, and killed immediately after. By comparing this drawing with those of Nicholson published in the *Quarterly Journal of Microscopical Science* (August 1921), which anybody may easily check by the natural specimens, it will be seen that the "dappled-winged mosquitoes" described by Ross could not have been recently hatched out of the original bottle—as Ross writes ("Memoirs," p. 222)—but must have fed on blood and have already digested it before they were observed by him.

Ross thinks that I ought to have deduced from his papers: (1) that his experiment with the "grey mosquito" was inconclusive; (2) that his experiments with the "dappled-winged mosquitoes" clearly pointed to the fact that human malaria was transmitted by Anopheles.

This is not correct. Ross had incriminated not only "dappled-winged mosquitoes," but also mosquitoes with unspotted wings. As naturally all mosquitoes have either dappled wings or unspotted ones, his data could be of no possible use to me—especially as he lived in India and I in Italy, two countries which have a very different fauna. Only after I had found developmental stages of malarial parasites in two Anopheles caught in a room where there were four malarial patients ("Memoirs," pp. 344, 349-350), did I consult Ross's notes, in order to try to ascertain what insects he had experimented with; and I then noticed the character of the eggs indicated in his papers, and having examined the eggs of Anopheles—which nobody had previously described—I found that they presented this character. (Charles's letter of November 25, "Memoirs," p. 344.) Nobody at that time could be expected to distinguish the Anopheles from the shape of their eggs, whilst entomologists knew that the genus was easily identified by the long palps of the females—a character which Ross did not mention at all.

Ross, availing himself of Charles's letters (which cannot be properly appreciated without collating them with Ross's answers, of which I possess only a part), and commenting upon them from his own point of view, attempts to show that I thought of Anopheles after reading his notes of December 1897 and February 1898. I have, fortunately, a written declaration by Charles, in which it is stated: (1) that Charles first made my acquaintance *after* November 4, 1898, *i.e.* after he had written that "*they*" (who?) were exactly informed about Ross's works, and therefore that I could not be included in this "they"—contrary to Ross's suggestion; (2) that my acquaintance was spontaneously sought by Charles; (3) that he never showed me the letters he was writing to Ross; (4) that it was Charles himself who lent me (evidently *after* November 4) the numbers of the *British Medical Journal* with the two notes by Ross, *which, as Charles states, I had not seen until then.*

The real question, however, is not whether I had or had not read the two papers by Ross; if I had not read them, the fault was mine, for it was my duty to be aware of all that had been done before me. The essential point is whether the knowledge of Ross's description could have led me to recognise Anopheles, and not other mosquitoes, as responsible for the propagation of human malaria. This I deny absolutely, and it is contradicted by all that I have previously explained.

The attentive perusal of my first note, both editions of which are reprinted in the *Documenti*, will suffice to convince any impartial person that I was not then acquainted with Ross's two papers. If I had relied upon them, I could not, for example, have declared *Culex pipiens*—so similar to Ross's "grey mosquito"—to be innocuous; and instead of *Anopheles claviger* (=*maculipennis*), which is very common and has four spots on the *middle* of the wing, I should have incriminated—as I did not do—*Anopheles pictus*, which was indicated in my note as a rare species, like Ross's "dappled-winged mosquito," and which has also the dark spots on the *anterior edge* of the wing.

May I refer now to the charges that Ross has borrowed from Calandruccio of Catania? The latter wrote some libels against me, and in a fit of insanity accused me of having pirated his work on the direct transmission of *Ascaris lumbricoides*—thus claiming for himself a discovery which, as he himself had stated in a previous paper, was made and published by me in Lombardy before I ever went to Catania, and therefore before he had begun to study zoology or had ever met me! Any one who wants to know more about Calandruccio should read my note of 1901, of which I still have some copies which will be sent freely on request.

Ross quotes also (p. 401) a letter from Manson to him dated June 8, 1900, in which he speaks of a "robbery" (*sic*!) committed by me in regard to Filaria. I am in possession of two letters that Manson addressed to myself: in one, of July 16, 1900, he writes, "The world knows how great the share is that belongs to you in establishing the reality of this parallelism" (he means the parallelism between the clinical and epidemiological phenomena of Filariasis and Malaria); in the other, of September 13, "I congratulate you heartily on your recent paper on Filaria." I leave it to the reader to correlate these two letters from Manson to me with his previous letter to Ross!

In conclusion, I would say that I have always acknowledged and still acknowledge the great merit of Ross in discovering the life-cycle of one of the malarial parasites of birds—a discovery that was anterior to my own work. Ross, however, and others with him, do not recognise, or recognise only in part, what is due to me in regard to human malaria.

The "Memoirs" of Ross contain many other inexactitudes concerning myself and my Italian colleagues, but I believe that the passages I have already noted will suffice to enlighten any impartial reader. At any rate, I am always ready to answer frankly every other charge, as I have done in the case of the few points raised in this letter.

BATTISTA GRASSI,
Senatore del Regno.

Rome, Via Manin 53,
January 16.

Stationary Clouds in Interstellar Space.

THE late Prof. J. H. Poynting, in 1903, directed attention to a singular consequence of radiation or light pressure, namely, that a locomotive radiating body must experience a minute retarding force, simulating etherial friction, because of the Doppler-crowding of the waves in front relatively to those behind, so that there was a balance of retarding reaction.

Doubtless the effect could be perceptible only on very rare or nearly mass-less material. But since the apparently stationary clouds recently discovered by Mr. J. S. Plaskett of the Dominion Astrophysical Observatory, Victoria, B.C., are tenuous, and since calcium and sodium are effective radiators, may it not be possible that this cause has tended to reduce them to rest in the ether? There is a difficulty about their retention of any temperature sufficient for radiation; and we may have to appeal to absorption rather than to emission of radiation. Near a star the impinging radiation would be actually propulsive, but in free space radiation would be received equally in all directions; while yet the head on radiation would have virtually shortest waves when meeting a moving particle, and therefore would be most effective as a propeller. The effect is very small, involving the ratio of locomotive speed to the square of the velocity of light (Poynting's Collected Papers, pp. 326, 330, 708); but there is no need to trouble about the smallness of the force, provided it is not zero, so long as it is persistent.

It is curious to detect a retarding medium without a trace of opacity, but that seems what uniformly diffused radiation would furnish. If R is the energy received or emitted per square centimetre per second (and at constant temperature the two must be equal), the acceleration experienced by a sphere of density ρ and radius a, moving with velocity v, is

$$\frac{dv}{dt} = -\frac{2Rv}{\rho a c^2}.$$

The value of R for a full radiator at absolute temperature θ is said to be

$$R = 5 \cdot 32 \times \theta^4 \times 10^{-5} \ c.g.s.$$

Now starlight is far from insignificant; it has been estimated as one-tenth of full moonlight, or the four-millionth part of sunlight. So, if interstellar dust is kept ten degrees above absolute zero by stray radiation—which is Poynting's considered estimate of what is reasonable by the theory of exchanges—the time period for reducing the speed of a particle, say, 10^{-6} centimetre in diameter, to $1/e^{\text{th}}$ of its value would be of the order

$$\frac{\rho a c^2}{2R} = \frac{10^{-6} \times 10^{21}}{10^4 \times 10^{-4}} = 10^{15} \text{ seconds,}$$

or, say, thirty million years. Quantum considerations would greatly reduce this period, partly by extending the action to particles as small as atoms.

Whether the superposed radiation from different sources might be regarded as equivalent to stationary waves, is doubtful, but the H, K, and D lines are very definite, and at least the frequencies from different sources could agree. Even the remoter possibility of the generation of matter by stationary waves is not altogether to be overlooked.

OLIVER LODGE.

Normanton House,
Lake, Salisbury,
February 14.

Radiation and Atoms.

IN the attempts to interpret theoretically the interaction between radiation and matter, two apparently contradictory aspects of the mechanism have been disclosed. On one hand, interference and dispersion demand a continuity similar to that of the classical theory of optics. On the other, the exchange of energy and momentum between radiation and matter presents discontinuous features which have even led to the introduction of the theory of light quanta, denying in its most extreme form the wave constitution of light. The two aspects must be really consistent, but the discontinuous side is apparently the more fundamental, and for this reason any attempt at a consistent interpretation in the present state of science must inevitably appear rather formal. Nevertheless, on the basis of Bohr's correspondence principle, it seems possible to build up a more adequate picture of optical phenomena than has previously existed, by associating the essentially continuous radiation field with the continuity of existence in stationary states, and the discontinuous changes of energy and momentum with the discontinuous transitions from one state to another.

Any atom may, in fact, be supposed to communicate with other atoms all the time it is in a stationary state, by means of a virtual field of radiation, originating from oscillators having the frequencies of possible quantum transitions, and the function of which is to provide for statistical conservation of energy and momentum by determining the probabilities for quantum transitions. The part of the field originating from the given atom itself is supposed to induce a probability that that atom lose energy spontaneously, while radiation from external sources is regarded as inducing additional probabilities that it gain or lose energy, much as Einstein has suggested. The discontinuous transition finally resulting from these probabilities has no other external significance than simply to mark the transfer to a new stationary state, and the change from the continuous radiation appropriate to the old state to that of the new.

The idea of the activity of the stationary states presented here suggested itself to me in the course of an attempt to combine the elements of the theories of electrodynamics and of light quanta by setting up a field to guide discrete quanta, which might move, for example, along the direction of Poynting's vector. But when the idea with that interpretation was described to Dr. Kramers, he pointed out that it scarcely suggested the definite coupling between emission and absorption processes which light quanta provide, but rather indicated a much greater independence between transition processes in distant atoms than I had perceived. The subject has been discussed at length with Prof. Bohr and Dr. Kramers, and a joint paper with them will shortly be published in the *Philosophical Magazine*, describing the picture more fully, and suggesting possible applications in the development of the quantum theory of radiation.

J. C. SLATER
(Sheldon Fellow, Harvard University).
Institute for Theoretical Physics,
Copenhagen, January 28.

Dutch Pendulum Observations in Submarines.

I MENTIONED in NATURE of December 1, 1923, p. 788, that Dr. F. A. Vening Meinesz, on board the Submarine K II of the Royal Dutch Navy, left Suez on October 31 to carry on his pendulum observations at sea with the Von Sterneck apparatus. In the Red Sea it was found that residence on board, where comfort is very scarce, was exceedingly trying because of the heat. The temperature of the sea-water was 31° C., but in the submerged vessel the thermometer went up to 37° C. Under the black cloth, which screens the apparatus from undesirable light-rays, an electric wolfram arc-lamp for the photographic registration further increased the heat. Accordingly the observer, who has to control the course of the observations, suffered from a temperature several degrees higher. The number of observations made in these unfavourable circumstances was four. On November 4, the sea being very smooth, Dr. Vening Meinesz tried to make an observation at the surface of the sea, but against all expectation the vertical accelerations proved to be so strong that no results could be obtained. The anomaly, *i.e.* the difference between the theoretical value—according to Helmerts' formula—and the actual value of the intensity of gravity deduced from the observations, had in the northern part of the Red Sea a positive, in the southern part a negative value.

Aden was reached on November 7; the squadron remained in this harbour until November 13 to enable officers and crew to recover from the hardships endured in the Red Sea; the cordial reception by the English residents was no small help in bringing about the desired result.

After leaving Aden, Dr. Vening Meinesz stayed a few days on the *Pelikaan*, as he had no intention of making any further observations before Sokotra was passed. On November 16 he went back to the K II. During the first ten days the weather was very fine, with a fresh wind from N.E. A very long swell was rolling from the south, which, though only slightly perceptible at the sea surface, gave to the submerged vessel a rolling of 1° to either side. On the last two days the swell had disappeared, but the N.E. wind became gradually stronger, and the motion of the ship increased to 20° or 30° to either side. At a depth of 10 metres below the sea-surface this was reduced to 1°. Obviously the long swell is transmitted to a much greater depth than the short waves. The number of observations made was seven. The preliminary computations show that in the western part of the Indian Ocean the intensity of gravity has the normal value; farther eastward a small negative anomaly is perceptible, increasing to about $-0{\cdot}020$ cm.sec.$^{-2}$ near Ceylon. As the charts of the Indian Ocean contain few data concerning the depth, this was determined in the places of observation by means of the "echo-plummet" of Behm.

On November 25 the squadron touched at Colombo, where it stayed until December 5. Between Colombo and Sabang Dr. Vening Meinesz made four observations, two of which, both on December 5, are of special interest. The first was made south of Galle in shallow water, 50 metres deep; the other, at a distance of only 13 miles from the first, above the foot of the slope where from a depth of 3920 metres the sea-bottom begins to rise towards the coast of Ceylon. The preliminary computations give the anomaly on the first spot $+0{\cdot}097$, on the second $-0{\cdot}070$ cm.sec.$^{-2}$, the anomaly in that part of the ocean farther from the coast being nearly $-0{\cdot}020$ cm.sec.$^{-2}$. The deviation at the first spot may therefore be estimated at $+0{\cdot}117$; on the second at $-0{\cdot}050$ cm.sec.$^{-2}$. According to the theory of isostasy the deviation at both spots, positive and negative, should have about the same numerical value. The result induces Dr. Vening Meinesz to suppose that the compensating masses of great density below the sea-bottom extend farther below the mainland than the theory requires, which perhaps may point to Wegener's hypothesis of floating continents.

The third observation, made on December 8 in mid-ocean, sea-depth 3850 metres, showed that the anomaly had increased to $-0{\cdot}025$ cm.sec.$^{-2}$. On December 10, at a place where from a depth of 4100 metres the bottom of the sea begins to rise towards the coast of Sumatra, the anomaly was found to be $-0{\cdot}036$ cm.sec.$^{-2}$.

On December 12 the squadron entered the harbour of Sabang in the island of Pulu Wai near the north coast of Sumatra. Dr. Vening Meinesz made there an observation on board. Afterwards he made another on shore, using the invar pendulums and following the same scheme he had used in his observations in Holland. Immediately after this observation he swung also the bronze pendulums on shore, for the purpose of controlling these.

In my former communication I omitted to mention that the Dutch Geodetic Committee has at its disposal two sets of pendulums, one of bronze, the other of invar. The advantage of the latter is that the temperature constant is very small; consequently the determination of the temperature requires less accuracy. On board a submarine this advantage is, however, neutralised by the strong magnetic field, which would have a disturbing influence on the motion of invar pendulums. Both sets were, therefore, taken along by Dr. Vening Meinesz for the making of observations on board as well as on land.

After leaving Sabang on December 18, two more observations were made, one on the day of departure, the other on the following day in the Straits of Malacca. After this Dr. Vening Meinesz went over to the *Pelikaan*, where he remained until the arrival at Tandjong Priok, the harbour of Batavia, on December 24. His whole voyage lasted 97 days, 48 of which were spent on board the submarine.

The total number of his observations on different spots was 25 in the submerged submarine and 5 on board when in harbour. Several of the first-mentioned kind were twofold: steering E. and W. in order to test the Eötvös-effect, or with and without a metallic cage covering the apparatus. As in this last case no difference was noticeable in the result, it was obvious that the influence of the electric field within the brass receiver covering the pendulums is practically of no importance.

After Gibraltar had been passed, all the observations were made with the use of the suspension apparatus, and after this the movements of the ship have not on a single occasion prevented or even disturbed the making of the observations.

My former communication concerning the use of the time-signals of the Eiffel Tower before the arrival at Alexandria was incorrect in so far as since October 20, in the eastern part of the Mediterranean, these signals had been changed for the time-signals of Lyon, which were better audible. These could have been used until the day before the arrival at Colombo, when they became imperceptible. During the rest of the voyage the time-signals of Calcutta were used. At Sabang harbour, however, the rhythmic time-signals of Bordeaux, which were distinctly heard by means of the receiving apparatus of the *Pelikaan*, were thought preferable. During the whole voyage the chronometer of Nardin proved to be a first-class timekeeper. Only in the abnormal circumstances of temperature in the Red Sea the rate showed a slight change; for the rest the daily variations did not amount to more than 0·1 sec.

It appeared from the diagrams that the vertical accelerations due to the movements of the vessel did not cause appreciable disturbances. After drawing for a succession of moments the resulting vectors from a pair of pendulums swinging in the same plane, the movement of which vectors was free from the disturbances caused by the horizontal accelerations, the decrease of the amplitude as well as the velocity of revolution proved to be as regular as could be desired. Whether the very slight divergencies are to be ascribed to vertical accelerations or to errors of draughting and measurement can only be decided by means of further investigations. Anyhow, the results of each pair of pendulums may be considered as independent data.

The second part of these investigations has confirmed the opinion expressed at the end of my former communication about the scientific importance of the observations on board the K II. Dr. Vening Meinesz may be congratulated about the fulfilment of his expectations. Thanks to his skill and endurance and to the invaluable co-operation of the Dutch Royal Navy, especially of the Commander, Luitenant ter Zee le Klasse L. A. C. M. Doorman, his officers and the crew of Hr. Ms. Submarine K II, a new way has been opened for the further development of our knowledge concerning the intensity of gravity on the earth's surface.

After the return of Dr. Vening Meinesz, who is expected back in Holland by the beginning of March, the observations will be thoroughly worked out, and a full account will be published as soon as possible. It is to be desired that the good example given by the Dutch Navy will be followed by others. The initiative, however, must be taken by geodesists, who will certainly direct their attention to the question at the second assembly of the Union Géodésique et Géophysique Internationale, which will meet in September of this year at Madrid.

As in the different countries of the world only the navies have submarines at their disposal, their co-operation will be indispensable, and it is therefore an absolute necessity to arouse their interest. To facilitate this it has been considered most effective to publish the result of Dr. Vening Meinesz' investigations in English; in this way the English and the American Navy are reached directly, and it may be supposed that among the Navies of other countries no foreign language is more generally known than the English.

The making of pendulum observations on the sea will give an opportunity to show the world that the submarine, known since the great War as a most terrible instrument of destruction, may in time of peace become an invaluable means for the advancement of science.

J. J. A. MULLER.

Zeist, February 5.

The Phosphorescence of Fused Transparent Silica.

IN the course of a research on the absorption of gases by glass and silica under the influence of the electric discharge, we have observed an interesting property of quartz glass. When oxygen or hydrogen is driven into fused quartz by the electric discharge the quartz acquires the property of phosphorescing. That this property is probably intimately connected with the absorbed gases is shown by the following observations.

A large quartz tube 100 cm. long and having a diameter of 3·5 cm. was provided with external electrodes of aluminium foil 35 cm. apart. The tube was filled with oxygen (from which mercury vapour and other condensible vapours had been removed by liquid air) at a pressure of about 0·035 mm. of mercury. The gas in the quartz tube was submitted to the action of the silent discharge. When the discharge was stopped it was noticed that the tube or its contents were glowing brightly and continued to glow for more than twenty minutes. The luminescence was at first thought to be the well-known afterglow of oxygen. It was, however, noticed that certain sections of the tube glowed and continued to glow much more brightly than the rest, whereas a gas in such a large tube and at such a low pressure would be of practically uniform composition in less than one second. The glow was therefore due to the quartz itself or to the oxygen absorbed near its inner surface. The latter explanation seems to be the right one, for the following reasons:

(1) When a small area of the tube is cooled with liquid air while the discharge is passing, that area glows much more brightly than the rest of the tube after the discharge has been stopped.

(2) Cooling a portion of the tube after the discharge has been stopped has no effect.

(3) The sections of the tube which absorb larger quantities of the gas during the discharge phosphoresce more brightly.

(4) After the glow has ceased the quartz can again be made to phosphoresce by heating it; but it entirely loses this property when all the absorbed gases have been removed. The phosphorescence caused by heating can be observed with no apparent diminution in intensity twenty-four hours after the stimulating discharge has been stopped, and the phenomenon is just as pronounced after all the unabsorbed oxygen has been pumped out of the tube.

Similar results have been obtained with hydrogen.

D. L. CHAPMAN.
L. J. DAVIES.

The Laboratory,
Jesus College,
Oxford.

Fish Otoliths from the Stomach of a Porpoise.

IN a letter published in NATURE of December 22, 1923, Dr. Johs. Schmidt of Copenhagen describes the capture of a porpoise off the south coast of Spain, in the stomach of which were found no less than 15,191 otoliths of fishes. Through the courtesy of Mr. C. Tate Regan, keeper of the zoological department of the Natural History Museum at South Kensington, I have had the opportunity of examining a portion of these otoliths.

They are all sagittæ, saccular otoliths, of which each fish possesses a pair, but as it is highly improbable that these all represent pairs, Dr. Schmidt's estimate of the minimum number of fishes, namely half the number of the otoliths, is conservative, and they probably amount to many more. For example, there are 4 otoliths of Scombresox, of which two form a pair, the other two being unrelated to each other.

The examples submitted number 4338, of which the great majority, namely 4324, are from fishes belonging to the Scopelidæ, of which there are 4 species represented. Of the 14 remaining otoliths, 4 are those of *Scombresox saurus*, allied to the garfishes, and 10 may belong to the Macruridæ. There are no otoliths of Scombroids or Clupeoids.

The characteristics of the six forms of otoliths, of which diagrams are given, are as follows :

(1) *Scombresox saurus*. 4 examples. Shape of otolith ovate, pointed in front. Sulcus straight, obliquely inclined downwards.

(2) *Scopelus* sp. 3 examples. A circular otolith with the usual scopeloid sulcus, the upper part of the otolith deeper than the portion below the sulcus, the ventral rim serrated.

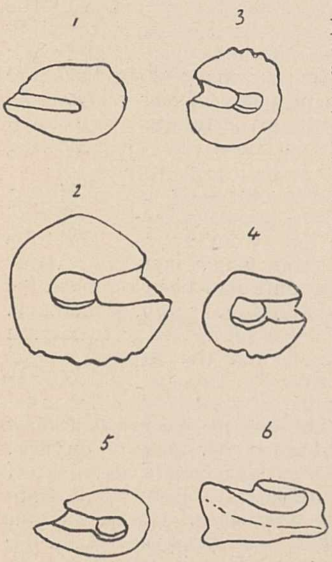

FIG. 1.—Fish otoliths. (1) *Scombresox saurus*; (2) *Scopelus* sp.; (3) *Scopelus* sp.; (4) *Scopelus caninianus*; (5) *Scopelus* sp.; (6) *Macrurus* sp. ×4½–1.

