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MODERN ELECTRODYNAMICS.

Électricité et Optique: la Lumière et ses Théories Électrodynamiques; leçons professées à la Sorbonne en 1888, 1890 *et* 1899. Par H. Poincaré. Deuxième édition, revue et complétée par J. Blondin et E. Néculcéa. Pp. x+642. (Paris: G. Carré et C. Naud, 1901.) Price Fr. 22.

IN the present state of electrical and general physical theory there are probably few undertakings more useful towards progress than a critical discussion of the views of other writers by one who has himself thought deeply and read widely on the subject.

We may recall the stimulus afforded to the progress of Maxwell's electric theory on the Continent by Helmholtz's early series of critical memoirs (now largely out of date, having served their purpose) that were devoted to the examination of the relation in which that theory stood to the views of electrical action then current.

The lectures of M. Poincaré, reported and published by his pupils about ten years ago, possessed great interest as being an account of the then fresh advances constituted by the experimental investigations of Hertz, from the hand of a writer who occupied one of the highest positions both in the domain of pure mathematics and in that of its physical applications. The writer's unlimited command of analysis and the range of his interests were certain to shed new lights on the subject-matter of which he undertook the exposition. A second edition of the "Électricité et Optique" is now published in a volume of 640 pages, of which about half consists of a report of lectures delivered at the Sorbonne in 1899 on the still more recent improvements of Maxwell's electrodynamic theory which are associated largely with the name of H. A. Lorentz. It is this latter half of the book, giving the writer's reflections and criticisms on a development which is still fresh, that will naturally present the chief interest for others who have meantime been following the progress of the subject.

The main feature of the new standpoint is the resuscitation of the idea of electricity as representing something permanent like matter. In Maxwell's later writings, in which he was mainly occupied in eliminating the hypothetical illustrations and models which had guided him to his theory, but were not logically necessary to its formal exposition, there was a tendency for the older idea of an electric charge, as representing something real, to be eliminated. According to his view, the electric current always flows in closed circuits like a current of an incompressible fluid, so that there is nowhere any tendency to accumulation of electrodynamically effective electricity. It seemed, therefore, possible to do without any introduction in detail of an entity whose flow was restricted by the condition that the quantity of it in any given volume could never alter. This conception of circuital electric flow (to use Lord Kelvin's term) required the ascription of properties the same as those of currents to electric excitation both in dielectric material substances and in the free æther itself. The displacement current thus introduced is, in fact, the fundamental feature of Maxwell's electrodynamics. Its assumption led directly to a simple and perfectly complete theoretical account of the electrodynamics of material systems at rest, on the basis of laws established long before by Ampère and Faraday; the application to bodies in motion was, however, left by Maxwell in an incomplete and tentative state. In 1872, when he published his treatise, the circumstance that the laws of electrolysis imposed the idea that electricity was in some sense or other atomic was definitely realised, but with a certain reluctance; while in the treatment of bodies in motion the explicit recognition that a moving charge acts as a current had, owing to an oversight arising from his preoccupation with the medium, to be formally introduced into his equations by Fitzgerald ten years later, though Maxwell fully accepted such action as a fact all the time.

This plan of ignoring electricity and treating electrodynamics on the basis of a uniform medium with physical constants affected by the presence of matter, and subjected to various vector disturbances whose nature is unknown, but which are connected by partial differential relations expressing the laws of Ampère and Faraday, has been very fully developed by Heaviside and by Hertz. In both cases compensation is sought for the variation of the energy of each element of the medium, solely in the work of tractions exerted on its surface by the surrounding parts. In Heaviside's discussion the problem was treated with great generality and comprehensiveness; it will suffice here to pass in review the salient features of the more concise analysis advanced by Hertz. The treatment of stress by the method of energy requires displacement of the medium; and so the problem of ponderomotive forces becomes related to the general question of moving media, which is the part of the subject that provides the crucial tests of theory. The electromotive phenomena in media at rest are, on the other hand, all involved in the adoption of the aforesaid laws of Ampère and Faraday, as a description of the properties and behaviour of the medium. The same description is extended to media in motion after the manner of Faraday, and the deductive part of the argument is there confined to the determination of the ponderomotive mechanical forces. To obtain them, Hertz subjects his single uniform medium, which he takes to be the seat of the electric and magnetic energy, to static strain without finite motion, and computes the time-rate of alteration of the energies thereby produced in a given element of its mass. He supposes that the polarisations per unit volume, being affections of the medium, are simply convected along with it. If the element of mass were dynamically self-contained and not subject to tractions from the surrounding parts, its energy would be conserved so that this time-rate of alteration should vanish. As it is, the alteration does not vanish, but represents the work done in the element by the tractions acting on its surface. As the element is part of an elastic medium, the work of such surface-tractions ought to be expressible in the form of work of the stress-system, existing in the element, to which these tractions belong. Now the expression for the variation of the energy is thrown by Hertz into the latter form, in fact without the use of any electrodynamics in the analysis; and this leads him at once, for the case of isotropic media, to a self-conjugate electromagnetic stress-system, the same as Maxwell's, as providing the reacting mechanical forces required for equilibration of the outstanding energy. But for æolotropic media there arises a bodily torque in addition to this stress; this torque is included in Maxwell's general type of magnetic stress, and prevents it from being self-conjugate for that case. Hertz is unwilling to admit such a type of stress, which could not exist in an ordinary elastic solid; but he is at a loss to know what to do with this new part, and simply drops it, retaining the self-conjugate part as the stress in his electric medium. But for this the theory would be a consistent one on his premisses; and the result for the free æther, or wherever there is no material polarisation, is, in fact, the stress which Maxwell showed was competent to represent the actual mechanical forces. It is to be observed that this is all that is to be got out of the statical application of the principle of energy to his medium; the kinetics of the electromotive play being assumed as known, outstanding variations of the energy in slow changes are to be ascribed to the work of mechanical forces. No success has been achieved in the problem of reducing the electromotive play in media in motion to definite self-contained dynamics on any other basis than the theory of electrons; the charge must consist of discrete independent elements, each with its own electric field. The mode of treatment here sketched introduces, among other things, a mechanical force of an electric field on changing magnetic polarisation as the counterpart of the known mechanical force of a magnetic field on changing electric polarisation; this, on the theory of electrons, is non-existent.

The treatment of electrodynamics on the basis of discrete electrons is a branch of statistical molecular theory, like the kinetic theory of gases, and involves the refined considerations connected therewith, including the estimation of averages instead of the following out of individuals. The care that is thus necessary in the analysis may be illustrated by a temporary slip that has crept in at the beginning of the discussion of Lorentz's theory (§ 333), in which the single principle of continuity of flux of true electricity appears as a consequence of the addition of two independent formulæ, (3) and (4), neither of which appears again. They cannot be both true, or there would be two such principles of continuity instead of one. It would seem that the term density has been inadvertently used in two different senses, ultimately as the volume density of electric charge in the medium which depends on how closely the electrons are packed in it, but meanwhile as the density of electricity in an electron, supposed to be itself a small and rigid though mobile volume-distribution of charge. This local oversight, doubtless due to imperfect reporting of the lectures, illustrates an actual disadvantage of a completed hypothesis, which insists on a full specification of an electron, over the less complete physical specification, which, recognising that there is more in the constitution of the molecule of matter than our philosophy may ever reach, is content to regard it simply as the unknown central point or pole of its surrounding field of force.

The general plan of development of electrodynamics on this basis that is adopted by M. Poincaré consists in writing down equations of motion for each electron, by assigning to it a mass and considering it to be acted on by the averaged or smoothed-out electric and magnetic forces of the field that surrounds it, and finally passing to equations for the medium in bulk by summing or averaging the results for all the electrons per unit volume. This method is in keeping with the astronomical traditions of mathematical physics, in which the problem is put in definite terms at the beginning, and the analysis is confined to surmounting the difficulties, purely mathematical, that arise in its unravelment. There is, however, a different kind of theoretical physics which has had more success in this country, which recalls the names of Young, Stokes, Kelvin and Maxwell, and has more recently in Germany been illuminated by the example and inspiration of Helmholtz. Care is taken to avoid an irrevocable formulation of the problem in advance, only its broad dynamical features being worked in; while all the light that cognate but better understood branches of physics can shed by way of illustration or analogy is pressed into service. Thus, instead of writing out isolated equations of motion for the ideal case of a single electron—on the tacit assumption that no other electrons are near which would disturb the averaged field that is alone supposed to affect it—it is recognised that electrons, possibly in very large number, are somehow involved in the structure of each individual molecule, and that the fundamental and essential element in the physics of matter in bulk is this permanent molecule considered as a single free vibrating system, with free periods producing a radiant spectrum, which are involved in the intrinsic mutual influence of these oscillating electrons. The simplest type of framework for the structure of such a system is to assume provisionally a gyrostatic orbital constitution of some kind, which assists in holding its parts together in some such way as the whirling motion holds together a vortex ring in fluid. Our dynamical plan is thus now no longer fixed, but flexible; in fact it must remain so until we can form a definite representation of such a molecule instead of only a general idea of it. Yet the uniformity of physical law for matter in bulk shows that we ought to be able to develop our synthesis without waiting for such knowledge, which may even quite possibly be unattainable. In this procedure we must attend primarily to such activities of the molecules as can be cumulated by addition, so as to produce aggregate results expressible per unit volume of the medium, and eliminate the remaining non-cumulative disturbance which is related to practically irreversible or thermal phenomena. Of the former class is the strain in the configuration of the molecule produced by the electric or magnetic field in which it is situated, this distortion being represented for statical purposes by a single vector quantity, the induced electric or magnetic moment of the molecule, which aggregates into induced polarity of the material medium. Such, also, are the types and energies of free vibration about the steady configuration, which have been analysed in their aggregate into definite periods by the spectroscope. Here our knowledge is related to general principles rather than special systems, and progress is possible, thanks to the purely abstract general formulation of dynamics by Lagrange and Hamilton, and also to the supports and signposts afforded by such phenomena as anomalous optical dispersion and the magnetic subdivision of spectral lines. Outside such properties our power of tracing relations is very limited and imperfect, in the absence of control over the individual molecules, there being little to go upon except the two principles of thermodynamics, those of energy and entropy. The difference between the two points of view, the definite but partial and limited mathematical illustration and the wider but largely undetermined model, crops up, for example, in M. Poincaré's discussion of conducting media (§417), in which the combination of polarisation with conduction arising from wandering ions does not appear to suggest itself: "remarquons d'abord qu' ayant affaire à des conducteurs on n'a plus de polarisation," so that he has only to deal with electric current and electric density, doubtless in some degree with a view to save complication in a didactic exposition; whereas from the other more physical representation of a material medium one does not readily conceive a state of affairs in which only conduction and convection of free charges are present, but would proceed rather to examine under what circumstances polarisation can be practically neglected in comparison with conduction. This, of course, can be done in ordinary electrodynamics, as was doubtless in the writer's mind. But in the optical phenomena of metals it was recognised by Maxwell himself from the earliest stages—thanks mainly to the physical models on which he cultivated his ideas—that both agencies were essential; while recent closer examination has shown (cf. *Phil. Trans.*, 1895 A, p. 711, and in detail in recent papers by Drude) how naturally their combination represents the general features of metallic reflection as revealed by the most valuable and extensive measurements of Drude and other experimenters.

In working out the analysis, our author follows Lorentz in calculating directly the electrodynamic effects propagated from the moving electric charges which are the source of all the disturbance. He expresses this in terms of the "retarded vector potential" of the true current, a vector whose components are the potentials of the three components of its distribution, considered, however, as travelling out from them and becoming established around them with the velocity of radiation. A procedure which concentrates attention on the simply extended though molecularly constituted medium, to the exclusion, as far as possible, of the individual moving electrons, can get on more simply in Maxwell's manner by using the fictitious total current, which includes æthereal displacement as well as translation of charges; then the retardation of the vector potential is dispensed with, and all the functions are referred to the same instant of time, so that attention can be concentrated on the processes of averaging, undisturbed by mathematical complexities.

The distinction above sketched between the crystallised mathematical and the fluent physical point of view is at the root of what is a prominent characteristic of the writer's criticism. The development of electrodynamics appears as split up into so many independent and largely irreconcilable theories; there are headings, "theorie de Weber, de Maxwell, de Helmholtz, de Hertz, de Lorentz, de Larmor." Whereas on the view which works by models and general ideas rather than by formulas there is but one theory of electrodynamics—at any rate only one æther-theory—which has put on various modifications and has adopted various forms of expression, in the course of gradual improvement so as to become a closer and closer mental picture of the orderly course of phenomena; the subject presents itself rather as a continuous historical development, into which somewhat different paths all converge, than as a series of competing modes of explanation.

There is one feature in M. Poincaré's exposition for which the English reader will be grateful. A considerable trouble in the assimilation of mathematical investigations on this subject is the diversity of the notations (not to speak of systems of units) that are in use. All the available letters of most available alphabets have been pressed into service to represent the numerous types of quantities that occur; and if there is not a consistent basis of usage it must follow that the same symbol will be made to represent different things by different writers. M. Poincaré has kept as close as circumstances allowed to Maxwell's own notation, thereby acting up to the appeal of Boltzmann ("Vorlesungen über Maxwell's Theorie," 1891, preface), who found it necessary to actually construct a key for his own use to connect the notations of the principal German writers. Although the simplifications introduced by Heaviside, and subsequently in more formal guise by Hertz, did much to clear away the unessential accumulations that had overlaid Maxwell's theory, they did not in any sense transform it; and recent developments may be held to have justified the superiority, as a working basis for further advance, of the original elastic framework in which Maxwell set the theory, over the condensed *précis* of established results by which Hertz temporarily replaced it. It seems, therefore, unfortunate that the condensation of notation which was a part of Hertz's modification should have reacted to introduce some confusion in the notation of the more complete theory.

The development of electrodynamics, which was firmly established as the proximate foundation of all physical science, certainly of all that has relation to the æther, by Maxwell about forty years ago, has been going on with rapidly accelerated progress, both on the experimenta- and on the theoretical side, during the last ten years. New points of view have rapidly come up, have sometimes been as rapidly transcended. It is not surprising, therefore, that the discussion in the last chapter, which mainly relates to the mechanical and *quasi*-mechanical models of the British school, is somewhat out of date; indeed, it is largely constructed on the basis of an abstract, published in advance, of an imperfect first draft of theory contained in a memoir of date 1894, much as a palæontologist reconstructs a fossil organism from some of its bones. In the recent Lorentz-memorial volume M. Poincaré has himself revised some of his positions.

It is by this sort of discussion that crude theories are sifted and worn down into symmetry and order. And it is matter of congratulation that an analyst of M. Poincaré's vast command of all the resources of modern mathematics finds time not only to apply his genius to a thorough revision of the methods of mathematical astronomy, but also to survey the field of general physics as he has done in this interesting volume. In these days of extreme specialisation such surveys promise a special harvest, but few have sufficient breadth of learning to undertake them. The modern development of the theory of functions arose largely from transplanting the ideas of flux and force of physical mathematics into purely abstract problems. In astronomy, M. Poincaré's work has partly repaid the debt; it remains to be seen whether in electrodynamics a further instalment will be repaid, or analysis again become the debtor.

Anyhow, while pure analysis is ramifying into vast new regions and becoming more and more specialised, it is fortunately still possible for a single person to acquire an effective knowledge of the whole domain of theoretical physics. As in literature, so in scientific exposition, the saving virtue is style. If we call to mind the history of any of the theories which form the established heritage of common knowledge—such as hydrostatics or pneumatics —in their early inception they presented just as complex problems as the theory of the æther does now. But by the efforts of successive generations of expositors they have gradually been worn down, and the artificial appliances of symbolic reasoning have been eliminated or illuminated by the cultivation of new ideas and modes of expression. A theory of the æther hardly existed in any adequate sense half a century ago. Progress has recently been so rapid both on the purely scientific side, and in the reaction of modes of thought that have been fostered by industrial developments, that in a short time we may be able to picture to ourselves the operation of the æther with as much clearness and directness as we now understand the functions of the atmosphere. J. L.

GILBERT WHITE OF SELBORNE.

The Life and Letters of Gilbert White of Selborne. Written and Edited by his Great-Grandnephew, Rashleigh Holt-White. Two Vols. 8vo. Pp., Vol. I., xv + 330; Vol. II., ix + 300. (London: John Murray, 1901.) Price 32*s*.

The Natural History of Selborne. By Gilbert White. Pp. vii + 381. (London: J. M. Dent and Co.) Price 1*s*. 6*d*. net.

NOTHING nearly as good as Mr. Holt-White's book has ever yet appeared about Gilbert White; it supersedes Bell's two volumes, to which we have so far had to go for the real characteristics of the great naturalist, and it is hardly possible that it will ever itself be superseded. In its skilful treatment of materials it is amply worthy of its dedication to a great scholar, the present Provost of White's College. The editor has been content to let White and his correspondents speak for themselves, but rarely interposing to set us right on some misconception, or to explain (often, it is clear, after much expenditure of time and trouble) who are the persons referred to in the correspondence; and the result is one of the most delightful stories of a quiet life ever told in our language. As we reluctantly close the second volume, we feel that we now know White perfectly well as he really was. There is no need for a reviewer to anticipate the pleasure of readers by attempting to copy the picture.

It should be said, however, that this is not only a book for naturalists or lovers of nature, but for readers of every kind. Indeed, the charm of it seems to lie chiefly in the picture of life and manners it gives us—of the life of quiet country folks, with sedate but real interests of their own, using their time well, and sharpening their faculties continually under the gentle and unconscious stimulus of their alert and keen-eyed neighbour, friend or uncle. Gilbert White is the centre of the group, and he seems to be setting all the members of it at work on something. He lets drop a hint, asks a question, administers a very gentle reproof, and the recipients of his letters treasure them up, and must, we feel, have acted on them.

