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The Progress of State Afforestation.

THE Geddes Committee recommended the abolition of the Forestry Commission, and the discontinuance of the scheme of State afforestation that was sanctioned by Parliament in 1919. Fortunately these drastic measures were not adopted by the Government. The Treasury, however, has now restricted the Commissioners' operations, by reducing considerably the annual instalment from the forestry fund, which had been fixed at 350,000*l*. In consequence, the forestry staff has been greatly reduced, all purchase of land for the purpose of afforestation is suspended, and planting operations are greatly curtailed. It is discouraging to be aware of these facts, while reading the second annual report [1] of the Forestry Commissioners, which is a record of continuous progress till the end of September 1921.

The report shows unexpected ease in the acquisition of suitable land for planting trees. In order to reduce current expenditure to a minimum, the policy has been pursued of leasing as much and buying as little land as possible. In September 1921 the Commissioners were actually in possession of 68,489 acres of "plantable land," of which two-thirds had been leased at a rent of about 2*s*. per acre, and one-third purchased at the low price of 1*l*. 8*s*. per acre. The afforestation of cheap land like this adds materially to the real wealth of the country, as the timber produced will be much more valuable than the poor grass, rushes, bracken, furze, and heather which now cover the ground.

Afforestation also provides a ready means of giving work to the unemployed during seasons of bad trade. In November last, 250,000*l*. was allotted to forestry from the Unemployment Fund; and in spite of the difficulty of organising and in many cases improvising forestry operations with unskilled labour, more than 4000 men were set to work. Landowners and corporations were induced to plant, by small grants which were unencumbered by any condition except that unemployed labour should be utilised. As a result about 11,000 acres were planted on private estates, and preparation was made for the planting of a further 11,500 acres in subsequent years, a notable addition to the woodland area of Great Britain.

The actual work of afforestation on the lands acquired by the Commissioners was restricted in the first year by the lack of young trees; but 7794 acres had been planted by the end of the second season. Large quantities of tree-seeds were imported and extensive nurseries established at convenient centres, the latter covering in September 1921 an area of 607 acres and carrying a stock of 111 million seedlings and 33 million

[1] Second Annual Report of the Forestry Commissioners: Year ending Sept. 30, 1921. (London: H.M. Stationery Office, 1922.) 1*s*. net.

transplants. The report gives detailed information in tabular form about the area, cost, and species of the various plantations and nurseries. The positions of the different State Forests and Crown Woods are indicated in a sketch map.

Four schools for apprentice woodmen have been established, where sixty men received training in 1920–1921. These are situated at Parkend in the Forest of Dean, Burley in the New Forest, Chopwell in Durham, and Beauly in Inverness-shire. Research was carried on by six members of the Commissioners' staff; experiments were made on the germination of seeds; on the protection of seed-beds from drought, frost, and weeds; on insect pests; and on planting procedure. Seventy-nine sample plots, scattered over England, Wales, and Scotland, were under observation in September 1921. These plots will be thinned and measured periodically, in order to provide data as to the rate of growth and the best methods of thinning plantations of the different species.

The report concludes with an interesting account of the drought of 1921. It caused great damage in England and Wales, the death-rate among newly planted trees being 35 per cent. It is satisfactory to note that the Commission's plantations formed in the previous year did not suffer to any appreciable extent.

The Green Flash at Sunset.

The "Green Ray" or "Green Flash" (Rayon Vert) at Rising and Setting of the Sun. By Prof. Dr. M. E. Mulder. Pp. 141. (London: T. Fisher Unwin, Ltd., 1922.) 6*s.* net.

WHEN the sun sets behind a distant and clear horizon, its last rays disappear with an emerald green flash. The coloration is due to the refraction of light in our atmosphere by which the sun's image is raised through about half a degree, the elevation increasing from the red to the violet end of the spectrum. As the violet and—to some extent—the blue rays are absorbed by the layer of air through which the light has to pass, it is the bluish-green part of the spectrum that is dominant at the ultimate moment of sunset. This seems clear enough and even obvious. But there are always certain minds that distrust the obvious—not always to the disadvantage of science—and others which rebel against a commonplace explanation of a striking effect. Imagination is always ready to supply more or less fanciful alternatives leading to controversies and correspondence in scientific journals.

In this manner a considerable amount of literature on the "green flash" has accumulated, and this is now collected by Prof. Mulder in a volume of 140 pages. The book is readable and interesting. But the interest is mainly psychological, depending on the descriptions by which observers record their impressions and on their knowledge of the conditions under which the green flash appears. The two serious alternative explanations that have been offered to replace the one based on the dispersion of light in our atmosphere might be dismissed in one sentence. The green flash cannot be the after-image in an eye fatigued by the red light of the sun, because it appears at sunrise, as well as at sunset; nor can it derive its colour from actual passage through the sea, because it is also seen when the sun disappears behind a land horizon.

The author aims at giving us a complete account not only of everything that *can* be said on the subject, but of everything that *has* been said on it. We are told how observers have put their impressions into words and find transcriptions of a large part of the correspondence that has appeared in NATURE, in the Journal of the British Astronomical Association, in the *Meteorologische Zeitschrift*, and in other publications. The same arguments are repeated over and over again, until we feel that a horse dead and duly flogged had better be buried; this might save us from being worried by its ghosts and reincarnations.

Nevertheless, the account has its value as a chapter of scientific history. We note with interest that the first printed description of the green flash that can be traced is contained in a novel by Jules Verne entitled, "Le Rayon Vert," and published in 1882. Perhaps some readers of NATURE can verify the *Leit-Motif* of the story, taken apparently from a Scotch legend, according to which those who have once seen the green ray acquire the power of seeing what is in the hearts of others as well as in their own.

I first noticed the green flash in February 1875 on several successive mornings at sunrise while traversing the Indian ocean. The appearance was so striking and the explanation seemed so obvious that I took it for granted I had only witnessed a common and well-known phenomenon. In July 1878 during a passage to the United States I directed the attention of several members of an eclipse expedition to the appearance at sunset; among them was Mr. Cowper Ranyard, whose subsequent views on the subject are quoted with approval by Prof. Mulder. I still failed to realise that the effect had never received the attention of scientific men, though I understand that astronomers were familiar with the fact that the light of a star near the horizon is drawn out into a vertical spectrum.

If the author's investigation of the scientific literature is as exhaustive as it appears to be, the first scientific notice of the green flash was published only in 1885, when Chevreuil communicated to the French Academy observations made in the Indian Ocean by Trèves, who adopted the erroneous theory of complementary colours. The reader must be referred to the book itself for the subsequent discussion. Some praise should be given to the author for the manner in which he keeps up the interest of his account and the fairness with which he recites the arguments of different writers. According to his own view a complete explanation is still wanting, although he agrees that it must be based on the dispersion theory. But I would suggest that his reservations depend almost entirely on the importance he attaches to discrepancies in the descriptions by different observers and in their estimates of the duration of the flash. Apart from real differences in atmospheric conditions that may be very considerable, it is not to be expected that men, not specially trained in such observations, could tell with any degree of certainty whether an outburst of light lasts a tenth of a second or two seconds. Some have described the flash as appearing in the form of a short line, while on others it has left no impression of shape. The author, who has been a professor of ophthalmology, is not likely to have forgotten the possible effects of astigmatism, but even a perfect eye might see a point of light drawn out into a vertical line if the eyelids have been partially closed to screen them from the glare of direct sunlight. When seen through a telescope the appearance seems to be much more regular, the green coloration first appearing at the corners of the cusp that remains above the horizon. There seems no reason to doubt that dispersion combined with absorption of light completely accounts for the effect.

Is it not time that the green flash should find its place in elementary text-books? It is eminently suitable for them, and only by this means shall we be saved from further discussions covering the same ground.

ARTHUR SCHUSTER.

Village Communities.

The English Village: The Origin and Decay of its Community. An Anthropological Interpretation. By Harold Peake. Pp. 251. (London: Benn Bros., Ltd., 1922.) 15*s*. net.

SEEBOHM in 1883 issued his well-known work on the English village community, which he examined in its relation especially to the manorial system and to common field husbandry. Among the general conclusions of his work was the view that "neither the village nor the tribal community seems to have been introduced into Britain during a historical period reaching back for 2000 years at least; . . . the village community of the eastern districts of Britain was connected with a settled agriculture which, apparently dating earlier than the Roman invasion and improved during the Roman occupation, was carried on, at length, under the three-field form of the open-field system which became the shell of the English village community." Without following out the discussion of Seebohm's views it may be said that the accumulation of archæological evidence since his day has made far more probable his view that there were agricultural settlements on cleared forest lands in Britain well before Roman times. The mapping of the catalogued Iron-Age finds from the lists given in the report on the Glastonbury Lake Village would furnish presumptive evidence on this point. It is, however, clear that Seebohm attributed great importance to Roman influence, which he says "enforced the settlement and introduced . . . fixed rotation of crops" "within the old Roman provinces (N. of the Alps) and in the Suevic districts along their borders," the area of "the geographical distribution of the three-field system."

In Seebohm's work there are frequent indications of his feeling out towards what was then the almost uncharted background of pre-history. It is the great merit of Mr. Peake's work that he has used his rich archæological knowledge as well as his historical reading in order to reach back beyond Seebohm. His interest is not in any question of origins of manorial organisation, but rather in the attempt to make the much-needed link between archæology and documentary studies for Britain. That this is one of the prime needs of our time admits of no question, and it is advisable that specialists on both sides should treat with special consideration pioneers who, like Mr. Peake, are trying to find the much-needed links.

For Mr. Peake the germ of the village community is to be found among the Neolithic agriculturists of the Swiss and Alemannic and Bavarian regions north of the Alps and is a social characteristic of the dark, broad-headed Alpine Race in those areas. He also suggests their domination by Nordic men of the Bronze Sword, but leaves the fuller working out of this subject to a companion-book to be issued shortly. These warriors set out about 1200 B.C. to dominate a large part of Europe and reached Britain within less than a century, as there is but very little difference in the types of sword found along their routes. Their followers were cultivators, and from evidence of bronze-sickles, of ploughs as substitutes for hoes, and of bronze axes in plenty, some late ones of which undoubtedly tell of forest-clearing, Mr. Peake believes they spread, at any rate, the germs of the village community in cleared forest areas. The landing of these people at Chelsea and Brentford as well as up the East Anglian Ouse gives a clue to their access to the parts of England where the three-field system is best shown. That they did not at once dominate the west is clear from archæological evidence, especially from the finding of rapier-like dirks in place of leaf-shaped swords in the south and south-west. In Wales and other hill-lands the moorland-village seems to have survived, and, when valley clearing spread there, family groups in single households moved downhill and built the Tyddyn near shelves of cultivation on the hill-sides. Mr. Peake thinks the Romans met the valley-village-community in Gaul and Alemannia and probably interfered with it as little as possible, so that according to him, the continuity from earlier times would be much greater than Seebohm thought. This is a view which obviously needs further examination on the basis of study of geographical distributions and we specially need maps of the distribution of open-field villages in France. Mr. Peake's view is, at any rate, far more helpful than the one which would ascribe the three-field system to the Saxon invaders of Britain, for these last came from a region where the one-field scheme was characteristic and they had little connexion with the Alemannic areas of distribution of the three-field system.

Having outlined this interesting opinion Mr. Peake follows, on fairly orthodox lines, the open-field valley-village down to its decay. He accepts the general view that diminution of fertility of the land was a factor of this decay, though that is now disputed and it is thought that the properly-organised folding of stock pastured on uncultivated lands would bring in enough manure. That this organisation was adequately maintained everywhere in view of competing claims for the manure between lord and tenant is, however, very doubtful, so the view in this book is probably not very far from correct.

Mr. Peake's years of public work in rural England give a special interest to his concluding chapter, which asks, what of the future? He sees that the old village is dead or dying from loss of internal cohesion and that there is too much tendency towards occupational as against neighbourly cohesion. He also fears the further urbanising of the people if the Garden City idea, which he admits to be the best urbanism, spreads. He thinks the Saxon village may have had less than 100 people, the mediæval village perhaps nearly 200, the modern survival about 200, more or less. As civilisation developed in classical lands villages fused, and there is, according to Mr. Peake, much need of larger units, especially for shopping and amenity purposes. The village has been losing its people, especially the best, at an alarming rate, but the tide of numbers turned a little after 1900 in several districts, thanks probably to the motor car and cycle. To redeem village life from dulness Mr. Peake thinks a population of about 1000 would be desirable, and that in such a unit most standard occupations could be represented, an important factor of contentment now commerce has enlarged our needs. He pleads for farm buildings around the outskirts of the village so that the labourer may be near his beasts, and for small holdings in the outer ring of the village. A village of 1000 can have a doctor, a lawyer, a bank, a bootmaker, a builder, a carpenter, a reasonable school, a public hall, and a few shops of some value. Such villages would encourage retired people, maiden ladies without specialised occupation, and so on to settle in them, and might well lead to a redevelopment of handicraft at all events in leisure time.

The detailed suggestions may raise dispute, but what is of value here, besides the long and intimate working-experience of rural life which the author possesses, is the fact that the present unfortunate tendency towards separatist specialism is avoided. It is seen that small holdings *per se* are not enough, and that the settling of wage-rates or drafting of housing schemes is only a partial help. It is Mr. Peake's desire to start from life, and from the provision of opportunities of healthy exercise of varied faculties, that marks out his book as worthy of careful and earnest consideration.

H. J. F.

Climbing Palms and the Sago Palms.

Annals of the Royal Botanic Garden, Calcutta. Vol. 12. Part 2: *Asiatic Palms—Lepiodocaryeæ.* Part 3: *The Species of the Genera: Ceratolobus, Calospatha, Plectocomia, Plectocomiopsis, Myrialepis, Zalacca, Pigafetta, Korthalsia, Metroxylon, Eugeissona.* By Dr. Odoardo Beccari. Text, pp. vi+231+6 plates. Plates, 120, size 21 in. × 14 in. (Calcutta: Bengal Secretariat Book Depot, 1918–1921.) Rupees 40; 3*l.*

IT is a matter of great regret that the late Prof. Beccari did not live to see the publication of the final part of his fine memoir on the Asiatic Lepidocaryeæ, which he had very fittingly dedicated to the memory of the late Sir George King, the founder of the Annals of the Calcutta Botanic Garden.

It was characteristic of Sir George that he selected the proper people to prepare the valuable memoirs that have preceded the one under review, though his own contributions are among the most noteworthy of the series. This present part constitutes the third of Prof. Beccari's memoirs on the Lepidocaryeæ, vol. xi. having been devoted to the important genus Calamus, and vol. xii., part 1, to the genus Dæmonorops. Like its predecessors, the present part is accompanied by a magnificent series of plates and analytical figures of the flowers and fruits of the various species, which very materially enhance the value of the memoir.

The genus Ceratolobus, which is confined to the Malay Peninsula, Sumatra, Java and Borneo, consists of six species, four of which are described by Beccari. Three of these four are found in Borneo and two in the Malay Peninsula—one of the later, *C. lævigatus*, Becc. having six varietal forms spread over the Malay Peninsula, Sumatra and Borneo. *Calospatha Scortechinii*, collected by that assiduous botanist, Father Scortechini, in Perak, and described by Beccari, comes next on the list and is an interesting and very distinct palm, having a homogeneous albumen in the seeds, two to three in a fruit. In the genus Plectocomia, which are large, spinous, calamoid palms with terminal inflorescences, six new species are described. One of these, *P. Kerrana*, was discovered at Doi Soetep, near Chiengmai, Siam, by Dr. Kerr, who has made known to us the riches of the Siamese flora by his admirable collections in that country.

Plectocomiopsis is a new genus described by Beccari, containing five species, from Lower Burma and throughout the Malay Peninsula and Sumatra, and is again a calamoid, diœcious genus allied to Myrialepis, Becc. In its vegetative organs it resembles Plectocomia, though it differs widely in the spadices and flowers. Beccari then passes to the genus Zalacca, a characteristic and exclusively Indo-Malayan group containing thirteen species, denizens of rich, deep and moist soil in the recesses of primæval forests. The home of *Z. edulis*, which is frequently cultivated for its edible acid fruits, is not definitely known, but it is considered to be a native of the Malay Islands. It has been known since the time of Clusius, who examined fruits of this palm sent over from Bali in brine about 1600. Two new varieties of this species are described, and six new species by Beccari; of the latter, *Z. dubia* is only certainly known from the male flowers.

The genus Pigafetta described by Beccari was regarded by Martius as a section of Metroxylon, the sago palm, but Beccari proves that it is a distinct genus by its polycarpic nature. It is not closely allied to any other genus of Lepidocaryeæ, and is a tall tree with diœcious, axillary spadices and small calamoid fruits. It is due to these small fruits, no doubt, which are eaten by birds, that this palm, *P. filaris*, has so wide a range in the Moluccas, Celebes, New Guinea and Indo-China.

The next genus dealt with, Korthalsia, is again a genus of climbing palms containing twenty-six species, mainly Malayan. They are of economic use as, being very tough, the naked canes are used for tying, etc., by the natives; they are also of interest in being myrmecophilous, with the appendage at the mouth of the leaf sheath (the ocrea) largely developed and sometimes transformed into a closed ant nidus. Extra-floral nectaries are also developed in the axillæ of the leaflets.

Metroxylon, the sago palm, and Eugeissona, one species of which, *E. utilis*, Becc., also yields a sago flour, complete the Asiatic Lepidocaryeæ, and the part is concluded with an enumeration of the extra Asiatic palms belonging to this family, among which Raphia and Mauritia are perhaps the best-known genera.

The genus Metroxylon contains six species with numerous varieties, and the account of these valuable palms occupies nearly forty pages of the memoir. They are arborescent palms with a terminal inflorescence, and the two best-known species are *M. Rumphii* and *M. Sagus*, which are widely cultivated in the Moluccas, for the sake of the starch or sago flour extracted from the stem. Both these palms yield many other commodities used by the natives. Prof. Beccari considers the Moluccas to be the home of these two species, and especially the island of Ceram, where a new species, *M. squarrosum*, Becc., has been found in abundance at the east end. Owing to their importance in affording food to the natives, they have now been carried far and wide.

To appreciate the value of this work it must be studied in detail, and all students of palms and botanists generally will realise how much they have lost by the death of Prof. Beccari, who was the pre-eminent authority on palms. A. W. H.

The Control of Electric Power.

Switching Equipment for Power Control. By S. Q. Hayes. Pp. vii+463. (New York and London: McGraw-Hill Book Co., Inc., 1921.) 20*s*.

SOME of the most important problems with which the engineer of a large power station has to deal are in connexion with the switches and control apparatus of his distribution system. Information on this subject can be found in a very condensed form in several text-books, but there is a demand for more detailed information, and in particular there is a great demand for a definite statement of the physical principles on which many of these devices are supposed to act. In Mr. S. Q. Hayes' book detailed information is given of many types of switchgear of American manufacture, but as a rule the descriptions are similar to the descriptions we get in manufacturers' catalogues, and in some cases they are actually taken directly from these catalogues. The author is one of the leading experts on switchgear, and occasionally the reader is gratified by a brief description of the theory on which the device is founded, but in many cases no hint is given.

In our opinion the value of the book would be very greatly increased by additional brief descriptions of the physical laws which govern the action of many of the devices used by engineers. For example, a table of the rating factors by which the voltage of a given circuit breaker must be multiplied so as to get its rating at various heights above the sea level is given. It would be useful to give the theory used by the General Electric Company of America in getting these numbers.

