NATURE

THURSDAY, AUGUST 11, 1921.

Editorial and Publishing Offices:

MACMILLAN & CO., LTD.,

ST. MARTIN'S STREET, LONDON, W.C.2.

University Finance.

THE problem of university finance is not a simple one, and the solution is not yet. Apart from endowments, which in this country are relatively small, the three main sources of income are students' fees, Parliamentary grants, and grants from local authorities, and these three bear no fixed relation to one another. Students' fees vary according to time and place; the Parliamentary grant, administered by the University Grants Committee, seems to be allocated according to no definite principle; and the local authorities may or may not contribute to the maintenance of the universities, and where they do contribute their subventions from the rates are by no means uniform in amount. Even in normal years the fluctuating character of the income makes the task of financing a university not a light one, while in abnormal times the task becomes one of difficulty and embarrassment. Under such conditions the marvel is that so many able business men have been found ready and willing in an honorary capacity to give their time and energy to help in directing the financial affairs of our universities. That they do so speaks much for the hold which higher learning has upon a valued and important section of the community, but such interest ought not to be looked upon as a justification of the system, or rather lack of system, of finance which exists at present.

One of the main principles which should govern university finance is that the income should be stable. There should be sufficient for necessary needs, and something over for development and expansion. Now it is no exaggeration to say that in most, if not all, of the universities at present this fundamental principle is more honoured in the breach than in the observance. Very few can budget with reasonable certainty for several years ahead, yet they most certainly ought to be able to do so if they are to fulfil the function for which they were founded. For one thing there should be reasonable assurance of reasonable salaries. But since in general the salary bill of the teachers is rather more than half the total expenditure, it is obvious that a fluctuating income makes it extremely difficult to give that assurance. The teachers may lament their hard lot, and it has been extremely hard with many, but if there is no certainty as to income from year to year there is little hope for a reasonable and proper amelioration. In view of such facts, it was well that university finance should be discussed at the Congress of the Universities of the Empire held recently at Oxford.

The subject was opened by Sir J. A. Ewing and Dr. Adami in two interesting and able papers. The former gave a comparative statistical survey of the larger universities, arriving at the conclusion that the average cost "per head," in the fourteen university institutions selected by him, amounted to approximately 65*l.*, and that of this sum 34*l.* was spent on salaries and superannuation, and about 6*l.* on administration. These figures, though they must be taken with some reserve, are interesting, but do not "cut much ice." Perhaps it is more helpful to learn that 25*l.* of the 65*l.* is paid by the student, and 20*l.* comes from Parliamentary grants, for then we gather that the student pays rather more than one-third of the cost of his education, while the Government pays rather less than one-third. But what further inferences we are to draw from these statements the learned principal of Edinburgh University omits to say. Incidentally, however, there emerge two facts which some of our more observing readers might have anticipated. The first is that among universities the economic advantage of large-scale working holds just as in ordinary business concerns; and the second, that the group of three Scottish universities is run at a lower cost per head than any of the other groups. In view of this it would have been interesting, and no doubt instructive, had the statistics contained a comparative table of salaries paid in the various universities or groups of universities quoted by Sir J. A. Ewing.

Turning to the paper read by the vice-chancellor of Liverpool University, one is struck by the clear exposition of the subject, the principles enunciated, and the policy suggested. The aggregate income of twenty-one institutions of university rank in Great Britain has been carefully analysed. From the figures given we find that students' fees amount to 39·7 per cent. of the total income; Parliamentary grants, 36·5 per cent.; and local authority grants, *plus* income from endowments, 23·7 per cent. As the aggregate income is more than 200,000*l.* short of the estimated expenditure, Dr. Adami suggests that the prospective deficit should be met by additional grants from local sources. He thinks that the contribution from the city in which the university is situated should be at least one penny in the pound, and that the other authorities, town and county, of the district served by the university should contribute at least one halfpenny in the pound. The only criticism we have to offer upon this is that Dr. Adami is too modest in his demands. There seems to be no sound reason why the whole of the local authorities in the Kingdom, urban and rural, should not contribute a uniform rate of one penny in the pound. The universities are not local, but national. Undoubtedly a penny rate for the whole country would ensure a greater measure of stability, and would go far to solve the problem of university finance.

Regarding the question of salaries, Dr. Adami quotes extensively and effectively from the memorandum prepared by the Interim Committee of the Conference of University Authorities and the Association of University Teachers. The scale of salaries suggested by the committee, and afterwards adopted by the conference, is given, as is also the estimated additional income required to put the scale into immediate operation in England and Wales. A rough estimate places the sum at about 350,000*l.*

On the subject of the superannuation of university teachers Dr. Adami is on firm ground when he says that the matter cannot rest where it is at present. The recent grant of 500,000*l.* from the Treasury (acceptable as it is) for the purpose of augmenting the superannuation allowances of certain of the senior members of the staffs of the universities is not only totally inadequate for its ostensible purpose, but also subjects those university teachers who have seen teaching in schools or technical institutions outside the university to differential treatment of quite an unjustifiable character. At present a schoolmaster of standing cannot accept a position in the university without a loss of pension benefits. This rift between the universities and the schools and technical and training colleges outside the universities cannot be allowed to continue. Anyone who has the best interests of the universities at heart will agree with Dr. Adami that "some method must be discovered whereby years spent in one service are duly recognised in the other for pension purposes." We have on more than one occasion expressed the same opinion in these columns.

One other point. No discussion of university finance where Parliamentary grants are involved would be complete without reference to the relation of the State to the university. There are some who see in the growing financial intimacy between the State and the university a threat to the autonomy of the latter. Whether this opinion is shared by our readers or not, we believe that the freedom of the university is so vital for its efficiency and its highest development that it is the duty of every university teacher to guard jealously this most valuable possession, and we believe they will not fail in this duty. It was natural and fitting, therefore, that the subject should come up for discussion at the Oxford congress, and, considering the issues involved, it is perhaps not a matter for surprise that it gave rise to one of the outstanding incidents of the congress—a brilliant speech by Sir Michael Sadler on the freedom of the university.

The Bible.

The Bible: Its Nature and Inspiration. By Edward Grubb. (Published for the Woodbrooke Extension Committee.) Pp. 247. (London: The Swarthmore Press, Ltd., 1920.) 2*s.* 6*d.* net.

IN this handy little paper-covered volume Mr. Grubb gives us a most readable and interesting historical account of the Bible and of our knowledge of its growth and development. As the advertisement on the cover justly says:—

"This . . . book explains what the Bible really is, and why Christians value it above all other books. Many suppose that if the Bible is not literally true, from beginning to end, it is of no use at all. That is quite unreasonable. The presence of human imperfection in the work of the men who wrote the Bible is no proof whatever that they had not a real and living message from God to the people for whom they wrote—and for us, if we will take the trouble to understand it."

Mr. Grubb is a believing Christian, and writes for Christians with a breadth of view that is a tribute to the writer's common-sense and humanity (in the higher sense of the word). One wishes one could say as much for many so-called "Rationalistic" writers, some of whom have been more bigoted and more intolerant, more narrow and uninformed, than the worst Roman "cagot" or Calvinistic heresy-hunter that ever lived. However, these professional anti-Christian fanatics are nowadays almost a thing of the past. A few who still survive here and there are mere relics of the mid-Victorian age who do not count. We are talking of Britain, of course; in France the species still lives and flourishes.

Nothing has contributed more to the rout of the old-fashioned Freethinkers than the discoveries that have been made since the 'seventies in the realm of ancient Oriental history and anthropology, which have shown that the Old Testament was not, as those of our grandfathers who considered themselves enlightened supposed, a collection of baseless fables, but real history, sometimes in the guise of legend, but more often in that of genuine copies of ancient annals. The cuneiform discoveries of Rawlinson and George Smith, the recovery of the ancient history of Egypt to the confusion of the supposedly intelligent but really extremely credulous Sir George Cornewall Lewis, the finding of the Moabite Stone, and the critical study of the text of the historical books of the Old Testament, have all shown that in the Bible we are dealing with real history and with tradition based upon facts. They show also that, in the obvious myths, such as those of the Creation and the Deluge, we have before us extraordinarily interesting accounts akin to the cosmogonical myths of the Babylonians, pointing to the origin of Hebrew civilisation. But in the relief which these discoveries gave to those Christians who demanded "belief in" the Bible (a phrase that meant everything to them, though to the more instructed it might mean anything or nothing) as a condition of their faith in Christianity, and in the triumph which the godly felt had been vouchsafed to them over the vain imaginings of the ungodly, the new discoveries were hailed as "proving the Bible"—as showing irrefragably that the Biblical books were all "inspired" truth, and that Moses wrote the Pentateuch after all. One sees that this would be of great interest and importance to a professing Jew, but one is puzzled to know how, even if Moses did not "write the Pentateuch," the fact could affect the faith of a Christian. A *new* law was given to the world by Our Lord, based, indeed, on the traditional beliefs and teachings of His people, the Jews, but owing nothing of its authority to them. Some Christians, however, of the Reformed Churches have always been more Jew than Christian!

There is, indeed, little fact behind the idea which one often meets that archæological discovery has "proved" the literal truth of the whole of the Old Testament, and incidentally "shown that Darwin was all wrong" (a very prevalent idea). Similarly, as little fact supports the idea of the old-fashioned Rationalists that the Bible was from beginning to end an invention of designing priests. Archæological discovery has certainly "proved the truth" of the Old Testament, but not in the literal sense which alone is comprehended by the simple-minded. Both the Tale of Troy and the Arthurian Legend are doubtless "true" in that they are indubitably based on fact, and that is what archæological discovery shows us with regard, for example, to the books of Joshua and Judges. "Kings" and "Chronicles" are annals; they are (as we can see by comparison with contemporary historical documents, Egyptian and Assyrian, as well as from internal evidence) more trustworthy than the others, as the Anglo-Saxon Chronicle may be more trustworthy than Giraldus Cambrensis or Geoffrey of Monmouth; they are on the same level as such chroniclers, no less, but also no more. The Bible must be looked at as objectively as any other scripture; and if we study it so, and also with reverence as the foundation of Christ's teaching, and as a Holy Book inspired by the Spirit of God, because it is the work of man, we shall understand many things that hitherto have been hid from us, and see clearly where formerly we were blind.

This is the position that Mr. Grubb holds in common with all enlightened Christians of to-day, whether English Catholic, Presbyterian, or Free. Rome still seems to affirm "verbal" inspiration officially. The English reformed branch of the Western Catholic Church, with its freedom won by the Reformation from the dead hand of ancient official pronouncements made in the days of ignorance, has during the last fifty years obtained for itself a reputation for freedom of discussion and scientific criticism of Biblical matters on the part of its learned divines even more honourable than that of its fellow Protestant Churches. Many of the greatest lights of the "Higher Criticism" have been English Churchmen, and obscurantist circles have often been scandalised by the fact.

Unhappily, one or two of the "Higher Critics" have gone much too far with their textual criticism, and honestly, but mistakenly, have invented a new Old Testament of their own imagining, and a very dull and uninspired thing at that. The text of the real Book is often obscure, and not seldom corrupt, so that it must be emended, but not so much so as to be a sort of Bacon-Shakespeare cryptogram which can be elucidated only by methods strongly reminiscent of Mrs. Gallup! The "Higher Criticism" does not connote this sort of fantasy; what it really is Mr. Grubb shows with both knowledge and skill. From his little book the interested reader can see just how far archæological discovery has confirmed the general historical character of the legendary and annalistic books of the Old Testament, and he will be able to realise what "textual criticism" means in the case of Hebrew manuscripts; the distinctions between the different schools of early Jewish religious writers that "wrote the Bible"—the Jahvist ("J"), the Elohist ("E"), and the Priestly ("P") writers—will be made clear to him. He will also be able to understand the fact of the various "strata" of Isaiah, which can be printed, if necessary, in different colours to distinguish them.

The Bible, treated scientifically and subjected to the same criticism as any other collection of ancient legends and poems, becomes extraordinarily interesting. If the sacred books of a religion cannot stand criticism, they are not worth much. The "Book of Mormon" cannot stand criticism; the Holy Bible can and does. Literal truth at all times and in all places is not the question. Christianity does not stand or fall by the "verbal inspiration" or literal "truth" of the Old Testament, but rests foursquare and secure on the teaching of its Founder as given to us in the New. He regarded the scriptures of His ancestors with the same reverence that we do, who seek out and study their origins and growth in order that we may the better understand the bases of our faith and so teach it *ad majorem Dei gloriam*. H. H.

Zoology for Medical Students.

An Introduction to Zoology. By Prof. C. H. O'Donoghue. Pp. x+501. (London: G. Bell and Sons, Ltd., 1920.) 16*s*. net.

THE object of this volume is to provide a text-book for the zoological portion of the syllabus in biology for the first examination for medical degrees of the University of London, and for the first examination of the Conjoint Examining Board in England.

For an introduction to zoology for medical students, the subjects discussed, the degree of fullness of treatment, and the order of their presentation are admirably suited. After a preliminary chapter, the frog is first treated as an introduction to anatomy, physiology, and histology; then follow accounts of two free-living and two parasitic Protozoa—Amœba and Paramæcium, Monocystis and the malaria parasite. A chapter is given to Hydra and Obelia, and another to the earthworm and Tænia, while the dogfish is treated at length. An account of the rabbit takes up nearly 100 pages, and this section includes—an excellent addition—descriptions of the skull of the dog, and of the brain and heart of the sheep. A chapter on histology and cytology follows, which deals mainly with cytology, including spermatogenesis and oogenesis; the section on embryology introduces the early development of Amphioxus and of the frog (which finds its place here instead of in the earlier chapters); while the chick and rabbit are treated more completely. A final chapter is devoted to evolution, variation, and heredity.

The present writer is convinced that such a course, beginning with a fairly full account of an animal that goes on four legs, the internal arrangements of which correspond in some degree with what the beginner already knows of his own body, and then working upwards from the Protozoa, is, as the author has found, the most satisfactory from the point of view of both teacher and student. The number of forms to be studied must be sufficiently large to serve as a basis for the wider appreciations and generalisations on the comprehension of which the value of the course to the medical student depends. Medicine is applied biology, and if the student does not grasp the fundamentals at this stage he will not do so from the specialised study of human anatomy and physiology at a later period. At the same time, as the author implies, the multiplication of types beyond what is strictly necessary to illustrate fundamental principles is to be deprecated as involving a study of unnecessary details. In the present state of the medical curriculum there is no excuse for presenting the ordinary student with a survey of the whole animal kingdom—a practice that perhaps still survives in places. A complete study of a few well-chosen forms, with similarly thorough laboratory work on those forms, is worth more for the purpose of giving an insight into biological principles—and infinitely more as a training in scientific method and thoroughness—than skimming over all or most of the phyla of Invertebrates and the classes, or even orders, of the Vertebrates.

A few criticisms of details are necessary. The account of the physiology of digestion is, quite suitably, an account of mammalian digestion, but this should be stated; instead, it is said to be an account of the physiology of digestion in general. The respiratory movements in the frog are badly explained; if they took place as described, no air would ever be expelled to the exterior, and the animal would ultimately burst. Also the description of the frog's truncus arteriosus is not easy to follow, and there is no explanation of how its mechanism works, and no statement that the arterial arches contain blood of different degrees of oxygenation. The author appears to distinguish ague from malaria ("three distinct diseases, malaria and two kinds of ague"); while only the maximum recorded length (36 metres) is given for *Taenia saginata*, which is surely liable to mislead the student as to its usual dimensions.

The last chapter, which is so important, is too compressed; the subjects of the forty-two pages include evolution; variation, heredity, and selection in connection with the Darwinian theory; Mendelism; and palæontology as illustrated especially by the reptiles; while the topics of the evolution of sex and its meaning do not appear to find a place. Some of the figures introduced from Bourne's "Comparative Anatomy of Animals" have suffered considerably—*e.g.* those of the nephridium of the earthworm and of the skeleton of Scyllium; they are unworthy of a place in the book.

Errors which have been noted are as follows: A trochanter is a prominence, not a depression (p. 30); to say that membrane bones are "formed by bone tissue being laid down in a membrane" is neither adequate nor correct. Among misprints one might note "Calkin," "Bütchli," "Weissmann," "strobilla," "alteration" (of generations), "aborizations" (p. 95), "cœcum" and "stomodœum" for "cæcum" and "stomodæum," and "anistropic" (p. 42). "Pre-caval" contains an unnecessary hyphen, while in "sub-cutaneous" and "sub-clavian" it is more than unnecessary.

The author, in his preface, acknowledges his indebtedness to Profs. Dendy and Hill, especially to the latter, on whose lecture-notes parts of the book are more or less directly based.

The Analysis of Steel.

The Chemical Analysis of Steel-Works' Materials. By F. Ibbotson. Pp. viii+296. (London: Longmans, Green, and Co., 1920.) 21*s.* net.

THE "Analysis of Steel-Works' Materials" of Brearley and Ibbotson has long enjoyed a reputation as a sound and trustworthy manual of the subject with which it deals, and its contents are familiar to most steel analysts. The revision of the work has been undertaken by one of the authors only, and advantage has been taken of the occasion to extend the treatment of steels, alloys, slags, etc., on the analytical side, and to gain space for such extensions by omitting the sections of the earlier work dealing with pyrometry and the use of the microscope. In the interval which has elapsed since the original publication many books on these two subjects have made their appearance, and their development has been so rapid that it has become undesirable to attempt their treatment in the course of a few short chapters in a work devoted mainly to a different branch of the subject. Mr. Ibbotson's experience of the analysis of steel-works' materials is exceptionally wide, and the methods which he describes have been in all cases personally tested and compared with alternative processes, so that the author may be accepted as a safe guide, especially in the difficult region of the analysis of high-speed tool steels and other complex alloys containing the rarer metals.

The separation of the rarer elements has been worked out with great care for the purposes of mineral analysis, and it is possible, by following somewhat laborious methods, to effect a complete separation of the metals contained in a mineral with a high degree of accuracy, as has been shown more particularly by American work on the composition of rocks. The analysis of complex steels, however, calls for processes which are rapid as well as accurate, since the results are usually required for commercial purposes within the shortest possible time. The high cost of the rarer alloy metals makes their exact estimation very important, whilst certain alloy steels are remarkably sensitive to minute variations in the proportions of the added elements, so that to devise methods which will yield, in the hands of the works chemist, results of the required accuracy in a reasonably short time is a task of some difficulty. The author lays great stress on accuracy, so that while his methods are not invariably the most rapid, they are such as can be trusted where a gain in speed might possibly be accompanied by a serious risk of error.

The work differs a little in its arrangement from most text-books on the subject. The opening chapter deals with certain reactions of a more or less general character, including the separation of iron from other metals, the reduction of solutions by nascent hydrogen by means of the Jones reductor, and the precipitation of chromium, molybdenum, tungsten, and vanadium by means of mercurous nitrate. The succeeding chapters describe the estimation in turn of the elements which usually occur. The methods for the direct combustion of carbon are comparatively slow, and it would have been advantageous to add a description of the rapid methods, using small electrically heated tubes, which were devised during the war for the enormous number of shell steel samples which had to be analysed in the Admiralty and other laboratories. In such rapid methods soda lime is used with advantage in place of the more cumbrous potash bulbs. In the Volhard estimation of manganese the simple method of precipitation with zinc oxide and titration without removal of the iron, which is very convenient in the analysis of ferro-manganese, is not mentioned. The estimation of sulphur and phosphorus in steels, about which disputes are most frequent, is treated very thoroughly.