One or two points of special interest may be noted here. It is very pleasant to find that Mr. Holt-White has been able to prove conclusively the falsity of the traditional Oriel notion that White retained his fellowship when he should not have done so. The four or five farms which he inherited brought him hardly more than a hundred a year; and towards the end of his life his relations with his College seem to have been quite cordial. It is, of course, natural that in a College where Fellowships were few in number, yet open to competition from the whole University, the locking up of a Fellowship for fifty years should at the time have roused a certain amount of criticism; but that criticism was made under the impression that White was a wealthy man, and to revive it, as it has been revived, in these days, is to do White a serious injustice. The Oriel of that day may be said to have endowed science unconsciously as it has never done since; for White, though not a man of science in the modern sense, has had a powerful influence in stimulating scientific habits.

Among the many delightful treasures in this book must be mentioned the letters of Thomas Mulso, now published for the first time—letters as bright, witty and natural as any that have ever been printed; and the two letters of Montagu, written after the publication of the "Natural History of Selborne," which offer a curious contrast, in their intense and almost feverish ardour, to White's quiet and leisurely way of going about his work. But perhaps those who love the eighteenth century and all its ways will find their greatest pleasure in the enthusiastic diary of Miss Kitty Battie, a visitor at Selborne. Little did that happy girl know that her notes, jotted down in the fulness of a grateful heart, would be treasured more than a century afterwards by readers as enthusiastic as herself.

Let us hope that this work, undertaken by a member of the White family, with full access to all records, and with the invaluable aid of Prof. Alfred Newton, may permanently satisfy all who wish to know about White's character and habits.

The second book mentioned at the head of this notice is a handy little volume in small octavo, which can be carried in the pocket, and has the great merit of being free from unnecessary notes and still more unnecessary illustrations. The few notes which it contains, by Mr. Charles Weekes, are at the end of the volume, and seem to be for the most part accurate and to the point. The text is reprinted from the first edition of the "Natural History," with a few slight alterations in spelling, which might perhaps have been dispensed with. If, for example, White wrote "plowed," there is no reason at all why an editor should substitute "ploughed." And it is a pity that the editor, in prefixing a few lines of Richard Jefferies' to the book, should not have spelt his name correctly. But on the whole the edition is a good one; far better, in fact, than many of much greater pretension.

COSMOGONY AND EVOLUTION.

Entstehen und Vergehen der Welt als Kosmischer Kreizprozess. Auf Grund des pyknotischen Substanzbegriffes. Zweite und erweiterte Auflage. Von. J. G. Vogt. Pp. viii+1005. (Leipzig: Ernst Wiest, 1901.)

A REVIEWER can scarcely be expected to read the whole of the thousand and odd pages which Herr Vogt has required to express his views on the origin and decay of the world. As one looks down the table of contents, he feels that it would require a mathematician, a chemist, a physicist, a biologist rolled into one to do justice to the many various subjects which here come under notice, and if oppressed with this view he begins with the "methodologische" introduction and struggles with the adjectives, "fearfully and wonderfully made," he may be tempted to turn for a little relaxation to the "explanatory illustrations" scattered through the text. One of these (p. 260) is to explain the genesis of the solar system. The author gives some account of the cosmogony of Kant and Laplace, and recalls some of the objections which have been urged against these views. He is particularly severe on the insufficient explanation offered for the density of the planets closest to the sun. Saturn, he states, retired from the ring-making process when the mass of the ring was 1/118 of its own mass; while in the case of Mercury the sun continued to produce a ring the mass of which is only 1/4,316,550. The evident distaste of Saturn to form rings of smaller mass leads the author to abandon the ring hypothesis altogether and to offer an alternative theory. He conceives spheres of operation (Wirkungssphäre) and Deformierungssysteme (not so easily translated). But if we will imagine three circles, the centres of which form an equilateral triangle and each of the circles touches two others, the circles will form "Deformierungssysteme," while the enclosed triangular space bounded by the three circles is a "field of operation." Now in the small space near the points of contact we get the smaller planets formed, Mercury and the earth on one side and Mars and Venus on the other, each planet touching two circles and the next larger planet. Jupiter in this way has room for his giant bulk, pushing Saturn a little on one side, but otherwise is not inconveniently crowded. Of course, the whole merit of such a cosmogony depends upon the "Deformierungssysteme," and for the manner of working these the reader must be referred to the book itself. The second diagram (p. 949) is to illustrate the precession of the equinoxes. Here one would say there is no room for imagination; we have to do with a problem in rigid dynamics which is susceptible of but one explanation. But if any one thinks this, he has not reckoned with Herr Vogt, who, as a man of ideas, begins at the beginning. Before attempting to explain the cause of any modification in the position of the polar axis it is necessary, he tells us, to understand the laws which determine the constant position of that axis. These laws he proceeds to unfold on "phoronomische" principles, and in his endeavour to follow the author in these same principles the student will be not a little startled to find it necessary to project the plane of the Milky Way on a diagram to explain precession. But he will probably not read beyond the following sentence:—

"The North Pole describes a circle on the sky in about 26,000 years. We can call this circle the projection circle of the absolute orbit of the earth, therefore indirectly the solar orbit, and denote these 26,000 years as the period of the sun in its orbit."

After this one is not surprised to learn that the sun will have a more or less intensive effect on the tension of the æther according to its position in this orbit, and thus to be led to a satisfactory explanation of the phenomenon of the Ice Age.

Herr Vogt is to be congratulated on having found a publisher willing to express these views in a book of handsome appearance, and when one learns that an earlier edition has long been exhausted he is tempted to doubt whether German education is of the elevated character that is sometimes represented. W. E. P.

OUR BOOK SHELF.

The Geological History of the Rivers of East Yorkshire. By F. R. Cowper Reed, M.A., F.G.S. Pp. vi+103. (London: C. J. Clay and Sons, 1901.) Price 4*s.* net.

SINCE Jukes, some forty years ago, explained how rivers cut through escarpments, the origin of their valleys has been well understood in a general way. Much, however, remained to be learnt about the development of particular rivers and the changes which have brought about present drainage areas; and these subjects have been so attentively and successfully studied by American geologists, notably by Prof. W. M. Davis, that their methods of interpretation have been followed by several observers in this country. The present work by Mr. Cowper Reed gained the Sedgwick Prize Essay for 1890, and is a capital exposition of the evolution of the rivers in East Yorkshire. After giving a general account of the various formations, he points out that the original "constructional surface" on which the present river system was initiated, was a plain formed by the Chalk and other Upper Cretaceous strata, and was upraised in early Tertiary times and perhaps partially eroded during the uplift. Having a greater elevation in the west, the direct ancestors of the present rivers took rise from the higher grounds and flowed eastwards, the Tees and Esk forming one river, the Swale and Ure flowing also direct to the coast, which formerly extended much further eastward, and the Nidd, Wharfe and Aire uniting and flowing out by the Humber. A long period of subaërial denudation followed the initiation of these consequent streams, there was a gradual lowering of the area, and there arose the subsequent river Ouse, which captured the Swale and Ure, the Nidd and Wharfe, conducting their waters into the Humber drainage. Towards the close of the Oligocene period, when the area had been nearly reduced to base-level by the formation of an extensive peneplain and the rivers had attained old age, there was considerable upheaval, accompanied by further movements along pre-Cretaceous lines of flexure, especially in the Moorland range of the Jurassic region. The rivers thereby regained youth and activity, their directions were locally modified, and thus were produced some of the main features in the present topography. Further changes, however, led to other modifications; there was depression towards the close of the Pliocene period, and subsequent elevation in Glacial times. With regard to the Boulder Clay the author judiciously remarks that "the land-ice theory appears to offer fewer difficulties than any others and to explain matters more satisfactorily." In any case large tracts, excepting some of the higher grounds, were buried beneath drift deposits, and the valleys were choked up. When the land had lost its icy mantle, some of these old valleys were revived, but in other cases the streams followed new courses. The story of all these changes is clearly told by Mr. Reed, and although there is room for difference of opinion in matters of detail, the main results are based on fact; and the essay may be profitably studied by those interested in the origin of our scenery.

Fergusson's Surveying Circle and Percentage Tables. By J. C. Fergusson, M.Inst.C.E. Pp. 84. (Published by the Author, 1901.)

THIS is an account of a device intended to replace the dial or circles in magnetic compasses and surveying instruments, with numerous illustrations of its application for the purposes of engineers, surveyors, naval and military officers and travellers. Half of the circle is divided into octants, and the graduation of each octant is effected by dividing its tangent, which is equal to the radius, into 100 equal parts and then drawing lines from these divisions to the centre of the circle. The divisions on the octant thus always subtend equal spaces on an offset laid out at right angles to the quadrantal radius. The spaces on the octant divided in this manner correspond to a hundredth part of the radius, and the angles being read in percentage divisions, trigonometrical formulæ are replaced by simple arithmetic. A considerable simplification of several practical problems is suggested by the examples given, but the advantages of the method can scarcely be judged without actual experience. It is stated, however, that many distinguished engineers and surveyors have expressed complimentary opinions as to its merits. Messrs. T. Cooke and Sons are the manufacturers of the new circle, which can be adapted to old or new instruments.

How to Know the Indian Ducks. By F. Finn. Pp. iv+101. (Calcutta: Thacker, Spink and Co., 1901.)

IF the right to include under the name of "ducks" both geese and swans be conceded to the author (and we have some doubt whether it should be), we have nothing but commendation for this excellent little volume. Years ago, when duck-shooting on the Ganges, we have a vivid recollection of our own regret at being unable to identify all the various representatives of the duck tribe included in our "bag," and we have little doubt that this regret has often been shared by other sportsmen. For the future, however, there should be no difficulty whatever in determining the species of any member of the tribe which may fall to the gun of the sportsman in India, as Mr. Finn's volume is small enough to be carried in the pocket without inconvenience, while the lowness of its price brings it within reach of every one. Needless to say, as the author is an accomplished ornithologist who has devoted special attention to the Indian Anatidæ, the descriptions are all that can be desired from a scientific point of view, while the simple language in which they are written, and the useful "keys" for the identification of species, render the volume admirably adapted to the needs of sportsmen.

It is for this class, indeed, that the work is primarily intended, as the author tells us in his preface; and the fact that the substance of the text has already appeared in the form of a series of articles in the columns of the *Asian* newspaper bears testimony to its favourable reception by Anglo-Indian sportsmen.

On more than one occasion we have directed attention in these columns to the confusion caused by the diverse systems of nomenclature followed by ornithological writers. In the present instance we are glad to see that the author endeavours to promote uniformity in this respect by following the classification and nomenclature adopted by Mr. W. T. Blanford in the "Fauna of British India." R. L.

LETTERS TO THE EDITOR.

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

On the Determination of Positions in Polar Exploration.

HAVING in view the importance of this subject in connection with the forthcoming Antarctic expedition, a brief comment on the results obtained in high latitudes, with which we have been favoured during the last few months, may not be out of place. The publication of the scientific results of the Nansen expedition is now before us, and these, together with a few which (without further explanation perhaps) scarcely merit the employment of this adjective, and which are to be found in the pages of Mr. Borchgrevink's account of the *Southern Cross* expedition, afford food for reflection, but whether they could be more satisfactorily dealt with by a professional man of science or a professional humorist may be open to question. The first have resulted in a series of deductions and suggestions which will strike thoughtful men as being eminently unpractical, and the latter is responsible for considerable confusion of mind in regard to the geographical positions of the most important points to which the expedition just about to start is instructed to proceed.

These being for the moment the more important, may be dealt with first. It has already been pointed out by NATURE that the recorded observations of the Borchgrevink expedition are extremely unsatisfactory, owing, possibly, to the work of a copyist ignorant of nautical calculations, but besides being improperly copied they are improperly computed. One, owing to the use of the secant of an erroneous latitude, is made to produce a longitude 22′ in error. Another computes the declination with a correction for longitude instead of for the Greenwich date. A third professes to find the chronometer error to a couple of seconds by a lunar eclipse (a feat which, if true, is miraculous); and a fourth produces a longitude of the harbour under the great ice barrier from which Mr. Borchgrevink effected his landing which is said by the navigating officer to be 164° 32′ W., by the commander 164° 10′ W., and by the president of the Royal Geographical Society 162° 30′ W.

The position of Cape Adare, again, is of the very first importance. Ross placed it in 71° 18′ S.; Mr. Borchgrevink finds this latitude correct, but places it 36′ further to the East; while Sir C. Markham, if we may judge by the *Geographical Journal* for July, has apparently been informed that it lies in lat. 71° 30′ S.

Much satisfaction has been expressed at the supposed verification of the position of two groups of islands (the Balleney groups), but much astonishment has also been expressed by thinking men that the Geographical Society can attach the faintest importance to the determination either of their number or their position by officers who, in discussing the subject, contradict each other flatly both as to the date, the appearance and the distance of the land at the time of the discovery; to say nothing of the suggestion that at distances ranging from 90 to 40 miles abundance of detail, including crevasses, and the shore line were plainly visible.

Turning to the scientific results of the Nansen expedition, and having in view the remark of your reviewer (NATURE, June 13), that the volume is to be welcomed as exceedingly opportune in view of the approaching Antarctic expedition, I should like to call the attention of the scientific staff to certain information which they may find it interesting to put to a practical test. It is here suggested :—

(1) That the value of refraction can be estimated from astronomical observations taken during a drift, when the latitude by which the altitudes are computed depends upon the unknown refraction and the refraction upon the unknown latitude. (See table of refractions).

(2) That the altitudes necessary for the computation of a lunar distance can be calculated by a man having no knowledge of his Greenwich time and being uncertain of his longitude to the extent of from 15 to 25 degrees. (See Nansen's lunar, taken August 10, 1895).

(3) That the discrepancy between two sets of altitudes taken, the one with a glass horizon labouring under suspicion and the

other with a pocket sextant and ice horizon, justify the conclusion that the terrestrial refraction was so abnormal, that the computation of the latitude necessitated the reversal of the sign of the dip; but that this state of things was local, and that the observations of another observer only ninety miles away would not be so affected, though the temperature and general conditions were in both cases practically the same. (See comparison between the corrections for refraction in the meridian altitudes of Nansen at his Farthest North, and Hansen on board the *Fram*, April 6, 1895).

Admitting to the full the truth and justice of the remark of your reviewer in connection with the observations taken during Nansen's sledge expedition, that "the fact that observations were taken at all is the strongest possible evidence that scientific zeal is compatible with the possession of remarkable courage," it must also be admitted that a comparison of these scientific results with those which Nansen obtained from the same times and altitudes proves that scientific zeal and the power of taking observations are also compatible with the inability to comprehend the very elementary fact that if the results of two or more observations differ widely from each other neither is trustworthy, and that geographical positions and condemnations of the work of such men as Julius Payer and Wyprecht cannot, and ought not, to be based upon them.

Should any student of practical nautical astronomy go to the trouble of making this comparison, he cannot, I think, fail to perceive at every step that, however painstaking Prof. Geelmuyden and his colleagues have been in their attempt to plot Nansen's route on his celebrated sledge journey, they have been compelled to ignore his own statements and his own workings, and while straining at the scientific gnat they have freely swallowed the practical camel. Dr. Nansen had led us to believe that the scientific results would explain and justify his already published results. It can be easily shown that one or the other is hopelessly wrong. They are totally irreconcilable. If it is for one moment admitted that Nansen had the opportunity and ability to work the common observation for longitude by chronometer, then Prof. Geelmuyden's primary hypothesis is unsound. If it is maintained that that hypothesis is even approximately correct, Nansen's own recorded results become ridiculous.

Turning from matters of fact to matters of opinion, two statements of great interest to explorers in high latitudes may be noticed. On p. 14 we are informed that one of the computers employed with advantage the difference of altitude near the prime vertical to determine the latitude. Now in low latitudes, where the change of altitude is rapid, say from 10′ to 15′ per minute of time, a result within five or ten miles of the truth is perfectly possible. In latitudes from 70° to 85° N., with altitudes changing at most 5′ or 6′ per minute of time, and affected by refraction abnormal in itself, and varying rapidly according to no well-defined law, the method entirely fails. If a chance observation appears to justify its use, the altitudes must be accidentally or miraculously correct.

The remark on lunars, p. 22, will strike experienced observers as exceedingly curious. "The results," says Prof. Geelmuyden, "are not satisfactory."

Table C., p. 44, shows that eight observations were taken at various times, and from them Greenwich Mean Time was determined with errors varying from 18 seconds to 2 minutes. On the assumption that the explorers might have been dependent on them, their positions would have been affected with a maximum error of 30′ of longitude, or about four geographical miles. Let future explorers note this. It may safely be affirmed that these results will seldom be surpassed by men taking lunars under Arctic conditions. It may with equal truth be said that for the purposes of such explorers greater accuracy is unnecessary; and the submission to a practical test of Prof. Geelmuyden's opinion, that better results can be obtained by deducing the moon's right ascension from the difference of azimuth of the moon and a star, will be a task not unworthy of the scientific expert accompanying the *Discovery*. E. PLUMSTEAD.

"First on the Antarctic Continent."

SOME rather venomous criticism of my book, "First on the Antarctic Continent," has appeared in one or two periodicals. Had my book been intended to be what it is not—a scientific report upon our work in the south—the venom would to some extent be justified. There are, however, other circumstances which prevented me from producing at the time a larger and more representative account of our work in the south. Preliminarily may I state that the observations have been submitted to the Council of the Royal Society, who have accepted them, and the Society is in due time going to publish a volume on the results? This speaks for itself of the efficiency of the staff I had chosen. The Natural History Museum of South Kensington has received the bulk of the collections and I understand that the report upon them is nearly finished, and the book, written by specialists of the Museum, will probably appear within a very short time. C. E. BORCHGREVINK.

(Commander, British Antarctic Expedition, 1898–1900).

Douglas Lodge, Bromley, Kent, July 5.

The Settlement of Solid Matter in Fresh and Salt Water.

