THURSDAY, AUGUST 22, 1907.

APPLIED OPTICS.

A System of Applied Optics, being a Complete System of Formulae of the Second Order, and the Foundation of a Complete System of the Third Order, with examples of their Application. By H. Dennis Taylor. Pp. xvi+334, with 24 plates. (London: Macmillan and Co., Ltd., 1906.) Price 30*s*. net.

THAT branch of geometrical optics which deals with the properties of lenses and lens systems has unfortunately been shamefully neglected in England during recent years. This neglect has extended from the theory to its practical applications, and the design and construction of lenses has, to a great extent, been relegated to other countries, notably Germany; although in the time of Dollond, M. Anthéaume was obliged to send to England in order to obtain lenses to carry into practice the theory of achromatism devised by M. Clairaut. Before the publication of the book which forms the subject of this review, there was no work, in English, by the guidance of which an ordinary photographic lens could be worked out in all particulars; and the fact that there has been practically no demand for translations of the numerous books on applied optics which have been published in Germany, shows how completely the subject has been neglected. If we seek for a reason to explain this state of affairs, it is not far to seek. The books on geometrical optics which have appeared in England during recent years have, for the most part, been written by mathematicians who could boast little or no acquaintance with the practical design of lenses; and as the formulæ which can be obtained for the correction and elimination of errors in lenses do not possess that "elegance" which is dear to the heart of the pure mathematician, practically no progress has been made since the time of Airy and Coddington: almost the only modern work which exhibits originality of treatment is a small volume [1] by Mr. Blakesley, published in 1903.

In these circumstances we cannot feel too grateful to Mr. Dennis Taylor for the volume before us. In this, everything which could aid the student in mastering the subject in the easiest and most pleasant way has been done, and done well. The numerous diagrams (drawn to scale, or as nearly to scale as is practicable), which are included in the twenty-four plates, must alone have entailed many hours of tedious labour. The principles underlying each problem that is attacked and solved are clearly and fully explained, while the steps in the analysis which have been omitted can easily be supplied by a reader with very moderate mathematical acquirements. But the chief interest of the book lies in the fact that it is the outcome of a successful attempt to design lenses for practical purposes. Finding that the formulæ arrived at by Coddington, for the curvature of the image formed by a lens, were not quite satisfactory, an attempt was made to solve the problem by some method not dependent on the calculus; and after several disappointments, Mr. Taylor was successful.

In this investigation the formulæ relating to "coma" or "side flare" were arrived at; it is significant that there is scarcely a book in English which even mentions this defect of lenses—a defect which is often of greater importance than those due to spherical aberration or astigmatism. As a practical outcome of the investigation, the Cooke lens was designed; and, finally, Mr. Taylor has embodied his investigations in the book before us.

It is, perhaps, a pity that Mr. Taylor has not adopted the usual convention as regards the signs of the quantities u, v, and r. In the end, one convention (when mastered) is as good as another; therefore, it would appear that the most suitable one is that which is most generally adopted. The convention adopted by Mr. Taylor makes u positive or negative (for a particular position of the object), according as the lens is collective or dispersive, while the signs of v, and the radii of curvature are determined in a similar manner. There appears to be no advantage in this procedure that is not shared equally with the one generally in use; and the reader accustomed to the latter is likely to experience some unnecessary difficulty in following Mr. Taylor's reasoning. This is, however, a detail of no vital importance.

The first chapter of Mr. Taylor's book is devoted to a brief recapitulation of the ordinary formulæ, of the first order of approximation, applied to mirrors and lenses. In the second chapter the "theorem of elements" is explained: a thick lens is shown to be equivalent to two thin lenses (called "elements"), and a plane parallel sheet of glass. The theory of thick lenses, and lens combinations, is discussed in chapter iii.; this theory, as explained by Mr. Taylor, is much simpler than that usually given in books on geometrical optics, although a further simplification is possible and desirable. At this early stage the reader is brought into touch with practice by using the formulæ that have been evolved, to calculate the focal lengths of some well-known lens combinations, including the Cooke process lens. Such calculations occur at short intervals throughout the book, and add much to its value.

Spherical aberration is discussed in chapter iv. The ordinary formula to the second degree of approximation is obtained, and is simplified by the aid of a device due to Coddington; a formula to the third degree of approximation is also worked out, but the reader may omit the investigation leading to this on a first reading, as it is somewhat complicated, though presenting no difficulty other than those generally met with in dealing with unwieldy formulæ. A geometrical device [1] due to Mr. Blakesley, might have been mentioned here with advantage. It can be easily proved that if all rays proceeding from a point on the axis of a lens pass through the lens in such a manner that the deviation of each is a minimum, then the spherical aberration is a minimum also; and Mr.

[1] "Geometrical Optics." By T. H. Blakesley. (London: Whittaker and Co., 1903.)

[1] "Geometrical Optics." By T. H. Blakesley, pp. 94–111.

Blakesley has given a simple geometrical means of designing a thin lens to comply with these conditions. Of course, spherical aberration is not the only, or even the most important defect of a lens; but the simplicity of the geometrical construction leads one to wish that expert mathematicians would devote some attention to the subject to see whether graphical methods could not be used in other cases.

Central and eccentric oblique refractions are discussed in chapters v. and vi. respectively. Eccentric oblique refraction is answerable for the phenomenon of "coma" or "side flare," which is discussed in great detail in chapters viii. and viii*a*. It would be impossible, in the short space of an article such as the present, to deal with the author's treatment of this interesting subject in detail; it must suffice to say that it has now for the first time been brought within the reach of any reader possessing ordinary mathematical attainments who will devote the necessary time and attention to the subject. Some of Mr. Taylor's results are similar to those obtained by Von Siedel, but many are novel. The most important advance effected by Mr. Taylor is the investigation of the foci of oblique and eccentric pencils of large aperture.

The distortion of the image formed by a system of lenses is very fully investigated in chapter ix., where it is shown that Coddington's method is defective in not carrying the spherical aberration of the first lens through to all succeeding lenses, a considerable error being thus introduced. The distortion produced by several combinations of lenses is worked out numerically, and it is shown that, in the case of an eye-piece of a telescope or microscope, an image which is really distorted *may* appear to be undistorted, owing to a peculiarity of the eye. Achromatism is dealt with in chapter x. In reading this, and, indeed, most other chapters of the book, one cannot help being struck by the care with which the author has experimentally tested the results obtained, sometimes finding that an extension of the theory is necessary (see, for example, p. 309). A brief sketch of the errors of the third order is given in chapter xi.

On closing Mr. Taylor's book, we are left to reflect on the living interest which he has given to mathematical investigations, essentially of a somewhat clumsy nature. Throughout the book, theory and practice go hand in hand, and we feel that the labour of solving the complicated problems which arise is well worth the while, for something tangible and useful is gained in the end. It would be well if the examining bodies of the various universities were to attach greater importance to geometrical optics, *studied from an essentially practical point of view*. At present the startling discoveries which have been made during recent years in other branches of physics absorb so much attention that many students who sit for advanced examinations in physics are culpably ignorant as to even fundamental properties of lenses. Questions on geometrical optics are rarely set by examiners; and when they are, they are too often merely mathematical exercises. Since accurate experimenting so often involves the use of lenses and other optical appliances, this state of things is greatly to be regretted.

EDWIN EDSER.

A THEORY OF THE ÆTHER.

Æther: A Theory of the Nature of Æther and of its Place in the Universe. By Dr. Hugh Woods. Pp. xii+100. (London: *The Electrician* Printing and Publishing Co., Ltd., n.d.) Price 4*s*. 6*d*. net.

THIS book is a more elaborated presentation of the views as to the nature of æther set forth by the author in a pamphlet published in 1898. The æther is "regarded as possessing properties such as might justify its being described as a gaseous fluid, composed of atoms almost indefinitely small as compared with recognised chemical atoms." Again, "Æther is a fluid whose ultimate particles, or atoms, are so small that they pass into the minute crevices of spaces in the most solid bodies." This view has much in common with some of the older theories of the æther, and is almost identical with that proposed by Mendeléeff in his tract, "An Attempt towards a Chemical Conception of the Æther" (1902), and which is referred to by the author in support of his views. No attempt is made to overcome the objection first urged by Maxwell to any theory as to the nature of the æther which postulates a discrete structure for it—that all the energy of the universe would have been transferred to it—and the same objection applies even if the æther is regarded as a limiting case of a medium possessing such a structure.

The theory proposed by the author cannot, therefore, be accepted as an ultimate theory of the æther. There remains the question whether this idea of the æther affords a satisfactory working model which could be used to give a concrete representation of physical and chemical phenomena, and enable their course to be definitely followed. The theory is applied to a wide range of phenomena, including gravitation, chemical changes and reactions, heat, light, electricity and magnetism. Many of the explanations that are claimed as consequences of the particular theory would follow from any theory of the æther that assigns to it the fundamental properties of a moving system. The reasoning is in general vague, and the argument is never pushed far enough to enable a quantitative comparison to be made. A few examples will suffice to show the character of the reasoning. On p. 3 the following argument is given:—

"The solar system appears to move through space, borne along in an enormous volume of swiftly flowing æther. Now the resistance offered to the free flow of the æther by the partially impervious bodies floating in it is evidently greatest in the line of greatest thickness of each body, and less as the thickness becomes diminished. Accordingly a difference of momentum is thereby caused in the mass of æther, dashing against the body, and there results a current in the æther from places of higher momentum to places where the momentum is lower, with the effect that a whirl, such as occurs in the air under similar circumstances, is produced. These whirls, then, by their continual action, make the bodies more or less spherical, and set them rotating, each on its largest axis, while the whirls, spreading out in ever widening circles, influence the movements of other bodies floating in the same medium." "In this way, the movements and mutual influence of the heavenly bodies may be explained, in a perfectly rational manner, and without imagining any occult power of attraction."

The reasoning here appears to be scarcely conclusive; there is some unexpressed assumption as to the nature of the action between æther and matter; and that whirls (by which some kind of vortex motion appears to be meant) would necessarily result wants demonstration. These whirls have, at a later stage in the book, to do duty in explaining terrestrial magnetism as well as the relations of the heavenly bodies and their gravitational attraction. Again, p. 11:—

"It has long ago been proved that in æther all bodies fall with equal rapidity towards the centre of the earth, and it may, therefore, be reasonably assumed that all atoms which displace equal amounts of æther have equal weight. There are, however, many and convincing reasons for believing that the atoms of different chemical elements have widely different weights." "The explanation, then, which suggests itself as accounting for this difference, according to the present theory, is the very simple one that the heavier atom is of larger bulk, and displaces more æther than the smaller atom. From this it follows that the sizes of chemical atoms are in the same ratio as their weights."

