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Prevention of Venereal Disease.

THE report of the Committee of Inquiry on Venereal Disease recently published[1] has been awaited with interest. It is a short report, as reports go, and it is an unanimous report, a result ardently desired by all those who have the matter at heart. The conflict of opinion on how best to root out venereal diseases from the community will still be remembered. It was voiced mainly by members of two societies, the National Council for Combating Venereal Disease and the Society for the Prevention of Venereal Disease, and was prominent both in the lay and medical press. The tone and publicity of the discussions seemed at the time regrettable, but it certainly aroused wide interest and helped to spread a knowledge of the main facts about the diseases, and in the end led to the calling together of this committee, the report of which, we believe, will give a fresh impetus to the attack on this world-wide infection. If the report should succeed further in uniting the fighting forces in this country, and make the two societies, which have in truth a common aim, join forces, this would be a crowning achievement. Both have expressed officially or otherwise their acceptance of the report. The co-operation of the two bodies would be so greatly for the good of the cause that we trust mutual goodwill will surmount any difficulties that remain.

The committee was a medical body appointed to consider the medical aspects of the subject under the chairmanship and vice-chairmanship of Lord Trevethin and Mr. Tomlin, K.C., respectively. Morals and medicine have always been liable to become entangled together, and on the subject of these diseases it is particularly difficult to avoid confusion in the public mind. The terms of reference to the committee made it very clear that the medical aspects only were to be considered. The terms were as follows :

"To consider and report upon the best medical measures for preventing venereal disease in the civil community, having regard to administrative practicability including cost."

The committee evidently realised that, in considering only medical measures for the prevention of venereal disease, it was not dealing with the whole problem of prevention. This is clearly set out at the beginning of the report—"having regard to the nature and origin of venereal disease the committee feel that . . . medical measures alone can never operate as an absolute preventive of disease, but their success must always depend largely upon the attitude towards them of the community and the co-operation of the community in securing their largest effect." How dependent a public

[1] Ministry of Health: Report of the Committee of Inquiry on Venereal Disease. Pp. 15. (London: H.M. Stationery Office, 1923.) 3*d.* net.

health authority is on public education and public co-operation for the effective control of any infectious disease was well demonstrated by the difficulties encountered in dealing with the recent smallpox epidemic at Gloucester, and yet smallpox is not usually a disease easy of concealment, there is no transgression of social standards implied in acquiring the disease, and it is of limited duration. All these facts should make it easy to control as contrasted with venereal diseases. In the latter, concealment is further aided by there being usually an absence of disablement from work; indeed, the symptoms may be so slight that the patient may be ignorant of being infected.

With these facts in mind, the conclusions of the committee on the question of notification of venereal diseases will, we think, meet with approval by the majority. The committee has reported against the introduction of notification in any form, on the grounds that as the disease can only become known to the doctor by a voluntary act on the part of the patient, concealment of disease is likely to follow notification, and it would prove a backward step. A modified form of notification, limited to those patients who, having presented themselves for treatment, failed to continue until cured, would impose a penalty on those who had at least shown some care for their health while letting the careless go scot-free. Another difficulty which is emphasised is the absence of any generally accepted standard of cure, and until this has been worked out, insistence under compulsion on a long course of treatment is wisely considered to be outside administrative practicability.

Turning to the controversial question of the *prevention* of disease by disinfection, either self-disinfection or skilled disinfection at the hands of a trained person, the committee agree that disinfectants do disinfect, given that the application is *thorough, prompt, and that the disinfectant is appropriate.* It stresses the fact that, to a large extent, exposure to infection takes place under conditions in which neither promptness nor thoroughness are likely to be exercised, and that the success of any public facilities for self-disinfection in the civil community is likely to be very small. But though in the opinion of the committee the majority would fail, a minority should succeed, and no obstacle should be placed in the way of private purchase of appropriate disinfectants. The law does not to-day permit the sale of *ad hoc* disinfectants. In order to obtain them the public must have either a doctor's prescription or be able to ask for what it wants by the exact name. The report advises the alteration of the law to allow of the sale of disinfectants in an approved form, with instructions for use approved by some competent authority. The suggestion that the Medical Research Council should be invited to undertake this task will, we hope, be received favourably. That body is already responsible for the standardisation of the arsenical compounds used in the treatment of syphilis, and its authoritative and independent position would make it particularly suitable for this undertaking. It is specifically advised that the commercial advertisement of such disinfectant should be prohibited. The importance of self-disinfection will find expression first among the educated classes, and from these will penetrate, as temperance did, into the minds of the community as a whole.

The general application of a system of skilled disinfection, which would necessitate the establishment and maintenance of buildings and also of attendants, is dismissed on the grounds of impracticability and cost, but in a later paragraph the committee shows an appreciation of the value of an experiment such as was made at the Manchester Ablution centres, and suggests that local authorities should be assisted to carry out experimental schemes for the prevention of venereal diseases, as for example in dock areas, where local conditions demand special measures. This, we think, is an excellent method of gaining administrative experience and of educating the public. It has already received official sanction in the past, and we hope that energetic local authorities will take advantage of the suggestion.

In addition, however, to medical measures for preventing disease in, or minimising the risk of disease to, persons exposed to infection, there are those for rendering non-infective, and curing, diseased persons. With regard to the latter, the committee remarks that

"speaking generally, the general medical practitioner is not yet adequately equipped with the most advanced knowledge of venereal diseases and their treatment to enable him to deal competently with all the cases that come before him, and that an improvement in medical education in regard to venereal disease is necessary."

The present clinic system receives a full measure of approval, and extension and improvements are asked for. The importance of the educative work that is done in the clinic is stressed. The actual sufferer from the disease is almost the most important person to teach where limitation of the spread of disease depends so greatly on voluntary individual action. The doctor's words will always carry most weight with the patient, and we believe that most medical officers of clinics realise this and carry out this part of their work with self-sacrificing devotion; but patients may be stupid and ignorant and need often repeated explanations, the doctor's time and endurance are limited. Printed instructions and warnings are less impressive than the spoken word. The recommendation that trained social workers should be attached to the staff of clinics to give supplementary teaching as well as general advice and assistance will, we hope, receive attention. We think that this is one of the most important of the recommendations. It is, in fact, no new departure, but at present the need for such work is not generally realised, and the number of clinics to which workers are attached is limited.

The work of ante-natal clinics is strongly commended. The position to-day as regards congenital syphilis is extremely encouraging. It seems within the bounds of possibility that inherited syphilis may cease to exist some day, so effective is the treatment of the syphilitic mother during pregnancy in securing a healthy baby, though sufficient time has not yet elapsed since the beginning of ante-natal treatment for any one to say that so insidious a disease as syphilis can be wiped out with certainty in every case.

A short paragraph summarily directs attention to three sources of disease which need tackling, although they present serious administrative problems. The three sources mentioned are infected immigrants, infected seamen, and infected mental defectives. The matter of arrangements for the treatment of infected seamen has already received much attention, but more remains to be done. The adequate care of the slightly feeble-minded and infected individual is of great importance to the community; as a focus of infection he or she may do an infinite amount of harm. No amount of teaching will develop a sense of responsibility, and temporary or permanent control is necessary.

The report shows us, in conclusion, how best to lay out our limited public money: first, in the treatment of disease; secondly, in teaching the public about the diseases; thirdly, in improvement of the conditions of living, *i.e.* houses, general education, and facilities for healthy recreation. It ends by directing attention to the decline in the numbers of sufferers from venereal diseases as shown by the clinic figures during the last two years. As, however, these still show an enormous prevalence of disease, no relaxation of effort can be allowed. The work of education on the subject of disease is, moreover, one that must be continued for all time. We cannot hope that venereal diseases will ever cease to exist, and their control will always depend on the enlightenment of the public. It is to be hoped that this report, issued at the very low price of 3*d.*, will be widely read, for it concerns a subject of world-wide importance, and any summary discussion must necessarily leave untouched many important points with which it deals.

Lord Dawson, through whose efforts the committee and consequently this report came into being, is to be congratulated on the performance of a valuable public service.

Physics and its Applications.

A Dictionary of Applied Physics. Edited by Sir Richard Glazebrook. In 5 vols. Vol. 4: Light—Sound—Radiology. Pp. viii + 914. (London: Macmillan and Co., Ltd., 1923.) 63*s.* net.

THERE was a time, not so very long ago, when the student of physics could reach down from his shelves "Ganot" or "Deschanel" and, "laying flattering unction to his soul," could proceed to master their contents with the comforting if misguided assurance that here all useful knowledge was displayed. No such vanity of outlook is possible to the present-day student. The physics of this generation is teeming with such vitality, is making such gigantic strides and devouring at such a pace the boundaries of its sister sciences, that it threatens to overwhelm those of its devotees who vainly seek to achieve an all-round distinction.

The full truth of this is patent to the reader (and reviewer) who has attempted to survey the amazing compendium of knowledge in the various volumes of the "Dictionary of Applied Physics" which have been issued under Sir Richard Glazebrook's editorship. The Dictionary has become, as it was bound to become, a pillar of physical science and a fascinating mine of information, indispensable alike to the teacher, student, and investigator. One had been tempted to wonder whether the high standard set in the earlier volumes could be sustained, but a critical survey of the latest new-comer is amply reassuring. Sir Richard goes on, in fact, from triumph to triumph. Volume 4, which is devoted to light, sound, and radiology, shares in common with its predecessors a clarity, vigour, and "first-handedness" which are characteristic only of the investigator who is in close contact with his subject and endowed with the art of expounding it.

By far the greater part of the present volume is occupied with optical subjects. The first article is one by Dr. A. E. H. Tutton, who gives a short account of crystallography dealing, *inter alia,* with a number of ingenious instruments of his own design which have been employed in his extensive and well-known researches. Dr. John A. Anderson, of the Mount Wilson Observatory, refers briefly to the manufacture and testing of diffraction gratings. One learns that the general impression which prevails that the construction of a successful ruling machine is bound up with the manufacture of a perfect screw is erroneous. It is not difficult to make screws uniformly accurate to $\frac{1}{100,000}$ inch, but much more difficult to avoid errors due to faulty mounting. "The Theory of Diffraction Gratings," by Mr. J. Guild, of the National Physical Laboratory, forms a succinct though abbreviated companion article to Dr. Anderson's. Mr. Guild is also responsible for an excellent summary of the physics of the human eye.

Several articles on glass follow, written from different points of view by Mr. E. A. Coad-Pryor, Mr. W. H. Withey, and the late Mr. Harry J. Powell. Mr. J. Rheinberg discourses on graticules and platinised glass; and the Paterson-Walsh height-finder, which found application in anti-aircraft work in the War, is described by Mr. J. W. T. Walsh.

Dr. W. W. Coblentz, of the Bureau of Standards, Washington, writes briefly on infra-red transmission and refraction data and includes a number of useful tables. An article on the kinematograph by Dr. J. W. French sets out in interesting fashion the main physical points which have had to be dealt with in bringing the instrument to its present state of development. Light filters are discussed by Dr. C. E. Kenneth Mees, and magnetic rotatory power by Prof. T. M. Lowry.

A long article on the optics of the microscope by Prof. A. E. Conrady deals comprehensively with a subject which normally receives inadequate attention. There is a wide gap between the optics of the text-book and that of the practical optician, and here we find the gap bridged by an acknowledged authority. Microscopy with ultra-violet light, and the enhanced resolution that it effects, are the subject of a very interesting article by Mr. J. E. Barnard.

Commander T. Y. Baker sets out in a noteworthy contribution the main underlying facts of navigation and navigational instruments. The mathematics of the Sperry gyroscopic compass are given, and the author makes reference to the atmospheric difficulties relating to the use of directional wireless. At times there appear to be long tracks in the atmosphere offering preferential facilities for the transmission of wireless waves, while at sunrise and sunset marked deviations may occur, directional errors of 20° or more being of frequent occurrence.

Mr. J. H. Sutcliffe unveils the mysteries of the specialised technique of ophthalmic optical apparatus. An article on optical calculations follows, by Mr. T. Smith, of the National Physical Laboratory, who, in association with Dr. J. S. Anderson, writes also on optical glass, including in the article a wealth of numerical data. The working of optical parts by Dr. J. W. French is a contribution of great practical interest, while Mr. T. Smith's very readable monograph on periscopes contains information much of which we imagine must here be set out for the first time. A lengthy and authoritative article by Mr. J. W. T. Walsh on photometry and illumination is notably up-to-date, and describes in detail the precision methods in use at the National Physical Laboratory and elsewhere. Photographic apparatus is treated very completely by Mr. C. W. Gamble, though the section on the modern development of aerial cameras calls for lengthier notice. Photographic lenses are dealt with by Mr. T. Smith.

Prof. C. G. Darwin sums up very briefly the present position of the quantum and radiation theories in physics, and this is followed very appropriately by Dr. W. W. Coblentz's admirable discussion of radiation from a practical point of view.

Two useful contributions on radioactivity and radium by Dr. E. A. Owen might advantageously have been permitted a lengthier treatment. The radium testing-work of the National Physical Laboratory has been of vital importance to the radium market in Great Britain, and we find the methods of test fully set out here. An article on radiology deals largely with the industrial developments of X-rays and the work of the National Physical Laboratory on X-ray protection.

The supreme importance of the short-base range-finder in the War needs no emphasis here. It forms the subject of an arresting contribution by Prof. F. J. Cheshire. The fighting services in Great Britain have favoured the "coincidence" type of range-finder, while the Germans employed the Zeiss "stereoscopic" pattern. In the laboratory there appears to be little in it as regards the two types, but under service conditions it is easier to train men to get accuracy with the coïncidence type, an advantage which is emphasised when an operator is working under the intense nervous strain induced by modern warfare. The battle of Jutland permitted a comparison between the two types with almost identical base lines; and on a balance of evidence the coincidence type must, Prof. Cheshire states, be given first place.

Lord Rayleigh writes on the scattering of light by gases, a subject with which his name and that of the late Lord Rayleigh have noteworthy association.

Prof. E. H. Barton has a long and interesting article on sound and musical instruments in which is included a good, if somewhat brief, discussion of the question of the acoustics of buildings, a subject which is greatly to the fore at present, and is now receiving attention at the National Physical Laboratory and elsewhere. Attention is directed to the investigations of Webster, and in particular of the late Prof. Sabine in the United States, work which is not sufficiently known in Great Britain. We cannot afford to have many repetitions of the new County Hall of London acoustical fiasco. Prof. W. L. Bragg touches briefly on sound ranging, a branch of military activity in which, thanks largely to the Tucker hot-wire microphone, we enjoyed conspicuous advantage in France during the War.

Dr. T. R. Merton writes authoritatively on modern spectroscopy. A very informative article on spectroscopes and refractometers by Mr. J. Guild includes a detailed account of the new National Physical Laboratory standard spectrometer. Spectrophotometry forms the subject of another article by Dr. K. S. Gibson, of the Bureau of Standards.

Major E. O. Henrici deals with spirit-levels and surveying. A recent development which does not find a place is the shaping of the level-tube so that the length of the bubble becomes independent of temperature changes. Prof. Horace Lamb contributes a short note on the vibrations of strings.

Prof. R. A. Sampson, Mr. T. Smith, and Dr. J. S. Anderson give between them an excellent and up-to-date treatment of telescopes, while Sir Richard Glazebrook himself is responsible for a number of short articles on optics. The volume concludes with an uninitialled contribution dealing comprehensively with the measurement of wave-lengths, which we gather from the list of contributors is by Dr. W. F. Meggers, of the Bureau of Standards. Dr. Meggers brings out in a useful summary table the striking fact that the range of electromagnetic waves known to science extends to more than 40 octaves, from the gamma rays of radium on one hand to the wireless and "electric" waves on the other. Since this article was written the gap of four or five octaves between ultra-violet and X-rays has been bridged, and now the only unexplored interval is one of two octaves between the infra-red and wireless waves.

We have perforce had to omit mention of many excellent contributions, but the reader will perhaps discern from what we have cited the quality of the fare that is set before him.

In conclusion, we consider that the substantial weight of the various volumes lends support to the view that they could advantageously be divided into two. We wonder, too, whether the "dictionary" mode of interpolating headings in alphabetical sequence between the various articles has much to justify it. Each volume is provided with an excellent index which could readily be made to serve every requirement, and will normally be the first resort of any reader seeking information. G. W. C. KAYE.

The Thermal Decomposition of Wood.

The Destructive Distillation of Wood. By H. M. Bunbury. Pp. xx+320. (London: Benn Bros., Ltd., 1923.) 35*s*. net.

ONE hundred and fifty million tons of "wood waste" are produced annually, most of which, it is claimed, finds no useful application. Possible methods for the utilisation of this material are its destructive distillation to give valuable products, its employment directly as a fuel, its use in paper production, or its fermentation to produce ethyl alcohol. The first application, and wood distillation generally, although forming the subject of an ancient industry, has not hitherto been taken as the sole title of an English text-book. On account of the important economic problem involved the author has much to justify his effort, and from many points of view his book is a success. The descriptions of plant and processes for wood distillation and of stills and evaporators employed in the recovery of the distillation products are lucid, and while technical details have been considered, exactness in statement has been maintained.

From an economic point of view the efficient working-up of the products of distillation is all-important, and naturally this problem has received careful attention. The works chemist is confronted, among other problems, with the isolation of various organic compounds from his crude liquor condensate obtained when wood is destructively distilled. This heterogeneous product contains, in aqueous solution, acids, bases, alcohols, aldehydes, ketones and other substances, and in suspension, tarry matter of a highly complex composition. The author enumerates more than sixty compounds generally present, in addition to many others found in the crude oil from soft wood distillation. On distillation, after the acid products are fixed, various azeotropic mixtures, both binary and ternary, are formed, which makes the isolation of the individual compounds difficult.

The observations of Guillaume and Sorel on the purification of alcoholic liquors generally by a steam distillation method are not referred to, but developments from these researches are considered in detail, particularly in regard to the production of pure methyl alcohol direct from crude wood spirit.

Wade and Merriman in their classical work on constant boiling-point mixtures adopt the term azeotropic for such mixtures instead of hylotropic as proposed by Ostwald. Young and Lecat also prefer the word azeotropic with its more defined meaning. It is therefore to be regretted that in this volume the older term is again introduced. The author writes of a particular hylotropic mixture of 90 parts by weight of acetone, and 10 parts by weight of methyl alcohol, and later refers to this mixture as the "pure" or "theoretical" methyl acetone.

The author has given numerous flow sheets, but these, it is hoped, may be developed in a future edition, if possible on more quantitative lines. In the technical records of the Ministry of Munitions the idea of flow sheets and flow diagrams, not only qualitative but also quantitative, have been emphasised, and should set a standard.

The analytical methods are condensed into a dozen pages, and offer no novel features. Owing to the drastic condensation employed, difficulties may occur in endeavouring to follow the directions. The alkalinity test for refined methyl alcohol is on the line of the Government "methyl orange alkalinity test" for wood naphtha for use as a denaturant, though this is not stated. In the abstracted form in which the test is described it may be misleading if applied generally.

The separate treatment of physical properties is a welcome feature in a technical volume of this type. The scope, however, is restricted, density and humidity only receiving consideration. The factor of wind velocity as an influence on humidity has not been indicated. More accurate practical means of measuring humidity (*e.g.* Assmann's hygrometer) are available than that described. The main source of reference appears to be the publications of the United States Forest Service.

On the assumption that wood cellulose first forms lævoglucosan on distillation, the author indicates how the two rings in this carbohydrate might be broken up to give many of the usual products obtained on the industrial plant. Pictet, however, obtained his lævoglucosan from a pure cotton cellulose, while it has yet to be shown that the cellulose from broadleaf or conifer trees will give appreciable amounts of lævoglucosan even on vacuum distillation. It must be recognised also that the non-cellulose portion of the wood has a profound influence on the nature of the decomposition. The author states that "it is now established that the complex carbohydrates found in plants are produced in the first place from formaldehyde which is photosynthesised in the leaves from CO_2 and water," and two references are given to the work of Baly and Heilbron. Possibly the word *established* is too strong at this stage in the chemistry of plant structure. The chemistry of wood is restricted to eighteen pages, and of necessity is incomplete. Two structural formulæ proposed by Irvine for cotton cellulose are given, but it is not made clear that even the resistant cellulose in wood has yet to be shown to be of similar constitution. Indication should be made to the fact that Irvine obtained his 2 : 3 : 6 trimethyl glucose from the highly methylated cellulose by hydrolysis.

It is stated that charcoals can be represented as $C_{16}H_{10}O_2$, and in a footnote it is implied that the formula is not intended to represent a single chemical compound. Again, in another connexion $C_{10}H_5O$ is indicated to be "primary charcoal," and $C_{30}H_{20}O_3$ to be "secondary charcoal." Giving definite molecular formulæ, rather than percentage composition only, to these residual products is not considered sound in the present state of our knowledge.

Charcoal was originally the main product sought after in wood distillation for metallurgical needs, but a substitute has been found in coke. Acetic acid and methyl alcohol are now the principal products desired. Organic and biological chemistry are, however, making rapid strides, and soon these products may possibly be produced more cheaply by processes other than the thermal decomposition of wood. The gases once considered unimportant may yet become the mainstay of the wood distillation process. The outlook, however, at present is not very hopeful, at least in coal-producing countries. Recent developments, which are very well described by the author, have been along two lines—the carbonising of wood in gas retorts and in gas producers or generators.

The text shows the mark of careful editing, and only a few errors and misprints have been noticed. In some instances a lack of uniformity in units occurs. Again, such statements as "Add H_2O_2 to decompose the remaining $KMnO_4$," or "1 gram CO_2 = 1.045 grams of H.COOH," might be expressed otherwise. There is too great a tendency to use molecular formulæ as a kind of shorthand in the text.

The volume is more in the nature of a well-written compilation of current literature than a record of the author's personal experiences. It contains one hundred and twenty tables, many of them full-page, as well as more than a hundred illustrations and photographs all excellently reproduced. In the printing and arrangement of the book there is little further to be desired. The only serious complaint that can be offered is that the price tends to restrict the book to the reference library rather than to place it on the shelves of the industrial chemist and technical student, where it would be extremely useful. JOSEPH REILLY.

Clinical Pathology.

A Manual of Clinical Diagnosis by Means of Laboratory Methods, for Students, Hospital Physicians and Practitioners. By Dr. Charles E. Simon. Tenth edition, enlarged and thoroughly revised. Pp. xxiv + 1125 + 23 plates. (London: Henry Kimpton, 1922.) 42*s.* net.

IN any branch of knowledge actively progressing in many divergent directions it is of course difficult to keep the whole field of investigation in proper perspective, and the very keenness of the workers in the different divisions tends to keep them immersed and somewhat solitary in their own grooves. This disadvantage specially concerns medicine, in which it is most desirable that the clinicians and the laboratory workers should be in close and constant touch with each other; to some extent this is effected by clinical pathology, and the clinical pathologist should be the equal and companion of the clinical physician. One of the deservedly best known text-books on this important subject is Dr. Charles E. Simon's, first published in 1896, the tenth edition of which is now before us. During its life of more than a quarter of a century, it has served as a kind of index of the extent of the subject, and in this connexion it may be noted that the present edition is more than double the size of the first. Dr. Simon, who speaks with the authority of a former professor of clinical pathology, a post he has given up for that of lecturer in medical zoology in the School of Hygiene and Public Health of the Johns Hopkins University, Baltimore, is emphatic in his opinion that even now too little attention is paid to clinical pathology by hospital physicians, and that accordingly students and general practitioners are without an accurate idea of the value of this means of diagnosis. Dr. Simon advocates the establishment in every medical school of a chair of clinical pathology, and that its occupant should in every respect rank equally with the clinical teachers.

The subject matter of clinical pathology is so constantly increasing that, as the author admits, it is impossible for a text-book to be actually up-to-date. The truth of this is indeed shown in this instance, for the date of its going to press is apparently June 1922, and there is not any reference to Hijmans van den Bergh's test for bilirubins in the blood serum, now much employed in the differentiation of obstructive from other forms of jaundice, which was first brought prominently to the notice of British readers by Dr. J. W. M'Nee's paper in the *British Medical Journal* of May 6, 1922.

The present edition has 273 pages more than its predecessor, and has been largely rewritten, especially the section on parasitology, which now occupies more than 100 pages and is illustrated by fifty figures.

The subject of the blood takes up more than a fourth part of the volume, and, naturally from the great interest taken in America in the subject of basal metabolism, gives the methods of estimating the hydrogen-ion concentration of the plasma, the determination of the carbon dioxide combining power of the plasma, and the determination of the alveolar carbon dioxide tension. The estimation of the blood sugar and the tests for renal efficiency have been brought up-to-date, though perhaps more might have been said about the lævulose test in connexion with hepatic insufficiency. The serological section has been entirely rewritten, and the author's method of carrying out the Wassermann reaction for syphilis is fully detailed and critically compared with that of Noguchi. It may be noted that in the section on parasites, under the heading of Leptospiras, the genus isolated by Noguchi, the organisms of spirochætosis icterohæmorrhagica and of yellow fever are described with a plate.

