H. 2669, 70 u. 77 sowie T.R. fehlen!

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH.

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The Unity of Science and Religion.

THE Cardiff meeting of the British Association will be marked with a red stone in the road of progress because of two noteworthy events. One was the suggestion of the president, Prof. W. A. Herdman, eagerly taken up by members of the Association, that the time had come for a new *Challenger* expedition for the exploration of the oceans, and another was the enlightened sermon, which we print in full elsewhere, delivered by Canon E. W. Barnes, a distinguished mathematician who is both a fellow of the Royal Society and a Canon of Westminster. We do not hesitate to say that not for a long time has such a conciliatory attitude been presented to men of science by a leader in the Church as is represented by Canon Barnes's address. The position taken up in it is one upon which the two standards of science and religion can be placed side by side to display to the world their unity of purpose. For Science and Religion are twin sisters, each studying her own sacred book of God and building a structure which remains sure only when established upon the foundation of truth.

The day of bitter controversy between dogmatic theology and often no less dogmatic science is, we hope and believe, past and gone, and no one would wish to recall it. We certainly have no intention of opening a discussion in our columns upon Biblical interpretation or the bearing of scientific discovery upon it. The complete story of the struggle through which the present position has been gained is told by Dr. A. W. White in his "History of the Warfare of Science with Theology in Christendom," published last year. It is difficult now to realise what was done to pour contempt upon Darwin's works and discredit him and his followers by theologians of all types; but towards the end of the conflict it became clear that leaders in all Churches were beginning to understand that men could be Christians and at the same time Darwinians, and in latter days they have not only relinquished the struggle against science, but have also frankly shown their willingness to make an alliance with it.

Canon J. M. Wilson, another distinguished mathematician who has also the highest credentials to represent the views of thoughtful Churchmen, referred to this change of attitude in an article contributed to our Jubilee issue in November last. He then wrote: "Insensibly a change has occurred which is not easy to define. Perhaps it may be described broadly as the discovery by the scientific world that the sphere of religion is not inherently anti-rational; that faith, like knowledge, rests ultimately on experience; that science has its sphere in the world of matter leading up to forces of unknown origin and nature; and that faith has its sphere in a world of personality leading up to a similarly unknown goal of personality: that their methods are not inconsistent; and that their goals may be identical."

Religion cannot, in fact, afford to ignore what is true, and can have no real interest in believing what is not true. We have passed the dangerous stage when apologists strained analogies to prove that science and orthodox Christianity, so far from being in conflict, are really in perfectly amicable agreement, and have reached a point at which it is understood that science and religion both contain systems of truth which must ultimately prove to be congruent. Theological beliefs no longer rest solely upon the ancient foundation of authority, but are built upon a basis of reason. Just as every event in Nature is a manifestation of natural law or principle, known or unknown, so religion is natural and not supernatural, and the conceptions to which it leads may be submitted to similar inquiry. It is not a simple phenomenon, but a complex of thought and emotion, and the components of this complex have yet to be resolved.

The insufficiency of human life itself as an end is dealt with philosophically by Prof. Boutroux in his "Science and Religion in Contemporary Philosophy," in which it is urged that the ideal of duty summons us beyond the specifically human to a noble struggle and a great hope, an ideal which implies faith and love, and demands divinity and a Being with Whom we can be in communion. It is in the "living reason" interpreted in the light of duty that science, without which we cannot live, and religion, without which we do not wish to live, find their reconciliation.

The scientific view of religion, now accepted by men of science and Churchmen alike, is that religion is the spiritual life of the individual, and subject to development. Progress is possible here as elsewhere, and in fact the history of the forms of religion shows a gradual purification and emancipation advancing with the gradual refinement of experience. The goal, as a reviewer has said in these columns, is a union of God and humanity, and the end must be the concrete realisation of unity in life and purpose for which, as for the unity of the world as object of the sciences, the reality of the Divine immanence is the only sure ground.

The origin of religion itself is still obscure. Whether it arose from belief in spiritual beings, in the worship of the soul, in ancestor worship, in ghost propitiation, or in any other of the various views which have been put forward, has yet to be decided. The system of social morality early developed when primitive communities were formed by man has little to do with religious perceptions. It is easy to pass, however, from the stage of veneration for great heroes or benefactors during life to that of ancestor worship after death, and later to soul worship. There would be sainted dead to worship, as well as malevolent dead and spirits of disease to propitiate. Eventually might arise the philosophic conception that continuation of life lies, not in the immortality of the soul, but in the perpetual remembrance of the righteous by mankind. All these matters are legitimate subjects of inquiry, and men of science may join with theologians in elucidating them. The problems are difficult, but not beyond solution, and they are approached to-day in a less dogmatic spirit than they were a few years ago by advocates on both sides. As regards the true relations between soul and body, we are in much the same position as that of the Persian poet who wrote long ago:—

> There was a Door to which I found no Key;
> There was a Veil past which I could not see.

Whatever the end may be, we are urged to the quest by that something within ourselves which has produced from a primitive ancestry the noblest types of intellectual man, and regards evolution, not as a finite, but as an infinite, process of development of spiritual as well as of physical life.

The Drying Up of South Africa—and the Remedy.

The Kalahari or Thirstland Redemption. By Prof. E. H. L. Schwarz. Pp. vi+163+xiv plates. (Cape Town: T. Maskew Miller; Oxford: B. H. Blackwell, n.d.) Price 8*s*. 6*d*. net.

WHILST Man of all races and skin-colours is once more involved in fratricidal quarrels —*how* Superior Intelligences in more advanced spheres must grin as they watch our wars against one another through super-telescopes or by æthereal telegraphy!—Nature is making one more effort to get rid of man. This time through Drought. She has seemingly hated everything that rose above the mediocre on this planet, whether it was in fish shape, or in the fish-saurian, the dinosaur, the struthious bird, the ungulate mammal, or the brain-worker, Man. She tried to nip us in the bud by reviving the Ice ages which she had used for other destructive purposes in the pre-Cambrian, Devonian, Permian, and Jurassic periods. But this succession of cold spells only braced Northern Man to greater efforts and greater triumphs, and sent Southern Man to grapple with the tropics, and to digest and partly overcome their germ diseases. Now the tropics, and above all the sub-tropical regions, are being threatened by drought. The desert is spreading in sub-tropical North America, in tropical South America, in temperate and sub-tropical Asia and eastern Europe, in northern and north-central Africa, and in that prolongation of the African continent which lies beyond the Zambezi and Kunene Rivers.

Prof. Schwarz theorises on the earlier theories of others—British, South African, French, and German—and propounds explanations and suggests remedies derived from his own geographical and geological investigations in South and South-west Africa; and the result is an exceedingly interesting little book, well illustrated and well worth the modest price asked for it—though insufficiently served by its maps. He points out that the main cause of the creation—the recent creation—of the Sahara, Libyan, and Kalahari deserts in Africa has been the diversion of river courses, most of which ran (more or less) from south to north and north to south, but now reach the sea by shorter courses almost at right angles to their former direction.

At no great distance in time it is highly probable that the Kunene River of southern Angola had made no breach in the western rock rampart of the Ovambo plain; it flowed instead by several dried-up water-courses into the Etosha lake, and thence along the Ovambo River to the Omatako and the Kalahari plain. Joined further by the waters of the Kwito, Okavango, and Kwando, this accumulated drainage of the lofty plateau of eastern Angola emptied itself, not—as now—into the Atlantic or the Zambezi, but into a huge expanse of water of which Lake Ngami and the Makarikari "depression" and salt pans are the shrunken remnants. Finally, in all probability this great lake of South-west Africa found an outlet through the Molopo River of western Bechuanaland into the Orange, and so at last reached the sea. But its moisture, through the soil and the atmosphere, rendered the western half of South Africa a fertile land endowed with exuberant vegetation and animal life, and able to support a large human population.

The author shows in this book how by engineering operations less difficult, probably, than those which have tripled the value of the Nile waters, the Kunene and Okavango and their tributaries might once more be diverted into the old channels and restore to human use and benefit an area of more than a million square miles. His schemes and plans will, of course, be riddled by the same "expert" criticism which declared the Suez Canal an impossibility, or be side-tracked through some political jealousy. But, all the same, if something more or less in the nature of his proposals is not soon put in hand, the habitable area of trans-Zambezian Africa will shrink considerably.

Man must give up internecine warfare and unite all his forces to defeat his arch-enemy, Nature. He must melt the ice at the North and South Poles, and put a stop to the spread of desert conditions in Asia, Africa, Australia, and the Americas. H. H. JOHNSTON.

Cement Manufacture and Testing.

Cement. By Bertram Blount. Assisted by William H. Woodcock and Henry J. Gillett. (Monographs on Industrial Chemistry.) Pp. xii+284. (London: Longmans, Green, and Co., 1920.) Price 18*s.* net.

THE need for a handy text-book on cement, which should include the modern processes of manufacture and also a review of the chemical aspects of the subject, has been felt for some time, and this work of Mr. Blount should go far towards meeting the requirements. The author has an extensive practical experience of cement manufacture and testing, and is accustomed to present accounts of the industrial processes in a readable form. With the exception of a few passages, the work is devoted to Portland cement, and the processes of manufacture selected for description are almost exclusively those which are adopted in England. Rather fuller references to Continental and American methods would have been welcome, since those methods, almost similar in principle, present great differences of practical detail.

The use of blast-furnace slag as a raw material for the manufacture of Portland cement receives only the briefest mention, but this is now extensively practised, and the product is likely to play an important part in building and engineering operations in such regions as the North of England and Scotland, where suitable clays or marls are rare, whilst slag is a waste by-product of the iron industry. The further use of finely ground slag as a pozzolanic material is not noticed, and, indeed, the whole subject of pozzolanas deserves more attention. It is not sufficiently recognised that the lime set free during the setting of Portland cement, like the slaked lime of an ordinary mortar, is capable of combining with silica when presented to it in a sufficiently active form, and that even a good cement may be considerably improved by the addition of a suitable pozzolanic substance. White Portland cement made from raw materials exceptionally free from iron is not mentioned, but its manufacture is of some interest, and has been conducted with success on a technical scale.

The account of the consumption of energy in grinding is not quite correct. It is not true of most minerals that the energy required to separate crystals along their cleavages is very small, and clinker, although it may be in a state of internal strain, nevertheless possesses a degree of hardness which requires great energy to disintegrate it, as may be shown by applying direct pressure to single granules of clinker in a testing machine. There are several suggestive notes regarding possible improvements in the manufacturing processes. The author has long argued that the logical goal of improved manufacture should be the production of such a high temperature as will yield a fused product, in which case a higher percentage of lime could be used and a homogeneous cement obtained. Certain improvements in regard to fuel economy and to the recovery of potash are also suggested.

On the subjects of chemical analysis and mechanical testing the author speaks with authority, and the chapters dealing with these sections may safely be used as a laboratory guide, whilst their usefulness is enhanced by the inclusion of a number of the more important official specifications for Portland cement in force in various countries. The fact is mentioned that "a cement which will pass the British standard specification is at least as good as that acceptable under any of the foreign specifications."

The vexed question of the chemistry of Portland cement is well treated, but a good deal of space is occupied by an account of researches and speculations which have no more than an historical interest. It is not so widely known as it should be that the whole chemistry of silicates has been revolutionised by the brilliant work emanating from the Geophysical Laboratory at Washington, which has replaced guesswork by an accurate physico-chemical survey of the systems concerned. The triangular equilibrium diagram of the lime-silica-alumina system is now known with sufficient accuracy to serve as a practical guide in the cement laboratory. The study of setting, on the other hand, has not advanced very greatly beyond the stage reached by Le Chatelier. The author, in referring to the work of Stern, has overlooked the fact that the examination of clinker and cement by reflected light after etching has been used successfully by several workers, and that excellent photographs of the fine eutectic structures have been published, some of them in Japan.

These points might well receive attention in a future edition, and would still further increase the value of a very useful book.

C. H. DESCH.

Psychology, Normal and Subnormal.

(1) *Psychology of the Normal and the Subnormal.* By Dr. H. H. Goddard. Pp. xxiv+349. (London: Kegan Paul, Trench, Trubner, and Co., Ltd., 1919.) Price 25*s.* net.

(2) *Psychology and the Day's Work: A Study in the Application of Psychology to Daily Life.* By Prof. E. J. Swift. Pp. ix+388. (London: George Allen and Unwin, Ltd., 1918.) Price 10*s.* 6*d.* net.

(3) *The Child's Unconscious Mind: The Relations of Psychoanalysis to Education: A Book for Teachers and Parents.* By Dr. Wilfrid Lay. Pp. vii+329. (London: Kegan Paul, Trench, Trubner, and Co., Ltd., 1919.) Price 10*s.* net.

UNTIL the last half-century psychology was based almost exclusively upon the observation of a highly intelligent and highly civilised type of mind—usually the mind of the psychologist himself. Modern psychology owes its remarkable progress chiefly to two factors: first, to the addition of the method of experiment to the method of observation; and secondly—a factor the importance of which is less generally recognised—to the extension of these two methods to the study of simpler types of minds—of the minds of animals, of children, of savages, and of abnormal adults. The recent increase in the number of institutions for feeble-minded children has now provided psychology with the simplest human material available for investigation. Just as the ultra-rapid camera slows down the kinematographic picture of the swift movements of leaping and running, so Nature, by an arrest of the brain, retards the normal development of the child so that it can be observed and tested at leisure.

(1) The laboratory attached to the Vineland School for Feeble-minded Children—a laboratory unique of its kind, not only in America, but also in the world—has been utilised for such studies since 1906. Dr. Goddard's book provides a popular *résumé* of the conclusions progressively accumulated by the investigators in that laboratory. It is not a psychology merely of the mentally defective child. It is a psychology of the normal human mind as illuminated and illustrated by the observations upon the mentally defective. The volume, however, is by no means confined to observations carried out at Vineland. Indeed,

these are all too modestly alluded to. The exposition incorporates, in a clear and for the most part non-technical form, much of the most recent work done elsewhere upon the obscurer functions of the nervous system and of the mind.

The first two chapters give a simple but up-to-date account of the nervous system, admirably illustrated by nearly forty diagrams and photographs. Due emphasis is laid upon the importance attached by recent studies to the part played in the formation of mind and temperament by the activities of the "sympathetic" portion of the nervous system. Mental deficiency is then treated; it is described as an arrest in the development of intelligence, due to an arrest in the development of the nervous system at or before twelve years of age—that is, at some period before the onset of puberty. Memory, attention, and association are discussed in turn, and considered primarily as properties inherent in the nervous mechanism. The higher mental processes—action voluntary and involuntary, language, and (to adopt the author's spelling) "thot"—are then discussed as the complex results of the interaction of the fundamental properties of the nervous system. The affective side of experience—the emotions, simple and complex, and that elusive quality called temperament—receive full attention; and a lucid exposition is given of the views more particularly of Mosso, Cannon, and McDougall. To the results of psycho-analysis no reference whatever is made.

The first half of the book is thus an exposition of psychology from a physiological point of view. The second part proceeds to apply the views so expounded to certain theoretical and practical problems. A brief account is given of the distribution of intelligence, of its diagnosis by means of mental tests, of its relations to will, and to emotion. The book concludes with two chapters embodying applications of the results achieved to questions of scholastic and moral training.

(2) Prof. Swift's book consists of a series of popular essays upon the psychology of daily life. It is not a systematic survey of the whole field of mental hygiene. Rather the choice of subjects has been determined, as the preface acknowledges, by the author's personal interests. It includes, however, several important topics upon which recent investigation has thrown great light—topics which are closely related to everyday problems, although the relation has not always been recognised: for example, the psychical aspect of matters commonly considered to be primarily physiological, such as digestion, fatigue, and biological adaptation. There are also chapters upon matters the psychological and practical aspects of which are alike better recognised—testimony and rumour, memory-training, habit-formation, and learning generally. Illustrative material is drawn not only from the latest researches, but also from a wide reading of fiction, biography, and general literature. A couple of papers are inserted upon problems connected with unconscious processes of the mind, such as multiple personality and the curiosities of recollection. But here the point of view and the data are not always quite up-to-date.

(3) In the two foregoing books Dr. Goddard and Prof. Swift scrupulously avoid all mention of psycho-analytical doctrines. Dr. Lay's book barely refers to any others. His chapters attempt an interesting and even urgent task. The recent work of the psycho-analytic school has emphasised the important part played by unconscious tendencies in the formation of human personality. The simpler of these tendencies appear to be inherited, much as instincts are inherited by lower animals, and first emerge during the immature period we term childhood. The more complex are acquired through experience after birth; but even these are largely built up during the first few years of life. It follows that parents and teachers who view their task as comprising not merely the education of the intellect, but also the training of the character, should be instructed in the nature and properties of these unconscious tendencies, since, just because they are unconscious, they are so easily overlooked and so subtly persistent. The application, therefore, to educational problems of the better-established facts and principles embodied in the doctrines of psycho-analysis constitutes a fruitful field of discussion.

In his book Dr. Lay has set himself to attack this field. Twenty years of teaching in a secondary school, he tells us, has convinced him that "the modes of thinking on the part of children are irremediably (without the teacher's knowing of the effects of the unconscious) twisted, and that they are so by virtue of their numerous complexes." Unfortunately, Dr. Lay's own mode of thinking is apt at times to become, like his sentences, itself a little involved; and one may venture to doubt whether the teacher or the parent whose first introduction to psycho-analysis is obtained from the present volume will reach a very clear conception of the nature or the value of the new doctrines. He is required to think in very technical terms—of *"libido,"* its "displacement," its "transference," and its "sublimation," of "identification" and its two forms of "projection" and "introjection," of "censorship" and "resistances" and "ambivalences" and "compensations." And for all these concepts very little empirical evidence is adduced.

Dr. Lay's method of demonstration inclines, too, almost exclusively to an *a priori* form: *"It is inconceivable* that all the sights, sounds, and other impressions we have had should not have an effect upon each other and so upon our present constitution . . ."; *"It seems in every way more rational to suppose* that conscious and unconscious thought and action are causally connected in both directions. . . ." This is scarcely the soundest type of scientific reasoning. Apart from a casual reference to William James (who incurs Dr. Lay's censure because he devoted to the sex-instinct only one page out of a thousand), pre-Freudian psychology is almost entirely ignored. That Ward and Stout and McDougall had constantly emphasised the importance of unconscious dispositions; that James, McDougall, and Shand had elaborated a widely accepted theory of instincts and emotions as the true basis of character; that the part played by conflict and repression in the formation of character had been succinctly stated by Stout; that the formation of a sentiment of maternal love has been described by Shand and McDougall in terms almost the same as Freud and Jung have used to describe the formation of parental complexes; that the nature and development of the sex-instinct had been closely studied by Stanley Hall and Havelock Ellis—of this the lay parent and the lay teacher learn nothing. Nor would they gather that for knowledge of the nature of the child's unconscious processes, of his complexes, his repressions, and his fantasies, the psycho-analyst is still almost entirely dependent upon the results gained by analysing the minds of abnormal adults, and can scarcely quote more than tiny fragments of first-hand observation made upon young and normal children. The psycho-analyst will perhaps contend that the earlier writers did not grasp the profound and all-pervading significance of the facts they partly formulated. But it is scarcely fair to suggest that upon such problems, before the new era of Freud had dawned, "no really scientific observations had been made." This is not the attitude or the method of Freud himself.

Yet, despite his shortcomings as a man of science and as a psychologist, Dr. Lay, as a practical teacher, has interspersed his somewhat diffuse and theoretical disquisitions with many shrewd observations upon child life, with many suggestive deductions from his master's principles, and with many opportune recommendations for home-training and for class-room procedure.

Three Philosophers.

(1) *Aristotle.* By Dr. A. E. Taylor. Revised edition. (The People's Books.) Pp. 126. (London and Edinburgh: T. C. and E. C. Jack, Ltd.; T. Nelson and Sons, Ltd., 1919.) Price 1*s.* 3*d.* net.

(2) *Auguste Comte.* By F. J. Gould. (Life-stories of Famous Men.) Pp. v+122. (London: Watts and Co., 1920.) Price 3*s.* 6*d.* net.

(3) *Thomas Henry Huxley: A Character Sketch.* By Dr. Leonard Huxley. (Life-stories of Famous Men.) Pp. vii+120. (London: Watts and Co., 1920.) Price 3*s.* 6*d.* net.

AT a time when constitutions are in process of adoption for so many unfamiliar areas on the map, it seems singularly appropriate that some attention should be diverted from such modern problems as relativity and Bolshevism to the older struggles and the ideas to which they gave rise. An excellent opportunity is provided by the three books under notice.

(1) Dr. Taylor's "Aristotle" is not quite in the same class as the others, for Aristotle's life occupies but little space. The author directs attention to the debt we owe to "the Stagyrite" for many of the commonest ideas in our language, and his avowed object is "to help the English reader to a better understanding of such familiar language and a fuller understanding of much that he will find in Dante, Shakespeare, Bacon, and Milton." It is a pity that he has not managed to avoid the frequent use of "logical" terms, probably unintelligible to those whose education has been merely commercial. It is possible that this usage has been deliberate, in order to support Aristotle's contention that a curriculum which includes only "useful" subjects does not constitute education at all—a doctrine that has not yet lost all its supporters. The book gives a fairly comprehensive sketch of Aristotle's progression, from "first philosophy" to physics, and on to practical philosophy—*i.e.* ethics and politics. It is interesting to recall that Aristotle objected equally to democracy and to oligarchy as understood by the Greeks, so that his ideal State would scarcely find favour with many of our present-day politicians. Dr. Taylor is impartial in dealing with points of difference between Aristotle and his quondam teacher Plato, but he does little to help to account for the ascendancy of Aristotle for so many centuries.