From this, Gay-Lussac's law and Avogadro's law are derived. Boyle's law and the deviations from it are treated much in the same fashion, and the author then finds it necessary to introduce another factor (p. 15), the *shape* of the molecules. A table of the chemical elements arranged with their atomic weights in ascending order of magnitude (the character—gas, liquid or solid—of each being stated) is given. Arguing from this table, the statement is made:—

"It must hence be admitted that elements with a low atomic weight are much more disposed to be gaseous than those of higher atomic weight, at ordinary temperature and pressure. This quite accords with the theory that their ultimate particles are smaller than those of elements with higher atomic weights."

The difficulty that there are so many solid elements of low atomic weight is got over by invoking the influence of shape. For example, the liquidity of mercury is explained by supposing the atom of mercury to be spherical. A curious reader might wish to know the approximate shapes of the atoms of argon or lithium, but on this point the author is silent. After some pages of the same kind of reasoning, two laws are enunciated: —

(1) "The condition of chemical elements or of chemical compounds, at similar temperature and pressure and under similar conditions generally, depends on their atomic or molecular weights (that is, on the size of their atoms or molecules) and on the shape of their atoms or molecules." (2) "The relative chemical activity and chemical properties of chemical elements or chemical compounds, at similar temperature and pressure and under similar conditions generally, depends on their atomic or molecular weights and on the shape of their atoms or molecules."

The term law appears to be used here in a somewhat unusual sense, as these statements do not constitute laws; to make them such, the laws of dependence should be known. Another good example of the author's mode of reasoning is to be found on pp. 53, 54, where the fact that glass is transparent to light but opaque to heat is explained by the peculiarities of the interstices filled with æther in the case of glass, the nature of these being inferred from the way in which glass fractures.

It will appear from these examples of the author's treatment that his theory cannot even make good a claim to be considered a reasonable working model. A great number, however, of the better known physical and chemical phenomena are brought together, and on this account the book may perhaps prove interesting to readers who have not sufficient leisure or inclination for the perusal of treatises and memoirs that have greater pretensions to scientific accuracy.

FOUNDRY PRACTICE.

General Foundry Practice. By A. McWilliam and P. Longmuir. Pp. vii+383. (London: Charles Griffin and Co., Ltd., 1907.) Price 15*s*. net.

THE opinion is generally held among metallurgists that with the rapid progress made of recent years in Great Britain in the metallurgy of iron the foundry has hardly kept pace. Mr. McWilliam and Mr. Longmuir take a more optimistic view, and believe that advances have been, and are being, made of a magnitude commensurate with those of other industries. Certainly signs of progress are apparent in this important branch of metallurgy. The empirical method of charging the cupola is giving place to the system of weighing all materials in proportions determined by the chemist. High-temperature measurement is being practised in the core and drying stove. The field for machine moulding is extending. Permanent moulds made of carbon or similar material are being tried; and the founder is just realising the fact that micrographic analysis has a commercial value. In short, in all branches of his work he is showing a praiseworthy desire to emerge from the slipshod ways of the past. The literature of the subject has, however, remained meagre, and not of a strikingly scientific character. Scattered through the pages of the Journal of the Iron and Steel Institute and of the iron trade journals there is much information of permanent value; but the special treatises on the subject are mostly of an elementary character. The exhaustive work by Mr. McWilliam and Mr. Longmuir may therefore fairly be regarded as marking an epoch in the history of iron founding, and should help greatly in effecting a clear understanding of the subject. The authors possess special qualifications for the work they have undertaken. Mr. Longmuir has held the position of foundry foreman, and is a Carnegie research medallist of the Iron and Steel Institute, while Mr. McWilliam, a distinguished Associate of the Royal School of Mines, has at the University of Sheffield had ample opportunity of ascertaining the needs of students. They have therefore been able to draw upon experience gained under normal foundry conditions and under the conditions of experimental laboratories, and the operations they describe have been personally followed.

The subject-matter is dealt with under the following heads:—General properties of matter; moulding sands; facing sands; foundry tools; moulding-boxes; handling material in the foundry; open sand moulding; cores; elementary aspects of moulding; green-sand moulding; securing cores in moulds; moulding from guides; bench, outside, and plate moulding; machine moulding; dry sand moulding; loam moulding; chill casting; casting on to other metals; weighing and binding moulds; shrinkage, contraction, and warping; dressing castings; common faults due to mould and pattern; wrought iron; cast iron; refractory materials; fuels and furnaces; mixing by analysis; further treatment of cast iron; high-temperature measurement; steel; metals other than iron; alloys; mechanical testing; micrographic analysis; common faults due to the metal; and foundry management. The illustrations, of which there are 246, have been carefully chosen, and, like the letterpress, are exceptionally well printed. From this enumeration of the contents it will be seen that, although bearing the comprehensive title of "General Foundry Practice," the work is almost entirely devoted to iron and steel founding. Metals other than iron are disposed of in some twenty pages, brass founding receiving chief attention. The ingenious *cire perdue* process of bronze casting is not mentioned, nor is the modern method of casting in sections bronze statuary of heroic size, such as Bartholdi's "Liberty" at New York and Schwanthaler's "Bavaria" at Munich. The plaster moulds used for this purpose might have been added to the green sand, dry sand, loam, and chilled moulds described by the authors. A few lines, too, might have been devoted to the moulds used for metals with low melting points, brass, slate (for toy soldiers), wood, and even paper (for stereotype plates) being employed.

As the eye of the metallurgist wanders up and down the authors' well-filled pages, it will be arrested by that section which deals with the influence of the various impurities in cast iron. Phosphorus, it is shown, increases the fluidity of cast iron and renders the metal suitable for art castings, such as those for which the Russian works at Kyschtym are famous. Sulphur tends to make castings harder and brittle. Silicon, by tending to throw the carbon out of the combined form and to make it appear in the metal as graphite, has a beneficent influence. Manganese, on the other hand, has a tendency to keep the carbon in the combined form. These facts have to be borne in mind in mixing by analysis, a method which, it is gratifying to find, is steadily replacing mixing by fracture, by guess-work, or by trial. As the underlying science of the founder's art becomes more and more clear, well-marshalled knowledge is increasingly helpful. As the authors point out, the real theoretical knowledge of the man of science is built on experiment, and his theories are tested by further experiment. The practical man constantly meets with difficulties in his work; and he also must, in a truly scientific way, devise a remedy by testing the results of his former experience. The apparatus may be cruder than that of the laboratory, but it will be used with a subtle judgment of the needs of the case. The man who combines a scientific training with practical experience is gradually, but surely, becoming the dominant type of industrial captain in the best equipped foundries.

The book is unusually free from typographical and other errors, and there is little in the arrangement of the material to which exception can be taken. It might perhaps have been well to have carried the subject a stage further, and to have given the student some information regarding the galvanising, nickel-plating, lacquering, and porcelain enamelling of castings, and regarding the repairing of faulty castings by melting in iron by means of the electric arc or the oxyhydrogen blowpipe. Pattern making is altogether ignored. It is true that it is a distinct trade involving the skill of the joiner and the turner. A practical founder should nevertheless have a general knowledge of the construction of foundry patterns; and the elaborate patterns, sharply chased in a tin-lead alloy, used for ornamental castings present many features of interest to the foundry managers and foremen for whom the work is primarily intended.

OUR BOOK SHELF.

Eversley Gardens and Others. By Miss R. G. Kingsley. Pp. x+280. (London: George Allen, 1907.) Price 6s. net.

It is always stimulating to meet with enthusiasm, and Miss Kingsley is not only an enthusiast with regard to individual plants, but possesses a keen eye for their artistic setting and arrangement. Eversley is situated on the Bagshot beds in a part of Hampshire that has received the *sobriquet* of "the rubbish-heap of the world"; and as much of Miss Kingsley's experience was gained in laying out and cultivating the garden of Keys House, in Eversley, her success may serve as a help to other amateurs whose energies are also concentrated on poor soil.

It would appear that roses have been Miss Kingsley's chief delight, especially the teas, hybrid teas, and climbers. She presents a lengthy choice, arranged in colour groups, containing besides such universal favourites as G. Narbonnand, Frau Karl Druschki, and Caroline Testout, others less generally known, as Madame Ravary, Coquette de Lyon, and Monsieur Trillier. The list of rhododendrons, a plant that finds a congenial home on the Bagshot soil, is especially noteworthy, and the plan of growing bulbs in peat fibre in bowls is recommended as a clean and rapid method for producing fine flowers. While it is probable that most gardeners will find many hints and references to species unknown to them, it is certain that all can learn much from the artistic combinations described by the author, some produced in her own garden, others in her friends' gardens.

The Friendly Stars. By Martha Evans Martin. With introductory note by Prof. Harold Jacoby. Pp. ix+253; illustrated. (London and New York: Harper and Brothers, 1907.) Price 5s. net.

EVIDENTLY written by one who for years has been in the habit of looking upon the stars as companions, rather than as conglomerations of known and unknown elements, this volume will appeal to the beginner in astronomy and to the general reader quite as much as to the astronomer.

After dealing generally with the apparent movements and with the brightest stars, the author proceeds to thirteen chapters containing causeries on particular stars, describing the relative position of each, its seasonal apparitions, its diurnal path, and its colour, &c., adding a few words as to the distance and physical conditions of each star.

More general problems are then discussed, such as the numbers, distances, and light of the stars. A brief chapter on double stars gives an excellent first idea of multiple systems, and is followed by nine chapters dealing with the constellations, frequent diagrams illustrating the text. By those who simply wish to recognise the individuals of the starry host, and to be *au fait* with sufficient characteristics of each to mark its individuality, the volume will be found a useful companion. W. E. R.

On the Evolution of Wound-treatment during the Last Forty Years. By Sir Hector C. Cameron, Professor of Clinical Surgery in the University of Glasgow. Pp. 96. (Glasgow: James MacLehose and Sons, 1907.)