Many types of the spark gaps used in practice are given, and it is pointed out that the sphere gap has a greater speed of discharge than the horn gap. The "impulse gap," which we believe was perfected during the war in connexion with the spark gap used in the magneto circuit of an aeroplane, is now adopted for lightning arresters. The Westinghouse Company state that it is more efficient than any other spark gap. An investigation of the action of the lightning arresters described would be a very promising field of research for the pure physicist.

In reading this book one gets accustomed to the American words "resistor" and "reactor" which are used for "resistance" and "reactance coil" respectively, and these words might well be adopted in this country. Electrical engineers talked about "omnibus bars" thirty years ago, it then became "bus bars," and now apparently it has become "busses." This book will be useful to the switchgear expert.

A. R.

A Modern Text-book of Chemistry.

Inorganic Chemistry. By Prof. T. M. Lowry. Pp. xi+943. (London: Macmillan and Co., Ltd., 1922.) 28*s.* net.

FIFTY years ago there were no books on physical chemistry. The work which had been done on the borderline of physics and chemistry was scattered in different journals and was not readily accessible to the student. The first volume of Miller's "Chemistry," Tilden's "Chemical Philosophy," and certain articles of the old Watts's "Dictionary" were the first available summaries of what is now one of the most important branches of the subject. The works of Lothar Meyer and Ostwald, published in the late 'seventies of the last century, did much to direct attention to the importance of physical chemistry. The first professor of physical chemistry was appointed only twenty-five years ago, and even now this branch of the subject is still allotted to a lecturer at some of the universities in this country.

It is now recognised that inorganic and organic chemistry will become a mere record of facts, the interpretation of which, without the aid of physical chemistry, will remain undisclosed. It is therefore a matter of congratulation to the publishers that they should have been able to arrange for the publication of a book on inorganic chemistry by a physical chemist. The author is also to be congratulated on the way in which he has fulfilled his task. Perhaps the severest test of such a book is to refer to all the parts of the work which one knows are stumbling-blocks to the ordinary intelligent student. Prof. Lowry's book stands this test remarkably well; in one case alone, the liquefaction of gases, will the information need to be supplemented by the teacher. If it had been possible to give the references to original papers the book would have sufficed for any chemist who was not intending to devote himself to inorganic chemistry as his main subject, although the author in his preface seems to disclaim the use of his book as a book of reference.

The book is admirably produced, and the illustrations are remarkable, no less for their number than for their clearness. The book may be heartily recommended.

H. B. BAKER.

Our Bookshelf.

Drahtloser Übersee-Verkehr. Von Dr. Gustav Eichhorn. Pp. 69+xx. (Zürich: Beer et Cie, 1921.) 7 francs.

IN the first two chapters of the publication under notice, an excellent description is given of the great German radio-station at Nauen and of the receiving station at Geltow, twenty miles south of it. The third chapter discusses the theory of thermionic tubes, and the method of indicating the paths of the various currents by marking them in different colours is to be commended. The last chapter on radio-telephony is concerned mainly with modern German practice. In the Appendix a few well-known papers by Howe, Vallauri, etc., are published. The book is clearly printed, and the photographs of the Nauen station and the great lattice towers with their networks of wires show on what a huge scale it is designed.

During the last few years the station has been practically redesigned. The standard system of transmission does not yet seem to have developed. For example, they are at present constructing seven new towers, each 210 metres high, to enable them to communicate with Argentina. Under favourable atmospheric conditions the Telefunken Co., who own the station, have maintained communication with Japan for several years, although the messages have to go overland across Europe and Asia. The antennæ can be separated into four separate sections, each of which can be attached to a separate transmitter. When weather conditions are adverse all the antennæ can be connected in parallel. They then have a joint capacity of 0·01 microfarad. The two largest sections are each connected with 400 kw. high-frequency alternators through two or three frequency doublers connected in cascade. The smaller sections are connected to smaller machines. Although the antennæ are close they can be operated quite independently of one another.

Dr. Meissner, the engineer-in-chief, hopes to reduce the earth-resistance of the antennæ to a fraction of its present value. How much this resistance lowers the efficiency can readily be seen by the figures given in this book. For example, in one case the effective current in the antennæ is given as 360 amperes and its resistance is 2·7 ohms. We learn that radio-telephonic systems are now established between Munich and Frankfort and between Berlin and Hamburg.

Phytopaläontologie und Geologie. Von Prof. Dr. W. Deecke. Pp. iii + 97. (Berlin: Gebrüder Borntraeger, 1922). 6*s.* 3*d.*

PROF. DEECKE's essays on broad questions of geology always provide interesting reading. The present work is perhaps unduly sceptical; but its stimulus to further comparison and correlation is based on careful reasoning. While mention is made of the importance of plants as rock-formers, the main thesis is their value for geologists as indicating topographic and climatic conditions in the past. The author shows how vegetation growing on cold uplands may become entombed in the downwash from mountain-sides, and he strongly opposes the notion that the flora of a sheltered Miocene marsh at Œningen may be used as an illustration of the contemporaneous flora on the Swabian Alb. Even the beautiful theory that the occurrence of rings of growth in fossil trees indicates an orderly recurrence of seasons, while their absence indicates a uniform climate, comes in for useful criticism. Though the author states the importance of calcareous algæ in forming Carboniferous limestones and, aided by their magnesium, Triassic dolomites, we miss a reference to the Cryptozoon question. This is a mere petrographic detail in the general discussion, which leaves us with the impression that geology, including the determination of local conditions of plant-growth, may be of more service to palæophylology than phytopalæontology can be to geology. G. A. J. C.

Practical Mathematics. By A. Dakin. Part 1. (Mathematical Series for Schools and Colleges.) Pp. viii + 362 + 12 + xxiv. (London: G. Bell and Sons, Ltd., 1921.) 5*s.*

THERE should be a considerable demand for Mr. Dakin's book, as it contains just the sort of mathematics that is required by those who have to learn some elementary mathematical processes for practical use: decimals, mensuration and a few other topics in arithmetic, algebraic formulæ and equations, graphical methods, the geometry of rectilinear figures, similar figures, the circle and the sphere, with some numerical trigonometry. The treatment is very pleasant, and the student who uses the book will certainly fail to experience the aridity that the popular mind associates with mathematics. Mr. Dakin's account of graphs is particularly good; the introductory portion with the comparison and correlation graphs cannot but grip the student's interest, and make him feel that the method of graphs is worth acquiring. Historical notes are incorporated in the main text, and occasionally they are worked in very skilfully. Presumably the tables are given the title "logarithmic tables" from force of habit: they contain only natural trigonometrical ratios.

If the second part maintains the high standard of the present volume, the author will have added a valuable treatise to available books on the subject. It is to be hoped it will not suffer the fate of so many sequels. S. B.

Cours complet de mathématiques spéciales. Par Prof. J. Haag. Tome 2, Géométrie. Pp. viii + 661. (Paris: Gauthier-Villars et Cie, 1921.) 65 francs.

THIS is the second part of Prof. Haag's complete treatise on pure mathematics as required by the ordinary student specialising in mathematics. The first part dealt with algebra and analysis: the present volume is geometrical in the widest sense. We thus have analytical and synthetic geometry in two and in three dimensions, all treated simultaneously. A correct description of the book is therefore to call it a compendium of modern methods in geometry; it contains a vast amount of information of a fundamental character, and makes excellent reading.

Contrary to usual practice, especially in this country, the author does not devote very much space to conics as such. Perhaps he is right in thinking that the general practice of making a long and detailed study of the curves of the second degree tends to endow them with an importance that their practical usefulness does not justify. On the other hand the methods of the calculus are used freely.

Exercises in illustration of the principles and methods are conspicuously scarce, and no examples are given for the student to work. One is led to wonder whether a student can derive any considerable benefit from reading mathematics like a novel.

The Foundations of Æsthetics. By C. K. Ogden, I. A. Richards, and James Wood. Pp. 95 + pl. I-XV. (London: G. Allen and Unwin, Ltd., 1922.) 7*s.* 6*d.* net.

THE aim of the authors of this short treatise on æsthetics, as stated by themselves, is to present in a condensed form the greater part of accredited opinion on the subject while relating it to the main positions of the theory of art criticism. The various theories are not brought into opposition, but are distinguished to allow to each its separate sphere of validity. Beauty is thus discussed as intrinsic, in relation to the medium, to mysticism, and to its social effects and the like. They themselves find the solution of the problem in synæsthesis, a term covering a state of equilibrium and harmony in which the percipient becomes more fully himself and at the same time is in sympathetic understanding with other personalities. Hence arises the educative value of art. This theory is acceptable so far as it goes, but, like much of the current theory of æsthetics, in describing the "how" it fails to answer the question "why," a matter in which the anthropologist, censured by the authors, may be able to assist, in view of the current vogue of non-European art.

Cancer of the Breast and its Treatment. By Prof. W. Sampson Handley. Second edition. Pp. xvii + 411. (London: Published for the Middlesex Hospital Press by J. Murray, 1922.) 30*s.* net.

IT is now more than six years since the exhaustion of the first edition of this book, in which Sampson Handley set out to place the operative treatment of cancer of the breast on a more rational basis by a closer study of the pathology of the disease. His main conclusions were: (1) that carcinoma spreads centrifugally by permeation of the lymphatic plexuses; (2) that reparative processes inadequate for cure are a normal part of the cancer process; (3) that inflammation and fibrous tissue formation are the principal of these defensive processes; (4) that invasion of the serous cavities is an event of critical importance in the process of dissemination; (5) that the embolic theory is only true for exceptional cases. The author instances much detailed evidence in support of these views, which have won widespread, though not universal, acceptance.

The present edition contains new chapters on radiological treatment, recurrence, Paget's disease of the nipple, lymphangioplasty, and injury as a causative factor in carcinoma. The book is well arranged and excellently illustrated.

A Handbook of Some South Indian Grasses. By Rai Bahadar K. Ranga Achariyar. Assisted by C. Tadulinga Mudaliyar. Pp. vi + 318 (Calcutta: Butterworth and Co., Ltd.; London: Constable and Co., Ltd., 1921.) 4 R. 8 As.

THIS book is intended to serve as a guide to the study of the grasses of the plains of South India for the use of officers of the Agricultural and Forest Departments and others interested in grasses. To remedy scarcity of fodder, foreign grasses and fodder plants have been imported, but so far none have been established on a large scale. The same amount of attention bestowed on indigenous grasses would have yielded better results. About one hundred grasses of wide distribution in the South Indian plains are described in this volume. The arrangement adopted is that of the "Flora of British India." Keys for the identification of genera and species are given, and good descriptions of each species are accompanied with figures of the whole plant and of the spikelet and details of the flower. The descriptions are preceded by a useful general account of the vegetative organs and flowers, and the histology of the stem and leaf. The figures, though not always quite sharp, are sufficiently clear to be a great help towards the identification of a given specimen. The handbook should prove of good service in South India.

The World About Us: A Study in Geographical Environment. By O. J. R. Howarth. Pp. 94. (London: Oxford University Press, 1922.) 2*s.* 6*d.* net.

MR. HOWARTH has written a small book on a most important subject. Its size is the only fault we have to find with this excellent volume, although it is a pity that a title more descriptive of the content was not chosen. Enthusiasm for the geographical point of view too often leads to exaggerated statements of the influence of environment on human activities and is prone to encourage generalisations which not infrequently ignore the facts. Mr. Howarth is too careful a geographer to fall into these bad ways. He traces the nature of geographical influences, and in selected cases tries to estimate the forces of the factors involved. Chapters on the factors of environment are followed by others on distribution, migration, and transport. Particularly suggestive are the chapters on geographical environment and political states, and the local application of environmental study. The latter expounds the idea of regional survey in its value as a co-ordinating study of the things and peoples around us. Mr. Howarth's thoughtful and lucidly written volume should help geography to find its proper place in educational schemes. It deserves to be read widely.

Eyes and Spectacles. By Dr. M. von Rohr. Rendered into English by Dr. A. Harold Levy. Authorised translation. Pp. vi + 130 + xxii. (London: Hatton Press, Ltd., n.d.) 6*s.* net.

WE believe that this little book by Dr. Moritz von Rohr will prove as helpful to other English readers as it has been to the translator. Not only should it be read by those who prescribe and those who make lenses, but also by those who have to teach medical students or others the elementary principles on which the science of ophthalmology is based. The first part of the book deals with the eye itself, and also perspective as a form of perception by means of which the arrangement in space of the outer world becomes manifest to the observer. The most important section of the book deals with spectacles, and stress is laid on the two fundamental problems of increased clearness of vision and the alteration of direction of the object perceived. The final portion of the volume deals with spectacle frames. The translation, which has had the advantage of the author's revision, appears to have been carried out in an efficient manner.

The Link between the Practitioner and the Laboratory: A Guide to the Practitioner in his Relations with the Pathological Laboratory. By C. Fletcher and H. McLean. Pp. 91. (London: H. K. Lewis and Co., Ltd., 1920.) 4*s.* 6*d.* net.

THIS little book is for the guidance of the medical practitioner when he is obtaining the assistance of the laboratory. It enumerates clinical conditions with the appropriate pathological investigations, and gives clear instructions for the collection and transmission of the necessary material; there is also a brief résumé of vaccine and serum therapy. Attention to the details given will certainly assist the practitioner in supplying to the pathologist the right material in the right way.

Émile Coué: The Man and his Work. By Hugh MacNaughten. Pp. xi + 52. (London: Methuen and Co., Ltd., 1922.) 2*s.* net.

AN ambitious title appears on this book, which is really an enthusiastic appreciation of a doctor from whom the writer has received benefit. It records vividly scenes at Nancy, Eton, and London when Coué gave demonstrations of his methods. The writer gives a very charming impression of Coué as a man. The book is not, however, nor does it purport to be, a scientific treatise on Coué's theories of suggestion.

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.*]

Capillarity.

IN Mr. W. B. Hardy's excellent "Historical Notes upon Surface Energy and Forces of Short Range" in NATURE of March 23, vol. 109, p. 375, he remarks that "the exact way in which the attractive forces act in causing the rise of fluid in capillary tubes and the spreading of fluid over solid and fluid surfaces is still obscure." He evidently rejects all explanations by any Laplacean conception of molecular attraction. He probably holds that the explanation is to be sought in the modern electric theory of the constitution of matter, but that this theory has not as yet been developed far enough to throw sufficient light on the question. By the use of the term "attractive," however, he restricts the inquiry to a limited class of forces in terms of which these phenomena are to be explained. It is just possible that this restriction may preclude the solution of the problem.

That it is possible to conceive of a force which cannot be put in this class of "attractive" forces of short range may be shown. If a mass consisting of a single molecule exists at all, it has a position in ether-space at some particular instant. It may be considered isolated from all other masses. There is, therefore, a closed boundary within which there are properties differing from those not within this boundary. Any part of this closed boundary may be conceived as an area between space-regions called "mass" and "no-mass." If we say that *this enveloping area tends to become less*, we have defined a "force" which cannot be included in the class of attractive forces. Further, if we say that, *when two such enclosures come into contact, the tendency to decrease does not exist in the area of contact*, since there is no distinction of properties on opposite sides of this area, we have defined the law of the "force" for like masses. Finally, if we say that *when the two masses are unlike there is this distinction of properties and, consequently, a tendency of the common area to decrease*, we have extended the law to unlike masses. Space will not permit the elaboration of these conceptions.

The question is: Does this "force" as conceived above really exist? Whether it does or not, repeated applications of the law as stated will account for surface tension of visible liquid and solid surfaces in terms of it. It will be admitted that the phenomenon of coalescence of visible spheres of like liquids is a direct application of the law. Also, in his letter on "Cohesion" in NATURE of January 5 (vol. 109, p. 10), the present writer has shown experimentally that visible exterior cohesion and adhesion in solids and liquids may be accounted for by surface tension forces alone, and, consequently, may be explained by molecular (surface) tension. Now these and even capillary-rise and fluid-spreading might conceivably be explained by intermolecular electric forces, though this has not yet been done; but it is difficult to see how the latent heat of a gas can be so accounted for, since in a gas the molecules are too widely separated to admit of short range intermolecular action at all, and the condensation of a gas, by which alone this great amount of heat is made available, is unattended by any chemical change or electrical effect. This latent heat, however, may be fully accounted for by molecular surface tension, as the writer has shown in the *Phil. Mag.* v. 41, p. 877.

Either, then, the latent heat of a gas must be otherwise accounted for, or the existence of molecular surface tension must be admitted. This in turn, it is true, may eventually be explained by the action of electric forces in the interior of the isolated molecule. In the meantime the writer offers the following explanation of capillary-rise and surface-spreading in terms of molecular tension as conceived above.

The following phenomena are illustrative. (*a*) If two free spheres of immiscible liquids be brought into contact, the mass of the one suddenly proceeds completely to envelop the mass of the other. (*b*) If a free liquid sphere and a small solid which does not dissolve in the liquid be brought into contact, one of two actions takes place: either the mass of the liquid forms a closed sheath about the solid, or the mass of the solid *tends* to envelop the liquid, but is prevented by internal cohesion from assuming the necessary form. The liquid then stands out as a curved mound on the solid with a distinct "capillary" angle. With water mercury acts as a solid metal. It is on this differential action of this surface force of water

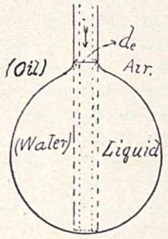

Fig. 1.

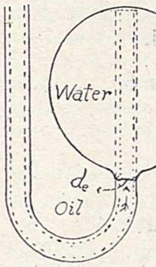

Fig. 2.

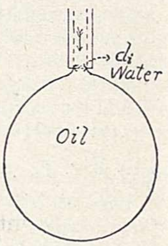

Fig. 3

in union with those of small particles of minerals and rocks that the flotation processes for mineral separation depend.

When a liquid is in contact with a solid, there are three areas in which this *tendency to decrease* exists in different degrees, namely, the liquid-air, the solid-air, and the solid-liquid areas, the sum of the latter two areas being constant. At contact the only possible way in which these latter two tendencies can result in action is to decrease the solid-air area from its maximum by increasing the solid-liquid area from zero. Experiments show (NATURE, *ibid.*) that this change always proceeds to some extent. In the case of water and rock it proceeds until *all* the solid-air area becomes solid-liquid area. It does not reach this limit in the case of water and a mineral.

But the extent of this fluid-spreading depends further on (1) the tendency to decrease of the liquid-air area, unrestricted by internal cohesion, (2) the force of gravity acting on the liquid mass, (3) the form of the solid area, and (4) the amount of the liquid. Thus, a small drop of an oil such as oleic acid may not spread far on a horizontal glass surface, since the decrease in the glass-air area involves increases in both glass-oil and oil-air areas; whereas in capillary-rise this decrease does not involve any increase in oil-air area. The oil will, therefore, ascend until its increasing weight balances the tendency to decrease (tension) in the glass-air area within the tube. Again, a hanging drop of the oil will completely envelop a large fragment of glass and hold it against its weight.

It is plain, then, that methods of measuring surface tensions of liquid-air and liquid-liquid areas should be, so far as possible, free from errors arising from unknown changes in liquid-air areas and from the unknown solid-air and solid-liquid tensions. It will be seen that the capillary-rise method in reality does this, though there is difficulty in measuring the internal bore and keeping it clean. But the following arrangement seems to be as nearly as possible free from these objections, and to be adapted to measure the tensions of interfacial liquid surfaces as well.

The liquid (mercury excepted) is made to drop from a fine capillary tube having thin walls as in Fig. 1. In forming the drop, it spreads upward over the exterior surface and reaches a limiting size, shape, and position. It then slips down the tube at a uniform velocity with little modification in size or shape, and after suffering a slight check in its motion breaks its connexion and falls. We may then equate the whole tension about the tube with the weight of the drop, so that $T_{la} \times \pi d_e = W_L$ or $T_{la} = W_L/\pi d_e$, where T_{la} is the liquid-air tension and d_e is the external diameter of the tube.