The analysis of ores, refractories, slags, fuels, and boiler waters is dealt with in later chapters. The section on slags suffers somewhat from its brevity, and many chemists would welcome a fuller account of this important subject. Thus in the analysis of basic slags no mention is made of the distinction between total and available phosphoric acid, on which the value of the slag so largely depends, and it would also have been well to include some account of the estimation of fluorine in such slags; the addition of fluorspar in the basic open-hearth process is frequently practised, and its effect is to convert a part of the phosphoric acid into an inert form.

Mr. Ibbotson's work may be confidently recommended to the analyst and student as a trustworthy guide to the subject by an author of ripe experience in the field in which he has worked so long.

Relativity and Gravitation.

Relativity: The Electron Theory and Gravitation. By E. Cunningham. Second edition. (Monographs on Physics.) Pp. vii + 148. (London: Longmans, Green, and Co., 1921.) 10s. 6*d.* net.

THE second edition of Mr. Cunningham's book, like the first, aims at presenting the problems of relativity in a form suitable for the general physicist. More than half the book deals with the special theory, giving the fullest account of the experimental side in any English book. This part is practically unchanged from the first edition—too little changed, for one would have liked to see the author's views on Majorana's experiments, which are not mentioned.

In discussing the general theory, he follows the historical order of development, commencing with Eötvös's experiment, which showed that the weights of two bodies of different constitution in the same gravitational field are proportional to their inertias within 5 parts in 10^8. From this he advances by a series of generalisations. First, light has inertia; if Eötvös's result is true for it, it must also have weight. Therefore it cannot travel in straight lines in a gravitational field. Therefore the differential ds, which is intimately related to the behaviour of light in the special theory, must, if it is still to maintain this relation to light, have a form in a gravitational field that takes the field into account. It has also a relation to the motion of a particle in the special theory; we knew already that it would have to be modified in form to maintain this in a gravitational field.

It is therefore assumed that the same form will still answer both purposes. Previously, again, the law of gravitation satisfied a condition that was unaltered by any displacement of the origin or rotation of the axes. Suppose, then, that the coefficients in the new ds satisfy a condition that is unaltered by any change in the co-ordinates used; the class of changes admitted is to be as wide as will permit some such condition to be satisfied. This leads at once to the irrelevance of the mesh system, and appears to the reviewer to be the best reason yet advanced for attributing to this principle any appreciable prior probability.

The crucial tests of the theory are described, and a chapter is devoted to Weyl's theory of electric and magnetic forces. The book is well arranged and written. Enough does not seem to be made, however, of the crucial tests. For anything that any professed exponent of the theory has said, there might be a million other theories, all as probable as Einstein's, which would give the same predictions. It may be pointed out that on p. 114 the assumptions given are not enough to ensure that the coefficient of $drdt$ shall be zero, which is assumed a few lines later; that in the footnote on p. 107 it is implied that a purely imaginary quantity can have a true minimum; and on p. 120 that the mere fact that the resultant velocity of an object is known is not enough to determine its path. But in the main the book is a careful and sound analysis, and can be recommended to all students of the theory.

H. J.

Our Bookshelf.

The North American Species of Drosophila. By A. H. Sturtevant. (Publication No. 301.) Pp. iv + 150 + 3 plates. (Washington: The Carnegie Institution of Washington, 1921.)

A SYSTEMATIC account of the North American species of Drosophila and related genera, which includes many new species from collections made in various parts of the continent, will be found in this volume. One of the chief features of interest in such a monograph lies in a comparison of the systematic differences distinguishing species with those distinguishing mutants. In the latter part of the work this subject is discussed. The species of Drosophila, although often closely alike in appearance, so that only intensive study has succeeded in separating them, are extraordinarily difficult to cross. This applies not only to those having different chromosome groups, which no one has yet succeeded in crossing, but also to those in which the chromosomes are alike.

Dr. Sturtevant points out that both species and mutants may differ from each other in such features as eye-colour, wing-shape, abdominal pattern, and size and shape of eyes; but in studying specific differences it is "often necessary to examine minute characters, such as wing-vein indices or the relative sizes of certain bristles, that are seldom examined in material bred for genetic purposes." Many of the mutant characters are, however, similar to those observed between species. The general impression is received that specific differences and mutations may both be found in practically any character studied. The species usually differ slightly in innumerable characters, while mutants often differ strikingly in a few. This does not indicate that specific and mutational characters are different in kind, but that only the smaller mutations, by upsetting less the economy of the species, usually survive as specific differences. R. R. G.

Introduction to General Chemistry. By Prof. H. Copaux. Translated by Dr. H. Leffmann. Pp. x + 195. (Philadelphia: P. Blakiston's Son and Co., 1920.) 2.00 dollars net.

IN its translation into "standard English" (*vide* preface) Prof. Copaux's excellent little book has suffered considerably. It may be that "chlorin," "sulfur dioxid," and "do not have" are "standard" English, but in many cases the translator does not appear to have understood what he was doing, and the result (*e.g.* p. 55) is quite unintelligible. There are numerous minor inaccuracies in translation, and others are added in the footnotes contributed by the translator. Through someone's lack of care, several dropped letters have been passed unnoticed. It is regrettable that before sending the book to the printers the translator did not submit his manuscript to someone with a knowledge of physical chemistry. In this way some serious errors might have been avoided. "Wolcott Gibbs" on p. 139 should be "Willard Gibbs."

A Last Diary. By W. N. P. Barbellion. With a preface by A. J. Cummings. Pp. xlviii + 148. (London: Chatto and Windus, 1920.) 6s. net.

To speak frankly, we prefer Bruce Cummings to Barbellion—that is to say, the man as he appeared to others rather than as he chose to appear to himself. This diary, no less than the former, contains some brilliant bits of writing, but its mixture of slang and literariness, of wit and self-exposure, grows wearisome. In style and in substance Mr. Arthur Cummings's account of his brother is more pleasing. Barbellion's life was a tragedy, but he succeeded, apparently with intention, in depicting it so as to arouse irritation instead of sympathy. One longs to pity him, but that is the last thing he will permit. As a psychological document, however, the book is profoundly interesting, and for the humanist it is redeemed by the gradually touched-in portrait of simple, lovable old Nanny.

Impressions and Comments. Second series. 1914–20. By Havelock Ellis. Pp. 248. (London: Constable and Co., Ltd., 1921.) 12s.

IT is a pleasure, and in these days a relief, to turn to diarial musings distinguished by sanity, simplicity, and sobriety of statement. Mr. Havelock Ellis may hold strong views, he may deal boldly with dangerous subjects, but he expresses himself so calmly, so frankly, and with an undercurrent of such delicate humour that it were surely impossible to take offence. Unkind fortune had not hitherto distributed the books of Mr. Ellis to this reviewer, who therefore was unprepared for the discovery that one of whose work in other directions he knew was also among the most delightful writers of our day.

Here is no room to quote, though one can scarcely refrain in the face of that exquisite "Christmas Day, 1919." Nor is this the place to challenge an occasional argument; yet in suggesting that familiarity made the ancient Greek insensitive to the charm of the Athenian atmosphere Mr. Ellis has surely forgotten the famous phrase of Euripides: ἀεὶ διὰ λαμπροτάτου βαίνοντες ἁβρῶς αἰθέρος.

That which gives a poignant and peculiar quality to the book is the clear-eyed realisation of approaching departure. We seem to see an honoured worker, resting from his labours on the deck of a vessel that bears him over calm waters to a serene sunset. He looks forward and backward with equal mind, and ever and anon pens some brief message of wisdom or good cheer for those whom he is leaving on the shore.

Diseases of the Ear. By Dr. Philip D. Kerrison. Second edition, revised and enlarged. Pp. xxi + 596 + vi plates. (Philadelphia and London: J. B. Lippincott Co., 1921.) 35s. net.

THIS is one of the best works on diseases of the ear that have been published for a long time. It is very full and comprehensive, and is written with lucidity and even literary charm. It cannot be too highly praised and recommended.

New Studies of a Great Inheritance: Being Lectures on the Modern Worth of Some Ancient Writers. By Prof. R. S. Conway. Pp. viii+241. (London: John Murray, 1921.) 7*s*. 6*d*. net.

PROF. CONWAY'S "Great Inheritance" is classical—in this instance Latin—literature, and the authors with whom he is chiefly concerned are Cicero, Vergil, Horace, and Livy. It is not necessary to dwell upon the numerous instances in which Prof. Conway's originality and insight are brought to bear upon the interpretation of doubtful or obscure passages. It is enough to say that, even in dealing with comparatively technical points such as the authenticity of the minor Vergilian poems, he sees and, what is more, can convey to his readers their broader significance as elements in the history of culture, and, in particular, their bearing upon the problems of modern life. Most readers, we expect, will turn again and again to the lecture on "Man and Nature in the Augustan Poets," which, with its illuminating parallel between the circumstances which led Vergil and Wordsworth respectively to seek consolation and inspiration in Nature, is, in a brief compass, one of the best studies extant of Vergil's point of view.

In the final essay, on "Freedom and Culture," which, in a sense, sums up Prof. Conway's whole position, he indicates how the classical conception of freedom has moulded the social and political life of this country through our traditional system of education. To point out that this system of education is confined to one class which is ceasing, if it has not already ceased, to be predominant, raises the question of the comparative merits of political ideals and tendencies, which it would be out of place to discuss here.

Some Investigations in the Theory of Map Projections. By A. E. Young. (R.G.S. Technical Series, No. 1.) Pp. viii+76. (London: Royal Geographical Society, 1920.) 6*s*. net.

THE first of the new series of technical publications issued by the Royal Geographical Society is an exhaustive investigation of map projections based upon Airy's idea of making the mean square scale error a minimum. This principle was applied by Airy to zenithal projections as affording a reasonable compromise between the stereographic projection and the projection of equal area. Mr. Young shows how the arbitrary constants in Airy's solution should really be determined, and then proceeds to compare the minimum error projection with others belonging to the zenithal class. The conclusion reached is incorporated in a recommendation to cartographers to use the equidistant projection with total area true as being the best zenithal projection for all cases, except when some specially desired feature necessitates a different projection.

Similar methods are applied to conical projections. It is shown that for a zone the minimum error conical projection is nearly identical with Murdoch's third projection—a remarkably accurate and simple process invented so far back as 1758, and in the opinion of the author the very best of all conical projections. Later chapters deal with the spheroidal shape of the earth, polyconic projections, finite errors, and the convergency of meridians.

The paper is mathematical throughout. The algebra is laborious, but the results are of great interest. Mr. Young's paper is a valuable contribution to the subject he deals with, and sets a high standard for the series it initiates.

Elementary Vector Analysis: With Application to Geometry and Physics. By Dr. C. E. Weatherburn. (Bell's Mathematical Series.) Pp. xxvii+184. (London: G. Bell and Sons, Ltd., 1921.) 12*s*. net.

AN excellent introduction to the subject of vector analysis is provided by this book. It is admirably clear, and a natural temptation to develop so fertile a theory in excessive detail and to multiply its applications has been successfully resisted. It is a more elementary work than Dr. Silberstein's "Vectorial Mechanics," and still more so than Joly's "Manual of Quaternions." All the ideas which are based on the differential operator of Hamilton are excluded, and the applications are limited to geometry and to the dynamics and statics of rigid bodies. Enough remains to place in a clear light the general principles of the subject, and its value is less apt to be obscured by the complexity of the material. It is understood, however, that the author contemplates a second volume, in which the higher developments will doubtless be treated. Without such a sequel the reader will be left unprovided with some of the most characteristic and important notions of the calculus.

The diversity of notation has always been, and is likely to remain, a hindrance to progress. The existence of Hamilton's system seems to have had a centrifugal result, and Tait's controversial methods probably had an effect precisely the opposite of that intended. The present author adopts the notation of Gibbs. At the moment the wider diffusion of vectorial methods is very desirable, and though the absence of a uniform notation increases the difficulty of pursuing the subject in different books, it is an obstacle on which too much stress can easily be laid.

The Formation of Colloids. By Prof. The Svedberg. (Monographs on the Physics and Chemistry of Colloids.) Pp. 127. (London: J. and A. Churchill, 1921.) 7*s*. 6*d*. net.

IN this small monograph the author, whose brilliant investigations on colloids are familiar to all interested in that important branch of science, has given a very concise account of much recent work on the formation of colloids. References to the literature are given, and the book is valuable in bringing together much scattered information on the subjects of which it treats. The printing and illustrations are well done.

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.*]

Atmospheric Refraction.

THE proposition that "the course of a nearly horizontal ray of light in the lower part of the atmosphere is a circular arc having a radius of 14,900 geographical miles" has been stated by Mr. Mallock in a letter in NATURE of June 9, p. 456. Mr. Mallock states later on in the same communication that rays that are pointed a few degrees up or down will still be arcs of a circle of 14,900 miles radius.

It has been customary for many years in all survey departments to assume that the angle of refraction on a ray bears to the angle subtended at the centre of the earth a ratio denoted by k, which is called the "coefficient of refraction," assumed to be constant at a given point for all rays. It is easy to see from this that the ratio of the curvature of the ray—tacitly assumed to be circular—to the curvature of the earth is $2k$; and that if $2k=1$ a horizontal ray would circle the earth. According to Mr. Mallock's result, $2k=3960/14{,}900$, taking the earth's radius as 3960 miles, which leads to $k=0{\cdot}133$. Now this is *not* a value ordinarily met with in practice. In Clarke's "Geodesy" values of k derived from observations of the Ordnance Survey are given as 0·0809 for rays over water and 0·0750 for rays over land. These values are not far different from values obtained from other surveys.

Mr. Mallock's reasoning is based on the equation

$$v_h=v_0\left(1-a\frac{H-h}{h}\right).$$

When $h=0$ this becomes $v_0(1-0{\cdot}00029)$, or v_0/μ, where μ is the refractive index of air at standard pressure and temperature. While this is correct, it appears to me to be quite erroneous to consider the equation as giving the correct velocity at heights of a few thousand feet. It may not be incorrect to state for a limited range of height that the velocity varies as the height; but surely it is incorrect to deduce the factor of this variation from an assumed law which gives the velocity at height H (the height of the homogeneous atmosphere=8·3 km.) equal to the velocity *in vacuo*?

If the refracted ray is circular and of the same radius of curvature for rays deviating several degrees from the horizontal, it would follow that the value of k at two considerably different levels would be the same. Now the refraction depends on $\mu-1$, which varies as the density of the air. It is manifest that k is smaller at a considerable height than at sea-level in the proportion of the densities at the two heights. The value of k varies not only with the height, but also with the angle of elevation of the ray. The most convenient plan so far evolved is to speak of the "coefficient of *horizontal* refraction," k_0, and to give values for this quantity at various heights. Under certain average conditions for a ray from A to B, points the heights of which are h_a and h_b, the refraction may be computed by using the coefficient of horizontal refraction appropriate to height $1/3(2h_a+h_b)$, while for the reverse ray $1/3(h_a+2h_b)$ should be used. The values of k_0, which follow from purely theoretical considerations if a temperature gradient of 3° F. per 1000 ft. be assumed, vary from 0·08 at sea-level to 0·05 at 19,000 ft. for temperatures and pressures 82°, 30 in., and 25°, 15 in., respectively. These values are found to account very well for refraction in numerous Indian observations.

Refraction is not, in general, constant throughout the twenty-four hours. It is usually smallest in the afternoon at about 3 p.m., and the minimum value then reached is approximately the same from day to day. On this account observations are often restricted to the hours between 2 and 4 p.m. It may easily happen that the refraction at 8 a.m. is double that at 2 p.m. The values of k given above refer to minimum refraction. Recent research has shown that the diurnal change is due mainly to the changes of temperature in the first 300 ft. of the atmosphere; in that region the form of the ray of light is by no means circular. Beyond a height of 300 ft. temperature changes in the air due to conduction practically disappear. For rays of light which remain most or all of their length within a distance of 300 ft. from the ground, highly anomalous values of k may, and generally do, exist. In such cases afternoon refraction is smaller than is indicated by values of k already given, and in some cases is zero, or even negative. Such rays require special consideration.

Results of a good many observations will be found in my "Formulæ for Atmospheric Refraction and their Application to Terrestrial Refraction and Geodesy" (Professional Paper 14, Survey of India, Dehra Dun, 1913); and a more recent article in "The Dictionary of Applied Physics" (Macmillan and Co.), now under publication, may also be consulted.

J. DE GRAAFF HUNTER.

Dehra Dun, United Provinces, India, July 13.

THE only points in Dr. Graaff Hunter's letter to which I need refer are (1) the objection raised against taking the refractive-index gradient for the lower levels of the atmosphere as being identical with that which would make $\mu=1$ at the height of the homogeneous atmosphere, and (2) the statement that "conduction" of heat extends to a height of 300 ft. above the ground.

With regard to (1), the pressure gradient near the ground, and the density and refractive-index gradients also, decrease linearly at such a rate that if the linear relation continues to hold, the pressure and density would be zero and the refractive-index unity at the height H, and this is the gradient which should be used in correction for refraction to such heights, as the linear relation is a sufficient representation of the facts. How far depends on the order of accuracy aimed at.

Temperature effects may make a difference of 1 or 2 per cent. per 1000 ft., but in such an uncertain correction as that for terrestrial refraction this is scarcely worth notice.

The presence of water-vapour will have an effect as well as variation of temperature, and it will generally be impossible at any particular time and place to know for certain what the refraction really amounts to, especially if the course of the ray is long.

(2) It is scarcely correct to speak of the irregular distribution of temperature near the ground as being due to conduction. True conduction in the air is quite insensible compared with diffusion by eddies and the general instability of flow.

A. MALLOCK.

The X-ray Structure of Potassium Cyanide.

WRITING in the current number of the Proceedings of the Royal Society, Prof. A. O. Rankine concludes from determinations of the viscosity of cyanogen gas that the cyanogen molecule "behaves in collision like a hard body formed by two overlapping hard spheres, each of which has the kinetic properties of a nitrogen molecule." He gives as the distance between the centres of these overlapping spheres $2{\cdot}3\times10^{-8}$ cm. Prof. Rankine also remarks: "It is significant that the crystals of potassium cyanide and those of the potassium halides are usually stated to be isomorphous, and that, in addition, we find that KBr and KCN have nearly identical molecular volumes—43·1 and 42·8 respectively. Thus if CN replaces Br there is no appreciable change in volume, and we may conclude, tentatively, that the cyanogen radicle and the bromine atom have the same size."

Acting on the suggestion of Prof. W. L. Bragg, the writer has made X-ray examinations of small single crystals of KCN by the ordinary spectrometer method, and of powdered crystals by the modification of the method recently described by Sir W. H. Bragg before the Physical Society of London. The results of this preliminary investigation indicate that the underlying structure of KCN is similar to that of KBr, the cyanogen radicle replacing the bromine atom. For instance, the strongest reflection is that given by the [100] face at a glancing angle of 6° 15′. This corresponds to a distance of 3·27 Å. between the planes, and the calculated mass associated with the unit cube the edge of which is of this length is one-half of the mass of the KCN molecule; this is a characteristic of the face-centred lattice. The first-, second-, and third-order reflections from the [100] face have intensities which decrease in the normal way, although at a greater rate than is usually the case; the first-order reflection given by the [111] face at a glancing angle of 5° 40′ is relatively small, while the second-order reflection at 11° 30′ is normal, as is also the first-order reflection from the [110] face. These spectra correspond to those given by NaCl, where the unit of the structure consists of a cube with atoms of one kind arranged at the corners and face centres, and atoms of the other kind at the mid-points of the edges and at the cube centre.

