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Preservation and Restoration.

ONE of the most important services which science can render to mankind is in the discovery of the precise origin of corrosion and decay brought about by natural causes and of methods of counteracting the destructive agencies. It becomes increasingly important to man to preserve, during such times as he may desire, the material fruits of his labour in their original form. Foodstuffs must be preserved during periods of plenty and during transportation to lands where they are scarce ; structures of wood, metal and stone must be safeguarded from the destruction caused by living organisms, water, frost, and the atmosphere ; fabrics must be protected from the deterioration brought about by light and bacteria. The annual monetary loss due to our lack of knowledge of the mechanism and counteraction of the phenomena involved is enormous and, in fact, incalculable.

For evidence of our ignorance in such matters it is only necessary to look at the stonework of almost any ancient building ; a cursory examination of some of our modern buildings will indeed suffice. Is it impossible completely to protect and preserve stone from decay and destruction ? Is the vast annual sum spent in protective paints for iron and steel structures really essential expenditure ? Such questions as these are at present unanswerable, but they are unlikely to remain so if adequate scientific research be directed to the problems so obvious to every one. Brearley's discovery of stainless steel, important as it is, is but a minor success in such a wide field, for the use of this material is greatly restricted by its price. Nevertheless, the discovery encourages the belief that, so far as metals are concerned, the broader problems are not insoluble.

Individually, the problems of corrosion and decay are not very attractive to the independent research worker of the present day ; the lure of more recondite fields of research is generally too powerful. But viewed collectively these problems are so important economically, and so far-reaching, as to call for co-ordinated investigation on a wide scale. In such investigations Government can and should play a valuable part as an organising and directing agency, and it is satisfactory to note the steps already taken in this country to initiate and to subsidise the necessary research. Perusal of the last Report of the Advisory Council for Scientific and Industrial Research (see NATURE, February 3, p. 165) shows that in addition to the assistance given to two professional bodies in aid of researches on special types of corrosion, the Department is carrying out several kindred inquiries under its own direction. Grants have been made to the Institute of Metals for the investigation of the corrosion of condenser tubes and of aluminium, and to the Institution of Civil Engineers for a research upon the deterioration of structures in sea-water. In direct association with the Department is the Food Investigation Board, which is dealing with the fundamental problems of food preservation. The Fabrics Research Committee and the Forest Products Research Board are interested in the protection of fabrics and woods respectively from decay, and we understand that a committee has recently been formed to inquire into methods of preserving stonework. Finally, by means of the laboratory which has been set up at the British Museum, the Department has provided for research into the causes and prevention of corrosion and decay occurring in museum specimens. Though each of these organisations has its own specific ends in view, judicious co-ordination of their efforts and intercommunication of the results they obtain will doubtless be of great assistance to the general progress.

A second report [1] on the investigations in progress at the British Museum has recently been published, and its appearance could scarcely be more opportune. The information it gives will be of great value to the curators of museums of antiquities and, we imagine, will be specially welcome to those who are engaged in the responsible and delicate task of recovering the archæological treasures of King Tutankhamen's tomb.

Little scientific research directly bearing upon the preservation and restoration of museum specimens has been undertaken in the past. Too often have the attempts at restoration been left in the hands of museum workmen whose empirical efforts have in some cases ended admirably, in others disastrously. Successful methods so devised have sometimes jealously been guarded as "trade secrets" guaranteeing continuity of employment. Such an unsatisfactory state of affairs cannot continue; if it is our duty—and indubitably it is—to preserve for future generations the evidences of past phases in the life of mankind, then it is essential that knowledge of trustworthy preservative processes should be communicated freely to all concerned. It is gratifying that Great Britain should take the lead in instituting scientific research of a very high order in this direction, and in publishing the results for the general benefit of all who are possessors or curators of valuable antiquities.

The Department has admittedly been very fortunate in enlisting Dr. Alexander Scott as director of the investigations which are being conducted at the British Museum laboratory. His second report, like its predecessor, shows abundant evidence of the high degree of experimental resource required in work of this kind, and of the very fragile character of many of the objects which he has successfully restored and protected from further deterioration. But in Dr. Scott the fears and caution of the antiquary are tempered by the confidence born of scientific knowledge; as a result of this happy combination we have on record the solutions to a number of problems which have long been a source of anxiety to museum curators. Prints and pictures, and objects of stone, silver, iron, lead, copper, bronze, and wood have all been brought to Dr. Scott for treatment, and subsequently have been returned to their places in the museum restored and insured against further attack.

The work at the British Museum laboratory has hitherto, naturally, been chiefly of a chemical character. But many museum problems have a microbiological aspect. The cellulose-destroying moulds and bacteria, for example, must play an important part in the decay of fabrics, paper, and other materials in museums; in time, doubtless, the laboratory will be able to turn its attention to these problems. Reference to such a development suggests the interesting possibilities which would be involved in a microbiological examination of the fabrics and cellulosic débris found in King Tutankhamen's tomb. Even though the examination proved negative so far as the discovery of spores of bacteria and moulds is concerned, valuable information would be yielded by the decayed material itself, for it is now known that cellulose fibres which have been attacked by such organisms show characteristic markings. We strongly hope that facilities will be given for such an examination to be made before the material has become infected with present-day organisms.

Attention should be directed to a feature of Dr. Scott's report unusual in Government publications, the excellent collotype illustrations; these supply striking visual confirmation of the successes he describes.

[1] "The Cleaning and Restoration of Museum Exhibits." Second Report upon investigations conducted at the British Museum. Published by H.M. Stationery Office, 1923. Price 2*s*. net.

Physiology in Medicine.

The Heart as a Power-Chamber: a Contribution to Cardio-Dynamics. By Dr. Harrington Sainsbury. (Oxford Medical Publications.) Pp. xii+248. (London: Henry Frowde and Hodder and Stoughton, 1922.) 12*s*. 6*d*. net.

IF we compare the text-books of physiology of to-day with those of twenty years ago, we cannot fail to be impressed, not only with the vast strides that have been made by the subject within this short time, but also with the fact that a large majority of the latest discoveries, which have an intimate bearing on the understanding and control of disease, could not figure at all in the physiological equipment of the men who studied medicine at that time and are now in the full tide of practice. Even the professional physiologist finds it difficult to keep himself abreast of the course of discovery in his own subject. It would seem, therefore, almost impossible to expect a man in a busy practice to appreciate what recent physiology has done and is doing for his science and for his craft. Many men, and those not the least successful, do not attempt the task, and trust to their craftsmanship and their powers of naming a diseased condition, that is, of placing it in a category familiar to them which they therefore believe they understand, and to their experience in treating such cases without, at any rate, harming the patient.

At the present time the condition is improved by the establishment of clinical units, of which the heads have time and opportunity, not only to advance their own subject, but also to keep abreast of the more important researches in the collateral sciences, so that they may serve to some extent as interpreters of the latter to their professional brethren. But even for the practitioner who is not so fortunately situated the task is not so impossible, if his training in physiology has been of the right character and has fallen on favourable soil. In the physiological training of the student it is not collections of facts or strings of arguments which are of supreme importance, but the method by which these facts are attained and the attitude of mind of the investigator. If he can carry this method and this attitude of mind into the wards, every case becomes for him a physiological experiment. Diagnosis is not the application of some appropriate label, but an understanding of what is happening in the body and how the disorder of any given function has come into existence. The whole of his practice becomes a research, and with one problem after another crying out for solution his attention and his curiosity are kept awake for any light which may be thrown by physiology or other science on the questions with which he has to deal. The true scientific physician must remain a physiologist during his whole life.

In the work under review Dr. Sainsbury shows that he has not forgotten the lessons in physiological method and thinking that he learned with Sidney Ringer. Taking as his text the action of the heart and the modifications that this may undergo in disease, he endeavours from his pathological and clinical experience to reconstruct the conditions in the living organism and, as he says, "to visualise the organs and tissues dynamically." He shows that in every case the test of structure must be the functional adequacy of the tissue. Given a case of heart disease the important thing is not an intimate analysis of the heart sounds and their modifications, but the knowledge of what the heart can do, and what are its powers as a pump, *i.e.* in maintaining the circulation of the blood.

In the first chapter, on the anatomical relations of the heart, the author gives an interesting series of measurements of the relative weights of the different parts of the heart, and shows that the muscular tissue surrounding each cavity is roughly proportional to the work that the cavity has to do in the maintenance of the circulation of the blood. Thus the muscular tissue of the auricles is roughly only one-tenth of that of the two ventricles, while the muscular tissue of the right ventricle as compared with that of the left ventricle is a little less than one-third of the latter (1 – 2·5). In this case there would seem to be a discrepancy between the mass of the muscle and the actual work done by each cavity. It is probable that the work of the left heart is five or six times as great as that of the right ventricle. The smaller difference in the muscular tissue may be due to the greater mechanical disadvantage attendant on the arrangement of the muscular fibres of the right ventricle.

When we consider the enormous strain that may be thrown upon the walls of the left ventricle during exercise it is astonishing to find that one part of its wall, namely, that of the extreme apex, is only a few millimetres thick. Dr. Sainsbury points out that the heart would tend to rupture at this point if it had to sustain the full pressure of the blood during the ventricular systole. He suggests that at the very beginning of systole the blood is squeezed out from the apex by the preliminary contraction of the vortical fibres at this spot. As a matter of fact, Lewis has shown that the vortex of the left ventricle is one of the places where the wave of negativity preceding contraction appears earliest, though the time is short which elapses between the appearance of the wave at the apex of the ventricle and that at other parts of the two ventricles. It must be owned that electrical measurements give no support to the further hypothesis of the author, namely, that the circular band of fibres surrounding the left ventricle must contract later than the spiral fibres.

Attention is directed to a fact which often escapes notice, namely, the large size of the aorta and big veins as compared with the heart. Here we have a pump putting out about 4 oz. of blood at each stroke into a vessel 1¼ inches in diameter, and the big veins entering the heart have a total cross-section even larger. We should be almost justified in speaking, therefore, of an arterial sac and a venous sac, each serving as a reservoir of blood to supply the arterial system and the heart respectively.

It is always difficult to judge of the relative value of results obtained by different methods in a science with which one is not in daily contact. In Dr. Sainsbury's account of the venous pulse he raises difficulties which are really due to the attempt to make a minute comparison between the results of two methods, one of which is accurate to one-thousandth of a second, and the other only to one-twentieth of a second. By the optical method we can obtain very accurate records of the intra-auricular pressure. These show small elevations of pressure, one due to the contraction of the auricle, the second to the beginning of the ventricular systole and the sharp closure of the auriculo-ventricular valves, and the third to the accumulation of blood in the auricle during the continued contraction of the ventricle. A tracing of the venous pulse in the neck taken with a polygraph also shows three elevations which must have a similar causation. The middle one has been called the 'carotid pulse' by Mackenzie, and was ascribed by him to the pulse in the carotid transmitted to or through the jugular vein. This extraneous element in the venous pulse may possibly be often present in the tracings taken by this method, but it is really of not much importance whether it is external or whether it is due to the propagation of the wave of pressure which occurs in the auricle at the beginning of systole. Within the limits of error of the apparatus the 'c' wave may serve to mark the beginning of the ventricular systole, since it occurs either at the very beginning or within two-hundredths of a second afterwards.

In his description of 'tone' as applied to the heart, the author, in common with many clinicians and guided by the physiology of a few years ago, takes a view which I believe is erroneous. He describes tone as resistance to distension and therefore as a property which comes into play during diastole to prevent over-distension of the heart. Such a property would hinder rather than further the action of the heart pump. The filling of the heart is determined by the inflow. If the inflow increases, the rate of the heart (in the intact animal) increases *pari passu* so that this organ shall not be too distended. To prevent over-distension the strong fibrous sac of the pericardium is provided. It is important that the heart during diastole should present as little resistance as possible to distension, since any resistance would cause a rise of venous pressure and impede the circulation. If we examine the clinician's idea of a heart with good tone, we find he is really speaking of a heart with good contractile power, *i.e.* one which contracts strongly and empties itself, or nearly so, at each beat. The 'tone' would be measured rather by the systolic volume than by the diastolic volume of the heart. The term, however, is so ambiguous and has given rise to so much confusion that it would be better not to employ it at all in connexion with the heart.

There are certain other points which one might criticise, such as the part ascribed to the capillaries in the maintenance of the normal resistance of the circulation, as well as the mechanism of the absorption of drugs administered subcutaneously. But it is on account of its point of view that Dr. Sainsbury's book is useful and can be recommended to students. It might, indeed, be set to senior students as a subject of commentary and criticism from a physiological standpoint. If they could take the habit of mind of the author with them into the wards, their training in physiology would not have been in vain. E. H. STARLING.

Normal and Abnormal Psychology.

(1) *Beyond the Pleasure Principle.* By Dr. Sigm. Freud. Authorised translation from the Second German edition by C. J. M. Hubback. (The International Psycho-Analytical Library, No. 4.) Pp. v+90. (London: G. Allen and Unwin, Ltd., 1922.) 6*s.* net.

(2) *Fundamental Conceptions of Psychoanalysis.* By Dr. A. A. Brill. Pp. vii+344. (London: G. Allen and Unwin, Ltd., 1922.) 12*s.* 6*d.* net.

(3) *Studies in Psychoanalysis: An Account of Twenty-seven Concrete Cases preceded by a Theoretical Exposition.* By C. Baudouin. Translated from the French by Eden and Cedar Paul. Pp. 352. (London: G. Allen and Unwin, Ltd., 1922.) 12*s.* 6*d.* net.

(4) *Medical Psychology and Psychical Research.* By Dr. T. W. Mitchell. Pp. vii+244. (London: Methuen and Co., Ltd., 1922.) 7*s.* 6*d.* net.

(5) *The Measurement of Emotion.* By W. Whateley Smith. (International Library of Psychology, Philosophy, and Scientific Method.) Pp. 184. (London: Kegan Paul and Co., Ltd.; New York: Harcourt, Brace and Co., Inc., 1922.) 10*s.* 6*d.* net.

(6) *Remembering and Forgetting.* By Prof. T. H. Pear. Pp. xii+242. (London: Methuen and Co., Ltd., 1922.) 7*s.* 6*d.* net.

(1) PROF. FREUD'S "Beyond the Pleasure Principle" is not a long essay; but it is exceedingly difficult to read, not only because of the style in which it is presented, but also on account of the philosophical ideas which the author attempts to express. It is packed full of observations, theories, and extensions of theories of great interest and originality.

The reader will not always, perhaps, be able to find himself in agreement with the argument; but he will certainly be stimulated to think. Originally, Freud's theory worked with fairly simple conceptions. The "pleasure-principle" emerged as a result of actual psychoanalytical practice. Any mental process originates in a state of tension, which is unpleasant; and, in virtue of this principle, moves towards relaxation. There is a tendency towards stability. But this tendency is met and checked by a "reality-principle" to which the sane psyche must adjust itself. Nevertheless, in the long run this too makes for pleasure. But phenomena are observed—certain forms of play in children, dreams in cases of war-neuroses, etc.—which seem to indicate a compulsion to repeat unpleasurable experiences. These show in a high degree an instinctive character. Considering this repetition of unpleasant activity, Freud accordingly puts forward the speculation that instinct might be "a tendency innate in living organic matter impelling it towards the reinstatement of an earlier condition."

Developing this speculation, Freud reaches the conclusion that the goal of instinct, as of life itself, is death. Originally, again, psychoanalysts had drawn a sharp distinction between the "ego-instincts" and the "sex-instincts." How, then, could an "ego-instinct" such as that of self-preservation have death as its goal? The answer is found to be given in Narcissism. Self-preservation is in reality libidinous. The libido is turned upon the ego and, *pro tanto*, away from the object. Accordingly, instead of the old distinction between the "ego-" and "sex-instincts," a distinction is now drawn between the "life-" and "death-instincts." These have striven together for mastery from the very beginning of the emergence of life from the inorganic. The "pleasure-principle" marks the "life-instincts" with the universal tendency of all living matter, namely, to return to the peace of the inorganic world. The "reinstatement-compulsion" lies behind it as well as behind the "death-instincts" of the organism.

Freud advances other highly ingenious and interesting speculations in his essay, of which one is a theoretical account of the development of the nervous system, open to the assaults of the exterior world only through a limited number of special channels which protect it from the prodigious energy without. But this nervous system is unprotected from the instinctive forces which arise within the body. These are not "bound" but free-moving nerve processes striving for discharge; and they give rise to disturbances comparable to the traumatic neuroses.

(2) Brill's "Fundamental Conceptions of Psychoanalysis" consists of the lectures of a course given to students in pedagogics in the University of New York. It is an elementary presentation of the Freudian principles and doctrine, and deals with the familiar topics of psychoanalytic literature—forgetting, stammering, lapses, mistakes, dreams, etc. There is an interesting chapter on the only child, another on selections of vocations. The book is diffuse, of a free-and-easy style, and full of Americanisms. It is published in England, but the type and spelling suggest that the plates were cast in America.

(3) Most people come to an inductive science with metaphysical presuppositions of one kind or another. In "Studies in Psychoanalysis" Baudouin makes a protest against the spirit of the systematiser, which has "been the bane" of the subject. The first part of the work is taken up with theoretical exposition. The second consists of 207 pages of case histories given in detail. The beginner is well advised, in the translators' preface, to commence with the cases and read the theory afterwards. He will thus be in a better position to examine the inductions made by the author in the light of the facts.

Baudouin links up psychoanalytic theory with general psychology. He is an eclectic, accepting principles from authors of widely differing views, and adding to them views of his own. His most personal contribution to the practice of psychotherapeutics is his conjoint use of psychoanalysis and suggestion. He is averse to the practice of either alone. The employment of the two methods together has been much criticised, many analysts condemning it outright; nevertheless it is difficult to see how suggestion can be kept out of an analysis. As the author remarks, "transference" is an effective relationship between the analyst and the patient, in which the ideoreflexes of suggestion occur naturally. This appears to be so; and therefore a controlled use of suggestion would seem to be reasonable. The present forms of suggestion and of analysis grew in two parallel lines of development from a common origin. Psychoanalysis was, in the first instance, practised on subjects in the hypnotic state.

The histories of the cases given are interesting, and range from those of quite young children to adults. The book is well translated. A good glossary of psychoanalytical terms is provided, as well as a bibliography and a very complete index.

(4) Dr. T. W. Mitchell is president of the Society for Psychical Research, and in his work on medical psychology he discusses a number of facts derived from abnormal and pathological psychology with the view of throwing light upon "psychic" problems. The main topic treated is multiple personality—for the account given of the appreciation of time by somnambules and the case of hysteria described in detail really relate to this. An account is given of an interesting series of experiments carried out by the author, which consisted in the performance of post-hypnotic suggestions involving the appreciation of lapse of time on the part of the subject. Mitchell considers that, whether

we treat the data as orthodox men of science or transcendentally, there is a large residuum of unexplained phenomena. In view of the controversy alluded to above, it is interesting to note that Mitchell's hysterical patient, in whom several "personalities" developed, was ultimately cured, partially by analysis carried out in the hypnotic state, and partially by word-association tests in the waking state. Besides the study of this case, the well-known classical cases are recounted and discussed.

The latter chapter deals with body and soul. The author examines the various psycho-physical theories in connexion with abnormal and pathological states; and the existence of a transcendental "soul," as the substrate of consciousness, is put forward as a legitimate hypothesis by which to account for some of the striking phenomena of multiple personality. Thus straying into the "vaguer regions of transcendental speculation," the author strangely makes no mention of hylomorphism, into which theory the facts would seem to fit as well as into those of Plato or Descartes.

(5) A great deal has been written on the emotions, both from the point of view of their expression and from that of introspective description. But it is only recently that much experimental investigation has been devoted to them. Mr. Whateley Smith attacks the problem in an experimental manner; and his "Measurement of Emotion" is one of the pioneer steps in that direction. The author, using the psychogalvanic reflex, reaction times, and reproduction tests as indicative of emotional changes, carried out a series of experiments on fifty subjects in order to ascertain the effect of emotion upon memory. Measurements were taken for 100 reactions to stimulus words (modified Jung list), and a number of these words were later learned by heart and reproduced at intervals by the subjects. Thus a memory-value, to be correlated with the affective value of the words in question, was obtained.

It was found that affective tone is of two kinds, positive and negative, and that positively toned words tend to be remembered, while negatively toned ones tend to be forgotten. The galvanometer records both kinds of tone. Reaction times and failures in reproduction are, in general, signs of negatively-toned words. Reaction-word experiments were also carried out with subjects under the influence of alcohol. It was found in these cases that highly-toned reactions gained and moderately-toned ones lost; and that the reactions in general regressed towards an all-or-none, or protopathic, type. The research is a well-planned one, and some of the conclusions valuable not only in themselves, but also in their applications to other problems in psychology.

(6) Prof. Pear's work on memory is not an ordinary text-book on the subject. In the first place, it is a popular exposition, growing out of a nucleus of lectures originally delivered to officers of the R.A.M.C. on the normal functions of memory, intended to help them to estimate abnormalities in their patients. In the second place, its net is cast wide enough to include much that is usually not treated in formal discussions of the topic.

Pear deals with the nature of memory and the mechanism of remembering, as well as of the process of forgetting. There is an important chapter on the functions of the image, in which the question of "imageless thought" is treated, and much on dreams, their mechanism and analysis. This last has become very prominent of recent years in relation to memory in connexion with psychoanalysis. The book has appendices on synæsthesia, number-forms, muscular skill, and the significance for problems of memory of some recent experiments (Head's and Rivers's) on the nervous system. It is written in the characteristic breezy style of Prof. Pear, and should be of value as an easy introductory avenue to the subject of which it treats.

Carotin-like Colours in Plant and Animal Tissues.

Carotinoids and Related Pigments: the Chromolipoids. By Prof. Leroy S. Palmer. (American Chemical Society Monograph Series.) Pp. 316. (New York: The Chemical Catalog Co. Inc., 1922). 4.50 dollars.

TO all who are interested in the investigation of the colouring matters produced by Nature in the vegetable and animal kingdoms this work should be welcome. It forms one of a monograph series, being produced under the auspices of the American Chemical Society in accordance with an arrangement with the Inter-Allied Conference on Pure and Applied Chemistry which met in July 1919. The series will form a very valuable addition to chemical literature in the English language if all the volumes deal as thoroughly with their respective subjects as does this one.

The author restricts himself to red, orange, and yellow pigments which can be extracted from the tissues by fat solvents—the carotinoids and related colouring matters. The opening chapter contains a very necessary review of the nomenclature in use, in the course of which the various irregularities and overlappings that exist are clearly indicated and the methods of nomenclature used in the treatise itself is set out. This chapter is, of necessity, rather disjointed in character, and the section dealing with non-carotinoid plant pigments is poor. For the sake of convenience the author, when passing to the description of the carotinoids which occur in plant life, adapts the subdivision of his field into carotinoids in Phanerogams (ch. ii.) and carotinoids in Cryptogams (ch. iii.). Although, as admitted by the writer, there is no logical reason for so doing, as the various pigments are widely distributed through Nature, this method of treatment has been worked up in an interesting manner and the interest deepens as each group is dealt with.

Passing from plant to animal life, the literature concerning the occurrence of carotinoid pigment in Vertebrates (ch. iv.) and Invertebrates (ch. v.) is surveyed. Later chapters deal with the very highly interesting problems concerning the chemical and biological relationships which may exist between plant and animal carotinoids; also with the ideas that have been put forward concerning the functions which carotinoids perform in plant and animal life.