(3) *Scopelus* sp. 2090 examples. Similar to Fig. 2, but the lower part deeper than the area above the sulcus and not so projecting. The ventral rim is smooth, and there are in some cases serrations on the dorsal edge.

(4) *Scopelus caninianus*. 2 examples. Shape of otolith ovate, with serrations on ventral rim. Resembles a fossil form from tertiary formations.

(5) *Scopelus* sp. 2229 examples. This form differs from the last described owing to a frontal extension of the lower part forming a sharply pointed rostrum, the otolith being drop-shaped.

(6) *Macrurus* ? sp. 10 examples. Unlike any known otolith, the shape is roughly that of a parallelogram. The sulcus, however, is very similar to a fossil form described by Dr. R. J. Schubert (*Jahrb. Geol. Reichsanst.*, Wien, lv., 1905) and referred by him to the Macruridæ ; in the absence of any alternative, I therefore refer these forms provisionally to this family as Macrurus ? sp.

The very small otoliths submitted separately are those which have been subjected for a longer time to the gastric juices, the sulcus not being discernible.

The majority of otoliths are from fishes about 4 years old, as shown by the annual rings of growth visible on some of them. G. ALLAN FROST.

Tubbenden Cottage,
Farnborough, Kent,
February 11.

Palæolithic Flakes.

IN a recent paper (Journ. Roy. Anthrop. Inst., 1923) Mr. Reid Moir suggested that Acheulean flakes could be distinguished from Mousterian by differences in certain angles, which he calls the " fracture angle " and the " wing angle " respectively.

Wishing to test this, I examined a number of flakes from the Farnham gravels (Terrace B), which on various grounds I regarded as being Acheulean. It had, indeed, been suggested, from the character of the secondary work, that a few of them might be Mousterian ; but the presence of that industry at so high a level seemed improbable, and none of them are of typical Mousterian form.

Some difficulty was experienced with the wing angle, which in a large number of flakes could not be determined satisfactorily ; but eventually it was measured in 53 cases, and the fracture angle in 82 cases. The average results were : fracture angle 130°, wing angle 144° ; and although these do not exactly correspond with any of Mr. Moir's figures, they are far nearer to his Acheulean than to his Mousterian series. The range of variation is high, so that the method is not applicable to single flakes ; but the fact that among the fracture angles only three specimens gave as low an angle as Mr. Moir's *average* for le Moustier (120°) suggests that even for short series valuable indications may be obtainable from this angle alone. I therefore took, at random, five series of five flakes each, and obtained for the fracture angle the following average figures : 129° (twice) ; 131° (twice) ; 134°—quite enough, if Mr. Moir's figures are at all correct, to mark even such small groups as Acheulean.

It is much to be hoped that others possessing the necessary material will follow up Mr. Moir's hypothesis. Among other things, I should be interested to know whether the difficulty I experienced with the wing angles may not be a characteristic of the earlier period —the more skilful flaking of later periods leading to more definite and uniform results.

HENRY BURY.

The Gate House, Alumdale Road,
Bournemouth West,
February 3.

Consumption of Fish by Porpoises.

WITH reference to my letter to NATURE of December 22, p. 902, the following remarks by Dr. R. Legendre, Director of the Zoological Station at Concarneau, France, will be found of interest. Dr. Legendre writes in a letter dated December 27 :

" I have just read your interesting notice in NATURE about the consumption of fish by porpoises. On the coast of Brittany, where I have been able to make a certain number of observations, I never found such a great number of otoliths and eye-lenses of fish, but beaks of Cephalopods and entire Octopus were frequently met with, as I have recently indicated in the *Bulletin de la Société Zoologique de France* (tome 47, No. 8-9, 1922).

" Does the food of the porpoise vary according to distance from the shore or species encountered ? "

JOHS. SCHMIDT.

Research Work and its Applications.[1]

By Sir WILLIAM BRAGG, K.B.E., F.R.S.

RESEARCH is a word that is much used nowadays: especially, we may perhaps say, in consequence of the tremendous applications of science during the years of the War. There is a conviction that we ought as a nation to take more trouble about our research work and put it on a stronger foundation. The very existence of a Department of Scientific and Industrial Research, with power to allot large sums of money to the encouragement of research in promising directions, is a witness to the firmness with which that conviction is held on the part of Government. The various Industrial Research Associations which have been formed are breaking new ground, and some at least are going to reap a harvest. The fighting services have each of them, Army, Navy, and Air Force, well-established research departments, doing fine work. Medical research is eagerly promoted, though, of course, the possibilities seem so great that it is easy to be impatient with the money and time devoted to it. In British Universities and Technical Schools the capacity for research is, as we all know, a test of constant application to candidates for teaching positions and to students seeking the better classes of degree. It is all good healthy movement. Let me try to show what we are doing and to bring our main aims into focus. In what way do we hope to benefit by research?

Let us begin with the work done by teachers and students in Technical Institutes. Why do we encourage the student to do research work at all? To put a very practical question, why does a student stand a better chance of preferment if he has read a paper before some scientific society? We should answer, I think, that his research work is a guarantee that he possesses something more than the capacity to learn from books: a something which is of urgent importance to himself and to others. It implies a training of a special character. Research work brings out a man's self-reliance and proves his capacity to march by himself.

A good research student is like a fire which needs but the match to start it. It is a discipline to put the text-book to one side and to get out further knowledge by one's own effort. It teaches the student how to value evidence; how to read with discretion, since he must weigh what others have done before he uses the previous knowledge as a foundation for his own advances. He learns to meet disappointment, to realise how little he can do in a day, and that weeks or months may go by without obvious progress. It is strange to discover that he must spend so much time on small things, that he must wait a week before he has succeeded in stopping a small leak, or go himself to buy some trivial thing, or spend weary hours in the adjustment of an instrument which in the end he learns to put straight in a moment. There is so much little work to be done before the good observations come; it may be that weeks are spent in preparation and five minutes in making the actual measurement. It is all very humiliating; and the blunders one makes are very foolish; and the one redeeming feature is that in its apparent perversity it is like every other piece of real work. Research is rather like playing against bogey at golf: Nature never has any weakness of which advantage may be taken; there is no hole to be won by bad play because our opponent plays worse. Yet research is very human, for the researcher finds himself one of a company who have in their turn striven and denied themselves, very happily; and have handed on their experience to those who take up the quest where they have left it.

It is, as I have said, a great discipline; and there is always the hope before every student that he may contribute something to the total of human knowledge. Perhaps the hope is not so often reached as it might be, but that does not mean that the work has been done in vain: a thousand times, no. It is a remarkable fact that if a man tries to set down on paper, in line or in colour, that which he sees in Nature, his own vision of what is beautiful is quickened. The pencil teaches him the beauty of line, and the brush opens his eyes to all sorts of delicate colour schemes which he never saw before. It does not matter if his efforts are a bitter disappointment to himself, and he need not show them to any one else. Similarly, the man who strives to understand the workings of Nature by experiment which may make him feel very feeble and stupid, is paid by his discovery of a richer world. There is a fellowship between all who have tried to understand, which enables the worker in one field to have a welcome power of appreciation of the work of others. He gains not only in the richness of appreciation of what is beautiful and interesting, but also in the power of making friends.

But we may well say these are not the things which we have most in mind when we devote the national funds to research. We expect results which will be of benefit to us all. We realise that activities of to-day are the consequence of discoveries of the past: that, for example, Faraday's discovery of electro-magnetic induction laid the foundation of all the electric development of our time; that Pasteur's researches in bacteriology gave us new insight into the meaning of disease and new powers to fight it; and that the work of the organic chemist has given us a host of new colours for our pleasure. We know that in a thousand ways the results of research have added to our riches, removed our disabilities and eased our pain.

If science has also been turned to sad purposes, as in the poison gas, we can say justly that for one life it has been made to take by evil use, it has saved a hundred in the hands of those fit to employ it. We cannot escape from the need to defend our country and establish her in the world: it is true, however sad it may be, that if we do not develop research among us so that we may improve our fighting capacity, then other nations can bring us to defeat and shame. Further, if we fail to bring the efforts of research to the help of our industries, we run the deadly risk of losing our means of livelihood. Willy-nilly, we must research, and with all our energy.

[1] From an address delivered at the Sir John Cass Technical Institute on January 30.

Here are various reasons for the encouragement of research: the benefit of the student, the addition to human knowledge, power and riches, and the needs of defence, military and industrial. But I think we still have failed to include the most important reason of all, the real reason of which the others are only derivatives. It is that the spirit of research is like the movement of running water, and the absence of it like the stagnation of a pool. Scientific research, in its widest sense, implies, of course, far more than exploring the questions of physics and chemistry and biology. It is not a religion; but it is the act of one. It is the outcome of a belief that in all things which we try to do we may by careful seeking and by a better understanding do them better; that the world, far beyond what we can see of it on the surface, is full of things which it would be well for us to know. It is our duty and our gain to explore: we have always grown by doing so, and we believe that the health of our souls depends on doing so. Shall we sit still when there are difficult questions to solve; and when the answers may give us new insight and new power? There is a hesitation which would beg us not to push forward lest we come to think less of the world. As against this, research is an act of faith in the immensity of things. There is no end to the search: it is a poor thought that there might be.

The spirit of research would drive us all to work to the utmost of our power, believing that the more we do and the better we do it, the better for the work and lives of others. It is vigorous, hopeful, trustful and friendly; it adds always new interest and new life. It is a spirit which should run through all our activities, and not be found in laboratories only. It is, in fact, a spirit which is essential to us as a nation trying to rise above ourselves to better things. All our efforts to encourage research have before them not only one or other such immediate object like that to which I have already referred, but also this great ideal.

Lightning and High-Voltage Phenomena.

MANY estimates have been made of the voltage required to produce a lightning flash. It is now possible to give a rough estimate. The very high pressures at present used in the everyday transmission of electric power by engineers have led to researches in high-voltage phenomena, and results have been obtained which prove that the average voltage before a lightning flash occurs is of the order of 100 million volts. The General Electric Co. of America has built a million-volt (1·4 million maximum) transformer for testing the strings of insulators which are used to suspend the power lines in California where the pressures between neighbouring wires are 220,000 volts. It is also necessary to test the lightning protective devices used, and so a two-million-volt "impulse" generator has been built for this purpose.

Mr. F. W. Peek, the consulting and research engineer of the General Electric Co., describes in the January number of the Journal of the Franklin Institute some interesting experiments he has made with these machines and deduces important results. He finds that up to two million volts, the maximum highest voltage obtained, the ordinary formulæ for the sparking distances between spherical electrodes are true. Consequently, with very large electrodes at considerable distances apart, an average voltage of 150,000 per foot is required to produce a spark. Estimates of the voltage required for a lightning flash can therefore reasonably be made.

Mr. Peek has found that the laws for disruptive discharge between needle gaps, between spherical electrodes and for visual coronas found at low voltages, still apply at high voltages. He proves experimentally that the curve connecting the disruptive voltage for a needle gap with its length is a straight line at least up to two million volts. The curve for a spherical electrode gap is in accordance with the assumption that the disruptive discharge takes place when the maximum value of the potential gradient in the gap attains a definite well-known value depending on the size of the spheres. It was found by experiment that the "impulse" spark-over voltage of the needle gap varies with the wave shape of the impulse, but the sphere gap disruptive voltage is practically unaffected by the wave shape.

The experiments prove that the wet and dry spark-over voltages for strings of insulators are practically the same whatever the applied pressure. The voltages used in these experiments were of the same order as the "lightning voltages" which appear on power transmission lines. It is known that a definite amount of energy is required to rupture gaseous, liquid, and solid insulating materials. For example, when a voltage is applied at a very rapid rate, as by an impulse generator, breakdown does not occur at the same pressure as that required by a continuously applied voltage. In air with spherical electrodes the time lag is practically negligible, but with needle points it is easily measurable. Hence this can be used to measure the rapidity of the impulse.

Photographs are shown of a spark-over on a string of porcelain insulators protected by two large rings at each end. The discharge is seen taking place between the rings, the insulators being completely shielded. Lightning voltages cause coronas on the wires. It was found that a corona produced by a "lightning" voltage which lasted less than the millionth of a second could be easily seen. The fact that coronas are caused by these sudden voltages explains why lightning discharge rushes are so rapidly damped on transmission lines.

Mr. Peek calls lightning an "electrical explosion," and states that time periods of less than the millionth of a second are required for the discharge in model "lightning" generators in order to get trustworthy results. He uses a generator giving two million volts, and it sometimes produces a current of the order of ten thousand amperes. A photograph is shown of a large wooden post split and blown apart by the discharge. After the explosion the wood had the odour of the gases produced by destructive distillation, but there was no sign of burning. A plate of glass coated with metal was perforated at this pressure.

When lightning strikes the earth where the soil is sandy, its path through the sand is often defined by a long tube, which may have branches like a tree. These tubes, called fulgurites, are well known. It was found that they could easily be produced by artificial lightning.

Most of the disturbances caused on transmission lines are due to electrical induction and are not due to a direct flash striking the line. A neighbouring flash, however, can release the "bound" charge, which then travels over the line in each direction with a velocity comparable with that of light. As these lightning waves travel over the lines they are gradually dissipated by the losses which occur. The author has measured induced lightning voltages in transmission lines in Colorado as high as 500,000. From experiments with a model three-phase transmission line with a large plate to represent the cloud directly above it, he deduces that with a cloud 864 feet high and a transmission line 30 feet high, two out of every fifty flashes would strike the line, the remainder striking the earth.

Wooden logs can easily be set on fire by a spark from a lightning generator. This proves that there is a direct risk of fire. Tests are being carried out to determine the best methods of protecting buildings from lightning. Miniature buildings, clouds, etc., are being constructed accurately to scale. So far as the experiments have gone, they prove that lightning conductors have a real value in protecting buildings. Whether they increase the chance of a building protected by them being struck by lightning has still to be investigated. In an appendix the practical formulæ for sparking voltages between spheres and cylinders are given.

A. R.

Helium-filled Airships.

IN the whole history of science there is no more wonderful example of practical service arising out of purely scientific study than is afforded by the use of helium gas in airships. As readers of NATURE are well aware, the gas was first detected in solar prominences by Sir Norman Lockyer in 1868, and as he was unable then to identify it with a terrestrial element, "I took upon myself," he said, "the responsibility of coining the word helium." So the gas remained until 1895, when Sir William Ramsay obtained it from the mineral cleveite, and it has since been found in many types of rocks and minerals as well as in natural gases and spring-waters.

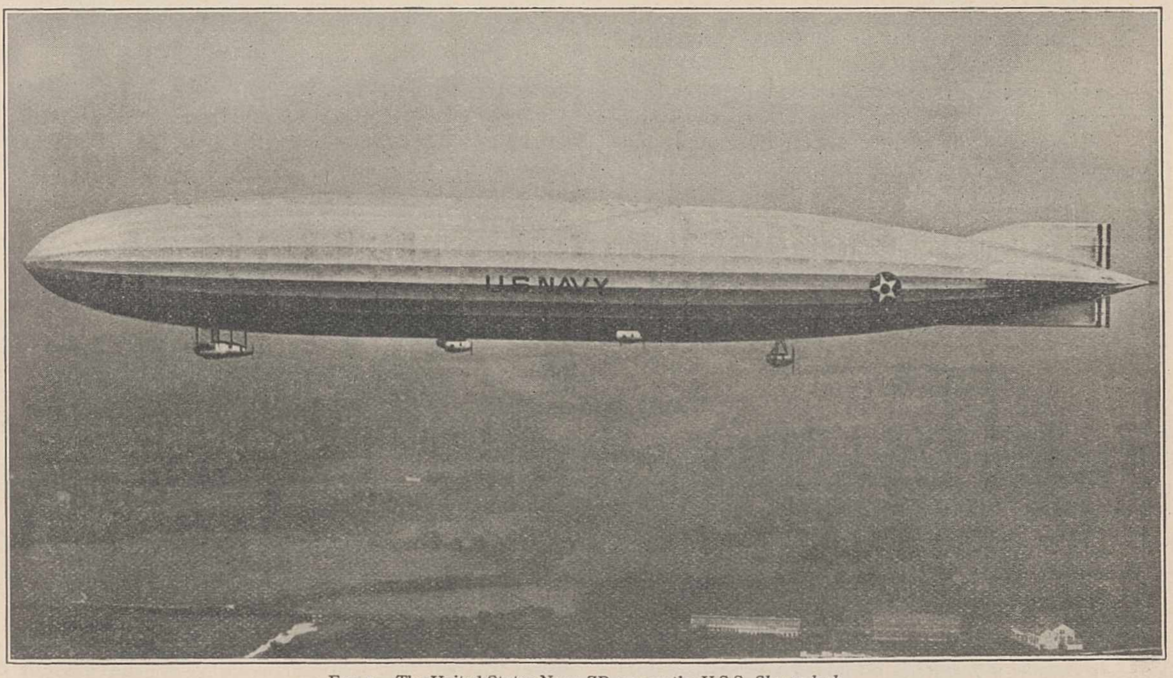

FIG. 1.—The United States Navy ZR-1, now the U.S.S. *Shenandoah*.

Length, 680 feet; diameter, 78 feet; height, 96 feet; gas capacity, 2,150,000 cubic feet; total lift (helium), about 130,000 pounds; dead weight, about 75,000 pounds; speed, about 70 miles per hour; horse-power, six 300-H.P. engines, total 1800 H.P.; cruising radius without stopping, more than 4000 miles; crew, 9 officers, 22 enlisted men.

In the year 1915 an inquiry was made into the helium content of natural gases within the British Empire from the point of view of their development for aeronautical purposes, and two years later a similar investigation was carried out in the United States, where the gas is now produced in sufficient quantity to inflate large airships.

Three months ago we wrote to Rear-Admiral W. A. Moffett, chief of the Bureau of Aeronautics, U.S. Navy Department, Washington, D.C., for particulars of this interesting development, and he has been good enough to send us the following details, with a photograph here reproduced (Fig. 1) of the large airship U.S.S. *Shenandoah*, which is inflated with helium. We are very glad to be able to publish this account.

The U.S.S. *Shenandoah* was authorised in 1919, construction was begun in 1920, and the ship made her first flight in September 1923.

The airship was designed by the Navy Department, fabricated by the Naval Aircraft Factory, Philadelphia, and assembled and erected at the Naval Air Station, Lakehurst, N.J. It is of U.S. Navy design and is operated by U.S. Naval personnel.

The structural frame-work is of duralumin, an alloy of copper, magnesium, and aluminium which has a tensile strength of more than 55,000 lb. per square inch and weighs about one-third as much as steel. It was necessary to develop the manufacture of this alloy, which was entirely new to us, in the United States before it was possible to commence the building of the airship. Two concerns, the Aluminium Company of America and the Baush Machine Tool Company, developed processes for its manufacture which have supplied material of a quality which is equal to that manufactured anywhere else in the world. The development of the art of the manufacture of duralumin is one of the incidental benefits to the United States from the construction of this airship.

The gas cells, which are lined with goldbeater's skin, were made according to the method followed in the manufacture of similar gas cells for the British rigids. It required 20 cells to equip the ship. These cells were manufactured by the Goodyear Tire and Rubber Company.

The ship is fitted with six 6-cylinder gasoline engines made by the Packard Motor Company. These engines will develop 300 H.P. at 1400 r.p.m., but are normally operated at around 200 H.P. The task set the designer and builders was to produce an engine equal to the German Maybach. From the results of tests it is believed that this requirement has been fulfilled.

The other items of the ship structure and equipment are generally similar to those of the German Zeppelins and the British rigids. An exception is the reduction gears used between the engines and propellers. These are of special design, using epicyclic gearing somewhat similar to the reduction gear at one time fitted on the Rolls Royce aeroplane engines.

The greatest novelty on the ship is of course its inflation with helium. This, as is generally well known, is a non-inflammable, non-poisonous, gaseous element, and the next lightest gas to hydrogen. It is found in the natural gas obtained from certain fields in Texas, Oklahoma, and Kansas. The proportion is very small, ranging from one-half to one and a half per cent. of the natural gas. The separation of helium from the other ingredients is done by means of refrigeration. Everything but the helium is liquefied and the gaseous helium can then be drawn off. A large plant for doing this work has been established at Fort Worth, Texas. The plant has a capacity of about 600,000 cub. ft. of helium per month.

This is not the first airship to be inflated with helium, as a non-rigid airship—the C-7—having a volume of 200,000 cub. ft., was inflated with helium from November 1921 to April 1922. During this time the ship was handled with great care to reduce the loss of helium to a minimum, and although it flew for about 18¼ hours, covering about 750 miles, there was no loss of helium through valving. This is in marked contrast to normal operation using hydrogen, in the course of which considerable quantities of gas are valved as a matter of course. The use of helium as an inflation gas for airships opens a new vista for lighter-than-aircraft. It is believed, however, that the present great cost will probably prevent its immediate use in commercial aircraft to any great extent. It may be possible to devise some method of protecting a hydrogen cell by a blanket of helium and valve from the less expensive gas. Such an arrangement, however, involves increased weight and additional difficulties in operation.

For lighter-than-aircraft intended for use in the presence of an enemy, the use of helium causes such a tremendous change in the possibilities that it is difficult to imagine just what may or may not be done. It appears certain, however, that the airship inflated with helium will be a much more powerful weapon than one inflated with hydrogen, and will be of immensely greater value as a fleet scout. At the present time, this latter use appears to be the only proper one for such aircraft.

One of the greatest benefits associated with the development of military aircraft, which are primarily intended for warfare, is the fact that, as already brought out, their development invariably leads to the production of materials and processes which are of immense practical value to industry in general, and thus add to the resources of the nation and of the world. Although the sword may not yet be beaten into a ploughshare, the quality of steel required for the making of swords makes possible better and tougher shares.

Current Topics and Events.

THE Council of the Royal Society, at its meeting on February 21, recommended for election into the Society the following fifteen candidates: Dr. Thomas Nelson Annandale, Mr. Joseph Edwin Barnard, Prof. James Fairlie Gemmill, Dr. Mervyn Henry Gordon, Prof. Percy Groom, Dr. Christopher Kelk Ingold, Prof. Percy Fry Kendall, Prof. Louis Vessot King, Prof. Louis Joel Mordell, Dr. Thomas Slater Price, Prof. Chandrashekara Venkata Raman, Prof. Leonard James Rogers, Dr. Alexander Russell, Prof. Charles Spearman, Mr. Frank Twyman.