The section devoted to the alimentary canal contains a good account of Rehfuss's fractional analysis of the stomach contents, which gives an insight into the entire cycle of gastric digestion, including both the secretory and the motor activities of the viscus. Lyon's method of obtaining bile by means of the duodenal tube is described, but the recent discussion on the validity of the distinction of the three categories of bile—from the common bile duct, the gall bladder, and the liver—obtained by this procedure is not mentioned.

In the second part of the work, occupying about one-quarter of its pages, the diseases are arranged in alphabetical order with the essential points in their laboratory diagnosis. In conclusion, this manual may be confidently recommended to clinical pathologists as a valuable source for daily reference.

Argumentum ad Communem Sensum.

Universe. By Scudder Klyce. With Three Introductions by David Starr Jordan, Prof. John Dewey, and Morris Llewellyn Cooke. Pp. x+251. (Winchester, Mass.: The Author, 1921.) 10*s*.

WE are told on the highest authority that there are things which God has hidden from the wise and prudent and revealed unto babes. The extraordinary claim which Mr. Klyce makes in this book is that the whole riddle of the universe has a verifiable solution which can be made plain to a child of six. Quantitatively indeed, the child might find this book an overdose, but qualitatively it would understand the argument. The author speaks from knowledge, for he tells us he has tried it and found it is so. The preliminary prospectus is so extravagant, and the account of the conception and production of the book (which we are told was rejected by eighteen publishers and turned down by twenty-five financiers, and consequently had to be printed by the author in a press set up by himself for the purpose) is so amusingly naïve that the serious student would probably decide on *a priori* grounds that its value is zero, were he not arrested by the names of three distinguished scholars who have made themselves sponsors for the author and his work. Two of them, Prof. J. Dewey and Dr. David Starr Jordan, enjoy a world-wide reputation. We are compelled, therefore, to treat Mr. Klyce's book seriously.

The first distinction to which we are introduced is that between qualitative and quantitative problems. It is the former which are easily solved: the latter are infinite in number, and as life is finite we cannot exhaust them. It is in regard to the qualitative problems in religion, science, and philosophy, that Mr. Klyce thinks we are being fooled by a trick of language, for this in his view is what "logic," which he opposes to "commonsense," is. Get behind language, behind the sign which merely serves the purpose of a finger-post, to the thing signified, and the problem disappears. We call to mind that Descartes said, "Give me matter and movement and I will make the world." But we also remember Pascal's remark concerning it, "Quand cela serait vrai, nous n'estimons pas que toute la philosophie vaille une heure de peine."

Mr. Klyce divides his task into three parts. Let us leave the philosophy and religion and attend only to the concrete science. The principle and method are the same in all three parts. All difficulties turn out to be a "trick of language," and, when this is exposed, the sophistication is obvious and the truth becomes dull in its very obviousness. When we come to the definite treatment of mathematics and physics the problems prove to be variations of the single problem of the One and the Many. It is not easy to give a clear example, notwithstanding the claim of lucidity, because the text is so laden with diffuse parenthetical remarks. Some notion of the principle may be gained, however, if we reproduce *verbatim* a few sentences from the treatment of Newton's three laws of movement, with which Part II. on Physical Science begins. "Clearly his first law is substantially equivalent to what we started with in formulating language—the verbal meaning of a One which we may arbitrarily divide. It is equivalent in detail to: *all* matter (the One), as such, has the 'property' of *not* changing. And that is no 'property' at all, but an assertion that 'all matter' is *not-divided*—which is a verbal truism at the beginning of monotheistic speech." And this: "It may be reasonably held that his first law is an assertion of or agreement to use God the Father or One words. The second law, then, is a statement of God the Holy Ghost, or 'force.' And we shall see that the third is explicit statement of God the Son."

The book covers very complete ground, and the author shows that he is acquainted with the modern mathematical and physical theories which he discusses in the above manner. There is a certain puzzling inconsistency, however, in finding in logic the principle of "unification" and then condemning logic as a trick. But whether or not readers are convinced by the author's argument, they cannot fail to be interested in the psychology of the author himself which it reveals. Yet it can scarcely have been this which has led Prof. Dewey to write the prologue. Mr. Klyce would render an inestimable service to philosophy if he would persuade Prof. Dewey to add an epilogue; for his prologue leaves us in some doubt as to whether he himself has verified this verifiable solution of the riddle of the universe.

Our Bookshelf.

The Study of English Speech by New Methods of Phonetic Investigation. By Dr. E. W. Scripture. (Published for the British Academy.) Pp. 31. (London: Oxford University Press, 1923.) 3*s.* 6*d.* net.

DR. E. W. SCRIPTURE's memoir deals with the employment of instruments and apparatus which "not only record the facts of speech automatically and permanently, but also provide for interpreting them with microscopic accuracy," and discusses a number of linguistic problems which have been or might be approached by these means. Philologists are divided more or less into two camps by the assertions of Prof. Sievers, of Leipzig, as to the intonation of ancient Hebrew, Greek, Swedish, Gothic, etc. Rejected by some as having no objective basis, his inferences are accepted by others as authoritative, and are now finding their way into the text-books, as in Streitberg's "Gotisches Elementarbuch." Meanwhile the number of phonetic laboratories on the continent is increasing. There are workers in this field in Paris, Hamburg, Prague, Uppsala, Utrecht, Louvain, Kristiania, and other places.

The recent correspondence on Shakespeare's Verse in *The Times Literary Supplement* (closured April 26) shows how attractive such problems of analysis can be to those who like to work at something difficult, and suggests the need of concentration. It is difficult to believe that Shakespeare's lines have ever been more admirably delivered than by Sir J. Forbes-Robertson. A gramophone record allows those prosodists who judge by ear to revise their impressions indefinitely, while a mechanical enlargement of the curves on the disc permits the metrical proportions of duration, amplitude, and frequency to be measured to a high degree of exactness, at the cost, certainly, of much highly skilled labour.

Théorie mathématique des phénomènes thermiques produits par la radiation solaire. Par Prof. M. Milankovitch. Pp. xvi+339. (Paris: Gauthier-Villars et Cie, 1920.) 20 francs net.

THE earlier chapters of this work are concerned with finding formulæ for the amount of "insolation" or reception of radiation from the sun at various latitudes on planets, first without atmospheres, and secondly with them. The formulæ involve the reflective power of the planetary surfaces; the propagation of heat-waves in the soil and the effects of change of obliquity and eccentricity of orbit are also considered. It is pointed out that a rapid rotation diminishes the difference between diurnal and nocturnal temperatures while slow rotation increases it.

The second part of the book applies the formulæ obtained to the case of the four inner planets and the moon. For the earth the author discusses secular changes of climate depending on changes of obliquity and eccentricity, and regards Croll's theory as still tenable, being thus in opposition to most recent climatologists.

Prof. Milankovitch concludes that the thin air on Mars allows a considerable amount of heat to reach the soil by day, but that the nights are intensely cold.

Mercury and the moon are concluded to suffer from great extremes of climate, while the high albedo of Venus indicates that much light and heat is reflected without reaching the surface, so that the temperature of the latter may be moderate. A. C. D. C.

Handbuch der biologischen Arbeitsmethoden. Herausgegeben von Prof. Dr. E. Abderhalden. Lieferung 94. Abt. IX: Methoden zur Erforschung der Leistung des tierischen Organismus. Teil 1, Heft 3: Methoden der zoologischen Forschung. Pp. 439-584. (Berlin und Wien: Urban und Schwarzenberg, 1923.) 6.3 Schw. francs.

THE present number of this extensive work is devoted to methods of zoological investigation. The first article, by L. Neumayer, deals with the fixation of tissues for histological purposes, and provides a useful, well-arranged account of the various fixatives, with notes on the different tissues to which they are applicable. There are also abundant references to the literature of the subject. The second article deals with entomological technique, and is contributed by Albert Koch. In this account are included descriptions of all the various entomological methods of collecting and mounting specimens, rearing larvæ, and the preparation of material for histological study. The third article, by W. A. Collier, deals with the determination of age in fishes by means of growth phenomena afforded by the otoliths, opercular bones, and scales.

The last article is by Th. Mollison, and treats of serum diagnosis as a test of affinities as applied to zoology and anthropology. Previous parts of this work have already received notice in our columns, and the present contribution is no exception to the general standard of excellence that characterises this encyclopædic treatise.

La Lampe à trois électrodes. Par Prof. C. Gutton. (Recueil des Conférences-Rapports de Documentation sur la Physique. Vol. 5, 1re Série, Conférences 11, 12, 13. Édité par la Société *Journal de Physique.*) Pp. 181. (Paris: Les Presses universitaires de France, 1923.) 15 francs.

RADIO engineers will welcome this book by Prof. Gutton. He starts by giving a complete account of the physical phenomena utilised in the three terminal thermionic valve. Full use is made of characteristic curves, and formulæ given by Richardson, Langmuir, and Clerk Maxwell are quoted. In the second chapter several good types of apparatus suitable for amplifying are described and approximate formulæ are obtained for them. In the next chapter oscillating circuits are given, the theory being well and clearly explained. The theory of the methods of using filters to eliminate harmonics is also given. Next comes the theory of detectors and detecting circuits. Finally the arrangement to get "negative resistance" is shown and the methods of obtaining high frequency currents by utilising suitable valves are described and their useful applications in making electric measurements are explained. The author defines the resistance of a circuit as "negative" when an infinitesimal reduction of the terminal voltage produces an infinitesimal increase in the current.

Biologie der Tiere Deutschlands. Bearbeitet unter Mitwirkung zahlreicher Fachleute und herausgegeben von Dr. Paul Schulze. Lieferung 1. Teil 2: Spongiaria. Von P. Schulze. Teil 3: Cnidaria. Von P. Schulze. (Berlin: Gebrüder Borntraeger, 1922.)

THIS is the first of a series of booklets giving an account of the general biology, physiology, life history, and ecology of the animals comprising the German fauna. Marine forms are omitted from considerations of space. No systematic treatment is attempted, and only so much of the anatomy, histology, and embryology of the animals is included as is necessary for a proper understanding of their biology. It is essentially a book of Nature study, wholly excellent in conception, popular in intention and strictly scientific in treatment. It will be issued in a series of pocket volumes, on the lines of Brauer's "Süsswasserfauna," and when completed will form a companion work to Brohmer's "Fauna von Deutschland," in which the systematics of the groups are dealt with. The work is intended for use in the field by students, teachers, and field naturalists generally, and should be of the greatest value in stimulating the study of Nature on a scientific basis. There is room for a similar work on the British land and freshwater fauna; but until such appears this book will, at any rate partially, fill the need.

The Common Birds of India. Described by Douglas Dewar and illustrated by G. A. Levett-Yeats. Vol. 1. The Sportsman's Birds, Wild Fowl, Game Birds, and Pigeons. Part I. Pp. viii+44. (Calcutta and Simla: Thacker, Spink and Co., 1923.) Rs. 2.8.

MR. DEWAR contemplates a series of volumes (five in all, of about 140 pages each) dealing with the birds of India as a whole and forming a profusely illustrated work of a popular nature designed for the guidance of sportsmen and the non-scientific resident. The first part deals with the ducks, swans, and geese, and though the style is too journalistic, the matter is excellent as a good account of the salient features of these birds and of their general natural history. A list of vernacular names and an easily used key for ready identification are two features of special value which we hope will be continued in later volumes. Mr. Levett-Yeats's illustrations add considerably to the usefulness of the work and are worth the expenditure of a little more care in reproduction. There is room for this book, and we hope that Mr. Dewar will receive sufficient support to justify him in carrying the project to completion.

La Vie des atomes. Par Prof. A. Boutaric. (Bibliothèque de Philosophie scientifique.) Pp. 248+4 planches. (Paris: E. Flammarion, 1923.) 7.50 francs net.

PROF. BOUTARIC deals in an interesting way with the recent advances in physics which led to the present view of the structure of the atom. The last part of the subject is treated only very briefly, but the fundamental experiments are clearly reviewed. The treatment is non-mathematical, and the book will be read with interest by those who wish to obtain some conception of the radical changes in outlook which have resulted from recent work. There is no index.

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 Mass-spectrum of Copper.

THE number of elements of which the isotopic nature has been determined is now large enough to give considerable weight to statistical relations. Among elements of odd atomic number two definite empirical rules stand out. The first is that none of them consists of more than two isotopes. This has no exception so far. The second is that the more abundant of the two constituents, or both, will be of odd atomic weight. The only exception to this is the element nitrogen; moreover, the only even isotopes at all are the weaker constituents of lithium and boron. That both of these rules should be violated by copper having the three isotopes 62, 64, 66, announced recently by Prof. Dempster, seemed therefore excessively improbable.

I have now been able to obtain the mass-spectrum of copper by employing cuprous chloride in the accelerated anode ray method used with the mass-spectrograph. The lines are faint, but their evidence is conclusive since they appear at the expected positions 63 and 65 and have the intensity ratio, about 2.5 to 1, predicted from the chemical atomic weight 63.57. The positions of the lines could be determined with great accuracy by comparison with the line 56 due to iron derived from the anode container. No deviation from the whole number rule was observed.

With regard to Prof. Dempster's results (NATURE, July 7, p. 7), it is very suggestive that the intensity and grouping of the lines he ascribes to copper agree exactly with those of the strong isotopes of zinc. It seems possible, therefore, that they are due to the presence of traces of that element either in the copper or more probably, together with the rubidium he mentions, in the furnace material.

F. W. ASTON.

Cavendish Laboratory, Cambridge, July 25.

Polar Temperatures and Coal Measures.

FOR some years I have held a view of the possible origin of some at least of the coal measures of the polar regions that is not found in the ordinary geological text-books. After discussing it with a dozen friends who are geologists, and some of them specialists in glacial geology, I have concluded—somewhat to my surprise—that the theory is new. A short statement of the theory may therefore be desirable.

It is generally considered that certain plants are not limited in their geographic range, by no matter how intense a cold in winter, if only they have an adequately hot summer. Apparently this hot summer may be very short and still the plants prosper. Notable examples are the black spruce of northern Canada and similar trees in the northern part of the Old World.

In the western hemisphere I have examined specimens of coal from 79° north latitude. So far as the material could be identified it was coniferous. In other deposits almost equally far north I have found gum and pine cones.

The northern limit of conifers in North America at present is between 68° and 69° north latitude. That this limit is determined not by intensity of cold in winter, but by lack of heat in summer, is shown by the luxuriance of the black spruce and several other trees in the mountain valleys of the Yukon, where the minimum temperatures in winter are from 10° to 20° lower than at the northern limit of trees. This northern limit is therefore determined by the proximity of the Arctic waters chilled by floating ice, which lower the summer temperature.

The United States Weather Bureau frequently reports temperatures above 90° in the shade, observed under standard weather bureau conditions, at Fort Yukon in Alaska, just north of the Arctic circle. The Bureau occasionally reports 95° F., and has reported even 100° F. The Canadian Weather Bureau reports nothing above 88° F., but that is because its northern stations are strung out at intervals along the northward flowing Mackenzie River. On two journeys down this river (1906 and 1908), and from common report as well as from weather bureau observations, I know that there is on most occasions a wind blowing with almost the steadiness of a trade up the Mackenzie valley from the Polar Ocean. Explorers who have been in the Canadian Arctic, away from the Mackenzie wind-trough, have observed temperatures much higher than those recorded by the Weather Bureau.

We have, then, observational confirmation of the theory, according to which the polar regions receive about as much heat for five weeks in summer as does the equator.

Most observers reporting climate from the polar regions have done so from locations on shipboard or on a sea-coast, where the downpour of the summer sun's heat has been neutralised by the chill of the ocean stored up through a long and cold winter. It is true that the ground in the Arctic is frozen, and that the temperature of the earth 40 or 50 ft. down has been found to be about +10° F., whereas the ocean 50 ft. down would have a temperature about +29° F. Soil and even rock are, however, poor conductors of heat, and the ground chill is imprisoned, while the ocean chill is freely liberated. Furthermore, the great heat of summer produces on most land surfaces a mat of vegetation, which is an even poorer conductor of heat than the earth itself. This is why a thermometer 6 ft. above a damp meadow in the arctic regions of Alaska, protected from the sun's rays in the usual weather bureau way, is able to record temperatures ranging from 60° F. to 100° F. almost every day for a period of several weeks in midsummer.

Consider now what the weather conditions in the Arctic Regions would be if, instead of the present ocean ranging in depth from one to three miles, we had an extensive low land—say a continent as low and as flat as Australia, with the North Pole near the centre of it. Better still, assume that the low land of northern Siberia, with physical characteristics such as it now has, were to extend to and beyond the North Pole, including a large part of the Canadian Archipelago, or even joining up with North America itself. Remembering that the sun delivers about as much heat in the Polar Regions as in the Tropics in midsummer, and also the observation that frozen ground has little effect upon the temperature of the air above it, then according to recorded midsummer lowland temperatures at present in the Polar Regions, we should have at the North Pole July heat of so-called "tropical" intensity, and conditions all over the Arctic suitable for dense forests of black spruce and other trees and shrubs, without calling upon any further alteration in environment—such as different chemical composition of the atmosphere, a shifting of the earth's axis, a change in the shape of the earth's orbit, or an increase of solar radiation.

It is well known that perpetual ground frost to within 10 or 15 inches from the surface does not interfere with the prosperity of a black-spruce forest. At Fort Macpherson, N.W.T., Canada, for example, we have trees a hundred feet high growing straight and close together, and yet I have observed in midsummer that the perpetual frost around their roots was less than a foot below the surface.

As stated above, I do not offer this explanation of certain of the coal measures in connexion with any allegation that the Arctic was once an extensive low land, but merely as an hypothesis which can be called upon in case other evidence shows that extensive low land may once have existed there.

Coal has been found in the Antarctic no less than in the Arctic. The Antarctic is at present in large part an extremely high continent, but it is at least worth considering whether it may not have been a low land at the time when the coal was formed there.

It has been abundantly shown that permanent snow on land in the polar regions depends on altitude and precipitation rather than latitude. Nansen has said that on the low land of northern Siberia no permanent snow has been found, and that he feels certain none can be found. Many travellers, including myself, have reported from northern Canada, northern Alaska, and from the islands to the north of Canada, the total absence of bodies of permanent snow large enough to be called glaciers, though there are small snow-drifts at the end of summer in the shadowed bottoms of deep ravines in some of the Canadian islands. Greenland is 90 per cent. covered with ice, but the largest ice-free area in Greenland is near its northern end, showing that altitude and precipitation rather than latitude are the controlling factors. The smaller glaciers of Franz Josef, Spitsbergen, Ellesmere, Heiberg, North Devon, and the one or two small glaciers of Baffin Island, depend similarly on altitude and precipitation. A mere change of altitude without change of area might therefore remove the whole ice-cap of Antarctica—or certainly it could be removed by a reduction to a general level below 2000 ft. and perhaps a slight increase in area. With the ice once gone, only the Antarctic shores would be kept cool in summer by the sea, the interior promptly adopting the extremely hot June and July weather now found in the Arctic lowlands, thus bringing conditions suitable for spruce forests and the development from them of beds of coal.

VILHJALMUR STEFANSSON.

The Trichromatic Theory of Colour Vision.

THE history of the spread of knowledge regarding the Young-Helmholtz theory of colour vision is a very curious one. As in the case of all other great theories, its range of possible application far exceeds the demands made upon it for the explanation of actual facts. Limitations have to be imposed upon it here and there in answer to inquiry as to which choice out of several has been the one adopted by Nature. This process is in accordance with the development of all great theories. In the earlier stages powerful restrictions are adopted in order that advancement may be made. When these are found to be too restrictive a wider postulate is made so as to include a wider group of facts within the scope of the theory; and, the whole development being simple and direct, the theory at last stands forth as no longer a theory but a fact greater and wider than any of the groups of facts which are contained within its bounds. Thus the electron theory is now, apart from certain tentative developments, a fact standing upon as wide a basis of experience as any so-called fact of which we are cognisant.

This statement also holds in the case of the kinetic theory in general. But, if the great developments by Clausius, Maxwell, and others more recently, were unknown; if nothing were known beyond the results of the early restrictive postulate of perfectly hard, spherical, smooth, and elastic atoms; commentators of to-day might readily be found condemning the theory, and asserting that it could not explain the facts which the recent workers have shown to be direct and simple consequences of its naturally developed postulates. This, or rather worse, is exactly the position with regard to many present-day criticisms of the trichromatic theory of colour vision. These are evidently made in entire obliviousness of developments actually made by Helmholtz himself.

A still more curious condition which subsists is that the commentators are not entirely worthy of blame. For the later developments have never become common scientific property in Britain, while the early developments became widely known.

As examples of the criticisms I give some statements taken from Dr. Edridge-Green's book on colour vision. In doing so I desire to make it clear that I am making no attack upon his valuable and interesting work; I am only replying to his strictures upon the Young-Helmholtz theory, in which he, in my view, inadvertently draws quite undeserved and wrong conclusions. In chap. xxx. he gives six arguments which he holds to be conclusive against the Young-Helmholtz theory of colour vision, and eight against the Young-Helmholtz theory of colour blindness. I assert, on the contrary, that the theory gives a simple and direct account of the phenomena in each case; and I give the mode of deduction in five cases.

"The theory does not explain why there should be a defect in hue perception in those who have lost one of their sensations." Now, actually, the theory explains it beautifully. Thus in any stretch of wavelengths in which two of the three sensation curves have opposite slopes, hue discrimination is correspondingly strong. Therefore annulment of one of these curves diminishes it.

"The theory does not explain why many dichromics have a luminosity curve similar to the normal." This is an example of overlooking the later developments of the theory. If the dichromasy arises from fusion of two of the sensation curves, the distribution of luminosity may be unaltered.

"There are not two or three definite varieties of colour blindness, as there should be according to the theory." Here again there is oversight. In the hard-smooth-elastic-spherical-atom stage of the theory this might have been asserted. Actually, according to the theory as left by Helmholtz, there may be a doubly infinite variety of cases of colour blindness.

"How could the loss of half of a hypothetical green sensation cause dichromatism?" The answer is simple. Given one sensation curve intersecting the other two, if lessening of its ordinates by one half makes it fall entirely within the others, dichromasy is present.

On p. 210, and also in the *Phil. Mag.*, Nov. 1922, Dr. Edridge-Green describes another case. "A man with shortening of the red end of the spectrum and normal colour discrimination will put together as exactly alike a pink and a blue or violet much darker. If, however, the pink and blue be viewed by a normal sighted person through a blue-green glass which cuts off the red end of the spectrum, both will appear identical in hue and colour. This proves conclusively that the defect is not due to a diminution of a hypothetical red sensation, because all the rays coming through the blue-green glass are supposed to affect the red sensation, and yet we have been able to correct the erroneous match by the subtraction of red light."

Now the question of a longer or shorter spectrum with otherwise absolutely normal vision is one which can be dealt with equally easily by all theories. To show the power of the Young-Helmholtz theory, I shall take the most extreme case possible, that in which the peculiarity amounts to dichromasy. Let the pink and violet colours be represented by $x_1R+y_1G+z_1B$ and $x_2R+y_2G+z_2B$ respectively, in the usual trichromatic notation; and let the colour abstracted by the blue-green glass be $a_1R+b_1G+c_1B$ in the case of the pink, and $a_2R+b_2G+c_2B$ in the case of the violet. So the colours seen by the normal eye are $(x_1-a_1)R+(y_1-b_1)G+(z_1-c_1)B$ and $(x_2-a_2)R+(y_2-b_2)G+(z_2-c_2)B$ respectively. If these appear to be identical, we have $x_1-x_2=a_1-a_2$, $y_1-y_2=b_1-b_2$, $z_1-z_2=c_1-c_2$. These are the relations which must subsist amongst the unifiable colours and the colours absorbed by the unifying medium. Now let the dichromasy correspond to the condition $\xi R+\eta G+\zeta B=O$. The pink and violet are then expressible as $(x_1-z_1\xi/\zeta)R+(y_1-z_1\eta/\zeta)G$ and $(x_2-z_2\xi/\zeta)R+(y_2-z_2\eta/\zeta)G$ respectively. These being identical, we have $(x_1-x_2):(y_1-y_2):(z_1-z_2)=\xi:\eta:\zeta$, which are the conditions for Dr. Edrige-Green's case. The trichromatic theory, so far from being helpless, as he asserts, not merely accounts generally for the phenomenon, but tells quantitatively as well as qualitatively what is happening. W. PEDDIE.

Distribution of Megalithic Monuments.