Three chapters are devoted to the description respectively of the methods of isolation, the properties and methods of identification, and the quantitative estimation of carotinoids. Interesting plates show the crystal forms of several pigments of this group, also spectrophotographic records of their absorption bands. A summary follows each chapter.

A comprehensive bibliography is included, and followed by author and subject indexes—which, however, cannot be described as complete. It is unfortunate that in places careless phraseology is used, which considerably detracts from the pleasure of the reader. The volume contains a very large mass of information that will be invaluable to all investigators working in this field.

Paradoxical Science.

The Constitution of the Universe. (The Theory of Intersistence), dedicated to my Subscribers. By Louis Stromeyer. Pp. xx+255+xv. (Bangalore: Higginbothams, Ltd., 1922.) n.p.

MOST secretaries of local scientific societies (as well as many other people) have experience of the man who possesses the type of mind exemplified in this book: a mind as attracted by scientific hypothesis as a moth to a flame, and as wanting in discretion as the moth. The author is a mining engineer in India, and in his preface writes not without some modest sense of his temerity in composing this book and inducing a number of friends to finance its publication. A few words of apology, however, and particularly the confession that it has been written hastily and without opportunity to consult proper scientific literature, will scarcely excuse so hardy a piece of presumption. No man occupied with practical affairs, especially if his work is based on the application of physical science, like mining, would fail to adopt an attitude of severe disapproval towards an amateur who, while confessedly ignorant, proposed to reverse all the conclusions arrived at by men experienced in these affairs, and to substitute wholly new theories and methods; yet most practical arts are relatively simple, compared with the vast and complex structure of modern science, which these amateurs are eager to raze and rebuild. It is, indeed, remarkable that this obvious consideration should not prevent men, often capable and successful in their own work, from embarking on so foolish an enterprise, and imagining that they

> "Can tell us easy how the world was made,
> As if they had been brought up to the trade,
> And whether chance, necessity, or matter,
> Contrived the whole establishment of nature."

This book bears the typical marks of its class: a title of ample scope, chapter-headings of appropriate vagueness ("The Fundamentals," "Form and Posture," "Co-ordination," "Phases," etc.), arguments in an involved pseudo-philosophical style, and, as befits such a work at the present day, preoccupation with relativity and the magic name of Einstein. By request, the author has included a chapter specially devoted to the criticism of "modern theories"; it is surprising to find the bulk of this devoted to objections against a subject so relatively simple as the kinetic theory of gases and of matter generally.

The objections here raised (and they are typical of those brought forward elsewhere in the book) merely show, when examined, that the theories in question have proved difficult to the author's comprehension, and difficulties of this kind are probably the bond of union between the author and his supporters. There is nothing to be ashamed of in finding theories difficult to understand, especially if, as is often the case, they require an acquaintance with mathematics and a background of physical knowledge which are themselves only attainable by careful study. Such difficulties are by no means a mark of inferior ability: the late Lord Rayleigh, for example, mentioned at a British Association meeting some years ago that he had formed no opinion on a certain result in the theory of diffraction, obtained by Sommerfeld, because to appreciate the matter properly would require a fortnight's serious reading, which he did not feel ready to give to it. But in science, as in religion, there are some who by their own unaided powers

> "Will undertake the universe to fathom,
> From infinite down to a single atom.
> . . . .
> And, where they've least capacity to doubt,
> Are wont t' appear most perempt'ry and stout."

One is tempted, indeed, to go on quoting Samuel Butler, who, whatever the quality of his rhymes, summed up the truth of this matter in a few caustic passages. For language adequate to describe the obscurity and tedium of such books one would need the powers of a Swift or a Pope.

Our Bookshelf.

(1) *A New Manual of Logarithms to Seven Places of Decimals.* Edited by Dr. Bruhns. Thirteenth Stereotype edition. Pp. xxiv+610. (London: Chapman and Hall, Ltd., 1922.) 12*s.* 6*d.* net.

(2) *Tables of* $\sqrt{1-r^2}$ *and* $1-r^2$ *for Use in Partial Correlation and in Trigonometry.* By Dr. J. R. Miner. Pp. 49. (Baltimore, Md.: The Johns Hopkins Press, 1922.) 1 dollar.

(3) *Two-figure Tables.* Compiled by C. R. G. Cosens. On Card, 10 in. × $4\frac{3}{4}$ in. (Cambridge: Bowes and Bowes; London: Macmillan and Co., Ltd., n.d.) 6*d.* Quantities of one or more dozens supplied at 4*s.* per dozen.

(1) THE first table in this edition of Dr. Bruhns' Manual is a reprint of Köhler's table of logarithms of integers from 10000 to 100000, covering 180 pages. Auxiliary tables of proportional parts at the side of each page give all necessary assistance to a computer in finding the logarithms of six- and seven-figure numbers. Eight-figure logarithms of integers between 100000 and 108000 are not given, as in the original Köhler and the modern Chambers, " since the addition of logarithms of numbers from 100000 to 108000 does not appear to offer a sufficient advantage." With this we do not agree. Eight places of decimals in logarithmic work in dealing with numbers that slightly exceed 1·0 are only equivalent to seven places for numbers in the neighbourhood of 9·0. Within recent years the present reviewer was engaged in computing work in which the eight-figure logarithm was essential for numbers just greater than 1·0. In fact it was only regretted that the eighth figure was not available from 100000 to 115000.

There follow tables of the logarithms of sin x, cos x, tan x, and cot x, the entries being given at intervals of one second from $x=0°$ to $x=6°$, and at ten-second intervals from 6° to 45°. In the latter range six pages are assigned to each degree of arc, whereas one page is given to each minute from 0′ to 10′. The tables of differences and proportional parts on each page give every help needed in interpolation.

A few minor tables are added to the above main ones. The tables on each page are set out in an attractive way, and the new edition will be found to be a very serviceable one.

(2) The tables in this pamphlet give the numerical values of $\sqrt{(1-r^2)}$ and $1-r^2$ to six decimal places for values of r between 0·0 and 1·0 at intervals of 0·0001. Differences and subsidiary tables for interpolation are not appended. Thus the table gives the consecutive entries

$$\sqrt{(1-r^2)}=0{\cdot}567026 \text{ when } r=0{\cdot}8237,$$
$$\sqrt{(1-r^2)}=0{\cdot}566881 \text{ when } r=0{\cdot}8238,$$

and a slide-rule calculation is necessary to evaluate the function when $r=0{\cdot}8237463$.

The tables were calculated with a view to their bearing on partial correlation coefficients: they serve equally to determine cos θ and $\cos^2\theta$ from sin θ by a single reading.

(3) On the two sides of this card are printed the numerical values of sixteen functions, x^2, x^3, $\sqrt{x}$, $\sqrt[3]{x}$, $x^{\frac{3}{2}}$, $1/x$, $\log_{10} x$, $\log_e x$, e^x, e^{-x}, sin x, cos x, tan x, sinh x, cosh x, and tanh x, the intervals for x being 0·1 between 0·0 and 5·0, and 0·5 between 5·0 and 10·0. In tabulating the circular functions, x is measured in radians and not in degrees. Except in special cases two-figure accuracy is retained throughout. This amount of accuracy is sufficient for plotting many elementary graphs for the purpose of which the card is primarily intended.

W. E. H. B.

Essai d'optique sur la gradation de la lumière. Par Pierre Bouguer. (Collection "Les Maîtres de la Pensée scientifique.") Pp. xx+130. (Paris: Gauthier-Villars et Cie, 1921.) 3 francs.

PIERRE BOUGUER was born at Croisic in 1698. At an early age he was initiated in mathematics and problems of navigation by his father, who was one of the best hydrographers of his time. When only fifteen years of age he occupied the chair of his father, who had just died, and afterwards distinguished himself by his researches in physics, astronomy, and navigation. He is remembered to-day principally by his work on photometry, and by his expedition to Peru in 1735 to carry out a measurement of a degree of latitude, thus contributing to the solution of the important problem of the figure of the earth. It was during this expedition that he obtained an estimate of the mean density of the earth from pendulum observations in the neighbourhood of Chimborazo. The present essay, in which he laid down the fundamental bases of the science of photometry, is reproduced from the original text of 1729. The author discusses methods of measuring the intensity of light, the manner in which the intensity is changed by reflection or by absorption, and explains how to calculate the diminution in the intensity after the light has passed through various thicknesses of the absorbing medium. His work is distinguished by its clarity and the masterly realisation of the essential points in the problem to be solved.

The Internal Combustion Engine: a Text-book for the Use of Students and Engineers. By H. E. Wimperis. Fourth edition (revised and enlarged). Pp. xvi+320. (London, Bombay and Sydney: Constable and Co., Ltd., 1922.) 12*s.* 6*d.* net.

IT is not surprising that a fourth edition of this valuable work should have been called for within thirteen years after its first publication. As is well known, the progress of the internal combustion engine during the war was very rapid, due largely to aviation. By rearranging some of the older matter, the author has been able to give an account of these advances, including recent experimental work on explosions in closed vessels, and modern fuels and fuel mixtures suitable for use in petrol engines. The chapter on the efficiency of petrol engines has also been brought up-to-date and now includes some matter referring to the loss of power at altitudes in aero engines. Two methods have been proposed for getting rid of this difficulty, namely, the production of an artificial atmosphere by means of a blower in the carburettor intake, or using an oversize engine, which is kept throttled down at low altitude. In either case, the object is to design an engine which can develop constant power up to a certain height. For altitudes up to 20,000 feet, the over-dimensioned engine appears to be considered the simpler solution.

Mazes and Labyrinths: A General Account of Their History and Developments. By W. H. Matthews. Pp. xviii+254. (London: Longmans, Green and Co., 1922.) 18*s.* net.

MR. MATTHEWS, who does not pretend to be a trained archæologist, tells us that his book originated in a question addressed to him by his little son as he played on the seashore, "Father, who made mazes first of all?" As his bibliography shows, he has studied the literature of the subject, and he has collected much information summarised in a popular way. He begins with the two great labyrinths of antiquity, that at Knossos in Crete, and the second near Lake Moeris in Egypt. In describing these, he depends on the safe guidance of Sir A. Evans and Prof. Flinders Petrie. The former was based on a tradition of the complex of buildings forming the royal palace, the latter was possibly used for sepulchral purposes. Though, as Sir James Frazer suggests, the dancing-places associated with these ancient labyrinths may have been used in some magical way connected with sun worship, it is difficult to connect them with modern mazes, like those at Hampton Court or Hatfield, adjuncts to garden planning, and intended for the amusement of visitors. The best part of the book is the collection from various sources of illustrations of various types of mazes. Many of these have been destroyed in modern times, and this book may serve a useful purpose in directing attention to their interest, and may tend towards the preservation of those which survive to our day.

The Outdoor Boy. Edited by Eric Wood. (The Modern Boy's Library.) Pp. 280. (London: Cassell and Co., Ltd., n.d.) 5*s.* net.

PROBABLY no class of the community takes a greater interest in the education of their sons than the readers of NATURE. While the most suitable form of education will long remain the subject of debate, few will deny the importance of the out-of-doors side, both from the point of view of awakening a love for and an interest in Nature and preparing for the duties of citizenship.

The book before us, one of a series edited by Mr. Eric Wood, is divided between scout-craft and Nature-craft, the idea being to convey to the boy in a clear and simple manner many of those things which he most wishes to know. The scout-craft section appears to us most admirable and should be a mine of information to many a boy who is unable to join an actual scout troop. The Nature-craft section consists of an excellent chapter on bird study and similar chapters packed with information about the insect world. Boys upon whom we have tried the test find it altogether admirable.

A Text-book of Quantitative Chemical Analysis. By Dr. A. C. Cumming and Dr. S. A. Kay. Fourth edition. Pp. xv+432. (London: Gurney and Jackson; Edinburgh: Oliver and Boyd, 1922.) 15*s.* net.

THE first edition of this book was published in 1913, and the appearance of the fourth edition less than ten years later shows that it has been found in practice a most useful guide to students. The present volume should provide a sound course of quantitative analysis for students in universities and technical schools. It is very practical, and gives many hints to students which will save the time of teachers. The reduction method with Devarda's alloy might have been given for the estimation of nitrates, instead of the one with reduced iron, which is less satisfactory. In the description of the Lunge nitrometer no mention is made of the important correction for the solubility of nitric oxide in the acid. The directions for the preparation of cupferron reagent on p. 410 will be found useful, as the price charged for this substance is almost prohibitive.

Group Psychology and the Analysis of the Ego. By Dr. Sigm. Freud. Authorised translation by James Strachey. (The International Psycho-Analytical Library, No. 6.) Pp. v+134. (London: G. Allen and Unwin, Ltd., 1922.) 7*s.* 6*d.* net.

A GOOD and clear translation of Freud's short essay on group psychology is given by Mr. Strachey. The work begins by a brief examination of the views of earlier writers, particularly of Le Bon and M'Dougall. Freud's own method of approach to social psychology is naturally by way of an analysis of the motives of individual behaviour. He treats the group as a collection of persons bound together by some form of love relationship, and to the formation of the group ascribes what to many will appear to be an overweighted importance to the leader. His discussions of the phenomena of "identification," and of the relations of "being in love and hypnosis," are interesting in themselves; but his application of the results of his discussions to the explanation of social behaviour is not convincing.

Elementary Organic Chemistry. By W. H. Barrett. Pp. 256. (Oxford: Clarendon Press; London: Oxford University Press, 1922.) 4*s.* 6*d.* net.

DURING the last two or three years a number of elementary books on organic chemistry have appeared, and it may be doubted whether any purpose is served by further multiplication of the same material treated in the same way. The present volume has no very new features, but it gives a very clear and interesting account of the fundamental facts and theories of organic chemistry suitable for students preparing for scholarships at the universities. It also provides a course suitable for those beginning the subject in the universities, and for medical students. Experiments are included. The section on stereochemistry is particularly good, and a chapter is devoted to general methods of synthesis and analysis. The very moderate price of the book and its undoubted merit should make it popular.

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 Optical Spectrum of Hafnium.

DURING the progress of the work of Coster and Hevesy on the concentration and isolation of the new element hafnium (atomic number 72), the discovery of which was announced in NATURE of January 20, p. 79, we have examined spectroscopically a large number of their preparations in order to establish the optical spectrum of hafnium, and at the same time to assist in the chemical work on its isolation. In all our exposures we have for the sake of comparison also photographed the spectrum of a specimen of very pure zirconium prepared by Coster and Hevesy from commercial zirconium by removing the hafnium content.

The spectra were photographed with a Hilger quartz spectrograph of largest size, and in our preliminary work we have confined ourselves to the spectral region between 2500-3500 Å.U., which could be exposed in a single setting of the spectrograph. The spectra were produced in an ordinary carbon arc, the salts being placed on the cathode. The lines which are given in the table below as the most prominent hafnium lines in the region mentioned, are all lines which were not visible in an intense spectrum of the purified zirconium, while their intensity increased gradually in the preparations which by X-ray analysis were found to contain hafnium in increasing amounts. In the last specimens prepared by Coster and Hevesy, and estimated to contain about 90 per cent. hafnium, the lines ascribed to hafnium were among the most intense lines in the spectrum. In the table is given the wave-length λ in international Å.U. in air, measured against iron normals, and an estimation of the relative intensity I in the usual scale (strongest lines denoted by 6).

λ.	I.	λ.	I.	λ.	I.	λ.	I.
2559·05	3	2845·75	5	2954·20	5	3181·00	3
2637·00	4	2851·00	4½	2964·85*	5	3189·65	2½
2638·70*	4	2866·35*	6	3016·65	5	3206·10	3
2668·25	3	2887·15	4	3018·25	4½	3249·70	3½
2705·60	5	2889·60	5	3050·75	4	3291·10	3
2713·80	4	2898·30*	6	3056·95	4½	3309·55	2½
2718·50	4	2904·40*	4	3072·90	5	3310·35	4
2761·65	6	2904·75*	4	3080·80	4	3312·82	5
2766·90	3½	2916·50*	6	3097·75	3	3332·70	5
2773·05	4	2918·50	4	3156·65	4	3358·90	3
2779·35	4	2924·55*	3	3159·80	4	3373·95	2
2817·70	3	2929·90	4	3162·60	4½	3472·45	4
2833·30	3	2940·80*	6	3172·95	5	3497·40	4½

We have examined the hafnium preparations for the presence of the lines belonging to the characteristic spectrum ascribed by Urbain (*Comptes rendus*, t. 152, 1911, p. 141) to an element celtium belonging to the family of rare earths, and the discovery of which was announced by him several years ago. By Dauvillier and Urbain (*Comptes rendus*, t. 174, 1922, pp. 1347 and 1349; NATURE, February 17, p. 218) this element was assumed to possess the atomic number 72. Not the slightest trace, however, of any of Urbain's lines appeared on our plates. Although the minerals used as starting-point for the work of Coster and Hevesy contained rare earth elements in considerable amount, the only elements besides hafnium which could be detected spectroscopically in their preparations were zirconium and titanium. It is interesting to notice that some of the most prominent hafnium lines have been present as weak lines in zirconium spectra measured by earlier investigators. Thus Bachem (Diss., Bonn, 1910) states the presence in his zirconium spectrum of the lines marked in the above table with an asterisk, and in several places he states without giving any measurements the presence of weak lines, which probably are identical with other of our hafnium lines.

A fuller account of the hafnium spectrum, with measurements of the wave-lengths of the characteristic lines throughout the region which is obtainable photographically, will appear shortly.

H. M. HANSEN.
S. WERNER.
Universitetets Institut for teoretisk Fysik,
Copenhagen, February 23.

Echinoderm Larvæ and their Bearing on Classification.

THE object of my reply to Prof. MacBride (NATURE, December 16, 1922, p. 806) was not to discuss the classification of Asteroids, but to protest against the character of his unprovoked attack on me. An adequate discussion of the question which group of starfishes is the more primitive, the Phanerozonia or the Spinulosa, requires very much more space than that allotted to a correspondence in NATURE. What I wanted to prove—and, I think, did prove—was the want of foundation in Prof. MacBride's sweeping statement that all admit the Spinulosa to be the more primitive group, tending to represent my view as to this point as perfectly absurd.

Prof. MacBride now states (NATURE, January 13, p. 47) that in my original work I "forgot that the Brachiolaria larva was found in Spinulosa but referred it to Forcipulata only." It is difficult to understand how I could have forgotten this, seeing that I have myself reared the larva of *Asterina pectinifera* and found it to be a Brachiolaria; moreover, in the very place (p. 220) where I arrive at the objectionable conclusion that the Brachiolaria is a specialised, not a primitive larval form, I begin with this statement: "While it would thus appear to be a rule that the larvæ of the Phanerozonia have no Brachiolaria-stage, the facts known of the development of the Spinulosa and the Forcipulata (Cryptozonia) seem to indicate that their larvæ are characteristic through having a Brachiolaria-stage." Is it too much to ask that, before thus criticising my work and accusing me of omissions, of which I am not guilty, or of absurd opinions (*e.g.* of regarding the metamorphosis of Echinoderms as metagenesis), which I have never set forth, Prof. MacBride would, at least, read the questionable paragraphs in that work? I have never stated that the case of the regenerating larva, *Ophiopluteus opulentus*, even if it undergoes complete metamorphosis a second time, must alter our views as to the signification of Echinoderm larvæ in general, only that this would represent a quite exceptional and unique case of metagenesis among Echinoderms.

Regarding the classification of Asteroids I will say only that the physiological and anatomical reasons given by Prof. MacBride for regarding the Astropectinids "as Asteroids secondarily modified for a life on sand" would scarcely be accepted as a sound basis for classification by any modern specialist on Asteroids, those "students of the external features of preserved specimens only," as Prof. MacBride rather contemptuously characterises them. May I only direct Prof. MacBride's attention to the fact that numerous Astropectinids live exclusively on a muddy bottom, and also that numerous Spinulosa and Forcipulata live on a sandy or muddy bottom.

Prof. MacBride states that my appeal to Dr. Bather's reply is quite mistaken, because I forget that "what Dr. Bather objected to was my [Prof. MacBride's] fathering of Dr. Mortensen's views on him." May I only quote the following sentence from Dr. Bather's reply: "It is not for me to break any lances in defence of Dr. Mortensen, but if Prof. MacBride is acquainted with Dr. Mortensen's 'Studies in the Development of Crinoids' . . . I am rather astonished that he should so belittle our Danish colleague's work on those lines." If this sentence is meant by Dr. Bather to express his substantial agreement with Prof. MacBride in their views on Echinoderms or to repudiate Prof. MacBride's fathering of my views on him, I am very sorry that I shall have to moderate very considerably the admiration which I have always had for his lucid way of expressing his opinions.

To Prof. Gemmill's remarks (NATURE, January 13, p. 47) I must reply very decidedly that I am not narrowing down the Phanerozonia to include only the family Astropectinidæ. The families Luidiidæ, Archasteridæ, and Goniasteridæ, at least, are likewise typical Phanerozonia. On the other hand, the position of the Asterinidæ and the Gymnasteridæ is just one of the weak points in the classification of Asteroids, and the latter can by no means be said to be "frankly Phanerozonate."

The conclusion that, since the larvæ of the two families, Astropectinidæ and Luidiidæ (not of the Astropectinidæ alone as Prof. Gemmill states, by inadvertence, of course), regarded (by most specialists on Asteroids) as the more primitive forms, have no sucking disc, the existence of such a disc in the larvæ of those groups regarded (by most specialists on Asteroids) as more specialised types, is a secondary adaption, may, possibly, not be "inevitable"; but, in any case, this conclusion is not illogical or absurd. I have no direct interest in maintaining the Brachiolaria to be a secondarily specialised larval type. If conclusive proof is given that the Brachiolaria is the primitive, the Astropectinid-larva the specialised form, I shall not hesitate to drop my present view. But I must maintain that this view is not unjustified by the facts so far known.

I am sure Prof. Gemmill will agree with me as to the desirability of researches on the development and metamorphosis (and, not least, the postembryonal development) of many more forms than those few, which have been studied up to now. Not even the development and metamorphosis of Asteropecten has been studied by means of modern methods, the researches of Joh. Müller and Metchnikoff still remaining the only base of our knowledge of this subject. I hope very sincerely that Prof. Gemmill will extend his admirable studies to this and many other Asteroids, as I also hope that both he and Prof. MacBride will agree that my efforts to widen our knowledge of the development of Echinoderms are not entirely without value, and that the views expressed in my work, however much they may disagree in them, are not entirely without reasonable foundation.

TH. MORTENSEN.

Zoological Museum, Copenhagen,
January 22.

I SHALL summarise the points at issue between Dr. Mortensen and myself as briefly as possible.

He complains that I made an "unprovoked personal attack" on him. Nothing was further from my intentions. The so-called attack was a criticism of certain views attributed to Dr. Mortensen by Dr. Bather in a review of one of Dr. Mortensen's recent works in NATURE. Dr. Bather seemed to think that Dr. Mortensen believed that after all the development of Echinoderms might be an alternation of generations as Johannes Müller originally suggested. As Dr. Mortensen has unreservedly repudiated this view there is nothing more to be said on this point.

But Dr. Mortensen did say that the fixed stage in the development of Asteroids (discovered by me in 1893) was of secondary character, because it was absent in two families (Astropectinidæ and Luidiidæ) classed together as Paxillosa. I had a perfect right to comment severely on statements such as these, because (1) the fixed stage is found in the most widely diverse families belonging to two of the great primary divisions of Asteroidea. (2) The fixed stage regarded as an ancestral reminiscence, enables us to understand how and why the ancestors of Asteroidea passed from the stage of free-swimming bilaterally symmetrical animals to the stage of radially symmetrical forms creeping over the bottom. (3) If Dr. Mortensen had known what he as a specialist in Echinoderms might reasonably be expected to know, namely, what has been determined as to the physiology and habits of Luidia and Astropecten, he could never have regarded them as primitive, but would have recognised them as what they are, the most specialised of all Asteroidea.