(2) It is a far cry from Aristotle to the nineteenth century, though we may regard many of the old Greeks, in spite of their mythology, as rationalists in much the same sense as Comte, though perhaps

not in the same degree. Comte's biography is the work of a professed disciple, but he seems unable to make the apostle of "Humanity" a sympathetic figure. There is a feeling of something lacking in the book in this respect. There is no evidence that it is intended only for the elect, and the ordinary reader may easily be repelled, even if he belongs to the class, so generally miscalled the proletariat, to whom Comte expected his social philosophy to appeal most powerfully. His Positivism certainly made a great impression on those of the working class who attended his free lectures on astronomy, the first science to become "positive" in Comte's view. The Positivist calendar, given as an appendix, is interesting, though to the general reader many of the names are quite unknown.

(3) This book is a really fascinating character-sketch of Huxley by his son, and the great "agnostic" stands out a vivid, rugged, but very sympathetic figure, an honest seeker after truth, a resolute opponent of "dogma." We meet also the outstanding personalities of his contemporaries, Darwin, with Hooker, Tyndall, and other members of the famous X Club. Had the question arisen forty or fifty years earlier, what would we not have given to be present at a symposium debating Einstein's theory of relativity? Huxley's indictment of "the Church" is as thorough as his championship of Darwin, and might be unanswerable could we altogether ignore the limitations of the human intellect. His logical conclusion that his views precluded the hope of future rewards as well as the fear of future punishments enforced on him the duty of living the most upright of lives, and Sir S. Walpole could not have been by any means the only one to endorse the verdict of the little girl who emphatically declared: "I think Prof. Huxley is the best man I have ever known."

W. W. B.

Our Bookshelf.

The Dyeing Industry. Being a third edition of *Dyeing in Germany and America.* By S. H. Higgins. Pp. viii+189. (Manchester: At the University Press; London: Longmans, Green, and Co., 1919.) Price 8*s.* 6*d.* net.

In the first edition of Mr. Higgins's book (Nature, November 7, 1907, p. 4) the subject-matter presented was mainly novel in character, giving, as it did, the author's impression of the state of development of the dyeing industry in Germany, Austria, and the United States. A second edition was issued during the war period (Nature, June 4, 1917, p. 303), and the present notice refers to a third edition.

It is gratifying to find such a steady demand for technical literature, which presumably arises from new readers, but the revised title of the book, "The Dyeing Industry," may lead such to look for a systematic treatment of the various aspects of the industry. This is not attempted in the book, the only new features of which are the inclusion of some of the author's recent valuable contributions to current literature and an extension of the section dealing with the manufacture of dyestuffs—which, by the way, is a quite distinct industry.

It is to be hoped that the author will find an opportunity of producing a book dealing with the dyeing industry of the present day, since information gathered so long ago as 1907 is unlikely now to represent the position with regard to such a rapidly developing scientific industry as dyeing.

W. M. G.

A Handbook of British Mosquitoes. By Dr. William Dickson Lang. Pp. vii+125+5 plates. (London: British Museum (Natural History), 1920.) Price 20*s.*

Dr. Lang's work makes it possible to determine with comparative ease most, if not all, British mosquitoes, including, so far as they are known, the larvæ in their various instars. The book consists mainly of three sections—(1) Introductory; (2) Identification; and (3) Systematic Account. In the introductory section are given the characters by which mosquitoes may be distinguished from gnat-like flies, a general account of the life-history, and such discussion of the morphology of the adults and larvæ as is necessary for purposes of identification. The second section is treated in an eminently practical manner, and the interpretation of the directions, lucid themselves, is rendered simple by the numerous excellent illustrations. In the third section the taxonomic aspect is considered, and our knowledge of each species summarised.

Dr. Lang's work will prove of great value to all interested in mosquitoes, and particularly to those who find the existing monographs on these insects too technical or involved. It gives a clear insight into the characters used in distinguishing these insects and their larvæ, but it must be remembered that the value of some of the points used, although great in separating the British species, is really exaggerated, and, therefore, neither they nor the sequence of instructions can be rigidly applied to foreign mosquitoes.

Volumetric Analysis. By J. B. Coppock. Second edition, revised and enlarged. Pp. 100. (London: Sir Isaac Pitman and Sons, Ltd., n.d.) Price 3*s.* 6*d.* net.

Though this volume has no special features to distinguish it from others of a similar type, it should prove useful to elementary students of chemistry preparing for examinations of Intermediate B.Sc. standard.

C

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Colour of the Night Sky.

So far as I have been able to learn, little or nothing is known about the colour of the night sky. The light is too faint for ordinary visual discrimination of colour, which disappears with diminishing intensity of illumination much before the light itself ceases to be perceptible.

I have obtained evidence, both visual and photographic, that the clear sky at night is much yellower or less blue than the clear, or even the cloudy, sky at twilight.

The visual observations were made in the following way:—Two gelatine films were prepared, one dyed yellow with flavazine and the other with methylene-blue, the relative intensities being adjusted by trial to give the effects that will presently be described. The films were mounted edge to edge at the end of a pasteboard tube, which was pointed at the sky. During the daytime the yellow film was confidently pronounced by all observers to be the brightest, the difference being too marked to be embarrassed by colour difference. As twilight advanced the Purkinje phenomenon came into evidence, and the blue film became much the brighter. This remained the case when the light had waned so far that the colour sensation had disappeared. As the stars came out the predominance of the blue became less marked, and before the Milky Way was distinguished there was equality. Finally, when the Milky Way was conspicuous the yellow film was notably the brighter, whether the tube was pointed to the Milky Way or to other parts of the sky.

The changes described were very marked. Their general course was the same whether the sky was clear or cloudy at any particular stage. The first change, when blue becomes predominant, is due solely to physiological causes. But the second change, which makes the yellow predominant again, occurs below the "threshold" of colour-vision, and, according to received views, there should be no marked complication from physiological causes within this range. Accordingly we may conclude that the observation affords definite evidence that the night sky is yellower or less blue than the day sky.

This conclusion has been confirmed photographically. A yellow and a dense blue filter were selected, and an Ilford panchromatic plate was exposed to the sky under these. It was seen at a glance that the density under the blue filter was the greater for the twilight sky, while for the night sky this relation was reversed.

The results point to the conclusion that the light of the night sky, whatever the cause of it may be, is not due to the scattering of sunlight by rarefied gas situated beyond the earth's shadow. The comparative absence of polarisation, formerly found, points to the same conclusion. RAYLEIGH.

Beaufront Castle, Hexham, August 20.

University Grants.

THE article and letters in NATURE upon the subject of university finance are very timely. It is essential that the country should be alive to the perilous condition of the universities from a financial point of view.

The raising of fees that has just taken place can be only a partial remedy. Fee revenue before the war provided at the various universities at the most for 40 per cent. of the necessary expenditure, the average being about 33 per cent. The recent increase in fees will barely re-establish the pre-war percentage.

I agree with the Principal of Birmingham University that the stipends of the non-professorial staffs must be increased; the urgency is not less great for the salaries of the professorial staffs. In London the utmost that has been done for the professorial staff is to increase the minimum full-time salary from 600*l.* to 800*l.*

Now the majority of the London professors receive the minimum. Considering the responsibilities of a university professor, what is 800*l.* a year for a man in that position under present conditions? Such a prospect will not induce young men of the right calibre to make university teaching their profession.

It is true that the Government has made non-recurring grants to pay off war losses, and for the time being doubled the grants. Having regard to the condition of the National Exchequer, the Government has perhaps done as much as could be expected for the current year. The all-important question for the universities is: What is the Government going to do next year?

It must be remembered that the maintenance grants are made for periods of five years, and are then revised. Revision was due in 1915, but was impossible during the war. The period 1910–15 had been one of unusual activity and development in all the universities. It was the general expectation in 1914 that the grant for the quinquennium 1910–15 would be doubled for the period 1915–20. By doubling the 1910–15 grant now the Government has done no more than redress the disadvantage due to the depreciation in the value of money.

Having that in mind, the deputation of Vice-Chancellors and Principals in 1919 impressed on the President of the Board of Education and the Chancellor of the Exchequer that the smallest grant that would meet the needs of the moment would be the 1910–15 grant quadrupled. That will do no more than enable the universities to carry on; it will not provide the capital necessary for new buildings, new plant, and equipment, nor will it enable justice to be done to the older men who bore the heat and burden of the day of pioneer work before the time of the establishment of superannuation funds.

There are many such men due to retire in the next few years; they are entitled to treatment at least as generous as that given to schoolmasters by the Fisher Act. GREGORY FOSTER.

University of London, University College, August 23.

THE only elements of our society which seem to benefit from the great increase in the wealth of the world through science are those which it will be one of the hardest problems of reconstruction to divert into more productive and honourable means of livelihood. Those who sow the seed and reap the harvest alike, year by year, by their labours seem to be able only to increase their dependence upon private charity and public doles. Universities sow the seed, and their claims, like the claims of the farmer for seed for his future harvest, ought to be absolutely the first charge upon the yearly revenue. It is as idle to say the country cannot afford it as it would be for a farmer to grudge the seed for his next year's crop. It affords a plethora of most expensive evils and unnecessary luxuries. In the spirit of one of the early Methodist preachers, I feel, whenever I see a specially sumptuous motor-car, "There, but for the grace of Parliament, goes a professor of chemistry"; and even a humble two-seater might in happier circumstances have become a demonstrator!

Under the scheme of a professor of literature with fifteen years' experience of fostering scientific research with the Carnegie Trust, a million of the taxpayers' money is now well on its way to provide more sumptuous motor-cars rather than professors of chemistry, and another two millions or so to foster dye-making as it is understood in the City by directors more innocent of the art than our early woad-besmeared ancestors. To such ill-informed and costly efforts the complete indifference with which science was regarded in official circles before the war is surely preferable. It may be difficult to break with the habit of a hundred years in patiently expecting a miraculous change of attitude on the part of the Government towards the debt- and wealth-producing elements of the community respectively. A more practical and practically attainable objective would, it seems to me, be for the universities to ally themselves with Labour to provide saner government. Incidentally, some long overdue reforms of the universities might then be effected, and the paralysing and deliberate stranglehold of the old vested interests upon science broken, once for all.

I may merely be more outspoken than my colleagues, but I believe I am more scientific in diagnosing the trouble before seeking a remedy. The failure of a century's efforts on the part of scientific men is not due to ignorance, apathy, or any other negative cause. But for the clever, tireless, and all-embracing activities of all those to whom science spells *finis*, the existing absurdities would not survive a year, and against this combination Labour is the only real force. The unpopularity of the proposed remedy with all those who have proved themselves such good friends of science in the past is, to my mind, an unsolicited testimonial to its efficiency from a quarter eminently in a position to judge.

FREDERICK SODDY.

The Separation of the Isotopes of Chlorine.

IN my letter to NATURE of June 17 I showed that inappreciable separability of the isotopes of chlorine by a special class of chemical change would be difficult to reconcile with Nernst's heat theorem, and in a later issue of NATURE (July 15), at the request of Prof. Soddy, the argument was more fully given. Mr. Core (NATURE, July 29) has endeavoured to remove the difficulty to which attention was directed by me. With most of his deductions I agree, but I am not satisfied that he has reconciled Nernst's theory with the inappreciable separability of the isotopes by the specified chemical method. Mr. Core contends that the solid compound ClCl′ which would actually be formed in the chemical change represented by the equation

$$Cl_2 + Cl'_2 = 2ClCl'$$

is a solid solution—since in the crystal the molecules would be indifferently oriented as ClCl′ or Cl′Cl—and Nernst's theorem cannot be applied to solutions.

Now Nernst states his theorem as follows:

"I have been led to the conclusion that not only do A and U become equal at the absolute zero of temperature, but that their *curves touch asymptotically at this point.* That is to say,

$$\lim \frac{dA}{dT} = \lim \frac{dU}{dT} \text{ (for T=o).}$$

It is to be remembered that this equation is only strictly applicable to pure solid or liquid substances" (H. T. Tizard's translation of Nernst's "Theoretical Chemistry," 1916 edition, p. 746).

Again, Nernst (*loc. cit.*, p. 748) identifies liquid with amorphous substances

Further, Planck limits the application of his more general enunciation of Nernst's theorem to chemically homogeneous substances, which he defines as substances made up of nothing but molecules of the same kind (Max Planck, "Thermodynamik," § 67).

Neither of these definitions appears to me to exclude the crystalline modifications of ClCl′, which are stable at finite temperatures. But even if it be conceded that the intention was to exclude such solid compounds, the difficulty I find in reconciling Nernst's heat theorem with the inappreciable separability of the isotopes by the specified chemical method has not been removed, for the solid substances taken in performing the thermodynamic cycles can (without modifying the reasoning) be the liquid (amorphous) forms.

D. L. CHAPMAN.

Jesus College, Oxford.

The Scratch-Reflex in the Cat.

I HAD lately the opportunity of examining in a young cat eight weeks old the conditions of the scratch-reflex, and the results would seem to be worth noting. The animal had been through an unusually heavy day of play on a hot day, and in the evening was lying asleep on the lap of one of her friends in a profound sleep. I thought this a good occasion for finding out what reactions she would show to gentle mechanical stimuli. A very light touch with a wooden match on the conchal surface of the pinna, or one extending to the meatus, produced immediate response as follows:—First, the facial muscles on the same side twitched irregularly; this ceased, and then the fore foot was moved irregularly towards, but not so far as, the ear; when this had ceased there occurred at once a *rhythmical* movement of the hind limb, with a rate closely similar to that of the scratch-reflex of the dog, the hind foot, as in the fore foot, being brought *towards,* but not up to, the ear.

This unusual sleep lasted for a quarter of an hour, during which repeated light mechanical stimuli of various kinds failed to wake the animal, and the above series of reactions was frequently evoked, but none of the later attempts produced results so strongly marked as the first. I was unfortunate then in not having read more than an abstract of the paper in the *Journal of Physiology* of December, 1917, by Prof. Sherrington on "The Pinna Reflexes in the Cat." He has now very kindly sent me a copy of the paper, and I see how much better I might have marked out the receptive field of this reflex if I had known the accurate observations he has made on it.

I examined in this animal then and since the various regions of the back and flanks, and have found there no receptive field like that of the dog, which seems to be in accordance with the experience of others. Since the first occasion I have found the foregoing reactions present many times, but the sleep has always been lighter and the results less orderly and striking.

WALTER KIDD.

Chalet le Mourezin, Château d'Oex, Switzerland, August 13.

Portraits Wanted.

I SHOULD greatly appreciate any information readers of NATURE may be able to give me which would lead to the discovery of portraits of any of the following early myriapodologists:—Shaw (who published a paper in 1789, Trans. Linn. Soc., vol. ii., p. 7), W. E. Leach, George Newport, John Edward Gray, and John Curtis. The last four all published papers during the first half of the nineteenth century.

S. GRAHAM BRADE-BIRKS.

16 Bank Street, Darwen, Lancs, August 19.

The Christian Revelation and Modern Science.[1]

By the REV. E. W. BARNES, M.A., Sc.D., F.R.S., Canon of Westminster.

"I am the Light of the world."—ST. JOHN viii. 12.

I HAVE been asked to preach here to-day in connection with the meeting of the British Association which has been held in this city during the past week. My subject is Christ, the Light of the world, and I ask you to think of the Christian revelation and scientific progress. For more than a hundred years there has been strife—sometimes veiled, but more often open—between "religion and science." I use the popular phraseology. More accurately, opinions as to the origins of the earth and of man which were held as a result of Christian tradition have been directly challenged by a succession of novel theories put forward by men of science. At the beginning of last century the foundations of geology were being laid, largely in this country. Gradually it became clear, from a study of our rocks and their fossil remains, that the earth had an almost unimaginable antiquity. The coal which we dig is all that is left of vast tropical forests that once flourished here for tens of thousands of years. In successive ages of vast duration the most diverse forms of animal life have existed in these lands. The East of England has repeatedly for long periods been submerged beneath the sea. The climate has varied from tropical heat to arctic severity.

Such knowledge is now a commonplace. But when it was being established by patient discovery during the early part of last century Christian theologians showed violent hostility to the new ideas. The curious may examine the expression of this hostility in Bampton Lectures of the period, which are now happily forgotten. On second-hand bookstalls it is not uncommon to find pathetic attempts to reconcile geology and Genesis such as were continually made even to our own time. But truth triumphed. Just as two centuries earlier the Roman Church had failed to prevent men from receiving the then new knowledge that the earth was not the fixed centre of the universe, so the new geological ideas won their way despite religious prejudice. Galileo triumphed; it is agreed that the earth moves round the sun; heliocentric books were removed from the Roman Index in 1835. The early nineteenth-century geologists triumphed; it is agreed that life has existed on this earth for something like a hundred million years. Though in each case the new views are directly opposed to those which Christianity took over from Judaism, we accept them with confidence and surely without harm to our faith in Christ.

But sixty years ago a far more vital controversy began when the Biblical account of man's origin was disputed. A series of discoveries in caves and river-beds in England and in France made it clear that primitive men had lived here when the mammoth, the cave lion, and the rhinoceros flourished in Western Europe. Evidence quickly accumulated which showed that even in this corner of the world human beings existed more than a hundred thousand years ago. Scarcely had these novel conclusions been reached when a scientific theory was put forward which to the great majority of the religious people of the time seemed destructive of essentials in our faith. It was in the year 1859 that Darwin, in his book "The Origin of Species," urged the truth of the doctrine of evolution. At the ensuing Oxford meeting of the British Association, Bishop Wilberforce denounced the idea that man shared a common ancestry with the higher apes. His speech showed deplorable prejudice; it contained a grave error in taste, and Huxley's dignified rebuke of the Bishop is still remembered. For forty years after that famous encounter evolution was a *casus belli* between religion and science. Christian opinion refused to accept the new doctrine, and religious teachers traversed it by arguments good and bad. It is not fair to regard them with the scorn which the younger people of to-day, trained in modern science, not seldom feel.

Evolution was, and still is, not an observed fact, but a very probable theory. Our forefathers saw that acceptance of it meant the abandonment of the story of Adam; it meant giving up belief in the Fall, and in all the theology built upon it by theologians from St. Paul onwards. Half a century ago, the evolutionary view of man's origin meant that what then appeared to be the strongest reasons for the belief that man has an immortal soul had to be set aside. But truth has triumphed. In our own time the leaders of Christian thought have, with substantial unanimity, accepted the conclusion that biological evolution is a fact; man is descended from the lower animals. It is even becoming common to say that there is no quarrel between science and religion. But let us be honest. There has as regards the origin of man been a sharp conflict between science and traditional religious belief, and the battle has been won by science. Furthermore, let us not when driven from one position take up another that may have to be abandoned. It is dangerous to assert that, although God may not have specially created man, nevertheless He did specially create life. Probably the beginning of terrestrial life was but a stage in the great scheme of natural evolution. We may even expect that some day in the laboratory the man of science will produce living from non-living matter.

The time has come when we must not try to evade any implications of the theory of natural evolution. We must, not silently, but explicitly, abandon religious dogmas which it overthrows. We must, moreover, avoid the temptation to allegorise beliefs which it is no longer possible

[1] Sermon preached in St. John's Church, Cardiff, on Sunday, August 29, to members of the British Association.

to hold. Allegory has its value, but it is misused when we employ it to obscure the revolutionary consequences of new knowledge. Religion is too important for us to base it upon, or to join it to, any theories of the nature of the universe that are doubtful or untrue. If Christ is the Light of the world, all intellectual discovery must be a part of His revelation. If He rightly explained the nature and purpose of God, then the more accurately we discover how God planned and guided the universe so that men have come to exist upon earth, the more natural will it be to accept Christ's teaching. If, on the contrary, the progress of knowledge really discredits the Christian faith, in so far as that faith comes from Jesus Christ, we must sadly admit that Christ cannot have been the Light of mankind. Whatever the consequences, we must accept truth by whomsoever it may have been discovered. A religion not based on truth is vain. A faith that fears the progress of knowledge anticipates its own dissolution.

Now, the Christian faith is belief in Christ, in His Person, and in His teaching. If Jesus was Divine, His spiritual revelation was without error, His example perfect. In so far as He was man we expect His secular knowledge to have been that of the Galilean carpenter's son. But we can no longer call ourselves Christians if we find that we are forced to admit that He was morally imperfect or mistaken in His view of God or of man's relation to God. I contend that the progress of science has not forced us to make any such admission. It has not destroyed the spiritual infallibility of our Lord, or done anything to upset His teaching as to the nature of God, or as to man's nature and destiny. It has rather, as I hold, confirmed His insight and made His spiritual wisdom more profoundly impressive.

Traditional Christian belief was built up of other things besides Christ's teaching. The early Christian Church took over the old Jewish Scriptures because it deemed them inspired by God. It placed among its sacred books writings of St. Paul and other earlier followers of the Lord because it found that they reflected the Mind of the Master. But there never was a time when thoughtful Christians could thoughtfully have maintained that the Jewish Scriptures were free from moral and historical error. The cursing Psalms are obviously un-Christian. Books like Kings and Chronicles are rival histories which disagree in spirit and in detail. As a matter of fact, the Church has never formally defined inspiration. We may say truly that inspired books are of peculiar spiritual value; but we may find such value in St. Paul's teaching, though we freely admit that his arguments were sometimes unsound. If we discover that old Christian beliefs which did not come from Christ are erroneous let us not be troubled. For Christianity the perfection of Christ's religious teaching and His revelation of His own supreme excellence are alone of decisive importance. Views of ancient Jews or of early Apostles we can abandon when we discover that they were wrong. Christianity is belief in Christ as Way, Truth, and Life; belief that He was the Light of the world, the Guide of the spiritual evolution of humanity. It is not belief in the scientific value of Genesis or even in the infallibility of St. Paul. Grasp this fact firmly and you will understand that last century's tragic quarrel between religion and science had its origin in a natural, but none the less deplorable, mistake. The mistake was natural, for there is so much of supreme value in the books of the Bible that men will always venerate them profoundly. In the recent past veneration led to exaggeration, to the claim of infallibility. Let us thank God that men of science have forced us to get a fuller, if more difficult, type of understanding of the value of the Bible.