DR. J. H. JEANS's discourse at the Royal Institution on Friday, February 15, on the origin of the solar system, which we reproduce in this week's supplement, is the necessary corollary of the system of cosmogony which he has previously built up from observation and gravitational theory. In the normal course of evolution of matter from a gaseous nebula into single, double, or multiple stars, there is no room for a solar system such as ours, and all the evidence indicates that the majority of stars have come into being along the normal course. It follows that the solar system is an exception. Instead of the imagined countless millions of suns, each with its family of inhabited planets, which appealed so vividly to the imagination of our grandfathers, we are left with a universe in which there may be only one other system like ours. The thought is perhaps no less awe-inspiring than that which it displaces. Dr. Jeans is forced, chiefly by a process of exhaustion, to the conclusion that external influences are responsible for the existence of the planets which attend our sun, and those influences he finds might very well have arisen from the close approach of a star in the very remote past. The chances of such an approach are exceedingly small, and the inference again is that there are very few, if any, solar systems other than those produced by the encounter referred to. In his book, "Problems of Cosmogony and Stellar Dynamics," published in 1919, Dr. Jeans asserted the probability that Laplace's nebular hypothesis would have to be abandoned. He now appears to have definitely rejected it, so far as the solar system is concerned, and to have employed its main principles in the larger problem of the origin of stars. Dr. Jeans apparently has not yet dealt with the problems of the asteroids and of Saturn's rings. In view of the analogy which he has drawn between the solar system and the systems of Jupiter and Saturn, possibly these two problems are ultimately one; although we are, in that event, confronted by the further problem of the absence of small satellite swarms from Jupiter. But the theory has made such a creditable beginning that we may view with equanimity, and even with confidence, its further development.

MR. NOEL BUXTON, Minister of Agriculture, speaking at a meeting of the Council of Agriculture for England on February 22, announced that Sir Charles Sherrington had consented to act as chairman of a committee to map out and undertake a field of research into the nature and treatment of foot-and-mouth disease. He stated that the Chancellor of the Exchequer had seen the vital importance of research, and was prepared to find the considerable sum of money which would be wanted. He hoped that exhaustive research would lead to a virtual eradication of the disease; but time was required, and they must not expect results for at least two or three years. In the House of Commons on Monday, February 25, Major A. G. Church suggested that there should be new blood on any such committee set up, and that the Medical Research Council should be asked to assist, because he thought the services of human pathologists were indispensable to the elimination of the disease. Mr. Buxton, in the course of his reply to the discussion of the subject, stated that both branches of scientific research—human pathology and veterinary science—will be represented on the committee, if possible in equal numbers.

IN the House of Lords last week, Lord Desborough asked for a report on the laboratory and field experiments carried out by the Joint Committee of the Ministry of Transport and the Ministry of Agriculture and Fisheries on the effects of washings from tarred roads on fisheries. Tar and tar products are acknowledged to be the most important of the poisonous effluents liable to be discharged into estuaries. Observation and experiment have proved that they are not only very destructive to fish, but also that the fish which manage to pass through the estuary acquire a tarry flavour which may be retained for many miles above. The pollution produced by the tarring of roads is of a local and intermittent character, caused by rains washing the tar products from the road into the stream or river which it crosses. According to circumstances of time and place, more or less destruction of fish and fish food is thus brought about. Mr. Butterfield's experiments and the work of the Joint Fisheries-Transport Committee have made this clear, and have demonstrated that the destruction can be easily avoided. The use of tar in the dressing of roads does no harm except in regions where the road crosses watercourses. At such places, if the road be dressed with bitumen instead of tar, the destruction of fish and fish food will be prevented. Experiments made on behalf of the Joint Committee in the laboratory and in the field have shown that the washings from different samples of asphalt were quite innocuous to fish. The report of the Joint Committee ought, therefore, to be published and circulated to County Councils and their Road Committees.

THE present attack of influenza is not severe, so far as can be gathered from the deaths shown in the weekly returns of births and deaths issued by the Registrar-General. Counting influenza as epidemic in London when the deaths from this cause exceed 20 per week for at least three or four consecutive weeks, the epidemic has been maintained since the week ending December 8. The deaths for London in any week did not exceed 30 until the week ending January 19, and it was not until February that the deaths exceeded 100, the highest record as yet being 178 in the week ending February 16. The age-incidence of death throughout has been for the greater part above 45 years, and in this respect it differs from the severe epidemic of 1918–19, and agrees with most other previous epidemics. The deaths from pneumonia and bronchitis, which as usual had increased in number as the influenza increased in virulence, each showed a slight decrease in the week ending February 16, when there was also a slight decrease in the deaths from all causes in London. There seems a strong probability that the influenza has attained its maximum severity. In the 105 county boroughs and great towns of England and Wales, the general death-rate had not yet shown a decrease. Previous epidemics have been almost entirely confined to the winter and early spring, but the occurrence or disappearance with periods of warm or cold weather is by no means marked. The present epidemic is the thirty-third since 1890; an epidemic has occurred in each year with the exception of 1896 and 1901, but in many years the attack has been very slight.

A TABLET was unveiled last June in the Goshen Pass, Virginia, U.S.A., to the memory of M. F. Maury, the American hydrographer, who in 1854 published the first bathymetrical chart of the North Atlantic, and in the following year produced his "Physical Geography of the Sea," which was one of the foundations of the science of oceanography. In unveiling the tablet, Prof. C. A. Smith gave an address on the life and work of Maury (Alumni Bulletin, University of

Virginia, January 1924), dwelling especially on his efforts to promote the study of meteorology and the establishment of a weather bureau to benefit both sailors and farmers. Prof. Smith has in preparation a memoir on Maury, and would be grateful for any reminiscences by those who knew him personally. They should be addressed to U.S. Naval Academy, Annapolis, U.S.A.

On March 5 occurs the tercentenary of the birth of John Collins, "a person of extraordinary worth," "the attorney-general for the mathematics," sometimes called the English Mersenne. Collins was the son of a nonconformist divine and began life as an apprentice to an Oxford bookseller. Showing a taste for mathematics, he received help from one of the Royal household. As a young man, during the Civil War, he went abroad, and it is said fought for the Venetians against the Turks. Returning home, he set up as a teacher in London, became an accountant, wrote on merchants' accounts and also on quadrants, dialling, and navigation. At one time he lived in Fenchurch Street, at another in Garlick Hill, and when he died he was buried in St. James's Church close by. Widely known in his day, he was admitted a fellow of the Royal Society in 1667, which then met in Gresham College, Bishopsgate Street. He was an indefatigable correspondent. After his death, letters were found among his papers from Newton, Gregory, Wallis, Flamstead, Slusius, Leibnitz, and others, and in 1712 the Royal Society ordered the publication of some of them. To Collins we also owe partly the printing of some of the works of Barrow, Kersey, Brancker, Wallis, and Horrocks. He died November 10, 1683.

In the development of the steamship no one did more important work than John Elder, the centenary of whose birth falls on March 8. Born in Glasgow in 1824, Elder was the son of one of the best practical engineers of the time, who was then foreman to Robert Napier. After rising to the position of chief draughtsman in Napier's works, Elder in 1852 joined the firm of Randolph, Elliot and Co. as a partner, and it was from this connexion that sprang the well-known Fairfield Shipbuilding and Engineering Co., Govan. In Elder Park, close to the works, is a statue to Elder, and this, we believe, is the only statue ever erected to a marine engineer. Elder's greatest contribution to steamship development was the compound steam engine. He took out a patent in 1853, made his first compound engine in 1854, and, though most shipping companies were against it, Elder proved its value in the vessels of the Pacific Steam Navigation Co. Compound engines were not tried in the British Navy until 1865, and the Cunard Company's first compound driven ship was not built until 1870. Elder died in 1869, but in the end his ideas proved right, and combined with surface condensers and moderately high-steam pressures the compound engine reduced the coal per indicated horse power per hour from more than four pounds to less than two pounds. Elder's work as a shipbuilder began in 1860, and his yard at Govan soon became the most important of all Clyde shipyards. A man of the highest integrity, he did much to further industry on the Clyde, and the chair of naval architecture in the University of Glasgow was founded in memory of him by his widow.

Prof. Max Planck has been elected an honorary fellow of the Physical Society of London.

Dr. F. W. Aston will deliver the annual May lecture of the Institute of Metals on June 4, his subject being "Atoms and Isotopes."

M. le Duc de Broglie will deliver the Guthrie lecture of the Physical Society of London on Thursday, March 20, as part of the Society's jubilee celebrations.

Prof. A. V. Hill will deliver the third Joule Memorial lecture of the Manchester Literary and Philosophical Society on Tuesday, March 4. His subject will be "Thermodynamics in Physiology."

Sir Sidney Harmer is to deliver a lecture at 3 p.m. on March 1 on "Whaling Research and the new *Discovery* Expedition" at a meeting of the Gilbert White Fellowship to be held at the Art-Workers' Guildhall, 6 Queen Square, Southampton Row, W.C.

A senior research chemist is required by the Research Association of British Flour-Millers. Candidates must have had research experience in physical or colloid chemistry, or in industrial applications of the same. Applications should be sent to the Director of Research, Research Association of British Flour-Millers, 40 Trinity Square, E.C.3.

The eleventh election to Beit fellowships for scientific research will take place on or about July 16 next. Forms of application and all information may be obtained by letter, addressed to the Rector, Imperial College of Science and Technology, South Kensington, S.W.7. The latest date for the receipt of applications for the fellowships is April 19.

Prof. Hans Driesch, of the University of Leipzig, will give four lectures in English on "The Possibility of Metaphysics" at King's College on March 12, 14, 18, and 19, at 5.30, at the invitation of the University of London. Prof. Driesch is widely known for his work in biology, the result of researches at the Zoological Station in Naples in the years 1897–1900. He was appointed Gifford lecturer at Aberdeen in 1907–8 and propounded the new vitalistic theory with which his name is now generally associated. It was the difficulties of this theory which led him to the study of the philosophical problem of the relation of mind and body.

It is announced that Lord Atholstan has offered to co-operate with the Quebec Government in a campaign against tuberculosis, towards the cost of which he will make a personal contribution of 500,000 dollars. He suggests that measures should be taken similar to those followed in Framingham, Massachusetts. This was an "experiment" conducted at the instance of the Metropolitan Life Insurance Company with the aid of the National Tuberculosis Association of America. It consisted essentially of a house-to-house medical inspection and sickness census, by means of which early cases of tuberculosis might be detected and placed at once under appropriate treatment.

THE gold medal of the Institution of Mining and Metallurgy, the premier distinction within the gift of the Institution, has been awarded conjointly to Mr. H. W. Gepp and Mr. G. Rigg in recognition of their joint and individual services in the advancement of metallurgical science and practice, with special reference to their achievements in the treatment of complex sulphide ores, and in the development of the electrolytic process for the production of zinc in the Commonwealth of Australia.

A BOTANIST is required by the Research Branch of the Agricultural Department of the Gold Coast. Candidates must hold an honours degree in natural science of a British university with special qualifications in botany and be able to carry out systematic research on the local flora and be familiar with herbarium routine. A knowledge of plant-breeding as applied to economic crops is desirable. Further information and application forms may be had upon written request to the Assistant Private Secretary (Appointments), Colonial Office, Downing Street, S.W.1.

AN agricultural chemist is required in Sierra Leone to take charge of the newly created division of research of the Lands and Forests Department of the colony. Candidates are expected to possess qualifications similar to those required for the certificate in agricultural chemistry (Branch D) of the Institute of Chemistry and to have had practical experience if possible in the tropics. The person appointed will investigate the composition and action of the soils of Sierra Leone and scientifically investigate the oil-palm industry. Forms of application are obtainable by writing to the Assistant Private Secretary (Appointments), Colonial Office, Downing Street, S.W.1.

THE following officers and council of the Royal Astronomical Society were elected at the annual general meeting held on February 8:—*President*, Dr. J. L. E. Dreyer; *Vice-Presidents*, Dr. A. C. D. Crommelin, Prof. A. S. Eddington, Prof. A. Fowler, Mr. E. B. Knobel; *Treasurer*, Lieut.-Col. F. J. M. Stratton; *Secretaries*, Dr. John Jackson, Rev. T. E. R. Phillips; *Foreign Secretary*, Prof. H. H. Turner; *Council*, Prof. A. E. Conrady, Mr. C. R. Davidson, Sir Frank Dyson, Mr. John Evershed, Dr. J. W. L. Glaisher, Mr. P. H. Hepburn, Dr. Harold Jeffreys, Prof. F. A. Lindemann, Mr. E. A. Milne, Prof. H. F. Newall, Mr. J. H. Reynolds, Mr. H. Thomson.

THE following officers and members of council of the Physical Society of London were elected at the annual general meeting on February 8:—*President*, Mr. F. E. Smith; *Vice-Presidents*, Dr. E. H. Rayner, Dr. J. H. Vincent, Mr. T. Smith, and Mr. C. R. Darling; *Hon. Secretaries*, Dr. D. Owen, Prof. A. O. Rankine; *Hon. Foreign Secretary*, Sir Arthur Schuster; *Hon. Treasurer*, Mr. W. R. Cooper; *Hon. Librarian*, Mr. J. H. Brinkworth; *Other Members of Council*, Mr. R. W. Paul, Prof. C. L. Fortescue, Dr. W. S. Tucker, Prof. S. W. J. Smith, Dr. J. S. G. Thomas, Mr. J. Guild, Dr. F. L. Hopwood, Dr. E. A. Owen, Dr. J. Robinson, Dr. G. B. Bryan.

ON Friday, February 15, the degree of doctor of science was conferred by the University of Leyden on Mr. Francis Arthur Freeth, for a thesis on "The Ternary and Quaternary Equilibria in the System Sodium Perchlorate-Ammonium Sulphate-Ammonium Perchlorate-Sodium Sulphate-Water." The examination was held as a "Public Promotion," and was conducted by Profs. Schreinemakers, Kamerlingh Onnes, Blanksma, van Itallie, and Donnan, and Drs. Crommelin and Jorissen. At the conclusion of the public examination, Prof. Schreinemakers, who acted as "Promoter," addressed the candidate in French. In the course of his remarks he said: "Vous n'avez pas seulement étudié profondément les problèmes théoriques de la doctrine des phases, mais, comme Directeur du Laboratoire scientifique de Brunner, Mond & Co., un des plus grands établissements chimiques de l'Angleterre, vous avez appliqué cette doctrine à la pratique d'une manière splendide."

IT is announced in the *Times* that the Société de Géographie of Paris has decided to award its gold medal to the French explorer M. Bruneau de Laborie, who recently returned from a journey in the little-known parts of the central Sudan and Sahara, during which he crossed the Libyan desert from Lake Chad by Borku and Tibesti to Kufra and Egypt. In 1920–22, M. B. de Laborie successfully completed another important journey across the Sahara. Commissioned by the French Government to report on the economic possibilities of French Equatorial Africa to the east and south of Lake Chad, he travelled from West Africa through Nigeria to Kano. Thence he passed eastward, explored the basin of the Shari and the Wadai region as far as Abeshr, returning north of Lake Chad and crossing the Sahara by Zinder and In Salah to Algiers.

PROF. R. A. FESSENDEN, of Chestnut Hill, Mass., writes to urge the desirability of excavation in the Caucasus with the view of recovering records of pre-Deluge civilisation. Prof. Fessenden holds the view that the Caucasus was the seat of a civilisation antedating those of Babylon and Egypt, of which the records should be found between the Terek and Sunsha and in the Upper Alizon valley. He points out that traditions handed down by Manetho, Sanchuniathon, the Phœnician, and Josephus agree in stating that records made before the Flood were on pillars and written in hieroglyphs, that the pillars were in the Seriadik country, and were made by the sons of Sydyk or of Seth, and that after or before the Deluge copies were made in hieroglyphs on the walls of, or in books deposited in, an extensive system of underground chambers. Arguing partly by the method of exclusion, partly from the identification of place names, Prof. Fessenden concludes that these pillars—sometimes confused with the Pillars of Hercules, which were not situated at the Straits of Gibraltar—are to be located, one, of brick, at Pssidache between the Terek and Sunsha; the other, of stone, at Kapareuli, which equates with the Sippara of tradition. The underground chambers in which the records from the pillars were stored were situated one at each of these spots. The tradition of Berosus that the writings of Sippara were dug up at Babylonia after the Flood would refer to an original Babylonia in the Caucasus, the plan and dimensions of which Nebuchadnezzar followed in rebuilding Babylonia on his return from an expedition to the Isthmus. The records would have been replaced for safety, and Prof. Fessenden thinks might now be recovered by excavation.

WE are informed by Mr. A. F. Bird, 22 Bedford Street, Strand, W.C.2, that he has been appointed English agent for the sale of the books published by the Chemical Catalog Co., Inc., of New York.

To celebrate its moving into new and more spacious quarters in Warwick Square, the Oxford University Press is holding a luncheon on Tuesday, March 4, at the new building, at which the Marquess Curzon, Chancellor of Oxford University, will preside. This removal from the well-known Amen Corner building, which it has held for forty-one years, marks one more interesting stage in the long history of the growth and expansion of the Oxford University Press.

MESSRS. BERNARD QUARITCH, LTD., 11 Grafton Street, W.1, have just circulated a catalogue (No. 382) of second-hand books and pamphlets—upwards of 1200—on botany, the history of botany, medicine, the history of medicine, etc., mainly from the library of the late Dr. E. Bonnet. Many choice and rare works are listed, and the catalogue should be of interest to many readers of NATURE.

Our Astronomical Column.

THE STATIONARY CALCIUM CLOUDS IN INTERSTELLAR SPACE.—Mr. Evershed contributes a letter on these clouds to the February issue of the *Observatory*. He notes that, at Kodaikanal, prominences have been photographed in the light of the H and K lines which were leaving the sun with accelerating velocity, and so presumably would be driven into outer space. This would be still more the case in stars of types B and A, on account of the greater radiation pressure at their surfaces. There must thus be a continual outpouring of calcium, and other gases, into space. Equilibrium is supposed to be eventually reached between the resultant radiation pressures and gravitational attractions due to all the stars and other masses in the stellar universe. It is noted that the fineness of the H and K lines in question shows that the calcium gas is not only at rest with regard to the system of stars, but is also in a state of "supreme internal tranquillity."

FAINT STARS WITH LARGE PROPER MOTION.—The number of faint stars known to be moving rapidly is mounting up considerably as the plates taken with the astrographic telescopes at an interval of 20 or 30 years are examined with the blink microscope or otherwise.

Dr. Innes gives a list of 53 rapid movers in Union Observatory Circular, No. 58; some stars so faint as 14 mag. have motions exceeding a second per annum. These are evidently dwarf stars not very remote; they should be examined for parallax where practicable.

Prof. Max Wolf notes in *Astr. Nach.* 5262 a large proper motion (1·811″ annually in Position Angle 120°) of a star of mag. 11 in Gemini. He notes that it is now very near a star of mag. 16, and suggests that the latter should be examined for Einstein displacement. A little consideration will show that the detection of this is practically hopeless. Assume the diameter of the near star to be 0·001″, then at a distance of 0·1″ from it the Einstein deflexion would be of the order of 0·01″, which could not be detected. Total solar eclipses appear to afford the only practicable method of determining this shift.

SYSTEMATIC ERRORS IN BOSS'S PROPER MOTIONS.—It has long been known that different values for the declination of the solar apex are obtained from the discussion of the proper motions of stars of different magnitudes. It was suggested some years ago by Prof. Kapteyn that the cause of this anomaly might be systematic errors in the fundamental catalogues. Such errors are easily explicable by imperfect reduction of the older catalogues used in obtaining the proper motions. The division errors of the instruments then in use are not so well known as those of modern instruments. It is clear that errors of this character have more effect on the direction of the small proper motions than on the large ones. Mr. William B. Varnum contributed a paper on the subject to the 1923 meeting of the American Astronomical Society. His new value for the solar apex is R.A. 271·24°, Decl. +26·72°, which is in better accord than the earlier value with the result from radial velocities.

The correction to the Boss proper motions in declination at Decl. +39° is +0·0043″ per annum, which is in good accord with the +0·0048″ found by the latitude observers from the apparent change of latitude of their stations. Mr. Varnum finds the large correction to Newcomb's equinox of −0·057$^{sec.}$ in 1908. A correction of approximately the same magnitude has been indicated by the results of all the leading observatories. The Cape Observatory has applied the correction in its last catalogue, but some observatories prefer to wait for common agreement before doing this.

THE CAPE CATALOGUE.—The great work inaugurated by the Astronomische Gesellschaft some fifty years ago, of publishing accurate positions of all stars down to the ninth magnitude, is now almost complete. The present Cape Catalogue of 20,843 stars between Declinations 40° and 52° south is the latest contribution to it.

It differs from most of the other volumes of the series in having been derived mainly from photographs, namely, those taken for the great Astrographic Catalogue. The latter catalogue does not give the positions of the stars in Right Ascension and Declination, but in rectangular co-ordinates; but it was felt to be desirable to reduce the positions of the brighter stars to R.A. and Decl.

Several of these stars (sufficient to form the plate constants) have been observed with the meridian instrument at the Cape; as the dates of these observations are not far distant from those of the photographs, proper motion cannot introduce much error, and does not appear to have been considered; but the errors of the plates, including a scale effect dependent on magnitude and the effect of tilt of plates, have been carefully studied, so that the positions, which are for the equinox of 1900·0 (the mean date of observation or photography is generally within five years of 1900), should be very precise. Many stars of magnitude 10 are included, and both photographic and visual magnitudes are printed, thus giving an idea of the colour index of each star.

Research Items.

STONE CELTS FROM THE NAGA HILLS.—An interesting sidelight is thrown on the stage of culture of early races inhabiting the Naga Hills by Mr. J. H. Hutton in the course of a description of two stone celts, of types not hitherto known, which appears in the February issue of *Man*. Of these celts, one is of exactly the same type as a celt found in the Malay Peninsula, with which an iron hoe from the Naga Hills has been compared, and, on the basis of this comparison, regarded as evidence of the Mon-Kmer occupation of the Naga Hills. The celt now described by Mr. Hutton would thus form a link between the Mon-Kmer implements of the Malay Peninsula and of Chota Nagpur, and would also indicate that some branch of the Mon-Kmer race inhabited or passed through the Naga Hills before it had learned the use of iron.