The data obtained, while being sufficient to fix the position of the CN radicle as a whole with respect to the potassium atom, afford practically no evidence as to the disposition of the carbon and nitrogen atoms towards each other. So far as the lower orders of spectra are concerned, the CN radicle behaves as a single unit, whose power of diffracting X-rays differs from that of the potassium atom. The edge of the unit cube in the KCN crystal is 6·54 Å. Taking 4·15 Å. as the diameter of the potassium atom (Prof. W. L. Bragg, *Phil. Mag.*, August, 1920), this leaves 2·39 Å. as the width of the space, measured along the cube edge, to be filled by the CN radicle. The diameter of the bromine atom is 2·38 Å.

The results of the investigation will be published in detail elsewhere. P. A. COOPER.

Manchester University, July 28.

An Ornithological Problem.

STAYING this last week-end with a friend at Overstrand, I was much puzzled on the morning of August 6 by a strange bird which I first saw sitting on some low iron gates at the end of the lawn, when I took it for some kind of hawk. It then settled for a time on a croquet-hoop, and ultimately flew away, when its long wings and tail and smooth flight again suggested a hawk. My host, who had seen it before, thought it might be a cuckoo, and this, when a little later we saw it again in flight, seemed a probable solution.

We did not see the bird again until the evening of August 7, when during a heavy shower it appeared on the lawn and perched on a croquet-hoop close to the house. I then saw that its plumage was not grey-blue, like the adult cuckoo, but a rich mottled brown, and I began to think that it might be a nightjar, though its beak seemed a little too long and its appearance in a beautifully trim garden on the edge of the sea, in the daytime, out of character. Moreover, both on gate and croquet-hoop it sat crosswise, not lengthwise as the nightjar does on a branch. It also occasionally hopped, somewhat clumsily, across the lawn and regaled itself with a worm like any thrush.

On my return to town it was suggested to me that the bird might be a young cuckoo. It so happened that I had never seen one, and so was not aware how different the plumage is from that of the adult bird. After consulting the authorities, however, such as Dresser and Lilford, I am satisfied that this is the right solution, for the mottled brown plumage is quite in order, and the beak and the length of wings and tail are clearly more those of a cuckoo than of a nightjar. Moreover, we are expressly told that the cuckoo when on the ground hops in an ungainly fashion, whereas it is doubtful whether a nightjar with its peculiarly constructed feet could hop at all. The cuckoo, like the nightjar, is normally insectivorous, but this bird might have been brought up by a thrush and imitated its foster-parent's method of dealing with worms on a lawn. Presumably the young cuckoo is not ready for its long flight across sea so soon as the adult bird, of whom we read, "In August, go he must." GEORGE A. MACMILLAN.

August 9.

Uniform Motion in the Æther.

IT seems to be fairly generally conceded that uniform motion relative to the æther is, in principle, undetectable by optical devices. Poincaré, for instance, who did not entirely accept the positions of relativity, stated as his opinion that "optical phenomena only depend on the relative motions of the bodies concerned, and this not to quantities of the order of the square or cube of the aberration, *but rigorously.*"

A very simple consideration, however, shows that such a view is untenable. Thus, if we have a vertical mirror, with a horizontal motion in its own plane relative to the earth, and if a horizontal beam strikes it, the angles of incidence and reflection must, as measured from the moving mirror, be equal, for otherwise the measured discrepancy would determine the earth's motion.

Owing to the aberration, however, these apparently equal angles are not, in general, truly equal, nor are they equal as measured from the earth. It is only when the direction of the earth's motion is in the direction of the horizontal axis of the mirror that they will be equal when so measured.

This determines the direction of the earth's motion, and from the discrepancy in the other cases the magnitude of the velocity could be found.

An effect of the FitzGerald-Lorentz contraction would be to distort angles, so that, for example, a measured right angle, the bisector of which was in the direction of the earth's motion, would be greater than a true right angle; but this would not be compensatory in the case of the mirrors, and would itself, in another connection, serve to determine the earth's motion.

In fact, angular measurements of the stars would suffer discrepancies of a maximum of about 0·001″, in opposite directions, at intervals of three months, owing to the earth's motion in its orbit, and any added motion would probably be detected if an accuracy of 0·001″ in the measurement of large angular distances could be obtained.

As another example of a different kind, the simple immersion in still or moving water of the Michelson-Morley apparatus ought, theoretically, to give a positive result, since the water moves relatively to the æther, and Fizeau's law indicates that the velocity of light in moving water is not the same in all directions; while if the water moves relatively to the apparatus, this velocity is independent of the particular contractions of the latter. E. H. SYNGE.

Dublin.

As Mr. Synge says, the angles of reflection and incidence as measured by an observer moving with the mirror must be equal. When the motion of the mirror relative to the earth is in its own plane, the effect of the FitzGerald contraction is the same on each angle, since it affects all distances parallel to the motion in the same ratio, while leaving those perpendicular to the motion unaltered. Thus the angles will appear equal to an observer fixed with regard to the earth. They would also appear equal if the motion was normal to the plane of the mirror, but not if it were in any other direction than these two. Even in the last case, however, the difference would depend, not on the motion of the earth, but on the motion of the mirror relative to the earth.

Again, it is true that the apparent distance between two stars must vary during the year on account of the variations in the direction of the earth's motion relative to the stars; if all larger disturbances were eliminated this could be detected, but observation of it could only determine the variations in the velocity of the earth relative to the stars, not its motion in æther or "space." The same applies to the immersion in water of the Michelson and Morley apparatus; none of these methods could tell us anything we do not already know more accurately by other means.

HAROLD JEFFREYS.

Conical Refraction in Biaxial Crystals.

AN arrangement for demonstrating conical refraction usually found in laboratories is a piece of aragonite crystal mounted inside a little tube which has one end covered with a metal foil pierced by a number of pin-holes, and an eye-lens in a focussing mount at the other end. When the tube is directed against a luminous object and the eye-lens focussed on the pin-holes through the crystal suitably oriented they are seen as luminous rings of light. Writers on physical optics who describe this experiment refer to it as illustrating *internal* conical refraction—that is, as due to the fact that the Fresnel wave-surface has a tangent-plane which touches it along a circle. I wish to point out that this is really an error. A little consideration will show that as the eye-lens is focussed on the pin-holes, which may be as small as we please, we are concerned here with the waves *diverging* from them in all directions within the crystal, and the observed effect is due to the fact that the two sheets of the wave-front intersect at a conical point. In other words, the experiment really illustrates *external* conical refraction. This is confirmed by the fact that an extended source of light may be used without interfering with the success of the experiment.

A remarkable effect is observed if, with the tube pointed towards an open window, the eye-piece is steadily drawn back from the crystal. It will be noticed that a well-defined image of each pin-hole may be traced behind the crystal for a distance of several centimetres. The formation of this continuous image by a crystalline plate with parallel faces cannot be explained on geometrical principles, and is of great interest. The effect appears to be due to the dimpled form of the wave-front within the crystal, and is being further investigated by Mr. V. S. Tamma and myself.

C. V. RAMAN.

22 Oxford Road, Putney, S.W.15, August 4.

Coiling of Underground Shoots of Convolvulus arvensis.

THE shoots ascending from the rootstock of *Convolvulus arvensis*, before they reach the surface of the ground, are frequently found to be coiled. The coils vary in diameter from one to two inches or more, and lie closely adpressed upon one another. A considerable length of shoot, in some cases three or four feet, is thus compressed into a small space. No object has been found enclosed by the coils which would serve as a stimulus; the soil contains very few stones to obstruct the straight upward growth of the shoots. In a few cases a similar coiling has been observed in the ascending shoots of *Carduus arvensis*. One of the "popular" names of *Convolvulus arvensis* is "Devil's Corkscrew." These white corkscrew coils of the shoots underground seem more likely to be the origin

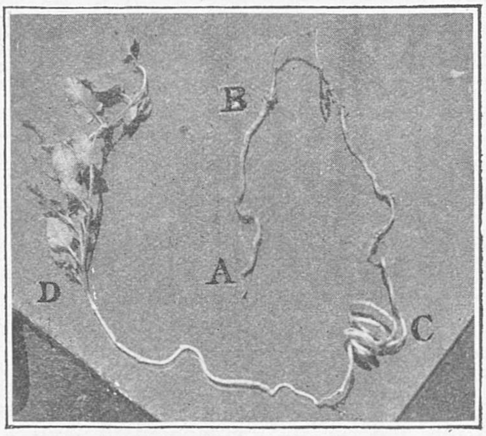

FIG. 1.—Devil's Corkscrew (*Convolvulus arvensis*).

of the name than the less noticeable above-ground coiling portion.

In the accompanying illustration (Fig. 1) A–B is the rootstock; the ascending shoot, originating at B, is coiled at C, and terminates in the leafy above-ground portion at D. (The coils were slightly pulled out before taking the photograph.)

J. E. H. BLAKE.

Bees and Scarlet-Runner Beans.

I SHOULD like to add to the remarks on bee visitors to the flower of the scarlet-runner bean contained in my letter in NATURE of July 28, p. 684, the following further observations. Some ten days from the time of making the original notes a complete change was found in the insect visitors to the flowers and in their behaviour towards them. Instead of the smaller black and black with grey humble-bees busy over the blooms in what I termed the legitimate way, there were numbers of a larger, yellow-banded species of humble-bee that had bitten every newly developed bloom and were searching the nectaries through the perforations made in the base of the flower. They all unhesitatingly scrambled to the underneath part of the blooms, which in every instance had been bitten before the observation was made. Many honey-bees were following in their wake, busily draining the exposed nectaries of every particle of the sweet liquid that had been left or had newly formed.

The results of the two ways of visiting the flowers are very marked and distinguishable. The earlier flowers and lowest on the racemes that were first visited without injury are replaced with a good show of pods, while the later bitten blooms drop off very quickly, with only barren pedicels remaining.

The season being so unusually forward gave opportunity to the earlier insect workers, which made some return to the plant for its sweet gifts, while the later humble-bees are mere depredators that only rob and injure the plant.

HARFORD J. LOWE.

The Museum, Torquay, August 3.

Remarks on Simple Relativity and the Relative Velocity of Light.[1]

By SIR OLIVER LODGE, F.R.S.

II.

The Relative Velocity of Light.

CONSIDER once more the assumption that is either tacitly or confessedly introduced into the establishment of the Larmor-Lorentz transformation and the consequent composition of velocities.

It is this: that the velocity of light outside and far away from matter is absolute; in the sense that it will be measured as the same velocity by every observer, no matter what his relation may be to space and time—*i.e.* no matter where or when he exists, or at what unknown speed he may be moving, not even if his speed were infinite.

A very extraordinary idea that, and one difficult to believe. It is true that it follows from the equations previously written down by Larmor and Lorentz, but they were originally limited to the small range of u/c that covered all practicable observations, and so were not meant to be of universal application and pressed into infallible consequences. The merit, or demerit, of Einstein is that he had no such compunction, and was ready to follow the argument whithersoever it led; and the result—made possible by his wonderful grasp of recondite machinery which he annexed from pure mathematicians, especially recondite when gravitation was included—was a far-reaching effort towards a universal synthesis, in the course of which a few definite features amenable to observation emerged—with the known brilliant results.

Now that the velocity of light in free æther is constant is admitted by everybody, the only reasonable alternative would be some dependence on wave-length, which would mean that the æther was coarse-grained; and that is experimentally negatived by several phenomena and by all manner of determinations of what used to be called a ratio of units, "v," but is more intelligibly and satisfactorily called a measure of the product of the magnetic and electric ætherial constants μK.

But that the relative velocity of light, determined by an observer travelling with speed u to meet it, should still appear the same, and be independent of his motion, is curious, not to say paradoxical. The relative velocity of the observer and the light must be $c+u$—common-sense forbids otherwise,—but if he seeks to measure it he will get, we are told and inclined to believe, not $c+u$, but $(c+u)\div(1+cu/c^2)$, and that is simply c.

So far as I know, no one has ever measured the apparent velocity of light from a star or from one of those spiral nebulæ from which the earth is receding at hundreds, or even thousands, of miles per second. It is not easy to see how it can be done, for the readily observed Doppler effect is always attributed to relative motion of source and observer; and if those are relatively fixed it has been definitely shown that no steady motion of the medium has any observable influence on either direction or frequency (Phil. Trans., 1893, vol. clxxxiv., p. 784). Gusts, however, cause wailing; and by utilisation of the variation of an already occurring Doppler effect something may be done (*l.c.*, p. 785). But, in view of the universality of the above transformation equations, we may admit that it is unlikely that any result other than c will be obtained. It is by assuming the velocity of light constant that the recession velocity is measured; the whole observed retardation is naturally attributed to relative velocity of source and observer; though if we could be sure that all the observed relative velocity really belonged to the receiver, and none of it to the source, we should know that the reason we were able to observe an apparent change in frequency was because of the resultant speed at which we received the waves. But that is just the difficulty—we cannot tell how much of the recession belongs to the source and how much to the receiver. If we could know the observer's speed through the æther we could clearly say that he met the waves more slowly or more quickly than he would otherwise get them; and this reasonable statement has never been disproved by observation.

We ought not to claim, therefore, as some philosophers do, that the fundamental hypothesis of Einstein about observed velocity of light has been directly verified and is a sound basis on which to found a theory. The hypothesis does not justify any theory, though a successful theory may justify the hypothesis. A mistaken claim for what has been done by experiment is often made; and as clear statements are always valuable, whether right or wrong, I select for quotation one from Lord Haldane's recent book, "The Reign of Relativity," on p. 82:—

> Long before 1905 it had been found by experiment that the velocity of light appeared to be always 186,330 miles per second, whether the passage of its rays was towards us while we were at rest with regard to its source, or whether we were ourselves moving towards that source.

Now whether what is here asserted to have been "found by experiment" about the velocity of light be a fact or not, no observation of a discriminating kind had been made before 1905; and I would myself deny that any such observation has been made since. Certainly no experiment of the direct kind suggested in this quotation has ever been made—it is doubtful if it can be made. Every purely terrestrial measurement of the velocity of light has been made, and must necessarily be made, on light which has travelled round a contour, or, what is the same thing, which has gone and returned over the same path. Such an experiment proves nothing, either for

[1] Continued from p. 719.

or against a discrepancy due to the observer's motion, in the measured 186,330 miles a second. In a to-and-fro journey there is complete compensation for any possible discrepancy in speed so far as small quantities of the first order, involving the ratio u/c, are concerned. The only outstanding discrepancy to be expected is of the second order of minutiæ; and that, as many of us think, is systematically neutralised by the FitzGerald-Lorentz contraction, which, though it is a consequence of the electrical theory of matter, is stigmatised as an unreal contrivance, a mere invented refuge, by the philosophers above referred to.

Relativity has only to do with second-order effects; it essentially depends on ds^2, the square of a small interval; but the statement above quoted is not entirely about second-order effects; it relates to the first order—to a journey in one direction—and would require for direct verification an observation of the difference in the time of a single journey, when the observer is moving (*a*) with, (*b*) against, an æther stream.

The nearest approach to a measurement of this kind that might conceivably be made would be a vastly improved determination of the velocity of light by a method based on the observation of some periodic feature in Jupiter's satellites during the course of Jupiter's year. To make a determination possible at all, the earth must be moving either to or from Jupiter at the time—it does not matter which—and the chance of obtaining a positive result depends on the varying angle which the line joining earth and Jupiter makes with the sun's way, or rather with the direction of locomotion of the solar system through the æther, whatever that direction may be.

But I think it is generally agreed—subject, however, to the opinion of the chief authority on those motions, Prof. R. A. Sampson—that the gravitational theory of the satellites, perturbed as they are by each other as well as by the oblateness of Jupiter, is not yet nearly perfect enough to enable us to decide the question whether the velocity of light deduced from their eclipses is dependent on the season of Jupiter's year,—in other words, whether light appears to reach us with the same speed when we are looking at Jupiter down-stream as it does when we are looking at him up-stream (see Phil. Trans., 1893, vol. clxxxiv., pp. 746, 779, and 785). For we have no means of determining the instant at which the light starts from Jupiter; all that we can really observe is the time that light takes to transit the distance travelled by the earth in the interval between two eclipses.

Apart from all astronomical observation, however, it has been claimed that the rather recent pair of experiments of Prof. Majorana, with moving mirrors (*Phil. Mag.*, February, 1918, p. 163, and January, 1919, p. 145), do establish the thesis that the observed velocity of light is independent of the relative motion of observer; but they, too, are observations made on a to-and-fro journey, and, therefore, for the present purpose, are beside the mark. If light were a projectile, it could be hit forward by a moving mirror, like a cricket ball, but no one can suppose that any kind of impact can alter the subsequent velocity of waves through a medium, nor is it to be supposed that motion of a source can affect the travelling rate of waves which it has emitted and abandoned.

More Careful Discussion of Doppler Effect.

Motion of the source does not affect velocity, but if a moving source emits waves at constant frequency n, the wave-length ought to be different in different directions θ, and this modified wave-length,

$$\lambda' = \frac{c\cos\epsilon + u\cos\theta}{n},$$

can be observed by a fixed observer, and, when compared with the normal $\lambda = c/n$, is called the Doppler effect. The small aberration angle ϵ, between ray and normal to wave-front, is defined by $c\sin\epsilon - u\sin\theta = 0$.

If, however, the source is fixed in the æther, and only an observer is moving, the velocity and the wave-length are both quite normal; but the frequency with which the waves are encountered by the observer will depend on the speed and direction of his own motion. Consequently there is again an observable Doppler effect expressed as

$$n' = \frac{c\cos\epsilon + u\cos\theta}{\lambda},$$

to be compared with $n = c/\lambda$.

Hence if an observer steadily chases a source, keeping a fixed distance between them, the two effects—the real wave-elongation and the apparent frequency increase—neutralise each other *exactly* whatever the direction of joint motion, because $n\lambda' = n'\lambda$. So drift through a medium produces no trace of a Doppler effect.

Nevertheless, the two types of effect—one with source only moving, the other with receiver only moving—are not identical; they are the same when both are moving in the same direction, so as to be relatively at rest, but not the same when they are moving relatively to each other. For, writing u/c as α, and taking the case of relative recession between source and receiver, we get, for the observed frequency ratio,—
if it be the source only which is moving,

$$\frac{n'}{n} = \frac{\lambda}{\lambda'} = \frac{1}{\cos\epsilon + \alpha\cos\theta};$$

while if it be the receiver only which is moving,

$$\frac{n''}{n} = \cos\epsilon - \alpha\cos\theta.$$

Hence

n'' is not equal to n', but

$$\frac{n''}{n'} = \cos^2\epsilon - \alpha^2\cos^2\theta$$
$$= 1 - \alpha^2\sin^2\theta - \alpha^2\cos^2\theta$$
$$= 1 - \alpha^2,$$

which is the square of the usual FitzGerald contraction. The relativity doctrine, in order to avoid recognising any such difference, would presumably distribute this factor between the two expressions, making them

$$\frac{\lambda'}{\lambda}=\frac{\cos\epsilon+a\cos\theta}{\sqrt{(1-a^2)}},$$

$$\frac{n''}{n}=\frac{\cos\epsilon-a\cos\theta}{\sqrt{(1-a^2)}};$$

so that there shall be equality between $n''\lambda'$ and $n\lambda$, and then it is impossible to tell to which body the motion belongs.