But some of you may say, Has not the new knowledge made it impossible to accept the teaching of Jesus with regard to God and human immortality? Can we accept evolution and yet believe that God, a loving Father, made the world? Can we accept the idea that man and the gorilla have sprung from a common stock and yet hold that man has an immortal soul? I answer emphatically that we can. I remain sure that God, Who is Love, made and rules the world, certain that man was created that he might enjoy eternal life in communion with God in the world to come. Do you doubt? Reflect for a few moments. Surely the universe had a beginning, and therefore a Creator. It cannot be a meaningless dance of atoms or a whirl of electrons that has gone on for an infinite time. Surely, too, evolution describes a wonderful development, an upward progress, which implies a design in the mind of God. Surely man is on earth the present end of this process, and his spiritual qualities, his love of beauty, goodness, and truth, are its crown. Surely, moreover, the God Who by a design extending over hundreds of millions of years has called these spiritual qualities into existence is Himself a spiritual Being Who made spiritual man for communion with Himself. And, last of all, surely the finest products of evolution have not been made for nothing. And yet, in the distant future, when all life vanishes from the earth, as it certainly must, heroes and saints will in vain have gained knowledge of God, in vain have spent their strength, unless they continue to live eternally in the spiritual world.

Evolution seemed disastrous to faith two generations ago because men fixed their attention narrowly on but one part of the process. Now a wider vista seems to be coming into view as theories are tested by experiment and unified by the speculative reason. From some fundamental stuff in the universe the electrons arose. From them came matter. From matter life emerged. From life came mind. From mind spiritual consciousness developed. At every stage, in this vast process and progress, something new has come, we know not how, into existence. There was a time when matter, life, mind, the soul of man were not; but now they are. Each has arisen as part of a vast scheme planned by God. And the soul of man is the glory of the whole design. Because of the soul within him man, as Jesus taught, is meant to be the child of God. As our souls grow through the quickening power of the Spirit of Christ we can on earth know and serve the Father of us all and begin to enjoy that Divine communion which is eternal life. The Christ Spirit within us, the "quality of deity," as it has been called, separates us from the animals whence we have sprung just as life separates them from the matter of which they are made. And through the Spirit of Christ we put on immortality, for the things that are of God are eternally with God.

Science describes the process by which man has come into being. Religion takes man as he is and offers him guidance towards his spiritual destiny. Between the religious revelation of Jesus and modern science there is no opposition. The two dovetail into one another with singular exactness. Evolution describes facts; the ultimate meaning of those facts Christ's teaching discloses. We need faith to accept the Lord's message; we cannot prove its truth by the methods of scientific inquiry, for the spiritual world is a type of reality which the organs of sense will not reveal. But by living the Christian life, by prayer and communion with God, we can continuously strengthen the faith which is not sight, and become ever more confident that the Lord was in very truth the Light of the world.

The British Association at Cardiff.

THE Cardiff meeting of the British Association came to an end on Sunday morning, when the Lord Mayor of Cardiff (Councillor G. F. Forsdike) and the Corporation, with the general officers of the Association and some of the members, attended the service at St. John's Church. The Association sermon was preached by Canon E. W. Barnes, F.R.S., and we are glad to be able to reproduce it this week. The scientific work of the Association concluded on Friday evening, August 27, when Sir Daniel Hall delivered a stimulating discourse under the title of "A Grain of Wheat from the Field to the Table." A comprehensive vote of thanks to the Lord Mayor, Corporation, and citizens of Cardiff was carried with acclamation, and was responded to by the Lord Mayor and by Dr. W. Evans Hoyle, whose valuable work as local secretary was much appreciated by all.

There were 1378 members present during the week, but the meeting, though relatively small, has been particularly interesting from the scientific point of view. Among the new features was a conference on "Science applied to Public Services," held on August 26, when Mr. F. E. Smith, director of scientific research at the Admiralty, described the admirable scheme of research which has recently been introduced (see NATURE, April 22, p. 245). Prof. C. F. Jenkin, Mr. J. Barcroft, Sir Francis Ogilvie, and Dr. J. W. Evans referred to similar research work in other Government Departments. It was felt that a similar conference, with perhaps some description of results obtained, so far as they can be made public, and opportunity for free and adequate discussion, could usefully be held at each meeting.

As we stated last week, a message was sent from the inaugural meeting to the King in Scotland, where the Association is to meet next year. The message was as follows:—"The members of the British Association for the Advancement of Science desire to express their loyal devotion to your Majesty, and at their meeting in the Principality of Wales hope that they may be permitted to congratulate your Majesty on the splendid work done by the Prince of Wales, which has drawn towards him the thoughts and the hearts of the whole Empire."

The King, in thanking the Association through Sir Charles Parsons, the retiring president, for this loyal greeting, added:—"I feel greatly touched at the kind references to my son, which are the more appreciated coming as these do from members of this distinguished society assembled in the Principality of Wales. I shall follow your deliberations with close interest, and I gratefully recognise all that is being done for the advancement of civilisation by the men of science."

There is probably no more remarkable example of the scientific spirit which animates the British Association than that displayed in the allocation of its annual grants for research purposes. Each section of the Association nominates research committees, and most of them apply for small grants to carry out the work and defray the clerical and other incidental expenses involved. A total of about 1000*l.* a year is voted by the Committee of Recommendations to these committees and approved by the General Committee, and every pound of this comes out of the subscriptions of the members. This year the amount voted in grants for research was about 1100*l.*, part of which will be required for expenses of publication. It is hoped some external support will be forthcoming for this branch of the Association's work, and that Government Departments interested in particular subjects will assist in the publication of some of the reports prepared by research committees.

Among the corresponding members and other foreign representatives present at the meeting were:—M. Brieux (Directeur de la Station Agronomique de Rouen, France); M. Bruno (Insp. Gén. des Stations Agronomiques, Paris); Prof. C. J. Chamberlain (Chicago); Prof. R. Chodat (Geneva); Dr. S. I. Franz (George Washington

University, representing the American Association); Prof. A. Gilson (Louvain, Belgium); Prof. R. W. Hegner (Dept. Medical Zoology, Johns Hopkins University); Prof. F. Jaeger (Groningen); Prof. C. A. Kofoid (University of California); Prof. Graham Lusk (Cornell University Medical College); Dr. Naser (representing the International Students' Union, University of Copenhagen); Don G. J. de Osma (Madrid); and Yoshimaro Tanaka (Japan). The General Committee has resolved that national associations for the advancement of science shall in future be invited to send representatives to meetings of the British Association.

The new members of the Council are:—Mr. Joseph Barcroft, Prof. J. Stanley Gardiner, Sir Daniel Hall, Dr. Chalmers Mitchell, and Sir W. J. Pope.

Next year's meeting at Edinburgh, under the presidency of Sir Edward Thorpe, will be from Wednesday, September 7, to Wednesday, September 14. In the following year the meeting will be held at Hull, an invitation tendered by the Lord Mayor and the Town Clerk of that city having been unanimously accepted by the General Committee. No further meetings were actually arranged at Cardiff, but there is a possibility that the meeting in 1923 will be in another northern city, and invitations have been received to meet in Canada in 1924.

Dr. R. V. Stanford, secretary of the Press and Publicity Sub-Committee at Cardiff, who acted as our local correspondent for the meeting, has sent us the following statement, which merits the attention of the Council of the Association and of local committees organising the annual meetings. We are sure that, in making these comments, Dr. Stanford is as desirous of increasing public interest in the work of the Association as we are in publishing it.

"It is difficult to resist taking this opportunity to make one or two observations, though in proposing to offer any criticisms of such a great and venerable institution as the British Association one has rather the feelings of a curate meditating a mild attack on his bishop. There are many members of the Association who are of the opinion that some changes in its policy are not only desirable, but are also rapidly becoming inevitable. Now is the time to take stock of the position, for the recent meeting at Cardiff was really the first normal post-war meeting. The Bournemouth meeting could scarcely be called so, because of its situation and of the very general curiosity regarding war secrets. It is, therefore, rather disappointing to find the membership no greater. What is more disappointing still is that the principal reason for this is the apathy of local people of the educated classes to the presence of the Association. The plain fact remains that it is the exception to find anyone who has even heard of the Association. Is this regrettable state of affairs due to something lacking in these individuals themselves, or is the blame to be laid at the door of the Association? It certainly is not peculiar to Cardiff.

"Everyone will agree that the Association serves three main purposes, namely, to furnish opportunity for scientific workers themselves to get in touch with men working on allied subjects, to try to encourage research, and (what some of us think most important of all) to make scientific knowledge accessible to the general public. It may very well be maintained that the first two of these purposes are being very much better served than the third. Some lay organ of the Press referred to this Cardiff meeting as a 'jamboree of science.' So it was—for the scientific men. But if we were the scouts, where were the delighted parents, who should be such a feature of the entertainment? The point of this analogy is that while the scientific men themselves had four whole days in twelve sections to meet each other and discuss matters in a way of little intelligibility to anyone but themselves, the educated man in the street could only expect a couple of evening discourses to interest him, with a possibility of three more if there was room. The local Press reported the meetings astonishingly well, but such reports, however widely they may be read, do not take the place of a full lecture.

"The importance of scientific work will never be recognised adequately by the general public until they are better instructed as to the practical results which are to be expected from it, and this end can be reached only by the leaders of scientific thought and discovery going out of their way to show the ordinary man that he has a personal interest in the matter. It cannot be pretended that this result is properly secured by five lectures during the whole meeting. We should like to have seen fifty of them, and to have seen them delivered, not to the scientific, but to the non-scientific, public. What is being suggested is not a return to what might be called the lecture habit of the Victorian period, often a perfectly useless type of scientific conjuring entertainment. It is also not necessarily implied that they shall be delivered in the largest hall in the district: sectional meeting rooms would do in many cases.

"Some change in this direction has been referred to above as desirable, and also as inevitable. The inevitability arises from the need which is bound to be felt by the Association of increasing its membership, and consequently its financial resources, rather than the reverse. There are certainly some points that might be thought of, which would result, for example, in a considerable reduction of expenditure for the meeting, and that is a matter which cannot be neglected in these days of high wages and long prices, either by the Association or by its hosts from year to year. A great saving of time and trouble would be effected by the adoption of some method of getting an idea before the meeting as to how many people were coming. This might be done by adding a surcharge to the price of any tickets sold after a certain date. The same consideration applies to excursions: it is easy to lose considerable sums in the way of guarantees for motor transport and meals."

The Internal Constitution of the Stars.*

By PROF. A. S. EDDINGTON, M.A., M.Sc., F.R.S.

LAST year at Bournemouth we listened to a proposal from the President of the Association to bore a hole in the crust of the earth and discover the conditions deep down below the surface. This proposal may remind us that the most secret places of Nature are, perhaps, not 10 to the *n*th miles above our heads, but 10 miles below our feet. In the last five years the outward march of astronomical discovery has been rapid, and the most remote worlds are now scarcely safe from its inquisition. By the work of H. Shapley the globular clusters, which are found to be at distances scarcely dreamt of hitherto, have been explored, and our knowledge of them is in some respects more complete than that of the local aggregation of stars which includes the sun. Distance lends not enchantment, but precision, to the view. Moreover, theoretical researches of Einstein and Weyl make it probable that the space which remains beyond is not illimitable; not merely the material universe, but also space itself, is perhaps finite; and the explorer must one day stay his conquering march for lack of fresh realms to invade. But to-day let us turn our thoughts inwards to that other region of mystery—a region cut off by more substantial barriers, for, contrary to many anticipations, even the discovery of the fourth dimension has not enabled us to get at the inside of a body. Science has material and non-material appliances to bore into the interior, and I have chosen to devote this address to what may be described as analytical boring devices —*absit omen!*

The analytical appliance is delicate at present, and, I fear, would make little headway against the solid crust of the earth. Instead of letting it blunt itself against the rocks, let us look round for something easier to penetrate. The sun? Well, perhaps. Many have struggled to penetrate the mystery of the interior of the sun; but the difficulties are great, for its substance is denser than water. It may not be quite so bad as Biron makes out in "Love's Labour's Lost":

> The heaven s glorious sun
> That will not be deep-search'd with saucy looks:
> Small have continual plodders ever won
> Save base authority from others' books.

But it is far better if we can deal with matter in that state known as a perfect gas, which charms away difficulties as by magic. Where shall it be found?

A few years ago we should have been puzzled to say where, except perhaps in certain nebulæ; but now it is known that abundant material of this kind awaits investigation. Stars in a truly gaseous state exist in great numbers, although at first sight they are scarcely to be discriminated from dense stars like our sun. Not only so, but the gaseous stars are the most powerful light-givers, so that they force themselves on our attention. Many of the familiar stars are of this kind —Aldebaran, Canopus, Arcturus, Antares; and it would be safe to say that three-quarters of the naked-eye stars are in this diffuse state. This remarkable condition has been made known through the researches of H. N. Russell (NATURE, vol. xciii., pp. 227, 252, 281) and E. Hertzsprung; the way in which their conclusions, which ran counter to the prevailing thought of the time, have been substantiated on all sides by overwhelming evidence is the outstanding feature of recent progress in stellar astronomy.

The diffuse gaseous stars are called *giants*, and the dense stars *dwarfs*. During the life of a star there is presumably a gradual increase of density through contraction, so that these terms distinguish the earlier and later stages of stellar history. It appears that a star begins its effective life as a giant of comparatively low temperature—a red or M-type star. As this diffuse mass of gas contracts its temperature must rise, a conclusion long ago pointed out by Homer Lane. The rise continues until the star becomes too dense, and ceases to behave as a perfect gas. A maximum temperature is attained, depending on the mass, after which the star, which has now become a dwarf, cools and further contracts. Thus each temperature-level is passed through twice, once in an ascending and once in a descending stage—once as a giant, once as a dwarf. Temperature plays so predominant a part in the usual spectral classification that the ascending and descending stars were not originally discriminated, and the customary classification led to some perplexities. The separation of the two series was discovered through their great difference in luminosity, particularly striking in the case of the red and yellow stars, where the two stages fall widely apart in the star's history. The bloated giant has a far larger surface than the compact dwarf, and gives correspondingly greater light. The distinction was also revealed by direct determinations of stellar densities, which are possible in the case of eclipsing variables like Algol. Finally, Adams and Kohlschütter have set the seal on this discussion by showing that there are actual spectral differences between the ascending and descending stars at the same temperature-level, which are conspicuous enough when they are looked for.

Perhaps we should not too hastily assume that the direction of evolution is necessarily in the order of increasing density, in view of our ignorance of the origin of a star's heat, to which I must allude later. But, at any rate, it is a great advance to have disentangled what is the true order of continuous increase of density, which was hidden by superficial resemblances.

The giant stars, representing the first half of a star's life, are taken as material for our first boring experiment. Probably, measured in time, this stage corresponds to much less than half the life, for here it is the ascent which is easy and the way down is long and slow. Let us try to picture the conditions inside a giant star. We need not dwell on the vast dimensions—a mass like that of the sun, but swollen to much greater volume on account of the low density, often below that of our own atmosphere. It is the star as a storehouse of heat which especially engages our attention. In the hot bodies familiar to us the heat consists in the energy of motion of the ultimate particles, flying at great speeds hither and thither. So, too, in the stars a great store of heat exists in this form; but a new feature arises. A large proportion, sometimes more than half the total heat, consists of imprisoned radiant energy—æther-waves travelling in all directions trying to break through the material which encages them. The star is like a sieve, which can retain them only temporarily; they are turned aside, scattered, absorbed for a moment, and flung out again in a new direction. An element of energy may thread the maze for hundreds of years before it attains the freedom of outer space. Nevertheless, the sieve leaks, and a steady stream permeates outwards, supplying the light and heat which the star radiates all round.

That some æthereal heat as well as material heat exists in any hot body would naturally be admitted; but the point on which we have here to lay stress is

*Opening address of the president of Section A (Mathematical and Physical Science) delivered at the Cardiff meeting of the British Association on August 24.

that in the stars, particularly in the giant stars, the æthereal portion rises to an importance which quite transcends our ordinary experience, so that we are confronted with a new type of problem. In a red-hot mass of iron the æthereal energy constitutes less than a billionth part of the whole; but in the tussle between matter and æther the æther gains a larger and larger proportion of the energy as the temperature rises. This change in proportion is rapid, the æthereal energy increasing rigorously as the fourth power of the temperature, and the material energy roughly as the first power. But even at the temperature of some millions of degrees attained inside the stars there would still remain a great disproportion; and it is the low density of material, and accordingly the reduced material energy per unit volume in the giant stars, which wipes out the last few powers of 10. In all the giant stars known to us, widely as they differ from one another, the conditions are just reached at which these two varieties of heat-energy have attained a rough equality; at any rate, one cannot be neglected compared with the other. Theoretically there could be conditions in which the disproportion was reversed and the æthereal far outweighed the material energy; but we do not find them in the stars. It is as though the stars had been measured out—that their sizes had been determined—with a view to this balance of power; and one cannot refrain from attributing to this condition a deep significance in the evolution of the cosmos into separate stars.

To recapitulate. We are acquainted with heat in two forms—the energy of motion of material atoms and the energy of æther waves. In familiar hot bodies the second form exists only in insignificant quantities. In the giant stars the two forms are present in more or less equal proportions. That is the new feature of the problem.

On account of this new aspect of the problem the first attempts to penetrate the interior of a star are now seen to need correction. In saying this we do not depreciate the great importance of the early researches of Lane, Ritter, Emden, and others, which not only pointed the way for us to follow, but also achieved conclusions of permanent value. One of the first questions they had to consider was by what means the heat radiated into space was brought up to the surface from the low level where it was stored. They imagined a bodily transfer of the hot material to the surface by currents of convection, as in our own atmosphere. But actually the problem is, not how the heat can be brought to the surface, but how the heat in the interior can be held back sufficiently—how it can be barred in and the leakage reduced to the comparatively small radiation emitted by the stars. Smaller bodies have to manufacture the radiant heat which they emit, living from hand to mouth; the giant stars merely leak radiant heat from their store. I have put that much too crudely; but perhaps it suggests the general idea.

The recognition of æthereal energy necessitates a twofold modification in the calculations. In the first place, it abolishes the supposed convection currents; and the type of equilibrium is that known as radiative instead of convective. This change was first suggested by R. A. Sampson so long ago as 1894. The detailed theory of radiative equilibrium is particularly associated with K. Schwarzschild, who applied it to the sun's atmosphere. It is perhaps still uncertain whether it holds strictly for the atmospheric layers, but the arguments for its validity in the interior of a star are far more cogent. Secondly, the outflowing stream of æthereal energy is powerful enough to exert a *direct mechanical effect* on the equilibrium of a star. It is as though a strong wind were rushing outwards. In fact, we may fairly say that the stream of radiant energy *is* a wind; for though æther waves are not usually classed as material, they have the chief mechanical properties of matter, viz. mass and momentum. This wind distends the star and relieves the pressure on the inner parts. The pressure on the gas in the interior is not the full weight of the superincumbent columns, because that weight is partially borne by the force of the escaping æther waves beating their way out. This force of radiation-pressure, as it is called, makes an important difference in the formulation of the conditions for equilibrium of a star.

Having revised the theoretical investigations in accordance with these considerations (*Astrophysical Journal*, vol. xlviii., p. 205), we are in a position to deduce some definite numerical results. On the observational side we have fairly satisfactory knowledge of the masses and densities of the stars and of the total radiation emitted by them; this knowledge is partly individual and partly statistical. The theoretical analysis connects these observational data on the one hand with the physical properties of the material inside the star on the other. We can thus find certain information as to the inner material, as though we had actually bored a hole. So far as can be judged, there are only two physical properties of the material which can concern us—always provided that it is sufficiently rarefied to behave as a perfect gas—viz. the average molecular weight and the transparency or permeability to radiant energy. In connecting these two unknowns with the quantities given directly by astronomical observation we depend entirely on the well-tried principles of conservation of momentum and the second law of thermodynamics. If any element of speculation remains in this method of investigation, I think it is no more than is inseparable from every kind of theoretical advance.

We have, then, on one side the mass, density, and output of heat, quantities as to which we have observational knowledge; on the other side, molecular weight and transparency, quantities which we want to discover.

To find the transparency of stellar material to the radiation traversing it is of particular interest, because it links on this astronomical inquiry to physical investigations now being carried on in the laboratory, and to some extent it extends those investigations to conditions unattainable on the earth. At high temperatures the æther waves are mainly of very short wave-length, and in the stars we are dealing mainly with radiation of wave-length 3 to 30 Ångström units, which might be described as very soft X-rays. It is interesting, therefore, to compare the results with the absorption of the harder X-rays dealt with by physicists. To obtain an exact measure of this absorption in the stars we have to assume a value of the molecular weight; but fortunately the extreme range possible for the molecular weight gives fairly narrow limits for the absorption. The average weight of the ultimate independent particles in a star is probably rather low, because in the conditions prevailing there the atoms would be strongly ionised; that is to say, many of the outer electrons of the system of the atom would be broken off; and as each of these free electrons counts as an independent molecule for present purposes, this brings down the average weight. In the extreme case (probably not reached in a star) when the whole of the electrons outside the nucleus are detached the average weight comes down to about 2, *whatever the material*, because the number of electrons is about half the atomic weight for all the elements (except hydrogen). We may, then, safely take 2 as the extreme lower limit. For an upper limit we might perhaps take

200; but to avoid controversy we shall be generous and merely assume that the molecular weight is not greater than—infinity. Here is the result:—

For molecular weight 2, mass-coefficient of absorption = 10 C.G.S. units.

