THURSDAY, APRIL 22, 1909.

MAN'S HAIRY COVERING.

Beiträge zur Naturgeschichte des Menschen. Lieferung i., Das Wollhaarkleid des Menschen (7 coloured and 3 uncoloured plates); Lieferung ii., Das Dauerhaarkleid des Menschen (6 coloured and 7 uncoloured plates); Lieferung iii., Geschlechts- und Rassenunterschiede der Behaarung, Haaranimalien und Haarparasiten (9 coloured and 4 uncoloured plates); and Lieferung iv., Entwicklung, Bau und Entstehung der Haare, Literatur über Behaarung (7 coloured plates). By Dr. Hans Friedenthal. Pp. 31+39+49+57. (Jena: Gustav Fischer, 1908.) Prices of volumes: 10, 20, 20 and 15 marks respectively.

THE distribution of the hair, its characters, and the curious phases of its growth present such obvious features of contrast between man and the other hair-clad vertebrates, as well as such marked differences in the various races of mankind, that they have formed a very frequent theme—the author of the work before us quotes the titles of more than 1270 memoirs, and says that the list is far from complete!—for the anatomist, zoologist, and anthropologist. Moreover, the anomalies of growth and distribution of hair are often forced upon the attention of pathologists and medical practitioners.

The author of this bulky monograph on the human hair calls his work a research on the physiology of "Behaarung," and explains his purpose by the statement that a knowledge of mankind which deals with morphology only and does not include physiology in its scope cannot be other than partial and unsatisfactory. His aim in this work has been to explain the interdependence of structure and function; to show that the position of man as a being set apart from other mammals, so far as many features of his hair equipment are concerned, is correlated with the correspondingly distinctive nature of his φύσις; and to indicate that anthropology is a field of research for the physiologist.

It is a well-known fact that the growth and distribution of the hair may be strangely influenced by internal secretions, especially of the genital glands. The development of the distinctive arrangement of the "terminal" hair at puberty is determined by the activity of these glands. Premature stimulation of the ovary, as, for example, by a malignant growth, leads to a precocious development of pubic hair. Malformations of the generative organs are sometimes associated with an altered distribution of hair resembling that of the other sex. After the hair distinctive of sexual maturity is fully developed, the ovary seems to exercise a restraining influence on the further growth of the body-hair (in contradistinction to the influence of the testicle in the male), for when the influence of the ovarian secretion is withdrawn at the menopause there is often a renewed activity in the growth of hair on the face and body in women.

But the physiological study of hair is not limited to the examination of such phenomena. According to Dr. Friedenthal, the intimate relationship that exists between the hair and the nervous system is responsible for the result that the emotional state of the individual is able to exert an influence on the growth of hair by reflexly affecting the blood supply of the hair roots. Moreover, in addition to this little-recognised relationship between hair-growth and the emotional life, there is a further intimate correlation between man's mental isolation and his physical isolation as a relatively hairless primate. The hairy covering of the body, which is necessary for the protection of most mammals, interferes with the sensitiveness of the skin as a tactile organ. By such an argument Dr. Friedenthal pretends that the height of man's intelligence is associated with his isolation among hairy mammals as a relatively hairless being, because the fulness of his mental life stands in intimate relationship with the number of impressions pouring into his brain. I need not follow him in his further flights into the psychological significance of hair, except to mention his curious conception of one of the uses of the woolly hair (lanugo) of the unborn child as an instrument for "reinforcing the feeling of contact between mother and child" and awakening the maternal instinct!

On the purely morphological side the author has made some very interesting observations. At the present time, when Schwalbe, Kohlbrugge, and Dwight are suggesting doubts as to man's affinity to the apes, the author is justified in emphasising once more, not only their general points of identity of structure, and especially the striking similarity in the arrangement of the hair, but also the positive evidence of a "blood-relationship" which the biological precipitin tests of blood afford.

There is a striking resemblance between the distribution and limits of the absolutely hairless skin areas in man and the anthropoid apes. However, the skin on the back of the ungual phalanges of both fingers and toes and on the outer part of the back of the foot in the human fœtus is quite free from the hair rudiments which are found in the chimpanzee in these situations. The distribution of the temporary hair (lanugo) of the human fœtus presents the closest resemblance to that of the permanent hair of the American apes, both Cebidæ and Hapalidæ; whereas the distribution of the hair which develops in the human being at the time of sexual maturity recalls that of the overgrown hair-tufts of the old-world apes. In a series of other features the human hair is disposed like that of various apes, in contradistinction to the arrangement found in other mammals, not excluding even the lemurs.

The work treats in considerable detail of the nature and significance of lanugo; the racial and sexual variability of the permanent hair, which develops in early childhood, and the "terminal" hair, which develops at puberty or during the period of maturity; the texture of the various kinds of hair and its mode of insertion in the skin, its coloration, its anomalies of distribution and of excess or defect, the changes it undergoes in old age, and the parasites that may populate it at various times of life.

The large number of illustrations, many of them excellently executed, is probably the reason for the large, unwieldy quarto form of these volumes and their considerable price. If the text had been printed in type of the size usually adopted in scientific works, and many of the wholly unnecessary and offensively-coloured illustrations of the nude human figure had been omitted, the book could have been produced in the form of a small and cheap octavo volume. In such a form the mass of valuable and often suggestive information which it contains would have been made available for a much larger body of serious students, to many of whom the present volumes will be inaccessible by reason of their cost.

G. ELLIOT SMITH.

THE HABITABILITY OF MARS.

Mars as the Abode of Life. By Percival Lowell. Pp. xx+288. (New York: The Macmillan Co.; London: Macmillan and Co., Ltd., 1908.) Price 10*s.* 6*d.* net.

WHEN a worker in science devotes a considerable portion of his life to a definite piece of research work, and enriches his science with a series of valuable publications embodying the details of such an inquiry, he renders a good service to mankind at large by expounding the main results of his investigation in general and popular form.

It is not often that the investigator is able to accomplish both of these, but in Prof. Lowell we have a man who is capable of bringing to a successful issue the one form as well as the other.

The title of the book under review is sufficient to inform the reader as to the lines on which Prof. Lowell has treated the interesting subject-matter concerning the planet which he has made his own. While his chief energies have been devoted to learning as much as possible about Mars when favourably situated, he has by no means ignored the opportunities afforded him of minutely studying the physical features of the other planetary members of the solar system. Such a general survey has thus enabled him to make an interesting comparison of the conditions on Mars with those seen on the other planets, and thus form an idea of the different stages of evolution in a planet's life as represented by members of our system.

It will be remembered that in the author's work entitled "Mars and its Canals," which was published in the year 1906, he was led to formulate the opinion that Mars was inhabited by beings of some sort or other, which he considered as certain as it was uncertain what those beings may be.

This view was in opposition to that formulated by Dr. Alfred Russel Wallace, who, in his book entitled "Man's Place in the Universe," published in 1903, claimed that there were enormous probabilities in favour of the earth being the only inhabited planet of the solar system, and, further, that the probabilities are almost as great against any other sun possessing inhabited planets.

Since both the above books were published, a very important fact has been observed which must undoubtedly alter some of the conclusions drawn. While Dr. Wallace held that Mars had not sufficient mass to retain water-vapour, and that the polar snows were caused by carbonic acid or some other heavy gas, Prof. Lowell was almost convinced that the dark rifts round the caps when they were in the process of melting were caused by water from the melted ice.

Recent spectroscopic evidence produced by Mr. V. M. Slipher has, however, shown that there is undoubted evidence of water-vapour in the atmosphere of Mars.

Granting, therefore, the presence of water-vapour in the Martian atmosphere, the observed changes on the planet can be more easily and naturally explained than the assumption of other matter the behaviour and effects of which are not so familiarly known. Thus the seasonal change of colour of the different portions of the planet Mars is readily associated with the melting at the two poles, thus giving rise to the seasonal variability of the canals as exhibited by Lowell's *cartouches*.

In the book before us the arguments used are in the main to show that Mars can be an inhabited planet, and the canals and oases, according to Prof. Lowell, are proofs that life of no mean order prevails there.

Thus in his final paragraphs he writes:—

"Part and parcel of this information is the order of intelligence involved in the beings thus disclosed. Peculiarly impressive is the thought that life on another world should thus have made its presence known by its exercise of mind. That intelligence should thus mutely communicate its existence to us across the far stretches of space, itself remaining hid, appeals to all that is highest and most far-reaching in man himself. More satisfactory than strange this; for in no other way could the habitation of the planet have been revealed. It simply shows again the supremacy of mind. Men live after they are dead by what they have written while they were alive, and the inhabitants of a planet tell of themselves across space as do individuals athwart time, by the same imprinting of their mind."

In the very brief interval of time in the evolutionary history of a planet, when the conditions are such that life in some form or another can exist, that interval, in the case of Mars, is approaching an end. The one great aim and object of the whole of the intelligent minds on Mars is concentrated on making the utmost use of the slowly diminishing water supply, and, as Prof. Lowell finally remarks, "the drying up of the planet is certain to proceed until its surface can support no life at all."

Our earth, fortunately, is not in such an advanced stage of its own life-history that like measures are necessary, but undoubtedly the time will come when all nations will have to work together to one common end, namely, to survive at all.

In the volume before us, which may be looked upon as a delightful essay on the birth and development of worlds, Prof. Lowell has presented us with a vein of thought which will appeal to a very wide circle of readers. Technicalities are avoided as much as possible, and when more detailed information is required the notes brought together in the second part of the volume can be referred to.

AN ATLAS OF THE EMPIRE.

The British Empire (and Japan). Its Features, Resources, Commerce, Industries, and Scenery together with the Physical and Economic Conditions of the World. By W. Bisiker. 213 maps and 272 illustrations. (London: The Geographical Publishing Company, 1909.) Price 1*l.* 1*s.* net.

THE author offers this volume "as a contribution to 'Education and the Empire,'" and since his contribution has taken the shape of an atlas, presumably he had in view geographical education. Now while the British Empire, as such, might well enter into college or university curriculum as an historical subject, it cannot be treated in a geographical course. The Empire is not a geographical unity; from a geographer's point of view it is a heterogeneous collection of the whole or parts of widely different natural regions. We must treat of fragments, large or small, of tropical West Africa, of an isolated scrap of South America, and a similar arbitrary selection from other continents. The majority of the colonies and dependencies of the Empire cannot be geographically treated apart from the regions to which they belong. To attempt to carry the criterion of political ownership into geography is, to say the least, unscientific. The author admits the necessity of considering the economic productions of the entire world as a basis for the study of British trade. In that he is right, but surely the only true understanding of the Empire and the right conception of its place in the world must be reached through a study of the geography of the entire globe.

However, if this atlas falls short of educational requirements it will certainly prove of great service as a work of reference, especially for commercial purposes. Each of the large regions within the British Empire has two maps devoted to it, a photo-relief map and an ordinary political one. The former are finely executed and very instructive, but we doubt if they have as great a value as good contour maps. However, the physical names which they bear have been wisely chosen, and—a feature of geographical value—the railways are shown in relation to the surface relief. Submarine relief is well portrayed in these maps. In addition there are pressure, temperature, and rainfall maps, and various small economic charts for each region, all crowded with information graphically or statistically displayed. Each colony is illustrated by several small views, but these have often more artistic than scientific value. A number of general physical and economic charts of the world, most of which are too crowded and small to be instructive, complete the atlas, except for two pages devoted to Japan. We do not understand why that country alone of extra-British lands should have been included. The author would surely have been better advised to include the United States of America as a country the commercial interests of which lie nearest to those of Britain.

The statistical information, if rather condensed and summary, seems to be thoroughly up-to-date, and, so far as we have tested it, accurate. But a little expansion in this direction might not have been out of place. The bare statement, for instance, regarding Ireland's total trade, that it was in 1907 17,767,657*l.*, might be misleading without a qualifying note that this refers only to trade with lands beyond the British Isles. In reality Ireland's total trade was (1906) more than six times that figure. The index to commercial products is too meagre to be of much use, and should have been considerably expanded.

One or two minor errors should be pointed out. The South Orkney and South Shetland Islands, despite recent assertions to the contrary, are not British, but Argentine possessions. South Georgia is used as a whaling station, and exploited for its sea-elephants and penguins rather than "as a field for mining" (p. 56), though gold and coal have been reported. King penguins do not breed on the Antarctic continent (p. 55). On plate 44, Fig. 18, the house shown is not, as stated, Napoleon's dwelling at St. Helena, but quite another building. The statement that the Nile floods are caused chiefly by the Blue Nile (p. 53) does not convey the whole truth, for the Sobat and the Atbara largely contribute. Nor is it quite accurate to assert that pearls are formed "round grains of sand or other hard substances," since they are generally formed round encysted larvæ of parasitic worms; and we are at a loss to understand who the Buddhists are who figure so largely in the south-western United States on plate 15. However, these are small points, and care and thought have evidently been expended on the work. But a less restricted outlook would undoubtedly have enhanced the value of this atlas. A cheaper edition at 16*s.* seems only to differ in the binding.

INDUSTRIAL ELECTRICITY.

Électricité Industrielle. By C. Lebois. Deuxième Partie, Deuxième Édition. Pp. 437. (Paris: Ch. Delagrave, n.d.) Price 4 francs.

THE author is Inspector-General of Technical Instruction in France, and on the title-page we read that his work has been honoured by a subscription by the Ministers of Commerce and Technical Instruction. In these circumstances the reader may expect a book of exceptional merit, but in this expectation he will be disappointed. The book is no better and no worse than scores of others with which the market nowadays is flooded. The subtitle is "Second Part; Complementary Study of Continuous and Alternating Currents and Their Applications."

This subtitle describes sufficiently the contents. We find the usual explanation of the generation of an E.M.F. in the wires of an armature moving in the interpolar space, various armature windings, some examples of brush gear, different forms of magnet frame, the calculation of the magnetisation curve, formulæ for the E.M.F. and torque of a machine, the latter called a new formula, although it is certainly not new to English readers, some hints and examples on the design of continuous-current machines, and then a similar treatment of alternating-current machinery, including synchronous and non-synchronous motors, for which the author has coined the name "alternomoteurs."

Further, there are chapters on transformers, measuring instruments, meters, and other accessories. The industrial application of the science is represented by examples of machinery made by French firms, some descriptions of transmission plant and wireless telegraphy. This short account of the contents will show that the book covers, within its compass of some 430 small octavo pages, a wide field, and that for this reason alone anything like exhaustive treatment cannot be expected. Its usefulness is also marred by the defect very frequently found in Continental books of having no index.

In one respect the book is, however, an improvement on other French works on the same subject, and that is the use of mechanical illustrations of electrical phenomena. French men of science have always been adverse to graphic treatment or mechanical analogies. They are content to represent the subject in a purely analytical manner, and although it must be confessed that in elegance of mathematical treatment the French school is supreme, this kind of treatment does not lead so easily to an understanding of the subject as the use of graphic methods and mechanical analogies, which is a characteristic of the English school. Even so highly-trained a mathematician as Maxwell did not disdain the use of some very simple mechanical contrivance in order to make clear an intricate electrical phenomenon, and since Maxwell's time all English writers and most German have followed this lead.

Now we find that the author of the book under review has also gone over to the school of Faraday and Maxwell, and uses mechanical analogies to express electrical processes. As a good example of his methods may be taken the vectorial addition of currents illustrated by the apparatus of Prof. Gaillard, which was primarily designed to illustrate an alternating current of so slow a periodicity that it can be shown by the harmonic movement of a spot of light to a whole class of students (p. 185). Another model to represent three-phase currents and their properties is shown on p. 311. The mechanical representation of the principle of the inductor alternator, although, strictly speaking, not a model, but merely an incomplete machine, should prove useful to beginners.

The book is, in fact, written for beginners, if we may judge by the omission of many matters of more intricate nature. Thus, after explaining the process of commutation in a general way, the author dismisses the subject of sparking in a few lines by saying that in modern machines there is hardly any necessity to shift the brushes when the load changes. Nothing is said about commutation by brush resistance or interpoles, or Deri winding, or Parsons' compensating coils. Again, the short paragraph on inductive drop in a transformer is quite inadequate; we are told that the drop is from 1 to 1½ per cent. in each coil, but not a word is said about the influence of the details of the design on the drop. In the matter of cooling a transformer, the author is equally superficial; he merely says that 20 sq. cm. cooling surface per watt lost will produce an admissible temperature rise. Such general statements are perfectly valueless, and, in fact, worse than that, for they are untrue.

The author seems to have a great aversion to the use of mathematical formulæ even when they are very simple and convenient. He seems to start from the supposition that his reader is so much of a beginner that he cannot even grasp the meaning of a very simple analytical expression, and to overcome this imaginary difficulty he uses numerical examples by preference. Most readers will consider this point of view to be wrong in principle. A man who is quite ignorant of even the simplest mathematics had better not attempt to study electrical matters, and if he has the modicum of mathematical knowledge required for the study of such elementary books as that under review, his task is not made easier, but more tedious, if matters that could be presented in three lines of mathematics are worked out in two pages of numerical examples. A striking instance of the cumbersomeness of this method is the deduction of the virtual value of an alternating current given on pp. 174 to 178. Here more than four pages of algebra and arithmetic are used to prove that the virtual current is equal to the crest value divided by the square root of 2. All this could have been shown by a few lines of very simple calculus, or, better still, by Blakesley's graphic method. GISBERT KAPP.

A GERMAN TEXT-BOOK OF ZOOLOGY.

Lehrbuch der Zoologie für Studierende. By Dr. J. E. V. Boas. Fünfte vermehrte und verbesserte Auflage. Pp. x+668; 603 figs. (Jena: Gustav Fischer, 1908.) Price 12 marks.

THE fact that Prof. Boas's well-known text-book has now reached its fifth edition speaks volumes for the importance attached to the study of zoology in Germany. The book, although it contains 668 large and closely-printed pages, is an elementary one, and is designed especially, as we are told in the preface, for students of medicine, veterinary science, and forestry.

German ideas as to the preliminary education of medical students must be very different from those which are held by the medical profession in this country. Perhaps the German students work harder, or it may be that they cover a wider field in a more superficial manner. Dr. Boas's text-book makes us suspect that it is a little of both, and although we think that the subject might well receive more attention from English medical students than it now does, yet we should hardly care to place the present volume in their hands. Excellent and interesting as it is in many respects, it appears to us to suffer greatly from over-condensation, from the attempt to cover far too much ground. We miss the detailed anatomical description of types to which English students have become accustomed, and although this can easily be, and we fear frequently is, overdone, it can hardly be altogether dispensed with in an elementary text-book. It is true we find a short description of the Amœba by way of general introduction to the study of structure and function, but this is the only special type which is at all adequately dealt with. Probably it is intended that the detailed study of types should be undertaken in the laboratory with the aid of a special practical text-book, but we have not noticed any reference by the author to the importance of such practical work.

The book illustrates very clearly the great difficulties which attend the teaching of zoology at the present day, and which are due, in the first place, to the enormous extent and variety of the animal kingdom, and in the second place to the many different points of view from which the subject may be approached. No elementary book can deal adequately with the entire field. In the present work, for example, the problem of heredity, which is of vital importance to medical students, is dealt with in a single page, while five pages are devoted to a general account of the Coleoptera. We should have thought that the medical student would have found the former altogether insufficient and the latter superfluous, and that a forestry student would require to know far more about beetles than can be compressed into five pages. Probably the latter studies entomology later on as a special subject, but if so it seems hardly necessary to attempt to deal with it systematically in his preliminary course.

We have already realised in our own country that systematic zoology, as such, is of very little use to medical students, and there can be no doubt that the insistence, in former years, upon an unnecessary degree of intimacy with the animal kingdom has done much to discredit the subject in the eyes of the medical profession, and has brought about a reaction which threatens to remove both zoology and botany from the medical curriculum. This, of course, would be a disastrous error. Medical studies must have a scientific foundation. The human body cannot be rationally interpreted except as the last link in a long chain of animal forms stretching back to the Protozoa. If the study of anatomy and embryology is to be inspiring it must be comparative. Scientific physiology must be founded on some knowledge of the lower animals, and the problems of heredity cannot be solved from the merely medical point of view. The zoology which is offered to medical students needs to be rigidly selected with such ends in view, and in this way only can the matter to be studied be kept within reasonable limits. In the book before us we cannot help feeling that the distinguished author has been unable to do justice either to himself or to his subject, but at the same time it is evident that his work has met with much appreciation in Germany. The numerous and excellent illustrations form a striking feature of the book. A. D.

SOME NEW CHEMICAL BOOKS.

(1) *An Organic Chemistry for Schools and Technical Institutes.* By A. E. Dunstan. Pp. viii+160. (London: Methuen and Co., n.d.) Price 2*s.* 6*d.*

(2) *An Intermediate Course of Laboratory Work in Chemistry.* By E. K. Hanson and J. W. Dodgson. Pp. viii+124. (London: Longmans, Green and Co., 1908.) Price 3*s.* 6*d.*

(3) *Laboratory Notes on Industrial Water Analysis. A Survey Course for Engineers.* By Ellen H. Richards. Pp. iii+49. (New York: John Wiley and Sons; London: Chapman and Hall, Ltd., 1908.) Price 2*s.* net.

(1) MR. DUNSTAN'S organic chemistry is intended for the use of the higher forms of schools and as a first-year course in technical institutions. Although the author disclaims writing to a syllabus, he thinks his book may be useful as a preparation for certain examinations, and especially for evening students connected with chemical industry. There are so many elementary text-books of organic chemistry at present available that one naturally looks for some special feature which may distinguish one from another. In the present case the fusion of the aliphatic and aromatic series is a somewhat new departure. As systems of classification of organic compounds are mainly matters of convenience, it is questionable whether any real advantage is offered by the new arrangement. The parent hydrocarbons of the two series, as well as the majority of their derivatives, present such marked differences in properties that their separation seems to us almost a natural one. The new system has, however, no serious significance, and does not detract from the sound merits of the book, which is clearly written, and illustrated by numerous experiments and plain outline drawings of apparatus.