MIGRATIONS OF THE GYPSIES.—A noteworthy contribution to the discussion of the gypsy problem is made by Dr. J. Sampson in vol. ii. pt. 4 (New Series) of the Journal of the Gypsy Lore Society. While the European gypsy dialects have been carefully studied, no information was available as to the Asiatic, excepting the Armenian, until Prof. R. A. S. Macalister published his account of the language of the Nawar of Palestine a few years ago. It is now apparent that the gypsy languages fall into a western and an eastern group. This separation took place in Persia, which the gypsies entered from India before A.D. 900. The affinities of the original tongue which they then spoke are still not clear. Their subsequent history, as suggested by a study of loan words in their vocabularies, would appear to be somewhat as follows. After a stay of some duration in Persia, one group, called the *Ben* group on phonetic grounds, travelled south into Syria, becoming the ancestors of the Nawar of Palestine, the Kurbat of Syria, the Karači of modern Persia and Trans-Caucasia, and the Helebi of Egypt. The other, the *Phen* group, after settling for a time in Armenia, migrated westward through Kurdistan and Byzantine Greece, and reached the Peloponnesus before the end of the eleventh century, whence, *circa* 1440, they overran Europe.

THE CULT OF DAGAN.—In a note on "The Inscriptions of the King of Agade" in the *Museum Journal* (Philadelphia), vol. xiv., No. 3, Prof. Legrain discusses *inter alia* the significance of a reference to the god Dagan in the lately recovered fragment of the Nippur tablet which was discovered by the third Nippur Expedition in 1894. The main portion of this tablet was published by Poebel in 1914. In the fragment which is the subject of the note, Sargon of Agade, after recording his victories in the south, which gave him access to the sea, states that he worshipped Dagan in Tutuli and gave unto him the upper land, Mari, Iarmuti, and Ibla as far as the cedar forest and the silver mountain. The significance of this is twofold. In the first place, it indicates that Sargon's successful campaign to the north along the Euphrates extended to, probably, the Lebanon and the Taurus; and secondly, that the place of the god Enlil, to whom the honour of Sargon's southern campaigns is attributed, has been taken by a new god, the centre of whose cult was at Tutuli, a site not yet exactly located but possibly to be identified with Hana. The shrine of Dagan marks one of the early centres of Amorite culture. Prof. Legrain argues that the references to Dagan in a contract tablet of Hana and in personal and place names suggest a wide spread of Amorite influence in Samara and Babylonia, and a possible extension to the Hittite land and along the commercial road that led toward North Syria and Cappadocia.

EARLY DEVELOPMENT OF THE MAMMALIAN HEART.—Dr. Katherine M. Watson (*Jour. Anat.* vol. lviii. pt. 2, p. 105, 1924) gives an account of the origin of the heart and blood-vessels in the cat, with special reference to the work of Wang on the ferret, in which the heart was described as originating as a single median endothelial tube in direct communication with the two vitelline veins, one on each side, this rudiment later separating into two distinct endothelial tubes. This method of origin of the heart is contrary to the generally accepted view of what takes place in mammalia, and Dr. Watson, in criticising Wang's results, concludes that Wang's "median heart rudiment" represents a ventral aorta which has developed and fused in the middle line earlier than it does in most mammals, and that the "vitelline veins" described by Wang as communicating with the heart rudiment are in reality the true original separate lateral endothelial tubes of the heart, which, therefore, in the ferret develop in the way typical of mammalia generally.

PRESERVATION OF ZOOLOGICAL SPECIMENS IN FLUID.—There are many media and methods suitable for this purpose, but the difficulty always has been to retain the natural colour. In the *Museums Journal* for February, Mr. J. Ritchie, of the Perth Museum, describes a method by which he claims to have retained the colour in creatures so diverse as a salmon parr and a slug, and to have so fixed it that they have been exposed to strong sunlight during three years without fading.

NORWEGIAN HAWKWEEDS.—The second part of Bergens Museums Aarbok for 1921–22 (1923) consists of a description by S. O. F. Omang of the Hieracia of the Hardanger district, based on a collection made by the late S. K. Selland and now in Bergens Museum. While not claiming completeness, or still less finality, for his study, Mr. Omang describes no less than 386 forms, of which 51 rank as varieties; and of these 111 are claimed as new species and 31 as new varieties. The flora is a mixture of continental and coast forms; many belong to the Atlantic flora and have a westerly extension over the British Isles. The memoir is in Norwegian, but the descriptions of all new forms are in Latin.

ANTARCTIC ANTIPATHARIA AND GASTROPODA.—In the Reports of the British Antarctic (*Terra Nova*) Expedition, Mr. A. Knyvett Totton (vol. v. pp. 97–120) describes the seven species of Antipatharia obtained off North Cape, New Zealand, and brings together the references to polychæte commensals and cirripede commensals of Antipatharia. Dr. Nellie B. Eales (vol. vii. pp. 1-46) gives an account of the more interesting features in the anatomy of the Gastropoda (except the Nudibranchiata). Both accounts are adequately illustrated by good figures.

THE SUPPOSED WESTERLY DRIFT OF GREENLAND.—Dr. Wegener partly based his theory of the movement of the continents upon the evidence available with regard to the increased longitude west of Greenwich of certain stations in Greenland. In the *Geographical Journal* for February, Sir C. Close examines critically certain longitude determinations, including those made use of by Dr. Wegener, in Greenland. His conclusions with regard to Godthaab are of great interest. Observations were taken there by Falbe and Bluhme in 1863, Ryder in 1882–83, and Jensen in 1922. In the first two cases the method adopted was that of moon culminations; Jensen received Greenwich time by wireless signals and determined local time by star-transits. The mean of six determinations in 1863 was, in seconds of time, 55·4; and of ten in 1882–83, 52·8 sec. This would imply an easterly, not westerly movement, but, as Col. Close points out, the uncertainty attached to values of longitude derived from moon culminations precludes such an inference. Jensen's final value in 1922 was $58{\cdot}7 \pm 0{\cdot}1$ (omitting again hours and minutes). This was assumed to be the most accurate of all the determinations. It differs from the mean of the two earlier means by 4·6, or from the mean of eighteen separate values in 1863 and 1882–83 by 4·9. Col. Close maintains that the proper conclusion from these data is that the mean of the eighteen values of longitude derived from moon culminations was about 5 seconds in error, a very likely result, and not that the position of Greenland is proved to have changed.

Soda Deposits of Lake Magadi.—The valuable and extensive soda deposits of Lake Magadi in Kenya Colony form the subject of a report in the Bulletin of the Imperial Institute, vol. xxi. No. 3. The lake, which has an area of about 30 square miles, occupies a narrow fault trough in the rift valley. No permanent fresh-water streams enter it, and it has no outlet. The soda deposits cover the whole width of the trough for a length of 12 miles. Near the shore there is only a thin layer over the mud of the lake bottom, but at 200 yards from the shore the crust of soda is at least 6 feet thick and appears to be on the floor of the lake. The amount of crude soda available is estimated to be more than 100 million tons. The liquor from which the crystalline crust has separated contains sodium chloride and some other salts, but the separated crystals are practically pure sodium sesquicarbonate. A number of analyses from different parts of the lake are given. The deposits are apparently derived from nepheline and other soda minerals, which are abundant in the rocks of the rift valley. Seepage waters, the solvent action of which is intensified by much carbon dioxide in solution, the outcome of recent volcanic activity, penetrate the rocks along cracks and fissures to a considerable depth. Eventually these waters are returned to the surface in the form of hot springs which discharge their alkaline waters into the lake. Calcining plant has been erected on the shores of the lake, and considerable amounts of soda are being exported by the Uganda railway and the port of Kilindini.

The Influence of Soil Acidity on Snails.—Dr. W. R. G. Atkins and Dr. Marie V. Lebour (Sci. Proc. R. Dublin Soc., vol. xvii. No. 28) have collected snails from various localities with the view of ascertaining how far the hydrogen-ion concentration of the soil exercises an effect upon the distribution of the snails. The results show that snails are commonest at P_H 7-8; that the number of species of snails found in the districts studied increases from P_H 5—four species—to P_H 7—twenty species—falling at P_H 8 to fourteen species, out of the total twenty-seven species found. Snails with hyaline shells may be found in any portion of the range, but those with calcareous shells are limited to the more alkaline end. Granite and quartzite regions have few species, basaltic districts have more, and in limestone areas both species and numbers of individuals give high values. Dr. Atkins (*Parasitology*, vol. xv.) points out that the distribution of the snails which act as intermediate hosts for trematodes (*e.g.* Schistosoma and Fasciola) is no doubt affected by the hydrogen-ion concentration of the water and soil, and urges the importance of measurements of P_H (by the colorimetric method) by field workers on the distribution of animals.

Camphor Cultivation in India.—The camphor tree, *Cinnamomum camphorum*, can certainly be grown successfully in India and Burma. It is another question, however, whether plantations for the commercial production of camphor can compete with the Japanese supply, although the Japanese forests are now exploited as a Government monopoly and the export carefully controlled. In any case, S. H. Howard, W. A. Robertson, and J. L. Simonsen, of the Forest Research Institute, Dehra Dun, India, have certainly performed a useful service in rendering available as part vii. of vol. ix. of the *Indian Forest Records* their experience in the cultivation and distillation of camphor. A small plantation was established at Dehra Dun in 1898, and has since been coppiced. The young shoots of the upper portions of the trees have then been cropped at various seasons and at varying intervals, the leaves collected and weighed, and afterwards the yield of solid camphor and camphor oil determined on the original dry-leaf weight as the result of careful, small-scale distillation processes. When the leaves are thus distilled instead of the timber, as in the Japanese practice, the valuable constituent, safrol, proved to be missing from the oil, which has therefore lower commercial value. It is concluded that, at the present price of camphor, its economic production in India seems unlikely, although better yields of camphor may be expected under the conditions prevailing in Southern India. Camphor plantations are usually grown from seed, but the authors doubt the advisability of this method, and in any case, if selection for yield is first practised, vegetative propagation is likely to follow. In this connexion an interesting note has been issued from the Edinburgh Royal Botanic Gardens (Transactions and Proceedings of the Botanical Society of Edinburgh, vol. 28, part 4) by Miss Oona Reid upon Mr. L. B. Stewart's recent observation that the rooting of cuttings of camphor can be materially accelerated by a previous etiolation of the growing branches.

The Pressure Effect in Discharge Tubes.—In the issue of the Journal of the Franklin Institute for December 1923, Dr. I. Langmuir, of the Research Laboratory of the General Electric Company, gives a theory of the discharge through a monatomic gas at low pressure which he considers explains the observed motion of the gas towards the anode. The electrons in the positive columns have velocities which decrease with increase of pressure of the gas and are in accord with Maxwell's law. Bodies in contact with the gas become charged negatively and repel the electrons, but attract the positive ions, the impact of which on the body raises its temperature. The momentum they communicate to the walls of the tube can be calculated and is found to agree with observation. It is this communication of momentum which in Dr. Irving's opinion accounts for the motion of the gas to the anode. The tendency of bodies in the ionised gas to become negatively charged introduces an error of 5 to 15 volts into the measurement of the potential in the gas by means of exploring electrodes.

X-Ray Diffraction in Liquids.—When a narrow beam of X-rays passes through a thin layer of liquid or amorphous solid and falls on a photographic plate, the image is found to consist of a central spot due to the undeviated beam and one or more rings separated from each other and from the central spot by relatively clear spaces. These rings have been attributed to the regularity of structure of the liquid as they were known to be in a crystalline solid, but the law according to which the intensity of the diffracted beam falls away rapidly on the inside and slowly on the outside of a ring has not hitherto been explained. Prof. C. V. Raman and Dr. K. R. Ramanathan in part 2 of vol. 8 of the Proceedings of the Indian Association for the Cultivation of Science trace the distribution of the scattered rays to the deviation of density in the medium from its mean value. The probability of a given deviation is assumed to decrease exponentially with the work required to produce the deviation by compression or extension. This hypothesis reproduces Hewlett's experimental determinations very closely.

THE ORIGIN OF SPECTRA.—Prof. F. A. Saunders's address as chairman of the Physics Section of the American Association for the Advancement of Science, on "Some Problems of Modern Spectroscopy," is reproduced in *Science* of January 18. Following a very brief review of the history of spectroscopy, Prof. Saunders considers the Bohr theory of the origin of spectra from the general and certain special points of view. He refers to the suggestion of Lewis, whose static model of the atom has been so successful in explaining chemical processes, that this model might be harmonised with Bohr's conceptions by identifying an electron with its orbit. Such an identification removes certain difficulties, but it is not at all clear that it does not introduce more. The tendency of electrons, at least in the presence of an atomic nucleus, to occur in pairs is referred to—a tendency of which little account is taken in Bohr's theory. Perhaps the most suggestive part of the address is that referring to the work now being carried on by Prof. Saunders himself and Prof. H. N. Russell. Certain groups of six lines—pp' groups—in the spectra of the alkaline earths have been studied, and appear to be formed by jumps of an electron from the lowest of the triplet p levels to other triple levels which appear to have no connexion with the ordinary series structure of the spectrum. These groups now appear to belong to series of their own, but they involve negative "terms." This means that in certain radiations more energy is emitted than the electron possesses, and the balance of energy is assumed to come from the other valence electron. This, as Prof. Saunders says, involves a nice degree of co-operation between the two electrons. Recent work on ionisation potentials is mentioned, and useful diagrams are given and commented upon, showing the variation of ionisation potential from element to element.

SENSITIVENESS OF SILVER BROMIDE EMULSIONS.—Mr. Walter Clark, of the British Photographic Research Association, publishes a further instalment of his work on the sensitiveness of silver bromide in emulsions in the Journal of the Royal Photographic Society for February. All the results given are in accordance with the view that the high sensitiveness of the silver halide grains in a photographic emulsion is due primarily to the presence in them of a limited amount of material other than normal silver bromide, which confers extra sensitiveness at points (or "centres") in the grains. The experiments described are investigations of the properties of plates prepared with an emulsion diluted so that there is only a single layer of silver-salt grains. Such plates were exposed to light, and desensitised by soaking in a dilute solution of chromic acid. The desensitising brings about a lateral shift of the characteristic curve. The smaller grains are relatively more desensitised than the larger, and there is a certain minimum sensitiveness, varying with the character of the emulsion, which the chromic acid seems unable to destroy. This varies from 5 H. & D. in the most rapid to 1 H. & D. in a "process" emulsion. A preliminary treatment with a dilute solution of sodium arsenite has the same effect as a preliminary exposure to light.

AN ELECTRICAL DENSITY METER.—The *British Journal of Photography* for February 15 gives an account of a paper read before the Royal Photographic Society on February 12 by Messrs. F. C. Toy and S. O. Rawling, of the British Photographic Research Association, on "A New Electrical Density Meter." The authors point out the disadvantages of the ordinary apparatus, in which the adjustment is made by comparing the brightness of two illuminated patches. Their apparatus has a single light-source (a locomotive head-light), from which two opposite beams are converged by a suitable optical system, so as to give two adjacent D-shaped patches of light, which are adjusted to meet accurately without overlap. A selenium cell is so arranged that it can be moved from one patch to the other without discontinuity of illumination. One beam passes through a calibrated optical wedge and the other through the density to be measured, and a mirror galvanometer shows when equality is established. The values obtained differ from those obtained visually, because the maximum sensitiveness of the selenium cell is in the infra-red, and the difference appears to be in the constant ratio of 0·74 to 0·76. The accuracy of the apparatus appears from preliminary trials to be within one per cent.

ELECTRICAL POWER IN OILFIELD DEVELOPMENT.—In most oilfields, the power commonly employed in working the drilling plant is steam, favoured mainly by economic considerations and particularly because the steam-engine is simple in design, sensitive to speed control, and capable of heavy overload-capacity, all factors of supreme importance in drilling oil-wells. The advent of the electric motor into oilfield equipment has been slow, but of late years on large oilfields, where one or more electrical generating stations are bound to be erected for various requirements, the use of electric motors for actual development-drilling has been commended as both practicable and economical, and in many respects superior to steam-plant. This was the essence of Mr. C. H. McCarthy-Jones's paper on "Electricity applied to the winning of Crude Petroleum, with special reference to the Yenangyoung Field, Burma," read before the Institution of Petroleum Technologists on February 12. The author, in discussing the cable-tool method of drilling in this connexion, pointed out that the essentials of a motor designed for this purpose included ability to withstand rough usage and racking strains which the system entails, a very wide and close speed control, an exceptional overload torque capacity to deal with temporary loads such as were occasioned by bailing and casing strings, and as low an inertia of the moving parts as possible; all these essentials, he maintained, were present in up-to-date motor plant designed for oilfield work. With the rotary system of drilling he was similarly optimistic, though he was careful to point out that in many cases of the employment of electric motors the application was in the experimental stage, and quoted specifically the Hild Universal System of electrically operated rotary rigs, now being tried out in California. Economy of cost and accelerated drilling time were the two chief advantages he claimed for electrical plant used for production, while the system of electrical pumping of oil-wells was (and usually is in large oilfields) advocated because of steadiness in unvarying output and general economy.

Psychological Types of the Human Race.

AT the anniversary meeting of the Royal Anthropological Institute held on January 22, Prof. C. G. Seligman delivered his presidential address, in which he discussed some possible developments of anthropological interest arising out of the views put forward by Jung concerning the importance of the two "types" which he calls extravert and introvert. Although Jung reached his conception by a consideration of the contrast in their reactions to the external world of sufferers from two pathological conditions, hysteria and dementia præcox, there is evidence that their peculiarities are not simply the product of illness, but that dissociations or eccentricities are known to have occurred in a large number of patients before the development of the disease. Again, although Jung was spoken of as the discoverer of the two types, this is not literally true, for years before, the soundest of observations had been made upon the two types by Henry James, who described them as "the tender-minded" [introvert] and "the tough-minded" [extravert]. Nevertheless it was Jung who first pointed out their practical importance.

The art products of the two types among the white races were then examined. Following Thornton and Gordon it was shown that when well marked, the two types produce different styles of painting. This is illustrated by considering the typical pictures of the following painters, Rubens, Delacroix and Signac, extravert, as against Poussin, Ingres and Marchand, introvert. Further, two styles of poetry apparently agreeing with the two "types" can be defined, while it was suggested that a critic sufficiently cognisant with plastic art would probably find that here too the difference existed.

The more direct biological questions connected with the two types were then considered. It was suggested that they occur in about equal numbers, though extraverts, being more adapted to the world and generally more immediately responsive, give the impression of being in the majority. Although both extraverts and introverts occur in the same family, the condition of any individual must be taken as innate, and the examination of a relatively small number of instances suggests that where alternate inheritance of physical characters (skin, hair) occurs, there may be a tendency to a correlated inheritance of type.

Applying the type question to savages, it is suggested that, compared with Northern Europeans, all or nearly all savages will be found to be extravert, though the degree of extraversion varies immensely. Thus while no one would doubt that Papuo-Melanesians and Veddas (the latter with their extreme tendency to dissociation) are almost extreme examples of extraversion, the Dinkas of the Upper Nile, with their utter lack of interest in the white man and his contrivances, their absence of desire for clothes and trade objects, with their extreme religiosity, their customary gravity and aloofness, are relatively introvert, contrasting strongly with such peoples as the Azandeh and presumably the West African negroes. The character of chiefs raises some interesting questions. In some instances—especially in the case of kings uniting temporal and spiritual authority—the chiefs are even more extravert than their commoners; this opinion is based on the frequency and ease with which dissociation occurs during religious ceremonies in which they are protagonists.

There was little time left to discuss the more civilised people of the far East, but it was pointed out that while old speculative India with its mystical habits of religion and philosophy is undoubtedly introvert, Japan—as illustrated by the writings of its best known official apologists—is extravert, while China—again on the evidence of its own literature—is very largely introvert.

With regard to dreams, Prof. Seligman pointed out that the dream mechanism of non-Europeans, whether savages or barbaric, seems to be the same as in ourselves. Dreams with symbolism, sometimes elaborate and recondite, often simple and obvious, occur, and these may be wish fulfilments or be provoked by conflict. Moreover, among the peoples investigated, dreams seem to be interpreted on much the same lines as among ourselves, *e.g.* either conventionally (often by opposites) or by association, *i.e.* by an elementary self-conducted analysis. At the present time the greatest interest appears to attach to those dreams with the same manifest content to which identical meanings are given (type dreams), occurring not only among individuals of the same stock, but also among peoples of diverse races and in every stage of culture. Among such dreams the tooth-losing dream and the flying dream offer particularly good instances of wide distribution. They are, indeed, found alike in Africa and over the greater part of the Eurasian land mass among peoples between whom it is difficult to believe that any passing on of the meaning of these dreams can have occurred.

Population and Longevity.

PROFS. RAYMOND PEARL and Lowell Reed have recently published in *Metron* (vol. 3, 1923) a paper on "The Mathematical Theory of Population Growth." This is a more detailed account of the method already described in two papers which appeared in the Proceedings of the U.S. National Academy of Sciences in June 1920 and December 1922.

The method originally suggested was the use of the equation,

$$y=\frac{be^{ax}}{1+c^{ax}},$$

where y is the population at any time, x is the time in years since the definite origin, and a, b, and c are positive values. This was found, when tested on the known population growth of the United States since 1790, to give a very good fit as a first approximation and a better graduation than the parabolic or logarithmic parabolic equation. Later, the method was extended and developed and a more general formula devised, namely,

$$y=\frac{k}{1+me^{a_1x+a_2x^2+a_3x^3}}.$$

This formula is an advance on the other in that it is more flexible and provides for asymmetry.

Profs. Pearl and Reed have made use of the latter formula in an investigation into the probable growth of the population of New York and its environs during the next 80 years. They estimate that in A.D. 2000 the total population of the region will have increased from 9,000,000 to 29,000,000, an increase of three times the present population. The negro population will have trebled itself and will then constitute 5 per cent. of the population. With the exception of the foreign-born section the population will have attained in A.D. 2000 almost the point of stabilisation.