THE appearance of this book at the present time is opportune, for the lectures deal very largely with Lord Lister's researches on antiseptic treatment, of which they form a brief history. Lister's treatment was founded on Pasteur's demonstrations and writings, and no man ever acknowledged an indebtedness more often and more unequivocally. At the commencement of the first lecture the procedure adopted in 1860, or thereabouts by Mr. Syme, the period immediately preceding the introduction of antiseptics, is detailed. In 1868 or so Lister's first method of treating wounds antiseptically was being tested by its author. This consisted in swabbing the wound with undiluted carbolic acid (a crude and impure preparation at that time) and covering it with lint saturated with the same substance, over which a piece of sheet-lead was placed; each day the lead was removed and the lint painted over with the carbolic. By such treatment, crude and simple as it may now appear, it was abundantly demonstrated that wounds, even the dreaded compound fracture, would heal without suppuration. Subsequently, a putty consisting of whiting and carbolic acid was used, and step by step carbolic gauze, corrosive sublimate, mercuric iodide, and the cyanide gauzes were evolved. The author holds that no mere dressing with dry sterilised wool or gauze, apart from germicidal solutions, will suffice to prevent suppuration in dirty wounds, and with this pronouncement many will cordially agree.

Vortex Philosophy: or the Geometry of Science Diagrammatically Illustrated. By C. S. Wake. Pp. 36. (Chicago: Published by the Author, 1907.)

"As negation is the expression of energy and position is the expression of force, the elements of undulation, which is the dynamic aspect of the molar energy light, are expansion in the atomic field and ionisation in the molecular field; and the elements of spiralisation, which is the dynamic aspect of molar force (gravitation), are contraction in the atomic field and convergence in the molecular field" (p. 20). This extract shows that Mr. Wake has an extensive vocabulary, and a fund of unconscious humour. His pamphlet has no scientific value, but is amusing in its way as an attempt to classify all human knowledge on principles ostensibly scientific and logical, but really vague and æsthetic. Even from this point of view the coloured diagrams vii. and xii. are unsatisfactory.

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.*]

The August Draconids—Perseid Fireballs.

IMMEDIATELY after coming out to watch the northern sky on August 15 at 9h. 23m., I observed a second-magnitude meteor appearing stationary at the point $288° + 61°$, near *o* Draconis. Four other meteors were seen directed from precisely the same position during a watch of forty minutes later on the same night, but clouds came over before 10h. 30m., and though the sky cleared at a later hour I did not look out again.

This radiant point in Draco is nearly identical with that of a brilliant shower of fifty-six slow-trained meteors which I observed on 1879 August 21 to 25. I also recorded it in several other years, but it was very feebly represented. It appears to be visible both in July and August, but though I have often eagerly awaited it, the striking activity it displayed in 1879 has never been repeated. Possibly this year it may have returned more richly than usual, and I trust other observers recorded it while watching the later stages of the Perseid shower.

The following are the positions I have obtained for the Draconic radiant in past years:—

(1)	1888, July	8–13	... 290°+59° ...	4	Slow.
(2)	1885, ,,	9–13	... 290+60 ...	5	Slow.
(3)	1900, ,,	24–30	... 291+59 ...	6	Rather slow.
(4)	1899, Aug.	12–16	... 293+60 ...	8	Medium.
(5)	1901, ,,	15	... 290+60 ...	3	Rather swift, suspected.
(6)	1900, ,,	16–18	... 291+60 ...	5	Slow.
(7)	1887, ,,	20–24	... 289+60 ...	5	,,
(8)	1879, ,,	21–25	... 291+60 ...	56	,,

Of all the meteors seen during the recent Perseid display, one of the finest appeared on August 12 about 11h. 12m. It was recorded at Bristol and at Stockport, and at the latter place Mr. J. P. Kenyon estimated that it burst out so brilliantly as to give a flash equal to the light of the full moon.

The object fell from a height of seventy-nine miles over Donington, Lincolnshire, to forty-four miles over Market Harborough, Leicestershire. Its length of visible course was fifty-two miles, and observed velocity forty-one miles per second. Other observers probably noticed this fine meteor, and I will be glad to receive further descriptions of its apparent path with a view to determine its real course more accurately.

There was another magnificent Perseid which gave a flash like lightning as it descended in the Milky Way north of Aquila, on August 13 at 14h. 10m., but the only account of this is one from Mr. W. Lucking, of Manuden, near Bishop's Stortford. W. F. DENNING.

Bishopston, Bristol, August 19.

The Heating of a Balloon Wire by Lightning.

THE following account of the heating of a balloon wire by a lightning discharge is interesting as furnishing approximate limits for the energy of the discharge.

Report by Mr. S. F. Cody on Striking of a Balloon by Lightning on July 22, 1907.

"At about 11 a.m. on Monday, July 22, a captive balloon, carrying meteorological instruments, was in the air. Some 4500 feet of 19 S.W.G. tin-plated piano wire was between the balloon and the winch. The balloon was probably about 3500 feet high. The winch was exceedingly well earthed, standing on a large solid iron plate, which was also buried about $1\frac{1}{2}$ feet in the earth.

"I was trying to locate the balloon, which was hidden by clouds at the time, when a flash of lightning came crossing horizontally, and then a quick stroke to the earth.

"Being unlike anything I had ever seen before, I set about inquiries to find out whether the wire had been struck or not. No smoke appeared from the flash. There was a deep hissing noise, followed by the natural crash of the thunder some seconds afterwards.

"The hissing noise was probably the wire falling, as it was in many cases driven 2 or 3 inches into the earth. I was about 180 yards from the winch in the same direction as the wire, and about forty yards from directly under the wire to the left, looking from the winch.

"The balloon went away free, carrying the instruments, and was watched for several minutes, as it happened to pass through a clear portion of the sky where no clouds were at the time. It has not since been recovered.

"On investigation of the wire I found that near the winch, say about 250 yards from it, the wire became less tempered; in fact, it would stand bending quite well in the portions found near the winch. It was fused right off at the first wheel of the winch, and was undamaged at the drum of the winch.

"The weather was sultry; there had been some rain. The wind was light but squally, W.N.W. There had been no lightning previously.

"The following are some observations taken on the ground about an hour before the wire was struck:—

Dry bulb	18° C.
Wet bulb	15°·8 C.
Barometer	29·85

Wind, N.W. to W.N.W.

"Specimens of the wire enclosed."

It appears from the specimens of wire that the heat developed was sufficient to melt the tin but not to fuse the wire. If then we assume that the process is too rapid for loss of heat by conduction, we get for the limits between which the heat energy must lie $6 \times 1400 \times 200 \times 0{\cdot}11$ and $6 \times 1400 \times 1300 \times 0{\cdot}18$ in gram degree units, the wire weighing about 6 grams per metre, and the mean specific heat of steel from 0° C. to 1300° C. being taken to be 0·18.

In ergs the limits become $7{\cdot}12 \times 10^{12}$ and $8{\cdot}28 \times 10^{13}$, or, assuming the height of the cloud to be 100 metres, the energy was sufficient to raise 73 kilograms, but not sufficient to raise 840 kilograms, to the cloud level.

The fact of the wire being less brittle in the lower portion points to a diminution of the energy developed, but no reasonable explanation of this is apparent unless it is due to an induction effect.

In the case of a similar discharge on April 11 of this year, the wire was completely fused from the balloon to the winch; the length of wire out was half a mile, and the height of the balloon 2000 feet. The balloon was in the clouds at the time. The discharge in this case also took place by a cross flash from cloud to balloon. An account of the occurrence is given in a paper by Colonel Capper, read before the Royal Meteorological Society in May.

E. GOLD.

Meteorological Office, 63 Victoria Street, London, S.W., August 19.

The Origin of the Domestic Striped Tabby Cat.

IN the Proceedings of the Zoological Society for February of this year I attempted to prove that English domestic cats are to be referred by their patterns to two distinct kinds, which were described as the blotched and striped tabbies; and in discussing the possible origin of these two cats, I set the blotched tabby aside as of unknown descent, and stated it as my conviction that the striped tabby was to be traced to the interbreeding of two well-known wild species, namely, the European wild cat (*Felis sylvestris*) and the so-called Egyptian or African wild cat (*Felis ocreata*). There were living at that time in the Zoological Gardens a male example of *F. sylvestris* from Scotland and a female example of *F. ocreata* from Uganda. The latter was captured as a kitten near Nairobi in March, 1906, and had never been put to a male. To test the truth of my belief that the progeny of these two species would resemble our domestic striped tabby, and also to discover if there was any foundation for the theory some authors had put forward previously that the blotched tabby was the result of such a cross, the two cats in question were placed in the same cage this summer. They took to one another at once, and last week the female produced a litter of kittens resembling in every respect a typical striped tabby such as may be seen any day in the streets of London.

R. I. POCOCK.

Zoological Society's Gardens, August 18.

A Fossil Tsetse-fly in Colorado.

AMONG the interesting materials obtained this year in the Miocene shales of Florissant, Colorado, is a large "biting" fly, with a remarkably long and strong proboscis, very well preserved. A very superficial examination was sufficient to show that it was no ordinary Tabanid or Muscid, and it at once occurred to me that it was a tsetse-fly. Having no specimen of the latter at hand, I turned to the admirable coloured figures in the second report of the Wellcome Laboratories at Khartoum, and, as was expected, it matched so nearly that it might well go in Glossina. There is a slight difference in the venation which may or may not be of generic value, but if the insect is not a Glossina it is at least closely allied. Curiously, it is not new, for it appears to be the species described by Scudder in 1892 as *Paloestrus oligocenus*, a supposed new genus of Œstridæ. The new specimen, practically complete, and with the mouth-parts, shows that it has nothing to do with Œstridæ, and anyone who will refer to Scudder's figure will see how closely the venation resembles that of Glossina.

The specimen obtained this year was found by Mr. George N. Rohwer, a member of our party from the University of Colorado. It is an obvious suggestion, following some remarks lately published by Prof. Osborn, that the existence of such flies may have had something to do with the extinction of some of the Tertiary Mammalia of America.

T. D. A. COCKERELL.

University of Colorado.

PHYSICS AND CHEMISTRY.

"ONE of the penalties of devotion to a progressive science is the constant feeling of being left behind." So says the president of Section B, in his address from the chair, at the recent meeting of the British Association; and although he adds that he does "not think there is any occasion for panic," yet the concluding portion of his address seems to indicate that something approaching fear accompanies the impression that the progress of science at the present time is almost too rapid. There were some other indications, also, at the meeting, that physics at the present time is mistrusted by some chemists, to an extent perhaps beyond ordinary and necessary caution.