It is not a question of the ground on which particular starfish can be dredged up. Every marine biologist knows that sporadic individuals of rock and gravel-inhabiting species can be dredged on sand or mud. The dredge, indeed, gives no precise information as to the habitat of a species, for the bottom is usually "patchy." But Luidia and Astropecten *when observed in life* are found to be *burrowing species*, which when at rest are almost completely buried in the sand or mud in which they live like many Ophiuroids, and the structure of the arms is modified in relation to such habits. A fixed stage in the ontogeny of such forms would be an impossibility, for in such an environment the larva would find nothing to which it could attach itself. By a happy coincidence I received a few days ago Part V. of W. K. Spencer's "Palæozoic Asterozoa." In this work I read "The existence of large marginals throws no light on the affinity of extinct species, but it does throw light on the shape of the arm" (*i.e.* it is adaptive). When the arm is flat and the dorsal skeleton reduced to a flexible membrane the borders of the arms must be strengthened.

Dr. Mortensen accused me of referring contemptuously to certain specialist students of external features only. I am afraid I must plead guilty on this count. I have spent weary time in going through the ponderous works of Sladen and Ludwig, and so far as any attempt to correlate structure with function is concerned, these authors might just as well have been describing postage stamps as Asteroidea. The distinction between Phanerozonia and Cryptozonia was made by Sladen. The Phanerozonia were stated by him to be the original and primitive group (*a*) because fossil starfish were all phanerozonate, (*b*) because cryptozonate forms when young are phanerozonate.

I have never been able to find the evidence on which (*b*) is based. I have often seen young imagines of Asterias and Asterina, but there is certainly nothing "phanerozonate" in their appearance. Statement (*a*) is absolutely inaccurate. If Dr. Mortensen is open to conviction on this point let him study W. K. Spencer's monograph, where he will find every fossil form from Palæozoic strata carefully described, and further, an attempt made to correlate its structure with its probable habits. He will learn that Cryptozonia are just as old as Phanerozonia, and that the oldest starfish of all are neither Cryptozonate nor Phanerozonate and have no plates corresponding to the marginals of Astropecten at all.

Lastly, as to the accusation that I belittled Dr. Mortensen's work. I have no wish to depreciate his work, but with the exception of the treatise on the Crinoids (Comatulidæ), which is irrelevant to the point at issue, it is not what I regard as embryology at all. By that term I understand the attempt to follow through the development of the organs of the adult from their beginnings in the embryo or larva with a view of obtaining light on the ancestry of species. Dr. Mortensen's researches on Echinoderm larvæ have been purely classificatory, and he has done valuable work in determining which larval forms belong to certain adults. His recent work on Comatulidæ, it is true, is embryological: he has confirmed the results of Bury and extended them to other species; but he dismisses the conclusions of other embryologists without sufficient consideration of facts.

E. W. MACBRIDE.

Imperial College of Science and Technology,
South Kensington, London, S.W.,
February 2.

Medical Education.

I AM sorry Prof. Dakin (NATURE, February 3, p. 151) should think my letter (NATURE, January 13, p. 50) merely an attempt to open another discussion on evolution. I do not know how I could have expressed myself more clearly. Manifestly, a knowledge of such things as the anatomy of frogs and dog-fish cannot persist in the minds of medical students or be useful to them intellectually or professionally unless linked with other studies. They can be so linked only through truths about development, variation, heredity, and evolution. But here the naturalist is in conflict with the physiologists, psychologists, pathologists, and medical men into whose hands the students pass and whose opinions, abundantly supported by evidence, they always adopt.

It is one thing to demonstrate that evolution has occurred, and for this none are better qualified than naturalists. But evolution is not disputed by medical men, though, owing to the biology they learn, few give it a thought. It is quite another thing to demonstrate the method of evolution. Here, to say the least, naturalists are not in a position to ignore evidence derived from other sciences. For example, they have very diverse opinions about fluctuations. But did ever a naturalist see a fluctuation in a living being existing under natural conditions, other than man (*e.g.* a sparrow or house-fly, the most familiar of all)? If he did, was he able to follow that being throughout its career and test the influence of that fluctuation on its life? If he did, was he able to note the effect on offspring and descendants?

Very obviously our whole *immediate* knowledge of fluctuations, natural selection, and the method of evolution is derived from human beings, among whom alone we are able to observe with that sufficiency and minuteness which extreme familiarity confers. Thus every man (not only Prof. Dakin's superman) knows that powers of resisting the carnivorous bacillus of tuberculosis occur in all shades between wide extremes (therefore they are fluctuations); that they tend to run in families (therefore they are inheritable); that the less resistant tend to perish and the more resistant to survive (therefore there is natural selection); that every race is resistant n proportion to the length and severity of its sufferings (therefore natural selection is the antecedent of evolution); and that what is true of tuberculosis is true also of every lethal and prevalent disease (therefore the instances are in thousands and include all the world and all humanity—indeed every case in which we are able to observe closely).

Naturalists, unable to observe either fluctuations or natural selection among plants and lower animals, must get their ideas about the method of evolution either from observations on man or else through mere guessing. Apparently they prefer not only to guess but to claim scientific status for their guesses. After all, man is an animal. I do not know why he should be thought unworthy of study.

What does Prof. Dakin mean by "it is highly desirable that first-year medicals, raw youths from school, should make their first acquaintance with the animal world through less expensive material than human bodies"? Expensive in what—money or time? Does Prof. Dakin suppose that raw youths dissect fewer humans because they dissect more frogs?

I gather that he disapproves of attempts by me to discuss evolution, "for his letter indicates a very imperfect acquaintance with biologists and their work." I think by biologists he means zoologists and botanists. But, if I be incapable, why not end the nuisance by indicating my errors. A jury always grows suspicious when not the evidence, but only the opposing attorney is attacked. There has been much of this hinting at my ignorance—doubtless with reason, if not with proof. Nevertheless, I know some elementary facts which, it seems, are outside the range of the average naturalist, *e.g.*, that events do not happen (characters do not develop) without antecedents (nature) and exciting causes (nurture); that living beings are bundles of adaptations; that the multicellular organism springs from a germ in which are none of the characters it afterwards develops, and therefore, inherits nothing but its nature (the sum of its potentialities for development), and develops nothing except in response to nurture; that our powers of observation are proportionate to our familiarity with the objects of study; that whenever we are able to observe sufficiently closely we always find natural selection in full swing; that the variations selected by Nature are always fluctuations; that the result is always adaptive evolution; that man, unlike Nature, frequently selects mutations; that therein lies the difference between natural and artificial selection; and so on.

On these elementary facts I have founded some equally elementary questions. Why are some characters supposed to be more innate, or acquired, or inheritable than others? What precisely was the great Lamarckian controversy about? Was it founded on anything but a play on words? Why is the word inherit used with two directly contrary meanings? What is meant by the statement that "Nature is five perhaps ten times stronger than nurture," and what by the statement that "mutations, but not fluctuations, have their representatives in the germplasm"? Why, in the face of enormously massive evidence, is it supposed that there is no natural selection, or that natural selection is merely a preserver, not a creator, of adaptations? Why in the face of equally massive evidence is it maintained that the inheritance, not merely the reproduction, of mutations is independent? And so on. I notice that the erudite people who are so ready to proclaim my ignorance are not equally ready to face these facts and answer these questions. Nevertheless, both facts and questions, however elementary, are fundamental. Unless they be met, posterity will regard a page of Darwin, who always met his difficulties with candour and without arrogance, of more value than all the thousand publications of those who bark at the dead lion.

As Prof. Dakin has been good enough to suggest that I am ignorant, may I supply him with proof and the readers of NATURE with a test? I do not know what naturalists (the biologists of Prof. Dakin) mean by their key-words "innate," "acquired," and "inherited" when applied to characters. Does Prof. Dakin know? Will he tell us?

G. ARCHDALL REID.

Southsea, February 19.

A Relativity-predicted Mechanical Effect in the Electromagnetic Field.

THE present writer would certainly starve if his bread depended on supplying a certain experimental verification here asked for. It should, however, be mere Boys' play to those who measure the gravitational constant with a little pile of sovereigns and a quartz fibre, or who photograph the wake of a flying bullet. The mathematical argument leading to the prediction indicated below is sent to England by the mail carrying this letter, for publication, but I cannot say where and when it will appear.

A body, say a crystal, at rest in an electromagnetic field should experience a force per unit volume, in Maxwell's notation and in E.M. units, equal in magnitude and direction to

$$\text{VKB} - \text{ES}\nabla\text{D} + \frac{d}{dt}\text{V}(\text{DB} - \text{EH}/4\pi c^2),$$

where **K** is conduction current and c the velocity of light *in vacuo*. It is possible that the third term has been given before, but I have not seen it anywhere. The verification here asked for is that of the existence of this term. **VEH** is in the direction of a light ray and **VDB** is normal to the corresponding front. In an isotropic transparent body, $\text{VDB} = k^2(\text{VEH}/4\pi c^2)$, where k is the index of refraction.

Unfortunately, $4\pi c^2$ is about $10^{22} \times 1{\cdot}131$, but my son, Dr. A. L. M'Aulay, tells me that the magnitude of **VEH** may readily be made equal to 10^{16}, so that the effect may be detectable.

The term indicates that when a wave-train traverses a point the matter at the point is always urged along the ray *towards* the nearest wave-crest or wave-trough, and normal to the front *from* the nearest crest or trough. Can any reader suggest a plausible physical reason why this should occur?

I may remark that Maxwell's expression for the force per unit volume is

$$\text{V}(\text{K} + d\text{D}/dt)\text{B} - \text{ES}\nabla\text{D},$$

and that probably most relativists would drop the $d\text{D}/dt$ from this expression. Let the physicist tell us which, if any, of the three expressions is verified experimentally.

ALEX. M'AULAY.

University of Tasmania, November 28, 1922.

The Measurement of the Rates of Oxidation and Reduction of Hæmoglobin.

WE have recently been engaged on the determination of the velocities of the chemical reactions of hæmoglobin. These are of interest both to the physiologist because of the important part played by this pigment in respiration, and also to the physical chemist because this pigment is an almost unique example of a large complex protein molecule which combines with gases in a simple chemical manner. Some of the results that we have obtained and the methods we have used may therefore be of interest to readers of NATURE.

In order to measure the rate of reduction two solutions were prepared: (*a*) a 1·5 per cent. solution of whole blood in tap water, (*b*) a solution of sodium hyposulphite ($Na_2S_2O_4$) in tap water which was rendered neutral to brom-thymol-blue by the addition of sodium carbonate solution. These two solutions were by suitable means forced under a pressure of, roughly, 500 mm. of mercury into the mixing chamber of the measuring apparatus through conical jets of small bore, so that the two solutions underwent vortex motion at a high rate of speed. Preliminary tests of the measuring apparatus, using as fluids a sodium hydroxide solution containing phenol phthalein and a rather stronger solution of acid, showed that mixing and chemical combination were complete with one measuring apparatus in less than 0·0055 sec., and with another apparatus in less than 0·0005 sec. The mixed blood solution and reducing agent passed from the mixing chamber of the apparatus in use down a glass tube with known velocity, being examined at different positions by means of the reversion spectroscope, by which we could ascertain the ratios of those amounts of hæmoglobin still combined with oxygen and those in the reduced state.

We thus obtained the concentration of oxyhæmoglobin (O_2Hb) at a series of instants, the intervals between which could be readily obtained from the rate of linear flow of the solution down the tube, and the positions of the points examined by the spectroscope.

Experiments on the rate of reduction of oxyhæmoglobin (O_2Hb) by the reducing agent ($Na_2S_2O_4$) have shown that with increase of concentration of the latter the rate of reduction increases to a maximum, beyond which it cannot be raised by a further increase. This we take to mean that the process consists of two stages:

(1) Reduction of oxyhæmoglobin, *i.e.*

$$O_2Hb \rightarrow O_2 + Hb.$$

(2) Removal of O_2 (liberated from O_2Hb) by combination with the reducing agent.

As the concentration of the reducing agent is increased, the free oxygen formed from O_2Hb by stage 1 is removed more quickly, until a concentration is reached at which the "free" oxygen is removed so quickly that the reaction $O_2Hb \rightarrow O_2 + Hb$ is not appreciably opposed by the reverse reaction $O_2 + Hb \rightarrow O_2Hb$. Further increase in concentration of the reducing agent cannot therefore further accelerate the velocity of the reduction of the O_2Hb, the latter being now solely determined by the velocity of the reaction $O_2Hb \rightarrow O_2 + Hb$. We have other evidence in support of this view, which we hope to present at length elsewhere. The time taken for complete reduction of O_2Hb when the concentration of $Na_2S_2O_4$ was sufficient to secure the "maximum" rate of reduction was about 0·5 sec. at 12° C. This rate of reduction is such as to be a factor of importance in considering the conditions which determine the rate of uptake of oxygen by organs within the body. We found further that the logarithm of the concentration of O_2Hb when plotted against time gave a straight line relationship, as should indeed be the case if the reduction of O_2Hb is a monomolecular process.

The measurements of the velocity of oxidation of hæmoglobin required the preparation of large quantities of reduced hæmoglobin solution. This was obtained by spraying a solution of blood in tap water heated to 50° C. into a large vacuous container, thus causing the gases combined with the hæmoglobin to be liberated. This reduced blood solution was mixed with water containing dissolved oxygen by forcing both fluids into the mixing chamber of one of the observing apparatus described above. It was found that the combination was a very rapid one, the reaction being complete in one hundredth part of a second at 10° C. At body temperature it is probable that the velocity would be even higher. This gives some idea of the intense rapidity with which oxygen entering the blood, as the latter passes through the lungs, becomes chemically combined with hæmoglobin. It seems to us possible that similar methods might be useful for determining the velocity of other rapid chemical reactions.

H. HARTRIDGE.
F. J. W. ROUGHTON.
Physiology Laboratory, Cambridge,
February 7.

Stages of Golgi Bodies in Protozoa.

IN the *Anatomischer Anzeiger* (47 Band, 1914) Jan Hirschler, in his paper "Ueber Plasmastrukturen in den Tunicaten-, Spongien-, und Protozoenzellen," gives a description of the trophozoite of *Monocystis ascidiæ*, in which he figures Golgi bodies. This has never hitherto been confirmed, nor are any other stages known.

For some time we have been carrying out work on an Adelea, and after considerable difficulty succeeded in getting excellent preparations of the Golgi apparatus in many stages of the life cycle. In the accompanying illustration (Fig. 1) is the young

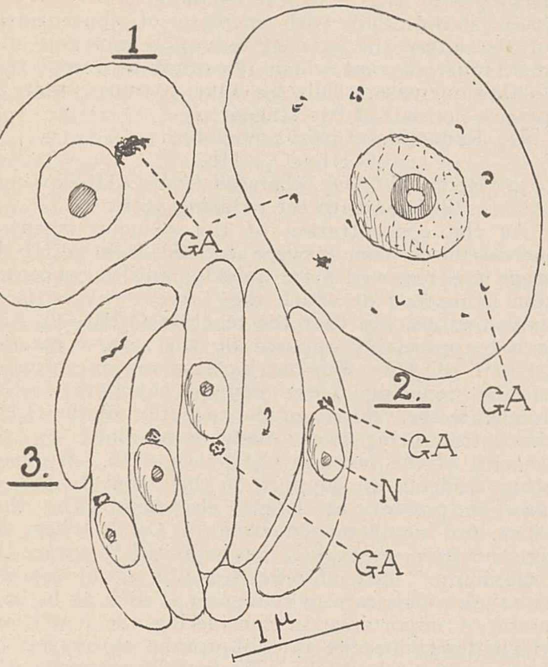

FIG. 1.

trophozoite showing an excentric and juxta-nuclear apparatus (GA); 2, the older trophozoite has a scattered apparatus, and in the "corps en barillet" stage in 3 the apparatus in each cell is again juxta-nuclear and excentric. The work is still proceeding in several species, and this is merely a preliminary announcement.

S. D. KING.
J. BRONTË GATENBY.
Zoological Department,
Trinity College, Dublin.

Selective Interruption of Molecular Oscillation.

MR. FAIRBOURNE (NATURE, February 3, p. 149) has reopened a subject which I believed was certainly closed; but since the fallacy is practically the same as before, though a little less easy to detect, I feel that I cannot then have been sufficiently clear for him.

None of Mr. Fairbourne's arguments has yet disposed of the validity of the ordinary treatment to be found in any text-book on the kinetic theory; for the elementary kinetic treatment of gaseous pressure is independent of the diameters of the gas-molecules, and would be perfectly valid if they were, as for the first approximation they are assumed to be, particles of a finite mass but zero radius; in this case, however, the mean free path would be infinite at every pressure, so that Mr. Fairbourne has introduced no new factor by confining himself to the case of long free paths.

This being so, it is not to be expected that space can be found in these columns for a disproof of whatever inadequate alternative to the accepted methods of analysis may be brought up; but I suggest that in this particular case he has not proved that he has satisfied a condition which he admits is vital, namely, that the numbers of molecules crossing XY and AD *in unit time* must be shown to be not proportional to their lengths. Many of the "superfluous" molecules which ultimately cross XY spend first a long time in the cone; there is nothing in his treatment which prevents such molecules being counted a very large number of times, since all points on their long paths may equally be taken as centres of small spheres O. Mr. Fairbourne's treatment is inconvenient; but it is obvious, since it does not discount the classical method, that, if carried out correctly, even it would have given the classical result.

I have always maintained that the length of the mean free path is irrelevant; I observe that he now admits this ("Subsequent intermolecular collision in the cone cannot destroy the excessive downward bias," etc.). The inevitable conclusion, as I pointed out last July, is that the pressure is without any influence except on the *magnitude* of the effect. It being granted that the molecules do not interfere with one another in any relevant way, the effect must be directly proportional to their number, *i.e.* to the total pressure. At atmospheric pressure, therefore, perpetual motion should be an accepted phenomenon even if the effect were measurable only with ambiguity at the pressures used by Mr. Fairbourne.

R. D'E. ATKINSON.
Hertford College, Oxford,
February 13.

A Biochemical Discovery of the Ancient Babylonians.

AT a lecture given recently in Cambridge by Prof. Okey my attention was directed to a passage written by Galileo in 1623 in which this pioneer of scientific method attacks the doctrines of the classical philosophers with his usual irony and vehemence. I refer to a section of his "Il Saggiatore," in which Galileo replies to his contemporary Sarsi, who had quoted Suida to the effect that the Babylonians used to cook eggs in an emergency and when no fire was available, by rapidly whirling them in slings. ("Babylonii iniecta in fundas ova in orbem circumagentes, rudis et venatorii victus non ignari, sed iis rationibus quas solitudo postulat exercitati 'etiam crudum ovum impetu illo coxerunt.' " Suida, Lessicografo. Bizant., x.)

Galileo makes the following caustic comments: " Se il Sarsi vuole che io creda a Suida, che i Babilonii cocesser l' uova col girarle velocemente nella fionda, io lo crederò; ma dirò bene, la cagione di tale effetto esser lontanissima da quella che gli viene attribuita, e per trovar la vera io discorrerò cosi: Se a noi non succede un effetto che ad altri altra volta a riuscito è necessario che noi nel nostro operare manchiame di quello che fu causa della riuscita di esso effetto, e che, non mancando a noi altro che una cosa sola, questa sola causa sia la vera causa. Ora, a noi non mancano uova, nè fionde, nè uomini robusti che le girino; e pur non si cuocono; anzi, se fusser calde, si raffreddano più presto; e perchè non ci manca altro che l' esser di Babilonia, adunque l' esser Babiloni è causa dell' indurirsi l' uova e non l' attrizion dell' aria: che è quelo ch' io volevo provare." (If Sarsi *commands* me to believe on the authority of Suida that the Babylonians used to cook eggs by swiftly swinging them in slings—I will. But I will certainly say that the cause of such results is far from that which he attributes; and in order to discover the true cause I will reason in the following way: If we do not succeed in obtaining a result which was successfully obtained at another time, some one factor at least must be lacking which is necessary for the successful production of the result. Now, we have no lack of eggs, nor slings, nor strong men to swing them, *and yet they do not cook; on the contrary*, if already warmed the swinging would cool them more quickly. Since the only factor that is lacking is that we are not Babylonians, therefore the fact of being a Babylonian is the cause of the eggs solidifying, and not the friction of the air: and this is what I set out to prove.) (Galilei, Opere, vi. Also in "Frammenti e Lettere" (1917), p. 66).

If Galileo had actually put his experiment to the test he might have written otherwise. Within the last few years it has been "discovered" that egg white under mechanical strain such as *vigorous shaking* or very high hydrostatic pressure undergoes coagulation (*vide* Robertson, "Physical Chemistry of the Proteins," 1918). In a paper to be published shortly in the Proc. Roy. Soc. (read at the meeting of February 15), I show that chemical changes which occur on heat-coagulation also occur on coagulating an egg by mechanical means.

The myths and anecdotes of the ancients are almost invariably built on some foundation of fact; and it seems highly probable that the Babylonians were aware that eggs could be coagulated by vigorous movement (such as swinging in slings). If this be so, the phenomenon of mechanical coagulation proves to be another example of a former observation rediscovered—in this case after the lapse of thousands of years!

LESLIE J. HARRIS.

Emmanuel College, Cambridge,
February 16.

Use of Yeast Extracts in Diabetes.

WE have recently shown (*Journ. of Physiol.* 57, p. 100, 1922) that there is present in the blood of normal persons a sugar of a reactive nature, which gives the same osazone as glucose, but has a lower rotatory power. In the blood of persons suffering from severe diabetes mellitus, this sugar is not present in amounts capable of detection. In conjunction with Dr. Devereux-Forrest, we have found that, after administration of insulin to diabetic persons, whereas the quantity of sugar in the blood is decreased, the amount of normal blood sugar is increased.

We have also shown (Proc. Physiol. Soc., December 16, 1922) that extracts of pancreas and liver together alter the rotatory powers of glucose and fructose *in vitro*. It was suggested that the absence or inactivation of either the pancreatic or liver factor was the cause of diabetes. Since the pancreatic and liver factors were markedly accelerated by the addition of phosphates, it seemed possible that one constituent of the pancreatic factor might be a sugar-phosphoric acid complex. As an essential step in the metabolism of sugar by yeast is held to be the formation of hexose-phosphoric acids, it seemed possible that an extract of yeast might take the place of the pancreatic factor.

We have obtained a solid preparation from yeast which would appear to have similar properties and effects.

When a solution of this substance is injected into rabbits, a very definite lowering of the blood sugar occurs, in every way comparable to that which we have found after injections of insulin. Rats when injected die in convulsions similar to those caused by insulin.

Some properties of insulin and of this extract of yeast are very similar. Both contain organic phosphorus and carbohydrate. Seliwanoff's reaction is positive in each case after hydrolysis.

We are at present engaged in a further investigation of these extracts.

L. B. WINTER.
W. SMITH.

Biochemical Laboratory, Cambridge,
February 16.

Meteorological Nomenclature and Physical Measurements.