Note that the introduced β factor cannot in this case be attributed to a FitzGerald contraction of the grating—if a grating is used as the measuring instrument,—for the aspect of the grating to the incident light, and therefore to the motion under examination, is normal, not tangential. But the law of reflection is interfered with by the motion, in a way investigated in Phil. Trans. for 1893 (vol. clxxxiv., A, pp. 793–800), and the result is to give a modified deviation which will be interpreted as part of the Doppler effect. The discrepancy is reckoned, on p. 798 *loc. cit.*, for any incidence angle i and any drift angle ϕ, as $a^2\cos^2 i\sin 2(i-\phi)$; so it is a maximum for normal incidence and for a drift direction making 45° with the ray.

This might readily have the average value $\frac{1}{2}a^2$ needed to replace the ordinary β factor, but in so far as it yields a factor depending on the angle ϕ its changes *seem* amenable to observation.

In the same Phil. Trans. paper I show (p. 787) that the Doppler effect observed by a moving grating is really an aberration effect, due to the motion being partly across the diffracted rays, although the incident ray may be along the drift. For a grating *must* deviate in accordance with wave-length, whether it be moving or stationary, so far as first order is concerned.

But the question arises, What happens when the grating is drifting partly in its own plane ($\phi=90°$) and thereby suffering a FitzGerald contraction?

The answer seems to be that the extra aberration due to this drift will just compensate the second-order Doppler effect otherwise to be expected from the ostensibly narrower-ruled grating. There are certain possibilities here, however, which need looking into.

Summary of this Portion.

The Einstein formulation seems to justify itself by results, and may be supposed to strengthen the claims of any philosophy suggested by it, as well as to establish the explicit assumptions on which the theory is based; but we should be careful to perceive that justification is of this subsequent inferential order, and that it is not primarily the outcome of experiment—certainly not of any old unexplained measurements. The whole thing depends on the law that we postulate for the composition of velocities. When two velocities in the same direction are compounded, is the resultant velocity $u+v$, or is it really $\frac{u+v}{1+\frac{uv}{c^2}}$? Einstein's assumption led to the latter as a physical truth, and if that is right it is algebraically undeniable that if one of the component velocities is c, the resultant will be c also; and any such criterion as my old experiment (1892–97) with rotating discs, whereby it was sought to observe a possible difference between $c+v$ and $c-v$, cannot give any positive result. Nor can it, by giving the result zero, prove that v is 0, because, as a matter of simple algebra, if $u=c$, no sort of v can make any difference, not even if it be infinite.

So also Michelson's experiment can show nothing, nor can any velocity compounded with the velocity of light exhibit itself in any way, if that is the true law of composition of velocities in general.

But why into this composition formula should there enter the velocity of light? If, for instance, the composition is between a ship and a tide, or a satellite and a planet, or the usual railway train and embankment, one cannot avoid the question, What on earth has the speed of light to do with it? any more than the speed of sound or of a messenger boy, or whatever agent it is which brings information to an observer. It is true that the law of composition is essential to the principle of relativity, but when we are engaged in establishing that principle it is scarcely fair to assume it.

The curious law of composition is deduced from the Lorentz transformation of space and time to other co-ordinates,

$$x'=\beta(x-ut)\ ;\ t'=\beta\left(t-\frac{ux}{c^2}\right)\ ;\ \beta^2(c^2-u^2)=c^2\ ;$$

and in the establishment of these equations it is assumed that all observers have the same value of c, or that $x^2-c^2t^2$ is invariant.

I apprehend that for this transformation, treated as formal correspondence, there is a good deal to be said, so that any law deduced from it may be true with all its consequences; but it is surely a mistake to say that the measured velocity of light has been experimentally proved constant. So far as the velocity of light is concerned, the reasoning is circular. I suggest that it is also dangerous to adopt a mode of exposition which denies reality to the FitzGerald contraction. Still more is it premature to assume, as more than a temporary conclusion, that no phenomenon demonstrating our motion through the æther of space can ever be discovered; which carries with it the implied suggestion that the inability is because such a medium does not exist; so that not only can all motion be treated undynamically as a purely geometrical or kinematic relation, but so also that in absolute truth there is no difference discernible between a dog wagging its tail and the tail wagging the dog. Kinematically, it is as easy to take the apple as standard of reference as it is to take the earth, but physically and energetically the treatment can only be satisfactory when their combined or reciprocal motion is balanced about their common centre of gravity.

Centres of gravity, however, presumably disappear from relativity; and, what is more serious, so does the conservation of energy. For if there is nothing absolute about speed there can be nothing absolute about kinetic energy. The relativity expression for kinetic energy contains an arbitrary constant; and whether energy is conserved or not becomes a matter of convenience and definition. The claim that relativity pressed to extremes does away with all conservation, as hitherto understood in physics, has been seriously made by the eminent mathematician, Prof. Hilbert, of Göttingen. On the other hand, it might be replied, according to Sir Joseph Larmor, that kinetic energy has always been treated as relative to some other body on which work might conceivably be done, and that the really invariant quantity is not energy, but the integral of energy with time, called "action"; or as it may be regarded, perhaps preferably for some purposes, i times angular momentum.[2] For this appears to be independent of frames of reference—which energy certainly is not.

Acceptance of the theory of relativity correlates results, but does not explain them. The theory does not even seek to explain or account for phenomena: they just are so. It is not a dynamical theory, it is a method of arriving at results, like the second law of thermodynamics and the conservation of energy. The full dynamical explanation remains to be worked out, and it may turn out to be on very much the old lines along which we had previously regarded physical phenomena. The true relation between æther and matter, and how their interaction generates and affects light, is an immense subject, not in the least exhausted, and barely encroached upon, by the perception that certain consequences inevitably follow from an admission that the velocity of light is a critical limiting velocity, which cannot be exceeded, and which when compounded with any other velocity retains its old value.

Whether the properties of the æther can ever be formulated in terms of the same sort of dynamics as we have found so fruitful and effective in dealing with matter is at present an open question. Quite possibly a different dynamics may be needed, one perhaps of which we have as yet no conception; but let us not shut the door on discovery, assume that nothing of the sort can ever be arrived at, and think that pure mathematical abstractions, glorified and complicated sufficiently, can be an ultimate embodiment of physical laws or can adequately express the facts of Nature.

(To be continued.)

[2] Which, by the way, is very suggestive of a constitutional gyrostatic æther structure.

The Conference of the International Union against Tuberculosis.

THOSE who have followed the course of tuberculosis in this country have noted that during the years of the war there was a sudden interruption in the fall of the curves illustrating case-rate and death-rate from that disease. Our work was then in fields abroad. Now, however, that we are getting back to pre-war conditions, peoples and nations are again joining forces in a new campaign against tuberculosis in our civil populations, and at the recent conference in London of the International Union against Tuberculosis delegates from forty nations, including China, Japan, Persia, and Czecho-Slovakia, met to discuss the great question of the cure and prevention of tuberculosis. Science knows no national borders, and it is obvious that the union is anxious to work with men from all nations, and to this end has drawn up a series of tentative regulations in order that when German physicians have composed the differences amongst themselves arrangements may be made for their reception into the councils of the union. The secretary of the old International Association against Tuberculosis appears to have assumed that everything would go on as before, and somewhat injudiciously made an attempt to call the old association together as a rival to the conference of the union of Allies and neutrals held in Paris last year. The wiser amongst his countrymen were against this, and at present the German physicians are divided into two camps. For the present the International Union is content to make good its own footing, go its own way, and lay down its own lines of operations, at the same time leaving the regulations so elastic that as asperities are smoothed down and difficulties removed German workers may come in and take their part in its great work; and it is hoped that steps towards this will have been taken when the meeting is held in Brussels next year, or, at any rate, in Washington two years later. By that time the League of Nations may have got under way, and the international character of the union may have become complete.

At the opening sitting of the London meeting the Foreign Secretary, Lord Curzon, and the Minister of Health, Sir Alfred Mond, blessed in no uncertain terms the ideals and work of the union, and their presence no less than their works may be accepted as of good omen that the Government authorities will, in their anti-waste difficulties, remember that a penny wise Health Ministry may be pound foolish where the public health is concerned, and that the same holds good as regards the Board of Education.

Prof. Calmette, in a most interesting opening address characterised by all the clearness of vision and beauty of expression for which this French savant is noted, outlined a new hypothesis as to the importance of tuberculosis "carriers"—*i.e.* centres of infection, themselves healthy to all intents and purposes. He claims (though the claim is not universally admitted) that von Pirquet's method of diagnosis is sufficiently characteristic to allow of a decision as to whether a patient is the subject of bacillary infection, or, in other words, capable of reacting to tuberculin as a result of the presence in the patient of a sensitising substance derived from the tubercle bacillus. Prof. Calmette holds, moreover, that by means of this reaction it is possible to work on a grand scale and to determine whether peoples and tribes, infants and adults, are infected by, or free from, tuberculosis. He quotes Col. Cummins and others to the effect that among African tribes about the equator where civilisation has not yet penetrated, and among the nomadic tribes of Arabs and Berbers, tuberculous infection is non-existent, or very rare, whilst in Natal, among the Zulus in the Transvaal, and in Madagascar, as also in the larger cities of North Africa, it is very prevalent. Those living in huts and native villages are, however, gradually becoming infected by contact with men from without. In the hinterland of the Cameroons from 3 to 6 per cent. only of adults are yet affected, whilst many aboriginal tribes are still quite free.

In civilised countries, although the reported percentage of tuberculous infection is comparatively high, a careful examination by the von Pirquet test and an examination of patients who die from other diseases would, Prof. Calmette claims, indicate the infection by the tubercle bacillus of many who, as yet, show no signs of tuberculous disease, and he believes that in the overcrowded cities of Europe and the United States few escape tuberculous infection, although the chances of death from tuberculosis are little more than one in eight. In the country districts the figures are not so high. Amongst the Kalmucks, even where the inhabitants have little intercourse with towns, 69·4 per cent. of the men and 30·6 per cent. of the women give a positive tuberculin reaction, whilst on the outskirts of the same territory, where commercial relations with the Russian population are very close, 95·7 per cent. of male adults and 88·5 per cent. of women give a positive reaction. Moreover, where differences occur, these are due very largely to the fact that tuberculous infection has been implanted in certain races over a longer or shorter period of time, although infections are also variable, being rare and slight or frequent and massive according to the particular mode of life of the people. Those who have been longest protected by virtue of their isolation from contact with the tuberculous prove to be most susceptible, aboriginal tribes and infants being the virgin soil on which the tubercle bacillus flourishes most luxuriantly. In the races that have been contaminated for centuries and exposed from infancy the disease assumes a chronic, slowly progressing form; but almost all become infected. He finds evidence in support of his contention in the susceptibility of the bovine species to tuberculosis in the domesticated condition, although the wild cattle of Madagascar and of the pampas of the Argentine are said to be free from this disease. (It was found by the Royal Commission on Tuberculosis that Jersey cattle, though free from tuberculosis in the island, were readily infected when brought over to this country.)

Prof. Calmette is of opinion that the spread of human tuberculous infection throughout the world is due entirely to disseminators of virulent bacilli, most frequently through persons suffering from phthisis, who scatter enormous numbers of bacilli in their sputum and intestinal excretions either directly or by means of objects soiled by them, or again through the agency of living carriers, such as flies. These open tuberculous cases are not the only factors in the dissemination of the disease. Many apparently healthy individuals suffering from latent or concealed tuberculous lesions which can be detected only by the tuberculin reaction are a source of danger in that they eliminate bacilli intermittently in their glandular or intestinal excretions, thus spreading infection in their environment.

E. C. Schroeder and W. E. Cotton found that 40 per cent. of cows giving a positive tuberculin reaction and showing no clinically demonstrable lesion discharged bacilli intermittently in their excreta, and that swine fed on these excreta easily became infected.

Similar observations were made by the Royal Commission on Tuberculosis, which, injecting tubercle bacilli into the circulating blood of healthy cattle, demonstrated their early appearance in the milk, whilst Calmette and Guérin showed that some of the bacilli injected into the blood-stream are eliminated through the bile passages. Lydia Rabinowitsch and Kempner, Tirze, with others in Germany, and Sheridan Delépine in England, have made similar observations with regard to the mammary glands of cattle. More recently it has been claimed by several observers that bacilli may often be found in the milk of tuberculous human mothers, even when the disease is in its early stages, or where only lymphatic glandular lesions are present. Prof. Calmette suggests that in the children of these mothers serious forms of tuberculosis are set up by slight but oft-repeated infections through breast-feeding or through prolonged or numerous accidental contacts with intermittent disseminators of bacilli. He goes further, and holds that when tuberculosis appears in environments where it has hitherto been absent it may have been introduced by a bacillus-carrier unrecognised because apparently healthy, which nevertheless has spread virulent germs either in excretions or through glandular secretions—*e.g.* milk in the case of lactating women; also that the disease in these more recently contaminated countries is more serious and more rapidly progressive than in the countries longer infected, and that it then assumes the form met with in young children rather than that met with in adults. He argues from all this that individuals with occult tuberculosis—the so-called healthy carriers of tubercle bacilli—are largely responsible for the spread of tuberculosis, not only amongst aboriginal and hitherto isolated peoples, but also amongst infants. He claims that this recently acquired knowledge of an unexpected danger makes the organisation of social defence against tuberculosis more difficult than when prophylaxis had to be based only on the education and isolation of phthisical patients, though he concedes that these are the principal disseminators of the disease. New peoples and infants should, wherever possible, be protected through a system of detection based both upon the judicious use of tuberculin tests and upon clinical examinations of the glandular system mainly by means of radioscopy.

It was interesting to find that Dr. Eric Pritchard, working along these lines during a period of ten years, had passed through his hands some thirty children who, by a process of injection with Koch's original tuberculin extending over five months, first in minute doses, to which they reacted, gradually increased up to 1 mg., to which the reaction was no more marked, could be immunised against any infection they were likely to be exposed to in the course of their lives. No accident such as might have been anticipated had occurred, and he was very hopeful that they had passed over that susceptible period of which Prof. Calmette had spoken.

It was felt by some who heard Prof. Calmette that his hypothesis, unless more fully explained, might lead to great misconception on the part of the public and be advanced as a reasonable excuse for inaction. If any apparently healthy person may be a "carrier"—and all may be infected in infancy—what good are elaborate precautions against tuberculous infection? The various public authorities might feel justified (and some might wish this) to sit with folded arms and tightly buttoned pockets. Later Prof. Calmette made it clear that his reference was only to those who had not hitherto been brought into contact with tuberculous patients, such as native races and infants, and that in civilised tuberculous communities other factors, surroundings, conditions of life, sources of infection, etc., must all receive due consideration. It was insisted that much information on these points had already been accumulated, and that the time had undoubtedly arrived when the aid of legislation should be called in for the prevention of tuberculosis. Two great sources of infection, human and bovine—expectorations from the former, and milk from the latter—containing massive doses of tubercle bacilli, must still be dealt with, and dealt with effectively. No measure conducing to the removal of mass infection should be neglected. Panic or phthisiophobia may well be discouraged when we learn from Sir George Newman that in seventy-three years, since 1847, when the death-rate from tuberculosis was 3189 persons per million living, there has been a fall of 74 per cent., the standard death-rate from phthisis in this country in 1920 being 842 per million living. In other countries the decline, though not so marked, is still very substantial.

One feature was very prominent throughout the whole of the discussions. Although the search for prophylactic aids should not be discontinued, it must be recognised that the processes involved in tuberculosis are of a type different from those involved in most of the acute infective diseases, such as typhoid, plague, and the like, and, accepting this, we must follow Sir George Newman in his advice that "there is no beaten track in the further conquest of tuberculosis"; "the healthy child and the adult must be protected from massive, frequent, and prolonged infection"; "the powers of resistance of the patient must be fortified." "Freedom of thought, wide and deep research, and mobility of action will be necessary. Of much are we still in doubt, but of three things we may be certain. Only by surveying the complex problem, as a whole, in the spirit of preventive medicine, and co-ordinating the respective factors concerned, only by thorough, constructive, and intensive practice of our principles and by searching and finding the hidden secrets of immunisation, shall we at last conquer this disease."

The Progress of British Forestry.

THE First Annual Report of the Forestry Commissioners (H.M. Stationery Office, 1921, 9*d.* net) deals with the period ended September 30, 1920, since which date a whole planting season has intervened; but a preliminary note gives information of the progress made to date. The Forestry Commissioners are now in actual possession of 103,100 acres of land, of which 68,100 acres are classed as plantable with timber trees. The planting operations of the season 1920–21 have been successful, and the total area of new plantations is now about 8000 acres, while the stock of young trees in the nurseries is sufficient to plant next season a largely increased area.

The Report opens with a sketch of the history of forestry in the United Kingdom, showing the stages which led to the passing of the Forestry Act in 1919. State forestry is a new departure in this country, and this part of the Report will instruct the public in the significance of a national forest policy. In the first period—that of destruction of the original forests, which lasted in some districts up to 1750—great clearances were made for agriculture, sheep pasture, and the smelting of iron-ore. In the next period—that of private enterprise, 1750–1885—landowners attempted by their own efforts to re-establish the depleted woodlands, and they were aided only by voluntary associations like the Society of Arts and the Dublin Society, which encouraged effectively the planting of trees by their prizes and premiums. During the war, when it was a choice between importing food or timber, it was the timber available in privately owned plantations that enabled the people to be fed.

In the third period—that of inquiry, 1885–1915—it was gradually borne in upon the public mind that unaided private enterprise could no longer cope with the growing demand for timber by our ever-increasing industries and that the primeval forests of the world were not inexhaustible. Imported timber increased continuously in price during these thirty years. Select Committees, Departmental Committees, and Royal Commissions on Forestry followed in quick succession and made recommendations which were mostly unheeded. The Development Commissioners appointed in 1909 failed "to purchase and plant land found after inquiry to be suitable"—one of the duties imposed upon them—but it must be admitted that they did useful pioneer work in providing increased educational facilities, in appointing advisory forest officers, and in encouraging with loans certain municipalities to afforest their water-catchment areas. The state of affairs, practically much inquiry and no afforestation, was unsatisfactory in time of peace. One year of war showed how critical the position was in a time of national emergency.

The final stage in our forest history—that of State action, which began in 1915 with the setting up of Lord Selborne's Committee to expedite home fellings of timber—is characterised by the adoption of a definite national forest policy by the Government, which was approved by Parliament when the Forestry Act was passed in 1919. This policy has two aims. Its ultimate objective is the creation in the British Isles of reserves of standing timber sufficient to tide the nation over three years in time of war. For this purpose the State must afforest 1,770,000 acres of new land—1,180,000 acres in forty years, and the whole in eighty years—and at the same time secure the continuance under timber (with an increased production) of the 3,000,000 acres of private forests which existed in 1914. The immediate objective is a ten-year scheme, based on a block grant of 3,500,000*l*. In this decade the Forestry Commission will afforest 150,000 acres of new land owned or leased by the State. The Commission is also bound to aid private owners and local authorities in planting 110,000 acres during the ten years.

The Report shows that there is no difficulty in the State acquiring and planting the acreage mentioned in the preceding programme. It is another story with regard to private forestry, for aid to which the Commissioners set aside 327,000*l*., of which 137,000*l*. has been allotted to proceeds-sharing schemes between private individuals or corporate bodies and the State, and 190,000*l*. to grants and loans. However, the proceeds-sharing schemes, being hedged round with cumbersome rules to safeguard the public purse for the period of a rotation (fifty to one hundred years), are unpopular with landowners. Similarly, the statutory regulations, under which 2*l*. grants per acre are made for planting, prove to be so onerous as to offer no inducements to private individuals. The Commission must obtain powers to amend these regulations, which defeat the object of assisting landowners to make plantations.