The same drop formation occurs when the tube is arranged to drop water downwards in any lighter oil, or by a bent tube (Fig. 2) upwards in a heavier oil. In this case we have the equation $T_{wo} = \frac{W_w}{\pi d_e} \times \frac{\rho_w - \rho_o}{\rho_w}$, where the suffixes w, o, e, and i denote the words water, oil, external, and internal, and ρ is density, from which the tension of the water-oil surface may be calculated.

On the contrary, when oil is dropped either downwards or upwards in water, it does not spread on any exterior water-glass surface, but forms its attachment as in Fig. 3. In this case we have $T_{wo} = \frac{W_o}{\pi d_i} \times \frac{\rho_w \sim \rho_o}{\rho_o}$.

These all give results agreeing with those recorded in the standard tables.

In accordance, then, with this conception of an elemental force not included in the class of "attractive" forces, we should expect that these changes in area would be always attended by a rise in temperature. If it be asked why the enveloping area about a free molecular mass tends to decrease, there is no answer; and neither is there an answer to the question as to why a large mass tends to approach another.

WILSON TAYLOR.

Physics Laboratory,
University of Toronto, Canada, July 15.

The Influence of Science.

THE seeming contradiction in my summary account of the case of Galileo (NATURE, August 5, p. 180), to which Sir Oliver Lodge directs attention in his letter (NATURE, August 26, p. 277), needs an explanation. The great work of Copernicus (1543) was dedicated to a Pope, Paul III.; none of the Roman Congregations found any objection to it, and (Whewell, "History of the Inductive Sciences," I. 418, ed. 1847), says "lectures in support of the heliocentric doctrine were delivered in the ecclesiastical colleges." This was because of its being taught as a purely scientific doctrine.

Sir Oliver writes that Galileo "had endeavoured to get the Church to realise that the doctrine was not really antagonistic to Scripture when reasonably interpreted." This statement needs qualifying. Had Galileo contented himself with teaching the Copernican doctrine as a scientific hypothesis, he would not have been molested. But, being a fierce controversialist, he wanted to confound his many adversaries, the mathematicians, and the Aristotelians, by showing that Scripture was on his side (cp. Sir David Brewster, "Martyrs of Science," p. 58). Hence, disregarding the advice of the Bishop of Fermo "not to raise the question," and that, too, of many other ecclesiastical friends, among them Cardinals and Prelates, to the same effect, he demanded that "the Pope and the Holy Office *should declare the Copernican system to be founded on the Bible*." *Hinc illae lacrymae*. On the other hand, the attitude of the Churchmen is well illustrated in a letter from Cardinal Bellarmine to the Carmelite friar, Foscarini, one of Galileo's friends, dated April 12, 1615. He writes: "If a true demonstration should be found that the Sun is placed at the centre of the world, and the Earth in the third heaven, and that the Sun does not turn round the Earth, but the latter round the former, then it will be necessary to proceed with great prudence in the explanation of Scripture, which seems to say the contrary, and rather to avow that we have not understood it, than to declare a demonstrated fact false." Astronomers had to wait until the discovery of aberration by Bradley before such a true demonstration was found.

Sir Oliver also writes that "Galileo was made to recant, to abjure, and curse the theory of the earth's motion." Whewell tells us (*loc. cit.*, p. 419), "He (Galileo) was accused before the Inquisition in 1615, but at that period the result was that he was merely recommended to confine himself to his mathematical reasonings upon the system, and to abstain from meddling with Scripture." After his contempt of court, in the second trial, of the year 1632, he was condemned as "vehemently suspected of heresy." He was sent to Arcetri, and had to recite a penance of certain prayers.

There was no implication in my former letter, as Sir Oliver writes, "that there was really no punishment, and that there was no call for anxiety and distress." The implication was that his troubles were largely, if not entirely, of his own seeking, and that his treatment was, according to the quotations I gave from Whewell, and from De Morgan, comparatively mild. I purposely quoted from non-Catholic writers, as they cannot be suspected of partiality towards the Roman Congregations. To these I add the testimony of Sir David Brewster (*loc. cit*, p. 88): "During the whole of the trial Galileo was treated with the most marked indulgence." Sir Oliver Lodge's quarrel is, therefore, with such eminent scientific men as Whewell, De Morgan, and Brewster (see also *op. cit*, p. 77), to whom we may add Huxley, who ("Life and Letters," ii. 424) avowed that "the Pope and the Cardinals had rather the best of it."

But my chief implication was, and is, that the case of Galileo cannot fairly be considered as evidence of the hostility of the Church to natural science, and as a hindrance to her legitimate influence. Finally, I trust Sir Oliver Lodge will not think me discourteous in not treating of the other points raised in his letter, as I do not consider them to be relevant to the present discussion.

A. L. CORTIE, *S.J.*

Stonyhurst College Observatory,
August 31.

The Production of a Standard Source of Sound.

I HAVE recently had occasion to consider the problem of constructing a simple standard source of sound, and have been favourably impressed by the possibility of employing a "hot wire" grid—as used in the Tucker microphone (Phil. Trans. A, vol. 221, pp. 389-430)—for this purpose. In this microphone the grid is made of fine platinum wire and heated by a current of 20-30 milliamperes. It is mounted in the orifice of a Helmholtz resonator, and when the latter is stimulated by a sound of suitable pitch, the vibration of the air in the orifice causes an increase in the average rate of loss of heat by convection, and consequently there is a fall in the temperature, and hence in the resistance, of the grid. The fall in resistance can be measured by a Wheatstone bridge method. The way in which it is suggested that this process may be utilised in the construction of a standard source of sound is as follows. The source is made in the form of a cylindrical Helmholtz resonator—say from brass tubing about 2 in. diameter. At one end is fitted a telephone in such a way that the diaphragm forms part of the inner wall of the resonator, while at the other end is the orifice carrying the hot-wire grid. If the telephone diaphragm is made to vibrate by current from a thermionic valve oscillator, and the frequency of the current is adjusted until it is equal or nearly equal to that of the resonator, a pure tone of moderate intensity issues from the orifice. At the same time the hot-wire grid suffers a change of resistance which provides a measure of the amplitude of the vibration in the orifice, and hence of the *strength* of the source. With a standard pattern grid and holder the change in resistance is about 12 ohms when a sound of pitch 200 vibrations/sec. is produced, which is loud enough to be just audible at a distance of 10 feet—the source being placed on the ground in the open. The strength of the source can be varied at will by using a variable series resistance or a shunt with the telephone. The use of resonance serves to purify the note which comes from the telephone, but it is desirable in addition that the oscillator should be of such a type that the telephone note is already fairly pure. The use of a very impure note may lead to poor results, as the resistance change will then depend only partly on the amplitude of the fundamental.

It may be noted that a simple method is known of obtaining the relation between the change in resistance of a grid and the amplitude and frequency of the vibration producing it—the motion being simple harmonic. Hence, if the frequency of the vibration is known, the strength of the source (defined as the rate at which fluid is introduced or abstracted at the source) can be found in absolute measure. The amplitude of the waves—or, if preferred, the flux of energy—in the surrounding medium can then be calculated in certain cases. A simple case is when the source is close to a rigid plane.

Any other means of producing a suitable sound can be used in place of the telephone diaphragm provided it is small enough to go inside a resonator. If the damping factor of the resonator has been determined experimentally, the acoustical output of the primary source inside the resonator can be calculated from the indications of the hot-wire grid in the orifice. The output of the internal source includes (1) the radiation of acoustical energy from the orifice, which has been dealt with above, and (2) work done against viscous forces in the orifice. Unless the orifice is large, (2) is far the more important part, and the radiation losses may be negligible by comparison.

E. T. PARIS.

Signals Experimental Establishment,
Woolwich, S.E.18, Sept. 2.

Occurrence of the Rare Whale, *Mesoplodon Layardi*, on the Tasmanian Coast.

SOME years ago, on July 30, 1918, my friend Mr. G. H. Smith of Leprena, Southern Tasmania, brought me word that a beaked whale, "without teeth," had been cast up on the beach at Recherche Bay near his property, that he had already removed the blubber, and that the carcase still remained on the shore. Being occupied at the time with university teaching I was unable to visit Recherche Bay till some weeks later. High seas were then running, and the remains had been lifted by these and thrown farther up on the basaltic boulders which strew the beach at this spot. This resulted in some considerable damage to the skeleton. Nearly all the ribs were broken, some into three or four pieces, and many of the neural spines had been smashed from the vertebrae. The skull was also damaged, although some of the flesh and integument was still adhering.

The body was naturally in a somewhat decayed and pulpy condition, but with Mr. Smith's help I was able to salve the remains and so obtain the skeleton. This is almost complete and is now in my Department.

My friend was good enough to hand me some measurements and notes which he had made and these I reproduce. The animal "was a female whale. The total length was about 18 ft.; its jaws were 2 ft. 6 in. long, about 4 in. in diameter at the end, no teeth above the gums. Fins 2 ft. long, 8 in. wide, and tapered, not round like the black whale type. Its flukes were about 4 ft. wide; it had the appearance of a fast fish as it was rather thin in the body. There was a small fin on its hump, about a foot high, with a decided rake towards the tail. The colour the same as the sperm whale, dark grey and light underneath." Mr. Smith further states that the blubber yielded 50 gallons of oil of the finest quality, and that he believes that the animal was driven ashore by "killers," of which there were a number in the bay at the time. There were, however, no marks of injury on the body.

The matter of recording this specimen seems called for, particularly in view of the description by Mr. E. R. Waite, director of the South Australian Museum (Rec. S. Aus. Mus., vol. ii., No. 2), of the discovery, on the South Australian coast, of an immature male of this species of Mesoplodon. The Tasmanian specimen was a female and mature, as is witnessed by the condition of the skeleton and by the fact that the pulp cavity of the tooth is entirely closed below.

The form of the tooth corresponds exactly with that figured by Gray for his *Callidon güntheri* (Ann. Mag. Nat. Hist., 1871), which, as Flower and Turner suggested, was a female of the present species. It seems now that we must conclude from Waite's description that the mature condition of the tooth in the female represents a stage which is early passed through in the male.

No pelvic bones were discovered, nor was there any trace of the denticles found in the integument of the jaws of other species. The oil has a density of 0·88 at 12·5° C. This whale has now been recorded from the coast of every Australian State except Victoria and West Australia.

T. THOMSON FLYNN.

University of Tasmania, Hobart, July 20.

Atoms and Electrons.

ON the basis of any theory of atomic structure which classifies the elements according to rare gas type, cerium and thorium should be comparable with one another, since the atoms of each are possessed of four electrons more than those of the corresponding inert gases, xenon and niton respectively. There are, however, in the thorium atom, thirty-two more extra-nuclear electrons than in the cerium atom. In spite of this fact, it appears that *the distances between atomic centres in crystals of these elements are practically the same* (Ce = 3·62 Å.U. and Th = 3·56 Å.U., according to Hull), the distance being, if anything, slightly the smaller for thorium. Both crystallise in the face centred cubic lattice. If the interatomic distances may be taken as representing atomic diameters, this means that in the same (or slightly less) volume there are concentrated in the thorium atom thirty-two more electrons than in the cerium atom, the total numbers in the two cases being, respectively, ninety and fifty-eight.

Thorium is the next to the last element in the periodic table possessed of particular stability. Between the last, uranium, and neodymium in the preceding period, a structural relationship exists similar to that between thorium and cerium. The crystal structures of these elements by the X-ray method have, unfortunately, not been worked out. However, an approximate idea of the relative sizes of the atoms of these substances may be gained by a comparison of their atomic volumes. According to Landolt-Börnstein's "Tabellen," the densities of neodymium and uranium are 6·96 and 18·7 respectively. Dividing the atomic weights (144·3 and 238·5 respectively) by these numbers gives for the atomic volume of neodymium 20·7, and for that of uranium 12·8. The corresponding quantities for cerium and thorium are about 20·5 each. It thus appears that *in the atoms of uranium there are concentrated in about one-half the volume thirty-two more electrons than in the atoms of neodymium*, the total numbers of electrons being, respectively, ninety-two and sixty.

It is perhaps significant in view of these facts that elements of higher atomic number than uranium are not known to exist, and that most of those of immediately lower atomic number are unstable. With increasing nuclear charge the attractive forces exerted by the nucleus on the surrounding electrons concentrate the latter nearer and nearer toward the centre of the atom. It does not appear improbable that the exceedingly powerful forces, both of attraction and repulsion, which must result from this concentration may be of sufficient magnitude to assist materially in bringing about those conditions of instability which result in radio-active disintegration. If the large numbers of electrons in the atoms of the radio-active elements be conceived as rotating about the nucleus within the small space which the relatively small atomic volumes allot to the atoms of these elements, with orbits of different periods, there will evidently come times periodically when numbers of electrons in excess of the average will all be exerting attractive forces on the positive nucleus in the same direction. In such circumstances it is conceivable that a positively charged constituent of the nucleus might be drawn out of its normal equilibrium position and, the local attractive forces which held it in its equilibrium position being overbalanced by the repulsive force between this new entity and the positive nucleus acting as a whole, be sent on its path as an α-particle. The rate of decay of the atoms of the elements would then depend on the frequency with which this favourable configuration of electrons, which is just sufficient to exert the critical attractive force, occurred. The more stable the nucleus, the greater would the numbers of electrons all acting in the same direction need to be. But the greater the concentration required, the less frequently will it occur, other things being equal. Hence, for a more stable nucleus the rate of decay must be less. The rate of decay would thus depend primarily on the stability of the nucleus, and the mechanism suggested would constitute the trigger action by which the actual disintegration was brought about. ROBERT N. PEASE.

(National Research Fellow in Chemistry).

Laboratory of Physical Chemistry,
Princeton University, Princeton, N.J.,
August 6.

The Freshwater Winkle.

I WAS, recently, fortunate enough to obtain a pair of the yellow-bodied variety of the freshwater winkle (*P. contecta*) from what I understand was the first consignment to be imported into this country. Unfortunately the female died, and when I removed it from the aquarium the body fell out of the shell, the snail having apparently been dead a day or two. I then noticed that there was a row of five fully-formed baby snails—about $\frac{1}{4}$ inch in diameter—in the gelatinous egg-sac.

Although I thought there was little possibility of their being alive, I released them with a pair of scissors and placed them in a saucer of water. For twenty-four hours or so there was no sign of life, but, on the second day, I noticed that an operculum was forming on each and that the tentacled head of two of them had been extruded. These were immediately placed in a well-established aquarium, and the following day the other three were similarly dealt with, they having also become active. All are now feeding upon the confervæ on the sides of the tank and apparently doing well.

I have never heard of such an experiment having met with success, and shall be glad to learn whether the result is new. A. E. HODGE.

The Effect of a Lead Salt on Lepidopterous Larvæ.

FOR some time we have been studying the effect of adding various metallic salts to the food of the larvæ of Lepidoptera, and, as the results will not be ready for publication for about a year, desire to direct attention to the surprising result of using a salt of lead. When a dozen larvæ of *S. ocellatus* were fed on sprigs of apple which had been treated with lead nitrate it was soon obvious that they were eating more freely and growing more rapidly than the controls; by the time they were about three-fourths grown they consumed double the daily ration eaten by the latter. There was considerable disease among the controls and in another experimental batch, but those getting lead remained perfectly healthy.and pupated about a fortnight earlier than the controls. The pupæ were a very fine lot, the males weighing on the average about 15 per cent. more than the controls, and the moths were large and somewhat peculiarly coloured; there were too few females for a comparison to be made. Confirmatory results have been obtained with the larvæ of other moths.

This curious result is not without parallel. The herbage near the chimneys of lead-smelting works contains appreciable amounts of lead, and cases of lead poisoning have occurred among sheep; in Weardale, however, it is a common practice to pasture sheep as near as possible to these chimneys when they are being fattened, as the farmers consider that they fatten much more quickly than on other parts of the moors. F. C. GARRETT.
HILDA GARRETT.

The Pigeon Tick.

THERE is a slight error in the statement of L. H. Matthews and A. D. Hobson in NATURE of September 2, p. 313, with regard to the latest previous record of the pigeon tick *Argas reflexus*. In 1917 I secured four specimens from the tower of Canterbury Cathedral. At least two living specimens were forwarded to Mr. C. Warburton at the time.

The Cathedral receives a special cleaning every four years and *Argas reflexus* is invariably dislodged on these occasions. A. G. LOWNDES.

Marlborough College, September 4.

The Theory of Numbers.[1]

By Prof. G. H. HARDY, M.A., F.R.S.

I FIND myself to-day in the same embarrassing position in which a predecessor of mine at Oxford found himself at Bradford in 1875, the president of a Section, probably the largest and most heterogeneous in the Association, which is absorbed by a multitude of divergent professional interests, none of which agree with his or mine.

There are two courses possible in such circumstances. One is to take refuge, as Prof. Henry Smith did then, with visible reluctance, in a series of general propositions to which mathematicians, physicists, and astronomers may all be expected to return a polite assent. The importance of science and scientific method, the need for better organisation of scientific education and research, are all topics on which I could no doubt say something without undue strain either on my own honesty or on your credulity. That there is no finer education and discipline than natural science; that it is, as Dr. Campbell has said, "the noblest of the arts"; that the crowning achievements of science lie in those directions with which this Section is professionally concerned: all this I could say with complete sincerity, and, if I were the head of a deputation approaching a Government Department, I suppose that I would not shirk even so unprofitable a task.

It is unfortunate that these essential and edifying truths, important as it is that they should be repeated as loudly as possible from time to time, are, to the man whose interest in life lies in scientific work and not in propaganda, unexciting, and in fact quite intolerably dull. I could, if I chose, say all these things, but, even if I wanted to, I should scarcely increase your respect for mathematics and mathematicians by repeating to you what you have said yourselves, or read in the newspapers, a hundred times already. I shall say them all some day; the time will come when we shall none of us have anything more interesting to say. We need not anticipate our inevitable end.

I propose therefore to adopt the alternative course suggested by my predecessor, and try to say something to you about the one subject about which I have anything to say. It happens, by a fortunate accident, that the particular subject which I love the most, and which presents most of the problems which occupy my own researches, is by no means overwhelmingly recondite or obscure, and indeed is sharply distinguished from almost every other branch of pure mathematics, in that it makes a direct, popular, and almost irresistible appeal to the heart of the ordinary man.

There is, however, one preliminary remark which I cannot resist the temptation of making. The present is a particularly happy moment for a pure mathematician, since it has been marked by one of the greatest recorded triumphs of pure mathematics. This triumph is the work, as it happens, of a man who probably would not describe himself as a mathematician, but who has done more than any mathematician to vindicate the dignity of mathematics, and to put that obscure and perplexing construction, commonly described as "physical reality," in its proper place.

There is probably less difference between the methods of a physicist and a mathematician than is generally supposed. The most striking among them seems to me to be this, that the mathematician is in much more direct contact with reality. This may perhaps seem to you a paradox, since it is the physicist who deals with the subject-matter to which the epithet "real" is commonly applied. But a very little reflection will show that the "reality" of the physicist, whatever it may be (and it is extraordinarily difficult to say), has few or none of the attributes which common-sense instinctively marks as real. A chair may be a collection of whirling atoms, or an idea in the mind of God. It is not my business to suggest that one account of it is obviously more plausible than the other. Whatever the merits of either of them may be, neither draws its inspiration from the suggestions of common-sense.