For molecular weight ∞, mass-coefficient of absorption = 130 C.G.S. units.

The true value, then, must be between 10 and 130. Partly from thermodynamical considerations, and partly from further comparisons of astronomical observation with theory, the most likely value seems to be about 35 C.G.S. units, corresponding to molecular weight 3·5.

Now this is of the same order of magnitude as the absorption of X-rays measured in the laboratory. I think the result is in itself of some interest, that in such widely different investigations we should approach the same kind of value of the opacity of matter to radiation. The penetrating power of the radiation in the star is much like that of X-rays; more than half is absorbed in a path of 20 cm. at atmospheric density. Incidentally, this very high opacity explains why a star is so nearly heat-tight, and can store vast supplies of heat with comparatively little leakage.

So far this agrees with what might have been anticipated; but there is another conclusion which physicists would probably not have foreseen. The giant series comprises stars differing widely in their densities and temperatures, those at one end of the series being on the average about ten times hotter throughout than those at the other end. By the present investigation we can compare directly the opacity of the hottest stars with that of the coolest. The rather surprising result emerges that the opacity is the same for all; at any rate, there is no difference large enough for us to detect. There seems no room for doubt that at these high temperatures the absorption-coefficient is approaching a limiting value, so that over a wide range it remains practically constant. With regard to this constancy, it is to be noted that the temperature is concerned twice over: it determines the character and wave-length of the radiation to be absorbed, as well as the physical condition of the material which is absorbing. From the experimental knowledge of X-rays we should have expected the absorption to vary very rapidly with the wave-length, and therefore with the temperature. It is surprising, therefore, to find a nearly constant value

The result becomes a little less mysterious when we consider more closely the nature of absorption. Absorption is not a continuous process, and after an atom has absorbed its quantum it is put out of action for a time until it can recover its original state. We know very little of what determines the rate of recovery of the atom, but it seems clear that there is a limit to the amount of absorption that can be performed by an atom in a given time. When that limit is reached no increase in the intensity of the incident radiation will lead to any more absorption. There is, in fact, a saturation effect. In the laboratory experiments the radiation used is extremely weak; the atom is practically never caught unprepared, and the absorption is proportional to the incident radiation. But in the stars the radiation is very intense and the saturation effect comes in.

Even granting that the problem of absorption in the stars involves this saturation effect, which does not affect laboratory experiments, it is not very easy to understand theoretically how the various conditions combine to give a constant absorption-coefficient independent of temperature and wave-length. But the astronomical results seem conclusive. Perhaps the most hopeful suggestion is one made to me a few years ago by C. G. Barkla. He suggested that the opacity of the stars may depend mainly on *scattering* rather than on true atomic absorption. In that case the constancy has a simple explanation, for it is known that the coefficient of scattering (unlike true absorption) approaches a definite constant value for radiation of short wave-length. The value, moreover, is independent of the material. Further, scattering is a continuous process, and there is no likelihood of any saturation effect; thus for very intense streams of radiation its value is maintained, whilst the true absorption may sink to comparative insignificance. The difficulty in this suggestion is a numerical discrepancy between the known theoretical scattering and the values already given as deduced from the stars. The theoretical coefficient is only 0·2 compared with the observed value 10 to 130. Barkla further pointed out that the waves here concerned are not short enough to give the ideal coefficient; they would be scattered more powerfully, because under their influence the electrons in any atom would all vibrate in the same phase instead of in haphazard phases. This might help to bridge the gap, but not sufficiently. It must be remembered that many of the electrons have broken loose from the atom and do not contribute to the increase.[1] Making all allowances for uncertainties in the data, it seems clear that the astronomical opacity is definitely higher than the theoretical scattering. Very recently, however, a new possibility has opened up which may possibly effect a reconciliation. Later in the address I shall refer to it again.

Astronomers must watch with deep interest the investigations of these short waves, which are being pursued in the laboratory, as well as the study of the conditions of ionisation by both experimental and theoretical physics, and I am glad of this opportunity of bringing before those who deal with these problems the astronomical bearing of their work.

I can allude only very briefly to the purely astronomical results which follow from this investigation (Monthly Notices, vol. lxxvii., pp. 16, 596; vol. lxxix., p. 2); it is here that the best opportunity occurs for checking the theory by comparison with observation, and for finding out in what respects it may be deficient. Unfortunately, the observational data are generally not very precise, and the test is not so stringent as we could wish. It turns out that (the opacity being constant) the total radiation of a giant star should be a function of its mass only, independent of its temperature or state of diffuseness. The total radiation (which is measured roughly by the luminosity) of any one star thus remains constant during the whole giant stage of its history. This agrees with the fundamental feature, pointed out by Russell in introducing the giant and dwarf hypothesis, that giant stars of every spectral type have nearly the same luminosity. From the range of luminosity of these stars it is now possible to find their range of mass. The masses are remarkably alike—a fact already suggested by work on double stars. Limits of mass in the ratio 3:1 would cover the great majority of the giant stars. Somewhat tentatively we are able to extend the investigation to dwarf stars, taking account of the deviations of dense gas from the ideal laws and using our own sun to supply a determination of the unknown constant involved. We can calculate the maximum temperature reached by different masses; for example, a star must have at least ¼ the mass of the sun in order to reach the lowest spectral type, M; and in order to reach the hottest type, B, it must be at least 2½ times as massive as the sun. Happily for the

[1] *E.g.* for iron non-ionised the theoretical scattering is 5·2, against an astronomical value 120. If 16 electrons (2 rings) are broken off, the theoretical coefficient is 0·9, against an astronomical value 35. For different assumptions as to ionisation the values chase one another, but cannot be brought within reasonable range.

theory, no star has yet been found with a mass less than $\frac{1}{7}$ of the sun's; and it is a well-known fact, discovered from the study of spectroscopic binaries, that the masses of the B stars are large compared with those of other types. Again, it is possible to calculate the difference of brightness of the giant and dwarf stars of type M, *i.e.* at the beginning and end of their career; the result agrees closely with the observed difference. In the case of a class of variable stars in which the light changes seem to depend on a mechanical pulsation of the star, the knowledge we have obtained of the internal conditions enables us to predict the period of pulsation within narrow limits. For example, for δ Cephei, the best-known star of this kind, the theoretical period is between four and ten days, and the actual period is $5\frac{1}{3}$ days. Corresponding agreement is found in all the other cases tested.

Our observational knowledge of the things here discussed is chiefly of a rather vague kind, and we can scarcely claim more than a general agreement of theory and observation. What we have been able to do in the way of tests is to offer the theory a considerable number of opportunities to "make a fool of itself," and so far it has not fallen into our traps. When the theory tells us that a star having the mass of the sun will at one stage in its career reach a maximum effective temperature of 9000° (the sun's effective temperature being 6000°) we cannot do much in the way of checking it; but an erroneous theory might well have said that the maximum temperature was 20,000° (hotter than any known star), in which case we should have detected its error. If we cannot feel confident that the answers of the theory are true, it must be admitted that it has shown some discretion in lying without being found out.

It would not be surprising if individual stars occasionally depart considerably from the calculated results, because at present no serious attempt has been made to take into account rotation, which may modify the conditions when sufficiently rapid. That appears to be the next step needed for a more exact study of the question.

Probably the greatest need of stellar astronomy at the present day, in order to make sure that our theoretical deductions are starting on the right lines, is some means of measuring the apparent angular diameters of stars. At present we can calculate them approximately from theory, but there is no observational check. We believe we know with fair accuracy the apparent surface brightness corresponding to each spectral type; then all that is necessary is to divide the total apparent brightness by this surface brightness, and the result is the angular area subtended by the star. The unknown distance is not involved, because surface brightness is independent of distance. Thus the estimation of the angular diameter of any star seems to be a very simple matter. For instance, the star with the greatest apparent diameter is almost certainly Betelgeuse, diameter 0·051″. Next to it comes Antares, 0·043″. Other examples are Aldebaran 0·022″, Arcturus 0·020″, Pollux 0·013″. Sirius comes rather low down with diameter 0·007″. The following table may be of interest as showing the angular diameters expected for stars of various types and visual magnitudes:—

Probable Angular Diameters of Stars.

Vis. Mag.	A	F	G	K	M
m	″	″	″	″	″
0·0	0·0034	0·0054	0·0098	0·0219	0·0859
2·0	0·0014	0·0022	0·0039	0·0087	0·0342
4·0	0·0005	0·0009	0·0016	0·0035	0·0136

However confidently we may believe in these values, it would be an immense advantage to have this first step in our deductions placed beyond doubt. If the direct measurement of these diameters could be made with any accuracy it would make a wonderfully rapid advance in our knowledge. The prospects of accomplishing some part of this task are now quite hopeful. We have learnt with great interest this year that work is being carried out by interferometer methods with the 100-in. reflector at Mount Wilson, and the results are most promising. At present the method has been applied only to measuring the separation of close double stars, but there seems to be no doubt that an angular diameter of 0·05″ is well within reach. Although the great mirror is used for convenience, the interferometer method does not in principle require great apertures, but rather two small apertures widely separated, as in a range-finder. Prof. Hale has stated, moreover, that successful results were obtained on nights of poor seeing. Perhaps it would be unsafe to assume that "poor seeing" at Mount Wilson means quite the same thing as it does for us, and I anticipate that atmospheric disturbance will ultimately set the limit to what can be accomplished. But even if we have to send special expeditions to the top of one of the highest mountains in the world, the attack on this far-reaching problem must not be allowed to languish.

I spoke earlier of the radiation-pressure exerted by the outflowing heat, which has an important effect on the equilibrium of a star. It is quite easy to calculate what proportion of the weight of the material is supported in this way; it depends on neither the density nor the opacity, but solely on the star's total mass and on the molecular weight. No astronomical data are needed; the calculation involves only fundamental physical constants found by laboratory researches. Here are the figures, first for average molecular weight 3·0:—

For mass $\frac{1}{2}\times$sun, fraction of weight supported by radiation-pressure=0·044.

For mass $5\times$sun, fraction of weight supported by radiation-pressure=0·457.

For molecular weight 5·0 the corresponding fractions are 0·182 and 0·645.

The molecular weight can scarcely go beyond this range,[2] and for the conclusions I am about to draw it does not much matter which limit we take. Probably 90 per cent. of the giant stars have masses between $\frac{1}{2}$ and 5 times the sun's, and we see that this is just the range in which radiation-pressure rises from unimportance to importance. It seems clear that a globe of gas of larger mass, in which radiation-pressure and gravitation are nearly balancing, would be likely to be unstable. The condition may not be strictly unstable in itself, but a small rotation or perturbation would make it so. It may therefore be conjectured that, if nebulous material began to concentrate into a mass much greater than five times the sun's, it would probably break up, and continue to redivide until more stable masses resulted. Above the upper limit the chances of survival are small; when the lower limit is approached the danger has practically disappeared, and there is little likelihood of any further breaking-up. Thus the final masses are left distributed almost entirely between the limits given. To put the matter slightly differently, we are able to predict from general principles that the material of the stellar universe will aggregate primarily into masses chiefly lying between 10^{33} and 10^{34} grams; and this is just the

2 As an illustration of these limits, iron has 26 outer electrons; if 10 break away the average molecular weight is 5; if 18 break away the molecular weight is 3. Eggert (*Phys. Zeits.*, 1919, p. 570) has suggested by thermodynamical reasoning that in most cases the two outer rings (16 electrons) would break away in the stars. The comparison of theory and observation for the dwarf stars also points to a molecular weight a little greater than 3.

magnitude of the masses of the stars according to astronomical observation.[3]

This study of the radiation and internal conditions of a star brings forward very pressingly a problem often debated in this Section: What is the source of the heat which the sun and stars are continually squandering? The answer given is almost unanimous —that it is obtained from the gravitational energy converted as the star steadily contracts. But almost as unanimously this answer is ignored in its practical consequences. Lord Kelvin showed that this hypothesis, due to Helmholtz, necessarily dates the birth of the sun about 20,000,000 years ago; and he made strenuous efforts to induce geologists and biologists to accommodate their demands to this time-scale. I do not think they proved altogether tractable. But it is among his own colleagues, physicists and astronomers, that the most outrageous violations of this limit have prevailed. I need only refer to Sir George Darwin's theory of the earth-moon system, to the present Lord Rayleigh's determination of the age of terrestrial rocks from occluded helium, and to all modern discussions of the statistical equilibrium of the stellar system. No one seems to have any hesitation, if it suits him, in carrying back the history of the earth long before the supposed date of formation of the solar system; and, in some cases at least, this appears to be justified by experimental evidence which it is difficult to dispute. Lord Kelvin's date of the creation of the sun is treated with no more respect than Archbishop Ussher's.

The serious consequences of this contraction hypothesis are particularly prominent in the case of giant stars, for the giants are prodigal with their heat and radiate at least a hundred times as fast as the sun. The supply of energy which suffices to maintain the sun for 10,000,000 years would be squandered by a giant star in less than 100,000 years. The whole evolution in the giant stage would have to be very rapid. In 18,000 years at the most a typical star must pass from the initial M stage to type G. In 80,000 years it has reached type A, near the top of the scale, and is about to start on the downward path. Even these figures are probably very much over-estimated.[4] Most of the naked-eye stars are still in the giant stage. Dare we believe that they were all formed within the last 80,000 years? The telescope reveals to us objects remote not only in distance, but also in time. We can turn it on a globular cluster and behold what was passing 20,000, 50,000, even 200,000 years ago unfortunately not all in the same cluster, but in different clusters representing different epochs of the past. As Shapley has pointed out, the verdict appears to be "no change." This is perhaps not conclusive, because it does not follow that individual stars have suffered no change in the interval; but it is difficult to resist the impression that the evolution of the stellar universe proceeds at a slow, majestic pace, with respect to which these periods of time are insignificant.

There is another line of astronomical evidence which appears to show more definitely that the evolution of the stars proceeds far more slowly than the contraction hypothesis allows; and perhaps it may ultimately enable us to measure the true rate of progress. There are certain stars, known as Cepheid variables, which undergo a regular fluctuation of light of a characteristic kind, generally with a period of a few days. This light change is *not* due to eclipse. Moreover, the colour quality of the light changes between maximum and minimum, evidently pointing to a periodic change in the physical condition of the star. Although these objects were formerly thought to be double stars, it now seems clear that this was a misinterpretation of the spectroscopic evidence. There is, in fact, no room for the hypothetical companion star; the orbit is so small that we should have to place it inside the principal star. Everything points to the period of the light pulsation being something intrinsic in the star; and the hypothesis advocated by Shapley, that it represents a mechanical pulsation of the star, seems to be the most plausible. I have already mentioned that the observed period does, in fact, agree with the calculated period of mechanical pulsation, so that the pulsation explanation survives one fairly stringent test. But whatever the cause of the variability, whether pulsation or rotation, provided only that it is intrinsic in the star, and not forced from outside, the density must be the leading factor in determining the period. If the star is contracting so that its density changes appreciably, the period cannot remain constant. Now, on the contraction hypothesis the change of density must amount to at least 1 per cent. in forty years. (I give the figures for δ Cephei, the best-known variable of this class.) The corresponding change of period should be very easily detectable. For δ Cephei the period ought to decrease 40 seconds annually.

Now δ Cephei has been under careful observation since 1785, and it is known that the change of period, if any, must be very small. S. Chandler found a decrease of period of $\frac{1}{20}$ second per annum, and in a recent investigation E. Hertzsprung has found a decrease of $\frac{1}{10}$ second per annum. The evidence that there is any decrease at all rests almost entirely on the earliest observations made before 1800, so that it is not very certain; but in any case the evolution is proceeding at not more than $\frac{1}{400}$ of the rate required by the contraction hypothesis. There must at this stage of the evolution of the star be some other source of energy which prolongs the life of the star 400-fold. The time-scale so enlarged would suffice for practically all reasonable demands.

I hope the dilemma is plain. Either we must admit that whilst the density changes 1 per cent. a certain period intrinsic in the star can change no more than $\frac{1}{800}$ of 1 per cent., or we must give up the contraction hypothesis.

If the contraction theory were proposed to-day as a novel hypothesis I do not think it would stand the smallest chance of acceptance. From all sides—biology, geology, physics, astronomy—it would be objected that the suggested source of energy was hopelessly inadequate to provide the heat spent during the necessary time of evolution; and, so far as it is possible to interpret observational evidence confidently, the theory would be held to be negatived definitely. Only the inertia of tradition keeps the contraction hypothesis alive—or, rather, not alive, but an unburied corpse. But if we decide to inter the corpse, let us frankly recognise the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the sub-atomic energy which, it is known, exists abundantly in all matter; we sometimes dream that man will one day learn how to release it and use it for his service. The store is well-nigh inexhaustible, if only it could be tapped. There is sufficient in the sun to maintain its output of heat for 15 billion years.

Certain physical investigations in the past year, which I hope we may hear about at this meeting, make it probable to my mind that some portion of this

[3] By admitting plausible assumptions closer limits could be drawn. Taking the molecular weight as 3·5, and assuming that the most critical condition is when $\frac{1}{3}$ of gravitation is counterbalanced (by analogy with the case of rotating spheroids, in which centrifugal force opposes gravitation and creates instability), we find that the critical mass is just twice that of the sun, and stellar masses may be expected to cluster closely round this value.

[4] I have taken the ratio of specific heats at the extreme possible value, $\frac{5}{3}$; that is to say, no allowance has been made for the energy needed for ionisation and internal vibrations of the atoms, which makes a further call on the scanty supply available.

sub-atomic energy is actually being set free in the stars. F. W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. The nucleus of the helium atom, for example, consists of four hydrogen atoms bound with two electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the four hydrogen atoms which enter into it; and in this, at any rate, the chemists agree with him. There is a loss of mass in the synthesis amounting to about 1 part in 120, the atomic weight of hydrogen being 1·008 and that of helium just 4. I will not dwell on his beautiful proof of this, as you will, no doubt, be able to hear it from himself. Now mass cannot be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent. of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

But is it possible to admit that such a transmutation is occurring? It is difficult to assert, but perhaps more difficult to deny, that this is going on. Sir Ernest Rutherford has recently been breaking down the atoms of oxygen and nitrogen, driving out an isotope of helium from them; and what is possible in the Cavendish Laboratory may not be too difficult in the sun. I think that the suspicion has been generally entertained that the stars are the crucibles in which the lighter atoms which abound in the nebulæ are compounded into more complex elements. In the stars matter has its preliminary brewing to prepare the greater variety of elements which are needed for a world of life. The radio-active elements must have been formed at no very distant date; and their synthesis, unlike the generation of helium from hydrogen, is endothermic. If combinations requiring the addition of energy can occur in the stars, combinations which liberate energy ought not to be impossible.

We need not bind ourselves to the formation of helium from hydrogen as the sole reaction which supplies the energy, although it would seem that the further stages in building up the elements involve much less liberation, and sometimes even absorption, of energy. It is a question of accurate measurement of the deviations of atomic weights from integers, and up to the present hydrogen is the only element for which Dr. Aston has been able to detect the deviation. No doubt we shall learn more about the possibilities in due time. The position may be summarised in these terms: the atoms of all elements are built of hydrogen atoms bound together, and presumably have at one time been formed from hydrogen; the interior of a star seems as likely a place as any for the evolution to have occurred; whenever it did occur a great amount of energy must have been set free; in a star a vast quantity of energy is being set free which is hitherto unaccounted for. You may draw a conclusion if you like.

If, indeed, the sub-atomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfilment our dream of controlling this latent power for the well-being of the human race—or for its suicide.

So far as the immediate needs of astronomy are concerned, it is not of any great consequence whether in this suggestion we have actually laid a finger on the true source of the heat. It is sufficient if the discussion opens our eyes to the wider possibilities. We can get rid of the obsession that there is no other conceivable supply besides contraction, but we need not again cramp ourselves by adopting prematurely what is perhaps a still wilder guess. Rather we should admit that the source is not certainly known, and seek for any possible astronomical evidence which may help to define its necessary character. One piece of evidence of this kind may be worth mentioning. It seems clear that it must be the high temperature inside the stars which determines the liberation of energy, as H. N. Russell has pointed out (Pubns. Ast. Soc. Pacific, August, 1919). If so, the supply may come mainly from the hottest region at the centre. I have already stated that the general uniformity of the opacity of the stars is much more easily intelligible if it depends on scattering rather than on true absorption; but it did not seem possible to reconcile the deduced stellar opacity with the theoretical scattering coefficient. Within reasonable limits it makes no great difference in our calculations at what parts of the star the heat energy is supplied, and it was assumed that it comes more or less evenly from all parts, as would be the case on the contraction theory. The possibility was scarcely contemplated that the energy is supplied entirely in a restricted region round the centre. Now, the more concentrated the supply, the lower is the opacity requisite to account for the observed radiation. I have not made any detailed calculations, but it seems possible that for a sufficiently concentrated source the deduced and the theoretical coefficients could be made to agree, and there does not seem to be any other way of accomplishing this. Conversely, we might perhaps argue that the present discrepancy of the coefficients shows that the energy supply is not spread out in the way required by the contraction hypothesis, but belongs to some new source available only at the hottest, central part of the star.