The Report gives a detailed account of the operations carried out during the year, illustrated with a map showing the land acquired and the present planting centres. Education, research and experiments, and publications are dealt with briefly. Tables of imports of timber, statutory orders and rules, and other official documents conclude a Report which deserves to be studied by all interested in the progress of forestry in this country.

Notes.

THE classical experimental plots which Lawes and Gilbert started at Rothamsted have been of the greatest service to agricultural science, and their importance is constantly increasing. Fundamental questions in the physics, chemistry, and biology of agriculture can be attacked with more confidence in the light of results obtained from long-continued field experiments carried out on a systematic plan. Further, the results are capable of statistical examination. The importance of the Rothamsted experiments led to the institution of a parallel series at Woburn in 1876 by the Royal Agricultural Society. The Woburn soil is light and sandy, but that at Rothamsted is a heavy loam. The two series of experiments enable instructive comparisons to be made between these two soil types. All interested in agricultural science received with concern the decision of the council of the Royal Agricultural Society to relinquish—owing to economic conditions—the Woburn experiments. Fortunately the danger has been averted. Arrangements have been made for the experiments to be continued under the auspices of, but legally distinct from, the Rothamsted Experimental Station. The general portion of the Woburn farm will continue under the direct control of Dr. A. J. Voelcker, who for many years has carried out the duties on behalf of the Royal Agricultural Society. The new arrangement will not only ensure the continuance of the valuable work already done, but will also lead to a closer contact with the work of Rothamsted.

AT our request, Prof. C. Runge, of Göttingen, has been good enough to send us the following list of leading men of science in Germany who have died since the beginning of the late war. The list is not, however, complete, and may be supplemented later. Short obituary notices of some of the men will be found in the *Geschäftliche Mitteilungen der Göttinger Gesellschaft der Wissenschaften,* 1918–19–20 (Weidmannsche Buchhandlung, Berlin S.W.68, Zimmerstr. 94):—W. Lexis, mathematician and statistician, August, 1914; W. Hittorf, physicist, November, 1914; A. von Auwers, astronomer, January, 1915; A. von Könen, geologist, May, 1915; E. Riecke, physicist, June, 1915; P. Ehrlich, physician, August, 1915; H. Solms-Laubach, botanist, November, 1915; R. Dedekind, mathematician, February, 1916; E. Mach, philosopher and physicist, February, 1916; K. Schwarzschild, astronomer, May, 1916; R. Helmert, mathematician and physicist, June, 1917; A. von Baeyer, chemist, August, 1917; G. Frobenius, mathematician, August, 1917; A. von Froriep, anatomist, October, 1917; H. Vöchting, botanist, November, 1917; C. Rabl, anatomist, December, 1917; G. Cantor, mathematician, January, 1918; L. Edinger, physician, January, 1918; E. Hering, physiologist, January, 1918; F. Merkel, anatomist, May, 1919; S. Schwendener, botanist, June, 1919; E. Fischer, chemist, July, 1919; H. Bruns, astronomer, 1919; Th. Reye, mathematician, July, 1919; W. Voigt, physicist, December, 1919; P. Stäckel, mathematician, December, 1919; W. Pfeffer, botanist, January, 1920; O. Bütschli, zoologist, February, 1920; and W. Förster, astronomer, 1920. J. Elster, physicist, and Joh. Thomae, mathematician, have died recently. In addition to the above, several other German men of science were referred to in the obituary notice of Prof. von Waldeyer in NATURE of May 19, and news has also reached us of the following deaths not previously recorded in these columns:—Prof. G. A. Schwalbe, Strassburg, on April 23, 1916, age seventy-one years; and Prof. Karl von Bardeleben, editor of the *Anatomischer Anzeiger,* on December 19, 1918, age sixty-nine years.

THE tendency towards a more popular form of official publications has been evident in recent annual reports of H.M. Chief Inspector of Factories. The report for 1920 is divided into twelve chapters dealing with such matters as safety, dangerous trades, welfare, lighting, etc., prefaced by an introductory general section. The work of the Departmental Committee on Lighting in Factories and Workshops has now been resumed, and the Committee is assisting in the preparation of a pamphlet summarising the chief essentials of industrial illumination. We observe that the scope of the Committee has been somewhat restricted by the prevalent demand for economy. We could wish that the demand was applied with less severity to research of this description, in a field where much remains to be learned and results of experiment may have great economic value. It is, however, gratifying to observe that the recognition of the value of good lighting is increasing. One of the strangest facts mentioned in this report is the habitual disregard, by some firms, of natural illumination. Window-space is not infrequently cramped; existing panes are found to be broken and covered with sacking, or obscured by paint, oil, or dirt. Seeing that daylight costs nothing, and, according to recent experiments in silk factories, leads to 10 per cent. better production than average artificial lighting, this is evidently a direction in which a demand for economy might be justly pressed and expenditure on publicity well repaid. Another point commented upon in the report is the need for protection of the eyes against the "flash" of arc-welding. Apparently exposure of a few seconds may have ill effects, though fortunately cases of permanent injury seem to be rare. The cataract prevalent among glass workers is now believed to be due, not to ultra-violet rays, but to the continual exposure to intense heat. Suitable Crookes glasses would afford protection, but it is difficult to induce workers to make use of them. Here, as elsewhere, educational work, such as that conducted by the British Industrial "Safety First" Association, is clearly needed.

WE regret to see the announcement of the death, at seventy-nine years of age, of Prof. G. T. Ladd, Clark professor of metaphysics and moral philosophy in Yale University, founder of the American Psychological Association, and author of many important works on philosophy and psychology.

DR. JAMES MARCHANT, director of the National Council for the Promotion of Race-Regeneration, has been appointed a Knight Commander of the Order of the British Empire.

THE council of the Society of Chemical Industry has decided to institute a Messel memorial lecture in memory of Dr. Rudolph Messel. A gold medal with an honorarium will be presented to the lecturer, and for the present the remainder of the income from the bequest to the society will be allowed to accumulate.

WE learn from the *British Medical Journal* of August 6 that the French Academy of Medicine has elected the following foreign correspondants:—Sir Robert Philip (Edinburgh), Sir Humphry Rolleston and Sir D'Arcy Power (London), Dr. Brachet (Brussels), Prof. Christiansen (Copenhagen), Prof. L. J. Henderson (Harvard University), Dr. Lucatello (Padua), Dr. Dominguez de Oliveira (Oporto), Dr. de Quervain (Berne), and Dr. Soubbotitch (Belgrade).

DR. J. CHARCOT, the French polar explorer, sailing in the North Atlantic in his exploring vessel, the *Pourquoi Pas,* has succeeded in landing upon the islet of Rockall, which lies some 260 miles west of the Hebrides and 185 miles from St. Kilda. Rockall is a pinnacle about 75 ft. high rising from a shallow bank which has more than once proved disastrous to shipping. It has seldom been visited, and the *Times* records only five authentic instances of landing previous to Dr. Charcot. The interest of Dr. Charcot's feat lies in the geological specimens which he is reported to have obtained from the rock.

IT is announced by the *Times* that Mr. Edwin Naulty, an American aviator, intends to attempt an aeroplane flight across the North Pole next month. He proposes to start from Point Barrow, in Alaska, and hopes to reach the north-western corner of Spitsbergen. The aeroplane will carry four men and fuel for a fifty hours' flight. If conditions permit, several landings will be made on the polar ice, but if this proves impossible the 1800-mile flight will be made without descent. From Spitsbergen Mr. Naulty proposes to continue his flight *via* Norway to London. Provided clear weather is experienced, it will be possible to make valuable observations on the distribution of ice and air-currents. The flight may throw some light on the doubtful existence of land in the eastern part of the Beaufort Sea.

A TRADING expedition to Siberia *via* the Kara Sea is on the point of leaving Europe. Two cargo-boats from Liverpool, two from Hamburg, and one from Göteborg are to meet at the Russian port of Murmansk, where they will be joined by the ice-breaker *Alexandria* from Leith. The expedition is carrying about 11,000 tons of cargo, most of which is destined to enter Siberia *via* the Yenisei River. This route to Siberia has been used from a very early date, but for a long time fell into disrepute owing to the difficulties presented by ice in the Kara Sea. These difficulties, however, have been exaggerated, and for some years past one or more vessels have made the passage every summer in August or September. The expedition is being organised by the All-Russian Co-operative Society, Ltd., London.

A PROGRAMME has been issued of the autumn meeting of the Institute of Metals to be held at Birmingham under the presidency of Eng. Vice-Admiral Sir George Goodwin on September 21–23. There will be a general meeting on the morning of September 21 in the hall of the Municipal Technical School, at which the Lord Mayor of Birmingham will deliver an address of welcome. The remainder of the morning and the whole of the morning session of September 22 will be devoted to papers dealing with the constitution and properties of various metals and alloys, and, so far as time permits, each paper will be followed by a brief discussion. In addition to the formal meetings, there will be excursions to various works in or near Birmingham, and on the afternoon of Wednesday, September 21, a visit will be paid to the University of Birmingham. The guests will be received by the Vice-Chancellor, Sir Gilbert Barling, Bart., and an address delivered by the Principal, Mr. C. Grant Robertson. Full details of hotel accommodation, railway arrangements, etc., are given in the programme, which can be obtained from the Secretary, the Institute of Metals, 36 Victoria Street, S.W.1.

AFTER seven years' cessation (the result of the war) the excavations at the Meare Lake Village, near Glastonbury (Shapwick and Meare are the nearest railway stations), will be resumed by the Somersetshire Archæological and Natural History Society on August 29, and continued for three weeks (exclusive of the filling-in). The work will be under the direction of Dr. Arthur Bulleid and Mr. H. St. George Gray, who have worked in double-harness at the lake villages for a number of years. The antiquities discovered in past years at Meare are exhibited in the Somerset County Museum at the society's headquarters, Taunton Castle, while those from the Glastonbury Lake Village (described in two royal quarto volumes) are to be seen, for the most part, in the museum at Glastonbury. There is a good deal of expense attaching to this work besides the labour of about eight men; the money in hand is quite insufficient for the work contemplated, and donations will gladly be received by Mr. St. George Gray at the Somerset County Museum, Taunton.

JULY was exceptionally warm and dry in many parts of England. The Greenwich Observatory records, using the civil-day values published in the Registrar-General's weekly returns, give 68·5° F. as the mean temperature for the month; the mean maximum was 81·5° and the mean minimum 55·5°. In the last eighty years, since 1841, July has been warmer only in two years, 1859 with the mean 69·5° and 1868 with the mean 68·9°, and in both 1852 and 1911 the mean temperature exceeded 68°. In July this year there were four days with the shade temperature 90° or above; the highest temperature was 94° on July 11. In 1868 there were six days in July with the shade temperature 90° or above, and July, 1881 and 1900, each had four days with 90° or above. The highest temperature on record in July at Greenwich is 97·1° in 1881. There were four days in July this year with the temperature in the sun's rays 150° or above. The total rainfall at Greenwich for the month which has just closed was 0·15 in., which is the smallest July measurement for nearly a hundred years; the only July with a smaller total was in 1825, when the amount was 0·10 in. The only other years with the July rainfall less than ½ in. are 1835, 1864, 1878, 1906, and 1911. The rainfall has been less than the normal in each of the last twelve months from August, 1920, to July, 1921, with the exception of September, 1920. In the twelve months the total rainfall at Greenwich is 14·98 in., which is 9·43 in. less than the average for the last hundred years, and only 61 per cent. of the normal. The *Times* for August 5 gives a communication from its weather correspondent, "Driest Twelve Months." It mentions that in the east and south-east of England many places besides London had less than 0·25 in. of rain in July, whilst the measurements in some of the western districts were well above the normal. The smallest rainfall for the twelve months is 11 in. at Howden, Yorkshire, and this is stated as quite without precedent in the United Kingdom, so far as can be seen at present. At Yarmouth the rainfall for the twelve months was 12·8 in.; at Benson, Oxon, 13·1 in.; Cranwell, Lincs, 13·7 in.; Kew, 15·0 in.; and Croydon, 15·3 in. The lowest previous fall for the corresponding period at Kew since 1866 is 16·75 in. in 1890–91.

IN the August issue of *Man* Major R. Burnett describes a remarkable tribe in the neighbourhood of Mosul, popularly known as "Slaveys," which possibly represents the Bedouin Solibala, of which the Russian writer Ponafidina gives an account in his "Life in the Moslem East." The "Slaveys" are a desert tribe supposed to be the direct descendants of the Crusaders. Their dress consists of gazelle skins, and they have a cross marked on their backs. "They have no religion and no marriage laws: very poor and peaceable. It is considered unlucky to kill one of them, and they help the wounded." The Mohammedan Arabs despise them and call them "wild dogs." They are known for the shortness of their stature and the great length of their spears. They are the carpenters, blacksmiths, and doctors for man and beast among the Bedouin, and live in tents made, not of hair, but of skins. They may possibly be connected with the Negrito race which Sir Percy Sykes describes in the recently published second edition of his valuable "History of Persia."

IN the *Museum Journal* for March last we find an account of an interesting series of marble vases from Ulua Valley, Honduras, which are of such an unusual type that they have given rise to much speculation. The Ulua culture, like other ancient American cultures, is without date, but it was certainly contemporary with the ancient Maya Empire, as well as with other cultured races that flourished in México, Pánama, and Costa Rica. The technique and ornamentation of these vases are certainly remarkable. Attempts made by Mrs. Zelia Nuttall to interpret the symbolism are sharply criticised by the writer of this paper, who remarks:—"It would be as useless to speculate concerning the symbolism of all this ornament as it would be to guess at the service for which the vessel was designed. We are at liberty to assume that so elaborate and refined an object had a ceremonial function, and that its symbolism corresponds to ideas associated with its use, but its interpretation is quite beyond our reach."

THE issue of the Journal of the Royal Society of Arts for July 15 is devoted to a lucid paper on the development of Bombay by Sir G. Curtis. The position of the city, including originally seven islands, had long exposed portions of the site to inundation, and the enormous commercial development necessitated extension. These difficulties are being met in various ways, the principal being the reclamation of the area known as Back Bay. The chairman, Sir W. Sheppard, commented on the magnitude of the proposed series of undertakings:—"With regard to cost, there were few works in India—indeed, none of the precise kind described—which had cost, or been expected to cost, so immense a sum as thirty millions. Even in Europe so large a scheme would be considered wonderful, and he believed the renovation of Paris cost only about half the proposed expenditure on Bombay." But this has not deterred the Governor, Sir G. Lloyd, from pressing on the work, and the people of Bombay evidently believe in the project, as they showed by raising a local loan of nearly ten millions.

DR. D. F. CURJEL has obtained records of the weights at birth of 1849 normal Indian infants; the average is 6·5 lb. This compares not unfavourably with that of European infants. The conclusion is that the high infantile mortality which prevails among Indian children is largely due to unfavourable post-natal conditions. The same author has also inquired into the duration of reproductive life of Indian women. The average age of the onset of puberty was 13·63 years, and the average duration of reproductive life 32·14 years, both of which do not differ materially from the limits for European races (Indian Journ. of Med. Research, vol. viii., No. 2, pp. 363 and 366).

EXPERIMENTS have been conducted by Major J. C. G. Kunhardt and Asst.-Surg. G. D. Chitre on the eradication of plague infection by rat destruction. The observations made strongly support the view that the reduction in the rat population, resulting from plague itself (which attacks rats), is the main factor in bringing infection to a natural end, and that it yet remains to be seen if the destruction of rats by any artificial means is capable of producing or accelerating the same result. A number of rat poisons was tested, but none was found better than barium carbonate, of which three grains is a fatal dose for the rat. It is best made into a bait with dough of some grain-flour (the best grain was found to be bajri, *Pennisetum typhoideum*), and without any addition in the form of fat, sugar, condiments, etc. (*Indian Journ. Med. Research*, vol. viii., No. 3, 1921, pp. 409, 446).

IN the July issue of *The Fight against Disease*, the organ of the Research Defence Society, excerpts are given from an address by Sir John Rose Bradford at Oxford on "The Place of Experiment in the Science and Art of Medicine." Dr. Drury communicates notes on an "experiment," made by Nature herself eighteen years ago, on the protection against smallpox afforded by vaccination. In a school at Ossett there were 169 children, of whom 92 were vaccinated and 77 unvaccinated. Smallpox was introduced by a scholar, and no fewer than 37 of the 77 unvaccinated contracted the disease. Only 5 of the 92 vaccinated contracted it, all of whom had been vaccinated ten or more years previously. None of the 14 scholars who had been re-vaccinated took the disease. In the class into which the disease was first introduced (Standard IV.) all the vaccinated escaped and every one of the unvaccinated promptly took the disease.

DR. PERKINS gives in the Journal of the Torquay Natural History Society (vol. iii., No. 1) an account of his investigations on the food of trout caught in the Torquay reservoirs in August and September. He found that the nature of the food in the reservoir fish was very different from that of river fish. The latter appeared to be feeding on aquatic insects only, to contain much less food, and to be in an inferior condition generally. In the reservoir fish the food seemed to be composed mainly of such land insects as happen to fall accidentally on to the surface of the water under the stress of weather conditions. Dr. Perkins is of the opinion that this difference in the nature of the food is due to the fact that in the reservoirs the aquatic insects are limited in species, and the rarity or absence of some forms specially favoured by trout is the result of the extermination of the insect by the fish. The reservoir trout have thus to fall back on a source of food denied to the river fish. In a single trout's stomach Dr. Perkins found no fewer than forty-six species of land insects, the majority of which were beetles. No insect seems to come amiss to the trout as an article of food, and so important is this source of food-supply that the active rising of the fish is dependent on the activities of the land insects at the time.

IN the Journal of the Torquay Natural History Society (vol. iii., No. 1) Mr. Harford J. Lowe gives an interesting account, compiled from original notes and manuscripts, of the excavation work accomplished by the Rev. J. MacEnery at Kent's Cavern in Devonshire in the early years of last century. MacEnery was the pioneer worker at this famous cave, and by his energies and enthusiasm dug up huge collections of the remains of extinct British mammals and of the work of early man in Britain. Unfortunatelv, the results of his work seem to have been overshadowed by the publications of his more illustrious contemporary Buckland, with whom he was in constant communication, and, although published after his death by Vivian in 1859 and Pengelly in 1869, the work accomplished by MacEnery never seems to have received due recognition and reward. It is interesting to learn from Mr. Lowe's paper that, in spite of the prejudices and antagonistic opinions prevailing at the time, MacEnery had more than a suspicion of the important bearing of his work on the antiquity of man in Britain. MacEnery's collection was, unfortunately, dispersed by auction at his death, and students of this subject will be grateful to Mr. Lowe for the information which he gives as to the ultimate destination of part of it at any rate. Some of it found its way to the Jardin des Plantes, Paris, the British Museum, the Athenæum Museum, Penzance, the Plymouth Institution, and possibly to Bristol, while some at least remained at Torquay.