IN a letter under the above heading in your issue of June 20, Mr. W. H. Wheeler discusses the effect of dissolved salt in promoting the subsidence of alluvial matter in water. He takes exception to the conclusion of Mr. Slidell that the mixture of sea water with river water exercises a preponderating influence on the formation of deltas. The question at issue is not one that can be settled simply by a consideration of the specific gravity and viscosity of the solutions employed, and Mr. Wheeler has made it the subject of experimental investigation. There can be little doubt that it is only in the case of very finely divided solid matter in suspension that the addition of salt solution causes increased precipitation, and so far his results can scarcely be called into question. They are confirmed by the investigations on the deposition of sediment by Carl Barus and Bodländer, to whose papers references are given below.

The precipitation of such "suspensions" or "pseudo-solutions" by the addition of an electrolyte is accompanied by the coagulation or flocculation of the solid matter. Schlœsing states that clay suspensions pass through a filter paper, but can easily be filtered if coagulated by a salt solution. If, however, the clay is washed free from salt, it can enter into suspension again in pure water and be precipitated afresh. These two operations can be performed in succession several times without apparent modification in the results. Picton and Linder found that the coagulum produced by the precipitation of a pseudo-solution of arsenic sulphide contained traces of the metallic ion, which could not be removed by washing.

The mud or ooze examined by Mr. Wheeler seems to have consisted entirely of matter which had already undergone precipitation, but it does not appear from his letter that any precautions were taken to remove traces of the metallic salts, so that it remains doubtful whether the sample really formed a suspension in the pure water. More satisfactory experiments could perhaps be made by collecting samples of turbid water from a river in flood and then adding sea water or a solution of salt.

I had occasion some time ago to consult the somewhat extensive literature dealing with the suspension of solid matter in a fluid and the allied one of colloidal solutions, and the following list of papers, though doubtless far from complete, may be of use to some readers of NATURE:—

Skey, *Chem. News*, xvii. p. 160; Waldie, *Chem. News*, July 24, 1874; *Journ. As. Soc. Bengal*, 1873; Th. Scheerer, *Pogg. Ann.*, lxxxii. p. 419, 1851, einige Beobachtungen über das absetzen auf geschwemmter pulverförmiger Körper in Flussigkeiten; Hunt, *Proc. Bost. Soc. Nat. Hist.*, pp. 302–4, 1874; Slidell, *Report* of Messrs. Humphreys and Abbott on the physics and hydraulics of the Mississippi, App. A, p. 11, 1861; Ch. Schlœsing, *Compt. rend.*, lxx. p. 1345, 1870, sur la précipitation des limons par des solutions salines très-étendues; David Robertson, Glasgow Geol. Soc. *Trans.*, iv. pp. 257–9, 1874; W. Durham, *Chem. News*, xxx. p. 57, 1874; *Chem. News*, xxxvii. pp. 47–8, 1878; *Proc.* Roy. Phys. Soc. Edin., iv. pp. 46–50, 1874; W. H. Brewer, *Proc.* Nat. Acad. Sci., 1883; *Amer. Journ.* (3), xxix., p. 1, 1885; C. R. Stuntz, Cincinnati Soc. Nat. Hist., Feb. 1886; E. W. Hilgard, *Amer. Journ.* vi., 1873, xvii., 1879, Forschungen auf d. Geb. d. Agriculturphysik von E. Wollny, ii. pp. 57–9, 441–454, 1879, ueber die Flockung kleiner Theilchen; A. Mayer, Forschungen auf d. Geb. d. Agriculturphysik von E. Wollny, ii. pp. 251–273; Hallock, *Bull.* of the U.S. Geol. Survey, xlii. p. 137, 1887; Carl Barus, *Bull.* of the U.S. Geol.

Survey, xxxvi. 1886, subsidence of fine solid particles in liquids, *Am. Journ. Sci.* (3), xxxvii. p. 122; Carl Barus und E. A. Schneider, *Zeitschr. f. Physik. Chemie.*, viii. p. 285, 1891, über die Natur der colloidalen Losungen; G. Bodländer, *Jahrb. f. Min.*, ii. pp. 147–168, 1893; *Götting. Nachr.*, p. 267, 1893, versuche über Suspensionen: Stanley Jevons, *Quart. Journ. Sci.*, viii. p. 167, 1878; Picton and Linder, *Chem. Soc. Journ.* lxi. pp. 114–172, 1892; lxvii. pp. 63–74, 1895; lxxi. pp. 568–573, 1897, solution and pseudo-solution; H. Schulz, *Journ. f. prakt. Chemie.*, xxv. p. 431, 1882; Hardy and Whetham, *Journ. of Physiology*, xxiv. p. 1899, *Phil. Mag.* Nov. 1899; Hardy, *Proc.* Roy. Soc., lxvii. p. 95, p. 110, 1900; W. J. A. Bliss, *Phys. Review*, No. 11, 1895 (2).

H. S. ALLEN.

Blythswood Laboratory, Renfrew, N.B., June 27.

The Teaching of Mathematics.

BEING myself a teacher of mathematics, I have followed with much interest the vigorous crusade against the neglect of suitable scientific and mathematical training conducted by Prof. Perry and others, and am in substantial agreement with Prof. Minchin's remarks in his review in your columns of the series of papers by Prof. Perry on "England's Neglect of Science."

One thing has struck me in connection with school "mathematical" teaching as being a very illogical course of procedure on the part of the dominant "classical cleric" instructors of youth alluded to—namely, the teaching of *arithmetic*. A boy, whether classically or otherwise educated, is considered a dunce if he is not merely not an expert with the multiplication table, but even if he is unacquainted with such things as recurring decimals, square and cube roots, &c., whereas no attempt is generally made to give an insight into *theory*, the results, *i.e.* the *rules*, are what he is expected to know.

So dissociated to the ordinary mind is the science of arithmetic from mathematics that I can remember a fellow collegian actually remarking, "Mathematicians are bad at arithmetic"! It seems to me, on the other hand, that Euclid is much more out of the line of what we mean by mathematics. In teaching Euclid as a mathematical "subject," and, as some claim, as an introduction to geometry, we are actually raising barriers to the progress of a learner in grasping the meaning and uses of geometry. We insist on the propositions being learned *in all their cases*, insisting on the absolute distinctness of propositions which are merely particular cases of the same proposition, thus tacitly suggesting the existence of some such commandment as "Thou shalt not recognise the Principle of Continuity"—we ignore the infinite and we teach to try and wriggle away from the notion of a "limit." In fact, nearly all that really constitutes mathematics is carefully avoided in teaching of Euclid, whereas I have found, when I have dared once or twice to depart from examination ideals, how true the following remarks of Mr. C. Taylor in his prolegomena to "The Introduction to the Ancient and Modern Geometry of Conics" are. When referring to the work of Boscovich, he says:—"It is remarkable that Boscovich enters upon these abstruse speculations in an elementary treatise for beginners.... The preface to the volume contains an earnest plea for the introduction of the modern ideas into the schools. He had taught the appendix *viva voce* to his own tyros with the happiest results .... demonstrations are put before him (the tyro) in an unsuggestive form which gives no play to his inventive faculty; and thus it comes to pass that of the many students so few turn out genuine geometers ..."

I must not encroach further on your valuable space, although many points come to one's mind, such as the exclusion from so-called "higher algebra" papers of the theory of determinants, arithmetic without logarithms, applied mathematics without the calculus, &c., but, in hopes that the attack may be vigorously pushed home, subscribe myself yours sincerely,

Henry Smith School, Hartlepool. F. L. WARD.

Curious Rain-drops.

ON Thursday last, July 11, about 6 p.m., the day having been sultry, the sky became dark and overcast, threatening rain. Only a few scattered drops fell, however (the threatened rain passing off), but these sparse rain-drops drew my attention by their curious appearance on the sill of the window near which I sat.

Each rain-drop had broken up into a number of smaller drops, which arranged themselves in a circular form around a central one, in the manner here shown

Perhaps some one of your readers would kindly explain the cause of this, and if it was due to some electrical condition of the atmosphere. M. S.

Bowdon, Cheshire, July 14.

THE MYCENÆAN QUESTION.[1]

THE occasion for the following remarks on that difficult and much disputed subject, the Mycenæan Question, is furnished by the appearance of the timely volume on the "Oldest Civilization of Greece," by Mr. H. R. Hall, of the British Museum, and as public interest in the whole question has been considerably quickened by the important discoveries of Mr. A. J. Evans in Crete, this book, in which certain of the principal results of the Cretan excavations are discussed, will be heartily welcomed by the broad-minded school of classical archæologists in general, and by the student of ancient Oriental civilisations in particular.

It is now some twenty-five years since the spade of Schliemann brought to light the remains of the oldest civilisation of Greece; and as it was soon recognised that these remains belonged to the period of the Bronze Age, it was clear that they must be older than the classical period of Greek culture. The excavations which were made subsequently in several parts of the Greek world by the various investigators who were emulating

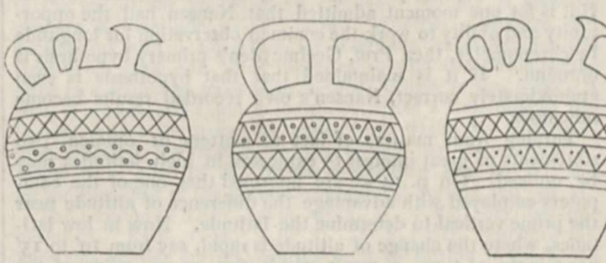


FIG. 1.—Representation of Mycenæan vases; from a fresco in the tomb of King Rameses III. at Thebes, B.C. 1200.

Schliemann's example proved that this Bronze Age culture was not confined to any particular part of Greece, but extended over the whole Hellenic area. Such discoveries compelled classical scholars to abandon many preconceived notions, and they found it necessary to revise entirely their ideas about the origins of Greek civilisation; it is not to be wondered at that many excellent scholars of the "old school" still find it difficult to make their views fall into line with the new order of things in classical archæology. This is most evident when the dating of Mycenæan antiquities has to be considered, for if the Mycenæan culture, being of the Bronze Age, is necessarily pre-classical, its *floreat* must be assigned to the second millennium before Christ. An important confirmation of this view seems to be supplied by the evidence derived from the excavations which have been made in Egypt in recent years, where a large number of objects, pottery, &c., of Mycenæan origin have been found; and in many cases such objects have been discovered side by side with native Egyptian objects which must belong to the period which lies between B.C. 1500 and B.C. 1000. The discoveries of Mr. A. J. Evans, however, all seem to point to a still earlier date for the first development of

[1] "The Oldest Civilization of Greece: Studies of the Mycenæan Age." By H. R. Hall, M.A., Assistant in the Department of Egyptian and Assyrian Antiquities, British Museum. Pp. xxxiv + 346; with 76 illustrations. (London: D. Nutt, 1901.) Price 15*s*. net.

Mycenæan culture out of primitive barbarism, and a useful indication of its antiquity is supplied by the discovery, recently announced, of a statue of King Khian of Egypt, in Crete. Now the existence of King Khian was made known to us by numerous scarabs and certain monuments which were found at Tanis in the Delta and Baghdad in Turkey-in-Asia, and it is generally thought that he was a Hyksos king, who reigned about B.C. 1800. Prof. Petrie, judging from the style of the work on Khian's scarabs alone, has assigned this king to a far earlier date, *i.e.* to the period between the sixth and eleventh dynasties, about B.C. 3000; there is, however, no sufficient foundation for this view, and, so far as we know, it is not accepted by the majority of Egyptologists. The discovery of Khian's statue by Mr. A. J. Evans in the Mycenæan Palace of Knossos takes its place naturally in the long series of facts derived from archæological evidence collected in

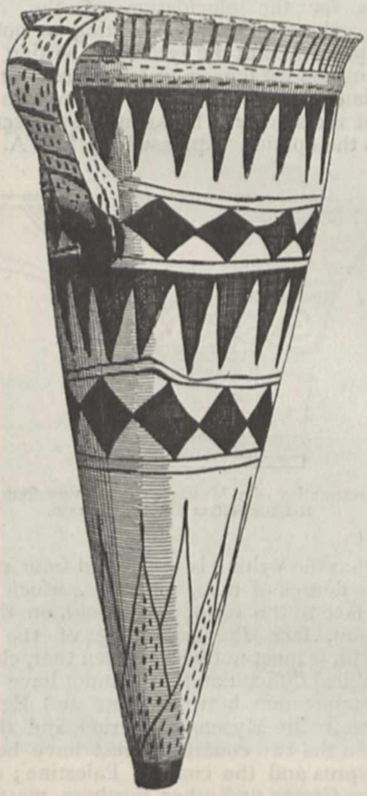


FIG. 2.—Egyptian vase imitating Mycenæan form, about B.C. 1350. (British Museum.)

Egypt and Crete, which point with one accord to a date before B.C. 1500 for the beginnings of the Mycenæan period properly so-called.

The first systematic arrangement of the evidence which was derived from the discoveries of Schliemann was embodied in the work "Mykenische Vasen," by Messrs. Furtwängler and Löschcke, to whom the classification of Mycenæan pottery is due, and an anticipation of the conclusions to which Mr. A. J. Evans' discoveries appear to tend in respect of the prominent part which the Cretans took in the early Greek civilisation was essayed by Dr. Milchhoefer, whose "Anfänge der Kunst in Griechenland" appeared about the same time. The position which Mycenæan archæology had reached about 1890 was well summed up in Dr. Schuchhardt's epitome of Schliemann's works, and in this book we already see the beginnings of an attempt to obtain accurate dates for the periods of the Mycenæan culture by means of conclusions drawn from results supplied by Egyptian excavations. Many of the available data employed by Dr. Schuchhardt and his successors were supplied by the excavations of Prof. Petrie at Kahun and Gurob, and above all at Tell el-Amarna, from which site conclusive evidence of the contemporaneity of Mycenæan culture with the heretic king Amen-hetep IV. and other monarchs of his dynasty can, *pace* Mr. Cecil Torr, be deduced.

But about this time attention began to be drawn to the remains of a pre-Mycenæan period of culture in Greece, and the discoveries of Prof. Dörpfeld at Troy resulted in a definite arrangement of the prehistoric civilisation of Greece in two well-defined periods, viz. the primitive or pre-Mycenæan, and the fully developed or Mycenæan Ages. The arrangement made by Dr. Dörpfeld became, in its turn, the base of a general sketch of Mycenæan archæology in the Mycenæan Age which was published in 1897 by Prof. Tsountas and Mr. Manatt, a work which, though based on Prof. Tsountas's earlier essay, was thoroughly revised and brought up to date in the light of the most recent research. This book, however, has one cardinal defect, and the evil effects of this defect are far-reaching: Prof. Tsountas, having arrived at certain conclusions, which from the nature of the case must be of a hypothetical character, states them as so many concrete facts instead of giving the reader to understand clearly that they are only his own opinions. Since

FIG. 3.—Bügelkanne of Mycenæan type made in Egypt, B.C. 1350. (British Museum.)

the publication of this book, however, Mycenæan archæology has entered upon a new phase, owing to the discoveries made by the British School at Athens on the site called Phylákopi, in Melos, and by Mr. A. J. Evans at Kephala, the site of the ancient Knossos in Crete, which have produced a mass of new and highly suggestive material for the archæologist to work upon; the results obtained from these excavations tend to indicate a comparatively high antiquity, *i.e.* about B.C. 1500, for the period when Mycenæan culture had attained its highest development. A different conclusion, however, seems to have been indicated as the result of the excavations which were carried out at Curium and Enkomi by Dr. A. S. Murray, of the British Museum, and his assistants, Mr. H. B. Walters and Mr. T. L. Myres, for the general evidence derived from the objects which they found in the course of their work shows that Cyprus continued to be included within the circle of Mycenæan culture as late as the ninth and eighth centuries before Christ. This date agrees with that assigned by Mr. A. J. Evans to the late Mycenæan treasure from Aegina which is now in the British Museum.

It has been necessary to make the above somewhat lengthy chronological statement on the Mycenæan question in order that the reader may be able to understand the exact position which Mr. H. R. Hall takes up on this disputed ground of research. He divides his work into eight chapters, which discuss the new chapter of Greek history generally, and the relation between the archæologist and historian in the elucidation of Mycenæan antiquities; the generally accepted Mycenæan hypothesis as modified by the latest discoveries; the questions of date and race; Mycenæ and the East and Mycenæ and Egypt; Mycenæ's place in history, including a discussion on the period of the introduction of the metals into Europe; and the decadence and renascence of Greek culture after the close of the Mycenæan period. The book contains in addition four appendices, seventy-six illustrations, full indices, notes, &c. Many of the facts which are given in Mr. Hall's book are familiar to us from other sources, but he has brought forward from the domain of Egyptology a considerable number which will probably be new to the majority of his readers; indeed, if we remember rightly, the Mycenæan Question has never before been handled by one whose training has made him familiar with both Greek and Egyptian archæology. His chapter, then, on the connection between Mycenæ and Egypt will be read with much interest, especially his remarks of the identifications of the northern Mycenæan tribes who attacked Egypt between B.C. 1400 and B.C. 1150. He has identified the tribe of the Uashasha with the Axians of Crete, and he has shown the probability that others of the tribes which are mentioned in Egyptian history at this period were of Cretan origin, including the Pulesatha, or Philistines.

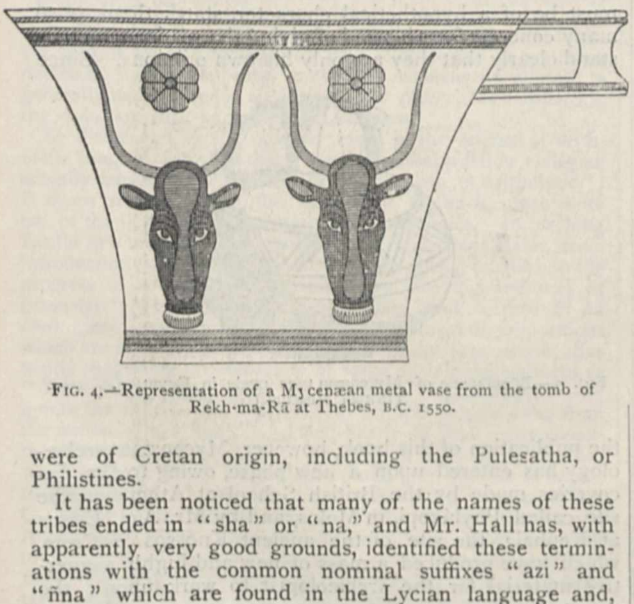


FIG. 4.—Representation of a Mycenæan metal vase from the tomb of Rekh-ma-Rā at Thebes, B.C. 1550.