We would direct the author's attention to a few inaccuracies. The definition of organic chemistry as "the chemistry of compound radicals" (p. 17) belongs rather to the past than to the present; it is not quite correct to say that Russian petroleum contains no paraffins (p. 79); the explanation of specific rotation is misleading (p. 98); the formula for copper acetylide is incorrect (p. 109), and there is something wrong about the two formulæ for sodium ethyl malonate numbered (1) and (2) on p. 117, which seem to be identical. The two space formulæ for *d*- and *l*-tartaric acids are not enantiomorphous but identical, and represent the *meso*-form, whilst the one on the following page, which is intended for the *meso*-acid, is in reality one of the active forms (p. 124). The differences are most easily recognised by means of models. Acetoxime is twice spelt wrongly on p. 148.

(2) Messrs. Hanson and Dodgson's intermediate course is intended for students preparing for the intermediate science examination of the London University. It consists of series of simple preparations of inorganic and organic compounds, which are followed by exercises in volumetric and gravimetric analysis and qualitative analysis. There is little that calls for criticism. The preparations are well selected, and cover a variety of operations and reactions, and the analytical exercises are thoroughly representative and instructive.

If it were not "assumed throughout that the student is not working by himself, but can obtain the advice and assistance of a teacher at all times," one might feel disposed to suggest the addition of equations to explain the different preparations, and of some reference to the use of the balance and the graduation of the volumetric apparatus.

We would also suggest that the yield in each preparation should be roughly estimated.

It is scarcely correct to describe acetone as a pale *yellow* liquid (p. 29), or the acid from olive oil as a *solid* (p. 30).

Photography applied to glass apparatus is rarely satisfactory. Simple outline or shaded drawings are much more convincing, and the teacher, it is to be feared, will be called in to explain many of the illustrations.

(3) The laboratory notes on water analysis are intended for the use of engineers, who, it is presumed, have already received a sound training in practical and theoretical chemistry. For there are no equations or explanations of the reactions involved in the various processes, which are described in the briefest manner, so briefly, indeed, that we should doubt if some of the operations could be successfully carried out. Thus, the reader is told (p. 19) to "neutralise with 1 c.c. of the reagent and compare the standards," without other reference.

It seems unnecessary and merely confusing to introduce indiscriminately both centigrade and Fahrenheit scales, and an over-elaboration to count the drops of a reagent the strength of which is not given (p. 20).

It may also be pointed out that the method described as Dr. Thresh's (p. 21) is usually known as Forchhammer's or Tidy's process. J. B. C.

OUR BOOK SHELF.

Mental Pathology in its Relation to Normal Psychology. A Course of Lectures delivered in the University of Leipzig. By Dr. Gustav Störring. Translated by Thomas Loveday. Pp. x+298. (London: Swan Sonnenschein and Co., Ltd., 1907.) Price 10*s*. 6*d*.

THE bearing of the study of abnormal mental processes upon general psychological doctrine has long been understood. In some cases invaluable light may be thrown upon the normal nature of a complicated psychosis by the abnormal heightening or lowering in degree of one of its constituents; in other cases a pathological phenomenon may supply the "negative instance" that checks the harmful progress of a plausible but erroneous theory. Thus the leading pathological cases are familiar to English readers from their appearance in one or other of these capacities in the pages of several treatises on general psychology.

Nevertheless, Prof. Loveday is undoubtedly right in thinking that a systematic collection of such cases by a psychologist competent to select them judiciously, to describe them accurately but without unnecessary clinical detail, and to illuminate them by a cautious commentary, would be a useful addition to the student's library. Further, we believe him to be right in thinking that Dr. Störring's lectures prove that he possesses these qualifications in at least as high a degree as any other writer on the subject.

The besetting sin of the morbid psychologist is to erect elaborate and novel systems of interpretation upon a too narrow basis of fact. Dr. Störring avoids this fault, and exhibits a conservatism and restraint which will favourably impress even those who, like his translator, do not find themselves able to accept all his conclusions.

No one who is acquainted with the present unsettled state of psychological opinion upon fundamentals will be surprised to find himself frequently unable to agree with the author's view, or at least compelled to translate his interpretations into what he deems a more satisfactory psychological idiom. But in any case it remains true that on fundamental questions of psychological theory—such as the nature of perception and of the consciousness of self—and on questions of great importance in the practical science of pedagogy—such as the teaching of reading and writing, and the "training of the will"—Dr. Störring's cases (though they need supplementing and correction by more modern instances) throw a light the strength of which is due largely to the way in which the several rays have been disposed and concentrated.

It is doubtful whether the translator did well to decline the task of finding English equivalents for such Teutonisms as "disease-picture," which occur rather frequently in his pages. In a second edition he should certainly Anglicise the index letters of his diagrams, which are, as they stand, provokingly difficult to use.

The Evolution of the Atmosphere as a Proof of Design in Creation. By John Phin. Pp. 191. (New York: The Industrial Publication Company, 1908.)

ACCORDING to its subtitle, this work is "a simple and rigorously scientific reply to modern materialistic atheism," and, after perusing it, we find no reason to dispute the first portion of the description. But when we see "rigorously scientific," we feel inclined to question the accuracy of the descriptive phrase.

The purpose and tenor of the volume may be gathered from the following extract (p. 184):—"Any one who will carefully read the works of Haeckel, Tyndall, Huxley and men of that stamp cannot fail to see that their intense hatred of ecclesiasticism has swayed their logic, embittered their language and even led them to distort their facts when they came to write about anything relating to the religious faith taught in the churches."

The greater part of the book is taken up by definitions, and by the demonstration of simple scientific experiments illustrating the physical and chemical properties of the atmosphere, the idea being to show that, had not an intelligent creator adjusted the proportions of terrestrial elements to the very finest conceivable degree, the atmosphere could not have been suitable for man's existence. That such creative design must have superintended the composition of the primitive nebula of the solar system, at least, and also its proper partition, is not stated by Mr. Phin, although to be "rigorously scientific" this aspect would, presumably, have to be considered.

The probable sequence of the evolution of the atmosphere is reasonably stated on lines similar to those indicated in Lockyer's "Inorganic Evolution." But the "proof" of design apparently consists of Mr. Phin's statement that, because man exists, therefore an intelligent designer mixed the eighty or so terrestrial—speaking more logically "cosmical"—elements in such proportions that, after all their combinations and dissociations, their expansions and condensations, there remained just enough nitrogen, oxygen, &c., uncombined, to provide an atmosphere exactly suited to the requirements of the preconceived organic life.

That such life might have developed with, say, even a little less oxygen, or even a little of the uncondensed sulphuric acid he mentions, and yet not have been radically different in form, is not considered by Mr. Phin; yet we know that one species, of one age and of one development, is able to exist under very different conditions of atmospheric pressure and composition.

The author concedes, for the moment, that previous "evidences" have been materially weakened by the theory of organic evolution, and gives that as his reason for considering "inorganic" phenomena, wherein Haeckel's "sexual cell-love" is, presumably, inoperative.

The readers to whom the book will appeal will no doubt feel reassured by the author's statement that, whilst betting or gambling for gain is immoral, "the throwing of dice . . . or the tossing of coins for the purpose of determining the scientific principles involved in the theory of probability" is innocuous.

W. E. ROLSTON.

Essays and Addresses. By the late J. H. Bridges. With an introduction by Frederic Harrison. Pp. xxi +307. (London: Chapman and Hall, Ltd., 1907.) Price 12*s*. 6*d*. net.

THE essays included in this volume (unobtrusively edited by Prof. L. T. Hobhouse) form an admirable memorial of one of the noblest spirits that have been touched to fine issues by the "religion of humanity." It is, naturally enough, chiefly as a splendid evangelist of the Positivist movement that Dr. Bridges is considered in the introduction—itself an interesting and illuminative essay—which Mr. Frederic Harrison has contributed to the book. But there is no reader, however unsympathetic with the Comtist propaganda, who could rise from the perusal of these essays without having acquired deep admiration for the earnestness and spiritual charm, the learning, worn lightly as a flower, and presented with extraordinary vividness and freshness, the wonderful industry, fecundity, and versatility of the man whose literary achievements were the fruit of the leisure hours of a busy physician and hard-worked Government inspector.

The scientific reader who first made Dr. Bridges' acquaintance as the learned and indefatigable editor of Roger Bacon will accept almost as a matter of course the masterly summary of his long study of the great Franciscan, delivered as a university extension lecture in 1903. He will find in the oration on "Harvey and his Successors" merely another delightful example of the combination of critical, historical, and expository powers that illuminated so effectively the "Opus Majus." He will be prepared also for the familiar knowledge of the mediæval world shown in the two essays on Dante. But in these latter essays, particularly in the one entitled "Love the Principle," he will have revelation of spiritual powers perhaps unsuspected and of the noblest type. Moreover, his progress through the book will constantly deepen the impression that, even more admirable than the ability, the industry, and the taste that made Dr. Bridges so interesting and instructive a critic of topics ranging from Thales to Calderon and Diderot, was the self-sacrificing enthusiasm ever burning at the core of his indefatigable life.

LETTERS TO THE EDITOR.

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Upper Air Temperatures.

WITH reference to Dr. Chree's letter published in NATURE of April 1, may I state that the conditions I postulated *were* violated in *every one* of the cases quoted in his former letter? The ascents were all made *after* sunrise and *before* sunset. They cannot, therefore, be fairly used as evidence to suggest that my conclusions were inaccurate in the direction of underestimating the influence of radiation. At the same time, it may be worth while to consider these cases.

Dr. Chree specified three occasions on which the differences between the temperatures recorded by two instruments of different types exceeded 2° C., the maximum differences being 2°·1, 2°·5, 2°·9 C. The first, as he stated, was probably due to a scale or zero error, one thermometer being continually below the other. The third, on which he lays most stress, occurred in an ascent at Uccle on February 7, 1907. The readings of both instruments agreed during the ascent, the greatest difference being 0°·8 C. (or 1°·1 C. at the highest point). During the descent larger differences occurred. Now, at the time of observation, the sky was covered by a veil of cirro-stratus, and it appears extremely probable that the instruments, in descending from cold to warmer, perhaps saturated, air, would be affected by condensation of ice-vapour. The difference in exposure and type, combined with the bad conductivity of hoar-frost, may quite reasonably account for the differences between the temperatures of the two instruments which arose when they left the isothermal region. In addition to this, at the time of maximum difference the downward velocity was about 10 m.p.s., and there would be some lag in the instruments. This descent was, in fact, exceptional.

In the second case, an ascent at Strassburg on the same day, the readings indicate a slight lag in one instrument until the lowest temperature is reached. The sudden passage to a relatively warm upper layer was accompanied by a sudden jump of 1°·2 C. in the difference between the readings of the two instruments. The type of instrument which shows the lower temperature is the same as that which showed the higher temperature in the Uccle descent. This is just what we should expect if the instruments passed from a saturated layer, in which they became covered with hoar-frost, to a drier region. There is no record of the upper clouds at Strassburg at the time of the ascent, but it occurred simultaneously with the Uccle ascent, so that the explanation is a possible one.

In an earlier letter Dr. Chree suggested the possibility of errors of ±10° F. in the instrumental records. In order to show as fairly and clearly as possible the errors that may arise, I have taken, for Munich, all the cases from January, 1907, to March, 1908, in which the readings from two types of instrument were obtained, and the following table gives the height of the ascent, the extreme differences that occurred, and the mean of the absolute values of the differences at all the points for which they are published. The types of instrument were the same as those considered by Dr. Chree.

Height in kilometres	Extreme values of T_1-T_2 (degrees C.)		Mean value of $\vert T_1-T_2\vert$
10·5	1·2°	-1·6°	0·7
10·5	0·5	-1·5	0·5
9·8	0·6	-1·3	0·5
14·8	0·6	-1·6	0·3
11·0	0·2	-0·6	0·2
12·4	2·4	-0·2	0·5
13·5	0·5	-0·2	0·2
12·7	1·8	-0·7	0·5
13·0	1·0	-0·2	0·3
17·0	3·5	-0·9	1·1
13·0	3·0	-0·6	0·9
12·9	1·3	-1·2	0·5
14·2	1·1	-1·1	0·5
13·4	1·5	-1·3	0·5
14·8	1·4	-2·8	0·8
16·0	1·7	-2·9	0·8

In interpreting these results, it ought to be borne in mind that they are chiefly from *ascents*, and include errors owing to lag, which could be largely eliminated in dealing with the observations. The records I have seen usually show that the thermometer, which is higher in the ascent, is lower during the descent, and that the lag occurs almost entirely in the worse instrument, so that the differences are representative of the absolute errors arising from this cause. Considering the very many sources of error to be guarded against, especially the difficulty of testing the instruments at very low temperatures under the conditions to which they are to be exposed, I can only regard these results as a tribute to the care and ingenuity displayed by those engaged in the experimental exploration of the upper air.

Dr. Chree does undoubted service in directing attention to the need for great care in testing and comparing instruments, but I think he is inclined to be a little unjust to those who are tackling the difficulties of upper-air investigation and nomenclature. These difficulties are exemplified by the examples he quoted and by a term which he himself accepts, apparently without demur, when he describes a phenomenon as an "inversion of temperature."

Personally, I am quite prepared to discard the term "isothermal" when another is suggested which is short, equally expressive, more accurate, and more characteristic. The greatest variation of temperature in a vertical direction in the region under discussion is about 20° C. in 14 kilometres. Usually it is much less. In the lower part of the atmosphere the variation is generally 60° C. to 70° C. in 10 km. The upper region is therefore *comparatively* isothermal. The rapid increase of temperature between 12 km. and 13 km. in the Uccle ascent of July 25, 1907, is an example of a phenomenon which occurs in the lower layer of the isothermal region in about one-third of the ascents. E. Gold.

Vienna, April 7.

The Greenwich Winter of 1908-9.

If the art of long-range forecasting is ever acquired, it will probably be through a careful study of past experience (of which we have now nearly seventy years' excellent *data* for Greenwich), including, among other things, what are known as "sequences," cycles (if any), and the relation of weather to solar, and possibly lunar, changes.

I propose to offer a few remarks on the state of available knowledge regarding winter, at the end of last autumn, and year, and its bearing on what followed.

By the end of November we had had only three frost days. After such autumns as the last, with all three months dry, December (as was pointed out in a table) has nearly always been mild (twelve cases out of thirteen). Thus it was pretty clear that the second half of 1908 would have less than the average of frost days (18). The actual number is 8.

The same table seemed to point to at least two of the three winter months being mild; and that is what happened, though, of course, with the cold of February the winter (proper) was slightly severe as to mean temperature and number of frost days (the latter 36, which is +3).

When the second half of a year has less than the average of frost days (18), the total, from September to May, is generally also under average (54). There are only three exceptions (1857–8, 1891–2, 1894–5) in a total of thirty-one cases. Thus it would be reasonable to expect that the total for the first half of 1909 would not be more than 46 (*i.e.* 54−8). There have been 42 up to the end of March, and it is possible the total may be a little in excess, conforming to the "exception" instead of the "rule" (a contingency which has often to be regarded). The number up to April 20 is 46.

If we indicate by a plus or minus sign the character (as regards frost days) of each winter (December–February) ending in a sun-spot *maximum* year, and the four winters following, we have the following table:—

Max. year	Max.	1	2	3	4
1848	+	−	+	−	−
1860	+	+	−	−	+
1870	+	+	−	−	+
1883	−	−	−	+	+
1893	+	−	+	−	−
1905	−	−	+	−	

We might here note (without pressing the point too much) that each of the first five groups has at least two *plus* values, and as the four winters 1905–8 show only one, a *plus* in the vacant place seemed the more likely. Further, one might show that the average for the first four vertical columns is a *minus* value; for the fifth, a *plus*. The winter, as stated, had an excess of three frost days.

Another point of view. We might fairly expect the next sun-spot *minimum* in 1912 (the series before is 1843, 1856, 1867, 1878, 1889, 1901). Then this year would be the third before. Consider how many frost days there were in the first half of the third year before previous *minima*. We find:—

	F.D.	Relation to average
1840	?	?
1853	42	+ 6
1864	40	+ 4
1875	42	+ 6
1886	55	+19
1898	21	−15
	Av. 40	+ 4

This points to a probable excess. The number to March 31 is 42.

After a very dry autumn the winter tends to be dry. Thus of the ten driest autumns, nine were so followed. The winter (December–February) was a very dry one.

I submit, then, that, on the threshold of last winter, there was reason to expect (1) December to be mild, and frost days in the second half of 1908 under average (18); (2) at least two mild months in winter (December–February); (3) frost days in first half of 1909 not more than 46, but (4) over average (36); (5) frost days in winter, over average (33); (6) a dry winter.

While most of the above facts were, I think, known to me at the outset, I quite agree that it is one thing to make a retrospective comparison like this, and another thing to predict successfully. Of many pieces of evidence, some may seem in conflict, and one has to try and judge which is the more weighty and trustworthy. I may further admit that the cold this year has a little exceeded what I looked for.

It may be useful, nevertheless, to direct attention to these comparisons, emphasising the fact that there *is* a large body of evidence (as I believe) in relation to the character of a coming season. It seems to have been too readily assumed, hitherto, that we have absolutely no light on the subject, and that any one venturing an opinion on an approaching season is, by that fact, declared a dreamer or a charlatan, his "hits" and his "misses" being alike mere chance.

That the winter season of 1908–9 would at least not be a very severe one (say, more than sixty frost days in September–May) I consider to have been provable by a strong consensus of facts.

Alex. B. MacDowall.

Fluorescence of Lignum Nephriticum.

With reference to Mr. Benham's letter in Nature of April 8 (p. 159), the following statements may be of interest.

The wood known as *Lignum Nephriticum* reached Spain probably about or before the middle of the sixteenth century. Monardes (1574) and Fr. Hernandez (about the same time) were familiar with the fact that a watery infusion of the wood in a short time assumes a blue colour, but they do not mention the peculiar dichroism of the infusion. This was described for the first time by Athanasius Kircher in his "Ars Magna Lucis et Umbræ" (1646), and, apparently independently of him, by Joh. Bauhin in his "Historia Plantarum Universalis" (1650).

The origin of the wood has so far remained obscure. Linnæus—and already Plukenet and Dale before him—referred it to *Moringa pterygosperma*, the horse-radish tree of India, but without reason, as was pointed out long ago. Researches, however, made at Kew within the last few weeks have convinced me that *Lignum Nephriticum* is the wood of *Eysenhardtia amorphoides*, H.B.K., a small Mexican tree or shrub of the order Leguminosæ. The blue inflorescence exhibited by an infusion of the wood of this tree is very brilliant indeed. O. Stapf.

Royal Botanic Gardens, Kew, April 16.

Morphology of the Enteropneusta.

In a paper "On the Morphology of the Excretory Organs of Metazoa: a Critical Review," recently published in the Proceedings of the American Philosophical Society (vol. xlvii., 1908), the author, Dr. T. H. Montgomery, states on p. 577, with reference to the Enteropneusta, that in Spengelia I described rudimentary pores along the whole trunk in 1899. What I did describe in this connection was a single pair of structures which I thought might represent a single pair of truncal canals and pores.

Dr. Montgomery says that he had not seen the original description, and consequently was unable to add further details. The work ("Zoological Results") can be obtained at an easy cost from the Cambridge University Press. Perhaps, however, the original description is not deemed worthy of perusal, although it would be charitable to assume that, like the proverbial egg, it must be good in parts. Arthur Willey.

Colombo, Ceylon, March 22.

GENERAL RESULTS OF THE METEOROLOGICAL CRUISES OF THE "OTARIA" ON THE ATLANTIC IN 1905, 1906, *AND* 1907.

We have already reported preliminary results of the expedition which we organised in 1905 for the study of the trade-wind and the anti-trade by means of free balloons the trajectories of which were determined by triangulation (Nature, vol. lxxiii., pp. 54–6, 449–50).

Since then two expeditions have been sent out on the Atlantic during the summers of 1906 and 1907, and Fig. 1 shows the regions which have been studied. As may be seen by the dates entered on the route of the *Otaria* (Fig. 1), many of the important points were visited in different years and at different times during the same year. This gives a much greater value to our conclusions, because the accidental phenomena are thereby, to a certain extent, eliminated.

In consequence of the investigations made by Prof. Hergesell on board the Prince of Monaco's yacht *Princesse Alice,* in the region between the Canaries and the Azores, this distinguished meteorologist was impressed by the existence of the north-west winds which he observed at a variable height above the surface winds, represented generally by the north-east trade. These north-west winds, which had not been observed before because the methods of aërial soundings have only been employed for a few years, do not themselves present marked peculiarities, since, as Dr. Hann has remarked, if one turns to the chart of isobars at different heights, which was published by M. Teisserenc de Bort twenty years ago, it is seen that at about 4000 metres there exists a barometric gradient extending from the Gulf of Mexico towards the north-east, a gradient which should in most cases, at these heights, produce currents from the west or north-west. In his communication to the Meteorological Conference at St. Petersburg, Prof. Hergesell questioned the existence of the ordinary south-west anti-trade, believing that these north-west winds were themselves a much deflected return branch of the equatorial current.