IN reply to Sir Napier Shaw's kindly rejoinder in NATURE of February 17, p. 218, to my meditations on the progress of meteorology, I prefer to his simile of a boat-race that of boats striving to tow the not yet quite ship-shape bulk of meteorological research forward on its destined course. Although Sir Napier Shaw's was the best equipped of the boats, in which he was able to experiment with new modes of propulsion, I am sure he recognises that I was pulling with all my strength, if independently, at least in the same direction as himself. That I pulled in grim earnest with the result of long disablement accounts for my present position (which strikes me as more desolate than dignified) on the shelf, from which I see the now graceful lines of the new meteorology moving ahead with Sir Napier's new engines installed, and though almost out of hearing I listen to their beat in order to form an opinion as to how they act. I should be sorry indeed if anything I said were to retard or discourage any one on board that craft or cast a shadow on the laurels with which Sir Napier Shaw has been crowned by the scientific world to the joy and pride of every British meteorologist.

Dropping metaphor, there is surely large room for helpful difference of opinion as to terminology and the relative value of facts and formulæ. I do not dislike the metric system in spite of its occasional awkwardnesses, nor would I hesitate to embrace the millibar if it seemed to me to be making for unity instead of adding a new ramification to diversity. In the works I was reviewing I failed to see the signs of the coming of the millenium of the millibar; but if it is on its way, " come it will for a' that."

HUGH ROBERT MILL.

February 19.

The Origin or Basis of Wireless Communication.[1]

By Sir OLIVER LODGE, F.R.S.

FINSBURY Technical College has done splendid work throughout its short history. It fills a distinct niche, it supplies a felt want in the education of the Central metropolis, and I hope that any idea of closing it has now subsided. It has had, moreover, a brilliant array of teachers, men who appeared specially adapted to serve the needs of its special kind of students. I will here only mention three contemporaries who worked together after 1885, when the initial start had been made, and the early traditions settled, by Ayrton and Perry. Silvanus Thompson became principal in 1885, and had as his colleagues John Perry and Raphael Meldola. John Perry was remarkable as a teacher, and did his best to cultivate a wider interest in the rather narrow technically trained students who came under his paternal supervision, encouraging them to read novels, to take an interest in literature, and—even in mathematics—to take a broader outlook than most teachers thought it worth while to cultivate. As for Silvanus Thompson, the breadth of his outlook and width of his interests are almost proverbial. He represented a rare combination of scientific aptitude and high artistic faculty, together with a fondness for literary study among archives, and he became in the eyes of all his contemporaries—including Lord Kelvin and Lord Rayleigh—a recognised historian of science. He had a keen love of the past and of discoveries in their nascent stages. Old documents and records were of real interest to him: and he used to do his best to dig out of obscurity some of the pioneers and early workers towards developments which afterwards became famous.

Early pioneering work is too often overlooked and forgotten in the rush of a brilliant new generation, and amid the interest of fresh and surprising developments. The early stages of any discovery have, however, an interest and fascination of their own; and teachers would do well to immerse themselves in the atmosphere of those earlier times, in order to realise more clearly the difficulties which had to be overcome, and by what steps the new knowledge had to be dovetailed in with the old. Moreover, for beginners, the nascent stages of a discovery are sometimes more easily assimilated than the finished product. Beginners need not, indeed, be led through all the controversies which naturally accompany the introduction of anything new; but some familiarity with those controversies and discussions on the part of the teacher is desirable, if he is to apprehend the students' probable difficulties. For though he does not himself feel them now, the human race did feel them at the new fact's first introduction; and the individual is liable to recapitulate, or repeat quickly, the experience of the race.

A large number of people now interested in the most modern developments of wireless have but little idea—perhaps none at all—of the early work, in apparently diverse directions, which preceded and made such developments possible. Even those who are high authorities in wireless telegraphy, and know nearly all that can be known about it—like the distinguished dean of this college, Dr. Eccles—can scarcely know the early stages quite as well as Silvanus Thompson and I knew them; no one, indeed, can afterwards feel in touch with the history so closely as those who have lived through the period covered by it. Only those who have survived the puzzled and preliminary stages of a discovery can appreciate fully the contrast with subsequent enlightenment. It may suffice to say that the term "inductance" or "self-induction," which we now use so glibly, did not at first exist; and that so late as 1888 Sir William Preece still spoke of it as "a bug-a-boo": whereas it is the absolute essential to tuning, and even to electric oscillation. Faraday was the first to direct attention to it, under the name "electrotonic state;" and he treated it experimentally with his usual skill. Lord Kelvin, who first introduced it as a mathematical coefficient, without any explanation, called it "electrodynamic capacity." The name self-induction was given to it by Maxwell, though it was long before it was understood or utilised, and the name "inductance" is a nomenclature of Heaviside.[2]

I wish in this lecture to say practically nothing about anything to do with wireless later than 1896. What I have to deal with is the early pioneering work apart from practical developments. Let me here say at once, to avoid misunderstanding, that without the energy, ability, and enterprise of Signor Marconi, what is now called wireless would not have been established commercially, would not have covered the earth with its radio stations, and would not have taken the hold it has upon the public imagination. Before 1896 the public knew nothing of its possibilities: and for some time after 1896, in spite of the eloquence of Sir William Preece and the demonstrations by Signor Marconi, the public thought it mysterious and almost incredible; and still knew nothing about the early stages. Indeed, I scarcely suppose that Signor Marconi himself really knew very much about them. He had plenty to do with the present; he felt that the future was in his hands; and he could afford to overlook the past.

It may be doubted whether the younger generation, who are so enthusiastically utilising, and perhaps improving, the latest inventions, will care much about the past either. Incidentally, however, I want to say two things to those who are occupied with the subject to-day. First, do not hesitate to speak and think of the *ether of space*, as the continuous reality which connects us all up, and which welds not only us but all the planets into a coherent system. Do not be misled by any misapprehensions of the theory of Relativity into supposing that that theory dispenses with the ether, merely because it succeeds in ignoring it. You can ignore a thing without putting it out of existence: and the leaders in that theory are well aware that for anything like a physical explanation of light or electricity or magnetism or cohesion or gravitation, the ether is indispensable. The ether has all these functions, and many more. We are utilising it every day of our

[1] From the first Silvanus P. Thompson memorial lecture delivered at Finsbury Technical College on February 1.

[2] Silvanus Thompson wrote a pamphlet on the early history of wireless, in connexion with a successful application before Lord Parker for the extension of my fundamental tuning patent of 1897. This pamphlet has never been published, but it ought to be. I had not time to quote from it.

lives; and it would be ungrateful, as well as benighted, if we failed to render due homage to its omnipresent reality and highly efficient properties. It lies at the origin of all electrical developments, and forms the basis for this new and broadcast method of communication.

That is one thing: the second is to congratulate all those whose wonderful and rapid advances have rendered possible the astonishing feat of, in any sense and by whatever means, carrying the human voice across the Atlantic. When Signor Marconi succeeded in sending the letter "s" by Morse signals from Cornwall or Ireland to Newfoundland, it constituted an epoch in human history, on its physical side, and was itself an astonishing and remarkable feat. The present achievement of changing over from Morse signals to ordinary speech, made possible by the valves of Prof. Fleming and Dr. Lee de Forest and others, is a natural though still surprising outcome and development of long-distance transmission, and must lead to further advances, of which at present we can probably form but a very imperfect conception.

Well now, I must go back to very early times. In or about the year 1875 Mr. Edison observed something, which at that time could by no means be understood, about the possibility of drawing sparks from insulated objects in the neighbourhood of an electrical discharge. He did not pursue the matter, for the time was not ripe; but he called it "Etheric Force"—a name which rather perhaps set our teeth on edge;—and we none of us thought it of much importance. Silvanus Thompson, however, took up the matter in a half-hearted sort of way, and gave a demonstration to the Physical Society of London in, I believe, June 1876—a paper which I have had a little difficulty in finding in the Proceedings of that Society. Nothing much came of it, however, though his argument tended to show that the sparks could be accounted for on known principles. The value of this is merely that it must have rendered Thompson susceptible to methods of detecting real electric waves, when they were discovered later.

It was found afterwards that Joseph Henry, at the Smithsonian Institution in Washington, had observed something of the same kind so early as 1842. He seems to have had an intuition of the possible importance and far-reaching consequences of his observation, for he speaks as follows (I quote from a passage cited in my "Modern Views of Electricity," an appended lecture "On the Discharge of a Leyden Jar"[1]):—

"It would appear that a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacity, and . . . it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from flint and steel in the case of light."

That is to say, so early as 1842 Joseph Henry had the genius to surmise that there was some similarity between the etherial disturbance caused by the discharge of a conductor and the light emitted from an ordinary high temperature source.

In the light of our modern knowledge, and Clerk Maxwell's theory, we now know that the similarity is very near akin to identity. Both sources emit ether waves, though prodigiously differing in length.

Subsequent to these early stray observations, a suggestive semi-private observation, of a partially similar kind, was made by that singular genius and brilliant experimenter, David Hughes, the inventor of the microphone or telephonic transmitter, and of the Hughes printing telegraph still used in France. He was a man who "thought with his fingers," and worked with the simplest home-made apparatus—made of match-boxes and bits of wood and metal, stuck together with cobbler's wax and sealing-wax. Such a man, constantly working, is sure to come across phenomena inexplicable by orthodox science. As a matter of fact, Hughes unknowingly was very nearly on the trail of what was afterwards discovered, in a much more enlightened manner, by Hertz. Hughes, too, got sparks in the course of his experiments, but he also got something very like coherer action by means of his microphone detectors. They enabled him to get actual galvanometer deflexions—such as Hertz never got.

I cannot at the moment fix the date, but it was early in the 'eighties and before either Hertz or me. Hughes was a telegraphist, and though he would never have worked out the subject mathematically as Hertz did, and would not have been interested in matters of theory, he might well have stumbled, even at that early date, on something like a rudimentary system of wireless signalling, had he been encouraged. But he was not encouraged. He showed his results to that great and splendid mathematical physicist, Sir George Stokes; and Stokes, alas, turned them down, considering that they were explicable either by leakage or some other known kind of fact.

That is the danger of too great knowledge; it looks askance at anything lying beyond or beneath its extensive scope; whereas an experimenter operating at first hand on Nature may quite well occasionally stumble on a fact which lies outside the purview of contemporary science, and which accordingly neither he nor any one else at the time understands. Crookes himself had a similar experience. In his pertinacious and systematic way he explored many unfamiliar and untrodden regions; and he also invited the attention of Stokes to a simple and easily investigated case of abnormal movement; Stokes, however, perceiving that such motion was physically impossible, declined to take any interest in it or even to see it. His reason told him (and the reason he gave was) that on recognised principles the asserted phenomenon could not happen. But that was precisely its point of interest, and that was why Crookes with his instinctive sagacity conceived that such things held within them the germ of a great science of the future.

In Crookes's case the germ still remains unfructified by orthodox science. In Hughes's case the germ was rediscovered and has borne fruit a million-fold. But this is to anticipate. Suffice it now to direct attention to the collection of Hughes's apparatus now unearthed by the energy and piety of Mr. Campbell Swinton, and exhibited in the Science Museum at South Kensington. Let us try, however, to avoid imitating the mistakes of our revered scientific ancestors: though I admit it is a difficult task. So much rubbish is brought to our

[1] See also NATURE, vol. 39, pages 471-474.

notice that we are bound to run the risk of neglecting a jewel among the chaff.

These spasmodic observations, however, are not exactly discoveries: they were more akin to vague intuitions. The first and gigantic step in the real discovery was made by Clerk Maxwell, in or about 1865: and he made it in mathematical form, not in experimental actuality, by one of those superhuman achievements which are only possible to our greatest mathematical physicists. He did not discover either the way to generate ether waves, or to detect them; but he did give their laws: he legislated for them before they were born. He knew the velocity with which they must move, and gave implicitly, though without elaboration, the complete theory of their nature.

Up to his time the nature of light was unknown. All the other theories of light had attempted to explain it on mechanical principles, like the vibrations of an elastic solid. Light was known to consist of transverse waves: the wave-length and the frequency of oscillation could be determined. But no one knew what was oscillating, nor what the mechanism of propagation was. With extraordinary genius Fresnel and MacCullagh had explained the phenomena of light in all detail as regards reflection, refraction, diffraction, interference, and polarisation. But the nature of the waves was unknown; and the elastic solid theory, though fascinating, was felt by those who dived most deeply into it to contain some flaw, and to be, strictly speaking, unworkable. Light did not seem explicable on dynamical principles—the principles which were so fruitfully devised by Galileo and Newton for dealing with ordinary matter.

MacCullagh's theory indeed was not dynamical, and in that respect had some advantage. But it was also vaguer and less definite on that account; though, being thus indefinite and yet enabling results to be achieved, it was less liable to be upset and replaced by future discovery.

To Clerk Maxwell we owe the epoch-making discovery that light was not a mechanical oscillation at all, that the ordinary mechanical properties of matter did not apply to it, but that it was explicable solely and wholly in terms of electricity and magnetism. It is impossible to sum up his discovery in a few words; but roughly we may say that the most obvious outcome was:

(1) That if electric waves could ever be generated they would travel with the velocity of light.

(2) That light was essentially an electromagnetic and not a mechanical phenomenon.

(3) That the refractive index of a substance was intimately related to its dielectric coefficient.

(4) That conductors of electricity must be opaque to light.

Maxwell showed further, though he did not then express it in language of this character, that the ether had two great and characteristic constants, of value utterly unknown to this day, though guessed at by a few speculators like myself;—one of them the electric constant of Faraday called K; the other the magnetic constant of Kelvin called μ. It was impossible then, and it is impossible now—though it is not likely always to remain impossible—to determine the value or even the nature of either of these constants. But Maxwell did perceive a way of measuring their product; and he was the first to measure it. Their product is known; and it is equal—as he showed it must be—to the reciprocal of the square of the velocity of light.

Well now, this great discovery aroused in us young physicists the keenest enthusiasm. In the early seventies of last century—I think about 1871 or 1872—I remember discussing it with the man we all now know and honour, J. A. Fleming, who at that time was a fellow student with me in Prof. Frankland's advanced chemical laboratory at the brand-new College of Science, South Kensington. A year or two later, at Heidelberg, I studied Maxwell's treatise pretty thoroughly, and formed the desire to devote my life if possible to the production and detection of Maxwell's electric waves.

I used to discuss the possibility of producing these waves with my great friend, G. F. FitzGerald, whose acquaintance I made at the meeting of the British Association in Dublin in the year 1878; and he wrote some mathematical papers discussing the possibility of producing such waves experimentally. I myself also spoke at the British Association about them, in 1879, 1880, and again in 1882 at the Royal Dublin Society. FitzGerald, as I say, examined mathematically what then seemed the abstruse question of electric wave production; and after some hesitation came to the conclusion that direct artificial generation of waves was really possible on Maxwell's theory, in spite of certain recondite difficulties which at first led him to doubt it. (See "Scientific Writings" of FitzGerald, edited by Larmor, pp. 90-101.) Indeed one of his papers on the subject was originally entitled "On the Impossibility of Originating Wave Disturbances in the Ether by Means of Electric Forces." The prefix "im" was subsequently dropped; although his first, or 1897, paper concluded thus:

> "However these [displacement currents] may be produced, by any system of fixed or movable conductors charged in any way, and discharging themselves amongst one another, they will never be so distributed as to originate wave-disturbances propagated through space outside the system."

In 1882 FitzGerald corrected this erroneous conclusion, and referred to some early attempts of mine at producing the waves. ("Scientific Writings," p. 100.) I state all this in order to emphasise the difficulty which in those early days surrounded the subject on its theoretical as well as on its practical side.

In 1883, at the Southport meeting of the British Association, FitzGerald took a further step and surmised that one mode of attaining the desired result would be by utilising the oscillatory discharge of a Leyden jar—the theory of the oscillations of which had been worked out, partly by Helmholtz and more fully by Lord Kelvin, 30 years before—if only we had the means of detecting such waves when they were generated.

Production of Waves.

In 1887 and 1888 I was working at the oscillatory discharge of Leyden jars (initially in connexion with the phenomena of lightning), and—with the assistance of A. P. Chattock—I then found that the waves could be not only produced but also detected, and the wave-length measured, by getting them to go along guiding wires adjusted so as to be of the right length for sympathetic resonance. Thus I obtained the phenomenon of electric nodes and loops, due to the production of stationary waves by reflection at the distant end, and in my own mind thus verified Maxwell's theory. (I gave a brief account of this work, with calculations of wave-length, in *The Electrician* for September 21, 1888, page 623. Many other passages of early history can be found in the same volume about that date. It was an important year.)

Transmission along wires popularly sounds different from transmission in free space, but it was well known to me that the process was the same, and that the waves travel at the same speed, being only guided by the wires, much as sound is guided in a speaking-tube, without the velocity of transmission being to any important extent altered. The theory is given near the end of my paper—an important one as I think, and as Silvanus Thompson agreed—in the *Philosophical Magazine* for August 1888, where the experimental production of much shorter waves is also foreshadowed.

The beginning of my experiments was reported to the Society of Arts in April 1888; they are recorded, as said above, in the *Phil. Mag.*, and they were more completely described orally at the British Association at Bath that year. (See the *Electrician*, vol. 21, pp. 607-8, September 1888.)

In that year, also, I heard for the first time of Hertz's brilliant series of experiments, where, by the use of an open-circuit oscillator, he had obtained waves in free space, and by reflection had also converted them into stationary waves and observed the phenomena of nodes and loops, and measured the wave-length.

Attention was directed to these experiments of Hertz by FitzGerald in his presidential address to Section A of the British Association meeting at Bath in 1888. No wonder they interested him; for they showed that his method of utilising the oscillatory discharge of a Leyden jar was effective; and, to the surprise of all of us, including Hertz himself, that the waves from an opened-out condenser had sufficient power to generate sparks in an insulated conductor upon which they impinged; the detecting conductor, as generally used by Hertz, being in the form of a nearly closed circle with a minute spark gap at which the scintilla appeared. The radiating power of even a small Hertz oscillator was calculated by me in a subsequent paper (*Phil. Mag.* for July 1889, p. 54), and was found to be 100 horse-power, while it lasted. The duration was excessively short, for, at that rate, practically all the energy was expended in a single swing (about the 100-millionth of a second), but its power of producing little sparks was explained.

This work of Hertz was splendid. He was then professor at Carlsruhe, still quite a young man. He had been trained under Helmholtz; and I had made his personal acquaintance in Berlin when I went to call on Helmholtz in 1881, on a tour of the universities of the Continent. He was then Helmholtz's demonstrator, and was thought highly of by that great master. He could speak English, and was very friendly. I did not see him again till some time after the publication of his great discovery.

Hertz was not at that time fully acquainted with Maxwell's theory, although he knew his equations better than any other German except Helmholtz. Maxwell had not then made any serious impression on the Continent. Even Hertz does not seem at first fully to have realised what he was doing, and did not use the words "electric waves." That title was attached to his subsequently translated book at the suggestion of Lord Kelvin. He spoke about the *out-spreading of electric force*; somewhat as Joseph Henry had done. That was the title of his book. He worked out the phenomena he observed with extraordinary skill, both experimentally and mathematically, rapidly perceiving that Maxwell's theory could be applied to them, and that it might be elaborated in detail so as to include the whole of his phenomena. He it was who drew those accurate diagrams of the genesis of the waves, showing what is happening near the oscillator at every phase—diagrams which now appear in most text-books and of which the upper half is represented as scouring across the country. He knew that true waves were not emitted till beyond a quarter-wave length from the source. He knew how they were polarised, and how their intensity differed in the equatorial and polar directions, and how it varied with what may be called latitude. In fact he rapidly came to know all about these waves. As to us, we knew not which to admire most—his experimental skill when working with a tiresome and irritating mode of detection; or his mathematical thoroughness in ascertaining the laws of their propagation. A synopsis of his equations will be found clearly cited in Preston's "Theory of Light," as well as in other books. I translated some of his papers into NATURE. Never was there the smallest iota of jealousy between us, or anything but cordial and frank appreciation. Maxwell and Hertz are the essential founders of the whole system of wireless. That is to say, they constructed the foundations solidly and well. Of the super-structure—splendid as it is now—we are as yet far from seeing the completion.

In March 1889 I lectured to the Royal Institution on "The Oscillatory Discharge of a Leyden-jar," and incidentally exhibited many of the effects of waves, both on wires and in free space, with overflow and recoil effects. But there was nothing akin to *signalling* exhibited in this lecture, as there was in the subsequent lecture in 1894.

Nevertheless, Sir William Crookes, on the strength of these experiments—which he mentions—wrote a brilliant article in the *Fortnightly Review* for February 1892 (vol. 51, p. 173) in which he foreshadows actual telegraphic accomplishment by that means, and indicates also the possibility of tuning or selective telegraphy, which was not actually born till 1897. He is evidently impressed with the experiments both of Hertz and of myself, and he quotes from my *Phil. Mag.* paper of August 1888 in confirmation and illustration of his prevision. For he says—after speaking of choosing wave-length with which to signal to specific people—"This is no dream of a visionary philosopher. All the requisites needed to bring it within the grasp of daily life are well within the possibility of discovery, and are so reasonably and clearly in the path of researches now being actually prosecuted in every capital of Europe, that we may any day expect to hear they have emerged from the realm of speculation into that of sober fact." Then he goes on—evidently referring to the experiments of D. E. Hughes, at which he must have been present[3]—"Even now indeed telegraphy without wires is possible within a restricted radius of a few hundred yards, and some years ago I assisted at experiments where messages were transmitted from one part of a house to another, without any intervening wire, by almost the identical means here described."

That article appeared in 1892, and was an anticipation of genius. Too little appreciation is felt to-day for the brilliant surmises and careful and conscientious observations of a great experimental worker like William Crookes; and on some of his researches orthodox science still turns its weighty and respectable back.

OTHER METHODS OF DETECTING WAVES.

In 1889 I had come across the effect of cohesion under electric impetus, and employed it to ring a bell under the stimulus of the overflow of a Leyden jar, as described in my paper to the Institution of Electrical Engineers in 1890 (vol. xix. pp. 352-4, where D. E. Hughes's comment on it is also recorded). In 1893 I heard — through a demonstration by Dr. Dawson Turner at Edinburgh—of Branly's filings-tube—an independent discovery of M. Branly, which really constituted an improvement on the first rough coherer idea. What I had called a coherer was not this, but a needle-point arrangement, or the end of a spiral spring touching an aluminium plate, which was and is extremely sensitive, but rather unmanageable.

With a Branly's filings-tube I made many more experiments, developing the subject; and on the untimely death of Hertz I determined to raise a monument to his memory by a lecture at the Royal Institution on these experiments (Friday, June 1, 1894), which I styled "The Work of Hertz"—meaning that it was a direct outcome and development inspired by that work. I soon found that the title was misleading, so that in the next edition I changed it into "The Work of Hertz and some of his Successors," and afterwards changed it still further into "Signalling across Space without Wires"; for that, of course, is what was being done in laboratory fashion all the time. The depression of a key in one place produced a perceptible signal in another—usually the deflection of a spot of light—and, as I showed at Oxford, also in 1894, employing a Thomson marine speaking galvanometer lent me by Alexander Muirhead, a momentary depression of the key would produce a short signal, a continued depression a long signal;—thus giving an equivalent for the dots and dashes of the Morse code —if the filings-tube were associated with an automatic tapper-back. One form of such tapper-back was then and there exhibited—a trembler or vibrator being mounted on the stand of a receiving filings-tube. This was afterwards improved, with Mr. E. E. Robinson's help, into a rotating steel wheel dipping into oiled mercury. Our aim was to get signals on tape, with a siphon recorder, and not be satisfied with mere telephonic detection. We succeeded; but more rapid progress would have been made had we stuck to the telephone, as wiser people did.