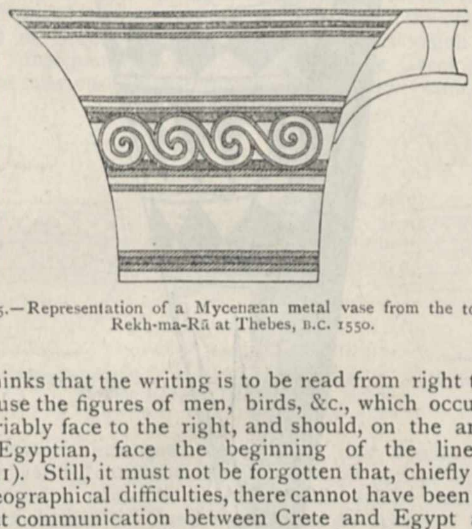


FIG. 5.—Representation of a Mycenæan metal vase from the tomb of Rekh-ma-Rā at Thebes, B.C. 1550.

It has been noticed that many of the names of these tribes ended in "sha" or "na," and Mr. Hall has, with apparently very good grounds, identified these terminations with the common nominal suffixes "azi" and "ñna" which are found in the Lycian language and, seemingly, also in other cognate speeches of Asia Minor. Mr. Hall seems also to have devoted his energies to the solution of the difficult problem of dating the early antiquities of Greece, and, so far as we understand him, he takes in this respect a position midway between those who hold that the latest date possible in Mycenæan archæology is B.C. 1100 and those who hold, with Dr. A. S. Murray, that this date is more likely to be the *earliest* which can be assigned to Mycenæan antiquities, *i.e.* he believes that in Greece proper and in Crete the Mycenæan culture began at a very early period—which, however, he does not define exactly—and had already reached its highest pitch of development about B.C. 1500, when its chief seat was in Crete, and when it was extending its influence to Egypt and Asia Minor. He considers that the discrepancy between the two extreme views can be reconciled on the theory that in Greece proper the Mycenæan age came to an end about B.C. 1000, but continued to exist in Asia Minor until about B.C. 800, and in Cyprus until a century later.

This view is perhaps confirmed by the fact that the Dorians, who, *ex hypothesi*, overthrew the Mycenæan culture in Greece, did not reach Asia until about B.C. 800, and never gained any foothold whatever in Cyprus. Another important point made by Mr. Hall is that, contrary to the usually accepted view, iron was already known to the Egyptians about B.C. 3500, when, as he says (see p. 198), "it appears named and depicted on the monuments in a manner which admits of no possibility of doubt as to its nature." He supports his statements by quotations from a learned article by the Swedish Egyptologist, Prof. Piehl, which appeared in *Ymer* (1888, p. 94 ff.), from which it may be safely concluded that the Egyptians were acquainted with the use of iron some 2500 years before it came into general use in Europe. We notice that the passages which Mr. Hall quotes from Egyptian texts are translated by him especially for the purposes of this book, and he weighs with discretion the evidence which many would derive from the cuneiform and from the so-called "Hittite" inscriptions for the elucidation of the origins of Mycenæan culture. It is interesting to note that he believes it possible that the system of writing which was in use among the Cretans may have been derived from the Egyptian hieratic, and he points out some probable instances of the similarity between the two scripts; but, contrary to the opinion expressed by Mr. A. J. Evans, he thinks that the writing is to be read from right to left, because the figures of men, birds, &c., which occur in it invariably face to the right, and should, on the analogy of Egyptian, face the beginning of the line (see p. 141). Still, it must not be forgotten that, chiefly owing to geographical difficulties, there cannot have been much direct communication between Crete and Egypt across the open sea in the Mycenæan period, and the connection between the two countries must have been carried out *viâ* Cyprus and the coast of Palestine; and it is a fact that the Cretan and other northern marauders who attacked Egypt in the reigns of Menephthah and Rameses III. made their way to Egypt by this route.

There are many other points of interest in the book to which we should like to draw attention, but our space is exhausted. The Mycenæan question is a difficult one, and one which, in our opinion, will not be settled for some years to come; the evidence which will bring about this result is accumulating, but there is not enough of it available yet. The most serious phase of the question as it now presents itself is the discrepancy between the dates assigned by experts for the beginning and end of the period of Mycenæan culture proper. Mr. Hall does not claim, if we understand him aright, to have settled this difficulty, but there is no doubt that he has collected a number of facts which will one day form valuable elements in the solution of the problem, and he has set forth the Egyptian aspect of the Mycenæan question in a clearer form than any of his predecessors. His volume contains an excellent summary of the work already done, and will give the reader a capital idea of the position of the workers in the Mycenæan field; it will also enable him to take an intelligent interest in the labours of future workers and to appreciate the developments of a most fascinating line of research.

THE SOUTH EASTERN AGRICULTURAL COLLEGE AT WYE.

THE new block of buildings just completed at the South Eastern Agricultural College at Wye, Kent, is to be opened by the Right Hon. R. W. Hanbury, president of the Board of Agriculture, as we go to press this week. As the College has been constituted a school of the University of London in Agriculture, it may be of interest to give a short account of this institution—one of the most advanced examples of the development of agricultural education under the administration of "the whisky money" by county councils.

The College began work in 1895, and is managed under a scheme of the Charity Commissioners by a governing body appointed by the county councils of Kent and Surrey, together with representatives of the Universities of Oxford, Cambridge and London, the Royal and the Bath and West Agricultural Societies. The buildings, which are situated at Wye, a little village on the South-Eastern line between Ashford and Canterbury, consist of a nucleus built about 1470, an ancient collegiate foundation due to the Cardinal Archbishop Kempe, with successive additions made in 1894 and the current year.

The old buildings form a small quadrangle with brick cloisters and include a fine and lofty hall, the refectory of the original College now restored to its original purpose, and a beautiful oak panelled room, which is used as the library. The later additions form a second quadrangle, with lecture rooms, &c., on the ground floor and students' living rooms on the first floor; the chemical laboratories occupy a further wing. There are two lecture rooms, one a theatre with raised seats accommodating 150 people; the drawing office provides working space for twenty-four students in such subjects as surveying, building construction and farm engineering. On the biological side there is a laboratory with working space for thirty students, furnished with Swift's histological microscopes; two smaller laboratories for the professors of botany and economic entomology, and a museum, of which the chief features at present are a collection illustrating the insect pests attacking fruit and hops, specimens illustrating the forestry course, pathological specimens in connection with farm animals, typical cereals, soils, &c.

FIG. 1.—Chemical Laboratory of the South Eastern Agricultural College.

The chemical laboratories consist of a general students' laboratory, measuring about 45 by 30 feet, lighted on both of the longer sides of the room; it is fitted with two double benches running longitudinally, reagent bottles being carried on glass shelves down the middle of the tables. The two benches give working room for thirty students, and other benches in the window recesses are provided for special work; water and gas are laid on to all the tables, and there are two fume chambers within and one outside the laboratory. Separated from the main laboratory by a glazed partition is the balance room and the larger of the analytical laboratories; adjoining this comes a smaller room reserved for gas analysis, titrations, &c., that require an acid or ammonia free atmosphere, and next to this comes a room for the furnaces and for ether extractions and other operations involving the use of inflammable liquids; in one corner of this room a drying chamber has been built.

The College farms about 250 acres of land adjoining;

it is preeminently a sheep farm, the soil being a light loam resting on chalk; the main features of the farming consist of a breeding flock of the Romney Marsh breed, a small dairy herd of shorthorns and a large stock of poultry. There is a small hop garden, planted in 1895, to test the various systems in vogue of training hops and for other experimental work. The two fruit plantations are both young; one is used for teaching purposes, the other is mainly laid out for experiments. On the farm are situated the dairy, forge, carpenter's shop, apiary, &c.

The staff of the College consists of seven resident professors and lecturers, together with instructors in practical subjects, demonstrators, &c., the necessity for this extensive staff being that the College is also a centre for much extra-mural work in the counties of Kent and Surrey, such as courses of lectures, analyses of soils and manures, reports on crop diseases, field experiments and similar investigations. As regards the latter, experiments on the manuring and cultivation of hops have been carried on consecutively for six years, and results of considerable practical importance are beginning to emerge. Other work extending over several years has been done on the quality of barley as affected by manuring, the cost of growing sugar beet and its food value, and a systematic examination of the soils of Kent and Surrey has also been in progress for some time.

The normal course of instruction extends over two or three years; the College grants a diploma of its own, and with the constitution of a board of agricultural studies in the University of London it is expected that regulations for the degree course will soon be forthcoming.

Short sessions for special purposes are held from time to time; in August, for example, there will be a normal course of instruction in "nature knowledge" for elementary school masters, the outcome of two years' practical experience with the children of the Wye Elementary School.

At present there are some fifty to sixty students in residence, mostly taking the ordinary course, but a few are doing special work in the laboratories; it is hoped that such students will increase with the facilities the institution now affords for research which requires work both in the field and the laboratory. Both at home and in our Colonies and dependencies agriculture wants trained investigators and teachers if we are to keep our place, and the South Eastern Agricultural College is making a serious attempt to supply within the London University the kind of institution that has done such good service for American and German farming.

FIG. 2.—Biological Laboratory of the South Eastern Agricultural College.

THE BRITISH ASSOCIATION.

GLASGOW MEETING, SEPTEMBER 11–18, 1901.

IN the first article which appeared in NATURE on May 23, particulars were given regarding the local arrangements as to reception room, rooms for sectional meetings, and the halls in which the presidential and other evening scientific lectures were to be delivered. The Friday evening lecture is to be given by Prof. W. Ramsay, on inert constituents of the atmosphere, and the Monday evening lecture is to be given by Mr. Francis Darwin, on the movements of plants.

Two important fixtures by the Excursions' and Entertainments' Committee have been made since the last notice, namely, the chartering of one of the Clyde steamers for a whole day's sail on Saturday, September 14, and the acceptance of an offer by Lord Blythswood, president of the Philosophical Society of Glasgow, to give a garden party in the Botanical Gardens on the afternoon of Monday, September 16.

Promises of numerous papers by eminent authors are already received by the recorders of the different sections. Indeed, one or two of the sections generally get more papers than is necessary to fill the time of the sittings. For example, Section A not only meets on Saturday, but is also on two days actually divided into two subsections. On Friday it is to be divided into (1) physics, and (2) astronomy. On Monday it is to be divided into (1) mathematics, and (2) meteorology.

At this early stage a full and definite programme of the different sections cannot be given. But the following brief provisional programme may be taken as conclusively indicating that the meeting promises to be a most successful one from an educational and scientific point of view.

Section A (Mathematical and Physical Science).—A large number of papers are already promised for this section. The following may be mentioned:—Five papers, dealing with elasticity, viscosity, magnetic fields, and stress and magnetisation of nickel and cobalt, are promised from the physical laboratory of the University of Glasgow by Prof. Gray, his assistants and the research students in his department. Dr. Larmor will give a paper on radiation, Dr. Hicks a paper on the Michelson-Morley effect and Dr. Glazebrook a paper on optical glass. In the meteorological department, Mr. W. N. Shaw and one of his assistants will give two papers treating of the seasonal variations of air temperature.

Section B (Chemistry).—The following papers have already been promised:—On the transitional forms between crystalloids and colloids, by Messrs. J. H. Gladstone and W. Hibbert; the oxidation of tin, including the action of light, by Messrs. J. H. Gladstone and G. Gladstone. Papers on the following subjects will also be submitted:—On the deposition of ocean salts, on electrochemical processes and on the manufacture of cyanides.

Section C (Geology).—Papers to this section are promised by Messrs. William Gunn, B. N. Peach, R. H. Traquair, Robert Kidston and H. B. Woodward. Several others have intimated a wish to read short papers on subjects in which they are specially interested.

Section G (Engineering).—A paper on the mechanical exhibits at the Glasgow International Exhibition is being arranged for. After a report on road traction is submitted by a committee appointed for the purpose, papers bearing on this subject will be read by Messrs. A. R. Sennett, A. H. Gibbings and Sir J. H. A. Macdonald. The following papers will also be given to this section:—Protection of buildings from lightning, by Mr. K. Hedges; dielectric hysteresis, by Mr. W. M. Mordey; Panama Canal, by Mr. M. Buon Varilla; tunnelling in quicksand, by Mr. M. A. Gobert; chain driving, by Mr. C. Garran; engraving machinery, by Mr. M. Barr; and aluminium as a fuel, by Sir W. C. Roberts-Austen. Mr. Barr will probably show his apparatus working in the municipal buildings during the evening of Thursday, September 12.

Section K (Botany).—There will be a discussion in this section on the teaching of botany, opened by Mr. Wager from the standpoint of botany teaching in schools, and by Prof. Bower from the point of view of University teaching. Profs. Ward, Scott-Elliott, Miall and others will take part in the discussion. It is intended to ask members of Section L, the educational section, to take a share in the discussion. Prof. Reynolds Green will probably give a semi-popular lecture on a botanical subject of general interest.

Section L (Educational Science).—After the president, Sir John E. Gorst, delivers his presidential address, it is expected there will be a discussion on Scottish educational systems. This discussion will probably be introduced by two papers, one by Mr. John Adams, on mechanism of education in Scotland, and the second by Dr. J. G. Kerr, on the training of the practical person.

On account of the International Exhibition there is a great influx of visitors to Glasgow, and members of the British Association who intend to be present at the Glasgow meeting are strongly recommended to make early arrangements for their rooms. The local committee have prepared a list of hotels and a preliminary list of lodgings and apartments. This list is ready to be sent to any inquirer. MAGNUS MACLEAN.

NOTES.

PROF. A. W. RÜCKER, professor of physics at the Royal College of Science and secretary of the Royal Society, has been appointed principal of the University of London. We are informed that, in consequence of his appointment to this post, Prof. Rücker will resign the secretaryship of the Royal Society at the next anniversary meeting.

PROF. VIRCHOW'S eightieth birthday will be celebrated in Berlin on Saturday, October 12, when he will personally receive delegates with congratulatory addresses from various scientific bodies, foreign as well as German. Prof. Waldeyer is the president of the executive committee.

THE Council of the Royal Society has awarded the Mackinnon studentship to Mr. J. J. R. Macleod, demonstrator of physiology in the London Hospital Medical College, for the purpose of enabling him to carry out researches in pathological chemistry. The studentship is founded under a bequest to the Royal Society by the late Sir William Mackinnon, Director-General of the Medical Department of the Army, for the foundation and endowment of prizes or scholarships for the special purpose of furthering natural and physical science, and of furthering original research and investigation in pathology. The studentship, for which fourteen applications were received, is of the annual value of 150*l*.

THE death is announced of Mr. J. Hamblin Smith, the well-known mathematical coach at Cambridge and author of numerous successful text-books of elementary mathematics. Mr. Smith was seventy-four years of age.

PROF. PASQUALE VILLARI has been elected president of the Reale Accademia dei Lincei, of Rome, in succession to the late Prof. Messedaglia.

AN excursion to the Auvergne district has been arranged by the Geologists' Association. The party will leave London on the evening of Thursday, August 15, and will journey to Clermont, which will be the centre of the excursions. The visit will last a fortnight, and an excellent programme has been arranged under the direction of Prof. Marcellin Boule, Prof. P. Glangeaud and M. J. Giraud.

A TELEGRAM from Ponta Delgada to the *Times* states that the International Meteorological Observatory in the Azores was inaugurated on July 10 by the King of Portugal. The Portuguese Prime Minister, the Minister of Marine, the civil governor of the islands and the different authorities, together with a number of Portuguese officers and the officers of the British cruisers *Australia* and *Severn*, were present at the ceremony.

WE are sorry to learn that the biological station which had been kept on Lake Baikal for a year by the East Siberian Geographical Society, at Goloustnaya, on the west coast, has been closed. A rich collection of fishes, especially of Cottus species, and a great variety of Gammarus species have, however, been secured, and the latter are in the hands of Prof. Sars, of Christiania.

ON Tuesday, July 16, a deputation of members of Parliament and engineering and shipbuilding societies waited upon Lord

Selborne (First Lord of the Admiralty) and Mr. Arnold-Forster (Parliamentary Secretary to the Admiralty), at Whitehall, to urge the necessity of improving the conditions of the service of engineers in the Royal Navy. Lord Selborne promised that the suggestions of the deputation would be carefully considered.

THE French Société d'Encouragement pour l'Industrie nationale announces the following awards of prizes:—Grand gold medal to the Chamber of Commerce of Lyons for the organisation of the commercial mission to China; 2000 francs to M. Horsin-Déon for his work on beetroot sugar; 500 francs to M. R. Fosse for his works on β-dinaphthol, and the same amount to M. Marcel Guichard for his works on molybdenum; 1000 francs to M. Triboudeau for his study of the Pas-de-Calais, and 1000 each to MM. Faure and Thénard for memoirs on the utilisation of waters in agriculture.

A REUTER telegram from St. Petersburg, dated July 11, says:—The St. Petersburg Academy of Sciences to-day received a telegram from the leader of the expedition which is shortly to bring to St. Petersburg the mammoth found in Siberia. The telegram, which is despatched from Yakutsk, reports that the expedition arrived at that place on June 14. It is proceeding by steamer up the river, and will then journey overland to Kolymsk, which is 3000 versts off, and where it expects to arrive in two-and-a-half months. The mammoth found is unique of its kind. Its hair, skin and flesh are entirely preserved, and there are remains of undigested food in its stomach.

AN interesting relic is reported by the *Times* to have been placed in the building of the Academy of Sciences at Tsarskoe-Selo. It is a large geographical globe, 11 ft. in diameter and made of copper. This globe was commenced in the year 1654 and completed ten years later during the reign of Duke Frederick of Holstein. The outside represents the earth, and the interior the celestial spheres of the world. There is a door giving access to the interior of this globe, and in the centre is a round table with space for twelve people to sit. By means of mechanism this great globe can be made to revolve upon its axis. The globe weighs about 3½ tons and was presented to the Academy of Sciences in 1725, but until now it has stood in the Zoological Museum at Tsarskoe-Selo.