MR. O. G. S. CRAWFORD, in NATURE of May 5, p. 602, criticises what he terms my "speculations" concerning the distribution of megalithic monuments in England and Wales. I am sorry that apparently he did not trouble to read the paper, and to see exactly what I had to say on the matter. My aim was to urge that there is a connexion, in England and Wales, between the distribution of megaliths and certain geological formations, the Granite in Devon and Cornwall, the Chalk in Dorset and Wilts, the Lias in Gloucester and Oxford, and so forth. In this I found that I had been anticipated in part by Mr. Crawford himself. Where we differ, of course, is in the interpretation of the evidence.

An examination of the paper will show Mr. Crawford that I am well aware of the difficulties involved in the theory that the builders of megalithic monuments were attracted to this country by the stores of gold, copper, lead, and so forth, that it contained, and that I discussed the very points to which he directs attention. It must never be forgotten, however, that megaliths are found in all parts of the world, and that possibly the explanation of the presence of these monuments in one country may serve to explain their presence elsewhere. All I have done is to put forward the theory, based on evidence from all parts of the world, that the megalithic civilisation of western Europe was derived from a metal-using civilisation in the Ancient East.

The attention of all who are interested in the matter is being directed to the excellent work now being done by Mr. Crawford at Southampton, and we are all eagerly expecting the publication of the fresh distribution maps of megalithic monuments that Mr. Crawford promises us. But, admirable as such work is, the final solution of the problems presented by these monuments may, after all, come from a wide survey of facts derived from all parts of the world, and not necessarily from detailed work in a limited part of the field. W. J. PERRY.

The University, Manchester.

The Concentration of Hæmoglobin in Blood Corpuscles.

I HAVE very little doubt that Dr. Gorter is right in suspecting that the method which is commonly used for determining the volume of the red blood corpuscles by centrifugalisation is not trustworthy (NATURE, June 23, p. 845). Whether the red corpuscles are biconcave discs or hollowed cones, or indeed, whatever their shape may be, they cannot be packed together without leaving spaces between them unless they are deformed: and if they are deformed there is every reason to be suspicious about their water content remaining unaltered.

The usual method is to centrifuge the blood until the volume of the cells ceases to become smaller, the apparatus generally making 3000 to 5000 revolutions a minute with a disc of something less than a foot. It is easy to convince oneself that the final result depends on just how the process is carried out, for it is different if the blood is first gently centrifuged, say at about 2000 revolutions, and then exposed to the full speed, from what it is if the high speed is used from the beginning. So dependent is the figure obtained on the precise details of the method that, if real comparisons between different bloods is required, it seems to be essential that they must be in the centrifuge simultaneously.

The method seems never to have been examined critically. What is wanted is a comparison between it and the results calculated from the concentrations in whole blood and in plasma of some substance present in plasma and not in red corpuscles, which can be estimated with a high degree of accuracy. Without some control of this kind the method must, for absolute values at any rate, remain under suspicion.

A. E. BOYCOTT.

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Effect of Plant Extracts on Blood Sugar.

OUR studies in connexion with insulin led us to the conception that carbohydrate metabolism is performed by an oxidising ferment mechanism. This theoretical conception induced us to test vegetable material, known to contain oxidases and peroxidases, for oxidising substances having an insulin-like action. In December 1922 we injected 5 c.c. of juice from a new potato intravenously into a 1500 gm. rabbit and noted a fall of blood sugar in one hour from 0·17 to 0·13 per cent. Since then we have found that sterile pieces of raw potato, and juice expressed from these, introduced into a glucose solution, after incubation for twenty-four hours at 37° C., caused this to lose from 26 to 36 mg. of glucose per 100 c.c. These results were published in the *Jour. Amer. Med. Assoc.*, June 2, together with results indicating a diminished glycolytic power of blood from diabetics.

Winter and Smith published a note in the *Journ. Physiol.* 57, 40 (Nos. 3 and 4), 1922, which reached the United States in April last, and in NATURE of March 10, p. 327, stating that they had obtained an insulin-like substance from yeast.

Collip, in NATURE of April 28, p. 571, states that he, working independently, found an insulin-like substance in various vegetables, in yeast, and in clams. Collip's studies on insulin are of inestimable value and made it possible to obtain insulin from animal pancreas in quantities for practical use. He expected to find an insulin-like substance wherever glycogen occurred in Nature, and for this reason looked for it in vegetable extracts. Our belief that oxidising ferments cause glucose metabolism led us to examine vegetables for these ferments and for substances with an insulin-like action. It seems that Collip's theory and ours dovetail. A storehouse of food (glycogen, starch, etc.) and a ferment for the metabolism of this food are necessary wherever growth occurs in vegetables.

Our studies have led us to the tentative suggestion that insulin, which is apparently not itself an oxidase or peroxidase, indirectly stimulates or activates oxidising ferments in the tissue cells to action upon glucose, whereas vegetable extracts contain active oxidising ferments and act directly when injected into animals.

It would seem that the work of Winter and Smith, of Collip, and of ourselves was being carried on simultaneously and independently. Collip, very properly, suggests that " These authors [Winter and Smith] would, therefore, share coincident priority with me in this particular." We think that we should be included in this share of priority.

WILLIAM THALLINNER.
MARGARET C. PERRY.
Laboratories of Columbia Hospital,
Milwaukee, Wis., June 20.

Scientific Names of Greek Derivation.

DR. J. W. EVANS's letter in NATURE (July 7, p. 9) may serve as an excuse for commenting on certain names which have recently been introduced into zoological literature without sufficient regard for etymological principles. Bathosella and Leiosella (Polyzoa) may be given as examples of a series of new genera, proposed in 1917 and later years, with the derivations, as stated, *bathos*, depth, and *leios*, smooth, respectively. In these genera the entire Greek word is used, instead of its root, and the generic name is completed by the addition of a Latin diminutive termination. The suffix *-sella* is in any case likely to cause confusion in Polyzoa, among which *-cella* is the termination of many familiar generic names.

A second series of new genera ending in *-nea* is also of recent introduction, to express an affinity to Idmonea, which was presumably based on ἴδμων. Mesonea and Pleuronea may be mentioned as examples of this misused employment of *-nea*. A third unfortunate suggestion has just been made, to the effect that the Latinised form of ξένος or ξένη (a guest) should be added to the generic name of a host, in forming the trivial name of its parasite. Among the illustrations of this supposed emendation in nomenclature are *ranaxena* and *bufoxena*, both based on Latin words.

According to the Rules of Nomenclature, generic and trivial names cannot be rejected on purely etymological grounds. The same rules do not apply to group-names, and it is accordingly justifiable to suggest that some of them may be amended; for example, that Aplousobranchiata, which has been proposed in Tunicata, should be replaced by the more euphonious name Haplobranchiata.

Dr. W. D. Lang (*Geol. Mag.*, N.S., December, vol. iv., 1917, p. 282) has previously discussed some of the points I have indicated. It may be useful, however, to raise a protest against the continued introduction of names formed in defiance of accepted principles, and I venture to think that this practice will not tend to raise scientific nomenclature in the estimation of scholars.

SIDNEY F. HARMER.
British Museum (Natural History),
July 7.

IN 1844 Sir John Herschel wrote to Owen regretting his spelling of the name of the fossil bird Dinornis, and urged that a Frenchman would pronounce the word Denornis, which he would not do had it been spelt Deinornis. To this Owen answered by directing attention to our pronunciation of the word receive.

Herschel does not seem to have retorted, but he might have done so by quoting—

" segnius irritant animos demissa per aurem
quam quae sunt oculis subiecta fidelibus et quae
ipse sibi tradit spectator."

And the retort would have been final.

F. JEFFREY BELL.
The Athenæum, Pall Mall, S.W.1,
July 8.

The Scattering of Light by Anisotropic Molecules.

PROF. L. V. KING's interesting letter on this subject in NATURE of May 19, p. 667, calls for comment, as his results do not seem to be acceptable in the light of the work carried out at Calcutta in this field during the past two years.

Any proposed scattering formula should satisfy two simple tests, namely, that for a fluid consisting of isotropic molecules it should reduce to the Einstein formula, and that for a sufficiently rarefied fluid it should become the Rayleigh law of scattering. Prof. King's formula (3) satisfies neither of these tests, as can easily be seen on putting $\rho = 0$ in it. The appearance of the adiabatic compressibility in the formula is inconsistent with thermodynamic principles. Einstein has very clearly pointed out that the expression for scattering must involve the isothermal and not the adiabatic compressibility. Further, the omission by Prof. King of the factor $(\mu^2+2)^2/9$ which appears in Einstein's formula, cannot be reconciled with the acceptance of the Lorentz refraction formula for a fluid consisting of isotropic molecules.

Prof. King's explanation of the diminution in the depolarisation in the case of liquids, which occurs as the critical temperature is approached, as due to the breaking up of crystalline aggregates, seems inappropriate in view of the fact that a precisely similar effect is shown by vapours, where obviously the conception of crystalline aggregates is entirely out of place. Mr. Ramanathan's paper on the scattering of light in benzene vapour at high temperatures, which is appearing in the *Physical Review*, clearly illustrates this. The effects observed both in liquids and vapours have been very simply explained without recourse to artificial hypotheses in my papers in the *Phil. Mag.* for January and March, where quantitative data strongly supporting Einstein's formulæ are set out.

The fundamental error in Prof. King's reasoning seems to arise at the point where he suggests that a fluid consisting of comparatively stationary anisotropic molecules, with equally probable orientations in all directions, would scatter only polarised light. This is certainly not the case. It can easily be seen on resolving the effect due to an ælotropic molecule oriented arbitrarily that the components perpendicular to the light vector in the incident wave are affected with a sign which may be either positive or negative at random, *i.e.* irrespective of the position of the molecule in space, and hence, in finding the total components in these directions, we have to add the intensities, not the amplitudes. A fluid consisting of anisotropic molecules oriented at random must therefore necessarily scatter unpolarised light in proportion to its density, and as remarked in my letter in NATURE of March 31, p. 428, considerations similar to those which enter into the Lorentz refraction formula introduce a further factor $(\mu^2+2)^2/9$, which increases the unpolarised scattering to be expected. The whole question will be found elaborately discussed in a paper by Mr. Ramanathan in the Proc. Indian Association for the Cultivation of Science, vol. viii., Part I., just published.

I think I should make it clear that the suggestion made in my letter in NATURE, March 31, and endorsed with some modifications by Sir William Bragg, regarding the relations between the liquid and the crystalline states, is very different from that put forward by Prof. King. In my opinion, neither the facts regarding the scattering of light nor the X-ray data require the assumption of the existence of crystalline aggregates in liquids. All that the experimental facts suggest is that the molecules in a liquid influence the orientations of their nearest neighbours to a sensible extent, and that this results in the amount of unpolarised light scattered being somewhat smaller than on the hypothesis of random orientations of the molecules.

C. V. RAMAN.

210 Bowbazaar Street, Calcutta,
June 15.

The Doublet Separations of Balmer Lines.

IN his theory of the structure of the lines of the Balmer Series based on the principle of relativity, Sommerfeld shows that each of the members of the series should consist of a doublet and that each of the components of these doublets should possess a fine structure. The calculations show besides that the frequency difference for these doublets should be constant over the whole of the Balmer Series and should be equal to 0·36 cm^{-1}. For H_α the separation should perhaps be slightly less. As the theory applies equally well to the doublets of the corresponding series in the spectrum of positively charged helium, these were investigated by Paschen and were found to have separations that lead to a value of $0{\cdot}3645 \pm 0{\cdot}0045$ for the frequency difference of the doublets of the Balmer Series.

Since the publication of Paschen's work on helium a number of investigators, including the writer, have attempted from the measurements on the separations of H_α and H_β and in some cases of H_γ and H_δ to look for evidence that would lead to a confirmation or rejection of Sommerfeld's theory. Up to the present the results obtained could not be considered as satisfactory. There was a lack of agreement in the values obtained for the separations by different investigators, and on the whole the values obtained were less than that demanded by the theory. In the case of the observations made by myself and Mr. Lowe on the separations of H_γ and H_δ, values were obtained that seemed to point in the direction of a steady decrease in the frequency differences as one passed to the higher members of the series.

At my suggestion the matter was re-investigated recently by one of the research workers in the Physical Laboratory of the University of Toronto, Mr. G. M. Shrum. In his experiments the tubes were of a special design and were cooled with liquid air.

His method of operating these tubes, which will be described later in his own paper, enabled him to eliminate practically the whole of the secondary spectrum and thus permitted him to include in the measurements of the doublet separations that of H_ϵ as well as those of H_α H_β H_γ and H_δ.

The results are the following:

Line.	Wave-length.	Separation of the Components.		Probable Error.
		$\delta\lambda$.	$\delta\nu$.	
H_α	6562·79 Å	0·143 Å	0·33 cm^{-1}	0·02 cm^{-1}
H_β	4861·33 ,,	0·085 ,,	0·36 ,,	0·01 ,,
H_γ	4340·46 ,,	0·070 ,,	0·37 ,,	0·02 ,,
H_δ	4101·73 ,,	0·061 ,,	0·36 ,,	0·02 ,,
H_ϵ	3970·07 ,,	0·055 ,,	0·35 ,,	0·02 ,,

It will be seen that as far as the doublet separations are concerned, they afford a striking confirmation of Sommerfeld's theory.

J. C. McLENNAN.

The Athenæum,
July 2.

"Guide to the Mollusca."

WITH reference to the review of the "Guide to the Mollusca" in NATURE of July 21, p. 93, may I be allowed to point out that our rather cautious statement, "A species of *Helix* has been said to tolerate a temperature of −120° C.," was based on Pictet's paper "De l'emploi méthodique des basses températures en biologie" (Arch. Sci. Phys. et Nat. Genève (3) xxx., 1893, pp. 293-314). The reviewer's remark about the scientific names of the pearl mussel and the pearl oyster scarcely makes it clear that we are simply keeping to the names used by the late Mr. E. A. Smith in 1908, *Margaritana margaritifera* for the mussel and *Margaritifera margaritifera* for the oyster. I hope that the other errors he has discovered are not more serious than these.

C. TATE REGAN
(Keeper of Zoology).

British Museum (Natural History),
Cromwell Road, S.W.,
July 23.

MR. REGAN is quite right to direct attention to the fact, which I should have noted, that the confident statement in the text-book concerning the survival of a species of Helix submitted to a temperature of −120° C. had been altered from "has been known" to "has been said," but I still think it would have been better to have omitted it altogether. Pictet in his paper does not say whether the degrees he cites were registered by any one of the more usual thermometers or by a scale of his own (the "C" is an addition in the text-book), and his paper altogether does not suggest that amount of accuracy which the subject demanded. The admission that a system of nomenclature nearly a quarter of a century old has been deliberately adhered to in a work supposedly brought up-to-date, speaks for itself. Much progress has been made in this section of systematic zoology since 1908, and according to all the Rules the pearl oyster (Pinctada) has no right to the name Margaritifera, which belongs to the pearl mussel. There are other examples in the "Guide" of what a malacologist of to-day would call misnaming.

THE REVIEWER.

The Temperatures of the Stars.

By HERBERT DINGLE.

THE measurement of the temperature of a star is one of the most difficult problems of physical astronomy. The difficulties are of two general kinds. In the first place, the very phrase, "the temperature of a star," has no meaning: we may as well speak of the latitude of the land surface of the earth. There can be no doubt whatever that the temperature varies from one part of a star to another over an enormous range—probably thousands of times greater than the interval between the temperatures of liquid hydrogen and the electric furnace. Secondly, for experimental methods of measurement the only available data are wrapped up in an inconceivably small fraction of the total radiation of the star which reaches the earth after the possible wear and tear of many years' journey through interstellar space and our own atmosphere. From the character of that radiation we have to deduce the temperature of the star. From these two general sources difficulties of many kinds issue forth.

Happily, the resources of modern physics make the problem anything but hopeless. The "temperatures" of a number of stars have been determined by different methods, though exactly what the figures mean, and how much reliance can be placed on them, are perhaps still matters of doubt. With regard to the first source of difficulty, considerable help is received from the spectroscope. More than ninety-nine per cent. of recorded stellar spectra consist of absorption lines on a continuous background—conclusive evidence that a star consists of at least two distinct parts. In the light of Kirchhoff's principle, the continuous spectrum is attributed to the hotter, deeper-lying part, and the absorption lines to a surrounding cooler, but still luminous, atmosphere. Accordingly, temperatures measured from the characteristics of the absorption lines must apply to the atmosphere, and temperatures measured from the continuous spectrum must apply to the interior.

The next questions are evidently: Do the atmosphere and the interior, as thus defined, comprise the whole star, or are there regions outside the one and beneath the other? In the former event, what parts of the atmosphere and the interior have the respective measured temperatures, and, in the latter event, what are the temperatures of the unconsidered regions? For the answers to these questions we are indebted mainly to the nearest star—our sun. We know, from observations made possible by a total solar eclipse, that outside the sun's atmosphere (*i.e.* the source of the absorption spectrum lines) there is the corona—evidently a permanent though ever-changing part of the solar structure. We know also that the source of the sun's continuous spectrum is effectively a layer of limited thickness near the surface, because the luminosity of the sun's disc does not fall off appreciably outwards from the centre until the limb is nearly reached. There must, therefore, be a core inside what we have called the "interior," about which, from direct observation, we know nothing. We may assume, then, that in addition to the regions the temperatures of which we measure from the spectrum of a star, there are other very extensive regions, the temperatures of which it is at present quite impossible to determine by any experimental means.

The temperature throughout the atmosphere of a star may be regarded as a constant quantity. To solar eclipses, again, we owe the knowledge that the sun's atmosphere is very thin compared with the depth of the whole globe. It is true that there are indications that its physical condition varies at different levels, but these variations are refinements of analysis which we cannot hope to apply to the stars for a long time to come. If we can determine a temperature from the absorption lines in the spectrum of a star, we are justified in supposing that we can state definitely the temperature at a particular part of the star. The case is not so clear when we come to the continuous spectrum. We do not know at all definitely from what part of the star the continuous spectrum comes. We know that it must come from beneath the atmosphere, and it has just been pointed out that it represents the radiation of a surface layer, which we may call the "photosphere," but how thick that layer is, and what part of it has the temperature deduced from its spectrum, are questions that are still unanswered.

The first set of difficulties, then, can be partly overcome. Assuming that the sun is a type of its kind, we can divide a star into four distinct parts—a corona, an atmosphere, a photosphere, and a core. Of the temperatures of the first and last, we know, by direct experiment, nothing. The temperature of the second can possibly be measured definitely, and that of the third, vaguely. Supposing these measurements to be made, theory indicates, for certain stars, what must be the temperatures at different parts of the core.

Turning now to the second set of difficulties—those connected with the actual measurement of the temperatures—we note that these may be subdivided into the difficulties of obtaining the requisite data, and those of interpreting the data when they are obtained. It is probably fair to say that, in measuring atmospheric temperatures, the former preponderate, while the latter are most in evidence in the measurement of photospheric temperatures. It was Lockyer who first showed the influence of temperature on the line spectrum of a substance, and urged that the relative temperatures of stellar atmospheres could be determined from a study of the lines by which particular substances were represented. More recent investigations, originated by Saha, have confirmed Lockyer's views, and have shown how the actual temperatures can be calculated. But it appears that, while temperature is probably the chief factor in determining the line spectrum, it is by no means the only one. Pressure, the absorption of photospheric radiation, the relative amounts of different substances in the atmosphere, the ionisation potentials of the elements—these at least play a part, and must be determined before the temperatures can be found. Unfortunately, they are, in most instances, unknown, and their values have to be assumed, on more or less plausible grounds. There is, therefore, a considerable element of uncertainty in existing estimates of the temperatures of stellar atmospheres.

The temperatures of the photospheres are deduced from the distribution of energy in the continuous spectra. Laboratory experiments on black-body radiation show that the spectrum of a perfect radiator at uniform temperature is characteristic of the temperature. Wien's displacement law states that the wave-length of the radiation carrying the maximum amount of energy is inversely proportional to the absolute temperature of the source of radiation, and Planck's radiation formula expresses, with great accuracy, the distribution of energy throughout the spectrum. Assuming that the photosphere of a star bears the same relation to its continuous spectrum as does the equivalent of a black body in the laboratory, the photospheric temperatures are found. Wilsing and Scheiner, of Potsdam, and Nordmann, of Paris, have used the method, and Sampson has recently shown that, by employing a photo-electric cell as an energy detector, the accuracy of the measurements may be greatly increased.

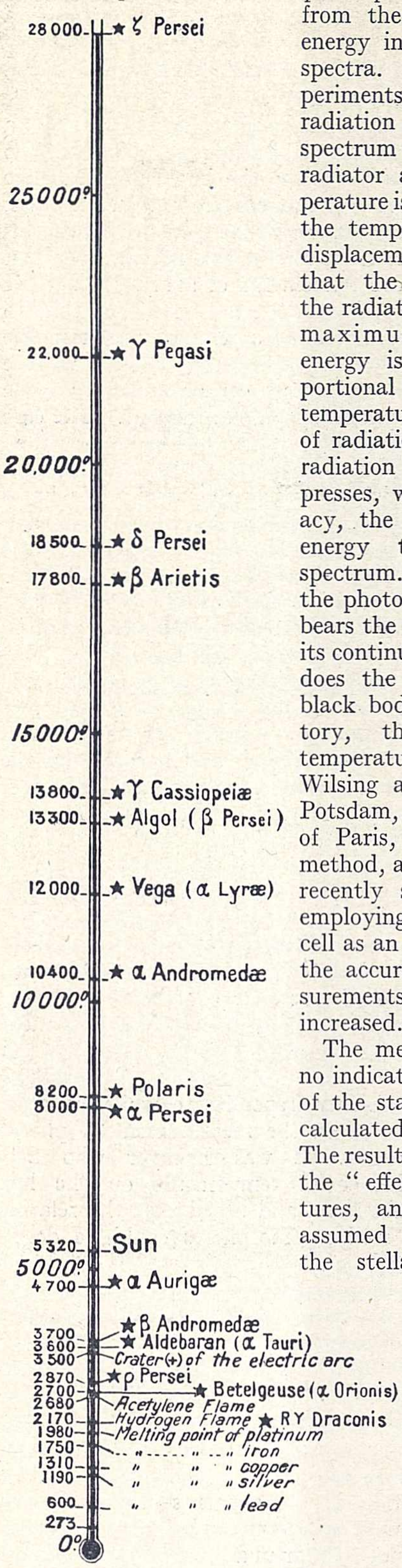

FIG. 1.—Scale showing, in absolute centigrade degrees, the temperatures attained in certain terrestrial processes compared with the effective temperatures of representative stars.

The measurements give no indication of the region of the star which has the calculated temperature. The results are spoken of as the "effective" temperatures, and are generally assumed to characterise the stellar surfaces, immediately beneath the atmospheres. Two remarks might be made concerning them. First,—granting for a moment the validity of the method of measurement, they represent minimum temperatures only, for if the stars are not perfect radiators, their temperatures must be higher than the calculated ones. Second,—it is a somewhat dangerous assumption that the resultant radiation from a globe of gas, perhaps millions of miles in depth and varying in almost every physical quality from point to point, will give a spectrum comparable with that of a thin solid surface at a uniform and probably very much lower temperature. We know practically nothing as yet of the processes of production of continuous spectra. We have no means of distinguishing one such spectrum from another except by measuring the distribution of energy in it; yet it is certain that there may be profound differences in the modes of origin. The continuous spectra of a cold fluorescent body, of an electric glow-lamp, of hydrogen radiating also the Balmer series—here at least are three spectra which probably have nothing in common except their appearance. The stellar nuclei of planetary nebulae, again, give spectra which suggest the operation of the classical laws of radiation rather than those of the quantum theory, unless the stars have temperatures so high that no one is prepared to accept them.

It is noteworthy, however, that the atmospheric and photospheric temperatures, estimated by totally different, and at best approximate, methods, are of the same order of magnitude. Fig. 1[1] shows, on a thermometric scale, the range of temperatures covered by present measurements. Temperatures have been measured at almost all points intermediate between the absolute zero and the temperature of ζ Persei. The cores of the stars, according to Eddington's theoretical researches, reach temperatures far too high to appear on the scale. It is probable that there are bodies in the universe at all temperatures between absolute zero and 20 million degrees centigrade or higher.