[3] Colonel Crompton now tells me that the experiments to which Crookes was probably referring were conducted not by Hughes but by Willoughby Smith, who seems to have demonstrated that some sort of communication was possible in this way.

TELEGRAPHY 1894 TO 1896.

My Royal Institution (1894) lecture was heard by Dr. Muirhead, who immediately conceived the desire to apply it to practical telegraphy. When my lecture was published—as it was in the *Electrician*, with diagrams roughly depicting the apparatus shown, drawn (some of them) skilfully but not always quite correctly, by the then editor of the *Electrician*, Mr. W. H. Snell—it excited a good deal of interest; stimulating, to the best of my belief, Capt. (now Admiral Sir Henry) Jackson, Prof. Righi, and Admiral Popoff to their various experimental successes which have been elsewhere described.

I was too busy with teaching work to take up telegraphic or any other development; nor had I the foresight to perceive, what has turned out to be, its extraordinary importance to the Navy, the Merchant Service, and indeed Land and War service too. But fortunately in Italy there was a man of sufficient insight to perceive much of this, and with leisure to devote himself to its practical development. In 1896 Signor Marconi came to this country—and the rest is public knowledge.

Man and the Ice Age.[1]

By Prof. W. J. SOLLAS, F.R.S.

THE great advance recently made in our knowledge of the Quaternary epoch begins with the observations of General de Lamothe on the ancient shore-lines which run along the coast of Algeria at heights of about 100, 60, 30, and 20 metres above the existing sea-level. They maintain their course with such remarkable uniformity that M. de Lamothe was unable to regard them as due to elevation of the land, and consequently attributed them to changes in the level of the sea, and was thus led to predict that similar shore-lines would be discovered on the opposite coast of the Mediterranean and particularly in Provence; a prediction which was subsequently verified by Prof. Depéret.

Next Prof. Gignoux, a friend and former pupil of Prof. Depéret, made a detailed investigation of these shore-lines and their associated deposits in the Western Mediterranean, and embodied his results in a masterly monograph.

Finally, Prof. Depéret himself extended these investigations to the Eastern Mediterranean and the west coast of the North Atlantic Ocean. In a comprehensive review of the whole subject he proposed the following classification of the Quaternary deposits, based on the four marine terraces of de Lamothe.

1. SICILIAN (Döderlein). Coast-line at from 90 to 100 m. The most perfect example of this stage is afforded by the Conca d'Oro or basin of Palermo, an ancient bay of the Mediterranean now filled up with Quaternary deposits. They commence with a blue clay containing near its base the famous fauna of Ficarazzo, which points to cold conditions and a depth of 90 metres. Traced towards those localities where the sea was clearer, the clay passes into a Polyzoonal

[1] A lecture delivered to the Geological Society of London on January 10.

limestone resembling our Coralline crag, while towards the shore it becomes sandy. Ascending in the series, the sand increases and finally passes into conglomerates, which at the summit (90 m.) extend over a rocky platform bored by Lithodomus and encrusted with barnacles—to end against the foot of steep cliffs which are undercut and penetrated by sea caves.

The Sicilian stage is sharply marked off from the Calabrian (Upper Pliocene) by a stratigraphical unconformity and a fauna which is distinguished by the disappearance of many Pliocene mollusca, and the advent of many "cold" species from the North Atlantic. These were brought probably by a cold marine current. At Reggio the Sicilian terrace has yielded an entire skeleton of *Elephas antiquus*.

2. MILAZZIAN (Depéret). Coast-line at from 55 to 60 m. The deposits of this stage are chiefly littoral with a fauna indicating a temperate climate, but warmer than that of the existing Mediterranean.

3. TYRRHENIAN (Issel). Coast-line at 28 to 30 m. This includes the well-known Strombus beds (*Strombus bubonius*) which are found all round the Mediterranean. The fauna is characterised by "warm" species, such as still live off the coast of Senegal and the Canary Islands.

4. MONASTIRIAN (Depéret). Coast-line 18 to 20 m. This is named from the city of Monastir in Tunisia, adjacent to a locality very rich in fossils of the stage. The fauna is almost identical with the Tyrrhenian, but on the north coast of the Mediterranean contains no "warm" species.

THE FOUR CORRESPONDING RIVER TERRACES.

General de Lamothe has shown that the four Quaternary beaches or shore-lines of Algeria correspond with the four Quaternary terraces of the river Isser in Algeria, and Prof. Depéret has similarly identified the Quaternary river terraces of the Rhone with the ancient beaches of Provence.

Thus the river terraces were determined by the base level of erosion, *i.e.* in the first place by the position of the sea-level at the time of their formation.

They are thus liberated from their supposed dependence on the four glacial episodes of Prof. Penck. This distinguished investigator had, as is well known, attributed the transport of the material of which they consist to the action of the comparatively feeble rivers which issued from the moraines of the glaciers at their full extension during a glacial episode,—a view scarcely inconsistent with palæontological evidence.

Commont has correlated the four terraces of the Somme with those of the Rhine, and de Lamothe has correlated them with the four Quaternary sea-levels. But the three lower terraces of the Somme, *i.e.* the first or Monastirian, second or Tyrrhenian, and third or Milazzian, all contain in their lowest deposits a warm fauna, which in the case of the lower two includes Hippopotamus—an animal which certainly was not swimming in the Somme at a time when Switzerland and the Baltic buckler were covered with ice and ice was floating in the English Channel. Messrs. Hinton and Kennard have further shown that a warm mammalian fauna characterises the greater part of the terraces of the Thames.

Thus both marine and river terraces unite in proclaiming a warm climate and so far, apart from the Sicilian, we have encountered no signs of an Ice Age.

CONNEXION OF THE TERRACES WITH THE MORAINES.

We turn then to the moraines which afford evidence of the intercalation of glacial episodes in the otherwise genial climate of the Quaternary age. Prof. Depéret considers that he has proof of the association of the Milazzian terraces with the external moraine of the Rhone (Mindel), of the Tyrrhenian with the intermediate moraine (Riss), and of the Monastirian with the internal moraine (Würm). This association by no means implies synchronism, but it enables us to assign the several moraines to their respective stages.

From the point of view we have now reached it will be perceived that the term "Great Ice Age" is a misnomer, and that instead of speaking of a glacial age interrupted by genial episodes, it would be far more in accordance with fact to speak of a genial age interrupted by glacial episodes.

Since these glacial episodes were quite certainly intercalated it will naturally be asked why they are not more obviously represented in the fauna of the Quaternary age. The answer to this is that the remains of a cold fauna are by no means infrequent, but the gravels at the base of a terrace are not the place to look for them. It is to the slopes between the terraces that we should turn, for it is these which correspond with glacial episodes, and when, as generally happens, they are covered with löss we find in it the bones and teeth of such cold-loving species, as we might expect.

It may further be pointed out that terraces, both marine and fluviatile, mark a stationary level of the sea when deposits were accumulating, in which the contemporary warm mammalia might easily be preserved.

In the intervals when the sea-level was changing the work of denudation ruled supreme and undisturbed deposits were formed but sparingly. Now and then no doubt the bones of some animal belonging to the cold fauna might escape destruction and find burial in a marine terrace along with the warm fauna proper to it, and thus possibly have arisen some of those perplexing anomalies of distribution with which we are only too familiar.

REGRESSIONS.

The movement of the sea-level does not appear to have been a simple fall from the Sicilian to the Milazzian

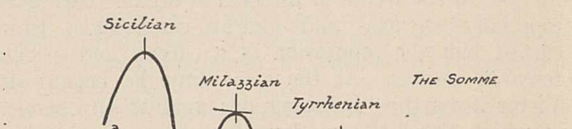

FIG. 1.—Oscillations of the Quaternary sea-level. *ab*, Sicilian stage; *bc*, Milazzian; *cd*, Tyrrhenian; *d* to Neolithic peat, Monastirian. The numbers indicate the heights of the marine terraces in metres.

coast-line and from the Milazzian to the Tyrrhenian; there is evidence to show that it fluctuated (Fig. 1), first sinking from the Sicilian to somewhere not far from the existing sea-level and then rising to the Milazzian, and similarly for all the succeeding stages.

Chronology of the Human Family.

The researches of Mr. Reid Moir have made us familiar with the existence of some member of the Hominidæ in the Red Crag, *i.e.* in the Calabrian stage of the Pliocene system, and if Prof. Depéret is right in referring the Forest Bed to the Sicilian, man seems to be also represented in this stage.

In the Milazzian (third terrace of the Somme) human artefacts are found associated with a warm fauna. Some of them are primitive forms of the Chellean boucher, and the industry as a whole is known as the Strepyan or Pre-Chellean.

The Tyrrhenian (second terrace of the Somme) affords the typical Chellean industry. It was indeed from this stage at Abbeville that Boucher des Perthes obtained the so-called "coup de poing" by which he established for the first time the existence of man at this remote period.

In the Monastirian, represented by the lowest gravels of the first terrace, the Chellean attains its final stage of evolution. It is still associated with a warm fauna. But these gravels are "ravinées" by a later one which brings with it Acheulean implements and the mammoth. If our preceding correlations are correct, the Acheulean must evidently be referred to a later stage of the Monastirian when the Würm glaciation was beginning to make itself felt. The Mousterian and all the succeeding industries of the Upper Palæolithic would then belong to the closing days of the Monastirian and the final retreat of the ice.

The interpretation seems to represent the present state of our knowledge, but it is not without its difficulties; one of the most perplexing is suggested by the "warm" Mousterian of Commont. More than one explanation may be offered of this, but the question may well be left to future research.

Comparison of the Coast-Lines of the Northern and Southern Hemisphere.

A eustatic movement of the sea-level is by itself unproved and unlikely, but a general deformation of the globe might well produce effects involving such a movement. That epeirogenic movements cannot be excluded is shown by the fact that the Tyrrhenian coast-line is deformed by local disturbances so that in the Strait of Messina it stands at 100 m. instead of 30 m., and in the Isthmus of Corinth even reaches 300 m. The Quaternary age was indeed by no means so reposeful as seems to be generally assumed; it includes movements of the earth's crust affecting wide areas and on no inconsiderable scale, as is shown by the recent observations of Prof. Bosworth in Peru, and Dr. Molengraaff in the East Indies.

This immensely complicates our problem. Prof. Depéret has sketched in bold outline a remarkable and suggestive history of the Quaternary age. To work it out in all its details will be the arduous task of more than one generation of geologists.

That a general deformation of the globe was in progress during Tertiary and Quaternary times is suggested by the general presence of raised beaches on both sides of the equator. On the north, General de Lamothe determined the existence of ancient coast-lines in Algeria at 325, 255, 204, 148, 108, 60, 30, and 18 m. On the south, they have been observed in Mejillones Bay, Chile, at 320-300, 225, 133, 111-108, 40, and 15-18 m.

It looks as though the earth accomplished its contraction by pulsations.

Obituary.

Prof. Paul Jacobson.

On January 26 the death occurred at Berlin of Prof. Paul Jacobson, who was widely known as the general secretary of the German Chemical Society and as the editor of important chemical works. He was born on October 5, 1859, at Königsberg in Prussia, and he studied under A. W. Hofmann at Berlin and Victor Meyer at Göttingen. In that university he became a lecturer, and followed Victor Meyer to Heidelberg, where he became professor. Jacobson carried out a number of researches in the field of organic chemistry, especially on azo- and hydrazo-compounds, which earned him the reputation of a careful and original research worker. At the same time he began with Victor Meyer the "Lehrbuch der organischen Chemie," in which these two workers have put on record an immense amount of knowledge and experiences. After the early death of Victor Meyer, Jacobson continued to work alone, unfortunately without being able to finish it.

In 1897 Jacobson removed to Berlin as editor of the Berichte der Deutschen Chemischen Gesellschaft and general secretary of the society. He transacted the business of this society with indefatigable industry and perfect tact until September 1911. He then became scientific editor of the Abteilung für Sammelliteratur, which was founded by the society for the purpose of re-issuing F. Beilstein's "Handbook of Organic Chemistry" and M. M. Richter's "Lexicon of the Carbon-Compounds," and between 1900 and 1906 Jacobson edited five supplementary volumes to the third edition of Beilstein's Handbook. Then he commenced the fourth edition of this standard work, which is to be completed in the near future. The new editions of M. M. Richter have been continued under the supervision of Jacobson by R. Stelzner, as "Literaturverzeichnis der organischen Chemie."

The death of Paul Jacobson will be deeply regretted by all who came to know him in the meetings of the Society and at international congresses. Scientific research suffers a great loss by his death.

Prof. W. N. Parker.

The death occurred on February 22, at the age of sixty-five, at his residence at Cardiff, of Prof. W. N. Parker, emeritus professor of zoology at the University College of South Wales and Monmouthshire.

Prof. Parker was a pupil of Huxley and for a time acted as his demonstrator. During 1881 and 1882 he was lecturer in biology at University College, Aberystwyth. He joined the staff of the University College of South Wales and Monmouthshire when it opened its doors in 1883, and retired in September 1922. He came of an illustrious family, being a son of the late Prof. W. K. Parker, and a brother of the late Prof. T. J. Parker. He married a daughter of the late Prof. August Weismann, who survives him, and leaves a family consisting of a son and two daughters.

Prof. Parker was for many years president of the biological section of the Cardiff Naturalists' Society and a member of the science committee of the National Museum of Wales. To the latter institution he presented a valuable collection of zoological material a few months prior to his death. In collaboration with his brother he wrote Parker and Parker's "Practical Zoology." He also translated into English Weismann's "Germ Plasm" and an abbreviated form of Wiedersheim's "Vergleichende Anatomie der Wirbeltiere." In addition, he published original papers on the following subjects: "Anatomy and Physiology of Protopterus," "Poison-organs of Trachinus," "The Structure of the Young of *Echidna aculeata*," "Persistence of the Left Posterior Cardinal Vein in the Frog, with Remarks on the Homologies of the Veins in the Dipnoi," "The Respiratory Organs of Rhea," "On some Points in the Anatomy of the Indian Tapir (*Tapirus Indicus*)," "The Anatomy of the Cæcum in the Rabbit (*Lepus cuniculus*) and Hare (*Lepus timidus*)"; in collaboration with F. M. Balfour, "On the Structure and Development of *Lepidosteus*"; and in collaboration with T. H. Burlend, "On the Efferent Ducts of the Testis in *Chimæra monstrosa*."

Prof. Parker devoted himself for nearly forty years to the interests of the College and University and to the development and organisation of the zoölogical department. It is impossible to speak too highly of the courage and determination which he brought to bear upon his work in the face of great difficulties in the early days of the College. He will be sadly missed by a large body of former students who passed through his hands, in whose personal welfare, both in the department and outside, he always took the keenest interest. The news of his death will be received with great regret by a large circle of friends and former colleagues.

J. H. L.

Mr. F. J. Lloyd.

The death of Mr. Frederick James Lloyd on February 8 removes an interesting figure from the ranks of the older workers in agricultural science in this country. Mr. Lloyd was born at Sketty, near Swansea, in 1852, and was educated at Bristol Grammar School. After leaving school he proceeded, for family reasons, to study law, but, showing a natural aptitude and interest in science, he soon rejected a legal career and found an opening in the laboratory of the late Dr. Voelcker. The training received there during the next four years was supplemented by evening studies at King's College, London, and subsequent experience in chemistry during a sojourn in Germany. On his return to England he became successively chief assistant to Dr. Thomas Stevenson, of Guy's Hospital, and at the laboratory of the Royal Agricultural Society, ultimately setting up in practice on his own account as an agricultural chemist.

Mr. Lloyd's knowledge of physiology and agricultural chemistry thus acquired led him naturally to a special interest in the subject of dairying, with which he became still more closely identified on the death of Dr. Voelcker by his appointment as consulting chemist to the British Dairy Farmers' Association. His close connexion with that body lasted throughout his life and directed his attention to questions of milk production and the feeding of dairy stock in relation thereto. In due course he began a series of investigations on the manufacture of Cheddar cheese, undertaken on behalf of the Bath and West and Southern Counties Agricultural Society, which proved very helpful to cheese-making farmers of the West of England and brought him into contact with the special agricultural interests of that area. Cider-making particularly attracted his notice. In association with Mr. Neville Grenville, and again on behalf of the Bath and West Society, he started experiments designed to improve the methods of manufacture then current on farms. These extended over some ten years and resulted in the establishment of the National Fruit and Cider Institute at Long Ashton in 1903, Mr. Lloyd acting as director until 1905. This Institute, now associated with the University of Bristol, and serving as its Agricultural and Horticultural Research Station, has been developed by the Ministry of Agriculture to function as the senior Fruit Research Station for this country, and stands as a direct result of Mr. Lloyd's work.

Mr. Lloyd lived also to see the establishment of the Research Institute for Dairying at Reading. His pioneer studies on both the subjects with which he was so closely identified have thus found fitting recognition.

Much of Mr. Lloyd's work was published by him in the form of a series of reports in the Journal of the Bath and West Society, of which for some twenty years he was associate editor. Those relating to cider were republished later by the Ministry of Agriculture. He also, while holding an appointment as lecturer on agriculture at King's College, London, published his lectures in book form under the title of "The Science of Agriculture," a volume which has been translated into several languages.

We regret to announce the following deaths:

Prof. A. S. Butler, lately professor of natural philosophy in the University of St. Andrews, on March 2, aged sixty-eight.

Sir Ernest Clarke, until 1905 secretary of the Royal Agricultural Society of England and the first lecturer in agricultural history in the University of Cambridge, on March 4, aged sixty-seven.

Prof. B. E. Fernow, emeritus professor and dean of the faculty of forestry at the University of Toronto, and first chief forester of the United States, on February 6, aged seventy-two.

Prof. G. Lefevre, professor of zoology in the University of Missouri, on January 24, aged fifty-nine.

Prof. Vladimir M. Shimkevich, professor of zoology in the University of Petrograd.

Rev. William Wilks, for twenty-five years secretary of the Royal Horticultural Society and the producer of the well-known Shirley poppies, on March 2, aged seventy-nine.

Current Topics and Events.

MUCH excitement was recently created all over the world by the sensational headline, " Cause of influenza discovered at Rockefeller Institute, says Dr. Flexner." The announcement was given a prominent position in the daily papers and everywhere was lauded as one of the greatest medical discoveries known. Almost alone among our contemporaries we stated the actual facts of the work of Olitsky and Gates, the reputed discoverers of the long-sought-for microbe of influenza, and we recommended the adoption of a cautious reserve until further data were revealed. Some of the inner history of this latest American press boom are now published in an editorial in the *Journal of the American Medical Association* (February 10), which has the greatest circulation of the medical papers of the United States and is a journal of the highest repute. It seems that after the sensational announcement above, the *Journal* telegraphed to Dr. Simon Flexner, who replied that his announcement was merely a summary of papers already published by Olitsky and Gates in the ordinary way in the *Journal of Experimental Medicine*. The summary was prepared for the New York State Department of Health. Dr. Flexner states that some one in the State publicity department had headlined the summary without his knowledge. Now that the actual statement of Dr. Flexner has appeared in the Health News Service, it is seen to be nothing that has not been known for the last three years, and as the *Journal of the American Medical Association* points out, the " organism cannot be said to have been conclusively shown to be the cause of the condition known as epidemic influenza," a view which we ourselves independently printed. In justice to the Press it is stated that in this instance it was not to blame, but it is not stated who was. The *Journal* deprecates this method of publication, leading as it does to false hopes for thousands of sufferers, and to the ultimate discredit of real advances in medical science.

TELEGRAMS from New York appeared in several newspapers of February 28, announcing the discovery of a fossilised human skull in the province of Santa Cruz, Patagonia. The *Times* of March 1 published particulars relating to the skull, which were obtained by its correspondent at Buenos Ayres from the discoverer, Dr. Wolf, formerly of the Canadian Geological Survey. The skull, it appears, was found not by Dr. Wolf but by a settler seven years ago in sand-hills in the pampas lying some twenty miles to the west of the port of Santa Cruz. The discoverer reports it to be " petrified " and " probably of tertiary origin." As regards its characters, all that is to be learned is that it is " long in proportion to its width," that its " frontal eminences are well marked," and that it may be a woman's skull. It is true that there exist in Patagonia deposits of the right age to yield fossil remains of Pliocene man, and on numerous occasions, during the past twenty-five years, claims of his discovery have been made. None has stood the test of inquiry ; when the remains proved to be human, it was found that a mistake had been made concerning their geological antiquity ; when their antiquity was upheld, the remains proved not to be human. Whether the discovery now announced will prove an exception remains to be seen.

IT is reported that excavations now being carried on at Ur of the Chaldees on behalf of the British Museum and the University of Pennsylvania have brought to light a temple of the Moon God. As Ur was the seat of the worship of deified kings and one of the greatest centres of ancient theology, its further investigation is likely to add considerably to our knowledge of the religious and social life of early Mesopotamia. The site and the purpose of the temple were first identified through the interpretation by Rawlinson of four cylinder seals discovered in 1854 by J. E. Taylor, who located the temple tower and excavated an adjacent building and burial mound. Further excavations were carried out by Mr. R. Campbell Thompson in 1918 and by Dr. H. R. Hall, on behalf of the Trustees of the British Museum, in 1919. Dr. Hall also investigated a neighbouring site at Tell el-Obeid, where he found much copper, including several lion heads and a large relief, in a pre-Sargonic building (*circ.* 2900 B.C.) beneath a platform of unburnt brick, probably of Dunghi of Ur (*circ.* 2450 B.C.). The site of Ur itself was occupied from neolithic down to quite late times, the temple having been restored by Nabonidus in the sixth century B.C. As regards its early inhabitants, Mr. Campbell Thompson, in the *Times* of March 1, points out that the present excavations may be expected to throw light upon his suggestion that the people of this area differed in race from the Sumerians. This view is based upon the character of the fragments of hand-made, painted pottery found by himself and Dr. Hall at Ur, Eridu and Tell el-Obeid, which is identical with that discovered by de Morgan at Susa in Elam. This latter, in turn, is referred to a similar, but rougher, type found at Anau in Turkestan.

A WELL-PRESERVED dolmen has been discovered by workmen while excavating at the back of a house at St. Ouens, Jersey. Associated with the dolmen was a kitchen midden full of limpet shells and containing an ancient human skull and a round stone for grinding corn. The skull is very much flattened in the frontal region, and it is no doubt on this ground that a very high antiquity, exceeding that of *Pithecanthropus erectus*, has been attributed to it locally, as is stated in a highly coloured report which appeared in the *Daily Mail* of February 26. It is also suggested that the kitchen midden is of mesolithic age. Although the find is of considerable interest, neither supposition appears to be well founded. Shell-fish must always have been, as they are still, an important element in the diet of the islanders, and therefore does not necessarily indicate a mesolithic culture, while the association with the dolmen and a stone for grinding corn would suggest that a very early date in the neolithic period for the skull is not probable. The flattened appearance of the skull, upon which stress is laid in the report of the discovery, may be due to pathological causes, but more probably is, as often happens, a case of flattening due to post-mortem pressure after burial. Further details of the measurements of the skull will be awaited with interest, as it will be important to note whether, notwithstanding its distortion, it is to be ascribed to the Mediterranean long-headed type.