I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end. I wonder what is the touchstone by which we may test the legitimate development of scientific theory and reject the idly speculative. We all know of theories which the scientific mind instinctively rejects as fruitless guesses; but it is difficult to specify their exact defect or to supply a rule which will show us when we ourselves do err. It is often supposed that to speculate and to make hypotheses are the same thing; but more often they are opposed. It is when we let our thoughts stray outside venerable, but sometimes insecure, hypotheses that we are said to speculate. Hypothesis limits speculation. Moreover, distrust of speculation often serves as a cover for loose thinking; wild ideas take anchorage in our minds and influence our outlook; whilst it is considered too speculative to subject them to the scientific scrutiny which would exorcise them.

If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalisations, if we seek for any theory to guide us, some degree of speculation cannot be avoided. Some will prefer to take the interpretation which seems to be indicated most immediately and at once adopt that as an hypothesis; others will rather seek to explore and classify the widest possibilities which are not definitely inconsistent with the facts. Either choice has it dangers: the first may be too narrow a view and lead progress into a cul-de-sac; the second may be so broad that it is useless as a guide, and diverges indefinitely from experimental knowledge. When this last case happens, it must be concluded that the knowledge is not yet ripe for theoretical treatment and that speculation is premature. The time when speculative theory and observational research may profitably go hand in hand is when the possibilities, or at any rate the probabilities, can be narrowed down by experiment, and the theory can indicate the tests by which the remaining wrong paths may be blocked up one by one.

The mathematical physicist is in a position of peculiar difficulty. He may work out the behaviour of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable. It is no more speculative than the binomial theorem. But when he claims a serious interest for his toy, when he suggests that his model is like something going on in Nature, he inevitably begins to speculate. Is the actual body really like the ideal model? May not other unknown conditions intervene? He cannot be sure, but he cannot suppress the comparison; for it is by looking continually to Nature that he is guided in his choice of a subject. A common fault, to which he must often plead guilty, is to use for the comparison data over which the more experienced observer shakes his head; they are too insecure to build extensively upon. Yet even in this, theory may help observation by showing the kind of data which it is especially important to improve.

I think that the more idle kinds of speculation will be avoided if the investigation is conducted from the right point of view. When the properties of an ideal model have been worked out by rigorous mathematics, all the underlying assumptions being clearly understood, then it becomes possible to say that such-and-such properties and laws lead precisely to such-and-such effects. If any other disregarded factors are present, they should now betray themselves when a comparison is made with Nature. There is no need for disappointment at the failure of the model to give perfect agreement with observation; it has served its purpose, for it has distinguished what are the features of the actual phenomena which require new conditions for their explanation. A general preliminary agreement with observation is necessary, otherwise the model is hopeless; not that it is necessarily wrong so far as it goes, but it has evidently put the less essential properties foremost. We have been pulling at the wrong end of the tangle, which has to be unravelled by a different approach. But after a general agreement with observation is established, and the tangle begins to loosen, we should always make ready for the next knot. I suppose that the applied mathematician whose theory has just passed one still more stringent test by observation ought not to feel satisfaction, but rather disappointment—"Foiled again! This time I *had* hoped to find a discordance which would throw light on the points where my model could be improved." Perhaps that is a counsel of perfection; I own that I have never felt very keenly a disappointment of this kind.

Our model of Nature should not be like a building—a handsome structure for the populace to admire, until in the course of time someone takes away a corner-stone and the edifice comes toppling down. It should be like an engine with movable parts. We need not fix the position of any one lever; that is to be adjusted from time to time as the latest observations indicate. The aim of the theorist is to know the train of wheels which the lever sets in motion—that binding of the parts which is the soul of the engine.

In ancient days two aviators procured to themselves wings. Dædalus flew safely through the middle air across the sea, and was duly honoured on his landing. Young Icarus soared upwards towards the sun until the wax which bound his wings melted, and his flight ended in fiasco. In weighing their achievements perhaps there is something to be said for Icarus. The classic authorities tell us that he was only "doing a stunt," but I prefer to think of him as the man who certainly brought to light a constructional defect in the flying-machines of his day. So, too, in science. Cautious Dædalus will apply his theories where he feels most confident they will safely go; but by his excess of caution their hidden weaknesses cannot be brought to light. Icarus will strain his theories to the breaking-point until the weak joints gape. For a spectacular stunt? Perhaps partly; he is often very human. But if he is not yet destined to reach the sun and solve for all time the riddle of its constitution, yet he may hope to learn from his journey some hints to build a better machine.

Memorial Tributes to Sir Norman Lockyer.

IN Sir Norman Lockyer the country loses one of the most ardent supporters of science in his time. As one who enjoyed his intimate friendship for more than half a century, I would fain add my personal contribution to the many expressions of regret and appreciation which the loss is sure to call forth.

There never was a man more thoroughly imbued than he with a sense of the importance of the cultivation of science, not only for its own sake, but also for the multitude of ways in which it may be made to minister to the welfare of mankind. Though he had made choice of astronomy as his own field of work, he was no mere specialist, but kept his sympathy in touch with the progress of science as a whole, and worked, harder than most of his contemporaries knew, to further that progress. Sir Norman's younger years as a clerk in the War Office, while affording him an insight into the methods of a Government Department, furnished also a training in business habits which served him in good stead through later life. The secretaryship of the Duke of Devonshire's Commission on scientific instruction, to which as a young man he was appointed, undoubtedly gave the impetus that made him so strenuous an advocate of a wider recognition of the claims of science for a place in our educational and industrial organisation. This appointment, by bringing him into personal acquaintance with the leading men of science of the day, strengthened and widened his sympathies. One of the first results of the experience thus gained was to convince him of the need for better teaching of the rudiments of science in our schools. He saw that one of the first requirements was the production of simple elementary treatises on the different departments of natural knowledge, written not by mere book-makers, but by the best living authorities on the several subjects. He confided to me the scheme which he drew up, and asked me to co-operate with him. It so happened that a similar proposal was about the same time laid before Mr. Alexander Macmillan, head of the publishing firm, by Profs. Huxley, Balfour Stewart, and Roscoe. It was finally arranged that the scheme of these eminent professors should be adopted, and that Lockyer and I should contribute to it. In this way arose the series of elementary text-books or Primers, of which millions of copies have been sold, some of them having been translated into most of the languages of Europe and into some of those of Asia.

Sir Norman's energy also led him to project a weekly journal entirely devoted to science. He convinced the same enterprising publisher that such a journal would be of much value in chronicling for the general public the progress of scientific opinion and discovery. Thus the present publication came into existence. Lockyer was, of course, its editor, and he continued to fill the editorial chair with amazing industry and success for fifty years. It would be difficult to appraise the value of this service to the cause of science. But the historian of the future, when he comes to describe the various influences which have fostered that cause in this country since 1870, will not forget to include Sir Norman and NATURE.

My old friend's enthusiasm spurred him to take part in a long succession of solar eclipse expeditions, which took him into remote parts of the world, and sometimes involved no little risk. These foreign journeys he continued to undertake until he was not far from seventy years of age.

Sir Norman Lockyer's many communications to the Royal Society and other learned bodies, and also the array of his separately published volumes, form the best monument of his life-work. Those who knew him best often wondered how, with only one serviceable eye, he could get through the amount of telescopic and spectroscopic work which he accomplished. His personal charm was great. The kindly nature, ready helpfulness, and infectious enthusiasm that were so characteristic of him endeared him to those who had the privilege of his friendship, and who feel that he leaves a vacant place among the men of science in this country which it will be hard to fill as he filled it.

ARCH. GEIKIE.

WITH the death, at an advanced age, of Joseph Norman Lockyer, a remarkable and in some respects unique personality passes away from the English scientific world. It would be unnecessary for me, even if I were competent, to describe the progress and achievements of his work in science; that has been done already in these pages; nor need I dwell on merely biographical detail. I have been invited to write an appreciation of Lockyer. I can only respond by giving my impression of the man drawn from tolerably close intimacy in an acquaintanceship extending over half a century. Some biographical detail is a necessary framework.

Lockyer's education, though doubtless sufficient, seems to have been unsystematic; part was obtained on the Continent, where he attended lectures at the Sorbonne. He did not receive the training either of a public school or of a university; he started in life unhampered by any educational shibboleths or the acquirement of academic status. This was amply made up to him in after-life, for it is more than doubtful if any contemporary man of science had more honorary degrees showered upon him. Lockyer's father was a man of scientific occupation and probably of some attainment; the son evidently received from him an impulse towards science which no schooling in the early half of the last century could have supplied.

At the age of twenty-one Lockyer was appointed to a clerkship in the War Office; there he remained for thirteen years. Hundreds of young men in such a position have merely matured or withered towards a pension. From that fate he was preserved by the tumultuous energy which characterised him all through life. In the face of opposition he carried out internal administrative reforms in the Office, and had his reward in 1865 in being appointed by Lord de Grey editor of the Army Regulations. I remember his telling me that their codification cost him two years' work, and that the strain of having to carry in his head for the purpose a vast mass of detail almost broke him down. It seems almost incredible that, apart from his official life, he was able to carry on successful astronomical research. He was elected to the Royal Astronomical Society in 1860, and in 1866 devised a method of observing the solar prominences without an eclipse; this was afterwards applied by Janssen and himself, and commemorated in a medal by the French Government. In 1868 he discovered in the sun helium, then unknown as a terrestrial element. In 1869, while still in the War Office, he was elected into the Royal Society.

With such a record of administrative and scientific ability it is not surprising to find Lockyer in the following year appointed secretary of the Royal Commission on Scientific Instruction. At its conclusion Disraeli transferred him to the Science and Art Department, for which he organised the extremely successful Loan Exhibition of Scientific Apparatus opened by Queen Victoria in 1876. In 1881 he became professor of astronomical physics in the Royal College of Science. Research into solar phenomena now became the dominant purpose of his life; it led him into fertile speculations in various directions. They engaged him to the last, and but a year ago he contributed a paper to this journal. The earliest was the correlation between climate and solar activity. I well remember the cold douche he administered when he pointed out to me that its effect, far from being direct, might be the reverse. The importance of this principle, which at the time seemed paradoxical, has now become fundamental in meteorological research; regions are now known to be affected oppositely by changes in the sun's heat supply. Lockyer was chief of eight Government eclipse expeditions in which a brief examination of the isolated chromosphere became possible. In these he had the assistance of the Navy, and their success was due not only to his capacity for leadership and organisation, but also to his ability to inspire interest and enthusiasm in the work in both officers and bluejackets. The installation of a temporary observatory in some remote and uninhabited spot was not seldom difficult.

In 1869 Lockyer and Alexander Macmillan founded NATURE; English science in other respects owes much to the latter and his successors. Henry Woodward, who is still with us, was present at a dinner at the Garrick Club to celebrate its birth. It needed a good send-off, for Sir H. Trueman Wood thought that at the time it "can scarcely have been regarded as a very promising speculation." Probably it was not, but both founders—and Lockyer the most—had a different aim. It may be permitted to quote from the present Vice-Chancellor of the University of Leeds a record of how it has been attained:—"The exacting care with which it has been edited, the impartiality and precision of its judgments, the wide range of its information, the accuracy of its reports, have given NATURE in its own sphere unique distinction and authority."

As to the first, I remember hearing from a distinguished man of science that at a dinner party at which both he and Lockyer were guests, the latter received an urgent printer's proof in the middle of dinner and corrected it then and there.

Lockyer's service with the Commission on Scientific Instruction gave him a thorough insight into the resources, or rather lack of them, throughout the country. It is certainly true, as the present Vice-Principal of Birmingham University tells us, that through the columns of NATURE "there has appeared an informed and helpful criticism that has furthered university growth and development." The criticism was sometimes pretty vigorous. Henry Smith, at Oxford, himself a mathematician of European fame, thought that the editor rather trespassed on the creative function of the Author of Nature.

At Oxford Henry Acland had devoted the best years of his life to getting biology and chemistry admitted to the medical curriculum. Tireless energy with "*aeterna mansuetudine*" succeeded not merely in this, but also in the erection of the New Museum, which was opened in 1861. This, with its Venetian gothic and Skidmore ironwork, is a shrine rather than a laboratory. It was, in fact, an outcome of the Oxford æsthetic renaissance, which in turn owed its filiation to the "Oxford movement." So far science was in the best of company with Dr. Pusey in its support. But Acland really only wanted biology and chemistry for his medical school; accordingly we find in the New Museum Rolleston overtaxing his strength in the attempt to cover the whole biological field; Brodie, emancipated from the cellar of the Ashmolean, treating inorganic chemistry with originality and freshness; and Vernon Harcourt working at chemical change in a reproduction of the Glastonbury kitchen. This was excellent, but unfortunately it was all. As to the rest of the science faculty, some never lectured, some were physically incapacitated, some were frankly non-resident. Much has changed since; new professorships have been founded and entrusted to men of assured accomplishment; new laboratories have been built; and the present Vice-Chancellor finds it convenient occasionally to borrow a number of NATURE to verify an appointment.

At Cambridge we have the testimony of Dr. Glaisher as to "the almost complete lack of interest in natural science that existed in the university when NATURE was founded"; even in mathematics "there was no encouragement—quite the reverse—to research of any kind." To the "great expansion of thought, study, and learning" that has taken place since, "NATURE has largely and worthily contributed."

These testimonials, borrowed from the record on the occasion of last year's jubilee, acquire a true significance when we read in them LOCKYER for NATURE. He never ceased to insist on the necessity of combining university teaching with research. He displeased the somnolent, and still more when he supported a better distribution of available funds in which the untimely death of Dr. Appleton deprived him of support from the side of the "humanities." He was in no way deterred by the sarcasm of Henry Smith and Sir John Evans—who ought to have known better—that the endowment of research only camouflaged—to use the phrase of the day—the research for endowment.

We may congratulate ourselves with Sir Donald MacAlister that NATURE "still informs, chastens, and stimulates the scientific worker and the scientific teacher." Chastening is now seldom called for, but in the early days it was vigorously applied. Teaching and text-books were largely obsolete, and received criticism that was often ruthless. Controversies in its pages were sometimes fierce; they cleared things up, and Lockyer kept the ring with complete impartiality. He would himself submit to a little chastening without ill-will. On one occasion a series of articles was commenced with a general approval from Huxley. But it immediately became apparent that the fundamental assumption was unsound. I sent him a short statement of the fact; he admitted that it was unanswerable, published it, and stopped the series.

If the early days were in some measure marked by storm and stress, Lockyer's transparent sincerity and enthusiasm carried him through. When NATURE had completed its twenty-fifth year the publishers assembled for a dinner at the Savoy Hotel in Lockyer's honour some fifty of the most active and representative of our scientific men then available. Huxley emerged from retirement to be present. With sly humour he hinted at the chastening, and recalled a story of an aggrieved wife who had received some discipline from her husband, but who explained to the magistrate that she "didn't look upon him as a 'usband so much as a friend."

Here I must stop. Otherwise I might touch on Lockyer's kind-heartedness, his capacity for making friends, his courage in family sorrow, his literary labours, and other aspects of a full and many-sided life. I conclude by once more drawing on the jubilee record for the testimony of the Royal Engineers Institute, Chatham, that the Editor of NATURE "never failed to enforce the great lesson that the search for knowledge, pursued for its own ends and with no immediate thoughts of material gain, should be one of the most potent driving forces in the life of a nation."

W. T. THISELTON-DYER.

MY acquaintance—and I may add my friendship—with the late Sir Norman Lockyer dates back from almost exactly half a century ago. It began in the autumn of 1870, when the details of the arrangements in connection with the projected Government expedition to observe the total solar eclipse of December 22 of that year were under consideration. Lockyer was then in the full tide of his intellectual vigour. Two years previously he had leaped into fame, and established a commanding position as one of the pioneers of the newly developed domain of solar physics, by his memorable discovery, made simultaneously with, but independently of, Janssen, of a spectroscopic method of observing, delineating, and analysing the chromosphere at any time the sun is unobscured. In conjunction with Frankland he had detected the existence of a new element in the solar atmosphere named by the discoverers "helium," which Ramsay and others, twenty-seven years later, proved to be present in many terrestrial rocks and minerals, and to occur among the gases evolved from springs. Helium, in fact, has been shown to be a widely distributed element, and to be capable of useful application. But with its singular properties, its origin and mode of genesis, and its relations to other "elements," we are not now concerned. The immediate point is that these two cardinal discoveries, with which Lockyer's name will be associated for all time, rendered it a matter of national honour and obligation that every effort should be made, and no opportunity lost, to follow up the line of inquiry he had initiated.

Even although half a century has elapsed, much of the physical history of the sun can be traced only by the observation and study of the phenomena of a total solar eclipse. In 1870 the present methods of attack were, comparatively speaking, in their infancy. Warren de la Rue first used his photo-heliograph during the eclipse of July 18, 1860. In the same year Bunsen constructed the first spectroscope, which was quickly applied to the examination of celestial objects. Tennant had directed it to the corona of the Indian eclipse of 1868, and Young to that of the eclipse of 1869. But the results were contradictory. Tennant found that the spectrum was the ordinary solar spectrum; Young detected the existence of bright lines, but was uncertain as to whether they might not be due to the outlying and nebulous portion of the chromosphere. To the world of science the question was of the greatest interest. Hence the importance of the eclipse of 1870, which, it was hoped, would settle the matter. Mr. Robert Lowe at that period was Chancellor of the Exchequer, but, even if he were so minded, the Government was unable to resist the appeal of the Royal and Royal Astronomical Societies that properly equipped expeditions should be sent to suitable places along the central line of totality. Lockyer was by common consent designated as a leader of one of the parties. With characteristic zeal and ardour he threw himself heartily into the work of organisation. Arrangements were made to observe at various stations along the eastern coast of Sicily. He elaborated a comprehensive plan of observation, mainly directed to the elucidation of the structure and physical nature of the corona, and secured the co-operation of competent physicists and trained observers. Unfortunately, all his forethought, labour, and anxiety came to nothing. The Admiralty dispatch-boat *Psyche*, conveying the party from Naples to Catania, struck a submerged rock near Aci Reale. All on board were safely put on shore, and so, ultimately, were the instruments, but the poor dispatch-boat became a total loss. It was characteristic of Lockyer, whose whole thought was concentrated on the work he had undertaken, that he should have telegraphed home as soon as he reached Catania:—"*Psyche* totally wrecked. Instruments saved." Anxious relatives might infer the rest as they pleased.

Misfortune, however, still followed us. We managed to get everything in readiness for the eventful day, but as the total phase approached, the rapid fall of temperature occasioned the formation of cloud; the corona was wholly obscured, and no spectroscopic or other observations during totality were possible, and photographic exposures were useless. The work entrusted to me consisted in determining the photometric intensity of the solar light during the progress of the eclipse, and was independent of the total phase. Fortunately, I was able to obtain a complete set of measurements, which afterwards found their way into one of the publications of the Royal Society. But our philosophy was sorely exercised on learning that a perfect view of the unclouded corona was obtained from the deck of the wrecked *Psyche* some few miles away. The greatest sympathy was felt by everybody for Lockyer, and his disappointment was naturally very keen. But he bore it stoically; if he had not secured success, he had at least deserved it. Of the band of observers associated with him—among them Sir Henry Roscoe, Sir George Darwin, W. K. Clifford, W. G. Adams, Ranyard, John Brett, the artist, Sir Alexander Pedler, Brothers, Bowen, and Seabroke—I believe I am now the sole survivor.

I was a member of another expedition of which Lockyer was the leader—viz. that sent to the island of Granada, in the West Indies, to observe the total solar eclipse of August 29, 1886, when we were generally more fortunate, good observations being made by the late Father Perry, S.J., at Carriacou, Prof. Turner, Savilian professor at Oxford, Sir Arthur Schuster, and Major Darwin. Lockyer was not in good health at the time, and appeared to suffer from the heat and humidity of the climate.

On my translation to the Normal School of Science, South Kensington, as successor to Sir Edward Frankland, I became closely associated with Lockyer as a member of the teaching staff. He lectured on solar physics, and directed the observatory then standing near the western side of Exhibition Road. His laboratory and private room were in the main building close to the chemical laboratories, and I naturally saw much of him at this period. He was an indefatigable experimenter, and I was not infrequently called upon to see his results. He was always ready to discuss his work with anybody who showed an interest in it, and never made the slightest secret of what he was doing and why he was doing it. He was fertile in ideas and prolific in working hypotheses, which were discarded as readily as they were formed if found barren of results. No man ever made a greater scientific use of the imagination, and at times, in the course of conversation, he seemed to give the loosest possible rein to his fancy. Much of his routine observatory work was, of course, done by assistants, by whom he was well served. But he took a very active part in the work of the laboratory, and generally made the crucial observations himself, or assured himself of their validity by repetition. He was an excellent teacher, with a remarkable gift of exposition. He spoke fluently, with a ready command of apt expression and telling phrase, and he had no difficulty in retaining the attention of any audience he addressed. At one period of his career he was in great request as a popular lecturer, and undoubtedly did much to arouse interest and disseminate information concerning celestial phenomena, especially in connection with solar chemistry and physics. He had little opportunity of creating a "school." The primary duty of the Normal School of Science, or, as it was afterwards called, the Royal College of Science, was to train science-teachers, and the subjects of his chair offered little promise of a lucrative career.

He was a loyal colleague, and, under Huxley's wise direction as Dean, took his fair share in discussion and advice. Of his many social gifts others will no doubt be able to testify. He was fond of the society of his fellows, a genial host, entertaining and hospitable, an excellent conversationalist, with a nimble wit, and an unfailing power of ready repartee. There must be very many who have the pleasantest recollections of the delightful dinner parties in his town house in Penywern Road, and of the conversaziones which usually followed them.