As we endeavoured to demonstrate by our first expedition of 1906, the anti-trade exists generally above the trade, and, as will be seen by the following results of the two later expeditions, the existence of the north-west winds is not incompatible with the presence of the anti-trade, and this fact should be emphasised. Of course, since the meteorological phenomena do not follow the regular zones that theory requires, but group themselves around barometric maxima having more or less ellipsoidal contours, it cannot be expected that the normal superposition of winds above the same place will be encountered every day. There are days, for example, when the north-east winds, ordinarily confined to a few hundred metres, extend up to five or six kilometres, or even more; in other cases a north-west current, superposed on the trade, encroaches more and more on the high atmosphere up to such a height that the balloons do not show any anti-trade. But the normal condition is easily deduced from the documents gathered by our three expeditions, and it occurs so frequently that each expe-

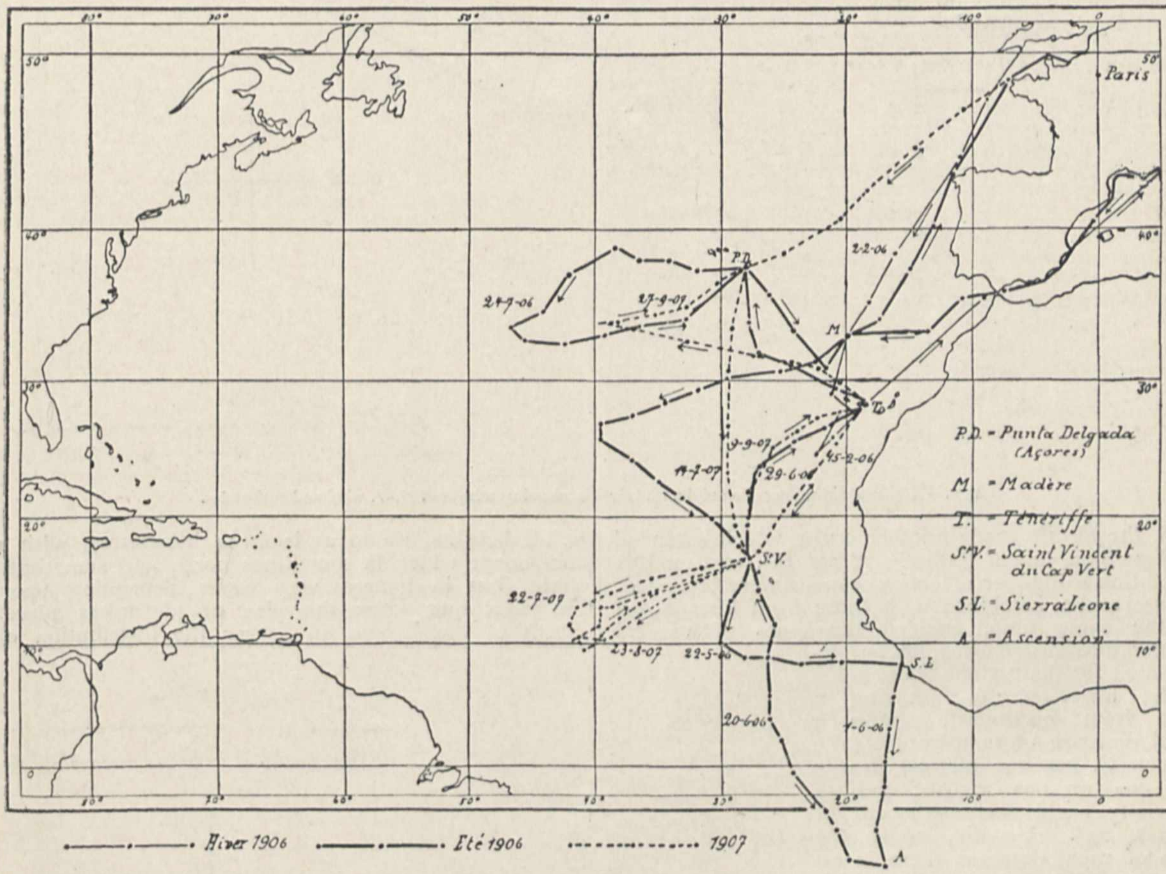


Fig. 1.—Map of the Cruise of the *Otaria.*

dition, considered individually, leads to the same conclusion. In general, the zone where the anti-trade is most regular appears to be situated to the eastward of the meridian passing through the centre of greatest pressure. To the southward of the maximum, and when it is very pronounced, northerly winds are frequent up to so great a height as eleven kilometres, which was the limit of observation here.

From the beginning of the year 1906 we were able to show by ascensions of pilot balloons, made over the open ocean to the south-west of the Canaries, that the winds with a southerly component, which we had already observed in 1905, also occurred far away from land, and even appeared to be more marked than near the islands.

During the months of May, June, July and August, 1906, the *Otaria* made another cruise which extended to Ascension Island (latitude 7° 55′ S.), and on this expedition a number of *ballons-sondes* were launched. These results, combined with those from kites flown at the same time, enabled the types of vertical temperature distribution to be determined for the regions traversed.

The lower stratum, having a drift from north-east, shows a rapid decrease of temperature (0°·9 to 1°·8 C. per 100 metres) in the first 500 or 600 metres, and an especially rapid decrease north of latitude 25°. Usually, above the zone of rapid decrease there is an inversion of temperature in which the wind velocity diminishes. Above the trade there is generally a north-west current (Fig. 3); then higher up, at about 2500 metres, near the Tropic of Cancer, and at 3000 or 3500 metres north of the tropic, there occurs a wind with a southerly component, except in the cases already mentioned. The direction of these winds possessing a southerly component, however, changes with the latitude, as might be expected from the effect of terrestrial rotation. They are south-east near 15° N. and west-south-west near 25° N., no doubt gradually changing from one to the other of these directions after passing by the south.

These same characteristics are found in the south-east trade, above which there are in general calm strata, sometimes of great thickness, then winds with a northerly component mixed with interlaced currents from the south-west, corresponding to the north-west winds of the northern hemisphere (Fig. 3). This region, however, has only been studied to about latitude 8° S.

At the limit of the two trades the winds are easterly at all heights, up to at least 14 kilometres, with a component which is sometimes north and sometimes south, but in general very weak, depending upon the exact spot where the rise of air takes place. North of the Tropic of Cancer the distribution of the winds becomes much more irregular, and it frequently happens that there is no anti-trade (Fig. 4). However, the regular *régime* of the trades appears to persist to about latitude 35° N. for places situated to the east of longitude 37° W. Farther west, winds from

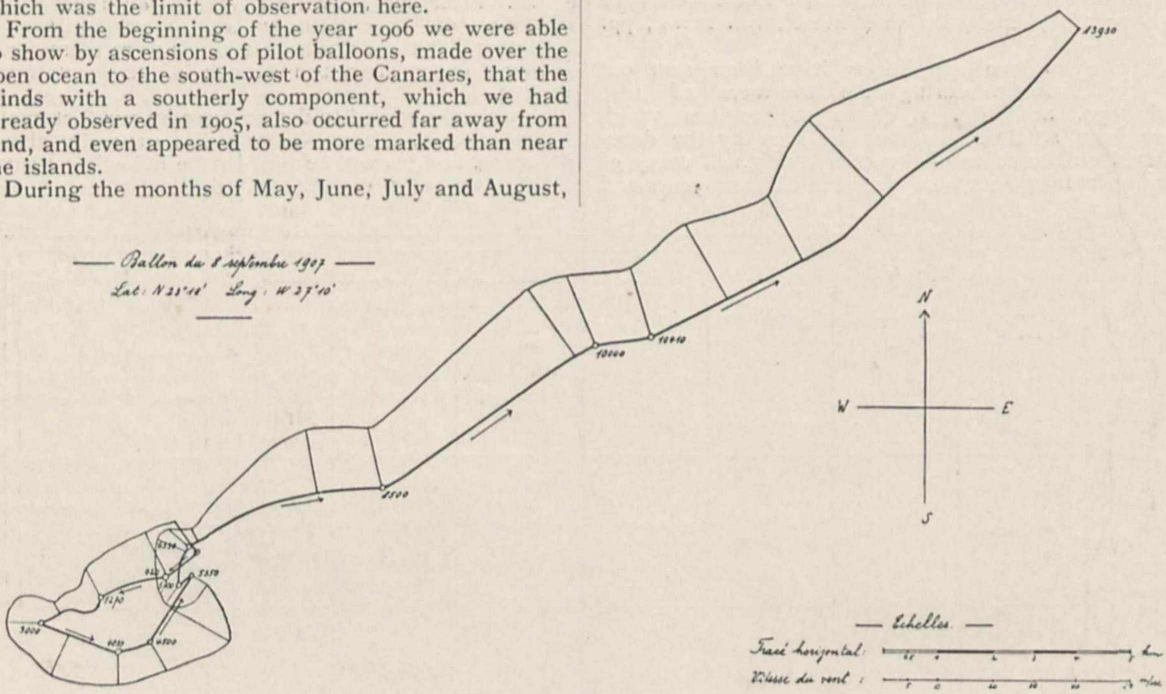


FIG. 2.—Motion of air in the region of the trade winds, showing a layer of N.W. wind and anti-trades.

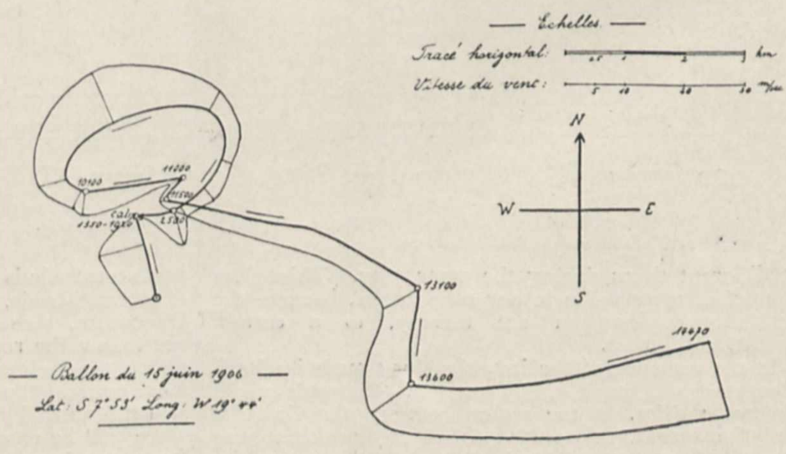


FIG. 3.—Mot'on of air at 7° south, showing S.E. trade winds and general eastwar l motion in upper atmosphere.

south to south-west prevail, which are explained by the distribution of the isobars.

Another campaign from July to October, 1907, gave the same general results, as well for the direction of the atmospheric currents as for the vertical distribution of temperature (Figs. 2 and 4). This voyage was not continued further south than 10° N., but the vessel remained twice during twelve days near this parallel, this latitude having been selected on account of the regularity of the trade wind. Again the easterly current predominated at all heights, and also the layer of inverted temperature near 1000 metres.

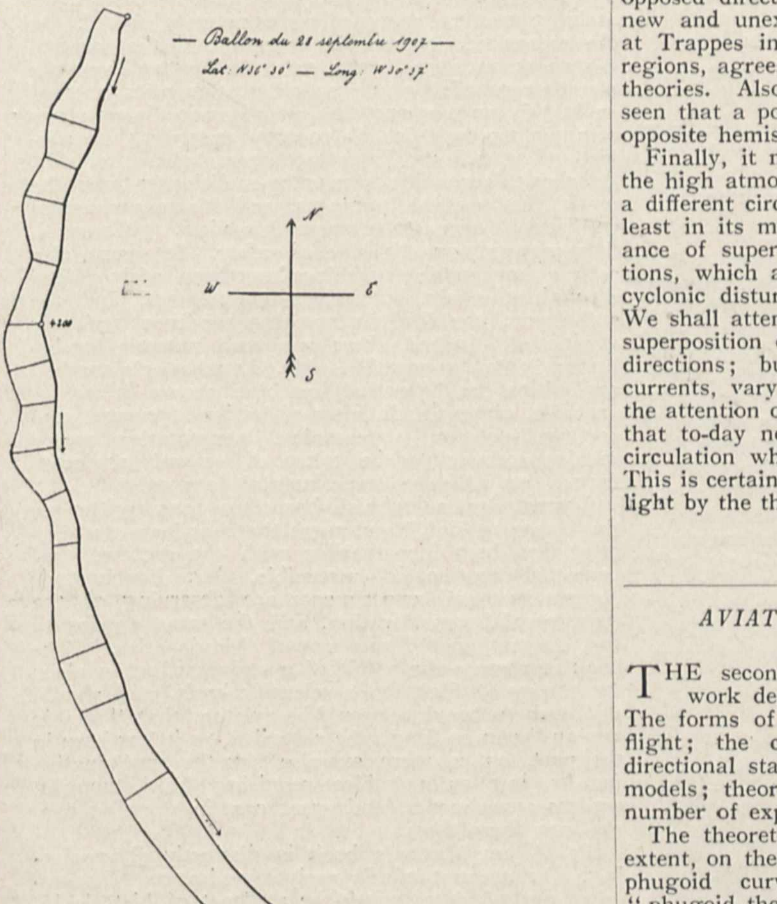


FIG. 4.—Motion of air in the atmosphere in the region of the Azores.

Note.—In all the figures the numbers give the altitude and the breadth of the lines the velocity of the wind.

The *ballon-sonde* ascensions made in 1907 were somewhat higher than before, and the isothermal layer was reached at 14 kilometres in latitude 25° 18′ N. North of 25° the isothermal layer was often met with at altitudes varying from 12 to 14 kilometres, while to the south of this parallel it was not reached, although the balloons many times exceeded 15 kilometres. Therefore, it appears that the altitude at which the temperature ceases to decrease is much greater near the equator than in moderate latitudes. This distribution of temperature is also confirmed for the regions of the Azores and Canaries, as far as about 25° N., by the ascensions of *ballons-sondes* made by Prof. Hergesell.

Our conclusions concerning the direction of the upper currents correspond in substance with former ideas about the anti-trades, apart from the situations which produce currents of very different and almost opposed directions lying one above the other. This new and unexpected fact, which was also observed at Trappes in the barometric maxima of our own regions, agrees tolerably well with certain of Maury's theories. Also near the equator it can be distinctly seen that a portion of the anti-trade comes from the opposite hemisphere.

Finally, it may be said that, if the exploration of the high atmosphere over the Atlantic does not show a different circulation from that already supposed, at least in its main features, it emphasises the importance of superposed strata flowing in various directions, which appear to persist in the regions where cyclonic disturbances of large diameter rarely form. We shall attempt later to give an explanation of the superposition of two or three strata having different directions; but the stratification of numerous thin currents, varying in their motions, is a fact worthy the attention of meteorologists, for we must recognise that to-day no theory explains this special mode of circulation which extends over a very large region. This is certainly one of the important facts brought to light by the three cruises of the *Otaria*.

L. TEISSERENC DE BORT.
A. LAWRENCE ROTCH.

AVIATION, MATHEMATICAL AND OTHERWISE.[1]

THE second volume of Mr. Lanchester's large work deals mainly with the following points:—The forms of the paths described by bodies in free flight; the conditions of longitudinal, lateral, and directional stability; the theory and use of scale-models; theories of soaring flight; and a large number of experimental verifications.

The theoretical discussions are based, to a large extent, on the consideration of what the author calls phugoid curves. According to the "Glossary," "phugoid theory" means "the theory dealing with the longitudinal stability and the form of the flight path," though in a footnote the author raises some doubt as to the appropriateness of the Greek derivative which he has himself coined. The simplest form of phugoid curve, to the study of which the author devotes considerable attention, might form the subject of problems that would delight the heart of the old-fashioned tripos examiner. Like the latter's particle on his perfectly smooth surface, the gliding body is supposed to have its mass concentrated at a single point and to travel without loss of energy, and the supporting surface is supposed to be small and to be always tangential to the direction of motion. In other words, the problem reduces to that of a particle acted on by gravity and by a supporting force (due to the air) which is always normal to the

[1] (1) "Aërodonetics." Constituting the Second Volume of a complete Work on Aërial Flight. By F. W. Lanchester. Pp. xvi+433. (London: A. Constable and Co., Ltd, 1908.) Price 1*l*. 1*s*. net.
(2) "Artificial and Natural Flight." By Sir Hiram S. Maxim. Pp. xv+166. (London: Whittaker and Co., 1908.) Price 5*s*. net.

direction of motion and proportional to the square of the velocity.

In view of the fact that classes on calculus for engineers form an integral part of every modern technical course of instruction, it is to be regretted that Mr. Lanchester has not made some attempt to bring his equations more into conformity with ordinary well-recognised notation. When he writes down the equation of his curve as

$$\cos\Theta = \frac{H}{3H_n} + \frac{C}{\sqrt{H}},$$

and the expression for the radius of curvature as

$$r = \frac{dL}{d\Theta},$$

his readers will take some time to find out what these equations mean; whereas any student who has attended the classes above referred to would understand at a glance the same equations if written as

$$\frac{dx}{ds} = \frac{y}{3v_0} + \frac{c}{\sqrt{y}} \text{ and } \rho = \frac{ds}{d\phi}.$$

When Mr. Lanchester applies his phugoid theory to investigate the longitudinal stability of aërofoils, he at once comes into conflict with the theory which the present writer, in conjunction with Mr. Williams, worked out some few years ago. There has been some difficulty in making out how Mr. Lanchester arrives at his results, and Mr. Harper has examined the matter independently. It was not intended at first to deal with what might be a controversial point in a review in NATURE, but the difference between the two methods is probably not so very difficult to explain.

According to our theory, the small oscillations about steady motion of an aëroplane, or indeed any body moving in a resisting medium in a vertical plane, depend on the roots of a biquadratic equation, and the conditions for stability are those given by Routh. This method enables account to be taken of every circumstance which may affect the stability, in particular, variations in the position of the centre of pressure for different angles.

Mr. Lanchester, on the other hand, considers only the case of a single aëroplane, the variations of the centre of pressure of which are not taken into account, stability being maintained by means of a tail. He starts with the assumption that his "phugoid" oscillations, when small, are simple harmonic in character, and that the effect of the moment of inertia of the machine, as well as of resistances, is to change the amplitude of these oscillations. In estimating the effect of these changes he assumes the equations of simple harmonic motion to hold good. For example, in considering the rotatory motion about the centre of gravity (§ 63) he writes

$$\tau_1 = \frac{2I\pi^2}{t_1^2} \times 2\Theta_1 = \frac{4\Theta_1 I\pi^2}{t_1^2},$$

where τ_1 is the maximum torque, Θ_1 the maximum angular displacement, t_1 the time of oscillation (calculated, it would seem, from the ideal "phugoid" motion), and I the moment of inertia.

This step is unjustifiable. The correct equation is

$$I\frac{d^2\theta}{dt^2} = \tau,$$

where τ is the torque at any instant. A similar consideration applies higher up, and when the necessary corrections have been made they are found to lead to the biquadratic equation of the Bryan-Williams theory.

It is as if, in working out the theory of the compound pendulum, it were attempted to treat one weight as a simple pendulum, and to assume that the motion of the other weight did not affect the period, or the relation between velocity and displacement, but merely produced variations of amplitude.

Mr. Harper has applied the Bryan-Williams method to the particular kind of tailed aëroplane considered by Mr. Lanchester, and he obtains a numerically different result, the discrepancy being accountable for by the assumptions contained in Mr. Lanchester's method.

There is thus a good bit of work of a theoretical character requiring to be done before the problem of stability can be regarded as completely solved. In the meantime, it must be remembered that airship designers have not, as a rule, undergone even the elementary training in practical mathematics referred to in this review, and that most extraordinary views commonly prevail in this country regarding the subject of stability. It is not improbable that Mr. Lanchester's conditions may be sufficiently near the mark for practical purposes, and his experimental verifications seem to support this view. Moreover, they may err on the side of safety. It seems also certain that unstable machines have been safely guided through the air by skilled manipulators, and the stability of the Wright machine has been seriously questioned. Indeed, there are good reasons for believing it to be unstable. Mr. Lanchester's method applied to the Lilienthal machine shows it to be unstable, although, in view of its broad curved supporting surfaces, a complete investigation would require account to be taken of several neglected factors for which no experimental data exist.

A great deal of rubbish has often been written on the "soaring bird," and much that has been stated in print has been incompatible with the doctrine that perpetual motion is impossible. Mr. Lanchester's observations and experiments are deserving of the most careful consideration, and the same applies to his chapter on "Experimental Aërodonetics." The book represents the result of a serious effort to place the theory of flight on a scientific basis, and should do much to convince would-be aviators that "airship design" can no longer be regarded, as it has been in the past, as a mere exercise for the imaginative faculty, but as a subject requiring hard thought, endless experiments, and great care in drawing conclusions from them.

Sir Hiram Maxim's book is distinctly disappointing. An account of his early experiments, if somewhat out of date, would be at least of historic interest; but when the author indulges in a tirade against mathematicians, the question which naturally suggests itself is, Where on earth does he find his mathematicians? He tells us that

"During the last few years a considerable number of text-books have been published. These have, for the most part, been prepared by professional mathematicians who have led themselves to believe that all problems connected with mundane life are susceptible of solution by the use of mathematical formulæ, provided, of course, that the number of characters employed are numerous enough."

Now Prof. Chatley, who certainly has got a pretty clear grasp of the present state of the flight problem, recently wrote:—

"A few great mathematicians (including Lords Kelvin and Rayleigh) have devoted some attention to the matter, but the author is not aware that any mathematician worthy of the name has considered it worth while to make an exhaustive study of the question. . . ."

When Sir Hiram says that "Up to twenty years ago Newton's erroneous law as relates to atmospheric resistance was implicitly relied upon, and it was not the mathematician who detected its error, in fact we have plenty of mathematicians to-day who can prove by formulæ that Newton's law is absolutely correct and unassailable. . . . " his information does not agree with the facts of the case. What about Kirchhoff's theory of discontinuous fluid motion, to mention nothing else?

Again, it is rather amusing to see mathematicians accused of demonstrating "by formulæ, unsupported by facts, that there is a considerable amount of skin-friction to be considered," when the usual complaint is that they will assume all their bodies to be perfectly smooth, and will not take account of frictional resistances in solving their problems. But some clue as to where Sir Hiram finds his mathematicians is afforded by his reference (preface, p. x) to a recent controversy in *Engineering*. Surely he cannot suppose that the authors of difficult mathematical researches would, as a rule, publish their best work in journals devoted to the interests of practical engineers, even if the editors would consent to print them! If he would consult the pages of journals and transactions devoted to researches in mathematics and mathematical physics, he would soon discover the paucity of papers to which Prof. Chatley refers.

On pp. 104 to 108 he publishes figures of stream-lines taken (so he says) from "mathematical" treatises, and all he is able to say is that "just how or why" the air moves in these particular ways is not evident. Now, in the first place, the diagrams show complex systems of eddies, the equations of motion of which no mathematician would ever attempt to integrate, and in the second place the question is not how the air is *likely* to move, but how it actually does move?

As an exponent of experimental *versus* mathematical methods, why did not Sir Hiram put the matter to a decisive and conclusive test by determining experimentally the form of the stream-lines produced in the neighbourhood of the various surfaces shown in these illustrations? Experimental, and in particular photographic, methods of plotting stream-lines are not difficult, and they can be conducted at a very trifling expense. Some of those who are, or have been, conducting such experiments are not altogether unmathematical in their methods. Surely Sir Hiram Maxim has missed a grand opportunity of scoring off his "mathematicians."