WE have received the first number of a new serial publication, the *Australian Museum Magazine,* issued by the Australian Museum under the editorship of the director, Dr. C. Anderson. The object of the magazine is to put the museum into more intimate relationship with its owners, the public of Australia, by keeping them in touch with the work that it is doing, by making its collections better known, by giving accurate and up-to-date information in simple language on the natural history and geology of the Commonwealth, and, in general, by showing how the museum can be of service to the nation and, conversely, in what ways the public can help the museum. Thus in this first number are to be found articles on the scope, work, and management of the Australian Museum and on museum groups, in which some insight is given into the technical work that has to be done in the preparation and exhibition of specimens, in addition to interesting accounts of Blackfellows' pictures, white ants and other Australian insects, snakes, crawling jelly-fish, and the lure of the big nugget. This experiment of rendering an account of its stewardship by the Australian Museum is one that might well be tried by other national museums. The museum is making a praiseworthy effort to stimulate a healthy pride among the people of Australia in their national institution and to secure that measure of interest and sympathy so essential if it is to develop its activities to the fullest extent. We hope the public will respond by leaving nothing undone that will place the Australian Museum among the first of its kind.

THE publications of the Naturhistorischer Verein der preussischen Rheinlande und Westfalens for the years 1913–19 have now reached us, and show the remarkable activity of the society even during years of war. The volume of the Verhandlungen for 1916 was completed in 1918, and the paper used and the mode of illustration show little falling-away from the high standard of 1914. As has happened in so many countries, deterioration sets in under the conditions following the war; but even now the plates do not suffer. The work of the society is largely geological, but chemists and biologists are concerned with August Thienemann's detailed "Physikalische und chemische Untersuchungen in den Maaren der Eifel" (1913–14). The marked differences in the plankton of the various crater-lakes depend on the distribution of oxygen in the waters. The mineral springs entering from the volcanic rocks show marked differences of composition in different lakes. The author of these researches adds in 1915 a study of the midge larvæ inhabiting the Maare, and in 1917 he describes the vertical zoning of the plankton in the Ulmener Maar. In 1916 F. Goebel gives a morphological description of the well-known district of the Ruhr, on the east bank of the Rhine. F. Winterfeld, of Cologne, publishes (1918) an illustrated paper on "Der aufrechte Gang des Menschen," in which he finds no room for pessimism. He concludes that "der Mensch der Zukunft wird im geistigen Sinne des Wortes aufrecht gehen, sich aufrecht halten, gehoben durch seine Ideale." We cannot help remembering the melancholy fact that hitherto physically upright man has been preserved mainly by the compulsion of military service. Enough has been said to show the range of research embodied in these undeterred publications of the war-time.

UNDER the editorship of M. Maurice Solovine, Messrs. Gauthier-Villars et Cie are issuing a collection of "Maîtres de la Pensée scientifique" in order to keep alive the memory of the advances made in the past by the great masters in every branch of science, whether these masters are French or of other nationalities. The volumes are $6\frac{3}{4}$ by $4\frac{1}{2}$ in., contain about 100 pages, and are issued at about 3 francs each. Huygens's "Lumière," Clairaut's "Géométrie," Carnot's "Réflexions," and d'Alembert's "Dynamique" are amongst the works issued, some of which extend over two volumes of the series. D'Alembert's work is reproduced from the second considerably enlarged edition which appeared in 1758, fifteen years after the first. It furnishes a good example of the clear and logical methods of development of a subject which were adopted by French scientific writers of a century and a half ago.

IN the July issue of *Science Progress* Prof. W. L. Bragg gives a summary of our knowledge of the dimensions of atoms and molecules. He points out that the kinetic theory of gases allows us, from measurements of the viscosity or the heat conductivity of a gas, to calculate the mean distance of the centres of two molecules of the gas apart when the molecules are in contact, that the constant *b* of Van der Waals furnishes another estimate of the distance, and that the two estimates agree in giving about 2×10^{-8} cm. for the mean radius of hydrogen and helium molecules and about 3×10^{-8} cm. for the mean radius of the molecules of argon, nitrogen, oxygen, carbon dioxide, and other gases. With these figures as a basis, X-ray crystal analysis then gives the relative positions of the atoms in the molecule of the material analysed. So far the most careful analysis of crystals of potassium chloride has, however, failed to reveal any structure corresponding to the KCl molecule. Each K atom is surrounded by six Cl atoms at equal distances from it. For chlorides the distances vary with the metal in the molecule, are large, -3 to 5×10^{-8} cm., for the first elements of a "period," and decrease to a limit 1·3 to $2{\cdot}7 \times 10^{-8}$ for the last elements.

MANY methods of harmonic analysis have been given of recent years. We need mention only the methods of Perry, Silvanus Thompson, and Russell. The question has now come prominently forward in connection with the disturbances induced in telephone and radio stations by the harmonics in the currents carried by overhead power lines. The power station engineer wants the manufacturer to guarantee that the electric generator he purchases from him shall give a pure sine-shaped wave of electromotive force. As it is impossible to make the machine give an absolutely pure sine wave, limits have to be fixed on the magnitudes of the amplitudes of the harmonics in the wave. Hence harmonic analysis is a necessity. In the Journal of the Institution of Electrical Engineers (vol. lix., p. 491) Mr. A. E. Clayton gives a *résumé* of the ordinary methods and two schedules for "harmonic analysis" by means of selected ordinates. One goes to the 25th harmonic and the other to the 13th. In the one case the assumption is made that no harmonic higher than the 25th is present, and in the other that there is none higher than the 13th. Seeing that in actual electromotive-force waves there is an infinite number of harmonics present, and as only a limited number of ordinates are drawn, we should have little confidence in results obtained by a "schedule."

THE Cambridge University Press will publish shortly "The Calendar," by A. Philip, the purpose of which is to provide a concise and popular summary of the history and construction of the Gregorian calendar, with special reference to the reform of the calendar and the fixing of the Easter date.

Our Astronomical Column.

BRIGHT OBJECT NEAR THE SUN.—Prof. Campbell, Director of the Lick Observatory, reports by telegraph an object brighter than Venus that was seen on August 7 3° east of the sun and 1° south. The message states that there is no doubt of the object being a celestial object. It is either a comet or a nova. The former appears more probable, owing to the distance from the Galaxy, where most novæ appear.

VARIABLE STARS.—The Bruce 24-in. photographic telescope at Arequipa has been used for taking spectrograms of the Large Magellanic Cloud, in which Miss Leavitt some years ago detected several variable stars (Harv. Ann., vol. lx., No. iv.). Miss Cannon, in Harv. Bull. No. 754, gives the spectral type and magnitude range of eight of them as follows:—No. 884, Mc, 11·4m. to 15·5m.; No. 900, M, 12·2m. to 13·6m.; No. 2257, K5, 12·4m. to 13·2m.; No. 2435, K5, 10·8m. to 11·7m.; No. 2447, K5, 12·0m. to 12·8m.; No. 2622, K5, 13·2m. to 14·0m.; No. 2822, Mc, 9·8m. to 10·6m.; and No. 2882, Mb, 11·0m. to 13·6m. The numbers are from Harv. Ann., vol. lx. It is satisfactory to find that such faint stars are within the reach of spectroscopic analysis.

Mr. Stanley Williams contributed a paper to Monthly Notices, R.A.S., vol. lxxxi., p. 332, on the star B.D.+44·994°, which he announced as a peculiar variable, possibly of the Cepheid type. Miss Cannon gives its spectral type as Ma; and Miss Leavitt has identified 150 images of the star on plates taken during the last twenty years. Its normal photographic magnitude is 10·5m., but on seven dates it was 10·2m.; it is very red (Harv. Bull., No. 754).

C. Hoffmeister, director of Sonneberg Observatory, noted on May 30 last, while observing Reid's comet, an 8th magnitude star that is not in the B.D. Its position for 1855·0 is 7h. 57·0m., N. 58° 14′, and it is shown on the Harvard plates. Prof. Küstner has examined the original observations of the B.D., and finds that a star of 9·5m. was observed in the place on February 19, 1858, but not seen again, so it is probably variable (*Astr. Nach.*, Circ. 22). In the same circular H. Fuss announces that B.D.+42·3351°, 7·5m., has the large proper motion of +0·065s., +0·16″.

MR. FLINT'S PARALLAX OBSERVATIONS.—Publications of Washburn Observatory, vol. xiii., part 1, contains the details of the series of meridian observations for stellar parallax made at Washburn between 1898 and 1905 with the Repsold meridian circle of 12·2 cm. aperture, fitted with a travelling-wire micrometer.

The programme extended from declination −35° to +90°, and embraced stars from magnitude 1·5 to 2·5, with some binaries and stars of sensible proper motion. A screen with thin metal slats rotating about their axes like the laths of a Venetian blind was used to equalise magnitudes, 7·0m. being made the standard. Two comparison stars, one preceding, the other following, the parallax star, were used in each case.

The mean probable errors of a single observation of unit-weight and of the final parallax of each star are 0·214″ and 0·031″ respectively. The last quantity is of about three times the size of the probable error in the best recent photographic determinations, showing that the meridian method cannot compete seriously with the photographic. Still, the experiment was well worth making, and the research will occupy a place in the history of the subject, so that it is well to have the details published. The list of parallaxes contains 124 stars, of which the deduced relative parallax is negative in thirty-five cases. The values for Algol and Castor, 0·122″ and 0·167″, are about three times the accepted values, while that of Altair, 0·071″, is only about one-third of it; but in many cases there is better agreement.

The Universities and Research.[1]

By PROF. J. JOLY, F.R.S.

THE argument for research in universities rests upon the broad basis of the value of the intellectual progress of mankind. I think I am correct in saying that most men who have adopted a life of research, or have made research the object of their special interest, have acquired their intellectual ideals in the days of their college life. It is through the university that the young man comes into contact with the investigators of his time, and it is their example and teaching which affect his future life. If his teachers are without interest in research the student learns indeed the text-book, but the enthusiasm to create new knowledge is not implanted in him. Whatever his intellectual capacities may be, he passes from his university but an ordinary member of the educated public. What he might have accomplished, and could have accomplished, had he found himself in a creative atmosphere during his student days remain entirely unknown.

I do not think that any other argument for the cultivation and promotion of research in universities need be stated. If the investigation of Nature is good in itself; if its effects are beneficial to our race; if it is desirable that we should advance in knowledge from generation to generation, then we should see to it that our brilliant young men get the chance of taking up this career in the service of mankind. There is, as I say, no answer to this argument unless we assail its basis and determine that obscurantism is the better thing and enlightenment the worse.

Great universities have done great good. They have also done great harm. Their inertia, their opposition to development, to following the evolutionary changes of their times, constitute their principal offence. Even to-day I hear in my own university surviving voices expressive of distrust in science as an educational subject; doubts as to the propriety of including science as a primary subject in the university curriculum; regrets that the so-called "great" or "fundamental" subjects of education—*i.e.* classics and mathematics—should no longer form the only road to fellowship.

Such views on science are the natural outcome of an upbringing in the traditions of the older educational methods. To attain the forefront of classical criticism or of mathematical advance is a more difficult task than to reach the exploratory front of a branch of modern science. And not only is it more difficult to arrive at the forefront; it is also more difficult, when the forefront is attained, to find work of any probable benefit to mankind. Only the most brilliant scholars and the most original minds can prevail. Compare these conditions with those attending research in any of the newer domains of modern science. No sooner has the student mastered the principles of his subject than he finds himself approaching an unknown territory. Everywhere he sees the words "Not known" written up, and any one of these innumerable avenues to knowledge is for him to tread if he so pleases and is equal to the task.

The contrast is remarkable. The older scholar, who has spent his days turning over the thoughts of others and the time-worn records of past efforts, gradually arrives at the fatal conclusion reached by the wisest of men: "There is nothing new under the sun." He has passed a lifetime of solid work and seen but little come of it. Must not the younger workers be branded by superficiality?

As regards the subject of expense, there is no doubt that, contrasted with blackboard and chalk, modern scientific apparatus and scientific laboratories are expensive. It is discouraging to compare British outlay with American outlay upon research in universities. We are supposed to have learned a lesson by the war. Let us hope it will bear fruit when business revives in this country. Meanwhile this lesson has placed a heavy demand upon the universities. For every branch of technology is crying out for research workers, and the universities cannot supply them. The fact must be faced that the day of research has come in all the scientific professions and in every domain where technology or business comes into contact with the natural laws governing production and economy.

The reactionary sitting in senate, council, or board, who would close the university to these demands, may indeed effect economies, but his economies are at the expense of the vitality of his university, of its very existence as part of the living, breathing life around it. It is a cheap road, but it leads to stagnation, decay, and death.

Perhaps the most striking feature of American universities, as viewed by the British visitor, is the prevalence of research and the lavish provisions made for its prosecution. It extends into every branch of university work. Special stress is, however, generally laid upon certain subjects. What these subjects are seems to depend upon the initiative and forcefulness of particular teachers of eminence, either past or present, who have been associated with the university. The great Research School of Education in Chicago, of which Prof. Dewey seems to have been the chief originator, may be cited. Highly organised and carefully staffed elementary and high schools are here attached to the university for research in pedagogy. The Nutrition Laboratory of the University of Illinois, founded by Prof. Grindley, is another instance. The State universities are very often in close touch with agricultural research, and not only benefit agriculture thereby, but also extend the influence of the university over the State by the valuable assistance given to the agriculturist. In our own country there is no class of the community more in need of such university influence than the agricultural. It is—in Ireland—not only ignorant of science, but also strongly anti-scientific. This applies almost as much to the so-called educated classes as to the small farmer.

For research in experimental science and chemistry and natural science extensively equipped departments are provided in all the great American universities and technological institutes. The equipment is on the most lavish scale. Everything possible seems to be done for the student.

There is one subject which I must refer to: the compulsory presentation of Latin or of Latin and Greek by students entering the older universities. I know we are a long way from reform in this matter, but its influence upon the present subject is sufficiently important to necessitate a reference to it.

As regards research in the physical and natural sciences, there is no doubt that the compulsory study of dead languages is injurious—indeed, seriously injurious. This is so for two reasons. It serves to keep out many from the universities, and it demands of the science student hours of toil which would be better spent on living tongues, which would help him later on to extend his scientific reading. I am every day in contact with brilliant young men whose minds are absorbed in the interests of physical or natural science, but who cannot read a German book, and

[1] From a paper read before the Congress of the Universities of the Empire at Oxford on July 8.

read a French one only with difficulty. These young men have spent many school years during which the study of Latin and Greek absorbed about one-third or one-fourth their total available study hours. What have they got for it? They cannot read a Latin author or a Greek author at sight. It is true that without their Latin they would not have attained the degree of the University of Dublin. The pro-classic says their minds are the better for it. Well, I freely admit that much mental training was involved, but I do not admit that a sound study of French and German would not have done for them just as much—nay, more.

The reproach that many students fail as research workers, while it has some foundation in fact, is not a fair one, for it ignores the educational value of even elementary research. I believe the outlook of a student who has carried out one single research of an elementary kind is different from that of one whose outlook is derived solely from the text-book and the examination. He learns first-hand the mental point of view of the investigator. He gets ideas of scientific truth and of the legion of errors which lie in wait around it as may in no other way be acquired. He sees the plausible, *prima facie* conclusion break down under the control experiment or in the light of the inexorable requirements of other participating laws of Nature. A new conception of the use of mathematical analysis and of careful observation is created in his mind. More generally he learns the necessity of "thinking round" his subject.

These things he learns in some degree even if he is only of average capabilities. If he is one of the higher spirits the interest of the work seizes on him and calls out every power, latent and developing, wherewith he is endowed. These higher spirits work out their own destiny. I shall not dwell on the ways of genius, but rather upon research as an instrument in the education of less gifted minds. I turn, therefore, to the interesting question: "Is it possible to teach research successfully?" To teach its methods and its spirit to the average student, whether of science or of the "humanities"?

The answer I would give unhesitatingly is "Yes." I would be careful to define that this does not imply the genesis of an original thinker from ordinary material. But it implies just as much as when we say we can teach students mathematics.

I plead, therefore, for lectures in our universities devoted exclusively to studies in research, and I would admit to these lectures students of both junior and senior standing, *i.e.* the beginners in science as well as those working for the Ph.D. as now instituted in all British universities.

Of course, I am not now referring to systematic lectures in this or that branch of science. These are essential to the training of the average student. I mean something different. I would define research lectures as mainly relating to the professor's own experience and to that of his assistants and co-workers, each worker contributing one or more lectures to the university course in research. Their subject-matter would relate to the objects aimed at by the research, the difficulties attending the work, and how they were surmounted. Such discourses might be supplemented by others of an historical or retrospective character. These might in some cases be delivered by honours students, and would refer to classic researches of the great masters. For recounting these, experimental illustrations should be given. The inspiration to be derived from such retrospective studies will be known to all who have read the original memoirs of great investigators. There need be no extra call upon the professor's time. He would simply substitute these for part of his existing routine lecture work.

The professor is at present too much tied down by routine courses. There is a sort of idea prevalent that it is not fair to his class that he should tell them of his own work, but that this should rather be kept for the academy and for the outside world. Well, I think it *is* fair; and I believe that with reasonable usage the best thing he can do for his class is to tell them of his own work. If this were admitted in high quarters it would be more often carried out. I can imagine nothing more stimulating than a few lectures each term on the work progressing in the laboratory of the professor and his co-workers, for not only is the student brought into touch with the making of knowledge, he is also sure to receive the story in the language of fresh and enthusiastic interest.

I am aware that occasionally and at scientific associations within the university such discourses are delivered. I would make them a part of the sessional work of the university. If not legally obligatory on the professor, it should be morally obligatory on him to contribute a few such lectures every term, or at least every session. I do not think it would impose additional labours on him. Fresh from his work, but little rearrangement would be required, and his facts would be ready marshalled in his memory. Nor would the telling of his ideas fail to react upon the lecturer—to his benefit and to the elucidation of his subject.

The one central result aimed at is the presentation of research as something of paramount importance. It should stand for the highest goal of university effort, for, in truth, success in the making of knowledge is the crown of all human endeavour, and as such the student should be taught to regard it. Teach him this one great ethical truth, and whatever else he may accomplish or fail to accomplish in his student days, he will enter on his life's work an enlightened and a valuable citizen, not only of his own country, but also of the world.

International Exploration of the Upper Air.

By C. J. P. CAVE.

A MEETING of the International Commission for the Exploration of the Upper Air was held at Bergen on the invitation of Prof. V. Bjerknes, president of the commission, in the week ending July 30. The commission was appointed by the Meteorological Conference held at Paris in 1919 to continue the work, in connection with the International Meteorological Committee, which was carried on with marked success from 1896 until the beginning of the war, and with which is specially associated the names of the late Teisserenc de Bort, Rotch, and Assmann, under the presidency of Prof. Hergesell.

The following countries were represented at Bergen: Belgium, Denmark, France, Great Britain, Holland and the Dutch East Indies, Italy, Japan, Norway, Spain, Sweden, and Switzerland; and the meetings were also attended by a number of prominent meteorologists from Norway and Sweden. The proceedings opened with a reception by the president and Mrs. Bjerknes at the Meteorological Office of the Geophysical Institute, and with a lecture by J. Bjerknes on recent advances in the study of the Polar front and its relation to a succession of cyclones. It was arranged that the morning sessions should be devoted to scientific communications presenting new points of view, and the afternoons to administrative details. This arrangement was disturbed in the course of the week in order to provide more time for administrative questions.