THE executive committee of the National Physical Laboratory have made the following appointments:—Superintendent of the engineering department, Dr. T. E. Stanton; assistants in the physics department, Dr. J. A. Harker, Mr. A. Campbell and Dr. H. C. H. Carpenter; junior assistants, Mr. B. F. E. Keeling and Mr. F. E. Smith. It is expected that one or two more junior assistants will be appointed shortly. The alterations to Bushy House and the new buildings for the engineering laboratory are well advanced, and it is hoped to commence work early in October. Of the staff, Dr. Stanton, after serving an apprenticeship with an engineering firm in the Midlands, has had a distinguished career at Manchester and Liverpool, and is now professor of engineering at University College, Bristol. Dr. Harker and Mr. Campbell have both done work of real value in thermometry and electric measurements, while Dr. Carpenter, who will have charge of the chemical researches, after a successful course at Oxford has gained further experience at Leipzig, under Ostwald, and more recently at Owens College. Mr. Keeling obtained a double first in natural science and mechanical science, respectively, at Cambridge, while Mr. Smith was the most distinguished student of his year at South Kensington, and for two years has been one of Prof. Rücker's assistants.

THE programme has been issued of the meeting of the Iron and Steel Institute, to be held in Glasgow on September 3-6 in connection with Section V. of the International Engineering Congress. The remaining eight sections of the Congress will be under the charge of other institutions. The meetings will be held in the University buildings, which are in immediate proximity to the International Exhibition. The president will deliver a short address, and the following are among the subjects of papers:—The nomenclature of metallography; the presence of calcium in high-grade ferro-silicon; the spectra of flames at different periods during the basic Bessemer blow; iron and copper alloys; the correct treatment of steel; the profitable utilisation of power from blast-furnace gas; Brinell's method of determining hardness and other properties of iron and steel; internal strains in iron and their bearing upon fracture; and a mechanical gas producer.

A SATISFACTORY trial of a new airship, devised by M. Santos Dumont, took place at Paris early on Saturday morning. It may be remembered that M. Henry Deutsch has offered a prize of 100,000 francs (4000*l*.) to the inventor of a flying machine which would travel from the heights of Saint Cloud to and round the Eiffel Tower and back again to the starting point in half an hour. M. Santos Dumont did the journey in forty-one minutes on Saturday, that is to say he made a voyage of nearly ten miles at a speed of about fifteen miles an hour, though the return journey was against the wind. He did not win the prize, but his attempt is very encouraging, for he has shown that an airship can be made to travel at a high speed in any direction. His airship consists of a cigar-shaped balloon, six metres in diameter, thirty-four metres long, and having a volume of 550 cubic metres. The gas pressure is kept constant automatically by means of a ventilator driven by the engine and communicating with the balloon by means of a long canvas pipe. Suspended from the balloon is a light framework containing a petroleum motor of 16 horse-power, driving shaft, propeller and rudder, and near one end is a small wickerwork car in which the aëronaut stands and controls the steering-wheel and the apparatus for regulating the motor. M. Deutsch is constructing a similar balloon to that of M. Santos Dumont, but having a volume of 2000 cubic metres and a motor of sixty horse-power. M. Santos Dumont proposes to make another trial trip next Saturday. When going at full speed the propeller makes 200 revolutions per minute.

THE annual report of the Russian Geographical Society for 1900, which has only recently reached us, is, as usual, full of interest. It is especially interesting to notice the growing activity of the young branches of the Society at Vladivostok, Kiakhta, Tomsk and Orenburg—their work being not limited to pure geography, but being mainly directed to the exploration of the geology, botany, zoology and prehistoric anthropology of the respective regions. A new local museum has consequently been opened at Troitskosavsk, near Kiakhta, in addition to those of Minusinsk and Yeniseisk. The chief medal of the Geographical Society, the Constantine medal, was awarded this year to V. Obrucheff, the explorer of the Nan-shan and Mongolia, who has also explored very large portions of Transbaikalia and the Pacific littoral, and whose preliminary reports are always of the deepest interest for both the geologist and the orographer. The Count Lütke medal was awarded to M. E. Zhdanko for his extensive geodetical and hydrographical works in the far North, the Semenoff medal to J. A. Kersnovsky for work in meteorology, and the Prjevalski medal to the Tomsk professor, V. V. Sapozhnikoff, whose explorations of the Altai highlands revealed hundreds of unknown glaciers, as well as widely-spread traces of glaciation, and threw much new light on the geography of the whole region. These researches are now embodied in a work, "The Katuñ and its Sources" (with maps and a summary in French).

THE arrangements made for the meetings of the fifth International Congress of Zoology, to be held in Berlin on August

12-16, were described at the beginning of this year (January 3, p. 236), when the invitation circular was distributed. We are now informed that 114 papers have been proposed by zoologists of various nationalities, so that the Congress promises to be of real scientific importance. The magnificent rooms of the German House of Parliament have been put at the disposal of the Congress, and, with the exception of a few lectures to be delivered in the Chemical Institute of the University, all the meetings will be held in the Reichstag building. The sections of the Congress will be: I. General zoology; II. Vertebrata (biology, classification, distribution); III. Vertebrata (anatomy, histology, embryology); IV. Evertebrata except Arthropods; V. Arthropoda; VI. Economic zoology (fisheries, &c.); VII. Nomenclature. Zoologists who wish to read papers should send abstracts not exceeding fifteen lines of print to the Præsidium of the Congress not later than August 1. The complete papers must be sent in not later than October 1. The Congress will be opened at the Reichstagsbäude on Sunday, August 11, at 8 p.m. The subjects of lectures to be delivered in the course of the meeting are: the malarial problem from a zoological point of view, by Prof. G. B. Grassi; vitalism and mechanism, by Prof. Bütschli; theories of fertilisation, by Prof. Yves Délage; the psychological attributes of ants, by Prof. Forel; mimicry and natural selection, by Prof. E. B. Poulton; fossil remains of man, by Prof. Branco. Letters and applications for tickets should be addressed to the Präsidium des V. Internationalen Zoologen-Congresses, Berlin N. 4, Invalidenstrasse 43.

THE new pathological institute connected with the London Hospital Medical College was opened by Sir Henry Roscoe on July 10. The building has cost 20,000*l.*, the fitting-up will need another 1000*l.* and the carrying on of the department will cost about 1200*l.* a year. The director of the institute is Dr. Bullock. Mr. Sydney Holland, who occupied the chair at the opening ceremony, remarked that in the new laboratories studies of the causes of disease could be made under conditions which made success possible and advance probable. There would be an orderly continuity of observation which had hitherto been impossible and which would be carried on by men who were specially trained in that science and who loved it for its own sake and for its benefit to their fellow men. This was work by which the whole country would benefit, and yet they got no help from the Government. Scotch hospitals and Irish hospitals obtained grants from the Government, but English hospitals got none and were cramped in every direction for funds. Every German town, even small ones, had its pathological laboratories. The County Council had established one at Claybury, but the one they were opening was the first attempt in London to deal so completely with this most important branch of medical knowledge. In opening the building, Sir Henry Roscoe remarked that it was unique among the large hospitals of London. It was the most completely equipped department for dealing with pathology in a manner worthy of the importance of that branch of medical science. For it was now generally acknowledged that pathology was an essential portion of those studies which made up the great science of medicine. It was necessary to have a centre where pathology was studied for its own sake and not for purposes of immediate practical application, still less for mere examinational purposes. The institute would form, he trusted, an introductory stage for entrance to that still higher and more advanced school of research which had its home in the Jenner Institute at Chelsea.

AN unfortunate controversy having arisen on the question of priority in the proof of the mosquito theory of the transference of malarial infection, Major Ronald Ross has published some correspondence on the subject which shows that the claims of some of the Italian observers cannot be substantiated ("Letters from Rome on the New Discoveries in Malaria," 1900). These eight letters were written by Dr. Edmonston Charles, a resident in Rome, to Major Ross, then in India, and date from November 4, 1898, to January 14, 1899; a letter from Dr. Daniels is included, and they are preceded by a critical introduction, and terminate with a postscript and bibliography by Ross. At this period the Italians, notably Grassi, Bignami and Bastianelli, were endeavouring to follow Ross's investigations on the development of the malarial parasites in the mosquito, and Dr. Charles acted as an intermediary, informing Ross of the progress made by the Italians, and similarly communicating to the latter Ross's observations and handing them his specimens. In the first letter, Charles asks for specimens for Marchiafava "of the mosquito in which human malaria develops." Grassi now denies that Ross ever detected this species. It is pointed out how closely the Italians followed and how well informed they were of the details of Ross's work, yet now Grassi states that his labours were independent of Ross. In the third letter, with regard to the cultivation of crescents in the "dappled winged mosquito" by Ross, Charles says, "he (Grassi) seemed perfectly satisfied that your description referred to the *Anopheles claviger*." Grassi now contends that he could not identify the malaria-bearing mosquito from Ross's description. Bignami, Grassi and Bastianelli have frequently stated that Ross's first successful experiments with human malaria were unsound, because the insects employed might have already bitten another animal before having been fed on man. Yet in Ross's publication it is clearly premised that the insects had been bred in bottles from the larvæ. These and other claims are dealt with in this publication.

THE art of producing decorative illuminating effects by the use of electric light is one that has in recent years called forth the application of great engineering skill. Particular attention has been given to this point at the Pan-American exhibition now being held at Buffalo, and those who are fortunate enough to visit this exhibition will be able to admire what is probably the most comprehensive and carefully studied system of illumination that has as yet been carried out. The less fortunate can obtain some idea of the great beauty of some of the effects from the very excellent photographs that have been recently appearing in the *Electrical Review* of New York. Those responsible for the illumination at Buffalo were to a certain extent favoured by circumstances in that they had a cheap supply of electricity available from the neighbouring power station at Niagara. It was thus possible, not only to get current cheaply, but also to control the lighting of the whole exhibition from one centre. Power is transmitted from Niagara, 20 miles distant, at a pressure of 11,000 volts, and after undergoing a transformation down to an intermediate voltage of 1800 is again transformed down to the voltage for supplying the lamps. An additional effect is produced by gradually bringing up the lamps from darkness to full candle power instead of switching on the full light instantaneously. To carry this out the whole of the current used for illuminating the buildings is passed through three large water rheostats, consisting of iron tanks 10 feet long by 3 feet wide and 3 feet deep, into which cast-iron plates, 8 feet long by ¾ inch thick, are slowly lowered; when the plate reaches the bottom of the tank it strikes a clip which short circuits the rheostat. The three rheostats are worked simultaneously, the plates being lowered and raised by a small electric motor; 45 seconds is taken to light up the lamps, which are put out in a somewhat longer period of about 75 seconds.

THE director of the Belgian Meteorological Service has recently published a very useful memoir on the direction of the wind at Brussels, compiled from fifty years' observations (1842–1891). The work forms the third part of the series, those referring to barometric pressure and air temperature having already appeared. The wind-directions are grouped in various ways; among the tables we find the relative frequency arranged under sixteen points of the compass, for each month and for each season, during the fifty separate years deduced from the hourly or two-hourly indications of a self-recording anemometer. The plates containing the average monthly and yearly wind-roses show the prevailing directions more clearly than anything else could do. It is seen at a glance that the winds between south-west and west are more frequent than all the other directions combined.

SIGNOR G. ODDO, writing in the *Atti dei Lincei* on the use of oxychloride of phosphorus as a solvent in cryoscopic observations, finds that the cryoscopic constant of this solvent is 69, and that, like water, but within narrower limits, it ionises dilute saline solutions.

THE stability of a given state of motion is a subject on which much has been written and more remains to be written. A paper on certain criteria of instability, by Signor T. Levi-Cività, appears in the *Annali di matematica pura ed applicata*. After discussing the general theory, the author proceeds to consider the problem of three bodies, and shows in particular that periodical solutions approximating to uniform circular motions are unstable, a conclusion contrary to what would naturally be inferred from considerations of celestial mechanics. The same author, writing in the *Atti dei Lincei*, deals with the stationary motions of a rigid body in the case of Kowalevsky.

DR. F. CAUBET, of the University of Bordeaux, has published a lengthy thesis on the liquefaction of gaseous mixtures, dealing in particular with the phenomena of the critical point and of retrograde condensation. In it the author describes an elaborate series of experimental determinations of the isothermal lines, the lines of Gibbs and Konowaloff, and the dew- and boiling-points of three series of mixtures each formed of two of the three gases, methyl chloride, carbonic anhydride and sulphurous anhydride. By tracing the isothermals for different degrees of concentration, both in the homogeneous and in the heterogeneous states, Dr. Caubet hopes to throw experimental light on the theories derived from considerations of the thermodynamic potential.

AN account of the earthquake of April 24, in the neighbourhood of Palombara Sabina, is given by Dr. Luigi Palazzo in the *Atti dei Lincei*, x. 9. The shock was registered at the Central Meteorological Office at about 15h. 20m. 25s. Italian time, and lasted about five to six seconds. The town of Palombara was visited without delay by Prof. Cancani, who found that the damage in the centre of the town was small; but in the village of Stazzano four or five houses were destroyed, others rendered uninhabitable, while considerable damage was done at Cretone, and the author thinks it probable that the epicentre was at a sulphur spring about a kilometre distant from Cretone, and that the origin of the shock was in the strata from which the spring arises, at a comparatively small depth.

THE current number of the *Journal of Hygiene* contains, amongst other articles, an appreciative obituary notice of Prof. Max von Pettenkofer, accompanied by an excellent portrait. Pettenkofer's name is now so invariably associated with investigations in the department of hygiene that his earlier work in pure chemistry runs a risk of being overlooked. As long ago as 1850 he presented to the Bavarian Academy a paper calling attention to some of the main facts which form the basis of the periodic law of the elements, but, like Newlands in this country, his work obtained no recognition at the time, and it was only in 1899 that the German Chemical Society conferred upon him the Liebig medal in tardy recognition of the merit of his researches of nearly half a century before. It was largely due to Pettenkofer that Germany has now chairs of hygiene with magnificent laboratories in all but one of her twenty universities, besides the famous Imperial Institute of Public Health in Berlin.

IN their Report for the past year the council of the Leicester Literary and Philosophical Society announce that they are cooperating with the National Trust for the Preservation of Objects of Historical Interest or Natural Beauty, and that steps are being taken to register and protect such objects in the county as may come within the scope of the Society's aims. The *Transactions* (issued with the Report) contain a lecture on heredity and the question of the inheritance of acquired characters, by Mr. C. J. Bond. Dr. J. G. Adams, professor of pathology at McGill University, sends us a reprint from the *New York Medical Journal* for June of an address on the same subject, in which he proposes to supersede Weismann's and allied theories on heredity and inheritance by one of his own.

AN article on the fauna of the Antarctic ("La Faune du pôle Sud"), contributed by M. E. G. Racovitza to the *Revue Scientifique* for July 6, appears opportunely. It is based on the results of the recent Belgian expedition and contains a general account of the fauna, with special details of the habits of the penguins, and a few notes on the flora. Since the whole of the Antarctic land (both continent and islands) appears to be invested with an unbroken sheet of inland ice (miscalled *inlandsis*), the author does not hold out hopes of the discovery of any important new types of life in the interior.

TO the January issue of the *Journal* of the Straits branch of the Royal Asiatic Society, Mr. H. N. Ridley, director of the Botanical Gardens at Singapore, communicates an extremely interesting paper on the flora of Mount Ophir. This flora includes three factors, a lowland Malay element, specially modified for existence at a higher elevation, the usual Oriental alpine (or Himalayan) element and—what is most interesting and unexpected—an Australian element. The same journal also contains a list of the butterflies of Mount Penrissen, Sarawak, and one of the reptiles of Borneo, both by Mr. R. Shelford, curator of the Sarawak Museum.

MR. L. M. LAMBE has published a revision of the genera and species of Canadian Palæozoic Corals ("Contributions to Canadian Palæontology," vol. iv. part ii. Geol. Survey Canada, 1901). His work is illustrated by thirteen plates.

IN a brief article on Indiana caves, Dr. O. C. Farrington discusses some peculiar forms of stalactites and stalagmites (*Publications* of the Field Columbian Museum, vol. i. No. 8, geol. series, 1901). The shape of certain vermiform stalactites is attributed, with Merrill, to the fact that drops of water may have been guided to other positions than those dictated by gravity by the directions assumed by spicules of calcite in crystallising. In one huge stalagmite both aragonite and calcite occur, and its minimum age is reckoned at 90,000 years. Lining the walls of a pool in one of the caves are calcite crystals in close association with stalagmites; here the crystals were formed in relatively still water, while the stalactites and stalagmites were formed when the water was moving. The author suggests that in a general way banded structures may be taken as indicating formation from waters in motion, while distinct crystals were formed from waters at rest; and he would apply these principles to the origin of mineral veins, agates, geodes, &c. He proposes the term stagmalites for formations produced by dropping water, and to include stalactites and stalagmites.

THE *Transactions* of the "Antonio Alzate" Society of Mexico contain a paper by MM. Marroquin y Rivera and P. C. Sánchez on the mountain chain of the Ajusco and its subterranean waters. In the northern part of the Vallée de Mexico, which is a closed basin, are two lakes named Chalco and

Xochimilco, containing excellent water, but their level is so low that it could only be made available for supplying the city of Mexico by enormous expenditure for pumping machinery. The basin of these lakes is bounded on the south and east by the mountains of the Ajusco and Sierra Nevada, and on the north by the Santa Catarina; a depression to the north-east connects it with the Vallée de Mexico, of which it forms a part. The lakes are fed by springs draining the underground waters from the volcanic formations of the Sierra Nevada and the Ajusco. The paper, which is an interesting study of the physical geography of the region, gives a preliminary account of attempts to tap these underground waters at a suitable level for gravitational supply to Mexico. The impermeable bed, believed to be andesitic, is being sought for below the basalt lavas and detritus by means of borings.