It was in 1894 that the author's gigantic experimental machine ran to and fro between rails. To all that has been done since that time only about five pages, including illustrations, are devoted in a chapter on "Some Recent Machines," and an equal number in a chapter headed "Balloons"; and yet the fifteen years that have just elapsed form the most eventful period in the whole history of artificial flight. It is the experimenters who have expended time and money, and have even sacrificed their lives, rather than the mathematicians, who have cause for disappointment at the scanty recognition they have received.

An address on "Recent Progress in Aëronautics," delivered before the engineering section of the American Association at Baltimore by Major George O. Squier, is published in *Science* for February 19. It is in the nature of a general summary, and deals both with balloons and aëroplanes, but the treatment of resistances on "arched surfaces" reveals an important gap in the experimental information dealt with in the address. It is tacitly assumed that the only effect of arching the surface is to increase the coefficient of resistance, the angle of flight being taken "to be the inclination of the chord of the surface to the line of translation." This would be all right if we were sure that the resultant reaction was always perpendicular to the chord, but it is pretty certain that such is not the case. If the aërocurve forms a circular arc, the resultant must (in the absence of skin-friction) pass through the centre of curvature, and if the centre of pressure is in front of the centre of the arc, the effective angle of flight will be less than the inclination of the chord, that is, the ratio of drift to lift will be less than the tangent of the inclination of the chord. Experimental information on this point is very scanty as a rule, a notable exception being Mr. Turnbull's investigations of plane, concave, convex, and doubly curved surfaces. Again, exception may be taken to the statement that "the helicopter type of machine may be considered as the limit of the aëroplane when by constantly increasing the speed the area of the supporting surfaces is continuously reduced until it practically disappears."

In his suggestions for "the stabilisation of aëroplanes" in *La Revue des Idées* (Paris, February 15), M. Étienne Maigre deals with lateral stability, and assumes that the lateral balance is to be maintained, not automatically, but by the voluntary or involuntary effort of the aviator. He suggests the use of two triangular surfaces attached to the main aëroplane and controlled by hand. He assumes Otto G. Luyties' law, according to which the normal resistance varies as $2 \sin a - \sin^2 a$, and finds a maximum lift for an angle of 37°.

Captain Renard has been giving a series of conferences before the Société d'Encouragement pour l'Industrie national, of which the first has appeared in the *Bulletin* (Paris, January). Captain Renard distinguishes six different methods of experimenting on air-resistance, including the use of experiments in water, with suitable allowances for difference of density. The need of further experiments in this direction is strongly emphasised.

It is to be noticed that the art of designing gigantic airships fitted with saloons, cabins, and mess-rooms has not yet faded away into past history, despite the recent advances in aëroplanes and dirigibles. About the beginning of February the *Standard* devoted more than half a column to an American project very suggestive of the *Minerva* of Robertson or the gigantic apparatus for which M. Petin raised 1000*l.* in the early days of ballooning, but for which the gas supply proved inadequate.

G. H. BRYAN.

DEW-PONDS.

A GROWING interest in the subject of dew-ponds has been exhibited in recent years, but it has yet to be proved whether there is actually such a thing as a true dew-pond. Dew-and-mist ponds there undoubtedly are, but dew and mist, similar in essence as they may be, are yet distinct and separate meteorological phenomena. The term "dew-pond" has arisen from the careless habit of assuming that every form of condensation of aqueous vapour which is not seen as rain or snow must be called dew.

The Journal of the Society of Arts (March 5) contains a paper on the subject by Mr. Geo. Hubbard, and he has therein endeavoured to show how artificial deposition of dew may be brought about in a pond. He maintains that by laying down a bed of straw beneath puddled clay, the water may be chilled sufficiently to cause the atmosphere to give out aqueous vapour as dew. In his earlier remarks on the subject, it was the chilling of the puddled clay to which he attributed deposit of dew. Of course, if a pond were fairly full there would be but little puddled clay exposed, so now he attributes the additional supply to the chilling of the water, and this, of course, presupposes that the pond is fairly full, otherwise there would be no water-surface at all to speak of. Perhaps it is intended that when empty the puddled clay commences the operation, and when the water has arrived this carries on the process. What evidence is there that this is so? The chief evidence is found in that ice is obtained in India by placing pans of water in shallow beds filled with rice-straw.

This is excellent so far as it goes, and Dr. Wells records that he performed the same experiment in England nearly a hundred years ago. Thus there is a presumption that if straw be laid down under a pond, it may act in such a way as to cut off the heat of the earth below. The difficulty lies in carrying out a similar process on a large scale, and at the same time in keeping the straw dry. Should it become moist, and it must do so if in contact with puddled clay, it will cease to be an efficient non-conductor of heat, and it must be borne in mind that whereas the straw under the ice-pans in India can be, and is, frequently changed, this cannot be done under a pond. Mr. Hubbard says:—"In numerous dew-ponds in this country the dew-point is reached without difficulty." Nothing is given in support of this statement, and we may well ask on what evidence it is based. It presumably means that the water itself reaches a temperature which is below the dew-point. A number of observations made by the writer have never yet revealed the fact. The water of a pond parts with its heat extremely slowly. To be of value toward the replenishment of a pond, the dew must be received in the height of summer, when there is but little rainfall, and when, as is admitted, the ponds at lower levels are drying up. The pond is heated during the day, and evidence is wanting as yet that it falls below dew-point at night. Of course, dew is being received on the grassy banks around, that is to say, on vegetation, but the radiating powers of water and grass are apart as the poles, and on radiation dew-fall, as we know it, depends.

As Mr. Hubbard states, the altitude of the ponds may result in some amount of condensation, owing to the lowering of temperature resulting from the expansion of the air. This would, however, be but a small factor, whilst the condensation would show itself as mist. There may be something, too, but not much, in the osmotic action of dew-pond water, containing as it does a small proportion of sodium chloride. But there must be some greater factor at work if we are to credit the few records which have been made of the acquisition of, for instance, "3½ inches of water after five nights of heavy dew."

Mr. Hubbard rightly judges the importance of the dew-pond principle, if fully established, in countries where there is no natural water-supply other than dew. He is also quite correct in dwelling upon the importance of vegetation in increasing the rainfall of a district. But when we hear of the rising of the water in a pond by an inch or more in a night, we desire to know if there are any overhanging trees, and whether there are grasses rooted in the bottom of the pond, with several inches of their growth exposed to the atmosphere. The dew deposited on these would be a large item, and would go to feed the pond. Still, there are undoubtedly some ponds, of large size, with no vegetation appearing above the surface, no drainage except from their own shelving bank, the only visible means of recruiting of which consists of rain and driving mists. Given a period of drought, yet these ponds seem to suffer but little. I am not at all satisfied that straw is really a necessity of the case. I have collected information from several different quarters as to the manner of construction of dew-ponds, but straw is not used in all cases, and when used it is frequently placed above the clay merely to prevent cattle from trampling through the bottom and so allowing the water to escape. If dew-point is reached in the air above a pond, there must be some other factor than the alleged chilling of the clay or the water to bring it about. There is room for more experiment.

E. A. M.

NOTES.

AT the last meeting of the Royal Society the following were elected foreign members of the society:—Prof. Santiago Ramón y Cajal, Madrid; Prof. Émile Picard, Paris; Prof. Hugo Kronecker, Berne; and Prof. George E. Hale, Mount Wilson.

LORD AND LADY RAYLEIGH, who have been travelling abroad for several months, have returned to Terling Place, Witham, Essex.

A CENTRAL NEWS message from New York states that Prof. F. L. Tufts, professor of physics at Columbia University, was killed on April 15 while testing some electric feed wires.

THE annual dinner of the Institution of Mining and Metallurgy will be held at the Hotel Cecil on Friday, April 30. The president, Mr. Edgar Taylor, will preside.

THE death is reported, at Louisville, Kentucky, of Dr. Letchworth Smith, at the age of thirty-seven. For several years he had been specially engaged in the study of milk, first at Cornell Medical School and afterwards at the research laboratory of the New York Board of Health. He was the founder of the Babies' Milk Fund Association.

AMERICA has lost one of her veteran naturalists by the death of Dr. W. H. Edwards, who was born in 1822, and made a voyage up the Amazon in 1846 to collect objects of natural history. In addition to a volume describing this expedition, he published three series of books on the butterflies of North America. He also contributed to scientific journals a large number of entomological articles.

BY the death of Prof. F. E. Hulme, which was announced in the last number of NATURE, botany has lost an assiduous votary. Although his professional career lay outside the subject, as he was for many years professor of drawing on the engineering side at King's College, London, he found time to prepare several popular illustrated works on flowers. The best known is "Familiar Wild Flowers," published originally in five series, then extended to eight, and a ninth was revised in proof by the author before his death. "Familiar Garden Flowers," produced in conjunction with Shirley Hibberd, was a contemporaneous publication, and another pleasurable volume, "Familiar Swiss Flowers," appeared last year. The charm of his illustrations lies in the combination of artistic feeling with accuracy of form and colour, while the text discloses an intimate knowledge of plants and flower lore.

A SHORT essay by Mr. Edward Greenly in the Rationalist Press Association Annual suggests that the ancient Greeks, if they were with us to-day, would be much more likely to favour the teaching of natural science than compulsory Greek in schools. Mr. Greenly points out that the geological observations of Pythagoras were as correct, and his conclusions from them as sound, as those of the founders of modern geology. Archimedes, Aristotle, and Eratosthenes of Alexandria also followed the plain and profitable paths which deviated later into the mazes of *a priori* metaphysics, and thus prevented the growth of a scientific Hellenic world. Had the scientific method of inquiry and experiment been pursued, Greek science would have been comparable with that of the nineteenth century, and the whole course of history would have been changed.

MR. R. HAY FENTON has presented to the Natural History Museum of Aberdeen University his fine collection of British birds' eggs—his labour of love for twenty years. The entire collection consists of about 7000 eggs, and includes good series of all the breeding species. There are some interesting rarities, such as the eggs of Ross's gull, the nesting place of which was discovered by Mr. Buturlin in the delta of the Kolyma River in 1905. There is also a fine series of cuckoo's eggs, about fifty altogether, and the foster-parents' clutches. The last addition to the collection was the egg of the great auk purchased a short time ago in London. In handing over his collection to the University, the generous donor makes the interesting statement that his gift has been largely prompted by his recollection of happy visits which he paid to the museum in his boyhood. The bulk of the collection is now well displayed in carefully protected drawers in the University museum at Marischal College, and may be consulted by all serious students.

IN reference to the recently recorded discovery of a skeleton of a mammoth on the beach at Selsey, Sussex (March 25, p. 104), Mr. W. J. Lewis Abbott writes to express the opinion that nearly complete skeletons both of this extinct elephant and of *Elephas antiquus* are commoner in English Pleistocene deposits than is usually supposed. He thinks that the fragmentary nature of many of the specimens recovered is due to unskilled collecting. To the skeletons recorded from Ilford and Ealing, Mr. Abbott adds one from Endsleigh Street, near Euston Station, described by Dr. Henry Hicks in 1892, and one from West Thurrock, Essex, discovered by himself in 1890. Mr. E. Heron-Allen informs us that the whole of the bones recovered at Selsey have been collected by him, and will in due course be deposited in the Natural History Museum, South Kensington. The presence among the remains of the right and left patellæ and a metatarsal bone, all three flawless, will enable experts to judge accurately of the size of the animal when living. Mr. Heron-Allen adds:—"A superficial microscopical examination of the mud in which the skeleton was found has been made by Mr. Clement Reid, F.R.S., and by myself, and proves the deposit to have been fresh-water. The seeds hitherto identified are those of the *Potentilla comarum* (cinquefoil), Myriophyllum (water milfoil), *Eleocharis palustris* (spike rush), *Ranunculus aquatilis* (water crowsfoot), Zannichellia (horned pond weed), Carex (sedge, two species), Potamogeton (pond weed), Stellaria (stitchwort), and *Hippuris vulgaris* (mares' tail)."

IN the report for 1908 of the museums and art-galleries under the control of the Corporation of Glasgow, it is mentioned that the total number of visitors to these institutions again touched a million and a quarter, and this without any special new attraction. A number of new specimens have been added to the natural-history collections.

A METHOD of mounting rotifers and protista in Canada balsam is described by the Rev. Eustace Tozer in the Journal of the Royal Microscopical Society (February). Various fixatives, namely, osmic, picric, and glacial acetic acid, absolute alcohol, and formalin, are used, the choice depending on the species to be prepared. An important feature is the performance of the processes of hardening, staining, and dehydrating on a glass slip under a cover-glass, which is kept from crushing the objects by a thread of white cotton placed under one side. The fluids are drawn off by placing blotting-paper outside the cotton thread, the subsequent fluids being applied at the opposite side of the cover. Euglenæ and diatoms are amenable to suitable modifications of the treatment.

IN the *Museums Journal* for March Mrs. Roesler (in a paper read at the Ipswich Museums' Conference of 1908) gives an account of the work done, chiefly by herself, in the matter of instruction by American museums. To teachers desirous of giving lectures to their classes on special natural-history subjects, the American Museum of Natural History offers the use of a class-room or lecture-hall, and a lantern with a large stock of slides from which to select. For classes desirous of visiting the exhibition-hall, the museum also provides the gratuitous services of an instructor, who meets the classes by appointment and explains the collections. At the Boston Museum a "docent" performs the same services for classes and visitors for a small fee. The author then proceeds to describe the arrangements for instruction made by herself at the New York Museum. These hung fire for a time, but eventually became much appreciated. Among the arrangements are two courses—in spring and autumn—of informal lectures for children, and the museum also provides several hundred cabinets of natural-history objects for loan to the public schools of New York and its neighbourhood.

AFFORESTATION and timber-tree growing in Great Britain and Ireland forms the subject of a paper read by Dr. J. Nisbet before the Society of Arts, and published in its Journal (March 26). It is mainly a criticism of the report of the Royal Commission issued this year.

AN article is contributed by Dr. T. S. Hall to the Melbourne *Argus* (February 20) on the national park which has been formed by the Government of Victoria on Wilson's Promontory, explaining the objects for which the land is being reserved. It is intended to preserve the indigenous plants growing on this area, and introduce such native animals as the grey kangaroo, emu, and lyre-bird. Certain parts of the promontory compare with the famous arboreal and fern scenery around Healesville.

BULLETINS on plants poisonous to stock, ramie cultivation, and pure maize seed have been received from the Transvaal Department of Agriculture, and the same topics are discussed by Mr. J. Burtt-Davy in his annual report for 1907–8. Maize cultivation in the colony has received a great impetus owing to the growth of an export trade, and farmers are pressing for a supply of pure seed of good quality. The bulletin points out how good seed may be obtained by selection or by breeding in accordance with Mendelian principles. Three plants are noted as being especially poisonous to stock, *Homeria pallida*, known as yellow tulp, which grows with lucerne, but dies down before the lucerne is ripe, *Chailletia cymosa*, and *Urginea Burkei*.

A NEW journal, *Mycologia*, edited by Mr. W. A. Murrill, in continuation of the *Journal of Mycology*, was initiated in January. It is announced that each number will contain a coloured plate of fungi in natural colours; the plate in this number illustrates five species of different genera, including *Boletus scaber* and *Hypholoma perplexum*. The editor contributes a first list of American

Boletineæ of North America, and favours the establishment of numerous small genera; besides adopting Tylopilus and Rustkovia of Karsten, he creates four new genera, one of which, Suillellus, is founded on the species *Boletus luridus*. Mr. J. B. Rorer communicates a note on a bacterial disease of the peach which is pretty certainly the same as *Bacterium Pruni*, reported by Dr. E. F. Smith as the cause of leaf spots on plum and peach.

There is considerable opportunity for critical observations regarding the classification of the Polypodiaceæ. In this connection an article by Dr. E. B. Copeland in the botanical series of the *Philippine Journal of Science* (vol. iii., No. 5) respecting the limitation of the genus Athyrium merits special attention. The opinion is advanced that Athyrium, Diplazium, and Anisogonium do not form distinct natural genera, and that certain species of Diplazium show closer affinities with certain species of Athyrium than with other species of Diplazium. Therefore it is recommended to unite the species under the one genus Athyrium, and the author has collated with a key the large number of Philippine species that would come under the genus. In the following number of the journal Dr. Copeland makes a similar suggestion with regard to Cyathea, Alsophila, and Hemitelia, and applies his views in naming a few Philippine species under the generic name of Cyathea.

Prof. Giglioli, of Pisa, has issued as a reprint a paper published in the *Bollettino della Società degli Agricoltori Italiani* in which he discusses some of the newer phases of manurial action. In particular he has collected the results of a number of experiments on the manurial value of manganese dioxide, which has frequently given an increase in crop, although it is not an essential plant food. Indirect manurial actions of this kind are of interest in connection with the idea now being developed in certain quarters that soils contain substances toxic to plants, and a discussion of the phenomena from this point of view is given in the paper.

Bulletin No. 131 of the Purdue University Agricultural Experiment Station contains a report on the working of the recent Feeding-Stuffs Act of Indiana, which, like ours, compels the merchant to guarantee the percentages of oil, protein, and the maximum amount of fibre, and imposes penalties in case the feeding-stuff does not come up to guarantee. There is some difference in detail, and, on the whole, the Indiana Act is more stringent, but it is said to have been entirely effective, and to have improved considerably the standard of goods supplied to the farmer.

The report from the Transvaal Government Laboratories for the year 1907–8 shows a decrease in the total number of samples examined, which, however, is more than accounted for by the falling off in the number of plague specimens. A large number of waters, milks, flours, meals, and other food-stuffs were examined, and attention is directed to the bad state of some of the tinned meats supplied. A number of poison cases were investigated, but it is pointed out that little or no progress can be looked for in dealing with native poisoning cases until a complete examination has been made of the plants indigenous to South Africa and the poisons they contain.

Mr. F. V. Emerson contributes a paper, entitled "A Geographic Interpretation of New York City," to the Bulletin of the American Geographical Society (Nos. 10 and 12, vol. xl.; No. 1, vol. xli.). An elaborate inquiry into the geographical position of New York, and examination of statistics concerning its position in relation to other towns on the Atlantic coast of the United States, leads the author to the conclusions that the commercial growth of New York is due, primarily, to its easy route to the interior, but that there is some evidence that the "momentum" which it has enjoyed from this cause is decreasing. Business men in New York have realised this danger, hence the enlargement of the Erie Canal.

In the March number of the Bulletin of the Imperial Academy of Sciences at St. Petersburg, Prince Galitzin discusses the records of the Calabrian earthquake obtained at Pulkowa. This observatory is specially devoted to a comparative study of the behaviour of various types of instruments, not to that of the movement of the ground; some interesting results seem to have been obtained, among which may be counted the value of electromagnetic damping. This is the only form which gives a constant coefficient of damping for all amplitudes of swing, and its efficiency in eliminating the idiosyncrasies of individual instruments is strikingly exemplified by the reproduction of a double record of two similar Zollner pendula, each recording independently on the same sheet of paper, in which the two curves follow each other with hardly a perceptible divergence in course. It is noticed that all the records indicated an initial movement towards the origin; Prince Galitzin explains this by assuming that the first displacement was outwards, but that the piers, acting as heavy pendula of short period, were tilted backwards by their own inertia, thus producing an apparent inward movement on the instruments which they supported.

In the February number of the *National Geographic Magazine* Captain F. M. Munger, of the U.S. Revenue Cutter Service, gives an account of "the most wonderful island in the world," that near Bogloslof, in the Aleutian Islands, Alaska. In 1886–7 a new island, named "Fire Island," made its appearance. In 1905–6 a new peak rose between this and the adjoining "Castle Rock." Next year a third elevation, called "McCulloch Island," showed itself in the same area. This seems to have exploded in September, 1907, a heavy fall of ashes having covered the entire region for a distance of sixty miles. At the survey in 1907 the formation of the remaining portions of these various volcanic peaks was found to consist of disintegrated rock, basalt, felspar, scoria, tufa, pumice, obsidian, trachyte, and other igneous rocks, with volcanic mud, all more or less discoloured with a deposit of sulphur. The series of excellent photographs obtained by Captain Munger gives an excellent idea of the successive stages of this remarkable exhibition of volcanic energy.

The British School at Rome recently issued an appeal for help towards excavations in the western Mediterranean, accompanied by a report, by Dr. Duncan Mackenzie, on the mysterious Nuraghi of Sardinia and their west-European relations. At Sena he found the so-called "Giant's Tomb," closely connected with the Nuraghe-castle, and remarks:—"On the other hand that the Nuraghe-villagers should turn out to have buried in constructed chamber-tombs that themselves were imitations of the Nuraghe-hut as well as in rock-shelters and rock-cut chamber-tombs would in itself not be a singular phenomenon, but one that has a wide illustration in the Mediterranean Basin and elsewhere in Europe. At the same time an ethnological puzzle of a curious order may well underlie the fact that the people of the Nuraghe-castles should arrogate to themselves for their exclusive use a type of tomb which owes its origin to the primitive dolmen at the same time that they inhabit houses of the same round type as the Nuraghe-huts of the simple villagers." He goes on to illustrate the connection of these remains with the migratory movement through Spain which civilised Europe in the early prehistoric period, France forming the chief highway from north to south. The same tomb-types characteristic of pre-Gallic France recur in the British Islands in the case of New Grange, and the Mediterranean types exhibit special analogies with the horned cairns of Caithness. Excavations in Sardinia and Malta are thus likely to throw much light on the ethnology and prehistoric past of western Europe, and it may be hoped that this appeal will result in liberal aid to a most important investigation, which will continue in the competent hands of Dr. Mackenzie.

The *U.S. Monthly Weather Review* for November, 1908, contains a suggestion, by Prof. A. G. McAdie, for the reform of meteorological methods, by the gradual adoption of metric and centigrade measures in the records and work of the Weather Bureau. For atmospheric pressure a distinctly new proposition is advanced; the author thinks that if pressure changes were charted in percentages of a standard atmosphere, the result would be more satisfactory to both meteorologists and the public. "Instead of 29·92 inches or 760 mm. we should have the value 1000, meaning thereby the pressure of the atmosphere at sea-level reduced to standard temperature and gravity. Then on any given weather map, in place of 30·3 inches we should have 1012. . . . The great advantage of this is that pressure gradients can be read at a glance, and the average man can readily understand the significance of pressure variation." Prof. McAdie considers this method as much superior to the metric system as that is to the one now in use. The editor of the *Monthly Weather Review* points out that the publication of the paper does not imply the approval of the chief or other officials of the Weather Bureau, but he invites discussion thereon.