Neither the philosophers, nor the physicists themselves, have ever put forward any very convincing account of what physical reality is, or of how the physicist passes, from the confused mass of fact or sensation with which he starts, to the construction of the objects which he classifies as real. We cannot be said, therefore, to know what the subject-matter of physics is; but this need not prevent us from understanding the task which a physicist is trying to perform. That, clearly, is to correlate the incoherent body of facts confronting him with some definite and orderly scheme of abstract relations, the kind of scheme, in short, which he can borrow only from mathematics.

A mathematician, on the other hand, fortunately for him, is not concerned with this physical reality at all. It is impossible to prove, by mathematical reasoning, any proposition whatsoever concerning the physical world, and only a mathematical crank would be likely now to imagine it his function to do so. There is plainly one way only of ascertaining the facts of experience, and that is by observation. It is not the business of a mathematician to suggest one view of the universe or another, but merely to supply the physicists with a collection of abstract schemes, which it is for them to select from, and to adopt or discard at their pleasure.

The most obvious example is to be found in the science of geometry. Mathematicians have constructed a very large number of different systems of geometry, Euclidean or non-Euclidean, of one, two, three, or any number of dimensions. All these systems are of complete and equal validity. They embody the results of mathematicians' observations of *their* reality, a reality far more intense and far more rigid than the dubious and elusive reality of physics. The old-fashioned geometry of Euclid, the entertaining seven-point geometry of Veblen, the space-times of Minkowski and Einstein, are all absolutely and equally real. When a mathematician has constructed, or, to be more accurate, when he has observed them, his professional interest in the matter ends. It may be the seven-point

[1] Presidential address delivered to Section A (Mathematics and Physics) of the British Association at Hull on Sept. 8.

geometry that fits the facts the best, for anything that mathematicians have to say. There may be three dimensions in this room and five next door. As a professional mathematician, I have no idea; I can only ask some competent physicist to instruct me in the facts.

The function of a mathematician, then, is simply to observe the facts about his own intricate system of reality, that astonishingly beautiful complex of logical relations which forms the subject-matter of his science, as if he were an explorer looking at a distant range of mountains, and to record the results of his observations in a series of maps, each of which is a branch of pure mathematics. Many of these maps have been completed, while in others, and these, naturally, are the most interesting, there are vast uncharted regions. Some, it seems, have some relevance to the structure of the physical world, while others have no such tangible application. Among them there is perhaps none quite so fascinating, with quite the same astonishing contrasts of sharp outline and mysterious shade, as that which constitutes the theory of numbers.

The number system of arithmetic is, as we know too well, not without its applications to the sensible world. The currency systems of Europe, for example, conform to it approximately; west of the Vistula, two and two make something approaching four. The practical applications of arithmetic, however, are tedious beyond words. One must probe a little deeper into the subject if one wishes to interest the ordinary man, whose taste in such matters is astonishingly correct, and who turns with joy from the routine of common life to anything strange and odd, like the fourth dimension, or imaginary time, or the theory of the representation of integers by sums of squares or cubes.

It is impossible for me to give you, in the time at my command, any general account of the problems of the theory of numbers, or of the progress that has been made towards their solution even during the last twenty years. I must adopt a much simpler method. I will merely state to you, with a few words of comment, three or four isolated questions, selected in a haphazard way. They are seemingly simple questions, and it is not necessary to be anything of a mathematician to understand them; and I have chosen them for no better reason than that I happen to be interested in them myself. There is no one of them to which I know the answer, nor, so far as I know, does any mathematician in the world; and there is no one of them, with one exception which I have included deliberately, the answer to which any one of us would not make almost any sacrifice to know.

1. *When is a number the sum of two cubes, and what is the number of its representations?* This is my first question, and first of all I will elucidate it by some examples. The numbers $2 = 1^3 + 1^3$ and $9 = 2^3 + 1^3$ are sums of two cubes, while 3 and 4 are not: it is exceptional for a number to be of this particular form. The number of cubes up to 1,000,000 is 100, and the number of numbers, up to this limit and of the form required, cannot exceed 10,000, one-hundredth of the whole. The density of the distribution of such numbers tends to zero as the numbers tend to infinity. Is there, I am asking, any simple criterion by which such numbers can be distinguished?

Again, 2 and 9 are sums of two cubes, and can be expressed in this form in one way only. There are numbers so expressible in a variety of different ways. The least such number is 1729, which is $12^3 + 1^3$ and also $10^3 + 9^3$. It is more difficult to find a number with *three* representations; the least such number is

$$175{,}959{,}000 = 560^3 + 70^3 = 552^3 + 198^3 = 525^3 + 315^3.$$

One number at any rate is known with *four* representations, namely,

$$19 \times 363510^3$$

(a number of 18 digits), but I am not prepared to assert that it is the least. No number has been calculated, so far as I know, with more than four, but theory, running ahead of computation, shows that numbers exist with five representations, or six, or any number.

A distinguished physicist has argued that the possible number of isotopes of an element is probably limited because, among the ninety or so elements at present under observation, there is none which has more isotopes than six. I dare not criticise a physicist in his own field; but the figures I have quoted may suggest to you that an arithmetical generalisation, based on a corresponding volume of evidence, would be more than a little rash.

There are similar questions, of course, for squares, but the answers to these were found long ago by Euler and by Gauss, and belong to the classical mathematics. Suppose, for simplicity of statement, that the number in question is *prime*. Then, if it is of the form $4m+1$, it is a sum of squares, and in one way only, while if it is of the form $4m+3$ it is not so expressible; and this simple rule may readily be generalised so as to apply to numbers of any form. But there is no similar solution for our actual problem, nor, I need scarcely say, for the analogous problems for fourth, fifth, or higher powers. The smallest number known to be expressible in two ways by two biquadrates is

$$635318657 = 158^4 + 59^4 = 134^4 + 133^4;$$

and I do not believe that any number is known expressible in three. Nor, to my knowledge, has the bare existence of such a number yet been proved. When we come to fifth powers, nothing is known at all. The field for future research is unlimited and practically untrodden.

2. I pass to another question, again about cubes, but of a somewhat different kind. *Is every large number* (every number, that is to say, from a definite point onwards) *the sum of five cubes?* This is another exceptionally difficult problem. It is known that every number, without exception, is the sum of nine cubes; two numbers, 23 (which is $2.2^3 + 7.1^3$) and 239, actually require so many. It seems that there are just fifteen numbers, the largest being 454, which need eight, and 121 numbers, the largest being 8042, which need seven; and the evidence suggests forcibly that the six-cube numbers also ultimately disappear. In a lecture which I delivered on this subject at Oxford I stated, on the authority of Dr. Ruckle, that there were two numbers, in the immediate neighbourhood of 1,000,000, which could not be resolved into fewer cubes than six; but Dr. A. E. Western has refuted this assertion by resolving each of them into five, and is of opinion,

I believe, that the six-cube numbers have disappeared entirely considerably before this point. It is conceivable that the five-cube numbers also disappear, but this, if it be so, is probably in depths where computation is helpless. The four-cube numbers must certainly persist for ever, for it is impossible that a number $9n+4$ or $9n+5$ should be the sum of three.

I need scarcely add that there is a similar problem for every higher power. For fourth powers the critical number is 16. There is no case, except the simple case of squares, in which the solution is in any sense complete. About the squares there is no mystery; every number is the sum of four squares, and there are infinitely many numbers which cannot be expressed by fewer.

3. I will next raise the question *whether the number* $2^{137}-1$ *is prime*. I said that I would include one question which does not interest me particularly; and I should like to explain to you the kind of reasons which damp down my interest in this one. I do not know the answer, and I do not care greatly what it is.

The problem belongs to the theory of the so-called "perfect" numbers, which has exercised mathematicians since the times of the Greeks. A number is perfect if, like 6 or 28, it is the sum of all its divisors, unity included. Euclid proved that the number

$$2^m(2^{m+1}-1)$$

is perfect if the second factor is prime; and Euler, 2000 years later, that all *even* perfect numbers are of Euclid's form. It is still unknown whether a perfect number can be odd.

It would obviously be most interesting to know generally in what circumstances a number 2^n-1 is prime. It is plain that this can be so only if n itself is prime, as otherwise the number has obvious factors; and the 137 of my question happens to be the least value of n for which the answer is still in doubt. You may perhaps be surprised that a question apparently so fascinating should fail to arouse me more.

It was asserted by Mersenne in 1644 that the only values of n, up to 257, for which 2^n-1 is prime are

$$2, 3, 5, 7, 13, 17, 19, 31, 67, 127, 257;$$

and an enormous amount of labour has been expended on attempts to verify this assertion. There are no simple general tests by which the primality of a number chosen at random can be determined, and the amount of computation required in any particular case may be appalling. It has, however, been imagined that Mersenne perhaps knew something which later mathematicians have failed to rediscover. The idea is a little fantastic, but there is no doubt that, so long as the possibility remained, arithmeticians were justified in their determination to ascertain the facts at all costs. "The riddle as to how Mersenne's numbers were discovered remains unsolved," wrote Mr. Rouse Ball in 1891. Mersenne, he observes, was a good mathematician, but not an Euler or a Gauss, and he inclines to attribute the discovery to the exceptional genius of Fermat, the only mathematician of the age whom any one could suspect of being hundreds of years ahead of his time.

These speculations appear extremely fanciful now, for the bubble has at last been pricked. It seems now that Mersenne's assertion, so far from hiding unplumbed depths of mathematical profundity, was a conjecture based on inadequate empirical evidence, and a somewhat unhappy one at that. It is now known that there are at least four numbers about which Mersenne is definitely wrong; he should have included at any rate 61, 89, and 107, and he should have left out 67. The mistake as regards 61 and 67 was discovered so long ago as 1886, but could be explained with some plausibility, so long as it stood alone, as a merely clerical error. But when Mr. R. E. Powers, in 1911 and 1914, proved that Mersenne was also wrong about 89 and 107, this line of defence collapsed, and it ceased to be possible to take Mersenne's assertion seriously.

The facts may be summed up as follows. Mersenne makes fifty-five assertions, for the fifty-five primes from 2 to 257. Of these assertions forty are true, four false, and eleven still doubtful. Not a bad result, you may think; but there is more to be said. Of the forty correct assertions many, half at least, are trivial, either because the numbers in question are comparatively small, or because they possess quite small and easily detected divisors. The test cases are those in which the numbers are prime, or Mersenne asserts that they are so; there are only four of these cases which are difficult and in which the truth is known; and in these Mersenne is wrong in every case but one.

It seems to me, then, that we must regard Mersenne's assertion as exploded; and for my part it interests me no longer. If he is wrong about 89 and 107, I do not care greatly whether he is wrong about 137 as well, and I should regard the computations necessary to decide as very largely wasted. There are so many much more profitable calculations which a computer could undertake.

I hope that you will not infer that I regard the problem of perfect numbers as uninteresting in itself; that would be very far from the truth. There are at least two intensely interesting problems. The first is the old problem, which so many mathematicians have failed to solve, whether a perfect number can be odd. The second is whether the number of perfect numbers is infinite or not. If we assume that all perfect numbers are even, we can state this problem in a still more arresting form. *Are there infinitely many primes of the form* 2^n-1*?* I find it difficult to imagine a problem more fascinating or more intricate than that. It is plain, though, that this is a question which computation can never decide, and it is very unlikely that it can ever give us any data of serious value. And the problem itself really belongs to a different chapter of the theory, to which I should like next to direct your attention.

4. *Are there infinitely many primes of the form* n^2+1*?* Let me first remind you of some well-known facts in regard to the distribution of primes.

There are infinitely many primes; their density decreases as the numbers increase, and tends to zero when the numbers tend to infinity. More accurately, the number of primes less than x is, to a first approximation,

$$\frac{x}{\log x}.$$

The chance that a large number n, selected at random, should be prime is, we may say, about $\frac{1}{\log n}$.

Still more precisely, the "logarithm-integral"

$$\mathrm{Li}\, x = \int_2^x \frac{dt}{\log t}$$

gives a very good approximation to the number of primes. This number differs from Li x by a function of x which oscillates continually, as Mr. Littlewood, in defiance of all empirical evidence to the contrary, has shown, between positive and negative values, and is sometimes large, of the order of magnitude $\sqrt{x}$ or thereabouts, but always small in comparison with the logarithm-integral itself.

Except for one lacuna, which I must pass over in silence now, this problem of the general distribution of primes, the first and central problem of the theory, is in all essentials solved. But a variety of most interesting problems remain as to the distribution of primes among numbers of special forms. The first and simplest of these is that of the arithmetical progressions: *How are the primes distributed among all possible arithmetical progressions* $an+b$*?* We may leave out of account the case in which a and b have a common factor; this case is trivial, since $an+b$ is then obviously not prime.

The first step towards a solution was made by Dirichlet, who proved for the first time, in 1837, that any such arithmetical progression contains an infinity of primes. It has since been shown that the primes are, to a first approximation at any rate, distributed evenly among all the arithmetical progressions. When we pursue the analysis further, differences appear; there are on the average, for example, more primes $4n+3$ than primes $4n+1$, though it is not true, as the evidence of statistics has led some mathematicians to conclude too hastily, that there is always an excess to whatever point the enumeration is carried.

The problem of the arithmetical progressions, then, may also be regarded as solved; and the same is true of the problem of the primes of a given quadratic form, say $am^2+2bmn+cn^2$, homogeneous in the two variables m and n. To take, for example, the simplest and most striking case, there is the natural and obvious number of primes m^2+n^2. A prime is of this form, as I have mentioned already, if, and only if, it is of the form $4k+1$. The quadratic problem reduces here to a particular case of the problem of the arithmetical progressions.

When we pass to cubic forms, or forms of higher degree, we come to the region of the unknown. This, however, is not the field of inquiry which I wish now to commend to your attention. The quadratic forms of which I have spoken are forms in two independent variables m and n; the form n^2+1 of my question is a non-homogeneous form in a single variable n, the simplest case of the general form $an^2+2bn+c$. It is clear that one may ask the same question for forms of any degree: are there, for example, infinitely many primes n^3+2 or n^4+1? I do not choose n^3+1, naturally, because of the obvious factor $n+1$.

This problem is one in which computation can still play an important part. You will remember that I stated the same problem for perfect numbers. There a computer is helpless. For the numbers 2^n-1, which dominate the theory, increase with unmanageable rapidity, and the data collected by the computers appear, so far as one can judge, to be almost devoid of value. Here the data are ample, and, though the question is still unanswered, there is really strong statistical evidence for supposing a particular answer to be true. It seems that the answer is affirmative, and that there is a definite approximate formula for the number of primes in question. This formula is

$$\frac{1}{2}\mathrm{Li}\,\sqrt{x}\times\left(1+\frac{1}{3}\right)\left(1-\frac{1}{5}\right)\left(1+\frac{1}{7}\right)\left(1+\frac{1}{11}\right)\ldots,$$

where the product extends over all primes p, and the positive sign is chosen when p is of the form $4n+3$. Dr. A. E. Western has submitted this formula to a most exhaustive numerical check. It so happens that Colonel Cunningham some years ago computed a table of primes n^2+1 up to the value 15,000 of n, a limit altogether beyond the range of the standard factor tables, and Cunningham's table has made practicable an unusually comprehensive test. The actual number of primes is 1199, while the number predicted is 1219. The error, less than 1 in 50, is much less than one could reasonably expect. The formula stands its test triumphantly, but I should be deluding you if I pretended to see any immediate prospect of an accurate proof.

5. The last problem I shall state to you is this: *Are there infinitely many prime-pairs* p, $p+2$*?* One may put the problem more generally: *Does any group of primes, with assigned and possible differences, recur indefinitely, and what is the law of its recurrence?*

I must first explain what I mean by a "possible" group of primes. It is possible that p and $p+2$ should both be prime, like 3, 5, or 101, 103. It is not possible (unless p is 3) that p, $p+2$ and $p+4$ should *all* be prime, for one of them must be a multiple of 3: but p, $p+2$, $p+6$ or p, $p+4$, $p+6$ are possible triplets of primes. Similarly

$$p,\ p+2,\ p+6,\ p+8,\ p+12$$

can all be prime, so far as any elementary test of divisibility shows, and in fact 5, 7, 11, 13 and 17 satisfy the conditions. It is easy to define precisely what we understand by a "possible" group. We mean a group the differences in which, like 0, 2, 6, have at least one missing residue to every possible modulus. The "impossible" group 0, 2, 4 does not satisfy the condition, for the remainders after division by 3 are 0, 2, 1, a complete set of residues to modulus 3. There is no difficulty in specifying possible groups of any length we please.

We define in this manner, then, a "possible" group of primes, and we put the questions: Do all possible groups of primes actually occur, do they recur indefinitely often, and how often on the average do they recur? Here again it would seem that the answers are affirmative, that all possible groups occur, and continue to occur for ever, and with a frequency the law of which can be assigned. The order of magnitude of the number of prime-pairs, p, $p+2$, or p, $p+4$, or p, $p+6$, both members of which are less than a large number x, is, it appears,

$$\frac{x}{(\log x)^2}.$$

The order of magnitude of the corresponding number of triplets, of any possible type, is

$$\frac{x}{(\log x)^3},$$

and so on generally. Further, we can assign the relative frequencies of pairs or triplets of different types; there are, for example, about twice as many pairs the difference of which is 6 as there are pairs with the difference 2. All these results have been tested by actual enumeration from the factor tables of the first million numbers; and a physicist would probably regard them as proved, though we of course know very well that they are not.

There is a great deal of mathematics the purport of which is quite impossible for any amateur to grasp, and which, however beautiful and important it may be, must always remain the possession of a narrow circle of experts. It is the peculiarity of the theory of numbers that much of it could be published broadcast, and would win new readers for the *Daily Mail.* The positive integers do not lie, like the logical foundations of mathematics, in the scarcely visible distance, nor in the uncomfortably tangled foreground, like the immediate data of the physical world, but at a decent middle distance, where the outlines are clear and yet some element of mystery remains. There is no one so blind that he does not see them, and no one so sharp-sighted that his vision does not fail; they stand there a continual and inevitable challenge to the curiosity of every healthy mind. I have merely directed your attention for a moment to a few of the less immediately conspicuous features of the landscape, in the hope that I may sharpen your curiosity a little, and that some may feel tempted to walk a little nearer and take a closer view.

The Organisation of Research.[1]

By Principal J. C. IRVINE, C.B.E., D.Sc., LL.D., F.R.S.

THE British Association was the product of an age rather than the inspiration of any one man, yet of those who first gave practical effect to the movement which has spread scientific learning and has bound its devotees in a goodly fellowship there was no more eager spirit than Sir David Brewster. It is not an exaggerated claim that it was he who founded the British Association. One may trace his enlightened action to a desire to combat the apathy and distrust shown by the Government of his day towards scientific work and even scientific workers. Only in the historical sense can I claim any relationship with Brewster. It is my privilege to occupy the Principalship he once held, and I cannot escape from the thought that the daily tasks now mine were once his.

It is thus inevitable that to-day a name often in my mind should spring once more into recollection, especially as my distinguished predecessor was present at the first Hull meeting in 1853, when he contributed two papers to Section A. Chemists should be among the first to pay grateful tribute to Brewster's efforts on behalf of science, and I propose, therefore, to include in my address a review of the position scientific chemistry has won since his day in public and official estimation. Moreover, at the express suggestion of some of our members whose opinions cannot be disregarded, I am induced to add the consideration of the new responsibilities chemists have incurred now that so many of Brewster's hopes have been realised. These were recently submitted by me to another audience and, through the medium of an article in NATURE (July 22, p. 131), are possibly known to you already, but I agree with my advisers that their importance warrants further elaboration and wider discussion.