ON February 12, Prof. Otto Pettersson, director of the Swedish Hydrographic Biological Commission, Gothenburg, celebrated the seventy-fifth anniversary of his birth, and both chemists and oceanographers in this country, to whom his genial personality is so well known, will wish to join in offering him their congratulations and their good wishes for his future prosperity. Having in early life made a European reputation as a chemist, Prof. Pettersson turned his attention to the study of oceanography, and much of the work in that subject during the last thirty years has owed its success to his initiative and inspiration. His name is particularly associated with the foundation in 1902 of the International Council for the Study of the Sea, of which organisation he was president for a number of years. It was largely owing to Prof. Pettersson's influence and efforts that the Council survived the trying period of the European war and has since renewed and extended the valuable co-operative researches which it is conducting in the interests of the fisheries. We rejoice to know that in spite of his advanced age, Prof. Pettersson's zeal for scientific work is in no way abated, and that he remains an active and energetic investigator, more especially of problems affecting the sea.

THE sixth Silvanus Thompson memorial lecture of the Röntgen Society is to be delivered on Tuesday, May 1, by Dr. C. Thurston Holland.

THE gold medal of the Astronomical Society of the Pacific was presented to M. B. Baillaud, director of the Paris Observatory, at the American Embassy in Paris on February 26.

DR. CHRISTOPHER K. INGOLD was awarded the Meldola medal of the Institute of Chemistry, for the second time, at the annual general meeting of the Institute held on March 1.

AN excursion to Devizes and Salisbury Plain, extending from May 18 to 21 inclusive, particulars of which are obtainable from Mr. B. H. Cunnington, Wiltshire Archæological Society, Devizes, has been arranged by the Prehistoric Society of East Anglia.

THE Medical Research Council has appointed the following scientific committee to organise an investigation into dog's distemper: Sir William B. Leishman (chairman), Mr. J. B. Buxton, Capt. S. R. Douglas, Prof. F. Hobday, and Dr. C. J. Martin. A member of the Council's staff will act as secretary to the committee, and communications should be addressed to the Secretary, Distemper Research Committee, 15 York Buildings, Adelphi, W.C.2.

THE British Association recently acted in co-operation with a number of other "travelling" societies in requesting the railway companies to revert to the pre-war practice of granting return tickets at single fare and one-third to members attending meetings, on presentation of a voucher. The Association has now been informed that in connexion with its next annual meeting, in Liverpool, September 12–19, this concession will be made by the companies.

AT the meeting of the Franklin Institute of the State of Pennsylvania, Philadelphia, held on February 21, Dr. Lee de Forest received the Elliott Cresson gold medal awarded to him by the Institute for his invention of the three-electrode audion. In presenting Dr. de Forest for this award, his invention was characterised as one of the most important ever made in the field of the electrical transmission of intelligence, and one which through its development has marked a profound revolution in the art of radio communication.

THE tenth annual general meeting of the Institution of Petroleum Technologists will be held at the house of the Royal Society of Arts on Tuesday, March 13, when an address will be delivered by Prof. J. S. S. Brame, the retiring president. The president-elect for the ensuing session is Mr. Herbert Barringer, and the vice-presidents are Mr. Alfred C. Adams, Sir George Beilby, Sir John Cargill, Viscount Cowdray of Cowdray, Mr. Arthur W. Eastlake, and Sir Thomas H. Holland.

AT the annual general meeting of the Optical Society, held on February 8, the following officers and council were elected: *President*: Prof. A. Barr. *Vice-Presidents*: Sir Frank Dyson, Mr. T. Smith, and Mr. R. S. Whipple. *Hon. Treasurer*: Maj. E. O. Henrici. *Hon. Secretaries*: (*a*) Business Secretary, Prof. Alan Pollard, Imperial College of Science and Technology, South Kensington, S.W.7; and (*b*) Papers Secretary, Mr. F. F. S. Bryson, Glass Research Association, 50 Bedford Square, W.C.1. *Hon. Librarian*: Mr. J. H. Sutcliffe. *Editor of Transactions*: Dr. J. S. Anderson. *Council*: Dr. J. S. Anderson, Instr.-Comdr. T. Y. Baker, Mr. W. M. Brett, Prof. F. J. Cheshire, Mr. R. W. Cheshire, Dr. R. S. Clay, Mr. H. H. Emsley, Mr. P. F. Everitt, Dr. J. W. French, Miss L. M. Gillman, Mrs. C. H. Griffiths, Dr. L. C. Martin, Prof. A. W. Porter, Mr. F. Twyman, and Mr. A. Whitwell.

A NEW meteorological observatory at Santa Cruz, Teneriffe, Canary Islands, was sanctioned by Royal Decree in July 1921. It is now announced that the building has been started, and will probably be completed shortly. The fact is noted in the *Meteorological Magazine* for February, and it is stated that the Island of Teneriffe already has a first-class observatory at Izana, situated 2307 metres above sea-level. Being on the direct route from Lisbon to Rio de Janeiro, these two observatories will be of great service to transatlantic aerial navigation. The note adds that a hydroplane station is also to be established on the island.

A MEMORANDUM on the probable character of the weather in north-west India in January, February, and March 1923 was prepared by Dr. G. T. Walker, director-general of Indian observatories, and submitted to the Government of India on January 5. The data which control the amount of rain and snow to be expected are:—(*a*) The recent weather conditions in Persia and north-west India; these are slightly favourable. (*b*) The seasonal change in the upper air in northern India, which is slightly adverse. (*c*) The atmospheric pressure over India in the previous October and November, which is neutral, October being above normal and November below normal. (*d*) Rainfall at Seychelles and Zanzibar; rainfall at Seychelles was in defect in November and December, and at Zanzibar it was in excess in December. On the whole the indications point to a slight defect in the winter precipitation, but the indications are said not to be sufficiently pronounced to justify a forecast of a deficiency.

REFERRING to the obituary notice of Prof. George Lunge in NATURE of February 17, p. 228, a correspondent has pointed out the last paragraph might give the impression that Dr. Hurter was of German nationality whereas he was a native of Schaffhausen, Switzerland. The writer of the notice was concerned rather with the influence exercised at the time by the German universities in providing opportunities, not necessarily for Germans alone, for scientific training as chemists, some of whom came to England to acquire knowledge and experience of the practical applications of the science.

WE have received from Messrs. A. Gallenkamp and Co. a catalogue of "Electrometric Apparatus for determining Hydrogen Ion Concentrations." This includes an apparatus for determining hydrogen ion concentrations both for work of high accuracy and for routine industrial work.

MESSRS. BOWES AND BOWES, 1 Trinity Street, Cambridge, have just issued a very useful catalogue (No. 417) of second-hand books, journals, and portraits of scientific interest offered for sale by them. It contains 1158 titles, which are classified under the following headings: Journals, etc.; Agriculture; Anthropology and Ethnology; Biography; Biology; Botany (including Forestry and Gardening); Geology; Microscopy; Zoology (including Ornithology and Entomology); General Science; Chemistry; Physics (including Einstein Theory); Medical (including Physiology); Portraits.

Our Astronomical Column.

INCREASE OF BRIGHTNESS OF BETA CETI.—There appears to be no reason to doubt the news that this star has brightened by more than a magnitude in the last week or so. The change was first observed by a British schoolboy named Abbott, resident in Athens; being a member of La Société Astronomique de France, he telegraphed to M. Camille Flammarion at Juvisy, whose assistant, M. Quénisset, confirmed the brightening. Apparently further confirmation has been received from the United States. Unfortunately the star is observable in England only by day or in very bright twilight, and the skies have not been propitious for studying it. Data for drawing the light curve are not yet to hand, so that it is premature to speculate on the probable cause of the increase of light. The news hitherto available comes through the daily press; the Astronomical Bureau at Copenhagen has made no communication.

THE ZODIACAL LIGHT.—Mr. W. F. Denning writes:—During the period from about March 8–20 and April 4–18, the zodiacal light may be well observed on clear evenings in the absence of moonlight. It will be visible about two hours after sunset as a faint glow extending upwards, obliquely, through the constellations of the Zodiac, and broadest at its base on the western region of the horizon. It apparently varies from night to night, for its visibility is evidently influenced by atmospheric conditions. Careful observations of the degree of luminosity, positions, and boundaries of the light on successive evenings will be valuable. The most probable explanation of the phenomenon is that it is due to the sun's reflected light on myriads of meteoric particles belonging to systems of little inclination and situated at moderate distances from the sun.

THE SPECTRA OF VISUAL DOUBLE STARS.—Mr. F. C. Leonard publishes in the Lick Observatory Bulletin (No. 343) an important contribution to the study of the spectra of visual double stars. If the components of a double star had a common origin, a knowledge of the spectral relationships existing in different systems, presumably at various stages in the course of evolution, might be expected to disclose the general trend and the comparative rates of development of these stars. It was with the intention of gaining more knowledge on this subject that Mr. Leonard commenced this investigation in 1920. From a study of eighty visual double stars specially observed for this work, he finds that the spectrum of the secondary component of a dwarf star is generally of a later class and that of a giant star is of an earlier class than the primary. In both giant and dwarf stars the greater the difference in magnitude between the primary and secondary the greater is the absolute difference in spectral class.

The spectrum of each component of a double star appears to be a function mainly of its absolute magnitude; or in other words, the spectra of the components of double stars are so related to each other that, with but few exceptions, these systems conform to the Hertzsprung-Russell arrangement for individual stars, plotted according to spectral class and absolute magnitude. In this configuration, the fainter component normally precedes the brighter one, regardless of whether the latter be a giant or dwarf in the order prescribed by the Lockyer-Russell theory of stellar evolution. The two earlier conclusions are special phases or necessary consequences of this generalisation. Thirteen binary systems, all stars of which were dwarfs, indicated that as the sum of the masses of the components increased, their disparity in spectral class approached zero. Of any two stars of unequal mass but of otherwise identical physical properties, that with the less mass will in general pass through its life history in advance of the more massive one.

Research Items.

THE OLDEST CHRISTIAN TOMB IN INDIA.—Agra, which possesses, in the splendid mausoleum known as the Taj, one of the finest sepulchres in the world, claims also the oldest Christian grave in northern India. It is known as the Martyr's Chapel, the tomb of a rich and very pious Armenian merchant called Martyrose, who died at Agra in A.D. 1611. The inscription on the tomb, now for the first time translated by Mr. Mesrovb Seth in the Journal of the Asiatic Society of Bengal, vol. xvii., 1921, runs: "In this tomb rested the pilgrim Martyrose son of Pheerbashi of Julfa. He died in the city of Agra and gave his goods to God for his soul. 1060 of the Armenian era." The Archæological Department has now restored the tomb of this worthy, a member of the important Armenian community of Julfa in Persia, who came to India as a merchant. An inscription in Persian to his memory has been placed on the tomb.

THE ISMAILI SECT OF ISLAM.—The important sect of the Ismailis or Assassins, the doctrines of which were preached by the Old Man of the Mountain, has exercised wide influence in Persia. The scattered material collected by historians, travellers, and theologians cannot compare in value with the genuine documents of the sectarian literature, but for five hundred years, when these materials came to an end at the time of the Mongol invasion which destroyed the power of the Assassins, the life of the sect is a blank. Mr. W. Ivanov, who has spent seven years in investigating the beliefs of the sectarians in Persia, has published under the title of "Ismailitiia," a translation of an important text which throws much light on the subject. This has been issued by the Asiatic Society of Bengal as part 1, vol. viii. of its memoirs. It will be interesting to European readers, as the leader of the sect is the Agha Khan of Bombay who did notable service to the Indian Government in the War, and has since devoted himself to the task of calming the agitation which has arisen in India on the Caliphate question.

THE INDIAN TRIBES OF CALIFORNIA.—The University of California, in its series of publications on American archæology and ethnology, has issued a large number of valuable memoirs, but a general survey of the inter-relations of the culture of these tribes has hitherto been wanting. This want has now been supplied by Mr. A. L. Kroeber, an ethnologist to whom we owe several of these tribal memoirs, who has prepared a general sketch under the title of "Elements of Culture in Native California," for the University series (vol. xiii. No. 8), in which he describes the arts of life, social organisation, religion, and ceremonies. This memoir, which gives an excellent survey of the industrial, social, and religious life of a primitive people, will be a valuable book of reference to ethnologists. It is provided with sketch maps, but unfortunately a general index is wanting.

MICROBIC TRANSMISSIBLE AUTOLYSIS.—One of the most interesting developments of modern bacteriology has been in relation to what is now called the Twort-d'Hérelle phenomenon. This has recently been the subject of the Cameron prize lecture given by Prof. J. Bordet, of Brussels, and published in the *Brit. Med. Journal* of February 3. In this lecture the main facts are clearly set forth and particularly the views of Bordet and his co-workers. For those who have not been following the subject specially it may be stated that in 1915, F. W. Twort, Director of the Brown Institution, London, described a peculiar glassy-like change which appeared in colonies of certain micrococci which he had isolated from calf lymph. A minute trace of the glassy agent added to a cultivation of bacteria dissolved the latter, and strange to say, the glassy agent could traverse fine porcelain filters without detriment. In 1917 d'Hérelle observed similar phenomena and regarded them as due to the activity of a living agent which he called "microbe bacteriophage" on account of its power of devouring bacteria. This view he has continued to defend with great vigour. Bordet and Ciuca adopted an entirely different explanation. They do not believe the active substance is a living agent at all but as a product of the bacterium itself induced in the first instance by some external influence and subsequently capable of indefinite transmission. A full treatment of the subject will be found in Bordet's lecture referred to above.

THE SPLEEN.—The functions of this organ are somewhat obscure. It is generally recognised, however, that it has something to do with the destruction of effete red blood corpuscles. A certain proportion of the corpuscles in general circulation are more fragile than the rest, in the sense that when distended by osmosis in hypotonic solutions they burst in solutions of a higher concentration than do the younger, more distensible ones. A recent paper by Bolt and Heeres, in the *Biochemical Journal*, vol. 16, p. 754, shows that after passing through the spleen, blood corpuscles are rendered less resistant, so that a larger proportion become hæmolysed when placed in the stronger salt solutions, that is, the less hypotonic solutions. Thus they withstand distension to a smaller degree than normally. This property is due to the adsorption of some substance supplied by the spleen and can be removed by washing with Ringer's solution. The previous work of Brinkman and van Dam had shown that the fragility of red corpuscles depends on the relative proportion between cholesterol and lecithine in their outer membranes, the former conferring stability, the latter, fragility. Apparently the spleen adds lecithine in larger amount than it does cholesterol.

CURING SLEEPING SICKNESS.—In the *Empire Review* for February Dr. Andrew Balfour has an interesting article entitled "Cure of Sleeping Sickness." He deals largely with the claims of the new German remedy "Bayer 205" and admits that it is the most powerful destroyer of the parasites of the disease so far tested. For a time sleeping sickness and other trypanosome diseases were looked upon as absolutely fatal, while later on partial success was achieved by more than one remedy containing arsenical or antimonial bodies. "Bayer 205" contains neither of these in any form, and although its exact composition is not known, it is suggested that it belongs to the benzidine dye series. It is a white powder, easily dissolved in water, neutral in reaction, without smell, and does not deteriorate on heating. It possesses extraordinary parasitotropic action on trypanosomes, and in minute doses can produce a *sterilisatio magna* in animals heavily infected with these parasites. These results worked out by Haendel and Joetten have been confirmed in man by Mühlens and Menk in Germany and by Wenyon and Manson Bahr in London. Dr. Balfour emphasises the need for chemical research in this country, and lays stress on the necessity for persistence, time, money, and far-sightedness. Ehrlich was fond of summing up the success of scientific researches in what he termed the four G's, *Geld, Glück, Geduld, Geschick*, which comes to the same thing.

Recent Pentacrinidæ.—In the Journal of the Washington Academy of Sciences of January 4 Mr. A. H. Clark publishes a revision of the recent representatives of the crinoid family Pentacrinidæ. For many years the name Pentacrinus has ceased to be applied to any crinoid now living, and now Isocrinus, to which genus most of the modern species were for a time referred, is also considered to be entirely extinct. For the only species that remained—the Atlantic *Pentacrinus wyville-thomsoni*—Mr. Clark founds the new genus Annacrinus.

A New British Enteropneust.—In the current number of the *Quarterly Journal of Microscopical Science* (vol. 66, part iv.) Prof. Alexander Meek records the discovery of an interesting addition to the British marine fauna. The Enteropneusta have hitherto been represented in British seas, so far as known, only by two species of the genus Dolichoglossus, from the west coast of Ireland and Scotland respectively. The newly discovered species is apparently referable to the genus Glossobalanus, and the name proposed by Prof. Meek is *Glossobalanus marginatus*, the species being regarded as distinct from any previously described. Unfortunately only a single imperfect specimen was obtained, off the coast of Northumberland at a depth of 52 fathoms. It is further suggested that a Tornaria larva sometimes met with in the North Sea plankton may be referable to this species.

Pomology.—A few years ago Mr. E. A. Bunyard, of the well-known Maidstone nurseries, upon his own initiative started a *Journal of Pomology*, in which contributions of very great scientific interest have been published. With its third volume this journal commences its career anew as the *Journal of Pomology and Horticultural Science*, with a powerful publication committee to support the original editor, the financial responsibilities now being transferred to the three horticultural research stations at Long Ashton near Bristol, Cambridge, and East Malling, Kent. In a foreword, Sir A. D. Hall expresses his interest in the new journal and his hope that while providing a medium for the publication of the results obtained by the investigators at these research stations, it may also "gather together new knowledge and experience from all kinds of public and private workers connected with fruit-growing in Great Britain." From the beginning the format of the journal has been good and many of its photographic reproductions exceptionally fine. The first number of the new volume contains a valuable series of papers upon the raspberry. The genus Rubus has long been a stumbling-block to systematists, and Mr. N. H. Grubb appears to have commenced for *Rubus Idæus* the task which the late Rev. Moyle Rogers carried out so thoroughly for *Rubus fruticosus*. Upon a series of characters the large and confusing number of varieties of raspberry grown in Great Britain are arranged within groups and a key given to permit the determination of some of the more important varieties. First importance is attached to the surface characters of the young canes, which fall into two groups, one pubescent, the other glabrous or nearly so; the colours of the spines then provide another valuable character. This important work is certainly a necessary preliminary to any cultural or experimental work with the raspberry. W. Boyes describes the characters of different types of apple-tree shoots, based largely upon the current nomenclature of the French horticulturist. F. V. Theobald describes the apple and plum case-bearer and its treatment. Herbert W. Miles discusses the control of the apple-blossom weevil, and G. S. Peren the value of spraying for the control of the logan beetle.

Surveys in the Eastern Kara-koram and Khotan.—A detachment from the Survey of India, under Maj. H. Wood, was attached to Dr. F. de Filippi's expedition of 1913 to undertake exploration and geophysical researches in the little-known regions of the Kara-koram at the headwaters of the Shyok and Yarkand rivers. Maj. Wood's report, which was delayed by the war, is now published ("Exploration in the Eastern Kara-koram and the Upper Yarkand Valley. Dehra Dun: Office of the Survey, 1922. 6*s*.). The work included the survey of the Depsang plateau, the San Remo Glacier, from which the River Yarkand proves to drain, and the upper valley of that river. Maj. Wood shows how he ascended what he believes to be the line of an old route leading across the head basin of the Oprang, but Dr. Filippi was forced to abandon his project of exploring that valley. An appendix contains a discussion of historical evidence bearing on certain disused or forgotten routes through the Kara-korams. The report is accompanied by a series of photographic plates and a coloured map, on a scale of 1 to 250,000, of the area surveyed by Dr. F. de Filippi's expedition.

Man as an Agent in Geographical Change.—Some of the ways in which man modifies the surface features of the earth were discussed in a lecture by Dr. R. L. Sherlock, given to the Royal Geographical Society on February 19. Mining and quarrying assist the natural agents of denudation and transform scenery. A calculation of the amount of rock removed in various kinds of excavation by man in Great Britain since the earliest times shows the significance of this work. The total excavation spread over the British Isles would amount to 3·83 inches. This may be compared with Geikie's estimate of the rate of erosion in the British Isles, which is 2·72 inches in 2000 years. Surface subsidence is an important effect of mining operations. Dr. Sherlock showed how this might be prevented or delayed by leaving pillars to support the roof, or by the method frequently adopted in the collieries of Upper Silesia of stowing waste materials in the cavities produced. The accumulation of waste on the surface may be utilised to fill up a foreshore as at Middlesbrough, where 4270 acres have been reclaimed in this manner; or it may form artificial hills. In the Black Country of Staffordshire some 230 million cubic yards of waste have been deposited on 23 square miles. Yet in this case subsidence has probably more than counterbalanced the gain. Under the site of London some 50 million cubic yards have been excavated, but brick or other linings have prevented subsidence. In fact, the level of London has actually risen by the accumulation of domestic and other waste. Excavations have shown this to be the case. On its own debris the height of London grows about one foot a century. It is probable that in three centuries the waste from the coal used in London has amounted to more than 42 million tons. Most of this directly, or indirectly, in the form of bricks and artificial flagstones, has been incorporated in the site of London. Dr. Sherlock also gave examples of man's interference with rivers, and, by means of pumping, with the circulation of underground waters.

Oil in Laccolithic Domes.—Of the many geological structures in which petroliferous sediments may be involved, elevated, dome-like masses of rock, resulting from igneous intrusion of the laccolithic type, are rarely productive of oil on a commercial scale, save possibly in certain cases in Mexico. There is, however, no reason *prima facie* why such a structure should not be favourable, unless secondary mechanical or thermo-dynamical effects on the superincumbent strata seriously influence the stability of organic material within the sediments. Thus it is not surprising to find that the United States Geological Survey is turning its attention to such possibilities in certain areas in the Western States, and a brief paper (Bulletin 736-F) dealing with oil accumulation in laccolithic domes in the Little Rocky Mountains region of Montana (the work of Messrs. A. J. Collier and S. H. Cathcart) is one of the first results of this inquiry. In the cases described, the uplifts are due to intrusions of porphyry, some of which are exposed, others, in the less denuded tracts of country, being still covered by sediments of varying ages, principally Upper and Lower Cretaceous. Of the former, the Eagle sandstone and the Mowry shale are both possible oil-bearing horizons, while the Kootenai formation (Lower Cretaceous) is well known to be favourable elsewhere. One or other of these horizons could be reached by drilling in at least two pronounced domes, the Guinn and the Grouse-Alder domes, within the area described, to the south of the Little Rocky Mountains. The authors do not of course prophesy commercial success for any fields which may be opened up here, but they have indicated the most likely areas in an otherwise discouraging region, and it will be interesting to observe, both from the scientific and industrial points of view, the results of any trials which may ultimately be made as a consequence of their report.

Lighting in Mines.—A striking illustration of the value of good illumination in enabling output to be increased in industrial operations is afforded by some experiments in coal mines described by Messrs. E. Farmer, S. Adams, and A. Stephenson in the Journal of the National Institute of Industrial Psychology. The report of the Miners' Nystagmus Research Committee, issued last year, confirmed the impression that this disease is due mainly to inadequate illumination. The present research shows how the miner's work is hampered and his output affected by deficient lighting. There are two chief drawbacks to most existing miners' lamps, the low illumination afforded and the exposure of the filaments, which, in such dark surroundings, give rise to highly inconvenient after-images on the retina. The authors describe a form of cylindrical shield which has a useful effect in avoiding this form of glare, and also give the results of work for an eight-hour period with the ordinary standard miners' lamp and with a special "porch-light" giving six times as great an illumination. It was shown that the improved illumination led to an increase in output from 2·47 to 2·83 tons, an increase of 14·57 per cent. The experiment serves to show the wide field for improvement existing in lighting conditions in coal mines and the benefits that might be secured by a moderate expenditure on research.