The Weather Bureau of the Philippine Islands has published part i. of its annual report for 1906; this volume occupies 153 quarto pages, and contains the hourly meteorological observations made during the year at the central observatory at Manila. Each of the tables shows also the hourly, daily, and monthly means; the extreme daily values of the various elements, together with the times of their occurrence, are given in a separate table. All the observations are expressed in the metric system, according to the practice adopted at the time of the reorganisation of the Weather Bureau by the United States Government. This detailed publication of observations and means, which began with 1885, is of great value for the purpose of scientific inquiry, and complies with an international understanding that each country should publish such data for one or more of its principal stations. From the general summary we note that the normal annual duration of sunshine (1890–1906) is 2266 hours; rainfall (1865–1906), approximately 76 inches, on 139 days; shade temperature (1880–1906), 80°·4 F. A statement printed in the English edition of the report of the International Meteorological Committee (Paris, 1907) gives the absolute extremes of temperature (1885–1907) as 100° F. and 59° F. The observations at outlying stations of this important organisation form separate parts of the annual report.

Drawings and photographs of a 40-feet gas-driven launch are given in the *Engineer* for April 9. This launch has been built for cruising purposes by Maclaren Bros., of Dumbarton, and is fitted with a 30 horse-power four-cylinder Crossley gas engine and suction gas-producers using anthracite. The gas is cleansed and cooled in a wet scrubber charged with coke; the ascending gas meets a spray of sea-water supplied at the top, and then passes downwards through a dry scrubber also charged with coke. The maximum speed of the engine is about 800 revolutions per minute, and with a compression of 120 lb. per square inch combustion is so complete that there is an entire absence of smoke and smell both in the engine-room and at the funnel. The engine when cold is started on petrol, and runs with this fuel until the producer has settled down to supply the necessary quality of gas. At the trial runs of the launch, under unfavourable weather conditions, a speed of 9 knots was attained, the cost of running being about 2½*d.* per hour.

Attention is directed in a leading article in *Engineering* for April 9 to the extent and special character of the plant necessary in warship construction, and to the fact that much of this plant is kept idle during prolonged periods owing to the method of the Admiralty in placing orders. Protective decks, armour, guns, and gun mountings require special plant involving very heavy capital charges without any possibility of return except in naval work. At present, armour for ten or twelve battleships per annum can be produced in this country, but in the past three years armour for eight ships only has been ordered. The time required from casting the ingot until completion of the plate is seventy-seven days, and, of this time, thirty-five days' work may be done before any dimension other than the thickness has been specified. It is therefore urged that armour could be ordered early in the preliminary design of a ship. A 12-inch gun takes ten months to construct, and eighteen months are required for the completion of the latest type of barbette mounting for two 12-inch guns. The *Dreadnought* gun mountings were ordered six months before the ship, and took nine months longer to construct than the ship itself. This plan of ordering gun mountings long before the detailed design of the ship is completed might usefully have been adopted with the ships at present contemplated. Woolwich is not adapted for modern gun mountings, and, in the absence of some guarantee of steady work, it is unreasonable to expect private firms to increase their plant to meet a rush. We have sufficient armour and ordnance plant at present to meet all needs if they are utilised with judgment.

Both the *Electrician* and the *Electrical Review* in their issues of April 9 direct attention to two new systems of electric wiring which seem destined to do much in the near future to popularise the electric light amongst those to whom the cost of an installation has hitherto been an obstacle. The new systems are the "Stannos" and the "Kuhlos," and both use a conductor encased in a thin brass or copper tube about a fifth of an inch in diameter. The tube can readily be bent by hand, and is attached to the wall by means of a small clip nailed to the wall and bent over the tube. The cost of wiring is thereby greatly reduced, and the wires are of so small a diameter as to be inconspicuous.

In the *Physikalische Zeitschrift* for April 1 Dr. O. Krüger describes an addition he has made to the Atwood machine in order to show experimentally that the time of swing of a pendulum depends on the acceleration of its point of support. One of the falling weights of the machine carries the pendulum, and is guided in its fall by two vertical wires, so that the swing of the pendulum will not drag it out of its path. The bob of the pendulum carries a fine brush kept inked by means of a tube in the bob, and this brush writes on a vertical strip of paper during its fall. By regulating the two falling weights the pendulum may be made to ascend or descend with a given acceleration; in the former case the time of swing is decreased, in the latter increased, and the two times are determined from the strip records of the upward and downward motions. In the example given by the author the agreement between the observed and calculated ratio of the times of swing is within one part in one thousand.

The *Journal de Physique* for March contains a description of the apparatus for radio-active measurements by the electroscope method exhibited by Messrs. C. Cheneveau and A. Laborde at a recent meeting of the Société française de Physique. The electroscope is of the type introduced by the late Prof. P. Curie, the leaf of aluminium being supported by a plate rising from the base of the instrument, where it is insulated by passing through a plug of "ambroid." To its lower end a short or a long rod may be attached. Radio-active solids are placed in a shallow cylinder under the electroscope into which a short rod connected with the leaf projects. Gases are tested in a longer cylinder, into which a long rod projects. The aluminium leaf is observed through a microscope with a scale in the eye-piece, one division of which corresponds to 0·4 volt. The normal rate of leak of the instrument is 1 volt in twelve minutes, and the minimum for a measurement 1 volt a minute. The lowest rate of production of radium emanation measurable with the larger cylinder is 0·002 milligram per minute. The authors hope that the simplicity of the apparatus will lead to its extensive use.

Messrs. J. W. Gray and Son, the well-known lightning-conductor experts, of 91 Leadenhall Street, have sent us a pamphlet, from the pen of their senior partner, Mr. Alfred Hands, entitled "Lightning and the Churches." The author estimates that not more than 25 per cent. or 30 per cent. of the ecclesiastical buildings in this country are provided with lightning conductors. Statistics which he has collected show that about twenty-four such buildings are damaged every year by lightning, and that of these about three are fitted with conductors which have failed to afford protection. Failure, he considers, may be due to original faults of construction or arrangement, to decay of important parts, or to alterations made in some of the metallic portions of the building after the conductor had been fixed. It is urged that the system of protection should be designed by an expert to suit each particular case, and should be inspected and tested at least once in every three years. The book contains many interesting photographs illustrating the destructive effects of lightning, and concludes with a list of 244 cathedrals, churches, and chapels which have been damaged by lightning during the last ten years.

The principal features of the *Bulletin de la Classe des Sciences* (1908, No. 12) of the Royal Academy of Belgium are a paper by P. Bruylants on the derivatives of trimethylene and a paper by J. Fraipont on the okapi and its affinities with living and with extinct giraffes. The former paper, extending over eighty-four pages, contains a description of a wide range of compounds containing the group $\begin{matrix} CH_2 \\ | \\ CH_2 \end{matrix}\!\!>\!CHX$, the boiling points and densities of which are contrasted with those of the isomeric allyl compounds $CH_2{=}CH.CH_2X$, and of the isopropyl compounds $\begin{matrix} CH_3 \\ CH_3 \end{matrix}\!\!>\!CHX$; the trimethylene compounds usually have a higher boiling point and a higher density than the corresponding allyl and propyl compounds. The latter paper is illustrated by an excellent picture in colours of the *Okapia johnstoni*, a map showing its distribution in the Congo basin, and a series of six comparative photographs of the skull of the okapi and related species.

Attention has been directed in these columns to observations by Noyes which indicated that the mobility of the hydrogen ion in hydrochloric and in nitric acids continued to decrease at dilutions considerably greater than those at which other ions exhibit constant mobilities. A recent paper by Chittock in the Proceedings of the Cambridge Philosophical Society records a number of observations in which the same experimental result was obtained, namely, an increase in the migration value of hydrogen in hydrogen chloride from the normal value 0·167 to 0·275 in very dilute solutions. The explanation given is, however, of a much simpler character, the suggestion being made that the decreased mobility of the hydrogen ion is due to its association with traces of ammonia present as impurities in the water, whereby the hydrochloric acid is converted into ammonium chloride. A similar conclusion had already been reached by Whetham and Paine from observations of the conductivity and migration velocities of dilute solutions of sulphuric acid. It should not be difficult, *e.g.* by distilling from phosphoric acid, to prepare water practically free from ammonia and basic impurities, and it would be of interest to know whether such samples of water would give rise to abnormalities similar to those described above.

Messrs. Witherby and Co. have in preparation a work on the "Birds of Kent," by Dr. Norman F. Ticehurst, who has for many years been well known among ornithologists as a close observer of the avifauna of the county. The work will be published by subscription, and only a limited edition will be issued.

A special meeting of the council of the Iron and Steel Institute was held on Tuesday, April 20, under the presidency of Sir Hugh Bell, Bart., to consider the situation created by the resignation of the president-elect, Sir W. Thomas Lewis, Bart. A resolution of regret that Sir W. Thomas Lewis had found himself unavoidably precluded from assuming the office to which he had been elected was passed, and at the unanimous desire of those present Sir Hugh Bell consented to retain the presidency for a further term of twelve months. In that capacity he will, therefore, take the chair at the annual meeting and at the dinner on May 13 and 14 respectively.

OUR ASTRONOMICAL COLUMN.

Halley's Comet.—*Science Progress* for the current quarter (No. 12, April, p. 543) contains an interesting article by Mr. Crommelin, who reviews the past history of Halley's comet and discusses the probable time of the approaching perihelion passage. In the retrospect Mr. Crommelin directs attention to the fact that a fifteen-months' variation of the period caused Halley to hesitate before accepting the conclusions regarding the object's periodicity; it is now known that the planetary perturbations may cause a five-year variation, from seventy-four to seventy-nine years.

The identifications of returns are now carried back so far as 240 B.C., although there is no certain identification of Halley's with any observed comet until 12 B.C.; its appearance at that return is very fully described in the Chinese annals. Mr. Crommelin regards it as certain that the comet will be re-discovered as soon as the region of Orion, where it now is, becomes observable in the autumn of the present year. January or February, 1910, should find it visible in small telescopes, or even to the naked eye.

As showing the enormous difference between the aphelion and perihelion velocities, Mr. Crommelin points out that nearly half the period, from December, 1856, to April, 1889, was spent on the small arc of the path which lies beyond the orbit of Neptune. At perihelion the similar arc will be traversed in two years. On the assumption that perihelion will occur on April 16, 1910, the comet should appear at its brightest for a few days after May 17, its distance from the earth being then only about 12,000,000 miles.

PRESSURE IN THE SUN'S ATMOSPHERE.—*Apropos* of the discussion as to the pressure obtaining in sun-spots, the results published by MM. Fabry and Buisson in No. 11 of the *Comptes rendus* of the Paris Academy of Sciences are of special interest. Using their interference method, they have investigated a number of the displacements of solar lines which may be due to pressure. They find that all the lines do not behave alike; some are displaced in one direction by different amounts, whilst others are displaced in the opposite direction. Such displacements may not be attributed wholly to pressure, but are allied to the asymmetrical broadening of lines in the arc investigated by Dr. Duffield.

For the solar work MM. Fabry and Buisson selected fine lines such as are symmetrically broadened, and for twenty lines between λλ 4000 and 4500 they find a displacement corresponding to a pressure of four or five atmospheres above atmospheric pressure. Twelve lines between λλ 5100 and 5500 gave a similar result, and it therefore appears that a pressure of 5 or 6 atmospheres obtains where the iron absorption takes place in the sun's atmosphere.

THE SPECTRA OF NEBULÆ.—In these columns on March 11 (NATURE, No. 2053, p. 19) we briefly summarised some results, dealing with the spectra of nebulæ, recently published by Prof. Wolf. Some interesting comments on these results are now published by Dr. Eberhard in No. 4318 of the *Astronomische Nachrichten*.

First he directs attention to the enumeration of the nebular lines adopted by Prof. Wolf; this is not in accordance with the conventionally accepted enumeration, and seems likely to lead to confusion. Prof. Wolf found that the central star of the Ring nebula in Lyra is apparently less active, actinically, than the ring itself; Dr. Eberhard points out that this is a matter of the relative aperture of the instrument used, and the result was to be expected from the instrument employed by Prof. Wolf. He also reminds us that the unknown line at λ 345, suspected by Prof. Wolf in the spectrum of N.G.C. 2023, was certainly observed by Palmer in Nova Persei and in N.G.C. 6886.

According to Prof. Wolf's observations, Hγ was found to be double in N.G.C. 6210, but Dr. Eberhard suspects that, as the dispersion employed was small, this was not an actual doubling, but the incidence of two separate lines, λ 4341 and λ 4363; he also questions some of the identifications given.

THE ORBITS OF SPECTROSCOPIC BINARIES.—In recent numbers of the Publications of the Allegheny Observatory the orbits of several spectroscopic binaries are discussed. In No. 10 (vol. i.) Mr. R. H. Baker discusses the orbit of the spectroscopic components of α Virginis, and derives final elements, which give the period as 4·01416 days, the eccentricity as 0·10, and the apparent semi-major axis as 6,930,000 km.

In No. 11 the same observer discusses the results obtained for the spectroscopic components of *u* Herculis, whilst in No. 12 the orbit of α Coronæ Borealis is derived, by Mr. F. C. Jordan, from measures of 136 plates taken during 1907 and 1908 with the Mellon single-prism spectrograph attached to the 30-inch reflector. The final elements give the period as 17·36 days, the eccentricity as 0·387, and the apparent semi-major axis as 7,671,000 km.

THE CIRCULARITY OF PLANETARY ORBITS.—From Prof. T. J. J. See we have received an abstract from the *Astronomische Nachrichten* in which he discusses the origin of the planetary system, and the reason for the circularity of the orbits of the planets and satellites. Rejecting Laplace's hypothesis of a central rotating nucleus, casting off successive portions which became planets, Prof. See suggests that the planets are bodies which came from outside into such a nebulous mass as Laplace's original "solar nebula." The circularity of the orbits then becomes the natural consequence of the revolution of such bodies, around the central nucleus, through the resisting medium of the nebulous matter.

THE NATURAL HISTORY MUSEUM.

THE subjoined letter appeared in the *Times* of Monday, April 19.

It will be generally admitted that the Natural History Museum is one of the greatest scientific institutions in this country. It receives a grant of more than 60,000*l.* a year of public money, and is the national centre for the cultivation and organisation of the natural-history sciences. It is therefore a matter of concern, not only to naturalists, but to the public generally, that this great national institution should be administered in the best possible way. Unfortunately, in the opinion of all independent naturalists now living and of all the leading naturalists of the last forty-five years, the system of administration of the natural history departments of the British Museum is so defective that the accomplishment of the great objects for which the museum exists is seriously hampered. Rumours of this have already reached the public ear. It is not, however, our design, for the present, to refer to these rumours beyond stating that there is a strong *a priori* probability of their truth, for they indicate a state of affairs which could hardly be avoided under the present system of administration. The defects in this system to which we now desire to direct attention are as follows:—

(1) The government of the Natural History Museum is nominally, and in the eyes of the public, in the hands of the trustees of the British Museum, a large body of distinguished men, forty-nine in number, of high rank and great importance in the State. This number is so large that the trustees cannot act effectively as a single body. The result is that the executive is restricted to a small section of them, known as the standing committee, an entirely irresponsible body, subject to no control or criticism except of a purely formal kind, though spending annually large sums of public money.

(2) While the actual government of the museum is in the hands of the standing committee, the appointment of all officers and servants is in the hands of the principal trustees—the Archbishop of Canterbury, the Lord Chancellor, and the Speaker of the House of Commons. This arrangement, by which the control after appointment is in different hands from those which make the appointment, is highly unsatisfactory. For, should the principal trustees appoint a director not acceptable to the other trustees, as has happened, it is clear that a very difficult position must necessarily be created, alike for trustees and director. Moreover, it is, in our opinion, inimical to the proper conduct of the museum that the appointment to the subordinate offices should be made by the principal trustees. This point has already been touched upon in the fourth report of the Royal Commission of 1874. The commissioners state that:—

"It is held to be singularly inappropriate that the three important personages who are the principal Trustees, occupied as they are in the discharge of the highest functions in Church and State, should be burdened with the duty of making appointments to offices of every grade in the British Museum."

(3) The standing committee of the trustees control, not only the Natural History Museum at South Kensington, but also the Library and the Museum of Art and Archæology at Bloomsbury. This arrangement cannot be regarded as satisfactory, because with the rapid growth of archæology and natural science in the last fifty years the interests represented by the two museums have become so vast, complex, and divergent that it is beyond the power of a single body of men, even of the knowledge of affairs and distinction of the trustees, to fully understand the interests involved. The subdivision of the subjects was recognised when the natural science part of the museum was removed to South Kensington, and it is obvious that two institutions situated so far apart, and dealing with

such different material, cannot be adequately administered by a single governing body.

(4) These remarks apply equally to the remarkable regulation according to which the director of the Natural History Museum is subject to the direction of the principal librarian. This arrangement, by which the director of the largest and most complex natural science institution in the country is subordinate, not merely theoretically, but actually, to a literary man at Bloomsbury, with no scientific knowledge, is so extraordinary, and has had such baleful effects, that we must devote a few words to it. Quite apart from the welfare of the Natural History Museum, it seems unfair to expect of the principal librarian that he should be responsible for the institution in Cromwell Road in addition to his other heavy responsibilities; but it is when we look at the other side of the question that the faultiness of the arrangement becomes fully obvious. To choose as director at South Kensington a man distinguished for his technical knowledge, and then to fail to give him reasonable freedom in the employment of his training and experience, seems as bad a plan as it is possible to conceive. We think it is clear that at one time the trustees were of this opinion. Sir Wiliam Flower was in 1884 granted a large amount of independence, and this might have been greater had he not declined part of the responsibility offered him. When the late director was appointed this freedom was curtailed. It was, we think, unavoidable that in these circumstances difficulties should arise, and we feel very strongly that the recurrence of such difficulties ought to be made impossible; and this can only be done with certainty by making the Natural History Museum an independent unit.

The Prime Minister, in his reply to the deputation on this subject which waited upon him last July, said that the trustees were men of wide experience and equally cognisant of natural history and archæology. These statements are doubtless true, but the question is, are they competent to interfere in the management (as apart from the general supervision and financial control) of a great institution like the Natural History Museum, as complex and highly technical in its constitution as a modern laboratory or observatory? In our opinion, and in that of others who have looked into the question, they are not competent to do this. Yet they do interfere in details of management, not only on their own initiative, but also under the guidance, not of their own "director" especially appointed for his knowledge of museum work and of the larger needs of science, but of a librarian who makes no claim to knowledge under either of these heads. A proposal to place an eminent man of letters or an archæologist at the head of the National Observatory or of the National Physical Laboratory would justly excite the ridicule of the literary no less than of the scientific world, yet under the present system, based upon an Act of Parliament of the eighteenth century, the librarian at Bloomsbury is forced into just such a position with regard to the National Museum of Natural History.

These are some of the principal defects in the present system of administration of the museum. There are others to which, had we space, we should like to direct attention, but we have said enough to show that the present system of administration is defective in important particulars, which it is very desirable in the interests of science and education to remedy without delay.

In conclusion, we may point out that, in addressing this letter to you at the present moment, we cannot be accused of acting in haste. The defects to which we have directed attention have long been known to men of science, and many attempts have been made to remedy them. A short history of these attempts from 1864 to 1898, in which every distinguished man of science has participated, is appended to this letter. The present attempt originated in September, 1907, when the professors of zoology of the United Kingdom addressed a petition to the Prime Minister asking for an inquiry into the methods of administration which, as they subsequently explained by a detailed statement made in a deputation to the Prime Minister in July, 1908, had had such lamentable results.

It only remains to be added that this last effort has apparently been as devoid of result as have been its predecessors, and it has therefore become necessary to place before the public the main facts of the case. That public pays the cost of the Natural History Museum to the tune of 60,000*l*. a year. It does so presumably because it regards the museum as an absolutely efficient and well-organised scientific institution, having the full confidence of the scientific experts of the country. It is, therefore, necessary that the public should realise that, in the opinion of the professional naturalists, a continuance of that confidence is being gravely jeopardised by the continued refusal of those in authority to bring up to date its administrative methods. What we demand is an inquiry by a Royal Commission into the present administration of the Natural History Museum. The way would then be open for the establishment of a satisfactory scheme of reconstruction.

J. C. Ewart.
A. Sedgwick.
Sydney J. Hickson.
Gilbert C. Bourne.

April 17.

DAYLIGHT AND DARKNESS.

A MEETING in support of the Summer Season Time Bill, otherwise known as the Daylight Saving Bill, was held at the Guildhall on Tuesday, April 20, when the following resolution was adopted:—"That the passing of the Daylight Saving Bill would improve the physical, mental, moral, and financial welfare of the nation, and that it is deserving of the support of all classes of the community."

The chief speaker at the meeting was Sir Robert Ball, who referred to the effect of latitude upon duration of daylight, and described the zone system of time reckoning. It is difficult to understand, however, how his remarks can be considered to give support to the proposals of the Bill. The fact that different meridians are used in different parts of the world as standards of time-reckoning, or that a date-line exists about longitude 180°, provides no reason for a seasonal change of the standard meridian. The reference to the difference in the duration of daylight in different latitudes was also unfortunate; for when the difference of latitude is taken into consideration we find that the people in North Britain already enjoy about an extra hour of daylight in summer compared with those in the south, so that they do not need legislative action to obtain it.