Whatever may be said of the absolute accuracy of stellar temperature measurements, it is scarcely questionable that they show the true order in which the temperatures are arranged. There is no doubt whatever that Vega is hotter than Aldebaran in corresponding regions. Consequently, if the order of stellar evolution can be established from other data, it becomes possible to determine the changes of temperature of a star throughout its life. Russell's well-known theory of evolution takes the order of increasing density of a star to be its order of development: contraction is a continuous process from childhood to old age. This implies that a star passes twice through the same series of spectral types, and therefore through the same series of temperatures. Beginning as a huge, rarefied, cool mass of gas, it contracts and becomes hotter until a stage is reached when it is too dense to obey the laws of a perfect gas. The temperature then soon reaches a maximum and begins to fall—contraction, however, continuing, though at a slower pace—and the star retraces its path through the sequence of spectral types which it traversed on its upward journey. While the temperature is rising, the star is a "giant," and after it begins to fall the star becomes a "dwarf." The career of a typical star, with time as abscissa and temperature as ordinate, is pictured in Fig. 2: continuous contraction is indicated by the decreasing diameter of the circles representing the star.

[1] The diagrams illustrating this article are adapted, by kind permission of Dr. Charles Nordmann, from an article by him on "La vie et la mort des étoiles," which appeared in *L'Illustration* of April 7, 1923.

The temperature reached at the maximum point depends on the mass of the star: the greater the mass, the higher the temperature and the longer the stellar life. Fig. 3 illustrates the careers of the sun and of stars the masses of which have nearly the extreme values found in Nature. Probably a star having a mass less than one-tenth of that of the sun would not become hot enough to be seen, while Eddington has shown that stars much more than ten times as massive as the sun would be unstable. Only the most massive stars can reach the B and Oe 5 stages of the Harvard spectral sequence. The lighter stars, like the sun, turn back at the A condition, or even at a still lower stage.

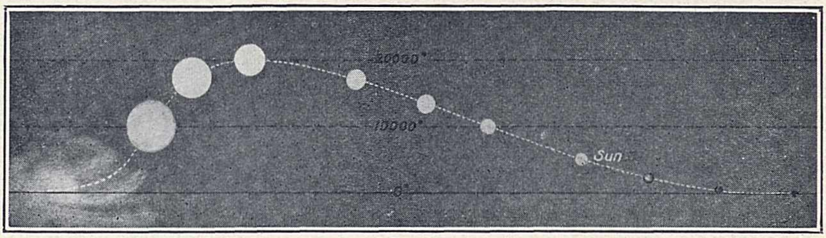

FIG. 2.—Diagrammatic representation of the theoretical development of a massive star from an original nebula to a final cold, dense body. The dotted curve is to be regarded as identical with the highest curve in Fig. 3.

Temperature appeared at first, in this great stellar drama, to play a dependent rôle. The star developed heat by contraction, and radiated heat into space. So long as the amount of heat developed exceeded the amount radiated, the temperature would rise, and when, through retardation of contraction and increase of radiation, the conditions were reversed, the temperature would fall. This view is satisfactory in every respect but one—it indicates a length of stellar life far shorter than geological and other evidence makes it possible to admit. In order to account for the amount of heat which a star radiates during its immeasurably long life, it is necessary to suppose that the heat generated by contraction is supplemented by an enormous supply of energy from some other source. Nothing is certainly known of the nature of this supply. Possibly, as Eddington proposes, it is to be found in the formation of heavier elements from hydrogen. But, wherever the energy comes from, it is difficult to avoid the hypothesis that it can be released only at the extremely high temperatures attained near the centres of stars. Contraction raises the temperature of a star up to a certain point, and then temperature takes charge and sets free energy from the unknown source at a rate almost equal to the rate of radiation, so that the star is in a condition of approximate equilibrium. It is a problem for the future to determine the origin of the almost inexhaustible supply.

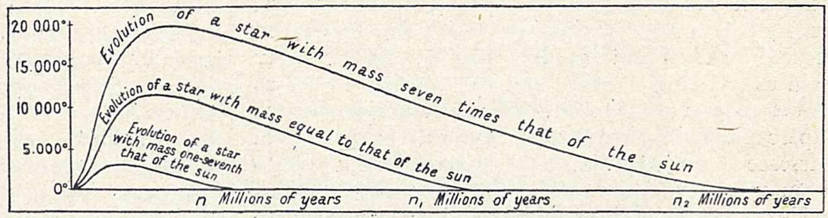

FIG. 3.—Curves illustrating the course of evolution of stars of differing mass, showing that, the more massive the star, the longer is its life and the greater is the range of temperature through which it passes.

Man and Scottish Animal Life.[1]

By Dr. JAMES RITCHIE.

IN the opportunity it affords for the study of the part man plays in the evolution of a fauna, the animal life of Scotland stands alone. This is largely due to a series of geological accidents: the Glacial Period, which made a clean sweep of former faunas; the post-glacial continental land bridge, which allowed immigration from the mainland of Europe, and the subsequent breaking of the continental connexion. Thus there was isolated on the *tabula rasa* of Scotland a fair sample of the post-glacial European fauna, which henceforth was removed from the possibility of subsequent migrations such as complicate the history of continental faunas, and the later evolution of which must in general be due either to the influence of physical and organic changes limited in time and space, or to the interference of man.

The influence of man was itself strictly limited in time, for the earliest human settlements so far recognised in Scotland date back only to Azilian times. It was also unequal in its incidence, gaining in intensity with the passing of time. Thus during the Neolithic, Bronze, and early Iron Ages only some four of the larger members of the original fauna disappeared—the giant fallow deer, the lynx, the lemming, and the rat vole—and it is doubtful whether the disappearance of any of these was due to man's presence. We may say, therefore, that when the Roman legions followed Agricola northwards through the marshes of Scotland in the early years of our era, they found a fauna which, except for the presence of primitive domesticated animals, differed little in kind from that which greeted man on his first arrival in Scotland some 8000 years before. But the following centuries saw more rapid changes, which so increased that by the sixteenth century many new and important elements had been added, while most of the larger members of the old fauna had been swept away, with the extermination of such as the reindeer, the elk and the wild boar, the brown bear and the beaver, the great bustard, the crane and the bittern. Nowadays the content and assortment of the fauna, the relative numbers of its members and their distribution, show little resemblance to the conditions of the original post-glacial immigrants.

The degree of man's interference may be compared

[1] Summary of an address delivered at the request of the Council to the Royal Society of Edinburgh on July 2.

with the influence of the ordinary forces of Nature which are constantly modifying the animal life of a country. There is a constant ebb and flow within a fauna, a swing of numbers due largely to seasonal changes and fluctuating about a mean—the "balance of life"; and where man's interference is temporary in its incidence it falls into this category. But there is, besides, a definite faunal evolution, a faunal drift; and where man's influence is persistent in one direction it must be reckoned as sharing with the great secular forces of Nature in propelling a fauna upon a path along which there is no return.

The nature of man's interference, directly or indirectly exercised upon the animal life of Scotland, is of great complexity, but it may be conveniently grouped according to results. In some ways man has reduced the numbers of animals, in some ways he has increased their numbers, and in some ways he has modified their habits and even their structures. Reduction of the fauna, which, commencing with a mere cutting off of the numbers of a species, may proceed to a marked limitation in the range of distribution and finally to extinction, has been brought about directly by deliberate destruction, as in the cases of the polecat and the urus, and indirectly by cultivation, which has destroyed feeding grounds and breeding haunts, driving away such as the great bustard and the bittern, as well as by destruction of the forest. with which disappeared the capercaillie and the red squirrel, both since reintroduced. Increase in the quantity of the fauna is largely due to an intensive cultivation which has provided bounteous food-supplies for such as rabbits and sparrows, and to deliberate protection of other creatures for food, sport, or amenity. The quality of the fauna has been increased by the addition of new elements from other lands, either introduced deliberately, like pheasants and the common rabbit, or carried hither by mischance of international commerce, like the black and brown rats and many an insect pest. Habits have been changed: the one-time cliff-dwelling swallow has been converted into an inhabitant of houses; and structures have been changed in converting wild into domesticated animals, and by the alteration of habitats, whereby the red deer has lost many points from its antlers and several cubits from its stature.

It must not be imagined, however, that a simple enumeration of first effects exhausts the tale of man's interference. The story of the effect produced, by protecting a few black-headed gulls, upon the vegetation of a heather moor and its fauna (which I have described elsewhere) illustrates how the slightest interference with wild life may produce complicated and far-reaching results, and that in a remarkably short space of time.

A final comparison of the modern fauna of Scotland with that found by Azilian man on his arrival on these shores, shows that the modern fauna is much more rich in numbers than the old fauna, and that in addition, in spite of the extermination of many forms, it is also more varied in species. The consistent tendency throughout the period of man's presence has been for the larger animals, which formed the most impressive contingent of the wild life, gradually to be rooted out; while the additions consist largely of lesser creatures, many of which have gained entry only because their minuteness has enabled them to escape detection. The great change therefore has been a notable diminution in the standard of size of the wild fauna, and this tendency is still strongly marked in the evolution of the Scottish fauna at the present day.

Obituary.

MR. S. S. HOUGH, F.R.S.

MR. SYDNEY SAMUEL HOUGH, H.M. Astronomer at the Royal Observatory, Cape of Good Hope, died on Sunday, July 8, at Gerrard's Cross. He had visited Europe last summer and had attended the meeting of the International Astronomical Union at Rome, but after his return to South Africa he was in poor health and ultimately cancer was diagnosed. He came back to England under the care of a nurse in the spring of this year and succumbed to the disease after a painful illness.

Mr. Hough was born at Stoke Newington on June 11, 1870. After distinguishing himself at Christ's Hospital School, he proceeded to St. John's College, Cambridge, as a foundation scholar. At Cambridge he had a brilliant career and graduated as third wrangler in 1892. He was awarded the first Smith's prize in 1894, and soon after was elected to an Isaac Newton studentship and to a fellowship at his college.

After taking his degree, Mr. Hough devoted himself to research work in astronomy and geophysics. It had recently been found by Küstner and Chandler that the free period of the variation of latitude differed from that predicted by Euler, and the investigation of this subject was undertaken by Mr. Hough. He passed on under the guidance of Sir George Darwin to an investigation of the tides on dynamical principles, and succeeded in deriving a more complete solution of the tidal problem than had been previously obtained, and indeed in making the most important contribution to this theory since Laplace. In his work he introduced the mutual gravitation of the water, and he determined the periods of free oscillation of the ocean. At this time he also did some work on periodic orbits.

When Mr. Finlay, chief assistant at the Cape Observatory, retired in 1898, Sir David Gill, who was then H.M. Astronomer, pointed out to the Admiralty the importance of selecting as his successor a man with the highest scientific qualifications who might be expected ultimately to become director of the observatory. In accordance with this plan Mr. Hough was selected for the post and he proceeded at once to take up his duties. He became H.M. Astronomer in 1907.

Mr. Hough threw himself into the work of the observatory and made valuable contributions to astronomy in organising and discussing observations, particularly those relating to the exact positions of the stars. This work is of a kind which does not attract much public notice, but it is absolutely fundamental to astronomy. Soon after his arrival at the Cape he was entrusted with the reduction of a triangulation of close circumpolar stars made with the heliometer. These observations were carefully discussed for systematic errors and combined with meridian and photographic observations so as to give accurate positions of all the brighter stars in this region. The subject of the accurate positions of southern circumpolar stars engaged Mr. Hough's attention to the end, and four parts of Vol. XI. of the Cape Annals deal with these stars.

Mr. Hough's chief work was done with the new Cape Reversible Transit-Circle. This instrument is probably the best of its kind in existence and was designed by Gill with a view of the elimination of all conceivable sources of error. The principal parts of the instrument arrived at the Cape in 1901, but a considerable time naturally elapsed before it was ready for use with its collimators and underground azimuth marks in position. In 1903 and 1904 Mr. Hough spent a large part of his time in the determination of the constants of the instrument, in particular the error of every one of the 5′ divisions of the fixed circle was determined. The new transit circle was brought into regular use in 1905. Under Mr. Hough's direction two catalogues of fundamental stars based on observations for the years 1905–11 and 1912–16, containing respectively 1293 and 1846 stars, have been published. Each star has been observed at least sixteen times, four times in each of the four positions of the instrument, and the resulting star places must be among the most accurate we have.

Under Mr. Hough's direction, rapid progress has been made in the completion of the Cape Astrographic Catalogue, Declination 40° to 52° South. Five volumes of measures have now been issued, and this year a magnificent volume giving the spherical co-ordinates of all stars down to and including the 9th magnitude of the C.P.D. scale. There are in all 20,843 stars in this catalogue, and the places have been deduced from all the material available both from the meridian observations and the photographic plates. The overlapping parts of the plates have been carefully compared and the plate constants adjusted so as to give the best agreement possible. This volume has entailed a large amount of work and must prove of the greatest value in the future.

It is impossible to enter here at length into the different phases of Mr. Hough's work. The meridian observations of the inner planets and the heliometer observations of the outer planets have been carefully collected and discussed. In conjunction with Mr. Halm he discussed the motions of the Bradley stars, and he has derived an accurate value of the solar parallax from the radial velocities of stars as observed at different seasons of the year. Besides giving observations of the greatest accuracy the Cape Publications contain valuable discussions for the derivation of the fundamental constants of astronomy.

Mr. Hough's contributions to astronomy were recognised in various ways. In 1902 he was elected F.R.S. He was president of the South African Philosophical Society in 1907, and on the reconstruction of that society as the Royal Society of South Africa he was elected its first president. Last year he was elected British vice-president of the International Astronomical Union. His death at the age of fifty-three is deeply felt by astronomers throughout the world.

J. J.

Sir Henry H. Howorth, K.C.I.E., F.R.S.

By the death of Sir Henry Hoyle Howorth on July 15, at the age of eighty-one, scientific circles lose a characteristic figure belonging to a generation which has almost passed away, while his many friends mourn the loss of one for whose qualities all had an intense respect and admiration. A man of strong individual character, he had foibles which he himself was not the last to regard with some humour. His most remarkable characteristic, however, was his wide intellectual range and the vast, and sometimes surprising, extent of his knowledge. A constant attendant at the meetings of many scientific societies, there were few subjects on which he was not prepared at a moment's notice to make a real contribution to discussion.

Born in Lisbon on July 1, 1842, Howorth was educated at Rossall School and called to the Bar by the Inner Temple in 1867. He soon, however, turned his attention to politics and historical and archæological studies, which became his main interests in life. Of the large number of scientific and historical works on a variety of topics which he published, the first were two papers dealing with the races of Northern Russia and the extinction of the mammoth respectively, which were presented to the British Association in 1868 and 1869. They were followed by a number of papers published in rapid succession in the journals of scientific societies such as the Royal Anthropological Institute, the Royal Historical Society, the Royal Asiatic Society, and the like. They dealt, among other subjects, with the ethnology and history of the peoples of Central Asia and Eastern and Central Europe and with geological topics connected with the polar areas, and may be regarded as preliminary studies for the works with which his name will mainly be associated in the future. Of these, one, his "History of the Mongols," of which the first volume, dealing with the Kalmucks and Eastern Mongols, was published in 1876, the second, dealing with the Tartars, in 1880, and the third, on the Mongols of Persia, in 1888, brought him recognition in the form of the K.C.I.E. in 1892 and election to the fellowship of the Royal Society in the following year. He also published a "History of Chengis Khan and his Ancestors" in the *Indian Antiquary*. He had begun to rewrite his "History of the Mongols"; but the revision was incomplete when he died.

For the ordinary individual these detailed studies of Asiatic history and ethnology might well have sufficed; but they were not adequate to satisfy the needs of an intellectual energy so indefatigable as that of Howorth. He took up the study of glacial problems with equal zeal, and, be it said, with his usual love of controversy. "The Mammoth and the Flood" appeared in 1887, and "The Glacial Nightmare" in 1893, both being parts of a vigorous attack on Lyell's glacial theory, based upon palæontological, geological, and archæological evidence and suggesting that the deposition of drift and boulders was due to wave, rather than glacial, action. He followed this up with "Ice or Water?" which appeared in 1905. At the time of his death he was engaged on the revision of "The Mammoth and the Flood."

Sir Henry was also keenly interested in the history of the Church, and was the author of a valuable and authoritative study of St. Gregory the Great, which was followed by "Augustine the Missionary." He also wrote "The Golden Days of the Early English Church," published in 1916, and edited a "History of the Vicars of Rochdale" for the Chetham Society.

It is surprising that, amid all this literary and scientific activity, Sir Henry should have been able to devote so much time to politics and public affairs, on which he was a frequent and voluminous writer in the correspondence columns of the Press. He was elected member of Parliament for South Salford in 1886, 1892, and 1895. In 1902 he did not seek re-election. Although he sat as a Unionist, he adopted an independent attitude, giving a free rein to powers of criticism and controversy which lost nothing by his command of language.

In addition to the honours already mentioned, Sir Henry Howorth was an honorary D.C.L. of Durham University, a trustee and honorary librarian of Chetham College, and, from 1899, a trustee of the British Museum. He had been president of the Royal Archæological Institute and the Viking Society, and was a vice-president of the Royal Asiatic and of the Royal Numismatic Societies.

DR. LOUIS BELL.

DR. LOUIS BELL died at his home at West Newton, Mass., on June 14. He was born in Chester, New Hampshire, in 1864, and twenty years afterwards graduated at Dartmouth College. He then specialised in physics and applied engineering, receiving the Ph.D. degree from Johns Hopkins University in 1888. In the same year he was elected professor of physics at Purdue University, Lafayette, Ind. He edited the *Electrical World* from 1890 to 1892, and was then appointed Chief Engineer of the power transmission department of the General Electric Company. In this capacity he installed at Redlands, California, the first three phase transmission plant which was used for general service. From 1895 to 1905 he lectured on power transmission to the Massachusetts Institute of Technology, while for twenty-seven years he was a consulting engineer in Boston.

Dr. Bell did excellent pioneering work on illuminating engineering and on power transmission. His "Electric Power Transmission," published in 1897, was for several years the standard textbook on the subject. For many years also his "Art of Illumination," published in 1902, was the standard work on illuminating engineering. He contributed articles on "Electrical Power Transmission" and on "Electric Motors" to the 10th and 11th editions of the "Encyclopædia Britannica," and published many technical articles chiefly on alternating currents, electric traction, illumination, physiological optics and radio-telephony. He was a manager of the American Institute of Electrical Engineers from 1891 to 1894 and was a past president of the American Illuminating Engineering Society. His work on photometry for the International Electrical Commission was much appreciated by engineers all over the world.

THE former Director-General of the German Continental Gas Co., Dr. W. v. Oechelhaeuser, died on May 31, at Dessau (Anhalt). He was born on January 5, 1850, at Frankfort-on-Main. He studied engineering science at the Technical High School in Berlin, made rather extensive journeys in foreign countries and entered in 1887 into the services of the German Continental Gas Co. at Dessau, of which firm he was Director-General during the years 1890–1912. His technical achievements, based upon sound scientific knowledge, have been acknowledged by the bestowal of the honorary degrees of Dr.Ing. and Dr.Phil. Dr. von Oechelhaeuser contributed largely to the development of the gas industry; for example, he substituted for the old type of horizontal gas retorts, with their great amount of hand work, the vertical retorts, in which the coal glides down by its own weight and at the same time is gasified. On the other hand, he constructed the first engine on the Oechelhaeuser system, by which it became possible to use the gas from a blast furnace directly for power production. In addition to this, he was successful in raising the social standing of the engineer in Germany, in his capacity of president, during many years, of the Society of Gas and Water Engineers and of the Society of German Engineers.

PROF. HERMANN SCHOLL, professor of technical physics of the University of Leipzig, died on June 27, aged fifty-one. His premature death will be much regretted. He was born on January 14, 1872, in Eupen, Rhenish Prussia, and studied at the Technical High School, Aix-la-Chapelle, and at the University of Giessen, where he became assistant to Prof. Otto Wiener, with whom he moved to Leipzig in the year 1899. In 1910 he was made professor of technical physics, and he organised the practical courses of this study at the university. His investigations were concerned mainly with the relation between light and electricity; for example, he was of opinion that electric action of the light plays an important part in the first-known photographic process, the daguerreotype process. Much important work was done by Scholl in his capacity as an expert of the Reichsgericht in patent cases. In numerous decisions concerning the validity of patents connected with electricity and mechanics, the senate of the supreme German court of justice followed Scholl's opinion. In consequence of his far-reaching scientific knowledge and thorough understanding of technical questions, Scholl exerted great influence upon the development of industry. Industrial circles, as well as his colleagues and pupils, will be much afflicted by the loss of this distinguished man.

WE regret to announce the following deaths:

Dr. E. Beckmann, on July 12, aged seventy. An appreciative note on his life and work appeared in our issue of July 21, p. 109, when the occasion of his seventieth birthday celebrated on July 4 was recorded.

Prof. L. Hiltner, president of the Bavarian Botanical Institute, on June 6.

Prof. E. W. D. Holway, of the University of Minnesota, known for his work on the rust-fungi, on March 31, aged seventy.

Prof. F. Krafft, professor of chemistry at Heidelberg, aged seventy-one.

Dr. Josef Nevinny, professor of pharmacology and pharmacognosy at the University of Innsbruck, aged seventy.

Prof. J. P. Langlois, of the Conservatoire national des Arts et Metiers, and editor since 1910 of the *Revue générale des Sciences*, on June 17.

Dr. J. G. Rutherford, chairman of the International Commission on Control of Bovine Tuberculosis and Canadian delegate at the International Institute of Agriculture at Rome in 1908, on July 24, aged sixty-five.

Current Topics and Events.

THE problems of physics are manifold, and tend to increase in number and in difficulty. Fifty years ago there was a general feeling that we had only to proceed steadily in the application of familiar dynamical principles to explain all the phenomena of inanimate nature. Some men of science would have included in such an explanation the facts of animate nature as well. How different is the position to-day! Sir Oliver Lodge, in the illuminating address which appears as a supplement to this issue, expounds the difficulties and perplexities which now face the natural philosopher, summing them up in the two words, "ether" and "electrons." The relativist may, for his own special purposes, ignore the ether, but Sir Oliver claims that as we find ourselves imbedded in ether and matter, it is necessary to take stock of our position and consider how much it is possible to ascertain as to etherial properties. The outstanding problems of our time, that of radiation on one hand and of atomic structure on the other, have been at least partially solved by the electro-magnetic theory of Clerk Maxwell and the electron theory which owes so much to his successors at the Cavendish Laboratory. But the still greater problem of relating these theories satisfactorily to one another and to the disquieting results embodied in the modern theories of quanta and relativity still awaits the revealing power of the master mind. The acceleration of an electron generates waves. In photo-electricity we find that radiation can fling out an electron with a surprising amount of energy. There is thus a remarkable reciprocal relation between light and electrons. With characteristic boldness Sir Oliver Lodge tackles the relations between radiation and matter and suggests—in the form of a question, it is true—that the actual generation of an electron by means of light is not an altogether impossible idea. The suggestion is perhaps not entirely new, but it has never been stated with such clearness and force, and deserves the serious consideration of scientific thinkers.

IT is a remarkable fact that, despite the immense advances in our knowledge of bacteria as the causative factors of infective disease, the viruses of the eminently contagious exanthematic diseases have not been unmasked. The causes of measles, scarlet fever, small-pox, chicken-pox, and typhus have not been found with certainty. Naturally, a great many researches have been carried out to discover these unknown causes, and in the earlier days of bacteriology many micro-organisms were incriminated which are now known to be accidental contaminations or are accessory to the main cause. The history of investigation on scarlet fever illustrates this admirably. Cocci of diverse kinds, bacilli, and even protozoa, have been alleged to cause the disease. The most recent report comes from Italy, where it is alleged that di Cristina of Palermo and Carolia of Rome have discovered the germ of scarlet fever in the form of an ovoid diplococcus. From what we know of bacteria in disease, it is improbable that the exanthemata are due to microbes of this class. The contagiosity, the eruption, and the high degree of immunity point to a special class of diseases differing altogether from the bacterial infective processes. Hektoen (1923) has recently published an interesting historical research detailing the various attempts which have been made to transfer scarlet fever intentionally to man, and he considers it very doubtful whether this has ever taken place. This is remarkable when one considers the ease with which the disease is transmitted under natural conditions.

THE Rothamsted Experimental Station is one of the Institutions to which the Empire Cotton Growing Corporation has made a grant of 1000*l*. for five years, for the development of research work likely to be of importance in relation to problems connected with cotton-growing. It is evidence of the enlightened outlook of the Corporation to research that the grant is free from any restrictions likely to hamper the progress of the work. The money will be employed in increasing the staff and equipment of the Soil Physics Department, in order that more rapid progress may be made in the study of the fundamental physical properties of soil. Special attention will be devoted to the water relationships, in view of their importance in districts where cotton is grown. The elucidation of these principles is necessary before trustworthy advice can be given to the growers, and, conversely, the practical problems that the local experts are expected to solve often present points that can only be answered after investigations in a research laboratory under controlled conditions. The function of the Soil Physics Department at Rothamsted will be to undertake these investigations as part of its study of the fundamental properties of soil. The Department will act as the headquarters of those men on study-leave who wish to discuss soil problems arising in the course of their work, and they will be provided with facilities for experimental investigations.