Lockyer early enlisted me into the service of NATURE, and I became a frequent contributor to the journal which, under his judicious and enlightened direction, has done so much to foster and advance the interests of science in this country. My relations with him as the Editor were of the most cordial character, and I collaborated with him occasionally in the production of a leading article. Such work when done in common with him in his sanctum, frequently late at night, necessarily took up much time when protracted, as was usual, by his too ready flow of ideas, which needed a certain power of compression to get them into literary form; and at times it was only in the small hours of the morning that I was able to wend my way home—a not infrequent experience, however, of leader-writers.

It cannot, of course, be maintained that all that Lockyer has published has withstood the test of time. Some of his experimental evidence, and certain of his deductions and generalisations, were hotly challenged at about the time he made them known. But when all is said that can be said in the way of criticism and detraction, it may be confidently asserted that he has left an indelible impress on the scientific history of his epoch. His memory will be cherished by all who have come under his influence, or have learned to appreciate his many excellent qualities of head and heart, and have knowledge of his untiring efforts to serve the highest interests of science.

T. E. THORPE.

I FIRST made the acquaintance of Lockyer in Clifton, where I met him at dinner at the house of the late William Lant Carpenter in or about 1874. Lockyer had come to give a popular lecture in Bristol on his own and other recent discoveries in celestial spectroscopy, and he was full of his new ideas about the origin and nature of the elements. I remember his asking me whether I considered calcium to be an element, and, having been brought up in the then prevalent view of the permanence of the chemical elements, I replied that I should certainly so regard it. The periodic scheme of Mendeléeff was comparatively new, and Mendeléeff himself did not encourage the notion that it involved the question of the origin of the elements. Lockyer was the first to pursue the subject systematically, and much of his astrophysical research was directed towards establishing his ideas as to the dissociation of the terrestrial elements in the hottest of the stars.

I also remember coming into contact with Lockyer at the time when he was secretary of the Duke of Devonshire's Commission on Scientific Instruction, in the operations of which I was, of course, deeply interested, owing to the position I held as senior science master in Clifton College. I never at that time could have expected to be thrown into daily communication with him, as I was twenty years later, owing to an arrangement with Frankland, then professor of chemistry in the Royal College of Science at South Kensington. Frankland and Lockyer had in 1869 been engaged in joint researches on the "Physical Constitution of the Sun," and when in 1881 Lockyer became professor of astrophysics at the Royal

College of Science he secured the use of a room in the new building to which access was obtainable only through the two intermediate rooms occupied by Frankland and his successors as the research laboratory for chemistry. The consequence was that the professor of astrophysics and his satellites were compelled to pass many times in the day through the chemical rooms. It cannot be said that this was an advantage to the chemical work, for, owing to draughts and general disturbance, some chemical operations were absolutely prohibited; but the arrangement had one compensation in the opportunities afforded of frequent talk with the professor, and of hearing from him what was going on. Lockyer was always very helpful to other less experienced workers with the spectroscope, and after the discovery by Ramsay of terrestrial helium it can be readily imagined what a bustle arose in the room occupied by the professor of astrophysics, to whom the original observation of the same element in the sun was due. This naturally gave rise to many conferences with the professor of chemistry on the subject of the minerals from which the gas was obtained.

Lockyer was also a genial and jovial member of the circle which assembled daily round the luncheon table in the museum, which included in later years some of the professors from the City and Guilds Central College. Nearly all are now gone, and only memories remain to the few survivors. Those recollections include the conviction that Lockyer was a strong man who always knew his own mind, and hence accomplished much both by practice and example where lesser men, though with the same aspirations, only met with failure and disappointment. WILLIAM A. TILDEN.

THE privilege of taking part in the memorial tributes to Sir Norman Lockyer is perhaps something more than I deserve, for, although it is true we were on terms of cordial friendship, we were never associated in official or scientific occupations, unless his invitation to join in the founding of the British Science Guild, and his many courtesies as Editor of NATURE, may be so described. To speak of Lockyer's researches as contributions to science seems inadequate. Incapable as I was of following his scientific work in detail, I felt it to be more than contributory; we admired him rather as a builder, and a builder on big and original lines. There was something large in his undertakings and in his vision, and happily to sustain them he had also within him a fountain of energy which seemed perennial. His prescience and his vigour together were enormous, and carried him into many spheres of activity. If sometimes I was tempted to grudge his spending in the clouds what was needed by mankind, this was ungrateful, for Lockyer put his driving power freely into many sublunary affairs.

That Lockyer was disposed to be combative he was the first to admit, but always on liberal and generous lines; and in personal differences he was always kindly and good-humoured. His was the combativeness that keeps societies from stagnation. On two important affairs he and I differed decisively, but always in good temper, and I gladly admit that in the main he was right on both issues. From the time of his departure from London most of us had to lament the loss, not, happily, at the time, of an original worker in science, but of a most ingenious and stimulating companion. At length we have lost a colleague not only a master of minute and diligent observation, but also endowed with that wide and abstract imagination which, if other than the individual imagination of the artist, is no less prophetic in the sphere of science. CLIFFORD ALLBUTT.

I CANNOT call myself an intimate friend of the late Sir Norman Lockyer, though we frequently met and talked, but two characteristics in him always impressed me. One was his energy. He seemed always to be at work, always to be full of interests, and whatever he took up he did well. The other was the many-sidedness of his mind, and his power of combining business ability and scientific acuteness. Quite early in his career he made important contributions to solar physics, discovering (with Janssen) how to examine the solar prominences apart from an eclipse, investigated meteors, and was one of the three simultaneously to explain the wonderful glows which followed the Krakatoa eruption. Besides this he wrote valuable and suggestive papers on Stonehenge and other British stone circles. With all this he was a very efficient public servant, both in the War Office and in the Science and Art Department, secretary to the Duke of Devonshire's Commission on Scientific Instruction in 1870, and Editor of NATURE for fifty years from its commencement. We shall not readily meet again with his like. T. G. BONNEY.

MR. ARTHUR SAVAGE writes:—Like many other people, I have been reading the life-story of Sir Norman Lockyer, the great man of science who was able to add so liberal a contribution to the accumulation of human knowledge. I was interested to learn from the obituary notice which appeared in the *Times* of August 17 that Sir Norman was the founder of NATURE, and I have thought the opportunity a fitting one to express a young man's humble word of appreciation of your excellent journal.

Although I have not received a university education, and my daily duties are outside the domain of scientific study, you may be glad to know that I follow your columns regularly in order to keep myself "up to date" in matters relating to those branches of knowledge in which I am interested. Should you consider this note worthy of publication I have no doubt that it would represent the feelings of numerous other "ordinary" individuals like myself.

Notes.

We have received from a correspondent in India a letter which states that the present director of the Indian Institute of Science at Bangalore, Sir Alfred Bourne, is to be succeeded by an administrator with no scientific experience. Such an appointment would be greatly deprecated by scientific workers, and we trust it is not too late to prevent it. In our view the head of such an institution should be a man who combines scientific experience with administrative ability; and if this principle is deliberately ignored in the case of the directorship of the leading scientific institution in India, the strongest protest should be made to the authorities responsible for the appointment.

When Mr. B. B. Woodward retired from the British Museum (Natural History) last June he was still occupied with the proofs of the supplement to his well-known catalogue of the natural history library. Naturalists will be glad to learn that his services have now been temporarily retained for the completion of this valuable work.

We learn from the *British Medical Journal* for August 28 that the fifth Congress of Physical Therapy, instituted by the Belgian societies of physical therapy and radiology and the Antwerp Association for Physical Therapy, will be held at Antwerp on September 11 and 12; also that the nineteenth Flemish Congress of Medicine and Natural Science will be held at Ghent on September 18 and 19.

The thirty-first annual general meeting of the Institution of Mining Engineers will be held at Manchester on September 15–17, under the presidency of Col. W. C. Blackett. The institution medal for the year 1919–20 will be presented to Dr. John Bell Simpson, and the following papers will be presented: The Normal Occurrence of Carbon Monoxide in Coal-mines, J. Ivon Graham; The Better Utilisation of Coking Slack, A. E. Beet and A. E. Findley; Richard Trevithick: His Life and Inventions, J. Harvey Trevithick; The Froth Flotation Processes as Applied to the Treatment of Coal, Ernest Bury; and An Improved Method of Determining the Relative Directions of Two Reference Lines or Bases for Mining Surveys, T. Lindsay Galloway.

Early last year a Speleological Society was founded in the University of Bristol, and a record of its activities has now been published in the first part of its Proceedings. The society is fortunate in being within easy reach of the Mendip Hills, where so many important caves have already been explored, and it has been able to obtain special facilities for field-work. It has also been favoured with a course of lectures by several leading authorities on prehistoric archæology. In his presidential address Prof. E. Fawcett describes some human skulls found by the society in a cave, associated with Late Palæolithic flint implements and remains of red deer, wild cat, and brown bear. The skulls are both dolichocephalic and brachycephalic, and Prof. Fawcett compares the discovery with one made at Ofnet, near Munich. Mr. L. S. Palmer gives an account of the exploration of another cave, in which numerous Late Celtic objects were found. There are also brief reports of the lectures at the meetings. The society is to be congratulated on its first year's work, and will have the best wishes of all who are interested in the study of early man, but its publication could be improved by more careful editing and by a judicious selection of illustrations produced in better style.

We have just received from Dr. Hornaday, the director of the New York Zoological Park, a very interesting and important summary of the results of the five-year close season which was deemed necessary in 1912 for the replenishing of the herds of the fur seal of the Pribilof Islands. The results have been everything that the close-season advocates foretold. This conclusion is supported by figures. In 1912 there were 215,738 seals of all ages; in 1917 they had increased to 468,692, and by 1919 these numbers had risen to 530,237. This protection was devised for purely commercial ends, and the result is most emphatically satisfactory. This much is shown by the fact that at the St. Louis fur auction held on February 2, 1920, there were sold for the United States Government 9100 skins of fur seals which averaged 140.98 dollars per skin. Skins of the same quality in 1918 averaged no more than 46.34 dollars, but in 1919 the price had risen to 78.38 dollars. Thus the cost of this fur has risen by leaps and bounds, and it is anticipated that it will rise yet higher. "In the future," Dr. Hornaday remarks, "when all other bearers of good fur have been utterly exterminated—*as they soon will be*—the protected fur seal herds will produce . . . a really vast quantity of the finest fur in the world. It needs no stretch of prophecy to foretell the annual increment to the three nations (the United States, England, and Japan) who are now so sensibly preserving the fur seals of Alaska from killing at sea."

The Forestry Commission has made considerable and satisfactory progress both in the acquisition of land and in planting. Landowners have shown considerable sympathy with the objects of the Commission, and in several cases free gifts of land or long leases on specially favourable terms have been obtained. The area of land acquired in the United Kingdom is as follows:—England, 9177 acres; Wales, 6329; Scotland, 23,472; and Ireland, 4716. This excludes lands acquired by the Department of Agriculture for Ireland, administered by the Commission. In addition, without taking into account estates which are merely under consideration, negotiations are proceeding in respect of the following areas:—England, 24,973 acres; Wales, 7900; Scotland, 6956; and Ireland, 7000. During the season 1919–20 the area of land planted, most of which is showing satisfactory progress, was 850 acres in England, 535 in Scotland, and 200 in Ireland. To meet the heavy cost of transport and the shortage of plants, new nurseries are being set up in the more important planting areas. Every effort is being made to increase the supply of seed and of seedlings. In conjunction with the

Ministry of Labour, the Commission has established at Brockenhurst, New Forest, a school for the forestry training of disabled ex-Service men, to be opened this month. A similar school is open at Birnam, near Dunkeld, and a new school for forest apprentices has recently started at Beauly, Inverness-shire. Offers of land may be addressed to the offices of the Assistant Commissioners at 22 Grosvenor Gardens, London; 25 Drumsheugh Gardens, Edinburgh; or 6 Hume Street, Dublin. All inquiries should be addressed to the headquarters of the Commission at 22 Grosvenor Gardens, London.

THE passages in classical literature which have been quoted in support of the assertion that mother-right existed in ancient Italy are discussed in the June issue of *Folk-lore* by Mr. H. J. Rose. One of the most daring attempts to support this position is a review of the succession of the members of the great Iulio-Claudian house (Journal of the Royal Anthropological Institute, vol. xlv., 1915, pp. 317 *sqq.*). A number of these counted their descent from women, but of matrilinear descent proper we have not a trace. In Italy, as Mr. Rose remarks, "we cannot, whatever we may do in Greece, weave sociological fantasies from the relationships of the gods, for the excellent reason that the Italian *numina* have no families. If any better arguments remain I shall await their production with interest, but, frankly, I have small expectation of anything of the sort."

IN a discourse delivered on March 12 last before the Royal Institution Mr. W. W. Rouse Ball, of Trinity College, Cambridge, gave an instructive illustrated account of the art of weaving string figures, usually made by weaving on the fingers a loop of string about 6½–7 ft. long so as to produce a pleasing design, often supposed to suggest a familiar object either at rest or in motion. Mr. Ball remarks that "friends who have learned the rules tell me that in convalescence and during long journeys the amusement has helped to while away many a tedious hour; moreover, the figures are easy to weave, they have a history, and they are capable of many varieties. Thus even in England the game may prove well worth the time spent in learning to play it; and admittedly to the very few who travel among aborigines it may sometimes be of real service."

MR. A. L. ARMSTRONG describes, in the *Naturalist* for July, with an illustration, an interesting series of six bone implements picked up by Mr. W. F. Jackson on a ploughed field at Rocher Head, Bradfield, near Sheffield, in 1888. They were associated with a number of well-worked implements of flint, consisting of round and horseshoe scrapers, a "spoon" scraper, and several trimmed flakes and knives. The collection has recently been examined by Mr. Reginald Smith, of the British Museum, and Sir W. Boyd Dawkins, who agree in regarding them as belonging to the Late Neolithic or Early Bronze age. The bones are those of an unidentified mammalian long bone, small metatarsal bones of a horse, and one of a bird. The largest specimen seems to have been rubbed down to form a blade on one long edge for about half its length, and it may have been used in dressing skins. They may be the grave-furniture of a tumulus now demolished, or may represent a hoard.

DR. WM. MCDOUGALL deals, in *Mind* (N.S., No. 115, July, 1920), with the problem of motives in the light of recent discussion. He reviews briefly his own point of view as expressed in "Social Psychology," the view, namely, that the instincts are the mainsprings or motives of all man's activity. He then considers, first, the criticism and suggested alterations of Prof. Woodworth, who in "Dynamic Psychology" maintains that, in addition to the instincts, other mechanisms, *e.g.* native capacities and acquired habits, have also driving force; and, secondly, those of Mr. Graham Wallas, who seeks to establish thought as an independent native capacity containing its own "drive." Dr. McDougall considers the various arguments brought forward by these critics, but fails to find them convincing, and himself brings further evidence in support of his original contention. The article is interesting and suggestive to all who, whether from the theoretical point of view of abstract thought or from that of the practical necessities of life, find themselves confronted with the difficult problems concerned with human motives.

IT is well known that the large European ground-beetle, *Calosoma sycophanta*, has been introduced into the eastern United States in order that it may prey upon caterpillars destructive to deciduous trees. Messrs. C. W. Collins and Clifford E. Hood have shown (*Journ. Agric. Research*, vol. xviii., No. 9, 1920) that an American tachinid fly, *Eubiomyia calosomae*, has formed the habit of laying eggs on the introduced beetle, within which the maggots feed.

WE have received No. 15 of the Journal of the East Africa and Uganda Natural History Society (November, 1919), which contains, among other interesting papers, a lecture on "The Geological History of the Rift Valley," delivered at Nairobi by Prof. J. W. Gregory, who narrates how on his journey of 1893 he had encamped on the unoccupied site of that now busy and important town. Dr. G. D. Hale Carpenter, in "Discursive Notes on the Fossorial Hymenoptera," summarises the results of his studies on the habits of tropical African Sphegidæ and Pompilidæ. This well-produced journal is, we notice, printed in England; so are the Annals of the Transvaal Museum, of which vol. vii., part 3 (June, 1920), comprises an important systematic entomological paper, "On the South African Notodontidæ," by Mr. A. J. T. Janse.

WHEN the surface of a lava-stream hardens, the lava below may continue to flow, forming a tube beneath the frozen crust. In some volcanic areas lava-tubes of small size are abundant, but those of great length are rare. In the Monthly Bulletin of the Hawaiian Volcanic Observatory for March, 1920, Mr. S. Power describes and illustrates a tube near Kilauea of unusual size, known as Thurston's tube, 1494 ft. in length, with a maximum height and width of 20 ft. and 22 ft. respectively. It opens into the Kaluaiki pit crater, and is one of the channels by which the crater was drained shortly before the final disappearance of the lava-lake.

IN the last issue of the *Bollettino* of the Italian Seismological Society (vol. xxii., 1919, pp. 129–42) Dr. Agamennone describes a series of slight earthquakes at Frascati on November 6–7, 1909, of the same nature as true volcanic earthquakes, and yet originating on the flank of the extinct volcano of the Alban Hills. Small as is the area covered by these hills, Dr. Baratta distinguishes within it nine seismic zones, and the interest of the shocks described lies in the evidence which they offer of the continual migration of activity from one of these zones to another. The same number of the *Bollettino* contains Dr. G. Martinelli's catalogue of 568 earthquakes felt in Italy in 1917 (pp. 164–87). This takes the place of the very full pre-war reports which formerly occupied several hundred pages every year. While the completeness of the record does not seem to have suffered under war conditions, the catalogue in its restricted form has lost none of its usefulness for statistical purposes.

IN the latest part of the Transactions of the Nova Scotian Institute of Science (vol. xiv., part 4) Prof. John Cameron describes two remarkable human skulls from South Malekula, in the New Hebrides. They seem to have been elongated by distortion through bandaging in infancy, and they exhibit the enormous development of the frontal air sinuses which are such a marked feature of the Melanesian skull. These sinuses not only produce very prominent superciliary ridges, but also, with the large maxillary sinuses, cause a flattening of the upper and lower margins of the orbit, imparting to it a quadrangular shape. The various features are compared with those of the known fossil human skulls from Europe.

DURING the war the Geological Survey of Egypt had the opportunity of obtaining some of the interesting fossil vertebrates from the Lower Miocene estuarine deposits at Moghara, and it has just published a description of the collection in a "Contribution à l'Etude des Vertébrés Miocènes de l'Egypte," by M. R. Fourtau. The fish-remains, on which there are notes by M. F. Priem, are unimportant, but among the reptilian remains there are fine skulls of new species of Crocodilus and Tomistoma and of a new genus of gavials named Euthecodon. The primitive artiodactyl mammals of the family Anthracotheriidæ are represented as usual by many valuable fragments, and there are several teeth of Mastodon. Dinotherium is curiously absent, and M. Fourtau finds it difficult to explain why this genus should be found with Mastodon further south in Africa. One tooth of a hyæna is the sole fragment of a carnivore, but there are two portions of mandibles of anthropoid apes which are of great interest. The figures of these two fossils are very unsatisfactory, but according to the description one seems to represent a new genus related to Hylobates, while the other may belong to a species of Dryopithecus.

Two lecture-demonstrations to teachers of science in secondary schools on "The Study of Crystals in Schools," given by Mr. T. V. Barker at the University Museum, Oxford, on August 10–11, are published in a small pamphlet by the Holywell Press, Oxford. In the first lecture some of the salient facts of crystallography were experimentally demonstrated on growing crystals by means of the lantern-microscope, in order to illustrate the lecturer's view that some instruction about crystals should be given in all chemical lectures and laboratories. It was pointed out that almost every answer to an examination question on atomic-weight determinations will include a dissertation on Mitscherlich's discovery of isomorphism learnt from a text-book, yet probably neither the candidate nor even the writer of the textbook had ever seen isomorphous crystals, and still less proved them to be so by simple measurement. In the second lecture some simple crystal measurements were made of microscopic crystals on the screen and of larger crystals with a contact goniometer, and the properties of cleavage, hardness, and density were also demonstrated on a variety of crystallised substances. The object aimed at, of showing the possibility of introducing simple experiments on crystals into secondary-school natural science teaching as part of the physics and chemistry courses, and of demonstrating how interesting to young people such experiments could be made, appears to have been fully attained, and doubtless many of the teachers who attended will make some effort to respond in their own schools, and thus to give our possible future chemists an early idea of the great value of crystallography to the chemist.

A VALUABLE contribution to South African botany is a paper on new and interesting South African mosses by Mr. H. N. Dixon (Trans. Roy. Soc. South Africa, vol. viii., part 3, 1920), in which the results are given of the work on collections received from various districts during recent years. A considerable number of novelties are described, new records established for South Africa, and many of the genera or species critically examined.

IN the *Kew Bulletin* (No. 5, 1920) Dr. A. W. Hill gives some account of the Tresco Abbey Gardens, Scilly Isles, emphasising their claim to be regarded as an Imperial asset of great importance to the botanists of this country whose woik concerns the botanical resources of the British Empire, and pointing out the desirability of arranging that systematic botanists should be given facilities for studying in the Gardens in the living condition the plants with which they have become familiar in the herbarium. In this favoured spot may be studied not only the principal features of the temperate regions of New Zealand and outlying islands, of Australia, and of South America, but also many of the characteristic features of the sub-tropical vegetation of South Africa. A great number of plants were introduced from Australia, New Zealand, and South Africa about the middle of last century by Mr. Augustus Smith, whose botanical enterprise and interest in gardening were continued by his nephew and heir, Thomas Algernon Dorrien-Smith, who succeeded to the lordship of the islands in 1872, and, since the death of Mr. Dorrien-Smith in 1918, by Major A. A. Dorrien-Smith. A feature of the collection is a large series of drawings of many of the interesting plants which have flowered in the Gardens. Dr. Hill refers to the possibility of growing New Zealand flax as a crop for the production of fibre in the Scilly Isles in co-operation with growers in Cornwall.