It would be idle to recall the lowly position of chemistry as an educative force in this country, or to reconstruct the difficulties with which the scientific chemist was confronted during the first thirty years of the nineteenth century. Present difficulties are serious enough, and press for all our attention, without dwelling unduly on troubles of the past. But we must at least remember that in the early days of the British Association "schools" of chemistry were in their infancy, and that systematic instruction in the science was difficult to obtain. Another point of fundamental importance which has to be borne in mind is that the masters of the subject were then for the most part solitary workers.

It is difficult for us, looking back through the years, to realise what it must have meant to search for truth under conditions which were discouraging, if not actually hostile. Yet, although his labours were often thankless and unrewarded, the chemist of the time was probably a riper philosopher and a finer enthusiast than his successor of to-day. He pursued his inquiries amid fewer distractions, and in many ways his lot must have been happy, save when tormented by the thought that a subject so potent as chemistry in developing the intellectual and material welfare of the community should remain neglected to an extent which to us seems incredible.

Public sympathy was lacking, Government support was negligible or grudgingly bestowed, and there was little or no co-operation between scientific chemistry and industry. As an unaided enthusiast the chemist was left to pursue his way without the stimulus, now happily ours, which comes from the feeling that work is supported by educated and enlightened appreciation.

Let me quote from one of Faraday's letters now in my possession and, so far as I can trace, unpublished. Writing to a friend immediately before the foundation of the British Association, he relates that a manufacturer had adopted a process developed in the course of an investigation carried out in the Royal Institution. The letter continues: "He" (the manufacturer) "writes me word that, having repeated our experiments, he finds the product very good, and as our information was given openly to the world he, as a matter of compliment, has presented me with some pairs of razors to give away." If ever there was a compliment

[1] From Part I. of the presidential address delivered to Section B (Chemistry) of the British Association at Hull on Sept. 7.

which could be described as empty, surely this was one; yet the letter gives the impression that Faraday himself was quite content with his reward.

It is perhaps unfair to quote Faraday as a type, for few men are blessed with his transparent simplicity of character, but there is obviously a great gulf fixed between the present day and a time when a debt of honour could be cancelled in such a manner. A little reflection will show that the British Association has played a useful part in discrediting the idea that because so much scientific discovery is given "openly to the world," those who profit by such discoveries should be absolved from their reasonable obligations. Even where scientific workers do not expect or desire personal reward, the institutions which provide them with their facilities are often sorely in need. The recognition, not yet complete, but more adequate than once was the case, that the labourer is worthy of his hire, represents only one minor change which the years have brought.

An even greater contrast, embodying more important principles, is found in the changed attitude of the State towards scientific education and discovery. Remember Brewster's fond hope that, by means of our Association, the whole status of science would be raised, and that a greater measure of support and encouragement would be received from the Government. How eagerly the venerable physicist must have listened to the Presidential Address delivered at the twenty-third meeting of the Association assembled in Hull for the first time. It dealt with many problems familiar to him. No doubt he followed with keen interest the account of the observations on nebulæ made with Lord Rosse's telescope, and appreciated the references to the work of Joule and Thomson. The address was a masterly synopsis of scientific progress, but from time to time a new note steals in. There is a significant reference to a consultation with the Chancellor of the Exchequer, another to a conversation with Mr. Gladstone, and a third to a working arrangement concluded with the Admiralty. These would fall sweetly on Brewster's ear, and he would cordially approve of the report of our Parliamentary Committee, which had established sympathetic contact with the House of Commons. He could not fail to be impressed with the changes a few years had brought.

Let us bridge the further gap of sixty-nine years which separates us from that day. The contrast is amazing, and once more we can trace the steady, persistent influence of the British Association in bringing about what is practically a revolution in public and official opinion. We have learned many lessons. The change has come suddenly, but it was not spontaneous. Many years had to be spent in disseminating the idea that research is a vital necessity, and toward this end presidents of our Association have not hesitated, year after year, to add the weight of their influence and eloquence. It was courageous of them to do so. I would refer you particularly to the forcible appeals made by Sir James Dewar at Belfast and Sir Norman Lockyer at Southport, when the plea for more research was laid before the Association, and thus found its way by the most direct channel to the press and to the public. No doubt many other factors have played a part in creating a research atmosphere in this country, but the steady pressure exerted by the British Association is not the least important of these influences.

The principles of science are to-day widely spread; systematic scientific training has found an honourable place in the schools and in the colleges; above all, there is the realisation that much of human progress is based on scientific inquiry, and at last this is fostered and, in part, financed as a definite unit of national educational policy. Public funds are devoted to provide facilities for those who are competent to pursue scientific investigations, and in this way the State, acting through the Department of Scientific and Industrial Research, has assumed the double responsibility of providing for the advancement of knowledge and for the application of scientific methods to industry. Scientific workers have been given the opportunities they desired, and it remains for us to justify all that has been done. We have to-day glanced briefly at the painful toil and long years of preparation; now it falls to us to sow the first crop and reap the first harvest.

Thanks to the wisdom and foresight of others, it has been possible to frame the Government policy in the light of the experience gained with pre-existing research organisations. The pioneer scheme of the kind is that administered by the Commissioners of the 1851 Exhibition, who since 1890 have awarded research scholarships to selected graduates. When in 1901 Mr. Carnegie's benefaction was applied to the Scottish Universities the trustees wisely determined to devote part of the revenues to the provision of research awards which take the form of scholarships, fellowships, and research lectureships. These have proved an immense boon to Scottish graduates, and the success of the venture is sufficiently testified by the fact that the Government research scheme was largely modelled on that of the Carnegie Trust.

In each of these organisations chemistry bulks largely, and the future of our subject is intimately connected with their success or failure. The issue lies largely in our hands. We must not forget that we are only at the beginning of a great movement, and that fresh duties now devolve upon us. It was my privilege for some years to direct the work of a chemistry institute, where research was organised on lines which the operation of the Government scheme will make general. If, from the very nature of things, my experience cannot be lengthy it is at least intimate, and I may perhaps be allowed to lay before you my impressions of the problems we have to face.

Two main objectives lie before us: the expansion of useful learning and the diffusion of research experience among a selected class. This class in itself will form a new unit in the scientific community, and from it will emerge the "exceptional man" to whom, quoting Sir James Dewar, "we owe our reputation and no small part of our prosperity." When these words were uttered in 1902 it was a true saying that "for such men we have to wait upon the will of Heaven." It is still true, but there is no longer the same risk that the exceptional man will fall by the way through lack of means. Many types of the exceptional man will be forthcoming, and you must not imagine that I am regarding him merely as one who will occupy a university chair. He will be found more frequently in industry, where his function will be to hand on the ideas inspired by his genius to the ordinary investigator.

I have no intention of wearying you by elaborating my views on the training required to produce these different types. My task is greatly simplified if you will agree that the first step must be systematic experience in pure and disinterested research, without any reference to the more complicated problems of applied science. This is necessary, for if our technical research is to progress on sound lines the foundations must be truly laid. I have no doubt as to the prosperity of scientific industries in this country so long as we avoid hasty and premature specialisation in those who control them. We may take it that in the future the great majority of expert chemists will pass through a stage in which they make their first acquaintance with the methods of research under supervision and guidance. The movement is already in progress. The Government grants are awarded generously and widely. The conditions attached are moderate and reasonable, and there is a rush to chemical research in our colleges. Here, then, I issue my first note of warning, and it is to the professors. It is an easy matter to nominate a research student; a research laboratory comfortably filled with workers is an inspiring sight, but there are few more harassing duties than those which involve the direction of young research chemists. No matter how great their enthusiasm and abilities, these pupils have to be trained, guided, inspired, and this help can come only from the man of mature years and experience. I am well aware that scorn has been poured on the idea that research requires training. No doubt the word is an expression of intellectual freedom, but I have seen too many good investigators spoiled and discouraged through lack of this help to hold any other opinion than that training is necessary. I remember, too, years when I wandered more or less aimlessly down the by-paths of pointless inquiries, and I then learned to realise the necessity of economising the time and effort of others.

The duties of such a supervisor cannot be light. He must possess versatility; for although a "research school" will doubtless preserve one particular type of problem as its main feature, there must be a sufficient variety of topics if narrow specialisation is to be avoided. Remember, also, that there can be no formal course of instruction suitable for groups of students, no common course applicable to all pupils and all inquiries. Individual attention is the first necessity, and the educative value of early researches is largely derived from the daily consultations at the laboratory bench or in the library. The responsibility of becoming a research supervisor is great, and, even with the best of good will, many find it difficult to enter sympathetically into the mental position of the beginner. An unexpected result is obtained, an analysis fails to agree, and the supervisor, out of his long experience, can explain the anomaly at once, and generally does so. If the pupil is to derive any real benefit from his difficulties, his adviser must for the moment place himself in the position of one equally puzzled, and must lead his collaborator to sum up the evidence and arrive at the correct conclusion for himself. The policy thus outlined is, I believe, sound, but it makes severe demands on patience, sympathy, and, above all, time.

Research supervision, if conscientiously given, involves the complete absorption of the director's energy and leisure. There is a rich reward in seeing pupils develop as independent thinkers and workers, but the supervisor has to pay the price of seeing his own research output fade away. He will have more conjoint papers, but fewer individual publications, and limitations will be placed on the nature of his work by the restricted technique of his pupils.

I have defined a high standard, almost an ideal, but there is, of course, the easy alternative to use the technical skill of the graduate to carry out the more laborious and mechanical parts of one's own researches, to regard these young workers as so many extra pairs of hands. I need not elaborate the outcome of such a policy.

There is another temptation, and that, in an institution of university rank, is for the professor to leave research training in the hands of his lecturers, selecting as his collaborators only those workers who have passed the apprenticeship stage. This, I am convinced, is a mistake. Nothing consolidates a research school more firmly than the feeling that all who labour in its interests are recognised by having assigned to them collaborators of real ability.

I am not yet done with the professor and his staff, for they will have other matters to attend to if research schools are to justify their existence and to do more than add to the bulk of our journals. In many cases it will be found that the most gifted of the young workers under their care lack what, for want of a better expression, is known as "general culture." Remember, these graduates have just emerged from a period of intensive study in which chemistry and the allied sciences have absorbed most of their attention. For their own sake and in the interests of our subject, they must be protected from the criticism that a scientific education is limited in outlook and leads to a narrow specialism. The research years are plastic years, and many opportunities may be found in the course of the daily consultations "to impress upon the student that there is literature other than the records of scientific papers, and music beyond the range of student songs." I mention only two of the many things which may be added to elevate and refine the research student's life. Others will at once occur to you, but I turn to an entirely different feature of research training, for which I make a special plea: I refer to the inculcation of business-like methods. You will not accuse me, I hope, of departing from the spirit of scholarship or of descending into petty detail, but my experience has been that research students require firm handling. Emancipated as they are from the restrictions of undergraduate study, the idea seems to prevail that these workers ought to be excused the rules which usually govern a teaching laboratory, and may therefore work in any manner they choose. It requires, in fact, the force of a personal example to demonstrate to them that research work can be carried out with all the neatness and care demanded by quantitative analysis. Again, in the exercise of their new freedom young collaborators are inclined to neglect recording their results in a manner which secures a permanent record and is of use to the senior collaborator. As a rule, the compilation of results for publication is not done by the experimenter, and a somewhat elaborate system of records has to be devised. It should be possible, twenty years after the work has been done, to quote the reasons which led to the initiation of each experiment, and to trace the source and history of each specimen analysed, or upon which standard physical constants have been determined. I need not enter into detail in this connexion beyond stating that, although a system which secures these objects has for many years been adopted in St. Andrews, constant effort is required to maintain the standard.

One of the greatest anxieties of the research supervisor is, however, the avoidance of extravagance and waste. The student is sometimes inclined to assume a lordly attitude and to regard such matters as the systematic recovery of solvents beneath his notice. My view is that, as a matter of discipline as much as in the interests of economy, extravagant working should not be tolerated. There is naturally an economic limit where the time spent in such economics exceeds in value the materials saved, and a correct balance must be adjusted. It is often instructive to lay before a research worker an estimate of the cost of an investigation in which these factors of time and material are taken into account. As a general rule it will be found that the saving of material is of greater moment than the loss of time. The point may not be vitally important in the academic laboratory, but in the factory, to which most of these workers eventually migrate, they will soon have the lesson thrust upon them that their time and salary bear a small proportion to costs of production.

You will see I have changed my warning from the professor to the student. A student generation is short. In a few years, when almost as a matter of course the best of young chemists will qualify for the Doctor of Philosophy degree, it will be forgotten that these facilities have come to us, not as a right, but as a privilege. Those who reap the advantages of these privileges must prove that the efforts made on their behalf have been worth while.

Looking at the position broadly, if one may criticise the research schemes of to-day, it is in the sense that the main bulk of support is afforded to the research apprentice, and the situation has become infinitely harder for the supervisor in that new and onerous tasks are imposed upon him. To expect him to undertake his normal duties and, as a voluntary act, the additional burden of research training is to force him into the devastation of late hours and overwork. The question is at once raised—Are we using our mature research material to the best advantage, and is our policy sufficiently focussed on the requirements of the experienced investigator? I think it will generally be agreed that members of the professor or lecturer class who join in the movement must be relieved in great measure of teaching and administrative work. I am decidedly of the opinion that the research supervisor must be a teacher, and must mingle freely with undergraduates, so as to recognise at the earliest possible stage the potential investigators of the future and guide their studies. To meet this necessity universities and colleges must realise that their curriculum has been extended and that staffs must be enlarged accordingly. There could then be definite periods of freedom from official duties for those who undertake research training as an added task. Opportunities must also be given to these "exceptional men" to travel occasionally to other centres and refresh themselves in the company of kindred workers. It is evident that our universities are called upon to share the financial burden involved in a national research scheme to a much greater extent than possibly they know.

I may perhaps summarise some of the conclusions reached in thinking over these questions. The first and most important is that in each institution there should be a Board or Standing Committee entrusted with the supervision of research. The functions of such a body would be widely varied and would include :—

1. The allocation of money voted specifically from university or college funds for research expenses.

2. The power to recommend additions to the teaching staff in departments actively engaged in research.

3. The recommendation of promotions on the basis of research achievement.

4. The supervision of regulations governing higher degrees.

Among the more specific problems which confront this Board are the following :—

1. The creation of research libraries where reference works can be consulted immediately.

2. The provision of publication grants, so that where no periodical literature is available the work will not remain buried or obscure.

3. The allocation of travelling grants to enable workers to visit libraries, to inspect manufacturing processes, and to attend the meetings of scientific societies.

There is one thing which a Research Board should avoid. It is, I am convinced, a mistake for a governing body to call for an annual list of publications from research laboratories. Nothing could be more injurious to the true atmosphere of research than the feeling of pressure that papers must be published or the Department will be discredited.

What I have said so far may seem largely a recital of new difficulties, but they are not insurmountable, and to overcome them adds a zest to life. It would have taken too long to go more fully into details, and I have tried to avoid making my address a research syllabus, merely giving in general terms the impressions gained during the twenty years in which the St. Andrews Research Laboratories have been in existence.

I have confined myself to the first stage in the research development of the chemist. His future path may lead him either to the factory or to the lecture-room, and in the end the exceptional man will be found in the director's laboratory or in the professor's chair. However difficult these roads may prove, I feel that with the financial aid now available, supported by the self-sacrificing labours of those who devote themselves to furthering this work, he has the opportunity to reach the goal. It is the beginning of a new scientific age, and we may look forward confidently to the time when there will be no lack of trained scientific intellects to lead our policy and direct our efforts in all that concerns the welfare of the country.

The Total Solar Eclipse of September 21.

By Dr. A. C. D. CROMMELIN.

THERE are at present in the Saros Cycle two series of eclipses which have unusual length of totality; one including those of 1865, 1883, 1901, 1919, the other including the great Indian eclipse of 1868, in which the spectroscope was first applied to the prominences; also those of 1886 (West Indies) and 1904 (Pacific). The forthcoming eclipse, September 21, being three Saroses after that of 1868, is in nearly the same longitude, but has moved southward, the only land stations available being the Maldives, Christmas Island, and Australia.

The Maldives have the disadvantages of a rather low sun, some difficulty of access, owing to the reefs surrounding the islands, probability of high wind, together with a poor health record for European visitors; they are, however, being occupied by Mr. Evershed. Christmas Island lies in the longitude of maximum totality (6 minutes), but being near the northern limit of totality it will enjoy only $3\frac{3}{4}$ minutes. This is, however, amply long enough for the programme planned. The station is occupied by Messrs. Jones and Melotte from Greenwich, their equipment consisting of the 13-inch astrographic equatorial, on a mounting specially constructed for the low latitude of the station. When the same instrument was used in Brazil in 1919 the star-images were diffused, owing probably to slight warping of the cœlostat mirror by the heat of the sun before totality. The unsuitability of the cœlostat had been foreseen, but the short interval between the armistice and the departure of the expedition made it impossible to provide an equatorial mounting.

On the present occasion it is desired to secure a completely satisfactory check on the 1919 results; these tended to confirm the amount of shift of light by the sun's gravitation predicted by Einstein; the difference in the results given by the two instruments in Brazil was, however, too large to permit the results to be taken as absolutely final, and a further test is desirable. The star-field at this totality is, unfortunately, much less favourable than that in 1919, which was probably the field containing the largest number of bright stars close to the ecliptic. There are, however, a fair number of stars of the eighth magnitude or brighter in the present field, and it is hoped that these may be photographed with somewhat longer exposures than those given before. The corona will probably be of the "Minimum" type, with little extension near the poles; this should enable stars fairly near the sun, which will have a large factor of shift, to be photographed.

Christmas Island is occupied by a Phosphate Company, under Scottish management, which has given great assistance to the expedition in transporting their baggage, in erecting huts, providing workmen, etc. Reports received in July stated that the adjustment of the instruments was complete, but that the weather during May had been very wet, and little observing was possible; check plates of the eclipse field had, however, been secured. The rainy season was, however, nearly at an end, and it was hoped that more work would shortly be possible; in addition to the eclipse programme it was planned to take a series of photometric plates, to connect the magnitude scales of the northern and southern hemispheres. Profs. Freundlich and Einstein also arranged to observe from Christmas Island, their programme being much the same as that of the British observers.

The station on the coast of West Australia has a high sun, long totality, and excellent weather prospects; but it is difficult of access, it being necessary to anchor some miles out, and land in small boats through surf. Several parties are there; that with the largest equipment is from the Lick Observatory, under Prof. Campbell. This party also makes the Einstein problem the chief item of the programme. To avoid a long stay at the eclipse camp the check plates were taken at Fiji on the voyage out. Other parties at this station are from Canada and from Perth (Australia).

The observatories of Adelaide, Melbourne, and Sydney are sending expeditions to stations in Central Australia and in Queensland. The weather prospects are good at both, but the sun in Queensland is rather low. They are understood to be attempting the Einstein problem, in addition to the older eclipse work of photography of the corona and its spectrum.

There is every reason to hope for success at some of the stations; fine weather at all of them should lead to results of a decisive character on the Einstein problem. The results will not be available for some time, as the plates will not be measured till the return of the different parties. They will, however, be developed, *in situ*, which will permit a good idea of their character to be formed. In this connexion it may be noted that there is no cable to Christmas Island, but it is expected that a Dutch man-of-war will be there, which might send a wireless message to Java.

The Deflection of Light in a Gravitational Field.

By HERBERT DINGLE.