In "The Interrelations of the Biometric and Experimental Methods of acquiring Knowledge: with especial Reference to the Problem of the Duration of Life," Prof. Pearl has published the substance of a lecture delivered to the Harvey Society in April 1922. As an illustration of the usefulness of the statistical method, he refers to his study of the duration of life in the fruit-fly, *Drosophila melanogaster*, in the *American Naturalist*, vol. 55, 1921. He constructed life-tables for 5400 male and 6332 female flies of this species, grown under constant conditions of temperature, food supply, etc. He finds an analogy between the life-span of the imaginal stage of the fly with a limit of 97 days, to which only one out of 1000 flies survives, and a human life-span of 86 years, the difference between 98 years, where 1 in 1000 survives in the American life-table, and 12 years, at which age the death-rate is at a minimum. These two life-spans he considers comparable, 1 year of human life being equal to 1·1279 days of Drosophila life.

Prof. Pearl finds that the form of the lx distributions, or degrees of longevity, is fundamentally the same in the two over the equivalent life-spans, from which he concludes that "the factors that determine individual longevity, and differences in this character, are biologically deeply rooted, and are at least as fundamental, apparently, as the factors which determine the specificity in the morphogenesis of organisms." While the laws of mortality are fundamentally the same in kind, they differ, however, quantitatively, as the human being has at every age a higher expectation of life than Drosophila. He puts forward the suggestion, although he states that it requires further research for verification, that this may be due to improvement of environment by hygiene. As the flies are kept under more or less ideal conditions, however, it is difficult to accept such an explanation. Pearl has shown, moreover, that different stocks of Drosophila show wide differences in duration of life. There are long-lived and short-lived strains. By suitable crossing experiments with the different strains he has also shown that segregation of these takes place in a Mendelian manner and that the differences are found to be constant.

"The Influence upon Duration of Life of certain mutant Genes of *Drosophila melanogaster*," published recently in the *American Naturalist* (vol. 57, July-August 1923) by B. M. Gonzalez, is based on another of the experimental studies on the duration of life conducted in the Department of Biometry and Vital Statistics, Johns Hopkins University. The paper is a first attempt at localising, in the chromosomes, factors controlling the duration of life.

Two stocks were used, namely—(1) a wild type of fly, (2) a "quintuple" stock, the latter a synthetic stock carrying five second chromosome mutations each in homozygous form, as follows: (*a*) *black*, a body colour factor; (*b*) *purple*, an eye colour; (*c*) *vestigial*, a rudimentary wing condition; (*d*) *arc*, a mutation with arched wings; (*e*) *speck*, a black spot on the wing.

These five mutants in the second chromosome of Drosophila were used as indices for five distinct factors that were recognised as having a definite effect on the duration of life. By crossing the quintuple stock with the wild stock twenty-two different combinations were obtained. These, with the two original, gave twenty-four strains in all. The behaviour of these combinations with respect to duration of life was investigated in 24,287 flies. The results obtained appear to show that under constant environmental conditions definite degrees of duration of life are associated with "extreme precision and exactness" with the presence or absence of certain genes of the chromosome. These same genes also appear to control certain morphological characters.

The New Science Department of Mill Hill School.

A SCIENCE building to-day must do more than provide accommodation for the science teaching of a school. It must express both in its structure and in its utilitarian possibilities those ideas which underlie the new conception of education which has been arising since the War. It must show a future generation what we are now feeling after, and must give them what we think they will want.

Such ideas are spaciousness, liberty, and a sense of adventure in life; self-determination, and education in the right use of leisure. The use of leisure was the theme of discussion at the conference of schoolmasters and university teachers at Cambridge in January 1923, and the science buildings at Mill Hill School, which were opened by H.R.H. the Prince of Wales on February 21, are to some extent the outcome of what was learnt there. The achievement of one of the boys, who, under great difficulties and with home-made apparatus, has maintained steady two-way communication with American amateurs while the building has been in progress, gave special point to the Prince's reference to such ideas. "The work," he said, "is done by a boy in his spare time: and I consider the proper and profitable use of spare time plays a very big part in education nowadays. To do things worth doing by ourselves, for ourselves, and because we want to do them—that only is the right way to use one's leisure. All that is necessary is opportunity, and these buildings, in addition to their formal use, create opportunity for Mill Hill boys." The Prince referred to the provision of opportunity for research side by side with formal teaching and for the pursuit of mechanical and scientific hobbies.

The new science schools were designed by Mr. Stanley Hamp to take advantage of a sloping site, providing a basement 24 feet wide; this runs the whole length of the building, and forms an ideal workshop. Here, too, is a motor room and an accumulator room giving direct current to the laboratories above. The central room on the ground floor is an octagonal museum lined with cupboards and forming the storage for physical apparatus. From this central hall access is gained to the two physical laboratories, each with its own optical room, and to the physics lecture room and the geography department. A short stairway descends to the basement, a portion of which is the physical workshop. A useful preparation room and a photographic dark room fill up odd corners. On the first floor are two large chemical laboratories, a biology room, and a chemistry lecture room, all communicating with a central store room. Here the well which lights the museum below is surrounded by a working bench, and the room forms a convenient research laboratory without interfering with its other uses. There is also a wireless room, a photographic enlarging room, a reference library, and a masters' room. The roof is interesting and valuable. The gabled portion surrounds a well concealing the chimney—an excellent arrangement for aerial and counterpoise. Around it is a walk commanding fine views. In front is an asphalt flat of obvious value.

The Prince of Wales praised the beauty of the buildings, which are designed as much to be a place in and from which a boy can receive impressions, as a place in which he can learn science or pursue a handicraft. The structural beauty of the building, which is part of Mill Hill School War Memorial, and its use, should alike make their appeal to his sense of the spaciousness and adventure of life.

W. H. BROWN.

University and Educational Intelligence.

BELFAST.—At a meeting of the Senate of the Queen's University held on February 20, new statutes were provisionally adopted creating a Faculty of Agriculture. A letter was received from the Ministry of Agriculture of Northern Ireland, agreeing : (1) To pay the salaries, bonus, superannuation of two professors and six lecturers in the Faculty of Agriculture, and of such additional professors and lecturers as may afterwards be required ; and (2) to provide the University with funds for the cost and maintenance of agricultural buildings, laboratories and necessary staff.

CAMBRIDGE.—The University has recently made important changes in the regulations affecting affiliated students, but the list of universities to which the new regulations apply has not yet been published. The chief changes which have been made consist in the limitation of the privileges to graduates of universities with first or second class honours, and in the removal of the test of Latin for graduates with first-class honours. The chief privileges of affiliation are the allowance of one, two, or three terms' residence, and, in certain cases, the exemption of a student from taking Part I. of a Tripos examination. In the case of approved universities in the United States of America, the general standard recognised as equivalent to a first class is that of graduating in the first sixth of a student's class, and the equivalent of a second class is that of graduating in the top half of a student's class. One object of these changes is to facilitate good honours graduates taking the second part of a Tripos at Cambridge and not being forced prematurely into a course of research; this happened sometimes under the old regulations, owing to the fact that the research courses were open to graduates without any question of the subjects taken by the student on entering his earlier university.

Students at an affiliated local lectures centre can still be affiliated under practically the same conditions as before.

New regulations are also proposed for the various medical diplomas.

N. J. T. M. Needham, Gonville and Caius College, has been re-elected to the Benn W. Levy research studentship in biochemistry. The coming resignation of a valued member of the teaching staff of the Chemical Laboratory is indicated in a Grace proposing a pension to Dr. H. J. H. Fenton on his retirement.

LEEDS.—Dr. C. K. Ingold has been appointed professor of organic chemistry as from October 1 next on the retirement of Prof. J. B. Cohen. Dr. Ingold was educated at University College, Southampton, and the Imperial College of Science and Technology, London. He conducted important research work for the Chemical Warfare Committee during the War, and assisted in the design and erection of extensive plant for the manufacture of substances required by the Committee. During the last four years Dr. Ingold has supervised the Organic Chemistry Research Laboratories of the Imperial College and has had a large share in the direction of a semi-large-scale organic chemistry laboratory.

The Leeds and District Leather Trades' Association has subscribed 50 guineas to the Leather Industries Department.

The Yorkshire Summer School of Geography, which has been conducted by the University on three previous occasions, is to be revived this year, and approval has been granted for the institution of a course of advanced lectures in astronomy to be given by the Mathematics Department during the session 1924–25.

The next academic year, which begins in October, will see the completion of the fiftieth year from the opening of the Yorkshire College of Science and also of the twenty-first year of the existence of the University. It is the intention of the Council and Senate of the University to celebrate simultaneously the jubilee of the Yorkshire College and the coming of age of the University. The celebrations will extend over a week, December 15–20. Special public lectures in relation to the University will be delivered in the Great Hall, and various social events are being arranged. Sir Michael Sadler is compiling a history of the University, which it is hoped will be ready for publication in the autumn.

LONDON.—The following doctorates have been awarded : *Ph.D.* (*Science*) : Mr. L. R. Hart (King's College) for a thesis entitled "Some Derivatives of 3-Oxy (1) Thionaphthen" ; Mr. H. C. Kassner (University College and School of Pharmacy of the Pharmaceutical Society) for a thesis entitled "The Histological and Chemical Examination of the Seeds of *Ipomœa hederacea* Jacquin, and other Species of Ipomœa" ; and Mr. A. F. Watson (University College) for a thesis entitled "Studies in the Preparation and Nature of the *Bacillus diphtheriæ*."

Free public lectures will be given (in English) on March 11 and 13 at University College, at 5.30, by Prof. J. W. Van Wijhe, of the University of Groningen, on "The Origin of the Vertebrate Skeleton." No tickets will be necessary.

MANCHESTER.—Mr. F. S. Sinnatt has resigned his lectureship in fuels in the Faculty of Technology in consequence of his appointment as assistant director of the Fuel Research Board under the Department of Scientific and Industrial Research.

Messrs. Brunner, Mond and Co., Ltd., have made a gift of 300*l.* to the Department of Physics in aid of research.

WE learn from the *Times* that Lady Strathcona has given 120,000 dollars (about 24,000*l.*) to McGill University, Montreal, to provide a permanent endowment for a department of zoology.

THE scheme of Empire study promulgated by the Board of Education in December in connexion with the British Empire Exhibition has, we are glad to learn, received sufficient support from the schools to justify the publication of a special weekly bulletin. The first number contains, in addition to notes for the first week's lessons, messages from the Prime Minister of Australia and other public men, information about educational features of the exhibition, such as the Canadian Exhibition Car which is touring the country under the direction of the Superintendent of Emigration for Canada, visiting schools *en route*. The car is accompanied by a lecturer prepared to give demonstration lessons and to distribute free copies of a descriptive atlas of Canada. The Bulletin contains a small-scale map of the world, in the construction of which a modification of Goode's "homolographic" projection has been used so as to show the continental land masses correctly as regards relative areas with a minimum distortion of shape. Besides giving an accurate general survey of the extent of the Empire, it shows clearly the main channels of intra-Imperial sea-borne trade and principal Imperial railway routes. The Bulletin, which is to be issued in twenty-four weekly parts, will be supplied to schools and educational bodies at one penny a copy post free. Single copies will be posted to private addresses if the order (to be sent to the Secretary, Educational Sub-Committee, Department of Overseas Trade, 35 Old Queen Street, London, S.W.1) is accompanied by a P.O. for three shillings.

Early Science at the Royal Society.

February 25, 1663. Mr. Beal's letter to the lord viscount Brouncker was read, in which he gave some account of the famous hot baths and the cool springs of Wells; and likewise of the strange cave called Wookey-hole.

1668. Some experiments were made, to find out what would be the resistance of air to bodies moved through it with several velocities.

1674. Mr. Oldenburg gave an account of arrears. —Mr. [Edmund] Waller [the Poet] put it off with an expression of merriment, that he thought it best to forget and forgive one another for what was past, and to begin upon a new score.

1684. A letter of Mr. Isaac Newton mentioning that the design of a philosophical meeting there [Cambridge] had been pushed forward by Mr. Paget, —"but that which chiefly dashed the business was the want of persons willing to try experiments."

February 26, 1662. Sir William Petty promised to produce on that day fortnight his paper concerning trades.

1679. Mr. Hooke presented from Mr. Boyle his "Sceptical Chemist," which treatise the committee for experiments were desired to peruse, and to give an account of, and to see what experiments contained therein were proper to be shewn at the meetings of the Society.

February 27, 1667. The experiment of compressing air upon a shining fish was made, which succeeded according to expectation; the light of the fish appearing more vivid after the compression than before.

1683. Dr. Mapletoft presented the picture of William Harvey, M.D.

February 28, 1666. Sir Peter Wyche, returned from Portugal, gave an account of what he had done concerning those instructions and inquiries recommended to him at his going thither as envoy extraordinary to the King of Portugal.

1677. Mr. Hooke produced an animadversion of Signor Cassini upon the observation of M. Gallet of the passage of Mercury under the Sun.

1682. It being mentioned that the Society wanted experiments, Dr. Tyson and Dr. Hare were proposed as persons very fit to assist.—Ordered that Dr. Tyson and Dr. Hare assisting this year in making chemical and other experiments, shall be rewarded each of them at the end of the year with a piece of plate to the value of twenty pounds.

March 1, 1664. The experiment made with birds put in common, rarefied, and compressed air being made again, and it being found that the bird in the common air was well; that in the rarefied, panting, and that in the compressed air, dead, the last was opened, but no water found therein: which since some of the members thought an argument against the bird's being drowned, it was ordered, that the experiment with the bird in the compressing engine should be tried the third time, putting the bird in a little cage, at the next meeting.

1665. Ordered—That the *Philosophical Transactions*, to be composed by Mr. Oldenburg, be printed the first Monday of every month, and that that tract be licensed by the council.

1675. Mr. Oldenburg moving, that now the sun and season being likely to serve for the making of Mr. Newton's experiment, called in question by Mr. Linus, an apparatus might be prepared for that purpose.

1676. Another inclinatory needle was tried, and proved to be better.

1681. Mr. Aston gave an account that Dr. Brown, with some other physicians, had dissected an ostrich.

Societies and Academies.

LONDON.

Royal Society, February 21.—C. Tate Regan: The morphology of a rare oceanic fish, *Stylophorus chordatus* Shaw; based on specimens collected in the Atlantic by the *Dana* expeditions, 1920–22. Stylophorus differs considerably in structure from its nearest allies, Trachypterus and Velifer. The skeletal differences are interpreted as related to the low elongate form, the large telescopic eyes, and the extremely protractile mouth. The unique mechanism of the protractile mouth is compared with that of the Labroid fish Epibulus.—F. P. Slater: A sensitive method for observing changes of electrical conductivity in single hygroscopic fibres. A simple electroscopic arrangement is capable of detecting the effect of small and controlled changes in relative humidity and temperature on 1 cm. length of a single hygroscopic fibre. The measuring system and specimen being tested are embodied in the same case, are always under the same controlled external conditions of temperature and relative humidity, and are reproducible.—T. S. P. Strangeways: Observations on the formation of bi-nuclear cells. The formation of bi-nuclear cells after mitotic division of the nucleus has been watched on living cells growing *in vitro*. Three types of change in the cytoplasm associated with the production of such cells are described.—J. A. Crowther: Some considerations relative to the action of X-rays on tissue cells. An analysis of the observations of Strangeways and Oakley on the immediate effects of soft X-rays on tissue cells suggests that the effect follows a probability law. This probability is the probability that ionisation will be produced in some particular body within the cell by the action of the rays. The size which this body must have to account for the observations is approximately 1/2500 mm. This appears to be of the order of magnitude of the centrosome in the cells employed in the experiments.

Royal Anthropological Institute, February 5.—Prof. C. G. Seligman, president, in the chair.—W. E. Armstrong: Rossel Island money. The Dap coins are polished pieces of shell drilled at one corner. The Kö coins are sets of ten roughly made discs fashioned from some other and evidently larger species of shell-fish. Except for recognised imitations of the lower Dap-values, the Rossel natives believe that all this money was made in a remote past by the chief god of the island. There are twenty-two values of Dap with distinctive names, and sixteen of Kö with names derived from those denoting the sixteen highest Dap values. The Dap and Kö constitute two more or less independent systems of value, although most financial transactions involve both types of money. If the Dap values be arranged in order from lowest to highest, then the loan of a coin having any one of these values, for a period of a few days, is repaid by means of a coin of value next in the series, and so on. The relative values of two Dap coins or two Kö coins may thus be expressed as a period of time, and so, indirectly, of the relative values of things. Borrowing and lending are of such importance that a class of individuals has become differentiated who derive their income almost solely from acting as brokers. The peculiar currency of Rossel is one among many of the features which differentiate the culture of this island from the Massim culture of the neighbouring islands and the mainland of Papua.

Geological Society, February 6.—Prof. A. C. Seward, president, in the chair.—O. T. **Jones**: The Upper Towy drainage-system. The whole longitudinal profile of the Towy valley consists of two concave portions, in each of which the gradient diminishes progressively downstream, separated by a short stretch with high and irregular gradient. The upper part of the valley is attributed to a former period of base-levelling. This was followed by a rejuvenation of the drainage-system due to an uplift of the area. The present valley between Rhandirmwyn and the mouth was eroded in consequence of that rejuvenation, and is base-levelled or graded in relation to the existing sea-level. The upper or Fanog base-level, to which the upper part of the valley conforms, now stands much above the present sea-level. The tributary valleys afford confirmation of the results obtained in the main valley. About half a mile above Nant Stalwyn (5 miles) there is a well-marked drop or step in the rock-floor of the Towy valley. Below this step the tributary valleys hang above the main valley-floor, the discordance increasing downstream. These hanging tributaries are pre-Glacial. The part of the valley above the rock-step was probably eroded in relation to a base-level older than the Fanog base-level. This, which is termed the Nant Stalwyn base-level, is estimated to stand at present nearly 600 feet above the level of the sea.

Physical Society, February 8.—Mr. F. E. Smith in the chair.—E. G. **Richardson**: Æolian tones. New experiments for testing the dependence of the pitch n on the fluid speed V, and on the diameter D, of the vibrator in a number of fluids are described. The results show V/nD to fall from 8 to 5, while D ranges from 0·02 to 2 cm., whatever the viscosity.—J. E. **Calthrop**: The effect of torsion on the thermal and electrical conductivities of metals. The small decreases in the thermal conductivities of metal wires obtained on twisting are proportional to the square of the twist, as is approximately the case for the electrical conductivities. Iron requires several days to recover its original value of the thermal conductivity after the twist has been removed, and if the twist is kept constant there is a slight return towards the initial value.—C. W. **Hawksley**: Microscope for observation of interference fringes. In examining the interference rings seen in a crystal by means of convergent polarised light it is common to use an additional optical system called a Becké lens placed over the eye-piece for the purpose of increasing the size of the image without impairing its definition. In a new arrangement the additional optical system consists of four lenses disposed as in the upper draw tube of an ordinary field telescope—viz., an erector and a Huyghens eye-piece. The image is thereby much increased in size without appreciable loss of definition, and this result may be still better effected by removing the eye lens of the microscope eye-piece. For optimum definition it is better to locate the analyser over the eye-piece of the telescope system. For monochromatic illumination use was made of the forked stems employed for supporting incandescent gas mantles, which were impregnated with sodium carbonate.

Royal Statistical Society, February 19.—Sir Henry Rew in the chair.—R. Y. **Sanders**: Foreign trade and shipbuilding. Business indexes designed to show the state of trade only reflect a trade movement after it has occurred, and so cannot give warning of a coming change. The value of any suggested set of figures can be judged by its time relation to the general trade cycle of the country. Since the pound sterling varies in value, the figures of the value of imports and exports are liable to be inaccurate as a measure of the volume of trade. The net tonnage of vessels entered and cleared with cargoes in the foreign trade is a more trustworthy index. Over the pre-War period of 1894–1913 a change in the value of imports was on the average followed by a change in the amount of tonnage under construction just over a quarter of a year later, while the amount of net tonnage entered with cargo in the foreign trade preceded the construction figure by almost exactly three months.

Royal Meteorological Society, February 20.—Mr. C. J. P. Cave, president, in the chair.—S. **Chapman**: (1) The lunar atmospheric tide at Mauritius and Tiflis. The moon is known to produce a tide in the atmosphere which reveals itself by a small semi-diurnal variation of barometric pressure during each lunar day. The range of this pressure oscillation is small, and can only be determined by computation from long series of records. In previous papers the results of such calculations have been given for Greenwich, Aberdeen, Batavia, and Hong Kong. The investigation has been continued because the phase of the tide is found not to have the value which would be expected (corresponding to high pressure nearly at upper and lower transit of the moon), but to vary with the station and also with the season. No conclusions are formed as to the causes of the irregularities of phase revealed. (2) The semi-diurnal oscillation of the atmosphere. The magnitude of the semi-diurnal variation of barometric pressure, and its regularity of occurrence over the greater part of the globe, support the view that the oscillation is magnified by resonance, its period of twelve solar hours being in near agreement with a period of free oscillation of the atmosphere. It has remained an open question, however, whether the oscillation was mainly caused by the daily heating and cooling of the atmosphere, or whether the sun's gravitational influence also played an appreciable part. An attempt is made to estimate the relative importance of the two causes, assuming that the semi-diurnal changes of temperature in the lower atmosphere are mainly due to the thermal conduction upwards from the ground. The result is somewhat indefinite, owing to the scarcity of trustworthy data concerning the semi-diurnal variation of temperature, and the eddy conductivity, over large parts of the earth. It seems not unlikely that the sun's thermal and gravitational influences are about equally responsible for the oscillation, and on this hypothesis the observed phase of the oscillation can be accounted for fairly well. The magnitude of the oscillation indicates that the approximation of the adjacent free period to twelve solar hours must be very close, that is, to within a few minutes: this is much nearer than existing theories of atmospheric vibrations can account for.—C. S. **Durst**: The relationship between current and wind. A large number of observations made by ships at sea are examined in the light of Ekman's theory of ocean currents. This theory is borne out by the average direction of flow, and the average velocity of current is directly proportional to the speed of the wind. Coefficients of turbulence are deduced for various regions of the seas.

MANCHESTER.

Literary and Philosophical Society, February 19.—Miss Irene J. **Curnow**: Western China. Western China, interpreted as the Upper Yangtse basin, falls simply into two natural regions, the Highlands on the Tibetan flank, and the Red Basin. In the Highlands the climate is extreme, agriculture limited, population scanty, mineral resources unknown. The Red Basin is an area of great fertility and dense population. Being in sub-tropical latitudes, Szechuanese agriculture is mainly of the intensive type with rice as the dominant crop, implying irrigation rather than dry-farming—thus making the graded terraced paddyfield the feature of the landscape. In a transition zone, Central China claims to grow everything grown anywhere in China. Szechuan can supply a multitudinous number of products, and, as in the south, tea and sericulture are important home industries. Foreign trade is concentrated in the Treaty Ports — the largest on the Upper River being Chungking. Internal communications are offered by road and river, but the great highway for trade in and out from the Province is by the Yangtse. Navigation is good for long distances, but a crucial section is marred by exceptionally dangerous rapids and gorges between Chungking and Ichang. Several railways are projected, and these will prove of value not only as commercial arteries, but also as conveyors of ideas.