PROF. AUGUSTINE HENRY and Miss Margaret Flood contribute to the Proceedings of the Royal Irish Academy (vol. xxxv., B5, May, 1920) a botanical and sylvicultural account of the species of Douglas fir comprising the genus Pseudotsuga. The Douglas fir of North America is one of the great timber trees of the world, and includes two species, one the Pacific Coast, Oregon, or green Douglas fir (*Pseudotsuga Douglasii*), and the other the Rocky Mountains, Colorado, or blue Douglas fir (*P. glauca*). The Oregon species forms forests of immense trees on the Pacific Coast, and is now much cultivated in the British Isles, where its rapid growth and enormous yields of timber in a short term of years render it very valuable. The Colorado species throughout its home in the Rocky Mountains is much inferior in size and vigour, and is of little or no value in commercial afforestation in this country. The paper is mainly concerned with a comparative study of these two species, but is extended to include an account of the whole genus. In all, seven species have been distinguished; besides the two already mentioned, a third American species with very large cones occurs in Southern California, another in Japan, and there are also three very closely allied species, two native in Yunnan and one in Formosa. The microscopic structure of the leaves has been found to be a distinct and constant character in each species, evidently correlated with the special climate in which the tree is native. The oil obtained by distillation of the leaves of the Colorado and Oregon species respectively proves to be very distinct in chemical composition.

THE Meteorological Service of the Philippines has recently issued part iii. of the Report of the Weather Bureau for 1917. The report is entirely statistical, and contains the observations made at the secondary stations during the year. All the results are carefully collated and examined at the Central Observatory under the supervision of the director, the Rev. José Algué, S.J., and the tables fill 360 pages, closely printed. The stations extend from latitude 6°–20° N. and longitude 118°–144° E. Observations at first- and second-class stations are for each four hours, six daily, for most elements, and annual summaries are given for these stations at the end of the volume. Observations at third-class and rain stations are made twice daily. In all cases the results are separated into months, and the means and totals given. The stations in all number fifty-four. For world-meteorology, and especially for aviation, the Meteorological Service of the Philippines is doing work of a very high order. Data are given for atmospheric pressure, air temperature, relative humidity, rain, wind direction and movement, and cloud form and direction. A rough examination of the wind directions and the movement of the clouds is of considerable interest. The movement of wind is greater, and calms are far less numerous, during the day than at night. Easterly and north-easterly winds prevail generally during the winter, and southerly and westerly winds during the summer. There is a distinct range of wind in the twenty-four hours. Upper and lower cloud observations are given twice daily; the direction of upper clouds is less regularly entered than that of lower, and the latter show commonly a veering of four points or more in comparison with the surface wind at the corresponding time.

ONE of the Meteorological Office publications, a section of the *Geophysical Journal*, gives daily values of certain elements at the Meteorological Office observatories. The data, which are not all observed every day, include air pressure, temperature and humidity, wind direction and velocity, sunshine and rain, cloud amount and weather, solar radiation and grass minimum, magnetic elements, earth temperature, atmospheric electricity, earthquakes and aurora, nephoscope observations, and pilot-balloon soundings. The normals are not of equal value, as we find at Kew Observatory 45, 35, 30, and 12 years for different elements, and at St. Louis (Jersey) 26, 25, 23, 22, and 16 years; while of the four wind observatories three give the daily maximum gust, while the fourth can give only the velocity during the windiest hour. There are other points of considerable dissimilarity between the different observatories. Eskdalemuir is, of course, of recent foundation, but it does not seem necessary to quote five-year normals in the ninth year, and it would be better to adopt fresh normals every year until at least the tenth in the case of a new observatory. The South Kensington normals are for a seven-year period, so that the quinquennium is obviously not insisted upon. The information about the height of the anemometer head above the cups at Holyhead appears to be conflicting, figures of 4·2 m., 6·1 m., and 4·0 m. being all derivable from the headings.

THE benefits accruing from a considered system of engineering standardisation have met with but a tardy recognition in Australia. It was not until the necessities of war pressed the subject to the foreground that any tangible steps were taken towards introducing a systematic scheme for engineering standardisation in the Commonwealth. The Institute of Science and Industry has given considerable attention to the matter, and after informing itself fully as to what has been done in other countries, and consulting various experts in Australia, it has put forward the outline of a scheme for the creation of an Australian Engineering Standards Association. This scheme has now been agreed to by the engineering societies, and a recommendation made to the Commonwealth Government for the establishment of a Standards Association. In the meantime, the Institute of Science and Industry has taken a further step and issued an Australian Standard Specification for Structural Steel, with appendices as to the forms of standard tensile test-pieces and data as to standard sections, sizes, weights, and sectional areas.

Our Astronomical Column.

LIVERPOOL UNIVERSITY TIDAL INSTITUTE.—The first annual report of this institute, established in 1919 with funds provided by Sir Alfred and Mr. Charles Booth, gives a brief and interesting account of the work so far taken up under the auspices of Prof. J. Proudman, the honorary director, and Dr. A. T. Doodson, the secretary. Besides theoretical work on the seiches in Lake Geneva and on the dynamical equations of the tides, the study of tide-prediction has been vigorously prosecuted. The official British and American predictions of the tides in the Mersey, calculated by machines of Lord Kelvin's type on the basis of analyses made many years ago by a committee of the British Association, often differ by a foot in height between themselves, and from the actual observed heights by amounts up to 3 ft. Dr. Doodson finds that the predicting machines are susceptible to error, though further examination is necessary to determine whether to an extent which unfits them for use in research. Meanwhile, the institute has embarked on an intensive study of the tides at Newlyn, near Land's End, from the continuous record taken by the Ordnance Survey. This work has also been assisted financially and otherwise by a British Association committee. Analysis has been made by computations on a novel plan; the five most important constituents in the tides were first removed, using approximate values inferred from the results of analyses for neighbouring stations. This reduced the range from 18·5 ft. to 2·5 ft., and disclosed the presence of quarter-diurnal constituents, which also were removed by a method suggested by theoretical considerations. This revealed constituents of higher orders and the presence of some unremoved semi-diurnal constituents, as was to be anticipated. By this method the real constituents are discovered, and these alone removed.

LONGITUDE BY AEROPLANE.—The *Comptes rendus* of the Paris Academy of Sciences for August 2 contains a paper by M. Paul Ditisheim describing a new determination of the Paris-Greenwich longitude by the repeated transfer of a series of chronometer watches between the two observatories by an aeroplane. Twelve watches were used which had previously been tested at Teddington with most satisfactory results. They were packed in wooden cases surrounded by layers of wool, and remained in a horizontal position during transit. They were compared with the standard clocks at Greenwich and Paris by Mr. Bowyer and M. Lancelin respectively. The average time of transit was 2¾ hours; on one occasion the double journey was completed on the same day.

The resulting longitude difference is 9m. 20·947s., with a probable error of 0·027s. It is only 0·005s. less than the mean of the British and French results in the 1902 determination. It is needless to say that the new value does not claim anything like so much weight as that of 1902, in which the observers were exchanged and personal equation was eliminated. It is, however, an interesting confirmation of it, and it illustrates the fact, already known, that the use of the travelling wire in observing transits greatly diminishes personal differences. This fact gives ground for hope that the method of wireless signals, without interchange of observers, will give close approximations to the longitudes of all the participating observatories.

OBSERVATIONS WITH THE PHOTO-ELECTRIC CELL.—Prof. Joel Stebbins's valuable pioneer work with the selenium cell (with which he discovered the secondary minimum of Algol) is now being continued with still greater refinement with the photo-electric cell. The *Astrophysical Journal* for May contains two of his researches. The first is on the Algol-variable λ Tauri. The light-curve much resembles that of Algol, a secondary minimum being shown here also. Elements are deduced from Prof. Stebbins's results combined with the spectroscopic ones. The masses of the two stars are 2·5 and 1·0 times that of the sun; the radii are 4·8 and 3·6 times the sun's; and a third body is suspected with mass 0·4. The side of the secondary that is turned towards the primary is much brighter than the other, which is ascribed to the intense radiation of the primary.

The other star examined is π^5 Orionis. The variability was detected before Prof. Stebbins noted that it had already been classified by Lee as a spectroscopic binary (with only one visible spectrum). The total range of light is only 0·06 magnitude, yet the observations suffice to give a consistent curve. As this proves to be a sine-curve with two periods in the time of revolution, it is concluded that the light-variation does not arise from eclipse, but from the spheroidal figure of the bright component. The ratio of axes is 0·95, which is quite a reasonable figure.

Prof. Stebbins states that he has at last succeeded in obtaining a potassium cell, with walls of fused quartz, that gives complete satisfaction. It was only after ninety-eight trials that this result was reached.

The Scientific Investigation of the Ocean.

NEED FOR A NEW "CHALLENGER" EXPEDITION.

THE outstanding feature of the proceedings of Section D (Zoology) at the meeting of the British Association at Cardiff was the discussion on August 26 on the need for the scientific investigation of the ocean.

In opening the discussion, Prof. W. A. Herdman, president of the Association, pointed out that this need may be considered under two heads—the scientific need and the industrial. Simply as a matter of advancing knowledge, the need for much further investigation of the ocean is very great indeed, and biologists realise that the industries connected with those marine animals—fishes and others—which are of economic importance are all of them badly in need of scientific investigation. There is not a single marine animal in regard to which it can be said that we know anything like all there is to be known and fully understand its mode of life. Even our commonest fishes, such as the herring and the cod, are in some respects unknown and mysterious to us. Prof. Herdman then proceeded to give a few examples of the need for further investigation.

The first report of the Tidal Institute of the University of Liverpool, issued a few weeks ago, shows that the two independent published predictions of the Liverpool tides—one issued by the Admiralty and the other by the United States Coast and Geodetic Survey—"seldom agree; they often differ by a foot in height; also, both of them sometimes differ from the actual tide by as much as 3 ft. in height." It is evident from this report that the present state of affairs urgently calls for more scientific research both in regard to the theory of the tides and to the accuracy of observations.

The work of the bio-chemist and of the physical chemist in connection with hydrography seems likely to be of fundamental importance, *e.g.* the possibility of determining the point of entrance to known currents of water by means of indicators showing the hydrogen-ion concentration may be of practical utility to navigators. Then again, Otto Pettersson's submarine waves in the Gullmar Fjord and elsewhere, and their possible influence on the winter herring fisheries, is a subject worthy of further investigation. Enough is known as to the influence of variations in the great oceanic currents upon the movements and abundance of migratory fishes to indicate the need for further and more complete investigation of the subject.

Prof. Herdman pointed out that, though we may suspect that the periodic changes in the physico-chemical characters of the sea may be correlated with the distribution at different seasons of the microscopic organisms that are an important source of food to larger animals, the matter has still to be proved and worked out in detail. The plankton curve has to be traced and the succession of organisms explained in terms of environmental conditions, including the ion-concentrations in the water and the amount and quality of solar radiation, and that not only in temperate seas, but also in the tropics and in all the oceans, and at various depths. It is known that many marine animals are profoundly affected in their distribution by the hydrographic conditions. For example, it has been shown that the herring of our summer fisheries is influenced in its movements by the temperature of the water, the catches being heavier in seasons when the water is colder, so long as it is not below 54·5° F., when the shoals break up and disperse.

Bionomics is the basis from which all oceanographic work on the biological side started, and there is still much to be done in tracing and explaining the life-histories and distribution and relations of marine plants and animals. In this connection, Prof. Herdman referred to the recent investigations of Dr. Joh. Schmidt, who has devoted the present summer to an oceanographic expedition in the Atlantic, the work of which included a search for the spawning eel.

The whole of the large question of the evaluation of the sea—a natural extension of the old-fashioned faunistic work—is a great field for research lying before the oceanographer of the future. Dr. Petersen in Denmark has done notable work in the Kattegat and the Limfjord, but it is probable that the "animal communities" which he has defined differ in other seas, and will have to be worked out independently in each marine area. Prof. Herdman cited the excellent marine surveys made by Sumner at Wood's Hole and by the Royal Irish Academy at Clare Island as work on the right lines as a preparation for the evaluation of large areas.

Similarly, systematic plankton work, studied intensively and treated statistically, and correlated with the food of migratory fishes and of the post-larval and other young stages of all food fishes, is a promising subject requiring much further investigation. Dr. Hjort's suggestion that the future year-classes of commercial fishes may depend not only upon favourable spawning seasons, but also upon an exact coincidence between the appearance of the phyto-plankton in sufficient quantity in spring and the time of hatching of the larval fishes, provides a subject of careful and difficult investigation and of far-reaching practical importance. A cognate subject bearing upon the same practical results—viz. future commercial fisheries—is Dr. Johnstone's demonstration of a natural periodicity in the abundance of certain fish. The extent and causes of this periodicity clearly call for further investigation; and in any discussion of, say, pre-war and post-war fishery statistics, the possibility of this periodicity affecting the question must be kept in mind.

Prof. Herdman emphasised the point that it is impossible to keep purely scientific research and investigations with a practical end in view completely separate. They are inter-related, and the one may become the other at any point. It was in the purely scientific investigation of the bionomics of the "warm" and "cold" areas of the Faroe Channel, in the *Triton* in 1882, that Tizard and Murray incidentally discovered the famous Dubh-Artach fishing-grounds which have been so extensively exploited since by British trawlers. It was a French man of science, Prof. Coste, who made the investigations and recommendations that started the flourishing oyster industries at Arcachon and in Brittany. It was his purely scientific studies of the deep-sea deposits that enabled Sir John Murray to discover the valuable phosphatic deposits of Christmas Island.

Metabolism, the cycle of changes taking place in the sea, the income and expenditure and the resulting balance available, is perhaps the department of oceanography which deals with the most fundamental problems and is most in need of immediate investigation.

The question of the abundance of tropical plankton as compared with that of temperate and polar seas, the distribution and action of denitrifying bacteria, the variations of the plankton in relation to environmental conditions, the determination of what constitutes uniformity of conditions over a large sea-area from the point of view of plankton distribution, the questions of the ultimate food of the ocean, the supply of the necessary minimal substances such as nitrogen, silica, and phosphorus to the living organisms, and the determination of the rate of production and rate of destruction of all organic substances in the sea—these are some of the fundamental problems of the metabolism of the ocean, and all of them require investigation. Most of these, moreover, are cases where the biologist or the oceanographer requires to appeal for assistance to the bio-chemist. In fact, in many oceanographic investigations team-work, in which the specialists of two or more sciences unite in tackling the problem, leads to the best results.

To the question, then: Is there need for further investigation of the ocean? Prof. Herdman answered emphatically in the affirmative, and referred, in conclusion, to the two suggestions made in his presidential address: (1) that there should be established at Cardiff a department of oceanographic and fisheries research, and (2) that there should be a great national oceanographical expedition—that is, another *Challenger* expedition, fitted out by the British Admiralty, and embracing all departments of the science of the sea investigated by modern methods under the best expert advice and control. Such an expedition would require long and careful preparation, so even though the present time may seem to some inopportune to press such an undertaking, if this suggestion is received with favour by oceanographers, it might be wise to form a preliminary committee to collect information and prepare a scheme.

Prof. J. Stanley Gardiner urged that to obtain results in economic work on fisheries there must be advance in wider scientific research. He endorsed the suggestion of the president for the establishment of oceanographical investigation in Cardiff, and said that, in his opinion, if this country is to keep in the forefront of oceanographical research, a new *Challenger* expedition has become necessary.

Dr. E. J. Allen supported the proposals for a new deep-sea expedition, and illustrated the need for further researches on marine organisms by reference to his recent experiments at Plymouth on the culture of plankton diatoms in artificial sea-water. He found that in solutions of pure chemicals, having as nearly as possible the composition of sea-water, to which nutrient salts such as nitrates and phosphates were added, diatom cultures did not develop, but when to such artificial sea-waters traces (say 1 per cent.) of natural sea-water were added, very good growth occurred. Experiments indicated that probably an organic substance in the natural sea-water stimulated growth, but its composition was still quite unknown. The culture method had also been used to obtain a minimum figure for the number of organisms living in a given volume of natural sea-water, and had shown that, whereas the number obtained in the usual way with the centrifuge was 14,000 per litre for a particular sample, there must actually have been at least 460,000 per litre.

Dr. E. C. Jee directed attention to the necessity for elucidating the movements of the current of dense water which pours out of the Mediterranean and forms an intermediate layer in the deeper waters of the near Atlantic Ocean. It seemed to him likely that the current moves northward, and in certain circumstances comes to the surface within the region of the pelagic fisheries of the British south-western area. It is important to ascertain the influence of this current on the northward migration of planktonic organisms and on the migrations of plankton-feeding fishes, and the investigation of its boundaries would throw light on the salinity variations observed in the surface waters of the English Channel, which are known to exhibit varying degrees of periodicity.

Mr. C. Tate Regan remarked that the study of the ocean was important in many other ways than in relation to fisheries, *e.g.* it was found, during the war, that a knowledge of salinity and currents was of great value in regard to submarine operations and the course of drifting mines. He suggested that work oversea should include the investigation of the seas on the coasts of our colonies; the fauna of the great area within the 100-fathom line that surrounds the Falkland Islands and extends northward to Montevideo is known only from two hauls made by the *Challenger* and five or six by the *Albatross*. In view of the pre-eminence of our Navy, mercantile marine, and fisheries, this country should lead the world in oceanographical research.

Prof. C. A. Kofoid pointed out that the magnitude of oceanographical problems and their diversity necessitate a definite but flexible programme and the co-operation of many investigators, for without such co-operation results must be fragmentary. Standardisation of methods, elimination of unnecessary duplication, and international co-operation are indispensable. He remarked upon the need for a monthly bulletin which should contain a bibliography of the subjects in this field of work together with synopses of the contents of these papers—a work which might well be undertaken by the International Commission for the Investigation of the Sea. He referred briefly to the project for the renewed exploration of the Pacific which is under consideration by a committee of the National Research Council of the United States.

Prof. J. E. Duerden urged that in the organisation of any extensive scheme of research in oceanography, or of a new *Challenger* expedition, the possibility of assistance from, and co-operation with, the various Dominions should be kept in mind. He had no doubt that, upon proper representation being made, the Union of South Africa would take its part, both financially and in *personnel*.

Mr. F. E. Smith, director of scientific research at the Admiralty, stated that his department had considered the question of a new *Challenger* expedition, and was of opinion that such an expedition was required, and he felt sure that the Admiralty would take its share in the organisation thereof.

At the close of the discussion a resolution was unanimously agreed to pointing out the importance of urging the initiation of a national expedition for the exploration of the ocean, and requesting that the council of the British Association should take the necessary steps to impress this need upon his Majesty's Government and the nation. On the following day, at the Committee of Recommendations, this resolution also received vigorous support from other sections, *e.g.* those dealing with chemistry, physics, geology, and geography, in all of which, as well as in zoology, investigations are required which could be undertaken by such an expedition. The General Committee of the Association recommended the Council to appoint an expert committee to prepare a programme of work and to consider the *personnel* and apparatus required. It is the hope of all those who have heard the cogent reasons for the expedition that it may be possible for the Government, in the not distant future, to undertake this great enterprise.

The New Star in Cygnus.

FROM the occasional observations which I have been able to make of the nova in Cygnus, I have formed the impression that the star has followed the normal course for such objects, except that the rise to maximum may have been more prolonged, and the subsequent decline in brightness more rapid, than usual. On August 22, two days after discovery, bright lines were not discernible with a small spectroscope attached to a 3-in. refractor, thus suggesting that the maximum had not then been reached. The star was seen for a short time on August 23, when it had risen to nearly second magnitude, but there was no opportunity of making spectroscopic observations. On August 26 observations were made by Sir Frank Dyson and myself with the 12-in. reflector of the Penylan Observatory, Cardiff, from 10h. to 11h. G.M.T. The star was then very slightly brighter than δ Cygni, but not so bright as γ Cygni, so that its magnitude would be about 2·8. Bright lines were then well developed, $H\alpha$ being conspicuous, and also the group of four lines in the green assumed to be $H\beta$, 4924, 5018, and 5169. On August 28, so far as could be observed with a 3-in. telescope (in London), the spectrum showed no marked change, though the star had then fallen to nearly fourth magnitude.

A. FOWLER.

The announcement of the discovery of the new star in Cygnus was received at the Hill Observatory, Sidmouth, on the afternoon of August 21, but the cloudy state of the sky prevented any observation being made on that night. The sky was, however, clear on the night of August 22, and several photographs of its spectrum were secured. The following table sums up the observations taken since that date, and shows the fluctuations in magnitude recorded and the number of photographs of the spectra taken :—

Day.	State of Sky.	Estimated Magnitude.	No. of Spectra obtained.
21	Cloudy	—	—
22	Clear	2·8	4
23	Cloudy	—	—
24	Clear	2·2	3
25	Clear	2·2	5
26	Clear	2·8	4
27	Cloudy	—	—
28	Clear	3·6	3
29	Clear	3·8	4

On the night of August 22 the spectra were all very closely similar to that of α Cygni, the type of star which presents the most prominent enhanced-line stage. The nova spectra indicate dark hydrogen absorption lines only a little broader than those in α Cygni, and the dark enhanced lines are sharp and well defined, and correspond line for line with those in α Cygni. The only conspicuous bright lines are those at $H\gamma$ and at $H\beta$ and to the red side of $H\beta$. The nova had increased in magnitude by August 24, and all the lines in the spectrum became more diffuse and broader, the bright lines increasing in number towards the violet.

On August 25 the star was estimated to have retained the same magnitude as on August 24. All lines appeared a little more diffuse and the bright lines more conspicuous.