FROM an experimental point of view, Einstein's general theory of relativity is at present in an ambiguous position. It is well known that there are three conceivable tests between its conclusions and those of the traditional ideas which it attempts to displace. With regard to the first of these—the movement of the perihelion position of Mercury—the success of the theory is decidedly impressive; all the more so, perhaps, because the result was stumbled upon, as it were, involuntarily. In seeking first the gravitational field of the sun, Einstein found the true orbit of Mercury added unto him. On the other hand, the predicted displacement of the solar spectrum lines certainly conjures up a serious obstacle. The evidence, it is true, is contradictory, but, such as it is, it seems to show a balance against the existence of the displacement. The extreme difficulty and complexity of the experimental work must, nevertheless, be borne in mind. Perhaps it is scarcely possible, in the present state of our knowledge and experimental equipment, to obtain a definite solution of the problem. The third test—concerning the deflection of light in a gravitational field—accordingly becomes of very considerable importance, and to many minds constitutes the deciding factor in their judgment of the theory.

Consequently, the chief item in the programme of the Royal Astronomical Society's expedition to Christmas Island, on the occasion of the total solar eclipse of September 21, will be the investigation of this particular problem. It will be remembered that the original test, on May 29, 1919, was considered by the observers and a large number of others to give conclusive evidence in favour of the relativity theory: it was this result, in fact, that directed general attention to the theory, and made Einstein, for a brief spell, a noteworthy figure in public esteem. The interpretation of the observations, however, has been subjected to various criticisms. A refracting atmosphere of the sun has been proposed. Attempts have been made to explain the effect as a result of terrestrial atmospheric refraction arising from a temperature gradient across the boundary of the moon's shadow-cone. Still more serious is the evidence of the mutual displacement of adjacent photographic images: the question arises whether the observed positions of the star images might not be, to some extent, dependent on the intensity of the coronal light. It must be admitted that the criticisms have been well met. Nevertheless, so fundamentally important a matter can scarcely be regarded as finally settled by a single set of observations, and the repetition which is about to take place is anything but a superfluous confirmation of previous knowledge.

It will be opportune at this time to recall the nature of the problem, and see wherein lies the difference between the traditional and the relativity conceptions which makes the prospective test possible. From the time of Newton until quite recently, gravitation has been looked upon as an essential property of matter—as characteristic as the property of inertia. Whenever we find matter showing the unmistakable effects of inertia, we find also evidence of gravitational influence. The universality of these twin phenomena has so impressed physicists that they have come to look upon them as the fundamental properties of matter. Matter is, by definition, that which has inertia and exerts gravitational attraction. Nevertheless, it is well to point out that inertia and gravitation are not the properties by which matter is generally recognised. With regard to most of the matter in the universe, there is no evidence that they exist. We announce the presence of matter when we *see* it: in other words, matter everywhere has the power of emitting or absorbing light—or, more generally, radiation. We see light, and we deduce a star; the light fluctuates, and we deduce absorbing matter. If, according to the true scientific method, we establish our fundamental conceptions on the groundwork of pure observation, we must place the power to radiate and absorb light at least as deep down in the nature of matter as the inertial-gravitational property. The recognition of either property is universally accepted as evidence of the existence of matter.

The difference with which we are concerned between the traditional and the relativistic conceptions may be expressed in this way: that whereas the older view gives no *a priori* indication of a relation between the two fundamental material influences, gravitation and light, it is an essential condition of the relativity theory that such a relation exists. A large and valuable system of thought has been built up—mainly during the last hundred years—in which radiation and gravitation are completely independent. Radiation submits to analysis and invites correlation with other physical phenomena; gravitation stands inaccessibly apart. The complex organism of electromagnetism, embracing as it does radiation, the æther, electricity, magnetism, the atom—even inertia (for radiation possesses inertia)—seems capable of assimilating the whole of physics—except gravitation. Matter appears to be the source of two streams of phenomena, one summarised in electromagnetism and the other in gravitation, and between them there is a great gulf fixed. The completeness of the duality lies, of course, only in the conceptions. Experimental evidence of a bridge across the gulf might have arisen at any time. Standing now on the bridge, it seems a little strange that it was not sought before. Inertia, in submitting to the electromagnetic scheme, might carry with it the gravitational property with which, in material bodies, it is always associated, and the electromagnetic inertia of light might be accompanied by a proportionate power to exert and respond to gravitational influence. There is no reason, according to pre-relativity physics, why it should, but neither is there any reason why it should not. The impotence of the electromagnetic theory even to suggest the more probable of the alternatives is its main defect.

There is no such ambiguity in the utterances of the relativity theory. Here gravitation—as a physical existence giving rise to a gravitational field—is ignored: the field alone is considered. The seat of the phenomenon is not sought in the secret nature of matter; it is sought in the space surrounding matter, and is, in fact, regarded as a property of that space—or, rather, space-time. The justification for this view is found in the facts, first, that the evidence for the existence of gravitation is the observed acceleration of one body in the neighbourhood of another; and, second, that the acceleration produced by one body in another is independent of every property of the latter except its position relative to the former. Now a phenomenon manifesting itself as an acceleration (involving the dimensions of space and time only) and producing effects depending only on position, can be submitted to a geometrical treatment, provided that the dimension of time is added to those of space. Instead of speaking of the curved paths of bodies in a homogeneous space-time, we can speak of the straight paths of bodies in a heterogeneous space-time. The same phenomenon is indicated by both statements. Expressed in this way, it seems as though there could be no difference between the two views, except that one might be more convenient than the other. Considered from a physical point of view, however, the difference between them is fundamental, and issues into the three experimental tests referred to. The deflection of light in a gravitational field follows naturally, if gravitation is attributed to the heterogeneity of space-time. Any entity—whether light or matter—pursuing its natural path, will appear to change the character of its motion when the space-time through which it travels departs from the simple Euclidean type. It does not matter what the moving thing is; all that counts is the region through which it moves. The dilemma of the older theory does not exist from the relativistic point of view: light must be deflected or the theory must be abandoned.

Fortunately, the amount of the deflection which relativity demands is measurably different from that which the electromagnetic theory allows. According to the relativity theory, a ray of light which just clears the sun's limb should suffer a deviation of about $1''{\cdot}75$; according to the other view, the deviation should be either half of this or nothing at all. It is this difference that makes possible the test which is about to be applied. On September 21 the sun will be leaving the constellation Virgo—very close to the celestial equator. The position is not so favourable with respect to neighbouring bright stars as was that of May 29, 1919, when the original test was made. On the other hand, the experience and criticisms arising from the previous attempt are available for the guidance of the present observers, and, granted favourable conditions, there seems to be no reason why the result should not become decisive.

The British Association at Hull.

YORKSHIRE hospitality is proverbial, and it has been very pleasantly manifested during the meeting of the British Association just concluded at Hull. The citizens have in many ways shown themselves to be proud to entertain the Association, and the facilities they have offered to the members have been exceptionally helpful. Each member was provided with a badge, and this was not only a free pass on the quick and convenient tramway system of the city, but also secured personal guidance and interest from citizens in the streets or in vehicles of any kind. It would be impossible for a city to show greater interest in its visitors or to do more to make their sojourn pleasant, and the many attentions have been much appreciated, particularly by officers and other active members of sections who usually have not the time to search for all the amenities which a place of meeting may afford. A number of free luncheons have been provided, and when the days' meetings have been over tea has been served in the writing-room at the Guildhall, and has been found both grateful and comforting to the members. For these and other unusual attentions the Association is no doubt chiefly indebted to the local secretary, Mr. T. Sheppard, curator of the Hull Museums, but with him is associated the town clerk, Mr. H. A. Learoyd, and the generous hospitality would not have been possible without the active interest and support afforded by the Corporation and people of the city. The Handbook to Hull and the East Riding of Yorkshire, edited by Mr. Sheppard and presented to each member, is a volume of permanent value, and as it will be on sale for the low price of five shillings we propose to publish a separate notice of it in an early issue.

At the meeting of the general committee at which the report of the council was presented, a resolution was passed conveying to Prof. Turner the most cordial thanks of the Association for the valuable services he has rendered to science in general and the Association in particular during his nine years' work as one of the general secretaries. In its report the council stated that it had received with great regret Prof. Turner's intimation that he would not be able to attend a meeting in Canada in 1924. Prof. Turner himself pointed out that it was desirable, on various grounds, that his successor should have experience of the working of an annual meeting at home before taking part in one overseas, and he therefore placed his office at the disposal of the general committee as from the Hull meeting. The council and the Association owe a deep debt of gratitude to Prof. Turner for his unremitting care for the interests of the Association as general secretary since 1913, and therefore during a time of exceptional difficulty, including as it has the Australian meeting, the war, the revival of the annual meetings since the war, and the period when, on the death of the late general treasurer and assistant treasurer in 1920, he acted for some months as treasurer in addition to his other work.

Mr. F. E. Smith, director of scientific research at the Admiralty, and secretary of the Physical Society, accepted the invitation of the council to be nominated as Prof. Turner's successor, and the general committee unanimously voted his appointment to the office of general secretary of the Association. The three new members of the council appointed by the general committee are Mr. E. N. Fallaize, Dr. C. S. Myers, and Prof. A. Smithells.

Next year's meeting will be at Liverpool with Sir Ernest Rutherford as president, and in the following year the place of meeting will be Toronto. The invitation to Canada was conveyed by Prof. J. C. Fields and Prof. J. C. McLennan, and it was announced that a grant of about 11,000*l*. would be available towards meeting the travelling and other expenses of visiting members.

SUMMARIES OF ADDRESSES OF PRESIDENTS OF SECTIONS.

EQUAL PAY TO MEN AND WOMEN FOR EQUAL WORK.

IN Prof. F. Y. Edgeworth's address to Section F (Economics) the question whether the wages of men and women should be determined on the same principles—in particular, through universal unrestricted competition—was discussed on purely economic grounds. Notwithstanding the general presumption in favour of *laisser faire*, it is maintained that some regulation is required for desperate competition tending to the degradation of labour. Such kinds of competition being ruled out, there is advocated an equal labour-market, the same blend of competition and combination, for both sexes alike. The unequal pressure of male unions, crowding women into comparatively few occupations, is deprecated, and it is pointed out that a sufficient safeguard against such pressure is not afforded by the interest of the employer seeking to maximise his profits. This insufficiency is explained by a principle widely applicable in economics which may be stated thus. When a quantity is in the neighbourhood of a maximum value, a small change in the conditions on which it depends—the independent variables—is generally attended with a *very* small change in the dependent quantity. Some suggestions were offered with respect to the difficulty that the value of work is not always measurable without regard to the sex of the worker; *e.g.* the employment of a woman is less profitable, so far as, other things being equal, a man is generally more useful in an emergency. Lastly, Prof. Edgeworth considered the serious impediment to equality in the labour market caused by the burden of supporting a family which is commonly undertaken by men. The proposal to obviate this difficulty by the endowment of motherhood was examined; objection is taken to the (commonly implied) socialistic transference of enormous sums from one class to another. The objection is not equally directed against subsidies in kind for the purpose of education; nor against the proposal that within the same social grade, or association, the childless should contribute to the support of children.

Dr. Rivers and the Development of Psychology.

In Section J (Psychology) the presidential address by Dr. C. S. Myers was "On the Influence of the late W. H. R. Rivers (President-elect of the Section) on the Development of Psychology in Great Britain." Rivers was invited in 1893 (in his thirtieth year) by Michael Foster to Cambridge, where he systematised the first course of practical work in experimental psychology in this country. His earliest experiments there were on colour vision and visual space perception, and he contributed to Schafer's "Textbook of Physiology" an exhaustive article on vision, which is still regarded as the most accurate and careful account of the subject in the English language. He soon extended his observations on colour vision and space perception to the Torres Straits Islanders, the Egyptians and the Todas, his membership of Dr. Haddon's Cambridge Anthropological Expedition to the Torres Straits giving him his first introduction to ethnology. These several investigations will ever stand as models of psychological method. In 1903, on his return from the Todas, he began his memorable share in the striking observations on the recovering cutaneous sensibility of Dr. Head's arm. The distinction therein reached between epicritic and protopathic sensibility laid the foundations of Rivers's later views on instinct, intelligence, dreams, and the unconscious. While working with Head, he was also engaged in studying the effects of alcohol, caffeine, and other drugs on muscular and mental work. These elaborate investigations he published as the Croonian Lectures to the Royal College of Physicians in 1908. By them he advanced the pharmacological study of the effects of drugs on man, showing how important it is to disguise the drug and to provide a control mixture indistinguishable in taste from the disguised drug mixture, so as to avoid the complicating effects of suggestion, interest, and sensory stimulation. From 1907 to 1915 he confined himself to ethnological work, but during the Great War his treatment of the psychoneuroses in the Army and in the Air Force led him back to psychology. A period ensued in which Rivers's psychological genius was released from its former shackles and his intuition was no longer controlled by intellectual doubt. It is difficult to exaggerate the fruitful, stimulating character of his criticisms of Freud and of his views on the unconscious, on instincts, and on dreams which poured forth with such astonishing profusion during the last years of his life. His main object was to give a biological interpretation to the data of psychology. His wide interests, sympathies, attainments, and knowledge, his generosity and honesty, and his devotion to scientific methods inspire us in our common aim—the Advancement of Science.

Organisation of the Agricultural Industry.

Lord Bledisloe, in his address on "The Proper Position of the Landowner in Relation to the Agricultural Industry," delivered before Section M, pointed out that organisation in the interests of the agricultural producer is the chief desideratum of British rural industry, and for this, enlightened leadership is essential. The leader and chief organiser should be the landowner, if he would but take his proper position after due training. Under present conditions it is evident that the unification of the rôles of the landowner and farm tenant is a condition precedent to the full, confident, and enterprising development of the agricultural industry on economic lines. Nevertheless, it must be recognised that the system of occupying ownership cannot exist in this country to the entire exclusion of that of landlord and tenant. English law and custom in relation to the settlement of estates and to the letting of farms are now frequently obstructive in nature under the changed conditions, and it might be well if modifications could be brought about—*e.g.*, if it were made possible in certain cases to sell part of a settled estate in order to provide the necessary capital for the cultivation or industrial equipment of the remainder of it.

In all continental countries the political power enjoyed by agriculture is founded on the fact that it is an organised industry, whereas in Great Britain it is not. As a result, the continental landowner derives as a rule a net income of 3*l*.-4*l*. per acre, as compared with 1*l*. per acre in the United Kingdom. Much of this may also be attributed to the failure of the British farmer, in the absence of the landowner's stimulus, to utilise the results of education and research, whereas abroad, especially in Germany, more scientific methods are readily adopted, notably in the economic employment of feeding stuffs and fertilisers.

Many suggestions may be made as to methods whereby British agriculture, under the direction not of the State but of the landowners, may be stabilised on a remunerative basis, among which may be mentioned the organisation of credit facilities, co-operative purchase and sale, utilisation of machinery and power, improvement of livestock sires, establishment of central dairies and bacon factories, the fuller exploitation of all farm products, especially in times of glut, and above all the elimination of superfluous and unnecessary middlemen. Apart from the heavy burden of local and Imperial taxation the toll levied by the middlemen is the main cause of the poverty-stricken condition of the English agricultural labourer; the disparity of the prices paid to the farmer and by the consumer for the same produce was well illustrated by tables.

During the last eight years occupying owners have increased by 49 per cent. and the acreage that they own by 100 per cent.; the political and industrial power resulting from this considerable reinforcement of their class should prove the greatest stimulus to enterprise on the part of landowners. The existence of the Central Landowners' Association is a welcome augury of future corporate efficiency, as its objects are to a great extent economic and constructive. In conclusion Lord Bledisloe emphasised once more the need for the effective organisation of agriculture and for the solidarity of all three classes of the agricultural community, without which continuous progress is difficult of attainment.

Current Topics and Events.

THE Rowett Institute of Research in Animal Nutrition, Aberdeen, was formally opened by H.M. the Queen on Tuesday, September 12. It will be remembered that the Institute, which in the two years of its existence has done valuable work on problems of animal feeding, is under the control of the University of Aberdeen and the North of Scotland Agricultural College; the director is Dr. J. B. Orr. The Institute owes much to the generosity of Dr. J. Quiller Rowett, after whom it was named, who contributed a sum of 10,000*l*. towards its endowment (NATURE, September 9, 1920, p. 67). This was followed by another gift for the purpose of purchasing a farm which would allow of expansion of the Institute; H.M. Treasury, on the recommendation of the Development Commission, promised a further sum of 20,000*l*. It is the establishment of such institutions as the National Institute of Agricultural Botany and the Rowett Institute of Research in Animal Nutrition which will go far towards improving the unsatisfactory state of our knowledge of food problems, both animal and human.

To the August number of the *Nineteenth Century* Sir Arthur Keith contributes a timely article on the present position of Darwinism as applied to the problem of man's origin. The strange action of a strong party among the legislators of Kentucky in America, and ill-informed articles in certain American newspapers, have met with some feeble response in this country; and an authoritative statement of the case which can be understood by the general reader is especially needed at the present time. Sir Arthur Keith has stated the case admirably, and he emphasises the fact that if a new edition of Darwin's "Descent of Man" were prepared to-day, the work would merely need large additions, and scarcely any important revision. The discoveries of the fossil remains of man made since 1871 agree in pointing towards a common ancestry with the apes. The progress in our knowledge of human embryology within the same period has revealed a succession of facts which can be explained only on the theory of descent from lower forms of life. The latest discovery, that the development and growth of all parts of the body are regulated and co-ordinated by a "hormone" mechanism (the pouring of substances into the circulating blood by the ductless glands), leads even to the hope that before long we may begin to learn something about the processes of evolution. To the investigator, indeed, Darwinism is not a mere theory, but an instrument of advance, trusted as implicitly as are the Admiralty charts by a navigator.

WE learn from the *Times* that an expedition headed by Capt. F. Hurley has left Sydney for Port Moresby with the object of exploring New Guinea from the air. The party will include an ethnologist and a naturalist. Two seaplanes are being taken and will be used in a four months' air survey of the western portions of British New Guinea. Meanwhile the scientific section of the expedition will navigate the Fly River in a ketch. The cost of the seaplanes is being borne by Mr. L. Hodson, of Sydney. Owing to the densely forested nature and steep slopes of the interior, exploration of New Guinea on foot is most arduous. Capt. Hurley's scheme promises some hope of success, but landing places, except along the coast, will be difficult to find. The leader's previous experience in exploration was obtained with the Australian Antarctic Expedition. He has also flown across the Australian continent.

THE earthquake reported on the morning of August 27 in the Midland Counties was possibly, as Sir George Fordham has suggested in the *Times*, caused by the bursting of a meteorite. A tremor and sound were observed at 9.12 A.M. (G.M.T.) over an area of about 650 square miles with its centre a few miles south of Birmingham; at Woodhouse Eaves, seven miles north-north-west of Leicester, at 9.13; and at Whissenthorpe, near Oakham, at 9.10. The observed times are so close that it seems probable that all three shocks were due to the same cause, and the detachment of the three areas and their nearly linear arrangement are certainly suggestive of successive explosions of a meteorite.

THE centenary of the Yorkshire Philosophical Society, which was founded in 1822, will be celebrated on Wednesday, September 20. The members of the Society and its guests will be received in the gardens of the Yorkshire Museum by the president, Mr. W. H. St. Quintin, and a number of congratulatory addresses from national as well as local learned bodies will be read by their representatives. Later, the gathering will go in procession to the Minster, where a short service will be held and an address delivered by the Bishop of Beverley.

THE following have been appointed to the Board of Trustees of the National Portrait Gallery: the Earl of Ilchester, Sir Martin Conway, and Mr. W. B. Hardy, in place of the late Viscount Bryce, Sir Edward Conway, and Viscount Harcourt, respectively.