PARIS.

Academy of Sciences, February 4.—M. Guillaume Bigourdan in the chair.—A. **Lacroix**: The analcitic lavas of North Africa, and, generally, the classification of lavas containing anacalcite. The discussion of the classification of these lavas is based on the complete analysis of fifteen specimens.—Charles **Richet**, Mlle. Eudoxie **Bachrach**, and Henry **Cardot**: The simultaneous action of two contradictory toxic effects on the same cell (tolerance and anaphylaxy).—Ph. **Glangeaud**: The hydro-mineral basin of Saint-Nectaire (Puy-de-Dôme), its dislocations and its volcanic framework.—R. **Gosse**: The equations $s+f(x, y, z, p, q, r)=0$, integrable by the method of Darboux.—W. **Sierpiński**: The extension of the homeomorphy between two ensembles.—R. **Dugas**: The movement of a material point with mass variable with its kinetic energy, submitted to a central force.—L. **Lecornu**: Observations on the preceding communication.—Louis **Kahn**: Comparison between living beings and mechanical engines from the point of view of the power necessary for propulsion in fluids. In a fluid at rest, a mechanical engine would reach a velocity comparable with that of birds or fishes with a power per ton of the same order.—J. **Guillaume**: Observations of the sun made at the Lyons Observatory during the third quarter of 1923. Observations were possible on 88 days during this quarter, and the results are given in three tables showing the number of spots, their distribution in latitude, and the distribution of the faculæ in latitude.—L. **Bochet**: The results of Watson's experiments relating to the expansion of water under high constant pressure. A discussion of these results, by comparison with the work of Amagat, Regnault, Davis, leads to the view that they are probably vitiated by a constant error.—R. **Bureau**: The meteorological origin of certain disturbances of the receivers in wireless telegraphy. Some of the disturbances may be regarded as due to the effect of snow-fields at high altitudes, partly owing to the rapid evaporation of snow at a temperature below its melting-point and partly owing to the action of the sun's rays upon these snow-fields.—Charles **Henry**: The function $\Delta\lambda=\phi(\lambda\theta)$ of the theory of radiation.—G. **Athanasiu**: The calorific action of radiation on metals dipped in solutions of their salts. Within the experimental error of the method adopted, the electromotive forces produced by light on the elements studied (cells constructed of metal—solution of salt of metal—metal) may be explained by the superposition of two effects: the first due to a chemical alteration of the surface of the electrodes, the second due to the rise of temperature produced by the radiation.—F. **Diénert** and F. **Wandenbulcke**: Study of colloidal silica. A study of the conditions of the change of colloidal into non-colloidal silica and the reverse action, based on the use of the molybdic reagent of Jolles and Neurat.—Maurice **Picon**: Thermal analysis of the system sodium thiosulphate—water.—A. **Bigot**: Kaolins and fused bauxites. Details of the changes in chemical composition, crystal formation and hardness produced by fusing bauxite and kaolin at temperatures between 1700° C. and 1900° C. The fused bauxite has proved useful as a refractory material and as an abrasive.—A. **Boutaric**, Ed. **Chauvenet**, and Mlle. Y. **Nabot**: The determination of the molecular mass of some sodium salts by cryoscopy in hydrated and fused sodium thiosulphate. In sodium thiosulphate as solvent, many sodium salts are not ionised, and the theoretical molecular weights are obtained from the cryoscopic measurements.—Charles **Dufraisse** and Henri **Moureu**: The action of piperidine on α-bromobenzalacetophenone: preparation of a new α-diketone, phenylbenzylglyoxal.—Michel **Polonovski**: Sulphochromic oxidation and β-oxidation. Controlled sulphochromic oxidation in weak solutions showed that acetic acid is unchanged, propionic acid is changed slowly into pyruvic acid and, ultimately, into acetic acid. In butyric and valerianic acids the β-carbon is first attacked.—R. **Fosse**, Ph. **Hagène**, and R. **Dubois**: The action of hydrazine on hydantoin and allantoin.—Jacques de **Lapparent**: The mineralogical constitution of bauxites and of the limestones with which they are found in contact.—Emile **Belot**: The genesis of continents and seas.—M. **Michel-Durand**: The state of the tannins in the plant cell.—A. **Hée**: Does the respiratory intensity of plants obey the law of surfaces? In plants the respiratory surface cannot be the only cause regulating the intensity of the respiratory phenomena.—L. **Mercier**: The atrophy of the flight muscles after the fall of the wings in *Lipoptena cervi*.—Alphonse **Labbé**: Allelogenesis in *Canthocamptus minutus* and the setal formula.—R. **Courrier**: The experimental permeabilisation of the virgin egg of *Paracentrotus lividus*.—Henri **Limousin**: The inoculation of the rabbit with human leprosy.

Official Publications Received.

Progress of Education in India, 1917–1922. By J. A. Richey. Eighth Quinquennial Review. Vol. 2: Statistics. Pp. vii+189. (Calcutta: Government Printing Office.) 1.4 rupees.

Botanical Survey of South Africa. Memoir No. 5: Researches on the Vegetation of Natal. By Dr. J. W. Bews and R. D. Aitken. Series 1. Pp. 70. (Pretoria: Government Printing and Stationery Office.)

Record of Fellows and Scholars and Catalogues of Publications by Fellows, Scholars and Recipients of Grants under the Research Scheme of the Carnegie Trust for the Universities of Scotland during the period 1903 to 1923. Pp. 211. (Edinburgh: The Merchants' Hall.)

Report of the Department of Mines for the Fiscal Year ending March 31, 1923. Pp. iii+58. (Ottawa: F. A. Acland.) 5 cents.

Canada. Department of Mines: Geological Survey. Memoir 135, No. 116 Geological Series: Geology of Fraser River Delta Map-area. By W. A. Johnston. Pp. ii+87+6 plates. (Ottawa: F. A. Acland.)

Astronomical and Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the Year 1920, under the Direction of Sir Frank Dyson. Pp. 8+Axx+A64+B13+C2+F10+Gvi+G54+Dix+D89+5+Exxi+E82+21. (London: H.M. Stationery Office.) 47*s*. 6*d*. net.

Cape Astrographic Zones. Vol. 7: Catalogue of Rectangular Co-ordinates and Diameters of Star Images, derived from Photographs taken at the Royal Observatory, Cape of Good Hope. Commenced under the Direction of Sir David Gill; Completed and Prepared for Press under the Supervision of S. S. Hough. Zone −47°. Pp. xxxvii+509. (London: H.M. Stationery Office.) 105*s*. net.

Proceedings of the University of Durham Philosophical Society. Vol. 6, Part 5, 1922–1923. Pp. 291-397. (Newcastle-upon-Tyne.) 5*s*.

Ministry of the Interior, Egypt: Department of Public Health. Reports and Notes of the Public Health Laboratories, Cairo: Plague Report. Pp. ii+114+3 charts+4 maps. (Cairo: Government Publications Office.) P.T. 20.

Proceedings of the American Society for Psychical Research. Vol. 16, part 1: Past Events Seership; a Study in Psychometry. By Dr. Gustav Pagenstecher. Edited by Dr. Walter Franklin Prince. Pp. vi+136+12 Plates. (New York: American Society for Psychical Research, Inc.) 2.50 dollars.

The Cactaceæ: Descriptions and Illustrations of Plants of the Cactus Family. By N. L. Britton and J. N. Rose. Vol. 4. (Publication No. 248.) Pp. vii+318+37 plates. (Washington: Smithsonian Institution.) 6.50 dollars.

Contributions to Embryology. Vol. 15, Contributions 72-77. No. 72: Description of a Human Embryo having Twenty Paired Somites, by Carl L. Davis; No. 73: On the Development of Tendon Sheaths, by Randolph T. Shields; No. 74: On the Lymph-Vessels of the Liver, by Ferdinand C. Lee; No. 75: Ovulation and Menstruation in *Macacus rhesus*, by George W. Corner; No. 76: The Structural Unit of the Human Kidney, by Herbert F. Traut; No. 77: The Branchial Vessels and their Derivatives in the Pig, by Chester H. Heuser. (Publication No. 332.) Pp. iii+139+19 plates. (Washington: Smithsonian Institution.) 3.50 dollars.

Experimental Pollination: an Outline of the Ecology of Flowers and Insects. By Frederic E. Clements and Frances L. Long. (Publication No. 336.) Pp. vii+274+17 plates. (Washington: Smithsonian Institution.) 4 dollars.

Body-Build and its Inheritance. By Charles Benedict Davenport. (Publication No. 329.) Pp. vi+176+9 plates. (Washington: Smithsonian Institution.) 2.75 dollars.

Diary of Societies.

SATURDAY, MARCH 1.

RADIO SOCIETY OF GREAT BRITAIN (at Institution of Electrical Engineers), at 2.—Conference of Affiliated Societies.

GILBERT WHITE FELLOWSHIP (at 6 Queen Square, W.C.1), at 3.—Sir Sidney F. Harmer: Whaling Research and the New *Discovery* Expedition.

MONDAY, MARCH 3.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.—General Meeting.

SOCIETY OF ENGINEERS, INC. (at Geological Society), at 5.30.—J. Jackson: Development of Methods for the Collection and Disposal of House (Domestic) Refuse.

INSTITUTION OF ELECTRICAL ENGINEERS (Informal Meeting), at 6.—R. D. Spurr and others: The Selection and Location of Converting Plant for supplying D.C. Networks.

INSTITUTION OF MECHANICAL ENGINEERS (Graduates' Section, London), at 7.—Lieut.-Col. E. Kitson Clark: Literature and Engineering.

ARISTOTELIAN SOCIETY (at University of London Club), at 8.—L. A. Reid: Creative Morality.

ROYAL SOCIETY OF ARTS, at 8.—E. V. Evans: A Study of the Destructive Distillation of Coal (Cantor Lectures) (2).

SOCIETY OF CHEMICAL INDUSTRY (London Section) (at Chemical Society), at 8.—R. Whymper and A. Bradley: The Setting of Cacao Butter with special reference to the Development of "Bloom" on Chocolate.—W. J. Powell and H. Whittaker: The Determination of Pentosans in Wood Cellulose.

INSTITUTION OF RUBBER INDUSTRY (at Engineers' Club, Coventry Street), at 8.—H. Rogers and B. D. Porritt: Life and Work of Thomas Hancock.

ROYAL SOCIETY OF MEDICINE (Tropical Diseases and Parasitology Section), at 8.30.—Lt.-Col. R. McCarrison: Rice in relation to Beri-beri in India.

TUESDAY, MARCH 4.

ROYAL SOCIETY OF ARTS (Dominions and Colonies Section), at 4.30.—Hon. T. G. Cochrane: Empire Oil: The Progress of Sarawak.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—Prof. J. Barcroft: The Respiratory Pigments in Animal Life and their Significance (4).

ZOOLOGICAL SOCIETY OF LONDON, at 5.30.—E. G. Boulenger: A New Giant Salamander, living in the Society's Gardens.—R. Gurney: The Larval Development of some British Prawns (Palæmonidæ). I. *Palæmonetes varians*.—W. E. Le Gros Clark: The Myology of *Tupaia minor*.—Dr. C. W. Andrews: Note on an Ichthyosaurian Paddle showing traces of Soft Tissues.

INSTITUTION OF CIVIL ENGINEERS, at 6.

WOMEN'S ENGINEERING SOCIETY (at 57 Mortimer Street, W.1), at 6.30.—Miss M. E. Phillips: Factory Legislation: Past, Present, and Future.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN (Scientific and Technical Group), at 7.—Prof. M. von Rohr: Contributions to the History of the Photographic Objective in England and America, 1800-1875.

ROYAL ANTHROPOLOGICAL INSTITUTE, at 8.15.—Sir Arthur Keith: Neanderthal Man in Malta, with an Account of Mr. George Sinclair's Survey of the Cave of Ghar Dalam, Malta.—Major R. A. Marriott: Some Recent Finds of Flints at Piltdown.

RÖNTGEN SOCIETY (at Institution of Electrical Engineers), at 8.15.—N. E. Luboshez: Hard and Soft Tubes in the Technique of Radiography, with Practical Demonstration.

WEDNESDAY, MARCH 5.

INTERNATIONAL SOCIETY OF MEDICAL HYDROLOGY (at Royal Society of Medicine), at 4.30.—Dr. Pierret and others: Discussion on the Treatment of Tropical Disease by Waters and Baths.

ROYAL SOCIETY OF MEDICINE (Medicine, Surgery, Therapeutics, and Pharmacology Sections), at 5.30.—Joint discussion on The Treatment of Severe Gastric and Duodenal Hæmorrhage. Speakers: Surgical, H. J. Paterson, A. H. Burgess, R. P. Rowlands, and G. Gordon-Taylor; Medical, Sir William Willcox, Dr. A. Hurst, Dr. I. Bennett; Therapeutical, Dr. P. Hamill.

NEWCOMEN SOCIETY (at 17 Fleet Street), at 5.30.—H. W. Dickinson and A. Lee: The Rastricks: Civil Engineers.—L. F. Loree: Memorandum on the Four Locomotives of the Delaware and Hudson Co., 1829.

INSTITUTION OF CIVIL ENGINEERS (Students' Meeting), at 6.

INSTITUTION OF ELECTRICAL ENGINEERS (Wireless Section), at 6.—Commdr. J. A. Slee: Development of the Bellini-Tosi System of Direction-finding in the British Mercantile Marine.

INSTITUTION OF HEATING AND VENTILATING ENGINEERS, INC. (at Engineers' Club, Coventry Street), at 7.—A. A. H. Scott and others: Discussion on Heating and Ventilating as applied to Ferro-concrete Buildings.

ROYAL MICROSCOPICAL SOCIETY (Biological Section), at 7.30.—H. Graham-Cannon: Excretion in the Crustaceæ.

SOCIETY OF PUBLIC ANALYSTS AND OTHER ANALYTICAL CHEMISTS (at Chemical Society), at 8.—G. D. Elsdon: (*a*) The Composition and Examination of Beef and Malt Wine; (*b*) The Determination of Coconut Oil in Margarine; (*c*) What is Bondon Cheese?—Dr. J. C. Drummond and H. J. Channon: Effect of Fatty Diet on the Composition of Butter Fat.—T. R. Hodgson: Cream Cheese.—R. T. Thompson and J. Sorley: Some Facts on the Composition and Decomposition of Eggs.

ROYAL SOCIETY OF ARTS, at 8.—Major-Gen. Sir Fabian Ware: Building and Decoration of the War Cemeteries.

MEDICAL SOCIETY OF LONDON, at 9.—Dr. R. A. Young: The Treatment of Pulmonary Tuberculosis (Lettsomian Lectures) (2).

THURSDAY, MARCH 6.

ROYAL SOCIETY, at 4.30.—E. D. Adrian and Sybil Cooper: The Electric Response in Reflex Contractions of Spinal and Decerebrate Preparations.—A. Fleming: A Comparison of the Activities of Antiseptics on Bacteria and on Leucocytes.

LINNEAN SOCIETY OF LONDON, at 5.—Miss Helena Bandulska: The Cuticles of some Recent and Fossil Fagaceæ.—Dr. E. J. Salisbury: A Phytogeographical Visit to Switzerland.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—Dr. J. S. Flett: Modes of Volcanic Action.

ROYAL AERONAUTICAL SOCIETY (at Royal Society of Arts), at 5.30.—Major Tucker: Sound Detection.

ROYAL SOCIETY OF MEDICINE (Balneology and Climatology Section), at 5.30.—Dr. J. Burt: The Causes of Brachialgia.

CHILD-STUDY SOCIETY (at Royal Sanitary Institute), at 6.—Dr. P. B. Ballard: The New Examiner.

INSTITUTION OF ELECTRICAL ENGINEERS, at 6.—P. E. Erikson and R. A. Mack: Transmission Maintenance of Telephone Systems.

CHEMICAL SOCIETY, at 8.—Prof. J. R. Partington and F. A. Williams: The Reaction between Lime and Nitrogen Peroxide.—J. J. Doolan and Prof. J. R. Partington: Tellurium Monoxide.—C. S. Gibson and the late D. C. Vining: The Dimorphism of Diphenylarsenious Chloride (Diphenylchloroarsine).—O. L. Brady and R. Truszkowski: The Isomerism of the Oximes. Part XVII. The Action of 2:4-Dinitrochloro-benzene on some Isomeric Aldoximes.

ROYAL SOCIETY OF MEDICINE (Children, Neurology, Obstetrics and Gynæcology, and Orthopædics Sections), at 8.—Combined Discussion on Birth Injuries, with special reference to Intracranial Injuries with Hæmorrhage, and to Nerve Injuries.

FRIDAY, MARCH 7.

ROYAL ASTRONOMICAL SOCIETY, at 5.—Geophysical Discussion on Seismological Problems, by Prof. H. H. Turner, J. J. Shaw, and others.

PHILOLOGICAL SOCIETY (at University College), at 5.30.—C. T. Onions: Dictionary Evening.

INSTITUTION OF MECHANICAL ENGINEERS (Informal Meeting), at 7.—Commander G. C. C. Damant, R.N., ret.: Recovering Gold from the *Laurentic*.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN (Pictorial Group), at 7.—H. Lambert: A Consideration of the Technical and Artistic Qualities of Printing Processes.

JUNIOR INSTITUTION OF ENGINEERS, at 7.30.—P. M. Fraser: An Introduction to the Study of Fire Danger in Factories, with some Notes on Fire Insurance.

ROYAL SOCIETY OF MEDICINE (Anæsthetics Section), at 8.30.—Prof. Storm van Leeuwen: The Anæsthetic Properties of the Purest Ether: Ether of Crystallisation.

ROYAL INSTITUTION OF GREAT BRITAIN, at 9.—Dr. W. Rosenhain: The Inner Structure of Alloys.

SATURDAY, MARCH 8.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Sir Ernest Rutherford: Properties of Gases in High and Low Vacua (1).

GILBERT WHITE FELLOWSHIP (at 6 Queen Square, W.C.1), at 3.—Founders' Day Conversazione.

PUBLIC LECTURES.

SATURDAY, MARCH 1.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Sir Ernest Rutherford: Properties of Gases in High and Low Vacua (1).

HORNIMAN MUSEUM (Forest Hill), at 3.30.—H. N. Milligan: Animals which live in Trees.

BIRKBECK COLLEGE, at 6.—Dr. F. H. Hayward: Homage Celebration to the Scientist.

WEDNESDAY, MARCH 5.

INSTITUTE OF HYGIENE (Devonshire Street), at 3.30.—Dr. C. S. Thomson: The Health of the Clerk.

ROYAL INSTITUTE OF PUBLIC HEALTH, at 4.—Dr. B. T. J. Glover: Difficulties in the Campaign against Tuberculosis.

UNIVERSITY COLLEGE, at 6.—Miss Julia Bell: Colour Vision and Colour Blindness from the Historical Aspect.

FRIDAY, MARCH 7.

BEDFORD COLLEGE FOR WOMEN, at 5.15.—Prof. F. Soddy: How Physical Science has altered the Economics of Life.

SATURDAY, MARCH 8.

HORNIMAN MUSEUM (Forest Hill), at 3.30.—Dr. H. S. Harrison: Man's Early Discoveries and Inventions.

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The Origin of the Solar System.[1]

By J. H. Jeans, Sec. R.S.

The astronomer of to-day has at his disposal telescopes which range in aperture from his naked eye, of aperture about one-fifth of an inch, up to the giant Mount Wilson telescope of more than 100 inches. If we lived in the midst of a uniform infinite field of stars, or in a field which was uniform as far as our telescopes could reach, the numbers of stars visible in different telescopes would be proportional to the cubes of their apertures.

In actual fact our naked eyes reveal about 5000 stars; with a one-inch telescope this number is increased to about 100,000, with a ten-inch to 5 million, and with the 100-inch telescope to perhaps 100 million. These numbers increase much less rapidly than the cubes of the apertures. We conclude that we are not surrounded by an infinite uniform field of stars. We live in a finite universe, which thins out quite perceptibly within distances reached by telescopes of very moderate size. It is estimated that the whole universe consists of some 1500 million stars, our sun being not very far from the centre of the system.

Imagine the various celestial objects in this universe arranged according to their distance from us. Disregarding altogether bodies which are much smaller than our earth, we must give first place to the planets Venus and Mars, which approach to within 26 and 35 millions of miles respectively. Next comes Mercury with a closest approach of 47 million miles, and the sun at 93 million miles. The remainder of the planets follow at distances ranging up to 2800 million miles, the radius of the orbit of Neptune.

But now comes a great gap. The first objects beyond this gap are the faint star Proxima Centauri at a distance of 24 million million miles, or more than 8000 times the distance of Neptune, and close to it, α Centauri at 25 million million miles. Next in order come the faint red star Munich 15,040 at 36 million million miles, and another faint star Lalande 21,185 at about 47 million million miles. Thus our nearest neighbours among the stars are at almost exactly a million times the distances of our nearest neighbours among the planets. After these comes Sirius, the brightest star in the sky, at 50 million million miles. From here on there is a steady succession of objects until we reach distances of more than 20,000 times that of Sirius; but long before these distances are reached other objects, spiral and spheroidal nebulæ, and ultimately star-clusters, are found to be mingled with the stars. The furthest object the distance of which is known with any accuracy is the star-cluster N.G.C. 7006, which Shapley estimates to be 25,000 times as distant as Sirius. This cluster is so remote that its light takes 200,000 years to reach us; even for light to cross the cluster takes hundreds of years. To all appearances the star-cloud N.G.C. 6822 is still more remote. According to Shapley its distance is about six million million million miles, a distance which light takes a million years to traverse. So far as is known at present, this brings us to the end of our universe, or perhaps I ought to say it brings us back to the beginning.