Meteorology at Southport.—Results of meteorological observations at Southport for the year 1921, and the annual report of the Fernley Observatory of the Corporation of Southport, compiled by the meteorologist, Mr. Joseph Baxendell, have recently been issued. The report is published in two editions, copies being circulated by the Southport Corporation, and by the Meteorological Office, Air Ministry. The Borough Observatory of Southport is the longest municipally-maintained meteorological station in the British Isles, observations having continued for the past 50 years. Daily, weekly, and monthly returns are supplied to the Meteorological Office. Much time has been devoted to the comparisons involved in the investigations of meteorological periodicities; among the clearly indicated cycles is one of 5 years, while a rainfall cycle of 53 years is said to be the chief. An appendix gives monthly averages, for 10 years, of the amount and duration of rainfall under different wind directions. It is shown that winds from southerly points are pre-eminently those of the rainy quarter. The most remarkable year during the half-century's existence of the observatory is stated to be 1921, although in the north-west of England it was not so dry as several previous years; the total deficiency of 4 inches of rainfall was trivial in comparison with the extraordinary drought over south-eastern England. For general fine-weather factors there is no known predecessor to equal it, the outstanding feature being the remarkably high mean atmospheric pressure. The underground water-level remained extremely low until the substantial winter rains in the latter part of December. Taken as a whole, the meteorological results will serve well as a guide for observations made by other municipal bodies.

Distance Thermometers.—Messrs. Negretti and Zambra have introduced a type of distance thermometer which appears to get over many of the difficulties and errors to which such instruments have been subject in the past. The new instruments depend on the expansion of mercury in a steel bulb to which a capillary tube of the required length is attached. This tube ends in a coiled Bourdon tube with the free end of which the pointer of the instrument gears directly. The pointer moves over a circular dial about 300° of which are occupied by the scale. The effect of change of temperature of the connecting capillary is eliminated by a wire of invar running down the tube and reducing the volume of mercury to such an extent that the change of its volume with change of temperature is identical with the change of volume of the steel tube. The errors of such an instrument tested at the National Physical Laboratory from 0° to 50° C. at no point of the scale exceeded 0·05° C.

Photo-elastic Research.—In a recent number of the Memoirs of the Society of French Civil Engineers (Bulletin de juillet-septembre 1922) Prof. E. G. Coker gives the text of a lecture, delivered by him last summer in Paris, which contains an up-to-date account of the method of exploring stresses in structures by means of celluloid models examined in polarised light, a method which is at present making rapid progress both here and on the Continent, and bids fair to become indispensable to every scientific engineer. Besides giving a sketch of the method and its general applications, Prof. Coker obtains new and interesting results concerning the testing of cement briquettes under tension, and compares the standard forms of such test-pieces adopted in Britain and France respectively. In particular, he shows that the standard briquettes adopted in both countries for cement tests lead to a strikingly unequal distribution of tensile stress across the middle section of the test-piece, and thereby to serious error in the deduced tensile strength. He suggests, as the result of photo-elastic research, a new shape of standard briquette which is free from this defect. Further illustrations of the method include a discussion of contact stresses and an investigation of the stresses arising from the action of cutting tools, both in the work and in the tool itself. This part of the lecture is partly a restatement of results previously described by the author and Dr. Chakko in the Proceedings of the Institution of Mechanical Engineers in April 1922, but various novel points are introduced.

The British Science Guild.

THE Mansion House was an appropriate *venue* for the great meeting organised by the British Science Guild on February 27 to acknowledge and proclaim the importance of scientific method, scientific knowledge, and scientific research as factors in promoting "national and Imperial interests." In the Egyptian Hall, with its high curved roof, its brilliant stained-glass windows, its serried banners recalling battles and heroes of long ago, the Lord Mayor presided over a distinguished company of representatives of modern science and industry. The first citizen of London is the honoured custodian of many great traditions, among which not the least precious is the city's historic generosity in promoting education and science. The City and its Companies have in the past given freely of their wealth in aid of these great causes, and it is fitting therefore that their faith in science, so amply proved, should stimulate the new crusade for its increased national recognition. Not less significant was the King's message of encouragement which Lord Askwith read to the meeting, welcoming the efforts of the Guild "to stimulate the scientific spirit, and to secure that application of science to industries, commerce, and, indeed, in all fields of human activities, so essential to efficiency and to the closer fellowship of all parts of the Empire."

The Lord Mayor, in his introductory remarks, emphasised the usefulness of the Guild's work of propaganda. When, he said, the British Science Guild was founded in 1905, its first object was stated to be to convince British people, by means of publications and meetings, of "the necessity of applying the methods of science to all branches of human endeavour, and thus to further the progress and increase the welfare of the Empire." Modern civilisation is so closely bound up with the advance of scientific knowledge that all progressive citizens can realise the service which a body like the Guild is able to render to this country and to Imperial development. This is an age of science, when such wonders as X-rays, radium, and wireless telephony, which have added so greatly to human powers and communication, are accepted almost as commonplace parts of our daily life. More scientific work is being carried on now than ever before, and we may expect results which will be of even greater value than those already achieved. British science in several directions leads the world, and it is right that this fact should be more widely recognised. Science stands not only for new devices and powers, but also for accurate knowledge and the right use of man's capacity and individuality. Scientific method must, therefore, be applied to social problems if the true principles of progress are to be determined. The Guild stands for national service in a wide sense: it includes representatives not only of pure and applied science, but also of industry and capital. After the Napoleonic wars, the nation found itself exhausted and impoverished. Our national position was re-established through the steam-engine and the industrial development which followed. We have now to look to the science laboratory to restore our economic position, and even to improved agricultural production. Later in the meeting the same note was sounded by Sir Joseph Cook, High Commissioner for Australia, who pointed out that a vast amount of capital had been wasted through the war, but the loss would soon be made good if two blades of grass could be made to grow instead of one or if the speed of steamships and other forms of transport could be doubled.

The principal resolution was moved by Lord Askwith as president of the Guild and accepted unanimously in the following terms:

"That this meeting, convinced that the progressive use of scientific knowledge is essential to industry and commerce, and that the application of scientific method to all public affairs would ensure increased efficiency and economy, pledges itself to support the efforts of the British Science Guild to promote national and Imperial interests by means of these powerful factors."

A letter of apology for absence was read from Sir Joseph Thomson, which stressed the need for the popularisation of science on the widest possible basis. "It seems to me," Sir Joseph said, "that the remarkable increase in the opportunities for scientific research which has taken place in the last thirty or forty years has not been accompanied by a proportionate increase in the means of bringing matters of scientific interest before the great mass of the people. . . . I do not forget the work of some of the great newspapers in spreading an interest in science by the admirable articles they publish at frequent intervals, but the public I am thinking of does not read the *Times* or the *Morning Post*." A more urgent need was to arouse an interest in science in the bulk of the population, which would facilitate the passage of measures to promote the progress of science in this country. Lord Askwith endorsed this plea and urged also that a great deal more might be done to endow discovery. It was of immense importance, he considered, that men of science without the hope of immediate reward should probe the mysteries of Nature, and that new discoveries should be brought quickly into general knowledge.

The appeal for some further endowment of "problem-solvers"—the elder men of science who devoted their lives to research as apart from the young trained laboratory workers—was vigorously pressed by Sir Ronald Ross. It might be supposed that the discoverer of the cause of cancer or tuberculosis would soon become a millionaire, but he pointed out that Sir David Bruce, who solved the problem of sleeping sickness, was now in Madeira unemployed, and there were three or four others whom he could name. He suggested that the nation should pension scientific discoverers of pre-eminent worth, and allow them to go on working as they pleased.

The vote of thanks to the Lord Mayor and the other speakers was proposed by Lord Askwith and seconded by Lord Bledisloe, who made an interesting speech on the application of science to agriculture. It appears that Continental agriculturists use the results of the researches on fertilisers and plant diseases at Rothamsted more than we do ourselves.

The meeting was a prelude to the launching of a national appeal by the British Science Guild for increased personal and financial support, and an appeal committee has been appointed, of which Lord Askwith is president. The list of members includes many distinguished representatives of science and public life. The director of the appeal is Commander L. C. Bernacchi, physicist of Scott's first Antarctic Expedition. A comprehensive plan of objects and methods has been drawn up and will be widely circulated in due course. The details of the scheme were not announced to the meeting, but the Lord Mayor said at the conclusion of his speech that whatever support was given to it would be returned a hundredfold in national honour and profit.

Research in the Scheme of Higher Education.[1]

By Dr. HERBERT H. HODGSON, The Technical College, Huddersfield.

THE present time is exhibiting none other than a break in the continuity of civilisation. No longer must the production and recognition of supermen be left to chance, since unusual genius, in whatever quarter it may be found, must have a field provided for its activities if our place as a leading nation has to be maintained. It is the province of higher education to discover this genius, a province, which, owing to haphazard evolution, is largely at the mercy of the dilettante, and, as a consequence, not yet in a condition to evolve those power stations of mind without which the necessary creative atmosphere remains ungenerated. The practical results of the German system of higher education have been the creation and development of key industries wherever possible, these ensuring an industrial system which afforded the security of continuous employment of an extremely varied character. This conferred a measure of national stability which was stout enough to defy the whole world for four years, and, but for lack of psychological balance, might have retired the actual winner of an apparently drawn battle.

The industrial exploitation of chemical science by Germany has entirely changed the international situation, inasmuch as a flourishing all-round chemical industry is now essential to the continued success and progress of all great manufacturing activities. This industry dominates the whole trade situation, and no country, however friendly at present, must ever be in a position to dictate by means of it such terms as can spell eventually our decadence and commercial annihilation. An unemployment problem of so vast a magnitude as ours demands the exploration of every reasonable avenue which may provide economic work, a demand which leaves no room for the neglect of key industries to be used against us. This in itself is an answer to the query as to whether the material importance of the organic chemical industry warrants its foundation in Britain. Recent combinations in other countries between firms engaged in key industries are ominous portents for the future.

The greatest key industry is that of synthetic dyestuffs, which, once established permanently, will prove the greatest source of well-being to our nation yet conceived. Its potentialities are bewildering in their immensity and can create and fashion the very future itself, so it becomes imperative that no external nation must be allowed to possess such a weapon as a monopoly. The war demonstrated the temperamental fitness of our countrymen for the dyestuffs industry, and, in spite of the current hostile criticism, I hold with Sir William Pope that only a few years are required for the organisation of a perfect lattice of fine chemical industries.

No industry dependent upon men of science for its progress will be able to survive external competition of a kind which Germany, the United States, and Japan are capable of exerting, unless a creative atmosphere is generated within the walls of our schools and our standards of intellectual attainment are raised to a much higher level than at present obtains. It is not sufficiently realised how much research work has to be done before any tangible results accrue, and therefore a multiplication of agencies is necessary, a practical proposition only to be realised by means of our higher educational system.

[1] From a paper read at the Annual Meeting of the Association of Technical Institutions on March 3.

Any comparison of pre-war British and German chemical ability which attempts to exalt the German as one apart, even as something chemically occult, must take the fact into account that so much of our best intellect as revealed by scholastic agency is absorbed into the civil service that the essence of Britain's research ability has never yet taken part in the industrial competition. In Germany the contrary has obtained. The British chemical mission to Germany after the Armistice found that industry there is systematically linked with the universities, and concluded that if our industries are to succeed in the future it is in this direction probably more than in any other that improvements must be effected.

It must be realised that higher education with respect to science and technology is at the parting of the ways, and whether the future is to emphasise the mediocre and the mechanical or to reveal latent genius will also decide whether the chemical industry, with its quota towards the solution of our unemployment problem, will also take root in this country. Our educational programme, therefore, must include a readjustment of the aims of technical education and the evolution of a new branch of the teaching profession to deal with the higher standard of student attainment necessary. No chemical department should be without a definite and distinct research section in which, at the earliest stage possible, students should be initiated into the methods of scientific inquiry. This was the practice of the great Hofmann, and it is as practicable to-day as it was in his brilliant period. The entire staff should also have service in the research section as part of their duties but with safeguards for individual expression. By this means a network of research colonies will be brought into existence, and the pivotal principle must be insisted upon that directors of research must not be prevented by details of organisation from actual personal participation. A large amount of individual responsibility will thus be generated with a greater resultant effort. As the late Prof. Meldola said, " I have not the least hesitation in declaring the belief that a school of chemistry which is not also a centre of research is bound to degenerate and to become a mere cramming establishment not worth the cost of maintenance." There should also be research centres on the lines of the Emperor William Institutes in Germany, an ideal proposed by Sir David Brewster seventy years ago for providing research careers for worthy men.

I would also suggest that patents should be examined by research organisations, and, where dishonest, the fact broadcasted, so that the intending fraudulent monopolist can be banished from our midst.

Another factor of far-reaching importance to industry is the establishment of English as a language for scientific publications at least co-equal with German. This can be secured on a stable basis only by the quality and quantity of our scientific output.

Only by the development of British research ability can our security as a nation be maintained and our prosperity advanced, since by it a lattice of industries will result, which, by reciprocity with the research agencies, will promote the extension of each. We shall then face the future with the determination to produce results in chemical science not inferior in quality or quantity to those in realms of knowledge where our leadership has never been in dispute.

Physics in Industry at the Wembley Laboratories.

THE General Electric Company, Ltd., is now a very large organisation, which employs some twenty thousand workers. It has engineering works at Birmingham, where it manufactures all kinds of electrical machines. At Stoke, near Coventry, telephones are manufactured. At the Osram lamp works at Hammersmith, lamps and valves of all kinds are made. At Erith, the company took over a few years ago the works of Messrs. Fraser and Chalmers, which manufacture steam turbines and mining plant. At Southampton, electric cables of all kinds are manufactured, and the company has glass works at Lemington-on-Tyne. Mainly on the initiative of Mr. Hugo Hirst, the managing director, it was decided some six years ago to establish a central laboratory to carry out the scientific and industrial researches which are essential for the progress of industry. Mr. Clifford Paterson, who was then the head of the electrotechnical department of the National Physical Laboratory, was appointed superintendent, and he is now helped by a staff of physicists and engineers many of whom have world-wide reputations.

The opening of the research laboratories on February 27 was a very interesting function. Lord Robert Cecil, speaking at the opening ceremony, said that the immediate task of the country is to repair the waste of the war. To do this the first and most essential requirement is to use every endeavour to increase the output of human energy and skill. This can only be done in two ways, namely, by reducing expenditure and by increasing the efficiency of production. Research, by making every man's skill go further, adds to the world's wealth. Science has no territorial boundaries. By promoting research the relations between this country and the world are improved. Sir J. J. Thomson, who also spoke, pointed out that it is absolutely necessary that a research laboratory should have a highly efficient staff. The capacity for the highest kind of research is rare. Training may increase the efficiency of a researcher, but it cannot put insight and originality into him. It is also certain that no research laboratory can guarantee delivery. The output of such a laboratory is always highly irregular and spasmodic. Sir Joseph Thomson also dwelt on the importance of cultivating the thinking powers of the community to the utmost.

The research laboratories are situated near Wembley and have a total floor area of 80,000 square feet, but they have ample room for expansion. The building has a north-light roof and nearly all of it is only one storey in height. The upper floor galleries carry most of the electric cables and the hydraulic pressure, steam, gas, and vacuum pipes required by the experimenters.

These galleries carry the arterial system essential for the laboratory without the necessity for conduits or ducts. This greatly increases the flexibility of the whole system.

The machinery in the central sub-station supplies the electric power, keeps the gases in circulation, and maintains the vacuum in the vacuum pipes throughout the building. Power at a pressure of 2850 volts and on the three-phase system is supplied by the North Metropolitan Electric Supply Co., and is converted into various pressures, both direct and alternating, for the distributing mains by means of motor generators and transformers. The lighting system is permanent, and is not touched for experimental purposes. The method of arranging the experimental distributing system is an extension of that which Mr. Paterson used at the National Physical Laboratory.

In addition to the electric machinery the sub-station contains the vacuum and compressor plant. Two rotary compressors feed into a horizontal boiler placed in the gallery, and this stores the compressed air which is required for experimental purposes. There are also three vacuum pumps driven by motors which are in continuous operation. These exhaust a fine vacuum main to the low pressure of 0·5 mm. of mercury and rough vacuum mains to pressures down to 6 mm. of mercury. There are also high-pressure hydraulic mains, compressed-air mains, and a one-inch hydrogen main.

There are two splendidly equipped workshops, one for metal and one for wood. These make the special apparatus required by the staff. They also engage in research work of their own, devising and improving automatic machinery and suggesting means of accelerating and improving the methods of production.

In the vacuum physics laboratory, X-ray analysis and analyses for detecting traces of gases are made. A novel question that is being investigated is the X-ray danger that may exist in connexion with the use of valve tubes at high voltages. For example, when these tubes are being exhausted, pressures of 10,000 volts and upwards are sometimes applied. Ordinary bulb glass which contains about 18 per cent. of lead is impermeable to these rays, but the special silica tubes often used in valve work are permeable; it is therefore necessary to know whether the work is dangerous or not. The problems produced by the static charges and other high voltage effects produced in bulbs are also being studied.

Some of the laboratories, for example the one for measuring the life of lamps, are used for routine testing. There were 800 lamps undergoing life tests simultaneously. They were of all kinds, carbon filament, metal filament, neon lamps, etc. Some of the neon lamps produce very novel effects and they are in great demand at present in physical laboratories. In photometry the equipment is very complete, and the various problems of illumination are being investigated by most scientific methods. A novel photometer was shown in operation which measured the absorbing power of various surfaces for light. By the use of this instrument the "blackness" of the inside coatings of lamp bulbs tarnished by use can be measured.

A specially novel and interesting feature of the laboratories is that they contain four small experimental factories for making electric lamps, tungsten wire, thermionic valves, and primary batteries, and these laboratory factories are regarded as tools which any of the research staff can use in connexion with the further development of a research. It is inadvisable to specialise research to too great an extent. Lamp research, for example, is not confined to the intensely interesting work on vacuum physics. It is equally concerned with metallurgical research, glass research, radiation from solids and gases, and high tension electrical phenomena in general. In a research laboratory it is necessary that the staff should be interested in practically every kind of research.

Many other interesting researches are being carried out at Wembley, and some of the work done has already proved of great commercial value in the factory. The importance of physics in electrical development may be illustrated by the case of the ordinary switch for the electric lamp. In the old days, an ominous bluish light sometimes made its appearance when the switch was turned off, and occasionally a switch was burnt out. The base of the switch was sometimes made of wood, which is a very poor insulator. Electricians then improved matters by accelerating the rate at which the terminal pieces separated when the current was broken. Later on it was found that a double air break was a vast improvement, and the base is now made of the best vitreous porcelain, which is practically a non-conductor. Those who use switches nowadays seldom if ever consider the thought that has been expended on their development. Every device in a progressive factory is undergoing continual improvement, and practical men recognise the value of an experimental and theoretical study of the physical laws which govern its development.

The new laboratory at Wembley is one of the largest research laboratories in this country. Compared with American standards, however, it is not large. The research laboratory of the Western Electric Co., Inc., of 463 West Street, New York, has a 13-storey building on a floor area of 400,000 square feet, and employs 1600 full-time researchers under the able guidance of Dr. Jewett, president of the American Institution of Electrical Engineers. The results obtained, however, are seldom in proportion to the size of a research laboratory, and we were much impressed by the ability of the staff at Wembley.

University and Educational Intelligence.

ABERDEEN.—The honorary degree of LL.D. was conferred, *in absentia*, on the Duke of Richmond and Gordon, Chancellor of the University, at a meeting of the Senatus Academicus held on Tuesday, February 27.

Prof. W. Mitchell, vice-chancellor and Hughes professor of philosophy in the University of Adelaide, South Australia, has been appointed Gifford lecturer for the sessions 1924–25 and 1925–26.

CAMBRIDGE.—The Grace approving the regulations for the admission of women students of Girton and Newnham Colleges to titular degrees in the University has now been approved and one stage of a long-drawn-out controversy has been completed. Among the other privileges granted to women students by the new regulations is included the right to be admitted to instruction in the University and to University laboratories and museums, though the number receiving such instruction at any one time is limited to five hundred. Women are now admitted as research students on the same footing as present candidates for the degrees of M.Litt., M.Sc. and Ph.D.

The Right Hon. T. Clifford Allbutt, Gonville and Caius College, Regius professor of physic, has been appointed as delegate to the celebration next June of the 800th anniversary of the foundation of St. Bartholomew's Hospital and the Priory Church of St. Bartholomew the Great.

THE opening of the new chemistry section of the Technical High School of Stockholm is announced in the *Chemiker Zeitung* of February 10. The building cost 3,300,000 kroner, and has four large laboratories for inorganic, organic, technical, and electrochemistry, and a smaller for the study of fermentation. The Director is Prof. W. Palmaer. It is stated that in size the building is exceeded only by that of Boston.

A REPORT on the development of adult education in rural areas has been issued (H.M. Stationery Office, 6*d*.) by the Adult Education Committee constituted in April 1921 by the Board of Education. The report reviews the work in this field of existing organisations—Local Education and other County Authorities, Women's Institutes, University Extension Committees, Workers' Educational Association, Association of Village Clubs, Y.M.C.A., Educational Settlements, and County Unions of village organisations,—the conditions of State aid, and the available sources of supply of books, and concludes with several practical suggestions. Among the opinions formulated by the Committee are: schemes of rural education properly organised can secure immediate and notable success provided village initiative and co-operation are encouraged; some form of county organisation, such as the Oxford Rural Community Council, is essential; national organisation is desirable and has been provided for by the recent establishment of a representative council by the National Council of Social Service; pioneer lectures and short courses of lectures are a necessary prelude to formal classes and merit State aid; the full development of the Carnegie Trust Rural Library Scheme will solve most difficulties as to the supply of books. As regards this last point, it is explained in a highly interesting memorandum appended to the report that it is the policy of the trustees to promote the establishment of county schemes controlled by County Council Education Committees, and 192,000*l*. was set aside by the trustees in February 1920 to enable every county to inaugurate one. By January 1922 thirty-eight were in operation. The key-stone of the whole system is the Central Library for Students (London and Dunfermline), from which any good-class modern book on a serious subject can be obtained through the county librarians.

STATISTICS of 670 Universities, Colleges, and Professional Schools, published by the United States Bureau of Education as Bulletin, 1922, No. 28, shows a total student enrolment for 1919–20 of 521,754, of whom rather more than one-third were women; by departments—preparatory 59,309, collegiate 341,082, graduate 15,612, professional 57,131. Of the 670 institutions, 109 were under public and 561 under private control: 82 were independent professional schools. Of 586 universities and colleges with undergraduate students, 354 were co-educational, and reported 162,558 men and 96,908 women; 117 were maintained exclusively for men and 115 exclusively for women. Enrolments in the professional schools were: law 20,992, medicine 14,242, dentistry 8809, theology 7216, pharmacy 5026, veterinary medicine 908. The percentage of women students ranged between 14 in pharmacy and 0·01 in veterinary medicine. Engineering schools enrolled 51,908 students, almost all men, distributed as follows: general engineering 10,231, civil 8859, mechanical 11,789, electrical 9469, mining 3048, chemical 5743. The number of engineering students more than doubled itself in the decade 1910–20. The total amount of benefactions — excluding government grants—was 65 million dollars. The total income per student—363 dollars in 1920—has risen steadily since 1890, when it was only 68 dollars. During the same period the percentage of receipts derived from the Federal Government, the State, and the city has increased from 12 to 27 and of student fees from 22 to 26, while the percentage from productive funds and private benefactions has decreased from 65 to 38. The following figures relate to universities and university colleges (excluding Oxford and Cambridge) in Great Britain in receipt of annual Treasury grants in 1920–21: income per student 54*l*., percentage of income from endowments 11, parliamentary grants 34, grants from local authorities 9, tuition fees 32.