THE sixty-seventh international annual exhibition of the Royal Photographic Society of Great Britain will be opened on Saturday, September 16, by Mr. S. J. Solomon, president of the Royal Society of British Artists, at 35 Russell Square, W.C.1.

THE Harveian Oration of the Royal College of Physicians of London will be delivered at the college at 4 o'clock on Wednesday, October 18, by Dr. Arnold Chaplin. Sir Maurice Craig is to deliver the Bradshaw Lecture (on "Mental Symptoms in Physical Disease") on November 2. The FitzPatrick Lecture will be given by Dr. R. O. Moon on November 7 and 9. The subject will be "Philosophy and the Post-Hippocratic School of Medicine."

IT has been decided that an Institute of Paint and Varnish Technologists shall be founded, and a dinner is to be held shortly to inaugurate the new Institute. The objects of the Institute will be the dissemination of practical and scientific knowledge by reading and discussing papers, the improvement of technical education, the promotion of research, and the establishment of close relations with the Government and with societies interested in the products of the industry and their application. Applications to attend the inaugural dinner should be forwarded to H. D. Bradford, 42 Ribblesdale Road, S.W.16.

AT a conference of the leading societies in North America that deal with biological subjects, recently held in Washington, it was decided to recommend to the constituent bodies the formation of a Federation of American Biological Societies. The members of the federation are to be societies, not individuals, and the governing body is to be a council consisting of two representatives from each society; the council is to choose an executive committee from its own members. One of the first questions to come before the council, if and when constituted, will be the improvement of biological publications, and a committee is already studying the question in co-operation with a committee from the Division of Biology and Agriculture of the National Research Council of the United States.

IN our issue of April 15, p. 486, reference was made to the preparations in progress for the celebration in France of the centenary of the birth of Pasteur. A committee was formed in this country, under the chairmanship of Sir Charles Sherrington, in support of the commemoration measures, among which were the promotion of an exhibition of hygiene and bacteriology in Strasbourg and the erection of a statue of Pasteur before the University. Various amounts have been sent to France from time to time by the British committee, in addition to which it is now stated that a sum of nearly 850*l*. is to be forwarded to the general treasurer of the fund, M. T. Héring. The British committee has expressed the wish that should the amount be greater than that the French committee desire to spend on the statue of Pasteur, the excess should be devoted to some other form of permanent memorial in the University of Strasbourg.

THE programme has been issued of the meetings of the Institute of Metals to be held during the session 1922–23 under the presidency of Mr. Leonard Sumner. The annual May lecture will be delivered on May 2 by Dr. W. Rosenhain. The Scottish Local Section, which has arranged for six meetings in Glasgow, will be under the chairmanship of Mr. James Steven; membership is open to all local members of the Institute, and applications should be addressed to the hon. secretary, Mr. H. H. A. Greer, 50 Wellington Street, Glasgow. The chairman of the Sheffield Local Section is Prof. C. H. Desch, and the hon. secretary, H. P. Gadsby, 193 Sandford Grove Road, Sheffield; an important event in the Sheffield programme is a joint meeting with the Faraday Society for a symposium on stainless and non-corrodible alloys to be held on February 9 at Sheffield.

THE third International Conference of "Psychotechnique appliqué à l'orientation professionnelle" will meet this year at Milan on October 2-5. In order to prevent waste of time it has been decided to limit the discussions to the following subjects:—(*a*) What is meant by vocational aptitudes? (Lahy); (*b*) Natural aptitudes and acquired aptitudes (Décroly, Patrizi); (*c*) The psychological analysis of work (Gemelli, Lipmann); (*d*) Vocational guidance and Taylorism (Bauer); and (*e*) An international unification of tests and individual ratings (Claparède, Mira, Myers). A short paper of about twenty minutes' duration will be given on each subject in order to guide the discussion, and the language used will be French. The general session will be occupied with these subjects, but other meetings to discuss more detailed problems will be held in rooms specially set apart for the purpose. Papers presented at the meeting will be grouped together according to the relationship between them. A room will be reserved for the exhibition of tests, instruments, etc. Those desirous of attending are asked to communicate with the Secretary, III[me] Conférence Internationale de Psychotechnique, Milan (XIV.), via S. Barnaba 38. October 5 will be spent in visits to scientific and industrial establishments in the neighbourhood of Milan.

DR. M. J. SIRKS takes exception to the notice of his handbook of heredity published in NATURE of July 22, p. 111. He deprecates especially the charge of affirming that mutations have been actually produced as a direct consequence of changed conditions, and he adds, "I do not accept mutations at all, neither as a direct consequence of changed conditions, nor spontaneously, as being sufficiently proven." The writer of the notice regrets that he should have misrepresented Dr. Sirks's opinions. The criticism arose out of passages in chap. xiv., and referred in particular to the confident narration of Tower's alleged discovery of a sensitive period in the life of a beetle, during which its germ-cells could be modified by external conditions. Dr. Sirks summarised the account in a statement which may be translated thus. "In other words: without any doubt Tower succeeded, by means of very abnormal conditions of life, in breeding from the original form *Leptinotarsa decemlineata* offspring which had lost one hereditary factor; he called these 'mutants,' the name which is generally given to forms suddenly arising which exhibit hereditary variations." Nothing could be more explicit. In continuation, however, Dr. Sirks definitely dissociates himself from the interpretation of the alleged new forms as mutants. The objection should therefore have been expressed differently. It was not the interpretation which seemed to the reviewer unfortunate or uncritical, but rather the unqualified repetition of sensational reports which, though they have attained some currency, are gravely in need of confirmation.

MESSRS. BENN BROS., Ltd., announce a new series of monographs dealing with gas and fuel. The first three volumes will be "The Administration and Finance of Gas Undertakings, with Special Reference to the Gas Regulation Act, 1920," by G. Evetts; "Gasworks Recorders," by Dr. L. Levy; and "Modern Gasworks Chemistry," by Dr. G. Weyman. The same firm will also publish shortly "Practical Optics for the Laboratory and Workshop," by B. K. Johnson, and "The Bronze Age and the Celtic World," by H. J. E. Peake.

Our Astronomical Column.

SEPTEMBER METEORS.—Though September is not a month in which any rich shower of meteors is periodically visible, a considerable number of meteors and many radiant points of moderate activity should be seen. Fireballs are also particularly abundant during the month, especially on September 13-15, and again on September 24-28. The principal systems of meteors at this time of the year radiate from Aries, Perseus, Auriga, and other constellations in that part of the heavens. At the middle of September there is usually a strong display of ϵ Perseids from $61° + 36°$; there is also a prominent shower from near α Cygni, at $314° + 48°$, and swift, streaking meteors from the Lynx frequently manifest themselves. September is, in fact, usually a productive period for the meteoric student, and further watching may reveal a somewhat rich annual display that hitherto has not received the notice it merits.

INVISIBLE SUNSPOTS.—In the year 1908 Dr. G. E. Hale published (Contributions from the Mount Wilson Solar Observatory, vol. i., No. 26) his discovery of solar vortices. This vortex hypothesis assumes that a sunspot resembles a vast tornado in which electrified particules, due to ionisation in the solar atmosphere, are rapidly whirled. The invariable presence of a magnetic field, caused by the revolving charges, confirmed this view, but it was also supported by other results of observation with the spectrograph and spectroheliograph. It was found also that most sunspots were associated in pairs of opposite magnetic polarity, and 61 and 33 per cent. respectively of 970 spots were observed as bipolar and unipolar. The fact that some groups oscillated between unipolar and bipolar types, one or more small spots appearing and disappearing within the mass of calcium flocculi, suggested to him the idea of looking for invisible spots. That these might be found seemed most probable, there being vortices giving appreciable magnetic fields without any actual visible sunspots. Dr. Hale now describes his recent investigation in this direction (Proc. Nat. Acad. of Sciences, U.S.A., vol. 8, No. 7). The method he adopts is a device for rendering feeble magnetic fields visible by the Zeeman effect, and details of the procedure are given in his paper. Suffice it to say that he has found a great number of cases in which a local magnetic field was observed where no spot was recorded. He points out the importance of making systematic observations of invisible spots, especially during the periods preceding and following the visible life of those that reach maturity, in order to assist in revealing the cause of sunspot formation.

ABSOLUTE MAGNITUDES OF STARS.—Several years ago Prof. H. N. Russell produced a striking diagram of absolute magnitudes, on which the theory of giant and dwarf stars was based; it met at first with some opposition, but has gradually won its way to general acceptance. The number of stars of which trustworthy parallaxes (photographic, spectroscopic, and hypothetical) have been obtained, has now been greatly increased, and Dr. Heber D. Curtis has prepared a revised diagram, which is reproduced in the Journal of the R.A.S. of Canada for July–August. It contains 2375 stars, and shows the division into giants and dwarfs very plainly. The gap between them is complete in type M, but begins to be filled by a few stragglers in type Ko, suggesting that this may be the maximum temperature attained by stars of small mass. In types G and K the giants outnumber the dwarfs, but the reverse holds in type F. The giants here are comparatively few, but they include some of remarkable luminosity.

Broadly speaking, the regions of maximum frequency form two straight lines; that of the giants forms a horizontal line at magnitude $+1$; that of the dwarfs slopes downwards from $+1\frac{1}{2}$ at type Ao (the giants and dwarfs being here mingled) through $+5$ at type G5 (so that the sun is an average star of its type) down to 8 or 9 for Ma.

The diagram suggests to Dr. Curtis the conclusion that a sensible number of B-stars have parallaxes of the order of $0''\cdot02$; he notes that stars of this type are being put on the working lists at the Allegheny and Leander McCormick Observatories. These stars cannot as yet be investigated for parallax by the spectroscopic method, so that the results of these measures will be awaited with interest. It will be remembered that the results have a bearing on the adopted distances of the globular clusters. The fainter the absolute magnitude found for the average B-star, the nearer we must put the clusters.

Research Items.

LAUGHTER.—In the *Fortnightly Review* for August J. A. T. Lloyd considers the problem of humour and mechanism. Bergson in his well-known study, "Le Rire," maintains the hypothesis that the essence of humour lies in the mechanisation of what ought to be spontaneous and not mechanical. The function of laughter is to punish and so to prevent the repetition of machine-like behaviour. The author of this paper criticises this point of view as being more true of Latin humour than of Anglo-Saxon and supports the theory that in the case of the latter, laughter is rather due to a feeling of superiority. He thinks, however, that recently a change has taken place, and that in Mr. Stephen Leacock's humour we approximate to humour as sensed by Bergson; he detects automata masquerading as human beings and we laugh, but not with the old laughter of superiority. A more fundamental treatment of the subject of laughter will be found in *Psyche* (vol. ii. No. 4), where Prof. McDougall develops at greater length a theory he put forward in NATURE some years ago. He believes that the theories usually advanced fail to answer the question, For what end did the human species acquire the capacity for laughter? Man is endowed naturally with the tendency to share the emotions of others, when he sees them expressed by them in action, thus rendering himself extremely susceptible to the suffering he sees around him. Were he to suffer sympathetically every pain he saw, he would very rapidly devitalise himself; hence he has developed a compensatory mechanism whereby he laughs at the pains and sufferings which are not serious or with which he can have no concern. Laughter is then primarily and fundamentally the antidote to sympathetic pain. It is necessary to distinguish laughter from the smile which is the natural expression of pleasure.

RADULA OF THE HELICINIDÆ.—This, judging from the somewhat jejune remarks concerning the method of preparation and mounting and seeming want of acquaintance with the work of earlier writers on the subject, appears to be the author's first encounter with the molluscan radula. In these circumstances, Mr. H. B. Baker (Proc. Acad. Nat. Sci. Philad. vol. lxxiv.) must be held to have acquitted himself well and has rendered a useful and well-illustrated account of the radulæ of the Helicinidæ that will prove useful to future students of the group. Although the title gives no hint of the fact, the author attempts a classification of the North American Helicinidæ largely based on the characters of the operculum, and further essays a phylogenetic scheme.

THE STRUCTURE AND BIOLOGY OF THE HOG LOUSE.—Memoir 51 of the Cornell University Agricultural Experiment Station, by Miss L. Florence, is devoted to a study of this insect and forms a carefully executed piece of morphological work. With the exception of the human louse, very few detailed studies have been made of any species of Anopleura, and the present paper fills a very noticeable hiatus. The complete life-cycle from egg to egg at a temperature of 35° C., followed out in vials worn next the body, was ascertained to require 29 to 33 days. Of this period 13-15 days was occupied by the incubation of the eggs and three ecdyses were passed through during post-embryonic life. The difficult subject of the mouth-parts is very fully discussed, but their homologies are not touched upon more particularly owing to the necessity for developmental studies made upon the embryo. The only investigator who has dealt, so far, with this aspect of the subject is Cholodkovsky, in his work on Pediculus. Miss Florence finds that the pharynx and mouth-parts of the hog louse are similar in plan to those of the last-mentioned insect. The result of the work, as a whole, is to emphasise the general morphological similarity of the Mallophaga and Anopleura, thereby supporting the conclusions of Mjoberg and of Harrison.

RESEARCHES ON DIPTERA.—In Bulletin No. 5 (n.s.) of the Canadian Department of Agriculture, Dr. A. E. Cameron contributes a well-illustrated paper on the structure and biology of *Simulium simile*, a small black fly infesting cattle in Saskatchewan. This insect, however, has not been observed to suck the blood of man, although it may cause mild annoyance to human beings by flying persistently around their heads. The aquatic larvæ and pupæ of this species are extensively preyed upon by a fish known as the sucker (*Catastomus commersonii*), which is proving itself one of the most successful controlling agents. As the result of experimental tests with miscible (phinotas) oil it is shown that the Simulium larvæ can be killed. The experiments, however, did not prove to be quite so satisfactory with the larvæ of *S. simile* in the river as with those of other species in a small stream. In *Bulletin of Entomological Research*, vol. 12, Part 4, Major W. S. Patton contributes revisionary notes on the genus Musca: in this first part of the paper he deals with Oriental and Australasian species. The rôle which these insects play in the dissemination of disease renders the exact determination of very closely allied species a matter of practical importance. Mr. F. W. Edwards (*Entomologist's Monthly Magazine*, July) describes a new species of Sciarid fly, *Plastosciara perniciosa*, the larvæ of which were found damaging cucumber roots and stems in a nursery at West Worthing, where they were present in very large numbers.

MANGANESE IN PLANT NUTRITION.—Since the discovery of manganese in the soil and in plant ashes by Scheele in 1774, numerous investigations have been made on the occurrence, distribution, and probable function of this element in its relation to agriculture. In the July number of the Journal of the American Chemical Society, Mr. J. S. McHargue, of the Kentucky Agricultural Experiment Station, describes a careful series of experiments, with purified materials (lack of care in this respect having caused errors in previous work), the results of which seem to point definitely to the conclusion that manganese has a function to perform in the production of chlorophyll, and consequently in carbon assimilation and possibly in the synthesis of protein.

FORMATION OF MARINE DEPOSITS ABOVE SEA-LEVEL.—The Report of the Secretary of the Smithsonian Institution for 1921 quotes some observations made by Dr. Paul Bartsch at the south-east point of Hanouma Bay, Hawaii, where he found a marine flora and fauna living at a considerable elevation (the precise height not given) above the level of the sea. Algæ, molluscs, crustaceans, echinoderms, and other marine organisms, says Dr. Bartsch, "occupy pools and puddles kept moist and supplied with fresh water by the spray from the breaking surf, which incessantly pounds that shore. I consider this an important observation, since the occurrence of fossiliferous laminæ bearing marine organisms between sheets of lava has been held to indicate that they were deposited at or below sea-level, and their occurrence above this has been held as evidence of elevation. We have here an instance which indicates that this is not necessarily the case, for such a lamina would be produced if a new outpouring of lava were to cover up the place mentioned."

CAINOZOIC FISHES OF CALIFORNIA.—In a paper on the fossil fishes of the diatom beds of Lompoc, California (Leland Stanford Junior University Publications, 1920), David Starr Jordan and James Zaccheus Gilbert direct attention to beds of Miocene age, probably formed in a quiet, shallow, marine bay; they are rich in large diatoms and "heavy" radiolaria. At one special horizon an extinct herring, *Xyne grex*, is represented by an immense number of individuals, all adult, all about six inches long, and unmixed with any other fish. They appear to have entered the bay with the view of spawning, and to have been killed suddenly "with no evidence of agony or distortion." In a subsequent paper, on "The Fish Fauna of the California Tertiary" (Stanford Univ. Publications, Biol. Sci., vol. i. No. 4, 1921), Dr. Jordan gives very interesting restorations of a number of fossil fish in a series of captivating plates, preceded by photographs of their skeletons. He refers again to the remarkable shoal of herring, which provides 8 or 10 specimens to the square foot over an area of four square miles. It is estimated that 1200 million individuals perished on this one occasion. Though it is said that Dr. Mann has offered an explanation, we are unable to trace it in these memoirs.

CARBON-BLACK IN THE UNITED STATES.—Recent articles in NATURE have directed attention to the various products obtainable from natural gas, among which petroleum, allied light oils, and helium figure prominently. An important industry also exists for the purpose of manufacturing carbon-black from this source, this product forming the basis of such commodities as printers' ink, paints, varnishes, polishes, cement colours, etc. It is also considerably used in the rubber industry for increasing the resiliency and toughness of rubber tyres. The processes of extraction of carbon-black from natural gas are confined principally to the United States, Louisiana being the leading state in this respect, the other producing states being West Virginia, Kentucky, Oklahoma, Pennsylvania, Montana, and Wyoming. The industry thrives best where there is an abundant supply of natural gas available in fields sufficiently isolated as to inhibit the use of the gas for domestic purposes. The yield of carbon-black per thousand cubic feet of gas ranges from 0·2 lbs. to 3·5 lbs., and in 1921 more than 31 million pounds were produced by Louisiana alone, the average yield being 0·97 lbs. per m. cub. feet. The total production for the United States for that year amounted to 59,766,315 lbs., valued at $5\frac{1}{2}$ million dollars (E. G. Sievers, Min. Resources Unit. States, 1921, pt. ii. p. 33). Recent legislation in some states, in particular Louisiana, has tended to check the progress of this industry, since the rapid advancement of natural gas gasoline manufacture (a far more valuable product) has resulted in the conservation of natural gas for that purpose. Operators are therefore compelled to extract the gasoline from the gas before the latter is burned in the carbon-black plants. In some cases this has served to eliminate the industry altogether, but by adapting and by using the gasoline and carbon-black plants in conjunction, such calamity can be, and is being, fortunately avoided.

THE COURSE OF PHOTOGRAPHIC DEVELOPMENT.—Forty-five years ago, Abney (*Phil. Mag.*, 1877) coated exposed plates with a second sensitive film before development. After development he stripped off the second film and found that a part of the image was in it—that is, the development had spread from the exposed to the unexposed sensitive material. On the contrary, common experience with gelatine plates shows conclusively that when the exposure is insufficient it is impossible to develop an image of the maximum density that the plate can yield. If development spreads at all from exposed to unexposed particles, the spreading effect must be very limited. Recently, this matter has been investigated in the modern, microscopical manner, by tracing the changes in the particles themselves. Prof. The. Svedberg has shown that developability is not conferred by developable grains upon contiguous grains, and that the percentage of grains developed is not increased when the grains are "closely packed together." In the September number of the Journal of the Royal Photographic Society, Messrs. A. P. H. Trivelli, F. L. Righter, and S. E. Sheppard, of the Research Laboratory of the Eastman Kodak Company, give details of their experiments, which show that where a group of two or more grains forms a "clump," this clump develops as a unit, and if only one of the constituent grains has been made developable, the whole group is completely developable. They used Svedberg's methods. They consider that Svedberg's results are conclusive evidence that developability in his case was not transferred from one grain to another, and ascribe their apparently contradictory results to the character of the emulsion. Svedberg used a special emulsion with mostly spherical grains of nearly uniform size, while the authors' emulsion had a wide range of grain sizes and contained many large polyhedral tablets. Obviously this matter is of fundamental importance from a theoretical point of view.