With a great part of the address agreement is easy. The plea that chemists should continue chemists, and that accurate manipulation and careful experimenting should be strenuously cultivated, is so reasonable as to be almost trite: for who would have it otherwise? That an atomic theory of matter, which has proved so useful in the past, should be adhered to as a guide in the future is also a natural desire against which no physicist has a word to say. Indeed, very much the contrary: the atom of matter is as useful a conception as ever, and has become even more real and concrete owing to the actual counting and measuring of individual atoms by physicists. But that physicists and mathematicians should leave the atom alone, and refrain from discomposing examination into its probable internal structure, should cease to break it up and otherwise modify it by appropriate agencies, and should turn a blind eye to any spontaneous explosions of energy whenever they have the bad taste to occur; also that no element shall be discovered and named which has zero chemical affinity, or which cannot be obtained in weighable amounts—all that is surely more than Section B has any right to expect, nor do I suppose that it seriously makes such a demand.

But the worst of a pessimistic outcry about the over-rapid development of science is that it is taken up by the general public, one section of which is always hoping that what is unintelligible is really meaningless, and may be safely ridiculed or ignored. So it has happened that a strange sentence in Prof. Smithells's address, which I will quote directly, is made the text for a singular attack by a leading article writer in the *Times* of August 6 apparently against the Cambridge school of mathematical and experimental physicists, which for a long time now has been in the eyes of the world one of the scientific glories of this island. The quotation is as follows:—

"There is never more cause for anxiety than when we see a mathematical theory awaiting the delivery of the confirmatory facts; and there is nothing more important for chemistry than the continual recruiting of that old guard which will be ever ready to stand to arms on the appearance of an eager theorist."

Now it is an old and recognised tradition that mathematical prediction of a fact to be subsequently confirmed by experiment is the highest achievement of science. The clearer the prediction, and the more rigorous the subsequent verification, the greater should be the joy among all those who wish for the advancement of natural knowledge.

That the theory should be completely intelligible to those comparatively ignorant of mathematics, until the fact has been arrived at experimentally, is not to be expected; and that a few should suppose that the prediction is only really forthcoming after the event—which is when they first hear of it—is also not unnatural. But the preparation of a theoretical niche for a fact, either just discovered or just on the verge of being discovered, is a piece of work involving the highest faculty of scientific insight; and it is to be hoped that the public are not going to be misled into a depreciation of the work of all except those who, very rightly, collect an assemblage of facts.

There is room for workers of all kinds towards the progress of science, and the encouragement and countenance of the public is one of the conditions; for often enough the difficulties of the work itself are more than discouraging, and if uncertainty as to its reception or appreciation by the contemporary human race is to be added, then it is to be feared that the discouragement may in some cases become complete. Such a catastrophe actually happened in the case of Thomas Young; but it was not the outcome of a meeting of the British Association for the Advancement of Science.

Probably the real intention of the president of Section B was to caution certain physical chemists, and perhaps to restrain or rebuke some of the Ostwald school in his own section; a matter well within his jurisdiction. Indeed, if he only wishes to express dislike at an attempted replacement of ordinary dynamics by a vaguer "energetics," he will find sympathisers among the physicists; as witness the following quotation from an article by the late G. F. FitzGerald in NATURE for March 12, 1896 (reproduced in his collection, "Scientific Writings," p. 387), with reference to an article by Prof. Ostwald called "Emancipation from Scientific Materialism." I must add, however, that FitzGerald was a keen admirer of the work of Prof. Ostwald in general, though in this particular doctrine, especially on its negative side, he did not consider that he was on a hopeful path. The quotation is as follows:—

"There are so many vague fallacies underlying it, that it would hardly be worth answering, only that there is considerable risk that others, chemists especially, may be carried away by the arguments of one whom they rightly value as a leader in their own domain, when he descants positively about the realm of mechanics."

For the present purpose I need not enter upon a discussion of this matter: there is doubtless much that can be said on both sides. If the president of Section B had so expressed himself as to drive home this kind of caution among the members of his own section, without appearing to refer to better known and more immediately prominent subjects of debate, I should have said no word; and I desire it to be clearly understood that I am not now expressing any opinion on this subject. But, unfortunately, that is not how his address has been regarded from outside, nor is it the interpretation to which certain phrases, such as "chemistry of phantoms," "exuberance of mathematical speculation," readily lend themselves. It is in the hope that damaging misunderstanding may be avoided that this article is written.

OLIVER LODGE.

A TRIAD OF SPORTING BOOKS.[1]

TO the author of the volume standing first on the list given below, the wilderness from time to time calls with such persistence and force that to hear is practically to obey; and, whether to shoot wild goats in the Taurus, to collect vultures' and eagles' eggs in Asia Minor or Spain, or to track the lordly moose and the branching-antlered caribou in the wilds of the far North-West, Mr. Selous returns year after year with unabated zest to the roving life of his earlier South African days. That the public benefits from this restless disposition can scarcely be denied, for although he cannot be credited with anything special in the matter of literary style, the author of "Recent Hunting Trips" writes with that freshness and *verve* that almost transports the reader to the very scenes of his adventures and triumphs. Nor is this all, for Mr. Selous is essentially of a generous nature, and it is but seldom that he returns from one of his sporting trips without some important addition accruing to the national museum.

In the volume now before us, the author gives an account of his experiences during several shooting trips to British North America, undertaken between the years 1900 and 1906 (inclusive) in search of moose, caribou, and wild sheep; these, which include two visits to Newfoundland, comprising the whole of his hunting in this portion of the New World. In the preface, Mr. Selous records his opinion with regard to the closure to the sportsman of the central districts of American Alaska—an opinion worth quoting, as it has a bearing on so-called game-preservation in other parts of the world. Although the sportsman, who would be content with a few good trophies of male animals to add to his collection, is completely shut out, the game is by no means protected. The Indians, for instance, armed with modern weapons, can apparently shoot as they will, and spare no animals of either sex or of whatever age which come in their way, while meat-hunters of European blood are no less destructive. Although the Indian doubtless has the justification that he shoots, in part, at any rate, for his own maintenance, yet it is he and his white fellow-countrymen who, in the author's opinion, will ultimately bring about the extermination of the game with which the land now abounds, unless the whole system of game legislature is altered, and that speedily.

[1] "Recent Hunting Trips in British North America." By F. C. Selous. Pp. 400; illustrated. (London: Witherby and Co., 1907.) Price 16s. net.
"Game and Game Coverts." By John Simpson. Pp. 83; 16 plates. (Sheffield: Pawson and Brailsford.) London: County Gentlemen's Association, Ltd., 1907.) Price 15s.
"How to Fish: a Treatise on Trout and Trout-fishing." By W. E. Hodgson. Pp. xii+377. (London: A. and C. Black, 1907.) Price 3s. 6d. net.

Although from the point of view of bodily stature the moose (or elk, as it is commonly called in England) is undoubtedly the finest animal in this part of the world, the various races of caribou (or reindeer) are calculated to attract the interest of naturalists to a special degree on account of the incredible numbers in which they occur, not only on the mainland, but also in the almost untrodden heart of Newfoundland; while their periodical migrations in certain districts are among the most wonderful phenomena in big-game life. Not the least marvellous feature in these "treks" is the manner in which whole herds sometimes swim in company, so as to form in certain cases, as described by a recent traveller in Labrador, a veritable living bridge. In other cases, however, they travel in small parties, or even in pairs, lying down to rest or pausing to feed as their inclinations prompt.

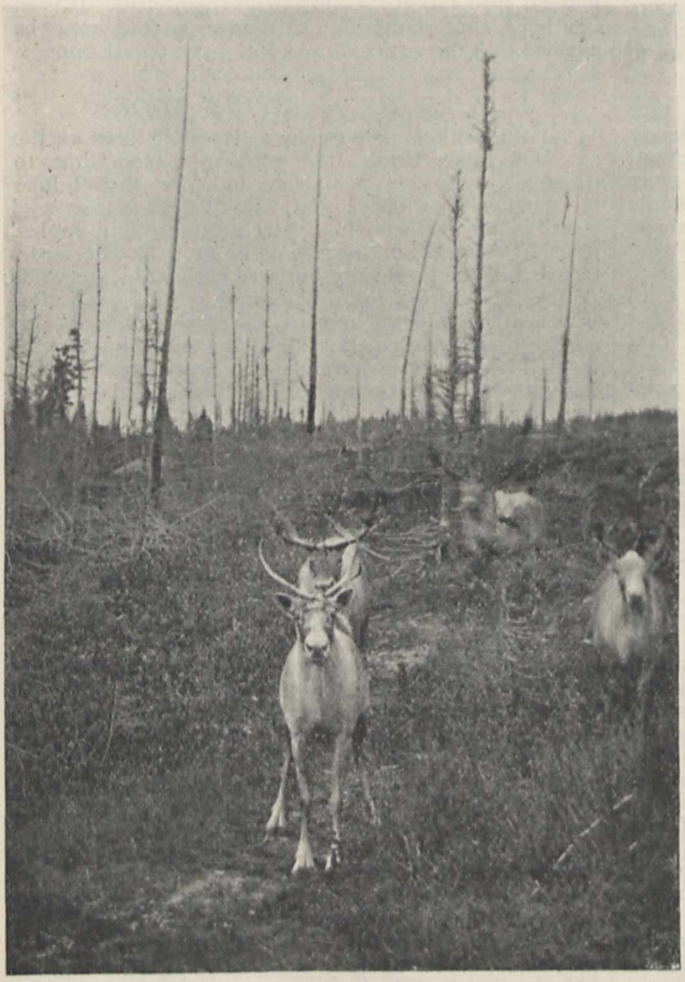

FIG. 1.—Caribou on migration. From "Recent Hunting Trips in British North America."

"On one occasion," writes the author, "herd after herd of caribou passed the end of the lake in full view from where we were sitting. These herds were all small, consisting of from three or four to ten animals. They were all following the same trail, and were evidently migrating from the north-east to the south-west. Although they kept stopping to feed they travelled fast, often trotting as if alarmed."