It is no easy matter to get all these different distances clearly into focus simultaneously, but let us try. The earth speeds round the sun at about twenty miles a second; in a year it describes an orbit of nearly six hundred million miles circumference. If we represent the earth's orbit by a pin-head or a full-stop of radius one-hundredth of an inch, the sun will be an invisible speck of dust, and the earth an ultra-microscopic particle one-millionth of an inch in diameter. Neptune's orbit, which encloses the whole of the solar system, will be represented by a circle the size of a threepenny-piece, while the distance to the nearest star, Proxima Centauri, will be about 75 yards and that to Sirius about 160 yards. On this same scale the distance to the remote star cluster N.G.C. 7006 is 2400 miles and that to the star-cloud N.G.C. 6822 about 12,000 miles, so that roughly speaking the whole universe may be represented by our earth.

It thus appears that we are on this occasion to discuss the origin and past history of a system which bears the same relation to the universe as a whole as does a threepenny-piece to our earth. Why are we so interested in this particular threepenny-piece? Primarily because, although a poor thing, it is our own, or at least one particle of it, one millionth of an inch in diameter, is our own. But there is a historical reason of a less sentimental kind. We have already noticed

[1] Discourse delivered at the Royal Institution on February 15.

the immensity of the gap between our system and its nearest neighbours. As regards astronomical knowledge this gap has taken a great deal of crossing. Well on into last century, human knowledge of the further side of this gap was infinitesimal; the stars were scarcely more than points of light, described as "fixed stars." In those days the problem of cosmogony reduced perforce to the problem of the origin of our own system.

Recent research has changed all this, and the modern astronomer has a very extensive knowledge of the nature, structure and movements of the various bodies outside our system. The cosmogonist of a century ago could assert that the solar system had evolved in such and such a way, and need have no fear of his theories being upset by comparison with other systems. But if I put before you now a theory of the origin of our system, you will at once inquire as to the behaviour of the 1500 million or so of systems beyond the great gap. Are they following the same evolutionary course as our own system, and, if not, why not? It may be well to consider these other systems first.

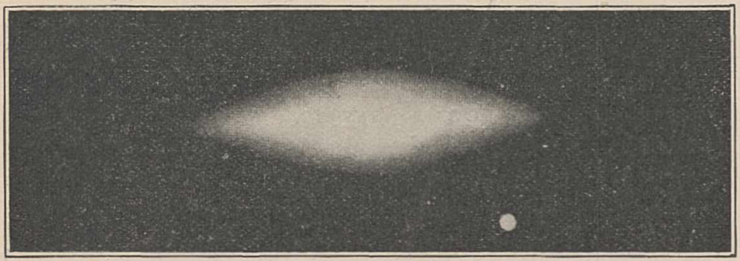

Fig. 1.—Regular shaped nebula (N.G.C. 3115).

Among these 1500 million or so of objects there are certain comparatively small classes the nature and interpretation of which are still enigmatical—the planetary nebulæ, the Cepheid variables, the long-period variables such as Mira Ceti, and a few others. Apart from these, practically all known bodies can be arranged in one single continuous sequence. The sequence is approximately one of increasing density: it begins with nebulæ of almost incredible tenuity and ends with solid stars as dense as iron. There is but little doubt that the sequence is an evolutionary one, for the laws of physics require that as a body radiates heat its density should increase, at least until it can increase no further. Let us begin our survey at the furthest point back to which we can attain on this evolutionary chain—the nebulæ.

After the enigmatical "planetary" nebulæ have been excluded, the remaining nebulæ fall into two fairly sharply defined classes, which may be briefly described as regularly and irregularly shaped nebulæ.

The irregularly shaped nebulæ comprise such objects as the great nebula in Orion, and the nebulosity surrounding the Pleiades. Until quite recently these irregular nebulæ were supposed to be of great evolutionary importance. It was noticed that they were usually associated with the very hottest stars: whence arose a beautifully simple cosmogony, asserting that these very hot stars were the immediate products of condensation of the nebulæ, and that their after-life consisted merely of a gradual cooling until they got quite cold. This cosmogony was too simple to live for long—it was buried some ten years ago by the researches of Russell, Hertzsprung, and others. Thanks to these researches, we now know that the very hot stars associated with irregular nebulæ, so far from being newly born, are standing at the summit of their lives awaiting their decline into old age.

A mass of hot gas isolated in space radiates heat, and this causes it to contract. If the mass radiated without contracting, it would, of course, get cooler; on the other hand, if it contracted without radiating, it would get hotter. But when radiation and contraction are proceeding together it is not obvious without mathematical investigation which of the two tendencies will take command. In 1870, Homer Lane showed that a mass of gas of density low enough for the ordinary gas laws to be approximately obeyed, will in actual fact get hotter as it radiates heat away. Cooling does not set in until a density is reached at which the gas laws are already beginning to fail—that is to say when liquefaction and solidification are already within measurable distance. Thus we see that maximum temperature is associated with middle age in a star, the age at which the star may no longer be regarded as a perfect gas. At this period of middle age the surface temperature of the star may be anything up to about 25,000° C., while the temperature at its centre will amount to millions of degrees. Its average density will probably be something like one-tenth of that of water. It is still not known why stars at this special maximum temperature are so commonly associated with irregular nebulæ. Possibly it may be that only stars at the very highest temperatures are capable of lighting up surrounding nebulosity which would otherwise remain invisible. Be this as it may, it is fairly clear that these irregular nebular masses are not an essential part of the evolutionary chain. They are probably mere by-products, and as such may be dismissed from further consideration.

We turn to the nebulæ of regular shape. A great number of these appear as circles or ellipses, some as ellipses drawn out at the ends of their major-axes, sometimes almost to sharp points. An example of this last type of figure is shown in Fig. 1 (Nebula N.G.C. 3115).

A number of these regular-shaped nebulæ have been examined spectroscopically, and in every case have been found to be rotating with high velocities about an axis which appears in the sky as the shortest diameter of the nebula. The mathematician can calculate what configurations will be assumed by masses of tenuous gas in rotation. If rotation were entirely absent the mass would, of course, assume a spherical shape. With slow rotation its shape would be an oblate spheroid of low ellipticity—an orange-shaped figure like our earth. At higher rotations the spheroidal shape is departed from, the equator bulging out more and more until finally, for quite rapid rotation, the shape is approximately that of a double convex lens having a sharp circular edge for its equator, the shape, in fact, exhibited by the nebula shown in Fig. 1. The whole succession of figures, if looked at along all possible lines of sight, will exhibit precisely the series of shapes which are found to be exhibited by the regular nebulæ under discussion.

nebula must have neighbours, and these neighbours will raise tides on its surface, just as the sun and moon raise tides on the surface of the rotating earth. Whatever the neighbours are, there will always be two points of high tide antipodally opposite to one another, and two points of low tide intermediate between the two points of high tide. Thus the equator, instead of being strictly circular, will be slightly elliptical.

If the equator of the nebula had been a perfect circle, and if the nebula had been in all respects symmetrical about its axis of rotation, the ejection of matter would have started from all points of the equator simultaneously. Indeed, there could be no conceivable reason why it should start at one point rather than at any other point. But in Nature we do not expect to find perfect balances of this kind; if the main factors are of exactly equal weight some quite minor factor invariably intervenes to turn the balance in one direction or another. In the present problem

There are, then, good grounds for conjecturing that these nebulæ are rotating masses of gas; but we can test this conjecture further before finally accepting it.

Fig. 2.—Regular shaped nebula (N.G.C. 5866) with band of dark matter on equator.

As a mass of gas radiates its energy away it must shrink. If it is in rotation, its angular momentum will remain constant, and the shrunken mass can only carry its original dose of angular momentum by rotating more rapidly than before. This conception, which formed the corner-stone of the cosmogonies of Kant and Laplace, is still of fundamental importance to the cosmogonist of to-day. Thus every nebula, as it grows older, will rotate ever more and more rapidly and, barring accidents, will in due course reach the configuration shown in Fig. 1. This configuration marks a veritable landmark in the evolutionary path of a nebula. Until this configuration is reached the effect of shrinkage can be adjusted, and is adjusted, by a mere change of shape; the mass carries the same angular momentum as before, in spite of its reduced size, by the simple expedient of rotating more rapidly, and restores equilibrium by bulging out its equator. But mathematical analysis shows that this is no longer possible when once this landmark has been passed. Further shrinkage now involves an actual break-up of the nebula, the excess of the angular momentum beyond that which can be carried by the shrunken mass being thrown off into space by the ejection of matter from the equator of the nebula.

We have so far spoken of the nebular equator as being of circular shape, as it undoubtedly would be if the nebula were alone by itself in space. But an actual

there could be no choice as between one point of the equator and another if the various minor factors were absent, but when these minor factors come into play, a discrimination at once takes place. Assuming, as seems likely, that the tidal irregularities are the minor factors which determine the choice of points for the ejection of matter, mathematical investigation shows that the ejection of matter will take place from the two antipodal points on the equator at which the tide is highest. The equator being slightly elliptical, these points are of course the ends of its major-axis. After the nebula has passed its critical landmark, shown in Fig. 1, its shape ought to be similar to the lenticular figure which formed the landmark, but with the additional feature of matter streaming out from two antipodal points on its equator.

This describes exactly what is observed in the spiral nebulæ. Fig. 2 (N.G.C. 5866) shows a nebula in which the ejection of matter is just beginning; we notice the bulge along the equator and the dark band which we may assume represents ejected matter which is already cooling. Fig. 3 (N.G.C. 4594) exhibits a more advanced state of development; and Fig. 4 (N.G.C. 891), a still later one in which the ejected matter already dwarfs the central nucleus in size, although probably not in total mass.

In all these figures we are looking at the nebulæ very approximately edge-on. Fig. 5 (M. 51) shows the well-known "whirlpool" in Canes Venatici, a nebula which may be very similar physically to that shown in Fig. 4, but we see it face on: we are looking along its axis of rotation. Again the central nucleus occupies only a small part of the picture. Figs. 6 (M. 101) and 7 (M. 81) show two nebulæ, the evolution of which has proceeded still further, so much so that in the last of these there is very little nucleus left, and by far the greater part of what we see is what we believe to be ejected matter.

Fig. 3.—Regular shaped nebula (N.G.C. 4594) with ring of dark matter surrounding equator.

In both of these last two nebulæ it will be seen that the arms of ejected matter proceed from two antipodal points, exactly as required by dynamical theory. So far we have spoken of the matter in these arms as ejected matter because theory has suggested this interpretation, but we need not be satisfied with theory; there is very direct observational evidence on the point. Various astronomers, especially Van Maanen, have detected motion in the arms of many nebulæ, including the three shown in Figs. 5, 6, and 7. Their observations show that the arms are in real truth jets of matter coming out of the nucleus. Fig. 8 shows the motion found by Van Maanen for about 100 points in the nebula M. 81, the arrows showing the motion in a period of 1300 years,[2] and the measures on the various other nebulæ show substantially similar results; you will see that there is little room for doubting that the arms consist of matter flowing out of the nucleus. On measuring the actual velocities of flow it is found that in nebula M. 51 (Fig. 5) a particle of the jet makes a complete revolution around the nucleus in about 45,000 years; in M. 81 (Fig. 7) the corresponding figure is about 58,000 years, and in M. 101 (Fig. 6) about 85,000 years. From these figures it is possible to estimate the density of the matter in the nucleus. It is found that the densities must be of the order of 10^{-16} gm. per cubic centimetre, a figure representing a vacuum more perfect than any obtainable in the laboratory. The small amount of gas in an ordinary electric light bulb, if spread out through St. Paul's Cathedral, would still be something like 10,000 times as dense as the nucleus of a spiral nebula.

The nebula shown in Fig. 4 exhibits a lumpy or granulated appearance in its arms. In M. 51 (Fig. 5) this takes the form of pronounced condensations, and in the outer regions of M. 101 (Fig. 6) and M. 81 (Fig. 7) these condensations have further developed into detached and almost star-like points of light.

When gas is set free out of an ordinary nozzle into a vacuum it immediately spreads into the whole of the space accessible to it. Why then does not the jet of gas shot off from the equator of the nebula do the same? The explanation is to be found in the gigantic scale on which this latter process takes place. As we increase the scale of the phenomenon the mutual gravitational attraction of the particles of gas becomes

Fig. 4.—Spiral nebula (N.G.C. 891) seen edge on.

of ever greater importance until finally, by the time nebular dimensions are reached, gravitation overcomes the expansive influence of gas pressure and is able to hold the jet together as a compact stream. But, as soon as this happens, dynamical theory predicts that a further phenomenon ought to appear. As regards the distribution of density along the filament, the influence of gas-pressure is in the direction of

[2] The points surrounded by small circles are stars which are believed to have no physical connexion with the nebula.

FIG. 5.—Spiral nebula in Canes Venatici (M. 51).

process we have been considering—we have been watching the creation of the stars. In Fig. 1 we saw the raw material—a gaseous mass of extreme tenuity, already moulded, as a result of shrinkage and consequent increase of rotation, to the stage at which disintegration is about to commence. Further shrinkage takes place, and in Figs. 2 and 3 we see the ejection of jets of gas from which the future stars will in due course be made. In Figs. 4 and 5 individual stars are beginning to form, although at present only as vague condensations in what is still a continuous nebular mass. Finally, the outermost parts of Figs. 6 and 7 show us the finished product—separate masses, although still far more tenuous than ordinary stars, starting off on their independent existences. Each of these masses will go through the changes we have already briefly described. It will contract, getting hotter in doing so, until it reaches a maximum temperature just as the gas-laws are beginning to fail, after which it cools and contracts into a dead dark mass.

The family of stars born out of a single nebula may be millions in number. They may either mingle with the general mass of the stars or, if the original nebula was sufficiently remote from the main universe of stars, may form a separate colony by themselves. In illustration of the former

keeping the density spread out uniformly, while that of gravitation is towards making the stream condense with compact globules. When nebular dimensions are reached the latter tendency prevails, and the issuing jet of gas breaks up into drops much as a jet of water issuing from a nozzle does, although for a very different physical reason. In the photographs reproduced in Figs. 4, 5, 6, and 7 we can trace this process going on.

Dynamical theory not only predicts that these globules of gas must form, but also enables us to calculate their size, mass, and distance apart. A comparison between their distance apart, as calculated in kilometres, and their angular distance apart, as observed in the sky, leads at once to an estimate of the distance of the nebula to which they belong. It is gratifying to find that estimates of nebular distances made in this way are in good agreement with estimates made in other ways. The calculation of the masses of these condensations leads to a still more interesting and significant result. In every nebula for which the calculation can be made, the calculated mass of a single condensation proves to be approximately equal to the mass of the average star.

This gives, I believe, the key to the evolutionary

FIG. 6.—Spiral nebula in Ursa Major (M. 101).

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alternative, numbers of groups of stars are known—*e.g.* the Pleiades, the stars of the Great Bear—in which all the members have a common velocity and, generally speaking, similar physical constitutions also. All the stars of any such group are voyaging through space together, and have obviously done so since they first came into being. The alternative possibility of a family of stars forming a detached colony by themselves is perhaps exemplified in the so-called "globular" star-clusters, such as the well-known cluster in Hercules (Fig. 9). These are globular only in name, for Shapley has found that they are of an elliptical structure, showing symmetry about a plane precisely as might be expected if they were the final product of a rotating nebula.

FIG. 7.—Spiral nebula in Ursa Major (M. 81).

FIG. 8.—Motion in the arms of the spiral nebula M. 81.

Probably we ought not to regard the two possibilities just mentioned as sharply cut alternatives. It is more likely that they represent the two extreme ends of a continuous chain of possible histories for the family of stars born out of a single nebula. It seems quite possible that what we describe as "the main mass of the stars" may be nothing more than a collection of clusters of stars, each cluster having originated out of a single nebula. The clusters are by now so intermingled that it is difficult to look on them as distinct groups of stars, although we can still find some evidence that this may be the proper way of regarding them. In 1905 Kapteyn showed that the stars in the neighbourhood of the sun formed what he described as two "star-streams," each stream moving with its own velocity in space. Except that it begs the question as to the extent of these streams in space, it would have been equally accurate to describe them as forming two intermingled moving clusters. Shortly after, Eddington and Halm, independently, found a third stream or moving cluster, constituted of the very hot stars which the astronomer classifies as stars of types B and O. In this case we know the extent of the cluster in space and also its approximate shape. According to Charlier, it is shaped like a round biscuit lying parallel to the Milky Way, its diameter being about 2·8 times its thickness. Any cluster of stars having a common origin, whatever shape it may assume at first, will be rapidly knocked out of shape when it begins to intermingle with other stars. Dynamical theory shows that after it has been knocked about *ad infinitum* in our universe of stars, such a cluster ought to assume the shape of a round biscuit parallel to the Milky Way, the ratio of its diameter to its thickness being about 2·5. This agrees sufficiently well with what is observed to suggest that all the stars in this stream have a common origin, and the same is true of many of the smaller known moving clusters, such as the Ursa Major cluster already mentioned. Thus, although we cannot claim that anything is definitely proved, there is every justification for thinking of the main mass of the stars as a jumble of intermingled moving clusters, each cluster owing its existence to a separate nebula. This possibility has no very direct bearing on the question of the origin of our solar system; it has been mentioned merely as rounding off our knowledge of what appears to be the main evolutionary process of the stars.

In all its essentials except one, this evolutionary process is similar to, and in its earlier stages almost identical with, that which Laplace, in his famous nebular hypothesis, imagined as the origin of the solar system. We have seen before our eyes the rotating and shrinking nebula finally shedding matter from its equator; we have watched the condensation of this matter into separate masses, and have finally witnessed the start of these detached masses on their voyages into space, all precisely as pictured by Laplace.

The one essential difference is that of size. The evolutionary process we have been watching occurs on a scale such as Laplace never dreamed of. His primeval nebula was supposed to be of about the size of Neptune's orbit, a size represented on the scale I used at the beginning of this lecture by a threepenny-bit. On this same scale the nucleus alone of a good-sized spiral nebula, such as those shown in Figs. 6 and 7, would be about the size of the Albert Hall, while the arms would sprawl over the whole of Hyde Park and Kensington. The pictures of these nebulæ that you have before you would have to be enlarged to the size of a whole country, or even possibly of a whole continent, before a body the size of our earth became visible in them at all.

Although the parent nebulæ we have been considering are all incomparably greater than Laplace's imaginary nebula, yet each tiny condensation, as it starts off into space, is a gaseous nebula the mass of which is just about equal to that imagined by Laplace and the size of which is not perhaps very greatly different. If, then, this younger generation of nebulæ meet with the same experiences in life as their giant parents before them, we should not have to look far for an explanation of the origin of the planets, and if the third generation again repeated the experience of their ancestors, the satellites of the planets are also accounted for. But mathematical research and observation agree in disposing of so simple an explanation of the origin of the solar system. As we have seen, it is only because the filaments in the spiral nebulæ are of such huge size that gravitation is able to cause condensation in opposition to the expansive tendency of gas-pressure. A nebula of mass comparable to our sun might go through the same life-history as the bigger nebula until matter began to be thrown off from its equator, but after this the difference of scale would begin to tell, and the subsequent course of events would be widely different. The ejected matter could not condense into filaments, still less into detached globules; it would merely constitute a diffuse atmosphere surrounding the parent nebula. As such a system shrunk by the emission of radiation, the constancy of angular momentum would, at first, merely demand that more and more gas should be transferred from the centre to the atmosphere.

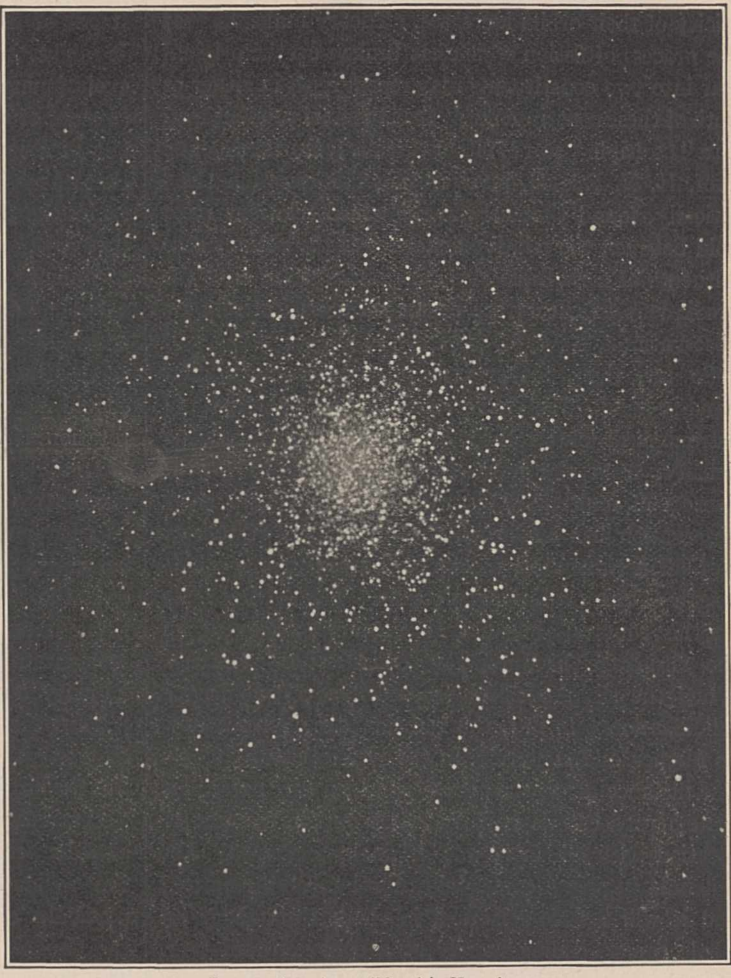

FIG. 9.—Star-cluster (M. 13) in Hercules.

But mathematical investigation shows that in time, after the central star had shrunk to a certain critical density, perhaps somewhere about one-tenth of that of water, a cataclysmic period would ensue, from which the mass would emerge as a binary star—two stars of comparable masses revolving about one another nearly in contact and in approximately circular orbits. This is a formation with which the practical astronomer is very familiar. He finds that a very large proportion—perhaps about one-half—of the stars in the sky are binary, and he can detect an evolutionary sequence in these binary stars. The sequence starts with the formation just described in which the two constituent stars are almost in contact. As it progresses the stars move ever farther and farther apart, while the eccentricity of their orbits increases. Theory indicates that the process of fission which has broken up the original star into two constituents may repeat itself in either or both of these constituents, so that the final product may be a " multiple " star of either three or four constituents. Prof. H. N. Russell, investigating this question theoretically, found that certain numerical relations must hold between the relative distances of the various constituents of a multiple star: he also showed that the predictions of theory are confirmed quantitatively by observation.