UPPER AIR RESEARCH.—Part I. of an aerological survey of the United States, the results of observations by means of kites, by Mr. W. R. Gregg, is published as Supplement No. 20, U.S. *Monthly Weather Review*. An abstract by the author is also given in the *Monthly Weather Review* for May last. The object of the discussion is to furnish results so much needed at the present time in connexion with aviation and ordnance. Much detailed information can be obtained from the numerous tables and diagrams as to the characteristics of the free air over the United States east of the Rocky Mountains. Kite observations are made at six stations established by the Weather Bureau during the period 1915 to 1918, and there are data from other sources. Various meteorological results for the upper air are given for the several months, the season, and the year. The values at Blue Hill, Mass., and Mount Weather, Va., each based on a long series of observations with kites, are included in the discussion. Free-air results will augment the general knowledge of atmospheric circulation, and of the movements of cyclones and anticyclones. It is rightly claimed that they will give information of value in connexion with the laying out of a permanent flying course or "air-way." Near the surface the turning of the winds is generally to the right, and the deviation is greater in winter than in summer; moreover, it is greater at northern than at southern stations. The average wind velocity at lower levels increases most above surface south-easterly to south-westerly winds, but at greater heights the largest increases are found above surface south-westerly to north-westerly winds. The velocities are least in all seasons and at all heights above surface north-easterly to east-south-easterly winds. A review of pilot-balloon observations is foreshadowed at a future date.

Einstein's Theories.

THE *Revue Philosophique* (Alcan, Paris), edited by Prof. Lévy-Bruhl, has just issued (July–August) a special number devoted to the consideration of Einstein's theories of relativity. It consists of four articles of exceptional ability and importance, all directed to the philosophical aspects of the problem. That Einstein's theory is established, in the meaning that it is applicable in physical science, is accepted by each of the writers, and their aim is to decide how far it forces upon us a new way of thinking about physical reality.

The first article is a translation by M. Léon Bloch from the German of Hans Reichenbach, "La signification philosophique de la théorie de la relativité." The philosophical interest centres on the concept of time. Must we give up the absolute meaning of simultaneity? If we do, the Michelson-Morley experiment at once ceases to be incomprehensible, light *can* have the same velocity in the moving system which it has in the system at rest. But can we give up absolute simultaneity without being involved in logical difficulties and finding ourselves confronted with a pure paradox? In a very skilful argument the writer concludes that we can and that we must. Relativity is both a logical necessity and an experimental fact. This leads to the consideration of the part played by the velocity of light in Einstein's theory.

In Nature the electromagnetic waves play a unique part and are of greater importance than any other phenomenon which serves us as a signal. They alone (if we set aside gravitation) transmit an action across empty space. Now as the forces which the individual particles of matter exercise on one another are of the same nature as electromagnetic forces, it follows that all propagation of material action resolves itself ultimately into an electrical transmission.

The essential ideas of the theory of relativity were forestalled by Ernst Mach forty years ago, and Einstein is continually reminding us of our indebtedness to him. The idea that movement as a spatial phenomenon can be recognised only in relation to other bodies was much older—we have it, for example, in Descartes and in Leibniz—but what distinguishes Mach's point of view is the idea that movement must have not only kinematic but also dynamic relativity, that what we call the forces of inertia must be bound up with the presence of other bodies. In Mach, however, the relativity of theory of knowledge is not distinguished from physical relativity; it remained for Einstein to show that the actions of movements, or what we name forces, can be reduced to a difference between the distributions of masses.

The second article, by M. G. Cerf, "Pour l'intelligence de la relativité," deals particularly with the exact meaning we are to give to the terms employed in the theory, and the writer draws largely on the works of Henri Poincaré.

The third article, "Einstein et la métaphysique," is by M. Edmond Goblot, the distinguished logician and philologist. He finds considerable amusement and no little instruction in the popularisers of Einstein, more especially those who competed for the Higgins prize of the *Scientific American* Publishing Co. In his conclusion he says: "Je résume et précise ma question: Einstein est-il mathématicien, physicien ou métaphysicien? Mathématicien il l'est. Physicien il l'est aussi. S'est-il borné à cela, ou s'est-il abandonné aux débauches de métaphysique inconscient qu'on nous fait lire en son nom? Dans les deux cas, il est grand temps d'exorciser tous ces fantômes."

The fourth and concluding article, by M. Richard-Foy, "Le temps et l'espace du commun sens," is a very clear statement of the whole problem to meet which the new principle is required. It deals mainly and sympathetically with M. Painlevé's protest against the refusal to allow any place for the concept of an absolute in physics. The rejection of time and space as absolutes is not irreconcilable, he argues, with such a position. To say that time, space, and movement are not absolutes means that, instead of being realities which impose their laws on phenomena, they are only abstractions, necessary to express those laws, but capable of assuming the most diverse forms. We choose among these forms with the simple aim of discovering the most convenient, but our choice has limits. For example, we cannot define simultaneity in any two points absolutely, yet we must define it so that it is not possible that my friend has read my letter to him before I have written it.

Educational Work of the Ministry of Agriculture.

THE Intelligence Department of the Ministry of Agriculture and Fisheries has issued a Report on the work of the department for the years 1919–1921, which is published at the price of 5*s.* by H.M. Stationery Office. The duties of this department are concerned with agricultural education, agricultural research, the agricultural training of ex-officers and men, horticulture, the improvement of live-stock, the destruction of rats, and the diseases of animals.

Agricultural education is provided through the agency of colleges, including agricultural departments of universities, and by farm institutes, local classes, and lectures. The first group comprises eleven institutions, of which all, except the Harper Adams, the Midland Agricultural College, the University College, Reading, and the Seale Hayne College, are connected with universities in which students may obtain a degree with agricultural science as their chief subject. In most cases the agricultural department is actually part of the university organisation, and it is recognised that in many respects this is an ideal arrangement. Agricultural students thereby obtain the intellectual stimulus that is associated with intercourse with students in other faculties. Future teachers, scientific workers, and agricultural experts all gain by the indefinable atmosphere which pervades a university course. On the other hand, it has been found that although, theoretically, expenditure should be saved by taking advantage of the courses in general science which a university provides, in actual practice it has proved necessary to provide special teaching even in preliminary scientific subjects designed for agricultural students.

By means of this special teaching an agricultural flavour is imparted to chemistry, botany, zoology, or whatever the fundamental science may be, and thus from the very beginning the student's interests are awakened and stimulated. Against such an arrangement it has been urged that association with other students tends to divert a certain number of agricultural men to other subjects. But it is probable that an agricultural department of a university tends to attract more men than it loses. The courses provided in university departments of agriculture are intended for the education of future landowners, land agents, and large farmers, but for investigators it is found best that they should pass through an honours school in pure science before taking up the study of the application of science to agriculture. The report lays special emphasis on the need for the study of accountancy, which in its application to costs of production may be a powerful instrument in determining the economic success of a farm.

It is pointed out that the cost of providing the necessary staffs in a university or college is now so great that it is impossible for each college to provide highly specialised instruction in every branch of agricultural education, but that extreme specialisation must be left to individual colleges. Again, tutorial instruction and encouragement of private reading are urged as a means of relieving pressure on formal lectures, and so of keeping down expenses.

While agricultural departments in universities and agricultural colleges are the agency of providing instruction to prospective landowners, large farmers, and public servants, the needs of the ordinary farmer's son are best supplied through farm institutes. The latter have been developed in recent years as the result of the recommendations of Lord Reay's Committee which in 1905 strongly urged their creation. These farm institutes are under the authority of County Councils, directed by the Ministry of Agriculture. An agricultural education committee having been set up, it submits its scheme to the ministry, and this, if approved, is supported by grants. The staff of an institute consists in most cases of an organiser, a director of agriculture, and certain teachers. The county organiser is usually the head of the farm institute, and towards his salary the ministry may contribute as much as four-fifths. It also pays annual grants up to two-thirds of the total general expenditure. Classes are provided as a rule for twenty-four weeks during winter, at a time, namely, when young farmers can leave their farms and devote their time to study. While a certain amount of manual training is possible at farm institutes, it is recognised that the best place for getting such instruction is on the farm of the student's father. The teaching of science is in close contact with practice, and is concerned principally with such subjects as varieties of crops, methods of cultivation, rotations, manures and feeding stuffs, principles of feeding and breeding, dairying, poultry, and farm book-keeping. Such farm institutes have been established in Cumberland, Essex, Hampshire, Carnarvonshire, Monmouthshire, Cheshire, Hertfordshire, Northamptonshire, Somerset, Staffordshire, Suffolk, and Denbigh, while others are contemplated in Durham, Kent, Carmarthen, West Sussex, and the Holland Division of Lincolnshire.

Besides providing instruction for students, the colleges and institutes are intended to serve as advisory centres for farmers generally. Such advisory officers are usually specialists in plant pathology, botany, chemistry, and general agriculture, and to these farmers are encouraged to turn in case of difficulty. This they are doing in increasing numbers every year, and one of the most gratifying features of the present position is the disappearance of prejudice on the part of cultivators to education and science.

University and Educational Intelligence.

ABERDEEN.—Mr. G. P. Thomson, lecturer in mathematics at Corpus Christi College, Cambridge, has been appointed professor of natural philosophy in succession to Prof. C. Niven, who has retired.

LEEDS.—The honorary degree of Doctor of Science has been awarded to Prof. A. F. Holleman, of the University of Amsterdam.

PROF. CHARLES CROWTHER has been appointed Principal of the Harper-Adams Agricultural College, Newport, Salop, in succession to Mr. P. Hedworth Foulkes, who has been Principal since the College opened in 1900.

AN important conference of representatives of British and Swiss universities took place at Basle last month. There were present fifteen delegates from Great Britain and Ireland, Oxford being represented by the Vice-Chancellor and the Warden of All Souls, Manchester by the Vice-Chancellor and Prof. T. F. Tout, Edinburgh by Sir Richard Lodge and Prof. J. Mackinnon, and Birmingham, Bristol, Cambridge, Leeds, London, Wales, St. Andrews, Dublin, and the National University of Ireland each by one delegate. Each of the seven Swiss cantonal universities was represented. At the three formal sessions of the conference, held on August 22 and 23 in the great hall of the University, the topics of discussion were the recognition by the British universities of university entrance examinations passed, university studies pursued, and degrees conferred in Switzerland, and *vice versa*, and interchange of university teachers. Of perhaps even greater importance than the formal discussions were the conversations for which ample opportunities were provided in the course of the numerous social functions at which the visitors were entertained. The Federal Ecole Polytechnique of Zürich was unfortunately not represented at the conference. Before the war this institution, like the cantonal universities, drew a large proportion of its students from other countries where economic conditions are at present unfavourable to the migration of students to Switzerland. Consequently there are plenty of vacant places in its laboratories, which are well equipped for advanced work in, for example, industrial chemistry and electrical engineering.

A PROVISIONAL programme has been issued by the Sociological Society, Leplay House, 65 Belgrave Road, Westminster, S.W.1, of a conference on the correlation of the social sciences, which it is proposed to hold at Oxford on October 7-9. The conference will not be open to the public, but invitations are being issued to members of the Sociological Society and to representatives of the social sciences from the universities of Great Britain. The object of the conference is to provide an opportunity for the discussion by specialists of various branches of social science with the view of co-ordination. Mr. F. S. Marvin (history), Sir Halford Mackinder (geography), Mr. Julian Huxley (biology), Prof. C. E. Spearman (psychology), Prof. L. T. Hobhouse (philosophy), Dr. R. R. Marett (anthropology), Prof. J. E. G. de Montmorency (law), and the Rev. A. J. Carlyle (political science), will probably address the conference, dealing with the various aspects of sociology named.

Calendar of Industrial Pioneers.

September 17, 1823. Abraham Louis Breguet died. —The foremost horologist of his day, Breguet was born in Switzerland in 1747, but at an early age removed to Paris, where he became a member of the Bureau des Longitudes and of the National Institute. He is remembered for his improvements in the escapement of watches and his invention of the sympathetic pendulum and of a sensitive metallic thermometer.

September 17, 1869. John Elder died.—One of the greatest marine engineers, Elder was trained under his father, David Elder, at Napiers'. In 1852, at the age of twenty-eight, he joined the engineering firm of Randolph Elliott and Co. and became the virtual founder of the great firm at Govan known since 1886 as the Fairfield Shipbuilding and Engineering Co. He was one of the first engineers to grasp the importance of the new science of thermodynamics, and he successfully introduced the use of the compound engine at sea, thereby effecting a saving of 30 to 40 per cent. of the coal burnt. The Elder chairs of naval architecture at Glasgow and at Liverpool were founded respectively by his widow and his brother Alexander, who died in 1915.

September 17, 1895. Johann Sigismund Schuckert died.—After working as a mechanic in various towns of Germany, Schuckert spent some years in America, where he became acquainted with Edison, and on his return home, in 1873, set up a workshop at Nürnberg. He then began the manufacture of dynamos and other electrical machinery, and became one of the best-known electrical engineers in Germany.

September 18, 1860. Joseph Locke died.—Born near Sheffield in 1805, Locke gained his first experience of railway engineering under George Stephenson on the Manchester and Liverpool Railway. Afterwards by himself, or with his partner Errington, he built many of the early railways, including those between Manchester and Sheffield, and London and Southampton, and the line from Paris to Rouen and Havre. His railways were notable for the absence of great and expensive works. From 1857 till his death he was President of the Institution of Civil Engineers.

September 19, 1899. Léon Bourdelles died.—An engineer of the Corps des Ponts et Chaussées, Bourdelles rose to be head of the Lighthouse Department, in which situation, by the display of uncommon energy and resource, he revolutionised the lighting of the French coast, increasing the aggregate candle-power from 4,000,000 to nearly 100,000,000 without increasing the annual cost.

September 20, 1885. Walter Weldon died.—The son of a Loughborough manufacturer, Weldon became a journalist in London. Turning his attention to practical chemistry, he sought means of recovering the manganese peroxide used in the manufacture of chlorine, and about 1868 patented the lime-manganese process, which reduced the cost of bleaching powder by 6*l.* a ton and added something like 750,000*l.* per annum to the national wealth.

September 22, 1852. William Tierney Clark died. —A well-known civil engineer, Clark for forty years was engineer to the Middlesex Water Works. His masterpiece was the great bridge erected across the Danube at Budapest in 1839–49 at a cost of 622,000*l.*

September 23, 1878. John Penn died.—For many years Penn was the leading marine engine builder on the Thames. He invented the lignum vitæ stern bush bearing for screw ships, and during the Crimea War he organised the manufacture of the engines for gunboats, completing 90 sets of engines of 60 n.h.p. each in ninety days, the first example of mass production of machinery for warships. E. C. S.

Societies and Academies.

PARIS.

Academy of Sciences, August 16.—M. Émile Roux in the chair.—M. de **Sparre**: Remarks on the depressions resulting from a breakage in a water main under pressure.—Kyrille **Popoff**: The integration of the equations of ballistics under general conditions of resistance.—Pierre **Auger** and Francis **Perrin**: The shocks between α-particles and atomic nuclei. An application of a modification of C. T. R. Wilson's method of studying the paths of α-rays. Photographs of the paths were taken with two cameras at right angles to each other, and details of results in argon and in hydrogen are given. For argon the value of the atomic number calculated from the results of the observations is 19 (instead of 18).—I. Newton **Kugelmass**: A new apparatus, the nephelectrometer. The change in the transparency of a colloidal solution is measured by the deflection of a millivoltmeter connected with a thermocouple. The light from an electric lamp, after passing through a cell containing distilled water, is allowed to fall on the thermocouple for a fixed time, and the deflection of the millivoltmeter measured (I). The water is then replaced by the colloidal solution and the deflection (I′) measured under the same conditions. The ratio I′/I gives the transparency index.—A. **Marcelin**: Measurement of the pressure of "superficial fluids." Detailed study of oleic acid.—F. **Granel**: The morphological signification of the pseudobranch of the teleosteans.

Official Publications Received.

Union of South Africa. Department of Mines and Industries: Geological Survey. The Geology of the Country around Heidelberg. By Dr. A. W. Rogers. Pp. 84. The Geological Map of the Country around Heidelberg. (Pretoria: Government Printing and Stationery Office.) Price, including Map, 8*s.* 6*d.*

Air Ministry: Meteorological Office. British Meteorological and Magnetic Year Book, 1918. Part IV.: Hourly Values. From Autographic Records, 1918. Pp. 73. (London: H.M. Stationery Office.) 17*s.* 6*d.* net.

Sultanic Agricultural Society: Technical Section. Bulletin No. 1: A Survey of the more Important Economic Insects and Mites of Egypt. By F. C. Willcocks. Pp. viii+483. (Cairo: Sultanic Agricultural Society.)

Guide to the Australian Ethnological Collection exhibited in the National Museum of Victoria. By Sir Baldwin Spencer. Third edition. Pp. 142+33 plates. (Melbourne.)

Edinburgh and East of Scotland College of Agriculture. Calendar for 1922–1923. Pp. 77. (Edinburgh.)

The North of Scotland College of Agriculture. Calendar, Session 1922–23. Pp. viii+145. (Aberdeen.)

The North of Scotland College of Agriculture: County Extension Department. Report on County Extension Work, 1921–22. Pp. iv+52. (Aberdeen.)

Ministerio da Agricultura, Industria e Commercio: Directoria de Meteorologia. Boletin Meteorologico: Anno de 1914. Pp. vi+121. (Rio de Janeiro.)

Imperial Department of Agriculture for the West Indies. Report of the Agricultural Department, Antigua, 1920–21. Pp. iv+19. (Barbados.) 6*d.*

Memoirs of the Indian Meteorological Department. Vol. 23, Part 4: The Effects of Oscillations and of "Lag" on the Readings of the Kew Pattern Barometer. By Dr. E. P. Harrison. Pp. 137-144+2 plates. (Calcutta: Government Printing Office.) 1·8 rupees; 2*s.*

Report on the Operations of the Department of Agriculture, Madras Presidency, for the Official Year 1920–21. Pp. ii+28+4. (Madras: Government Press.) 4 annas.

Report of the Government Chemist upon the Work of the Government Laboratory for the Year ending 31st March 1922; With Appendices. Pp. 33. (London: H.M. Stationery Office, 1922.) 1*s.* 6*d.* net.

Museums of the Brooklyn Institute of Arts and Sciences. Report upon the Condition and Progress of the Museums for the Year ending December 31, 1921. By Wm. Henry Fox. Pp. 56. (Brooklyn, N.Y.)

Rendiconti delle Sessioni della R. Accademia delle Scienze dell' Instituto di Bologna. Classe di Scienze Fisiche. Nuova serie, Vol. 24, 1919–20. Pp. 152+xxviii. Nuova serie, Vol. 25, 1920–21. Pp. 155+xxxvi.

Prospectus of University Courses in the Municipal College of Technology, Manchester. Session 1922–23. Pp. 219. (Manchester.)

Air Ministry: Meteorological Office. International Meteorological Committee. Report of the Eleventh Ordinary Meeting, London, 1921: And of Meetings of the Commissions for Weather Telegraphy, Maritime Meteorology, Aerial Navigation, Réseau Mondial, and Polar Meteorology. (M.O. 248.) Pp. 128. (London: H.M. Stationery Office, 1922.) 4*s.* 6*d.* net.