With the assistance of various friends, the author has been able to illustrate his book with a number of striking photographs of migrating caribou, some showing the animals as they traverse the scrub in well-beaten tracks, and others their appearance when swimming lakes or rivers. As a permanent memento of the latest trip, reference may be made to the two magnificent caribou shot by the author and presented by him to the British Museum (Nat. Hist.), where they are now set up in the mammal gallery, one of these representing the large dark-coloured *Rangifer tarandus osborni*, and the other the smaller and whiter *R. t. terrae-novae*. Special interest, it may be added, attaches to the mention (p. 73) of the manner in which the spreading feet of the caribou enable the animal to traverse boggy ground, where horse, ox, or ass would be helplessly mired. Lack of space prevents mention of a number of interesting points in this fascinating book of adventure and sport, but we must refer to the author's measurement of his finest bull moose (p. 215), the height of which is given as 6 feet 11 inches. Reference must also be made to an interesting opinion in connection with wild sheep, namely, that the white *Ovis dalli* probably grades into the grey *O. fannini*, and the latter into the black *O. stonei*. The view that these sheep are but local races of the Kamchatcan *O. nivicola* is supported; the true *O. canadensis*, like the true grizzly bear, having departed further from the northern type owing to its having travelled further south, and perhaps having entered the country at an earlier date than the others.

The second (like the third) work on our list is entirely for the stay-at-home sportsman, and is intended to emphasise the importance of much greater care being exercised by game-preservers as to the culture of covert suitable for different kinds of game. Hitherto the general practice has been to let coverts grow more or less as they will; but the author, who has had great practical experience of the subject, shows that this is altogether wrong. Not only is one kind of tree or bush specially suited to particular species of game, but care is needed in order that such trees or bushes may have proper opportunities for full development. A case in point is afforded by the blackberry bramble, which needs open space and sun, when it affords not only excellent covert, but also a valuable food-supply. In this connection it may be mentioned that in Mr. Simpson's opinion gamekeepers err in over-feeding their charges; which, to say nothing of economical considerations, would be far better in many ways if left to get their own living in properly planted coverts. Game-preservation, according to the author, is likely to become more and more profitable to English landlords; and special attention is directed to the economic value of rabbits on estates. The special feature of the book, which should be in the library of every landowner and game-preserver, is the beauty of the illustrations of different kinds of covert, when properly developed; the most exquisite of all being the photograph of a mound of blackberry bramble in fruit.

Sometimes we venture to think that authors do not select sufficiently comprehensive titles, but in the case of Mr. W. E. Hodgson's book, standing third on our list, the main title seems to err in the opposite direction. For in place of instructing the angler in the art of alluring river-fish of all kinds, this volume, as, indeed, is indicated in its supplementary title, tells him only how to capture the wily trout. Since, however, this is, *par excellence*, the sporting fish of English rivers, there may be some justification for the designation. The author has already published a more ambitious work on trout-fishing, which has, we believe, been well received by anglers; but that volume is intended mainly for the benefit of those who are already experts in the gentle art, whereas in the one now before us it is sought to instruct the beginner in the elementary principles of trout-fishing.

Mr. Hodgson is evidently one of those who believe that salvation is to be found otherwise than by "dry-fly" fishing; and a considerable portion of his work is accordingly devoted to other methods, inclusive of spinning with minnows, and luring with the luscious wasp-grub. That the author will not please every angler in all details may be regarded as a matter of course; but, speaking generally, he seems to have treated his subject in a manner which ought to satisfy those who are making their first essays at trout-fishing. The book is well illustrated, and likewise contains a number of observations on the natural history of the subject, and, indeed, on nature-study generally. R. L.

GENETICS.[1]

THE last contribution to the fast-increasing pile of Mendelian literature is unique. It is at once the bulkiest, within the limits of two covers, that has been made to this subject, and at the same time the most condensed, the most varied, and the most valuable.

The third International Conference on Genetics, held under the auspices of the Royal Horticultural Society, and under the presidency of Mr. Bateson, was a veritable Mendelian orgie. The history of all new theories is the same. They are judged not so much on their own merits as on the number and variety of natural processes, previously unintelligible, which they explain. The result of the publication of the "Origin of Species" was, as Mr. Bateson has pointed out, the distraction of the attention of biologists from the process of evolution itself and its diversion into the hitherto dry channels of palæontology, classification, embryology, comparative anatomy, and distribution. It was not until the end of the nineteenth century that men returned to the study of evolution. The relation between man and a new theory is the same as that between a child and a new toy. When we first get the toy we are occupied in playing with it in every possible way, and as often and as much as we can. But when all legitimate sources of interest have been tapped, we tire of playing with the toy and begin to wonder how it works; and, to satisfy our curiosity, we pull it to pieces. The result of the attempt to satisfy this curiosity in the case of Darwin's theory was the growth of a conviction that natural selection did not provide a sufficient explanation of the diversity of organic forms. The history of Mendelism has been like that of Darwinism. The flood of energy let loose by the re-discovery of Mendel's papers has spent itself rather in work based on the assumption that the interpretation which Mendel put on the facts he discovered was true than in the attempt to discover whether that interpretation were true or not; and in our opinion it is right that this should be so. The merely critical spirit is a barren one. The enthusiasm of the kind which follows the birth of a new theory such as Darwin's or Mendel's has been as productive of discovery in the case of the latter as it was in that of the former. At the same time, we should not forget that Mendelism is now in the stage in which Darwinism was before it was subjected to the process of being overhauled; and though we may perhaps be right in holding that criticism is barren of discovery, we should guard against the possibility of entering that frame of mind which regards criticism as blasphemy. Mendel's peas have already been called classical; and it is a very remarkable fact that no one has repeated Mendel's experiments with the deliberate intention of testing the Mendelian interpretation of the results. People speak as if Mendel got to the bottom of the inheritance of roundness and wrinkledness, yellowness and greenness, and as if there was nothing more to be said on the subject. On p. 88 of the report before us there is a table exhibiting the result of crossing a yellow with a green pea to the fifth generation. The proportion of pure yellows, impure yellows and greens is given both for the fourth and for the fifth generation as 1 : 2 : 1, and it is stated on the bottom of p. 88 that this process of segregation will be continued "practically for ever." It is highly probable that the three categories do form respectively 25, 50, and 25 per cent. of generations four and five; but Mendel never published any figures which prove this to be so. All he said was: "The proportions in which the descendants of the hybrids develop and split up in the first and second generations presumably hold good for all subsequent progeny. Experiments one and two have already been carried through six generations, three and seven through five, and four, five, and six through four, these experiments being continued from the third generation with a small number of plants, and no departure from the rule has been perceptible."[1]

We offer no apology for adopting this critical attitude towards Mendelism. There is plenty of admiration for "Mendel's incomparable achievement," and we share it; but we do not find it impossible to combine it with a suspicion that Mendel's interpretation of his results may not have been right after all.

The report is, of course, absolutely indispensable to every student of genetics, whether his interest is purely scientific or purely horticultural, or both. The keynote of the conference was struck by a pealing of the marriage bells of Science and Practice. We could have no better guarantee that their union will be fertile than that their hands were joined by the Rev. W. Wilks, who has earned the gratitude of every study of heredity by editing this report, and of every lover of flowers by creating the Shirley poppy.

NOTES.

PROF. H. LE CHATELIER has been officially nominated professor of chemistry at the Paris Faculty of Sciences in succession to the late Prof. Henri Moissan.

IT has been decided by the Paris Municipal Council to perpetuate the memory of Prof. Berthelot by re-naming the Place du Collége de France the Place Marcelin Berthelot.

WE regret to have to record that Prof. Karl Vogel, director of the Astrophysical Observatory at Potsdam, died on August 13.

WE regret to have to announce the death of the Rev. Dr. John Kerr, F.R.S., formerly lecturer on mathematics in the Glasgow Free Church Training College.

[1] Report of the Third International Conference, 1906, on Genetcs. Edited by Rev. W. Wilks. Pp 486. (Printed for the Royal Horticultural Society by Spottiswoode and Co., Ltd., n.d.) Price 15s.

[1] This is Bateson's translation, Mendel's "Principles," p. 57. The original may be consulted, most accessibly, at p. 16 of No. 121 of Ostwald's *Klassiker der exakten Wissenschaften*, Versuche über Pflanzenhybriden Price 1 mark.

THE death, from heart failure, of Sir William Robertson Copland took place at Glasgow on Monday last. Sir William Copland made a special study of drainage and of water supply, and took great interest in promoting technical and university education, being chairman of the governing body of the Glasgow and West of Scotland Technical College, and a member of the Glasgow University Court. He was knighted last year.

DR. WILLIAM THOMSON, who died at Philadelphia on August 3 at the age of seventy-four, had not only written largely on medical and surgical subjects, but had introduced several reforms in field service. At the battle of Antietam, in the American Civil War, he abandoned the old practice of bringing all the wounded into one hospital, and improvised a number of smaller hospitals in various parts of the field. The success of this innovation led to its adoption during the rest of the war, and later in the Franco-German War. Dr. Thomson will be further remembered for adopting the local application of carbolic acid as a disinfectant in the treatment of wounds, and for the introduction, in connection with the Pennsylvania Railroad, of the testing of engine-drivers for colour-blindness.

A REUTER telegram states that the instruments of the chief seismographical station at Hamburg registered several earthquake shocks in the afternoon of Saturday last. Slight shocks were recorded at about 1.22 p.m. and shortly after 2 p.m., while at 6.40 p.m. the instruments began to record a series of distant shocks of medium strength, which lasted nearly two hours. The disturbance reached its height shortly before 7 p.m., and ceased at about 8.45 p.m. It is estimated that the earthquake occurred at a spot 8000 kilometres south-east of Hamburg; a seismic shock was also recorded at Grenoble at 5h. 43m. 40s. on the same day.

IT is announced in *Science* that the yearly sum of 5000 dollars has been voted by the Minnesota Legislature towards the maintenance of a Pasteur institute at Minneapolis.

A CIRCULAR letter has been distributed by the president and general secretaries of the second International Congress on School Hygiene concerning the important new departure made by the congress in the matter of school hygiene. Arising out of the question of whether it would not be advisable to establish a bureau, with a permanent staff, library and museum, &c., in some central but neutral spot, such as a Swiss or Dutch town, it was decided that it would probably lead to greater progress if such bureau was not localised, but if each country had its own centre for the diffusion of knowledge, and to act as a clearing-house in the matter of school hygiene, statistics, laws, and regulations. Finally, to supervise in scientific matters and generally to do all that is possible at all times or places to forward the human interests which are bound up in the special lines of knowledge included in school hygiene, the International Committee has formed a small council, consisting of the president of the past congress, the president of the one lately held, the president of the next congress, and nine other members to be elected, which will have all the powers of an ordinary committee. The following questions will come under the consideration of the council almost immediately:—How medical inspection of schools can best be carried out with the maximum of efficiency and minimum of cost; how far the laws of health can best be imparted to the coming generation, so that later they will know how to care for themselves and those dependent on them; the best systems or methods of physical training for both sexes at various ages; and the feeding of children requiring proper nutrition, so that it shall be done without developing pauperism and with regard to those upon whom the cost falls.