Societies and Academies.

LONDON.

Royal Society, March 1.—A. **Mallock**: The effect of temperature on some of the properties of steel. The period of torsional vibration and the length of a steel wire were automatically and continuously recorded in terms of time, while the temperature was varied between 15° and 1000° C. The results show (1) that the variation of the rigidity of steel between ordinary temperature and a dull red heat is small (less than 1 per cent.); (2) that above the critical temperature (about 800° C.) the rigidity decreases rapidly; (3) that the temperature coefficient of expansion does not show any marked change as the metal passes through the critical temperature; but (4) that a comparison with the cooling curves of iron and steel proves that the specific heat of the high temperature form of the metal is much less than it is at temperatures below the critical point.—C. H. **Lees**: Inductively coupled low-resistance circuits. The oscillations in each of two circuits of low resistance coupled by their mutual inductance can be simply expressed in terms of a certain product of capacitance and inductance. The expressions for the currents lead to a simple graphical solution of the problem.—Lord **Rayleigh**: Studies of iridescent colour, and the structure producing it.—(1) The colours of potassium chlorate crystals. The structure of the iridescent potassium chlorate crystals investigated by Stokes and the late Lord Rayleigh is examined microscopically. The periodic twinned structure inferred by the latter is clearly shown in the photographs taken under the microscope with polarised light. Some crystals have exceedingly complex structure, showing many groups of evenly spaced twin planes and a very complex reflection spectrum. This results from high interference from twinned layers situated a considerable distance apart. Chlorate crystals, giving a silvery reflection, were obtained by Madan, who heated the ordinary colourless crystals to about 250° C. A complex twinned structure is induced, and photographs of the structure of the crystal and of the reflection spectrum show corresponding irregularities in each, resulting from want of flatness in the twin planes. (2) Mother-of-pearl. The results generally confirm those of Brewster and A. H. Pfund. Micro-photographs show the grating structure of a pearl oyster shell and the structure of parallel layers of an "ear" shell. The absorption spectrum of the latter shows that in agreement with the spacing of the layers the reflection is of the second order. (3) The colours of Labrador felspar. The colours seen by reflection arise from two distinct origins:—(*a*) Specular reflection from tabular inclusions, which show the colours of thin plates and are often 0·2 mm. in dimensions; they are distributed parallel to one of the cleavages. (*b*) Diffuse reflection from a plane about 15° away from the cleavage mentioned; this is the source of the striking colours observed. When the diffuse reflecting plane is examined microscopically under conditions which ensure that the light only comes from a very thin stratum, it is found that the plane of reflection is patchy. The patches are of irregular outline. The diffuse character of the reflection is accounted for by the small diameter of these reflecting surfaces, regarded as independent optical apertures. Their size (0·005 mm.) accounts approximately for the angular diameter of the diffuse image of a point source seen by reflection. The colour of the reflection is not sharply limited to special regions of the spectrum, and can be explained by the interference of streams of light from the two surfaces of each patch. The patches may be fissures in the material, and there is evidence that their thickness is not absolutely uniform. The brightness of the colour is explicable by the large number of reflecting patches adding their effects, without definite phase relation such as would give rise to regular interference.—L. V. **King**: On the complex anisotropic molecule in relation to the dispersion and scattering of light.

Society of Public Analysts, February 7.—Mr. P. A. Ellis Richards, president, in the chair.—E. **Griffiths-Jones**: Titanium in Nile silt. Titanium is determined by a colorimetric method after freeing the sample from silica; 1·3-2·55 per cent. of titanium oxide was found. Egyptian straw showed only 0·4 per cent. of titanium oxide on the ash.—Osman **Jones**: Notes on the examination of preserved meats, etc. The presence of a trace of zinc chloride in the tin container (which sometimes arises through the use of this salt as a soldering flux) causes a more rapid absorption of tin by the food contents; the use of sealing fluid containing a high boiling-point solvent also causes a disagreeable flavour to be imparted to the food material. The absorption of tin by the meat contents of a can is greatest at the time of processing and almost ceases after about 4 months. A dilute solution of iodine in potassium iodide gives a crimson colour with agar, while with gelatin an orange-coloured precipitate is produced.

Optical Society, February 8.—F. W. **Preston**: On the properties of pitch used in working optical glass. Pitch as a material for mounting lenses for polishing possesses many remarkable advantages. Its colour is valuable; the dull black surface in contact with the lens prevents reflection of light at the second face of the glass. Its coefficient of expansion approximates to that of glass, it melts at a relatively low temperature, and remains plastic through a considerable range. Pitch, being an undercooled liquid, may be made sufficiently solid to resist deformation by external pressures during the polishing operation, and yet left sufficiently plastic to yield to internal stresses, so as to be self-annealing at ordinary temperatures. The alteration of properties on prolonged heating is its most serious disadvantage.—T. Y. **Baker**: Prismatic astrolabe designed and made at the Admiralty Research Laboratory, Teddington. This instrument, used for accurate geodetic survey work, is a modification of that designed by MM. Claude and Driencourt, which has been extensively used in Egypt. The modifications are: (i.) The prism can be rotated about an axis parallel to its edges, and the angles of the prism are allowed to depart slightly from 60°; by using each edge of the prism in turn as the front edge, three observations of the star can be made instead of only one, the mean of the measured altitudes being exactly 60°. (ii.) A refracting prism of small angle is mounted to cover one quadrant of the object glass and a duplicate image of one star is thus produced in the field of view. The duplicated images are on the same horizontal level. Observation for contact is made by noting the instant when the descending image is on a level with and between the duplicated images. Laboratory trials show that whereas the mean error of observation with the old scheme was 0·2″, with the new arrangement it is 0·12″.

Aristotelian Society, February 19.—Prof. Wildon Carr in the chair.—C. E. M. **Joad**: The problem of free will in the light of recent developments in philosophy. It is generally admitted that on the basis of the Materialist and Mechanist theories, the conditions which must be satisfied if free will is possible are not fulfilled, since causation is conceived as proceeding always from the material to the mental. It is not generally recognised, however, that the Vitalist theories, for which the ultimate reality is life or spirit, also preclude the possibility of free will. There are two difficulties in regard to the theory of a vital principle or spirit. (1) How can a homogeneous unity differentiate itself into individual manifestations which are in some sense less real than itself, unless it contains in itself the principle of difference *ab initio*, that is to say, unless it is a plurality and not a unity? (2) Assuming that this difficulty could be surmounted, how can the individuals so formed act, desire, or will with a motive force, other than that derived from the underlying vital principle? If the energy with which they desire is that of the vital principle, and the will with which they suppress the desire, if they do suppress it, is also that of the vital principle, it follows that they are responsible neither for their desires nor for their suppression. These difficulties cannot be solved on the basis of a reality which consists of an initial unity, but the problem of free will, if not actually soluble, takes an entirely different complexion if an initial dualism or pluralism be assumed.

Association of Economic Biologists, February 23.—Prof. E. B. Poulton, president, in the chair.—Sir John **Russell**: Partial sterilisation of soil. The discovery that partial sterilisation increased the bacterial activity of soil was accidental, but when followed up it showed protozoa were present in the soil depressing bacterial numbers. It also showed that certain soil bacteria have the remarkable power of breaking the benzene ring, decomposing such unlikely substances as benzene, toluene, naphthalene, phenol, etc., and utilising them as food. Partial sterilisation kills or reduces disease organisms: here, however, heat is the only certain agent, the various chemical substances having specific properties rendering their general use difficult. A knowledge, however, of disease organisms to be suppressed and of the substances toxic to those organisms, allow the costly heating process to be superseded by the much cheaper chemical treatment. Finally, partial sterilisation produces chemical changes in the soil, some of the products of which have important effects on the plant. Thus, heating soil produces something which stimulates root development. At present partial sterilisation is used by the scientific worker to open up new fields of investigation, and by the practical grower to obtain better crops as the result of the increased bacterial activity, the freedom from disease organisms, and the presence of the root-stimulating substances.—H. G. **Thornton**: The destruction of aromatic antiseptics by soil bacteria. Soil antiseptics fall into two groups: those resembling toluene in mode of action, and those resembling phenol. The second group produces a sudden and great increase in the bacterial numbers in the soil, which is only temporary and is not accompanied by any considerable increase in ammonia production. The effect suggested that organisms fed on this group of compounds. Phenol, cresol, and naphthalene, when added to ordinary manured soil, disappear rapidly, due largely to a biological cause, and bacteria were found which in pure culture were able to derive the energy necessary for growth by decomposing these compounds. These organisms fell into three groups, non-motile resembling *B. phloei*, large rods producing clostridial sporangia, and short oval pseudomonads. The Pseudomonas group is of chief importance in producing phenol destruction in the soil.

PARIS.

Academy of Sciences, February 12.—M. Albin Haller in the chair.—C. **Guichard**: Two triple orthogonal systems which correspond in such a manner that the second tangent of one shall be the polar reciprocal of the third tangent of the other with respect to a linear complex.—A. **Andant**: The application of photography to the study of critical opalescence. The phenomenon was studied by means of a Hilger spectrograph, and the opacity measurements were made with a Fabry and Buisson microphotometer. Curves are given showing the variation of opalescence in ethyl acetate with temperature and with the wave-length.—M. **de Broglie** and J. **Cabrera**: The K absorption spectrum of element 72 (celtium). Some specimens containing zirconium show a feeble band with wave-length $\lambda = 0 \cdot 1905$Å. From corresponding spectra of ytterbium ($N=70$) and lutecium ($N=71$) this line would belong to the element of atomic number 72.—Mlle. Irène **Curie**: The distribution of length of the α-rays.—L. J. **Simon**: Viscosity, neutralisation, and isomorphism. The gradual neutralisation of arsenic and phosphoric acids has been followed by viscosity measurements. NaH_2PO_4 is indicated by a well-marked viscosity minimum: Na_3PO_4 shows a viscosity maximum. The arsenate and phosphate viscosity curves are very similar.—H. **Colin** and Mlle. A. **Chaudun**: The diastatic hydrolysis of the glucosides of alcohols. Determination of the molecular weights. An experimental method of fixing the molecular weight of a glucoside by measuring the quantity of enzyme for which the glucose set free from a fixed weight of glucoside no longer increases with the amount of the enzyme. Measurements of propyl, isopropyl, butyl, and isobutyl glucosides are given.—P. **Job**: The complex ions formed by silver salts and aqueous solutions of ethylenediamine.—Marcel **Delépine**: The potassium irido-dipyridino-dioxalates.—Marcel **Godchot**: The 1 : 2 cyclohexanediols and ortho-chloro-cyclohexanol.—Paul **Pascal** and M. **Garnier**: Two definite combinations of nitrogen peroxide and camphor. The melting-point curve of camphor-nitrogen peroxide indicates two definite compounds, $5N_2O_4 + 4C_{10}H_{16}O$ and $2N_2O_4 + 3C_{10}H_{16}O$.—Charles **Baron** and Albert **Verley**: Contribution to the study of a national petrol. Study of the miscibility of alcohol (94-100 per cent.) with ordinary petrol.—F. **Diénert**: Contribution to the study of the circulation of water in the chalk. Results of experiments with fluorescin. Water circulation in the chalk takes place by fissures only and not by filtration. A detailed experimental study of each region is necessary to determine the course of the water underground.—Pierre **Bonnet**: The existences of limestones containing Ural Fusulina in southern Transcaucasia.—Raoul **Blanchard**: The terraces of glacial closing.—Sabba **Stefanescu**: The contraction of the lower maxillary of mastodons and elephants.—Emile F. **Terroine**, A. **Feuerbach**, and E. **Brenckmann**: The unit of energy metabolism and the active mass of organisms.—Albert **Lécaillon**: The tendency to albinism in the hybrids of *Dafila acuta* and *Anas boschas*.—Jules **Amar**: The law of minimum in biology.

Official Publications Received.

Carnegie Institution of Washington. Year Book No. 21, 1922. Pp. xxii+414. (Washington.)

Annual Report of the Director, United States Coast and Geodetic Survey, to the Secretary of Commerce for the Fiscal Year ended June 30, 1922. Pp. iv+148+38 charts. (Washington: Government Printing Office.)

Department of the Interior: United States Geological Survey. Forty-third Annual Report of the Director of the United States Geological Survey to the Secretary of the Interior for the Fiscal Year ended June 30, 1922. Pp. ii+80. (Washington: Government Printing Office.)

Proceedings of the University of Durham Philosophical Society. Vol. 6, Part 4, 1922-1923. (John Theodore Merz Memorial Number.) Pp. 215-290. (Newcastle-on-Tyne.) 2s. 6d.

Carnegie Institution of Washington. Annual Report of the Director of the Department of Botanical Research. (Extracted from Year Book No. 21 for the Year 1922.) Pp. 47-76. (Washington.)

Report of the Director of the Observatory to the Marine Committee, and Meteorological Results deduced from the Observations taken at the Liverpool Observatory, Bidston, Birkenhead, in the Years 1920-21. (Published by order of the Mersey Docks and Harbour Board.) Pp. 75. (Bidston.)

Diary of Societies.

SATURDAY, MARCH 10.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Sir Ernest Rutherford: Atomic Projectiles and their Properties (4).

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

MONDAY, MARCH 12.

VICTORIA INSTITUTE (at 1 Central Buildings, Westminster), at 4.30.—Rev. Prof. A. S. Geden: Value and Purpose of the Study of Comparative Religion.

ROYAL SOCIETY OF MEDICINE (War Section), at 5.—Col. J. F. C. Fuller: Problems of Future Warfare; to be followed by a discussion on The Medical Problems of Future Warfare.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir Arthur Keith: Man's Posture: its Evolution and Disorders (4) (Hunterian Lectures).

ROYAL SOCIETY OF ARTS, at 8.—J. E. Sears, jun.: Accurate Length Measurement (2) (Cantor Lectures).

SURVEYORS' INSTITUTION, at 8.—C. G. Eve: The Re-valuation for Landlord's Property Tax, Schedule A.

ROYAL GEOGRAPHICAL SOCIETY (at Æolian Hall), at 8.30.—O. G. S. Crawford: Air Survey and British Archæology.

TUESDAY, MARCH 13.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Prof. C. G. Seligman: Rainmakers and Divine Kings of the Nile Valley (1).

ROYAL SOCIETY OF MEDICINE (Therapeutics and Pharmacology Section), at 4.30.—Prof. F. R. Fraser, Dr. A. N. Drury, Dr. A. E. Clark-Kennedy, and Dr. T. F. Cotton: Discussion on the Action of Quinidine in Cases of Cardiac Disease.

ROYAL SOCIETY OF MEDICINE (General Meeting), at 5.—J. E. Adams: The Urgent Need for Education in the Control of Cancer, to be followed by a discussion by Dr. C. P. Childe, Lord Dawson, Dr. H. Spencer, and others.

ROYAL COLLEGE OF PHYSICIANS OF LONDON, at 5.—Dr. G. Evans: The Nature of Arterio-sclerosis (3) (Goulstonian Lectures).

INSTITUTION OF PETROLEUM TECHNOLOGISTS (Annual General Meeting) (at Royal Society of Arts), at 5.30.—Prof. J. S. S. Brame: Presidental Address.

MINERALOGICAL SOCIETY (at Geological Society of London), at 5.30.—A. Hutchinson: A Graphical Method of correcing Specific Gravity Determinations.—C. E. Tilley: Genesis of Rhombic Pyroxene in Thermal Metamorphism. Mineral Associations and the Phase Rule.—C. S. Garnett: A Peculiar Chlorite-rock from Ible, Derbyshire. The Dissociation of Dolomite.—A. Brammall and H. F. Harwood: The Dartmoor Granite: (a) Porphyritic Felspars, and Biotite; (b) Andalusite, Sillimanite, Cordierite, and Spinellids.—J. G. C. Leech: Some Occurrences of Titanium Minerals on St. Austell Moor.

BRITISH PSYCHOLOGICAL SOCIETY (Education Section) (at London Day Training College), at 6.—H. Gordon: The Effect of Schooling on Intelligence Tests.

INSTITUTE OF MARINE ENGINEERS, INC., at 6.30.—W. A. Dexter: The Development of the Air Pump for High Vacuum.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN, at 7.—Annual General Meeting.

QUEKETT MICROSCOPICAL CLUB, at 7.30.—C. H. Caffyn: Rocks under the Microscope.

ROYAL ANTHROPOLOGICAL INSTITUTE, at 8.15.—Miss M. Edith Durham: "Bird-men" and related Customs in the Balkans.

SOCIOLOGICAL SOCIETY (at Leplay House, 65 Belgrave Road), at 8.15.—J. A. Hobson: Bias in the Social Sciences.

ROYAL SOCIETY OF MEDICINE (Psychiatry Section), at 8.30.—Dr. C. S. Myers: The Association of Psychoneuroses with Mental Deficiency.—Dr. H. J. Norman: Genius and Insanity.

WEDNESDAY, MARCH 14.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir Arthur Keith: Man's Posture: its Evolution and Disorders (5) (Hunterian Lectures).

GEOLOGICAL SOCIETY OF LONDON, at 5.30.

FELLOWSHIP OF MEDICINE (at Royal Society of Medicine), at 5.30.—A. J. Walton: The Differential Diagnosis of Surgical Dyspepsias.

INSTITUTION OF AUTOMOBILE ENGINEERS, at 7.45.—Major T. G. Tulloch: Multiple-wheel and Track Motor Vehicles.

ROYAL SOCIETY OF ARTS, at 8.—Sir William Warrender Mackenzie: Industrial Arbitration.

THURSDAY, MARCH 15.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Lt.-Col. E. A. Strange: Japanese and Chinese Lacquer (1).

ROYAL SOCIETY, at 4.30.—G. C. Steward: Aberration Diffraction Effects.—Lord Rayleigh: Further Observations on the Spectrum of the Night Sky.—Lord Rayleigh: Studies of Iridescent Colour, and the Structure producing it. IV. Iridescent Beetles.—Prof. J. W. Nicholson: Oblate Spheroidal Harmonics and their Applications.—Prof. J. W. Nicholson and Prof. F. J. Cheshire: The Theory and Testing of Right-angled Prisms.—Prof. J. C. McLennan and D. S. Ainslie: The Fluorescence and Channelled Absorption Spectra of Cæsium and other Alkali Elements.—Dr. W. Stiles: The Indicator Method for the Determination of Coefficients of Diffusion in Gels, with special reference to the Diffusion of Chlorides.—H. T. Flint: A Generalised Vector Analysis of Four Dimensions.

LINNEAN SOCIETY OF LONDON, at 5.

ROYAL COLLEGE OF PHYSICIANS OF LONDON, at 5.—Dr. A. J. Hall: Encephalitis Lethargica (Epidemic Encephalitis) (1) (Lumleian Lectures).

ROYAL SOCIETY OF MEDICINE (Dermatology Section), at 5.

ROYAL AERONAUTICAL SOCIETY (at Royal Society of Arts), at 5.30.—Prof. B. Melvill Jones: The Control of Aeroplanes at Slow Speeds.

INSTITUTION OF MINING AND METALLURGY (at Geological Society of London), at 5.30—Annual Meeting.

CHILD-STUDY SOCIETY (at Royal Sanitary Institute), at 6.—Sir Richard Gregory: The Position and Character of Science in Schools.

INSTITUTION OF ELECTRICAL ENGINEERS AND ROYAL INSTITUTE OF BRITISH ARCHITECTS, at 6.—F. Hooper and J. W. Beauchamp: Co-operation between the Architect and the Electrical Engineer.

CHEMICAL SOCIETY, at 8.—E. H. Usherwood and M. A. Whiteley: The Oxime of Mesoxamide (*Iso*nitrosomalonamide) and some Allied Compounds. Part III. Ring Formation in the Tetra-substituted Series.—H. H. Morgan: The Preparation and Stability of Cuprous Nitrate and other Cuprous Salts in the presence of Nitriles.—F. Challenger, A. L. Smith, and F. J. Paton: The Interaction of Hydrogen Sulphide, Thiocyanogen, and Thiocyanic Acid with Unsaturated Compounds.—Prof. T. M. Lowry: The Polarity of Double Bonds.

INSTITUTE OF METALS (London Local Section) (at Institute of Marine Engineers, Inc.), at 8.—W. B. Clarke and others: Discussion on The Heat Treatment of Non-Ferrous Metals.

CAMERA CLUB, at 8.15.—C. P. Crowther: The Man behind the Camera.

FRIDAY, MARCH 16.

ROYAL SOCIETY OF ARTS (Dominions and Colonies and Indian Sections), at 4.30.—Lt.-Col. Sir Leonard Rogers: Recent Advances towards the Solution of the Leprosy Problem.

ROYAL SOCIETY OF MEDICINE (Otology Section), at 5.—Dr. F. M. R. Walshe: The Symptomatology of Eighth Nerve Tumours.—W. Trotter: The Surgical Treatment of Tumours of the Eighth Nerve.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir Arthur Keith: Man's Posture: its Evolution and Disorders (6) (Hunterian Lectures).

INSTITUTION OF MECHANICAL ENGINEERS, at 6.—Second Report of the Steam-Nozzles Research Committee.

RADIO SOCIETY OF GREAT BRITAIN (at Institution of Electrical Engineers), at 6.30.—L. F. Fogarty: Accumulators, Dry Cells, and the Currents used in the Reception of Radio Telephony, illustrated by Experiments.

JUNIOR INSTITUTION OF ENGINEERS, at 7.30.—C. H. Woodfield: Comparative Power Costs.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN (Pictorial Group Meeting, in conjunction with the Affiliation of Photographic Societies), at 8.—R. H. Lawton: A Criticism of the Prints in the Affiliation Print Competition.

ROYAL SOCIETY OF MEDICINE (Electro-therapeutics Section), at 8.30.—Prof. Gosta Forsell: Some Observations on Movements of Gastro-intestinal Mucosa.

SATURDAY, MARCH 17.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Sir Ernest Rutherford: Atomic Projectiles and their Properties (5).

PUBLIC LECTURES.

SATURDAY, MARCH 10.

HORNIMAN MUSEUM (Forest Hill), at 3.30.—H. N. Milligan: The Great Sea-serpent.

MONDAY, MARCH 12.

INNER TEMPLE HALL, at 8.—Dr. C. Porter: The Principles and Practice of Sanitary Legislation (Chadwick Lecture).

TUESDAY, MARCH 13.

LONDON SCHOOL OF ECONOMICS, at 5.—Sir Henry Reid: Food Supplies (Statistics, before, during, and after the War) (4).

UNIVERSITY COLLEGE, at 5.15.—Prof. E. T. Whittaker: Electric Fields in Atomic Physics (succeeding Lectures on March 15, 20, and 22).

WEDNESDAY, MARCH 14.

KING'S COLLEGE, at 5.30.—Sir Richard Gregory: The Influence of Science.

THURSDAY, MARCH 15.

ROYAL INSTITUTE OF BRITISH ARCHITECTS, at 5.—H. S. Goodhart-Rendel: Architecture—a Necessity or a Luxury?

CENTRAL LIBRARY (Fulham Road), at 8.—A. H. Page: Architectural and Record Photography.

FRIDAY, MARCH 16.

UNIVERSITY COLLEGE, at 5.30.—Sir Gregory Foster: Lectures—their Use and Abuse.

CHELSEA POLYTECHNIC, at 8.15.—Prof. A. C. Seward: A Summer in Greenland.

SATURDAY, MARCH 17.

HORNIMAN MUSEUM (Forest Hill), at 3.30.—Dr. A. Abram: Travelling in the Middle Ages.