THE Polish Physical Society was founded in April 1920, with five branch sections in Warsaw, Cracow, Lwów, Wilno, and Poznań respectively. Prof. Ladislas Natanson, of the Jagellonian University of Cracow, was the first president of the Society for the period 1920–23, and in the general assembly held in Warsaw in April last Prof. St. Pieńkowski was elected president and Prof. Natanson vice-president. The first part of the Society's Transactions, referring to the period 1920–21, has been recently issued. It is an interesting volume containing a number of important contributions. There is an obituary notice of Prof. Tad. Godlewski; Prof. Natanson's presidential address; and a number of papers: on the diffusion and scattering of light, especially in water, by Prof. Cz. Bialobrzeski; on discharge in electrodeless tubes, by Prof. J. Wierusz-Kowalski; on the spectra of iodine vapour, by Mr. Landau-Ziemecki; on the magnetic anomalies in Poland, by Prof. St. Kalinowski; on the electrometric study of radioactive fluctuations, by Messrs. Wertenstein and

Muszkat; on the equilibrium of a radiating gaseous sphere, by Mr. W. Pogorzelski. The original text is in Polish; there is, however, a French translation or résumé of every item. The Society has about 120 members, and its address is 69 Hoza Street, Warsaw, Poland. By strenuous and careful work, the Society should do much to promote the progress of physical science in Poland.

On September 17-30, the American Association for the Advancement of Science will meet at Los Angeles with the Pacific and South-western Divisions, and a number of other societies are also gathering at the same place. The path of totality of the total eclipse of the sun on September 10 passes close by Los Angeles, so many distinguished astronomers who have journeyed to the neighbourhood for observing the eclipse are expected at the meeting. According to *Science*, Section D (Astronomy) is to hold joint meetings with the American Astronomical Society and the Astronomical Society of the Pacific at the University of Southern California, at the Mount Wilson Observatory and at the California Institute of Technology. A symposium on "Eclipses and Relativity," at which Dr. W. W. Campbell, president of the University of California, Dr. C. E. St. John, of Mount Wilson Observatory, and Dr. S. A. Mitchell, of the University of Virginia, are to deliver addresses, has been arranged for the opening day of the meeting.

"Æolus," on whose letters in the *Wimbledon Borough News* we commented in our issue of June 30, p. 889, has addressed to us a further letter in which he renews his protest against the by-pass road planned alongside Beverley Brook, and bespeaks our sympathy for the human users of Wimbledon Common no less than for the other animals. Unless the whole of the Fitzgeorge estate is bought for the public (a somewhat hopeless hypothesis), there will be roads of some kind, and we are not aware of any scheme better than that which was reached by representatives of the varied interests concerned. It has, we understand, been proposed that a belt of trees shall be planted to screen the road, a practicable measure which has our full support. No excessive stream of motor cars is anticipated, and indeed our own experience of Wimbledon Common is that small boys and the scatterers of paper are more destructive of its natural peace and beauty than is any of the high-road traffic.

It is curious how often scientific announcements made in British journals are overlooked by the general Press at home, but appear later as messages "From our own Correspondent" abroad. An example of this is a message from the New York correspondent of the *Times*, published in the issue of July 30, upon the discovery, by Prof. J. B. Collip, of an insulin-like plant hormone to which he gave the name "Glucokinin." The discovery was described by Prof. Collip in Nature of April 28, p. 571. It seemed scarcely worth while, therefore, to cable from New York that it "was announced here yesterday by Prof. J. J. Wiltman, of the University of Minnesota, through the American Chemical Society," especially as Prof. Collip's own letter of three months ago provided much fuller information.

Mr. Alec Ogilvie has been elected chairman of the Royal Aeronautical Society for the year 1923–1924, in succession to Prof. L. Bairstow.

The Secretary of State for the Colonies has appointed Lieut. J. R. Stenhouse to be master of the research ship *Discovery*, which, as announced in Nature of April 21, p. 540, is to proceed to the neighbourhood of South Georgia and the South Shetlands in order to obtain scientific evidence bearing on the whaling problem.

With reference to a note in Nature (July 7, p. 19) on the work in archæology of the late Prince of Monaco, Mr. F. Fawcett writes that while the excavation of the caves and the collection of the relics are due to the Prince, the building in which they are stored was constructed through the liberality of the late Sir Thomas Hanbury of La Mortola.

The Department of Scientific and Industrial Research requires a research engineer to take charge of the Building Research Board's Experimental Station, East Acton. Candidates should be honours graduates in civil engineering, or possess equivalent qualifications, and if possible have had experience in research in building materials and construction. Applications, with testimonials, etc., must be made in writing by, at latest, August 20, to the Secretary, Department of Scientific and Industrial Research, 16 Old Queen Street, S.W.1.

Applications are invited for the Yarrow scholarships in connexion with the Institution of Civil Engineers. The scholarships vary in value from 50*l.* to 100*l.* per annum and are open to British subjects who, desiring to become engineers, lack sufficient means to enable them to pursue their practical or scientific training. The regulations concerning the scholarships are obtainable from the Secretary of the Institution of Civil Engineers, Great George Street, Westminster, S.W.1. The latest date for the receipt of applications is September 30.

The following awards have been made by the Royal College of Physicians: The Baly gold medal, given every alternate year to the person who shall be deemed to have most distinguished himself in the science of physiology during the two years immediately preceding the award, to Mr. J. Barcroft; the Bisset-Hawkins medal, bestowed triennially on some duly qualified practitioner, who is a British subject, and who has, during the preceding ten years, done such work in advancing sanitary science or in promoting public health as, in the opinion of the College, deserves special recognition, to Dr. T. M. Legge. The Harveian Oration on St. Luke's Day will be delivered by Prof. E. H. Starling.

In connexion with the visit of members of the Society of Glass Technology to France, during the first week of July, two meetings with French glass manufacturers were held on Monday, July 2. In the morning the visiting party was received by the Chambre Syndicale des Maîtres Verreries, and was welcomed by its president, M. L. Houdaille, who described to the visitors how the French glass manufacturers are all united in one body—the Chambre Syndicale—which is divided into six sections representing various branches of the industry. The work of these sections is organised in such a way as to prevent ruinous competition between members, and at the same time encourages individual research and development. In the afternoon a joint meeting was held with the Société des Ingénieurs Civils, in the course of which the following papers were presented: "La Méthode Scientifique dans l'Industrie," by Prof. H. Le Chatelier; "Les Verres, Opaques et Colorés, et les Glacures Céramiques de Même Espèce," by Dr. A. Granger; "La Dilatation des Verres et Cristaux," by M. Lafon; "Specifications for Glass Products," by Prof. W. E. S. Turner; "Improvements in the Design of Recuperative Glass Pot Furnaces," by Mr. T. Teisen; and "The Physical Properties of Boric Oxide Glasses," by Mr. S. English and Prof. W. E. S. Turner. During the week, visits were paid to glass works at St. Denis, Rheims, Chantereine, St. Gobain, Chauny, and Cirey. A visit was also paid to the sand quarries at Nemours and to the forest and castle of Fontainebleau. Altogether, some thirty British members of the Society and their friends took part in the visit. Encouraged by the success of this and the visit in 1920 to America, it is proposed to arrange other tours as opportunity arises.

A MEMORANDUM regarding the probable amount of monsoon rainfall in 1923 was submitted, in the early part of June, to the Government of India by Mr. J. H. Field, officiating Director-General of Observatories. For the purpose of a forecast of the monsoon, India is divided into five sections, and the several conditions which are favourable for the various sections are given—the conditions ranging over a large part of the globe, and at different seasons of the year. It is noted that a marked feature of the weather in May was the comparative absence of temporary advances of the monsoon in the Arabian Sea, where the monsoon proper was behind time. Details are given of the influencing conditions in different parts of the globe, and from these it is concluded that there would be some delay in the establishment of normal monsoon conditions within the Indian area, but it was estimated that the delay was not likely to be prolonged. With regard to the total amount of monsoon rainfall, it seemed that in the Peninsula there should be a small excess, with a corresponding excess in Mysore and Malabar. For northern India and Burma no forecast could be issued. Recent telegraphic communications from Bombay received in the middle and towards the end of July state that the agricultural outlook is now satisfactory over almost the whole of the Bombay Presidency, where enough or more than enough rain has fallen nearly everywhere. According to usual custom a further monsoon forecast will be issued in August; past experience shows that the earlier forecast issued in June is usually on the whole the more successful.

A MURAL tablet to the memory of the great naturalists and lifelong friends—Frederick Du Cane Godman and Osbert Salvin—was unveiled at the Natural History Museum on July 28 by Lord Rothschild, and was accepted by the Archbishop of Canterbury on behalf of the Trustees of the British Museum. Upon the death in 1919 of Godman, who was for many years a Trustee and a generous benefactor to the Museum, a Committee was set up with the object of placing in the building a memorial to him and to Salvin, who had died in 1898, and it was decided to use the balance of the money collected as the nucleus of an exploration fund for the benefit of the Museum; to this Fund Dame Alice and the Misses Godman later added the sum of 5000*l*. The memorial was designed by Sir Thomas Brock, and after his death the task was completed by Mr. Arnold Wright. Godman and Salvin, both of whom were fellows of the Royal Society, will be remembered for the remarkable work entitled "Biologia Centrali-Americana," which was planned by them, and finally completed by Godman after Salvin's death. It consists of sixty-three volumes; the first forms the introduction, fifty-one deal with zoology, five with botany, and six with archæology. For the work the aid of many specialists was called in, but Godman and Salvin themselves undertook the chapters on birds and diurnal lepidoptera. The whole of their marvellous neo-tropical collection was presented to the Natural History Museum; many of the specimens they had themselves collected during their travels in Central America and Mexico. In addition, Godman's gifts to the Museum were many and valuable. The tablet hangs on the wall at the head of the main stairs in the Central Hall on the east side of the statue of Darwin.

THE Report for the year 1922 of the National Physical Laboratory extends to 227 pages and is provided with an index of 24 pages. Sufficient information is given to allow the reader to understand the methods in use at the Laboratory and to follow the advances made. The diagrams and illustrations add materially to the value of the report from this point of view. The number of tests made during the year is still on the down grade, as one would expect from the statistics of trade. The various research boards and government departments continue to depend on the Laboratory for the conduct of the investigations they initiate, but the Executive Committee has found it advisable to appoint a research committee consisting of Sirs J. J. Thomson, W. H. Bragg and E. Rutherford to assist in the organisation of research at the Laboratory. This committee has made valuable suggestions as to the future work and needs of the Laboratory. There have been few changes in the senior staff during the year, and those that have taken place are due to other government departments claiming men with special knowledge. An extension of the metallurgy building which had been needed for some time has been completed and is now in use, but the work in the physics building is still congested.

AN opportunity has been afforded us to examine and test the Hatchett planimeter and the pantograph designed and manufactured by W. H. Harling, 117 Moorgate, E.C. 2. The planimeter is very simple and compact in form: after a little practice it can be made to give results of considerable accuracy. The writer has tested it on variously shaped areas of different sizes, with good results. The pantograph is a more complicated instrument, designed on the principle of simple parallelograms, and can be set to sixteen different ratios. Geometrically, the instrument seems to be quite satisfactory. In use, however, one or two faults can be detected. First, there is not enough pressure on the pencil point to produce a useful drawing. Second, the pencil is not steady, there being insufficient constraint for keeping it perpendicular to the plane of the instrument. If these faults are remedied, the pantograph should prove very useful. Both instruments are offered at moderate prices.

THE Milroy Lectures on "Canned Foods in relation to Health," which were delivered a short time ago by Dr. W. G. Savage, are to be published in the autumn, in the Cambridge Public Health Series, by the Cambridge University Press.

MESSRS. J. AND A. CHURCHILL are about to publish "Clouds and Smokes: the properties of disperse systems in Gases," by Dr. W. E. Gibbs, in which will be described the various ways in which disperse systems in gases can be formed; their mechanical, thermal, optical, and electrical properties, and the conditions which determine their stability are discussed. This information is then applied to the practical problems of meteorology, fume condensation, gas filtration, the manufacture of substances in a finely powdered condition, and the use of smoke in warfare.

Our Astronomical Column.

THE GREAT PERSEID METEOR SHOWER.—Mr. W. F. Denning writes: "The maximum of the Perseids occurred in 1921 on the morning of August 12, and the earth may be expected to be in the same position of its orbit in the early afternoon of August 12 next. There may therefore be no very rich shower, though one may possibly recur in Japan and in the East.

Our acquaintance with the distribution of the Perseids is, however, by no means perfect, and it is fairly certain that a fairly good display will be observed in England this year if the weather allows suitable observations to be made. The moon being new at the time of the maximum will be a favouring feature.

Observations should be made on the early morning of August 12 and during the night following that date. The hourly number of meteors should be counted, and the apparent paths among the stars of the larger ones should be accurately recorded so that their real paths may be computed.

It will be interesting to note if there are any strong displays of the minor radiants of this period, such as the Cepheids, Cygnids, Lyrids, or Arietids."

OBSERVATIONS OF JUPITER.—*L'Astronomie* for July contains three drawings of Jupiter on April 24 and May 1 by M. Pierre Feurtey, using an equatorial of 0·19 metre aperture screened down to 0·14 metre. The Red Spot, the tint of which is described as *saumon gris*, seems to have revived since last year, as it is shown quite strongly marked in the drawing of May 1, with two lighter patches in its interior, and the surrounding bays, both north and south, very dark. A row of six round white markings follows the spot, slightly to the north of it.

Several narrow dark bands cross the light equatorial zone obliquely.

PARALLAXES OF FIFTY-SEVEN STARS.—The Memoirs of the National Academy of Sciences (Washington), vol. xix., contains the results of the determination of the parallaxes of fifty-seven stars made by Mildred Booth and Frank Schlesinger. In twenty-two of these no previous determinations of parallax have been made. The authors point out that, without the co-operation of several institutions, a long interval would have elapsed before these results could have been made available. Thus the photographic plates were secured with the Thaw photographic refractor of the Allegheny Observatory: the measures and reductions were completed at the Yale Observatory under a grant from the National Research Council (Division of Physical Sciences) to the committee on Stellar Parallaxes of the American Astronomical Society. The measuring machine was purchased several years ago with a grant from the Draper Fund of the National Academy of Sciences. The measurements and computations were carried out by Miss Booth, the methods employed having been previously published. The average number of comparison stars used was 3·8, and the average number of plates for each region was 15. It is interesting to note that a great many of the stars in the list are double; thus nearly every object is one of more than average difficulty, the stars having in fact been selected on that account from a long list awaiting attention at Allegheny. A summary of the parallaxes is given in a table showing the visual magnitude and spectrum type, total proper motion, relative parallax and probable error, and finally, the probable error for one good plate.

SPECTROSCOPIC BINARIES.—The Dominion Astrophysical Observatory, Victoria, is determining orbits for a number of these binaries. The chief interest in this research is the material for determining the average mass of stars of each spectral type. The masses of individual stars are indeterminable owing to the uncertainty of the inclination, but in studying a large number of binaries we may assume that the inclinations follow the law of random distribution. The elements of the following three stars have lately been determined:

	Spectral Type.	Period.	Mass in terms of sun's mass.
BD 44° 3639	Oe 5	48·608 days	0·374
Boss 4870	B 2	1·0309 ,,	0·00019
ϕ Aquilae	A 2	3·3204 ,,	0·0192

The first is of special importance, since few masses of this type are known. One spectrum only is visible in each of these cases, so the companion is likely to have smaller mass. In the Oe 5 spectrum, the sharp H and K lines do not share in the oscillations and give a velocity of −11·3 km./sec., as compared with −5·8 km./sec. for the centre of gravity of the system.

Research Items.

TONGAN ASTRONOMY AND THE CALENDAR.—In one of the Occasional Papers of the Bernice Pallahi Bishop Museum (vol. viii. No. 4, 1922) Mr. E. E. V. Collocott has collected all that can be known of Tongan astronomy. They had the vaguest notions of the heavens and of the relations of the sky to stars, sun, and moon. They treated astronomy as a branch of navigation, but since the use of the mariner's compass, few if any living Tongans are able to point to or name more than a very small proportion of the stars. Practically all the available star lore of the Tongans is included in sailing directions written by the late Tukuaho, who was Premier of Tonga about thirty years ago, and this account, copied by permission of his son Tungi, consort of H.M. the Queen of Tonga, has been followed by Mr. Collocott in compiling this paper.

DISCOVERY OF AN EARLY PALÆOLITH IN NORFOLK.—In the *Antiquaries Journal* for April (vol. iii., No. 2), Mr. J. Reid Moir describes the discovery by Mr. J. E. Sainty of an Early Palæolith from the Glacial Till at Sidestrand, Norfolk. The specimen contains, in its interstices, material which, in appearance, is identical with the matrix of the Boulder Clay, and the colour of the flake-scars is precisely similar to the broken surfaces of many flints in the Till at Sidestrand. Mr. Reid Moir believes that the implement originally belonged to the Upper Freshwater Bed, the highest division of the Cromer Forest Bed Series. The Early Palæolithic implements of these strata are believed to afford the only evidence in East Anglia of the warm climate prevailing in Early Palæolithic times, and possibly representing the Günz-Mindel inter-glacial epoch. This gives definite evidence that the manufacture of early Chellean hand-axes was begun in what is now East Anglia, before the arrival of the glaciers which were responsible for the deposition of the Lower Glacial beds. Mr. Sainty has generously presented this Sidestrand implement to the Ipswich Museum.

CHROMOSOME MOVEMENTS IN NUCLEAR DIVISION.—Many attempts have been made to explain the forces at work in connexion with the mitotic figure in nuclear division. Attractions and repulsions are evidently involved, and these may result partly from the presence of electrical charges on the colloidal particles. The latest contribution to this subject is by Mr. Graham Cannon (*Journ. of Genetics*, vol. 13, No. 1), who applies Lamb's hypothesis that the centrosomes are pulsating or oscillating bodies, and also extends it to a consideration of the movements of the pronuclei in fertilisation. Mr. Cannon discusses many of the consequences which would follow from this assumption, but as yet there is little direct evidence that the centrosomes are actually centres of oscillation or pulsation. A very interesting feature included in the paper is the demonstration, by means of figures of the chromosome groups in the equatorial plate of various animals, that the chromosomes do in fact arrange themselves like floating magnets, the particular arrangement depending upon the number of bodies present. This result was predicted by R. S. Lillie in 1905.

EFFICIENCY IN COTTON WEAVING.—As the number of studies made in different industries and in different departments of the same industry increases it becomes more and more apparent that there are conditions militating against efficiency which are of an alterable character. A careful study of efficiency in cotton weaving, by Mr. S. Wyatt (Reports of the Industrial Fatigue Research Board, No. 23; London: H.M. Stationery Office, price 3s. net), demonstrates once more the value of natural, as against artificial, illumination; this factor has been shown in previous reports to hold true in silk weaving and in fine linen weaving. The relations between humidity, high temperatures, and efficiency are very well brought out; under existing conditions it is maintained that the best conditions for productive efficiency in weaving, in humid sheds of the type investigated, seem to be with a dry bulb temperature of 70°-75° F. and a relative humidity of 80-85 per cent. The writer also shows that, on the two-break day system, the efficiency during the pre-breakfast spell is consistently low. Various other aspects of the problem are considered, such as personal ability, individual differences, incentives and fatigue, and suggestions are given both for the amelioration of the conditions of the cotton weaver and for further study. The Fatigue Research Board in its cautious preface to the report is careful to point out the limits within which the conclusions must be interpreted. It is interesting, though, to note that in considering one report after the other there is a rapidly increasing weight of evidence in quite definite directions.

THE CONTROL OF THE PADDY STEM BORER IN INDIA.—In Memoirs of the Dept. of Agriculture in India, Entomological Series, vol. vii. No. 13, 1923, Mr. E. Ballard gives an account of experiments on the control of the paddy stem borer *Schœnobius incertellus* in the Godaveri delta. The insect occurs wherever paddy is grown in the East, and causes an annual loss of 10 per cent. of the entire Indian crop. Various schemes have been put forward for controlling the insect, the use of light traps being largely advocated. Mr. Ballard expresses doubts as to the efficacy of the latter method. Such traps are expensive and liable to be stolen, sometimes they only attract moths towards the light without destroying them, while many of the moths have laid their eggs before being trapped. The method of selecting attacked seedlings at the time of transplanting gives some hope of constituting an effective means of control. Close cutting, used in combination with seedling selection, should also destroy a large proportion of the larvæ from which the succeeding crop could be infected. The extent to which stubble helps the insect to tide over the time between the crops needs research. The natural insect parasites of the borer also require study, and the possibility of growing immune varieties of paddy should be tested.

CORRELATION OF OIL-SANDS: THE DAKOTA GROUP.—The correlation of oil-bearing sands on the basis of palæontological or petrographical work or both, is a phase of the geology of petroleum which has not always received the detailed attention in the United States that it undoubtedly merits. It is therefore gratifying to note that the importance of such work is now more generally realised, and Mr. W. T. Lee's contribution to Bulletin 751 on the "Continuity of some Oil-bearing Sands of Colorado and Wyoming" is a case in point. As Mr. Lee remarks, nearly every important sandstone in the great Cretaceous system of the western states contains oil, and the differentiation and correlation of these oil-horizons form problems the ultimate solution of which is demanded before this vast region can be economically developed. The rocks dealt with in this report belong principally to the Dakota Group, and apart from valuable local details regarding the different sands and shales included, the author establishes the important stratigraphical fact that there is no single, definite, persistent and easily recognisable sandstone corresponding to the formerly so-called Dakota Sandstone, but that a group of sediments to which the name Dakota is given represents successive accumulations of sediments near the strand-line of an advancing Cretaceous sea, presumably Upper Cretaceous, but not necessarily so. Hence the group differs in age from place to place according to the time taken by the advance of this strand-line across intervening distances.

WEATHER AT FALMOUTH IN 1922.—Falmouth Observatory has recently issued meteorological notes and tables for the year 1922, prepared by Mr. J. B. Phillips, superintendent of the Observatory. The mean barometric pressure for the year was 29·96 in., which is 0·02 in. below the normal. The mercury reached a maximum height of 30·79 in. during November, which is also the highest November reading on record since the commencement of observations in 1871. The high-pressure system associated with this reading prevailed from November 10 until December 15. In July the barometer fell to 28·98 in., which is the only record with the barometer below 29 in. in July. Temperature reached its highest reading, 75° F., on May 31, and the minimum, 28° F., on March 23. The summer was cool, the day temperature registering 70° F. or above on 6 days only, 4 in May and one in June and September respectively. Bright sunshine for the year registered 1663 hours, which is 90 hours less than the normal; the brightest month was May with 260 hours of sunshine, and the least sunny was December with 48 hours. Rain fell on 211 days, yielding a total of 45·75 in., which is 0·14 in. more than the normal. December, which is normally the wettest month of the year, had the highest monthly rainfall, amounting to 8·02 in., of which 7·64 in. fell in the last 16 days. During the year the equability of the climate of Falmouth withstood two severe tests. In an exceptionally cold period at the beginning of April, of 108 stations in the British Isles it was one of 9 at which the temperature did not fall below 34° F., and during a hot spell from May 21 to 24, when temperatures above 80° F. were general on the coast, the maximum did not rise above 68° F.

COMMERCIAL PRODUCTION OF OXYGEN. — The *Chemical Trade Journal* for June 15 contains an account of a long paper by Mr. T. Campbell Finlayson on "Industrial Oxygen," which was read before the Institution of Chemical Engineers. The aim of the work was to find a means of producing oxygen industrially at a price of 1s. per 1000 cu. ft. This was not realised, but a large number of possible methods were tried, some of which were found to be quite practicable. Chemical methods are impossible, as they are invariably too expensive; the most promising method is based on the differential solubility of oxygen and nitrogen under pressure in different liquids. Mr. Finlayson remarked that the discovery of a more suitable solvent might put the matter in a very different light. It will be recalled that this method was used, with water as a solvent, by Mallet half a century ago.