So far, then, theory and observation have gone hand-in-hand. We have traced the evolution of astronomical matter through stages of ever-increasing density, from the most tenuous of nebulæ to the densest of multiple stars, and at almost every stage observation has confirmed the predictions of theory. Not all astronomical matter will traverse the whole length of this evolutionary course. The driving force on this course is increase of rotation consequent on the shrinkage produced by emission of radiation. When the shrinkage has proceeded a certain length solidification sets in; the rotation can increase now no further, and evolution, in the physical sense, stops. The distance along the course to which any particular system proceeds depends in effect on the amount of rotation with which it was originally endowed. Let a nebula begin its career with absolutely no rotation, and it will remain spherical in shape throughout its whole career, ending merely as a cold non-radiating, but always spherical, mass. Such a nebula never even gets away from the starting-post. It is true that this is not a likely event, but for aught we know many a nebula may freeze and die before reaching the critical configuration (Fig. 1) at which the birth of stars first commences. Similarly many of the stars may become cold and so cease to develop without ever attaining the stage at which binary systems are formed. In the same way many binary systems must fail to develop into multiple systems. Here again observation is with us: there are ten times as many purely binary systems known as there are multiple systems which have proceeded beyond the binary stage. Theory has traced out for us the whole length of the evolutionary course, but theory and observation agree that not many systems stay out the whole course.

We now come to the crux of the whole question. Nowhere on this course have we found our solar system, or anything in the least degree resembling it. If our sun had been unattended by planets we should have had no difficulty in guessing its origin. It might reasonably be supposed to have been born out of a nebula in the normal way, but to have emerged with insufficient rotation to have carried it on to the later stages of fission into a binary or a multiple system. It might, in fact, be supposed to have had the same evolutionary career as half of the stars in the sky. In support of the conjecture that our sun had been born out of a nebula in the ordinary way, we could note that its mass is about equal to what we calculate ought to be the mass of a star born out of a nebula, and that it is, apart from its planets, similar in every way to millions of other stars to which we may ascribe a nebular origin. In support of the conjecture that it had stopped short on its evolutionary course from want of adequate rotation to carry it on further, we should merely have to note the slowness of its present rotation. A simple calculation shows that the sun has only a small fraction of the amount of angular momentum requisite for fission. Even if we add the angular momentum of all the planets, as we ought if we suppose that these at one time formed part of the sun, the result is the same—the whole system can never have had more than a fraction of the angular momentum necessary for a rotational break-up into a binary star.

Thus the sun is a quite intelligible structure. The difficulty of our problem is not the origin of the sun, but the origin of the planets and of their satellites.

Certain special types of astronomical structure have already been mentioned as not falling into place on the main line of evolutionary development. The particular examples chosen were the planetary nebulæ, the Cepheid variables, and the long-period variables. The question now arises as to whether we must add the solar system to the list. The circumstance that certain structures do not find a place in the evolutionary main line suggests that off this main line may be branch lines on to which the development of a system may in certain circumstances be turned. This indeed is only what might be anticipated. We should no more expect two stars to have precisely the same experiences in their careers than we should expect it of two humans. Our normal star has been supposed to develop in a universe of its own, where its angular momentum remained constant and where it was in every way unmolested by its neighbours. The mathematician finds it convenient to allot a whole infinite universe to each star, but Nature does not. Nevertheless, the conditions postulated by the mathematician are nearer to the truth than is often the case in his idealised problems. On the scale we have already used, on which the sun was represented by a microscopic particle $\frac{1}{10000}$ inch in diameter, the most gigantic of known giant stars may be represented by a pin-head one-thirtieth of an inch in diameter. The present spacing of the stars is such that on this scale there is less than one star to a volume equal to the interior of St. Paul's Cathedral. Space then cannot be said to be overcrowded, and although it is possible that the stars may disturb one another as they move in their courses, it is clear that any serious disturbance of one star by another must be a rather exceptional event. Obviously we have been right in regarding the evolution of a star entirely undisturbed by its neighbours as the normal course of evolution, and we can now see why the vast majority of stars follow this normal course.

To all appearances, the stars which have been side-tracked off this normal course are extraordinarily few in number. The total number of stars in the sky is about equal to the total population of the earth; the number of known exceptional systems would at most populate one small town, although, of course, we can scarcely even conjecture how many exceptional systems there may be which are still unknown to us. There is no reason for supposing that the side-tracking influence has in every case been a neighbouring star, but the systems known to be exceptional are sufficiently few to suggest that this may have been the cause in a large proportion of cases.

The immediate question before us, however, is not that of the exceptional systems in general, but of our own solar system. Was it a neighbouring star that threw it off the main line of evolutionary development? Here, for the first time, observational astronomy denies us any help. Not a single system is known outside our solar system which resembles it in the least degree. The reason is not that no such system exists, but that we could not see it if it did. An astronomer on a distant star observing our system would see Jupiter as the brightest object after our sun, but the ratio of their luminosities would be as three hundred million to one. Seen from our nearest known neighbour in space, Proxima Centauri, the sun would appear as a first magnitude star, and Jupiter as a star of magnitude 22·2, the distance between them being at most four seconds of arc. A star of magnitude 22·2 is still well beyond the range of our largest telescopes, and would be doubly invisible if it had a first magnitude star only four seconds away. We must wait for a very great increase in the power of our telescopes before there will be any hope of seeing systems similar to our own in the sky, even if they exist no further away from us than Proxima Centauri. Thus it is clear that our discussion has now left the regions in which observation can be called upon to make suggestions or to check our conclusions: henceforth we have theory alone to guide us.

Let us start on our quest by noticing that our solar system has quite clearly marked characteristics. It is no mere jumble of bodies looking as though they had fallen together by accident—had it presented this appearance the problem of its origin might reasonably be dismissed as hopeless. Not only has the principal system of the sun and its planets got clearly marked characteristics, but also these same characteristics reappear in the smaller systems formed by Jupiter and Saturn, each with its family of satellites. Each of these small systems is, to all intents and purposes, a replica in miniature of the solar system, so much so that no suggested origin for one system can be regarded as satisfactory unless at the same time it explains the origin of the other two. The principal features common to the three systems are, that the orbits in all three systems are, with few exceptions, all in or close to one plane, that these orbits are all described in the same direction, and that the masses of the secondaries, whether planets or satellites, are all small in comparison with those of the primaries around which they revolve. Thus the sun has a mass equal to 1047 times that of his greatest planet Jupiter, while Jupiter's mass is about 11,000 times that of his most massive satellite. The smallest disparity in mass is found in our own Earth-Moon system with a mass ratio of 81 to 1. In systems possessing many satellites (those of the sun, Jupiter, and Saturn) there is a general tendency for the masses to increase up to a maximum as we pass outwards through the system, and then to decrease to a minimum. Thus in the main system there is a regular progression through Mercury, Venus, Earth, Mars to the maximum mass of Jupiter, broken only by the anomalous position of Mars, while on the descending side the progression through Jupiter, Saturn, Uranus, Neptune fails in regularity only through Neptune being some few per cent. more massive than Uranus.

The main line of evolutionary progress has been supposed to be that of a mass of shrinking, rotating matter—first gaseous, then liquid, then solid—left to itself in space. Such a system must show one very marked characteristic throughout its whole career, namely, a plane of symmetry. In its earliest stage of all, when the system is a mere chaos of independent molecules, the plane will coincide with what mathematicians describe as the "invariable plane" of the system. Later, when the mass has assumed the regular shape of a rotating nebula, the plane is the equatorial plane of the nebula, the plane in which the arms subsequently appear and in which the stellar condensations start off in their orbits. The symmetry of spiral nebulæ about their equatorial planes would of itself suggest strongly that they have developed to their present formations as rotating bodies practically undisturbed by external influences.

If our solar system had developed out of an undisturbed rotating mass, it too ought to exhibit a plane of symmetry. The orbits of nearly all the planets and their satellites do, in actual fact, lie very nearly in one plane, which, to this extent, is, of course, a plane of symmetry. But the sun's axis of rotation is not perpendicular to this plane; the sun has its own plane of symmetry in its equator, and this is inclined at an angle of 7° to the plane of orbits.

The existence of these two distinct planes is enough in itself to suggest that our system has not developed simply out of an undisturbed rotating mass. Thus, in tracing our system back to its origin, we naturally look at the effects to be expected from rotation plus some external influence. To a first rough approximation, it is natural to suppose that the plane of the sun's equator records the plane of rotation of the original system, while the plane of the planetary orbits was in some way determined by the extraneous disturbance.

Of all the interactions between two separate astronomical bodies, gravitational attraction is likely to be by far the most potent. The moon has been accused of exerting all kinds of influences on our earth, as, for example, on its weather, on the destinies, the emotions, and even on the sanity of its inhabitants; but the only influence which survives scientific examination is gravitational attraction as evidenced by the semi-diurnal tides. It is true that a head-on collision between two astronomical bodies would produce more immediately dramatic results than a mere tidal pull; but we shall not consider such an event here. Head-on collisions must of necessity be exceedingly rare; systems that experience them would undoubtedly be deflected from the main line of evolutionary progress on to a branch line; but it does not seem likely that this branch line contains systems like our own. As time does not permit the exploration of all conceivable branch lines, let us turn at once to that which seems most likely to reveal the origin of our system—the branch line that diverges from the main line at the occurrence of a violent tidal encounter.

On the earth, our moon raises tides the average height of which at high tide is only a few feet. This height of high tide is only about a ten-millionth part of the earth's radius, a fraction which we may designate as the tidal fraction. If the moon were ten times as massive, the tidal fraction would be increased ten-fold; if it were brought to half its present distance, the tidal fraction would be increased eight-fold. If we agree to measure masses in terms of the body on which the tide is raised as unity and to measure lengths in terms of the radius of the same body, then the tidal fraction is equal to the mass of the tide-generating body divided by the cube of its distance, say M/R^3. Using this formula, we find that our nearest neighbour Proxima Centauri raises on the sun a tide of quite infinitesimal magnitude; the tidal fraction is about 10^{-26}, and the actual height of tide is of the order of 10^{-15} cm. or, say, one-fiftieth of the radius of an electron. This single illustration will show, and with some margin to spare, that under normal conditions the tidal influence between neighbouring stars is utterly insignificant. For tidal forces to become important to cosmogony, conditions must be abnormal.

Our sun happens at the present moment to have no especially near neighbour; but it is fairly certain that at some time, in its wanderings through the stars, it must have passed stars within a much less distance than that which now separates it from Proxima Centauri. The most trustworthy lines of evidence as to the earth's age, namely, those from geology and radioactivity, indicate an age of from 800 to 1100 million years. For precision, let us think of the sun's age as 1000 million years. Let us imagine for the moment, what is no doubt very far from the truth, that throughout all this thousand million years the sun and all the stars have moved just as they are moving now, with the same average velocities as now and keeping at the same *average* distance apart. Throughout this thousand million years the distance of our sun from its nearest neighbour will have been continually changing, and one star after another will, of course, have taken up the rôle of nearest neighbour. But there must have been some one instant in this thousand million years at which our sun was nearer than at any other instant to its nearest neighbour. A calculation based on the theory of probability indicates that this nearest distance is likely to have been of the order of 7×10^{15} cm., a distance which, although only a six-hundredth of that which now separates us from Proxima Centauri, is still equal to fifteen times the radius of Neptune's orbit. Even if the sun had filled the whole of Neptune's orbit, the tidal fraction at this closest encounter, on the supposition that the nearest star had a mass equal to the sun, would only be equal to $1/(15)^3$ or $1/3375$, giving a height of tide which is quite unimportant from the point of view of cosmogony. So long as things have been as they now are, tidal actions between separate stars must have been quite devoid of cosmogonic interest, except possibly in very special cases of quite exceptionally close approaches.

It is, of course, possible that our sun was the victim of one of those exceptionally close encounters. Nothing can be brought against the supposition of such an event, except its *a priori* improbability. The result of such a close encounter might, as we shall see, be the creation of a system in many ways resembling our solar system.

Our calculations of probabilities and improbabilities have, however, rested upon the admittedly erroneous assumption that stellar conditions have been similar to the present ones for a period of a thousand million years. On looking back through the past history of the universe, we come to a time when conditions must have been very different from what they are now. We come to a time, which we have already considered, when our sun had not yet assumed its present stellar characteristics. It was a condensation in the arm of a spiral nebula moving with thousands of similar condensations towards a free career in space. Its density was enormously lower than it now is, and its size correspondingly greater. It was also much nearer to its neighbours than, in all probability, it has ever been since. In this early stage of its existence, the tidal effects of its neighbours may well have been enormous; we shall pass to exact figures in a moment.

In general, the passage of one star past another merely raises a tide which subsides as the tide-raising body recedes. Even when the approach is so close that the height of the tide raised is greater than the original radius of the star, the recession of the disturbing star may result in the disturbed star relapsing merely to its original spherical form. But there is a limit which must not be passed, and if the disturbing body passes this limit, all hope of the star resuming its original shape is lost. The distance of the limit depends primarily on the mass of the disturber; to a lesser degree it depends on the rotation, shape, and density-distribution of the primary star; and to some extent it depends on the velocity of the two stars relative to one another. We shall get a tolerable idea of the march of events if we suppose the primary star to be surrounded by an imaginary sphere the radius of which depends solely on the mass of the disturbing star. If this mass is equal to the mass of the primary, the radius of this imaginary sphere will be about $2\frac{1}{4}$ times the radius of the primary; if the disturbing star has eight times the mass of the primary, the radius of the imaginary sphere will be $4\frac{1}{2}$ times that of the primary, and so on. So long as the centre of the visiting star remains outside the sphere, a tide is raised which recedes as the visiting star disappears, but the moment the visiting star invades this sphere, an entirely new phenomenon appears.

As the approach of the disturber raises the tide to higher and higher levels, the highest points of the tide move ever farther away from the star's centre, into regions where the gravitational attraction of the star gets weaker and weaker. At the same time, of course, the gravitational pull of the visiting star gets stronger and stronger. Finally, just as the visiting star crosses the critical sphere, its gravitational pull just balances that of the primary—it is this condition that defines the critical sphere. If the visiting star further invades this critical sphere, the particles at high tide are shot away from the primary star, the resultant gravitational force on them now being definitely towards the visiting star; they are of course immediately replaced by others which are shot off in turn, and so on. The total effect is that a filament or jet of gas is shot out from the point of high tide. Each particle of this jet moves under the combined forces of the primary and of the visiting star, and the problem of determining its orbit is a special case of the problem of three bodies, which unfortunately is not soluble. But the general result is that the jet undergoes various contortions while moving all the time in the plane which contains the orbit of the visiting star.

If such a jet had been thrown off the sun simply by an increase of rotation consequent on shrinkage, its gravitational attraction would, as we have seen, be inadequate to resist the expansive effect of its own gas-pressure, and it would have been rapidly dissipated away into space. In the present situation conditions are very different, the essential difference being that, while shrinkage from loss of radiation is a very slow process, tidal disruption may be a very rapid process. The rate of a star's rotation will alter but slightly in a thousand years, whereas ten years may suffice for a tide-raising body to come, do its work, and go away again. The filament of gas set free by increase of rotation would be of extreme tenuity; a filament set free by a tidal cataclysm might easily be of sufficient substance for its own gravitation to hold it together as a compact whole.

If gravitation is potent enough to do this, it will also be potent enough to break up the filament into condensations, just as the filaments of spiral nebulæ are broken up into condensations. But here again an essential difference must be taken into account. The shrinkage of a spiral nebula is a slow secular process. Year after year, and century after century, the filament will be ejected without change of character—the process may be compared to the paying out of a coil of rope. But the tidal disruption of a star is a rapid, even cataclysmic event: within a few years the emission of the filament starts, reaches a maximum, declines, and ends. There is no steady paying out here; the process ought rather to be compared to the discharge of a torpedo, or other body which is thickest in the middle and tapers off at the two ends. When a filament of this shape breaks up into condensations it will form no long chain of similar masses, but a small number of unequal masses. It is natural to conjecture *a priori* that large masses are likely to form out of the central portions where matter is most plentiful, and smaller masses at the ends where matter is scarce. Such a question cannot of course be finally settled by *a priori* conjectures, but in the present case an exact discussion of the problem indicates that the *a priori* view is the right one, and suggests that the comparative abundance of matter in the central part of the filament may provide an explanation of the appearance of the more massive planets, Jupiter and Saturn, near the centre of the sequence of planets.

Obviously, if a tidal cataclysm can explain the existence of the planets, it can also, in general terms at least, explain the existence of the satellites of these planets. For immediately after the birth of any planet, say Jupiter, the original situation repeats itself in miniature. Jupiter now plays the part originally assigned to the sun, while either the wandering star or the sun itself, or possibly the combination of the two, acts the part of the tide-raising disturber. Again we get the emitted filament, again the formation of condensations, and again, as the ultimate result, a sequence of detached bodies with the most massive in the middle. Since Jupiter, the sun, and the disturbing star all move in the same plane, namely the plane of Jupiter's orbit, it follows that Jupiter's satellites, when formed, ought also to move in this plane, as in actual fact they are observed to do.

So long as we merely discuss the matter in general terms it looks as though the process might go on for generation after generation, each member of a family of satellites producing minor satellites to circle round itself, and so on *ad infinitum*. Common sense suggests that this cannot go on for ever: there must be a limit somewhere. Exact calculation confirms the view of common sense, with the disconcerting addition, that we are in danger of overstepping the limit if we attempt to account for the whole of the satellites in the solar system in the way just suggested.

I have already mentioned a mathematical formula which enables us to calculate the masses of the bodies formed out of the condensations in the arms of spiral nebulæ. The same formula puts us in a position to calculate the masses of the planets which ought to be formed from the filament drawn out of the sun. Let us suppose that when the tidal cataclysm took place the sun had a radius equal to that of Neptune's orbit, and therefore a mean density of $5{\cdot}5 \times 10^{-12}$. Let us suppose that at the middle parts of the ejected filament the mean density was one-tenth of this, or $5{\cdot}5 \times 10^{-13}$. Let us further suppose that the temperature of the ejected matter corresponded to a molecular velocity 4×10^4, this being about the molecular velocity of hydrogen or oxygen at their ordinary boiling-points. Then our formula indicates that the masses of the planets formed out of the middle parts of the filament ought to be about 10^{30} gm., a mass intermediate between those of Jupiter and Saturn. This is satisfactory as showing that there is no numerical difficulty in supposing Jupiter and Saturn to have come into being in the way we have imagined. If we like to accept the tidal theory of their birth, we can reverse our calculation and can calculate from their present known masses what must have been the density of the matter from which they were formed.

Naturally an inverted calculation of this kind is not applicable only to Jupiter and Saturn; if the tidal hypothesis is correct, it must be applicable to all the planets and to all their satellites. For example, the first five satellites of Saturn all have masses of about 5×10^{23} gm.; our calculation shows that if these satellites came into being as gaseous condensations in a filament, the gas in this filament must have been anything from one to a million times as dense as lead. Such a conclusion is, of course, preposterous: the only proper conclusion is that these satellites cannot have originated as gaseous condensations.

This conclusion is not surprising, or even unexpected. Even now these satellites, on account of the smallness of their mass, are incapable of retaining a gaseous atmosphere, whence it follows that they can never have existed in the gaseous state. They must have been born either liquid or solid.

In this way we come upon the practical limitation to the possibility of endless generations of satellites being born. Primarily it is that after a time the satellites would be too small for their gravitation to hold them together. A brief reprieve from the operation of this law is afforded by the possibility of the matter liquefying or even solidifying before it scatters into space, and it is probably owing to the operation of this reprieve that all the satellites of the planets, and probably also the smaller planets themselves, owe their existence.

What of our Earth, which interests us above all other planets? Its present mass is rather too small to have been born out of a purely gaseous filament, but we must remember that if it were born gaseous, a large part of its mass might be immediately dissipated away into space, the present Earth representing only a remnant of a once much more massive planet. This line of investigation leads nowhere. A more promising line of attack is through a consideration of our satellite the moon. The more liquid a planet was at its birth the less likely was it to be broken up tidally by the still gaseous sun, but, in the event of this breaking up taking place, the ratio of mass between satellite and primary would be much nearer to unity than in the case of a wholly gaseous planet. Thus, as we pass from planets which were wholly gaseous at birth to planets which were wholly liquid, we ought to start from planets with large numbers of relatively small satellites and, after passing through the boundary cases of planets with a small number of relatively large satellites, reach planets having no satellites at all. This is precisely what we find in the solar system. Leaving Jupiter and Saturn each with their nine relatively small satellites we pass through Mars with its two satellites to the Earth with one relatively very large satellite, and after this come to Venus and Mercury with no satellites at all. Proceeding in the other direction from Jupiter and Saturn, we pass through Uranus with four small satellites to Neptune with one comparatively big satellite. Looked at from this point of view, the Earth-Moon system figures as the obvious boundary case between the planets which were originally liquid and those which were originally gaseous, the corresponding boundary case on the other half of the chain being Neptune. Thus we can conjecture that Mercury and Venus were born liquid or solid, that the Earth and Neptune were born partly liquid and partly gaseous, and that Mars, Jupiter, Saturn, and Uranus were born gaseous.

We have already noticed that Mars and Uranus both have masses which are too small for their positions in the sequence of planets. If the planets were born out of a filament of continuously varying density, the mass of Mars at birth ought to have been intermediate between that of the Earth and that of Jupiter, and similarly the mass of Uranus at birth ought to have been intermediate between that of Neptune and that of Saturn. We have, however, just seen reasons for conjecturing that the two anomalous planets, Mars and Uranus, were the two smallest planets to be born in the gaseous state; they would therefore be likely to lose more mass by dissipation of their outer layers than any of the other planets. Let us introduce the supposition that Mars, and to a lesser degree Uranus, lost large parts of their mass by dissipation into space; let us suppose that they are mere fragments of what were originally much more massive planets, then all anomalies disappear, and the pieces of the puzzle begin to fit together in a very gratifying manner.

Nevertheless, and in spite of the high promise which the tidal theory seems to hold out, it is far too early to claim that it can finally explain the origin of our system; its claim to consideration at present is rather that, so far as I know, it provides the only theory of that origin which is not open to obvious and insuperable objections.

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