The sessions for scientific discussions were opened by a paper by Prof. V. Bjerknes giving theoretical explanations, on the basis of wave motion at the mutual boundary of two discontinuous media, of the series of phenomena which had been set out by his son, representing the result of observations upon the Polar front in cyclones. This was followed by a paper by Sir Napier Shaw on the structure of the atmosphere and its thermodynamics, to suggest a thermodynamic basis for the study of convection in the atmosphere, and the transformations of energy associated therewith. A paper by L. F. Richardson directed attention to the necessity for studying pilot-balloon observations in relation to the continuity of mass, a subject which in spite of its importance has hitherto not received adequate treatment. Prof. van Everdingen gave an account of a method of obtaining regular observations of pressure, temperature, and humidity in the upper air by means of aeroplanes, using a balloon meteorograph with the usual clock-work drum; such observations had been carried out on upwards of 340 occasions in the past year at Soesterberg and other stations in Holland. S. Fujiwhara, of Tokyo, discussed turbulent movements which are to be observed in clouds, and their relation to eddies in water. Dr. W. van Bemmelen, of Java, gave an account of comprehensive results of great importance of observations of wind in the upper atmosphere up to 30 kilometres, obtained at the observatory at Batavia. H. Köhler, of Holdda, discussed the study of the condensation of water vapour in a cold atmosphere into ice crystals and super-cooled water drops, and the effects which may be attributed to very small quantities of chlorides.

L. F. Richardson discussed the application of the geostropic principle to winds in the stratosphere. Dr. A. de Quervain, of Zurich, brought up proposals for the establishment of a geophysical observatory at the terminus of the Jungfrau railway, at a height of 3600 metres, which received the cordial commendation of the meeting. P. Schereschewsky, of the Corps of Mines, Paris, explained the method of determining the winds in the upper air by means of sound-ranging applied to detonators carried by pilot balloons, a method which is applicable alike in clear and cloudy weather. R. Sekiguti, of the Observatory of Kobe, explained the application in forecasting of isobaric charts for the level of three kilometres. Col. L. Matteuzzi, director of the Meteorological Service of Italy, presented an atlas of the principal cloud forms, and explained a method of applying the periodicity of barometric oscillations to the anticipation of barometric distribution in the future. O. Devik, of Tromsø, described a new method of observing balloons and its application in forecasting. G. I. Taylor gave an account of the result of his investigation of turbulence in the atmosphere and its symmetric propagation in the three dimensions. M. Dongier discussed the observations of temperature and wind at the Eiffel Tower and the discontinuities which they disclose. J. Bjerknes directed attention to the unique accumulation of observations of the upper air during the war which had been communicated to the president by the countries on both sides, and gave illustrations of the observations on selected occasions in the study of the method of the Polar front. P. Schereschewsky gave an account of some new methods of forecasting, and the proceedings of the meetings for scientific discussion were concluded with a paper by L. F. Richardson on ideal arrangements of stations on the map for the purpose of numerical computations for forecasting.

The business meetings were devoted largely to the development of a scheme for the collection, compilation, and publication of observations in the upper air on an international basis, in continuation, with such modifications as experience has suggested, of the international scheme which was agreed upon at Petrograd in 1904 and supported by subventions from Government organisations of nineteen countries. The outline of a proposal was agreed upon, and the president was requested to report it to the meeting of the International Meteorological Committee to be held in London in September. The commission adopted resolutions in favour of a geophysical observatory on the Jungfrau, and also appointed a sub-committee to deal with the question of the anomalies in the audibility of the sound of explosions, which was also the subject of a communication by Dr. de Quervain.

It was noticeable that the commission devoted the greater part of its attention to the mode of dealing with the observations of the upper air based upon the supposition that there should be twenty-four days in the year on which balloons for sounding the highest layers of the meteorological atmosphere, including the stratosphere, should be sent up in a sufficient number of countries to secure a general representation of the whole. At present the number of observations is extremely limited, and the reorganisation of the observations would need the support of meteorological institutes in many parts of the world. Beyond pointing out the urgent necessity for such observations over the sea, the technique of which had already been worked out by Teisserenc de Bort and the German meteorologists, but which had not become international in any sense, the commission confined itself to a general invitation to the meteorological organisations of the globe for co-operation on the international days.

An account of the proceedings of the meetings would be incomplete without reference to the hospitality of the citizens of Bergen. It will be remembered that the greater part of the inner town was destroyed by a disastrous fire five years ago, and it has not yet been rebuilt; all the hotel accommodation was required for tourists, and the delegates to the meeting were all entertained with cordial hospitality by the foreign Consuls and the citizens of Bergen, and also honoured by an official dinner given by the municipality, at which the Chief Burgomaster presided. The municipality also placed its ancient and picturesque Council House at the disposal of the commission for the meetings.

A Small Brinell Hardness Testing Machine.

HARDNESS, as recent correspondence in NATURE (vol. cvi., pp. 377, 440, 534, 599, 662, November, 1920–January, 1921) has shown, is a subject of interest to both the engineer and the physicist. Whatever may be the exact physical significance of the term, there can be no doubt that measurements of this property, or group of properties, are of increasing practical importance. In the Brinell method of measuring hardness, as commonly applied, a steel ball of diameter about 1 cm. is applied to the surface of the test piece under a load of the order of 3000 kg., and the size of the resulting impression is measured. In practice the use of a ball of this size is limited to specimens not much less than one-tenth of an inch in thickness and half an inch in width. In 1913 the necessity arose for the accurate determination of the hardness of the walls of small-arm cartridge-cases at different positions along the length. The thickness of wall, diminishing in some cases to about one-hundredth of an inch near the shoulder, is quite insufficient for the application of the usual Brinell test. Accordingly, a machine was designed and constructed by Messrs. H. Moore and R. Mather for the Research Department, Woolwich, in which very small balls with correspondingly small loads could be employed. A description of this machine has been given by Mr. Moore in

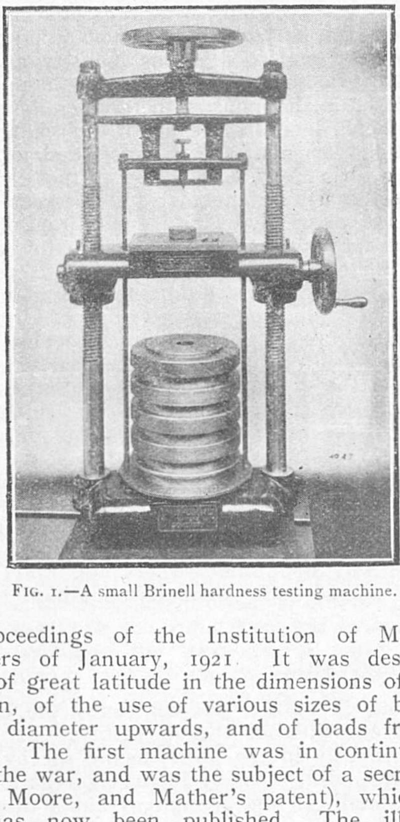

FIG. 1.—A small Brinell hardness testing machine.

the Proceedings of the Institution of Mechanical Engineers of January, 1921. It was designed to permit of great latitude in the dimensions of the test specimen, of the use of various sizes of ball from 1 mm. diameter upwards, and of loads from 5 to 100 kg. The first machine was in continuous use during the war, and was the subject of a secret patent (Craig, Moore, and Mather's patent), which, however, has now been published. The illustration (Fig. 1) shows a simplified form of the machine constructed by Messrs. Alfred Herbert, Ltd., of Coventry.

The machine stands upon a base-plate furnished with levelling screws. This plate supports two vertical threaded columns which carry the table for the reception of the specimens to be tested. By turning the hand-wheel at the side of the machine the table may be set at the required height. The load seen in the lower part of the photograph is composed of a set of graduated cylindrical weights totalling 50 kg. It is carried by the loading stirrup, to the upper portion of which is attached the ball-holder. The ball is fastened to the ball-holder by india-rubber solution so as to render the changing of balls an easy matter. The most important point in the design of the apparatus is the method by which the load is transferred from the cross-head of the machine to the specimen under test. By turning the hand-wheel at the top a non-rotating screw of fine pitch can be raised or lowered. The lower end of the screw carries a suspension stirrup, which is prevented from rotating by arms bearing against the columns, and from this suspension stirrup is hung the loading stirrup by means of a ball-and-socket joint. When the stirrup is lowered gently, so that the ball rests upon the specimen, the loading stirrup becomes free and disconnected from the suspension stirrup. At this stage the whole of the weight is upon the specimen, there being no parts in friction or rubbing contact. The upper hand-wheel is then turned back to take the load off the specimen, which can now be removed for the purpose of measuring the diameter of the impression by means of a high-power microscope with graduations of 1/200 mm. on the graticule. The hardness numbers are calculated as in the ordinary Brinell test, the load being divided by the area of the impression, and are directly comparable with the usual Brinell numbers when a load proportional to the square of the ball diameter is employed.

The impressions are so small as to be scarcely perceptible to the eye, and tests may be made on parts of delicate mechanisms without injury to the part tested. Loaded small-arm cartridge-cases may be tested without removal of bullet or charge. The hardness of wire at successive stages of drawing can be measured. Cutlery blades, however thin, may be tested, and the hardness of a cutting tool may be determined close to the cutting edge. Interesting applications of this microscopic Brinell test have been made in the exploration of strain-hardening, for when a metal object has been unequally strained the distribution of strain will usually be indicated by differences in hardness from point to point.

Attention may also be directed to the micro-Brinell apparatus developed by the Ordnance Department of the U.S. Army (Bureau of Standards, Bulletin 16, 1920, p. 557). This has been used with a load of 15 kg. for 30 seconds upon a ball 1/16 in. in diameter for measuring the hardness of individual crystals or small aggregates in annealed carbon steels.

H. S. A.

The Coal-mining Industry.

By PROF. H. LOUIS.

THE July issue of the *Quarterly Review* contains an article upon the recent coal dispute by Dr. Arthur Shadwell, to which he has given the somewhat unfortunate title "The War of the Mines." Dr. Shadwell points out at the beginning that this dispute was really not a war, and that there was in reality no need at all for a difference, which might have been arranged by mutual concessions, to have degenerated into industrial strife. He recognises that this was not a case of the men striking against any arbitrary action of the employers, but was rather an expression of their irritation at the inevitable development of the economic situation, and he states clearly and definitely the only remedy: "There is only one way out—the way of work. Other nations in a similar position have taken it; they are at work, and working hard. Here less work is being done than ever before."

It is pointed out quite correctly that the mining industry is distinguishing itself above all others in the readiness with which it resorts to industrial strife, and that the real cause of many of these difficulties, and the basal reason for the present grave position of the coal industry, are to be sought in the Minimum Wage Act of 1911, which is accurately described as "the first instance of a minimum wage established by Parliament in an industry in which the workmen are well organised and able to protect themselves."

Dr. Shadwell is undoubtedly right in saying that the demand for this Act arose on account of the existence of "abnormal places" in coal-mines—that is to say, places in which men cannot make normal wages even though they work up to the normal standard, and that these conditions are due to natural causes which can be neither controlled nor foreseen. He appears to accept the Minimum Wage Act as the only means of meeting the difficulty, but in this view experienced coal-miners are not likely to concur. It should be perfectly possible to devise means other than this Act, which unfortunately encourages slack work, without the grave drawbacks which the Act has brought in its train, but such other methods must be founded on mutual confidence between masters and men.

It must be admitted that this antecedent condition does not exist; masters have in the past been only too ready to look upon hard work or successful work on the part of the men as a fair pretext for cutting piece rates, and this action has sown in the men's minds the seeds of the suspicion that they cannot rely upon the masters for fair treatment in the case of abnormal difficulties. Colliery managers to-day are, no doubt, wiser, and have learnt to appreciate the fact that it is to their advantage, no less than to that of the men, that the latter should be in a position to earn high wages, provided, of course, that they give a commensurate amount of work in return. The old suspicious feeling, however, remains, and it has been responsible for the introduction of legislation which has probably done more harm to the coal industry than any other single step that can be named.

Dr. Shadwell devotes considerable attention to the discussion of the proposal for a national pool, but he evidently fails to see the real object underlying the proposal. He says that "it is impossible to maintain that there is anything impracticable or economically ruinous in pooling or amalgamation," and cites Sir George Elliott's old proposal to amalgamate all the collieries in the kingdom into one concern. He fails to see the difference between voluntary amalgamation and compulsory pooling, which latter would necessarily bring in a large number of collieries that are no longer able to produce coal for less than its market price. He suspects, indeed, that the object with which the pool was put forward was political, but does not appear to see the real motive underlying the scheme. As a matter of fact, all the proposals put forward for a considerable time past by the Miners' Federation, the Minimum Wage Act, repeated shortening of the hours, nationalisation, the pool, as well as the less openly avowed tendency to restrict production wherever possible—all these have one and the same underlying object, namely, to keep the largest number of men in the industry.

This object has been only too successful; the coal-miner to-day produces only two-thirds of what he did fifteen years ago, so that for an equal production the number of men employed in the industry is proportionately greater. Obviously, the larger the number of men employed in the industry the greater the political power of the Federation, because it thus obtains control of a larger number of votes and of larger monetary contributions. This gain to the Federation is, however, dearly purchased by the decrease in the efficiency and prosperity of the industry, and obviously such a road can only lead to ultimate ruin and destruction. No industry can prosper if it has in its ranks more men than it can legitimately maintain. The object of nationalisation was to support out of the pockets of the taxpayers the mines incapable of producing economically; the object of the pool was to support them at the expense of the mines that could pay their way. Both schemes were political, in the sense that their object was to keep a number of men in the industry who were working at a loss, and to devise means by which that loss might be made good by someone else. If Dr. Shadwell will consider the effects of the proposed pool upon the mining industry of the country as a whole in the above light, he will readily see why both mine-owners and the Government have offered such strenuous opposition to it.

Botanical Papers from Pennsylvania.

TWO parts of the Journal of the Botanical Laboratory of the University of Pennsylvania recently received (vol. iv., No. 2, and vol. v., No. 1) contain a number of interesting papers. Dr. D. W. Steckbeck has studied the comparative histology and irritability of sensitive plants. The majority of the highly sensitive species are natives of subtropical and tropical America, and their most widespread irritable response is the nyctitropic or "sleep-movement." The author suggests that the phenomenon of propagation of stimuli is centred in the endodermis, the cells of which contain a greater or less number of crystals of oxalate of lime, the number, regularity of shape, and degree of restriction to the endodermis increasing with the increase of sensitivity shown by the plant; the climax is reached in the two highly sensitive plants *Mimosa pudica* and *Biophytum sensitivum*. Each crystal is surrounded by a protoplasmic sac, threads from which pass through adjacent cell-membranes so as to form continuous protoplasmic connections throughout the endodermal tissue; the crystals with their protoplasmic connections are regarded as the special conducting lines for stimuli. The cells of the pulvinus of the leaves are found to contain aggregation bodies, resembling those described by Darwin and others, increasing in amount and complexity with increasing sensitiveness; these show contraction and aggregation changes under stimulation. They are proteinaceous in nature, and all contractile changes resulting from external stimuli seem to be due to changes primarily in the protoplasmic sac by which each is surrounded, secondly in the aggregation body itself, and finally in the amount of liquid these may absorb or give off.

Dr. J. S. Hepburn and Dr. E. Q. St. John describe the results of their investigation of the active digestive agent in the liquor secreted in the pitchers of the pitcher-plant (Nepenthes). Does digestion result from the action of a protease secreted by the pitchers or is it due to bacterial action? The authors found that liquor taken aseptically from unopened pitchers was sterile, but liquor in partly opened pitchers which were free from insects contained bacteria. The slowness with which bacterial digestion of the protein occurred shows that bacteria play a secondary *rôle* in the digestion of insects; the leading *rôle* is undoubtedly played by the protease of the pitcher liquor. The enzymes contained in the bodies of the insects may also assist in digestion.

Miss Alice M. Russell gives a comparative study of the macroscopic and microscopic structure of some hybrid Sarracenias and their parent species. Sarracenia is the genus of pitcher-plants native to swampy districts in Atlantic North America from Labrador to Florida, and several natural hybrids have been reported. The hybrid forms are found to be intermediate, in comparison with the parents, in almost all details, namely, shape of leaf-pitcher and lid, colouring, size and shape of flower (though the flower of the hybrid is inclined to be larger and more showy than the parent), and size and shape of the petals. The intermediate relation also extends to microscopic details, such as character of cells of the epidermis, number of stomata, and characters of the internal tissues.

Dr. H. W. Youngken has studied the comparative morphology, taxonomy, and distribution of the Myricaceæ (bog-myrtles) of the eastern United States. The author finds that the infesting organism in the characteristic root-tubercles is an Actinomyces, and he has also observed it in the cells of the fruit-wall; after the fall and decay of the fruit it will again make its way into the soil and infect roots of other Myricas. Coccus-like forms, believed to be involution forms of the infesting Actinomyces, were found in the pitted wood-vessels, and apparently indicate the pathway taken by the parasite in order to reach the fruit-wall.

Miss Margaret Henderson describes the results of a comparative study of the structure and saprophytism of the Pyrolaceæ and Monotropaceæ in relation to the Ericaceæ (heaths). The author suggests that the two former families differ from the Ericaceæ only in their gradually increasing saprophytism and in those characters which go hand-in-hand with this, namely, loss of green colouring matter, reduction from shrubs to herbs, reduction of leaves to scales, increase in the number of seeds, and the reduction in their size and in the number of cells of the endosperm and embryo. Similar degradation changes occur in the orchid and gentian families, and the author therefore supports the view which would regard the Pyrolaceæ and Monotropaceæ, not as distinct families, but as representing subfamilies of the Ericaceæ.

The Claude Process for Ammonia Synthesis.

IN the issue of the *Revue scientifique* for May 28 M. Georges Claude gives an interesting account of his process for the synthesis of ammonia, depending on the use of pressures approaching 1000 atmospheres. The work of compression of a gas at constant temperature varies as the logarithm of the pressure, so that if the work of compression from 1 to 200 atm. is 2·3, that from 1 to 1000 atm. will be only 3, or at most 3·5, if the diminution of compressibility at high pressures is taken into account. At high pressures, however, the percentage of ammonia in equilibrium with hydrogen and nitrogen will be greatly increased. Claude announced in 1917 that his experiments indicated that the yield could be increased from about 13 per cent. at 200 atm. to more than 40 per cent. at 1000 atm., the temperature being the same in both cases. A production of 6 grams of ammonia per gram of catalyst an hour, as compared with 0·5 grams in the Badische process, is attained. Whereas it is necessary at 200 atm., employed by the Badische Co., to circulate the gas several times over the catalyst, and to separate the ammonia after each circulation, it is sufficient to circulate only three or four times at 1000 atm. The volume of the apparatus required for the same production is only about one-tenth that required at 200 atm. pressure. The main source of difficulty in working at high pressures is the evolution of heat, which is 25 to 50 times greater than in working at 200 atm. The difficulty is then, not to conserve the heat of reaction to make the process autothermic, as is the case in the Badische method, but to eliminate this heat. The Claude apparatus has been operated with success at La Grande Paroisse with a unit producing 1·25 metric tons of ammonia per day, and a larger unit, for 5 tons per day, with a compressor dealing with 700 cu. m. of gas per day, has recently been put into operation with success.