RECORDING WATER-LEVELS ELECTRICALLY.—A new form of electric transmission, for long-distance indication of variations in water-level and similar purposes, has been devised and put on the market under the designation of the Telechron Transmitter. The drawback in regard to systems of electric transmission in such cases is that dependence has to be placed on the unfailing action of the receiver to record the series of impulses sent out from the transmitter. With the ordinary electro-magnetic apparatus, owing to difficulty in exciting the magnetic field with sufficient promptitude, there is a possibility of failure to transmit signals which succeed one another rapidly. In other words, the receiver and transmitter are liable to "get out of step." In the Telechron instrument, signals are transmitted at a constant rate, independent of the speed of movement of the float or other actuating agent; the impulses are accumulated by the transmitter and are despatched in sequence at a rate within the capacity of the receiver to record them. It is thus possible for a float to make a rapid rise without the omission in the recorder of any one of the impulses in the series generated. Falls in level are equally accounted for, and when alternations take place rapidly the instrument records the net difference in either sense. It is possible to store up any number of impulses in the transmitter, though for practical purposes it is only necessary to provide for a hundred revolutions of the counter. Should the circuit by any chance be broken or the battery fail, the transmitter automatically sets itself and the receiver in step on the restoration of the current. There are other possible applications of the Telechron besides the long-distance record of water-levels; it is a trustworthy telegraph to indicate the position of a ship's helm, or of a lock-gate, or other moving object. It can also be adapted to the purpose of recording the pressure in gas mains. The apparatus is introduced by the Telechron Electric Transmitter Company, of 53 Victoria Street, S.W.

LUMINESCENCE.—Luminescence, as defined by Wiedemann, includes all cases of radiation except those due to temperature alone. In this sense the term is used in the valuable report published as a Bulletin of the National Research Council of the United States, entitled, "Selected Topics in the Field of Luminescence." The report, which is the work of Prof. E. Merritt, E. L. Nichols, and C. D. Child, covers a wide range, but most of the topics chosen for discussion are connected with fluorescence and phosphorescence. Prof. Merritt, who is responsible for the greater part of the volume, contributes an important chapter on theories of luminescence, dealing with the work of Lenard, Kowalski, Kennard, Baly and Perrin. A perusal of this chapter confirms the opinion that the most important problem at the present time in the field of luminescence is that of developing some satisfactory and comprehensive theory which may serve as an aid in correlating the observed phenomena and as a guide in planning new investigations. Such a theory must link together the varied subjects dealt with in later chapters—luminescence at high temperatures, luminescence and photo-activity, and, in particular, fluorescence and chemical change. Special attention may be directed to the work of Pringsheim, who, after a somewhat extended discussion of photochemical theories of fluorescence, is inclined to decide against such theories, putting forward certain suggestions in explanation of the chemical changes that so often occur in connexion with fluorescence. In the bibliography of luminescence, forming the last chapter of the report, Dr. J. A. Becker has made a serious attempt to include references to all books and articles on luminescence that have appeared between the years 1906 and 1922. References to papers on spectroscopic work have been included when, as in the case of flame spectra, they appear to have a direct bearing on the theory of luminescent radiation.

The Electron in Relation to Chemistry.

THE Faraday Society's conference on "The Electronic Theory of Valency," held at Cambridge on July 13 and 14, may be regarded as marking a new stage in the welding together of physics and chemistry, which has been so notable a feature of the recent history of these two sciences. The conference was attended by about 120 delegates from different universities, about half of whom were drawn from outside Cambridge. Some forty visitors were entertained in Trinity Hall, through the kindness of the master and fellows, to whom a deep debt of gratitude is due for contributing in this way to the pleasant social features of the conference. The foreign guests included Prof. G. N. Lewis, Prof. W. A. Noyes, Prof. Lyman, and Prof. Victor Henri of Zurich; the physicists included Sir J. J. Thomson, Sir Ernest Rutherford, Sir William Bragg, Prof. Barton, Prof. W. L. Bragg, Prof. Porter, Prof. Rankine, Dr. F. W. Aston, and Mr. R. H. Fowler; the chemists, in addition to Sir Robert Robertson, the president of the Faraday Society, included Sir William Pope, Prof. Heilbron, Prof. Lapworth, Prof. Lowry, Prof. Robinson, Prof. Smithells, Prof. Thorpe, Mr. C. R. Bury, Dr. Flürscheim, Dr. W. E. Garner, Dr. Henstock, Dr. Kenner, Mr. W. H. Mills, Mr. E. K. Rideal, and Dr. N. V. Sidgwick.

The conference was held in the new Department of Physical Chemistry, which is housed very appropriately in a block of buildings lying between the Chemical Laboratory in Downing Street and the Cavendish Laboratory in Free School Lane. These buildings, which were formerly in the occupation of the Department of Engineering, now provide ideal quarters for work in physical chemistry. They have been completely refitted and are admirably suited to their new use; they are also so commodious that there is a reserve of floor-space which has been loaned to workers from other departments, pending the time when it may be required for further extensions of physico-chemical work. Tea was served before the conference opened, in the large laboratory of the Hopkinson wing, which was erected in 1900 as a memorial to the late Prof. Bertram Hopkinson and his son, while the discussions were held in the lecture theatre adjoining.

The Friday afternoon session, dealing mainly with the application of the electronic theory to the problems of inorganic chemistry, was presided over by Sir J. J. Thomson, who in his opening address referred to the fact that, while the force which retains the electrons in an atom is proportional to the positive charge on the nucleus, the disruptive force which tends to make them scatter is proportional to the third power of their number, so that a limit is set to the number of electrons which can be crowded into one atom. The law of force is such that when the number of electrons is small, the octet is a particularly stable grouping; but with a different law of force, a sextet might be more stable than an octet. The problem of molecular structure can be attacked most readily by the study of cases of substitution; thus the electric moment introduced on replacing hydrogen by chlorine can be calculated, and measurements of the specific inductive capacity of molecules of different types have shown that this moment is constant in magnitude.

Prof. G. N. Lewis, in presenting his paper on "Valence and the Electrons," directed attention to the reconciliation which has recently taken place between the views of physicists and chemists in reference to the structure of the atom. Since physicists have now adopted a model in three dimensions, it is possible to regard the orbit of the electron as having a fixed orientation, although the electron itself is in rapid motion. The chemist's theory of static electrons has, therefore, been merged quite naturally into a scheme of static orbits. Prof. Lewis directed attention to the fact that, in Bohr's atomic structures, each of the rare gases from neon to niton, and all the stable elementary ions, possess eight electrons in the outer shell, thus affording full justification for what came to be known as "the octet theory." The fundamental phenomenon of chemistry is, however, the formation of *pairs* of electrons; and of some hundred thousand known substances only about half-a-dozen contain uneven numbers of electrons. This pairing may perhaps be due to magnetic forces, since unpaired electrons always give rise to a magnetic moment. When four of these electron-pairs are grouped at the corners of a regular tetrahedron the still more stable configuration of the octet is obtained.

Prof. Lewis attaches great importance to the view that the sharing of a pair of electrons constitutes a chemical bond between two atoms. When this bond is broken, the electron-pair usually remains attached to one atom, which acquires a negative charge, while the associated atom (which loses its share of the electron-pair) acquires a positive charge on disruption. This contrast is described in Langmuir's nomenclature as the conversion of a "covalence" into an "electrovalence"; and most English readers have accepted this nomenclature as an essential feature of the "Lewis-Langmuir hypothesis." Prof. Lewis, however, regards the ionised bond as being no longer a bond at all, and even objects to the use of the term "valence" to express the electrical state of the atom, although for nearly seventy years bismuth and aluminium have been described, like phosphorus and nitrogen, as tervalent elements.

The two following papers, by Mr. R. H. Fowler on "Bohr's Atom in Reference to the Problem of Covalency" and by Dr. N. V. Sidgwick on "The Nature of the Non-polar Link," were of interest as exhibiting two parallel lines of thought in the application of Bohr's theory of the structure of atoms to the unsolved problem of the electronic structure of molecules. The close agreement between the conclusions reached on this subject at Oxford and at Cambridge is noteworthy. As might perhaps have been anticipated, the Cambridge physicist was much more apologetic than the Oxford chemist, since he evidently realised more fully the risks that must be taken when forsaking the mathematical concepts, verified by observations of spectra, on which the structure of the atom is based, for purely qualitative conceptions of molecular structure, which are at present beyond the range of mathematical analyses and of experimental verification. The chemist, on the other hand, boldly translating the shared electrons of Lewis into shared orbits (compare Dr. N. P. Campbell's letter in NATURE of April 28, p. 569), was ready at once to gather a harvest of new conceptions from this speculative extension of Bohr's theory. A study of the printed papers shows, however, no important discrepancies between the views of the two authors as to the results of extending the theory of orbits from atomic to molecular structure.

In the discussion following upon the reading of these papers, Sir J. J. Thomson pointed out that two electrons are not necessary to make a bond, since H_2^+ is one of the most persistent aggregates met with in positive rays, although there is only one electron left to hold the two protons in combination. To this Mr. Fowler replied that although this may be stable for an indefinite period in a vacuum, aggregates of this type appear to be quite incapable of resisting chemical combination—perhaps because an odd electron passing from one nucleus to the other would impart an electrical charge alternately to one atom and the other, giving rise to an unstable condition which would be particularly ready to undergo chemical change.

Prof. W. A. Noyes, after reviewing briefly the history of the development of the theory of ionisation, laid stress on the fact that the distinction between polar and non-polar union is one of degree and not of kind, the fundamental factor in both types of union being the pairing of electrons. He also directed attention to the fact that the elements, such as lithium, sodium, potassium, rubidium, and cæsium, which are monoatomic in the gaseous condition, are exactly those which have a single valency-electron in the outer shell.

Sir William Bragg made an important statement in reference to the lengths of the carbon chains in the fatty acids and esters. Each additional carbon atom in the alcohol radical increases the length of the carbon chain by 1·22 Å.U.; but for the acid radical the average increment is only 0·97 Å.U. This remarkable result can be explained, while maintaining a fixed distance of 1·5 Å.U. between the carbon atoms, by assuming, on the alcohol side of the molecule, a simple zigzagging of the chain of atoms, with a fixed tetrahedral angle of 109° 28′, the branching of the chain being to the left and right alternately. The increment on the acid side of the chain can be explained by assuming the formation of a zigzag chain of another type, the deflexions being in the order LLRRLL, etc., instead of LRLRLR. These two forms of zigzag appear to be initiated by the two types of oxygen-linking in the –CO.O– group, and then to be rigidly maintained in the two chains. The structure suggested on the acid side of the molecule may explain the alternation of physical properties observed in the well-known odd and even series of acids, since the increment of length is alternately parallel to the chain and inclined at an angle of 109° 28′ to it.

At the close of the session Prof. Victor Henri made a brief communication in anticipation of the important paper which he delivered on the following day. The discussion took place under tropical conditions, which were so extreme that the session was adjourned before the discussion had become completely informal. An informal discussion was, however, carried on in the cooler atmosphere of the evening in the fellows' garden of Trinity Hall, to which some sixty delegates adjourned after dining together in the Hall of the College. The bringing into direct personal contact of workers who had previously known one another only by correspondence, or by reading one another's published communications, was a most valuable feature of the conference, and full advantage was taken of the opportunities thus presented.

The discussion, on Saturday morning, of the applications of the electronic theory of valency to organic chemistry, was presided over by Sir Robert Robertson. In opening the discussion, Prof. Lowry urged that the electron has come to stay, and that sooner or later organic chemists must take into consideration the electronic structure of atoms and molecules. These may prove to be a mere translation into a new language of the structural formulæ of Kekulé and van 't Hoff; giving rise to a new nomenclature but to no new conceptions. This is, however, unlikely in view of the enormous advances that have followed from the discovery of Dalton's atom, and of each fresh detail of its structure. The electronic theory of valence has already made a contribution of real value by discriminating between two types of valency, since a single bond can now be classified as depending either upon electron-sharing or upon electron-transference. Prof. Lowry's own contribution had consisted in the suggestion that a double bond may assume a form in which one linkage of each type is present. This has led to a number of novel conclusions which have been set out in a paper published in the April number of the Journal of the Chemical Society and in a paper on "Intramolecular Ionisation in Organic Compounds" contributed to the present discussion.

In summarising a second paper on "The Transmission of Chemical Affinity by Single Bonds," Prof. Lowry raised the question as to how many types of valency the chemist would wish the physicist to provide, and how many different mechanisms must be invented to account for the transmission of chemical affinities through chains of atoms. Prof. Lowry believes that only two types of valency are necessary, and that principal and subsidiary valencies, partial valencies, conjugated double bonds, carbonium bonds, mobile hydrogen atoms, centric bonds and para-linkages in aromatic compounds, are all manifestations of those electrostatic forces which Langmuir describes as electrovalence. In the same way, the unidirectional "general" effect, and the "alternating" effects observed in conjugated chains, appear to account for nearly all the phenomena observed in the transmission of chemical affinity. Lapworth and Flürscheim have suggested cases in which alternating effects appear to be produced in chains of single atoms; but other explanations (such as the steric effects described by Sir William Bragg) appear to be capable of accounting for most of these observations, and further evidence is needed before a third mechanism of transmission need be admitted. The evidence now brought forward by Lapworth and Robinson may perhaps provide the unexplained residue of observation which would make such a mechanism necessary. Prof. Lowry's paper also contained a vindication of Vorländer's view that, when its direct neutralising action is eliminated, the amino-group possesses well-defined acylous properties; it is therefore no anomaly for an amino-acid to be stronger than the fatty acid from which it is derived.

Prof. Lapworth, in communicating a paper on "Some Recent Contributions to the Theory of Induced Alternate Polarities in a Chain of Atoms," described six different theories which have been put forward in order to account for these phenomena. In criticism of the previous speaker, he stated that he himself twenty years previously emphasised the tendency of organic compounds to assume a "homogeneous" in place of a "heterogeneous" distribution of valency. This is precisely the same phenomenon that Prof. Lowry discussed under the heading of "Crossed Polarities." As evidence of alternate polarities in chains of atoms held together by single bonds he quoted the biochemical oxidation of butyric acid to β-hydroxybutyric acid and then to aceto-acetic acid. This case has also been quoted independently by Robinson.

Prof. Robinson contributed a paper on "Octet Stability in Relation to Orientation and Reactivity in Carbon Compounds." He directed attention, as Prof. Lapworth had done, to the fact that optical activity is often preserved in chemical changes, although, if the intermediate stages are those which are conventionally postulated, the optical activity must necessarily disappear, since the intermediate product would be symmetrical. The preservation of asymmetry can, however, be explained by means of partial valencies, through which the asymmetry of one atom is maintained until it has been stabilised again either in the same atom or in a different one.

The bearing of the theory of polarity on the reactivity of organic compounds was discussed by Mr. E. K. Rideal, and, as illustrating this aspect of the problem, Mr. R. G. W. Norrish described some experiments which he has just carried out, according to which the union of ethylene and bromine is almost stopped by enclosing the mixed gases in a vessel lined with paraffin wax, whereas in contact with the polar surface of a glass vessel combination takes place rapidly.

In the general discussion, to which about a dozen different speakers contributed, Prof. J. F. Thorpe urged that the theory of polarity "explains everything but predicts nothing," in marked contrast to van 't Hoff's stereochemistry, which made organic chemistry into the most exact of all the sciences. This contention was strenuously denied by Mr. Burkhardt from the Manchester laboratory and by Profs. Noyes, Lapworth, Heilbron, and Robinson, who proceeded to put on record two definite predictions in order to get over the difficulty that under normal conditions the prediction and the verification are published together, so that the reader cannot be quite certain which really came first. Dr. Flürscheim also replied in a very vigorous manner to the criticisms by Prof. Lowry of his views in reference to the influence of substitution on the strength of carboxylic acids, and urged that in several examples the formation of internal salts, which had been suggested as an alternative explanation by Prof. Lowry, cannot in fact take place. Prof. Lewis directed attention to the fact that the breaking of a double bond does not necessarily get rid of *cis* and *trans* isomerism, even if free rotation can take place; and in support of this view Mr. Bury quoted the fact that quadrivalent sulphur compounds retain their optical activity even when one of the four groups is ionised.

In the final session of the conference, Prof. Victor Henri presented a paper on "Molecular Polarity deduced from the Study of Absorption Spectra." This proved to be a most remarkable contribution, in which the application of considerations based on the quantum theory led to the conclusion that quantified motion may occur in electrons, atoms, or molecules, giving rise to broad absorption-bands, narrow absorption-bands, and a fine structure of these bands, respectively. By making use of a source of continuous ultra-violet light, Prof. Henri has been able to study the fine structure of the absorption bands of a large number of compounds, and in some instances to measure as many as two thousand bands in the fine structure. A large number of photographs were shown to illustrate the various types of absorption spectra. Prof. Henri's paper produced a very profound impression by the masterly character both of the theory and of the experiments which he described. It may well mark a new era in the history of absorption spectra. In apologising to Prof. Henri for the fact that the late hour did not allow of an opportunity for discussion, the president added that the question of holding a general discussion on absorption spectra is already under consideration by the council of the Faraday Society, and that such a discussion would allow of a fuller consideration of the view which Prof. Henri had put forward.

International Conference of Phytopathology and Economic Entomology.

THE first International Conference of Phytopathology and Economic Entomology was held in Holland on June 24-July 2 by the kind invitation of the Netherlands Government in co-operation with the authorities of the various laboratories and institutes conducting work on agricultural phytopathology and entomology. The Conference was noteworthy as being the first occasion on which phytopathologists and entomologists from all countries have been invited to meet together to discuss matters of mutual interest. Prof. H. M. Quanjer of Wageningen, presided over the Conference, while Dr. L. O. Howard, Chief of the United States Bureau of Entomology, acted as president of honour. Upwards of 65 members attended—in addition to the Dutch participants—representing some 26 countries. A report of the Conference, giving an account of the demonstrations, papers read, etc., will be issued by the Committee of Management (obtainable from the Secretary, Mr. T. A. C. Schoevers, of the Netherlands Phytopathological Service, Wageningen), but in the meantime the following brief account may be of interest.

Members assembled at Wageningen on June 24, when an address of welcome was delivered by Prof. Kielstra, Rector Magnificus of the University. On the following morning the Conference was formally opened by H.E. the Minister for Home Affairs and Agriculture, and during the day members attended the inauguration, by Jonkheer van Citters, of the new Laboratory for Potato Research in which Prof. Quanjer's Department is now housed. In the laboratory and adjoining experiment station experiments of great interest, notably in connexion with the "virus" diseases of the potato, were inspected. After spending two days in conference, the members divided into two parties, one visiting Groningen and the other Boskoop, Aalsmeer and Haarlem. The first party inspected the practical application of Prof. Quanjer's experiments in the selection fields and farm of Dr. O. Botjes, who demonstrated his methods of obtaining "seed" potatoes on a large scale entirely free from virus diseases, and the second visited nurseries producing ornamental plants, shrubs and bulbs; at Lisse (near Haarlem) they visited Dr. van Slogteren's new laboratory for the study of bulb diseases, and were treated to an excellent address and to demonstrations. The two parties combined forces again at the laboratory of Prof. Westerdijk, Director of the W. C. Scholten Phytopathological Laboratory at Baarn, where the final papers were read and the discussions concluded. Subsequently H.E. the Minister for Home Affairs and Agriculture held a farewell reception at the Hague, and members were entertained to dinner at Scheveningen. Many members remained until the following week to take part in an expedition to the glass house district of the Westland.

Within a short space it is impossible to do justice to the papers read. Two subjects were, however, specially prominent—namely, (1) the research, both botanical and entomological, which centres round the plant-diseases of the "virus" type, and (2) the efficiency or otherwise, of controlling the spread of insect and fungus pests from one country to another by means of a phytopathological service.

Discussions on the latter concerned chiefly plant import regulations and quarantines, the point of view of the exporting country being ably expounded by Mr. van Poeteren, who is director of the Netherlands Phytopathological Service. The controversial and difficult nature of this subject is well known, and it is satisfactory to record that the following resolution was approved:

"The representatives of all nations assembled at the International Phytopathological Conference at Wageningen, June 25-30, 1923, desire to place themselves on record as in full agreement with the essentials of international trade and commerce in living plants and plant-products, namely, reasonable freedom from all insect-pests and plant-diseases of all kind of materials imported into or exported from any country."

It should also be mentioned that so greatly impressed were members with the results of the Conference in bringing about international sympathy and co-operation as to the control of diseases and pests, that it was considered imperative that similar conferences under the same title should be held in future, and a small committee, under the chairmanship of Prof. Quanjer, with Mr. Schoevers as secretary, was appointed to undertake provisionally the duties of arranging for the next conference and of dealing with the various resolutions which had been passed.

This brief summary would be incomplete if some reference were not made to the hospitality and kindness experienced. Special mention must also be made of the admirable manner in which Prof. Quanjer carried out his duties as president, and the debt members owe him for rendering the discussions clear to all by rapid translation. As secretary, Mr. Schoevers was untiring, working literally night and day for the good of the Conference.

Sir William Thiselton-Dyer.

Tribute from British Botanists.

On July 28 Sir William Thiselton-Dyer attained his eightieth birthday and was the recipient of the subjoined letter from botanists throughout the country. Sir William's work as assistant director of the Royal Botanic Gardens, Kew, under Sir Joseph Hooker, and then as director for a memorable period of twenty years, is so well known that it is not necessary to refer to the many important things he did during his term of office. The present condition of the Gardens, and the prestige of Kew all over the world, are sufficient testimony to his ability and prescience. We beg to extend to Sir William in his retirement our congratulations and best wishes that he may long continue to enjoy his health and carry on his botanical activities.

Dear Sir William,

The occasion of your eightieth birthday affords us the opportunity of which we gladly avail ourselves, not only of offering you our congratulations upon having attained so venerable an age, but also of assuring you of our continued regard and esteem. In doing so we who sign this letter do but acknowledge our indebtedness to you for the inspiration and guidance which we, both as teachers and researchers, have derived directly or indirectly from your own early work as a professor of botany. We regard that work, and more especially the courses of practical instruction conducted by you at South Kensington in the years 1875 and 1876, as having inaugurated the renaissance of the study of the structure and functions of plants which had been so brilliantly carried on by British botanists in earlier times. It must, we feel sure, afford you great and justifiable satisfaction to contemplate the marvellous development of such studies in this country during the years that have passed since you quickened them into new life.

The professorial career on which you had embarked so brilliantly was unfortunately, as it may have seemed at the time, brought to a close by your appointment to the assistant directorship of Kew in 1875 and your subsequent appointment as director ten years later. The work that you were enabled to carry out at Kew has been of such national importance, that, however much we may regret the loss of the stimulating influence you would undoubtedly have exerted as a professor, we all realise the great and lasting services you have rendered to botany, not only from the purely scientific point of view, but also in relation to the development and encouragement of botanical enterprise throughout the British Empire.

Another notable result of the interest you inspired was the successful launching of the *Annals of Botany*, which has come to be one of the leading botanical periodicals of the world. We do not forget that it was your enthusiasm that turned the scale when the question of "to be or not to be" hung in the balance. The *Annals* is a lasting monument to your courage and prescience.

It would need a lengthy document were we to attempt to set out in detail the value of your many efforts for the promotion of our science, but in conclusion we feel we must refer to the noble work you did in saving the old Chelsea Physic Garden from destruction. Thanks to you, London has now a botanic garden where students and teachers can study the structure and functions of plants and pursue those studies which you did so much to promote.

With our very kind regards and good wishes,

Believe us to be, dear Sir William,

Yours very truly,

D. H. Scott, S. H. Vines, F. O. Bower, Balfour, H. T. Brown, D. Prain, F. Darwin, H. H. Dixon, A. C. Seward, J. B. Farmer, F. Keeble, A. B. Rendle, A. Shipley, H. Wager, F. F. Blackman, V. H. Blackman, F. W. Oliver, A. G. Tansley, F. E. Weiss, A. W. Hill

and all the leading botanists in Great Britain and Ireland.

University and Educational Intelligence.

EDINBURGH.—At a special graduation ceremonial, held in the University Library Hall on July 25, the following members of the eleventh International Physiological Congress, then meeting in Edinburgh, were presented to the vice-Chancellor (Sir Alfred Ewing), by Sir E. Sharpey Schafer, for the Honorary LL.D. :—Prof. F. Bottazzi, professor of physiology, University of Naples; Prof. W. Einthoven, professor of physiology, University of Leyden; Prof. W. H. Howell, professor of hygiene, Johns Hopkins University, Baltimore; Prof. J. E. Johansson, professor of physiology, University of Stockholm; Prof. A. Kossel, professor of physiology, University of Heidelberg; Prof. H. H. Meyer, professor of pharmacology, University of Vienna; Prof. I. P. Pawlow, professor of physiology, University of Petrograd; and Prof. C. Richet, professor of physiology in the Faculty of Medicine, Paris.

LONDON.—Dr. Lydia Henry has been appointed Warden of the Household and Social Science Department, King's College for Women, Campden Hill Road, W.8.