As much misconception appears to exist as to the system of time-reckoning and the effect of latitude upon the length of day, it may be of interest to state a few elementary facts relating to them. The number of hours the sun is above the horizon of different latitudes within the limits of the British Isles is shown in the subjoined table for the ends of the months from April to August:—

	Latitude											
	50°		52°		54°		56°		58°		60°	
	h.	m.	h.	m.	h.	m.	h.	m.	h.	m.	h.	m.
April 30... ...	14	37	14	40	15	2	15	16	15	33	15	51
May 30	16	0	16	18	16	40	17	5	17	34	18	8
June 30	16	18	16	40	17	4	17	32	18	5	18	45
July 30	15	18	15	34	15	50	16	10	16	32	16	58
Aug. 30... ...	13	35	13	42	13	50	13	58	14	8	14	19

In this table no account is taken of the lengths of dawn and twilight, which vary both with the season and the latitude. Twilight lasts until the sun is about 18° below the horizon, and where the apparent diurnal path does not descend to this point there is twilight all night. Thus defined, at the end of April places in our islands between latitudes 57° and 60° have twilight all night; during June the sun does not get 18° below the horizon of any place in our islands, and all places north of Edinburgh have twilight all night from about the end of April to the end of July. It is not suggested that work or recreation can be carried on without artificial light during the whole duration of twilight, but even if half this duration be taken it will be found that over a large part of our islands there is sufficient natural light for these purposes up to 10 p.m. or 11 p.m.

Suppose we consider eight hours' sleep as the normal amount to be taken out of the twenty-four hours of a day, and that a reasonable hour to retire is 11 p.m. The difference between this hour and the time at which the lamps of road vehicles have to be lighted will show the interval during which work or recreation cannot be carried on out of doors, assuming that the lighting-up time is always one hour after sunset. In Scotland there is sufficient light to work or play during a large part of the summer months for a much longer period than one hour after sunset, but for simplicity the rule for lighting-up time may be applied to the whole of our islands. The following table gives the interval between this time as thus defined and 11 p.m. Greenwich Time:—

	Latitude					
	50°	52°	54°	56°	58°	60°
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
April 30 ...	2 40 ...	2 35 ...	2 30 ...	2 20 ...	2 10 ...	2 0
May 30 ...	2 0 ...	1 50 ...	1 40 ...	1 30 ...	1 10 ...	0 55
June 30 ...	1 55 ...	1 40 ...	1 30 ...	1 20 ...	1 0 ...	0 45
July 30 ...	2 25 ...	2 20 ...	2 10 ...	2 0 ...	1 50 ...	1 40
Aug. 30 ...	3 10 ...	3 10 ...	3 5 ...	3 0 ...	2 55 ...	2 50

An examination of this table shows that, taking the bed-time hour as 11 p.m., daylight can be used within two hours of this time during June in the lowest latitude of the British Isles, and within about one hour of 11 p.m. at any place north of Edinburgh. If we consider 9.30 p.m. a time at which people have worked long enough at business or pleasure out of doors to desire rest or recreation indoors before retiring at 11 p.m., it will be seen that the actual interval of darkness before 9.30 is small. For instance, taking latitude 52°, which is a little north of London, during May, there is one hour before 9.30 p.m. during which artificial light may be necessary; during June, there are less than twenty minutes; during July, about fifty minutes; and during August, about one and a half hours. If the latitude of Edinburgh (56°) be considered, then at 9.30 p.m. there is no darkness during May and June; in July people give up outdoor occupations needing daylight about half an hour before 9.30 p.m., and in August about one and a half hours before that time. In higher latitudes the people can play or work out of doors up to 9.30 p.m. or longer during the whole of May, June, and July. The only argument that can be derived from latitude is that North Britain should be excluded from the provisions of the Bill.

Now as to the zone or international system of time-reckoning. In the days when places were not within easy communication with one another, either by rail or telegraph, local time was commonly used. The necessity for a uniform standard became clearly evident when railway time-tables had to be printed. We have now become so used to this single system of time-reckoning that few of us remember that formerly it was common to see the announcement of railway companies, "London (Greenwich) Time observed at all stations." By the introduction of standard time, order was called out of chaos, though it meant that for places west of the Greenwich meridian time indicated by the sun is after the time indicated by clocks. A still further advance was made when the Greenwich meridian was adopted as the prime meridian for the international system of time-reckoning.

Thanks chiefly to the persistent advocacy of Sir Sandford Fleming, twenty-four standard meridians are now recognised, beginning with Greenwich, and counting toward the east. The time of each of these meridians is thus one hour behind that of the next meridian to the east of it, and one hour in advance of the next meridian to the west. Each meridian may be regarded as the mid-line of a zone 15° of longitude in width, so that the twenty-four meridians give the standard times on the international system for the whole world. It is usual for places within half an hour of the standard meridian to adopt the time of that meridian as its mean time, but in some cases the line midway between two consecutive meridians of the twenty-four hour system is taken as the standard meridian.

As Sir Robert Ball has given his support to the Daylight Saving Bill, it is of interest to notice what he says in his "Popular Guide to the Heavens" as to the value of uniformity and system in the reckoning of time. Describing standard time, he remarks:—"As soon as communication by railway and telegraph is established in a country, it is convenient to adopt throughout the country a uniform system of time. Very usually the time adopted has been at first the mean time of the capital. But as communication between different countries increases, great inconvenience arises when allowance has to be made for a difference of adopted time involving an odd number of minutes and seconds. A large number of countries and States have therefore adopted a standard system of time based upon that of Greenwich, and differing from it by an exact number of hours, with occasionally an odd half-hour."

The subjoined table, from "Whitaker's Almanack," shows the countries in which this system of standard time, with the prime meridian at Greenwich, has been adopted:—

Country.	Central Meridian.	Fast or Slow on Greenwich Time
Mid-Europe	15° E. ...	1h. fast.
East Europe, British S. Africa, Egypt	30° E. ...	2h. fast.
Mauritius and Dependencies	60° E. ...	4h. fast.
Chagos Archipelago...	75° E. ...	5h. fast.
India	82½° E. ...	5½h. fast.
Calcutta	90° E. ...	6h. fast.
Burma	97½° E. ...	6½h. fast.
Hong Kong, Borneo, West Australia	120° E. ...	8h. fast.
Japan	135° E. ...	9h. fast.
South Australia...	142½° E. ...	9½h. fast.
Victoria, New South Wales, Queensland, Tasmania	150° E. ...	10h. fast.
New Zealand	172½° E. ...	11½h. fast.
Iceland	15° W. ...	1h. slow.
America.		
Atlantic...	60° W. ...	4h. slow.
Eastern	75° W. ...	5h. slow.
Central	90° W. ...	6h. slow.
Mountain	105° W. ...	7h. slow.
Pacific	120° W. ...	8h. slow.

Greenwich Time is used in Spain, Belgium, Holland, Gibraltar, and Faröe (Sheep Islands).

What the Daylight Saving Bill proposes, therefore, is that from the third Sunday in April to the third Sunday in September we shall use the mid-Europe meridian as our standard meridian, and the Greenwich meridian during the rest of the year. It is only necessary to state this fact to show how the proposals of the Bill would introduce confusion into what is now a simple and scientific system. If, as is suggested, some other countries in various latitudes may follow suit and change their standard meridians during various months, the result would be absolute chaos instead of scientific order.

There is only one other point to which we can refer now; it relates to the portion of the year during which the provisions of the Bill are to take effect. As Mr. L. C. W. Bonacina pointed out in NATURE of March 18, the division is unscientific, and follows no natural order.

The following table shows the days on which the sun's declination is approximately the same. In any given latitude the duration of sunlight upon each day in any pair is equal, that is to say the amount of daylight is the same.

Sun's Declination	Days of Equal Sunlight	
20° N.	May 21	July 24
15° ,,	May 1	Aug. 13
10° ,,	April 16	Aug. 28
5° ,,	April 3	Sept. 11
0° ,,	March 21	Sept. 23

The Bill proposes that the change of time shall be from the third Sunday in April to the third Sunday in September, but it is evident that whereas the latter date is about the autumnal equinox, the former is nearly a month after the spring equinox. The declination of the sun in the third week of April is about 12° N., and the corresponding declination after the summer solstice is about August 22. If, therefore, the duration of daylight is intended to determine the dates of change of time, these dates should be the third week in March and the third week in September, or the third week in April and the third week in August.

PRODUCER GAS FOR ENGINES.[1]

II.—TESTS AND EFFICIENCIES.

MR. DUGALD CLERK had careful tests made with a 30-B.H.P. plant and a 40-B.H.P. plant of the type shown in Fig. 2 of the article published last week, and found that the heat efficiency of the former gas was 83 per cent. and that of the latter as high as 90 per cent., both with hot starts.[2] In Table A, I give the results obtained with the last-named suction plant, and for comparison the results with a steam-jet pressure plant of the same power, and the average of results with seven other pressure plants of different sizes :—

TABLE A.

Comparison of Suction and Pressure Plants.

	Suction plant 40 B.H.P. (hot start)	Pressure plant 40 B.H.P. (hot start)	Pressure plants. Average of 7 plants (hot start)
Fuel used	Anthracite	Anthracite	Anthracite
Composition of gas (per cent. by volume)—			
Hydrogen	15·64	19·8	17·36
Methane	1·16	1·3	1·20
Carbon monoxide	20·13	23·8	25·55
Carbon dioxide	6·09	6·3	5·77
Oxygen	0·74	—	0·30
Nitrogen	56·24	48·8	49·82
Total combustible gases (per cent. by volume)	36·93	44·9	44·11
Calorific power (higher scale)—			
Calories per cubic metre	1204	1463	1432
B.Th.U. per cubic foot	135·3	164·4	161·0
Air required for combustion of unit volume	0·927	1·162	1·122
Yield of gas—			
Cubic metres per kilo. of fuel ...	5·80	5·04	5·01
Cubic feet per ton of fuel	208,000	181,000	180,000
Approximate power given by an engine which will give 100 H.P. with gas of Column 3	93	100	100

The practical outcome of many tests made with engines worked with suction plants is that with a full load, or nearly full load, the consumption when running is a little under 1 lb. of anthracite, or about 1⅙ lb. of gas-coke per B.H.P.-hour. This is exclusive of the fuel burnt when starting and during the stand-by hours. The consumption of fuel and water in the small plants (about 20 B.H.P.), tested at Derby in 1906 on behalf of the Royal Agricultural Society was as follows :—

Anthracite { Full load ... 1·1 lb. per B.H.B.-hour, including fuel for starting and banking during the night.
Half load ... 1·6 lb. per B.H.P.-hour, including fuel for starting and banking during the night.
Water ... 1 gallon per B.H.P.-hour at full load.
¾ ,, ,, half ,,

Coke { Full load ... 1·3 lb. per B.H.P.-hour, including fuel for starting.
Water ... 1·5 gallons per B.H.P.-hour at full load.

I had an interesting test made with a 250-B.H.P. engine and suction plant, working night and day for 123 hours without a stop. The engine worked a dynamo, and readings were taken every half-hour of the current generated. The general result was that the consumption of small anthracite, including all sources of waste, was only 1·23 lb. per kilowatt-hour. On the assumption that the efficiency of the dynamo was 90 per cent., this corresponds with 0·82 lb. per B.H.P.-hour.

Close attention is usually given to the consumption of fuel per H.P.-hour, sometimes to the thousandth of a pound, and it is not a little remarkable that a separate account is seldom taken of the consumption of fuel while the steam or gas plant is standing with a fire in it. The stand-by loss of a boiler is much greater than that of a gas producer, and the explanation is not far to seek; for a given H.P. the producer is much smaller, and has far less radiating surface than a boiler; it has no water in it to be heated, and it can be worked up to its maximum production in about fifteen minutes, after standing almost any length of time. With a boiler, except in the vertical or portable type, there is a large amount of external brickwork to be heated, and there is a considerable quantity of water, even in the tubular type. When the boiler is standing the water and the brickwork lose heat, and not only more time, but more fuel, is required to make up this loss than in the case of a gas producer. Doubtless the heat efficiency of a good boiler is high when it is working to nearly its full capacity, but the reverse is the case when it is standing. Table B gives some comparative results :—

TABLE B.

Consumption of Fuel in Stand-by Hours.

Steam power			Gas power	
Type of boiler	Max. H.P. of boiler	Coal consumed per standing hour	Max. H.P. of producer	Coal consumed per standing hour
		lbs.		lbs.
Various	100	14·0	250	5·1
Lancashire	450	37·5[1]	250	3·9
Babcock and Wilcox.	210	67·0	100	2·1
,, ,,	210	67·0	250	4·5
,, ,,	500	180·0	225	3·8
,, ,,	500	112·0	375	1·8
Niclaussé	400	50·0		
Lancashire	400	44·7		
	Average ...	71·5	Average ...	3·5

On this basis, if a 200-B.H.P. steam plant works eight hours and is standing sixteen hours, and if it consumes 2·5 lb. per B.H.P.-hour, the stand-by loss will be more than 20 per cent. of the total fuel consumed in twenty-four hours. Under like conditions, if a gas plant of the same power consumes 1 lb. per B.H.P.-hour, the stand-by loss will be under 4 per cent. With a 500-B.H.P. plant the stand-by loss with steam will be about 15 per cent., and with gas under 2 per cent. If we take the percentage of the stand-by loss on the fuel consumed during the working hours, we have the following results :—

	200 B.H.P.	500 B.H.P.
Steam power	26·8 per cent. ...	17·9 per cent.
Gas power	3·8 ,,	2·0 ,,

The accompanying Figs. 3, 4, 5, and 6 show at a glance the relative heat efficiencies of a steam boiler and steam engine, and of a gas plant and gas engine of the same power; Figs. 3 and 4 are each for 250 B.H.P., and Figs. 5 and 6 are for 40 B.H.P. The blank space at the top of each column represents the number of heat units (100 calories or B.Th.U.) in the fuel consumed to produce the same amount of useful work. For the 250-B.H.P. steam plant I have taken 80 per cent. as the heat efficiency of the boiler, and for the 40 B.H.P. 75 per cent.; for the condensation in pipes, driving feed-pumps and other usual losses, I have taken 10 per cent. of the total heat for the larger plant and 5 per cent. for the smaller one. For the larger steam engine I have assumed a heat efficiency of 15 per cent., and for the smaller one 10 per cent. For the 250-B.H.P. gas power I have assumed that the gas plant is of the steam-jet pressure type, and that, including its small boiler, the heat efficiency is 80 per cent. For the 40-B.H.P. gas power I have assumed that the gas plant is of the suction type, and that its heat efficiency is 85 per cent. With gas plants there are no losses from condensation or other causes beyond those allowed for in the above percentages. For the gas engines I have assumed a heat efficiency of 28 per cent., and in all the diagrams I have taken the friction of the engine as 15 per cent. The figures given for the fuel consumed correspond approximately with the following consumptions of fuel of average quality :—

900 grams (2 lb.) per B.H.P.-hour for 250 B.H.P. steam power
450 ,, (1 lb.) ,, ,, ,, ,, gas power (pressure plant)
1350 ,, (3 lb.) ,, ,, 40 ,, steam power
400 ,, (0·9 lb.) ,, ,, ,, ,, gas power (suction plant).

In Fig. 3, 1120 heat units are absorbed in the boiler, and of these 224 are taken as lost in ashes, radiation, flue

[1] Exclusive of raising the steam pressure from 90 lb. to 120 lb.

[1] Continued from p. 203.

[2] For full details of these trials see "Producer Gas," 2nd edition (Longmans).

gases, &c.; the steam generated represents 896 units, and of these 112 are lost by condensation, &c. The steam supplied to the engine represents 784 units, and of these 667 are lost in the exhaust, so that only 117 are converted into indicated work, and from this 17 are deducted for friction. In Fig. 4 525 heat units are absorbed in the producer, and of these 105 are taken as lost in ashes, radiation, cooling of gas, &c. The gas supplied to the engine represents 420 units, and, as in Fig. 3, 117 units are converted into indicated work, and of these 17 are deducted for friction. In Fig. 5 1680 heat units are absorbed in the boiler, and of these 420 are lost in ashes, radiation, &c.; the steam generated represents 1260 units, and of these 84 are lost by condensation, &c. The steam supplied to the engine represents 1176 units, and of these no less than 1059 are lost in the exhaust. In Fig. 6 494 heat units are absorbed in the producer, and of these 74 are taken as lost in ashes, radiation, cooling of gas, &c. The gas supplied to the engine represents 420 units, and the remaining losses are similar to those in Fig. 4.

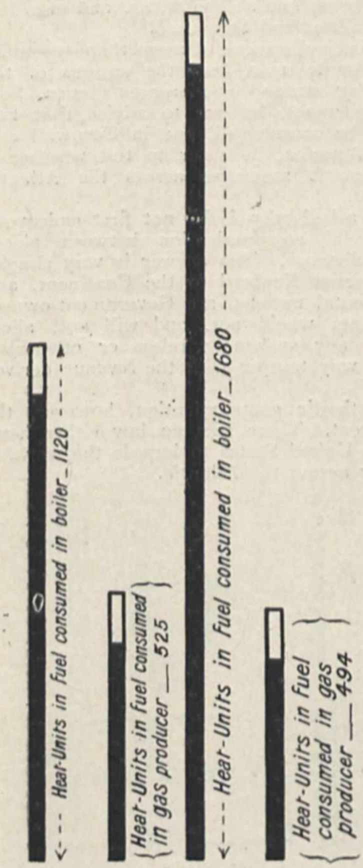


Fig.3. Fig.4. Fig.5. Fig.6.

FIG. 3.—250 B.H.P. steam.
FIG. 4.—250 B.H.P. gas.
FIG. 5.—40 B.H.P. steam.
FIG. 6.—40 B.H.P. gas.

On these bases the general result is that for the 250-B.H.P. size, in order to obtain 100 heat units in useful work with steam power there must be 1120 heat units in the fuel consumed in the boiler; whereas with gas power there need only be 525 units in the fuel consumed. This shows a saving of 53 per cent. in the weight of fuel in favour of the gas plant. The result is still more striking in the case of the 40-B.H.P. size, as there must be 1680 units in the fuel consumed for steam power compared with 494 for gas power. This is a saving of 70 per cent. in favour of the gas plant. These figures do not include any allowance for stand-by losses, which would be considerably less for gas than for steam power.

After considering the two types of plant, I think our general conclusions may be as follows:—A suction plant has certain practical advantages—it costs less and occupies a smaller ground-space; but the gas made in it is not so strong as in the older form of pressure plant, and in the case of large engines this advantage may be important, as it affects the maximum power of the engine. The fuel consumption per H.P.-hour and the labour required are about the same in both types of plant, provided the steam required is raised without an independent boiler. The consumption of water is the same in both types. Where there are several engines to serve, a pressure plant is better, as all can be served with one main from the gas-holder, with a branch to each engine. This simplifies the piping and reduces its cost considerably; it also facilitates the starting of the engines. It seems to me that each plant has its own province, and that in some cases the pressure type is better than the suction type; in others suction is better than pressure.

Looking at the matter broadly, one cannot but be struck with the enormous development in gas power which has taken place during the last ten, and especially during the last five, years. Small steam engines are being rapidly superseded, and in several cases the makers of steam engines are now making gas engines. At first only small gas engines were supposed to be within the range of practical politics, but those days are over, and there are many gas engines developing more than 1000 H.P. each which are working satisfactorily. Gas power has come to stay, and now has a recognised position among engineers.

J. EMERSON DOWSON.

TRANSATLANTIC WIRELESS TELEGRAPHY.[1]

ON previous occasions I have had the honour of describing before this institution some of the stages through which the application of electric waves to telegraphy through space has passed. This evening I propose to confine myself chiefly to describing the results and observations recorded during the numerous tests and experiments which my collaborators and I have been carrying out with the object of proving that wireless telegraphy across the Atlantic was possible, not merely as an experimental feat, but as a new and practical means for commercial communication (Journ. Inst. Elec. Eng., xxviii., 1899, p. 291).

In March, 1899, communication was established by means of my system of wireless telegraphy across the Channel between England and France (see Fig. 1), and the *Times*

of March 29 of that year published the first Press telegram ever transmitted to England from abroad by means of electric-wave telegraphy.

At that time a considerable discussion took place in the Press as to whether or not wireless telegraphy would be practicable for much longer distances than those then covered, and a general opinion prevailed that the curvature of the earth would be an insurmountable obstacle to long-distance transmissions, in the same way as it was, and is, an obstacle to signalling over considerable distances by means of optical signals such as flashlights, the heliograph, or the semaphore.

Other difficulties were anticipated as to the possibility of being able practically to employ and control a transmitter capable of radiating an amount of electrical energy large enough to actuate a receiver at really great distances, and,

[1] From a discourse delivered at the Royal Institution on Friday, March 13, 1908, by Commendatore G. Marconi.

granting the possibility of this, whether such a powerful radiator would not interfere with the working of all other wireless stations which might be established on shore or ships within the sphere of influence of the long-distance sender.

What so often occurs in most pioneer work has repeated itself in the case of long-distance wireless telegraphy—the anticipated obstacles and difficulties were either imaginary or else easily surmountable; but in their place unexpected barriers manifested themselves, and my efforts and those of

FIG. 2

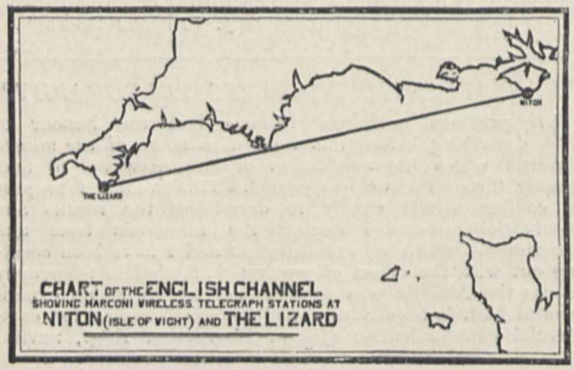


CHART OF THE ENGLISH CHANNEL SHOWING MARCONI WIRELESS TELEGRAPH STATIONS AT NITON (ISLE OF WIGHT) AND THE LIZARD

my collaborators have been mainly directed to the solution of problems presented by difficulties which were not anticipated when the tests over long distances were first initiated.

In January, 1901, wireless communication was established between St. Catherine's Point in the Isle of Wight and Lizard in Cornwall, over a distance of 186 miles. The height of these stations above the sea-level did not exceed 300 feet (100 metres), whereas to clear the curvature of the earth a height of more than a mile at each end would have been necessary.