THE technique of basketry as manufactured by the Amerinds is the subject of a very valuable little paper by Dr. Otis T. Mason in the *American Anthropologist* (n.s., vol. iii. p. 109). Those who have desired to describe baskets and other objects plaited by primitive peoples have long wanted a system upon which to base their studies. This Dr. Mason has supplied, and all who study primitive industries once more have to thank their diligent and systematic American colleague.

THE *Kew Bulletin of Miscellaneous Information* (Appendix iii. 1901) contains the usual annual list of new garden plants recorded during last year in botanical and horticultural publications. The list includes, not only plants brought into cultivation for the first time during 1900, but the most noteworthy of those which have been re-introduced after being lost from cultivation.

A SECOND edition of "Marine Boiler Management and Construction," by Mr. C. E. Stromeyer, has been published by Messrs. Longmans, Green and Co. The book is described in the sub-title as "a treatise on boiler troubles and repairs, corrosion, fuels and heat, on the properties of iron and steel, on boiler mechanics, workshop practices and boiler design"; it was reviewed in these columns when the first edition appeared (vol. xlix. p. 410). About sixty pages of new matter have been added, including a chapter on steam, water and the boiling phenomena. No detailed accounts are given concerning water-tube boilers, because little exact information about the various types is available.

THE additions to the Zoological Society's Gardens during the past week include a Sooty Mangabey (*Cercocebus fuliginosus*) from West Africa, presented by Mr. E. Robinson; a Diana Monkey (*Cercopithecus diana*) from West Africa, presented by Mr. L. Gough; a Northern Mocking-bird (*Mimus polyglottis*) from North America, a Common Chameleon (*Chamaeleon vulgaris*) from North Africa, a Green Lizard (*Lacerta viridis*), European, presented by Miss Betty Cox; two Chaplain Crows (*Corvus capellanus*) from the Persian Gulf, presented by Mr. B. T. Ffinch; two Olive Weaver-birds (*Hyphantornis capensis*), two Alario Sparrows (*Passer alario*), eight Sulphury Seed-eaters (*Crithagra sulphurata*) from South Africa, presented by Mrs. R. Templeman; a Jackdaw (*Corvus monedula*), British, presented by Mr. L. Peevor; a Green Monkey (*Cercopithecus callitrichus*), a Jardine's Parrot (*Paeocephalus gulielmi*) from West Africa, a Pine Marten (*Mustela martes*), British; three King Snakes (*Coronella getula*), two Mexican Snakes (*Coluber melanoleucus*), a Chained Snake (*Coluber catenifer*), two Corn Snakes (*Coluber guttatus*), two Chicken Snakes (*Coluber obsoletus*), three Testaceous Snakes (*Zamenis flagelliformis*), a Long-nosed Snake (*Heterodon nasica*), an Amphiuma (*Amphiuma means*), three Menopomas (*Cryptobranchus alleghaniensis*), two Menobranchs (*Necturus maculatus*) from North America, deposited; two Barbary Wild Sheep (*Ovis tragelaphus*), a Japanese Deer (*Cervus sika*), a Yak (*Poephagus grunniens*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

WAVE-LENGTH OF GREEN CORONA LINE.—In the *Mem. della Soc. Degli. Spett. Ital.* (vol. xxx. pp. 124-128), Sig. Ascarza describes the results of observations made at Plascencia by the party from the Madrid Observatory during the total eclipse of the sun on May 27, 1900.

The instrumental equipment consisted of a Grubb coelostat with a mirror 20 centimetres diameter, furnishing light for a Steinheil objective of 12 centimetres aperture and 1·80 metres focus. This produced on the slit of the spectroscope an image of the sun about 16 millimetres in diameter.

A Dubosq spectroscope was used, furnished with six prisms and eyepiece micrometer reading to 1 : 300th of a millimetre. On account of the absorption of the prisms, only *three* were used for the final observations.

For the determination, measures were made on the lines 5328·696, 5270, 108 (E), and 5183·792 (b_1), and the resulting measures of the corona line reduced by interpolation formulæ. The spectroscope not being sufficiently powerful to separate the components of E, the mean of the wave-lengths of the two was adopted.

Preparation was made for both radial and tangential measures, but on account of the diffuse character of the line the tangential method was applied. The results were reduced by two interpolation formulæ, Gibbs and Hartmann, slightly varying values being obtained. The wave-lengths found on Rowland's scale were 5298·7 and 5298·818 respectively. The paper concludes with a note stating the difference of 4 tenth-metres between this value and that of 5303 obtained by Lockyer and Campbell from photographs taken during the total solar eclipse in India on January 22, 1898.

DEFORMATION OF THE SUN'S DISC.—In the *Mem. della Soc. Degli. Spett. Ital.* (vol. xxx. pp. 96-110), Sig. A. Ricco describes a long series of observations, both visual and photographic, of the varying deformations of the disc of the sun by the effect of atmospheric refraction, made at the observatories of Palermo and Catania (Etna). Many of the visual observations were made with a small Ramsden telescope having a terrestrial eyepiece, magnifying five times; photographs were also taken with a Merz telescope of 0·115 metre aperture and 1·93 metres focal length, adjusted to the chemical focus, giving an image about 0·0175 metre diameter.

The paper is illustrated by drawings and reproductions from many of the photographs, which are similar in many respects to those obtained by Colton at the Lick Observatory and published about 1895.

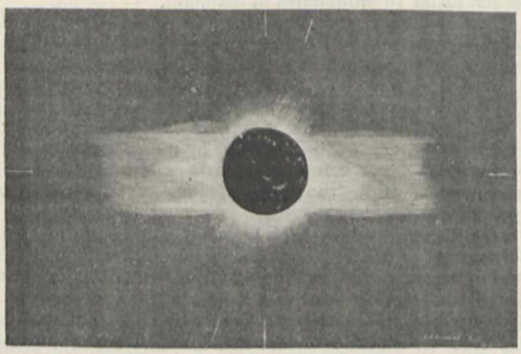
THE MINOR PLANET TERCIDINA.—In the note on p. 265, Prof. Hartmann's observations were misinterpreted. The photographs obtained at the Potsdam Observatory do not confirm the suspected variability suggested by the photograph obtained by Prof. Wolf in November 1899, nor do the later photographs of Prof. Wolf. The apparent variation may possibly be due to instrumental irregularities.

THE TOTAL ECLIPSE OF MAY 18, 1901.

THE following account of the total eclipse of the sun, May 18, is taken from a letter received from Mr. J. Cresswell, who was formerly a student of astronomical physics at the Royal College of Science, and is now engaged at a mining camp near the centre of Borneo (lat. 0° 45′ S., long. 113° E.).

The eclipse commenced about 12.20 in a cloudy sky, but fortunately about 15 minutes before totality the whole sky cleared and revealed a crescent sun. There were only one or two small clouds near the horizon, and the landscape appeared to have a peculiar violet tinge. There was no fall in temperature up to this point, the thermometer having remained stationary at 34°·75 C. Four minutes afterwards the landscape appeared as if seen through smoked glass, the temperature now being 34°·5. After the lapse of another 8 minutes the light was like that when a heavy storm is gathering, and shadows had a peculiar transparency; a number of stars appeared in the heavens distant from the sun. After 2½ minutes more had elapsed, second contact occurred and we were in darkness. The accompanying sketch of the corona was made and a photograph was taken with a small camera. The darkness was such that a small paraffin candle burning 100 metres away appeared quite bright. After about a minute the bright prominence *a* was seen, and it seemed to penetrate slightly into the dark body of the moon; this was seen for quite two minutes. Just before third contact the blood-red chromosphere appeared. At this time the temperature had steadily fallen to 31° C., but the lowest temperature recorded was $30^\circ\cdot25$, at 2h. 6m. 30s., making a total fall in temperature of $4^\circ\cdot5$ C.

Through a ruby glass the corona was invisible, except an irregular rim about one-eighth of the sun's diameter in width.



No air movements were noticed during the eclipse. Birds were not noticed to go to roost, but it was stated that some fowls did so. There is an insect known to the Dyaks as the "six o'clock insect," which invariably gives utterance to a very loud horn-like cry just before dark (*i.e.* about 6 p.m.), but its call was not heard during the eclipse.

THE AIMS OF THE NATIONAL PHYSICAL LABORATORY.[1]

THE idea of a physical laboratory in which problems bearing at once on science and on industry might be solved is comparatively new. The Physikalisch-Technische Reichsanstalt, founded in Berlin by the joint labours of Werner von Siemens and von Helmholtz during the years 1883–87, was perhaps the first. It is less than ten years since Dr. Lodge, in his address to Section A of the British Association, outlined the scheme of work for such an institution here in England. Nothing came of this; a committee met and discussed plans, but it was felt to be hopeless to approach the Government, and without Government aid there were no funds.

Four years later, however, the late Sir Douglas Galton took the matter up. In his address to the British Association in 1895, and again in a paper read before Section A, he called attention to the work done for Germany by the Reichsanstalt and to the crying need for a similar institution in England.

The result of this presidential pronouncement was the formation of a committee which reported at Liverpool, giving a rough outline of a possible scheme of organisation. A petition to Lord Salisbury followed, and as a consequence a Treasury committee, with Lord Rayleigh in the chair, was appointed to consider the desirability of establishing a National Physical Laboratory. The committee examined more than thirty witnesses and then reported unanimously "that a public institution should be founded for standardising and verifying instruments for testing materials and for the determination of physical constants."

It is natural to turn to the words of those who were instrumental in securing the appointment of this committee, and to the evidence it received, in any endeavour to discuss its aim. As was fitting, Sir Douglas Galton was the first witness to be called. It is a source of sorrow to his many friends that he has not lived to see the Laboratory completed.

And here may I refer to another serious loss which, in the last few days, the Laboratory has sustained. Sir Courtenay Boyle was a member of Lord Rayleigh's committee, and as such was convinced of the need for the Laboratory and of the importance of the work it could do. He took an active part in its organisation, sparing neither time nor trouble; he intended that it should be a great institution, and he had the will and the power to help. The country is the poorer by his sudden death.

Let me now quote some of Sir Douglas Galton's evidence. "Formerly our progress in machinery," he says, "was due to accuracy of measurement, and that was a class of work which could be done, as Whitworth showed, by an educated eye and educated touch. But as we advance in the applications of science to industry we require accuracy to be carried into matters which cannot be so measured. . . . In the more delicate researches which the physical, chemical and electrical student undertakes, he requires a ready means of access to standards to enable him to compare his own work with that of others." Or again, "My view is that if Great Britain is to retain its industrial supremacy we must have accurate standards available to our research students and to our manufacturers. I am certain that if you had them our manufacturers would gradually become very much more qualified for advancing our manufacturing industry than they are now. But it is also certain that you cannot separate some research from a standardising department." Then, after a description of the Reichsanstalt, he continues, "What I would advocate would be an extension of Kew in the direction of the second division of the Reichsanstalt, with such auxiliary research in the establishment itself as may be found necessary." The second division is the one which takes charge of technical and industrial questions. Prof. Lodge, again, gave a very valuable summary of work which ought to be done.

It is now realised, at any rate by the more enlightened of our leaders of industry, that science can help them. This fact, however, has been grasped by too few in England; our rivals in Germany and America know it well, and the first aim of the Laboratory is to bring its truth home to all, to assist in promoting a union which is certainly necessary if England is to retain her supremacy in trade and in manufacture, to make the forces of science available for the nation, to break down by every possible means the barrier between theory and practice, and to point out plainly the plan which must be followed unless we are prepared to see our rivals take our place.

"Germany," an American writer who has recently made a study of the subject has said, "is rapidly moving towards industrial supremacy in Europe. One of the most potent factors in this notable advance is the perfected alliance between science and commerce existing in Germany. Science has come to be regarded there as a commercial factor. If England is losing her supremacy in manufactures and in commerce, as many claim, it is because of English conservatism and the failure to utilise to the fullest extent the lessons taught by science, while Germany, once the country of dreamers and theorists, has now become intensely practical. Science there no longer seeks court and cloister, but is in open alliance with commerce and industry." It is our aim to promote this alliance in England, and for this purpose the National Physical Laboratory has been founded.

It is hardly necessary to quote chapter and verse for the assertion that the close connection between science and industry has had a predominant effect on German trade. If authority is wanted, I would refer to the history of the anilin dye manufacture, or, to take a more recent case, to the artificial indigo industry, in which the success of the Badische Company has recently been so marked. The factory at Ludwigshaven started thirty-five years ago with 30 men; it now employs more than 6000 and has on its staff 148 trained scientific chemists. And now, when it is perhaps too late, the Indian planters are calling in scientific aid and the Indian Government are giving some 3500*l.* a year to investigation.

As Prof. Armstrong, in a recent letter to the *Times*, says, "The truly serious side of the matter, however, is not the prospective loss of the entire indigo industry so much as the fact that an achievement such as that of the Badische Company seems past praying for here." Another instance is to be found in the German exhibit of scientific instruments at the Paris Exhibition, of which a full account appeared in the pages of NATURE.

And now, having stated in general terms the aims of the Laboratory and given some account of the progress in Germany, let me pass to some description of the means which have been placed at our disposal to realise those aims. I then wish, if time permits, to discuss in fuller detail some of the work which it is hoped we may take up immediately.

The Laboratory is to be at Bushy House, Teddington. I will

[1] A discourse delivered at the Royal Institution on Friday, May 24, by Dr. R. T. Glazebrook, F.R.S., Director of the Laboratory.

pass over the events which led to the change of site from the Old Deer Park at Richmond to Bushy. It is sufficient to say that at present Kew Observatory in the Deer Park will remain as the Observatory department of the Laboratory, and that most of the important verification and standardisation work which in the past has been done there will still find its home in the old building.

Bushy House was originally the official residence of the Ranger of Bushy Park. Queen Anne granted it in 1710 to the first Lord Halifax. In 1771 it passed to Lord North, being then probably rebuilt. Upon the death of Lord North's widow in 1797, the Duke of Clarence, afterwards William IV., became Ranger; after his death in 1837 it was granted to his widow, Queen Adelaide, who lived there until 1849. At her death it passed to the Duc de Nemours, son of King Louis Philippe, and he resided there at intervals until 1896.

In spite of this somewhat aristocratic history, it will make an admirable Laboratory. A description of the Laboratory, with illustrations, will be found in NATURE, vol. lxiii. p. 300.

The floor space available is much less than that of the Reichsanstalt. But size alone is not an unmixed advantage; there is much to be said in favour of gradual growth and development, provided the conditions are such as to favour growth. Personally I should prefer to begin in a small way if only I felt sure I was in a position to do the work thoroughly, but there is danger of starvation. Even with all the help we get in freedom from rent and taxes, outside repairs and maintenance, the sum at the disposal of the committee is too small.

Science is not yet regarded as a commercial factor in England. Is there no one who realises the importance of the alliance, who will come forward with more ample funds to start us on our course with a fair prospect of success? One candid friend has recently told us in print that the new institution is on such a microscopic scale that its utility in the present struggle is more than doubtful. Is there no statesman who can grasp the position and see that with, say, double the income the chances of our doing a great work would be increased a hundredfold?

The problems we have to solve are hard enough; give us means to employ the best men and we will answer them, starve us and then quote our failure as showing the uselessness of science applied to industry.

There is some justice in the criticism of one of our technical papers. I have recently been advertising for assistants, and a paper in whose columns the advertisement appears writes, "The scale of pay is certainly not extravagant. It is, however, possible that the duties will be correspondingly light."

Now let me illustrate these aims by a more detailed account of some of the problems of industry which have been solved by the application of science, and then of some others which remain unsolved and which the Laboratory hopes to attack. The story of the Jena Glass Works is most interesting; I will take it first.

An exhibition of scientific apparatus took place in London in 1876. Among the visitors to this was Prof. Abbé, of Jena, and in a report he wrote on the optical apparatus he called attention to the need for progress in the art of glass making if the microscope were to advance, and to the necessity for obtaining glasses having a different relation between dispersion and refractive index than that found in the material at the disposal of opticians. Stokes and Harcourt had already made attempts in this direction, but with no marked success.

In 1881 Abbé and Schott, at Jena, started their work. Their undertaking, they write five years later in the first catalogue of their factory, arose out of a scientific investigation into the connection between the optical properties of solid amorphous fluxes and their chemical constitution. When they began their work some six elements only entered into the composition of glass. By 1888 it had been found possible to combine with these, in quantities up to about 10 per cent., twenty-eight different elements, and the effect of each of these on the refractive index and dispersion had been measured. Thus, for example, the investigators found that by the addition of boron the ratio of the length of the blue end of the spectrum to that of the red was increased; the addition of fluorine potassium or sodium produced the opposite result.

Now in an ordinary achromatic lens of crown and flint, if the total dispersion for the two be the same, then for the flint glass the dispersion of the blue end is greater, that of the red less than for the crown; thus the image is not white, a secondary spectrum is the result.

Abbé showed, as Stokes and Harcourt had shown earlier, that by combining a large proportion of boron with the flint its dispersion was made more nearly the same as that of the crown, while by replacing the silicates in the crown glass by phosphates a still better result was obtained, and by the use of three glasses three lines of the spectrum could be combined; the spectrum outstanding was a tertiary one, and much less marked than that due to the original crown and flint glass. The modern microscope became possible.

The conditions to be satisfied in a photographic lens differ from those required for a microscope. Von Seidel had shown that with the ordinary flint and crown glasses the conditions for achromatism and for flatness of field cannot be simultaneously satisfied. To do this we need a glass of high refractive index and low dispersive power, or *vice versâ*; in ordinary glasses these two properties rise and fall together. By introducing barium into the crown glass a change is produced in this respect. For barium crown the refractive index is greater and the dispersive power less than for soft crown.

With two such glasses, then, the field can be achromatic and flat. The wonderful results obtained by Dallmeyer and Ross in this country, by Zeiss and Steinheil in Germany, are due to the use of these new glasses. They have also been applied with marked success to the manufacture of the object glasses of large telescopes.