The percentage of ammonia after passing the catalyst is about 25 at 1000 atm., as compared with about 6 at 200 atm. The partial pressure is therefore 250 atm., as compared with about 12 atm. at 200 atm. total pressure. The vapour tension of liquid ammonia at atmospheric temperature being from 7 to 8 atm., it will be seen that this is negligible in the gas obtained by the Claude process, but most appreciable with the gas obtained by the Badische process. It is sufficient, in Claude's apparatus, to pass the gas through coils immersed in cooling water in order to separate practically all the ammonia, and the residual gas, after separation of liquefied ammonia, is sent directly, without further compression, to a second catalyst chamber. Three or four catalyst chambers suffice to convert the gas into ammonia. In the Badische process, on the contrary, it is necessary to wash out the ammonia with water under pressure, requiring a complicated apparatus and expenditure of work to bring the gas again to 200 atm. after mixing with fresh gas, and 15 catalyst chambers are required. It is also necessary to use heat to separate the ammonia gas from the solution so obtained, whereas in Claude's process the liquefied ammonia is merely allowed to evaporate, producing cold which can be utilised.

The Claude process, which offers great possibilities in the synthesis of ammonia and in the utilisation of atmospheric nitrogen, is to be installed in England. The patent rights have been acquired by the Cumberland Coal and Chemicals Co., who are to erect a works in the centre of the coke-oven district in Cumberland.

Field-work of the Smithsonian Institution.

THE Smithsonian Institution has just issued its annual Exploration Pamphlet, describing and illustrating its scientific field-work throughout the world during 1920. Twenty-three separate expeditions were in the field carrying on researches in geology, palæontology, zoology, botany, astrophysics, anthropology, archæology, and ethnology, and the regions visited included the Canadian Rockies, fourteen States of the United States, Haiti, Jamaica, four countries of South America, Africa from the Cape to Cairo, China, Japan, Korea, Manchuria, Mongolia, Australia, and the Hawaiian Islands. The pamphlet serves as a preliminary announcement of the results obtained, though many of the expeditions will be more fully described later in the various series of publications under the direction of the Smithsonian Institution.

Dr. C. D. Walcott, secretary of the Smithsonian Institution, continued his geological work in the Cambrian rocks of the Canadian Rocky Mountains in the region north-east of Banff, Alberta. The work was hindered considerably during July and August by forest fires and by continuous stormy weather in September, but the particular questions involved in the season's research were settled satisfactorily, and some beautiful photographs of this wild and rugged region are shown in the pamphlet. Other geological field-work was successfully carried on in various States of the United States by members of the staff.

In astrophysical research the institution was unusually active. Through the generosity of Mr. John A. Roebling, of New Jersey, the Smithsonian solar observing station located on the plain near Calama, Chile, was moved to a mountain peak near by, where the observations will be unaffected by dust and smoke; and a new station was established on the Harqua Hala Mountain, Arizona, probably the most cloudless region in the United States. From daily observations of the radiation of the sun at these two widely separated stations it is hoped to establish definitely the value of "solar constant" observations in forecasting weather. Dr. C. G. Abbot, director of the work, also describes the successful operation on Mount Wilson, California, of a solar cooker devised by him. With this apparatus it was possible, using only the sun's heat, to cook bread, meat, vegetables, and preserves.

Mr. H. C. Raven represented the Smithsonian Institution on an extensive collecting expedition through Africa from south to north. Although many difficulties were encountered, among others a railway wreck in which two members of the expedition were killed, Mr. Raven shipped to the institution much interesting zoological material which was greatly needed for purposes of comparison in working up the famous Roosevelt and Rainey collections already in the National Museum. Many interesting photographs of the animals, the natives, and the country itself are shown in this account and in that of Dr. Shantz, who accompanied the expedition as botanical collector. In Australia a Smithsonian naturalist collected, through the generosity of Dr. W. L. Abbott, specimens of the fast-disappearing remarkable fauna of that continent, while Dr. Abbott himself secured for the National Museum a great number of plants, birds, and other natural history material in various regions of Haiti. A number of other zoological and botanical expeditions are briefly described and illustrated.

In anthropology Dr. Ales Hrdlicka, of the National Museum, conducted extensive investigations in the Far East with the objects of continuing the study of the origin of the American Indian, examining the oldest skeletal remains in Japan, furthering the interests of physical and medical anthropology in China, and studying the rapidly disappearing full-blooded Hawaians. The work was successful in every respect.

Dr. J. Walter Fewkes continued his archæological field-work on the Mesa Verde National Park, Colorado. During the field season of 1920 he excavated and repaired a remarkable prehistoric building, which he designates "Fire Temple" on account of the undoubted use of this structure by the Indians in connection with the worship of fire. The ruin is described and illustrated in the pamphlet.

The book concludes with numerous accounts of field-work among the American Indians by members of the staff of the Bureau of American Ethnology, Smithsonian Institution, including researches among the Hopi, the Papago and Pawnee, the Fox and Cree, and others; and archæological investigations of prehistoric aboriginal structures and dwellings in various regions of the United States.

University and Educational Intelligence.

OXFORD.—Two important elections to professorships have been made since the end of term. The vacant Drummond professorship of political economy has been filled by the appointment of Prof. David Hutchison Macgregor, Stanley Jevons professor of political economy in the University of Manchester, sometime professor of political economy at Leeds, and fellow of Trinity College, Cambridge. Prof. Macgregor is known as a writer and lecturer on industrial and philosophical questions, and has also done work in connection with the Board of Trade.

The Linacre chair of zoology and comparative anatomy, vacant by the regretted retirement of Prof. G. C. Bourne, has been filled by the appointment of Prof. E. S. Goodrich, fellow of Merton College, and hitherto professor of comparative embryology and Aldrichian demonstrator in comparative anatomy at Oxford. Prof. Goodrich enjoys a high reputation among zoologists, and his artistic attainments are also widely recognised. He is president this year of Section D (Zoology) of the British Association, and the address which he is to deliver at the forthcoming Edinburgh meeting is awaited by zoologists with keen interest.

The University has lately bought a large house in Mansfield Road, part of which will furnish the School of Geography with increased accommodation.

THE Board of Education has at last issued the long-awaited report of the Burnham Committee dealing with scales of salaries for *full-time* teachers in technical schools, schools of art, evening schools, and day continuation schools, in which the local education authority accepts responsibility for the salary scales. The report follows in natural sequence upon the reports of the other two Joint Committees, and is correlated especially with that of the Joint Committee on Secondary Schools. Teachers are graded as (1) principals, headmasters, or headmistresses, (2) heads of departments, (3) graduate assistants, (4) non-graduate assistants, and (5) instructors. For the graduate class the scale determined is 240*l.*, rising to 500*l.* by annual increments of 15*l.*, and for non-graduates 190*l.*, rising to 400*l.* by annual increments of 12*l.* 10*s.* The corresponding figures for women are 225*l.*, rising to 400*l.* by annual increments of 15*l.*, and 177*l.* 10*s.*, rising to 320*l.* by annual increments of 12*l.* 10*s.* For male graduates in the London area the scales are increased by the addition of 50*l.* to both the minimum and the maximum, corresponding additions being made also to the other scales. In order to attract highly trained teachers to the technical service, the local education authority may raise the minimum by 25*l.* and the maximum by 50*l.* in the case of a good honours degree or its technological equivalent. Further, other additions may be made in respect of post-graduate training and posts of special responsibility. It is possible, therefore, for a graduate teacher to secure a maximum salary of 650*l.* in London and 600*l.* in the provinces, with the opportunity of promotion to the higher grades. Under such conditions a real career is offered in the service to both men and women, and the Committee is to be highly congratulated on the satisfactory completion of an extremely difficult problem. The Committee regrets that it has been unable to determine scales for the other grades of teachers owing to the many and various types of schools and the wide divergence of local conditions. Local education authorities are asked, however, to formulate suitable scales by agreement, and it is pointed out that these scales should be comparable with those adopted for similar classes of teachers in the secondary schools of the area. The adoption of the report will influence the desired development in technical and scientific education, which depends so largely on the securing to, and retaining in, the service the best type of teacher. It is in this respect satisfactory to find that technical and commercial qualifications and other experience may be regarded as the equivalent of an academic degree.

Calendar of Scientific Pioneers.

August 11, 1857. Marshall Hall died.—A distinguished physiologist, Hall while practising in London as a doctor studied the circulation of the blood, and in 1832 made his important discovery of reflex action.

August 12, 1865. Sir William Jackson Hooker died.—Few men have done more to advance the study of botany than Hooker, who from 1820 to 1841 held the chair of botany at Glasgow, and from 1841 to 1865 was director of the Royal Gardens at Kew. His herbarium—an exceptionally rich one—was bought by the nation.

August 12, 1896. Hubert Anson Newton died.—Made famous by his study of meteors and his prediction of the memorable display of November 13, 1866, Newton from 1855 until his death held the chair of mathematics at Yale, and for a time he directed Yale Observatory.

August 13, 1907. Hermann Karl Vogel died.—One of the pioneers in the application of Doppler's principle to stellar spectroscopy, Vogel worked with Zöllner and Spörer, and from 1882 was director of the Astrophysical Observatory at Potsdam. In 1883 he published his first spectroscopic star catalogue.

August 15, 1758. Pierre Bouguer died.—A Royal professor of hydrography, Bouguer studied naval architecture, and to him we owe the term "metacentre." He accompanied Godin and La Condamine on the great meridian expedition to South America (1735–45), and is also known as the inventor of a heliometer.

August 15, 1852. Johann Gadolin died.—An early exponent of Lavoisier's views, Gadolin was one of the most distinguished scientific men of Finland. He was the first to introduce the term "specific heat."

August 15, 1856. William Buckland died.—The first reader in geology at Oxford, Buckland made many pioneering geological excursions, wrote one of the Bridgewater treatises, and in 1822 received the Copley medal for his discoveries in a cave at Kirkdale. He was for some years Dean of Westminster.

August 16, 1705. James Bernoulli died.—From 1687 until his death James or Jacob Bernoulli held the chair of mathematics at Basle. His lectures of 1691 contain the first published attempt to construct an integral calculus.

August 16, 1899. Robert Wilhelm Bunsen died.—Holding the chair of chemistry at Heidelberg for thirty-seven years, Bunsen, like Liebig and Hofmann, was a great investigator and an inspiring master. His important work included the study of gasometric analysis and the chemical action of light, the invention of the Bunsen battery, the Bunsen burner, a photometer, and an ice calorimeter, and with Kirchhoff in 1859 he began his epoch-making researches in spectrum analysis.

August 16, 1920. Sir Joseph Norman Lockyer died.—Originally a clerk in the War Office, Norman Lockyer became famous for his pioneering work in astrophysics. Simultaneously with Janssen in 1868 he devised and used a method of viewing the solar prominences in ordinary sunlight, and shortly afterwards discovered helium. Transferred in 1875 to the Science and Art Department, he was from 1885 to 1913 director of the Solar Physics Observatory at South Kensington. He was the founder of this journal, and has been described as "one of the greatest astronomers of all time."

August 17, 1856. Constant Prévost died.—Known for his geological studies of the Vienna basin and of volcanoes, Prévost in 1830 with Boué, Deshayes, and Desnoyers founded the Geological Society of France.

E. C. S.

Societies and Academies.

PARIS.

Academy of Sciences, July 25.—M. Georges Lemoine in the chair.—E. **Borel**: The fundamental hypotheses of physics and geometry.—G. **Lemoine**: The mutual reaction of oxalic acid and iodic acid, iii. The influence of sunlight. The experimental difficulties are considerable, owing to the rise of temperature which necessarily takes place during the exposure. In round figures, it may be concluded that in sunlight the time of half-decomposition for a given temperature is 0·4 that found in the dark.—E. **Haug**: The dysharmonic folds in the mountains to the north of Toulon.—L. **Joubin**: Oceanographic cruises now being carried out. An account of the work allotted to France by the International Commission at Copenhagen and the researches already in hand.—F. **Widal**, P. **Abrami**, and E. **Brissaud**: Experimental researches on auto-colloidoclasia by cold. Experiments on dogs have shown that immersion in cold water (2° to 3° C.) for periods of from fifteen to forty-five minutes produced changes in the blood identical in character with those due to anaphylactic and other forms of shock. The leucocytes were reduced in number, the leucocytic formula was changed, coagulation of the blood occurred in a shorter time, and the refractive index of the blood serum was lower. The effect was transitory, and the more serious symptoms of anaphylactic or proteid shock were not produced.—P. **Sabatier** and B. **Kubota**: The catalytic decomposition of allyl alcohol; action of various oxides. The catalysts studied were blue tungstic oxide, alumina, thoria, zirconia, uranous oxide, and manganous oxide. The gases evolved included carbon monoxide, hydrogen, carbon dioxide, ethylene, and propylene, the last-named being in the highest proportion. Propanal and acrolein were present in the liquid distillate.—P. **Humbert**: Formula of multiplication for the Kummer function $\Phi(\alpha, \gamma, x)$.—S. **Carrus**: Triple orthogonal systems.—L. **Amaduzzi**: A new property of feeble electrical conductors. A discussion of the interpretation of an experiment recently described by M. G. Reboul.—E. **Dubois**: The minimum potential of electrical discharge in hydrogen at low pressures.—L. and E. **Bloch**: Critical potentials and band spectra of nitrogen. The negative band spectrum of nitrogen appears at a higher potential than the positive spectrum, and a little higher than the ionisation potential generally attributed to this gas. It appears natural to attribute the positive band spectrum of nitrogen to the neutral molecule N_2 and the negative band spectrum to the positively charged molecule N_2^+.—F. B. **de Lenaizan** and L. **Maury**: The conductivity of the solution of cuprammonium citrate compared with that of copper sulphate. The two salts obey the law of Arrhenius, and the copper ion is free to the same extent in both.—A. **Boutaric** and M. **Vuillaume**: The flocculation of colloidal arsenic sulphide. The influence of the dilution and the quantity of the electrolyte.—J. **Barlot** and J. **Pernot**: Combinations of the halogen derivatives of mercury and thallium.—A. de G. **Rocasolano**: The variations produced by stabilisers in the catalytic power of electroplatinosols. Sodium protalbinate, sodium lysalbinate, gum arabic, and gelatine were the stabilisers used in these experiments; in all cases the catalytic power, as measured by the decomposition of hydrogen peroxide, was reduced.—G. **Andoyer**: An apparatus for the technical analysis of gases.—V. **Auger** and Mlle. M. **Vary**: Sulphonations in the presence of iodine. The results obtained by the sulphonation of benzoic acid and pyrocatechol in presence of iodine are not in agreement with the experiments of J. N. Ray and M. Lac Dey as regards the production of the ortho-isomer.—H. A. **Brouwer**: The eruption of a hornblende andesite in the Malay Archipelago.—L. **Lutaud**: The post-Sahalian movements and their influence on the morphology in the pre-Riffian zone of the northern R'arb, Morocco.—P. H. **Fritel**: The discovery in Senegal of two fossil fruits belonging to the genera Kigelia and Nipadites. —M. **Romieu**: The eleocytes of *Perinereis cultrifera* (Grube).—R. **Herpin**: The origin and the *rôle* of the reserve cells of the general cavity in *Perinereis cultrifera* and *P. Marioni*, and the early differentiation of their eggs. The reserve cells probably arise from the lymphocytes, and have for their principal function the nourishment of the sexual products in course of elaboration. The development of the eggs is extremely slow, and requires more than a year.—M. **Mirande**: The extraction and the nature of the substance producing sulphuretted hydrogen in the seeds of certain Papilionaceæ. A protein has been isolated from the seeds of *Lathyrus sativus*. Heated with water to a temperature of about 40° C., there is a spontaneous development of hydrogen sulphide; after this reaction is complete the residue still contains sulphur.—Mme. Z. **Gruzewska** and M. **Fauré-Frémiet**: The localisation of the glycogen in the liver and the muscles of dogs fed with a view to the maximum production of this reserve.—E. **Grynfeltt** and Mlle. R. **Lafont**: Experimental porphyrinuria. Lesions of the kidney of the rabbit produced by sulphonal intoxication.—MM. **Desgrez**, **Bierry**, and **Rathery**: Some modifications of the blood-plasma and of the urine during fasting in diabetic subjects.—P. **Masson**: The nervous lesions in chronic appendicitis.

Books Received.

Public Library, Museum, and Art Gallery of South Australia. Records of the South Australian Museum. Vol. ii., No. 1. Pp. 208. (Adelaide.)

Fisheries: England and Wales. Ministry of Agriculture and Fisheries: Fishery Investigations. Series 1: Fresh-water Fisheries and Miscellaneous. Vol. ii., No. 1: The Methods of Fish Canning in England. Pp. 25. (London: H.M. Stationery Office.) 2*s*. 6*d*. net.

Rapport sur une Expédition d'Aurores Boréales à Bossekop et Store Korsnes pendant le Printemps de l'Année 1913. By Carl Størmer. (Geofysiske Publikationer, vol. i., No. 5.) Pp. 269+civ plates. (Kristiania: Grøndahl & Søns.) 15.00 kroner.

A French-English Dictionary for Chemists. By Dr. A. M. Patterson. Pp. xvii+384. (New York: J. Wiley and Sons, Inc.; London: Chapman and Hall, Ltd.) 18*s*. net.

Travels of a Consular Officer in North-West China. By E. Teichman. Pp. xiv+219+lviii plates. (Cambridge: At the University Press.) 25*s*. net.

A Critical Revision of the Genus Eucalyptus. By J. H. Maiden. Vol. v., part 6. (Part xlvi. of the complete work.) Pp. iv+161–85+plates 188–91. (Sydney: W. A. Gullick.) 3*s*. 6*d*.

Proceedings of the Royal Society of Victoria. Vol. xxxiii. (New Series). Pp. iv+285+xi plates. (Melbourne.)

A Philosopher with Nature. By Benjamin Kidd. Pp vii+211. (London: Methuen and Co., Ltd.) 6*s*. net.

The North of Scotland College of Agriculture. Guide to Experiments at Craibstone, 1921. Pp. 45. (Aberdeen: North of Scotland College of Agriculture.)

Lawes Agricultural Trust: Rothamsted Experimental Station, Harpenden. Report 1918–20, with the Supplement to the "Guide to the Experimental Plots," containing the Yields per Acre, etc. Pp. 86. (Harpenden.) 2*s*. 6*d*.

The Direction of Human Evolution. By Prof. E. G. Conklin. Pp. xiii+247 (London: Oxford University Press.) 12*s*. 6*d*. net.

Des Fondements de la Géométrie. By Henri Poincaré. (Bibliothèque de Synthèse scientifique.) Pp. 65. (Paris: E. Chiron.) 3 francs.

Mikroskopische Physiographie der Petrographisch Wichtigen Mineralien. By H. Rosenbusch. Band I., Erste Hälfte. Untersuchungsmethoden. Fünfte völlig Umgestaltete Auflage, von Prof. E. A. Wülfing. 1 Lieferung. Pp. xvi+252. (Stuttgart: E. Schweizerbart.) 16*s*.

L'Evolution Universelle: Exposé des Preuves et des Lois de l'Evolution Mondiale et des Evolutions particulières (Inorganique, Organique, Intellectuelle et Sociale); l'Evolution Mondiale, Inorganique et Organique. By Prof. B. Petronievics. Pp. viii+212. (Paris: F. Alcan.) 7.50 francs.

Theoretical Mechanics: An Introductory Treatise on the Principles of Dynamics. By Prof. A. E. H. Love. Third edition. Pp. xv+310. (Cambridge: At the University Press.) 30*s*. net.

The Geography of Illinois. By D. C. Ridgley. (Regional Geographies of the U.S.A.) Pp. xvii+385. (Chicago: University of Chicago Press; London: Cambridge University Press.) 16*s*. 6*d*. net.

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