Dimming down to magnitude 2·8 on the night of August 26, the main spectral changes indicated an increase in intensity and width of the bright lines, so much so that the dark hydrogen lines became less broad, owing to the overlapping of the bright components. By August 28 the magnitude of the nova had reduced considerably, but the spectrum exhibited no great changes except that the bright hydrogen components showed signs of splitting up into two parts. On the night of August 29 the fall in magnitude had decreased somewhat, the star being about 3·8. The splitting up of the bright hydrogen components was more pronounced. All photographs exhibit extensive movement in the line of sight.

The foregoing general features illustrate only the most conspicuous changes in the spectrum up to date. The nova seems now to be following the ordinary course of the sequence of phenomena of previous new stars. WILLIAM J. S. LOCKYER.

This object continued to brighten until the night of August 23, when it attained the second magnitude; since that date the decline of lustre has been considerable, and on August 29 I estimated the magnitude as 3·9.

It is probable that in a week's time the star's light will be reduced to the sixth magnitude, in which case it will only be just visible to the naked eye on a clear, dark night.

The astronomical world has been fortunate during the last twenty years in being able to study the phenomena of three bright temporary stars, viz. Anderson's Nova Persei of 1901, Nova Aquilæ of 1918, and the one now visible. W. F. DENNING.

British Agriculture during Great War Periods.

A VERY interesting article by Lord Ernle appears in the June issue of the *Journal of the Ministry of Agriculture*. The subject is "Agriculture during Two Great Wars," and the state of agriculture in Great Britain during the Napoleonic wars is compared and contrasted with that prevailing during the recent struggle.

Shortage of corn was the great fear of our ancestors, and if the home harvests were deficient the deficiency had to be met by supplies from Northern Europe. But, since the climatic conditions of the two regions are practically the same, scarcity at home generally meant scarcity abroad. The weather was of the utmost importance, and everyone watched the skies with great anxiety. Provision for such a deficiency was, therefore, one of the main features of the Corn Laws down to 1815. If home harvests were abundant, then exports were encouraged by a bounty and imports of foreign corn were limited. Foreign corn, with its additional costs of freight and insurance, could rarely have lowered the price of English corn, while during scarcity home consumers benefited from the large corn acreage which was maintained by the export bounty. Between 1801 and 1816 the yearly average of foreign wheat imported was under 600,000 quarters, while in 1821 the imports were only 450,000 quarters. Yet between 1801 and 1821 the number of people supplied with home-grown breadstuffs had risen from 14,000,000 to 20,500,000.

Weather conditions were adverse for the greater part of the Napoleonic wars, yet food was not rationed, neither were prices controlled. The Government probably relied on the high prices to prevent extravagance in the use of a scanty supply. The condition of some of the poor people was improved by increases in wages and by the distribution of privately raised funds. To give relief the Poor Law was invoked, and this was a fatal blunder, the full consequences of which appeared only after the peace. Various other measures were adopted at different times: bread could not be sold until it was twenty-four hours old; the manufacture of spirits and starch was suspended; rice and maize were imported to mix with cornflour; the growth of potatoes was encouraged, and the corn bounties rose continually.

In spite of all this, the war period was a time of great prosperity for landowners and farmers. Enormous sums were spent on the erection of farm buildings, cottages, etc., and on the reclamation and improvement of land. A much higher standard of farming was adopted, and a better class of men was attracted to the land. After the war wages fell, unemployment was rife, and a period of great poverty followed. During the succeeding hundred years the economic importance of agriculture dropped from one-third to one-twentieth in terms of gross national assessment. The breadstuffs grown in 1821 would have supplied double the number of people provided for in 1914, and the agricultural interest, which was paramount in 1814, has now lost the greater part of its political power.

Naturally, these changes have been reflected in the agricultural policy adopted during the recent war. Although the agricultural industry has prospered, its prosperity has been small as compared with the period 1793–1815. The incentive of high gains, which provided the spur for great efforts during the French war, was not allowed to operate fully. During the latter part of the recent war much more was done for the consumers than for the producers, and the great exertions of the farmers in the face of unexampled difficulties were therefore all the more creditable. Fixing a flat maximum price for wheat meant that what was a good price in a good year would be a bad price in a bad year, and the whole loss fell on the farmer. In the French war the poorest consumers were subsidised out of the rates, while in the late war all consumers were subsidised out of the taxes. In all probability the farmer saved the taxpayers about 25,000,000*l*. between the years 1917 and 1919. The farmers experienced further difficulties in the shortage of labour, and to have secured an increased food-supply under these conditions was not only a notable achievement, but also a most valuable contribution to victory.

The labourer was the worst sufferer after the French war, but during the recent war and since the armistice agricultural wages have been increased and the hours of labour have been shortened. It is now universally recognised that the position of agricultural labourers must be improved. If high wages and shorter hours result in greater efficiency, then the industry will prosper; if they do not, then the industry will exist only under conditions which restrict employment. Lord Ernle concludes: "It rests with the men—and their leaders. Unless a new earth is created, there can be no new heaven to inherit."

Experimental Cell Studies.

IN an experimental study of cell and nuclear division, especially in *Vicia faba*, Sakamura (Journ. Coll. Sci., Imp. Univ. Tokyo, vol. xxxix., article 11) has made an important contribution, particularly with regard to the factors that may influence the form, size, and number of the chromosomes. He finds, in agreement with most previous investigators, that *V. faba* has twelve chromosomes, the earlier counts of fourteen being due to a constriction which appears constantly at a certain point on the longest pair of chromosomes. Two other species of Vicia have fourteen chromosomes, three have twelve, while *V. unijuga* is tetraploid, having twenty-four.

The investigations of Němec and others in chloralising root-tips and studying the effects on mitosis and the multiplication of chromosomes have been considerably extended, including treatment with benzene and chloroform vapour, ether, carbon dioxide, high temperature, electric currents, Röntgen rays, plasmolysis, and infection by the Nematode worm Heterodera. The chromosomes often shorten and thicken under this treatment, irregular mitoses occur, and frequently the number of chromosomes is multiplied, but there is no evidence of later reduction divisions in somatic tissues. Irregular reduction divisions in the pollen formation were also obtained by similar treatment.

A study of the chromosomes of wheat gives very different results from those of previous investigators. *Triticum monococcum* is found to have fourteen chromosomes (2 ×), four derivatives of Emmer wheat are found to have twenty-eight (4 ×), while three descendants of Dinkel wheat have forty-two (6×). This is a confirmation of the view that *T. monococcum* is the most primitive, while *T. vulgare* belongs to the most advanced, type—a view which is supported also by the phytopathological studies of Wawiloff, the serological tests of Zade, and the evidence from sterility of the various hybrids as obtained by Tschermak. The fundamental importance of cytological studies of agricultural plants is thus apparent. R. R. G.

University and Educational Intelligence.

CAMBRIDGE.—Mr. H. H. Brindley, St. John's College, has been re-appointed demonstrator of biology to medical students; Mr. J. T. Saunders, Christ's College, demonstrator of animal morphology; and Mr. J. Gray, King's College, demonstrator of comparative anatomy. Mr. E. J. Maskell, Emmanuel College, has been appointed to the Frank Smart University studentship in botany.

Graduate research studentships at Emmanuel College have been awarded to E. J. Maskell for research in plant physiology, to C. H. Spiers for research in stereochemistry, and to G. L. Jones for research in Celtic and Frankish institutions.

DR. GRIFFITH TAYLOR, at present physiographer in the Commonwealth Weather Service, Melbourne, has been appointed to a specially created position of associate professor of geography in the University of Sydney. He will take up the duties of his new position in the early part of 1921.

A REUNION of old students of the Royal College of Science, London, will be held on Tuesday, September 14, at 7 p.m., at the Imperial College Union, Prince Consort Road, South Kensington, London, S.W.7. The president, Sir Richard Gregory, will take the chair at 8 p.m., and Prof. H. E. Armstrong will deliver an address entitled "Pre-Kensington History of the Royal College of Science and the University Problem."

THE issue of the *Lancet* for August 28 is a medical students' guide for the session 1920–21. The various curricula are described in detail, and under their respective headings the necessary information concerning the facilities for medical study offered at the different teaching centres of the United Kingdom is given. The regulations for the examinations, both preliminary and professional, at these centres are set out so that the student desiring to obtain a medical degree from a university or a diploma from any medical corporation may ascertain the course of clinical instruction and the conditions under which submission for examination is allowed. The metropolitan medical schools and hospitals are grouped under the University of London; similarly, all hospitals in direct connection with provincial universities are described under the appropriate university. Finally, an account is given of the conditions under which commissions can be obtained in the Navy, Army, Air Force, Indian, and Colonial Medical Services.

WE have just received a "Handbook of Lectures and Classes for Teachers," issued by the London County Council. The range of subjects offered is very wide, and all the courses will be conducted by experts. Under the heading of geography, lectures will be given on physical geography, the use of instruments, and regional and historical geography—a course which will extend over two years. In addition, there will be lectures on the past and the future of the great towns of the world, and one lecture on regional surveys. In the department of mathematics the teaching of arithmetic, of mensuration and geometry in junior schools, and of elementary mathematics will be dealt with in five courses of lectures during the year. Science will be represented by courses of lectures on modern theories of time, space and matter, psychoanalysis, psychology, elementary astronomy, the special senses, experimental investigation of children, the industries of the Stone age, insects in relation to agriculture and disease, and laboratory arts, and there will be one lecture on insects as disease-carriers. As usual, there will be a course of single lectures on special subjects: Prof. J. N. Collie will lecture on the rare gases in the atmosphere; Prof. A. Fowler on recent developments in astronomy; Prof. A. Keith on the antiquity of man; Prof. R. Biffen on agricultural botany; Dr. Bateson on the heredity of sex; Dr. Forster on chemical technology; and Sir W. H. Bragg on the romance of science. The lectures will be open to all teachers employed either within or outside the administrative county of London. Full directions for the application for tickets of admission will be found in the handbook.

Societies and Academies.

PARIS.

Academy of Sciences, August 9.—M. Henri Deslandres in the chair.—A. **Blondel**: A new optical or electrical apparatus for the measurement of oscillations of velocity and angular deviations. The method is based on the registration on a photographic film moving at a uniform rate of the angular displacements of a disc carrying a series of equidistant slits, the disc being attached to the axis of the machine under examination. An application of the method to the study of an internal-combustion engine is given.—M. **Petot**: Extract from a letter to M. Appell concerning the spherical representation of surfaces.—B. **Delaunay**: The number of representations of a number by a binary cubic form with negative discriminant.—F. **Carlson**: The zeroes of the series of Dirichlet.—C. **Frémont**: Cause of the frequency of breakages of rails at their extremities.—J. **Rey**: Perrot's experiment relating to the movement of rotation of the earth. In 1859 Perrot observed certain rotations in a jet of water flowing from a hole in the base of a cylindrical vessel, and regarded these as due to the rotation of the earth. Laroque, in 1860, concluded that the observed phenomena were not connected with the earth's rotation, but many phenomena in geophysics have since then been explained on the assumption of the validity of Perrot's views. Experiments are described which lead to the conclusion that Perrot's views are erroneous, and fully confirm Laroque's criticisms.—H. **Godard**: Observations of the periodic comet Tempel II. made at the Bordeaux Observatory with the 38-cm. equatorial. The apparent positions of the comet and comparison stars for July 24, 25, and 27 are given.—A. **Buhl**: The symmetries of the electromagnetic and gravific field.—E. **Darmois**: The influence of ammonium molybdate on the rotary power of malic acid. A crystallised compound of malic acid and ammonium molybdate has been isolated. Its rotatory power is very high (+219°), and constant over a wide range of concentrations. There is a probability that this is not the only complex compound formed when ammonium molybdate is added to solutions of malic acid.—A. **Portevin**: The similitudes of micrographic aspect existing in various states between the iron-carbon (steels), copper-tin (tin bronzes), copper-zinc (brasses), and copper-aluminium alloys (aluminium-bronzes).—J. **Bougault** and P. **Robin**: Catalytic oxidation by unsaturated bodies (oils, hydrocarbons, etc.). Dichloroethyl sulphide, which alone or in solution is unaffected by oxygen, in presence of turpentine readily oxidises on exposure to air, the sulphoxide $SO(CH_2.CH_2Cl)_2$ being formed. By a similar oxidation, thiodiglycol in presence of citral is readily oxidised by air to the sulphoxide $SO(CH_2.CH_2.OH)_2$.—R. **Souèges**: The embryogeny of the Compositæ. The last stages of the development of the embryo in *Senecio vulgaris*.—P. **Lesage**: Experiments utilisable in plant physiology on osmosis and on the aspiration due to evaporation.—M. and Mme. G. **Villedieu**: The action of rain on the deposits of copper mixtures on plants.—J. **Amar**: How to determine the output of workmen.—A. **Migot**: The formation of the axial skeleton in *Eunicella* (*Gorgonia*) *Cavolinii*.—MM. **Fauré-Fremiet**, J. **Dragoiu**, and Mlle. Du Vivier **de Streel**: The histochemical differentiation of the fœtal pulmonary epithelium in the sheep.—M. **Piettre** and A. **Vila**: Some properties of serine.—C. **Lebailly**: The virulence of the milk in apthous fever. The milk is virulent before any appearance of symptoms characteristic of the disease, the high temperature being the only indication of departure from the normal healthy condition.—E. **Alilaire** and E. **Fernbach**: Some observations on the culture of the tubercle bacillus in non-glycerinated media.

ROME.

Reale Accademia dei Lincei, April 11.—A. Róiti, vice-president, in the chair.—U. **Cisotti**: Integration of characteristic equation of waves in a canal of any depth, iii. Formulæ for the effect of local perturbations are found.—P. **Comucci**: So-called hydrocastorite of Elba. Analysis shows that this is not a definite species of mineral, but rather a product of alteration found as a rule in different minerals of uncertain composition.—G. **Valle**: Interrupted incoherent sounds. An examination of the acoustical effects produced by sirens in which the openings are arranged in groups, the interval between which is not an exact multiple of the interval between the members of a group.—M. **Tenani**: Diurnal oscillations of wind velocity at different heights. Between May and September, 1917, regular observations were made with *ballons-sondes* at different times of the day at the Royal Aerological Station of Vigna di Valle for the purposes of aviation. The mean amplitude of diurnal oscillation decreases upwards, starting with 3 m./sec. at the ground. The oscillations parallel to the shore-line are almost negligible compared with those in a perpendicular direction. An attempt is made to determine a coefficient of correlation between the difference of temperature of earth and sea and the wind velocity perpendicular to the shore, but the mean error is too large to allow of the results being practically applicable up to the present.—C. **Ravenna** and G. **Bosinelli**: The dipeptid of aspartic acid and the function of asparagin in plants. In these experiments the dipeptid was obtained directly in a state of purity by prolonged boiling of a solution of asparagin.—R. **Raineri**: Corallinaceæ of the Tripoli coast, i. A description (accompanied by three figures) of the five species of the genus Lithothamnium found along the floor of the Tripoli Sea, namely, *L. crispatum*, *Haucki*, *Lenormandi*, *Philippii* (encrusting), and *fruticulosum* (lightly branching). Seven other species, to be described later, were also found.—M. **Ascoli** and A. **Fagiuoli**: Sub-epidermic pharmacodynamic experiences, iv. This deals with the action of thyroids and indirect effects.—L. **de Marchi**: Obituary notice of Prof. Vincenzo Reina, with list of works.—The Academy passed a resolution expressing the hope that the Government would secure for the nation the "Villa Gioiello," near Arcetri, in which Galileo spent the last ten years of his life.

April 25.—F. D'Ovidio, president, in the chair.—L. **Tonelli**: Points in the calculus of variations.—E. **Clerici**: A pulverulent mineral from Dorgali, in Sardinia. This mineral produces luminescence when heated. By comparing the corresponding effects with various fluorites it is inferred that this phenomenon arises from the presence of traces of rare earths.—R. **Raineri**: Corallinaceæ from Tripoli, ii. The remaining species are *Lithophyllum expansum*, *lichenoides*, *byssoides*, and *decussatum*, and *Melobesia Lejolisii*. In addition, two corallines with articulate thallus occurred.—L. **Pigorini**: Colouring matters from the eggs of silkworms. One gram of eggs was treated with a mixture of alcohol and acetone and the coloured extract tested with a Duboscq colorimeter. The coefficients of extinction of the various colours were found to differ according to whether the eggs were laid by the white, golden, or yellow type of female, or, again, by crosses of the two latter, and the results were sufficiently marked to be practically useful in testing the variety to which the ova belong.—L. **Pigorini** and R. **Grandiori**: Action of sulphide of lime on Lepidopterous ova. Pigorini found that sulphide of lime dissolves the shell of the egg without damaging the living elements, and Grandiori uses the method in his embryological studies of the eggs of *Bombyx mori*, *B. Yamamai*, and *Orgiya antiqua* with great success.—R. **Grandiori**: Symbiotic micro-organisms in *Pieris brassicae* and *Apanteles glomeratus*. Observations were made on four embryonic stages of the Pieris, in which were found symbiotic forms similar to others previously seen to penetrate the hypoderm of the parasitic Ichneumon.

CAPE TOWN.

Royal Society of South Africa, July 21.—Dr. A. Ogg, vice-president, in the chair.—P. A. **van der Bijl**: The genus Tulostoma, Persoon, in South Africa. This is a widely distributed genus, and in South Africa two species are thus far known, viz. *Tulostoma cyclophorum* and *T. Lesliei*, a new species, which the author describes in this paper.—P. A. **van der Bijl**: A fungus, *Ovulariopsis papayae*, n.sp., which causes powdery mildew on the leaves of the pawpaw plant (*Carica papaya*, Linn.). The author describes a fungus found in Natal on the under-surface of the pawpaw leaves as a new species, for which the name *Ovulariopsis papayae* is suggested.—P. A. **van der Bijl**: South African Xylarias occurring around Durban, Natal. Four species of Xylarias have thus far been collected by the writer around Durban, and of these three have not been previously recorded from South Africa.—W. A. **Jolly**: Note on the spinal reactions of the Platana. The author gives a note of reflex times observed in the spinal preparation of the Platana of the Cape Peninsula (*Xenopus laevis* or an allied species).—J. R. **Sutton**: A possible lunar influence upon the velocity of the wind at Kimberley (third paper). This paper deals with the variations in the speed of the wind when the moon is furthest from the earth. The discussion follows the same lines as the previous one, which dealt with the speed variations at perigee. The results obtained go to confirm the earlier ones. The diagram curves show generally the same turning-points as the perigee curves, but later in time, and the moonrise minimum is not so pronounced. The apogee curves average lower on the scale than the perigee curves. While the velocity of the wind tends to rise at perigee when the moon is above the horizon, it tends to fall at apogee.

SYDNEY.

Royal Society of New South Wales, July 7.—Mr. T. H. Houghton, vice-president, in the chair.—Dr. S. **Smith**: *Aphrophyllum Hallense*, gen. et sp. nov., and Lithostrotion from the neighbourhood of Bingara, N.S.W. The corals are referred by the author to *Lithostrotion arundineum* and *L. Stanvellense*.—J. H. **Maiden**: Descriptions of three new species of Eucalyptus. The first is a dwarf, mallee-like stringy-bark, from between Port Jackson and Broken Bay, closely allied to a moderately large tree, *Eucalyptus capitellata*. The second species comes from the summit of Mount Jounama, at an altitude of about 5400 ft., thirty miles south of Tumut. It is a large tree, a gum, and the bark falls off in strips as much as 30 ft. long. It is allied to the snow gum, *Eucalyptus coriacea*, and to one of the mountain ashes, *E. gigantea*. The third species comes from the drier parts of Western Australia, and it may be spoken of as the dry country representative of the Yate, *E. occidentalis*.

Books Received.

The Land of the Hills and the Glens: Wild Life in Iona and the Inner Hebrides. By S. Gordon. Pp. xii+223. (London: Cassell and Co., Ltd.) 15s. net.

Die Binokularen Instrumente. By Prof. M. von Rohr. Zweite Auflage. Pp. xvii+303. (Berlin J. Springer.) 40 marks.

A Text-book of Electrical Engineering. Translated from the German of Dr. A. Thomälen by Dr. G. W. O. Howe. Fifth edition. (London: E. Arnold.) 28s. net.

Einführung (Handbuch der biologischen Arbeitsmethoden). By E. Abderhalden. Pp. 44. (Berlin und Wien: Urban und Schwarzenberg.) 4 marks.

The Advancement of Science, 1920: Addresses delivered at the Eighty-eighth Annual Meeting of the British Association for the Advancement of Science, Cardiff, August, 1920. (London: John Murray.) 6s. net.

The Andes of Southern Peru: Geographical Reconnaissance along the Seventy-third Meridian. By I. Bowman. Pp. xi+336. (New York: The Geological Society of New York; London: Constable and Co., Ltd.) 27s. 6d. net.

Water-Plants: A Study of Aquatic Angiosperms. By Dr. Agnes Arber. Pp. xvi+436. (Cambridge: At the University Press.) 31s. 6d. net.

A First Course in Nomography. By Dr. S. Brodetsky. Pp. xii+135. (London: G. Bell and Sons, Ltd.) 10s. net.

Etude sur le Système Solaire. By Dr. P. Reynaud. Pp. xiv+83. (Paris: Gauthier-Villars et Cie.)

The Sea-Shore. By W. P. Pycraft. Pp. vi+156. (London: S.P.C.K.) 4s. 6d. net.

A First German Course for Science Students. By Profs. H. G. Fiedler and F. E. Sandbach. Pp. x+99. (London: Oxford University Press.) 4s. 6d. net.

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