But the Jena glasses have other uses besides optical. "About twenty years ago"—the quotation is from the catalogue of the German exhibition—"the manufacture of thermometers had come to a dead stop in Germany, thermometers being then invested with a defect, their liability to periodic changes, which seriously endangered German manufacture. Comprehensive investigations were then carried out by the Normal Aichungs Commission, the Reichanstalt and the Jena Glass Works, and much labour brought the desired reward."

The defect referred to was the temporary depression of the ice point which takes place in all thermometers after heating. Let the ice point of a thermometer be observed; then raise the thermometer to, say, 100°, and again observe the ice point as soon as possible afterwards; it will be depressed below its previous position. In some instruments of Thuringian glass a depression of as much as 0°·65 C. had been noted. For scientific purposes such an instrument is quite untrustworthy. If it be kept at, say, 15° and then immersed in a bath at 30°, its reading will be appreciably different from that which would be given if it were first raised to, say, 50°, allowed to cool quickly just below 30°, and then put into the bath. This was the defect which the investigators set themselves to cure.

Table I. gives some details as to thermometers.

TABLE I.

Depression of Freezing Point for various Thermometers.

Humboldt, 1835	0°·06
Greiner, 1872	0·38
Schultzer, 1875	0·44
Rapps, 1878	0·65
English glass	0·15
Verre Dur	0·08
16'''	0·05
59'''	0·02

Analysis of Glasses.

	SiO_2	Na_2O	CaO	Al_2O_3	ZnO	B_2O_3
16'''	67·5	14	7	2·5	7	2
59'''	72	11	—	5	—	12

Weber had found in 1883 that glasses which contain a mixture of soda and potash give a very large depression. He made a glass free from soda with a depression of 0°·1. The work was then taken up by the Aichungs Commission, the Reichsanstalt and the Jena factory. Weber's results were confirmed. An old thermometer of Humboldt's, containing 0·86 per cent. of soda and 20 per cent. of potash, had a depression of 0°·06, while a new instrument, in which the percentages were 12·7 per cent. and 10·6 per cent. respectively, had a depression of 0°·65.

An English standard, with 1·5 per cent. of soda and 12·3 per cent. of potash, gave a depression of 0°·15, while a French "verre dur" instrument, in which these proportions were reversed, gave only 0°·08.

It remained to manufacture a glass which should have a low depression and at the same time other satisfactory properties. The now well-known glass 16‴ is the result. Its composition is shown in the Table.

The fact that there was an appreciable difference between the scale of the 16‴ glass and that of the air thermometer led to further investigations, and another glass, a borosilicate containing 12 per cent. of boron, was the consequence. This glass has a still smaller depression.

Previous to 1888 Germany imported optical glass. At that date nearly all the glass required was of home manufacture. Very shortly afterwards an export trade in raw glass began, which in 1898 was worth 30,000*l.* per annum, while the value of optical instruments, such as telescopes, field-glasses and the like, exported that year was 250,000*l.* Such are the results of the application of science, *i.e.* organised common sense, to a great industry. The National Physical Laboratory aims at doing the like for England.

I have thus noted very briefly some of the ways in which science has become identified with trade in Germany, and have indicated some of the investigations by which the staff of the Reichsanstalt and others have advanced manufactures and commerce.

Let us turn now to the other side, to some of the problems which remain unsolved, to the work which our Laboratory is to do and by doing which it will realise the aims of its founders.

The microscopic examination of metals was begun by Sorby in 1864. Since that date many distinguished experimenters, Andrews, Arnold, Ewing, Martens, Osmond, Roberts-Austen, Stead and others, have added much to our knowledge. I am indebted to Sir W. Roberts-Austen for the slides which I am about to show you to illustrate some of the points arrived at. Prof. Ewing a year ago laid before the Royal Institution the results of the experiments of Mr. Rosenhain and himself.

This microscopic work has revealed to us the fact that steel must be regarded as a crystallised igneous rock. Moreover, it is capable, at temperatures far below its melting point, of altering its structure completely, and its mechanical and magnetic properties are intimately related to its structure. The chemical constitution of the steel may be unaltered, the amounts of carbon, silicon, manganese, &c., in the different forms remain the same, but the structure changes, and with it the properties of the steel.

Sections of the same steel polished and etched after various treatments show striking differences. For instance, if a highly carburised form containing 1·5 per cent. of carbon be cooled down from the liquid state, the temperature being read by the deflection of a galvanometer needle in circuit with a thermopile, the galvanometer shows a slowly falling temperature till we reach 1380° C., when solidification takes place; the changes which now go on take place in solid metal. After a time the temperature again falls until we reach 680°, when there is an evolution of heat; had the steel been free from carbon there would have been evolution of heat at 895° and again at 766°. Now throughout the cooling, molecular changes are going on in the steel. By quenching the steel suddenly at any given temperature we can check the change and examine microscopically the structure of the steel at the temperatu re at which it was checked.

[Slides were shown representing the microscopic structures of steels subjected to different treatment as regards temperature and annealing.]

These slides are sufficient to call attention to the changes which occur in solid iron, changes whose importance is now beginning to be realised. On viewing them it is a natural question to ask how all the other properties of iron related to its structure; can we by special treatment produce a steel more suited to the shipbuilder, the railway engineer or the dynamo maker than any he now possesses?

These marked effects are connected with variations in the condition of the carbon in the iron; can equally or possibly more marked changes be produced by the introduction of some other element? Guillaume's nickel steel, with its small coefficient of expansion, appears to have a future for many purposes; can it or some modification be made still more useful to the engineer?

We owe much to the investigations of the Alloys Research Committee of the Institution of Mechanical Engineers. Their distinguished chairman holds the view that the work of that committee has only begun, and that there is scope for such research for a long time to come at the National Physical Laboratory. The executive committee have accepted this view by naming as one of the first subjects to be investigated the connection between the magnetic quality and the physical, chemical and electrical properties of iron and its alloys, with a view specially to the determination of the conditions for low hysteresis and non-agency properties.

At any rate we may trust that the condition of affairs mentioned by Mr. Hadfield in his evidence before Lord Rayleigh's Commission which led a user of English steel to specify that before the steel could be accepted it must be stamped at the Reichsanstalt, will no longer exist.

The subject of wind pressure, again, is one which has occupied this committee's attention to some extent. The Board of Trade rules require that in bridges and similar structures (1) That a maximum pressure of 56 lbs. per square foot be provided for; (2) that the effective surface on which the wind acts should be assumed as from once to twice the area of the front surface, according to the extent of the openings in the lattice girders; (3) that a factor of safety of 4 for the iron work and of 2 for the whole bridge overturning be assumed. These recommendations were not based on any special experiments. The question had been investigated in part by the late Sir W. Siemens.

During the construction of the Forth Bridge Sir B. Baker conducted a series of observations. The results of the first two years' observations are shown in Table II., taken from a paper read at the British Association in 1884. Three gauges were used.

TABLE II.

Revolving gauge.		Small fixed gauge.		Large fixed gauge.	
Mean pressure.		Easterly.	Westerly.	Easterly.	Westerly.
lb.	lb.	lb.	lb.	lb.	lb.
0 to 5	3·09	3·47	2·92	2·04	1·9
5 to 10	7·58	4·8	7·7	3·54	4·75
10 to 15	12·4	6·27	13·2	4·55	8·26
15 to 20	17·06	7·4	17·9	5·5	12·66
20 to 25	21·0	12·25	22·75	8·6	19
25 to 30	27·0		28·5		18·25
30 to 35	32·5		38·5		21·5
Above	65		41·0		35·25
(One observation only above 32·5).					

In No. 1 the surface on which the wind acted was about $1\frac{1}{2}$ square feet in area; it was swivelled so as always to be at right angles to the wind. In No. 2 the area of surface acted on was of the same size, but it was fixed with its plane north and south. No. 3 was also fixed in the same direction, but it had 200 times the area, its surface being 300 square feet.

In preparing the table the mean of all the readings of the revolving gauge between 0 and 5, 5 and 10, &c., lbs. per square foot have been taken, and the mean of the corresponding readings of the small fixed gauge and the large fixed gauge set opposite, these being arranged for easterly and westerly winds.

Two points are to be noticed: (1) only one reading of more than 32·5 lbs. was registered, and this, it is practically certain, was due to faulty action in the gauge.

Sir B. Baker has kindly shown me some further records with a small gauge.

According to these pressures of more than 50 lbs. have been registered on three occasions since 1886. On two other occasions the pressures, as registered, reached from 40 to 50 lbs. per square foot. But the table, it will be seen, enables us to compare the pressure on a small area with the average pressure on a large area, and it is clear that in all cases the pressure per square foot as given by the large area is much less than that deduced from the simultaneous observations on the small area.

The large gauge became unsafe in 1896 and was removed; but the observations for the previous ten years entirely confirm this result, the importance of which is obvious. The same result may be deduced from the Tower Bridge observations. Power is required to raise the great bascules, and the power needed depends on the direction of the wind. From observations on the power some estimate of the average wind pressure on the surface may be obtained, and this is found to be less than the pressure registered by the small wind gauges. Nor is the result surprising, when the question is looked at as a hydrodynamical problem; the lines of fluid near a small obstacle will differ from those near a large one, and the distribution of pressure over the large area will not be uniform. Sir W. Siemens is said to have found places of negative pressure near such an obstacle. As Sir J. Wolfe Barry has pointed out, if the average of 56 lbs. to the square foot is excessive, then the cost and difficulty of erection of large engineering works is being unnecessarily increased. Here is a problem well worthy of attention, and about which but little is known. The same, too, may be said about the second of the Board of Trade rules. What is the effective surface over which the pressure is exerted on a bridge? On this again our information is but scanty. Sir B. Baker's experiments for the Forth Bridge led him to adopt as his rule, Double the plane surface exposed to the wind and deduct 50 per cent. in the case of tubes. On this point again further experiments are needed.

To turn from engineering to physics. In metrology, as in many other branches of science, difficulties connected with the measurement of temperature are of the first importance.

I was asked some little time since to state, to a very high order of exactness, the relation between the yard and the metre. I could not give the number of figures required. The metre is defined at the freezing point of water, the yard at a temperature of 62° F. When a yard and a metre scale are compared they are usually at about the same temperature; the difficulty of comparison is enormously increased if there be a temperature difference of 30° F. between the two scales. Hence we require to know the temperature coefficients of the two standards. But that of the standard yard is not known; it is doubtful, I believe, if the composition of the alloy of which it is made is known, and in consequence Mr. Chaney has mentioned the determination of coefficients of expansion as one of the investigations which it is desirable that the Laboratory should undertake.

Or, again, take thermometry. The standard scale of temperature is that of the hydrogen thermometer; the scale in practical use in England is the mercury in flint glass scale of the Kew standard thermometers. It is obvious that it is of importance to science that the difference between the scales should be known, and various attempts have been made to compare them. But the results of no two series of observations which have been made agree satisfactorily. The variations arise probably in great measure from the fact that the English glass thermometer, as ordinarily made and used, is incapable of the accuracy now demanded for scientific investigations. The temporary depression of the freezing point already alluded to in discussing the Jena glass is too large; it may amount to three- to four-tenths of a degree when the thermometer is raised 100°. Thus the results of any given comparison depend too much on the immediate past history of the thermometer employed, and it is almost hopeless to construct a table, accurate, say, to ·01, which will give the difference between the Kew standard and the hydrogen scale, and so enable the results of former work in which English thermometers were used to be expressed in standard degrees.

TABLE III.—*Values of Corrections to the English Glass Thermometer Scale to give Temperatures on the Gas Thermometer Scale found by various Observers.*

Temp.	Rowland.	Guillaume.	Wiebe.
° 0	° 0	° 0	° 0
10	– ·03	– ·009	+ ·03
20	– ·05	– ·009	+ ·00
30	– ·06	– ·002	+ ·02
40	– ·07	+ ·007	+ ·09
50	– ·07	+ ·016	+ ·14
60	– ·06	+ ·014	
70	– ·04	+ ·028	
80	– ·02	+ ·026	
90	– ·01	+ ·017	
100	0	0	

This is illustrated by Table III., which gives the differences as found (1) by Rowland; (2) Guillaume; (3)? Wiebe between a Kew thermometer and the air thermometer.

It is clearly important to establish in England a mercury scale of temperatures which shall be comparable with the hydrogen scale, and it is desirable to determine as nearly as may be the relation between this and the existing Kew scale.

I am glad to say that in the first endeavour we have secured the valuable cooperation of Mr. Powell, of the Whitefriars Works, and that the first specimens of glass he has submitted to us bid fair to compare well with 16'''.

Another branch of thermometry at which there is much to do is the measurement of high temperature. Prof. Callendar has explained here the principles of the resistance thermometer, due first to Sir W. Siemens. Sir W. C. Roberts-Austen has shown how the thermopile of Le Chatellier may be used for the measurement of high temperatures. There is a great work left for the man who can introduce these or similar instruments to the manufactory and the forge, or who can improve them in such a manner as to render their uses more simple and more sure. Besides, at temperatures much over 1000° C., the glaze on the porcelain tube of the pryometer gives way.

So far we have discussed new work, but there is much to be done in extending a class of work which has gone on quietly and without much show for many years at the Kew Observatory. Thermometers and barometers, wind gauges and other meteorological apparatus, watches and chronometers, and many other instruments are tested there in great numbers, and the value of the work is undoubted. The competition among the best makers for the first place, the best watch of the year, is most striking and affords ample testimony to the importance of the work.

Work of this class we propose to extend. Thus, there is no place where pressure gauges or steam indicators can be tested. It is intended to take up this work, and for this purpose a mercury pressure column is being erected.

Again, there are the ordinary gauges in use in nearly every engineering shop. These, in the first instance, have probably come from Whitworth's, or nowadays, I fear, from Messrs. Pratt and Whitney or Browne and Sharpe, of America. They were probably very accurate when new, but they wear, and it is only in comparatively few large shops that means exist for measuring the error and for determining whether the gauge ought to be rejected or not. Hence arise difficulties of all kinds. Standardisation of work is impossible.

In another direction a wide field is offered in the calibration and standardisation of glass measuring vessels of all kinds, flasks, burettes, pipettes, &c., used by chemists and others. At the request of the Board of Agriculture we have already arranged for the standardisation of the glass vessels used in the Babcock method of measuring the butter fat in milk, and in a few months many of these have passed through our hands. We are now being asked to arrange for testing the apparatus for the Gerber and Leffman-Beam methods, and this we have promised to do when we are settled at Bushy. Telescopes, opera-glasses, sextants, and other optical appliances, are already tested at Kew, but this work can, and will, be extended. Photographic lenses are now examined by eye; a photographic test will be added, and I trust the whole may be made more useful to photographers.

I look to the cooperation of the Optical Society to advise how we may be of service to them in testing spectacles, microscope lenses and the like. The magnetic testing of specimens of iron and steel, again, offers a fertile field for inquiry. If more subjects are needed it is sufficient to turn over the pages of the evidence given before Lord Rayleigh's Commission, or to look to the reports which have been prepared by various bodies of experts for the executive committee.

In electrical matters there are questions relating to the fundamental units on which, in Mr. Trotter's opinion, we may help the officials of the Board of Trade. Standards of capacity are wanted; those belonging to the British Association will be deposited at the Laboratory. Standards of electromagnetic induction are desirable; questions continually arise with regard to new forms of cells other than the standard Clark cell, and in a host of other ways work will be found.

I have gone almost too much into detail. It has been my wish to state in general terms the aims of the Laboratory to make the advance of physical science more readily available for the needs of the nation, and then to illustrate the way in which it is intended to attain those aims. I trust I may have shown that the National Physical Laboratory is an institution which may deservedly claim the cordial support of all who are interested in real progress.

ON THE SEPARATION OF THE LEAST VOLATILE GASES OF ATMOSPHERIC AIR, AND THEIR SPECTRA.[1]

THE separation of these gases from each other was effected by collecting them in a bulb in the solid state and allowing the solid gradually to evaporate at as low a temperature as possible, while the vapour was continually pumped away with a mercurial pump. Between the bulb containing the solidified gases and the pump a sparking tube was interposed, where the spectrum emitted by the vapour under the influence of an electric discharge was from time to time observed.

The success of the operation of separating the gases which boil at different temperatures depends on keeping the temperature of the solid mass as low as possible, as will be seen from the following consideration :—

The pressure, p, of a gas G, above the same material in the liquid state, at temperature T, is given approximately by the formula

$$\log p = A - \frac{B}{T},$$

where A and B are constants for the same material. For some other gas G′ the formula will be

$$\log p_1 = A_1 - \frac{B_1}{T},$$

and

$$\log \frac{p}{p_1} = A - A_1 + \frac{B_1 - B}{T}.$$

Now for argon, krypton and xenon, respectively, the values of A are 6·782, 6·972 and 6·963, and those of B are 339, 496·3 and 669·2 ; and for these and many other substances $A - A_1$ is always a small quantity, while $\frac{B_1 - B}{T}$ is considerable and increases as T diminishes. Hence the ratio of p to p_1 increases rapidly as T diminishes, and by evaporating the gases always from the solid state and keeping the solid at as low a temperature as possible, the gas first coming off consists in by far the greatest part of that which has the lowest boiling point, and is succeeded, with comparative abruptness, by the gas which has the next higher boiling point. So abrupt indeed is the succession that the nitrogen is almost completely removed before the argon makes its appearance, and the necessity for removing the nitrogen by sparking with oxygen almost wholly avoided. The change from one gas to another is easily detected by examining the spectrum in the sparking tube, and the reservoirs into which the gases are pumped can be changed when the spectrum changes and the fractions separately stored.