MANCHESTER.—The Empire Cotton Growing Corporation has recently offered to the University, for a period of five years, a grant to promote study and research in mycology and entomology, more particularly the diseases of plants caused by animal and fungal parasites known to be, or likely to be, of importance to cultivators of cotton. It is made a condition of the grant that the University should admit cotton research scholars and assistants on study leave to its laboratories, and it is also asked to deal so far as it can with inquiries from scientific advisers to cotton growers. The work will be carried out in the Departments of Botany and Zoology under Mr. S. Williams and Mr. R. A. Wardle respectively. In this connexion the large and valuable collections of insects in the Manchester Museum will be of considerable assistance in the identification of insect pests, while the experimental grounds and greenhouses which the University has recently established in Fallowfield will greatly facilitate the study of plant diseases.

SHEFFIELD.—The title of emeritus professor of mechanical engineering has been conferred on Dr. W. Ripper in recognition of the services he has rendered to the Department of Engineering and to the University.

Mr. Denton Guest has been appointed assistant bacteriologist.

DR. K. FASSLER of Freiburg (Switzerland) has been appointed, according to the *Chemiker Zeitung*, assistant and reader in mineralogy and geology at Laval University, Quebec.

THE Educational Directory, 1922–23, published by the Bureau of Education, Washington, as Bulletin 1922, No. 50, contains not only the names of administrative officials—federal, state, county, town, university, college, and library—but also lists of boards, societies, and other organisations having educational aims, and a list of educational periodicals in the United States. The list of summer schools in connexion with universities, colleges, and normal schools is an astonishingly long one, containing more than 500 entries: in most cases the summer session lasts for from six to ten weeks.

THE Clothworkers' Company of the City of London has offered an annual contribution of 300*l.* for the period of five years 1923–1927 to the Imperial College of Science and Technology, South Kensington, to be applied towards the maintenance and development of the City and Guilds (Engineering) College, one of the three constituent colleges of the Imperial College of Science. This donation is supplemental to the sum voted some years ago by the Goldsmiths' Company, a gift amounting to 85,000*l.*, which enabled the Engineering College to extend its premises, and is quite distinct from the annual vote of 5000*l.* from the City and Guilds of London Institute which has been paid to the Imperial College since the charter was granted some fifteen years ago and applied to the City and Guilds (Engineering) College. It is another indication of the value which practical men in the City of London attach to the research and general teaching in science specially in relation to industry.

IN 1917 the Government, acting through the Board of Education and the Department of Scientific and Industrial Research in conjunction with the London County Council and the Governors of the Imperial College, South Kensington, established at the Imperial College, for a period of five years in the first instance, a Department of Optical Engineering and Applied Optics, in the charge of Prof. F. J. Cheshire. As the Department was originally sanctioned for five years only, the question of its future has recently come up for consideration, and it has been decided that it shall be put upon the same basis as regards permanency as the other Departments of the College. The work of this Department should do much to prevent a recurrence of the position in Great Britain in 1914, when optical manufacturers were severely hampered by the insufficient number of optical experts available both for the scientific direction of production and also for the designing and computing of new optical systems demanded by the Government.

IN view of the jubilee celebration of the Cambridge University local lectures on July 6-9, special interest attaches to a review published in the May number of *School Life* (Washington, U.S.A.) of university extension work in America. The writer, who is president of the National University Extension Association formed in 1915, recalls that this work has been carried on in America since the inauguration of the Chautauqua gatherings in 1885, but it was not until 1906 that the University of Wisconsin, the pioneer State university in this field, organised its university extension division as an extramural college with a dean and separate faculty. Since 1913 the movement has spread so rapidly that now practically every institution of learning—university, college, normal, technical, or professional school, whether public or private, engages in some form of extension activity. The goal of the movement is thus described: "to fit every man and woman for his or her job, thereby making a better economic and social asset for the State." This insistence on the ideal of service to the State is characteristic of American writers on this subject and on elementary and secondary education. The National Association aims at standardising the character and content of courses, conditions of admission, etc. Among important recent developments of extension work in America are: co-operation with agencies such as state medical and dental societies and boards of health, extension courses for medical practitioners, and the utilisation of broadcasting stations. Nearly every state has now a correspondence school system supported by taxation, usually organised as a department of the state university.

Societies and Academies.

CAMBRIDGE.

Philosophical Society, July 16.—Mr. C. T. Heycock, president, in the chair.—W. M. H. **Greaves**: The possible mechanics of the hydrogen atom.—S. **Chapman**: The motion of a neutral ionised stream in the earth's magnetic field.—J. D. **Bernal**: Analytical theory of crystals.—H. F. **Baker**: Two geometrical notes: (1) Theory of confocal quadrics and Poncelet's porism of inscribed triangles. (2) A self reciprocal figure, and the associated cubic surfaces.—L. **Godeaux**: Sur la representation analytique des congruences de coniques.—C. T. **Preece**: Dougall's theorem on hypergeometric functions.—W. L. **Marr**: A quintic locus defined by five points in a plane.—J. **Brill**: On the problem of three bodies.—C. G. F. **James**: Extensions of a theorem of Segre's, with their natural position in space of seven dimensions.—T. M. **Cherry**: The form of the solution of the equations of dynamics.—R. A. **Fisher**: Note on Dr. Burnside's recent paper on errors of observation.—C. G. **Darwin** and R. H. **Fowler**: Further examples of partition functions.—H. W. **Richmond**: Real twisted cubics which are geodesics on quadric surfaces.

DUBLIN.

Royal Irish Academy, June 25.—Prof. Sydney Young, president, in the chair.—A. C. **O'Sullivan**: Corresponding points on the curve of intersection of two quadrics. Corresponding points on the curve of intersection of two quadrics u, v are defined as pairs of points the tangents at which to the curve are generators of the same species of the same quadric $\lambda u - v$. There are three kinds of correspondence, each related to one of the three ways in which the roots of the discriminant of $\lambda u - v$ may be grouped. If four points lie in a plane they, with their correspondents of the three kinds, lie in fours on 64 planes, 16 planes passing through each point. There exists a correspondence between the lines joining corresponding points and the points of the quartic curve, so that from any proposition relating to the points a proposition relating to the lines can be deduced. This transformation is expressed in elliptic functions by a quadric transformation which is equivalent to one of the forms of Landen's transformation, thus giving a geometrical interpretation in three dimensions of Landen's transformation for real arguments.

PARIS.

Academy of Sciences, July 9.—M. Albin Haller in the chair.—Gabriel **Bertrand** and Mlle. S. **Benoist**: The nature of "celloisobiose." The celloisoboise of Ost and Prosiegel and of Ost and Knoth appears from its properties to be a mixture of procellose, described by the authors in a previous communication, and cellose.—André **Blondel**: The conditions of yield of generating valve lamps having a characteristic of the singing electric arc: the definition of their power.—V. **Grignard** and R. **Escourrou**: The tertiary methylheptenols: their catalytic hydrogenation. The product of hydrogenation varies with the catalyst (platinum black, nickel), and also with the pressure of the hydrogen. The best results were obtained with nickel working under a pressure of about 15 mm. of mercury.—Serge **Bernstein**: The best approximation of functions possessing one essential singular point.—Nikola **Obrechkoff**: A problem of Laguerre. F. **Selety**: A distribution of masses with a mean density zero, without centre of gravity.—Th. De. **Donder**: The synthesis of the gravific.—André **Kling** and Arnold **Lassieur**: Aqueous solutions. A sketch of a theory explaining the behaviour of water towards indicators and the hydrogen cell independently of the ionic hypothesis.—Pierre **Bedos**: Ortho-phenyl-cyclo-hexanol and the bromhydrin of 1·2, cyclohexane diol. Ortho-phenyl-cyclo-hexanol is the main product of the reaction between the oxide of cyclohexane and phenylmagnesium bromide. It would appear to be a stereoisomer of the compound of the same composition obtained by Braun, Gruber and Kirschbaum by the addition of hydrogen to ortho-oxydiphenyl.—Pierre **Jolibois** and **Chassevent**: The setting of plaster. An account of experiments on the maximum solubility of anhydrous calcium sulphate as a function of the temperature to which it has been heated.—Victor **Lombard**: The permeability of nickel to hydrogen. If d expresses the volume of hydrogen passing through the nickel plate of area 1 sq. cm. then it was found that at constant difference of pressure on the two sides of the plate, $d = a^t$: at constant temperature, the yield of gas $d = \mathrm{K}\sqrt{\mathrm{P}}$, where P is the difference of pressure.—Roger G. **Boussu**: Contribution to the study of supersaturation. Details of experiments with supersaturated solutions of potassium bitartrate and calcium sulphate.—L. J. **Simon**: The oxidation of graphite by a mixture of silver bichromate and sulphuric acid.—H. **Gault** and G. **Ehrmann**: The soluble cellulose-ether salts of the higher fatty acids. Hydrocellulose is treated with an acid chloride and pyridine in the presence of a solvent. Descriptions of cellulose distearate, dipalmitate, and dilaurate are given.—Max and Michel **Polonovski**: Eserolmethene and its alcoholate.—F. **Delhaye**: Relations between the orogenic movements and the great depressions of Central Africa. The *graben* of Lufira (Katenga).—Mlle. J. Boisse **de Black**: The mode of formation of a *frane* in Cantal.—Allyre **Chassevant** and **Chouchak**: The measurement of the degree of ionisation of mineral waters.—H. **Ricôme**: Growth and heliotropism.—Jean **Politis**: The mitochondrial origin of the anthocyanic pigments in flowers and leaves.—M. **Piettre**: The humus in the coffee plantations in Brazil.—M. **Aron**: The influence of temperature on the action of the testicular hormone.—A. **Desgnez** and A. **Bierry**: The action of Vichy water on the urinary reaction.—Jean **Camus**, J. J. **Gournay**, and A. **Le Grand**: Experimental diabetes.—M. **Lévy-Solal** and A. **Tzanck**: Puerperal eclampsia and the phenomenon of shock. Arrest by pilocarpine.—M. **Bazin**: Animal and human neoplasms.

Official Publications Received.

Zoologische Mededeelingen, uitgegeven vanwege 's Rijks Museum van Natuurlijke Histoire te Leiden. Onder Redactie van Prof. Dr. E. D. Van Oort. Deel 7, Aflevering 3-4. Pp. 129-252. (Leiden: E. J. Brill.)

Department of the Interior: Bureau of Education. Bulletin, 1923, No. 6: Home Economics Education. By Henrietta W. Calvin. Pp. 19. 5 cents. Bulletin, 1923, No. 15: The Bible in the Public Schools; Legal Status and Current Practice. By William R. Hood. Pp. 13. 5 cents. Bulletin, 1923, No. 16: Statistical Survey of Education, 1919-20. By Florence DuBois. Pp. 41. 10 cents. Bulletin, 1923, No. 22: Educational Work of the Knights of Columbus. By Mark J. Sweany. Pp. iii+12. 5 cents. Bulletin, 1923, No. 25: Recent Developments in Educational Journalism. By Prof. W. Carson Ryan, Jr. Pp. 14. 5 cents. (Washington: Government Printing Office.)

The North of Scotland College of Agriculture. Guide to Experiments at Craibstone, 1923. Pp. 45. (Aberdeen.)

Department of the Interior: United States Geological Survey. Bulletin 732: Geology and Ore Deposits of Shoshone County, Idaho. By Joseph B. Umpleby and E. L. Jones, Jr. Pp. v+156+16 plates. 40 cents. Bulletin 740: Mica Deposits of the United States. By Douglas B. Sterrett. Pp. xi+342+29 plates. 50 cents. Bulletin 741: The Jarbridge Mining District, Nevada; with a Note on the Charleston District. By Frank C. Schrader. Pp. v+86+20 plates. 20 cents. Bulletin 743: Geology of the Oatman Gold District, Arizona; a Preliminary Report. By F. L. Ransome. Pp. iv+58+12 plates. 10 cents. (Washington: Government Printing Office.)

Department of the Interior: United States Geological Survey. Water-Supply Paper 488: The Floods in Central Texas in September 1921. By C. E. Ellsworth. Pp. iv+56+8 plates. 15 cents. Water-Supply Paper 493: Hydroelectric Power Systems of California and their Extensions into Oregon and Nevada. Prepared in co-operation with the United States Forest Service. By Frederick Hall Fowler. Pp. xlix+1276+73 plates. 2.25 dollars. (Washington: Government Printing Office.)

Supplement to NATURE

No. 2805 AUGUST 4, 1923

The Ether and Electrons.[1]

By Sir OLIVER LODGE, F.R.S.

PRELIMINARY.

I HAVE been asked to speak on possibilities of research in pure physics, and I rejoice that attention has always been paid to the vital importance of pure science in an Institution the backbone of the work of which must be of a metrical character connected with industrial applications, and much of it necessarily subject to routine.

The main lines on which physics has recently and is still so rapidly advancing are well known. There is no need to direct attention to such inquiries as are the direct outcome of radioactivity, in its various forms spontaneous and induced: problems which range from atomic investigations like those which go on in the Cavendish Laboratory, through X-ray and ordinary spectrum analysis, down to the various devices of wireless telegraphy. In all these things there are among the members of the staff of the National Physical Laboratory, and on the Governing Body, more than competent advisers.

I must just deal with such ideas as have been occupying my attention of late. I have found it interesting recently to look up some forgotten remarks of my own—made soon after a National Physical Laboratory was decided on, but before it was founded—in the preliminary portion of a presidential address to the Physical Society of London on February 10, 1899, as reported in the Proceedings of that Society, vol. xvi. Part VI., June 1899. Among other things there referred to, is a suggestion by FitzGerald that circularly polarised light sent through an absorbing medium might constitute it a magnet—a discovery not yet made. I see there also a reference to a Blue-book of 1898 recording a Government conference about the founding of this Laboratory. Sir Richard Glazebrook has also kindly directed your attention to my address to Section A of the British Association at Cardiff in 1891, in which the foundation of a National Physical Laboratory was specially advocated.

[1] From an address on "Some Possibilities for Research in Pure Physics, especially on the Ether," delivered to the staff of the National Physical Laboratory on March 14. The first section of the lecture is omitted for lack of space. It dealt with possible research in boundary or overlapping regions of specific sciences, and on the relation between physics on one hand, and chemistry, physiology, and biology on the other. This section, and one giving further details about the possibility of a renewed attempt to detect or to disprove an ether flow along lines of magnetic force, will be published elsewhere.

PROPERTIES OF THE ETHER.

The question of what constitutes a distinction between physics and chemistry is difficult to decide, but in general it may be said that chemists deal chiefly with static relations and groupings, while physicists are more inclined to treat phenomena kinetically. Another clear distinction, at any rate at present, between these two sciences is that one deals with matter only, and the other deals with the ether also. It seems that the electric charge is the unifying or connecting entity between matter and ether. Uncharged matter appears to have no effect on ether at all. But its charged particles, or electrons, in so far as they quiver or rotate, do disturb the ether and generate waves in it. Moreover, if they revolve or travel, as by locomotion, they generate magnetism in it; and even when they are stationary, they generate in it or rather are inevitably accompanied by what is called electric force. That is, they appear to attract or repel each other from a distance. Furthermore, for some reason—which I and some others think to be residual electric or possibly magnetic attraction,—they exert over minute ranges the force known as cohesion, which again must be exerted entirely through the ether, since particles of cohering matter are not in contact. And again they exert, even at the most enormous distances known to astronomy, the minute residual force known as gravitation; which in the case of bodies of astronomical size amounts to a force of gigantic magnitude.

Light, magnetism, cohesion, gravitation,—all these are affairs of the ether, and are all studied in physics. A superstition has recently arisen that the ether is an exploded heresy, and is unnecessary; but that is an absurd misunderstanding. The theory of relativity says nothing of the kind. As a mathematical method it need not mention the ether, any more than Laplace in his "System of the World" felt that he need mention the Creator. He was entirely within his rights in ignoring the Deity; and so is a relativist in ignoring the ether; at least when neither attempts to philosophise on that basis. For ignoring a thing is not the same as putting it out of existence. Extinction is as impossible to us as Creation. We have to take things as we find them: and we find ourselves imbedded in ether and matter. So we had better make the best of it.

PRESENT KNOWLEDGE ABOUT ETHER.

How much do we know about the ether, and how much is it possible to ascertain? We do not know as much as we ought, but we know a few things; and we hope by further investigation to know more.

Unfortunately, the ether is a very perfect, elusive, highly-endowed substance, which makes no direct appeal to any of our sense organs. Accordingly, it is only investigated with some difficulty; and its properties are so different from those of matter that very elaborate and expensive arrangements have to be made in order to cope with it. Even when we have made those arrangements, it may decline to give an answer, and the result may be negative. Still, a truly negative result is something definite, and is better than nothing. But every positive result is of extreme value. Let us summarise the results we already know.

First of all, we know that the ether can transmit waves at a definite and finite speed of 300,000 kilometres a second. Next, that those waves are electromagnetic, with the electric and magnetic vectors at right angles to each other, and in the same phase. We also know that the superposition of related electric and magnetic vectors in the same phase results in propagation with the speed of light.

Then we know that light cannot be transmitted by conductors of electricity, which act like a solution of optical continuity. Furthermore, inside a transparent body light travels more slowly than in free space, showing that the ether is affected somehow by the neighbourhood of matter; the amount of this affection being sometimes called the refractive index, which is the measure of the retardation experienced by light, and sometimes, from another point of view, the dielectric coëfficient.

We further know that if transparent matter is moved in the direction of the light inside it, a certain fraction of its velocity is added to the light—added of course algebraically. Thus demonstrating, not that matter has any power of conveying light—which it has not—but that some influence or reaction on the ether belongs to the matter and travels with it; that influence being just the one which effected the retardation and is responsible for the refractive index, the fraction of added velocity being, as surmised by Fresnel, $1 - 1/\mu^2$.

We also know, from certain experiments conducted by myself, that this property of matter does not extend in the slightest degree beyond its boundary; so that however fast matter is moving, light just outside it is not affected at all. Or, as we may express it, matter has no power of carrying the ether with it. The ether has nothing of the nature of viscosity. If a fluid at all, it is a perfect fluid. Not even if the matter is charged or magnetised does the influence extend beyond its boundary so as to affect the stream of light close to it. (See *Phil. Trans.* 1893 and 1897.)

Yet inside transparent matter the phenomena of light show that the ether must be modified in many ways, giving rise to all manner of crystalline effects—the optics of crystals—and the various phenomena of polarisation; especially that interesting one discovered by Faraday, that the plane of vibration is rotated, in one direction or another, by even non-crystalline and fluid matter when immersed in a longitudinal magnetic field: and those other phenomena discovered by Kerr, all of which may be summed up under the names elliptic and rotatory polarisation.

It is easy enough to say that light is retarded to a definite extent by transparent matter, but the complete theory of it is not so simple. Something about it will be found in the writings of Sir J. J. Thomson. All the phenomena of dispersion and anomalous dispersion must be taken into account if we would understand the inter-relation between matter and ether.

New Effects.

Not long ago the interesting phenomenon was discovered by Prof. Richardson that the act of magnetisation rotates a piece of iron, and a quantitative investigation of this delicate effect has been made recently by Prof. Chattock and Mr. Bates. I understand that a converse effect has now also been observed by Mr. Barnett, namely, that rotating a piece of iron magnetises it. I remember making an attempt to discover such a phenomenon long ago at University College, London; but I found nothing securely. Capricious and spurious effects were difficult to avoid, and I suppose I had not sufficient perseverance. We knew nothing in those days about electrons or their orbits, though we felt that there was something rotatory about magnetism; nor was it more than a suspicion that electricity itself might possess a trace of inertia, in addition to the recognised quasi-inertia of self-induction. Modern skill may have been able to overcome the difficulties inherent to such an experiment, but caution is desirable, since it is not clear why rotation should develop one polarity rather than another, if the atomic arrangement were truly random.

There is more to be got out of the original discovery by Richardson than has yet appeared—and I venture to predict that we have by no means heard the last of it. The Zeeman effect seemed small at one time; and if it be said that the Richardson effect could have been anticipated, I reply that Larmor anticipated the Zeeman effect; though it is true he did not expect the right magnitude, because the mass of the particle responsible for radiation was not then known. The quantitative relations of the Zeeman phenomenon clearly showed, for the first time, that the radiating particle was one of electronic and not of atomic mass.

I mention these two apparently disconnected phenomena together advisedly; for while the orientation or precession of electronic orbits in gases account for the Zeeman effect, the orientation of electronic orbits in iron accounts for the Richardson effect. Both are small, but the Zeeman effect is the smaller of the two; it needs the appliances of spectrum analysis for its detection. It is far bigger than it would have been if the atom had been the radiating element instead of the thousand-times smaller mass of the electron. As to the Richardson effect, it is surprising that it has been observed and measured at all, for the smallness (in mass) of the electron is no help to that; and the detected reaction is not something optical or etherial, but the gross movement of a mass of ordinary matter. Not much movement, truly,—quartz fibres must be used of course, and plenty of refinements,—but still a material movement is observed as the result of orientation of electronic orbits; and that is noteworthy. Reaction of radium from atomic projectiles was observed before, and reaction of radiometer vanes too; but alpha rays are atoms, and these effects are connected with atomic bombardment; so in that respect they differ from the effects just mentioned.

I would liken the Richardson effect in some respects more to the Lebedew and Nicholls and Hull detection of the pressure of light, as suggesting an etherial reaction on ordinary matter.

Referring to this light-pressure; it is so small that Crookes failed to detect it, just as Faraday failed to detect the Zeeman effect with the appliances of his day and without a Rowland grating; but the most trivial fact, so it be a fact, is of enormous and may be of cosmic importance. Poynting invoked light-pressure to account for cometary and other astronomical results; and now Eddington calls upon it to sustain the Atlas-like burden of holding up the billions of tons of superincumbent material which constitutes the crust or envelope of a giant star. An amazing application of the (terrestrially) almost infinitely small.

Parenthetically, in using the term so-and-so's "effect," I do so under protest. This personal kind of nomenclature should be temporary, and not outlive the generation of discovery. This kind of naming began with either the Doppler or the Peltier effect, and was right enough when novel effects were few; but now that they constitute a multitude, we older folk are apt to get confused among the plentiful crop which the more fortunate youngsters are continually evolving. Prof. Richardson is entirely free from blame, for he calls his discovery a gyro-magnetic effect; which is explicit and satisfactory.

The Positive Electron.

Before leaving this part of the subject I should like just to direct attention to what I have written in Nature for November 25, 1922, p. 696, that we have not yet securely discovered the positive electron. The proton has to serve that function for the present, but what the constitution of the simplest known nucleus of an atom is, remains to be determined. Something is known about the proximate or apparent constituents of some heavier atomic nuclei, though not much, but nothing at all of the constituents of the nucleus of a hydrogen atom. It *may* be an indivisible particle so small and concentrated as to have a mass 1800 times that of a negative electron; but to me it seems unlikely that this is the right solution. It *may*, on the other hand, be built up of a stable grouping of hypothetical electrons both positive and negative,—each one being like a mirror image of the other. If so, it remains to be explained why the outstanding charge of all atomic nuclei is apparently positive, and whether that is accurately true. All I advocate is to keep the door open for further investigation, and to persevere with the quest of the positive electron by any methods that may suggest themselves.

Why negative electricity should differ from positive so greatly, or in any respect save in sign, is not at all clear; and it is difficult to understand how one of these entities can have been constructed out of the ether, without the simultaneous production of its opposite partner.

Electrical Theory of Matter.

The mechanics of the ether are not yet known; and until we have devised some system of mechanics which applies, not in a blindfold, but in a clear and lucid, manner to the behaviour of the ether, we must remain to some extent in the dark. Here, then, is scope for experiment. At present we are using ether waves to examine the properties of matter, the structure of crystals, the structure of molecules, and even the structure of the atom. But we must go on in due time to use these phenomena for an investigation of the ether itself. We know that movement of matter does not affect the refractive index nor the polarising properties of that matter. But we know that if matter is moving fast enough, it tends to carry some ether with it, and thereby adds to its own inertia to a known and predicted extent. We also know that inertia itself is a magnetic and therefore etherial phenomenon. The way in which J. J. Thomson, Heaviside, and Larmor have worked out the electrical relations between ether and matter, as regards inertia, changes of inertia with speed, and radiation consequent on acceleration, has been a marvellous achievement of our time, of which quite inadequate popular notice has been taken. Still there it is. They have laid the foundation of the Electrical Theory of Matter, and have opened up a way for our descendants to explain nearly all the properties of matter in terms of the ether, and possibly the very existence of matter itself.

We do not yet know how an electron is composed. We know still less—if that is possible—how a proton is composed. But that they ultimately will turn out to be etherial structures of some kind is possible and, as I think, probable.