IN reply to an inquiry put to him in the House of Commons as to whether, in view of the work already accomplished by the Liverpool School of Tropical Medicine in combating tropical diseases, he could arrange for an increased grant to be made in order that the work might be further extended, the Under Secretary for the Colonies said that a further grant will be made, of which the Secretary of State will be able to specify the amount after consultation with the Treasury.

THE Keith prize (consisting of a gold medal and 50*l.*) has been awarded by the council of the Royal Society, Edinburgh, to Dr. T. H. Bryce for his two papers on "The Histology of the Blood of the Larvæ of *Lepidosiren paradoxa*," published in the Transactions of the society.

THE seventh International Physiological Congress met in Heidelberg last week, with Prof. Kossel as president. About 300 members were present, and 200 communications were made in the four sections into which the congress was divided. At the opening meeting Prof. Kronecker paid a glowing tribute to the late Sir Michael Foster. Prof. Dastre, of Paris, gave a short biography of the late Sir J. Burdon-Sanderson, while Prof. Sherrington spoke of the loss sustained by the congress through the deaths of Prof. Errera, of Brussels, and Prof. A. Herzen, of Lausanne. By order of Grand Duke Friedrich of Baden each member of the congress was given a bronze medal in memory of the meeting. The medal bears on one side an impress of "Helmholtz—MDCCCLVIII–MDCCCLXXI."

THE French Congress of Medicine will be held in Paris under the presidency of Prof. Debove from October 14 to 16. It is proposed to hold discussions on, among other subjects, the question as to the origin of pulmonary tuberculosis; acid-resistant bacilli; the therapeutic action of radium; ionic medication; the use of collargol; the therapeutic value of tuberculin; and the serumtherapy of dysentery and cutaneous sporotrichoses.

THE third International Sanitary Convention is to be held in the city of Mexico from December 2 to 7 next. Each delegate attending is expected to bring a paper relating to the nation represented by him, with a report on the existence of any transmissible diseases—especially bubonic plague, yellow fever, cholera, malaria, beri-beri, and trachoma—that may prevail within its boundaries. Among the questions to be discussed are the transmission of yellow fever, the means to be used in combating the *Stegomyia fasciata*, tuberculosis, and various administrative measures.

THE eighteenth annual general meeting of the Institution of Mining Engineers will take place in the Firth Hall of the University of Sheffield on September 4, when the following papers will be read, or taken as read:—The sinking of Bentley Colliery, by Messrs. J. W. Fryar and Robert Clive; roof-weights in mines, by Mr. H. T. Foster; and deep boring at Barlow, near Selby, by Mr. H. St. John Durnford. A number of visits to collieries, works, &c., have been arranged.

THE recent opening of the medical academy at Düsseldorf was, according to the Berlin correspondent of the *Lancet*, an event of some importance, and was attended with considerable display. The academy and that at

Cologne have a two-fold object. In the first place they import a new feature into medical study by introducing newly qualified men to the practical side of medicine more than is done at the universities. After the university medical curriculum has been completed and the State examination has been passed, the practical year which is required by recent regulations may be spent at the academies. Their second purpose is to supply the medical practitioners of the district with opportunities for post-graduate study. The Düsseldorf Academy is the first for which clinics and lecture-rooms were specially built, because at Cologne the existing municipal hospitals were adapted for teaching purposes. The structural and other arrangements are described as excellent, and the clinical material will be abundant. Prof. Witzel, formerly of the University of Bonn, is the director of the new academy, while the teaching staff includes Prof. Schlossmann in the subject of pædiatrics and Prof. Lubarsch in the subjects of pathology and pathological anatomy.

The Paris correspondent of the *Chemist and Druggist* states that the committee on analytical methods has defined the programme for the competitions for the prizes offered for alcohol-denaturation in connection with the law of November 29, 1905. This Act instituted two prizes, one of 800*l.* for the discovery of a "denaturator" more advantageous than those now used while safeguarding the revenue against frauds, and a second (value 2000*l.*) for a system of utilising alcohol for lighting in the same manner as paraffin. The denaturator must have a taste and smell which will effectually discourage any desire to use the alcohol as a beverage; wine or date must, oil of thyme and rosemary, and similar flavours are thus eliminated. The denaturant should not be sufficiently objectionable in smell to prevent its domestic or industrial use—thus acetylene, asafetida, and garlic are excluded. No soluble substance which could leave a deposit on lamp-wicks, and thus render combustion difficult, may be used, such as sea-salt, sodium sulphate, alum, ammonium chloride, potassium ferrocyanide, picric acid, tobacco-juice, and aloes. It must not consist of a substance much more or less volatile than alcohol, and which could thus (besides other disadvantages) be removed by fractional distillation, as ether, carbon bisulphide, light fractions of petroleum or turpentine, cresyl, carbolic acid, camphor, or naphthalene. It should contain no substance which might injure the metallic part of lamps or motors (ammonia, nitrobenzene, sulphuric acid). It should not be poisonous (as mercuric chloride, methyl cyanide, sodium arseniate, and aniline) or contain poison (hyoscyamus, aconite, or digitalis). It should be sufficiently inexpensive, should not normally exist in commercial alcohol, and its presence in alcohol should be capable of easy and certain detection.

A prize of 150*l.* is offered by the German Colonial Society for a method to produce an extract from mangrove bark that will impart as light a colour as possible to leather, and such as will only slightly darken by exposure to light. The mangrove bark contains a large amount of tannin, and also a red colouring matter that prevents the bark and its extract from successfully competing with other tanning agents. The problem to be solved is the practical removal of this red colour. Competitors are invited to send in particulars of their methods by July 20, 1908, to Deutsche Kolonialgesellschaft, Schellingstrasse 4, Berlin.

The Board of Agriculture is considering the terms of an order prohibiting the importation of plants and bushes bearing edible fruit, except by a licence to which conditions will be attached, with the object of preventing the introduction of the gooseberry mildew and other pests injurious to horticulture.

According to *Engineering*, an Australian record in wireless telegraphy has been achieved by the successful transmission of messages from H.M.S. *Challenger*, one of the Australian squadron at present stationed in Hobson's Bay, to the flagship *Powerful*, which at the time was moored in Farm Cove, Port Jackson. The *Challenger* was in communication with the flagship by means of wireless telegraphy the whole of her voyage. The longest message was one flashed over a distance of 410 miles in a direct line, and this constitutes an Australian record, as previously never more than 240 miles had been achieved by warships on the Australian station.

According to *Science*, an equatorial telescope has been given to the Nantucket Maria Mitchell Association, and plans of an observatory to house the instrument are being considered. An appeal has been made for funds properly to equip the observatory that it may be available for astronomy classes in the near future.

A memorandum, dated August 5, issued by the Director-General of the Egyptian Survey Department, on the meteorological conditions of the monsoon season and the prospects of the Nile flood, is far from encouraging. The rains in June and July have been exceptionally weak, and some 16 per cent. of an average flood volume may be considered as deficient at the above date. On the whole, Captain Lyons thinks it more probable that this deficiency will be increased in August than that it will be diminished.

In the *Meteorologische Zeitschrift* (part iv., 1907) Dr. V. Conrad gives an epitome of an interesting lecture delivered by him on the formation and constitution of the clouds. The author points out that to obtain a clear idea of the subject we require to know (1) the size of the separate fluid drops, (2) the rate of their descent, and (3) the number contained in a cubic centimetre. An idea of the pains bestowed upon the inquiry may be formed from the fact that sixty-nine references to authorities are quoted in the paper. The size appears to have been first microscopically measured by Kratzenstein, who published the results at Halle in 1746; recent measurements by Assmann, Dines, and others give their mean diameter as about 20 μ, or 10^{-3} cm. radius. The vesicular theory was not displaced until A. Waller published his paper in the Phil. Trans. in 1847, and subsequent investigators showed that the optical phenomena observed in clouds could only be explained by the existence of complete drops; much information upon this subject will be found in Dr. Pernter's "Meteorologische Optik." The researches of Stokes and others have shown that a droplet of 10^{-3} cm. radius would fall 1 cm. per second in calm air; with increasing radius, up to a certain limit, V increases with r^2, so that a drop of 10^{-2} cm. radius would attain a velocity of 1 metre per second. From independent investigations the author found the number of drops ($r=10^{-3}$ cm.) in a cubic metre of dense cloud to be 10^9 (a thousand millions), or 1000 in a cubic centimetre. The question of the formation of the first condensation elements is one of great difficulty, since it has been shown by Aitken and others that the presence of some nucleus in the atmosphere is necessary; possibly observations made in balloons may eventually elucidate the matter.

THE important position occupied by the electric spark in wireless telegraphy will account for the many attempts which have been made in recent years to obtain some knowledge as to its effective resistance. The principal methods used fall into two groups, based either on the original resonance arrangement of Bjerknes or on the substitution process of Simons. Unfortunately, the two groups give different results, and Dr. W. Eickhoff has rendered valuable service by his examination of the validity of the various methods which appears in the *Physikalische Zeitschrift* for August 1. He comes to the conclusion that deductions as to the most suitable arrangements for telegraphic purposes cannot legitimately be made from results obtained by the method of Simons.

THE memorandum of the Manchester Steam Users' Association for the year 1906 consists mainly of a report on the tests of pressure gauges carried out at the National Physical Laboratory and the remarks of the chief engineer of the association on these tests, and on pressure gauges in general. The ten gauges tested were all by trustworthy makers and of first-class workmanship, and the report of the tests states that, as regards freedom from friction and backlash, they leave little to be desired. Greater agreement between the records at different temperatures ought to be aimed at, and for gauges subject to vibration some form of balancing should be adopted. The whole report will be of great value to pressure-gauge makers, and may be taken as a typical illustration of the way in which improvements can be brought about by the cooperation of manufacturers and an institution like the National Physical Laboratory.

DR. C. NORDMANN, of the Paris Observatory, published several years ago in the *Annales de l'Observatoire de Nice*, vol. ix., a theory of the diurnal variation of terrestrial magnetism, according to which the convection currents of the upper atmosphere, crossing the lines of magnetic force, generate electromotive forces which, in a region rendered conducting by solar radiation, produce electric currents and so affect the magnetic needle. In the March number of *Terrestrial Magnetism* he shows that the observations he made in Algiers during the total solar eclipse of August 30, 1905, confirm a deduction from his theory, namely, that during an eclipse the magnetic needle should tend to return from its normal position at the time of occurrence of the eclipse towards its mean position for the day.