The general sequence of spectra, omitting those of nitrogen, hydrogen and compounds of carbon, which were never entirely removed by the process of distillation alone, was as follows : The spectrum of argon was first noticed in succession to nitrogen, and then as the distillation proceeded the brightest rays, green and yellow, of krypton appeared, and then the intensity of the argon spectrum waned, and it gave way to that of krypton until, as predicted by Runge, when a Leyden jar was in the circuit, the capillary part of the sparking tube had a magnificent blue colour, while the wide ends were bright pale yellow. Without a jar the tube was nearly white in the capillary part, and yellow about the poles. As the distillation proceeded, the temperature of the vessel containing the solid mixture being allowed to rise slowly, the brightest of the xenon rays began to appear, namely, the green rays about λ 5420, 5292 and 4922, and then the krypton rays soon died out and were superseded by the xenon rays. At this stage the capillary part of the sparking tube is, with a jar in circuit, a brilliant green, and is still green, though less brilliant, without the jar. The xenon formed the final fraction distilled.

The authors give a long list of the approximate wave-lengths of rays they have observed to be emitted by xenon and krypton under the influence of electric discharge.

The variation of the spectra of both xenon and krypton with variation in the character of the electric discharge is very striking, and has already been the subject of remark,[1] in the case of krypton, by Runge, who has compared krypton with argon in its sensitiveness to changes in the electric discharge. Runge distinguishes krypton rays which are visible without a jar and those which are only visible with a jar discharge. The difference in the intensity of certain rays, according as the discharge is continuous or oscillatory, is no doubt very marked, but, with rare exceptions, the authors have found that the rays which are intensified by the oscillatory discharge can be seen with a continuous discharge when the slit of the spectroscope is wide. Runge used a grating, whereas they have, for the sake of more light, used a prism spectroscope throughout, and were therefore able to observe many more rays than he.

There is one very remarkable change in the xenon spectrum produced by the introduction of a jar into the circuit. Without the jar xenon gives two bright green rays at about λ 4917 and λ 4924, but on putting a jar into the circuit they are replaced by a single still stronger ray at about λ 4922. In no other case have the authors noticed a change so striking as this on merely changing the character of the discharge. It is noteworthy that the ray λ 4922 is close to a well-known helium ray, but other helium rays were not seen in the same spectrum. Changes of the spectrum by the introduction of a jar into the circuit are, however, the rule rather than the exception, and there are changes in the spectrum of krypton which seem to depend on other circumstances. Of many tubes filled with krypton in the manner above indicated, some give with no jar the green ray λ 5571, the yellow ray λ 5871 and the red ray λ 7600 very bright, while other rays are very few, and those few barely visible. Putting a jar into the circuit makes very little difference ; the three rays above mentioned remain much the brightest, nearly, though not quite, so bright as before, and the blue rays, so conspicuous in other tubes, though strengthened by the use of the jar, are still very weak. In other tubes the extreme red ray is invisible, the rays at λ 5571 and 5871 absolutely, as well as relatively, much feebler, while the strong blue rays are bright, even brighter than the green and yellow rays above named. In one tube the blue rays could be seen, though not the others. This looks very much as if two different gases were involved, but the authors have not been able to assure themselves of that. The case seems nearly parallel with that of hydrogen. There are some hydrogen tubes which show the second spectrum of hydrogen very bright, and others which show only the first spectrum ; the second spectrum is enfeebled or extinguished by introducing a jar into the circuit, while the first spectrum is strengthened ; and the conditions which determine the appearance of the ultra-violet series of hydrogen rays have not yet been satisfactorily made out.

It is to be noted that putting the jar out of circuit does not in general immediately reduce the brightness of the rays which are strengthened by the jar discharge. Their intensity fades gradually, and is generally revived, more or less, by reversing the direction of the current, but this revival gets less marked at each reversal until the intensity reaches its minimum. The rays strengthened by the jar discharge also sometimes appear bright, without a jar, on first passing the spark when the electrodes are cold, and fade when the electrodes get hot, reappearing when the tube has cooled again. Moreover, if the discharge be continued without a jar, the resistance in the krypton tubes increases rather rapidly, the tube becomes much less luminous and finally refuses to pass the spark. With an oscillatory discharge the passage of the spark and the brightness of the rays are much more persistent. This seems to point to some action at the electrodes which is more marked in the case of krypton than in that of xenon.

The xenon spectrum is characterised by a group of four conspicuous orange rays of about equal intensities, a group of very bright green rays of which two are especially conspicuous, and several very bright blue rays. The list of xenon rays published by Erdmann does not present any close agreement with that of the authors except as to the strongest green lines. The number of xenon rays observed is very considerable, and some of them lie very near to rays of the second spectrum of hydrogen, but inasmuch as these rays are more conspicuous with a jar in circuit than without, which is not the character of the second spectrum of hydrogen, and as, moreover, many of the brightest of the hydrogen rays are absent from the spectrum of the tubes, the authors conclude that these rays are not due to hydrogen.

Certain rays, tabulated separately, have been as yet observed in only one xenon tube ; they include a very strong ultra-violet ray of unknown origin, and due either to some substance other than xenon or to some condition of the tube which has not been repeated in the other tubes.

[1] Abridged from a paper by Prof. G. D. Liveing, F.R.S., and Prof. J. Dewar, F.R.S., read before the Royal Society on June 20.

The authors' krypton rays agree tolerably closely with Runge's list, but outnumber his very considerably, as might be expected when prisms were used instead of a grating. The authors think that the krypton used by Runge must have contained some xenon, and that the rays for which he gives the wave-lengths 5419·38, 5292·37 and 4844·58 were really due to xenon, as they are three of the strongest rays emitted by their xenon tubes, and are weak in, and in some cases absent from, the spectra of their krypton tubes.

Appended to the paper are tables showing wave-lengths of xenon and krypton lines to four figures.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE University of St. Andrews has received information that a legacy of 4000*l.* has been left to it by the late Miss Malcolm for the establishment of medical bursaries and scholarships.

ACTING on the suggestion made by Mr. Chamberlain, the general purposes committee of the Birmingham City Council has resolved to recommend the Council to make a grant to the Birmingham University of the proceeds of a halfpenny rate. This will provide an annual sum of 5000*l.*

THE Agent General for New South Wales intimates that applications are invited from gentlemen qualified to fill the chair of pathology in the University of Sydney. Particulars may be obtained from the Agent General for New South Wales, 9, Victoria Street, London, S.W.

THE Technical Education Board of the London County Council has directed the higher education subcommittee to inquire and report (*a*) as to the need and present provision for special training of an advanced kind in connection with the application of science (especially chemistry and electricity) to industry; (*b*) as to what, if any, developments are needed to secure efficient training in these subjects for senior county scholars and other advanced students who desire to qualify themselves to take leading positions in scientific industries. The Board has arranged to make a grant of 10,000*l.* a year to the University of London, and is thus directly interested in the development of advanced scientific instruction in London.

SCIENTIFIC SERIAL.

American Journal of Mathematics, vol. xxiii. No. 3.—Geometry on the cubic scroll of the second kind, by F. C. Ferry, is the conclusion (34 pp.) of a paper commenced in the last number.—Congruent reductions of bilinear forms, by T. J. I'A. Bromwich, contains an account and a slight extension of a method due to Kronecker (*Gesamm. Werke*, Bd. i. p. 349). This method was employed in the first place for the reduction of two quadratic forms. In the present paper it is applied to four cases of reductions, viz. (1) two symmetric forms (the same as Kronecker's case); (2) a symmetric and an alternate form; (3) two alternate forms; and (4) two Hermite's forms. In cases (1)-(3) the substitutions are congruent, while in (4) they are conjugate imaginaries. Mr. Bromwich gives a list of the principal papers which deal with the problems he has considered in his article. On the imprimitive substitution groups of degree fifteen and the primitive substitution groups of degree eighteen, by E. Norton Martin, was presented, in abstract and in a slightly different form, at the summer meeting of the American Mathematical Society in 1899. Herein he has added two new groups to his original list, viz. the groups with five systems of imprimitivity simply isomorphic to the alternating and symmetric groups of degree five, and he mentions that Dr. Kuhn reported at the February (1900) meeting of the Society that he had carried the investigation further by adding twenty-eight to the seventy groups found by Mr. Martin. The list even now does not claim to be absolutely complete, since omissions are always possible. A somewhat long list of recent papers on the subject is appended to the article.—Removal of any two terms from a binary quantic by linear transformations, by Bessie G. Morrison, discusses these linear transformations and gives applications to the non-singular cubic, quartic, quintic and sextic.

SOCIETIES AND ACADEMIES.

LONDON.

Geological Society, June 19.—Mr. J. J. H. Teall, V.P.R.S., president, in the chair.—On the use of a geological datum, by Mr. Beeby Thompson. A proper interpretation of geological phenomena frequently requires that allowance shall be made for differential earth-movements that have taken place since the period under consideration. Present differences of level in rocks of the same age may be due to actual differences in depth of the sea-floor on which they were deposited; but they may also be the result of subsequent differential earth-movements. The rock selected as a datum should combine as far as possible the following characteristics:—It should be thin, of considerable horizontal extension, having similarity in physical characters and palæontological contents over a large area, and situated as near as possible, in vertical sequence, to the reference-deposit. In Northamptonshire three formations meet these requirements—the Rhætic Beds, the Marlstone Rock-bed and the Cornbrash. The author applies the Marlstone Rock-bed as a datum to the study of the five chief deep explorations in Northamptonshire, with the following results:—While the old land-surface (below the Trias) now varies in height by more than 250 feet, the variation in thickness of the rocks between it and the Middle Lias only reaches 56½ feet; and although the old land-surface is actually lowest where the Rhætic rocks have not been detected, when compared with the position of the Marlstone it is found to be the highest. The further application of the same method enables the author to recognise Rhætic rocks at Northampton, to correct the record of the Kingsthorpe shaft, and to explain the presence of Triassic saline water in the Marlstone. A revised section of the Kingsthorpe shaft is given. Another point proved is that a general levelling-up process was going on just before the beginning of the Lower Liassic Period, and another at the close of the Middle Liassic Period.—On intrusive, tuff-like, igneous rocks and breccias in Ireland, by Messrs. James R. Kilroe and Alexander McHenry.—Many fragmental igneous rocks, although resembling tuffs, cannot be regarded as ejectamenta on account of their character and mode of occurrence in the field. A series of sections is exhibited to illustrate how tuff-like masses invade black slate of Llandeilo age in the South-east of Ireland, generally adhering to the direction of bedding, but frequently cutting across it and detaching numerous pieces from the slate, which are more abundant near the margins of the intrusion than elsewhere.

PARIS.

Academy of Sciences, July 8.—M. Fouqué in the chair.—On new derivatives of benzylcamphor and benzylidenecamphor, by MM. A. Haller and J. Minguin. In continuation of previous researches it is now shown that the unsaturated acid, $C_6H_5-CH=CH \cdot C_8H_{14} \cdot CO_2H$, obtained by the action of hydrobromic acid on benzylidenecamphor, or by treating bromobenzylcamphor with alcoholic potash or ammonia, combines with a molecule of hydrogen bromide to form phenylbromohomocampholic acid, which, when warmed with hydrobromic acid in acetic acid solution, loses bromine and yields the corresponding hydroxy-acid. The action of bromine on dextrobenzylcamphor results in the formation of two stereoisomeric bromobenzylcamphors which yield benzylidenecamphor on treatment with alcoholic potash. Further bromination of benzylcamphor gives rise to unstable dibromo-derivatives which are converted by the action of potash into ortho- and para-bromobenzylidenecamphors; the para-compound forms bromophenylhydroxyhomocampholic acid on treatment with hydrobromic acid at 100°.—Osmotic pressure and its *rôle* as a protection from cold in the living cell, by M. D'Arsonval. At the low temperature of liquid air animal and vegetable tissues in general become extremely hard and friable, whereas the vitality of yeast and various pathogenic micro-organisms is not impaired even by several weeks' exposure to cold. In explanation of this fact it is suggested that the solidification of such minute cells is prevented by the enormous osmotic pressure exerted therein, and it is shown that in the case of yeast the osmotic pressure may be reduced by the action of hypertonic solutions of certain salts to such an extent as to destroy the power of resisting the influence of cold.—New nebulæ discovered at the Paris Observatory, by M. G. Bigourdan.—Observations of Hall's comet 1901(*a*) at the Rio de Janeiro Observatory, by M. H. Morize.—Solar observations at the Lyon Observatory during the first quarter of 1901, by M. J. Guillaume.—On the conjugate nets of orthogonal and isothermal curves, by M. Demartres.—On the use in series of disjunctive voltameters, by M. Ch. Pollak. A note on a previous communication by the author.—On manganic phosphates, by M. V. Auger. The phosphate obtained by heating manganese nitrate with phosphoric acid at 210° and extracting the fused mass with water has the composition $MnP_6O_{21} + 14H_2O$, and is evidently a pyrophosphate; it is dissolved by phosphoric acid, forming a violet solution which soon becomes opalescent and deposits the normal phosphate, $MnPO_4 + H_2O$. Manganese metaphosphate, MnP_3O_9, is obtained by heating phosphorus pentoxide with hydrated manganese dioxide.—Action of acid chlorides on methanal, by M. Louis Henry. The author confirms Descudé's recent observation that the presence of zinc chloride facilitates the action of acid chlorides on aldehydes. Benzoyl chloride alone has no action on methanal (trioxymethylene), but in the presence of zinc chloride a rapid reaction takes place with the form of a substance, crystallising in needles, which appears to be chloromethyl benzoate.—Action of vegetable alkaloids on some indicators, by M. A. Astruc. The behaviour of a number of alkaloids towards the indicators helianthin, rosolic acid and phenolphthalein was examined. In order to avoid the dissociating influence of water, ethyl alcohol, amyl alcohol and benzene were employed as solvents. The results obtained depend on the solvent used in each case, as well as on the nature of the alkaloid.—On dinaphthoxanthene, by M. R. Fosse. The action of bromine on dinaphthoxanthene leads to the formation of bromodinaphthoxanthene, a red, crystalline substance melting at 218-220°, which is remarkable in that when warmed with alcohol it undergoes a reaction similar to that exhibited by diazo-derivatives, hydrogen bromide, aldehyde and dinaphthoxanthene being produced. Bisdinaphthoxanthenamine, obtained by the action of alcoholic ammonia on the above-described bromine derivative, is a crystalline compound melting at 230°. Chlorodinaphthoxanthene crystallises in red needles melting at 150°.—Study of the product of the nitration of acetoacetic ether, by MM. L. Bouveault and A. Bongert. The compound previously described as produced by the nitration of acetoacetic ether is shown to be isomeric with, but quite different in its reactions from, the substance which Scholl obtained by the action of silver nitrate on ethyl bromacetate.—On a method of synthesis of acetylenic aldehydes, by MM. Ch. Moureu and R. Delange. The condensation of the ethers of formic acid with the sodium derivatives of true acetylenic hydrocarbons, $R-C\equiv CH$, leads to the formation of acetylenic aldehydes, $R-C\equiv C-CHO$, whilst the ethers of higher acids give rise to acetylenic ketones.—Attempts to render vegetables immune against cryptogamic diseases, by M. J. Beauverie. Seeds and cuttings grown in soil in which the fungus *Botrytis cinerea* had been previously allowed to develop were found to produce plants capable of resisting the action of the fungus.—On the *rôle* of leucocytes in elimination, by M. Henry Stassano.—Glucoproteins as new culture media, of definite chemical composition, for the study of microbes, by M. Charles Lepierre. Nearly all microbes, whether pathogenic or not, grow perfectly in liquids in which the nitrogen is furnished exclusively by glucoproteins.—The structure and function of the nervous system of an acephaloid, by MM. N. Vaschide and C. Vurpas.—Acoustic conductivity and audition, by M. Pierre Bonnier.—On the intermittent spring at Vesse, near Vichy, by M. F. Parmentier. The action of this spring lasts for a period of an hour and takes place three times in 25-27 hours. The water is thrown to a height of 7-8 metres and is accompanied by a copious evolution of carbon dioxide; it has a temperature of 31°, and yields a solid residue of 5·354 grams per litre consisting chiefly of sodium carbonate.

NEW SOUTH WALES.

Royal Society, May 1.—Prof. Liversidge, president, in the chair.—Mr. H. C. Russell, C.M.G., F.R.S., was elected president for the current year.—Prof. Liversidge delivered an address, in the course of which he referred to the Intercolonial Catalogue of Scientific Literature. This work, he said, would annually fill seventeen volumes, and would contain from 160,000 to 200,000 entries yearly, and would prove an inestimable boon, as it would relieve scientific people from much of the trouble now attendant upon hunting up references to scientific subjects. He trusted that some effort would be made to collect and forward material from Australia for inclusion in this catalogue. He was also strongly in favour of a federation of the leading scientific societies in Australia and the establishment of a national Australian academy, and suggested that a site for such an academy, museums, art galleries, and a Federal University and other scientific and educational societies might be reserved in the capital of the Commonwealth. The organisation proposed would somewhat resemble the Continental academies so far as its scope was concerned, but under rules more like those of the Royal Society of London. If the proposal were carried out it would be of great benefit to Australia, not only in its general usefulness, but in the stimulus it would give to the younger scientific men, since election to it would depend upon fitness and merit. It would be very gratifying to all who were interested in the matter if, with the new century and the inauguration of the Commonwealth, there was increased attention paid to the question of instruction in science in the schools and better provision made in this direction, for it would be of great usefulness in training the power of observation of the children and teaching them to think about what they saw and heard. Some of the teaching now done at the University should be given in the schools, and the student would then gain valuable time at the University for things he could not do at school. He did not advocate the teaching of technical or applied sciences in ordinary schools. It was to be regretted that the Sydney University was probably the only modern University that excluded science from its entrance examinations. Prof. Liversidge also made some observations in connection with the advantages of a metric system of weights and measures and a decimal system of coinage. He strongly recommended that its teaching should be compulsory in all the schools of the State. The chief defect of our present system of weights and measures was that there was no simple connection between measures of length, weight and capacity. Investigation showed that in countries where the change to the metric system had been made, no great difficulty was experienced, and an increase of trade had resulted. He strongly urged that increased attention should be paid to commercial education and suggested that, not only should it include a certain amount of instruction in science, but that the standard for the higher branches should be as high as for any of the learned professions, also that part of the course should be given at the University.

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