The result of these tests went far to convince me that electric waves produced in the manner I had adopted were able to make their way round the curvature of the earth, and that therefore it was not likely that this factor would constitute a barrier to the transmission of waves over greater distances. At this time I had achieved a considerable measure of success, by means of syntonic or tuning devices, in preventing mutual interference between stations, and Prof. Fleming described, in a letter to the *Times*, dated October 4, 1900, the results obtained, and which he and others had witnessed (Journ. Soc. Arts, xlix., No. 2530, 1901). The principle on which the transmitters and receivers at St. Catherine's Point and the Lizard were worked is shown in Figs. 3 and 4.

At the transmitting end a condenser, usually taking the form of a battery of Leyden jars, had one terminal connected to one spark-ball of an induction coil or transformer and the other to the primary circuit of an oscillation transformer. The opposite terminal of this transformer circuit was joined to the second spark-ball. The condenser was charged to the potential necessary to produce a suitable spark by means of an induction coil. The secondary circuit of the oscillation transformer was inserted between the vertical conductor, or aërial wire, and earth, and an adjustable inductance coil included in the circuit.

The circuits, consisting of the oscillating circuit and radiating circuit, were more or less closely "coupled" by varying the distance between the primary and secondary of the oscillation transformer. By the adjustment of the inductance inserted between the elevated conductor and earth, and by the variation of the capacity of the primary circuit of the oscillation transformer, the two circuits of the transmitter could be brought into resonance, a condition which I first found was absolutely necessary in order to obtain efficient radiation.

The receiver consisted also of a vertical conductor or aërial connected to earth through the primary of an oscillation transformer, the secondary of which included a condenser and a coherer, or other suitable detector, it being necessary that the circuit containing the aërial and the circuit containing the detector should be in resonance with each other, and also in tune with the periodicity of the oscillations transmitted from the sending station.

The energy employed to signal over a distance of 186 miles could be brought as low as 150 watts, and even less if a higher or larger aërial had been used.

The facility with which distances of more than 100 miles could be covered prior to 1900, and the success of the methods for preventing mutual interferences (Journ. Soc. Arts, xlix., No. 2530, 1901), led me to advise that two large power stations be constructed, one in Cornwall and the other in North America, in order to test whether it was possible to transmit messages across the Atlantic Ocean.

I have often been asked why I did not first endeavour to establish commercial communication between places situated at a shorter distance. The answer is very simple. The cables which connect England to the Continent, and between most Continental nations, are Government-owned, and these Governments would not, and will not, allow the establishment of any system, wireless or otherwise, which might in any way tamper with the revenue derived from these cables.

As regards Transatlantic communication, however, the conditions were different. There was no law either here, in Canada, or in the United States to impede the working of wireless telegraphy across the Atlantic.

FIG. 3

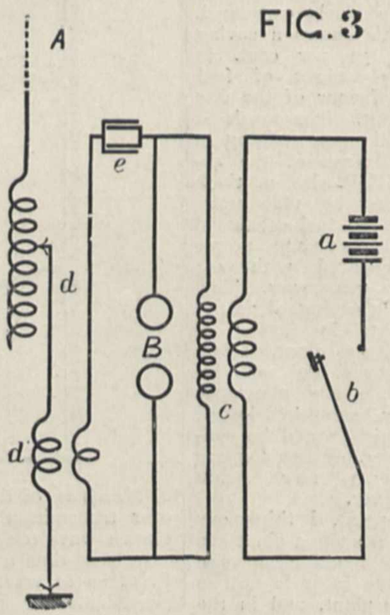
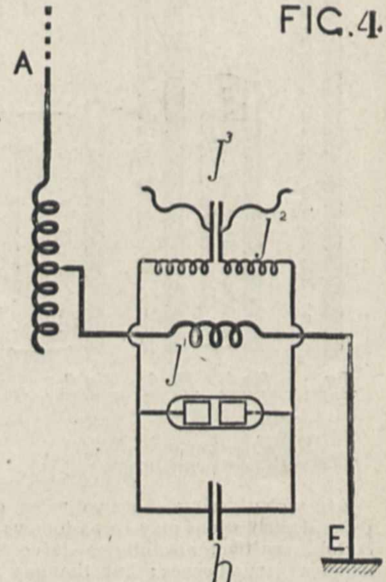


FIG. 4



A further potent reason, moreover, an economical reason, prompted me to attempt communication with America. Notwithstanding the cost of high-power stations, I am convinced that it is more profitable to transmit messages at 6*d*. a word to America than at, say, ½*d*. a word across the Channel, and that the economical advantage of wireless over cables and land-lines increases instead of diminishing with the distance.

A site suitable for a long-distance station was chosen at Poldhu, in Cornwall, and here in 1900 work was commenced in earnest—work in which I was ably assisted by

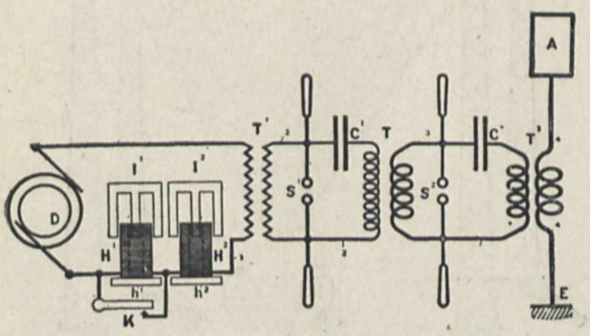
Prof. J. A. Fleming, of the University of London. The transmitter at Poldhu was similar in principle to the one I have already described, but it is obvious that the considerable distance over which it was proposed to transmit signals necessitated the employment of more powerful electromagnetic waves than those ever previously used. These were obtained by means of a generating plant consisting of an alternator capable of an output of about 25 kilowatts, which, through suitable transformers, charged a condenser having a glass dielectric of great strength.

Time does not permit me to describe in detail all the engineering difficulties which were encountered in controlling electrical oscillations of a power which at that time was certainly unprecedented, and as the tests were made possible by commercial organisation, the objects of which do not consist solely in the advancement of science, you will understand that a detailed description of the plant used at the Transatlantic stations cannot, for the present at least, be made public.

My early tests on wireless transmission by means of the elevated capacity method had convinced me that when endeavouring to extend the distance of communication it was of little utility merely to increase the power of the electrical energy applied to the transmitting circuits, but that it was also necessary to increase the area or height of the transmitting and receiving elevated conductors.

As it was economically impracticable to use vertical wires of very great height, the only alternative was to increase their size or capacity, which, in view of the facts I had first noticed in 1895, seemed likely to make possible

FIG. 5



the efficient utilisation of large amounts of electrical energy (Journ. Inst. Elec. Eng., xxviii., 1899, pp. 278–9).

The form of aërial which I first proposed to employ consisted of a conical arrangement of wires insulated at the top and gathered together at a lower point in the form of a funnel. This aërial was supported by a ring of twenty masts each 200 feet high, arranged in a circle 200 feet in diameter.

During the first tests an arrangement of circuits (Fig. 5) proposed by Dr. Fleming, and consisting of a modification of the system shown in Fig. 3, was employed. In this arrangement, in place of one high-frequency oscillation circuit, two are employed, and the constants of the two circuits are so arranged that very high-tension discharges can be obtained from one of the condensers—the one which is inductively connected with the aërial—without danger of damage to the circuits of the generator ("The Principles of Electric Wave Telegraphy," 1906, p. 506).

Simultaneously with the construction of the station at Poldhu, the erection of another one on substantially the same plan was undertaken at Cape Cod, in the United States of America.

The completion of the arrangements was delayed owing to a storm, which wrecked the masts and aërial at Poldhu on September 18, 1901, but by the end of November the aërial was sufficiently restored to enable me to complete the preliminary tests which I considered necessary prior to making the first experiment across the Atlantic.

Another accident to the masts at Cape Cod seemed likely to postpone the tests for several months more. I therefore decided that in the meantime I would use a purely temporary receiving installation in Newfoundland for the purpose of testing how far the arrangements in Cornwall had been conducted on right lines.

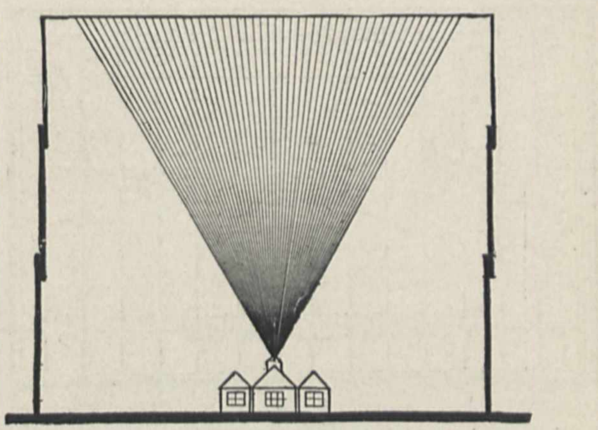
The transmitting elevated conductor employed at Poldhu during the experiments with Newfoundland consisted of fifty almost vertical copper wires supported at the top by a horizontal wire stretched between two masts 48 metres high and 60 metres apart. These wires converged together at the lower end in the shape of a large fan, and were connected to the transmitting instruments situated in a building (Fig. 6).

The transmitting condenser used with this aërial had a capacity of one-fiftieth of a microfarad, and was charged to a potential sufficient to produce a suitable spark discharge between spheres 3 inches in diameter, 1½ inches apart, the wave-length being 1200 feet. The actual power employed for the production of the waves was about 15 kilowatts.

I left for Newfoundland on November 27, 1901, with two assistants. As it was impossible at that time of the year to set up a permanent installation with poles, I decided to carry out the experiments by means of receivers connected to elevated wires supported by balloons or kites—a system which I had previously used when conducting tests across the Bristol Channel for the Post Office in 1897.[1]

It will be understood, however, that when it came to flying a kite on the coast of Newfoundland in the month of December this method was neither an easy nor a com-

FIG. 6



fortable one. When the kites were got up much difficulty was caused by the variations of the wind producing constant changes in the angle and altitude of the wire, thereby causing corresponding variations in its electrical capacity and period of electrical resonance. My assistants at Poldhu, in Cornwall, had received instructions to send on and after December 11, during certain hours every day, a succession of S's followed by a short message, the whole to be transmitted, at a certain pre-arranged speed, every ten minutes, alternating with five minutes' rest.

Owing to the constant variations in the capacity of the aërial wire in Newfoundland, it was soon discovered that an ordinary syntonic receiver was not suitable, although, at one time, a number of doubtful signals were recorded. I therefore tried various microphonic self-restoring coherers placed either directly in the aërial or included in the secondary circuit of an oscillation transformer, the signals being read on a telephone.

On December 12 the signals transmitted from Cornwall were clearly received, at the pre-arranged times, in many cases a succession of S's being heard distinctly, although probably in consequence of the weakness of the signals and the constant variations in the height of the receiving aërial no actual message could be deciphered. The following day we were able to confirm the result. The signals were actually read by myself and by my assistant, Mr. G. S. Kemp.

1 "Signalling through Space without Wires," lecture by Sir William Preece, Royal Institution, June 4, 1897. Proc. R.I., xv., p. 467.

I have often been asked why I adhered to the practice of transmitting series of the letter S for these tests. The reason is that the switching arrangements at the sending station at Poldhu were not constructed at that time in such a manner as to withstand long periods of operation—especially if letters containing dashes were sent—without considerable wear and tear, and that if S's were sent an automatic sender could be employed. Moreover, the immediate object of these experiments was not to transmit actual messages across the ocean, but to ascertain the possibility of detecting the effects of electric waves at a distance of 2000 miles.

The result obtained, although achieved with imperfect apparatus, was sufficient to convince me and my co-workers that by means of permanent stations (that is, stations not dependent on kites or balloons for sustaining the elevated conductor) and by the employment of more power in the transmitters it would be possible to send messages across the Atlantic Ocean with the same facility with which they were being sent over much shorter distances.

About two months later, in February, 1902, further tests were carried out between Poldhu and a receiving station on board the American liner *Philadelphia, en route* from Southampton to New York. The sending apparatus at Poldhu was the same as that used for the Newfoundland experiments. The receiving aërial on the ship was fixed to the mainmast, the top of which was 60 metres above sea-level. As the elevated conductor was fixed, and not floating about with a kite, as in the case of the Newfoundland experiments, good results were obtained on a syntonic

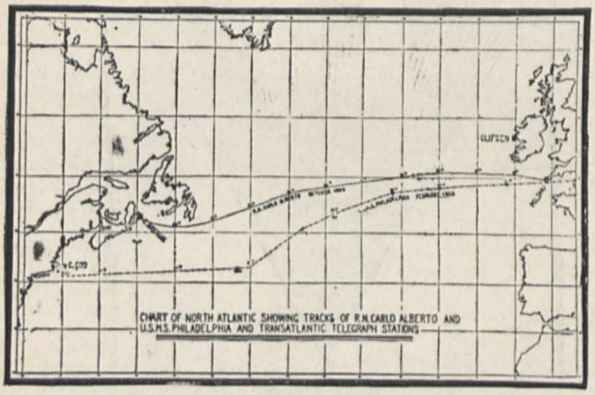


Fig. 7.

receiver, and the signals were all recorded on tape by the ordinary Morse recorder. On the *Philadelphia* readable messages were received from Poldhu up to a distance of 1551 miles, S's and other test letters as far as 2099 miles.

Although I never had the slightest doubt in my mind as to the genuineness of what was accomplished between Poldhu and Newfoundland, the results obtained on the *Philadelphia* amply prove that the station at Poldhu was capable at that time of transmitting signals to a distance of at least 2000 miles, which is the distance separating Cornwall from Newfoundland, and that if it was practicable to send a message over 2000 miles of sea from shore to ship, it should also be practicable to send it over the same space of ocean from shore to shore.

A result of some scientific interest which I first noticed during the tests on the s.s. *Philadelphia* was the very marked effect of sunlight on the propagation of electric waves over great distances.[1]

At the time of these tests I was of opinion that this effect might have been due to the loss of energy at the transmitter by daytime, caused by the dis-electrification of the highly charged transmitting elevated conductor operated by the influence of sunlight. I am now inclined to believe that the absorption of electric waves during daytime is due to the ionisation of the gaseous molecules of the air effected by ultra-violet light, and as the ultra-violet rays which emanate from the sun are largely absorbed in the upper atmosphere of the earth, it is probable that the portion of the earth's atmosphere which is facing the sun will contain more ions or electrons than that portion which is in darkness, and therefore, as Prof. J. J. Thomson (*Phil. Mag.*, August, 1902, Ser. 6, iv., p. 253) has shown, this illuminated and ionised air will absorb some of the energy of the electric waves.

The fact remains that clear sunlight and blue skies, though transparent to light, act as a kind of fog to powerful Hertzian waves. Hence the weather conditions prevailing in this country are usually suitable for long-distance wireless telegraphy.

Apparently the amplitude of the electrical oscillations and the lengths of waves radiated have much to do with the interesting phenomena, small amplitudes and long waves being subject to the effect of daylight to a less degree than large amplitudes and short waves. I never considered that this daylight effect would be an insuperable obstacle to Transatlantic telegraphy, as sufficient sending energy could be used during daytime to make up for the loss of range of the transmissions.

Turning again to Newfoundland, I ought to add that the experiments could not there be continued or extended in consequence of the hostile attitude of the Anglo-American Telegraph Company, which claimed all rights for telegraphy, whether wireless or otherwise, in Newfoundland.

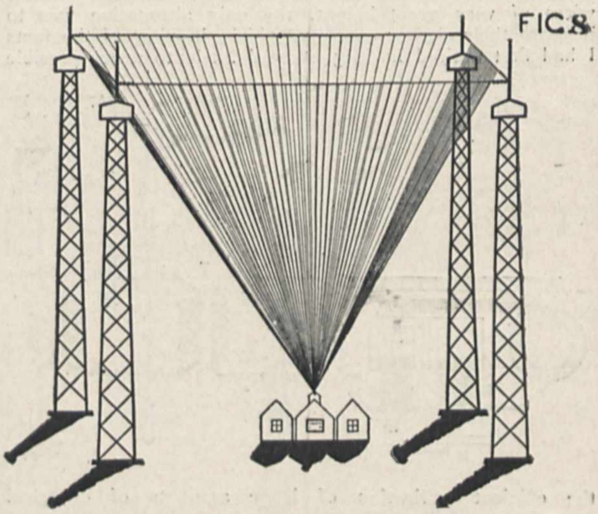


FIG. 8.

However, as I had received an offer of assistance from the Canadian Government, it was decided to resume the tests between Great Britain and Canada, and these tests were very greatly facilitated by the subsidy of 16,000*l.* granted by the Canadian Government to support my experiments. The construction of another long-distance station was, therefore, commenced at Glace Bay, in Nova Scotia, and very extensive tests and experiments were carried on with Poldhu during the latter part of 1902.

Contemporaneously with the construction of the station at Glace Bay, alterations and modifications were executed at Poldhu. Four wooden lattice towers, each 210 feet high, were erected at the corners of a square of 200-feet side. The towers carried insulated triatic stays, from which was suspended a conical arrangement of four hundred copper wires forming the aërial, put up in sections, so that more or less could be employed (Fig. 8). The buildings for the generating plant were placed in the middle of the space between the towers. Additional machinery was obtained, and alterations carried out in accordance with the experience obtained from previous tests.

Identical towers and aërial arrangements were at that time adopted at the stations at Glace Bay, and at the similar installation in course of erection at Cape Cod, Mass.

In most of the experiments carried on from Poldhu the capacity of the sending condenser was one-thirtieth of a microfarad, the spark-length $1\frac{3}{4}$ inches, and the wave-length 3600 feet. In these and subsequent tests the double con-

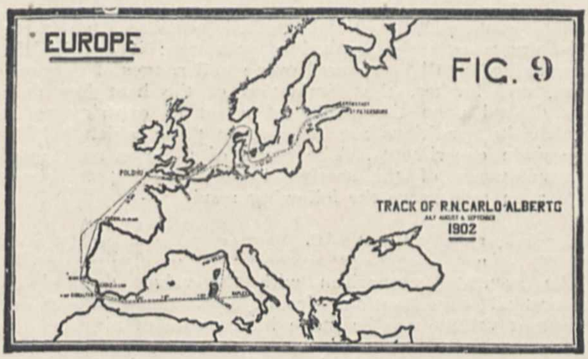
[1] Proc. Roy. Soc., lxx., p. 344, "A Note on the Effect of Daylight upon the Propagation of Electro-magnetic Impulses over Long Distances." Paper read June 12, 1902.

denser arrangement of Dr. Fleming was replaced by a single condenser, the arrangement being similar to that shown in Fig. 3.

During the time that constructional work was in progress at Glace Bay, I carried out some tests with Poldhu over considerable distances, and these tests were greatly facilitated by the interest taken in them by the Italian Government, which placed the cruiser *Carlo Alberto* at my disposal.

During these experiments the interesting fact was observed that, when using waves of more than 1000 metres in length, intervening land or mountains do not bring about any considerable reduction in the distance over which it is possible to communicate. Thus messages and Press despatches were received from Poldhu at the positions marked on the map (Fig. 9), which map is a copy of the one accompanying the official report of the experiments (*Revista Marittima*, Rome, October, 1902).

In December, 1902, messages were for the first time exchanged at night between the stations at Poldhu and Glace Bay, but it was found that communication was exceedingly difficult and untrustworthy from England to Canada, whilst it was good in the opposite direction. The reason for this is that the Glace Bay station was equipped with more powerful and more expensive machinery—a condition rendered possible by the subsidy granted by the Canadian Government; whilst as regards Poldhu, owing to



the uncertainty of what might or might not be the attitude of the British Government at that time towards the working of the station, my company was unwilling to expend large sums of money for the purpose of increasing its range of transmission.

As, however, messages could be sent then for the first time by wireless telegraphy from Canada to England, inaugural messages were dispatched to the Sovereigns of England and Italy, both of whom had previously given me much assistance and encouragement in my work, and who, by their gracious replies, attested their appreciation of the results which had been achieved. Other messages were also sent to England by the Government of Canada.

Further tests were shortly afterwards carried out with the long-distance station at Cape Cod, in the United States of America, and a message from President Roosevelt was transmitted from that station to His Majesty the King in London.

It is curious to note, in regard to the transmission of this message, that the energy employed at Cape Cod was barely 10 kilowatts, and it was not anticipated that this amount of energy would be sufficient to carry direct to Poldhu. The message was therefore transmitted from Cape Cod, instructions having been given to the operators at Glace Bay to be on the look-out, and to repeat wirelessly on to Poldhu any message received from Cape Cod, and my assistant, Mr. P. J. Woodward, at Poldhu, took in the message on one of my magnetic detectors.[1] The electromagnetic waves conveying this message travelled, therefore, 3000 miles through space over the Atlantic, which distance included about 500 miles of land, following an arc of 45 degrees on a great circle.

(*To be continued.*)

[1] Proc. Roy. Soc., lxx., p. 341, "Note on a Magnetic Detector of Electric Waves which can be employed as a Receiver for Space Telegraphy."

THE PHYSICS OF GOLF.

IN two articles recently published in the *Times* (March 16 and 23) Sir Ralph Payne-Gallwey has extended in an interesting way the earlier results in the physics of golf which the late Prof. Tait communicated to NATURE between the years 1887 and 1894. In Sir Ralph's experiments the golf balls were projected mechanically by means of a catapult, the ball being either thrown from a cup at the end of the rotating arm or hit off as it hung at the end of a gossamer thread by a blow from the arm. In either case the initial conditions of projection must differ from those which exist in the ordinary mode of propulsion, and it would have been interesting to have had some comparisons. As Tait conclusively showed, the great factor in long driving was the underspin communicated to the ball by the impact of the club in a line below the centre of gravity. Hence the value of the roughened ball, causing not only a better grip between the ball and the club, but also making more efficient the effect of the resistance of the air in producing the uplifting force. Sir Ralph Payne-Gallwey shows experimentally that the ball must not be too much roughened, and that, indeed, a distinctly less roughening than is usual is sufficient to ensure the maximum carry. It is obvious that with a very rough surface the resistance of the air will rapidly cut down the rotation, and thereby diminish the transverse force which lifts the ball against gravity.