PARTS i. and ii. (comprising 335 pp. and 18 plates) of vol. lxxxvii. of the *Zeitschrift für wissenschaftliche Zoologie* are occupied by an elaborate paper on the comparative developmental history of sexual individuals in the hydroid polyps. The author, Mr. A. Goette, of Strassburg, formulates some important conclusions with regard to this development, which are, however, too complex to be summarised within our limitations of space.

IN a paper on the navicular of the tarsus of man and monkeys, published in vol. xli., part iv., of the *Journal of Anatomy and Physiology*, Mr. T. Manners-Smith describes and illustrates the remarkable variations obtaining in that bone in the human subject. In some degree, at any rate, these variations appear to be connected with the degree of mobility of the foot, certain features being more constantly developed in this bone in the skeletons of ancient Egyptians than among modern Europeans. The occasional existence of a separate element in the tuberosity of the navicular is also noticed.

FROM the astounding feat accomplished by Prince Borghese in his wonderful journey in a motor-car from Pekin to Paris, many lessons may be drawn. The greatest, the *Engineer* of August 16 points out, is the wonderful adaptability of the power-driven vehicle. The distance traversed is estimated at 7000 to 8000 miles, and the time occupied was sixty-two days, the daily average being about 121 miles. When the time is taken off for pulling the car through loose sand and for extricating it from morasses, the speed seems almost incredible, considering that for half the journey there were practically no roads. Serious obstacles were encountered. Overwhelmed in a cyclonic sandstorm, dragged through rivers, precipitated from a weak bridge into a fast-running river below, immersed in bogs, the wood-work of the car on fire, and being nearly run into by a train on the Trans-Siberian Railway, were a few of the experiences of the intrepid traveller, any one of which would be sufficient to stop most people from continuing such a perilous journey.

ON August 17, 1807, Robert Fulton's steamer, the *Clermont*, ran her trial trip from New York to Clermont, and in order to commemorate the centenary of steam navigation, an interesting account is given in the *Engineer* of August 16 of the events that led up to this development. Illustrations are given of the paddle-wheel steamboat *Clermont* and of her machinery.

THE prospects of the Indian manganese ore industry are discussed by Mr. A. Ghose in the Journal of the Society of Arts (August 2). The demand for Indian manganese ore has grown with great rapidity. In 1905 the export to Great Britain amounted to 71,660 tons, whilst last year the total amounted to 490,612 tons, valued at 865,443*l*. India supplied most of the manganese ore used in the British furnaces, Brazil with 127,257 tons being second, and Russia following with 103,276 tons.

AN interesting account of the Museum of Traffic and Engineering at Berlin is to be found in the August number of the *Engineering Magazine*. The museum is located in the old Hamburg passenger railway station, which has, however, been thoroughly reconstructed for the purpose so far as its basement is concerned. The museum has three main departments, devoted to railway, naval, and civil engineering respectively. Explanatory notes are added to many of the exhibits, and in some instances a cross-section is given to illustrate the internal arrangement. Some of the models may be worked by visitors, and to these a special notification is attached. Models of plant of exceptional interest are to be demonstrated and explained from time to time by officials of the institution. In addition to models, the museum possesses many diagrams, photographs, statistical tables, a reading room, and library, all of which should make it of real service to engineers.

THE extension section of the Manchester Microscopical Society has sent us its new list of lectures arranged for delivery by members of the society during the coming winter in Yorkshire, Manchester and district. The work of lecturing and demonstrating is entirely voluntary and gratuitous on the part of the members, but a charge is made for the hire of slides, travelling, and out-of-pocket expenses. The purpose of the section is to bring scientific knowledge, in a popular form, before societies unable to pay large fees to professional lecturers, but in all cases where lectures are given before societies which are commercial undertakings, or are subsidised out of Government or public grants, a fee is charged in addition to the out-of-pocket expenses. All fees paid for lectures are devoted to the working expenses of the section.

IN a paper by Mr. W. F. Allen on the subcutaneous vessels of the head in certain fishes, published in the Proceedings of the Washington Academy of Sciences (vol. ix., p. 79), we regret to observe that Lepidosteus, the well-known name of the bony pike, is changed to Lepisosteus. Even if the latter be the original rendering, the former is grammatically correct, and should be maintained. We have also received copies of two papers on Mendelism, one by Mr. C. B. Davenport and the other by Mr. O. F. Cook, published in the same series. Variation and correlation in the crayfish, by Messrs. Pearl and Clawson, and researches on North American Acrididæ, by Mr. A. P. Morse, form the subject of papers issued by the Carnegie Institution of Washington, of which copies have reached us.

ACCORDING to the report in the July number of the *Victorian Naturalist*, Mr. F. C. A. Bernard selected as the subject of his presidential address to the last annual meeting of the Field Naturalists' Club of Victoria the increased facilities for the study of natural history in Australasia since 1880. After a well-deserved compliment to the Linnean Society of New South Wales, which he believed to be the only organisation existing at that date in Australia devoted solely to promoting the interests of natural history, the president traced the origin and progress of the numerous bodies which now exist for the same purpose.

IN an illustrated pamphlet entitled "The Brent Valley Bird Sanctuary" (published, at the price of sixpence, by the local branch of the Selborne Society) Mr. W. M. Webb gives a picturesque account of an attempt to encourage and protect the bird-life of the district. A wood of some nineteen acres in extent has been secured and put in charge of a keeper, and it is satisfactory to learn that it has afforded nesting sites for twenty-seven species of birds.

AN addition to the fauna of the British Isles is recorded by Mr. J. W. Taylor (in the *Irish Naturalist* for August) in the shape of *Vitrina elongata*, a land mollusc inhabiting the mountains of many parts of the Continent. The Irish specimens were discovered in 1904 and 1905 near Collon, county Louth.

THE contents of vol. xl. of *Neue Denkschriften der allg. schweiz. Gesellschaft* (1906) include an article by Dr. Theodor Studer on additional remains of the ground-sloth, *Grypotherium listaei*, from the well-known cavern of Ultima Esperanza, S. Patagonia. Separate copies of this article were issued in 1905. The author confirms the opinion that this ground-sloth inhabited the cave contemporaneously with aboriginal man, by whom it appears to have been kept in a semi-domesticated state. These aborigines seem to have been identical with the ancient Patagonians. Grypotherium appears to have been a stouter-built animal than Mylodon, with the orbital region of the skull smaller. Stratigraphists will find much to interest them in an article in the same volume by Dr. E. Gerber, of Bonn, on the geology of the Alps to the eastward of Kienthal, embracing the district between that valley and Lunterbrunnen.

A BULLETIN (No. 4) from the Agricultural Research Institute, Pusa, indicates the preliminary arrangements in connection with a series of fruit experiments initiated under the direction of Mr. A. Howard. The planting, pruning, and manurial experiments are in the main similar to those at Woburn. In addition, weathering experiments are proposed, which consist in removing the soil from round the stems and laying bare the roots for a period after the close of the rains; the object is to check vegetative growth, especially before the flowering period. The largest plots are planted with citrus fruits, peaches, mangoes, litchis, and figs.

IN plant experiments to test Mendelian principles several apparently anomalous results have been obtained by crossing white-seeded strains with plants having coloured seeds. In papers published in *Science*, vol. xxv., Nos. 646 and 647, Dr. G. H. Shull refers to results obtained by crossing the flowers of white flageolet beans with those of black-, brown-, and yellow-seeded forms in which the hybrids showed purple and mottled characters. The author adopts the explanation offered by Cuénot that in such a case there are three characters, the pigment character P, the purple modification B, and the mottling M. The black beans show PB dominant, M recessive, the white beans show all three characters dominant. Therefore, instead of considering the *allelomorph* or distinguishing character as necessarily single, Dr. Shull holds the view that it may be compound.

SEVERAL important contributions to the study of the proteins of the wheat grain have been made from time to time by Dr. T. B. Osborne in conjunction with other collaborators. The results have been brought together in Publication No. 84 of the Carnegie Institution of Washington, in which is given a full account of the experimental work, as well as a brief review of the literature. It is found that *gliadin*, a protein soluble in 70 per cent. alcohol, and *glutenin*, which together constitute the substance *gluten* obtained by washing the dough, form nearly the whole of the proteins in the endosperm; in the embryo the proteins are much smaller in amount, and consist chiefly of globulin, an albumin termed *leucosin*, and a proteose.

AN article by Mr. H. A. Smith entitled "Saving the Forests" appears in the *National Geographic Magazine* for the present month, and deals with the work of the United States Forest Service, which has charge of Government resources valued at 1,500,000,000 dollars. The U.S. national forests contain more than 150,000,000 acres. In economic usefulness the forests increase in importance almost day by day, and they are fast becoming self-supporting. In the year ending June 30, 1904, the national forests yielded a total revenue of 60,000 dollars, while for the year 1906–7 the sum realised amounted to 1,600,000 dollars, and it is thought that by 1910 the receipts from this source will be equal to the appropriations for the forest service.

ON account of a remarkable discovery of reptilian footprints, the Higher Bebington sandstone quarry at Storeton, Cheshire, has been visited by many geologists during the past year. From time to time Cheirotheroid, Rhynchosauroid, and Chelonoid footprints have been found at this quarry, but since the present owner introduced a stone-channelling machine much more work is being done, and the slabs are got out with less breakage. The quarry, which is worked in the Keuper Sandstone, has a vertical face of 130 feet, and at two horizons half-way down the face occur two thin beds of marl on which the interesting footprints are found, and casts of them occur on the layer of sandstone immediately overlying the marl. Photographs, by Mr. G. J. Williams, of the face of the quarry and of some of the footprints are reproduced in the report of H.M. Inspector of Mines for the Liverpool district for 1906 (Cd. 3449, vi.).

THE *Journal of Hygiene* for July (vii., No. 4) contains a number of interesting articles. Among others, Castellani shows that human yaws is transmissible to monkeys, and that in the lesions, spleen, and glands the same spirochæte (*S. pertenuis*) is present as in man.