Meanwhile we know that not only the mass of bodies, but their shape, is affected by motion through the ether; this was demonstrated by that great experiment of Michelson's, which I regard primarily as an experiment on matter by means of light, and not an experiment on light by means of matter. It may hereafter be regarded by a sensible though preposterous historian—that is, one who puts the cart before the horse—as the first and only verification of the FitzGerald-Lorentz theory of modified electrical cohesion, or peculiar interaction between moving particles. It has been used as the foundation of the Theory of Relativity. But that is an ingenious offshoot or excrescence. I should like everybody to realise that the Electrical Theory of Matter had already accounted for nearly all the things which drop out so naturally from the theory of relativity: such as the increase in mass, the FitzGerald contraction as a reality, the Fizeau effect on light; even an extra revolution of the axes of a planetary orbit, unless gravitation itself is modified by motion. (See several Articles in the *Phil. Mag.* between August 1917 and June 1918, by Prof. Eddington, G. W. Walker, and myself; beginning with page 81 of vol. 34, and with conclusions summarised on pp. 143, 482, and 486 of vol. 35.) The Electrical Theory of Matter may conceivably be made to account for the two other as yet incompletely verified gravitational effects so brilliantly predicted by Einstein. But that remains to be seen.

Possible Experiments.

Limits of space will not permit me to deal here with the possibility of an experiment to determine whether there really is etherial circulation along magnetic lines of force; attention may be directed, however, to papers describing my early attempts at such experiments, as partly described in the *Philosophical Magazine* for April 1907 and May 1919. In making experiments on the ether we must recognise that what we set out to look for we may not find: but we can also remember that careful and conscientious experiment, conducted with good apparatus, must lead us somewhere, and may result in a discovery exceeding in importance and interest any property we had set out to examine.

Kinetic Elasticity.

The contrast between the kinetic and the static mode of regarding things runs all through physics. Most physicists are imbued with the more fundamental character of a kinetic explanation, and never feel really satisfied with an explanation in terms of static or potential energy. Of the two kinds of energy, kinetic appears to them the more fundamental kind.

So, as we all know, Lord Kelvin tried to explain the elasticity of a spring balance, or any spiral spring, by means of gyrostats, or spinning tops. He was able to devise, at any rate theoretically, two concealed mechanisms, one of which was static and the other kinetic; that is to say, one of which contained a spiral spring with a protruding hook at the end, while the other contained a precessing system of gyrostats, also with a protruding hook. The observer was challenged to hang things on the hook, or to feel its recoil, and to say which was which. Or, in more general terms, Lord Kelvin endeavoured to devise a kinetic theory of elasticity. His famous theory of the vortex atom, in which he tried to explain some of the properties of atoms in terms of vortex rings and their collisions and interactions, was of this nature. It was extended by J. J. Thomson at an early date into almost chemical regions, in his early Adams' prize essay.

In his later life, Lord Kelvin was inclined to abandon this view of elasticity as regards solids; and his vortex atom declined to satisfy either him or others, on further development. But none of these ideas should be completely abandoned. In so far as they successfully illustrated any of the properties of matter, they are worthy of consideration. Although we now know that the atom is not a vortex ring, or anything like it, I would challenge any one to say the same of an electron. The electron has become the fundamental material unit: and what its constitution may be, we none of us know. It must be in close relation with the ether, and must ultimately, as I think, be explicable in terms of the ether. But the fundamental properties of the ether are too little known at present to enable this to be done. We cannot say whether the electron is to be explained statically as a knot or other geometrical configuration or strain centre, on one hand, or as some kind of circulating or vortex movement on the other. The constitution of the electron remains for discovery, in spite of all the work of Larmor on the subject in his brilliant book "Ether and Matter," and other papers imbedded in the *Phil. Trans.* If it should turn out that an electron can be thought of as a knot or any kind of static strain, then I for one feel that that cannot be regarded as an ultimate explanation, though a most useful approximate one, and that the strain will have to be resolved into or accounted for by some kind of etherial vortex motion.

Not only have the electric and magnetic forces belonging to an electron, respectively at rest and in motion, to be explained; but also the slight residual strain depending on the square of the charge, and therefore irrespective of sign, which we call gravitation, has in a complete theory to be explained also. For few can doubt, I think, that gravitation must now be regarded as a function of the electron and the proton, that it is not something which springs into being when these units are associated so as to constitute an atom of matter; it is unlikely that the tight packing of a large number of hypothetical positive and negative units in the proton could account for it. More likely gravitation will turn out to be an etherial phenomenon explicable in terms of the beknottedness which distinguishes the singular point of an electron from the rest of the ether. The rest of the ether is not affected by gravity, but possesses qualities akin to what in mechanics we call elasticity and inertia. Otherwise the unspecialised ether of space could not transmit radiation, or sustain a magnetic field—as we know it does. For it is necessary always to remember that though electric lines of force terminate on material units, most of their course lies in undisturbed ether: while magnetic lines of force do not terminate at all, but are always closed curves, surrounding electrons in motion, but themselves existing, I presume, wholly in the ether, and showing every sign of being essentially a kinetic phenomenon, demonstrative of inertia.

Inertia itself I see no way of explaining in any fundamental manner. It seems to be a property that we must postulate as existing in the ether,—a property akin to density; though it is true we can explain the inertia of matter—that is, of any material unit—in terms of the concealed magnetic fields inevitably associated with its motion.

To leave these more transcendental regions for the present, we may recall that although the kinetic theory of elasticity has hitherto failed to develop in connexion with solids, it holds perfectly for the case of gases. The elasticity or recoil of compressed air used to be thought of as analogous to the recoil of an elastic spring. But Waterston first, and then Joule and others—including especially Maxwell and Clausius and Loschmidt,—explained it brilliantly, together with many other of the metrical properties of gases, as the result of molecular motions and bombardment; so that it has become a familiar and elaborate theory —the Kinetic Theory of Gases. Hence, in that form of matter about which we know most, the kinetic theory of elasticity holds the field.

Radiation and Matter.

Now come a series of questions which it is difficult to formulate precisely because of our inadequate knowledge, and concerning which we must make the best of the hints which from time to time are afforded us by Nature,—questions which are mainly concerned with the nature of radiation, and with the interactions between ether waves and ordinary matter.

It is unnecessary to point out in the first instance that light is now known to exert pressure, and therefore to convey momentum. An advancing wave-front possesses momentum, which it can transmit to any obstacle which either reflects or absorbs it. If reflected, the pressure it exerts is double what it exerts when absorbed; all quite in accordance with common sense. But I rather want to concentrate attention on the state of things when the wave-front is advancing—it may be for hundreds of years—through so-called empty space. It carries with it a pressure equal to the energy per unit volume. If the Third Law of Motion is true without exception,—and it is surely politic to assume the truth of that law until it is negatived,—there must be a longitudinal stress in that stream of light, with a reaction on the source at one end and on the advancing wave-front on the other.

The source is always something material. Light can only emanate from an accelerated—that is, from a revolving or vibrating—electron. Hence, at that end the reaction has a material basis, in accordance with the customary experience that a line of stress must stretch from one piece of matter to another. But what happens at the other end? When it encounters matter, the reaction is exerted on that matter, and everything is plain sailing. But while it is advancing in free ether, what is it that sustains the reaction? We can only answer, the wave-front. The wave-front cannot sustain it statically. It can only do so by advancing at the speed of light. But it is remarkable and worthy of note that in this particular the advancing wave-front simulates one of the properties of matter, namely, the power of sustaining stress.

Now to me this is very suggestive. We do not know what precisely is the kind of motion occurring in the associated electric and magnetic vectors which are travelling with the speed of light. We do not know the kind of motion associated, more statically, with an electron. But the guess is almost forced upon us that possibly these two kinds of motion are not entirely distinct. We could not say that perhaps they are one, and not two; for there are certainly differences between them. One must advance; the other may stay still. But is it possible to regard one as a consequence, or as a generator, of the other?

The electron generates light.

Does light generate an electron?

(I am using the term "Light" in a very general sense, not limiting it to the physiological kind which excites the sense of vision, but including X-rays, and all other forms of short wave radiation.)

What do we know about the effect of this kind of radiation upon matter? We know that it can produce the irregular movements that we call heat, and also that it can stimulate chemical action. But the discovery of photoelectricity shows us that it may do more. It may fling out an electron, with a surprising amount of energy, dependent upon the frequency, that is, upon the wave-length, of the incident radiation. This is a hint not to be ignored. Nor is it ignored; and there must have been many speculations as to the kind of way in which it achieves this result. One would naturally suppose at first that it must do it by means of resonance, that is, by the accumulation of properly timed impulses, until an explosion occurs. But the evidence is, on the whole, rather against a resonance view; because the result seems almost independent of the intensity of the incident radiation, and to depend only on its wave-length. Nor does it seem as if a great length of radiation was necessary in order to produce the result: though this is a matter which requires further and more conclusive experiment. If a beam of light is interrupted and cut up into small sections—as might be done by a narrow slit in a very rapidly revolving disc,—would this intermittent light be equally effective? For if it is equally effective, the fact would tend against the continuous accumulation of a small synchronous disturbance.

I believe that some experiments have been made in this direction, and that the answer—so far as it goes—is that intermittent is as effective as continuous illumination, and feeble light as efficient as strong. The energy falling upon a minute surface in a beam of diffuse light is insufficient to account for the energy of the resulting effect, unless it is a trigger effect.

But this rather wants pressing to extremes. To cut up a beam of light into really short portions is not very easy. If a radial slit a millimetre in width is made in a disc a metre in diameter, revolving a hundred times a second, light sent through it is diluted and cut up into sections; but the length of each section is still about a mile, and accordingly would contain more than a thousand million waves,—which is amply sufficient for resonance.

However, the evidence so far is supposed to negative the resonance idea: so much so that it has been supposed that the wave-front is not a uniform surface, but a speckled one; that it is discontinuous; and that the amount of energy concentrated in one of the specks may be vastly greater than would be reckoned on the diffuse or continuous theory.

The idea of a speckled wave-front would have seemed to our scientific ancestors rather wild; though it must be remembered that Newton, with his Corpuscular Theory, was temporarily satisfied with something very like it. Nevertheless, the Corpuscular Theory had to be abandoned because of the artificial way in which it explained polarisation, and because it seemed to require that light should travel quicker inside matter than outside, instead of slower—as Foucault proved it to do,—and because there are real difficulties in explaining interference and diffraction, unless the wave-front is continuous.

However, it does not do to turn down a theory too readily and prematurely, merely because we encounter a few difficulties. No hypothesis is wild which has attracted the serious attention of J. J. Thomson, and other brilliant physicists, including—as I think we must—even Faraday; as evidenced by his "Thoughts on Ray Vibrations" ("Experimental Researches," vol. iii. p. 447).

Moreover, though these ideas, as we perceive them at present, may not be able to substantiate themselves, yet they are the outcome of observed facts; and it may yet be found that, in a modified and revolutionised form, they may contain elements of truth at present unsuspected.

Wilberforce Model.

It may be said that if we depend on the pressure of light as conveying energy, it is a longitudinal phenomenon; whereas an electron is probably a rotatory or rotational phenomenon. Or again, if we attend to the magnetic oscillation in the beam of light, and consider that the electric oscillation is separated from it, or neutralised, by matter, that still there is nothing of the rotational kind about it.

One answer would be that circularly polarised light clearly has a rotational aspect. Another and more fundamental answer would direct attention to the transition, or interchange, that may go on between a linear oscillator and a rotational oscillator when they are of the same frequency, or properly attuned.

In illustration of that, I would invite attention to the illustrative models constructed by Prof. Wilberforce, my successor in the chair of physics at Liverpool, which show that a continual interchange of energy between a linear vibration in one direction, and a rotational vibration in a plane at right angles, naturally goes on when the two modes are synchronous. Thus the energy alternately takes first one form and then the other; and then back again, without intermission.

Of course the dynamics of the model is thoroughly understood, and Wilberforce himself has explained it, that is, has recorded the relevant equations;[2] and in that sense there is nothing puzzling about it, though its behaviour can be made to look rather paradoxical. But I feel that there is some meaning underlying the possibilities here indicated, which are not yet completely exhausted, and that they may, when more deeply considered, throw some light upon the interaction between electricity and magnetism—if that should still be necessary,—and possibly on the interaction between ether and matter, and perhaps between waves and electrons, where more information is certainly necessary. At any rate, I regard the behaviour of the model as suggestive, and am content for the present to direct attention to it, from this point of view.

Origin of Electrons.

Let us assume, then, for a moment that there may be some truth in the idea of a discontinuous wave-front. To what are we led? I should reply, that the motion in a wave-front seems more akin to the kind of motion that constitutes the discontinuous and isolated speck that we call an electron; and that the actual generation of an electron by means of light is not an altogether impossible idea.

So I repeat the question:

An electron suddenly set in motion generates light: does light when it is suddenly stopped generate an electron?

Sir William Bragg has often directed attention to the singular relation existing between X-rays and beta radiation. The impact of a beta particle emits X-rays. The impact of X-rays emits a beta particle. The energy of the original and the excited beta particle are so closely proportional as to be practically identical. It is as if the same beta particle, that is, the same electron, had gone out of existence at one place, and been recreated at another, the intermediate link being constituted by specific radiation of a perfectly definite wave-length.

There is no need to *assert* that one particle has gone out of existence and the other come in; and yet we know of no reason for denying it. It may have to be denied, but I think it wise to keep an open mind on the subject, however bizarre the notion may be. There are strange relations between energy and matter now coming to the front. Matter contains intrinsic energy, as if it were something circulating with the velocity of light. There must be some meaning in this. The ratio c^2 between matter and energy is not to be ignored.

Somehow or other the ether possesses inertia. It must, or it could not sustain magnetism, or account for the increase of inertia due to motion. The ether also contains an intrinsic and characteristic velocity, which is perfectly definite. It is known that the vibrations of vortices, and the speed at which a vortex medium can transmit transverse waves, are closely connected with the constitutional velocity of rotation. The two velocities are in fact equal, or connected by a numerical factor, of a magnitude which some theories make $\sqrt{2}$, but other theories make unity. In any case the numerical factor is not far from unity. We are justified in supposing that if the ether is full of circulatory motion, that motion must be practically the velocity of light. In that case, the fundamental nature of matter would appear to be giving up its secret; and the relation between matter and energy would be explained.

There does not then seem any insuperable difficulty about hoping that some future discovery will be able to generate matter, or at least to generate an electron, by aid of X-rays or other form of radiation. I can dimly conceive a theory of light which, when its advance was stopped, should terminate not in the irregular jostle called heat, but in the regular circulation or vortex motion that we call an electron. The intimate relation between energy and frequency associated with the quantum seems to me to negative the mere irregularity of thermal agitation, and to suggest something quite regular and constitutional.

We can go further, and can reckon how much matter would be generated by a given amount of luminous energy, if none of it were wasted as heat. A beam of ordinary sunlight ten centimetres square shining continuously, and supposed to be all converted or interpreted as matter, would generate a weighable amount, namely one-tenth of a milligram, in seventeen years.

The density of sunlight near the earth is equivalent to 2×10^{-15} gram per second per square centimetre. So if it were interpreted as matter, the earth would catch 80,000 tons of it per annum. Of course, some of it is wasted. Only radiation of the right frequency is effective, just as only energy of the right frequency is generated by a metrical impact. A lot of the radiation may be due to irregular jostling, and this portion when absorbed may result in heat. But it is the more precise kinds of occurrence which are instructive, and which must inevitably attract attention.

I know that the Bohr Theory of the Atom seems at first against these speculations. Electrons appear

[2] *Phil. Mag.*, October 1894.

to jump from one orbit to another, and thereby give out a certain quantum of energy. But this may be a supplementary and not a contradictory statement. What makes the electrons jump? Which electron jumps out? Sometimes it is from the K ring, sometimes from the L ring; and so on. All those things may be known. But still I ask, What started the disturbance? If an electron is generated by the impact of light, it does not follow that that particular electron is the one ejected. Its entry may be the means of ejecting another. Somehow or other the atom must get another, in order to restore its constitution. There are doubtless many ways in which a strayed electron could be recaptured; and I venture to suggest that our speculation suggests one of them.

Possible Utilisation of Waste Radiation.

There is an immense amount of radiation travelling about space. The whole amount of solar radiation is portentous. The fraction which the earth catches, though terrestrially so important, is but a minute fraction of the whole—less than the two thousand millionth part,—and it seems to have been going on for hundreds of millions of years. The radiation from many of the stars is greater. What becomes of all that radiation? Is it all waste? Space is so enormous that though thousands of millions of suns have been pouring out their energy for thousands of millions of years, space is no warmer. The ether is not warmed by it: the ether does not absorb it. The ether is perfectly transparent. Yet our instinct rebels against the idea that all this radiation results in nothing. Sir W. Siemens speculated as to its possible concentration by total reflection at an ether boundary. But I cannot imagine an ether boundary. I can more readily imagine that light results somehow in the generation of matter; and that there is a reciprocal interaction between matter and ether waves; so that each is generated by the other,—a sort of constant and perennial interchange.

Electrons have come into existence somehow. The subject of origins usually lies outside science. The origin of matter is as beyond our ken as the origin of life; and yet people speculate about the origin of life. Some highly estimated men of science *hope* at any rate that some day the chemistry and physics of life may be so far understood that a highly complex assemblage of organic molecules may simulate and perhaps adopt its functions. I see nothing inconceivable in this. Life has originated somehow; and if we can get to understand anything about its origin, the effort is legitimate. It may fail; but it would be a very superficial view of religion which resented its success. Mind dominates matter; and the mind of man is not altogether of a different order from the mind of the Creator. But this is a subject on which I could say more on a more suitable occasion. I only say thus much now in order to repel any idea of impiety in speculating on a possible origin for matter.

Hypothetical Conversion of Radiation.

The possibility that a small body may gradually grow in mass under the influence of an etherial transformation, does not seem one to be scouted without proper examination. The amount of matter scattered about in space is by no means inconsiderable, and the problem of its origin has never been attacked. Given matter, the origin of radiation has been more or less solved. But, given radiation, the idea of its conversion into matter has not, so far as I know, been mooted. Possibly the idea is erroneous. But interactions in Nature are so frequent, and the interrelations between ether and matter are so ill understood, that I think we should not shut our eyes to the possibilities of some reciprocal interaction, even of a generative kind.[3]

Sometimes I see the difficulties of the hypothesis; sometimes I feel impressed with a sort of probability about it. It is easier to see the difficulties than the probabilities. But the relationship between energy and matter—connected as they appear to be with the second power of the characteristic ether velocity, and with the conception of an intimate fine-grained rotational structure for the ether—is not a hint that should be too lightly ignored or neglected.

Electrons build up matter. What builds up electrons? They are somehow intimately connected with the ether; their motion through it displays to us the phenomenon of magnetism; and their acceleration generates waves. So far, we are on firm ground. When we come to the converse or reciprocal relations, we have but few facts to stand on. But the emission of electrons by means of light is one of them; and the bearing of this fact, until it is properly understood inevitably justifies speculation.

Previous Guesses.

When I say that the idea of reciprocal conversion has not been mooted, I am going beyond the facts. In Loring's "Atomic Theories," page 80, I find the following sentences:

"Thus it would seem that the energy phenomena are reversible, so that the radiation is as it were convertible into moving electrons and moving electrons are convertible into radiation. It is of course only the energy which is thus convertible. The mechanism of conversion is not, however, known."

Again, in Millikan's book "The Electron," when speaking of Barkla's discovery of the remarkable absorbing property of matter for X-rays, he says:

"It will be seen from these photographs that the atoms of each particular substance transmit the general X-radiation up to a certain critical frequency, and then absorb all radiations of higher frequency than this critical value. The extraordinary significance of this discovery lies in the fact that it indicates that there is a type of absorption which is not due either to resonance or to free electrons. But these are the only types of absorption which are recognised in the structure of modern optics. We have as yet no way of conceiving this new type of absorption in terms of a mechanical model."

Sir William Bragg, in Nature (1921), vol. 107, p. 79, with reference to the experiments of Duane and Hunt, says: "Exactly how this strange transfer of energy from one form to another takes place we do not know: the question is full of puzzles." He has several times urged the extraordinary character of the fact that a stream of radiation excited by the

[3] Cf. letter in Nature, May 26, 1923, p. 702.

impact of one electron, after travelling a long way and becoming greatly enfeebled, can eject another electron with the same or nearly the same energy as the first. Facts such as these have suggested the discontinuous nature of a wave-front, and the actual concrete existence of discrete tubes of force, which are apparently analogous to, or suggestive of, vortex filaments in the ether. Again, there is the fact that the electrostatic potential energy of a charge is similar to what the equivalent mass would possess if it were moving with the speed of light. Also, *à propos* of this, I understand that Sir J. J. Thomson has expressed himself thus:

"When the energy of a system passes from kinetic into potential, there need be no transformation of fundamental energy, but merely the flow of a mass-producing material, with its intrinsic kinetic energy, from one position of space to another, under the guidance of the lines of electric force."

In accordance with a few other physicists, Thomson has been led to postulate a fine-grained structure for the ether, which I think rotational, but which he speaks of as particles. He suggests that mass is made up of identical particles all of the same kind, very small compared even with an electron, moving with the velocity of light, and subject only to a deflecting acceleration, not to any change of energy; the mass and energy of each particle being constant, but their distribution depending on the number or concentration of lines of force, each line being as it were anchored normally to positive and negative electrons, but capable of being thrown by motion into loops or closed curves, which would then move away with the velocity of light and constitute radiation. Wherefore it would follow that emission of radiant energy must be accompanied by a diminution in the mass of the radiating body.

The converse, therefore, that absorption of radiant energy might be accompanied by an increase in mass, almost naturally follows.

My presidential address to the Physical Society of London on the subject of opacity, that is, on the orthodox theory of absorption generally,—electrical as well as optical,—is contained in the *Phil. Mag.* for April 1899, and also in the Society's Proceedings; in the latter place it is preceded by preliminary matter not irrelevant to the present discussion.

Mechanism of Absorption and Emission.

To understand the mechanics of absorption we can learn from the mechanics of emission. In a wave the electric and magnetic vectors are simultaneous; that is, the electric and magnetic displacements exist together, superposed. At a source they are only coexistent in space, not in time; one succeeds and gives rise to the other, with successive alternations. A source may be at rest, and is merely an alternator: a wave is necessarily in motion. The relative phases of electric and magnetic oscillations in the neighbourhood of a source determine the fact and the direction of motion at each locality. Combined in one phase they expand or advance, combined in another phase they contract or recede; all this is known to occur near the source, that is, near a Hertz vibrator. In that region, within a radius of $\lambda/2\sqrt{\pi}$, the etherial disturbance oscillates to and fro; and beyond that range a portion of the energy acquires its locomotive character, and sets out with the velocity of light.

Shall not the converse take place when this speed of propagation is annihilated, and the ether disturbance is reduced to locomotive rest, within a similar range near an absorber? In that region the simultaneous electric and magnetic disturbances would be separated, and converted into a stationary oscillation, by a process inverse to that of radiative emission.

Considerations of this character are indicated by me more quantitatively in the *Phil. Mag.* for June 1913, pp. 770-788, and in February 1920, p. 173; also in April 1921, pp. 555-557, where I endeavour to associate the ultimate fate of radiation with a kind of Einsteinian gravitational theory. It there turns out to be necessary to examine electrically the essential nature of absorption; and the illustration or analogy with a Hertz vibrator, as either source or sink, is employed. I return to the subject in June 1921, p. 943, and again in July 1921, pp. 181-183; though in the last paper the chief point is the disintegration of atoms which is to be expected at a certain calculated very high temperature—such as has since been considered by Prof. Eddington likely to occur in the interior of giant stars.

Eddington has taught us—at any rate hypothetically—that in the interior of giant stars, where the temperature is excessive and the radiation powerful beyond easy imagination, the substance of the star is distended, blown out, supported, as it were, by radiation bombardment, as the skin of a football, or an india-rubber tyre, is distended by the molecular bombardment of the air inside. He has further speculated, so I understand, that the interior of these stars may constitute a laboratory in which the more complex atoms can be built up,—those same heavy atoms of which we have now at length begun to witness the breaking down, under the operations of spontaneous radio-activity. There cannot be breaking down everywhere: there must be building up somewhere. We do not yet know what can be accomplished under conditions of extreme heat and pressure,—nor, I may add, under conditions of great pressure combined with extreme cold.

It may be said: the analogy fails, since what I am trying to suggest is the generation of electrons, and we nowhere know of the breaking down of electrons. That is true: we do not know either of their breaking down or their building up. It may be that we shall discover the untying of an electron first; or it may be that we shall discover a tying-up first, and the untying later. Or it may be that, once tied, they are permanent. Or of course it may be that they cannot be tied. But these questions seem to me all open. The time for discovery is not yet; but he would be rash who would say that discovery in any particular region is impossible. If there are any clues, it is the privilege and indeed the duty of science to follow them up. If the clues are imaginary and useless, then open discussion will demonstrate their futility. But if we can see any distance, however dimly, into the unknown, then sooner or later we may be sure that pioneers will explore those dim regions until they are illuminated with the searchlights of systematic knowledge.