Sir Ralph does well in directing attention to the necessity of a truly centred ball. The golfer can readily test the ball in this respect by floating it in water (Tait used to use mercury) and noting whether or not it comes quickly to the same position. If it comes quickly always to the same position it is badly centred, and must be rejected. As all bowlers know, the lack of true centring will give a bias which cannot but produce inaccurate putting. In the flight through the air the bad effects due to the centre of gravity being non-coincident with the centre of figure will probably come into evidence because of the shifting of the axis of rotation. Such a badly centred ball, when projected from the tee, will in general be sent off rotating about an axis which, though initially horizontal, is not a principal axis of inertia. Of necessity precessional motion will result, and the axis of spin will move away from its horizontal position. The phenomena which lead to the evils of slicing and pulling will at once declare themselves. Moreover, if the precessional motion be rapid enough, it is conceivable that the ball might swerve in one direction during one part of its course, but in the other direction during another part of the same trajectory. A sinuous flight is, indeed, occasionally observed, but is generally attributed to the direct action of the wind. Sir Ralph Payne-Gallwey does not himself touch upon the ultimate dynamics of the problem, but confines himself entirely to the direct teaching of experiment. His conclusions are of great practical value to all devotees of the game, and it would be well if the manufacturers of golf-balls would test every ball they put on the market by the simple methods described by him.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE Bristol Town Council has decided to contribute in the proportion of one penny in the pound on the rate, or about 7000*l.* per annum, towards the support of the proposed university for Bristol and the West of England, for which more than 200,000*l.* has been subscribed, mainly by members of the Wills family.

THE estimate of the amount required in the year ending March 31, 1910, for grants in aid of the expenses of certain universities and colleges in Great Britain is 217,400*l.* The following are the sub-heads under which this vote will be accounted for by the Treasury. Grants in aid, universities and colleges:—(A) University of London, 8000*l.*; (B) Victoria University of Manchester, 2000*l.*; (C) University of Birmingham, 2000*l.*; (D) University of Wales, 4000*l.*; (E) University of Liverpool, 2000*l.*; (F) Leeds University, 2000*l.*; (G) Sheffield University, 2000*l.*; (H) Scottish universities, 42,000*l.*; (I) colleges, Great Britain, 100,000*l.*; (J) university colleges, Wales, 12,000*l.*; (K) Welsh university and colleges, additional grant, 15,000*l.*; increase, 15,000*l.* University College of North Wales (building fund), decrease, 20,000*l.* Provision is made as follows in other estimates for expenditure in connection with the University of London:—buildings, external maintenance and repairs, 3358*l.*; rates, 4500*l.*; non-effective, 1317*l.*; total, 9175*l.*

THE *Times* announces that "the German Aërial Navy League is organising a school for aëronauts which, it is said, will be opened at Friedrichshafen on October 1 of this year. The object of the school is to provide the necessary scientific and practical training for the crews of military and other airships. Only those who have been through an 'intermediate' school and, in addition, have worked for a year in engineering shops, will be admitted as pupils. The course will extend over three years, of which the first will be devoted to theoretical instruction, the second to work in a construction yard, and the third to ascents in airships and flying machines." This announcement will be read with the more interest as a somewhat similar project forms a part of the programme of the recently formed Aërial League of Great Britain, the inaugural meeting of which at the Mansion House was so highly successful. It is much to be hoped that the promoters of the English scheme will succeed in maintaining the same high standard of admission, and the same length of training, that are contemplated in the above notice. It would be highly undesirable that an institution founded for the training of aëronauts should have to waste its resources by providing classes in elementary calculus and mechanics such as can be found at any technical college.

THE National Union of Teachers held its annual conference of delegates at Morecambe from April 10 to 15, and the meeting was thoroughly successful and the discussions full of interest, notwithstanding the rather unusual circumstance that there was no new Education Bill to be considered. The president, Mr. C. W. Hole, delivered the inaugural address, in the course of which he stated that the elementary schools have made great progress during recent years. The ancient system of payment by results has passed away, leaving all concerned happier and better for its disappearance; the liberty and confidence reposed in the teachers have resulted in the children being, not only rationally instructed, but also more properly educated. It remains for the Government to provide financial assistance in order that the size of the classes may be reduced and the staff rendered efficient in number and quality. In this connection Mr. Hole warmly approved Mr. Runciman's recent staffing circular. Resolutions were carried unanimously (1) in favour of larger grants from the National Exchequer; (2) regretting attempts made by certain local authorities to repudiate settled contracts of teachers in their service. At the sectional meetings papers were read by Mr. C. H. Wyatt and Mr. Ernest Gray on the supply and training of teachers, by Mr. A. R. Pickles on leaving examinations and scholarship competitions, and by Mr. Charles Bird on the teaching of handwork. Mr. Pickles quoted with approval the report of the British Science Guild on the relations of primary and secondary education, particularly the recommendation that the reports of teachers should supersede largely the present system of estimating ability by examinations.

THE Colonial Conference in 1907 pronounced in favour of reciprocity between the Governments and examining bodies throughout the Empire. The council of the Surveyors' Institution has taken an important step forward by submitting a memorandum to the Colonial Secretary, which Lord Crewe has approved and dispatched to the officers administering the Governments of Canada, Newfoundland, Australia, New South Wales, Victoria, Queensland, South Australia, Western Australia, Tasmania, New Zealand, Cape of Good Hope, Natal, Transvaal, and Orange River Colony. The memorandum states that under existing conditions a surveyor has to pass examinations and comply with requirements, varying in different parts of the Empire, before he is allowed to practise. It is hoped, as a result of the present movement, to arrive at a uniform standard of qualification. A surveyor would then, having taken his diploma in England or one of the colonies, be eligible to practise in any part of the Empire, subject to an examination in the local land laws and conditions. In the event of an Imperial conference of surveyors being held, it will take place at the Surveyors' Institution, and the chief points, so far as they have been formulated, for discussion would probably be the desirability of establishing reciprocity throughout the Empire:—(*a*) that a candidate must have matriculated at some recognised university, or passed an equivalent examination; (*b*) that an examination in the theory of land surveying be then taken, the standard of this examination to be as high as that now in force in South Africa; (*c*) that the candidate be then required to pass an examination in practical surveying, and that he be ineligible to sit for this final examination until he has had at least two years' experience with a practising surveyor.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 10, 1908.—"The Specific Heat of Air and Carbon Dioxide at Atmospheric Pressure, by the Continuous Electrical Method, at 20° C. and at 100° C." By W. F. G. **Swann**. Communicated by Prof. H. L. Callendar, F.R.S.

The continuous electrical method possesses two main advantages over the method of mixtures; it enables the specific heats to be measured over small ranges of temperature, and further, the elimination of the heat loss does not depend upon the results of a set of experiments in which the conditions are different to those which hold in the main experiments. The mean of a large number of measurements of the specific heats, agreeing to about 1 part in 1000, gave the following results:—

Air	Carbon dioxide.
0·24173 cal. per gram degree at 20° C.	0·20202 cal. per gram degree at 20° C.
0·24301 „ „ 100° C.	0·22141 „ „ 100° C.

An accurate comparison with the values deduced on theoretical considerations from Joly's measurements at constant volume can be made in the case of air, and the agreement is shown to be nearer than to 1 part in 1000. The comparison can only be made in a rough manner for carbon dioxide, and the agreement is to 1 per cent.

The results obtained are about 2 per cent. higher than those obtained by former investigators. The experiments of Regnault are discussed as a typical example, and it is pointed out that an uncertainty amounting to 5 per cent. (tending to make the results too low) probably exists in those experiments, owing to the fact that the heat loss was determined by a set of observations in which the conditions were different to those which held in the main experiments.

March 25.—Sir Archibald Geikie, K.C.B., president, in the chair.—Liberation of helium from radio-active minerals by grinding: J. A. **Gray**. (1) Helium is liberated from thorianite, and a liberation of 28 per cent. has been effected; (2) the smaller the mineral is ground the more helium is liberated; (3) this liberation has a temporary limit when the mineral is reduced to a size of about 3μ; (4) it is impossible to say how the remaining 72 per cent. of helium is contained in the mineral, and to how much finer than $1\ \mu$ the mineral would have to be reduced to liberate the helium.—The expulsion of radio-active matter in the radium transformations: Sidney **Russ** and W. **Makower**. When the radium emanation is transformed into radium A, the process is accompanied by the emission of α particles with a velocity of $1{\cdot}70 \times 10^9$ centimetres per second. The portion of the atom from which the α particle has been emitted, which constitutes the radium A, must therefore recoil in a direction opposite to that in which the α particle is projected. If we further consider that the mass of the α particle is $4(H=1)$, and that of the active deposit of the order 100, it follows that at the moment of its formation this product must be travelling with a velocity of the order 10^7 centimetres per second. In ordinary circumstances, when the emanation is mixed with air at atmospheric pressure, the radium A particle will possess only sufficient energy to permit it to travel a fraction of a millimetre before being stopped by collision with air molecules. On the other hand, at very low pressures, these particles should travel considerable distances without being stopped by the rarefied air, and come to rest on the enclosure containing the emanation. The case is similar for the formation of radium B from radium A. To investigate these phenomena, discs were suspended, *in vacuo*, above surfaces rendered active by the various disintegration products of radium, and the activity obtained on the discs after exposure was measured in the normal manner by a quadrant electrometer. The principal results obtained in this paper may be summarised as follows:—(1) When radium emanation, in radio-active equilibrium with its products of disintegration, is condensed at the bottom of an evacuated tube immersed in liquid air, active deposit particles are radiated up the tube. This phenomenon is ascribed to the recoil of the residual atom when an α particle is emitted. (2) The law of absorption of this radiation, both in air and hydrogen, has been investigated. The radiation reaching a surface at a fixed distance from the condensed emanation is an exponential function of the gas pressure. (3) From the rate of decay of the activity collected on a surface exposed to the radiation from the emanation, it appears that both radium A and radium B reach the surface. (4) Radium B and radium C are both radiated through a vacuum from a surface previously rendered active by exposure to the emanation. Supposing that radium B emits only β particles, the radiation of radium C must be due to the recoil of the atoms when β particles are emitted.—*Sphaerostoma ovale*, n. gen., and *Crossotheca Grievii*, n. spec., an account of the structure and relations of the reproductive organs of *Heterangium Grievii*: Dr. Margaret **Benson.** *Sphaerostoma ovale* (*Conostoma ovale et intermedium*, Williamson) is the earliest Palæozoic ovule so far known structurally. It is a small ovule 3·5 mm. in length, and shows the same general type of organisation as the "Lagenostoma" series of ovules. The pollen-chamber, however, does not engage with the micropyle, but opens and closes with a very perfect mechanism, somewhat reminiscent of the peristome and epiphragm of Polytrichum. The paper also deals with the relation of this ovule to *Heterangium Grievii*, and with a new Crossotheca which is attributed to the same plant.

Physical Society, March 26.—Dr. C. Chree, F.R.S., president, in the chair.—The production of steady electrical oscillations in closed circuits, and a method of testing radio-telegraphic receivers: Dr. J. A. **Fleming** and G. B. **Dyke.** By the use of two such nearly closed oscillatory circuits, one being employed as a transmitting station and the other as a receiving station, these being placed at a distance of a few hundred yards from each other, what is practically equivalent to radio-telegraphic stations with open oscillators at very large distances can be constructed. Methods were described for producing in one of the closed circuits extremely constant damped oscillations by means of an induction coil or transformer, a spark-gap on which a steady jet of air is allowed to impinge, and a suitable mercury break. Means were described for ascertaining when the current in this transmitting circuit is constant. Instances were given of the ease with which detectors of various types, such as a magnetic detector, electrolytic detector, crystal detector, and ionised gas detector, could be compared for relative sensibility.—Effect of an air blast upon the spark discharge of a condenser charged by an induction coil or transformer: Dr. J. A. **Fleming** and H. W. **Richardson.** When an oscillatory discharge of a condenser takes place across the spark-gap in the usual manner by charging the condenser by an induction coil or transformer, the intermittent spark which takes place is a complex effect. It consists partly of a true condenser discharge and partly of an alternating-current arc due to current coming directly out of the induction coil or transformer. This arc discharge is a source of difficulty in making accurate quantitative measurements with electrical oscillations, and to produce a uniform oscillatory discharge this true arc discharge must be prevented or arrested. It was shown in the paper that this can be done by a regulated air blast produced in any convenient manner, thrown upon the spark-gap, provided that the spark-gap is small. The paper also described experiments made to investigate the effect of breaking up the spark-gap into smaller spark-gaps in series, both when the gaps were subjected to an air blast and also without the air blast.—The action between metals and acids and the conditions under which mercury causes evolution of hydrogen: Dr. S. W. J. **Smith.** The action between an acid and a metal, which results in the replacement of hydrogen, can be formulated without the aid of any hypothesis beyond the assumption that it is approximately reversible. The mode of formulation suggests a kinetic picture of the process by which equilibrium is in certain cases attained. This was described by the author, and it was pointed out that if a steady state is reached, after a certain quantity of hydrogen has been evolved, it will be defined by an equation of the form $ahM = bmH$. In this, a and b are constants at a given temperature, h and m are the concentrations of the hydrogen ions and of the metal ions respectively in solution, and H and M are specific constants of hydrogen and of the metal. The experiments described in the paper may be regarded as an attempt to justify the above equation when the metal is mercury.

Zoological Society, April 6.—Mr. F. Gillett, vice-president, in the chair.—Description of a new form of Ratel (Mellivora) from Sierra Leone, with notes upon the described African forms of this genus: R. I. **Pocock.**—An ichthyosporidian causing a fatal disease in sea-trout: Muriel **Robertson.**—A small series of fishes from Christmas Island, collected by Dr. C. W. Andrews: C. Tate **Regan.** Seven new species were described, comprising five blennies, a Pampeneus, and a Cirrhites. In connection with the last-named, it was pointed out that the Cirrhitidæ, as defined and limited by Dr. Günther, with the addition of Haplodactylus, form a very natural family.—Some new and little-known Hesperidæ from tropical West Africa: H. H. **Druce.** The paper contained remarks on, and descriptions of, some new forms of these butterflies lately obtained by Mr. G. L. Bates on the Ja River, Cameroons, and others from Nigeria. New species of the genera Abantis, Acleros, Gorgyra, Parnara, and Ceratrichia were described.

PARIS.

Academy of Sciences, April 13.—M. Bouchard in the chair.—The diffraction of Hertzian waves: H. **Poincaré.**—A general solution of the spectroheliograph: H. **Deslandres.** The spectroheliograph described, which is installed at Meudon, consists of four different spectroheliographs arranged round one collimator and astronomical objective, and controlled by four synchronised electric motors. These spectrographs are arranged for different classes of work, some having two and others three slits. The apparatus has already given interesting results on the black filaments of the upper layers of the solar atmosphere, especially the images of K_3 and Hα.—The transformations of the associated O networks: C. **Guichard.**—The integration of certain functional inequalities: Arnaud **Denjoy.**—A problem of Fourier: Henri **Larosse.**—The action of a continuous current on symmetrical chains of electrolytes not having common ions: M. **Chanoz.** Study of the gases disengaged by the action of copper salts on steels: E. **Goutal.** Three steels were studied, containing respectively 0·29, 0·64, and 1·38 per cent. of carbon, the solution used for the attack being that of the double chloride of copper and potassium containing a few drops of hydrochloric acid to the litre. The carbon dioxide, carbon monoxide, and hydrocarbons evolved were determined separately. The loss of carbon thus determined amounted to 0·01 to 0·05 per cent., and this loss is reduced by about one-half if the carbon in the residue is estimated without drying.—The quantitative analysis of the occluded gases in the lava from the last eruptions of Mt. Pelée and Vesuvius: M. **Grossmann.** Estimations were made of the total quantity of gas per 100 grams, and figures given for the amounts of carbon dioxide, oxygen, nitrogen, hydrogen, carbon monoxide, and methane. The various products from Vesuvius show marked differences in the quantity and composition of the gases evolved.—

The distribution of ferments in plant members and tissues: C. **Gerber.**—The hypotensive function of choline in the organism: Jean **Gautrelet.** By means of the Florence reaction, choline has been recognised in various glands of the horse, sheep, pig, ox, and dog. The hypotensive action of the alcoholic extract is shown to be due to the choline present, since this action disappears if the choline is precipitated. The alcoholic extract of the glands exactly neutralises the hypertensive action of adrenalin.—The intra-dermo-reaction to tuberculin in the treatment of tuberculosis: Charles **Mantoux.** The intradermal reaction can be used to measure the sensibility of the subject, and to control the quantity of tuberculin necessary for injection. The local reactions serve as a guide for the conduct of the treatment, and render it much more certain.—The treatment of genito-urinary troubles by direct action on the nervous centres: Pierre **Bonnier.** Details are given of the beneficial effects of slight cauterisations of the nasal mucous membranes in various diseases, especially those connected with the genito-urinary functions.—Sero-anaphylaxis of the dog: Maurice **Arthus.** —Sero-anaphylaxis of the rabbit: Maurice **Arthus.** —Some new facts concerning the transgressivity and the tectonic observed in the mountains of Algeria and Tunis: J. **Roussel.**—The polar magnetic storms in 1882 and 1883: M. **Birkeland.**

DIARY OF SOCIETIES.

THURSDAY, APRIL 22.

ROYAL SOCIETY, at 4.30.—Dynamic Osmotic Pressures: The Earl of Berkeley, F.R.S., and E. G. J. Hartley.—(1) The Theory of Ancestral Contributions in Heredity; (2) The Ancestral Gametic Correlations of a Mendelian Population Mating at Random: Prof. Karl Pearson, F.R.S.—The Intracranial Vascular System of Sphenodon: Prof. A. Dendy, F.R.S.—On the Graphical Determination of Fresnel's Integrals: J. H. Shaxby.

MATHEMATICAL SOCIETY, at 5.30.—The General Principles of the Theory of Integral Equations: F. Tavani.—The Equations of Electrodynamics and the Null Influence of the Earth's Motion on Optical and Electrical Phenomena: H. R. Hassé.—The Solution of a Certain Transcendental Equation: G. N. Watson.—The Physical Applications of Certain Conformal Transformations of a Space of Four Dimensions and the Representation of a Space Time Point by Means of a Sphere: H. Bateman.—Some Criteria for the Residues of Eighth and Other Powers: A. E. Western.—On the Discontinuities of a Function of One or More Real Variables: Dr. W. H. Young.

INSTITUTION OF MINING AND METALLURGY, at 8.—The Valuation of Mining Areas on the Rand: W. Fischer Wilkinson.—The "Wholesale" Idea in Gold Mining: W. R. Feldtmann.—The Computation of the Present Value of Developed and Undeveloped Mines: W. H. Goodchild.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Electrical System of the London County Council Tramways: J. H. Rider.

FRIDAY, APRIL 23.

ROYAL INSTITUTION, at 9.—Tantalum and its Industrial Applications: A. Siemens.

PHYSICAL SOCIETY, at 5.—On a Want of Symmetry shown by Secondary X-Rays: Prof. W. H. Bragg, F.R.S., and J. L. Glasson.—Transformations of X-Rays: C. A. Sadler.—Theory of the Alternate Current Generator: Prof. T. R. Lyle.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Development of Hydro-electric Power Schemes; with Special Reference to Works at Kinlochleven: J. M. S. Culbertson.

INSTITUTION OF MECHANICAL ENGINEERS, at 8.—Presidential Address: J. A. F. Aspinall.

SATURDAY, APRIL 24.

ROYAL INSTITUTION, at 3.—The Earth Movements of the Italian Coast and their Effects: R. T. Günther.

MONDAY, APRIL 26.

ROYAL SOCIETY OF ARTS, at 8.—Aërial Flight: F.W. Lanchester.

INSTITUTE OF ACTUARIES, at 5.—Notes on Mortality and Life Assurance in India: A. T. Winter.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Road Motors ("James Forrest" Lecture): Colonel H. C. L. Holden, F.R.S.

TUESDAY, APRIL 27.

ROYAL INSTITUTION, at 3.—The Brain in Relation to Right-handedness and Speech: Prof. F. W. Mott, F.R.S.

ROYAL STATISTICAL SOCIETY, at 5.

ZOOLOGICAL SOCIETY, at 8.30.

FARADAY SOCIETY, at 8.—Experiments on the Current- and Energy-Efficiencies of the Finlay Alkali Chlorine Cell: Dr. F. G. Donnan, Dr. J. T. Barker, and B. P. Hill.—On the Coefficients of Absorption of Nitrogen and Oxygen in Distilled Water and Sea-water, and of Atmospheric Carbonic Acid in Sea-water: Dr. C. J. J. Fox.—On the Electromotive Force of Certain Platinum Compounds, with Special Reference to the Oxygen-Hydrogen Gas Cell: Dr. P. E. Spielmann.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Annual General Meeting.

WEDNESDAY, APRIL 28.

GEOLOGICAL SOCIETY, at 8.—The Boulders of the Cambridge Drift: their Distribution and Origin: R. H. Rastall and J. Romanes.—The Nephrite and Magnesian Rocks of the South Island of New Zealand: A. M. Finlayson.

ROYAL SOCIETY OF ARTS, at 8.—The Resources of the Peruvian Andes and the Amazon: C. R. Enoch.

BRITISH ASTRONOMICAL ASSOCIATION, at 5.

THURSDAY, APRIL 29.

ROYAL SOCIETY, at 4.30.—*Probable Papers*: A Phenomenon connected with the Discharge of Electricity from Pointed Conductors (with a Note by Prof. J. Zeleny): Prof. H. T. Barnes and A. N. Shaw.—On the Effect of Temperature on Ionization: J. A. Crowther.—The Wave-making Resistance of Ships; a Theoretical and Practical Analysis: T. H. Havelock.—The Ionisation in Various Gases by Secondary γ Rays: R. D. Kleeman.

ROYAL SOCIETY OF ARTS, at 4.30.—The Problem of Indian Labour Supply: S. H. Fremantle.

FRIDAY, APRIL 30.

ROYAL INSTITUTION, at 9.—The Pitfalls of Biography: Dr. Edmund Gosse.

SATURDAY, MAY 1.

ROYAL INSTITUTION, at 3.—The Earth Movements of the Italian Coast and their Effects: R. T. Günther.

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