AN interesting account of the evolution of the steam turbine and a sketch of the career of its inventor—the Hon. C. A. Parsons, F.R.S.—by Mr. A. A. Campbell Swinton, appears in the current issue of the *World's Work*. Other articles of scientific interest in the number are "Lobster Farming," by Mr. F. A. Talbot, dealing mainly with the work carried on at Mill Cove, Wickford, Rhode Island, by Dr. A. D. Mead, and "Scientific Taxidermy," by Mr. H. J. Shepstone. The two last-named contributions are strikingly illustrated.

WE recently published a review of part i., vol. i., of "Research in China," dealing with descriptive topography and geology (NATURE, August 8), and have now to record the receipt of part ii. of the same volume of the work. The bulk of the section before us treats of petrography and zoology, and is the work of Mr. Eliot Blackwelder, but there is also a syllabary of Chinese sounds by Dr. Friedrich Hirth, professor of Chinese at Columbia University. The work is issued by the Carnegie Institution of Washington.

A SECOND edition of "Impianti Elettrici a Correnti alternate semplici, bifasi e trifasi" has recently been received from Mr. U. Hoepli, Milan. The book forms one of the very practical series of Manuali Hoepli, and will be of service to students and electrical engineers able to read Italian.

MESSRS. A. AND C. BLACK announce a book entitled "The Norwegian Fjords," which is to be written and illustrated by Mr. A. H. Cooper. The work will describe the home life, domestic industries, religion, superstition, and folk-lore of the peasants of Norway.

THE Patent Office has just published a subject list of works on military and naval arts, including marine engineering, in the library of the Patent Office.

OUR ASTRONOMICAL COLUMN.

DANIEL'S COMET, 1907*d*.—This comet is now at its maximum brightness, and with a clear sky and good horizon may be seen quite easily by the naked eye for some time before sunrise. Its naked-eye magnitude on August 12 was estimated to be equal to that of μ Geminorum, about 3·5.

The comet rises about 25° north of east, in London, at about 2 a.m., and on August 22 will be some 12° 11′ directly south of Pollux.

Two excellent photographs of this object were secured by M. Quenisset, at Juvisy, at 2 a.m. on July 19 and 20 respectively, and are reproduced in the August number of the *Bulletin de la Société astronomique de France*. On the former date the photograph showed five tail streamers, but on the latter seven were to be seen on the plate. The longest tail extended some 4° from the nucleus, representing at least some 12,000,000 kilometres (7,500,000 miles); on July 20 the diameter of the nucleus was about 4′, or 173,000 kilometres (about 108,000 miles).

SEARCH-EPHEMERIDES FOR COMETS 1894 IV. AND 1900 III. —No. 4195 of the *Astronomische Nachrichten* (p. 319, August 7) contains two sets of search-ephemerides, one by Prof. Seares for the De Vico-E. Swift comet (1894 IV.) discovered in 1894, but not seen on its return in 1901, the other by Herr Scharbe for Giacobini's comet, 1900 III.

The former was referred to in these columns on August 1, and the comet's brightness on August 25, according to the ephemerides, will be either 0·61 or 0·86, its brightness when its magnitude was 13·1 (November 21, 1894) being taken as unity.

Ten alternative ephemerides are given for comet 1900 III.

MARS.—In a telegram published in No. 4195 of the *Astronomische Nachrichten* (p. 323, August 7), Prof. Lowell announces that the Martian double canal Gihon has been photographed as double both by Mr. Lampland and himself.

In Bulletin No. 30 of the Lowell Observatory the same observer discusses the results of the observations of the North Polar Cap of Mars during the period March–June, 1907. It appears that the cap commenced quite suddenly and in an extensive manner just as it did in 1903 and 1905, and on practically the same date, the Martian August 22–23. Further, the first frost melted again on the succeeding days and was followed by another fall a little later, again as it did in 1903 and 1905.

This striking fact led Prof. Lowell to investigate mathematically the problem of the daily insolation upon a planet, and he shows that the Martian phenomenon is in accordance with his deductions.

Among other points he demonstrates the existence of an atmosphere sufficient to retard the general deposition of frost by some nineteen days. He also states that the arctic and antarctic regions of Mars are actually warmer in the Martian summer than are ours, although the mean temperature of the planet, 48° F., is some twelve degrees less than the mean temperature of the earth.

THE TOTAL ECLIPSE OF JANUARY, 1908.—From No. 114 (p. 167, vol. xix., June 10) of the Publications of the Astronomical Society of the Pacific we learn that arrangements have been made for an expedition from the Lick Observatory to observe the total solar eclipse of January 3, 1908.

Only two islands are crossed by the shadow-path, and of these the Lick expedition has selected Flint Island (long. 151° 48′ W., lat. 11° 26′ S.), which lies in the central Pacific Ocean some 390 miles north-west of Tahiti.

Under the existing conditions the eclipse will occur at 11h. 18m. (local mean time), with the sun 15° from the zenith. The duration of totality, according to the American ephemeris, will be 4m. 6s.

The expedition, the sending of which has been made possible by the generosity of Mr. William H. Crocker, will leave San Francisco on November 22, journeying thence to Tahiti, and will be conveyed from the latter island by a U.S. gunboat.

At the instigation of Prof. Campbell, Prof. Abbot, of the Smithsonian Institution, will accompany the Lick expedition in order to secure bolometric observations of the corona. The two expeditions will be independent scientifically, but will be united in the travelling and subsistence arrangements.

In the August number of the *Observatory* (p. 333, No. 386) it is tentatively suggested that it may be possible for some European astronomer, who could not otherwise see the eclipse, to obtain some assistance from the Lick expedition.

THE LEEDS ASTRONOMICAL SOCIETY.—The fourteenth annual Journal and Transactions of the Leeds Astronomical Society contains some interesting papers communicated by the members during 1906.

An observatory, in connection with the University and the city council, was opened on May 4, 1906, on Woodhouse Moor, and contains an 18¼-inch Newtonian reflector and a transit instrument. These instruments are to be used by members of the University staff, certain university students, teachers and selected students from the Education Committee's schools and by members of the astronomical societies.

Among the papers published in the Journal, one may mention a discussion of the existence of an intra-Mercurial planet, an illustrated description of the immense Jai Singh observatories located at Benares, Delhi, and Jaipur, and a lengthy discussion of Tennyson's astronomy.

THE MAKING OF MOUNTAINS.

THE profound impression made on contemporary geological thought by what is known as the Schardt-Lugeon theory of overfolding is well seen in Herr H. Hoek's last paper on "Das zentrale Plessurgebirge" (*Berichte d. Naturforsch. Gesell. zu Freiburg i. B.*, Bd. xvi., 1906, p. 367). In this he completely modifies his reading of the district, published three years ago, in favour of one that brings it into structural harmony with recent views as to the existence of "Ueberschiebungsdecken," "Nappes de recouvrement," or overlapping and overthrust recumbent folds. The region south-east of Chur has received a good deal of geological attention, and Herr Hoek claims that it now falls into its place as a structural link between the overfolded areas of Switzerland and the Austrian Alps. His paper is written in a considerately argumentative spirit that provides cheerful reading, and concludes with a tilt at Rothpletz, who has sprung into the same field of inquiry. Both writers agree, however, that the overlying rock-sheets of the Plessurgebirge have been brought into the area from a distance; and that is the point which interests the watchers of the tourney.

Dr. W. Hammer, in a review of the above paper (*Verhandl. d. k.k. geol. Reichsanstalt*, 1906, p. 383), evidently regards it as an attempt to put an old wine of good quality into new and uncertain bottles. But Herr G. Steinmann adopts Hoek's typical landscape of the four overthrust sheets above Parpan in his "Geologische Probleme des Alpengebirgs" (p. 40), and he has had ample opportunity of discussing the structure with the author. Steinmann's paper, published by the Deutscher und Osterreichischer Alpenverein at Innsbruck in 1906 (Bd. 37 of their *Zeitschrift*), is a delightful exposition of the older and later views, beginning with the Juras and ending in the east of Switzerland, and is written for the ordinary traveller as well as for the geologist. The landscapes, in which the beds are duly labelled, are accompanied by an admirable series of sections, often showing the two rival readings, and gathered from various authors. Steinmann's own sections show clearly the stages of growth which are held to have culminated in the system of overfolds in Switzerland. This lucid paper seems to us perfect for its purpose, and the author is able, in his separately issued copies, to reproduce Dr. von Seidlitz's panorama from the Künihorn, which was published in part only in the *Zeitschrift des Alpenvereins*.

Herr Schardt himself has furnished, in the *Verhandlungen der Schweizerischen Naturforschenden Gesellschaft*, St. Gallen meeting, 1906, p. 308, a welcome account of "die modernen Anschauungen über den Bau und die Enstehung des Alpengebirges." Already this distinguished author suffers from an extremist group of followers, and he humorously characterises some of Termier's work as an exhibition of "Ultranappismus." The coloured sections illustrating Schardt's paper supplement this very clear exposition of his views. On p. 343 he emphasises the importance of gravitational sliding in producing certain features of steeply elevated and compressed folds, and points out that this influence has been too often underestimated.

Travellers in the most familiar part of Europe will also profit greatly by Herr A. Baltzer's "Das Berneroberland und Nachbargebiete" (Berlin: Gebrüder Borntraeger, 1906, pp. xv+347). The price of this handy book, 12.50 marks, includes a general volume, which has not yet reached us, though it was announced to appear during last winter. Rothpletz's work on the overfolded area of the Rhætic Alps, to which Hoek refers in the paper above noticed, has been published in the same "Sammlung geologischer Führer." Baltzer's volume starts in the Bernese Juras, among the romantic *cluses*, guides the pedestrian of geological tastes to the typical sections around Grindelwald, and brings him back by the St. Gothard railway and Lucerne. The illustrations are not always so neat as those of Steinmann, but cover a great amount of detail. The transparent sheets on which the names of the rock-layers are printed, which are used as indexes to several of the photographs, strike us as a little awkward in a book that must be used in all weathers in the field. Among the many useful diagrams is one (Fig. 69) showing the folded strata as viewed from the steamer on both sides of the lake of Uri.

Dr. C. Sandberg (Transactions of the Geological Society of South Africa, vol. ix., Johannesburg, 1906, p. 82) gives a new reading of the folded structure underlying "the innocent looking, softly undulating Karroo Formation" in the Prince Albert district of Cape Colony. The country, with its bare kopjes and its abundance of rock-exposures, lends itself to stratigraphical investigation. The Tygerberg shows on its south side the Witteberg series resting on the Dwyka conglomerate, which properly overlies it, while the Dwyka series occurs again on the north side of the hill. This can be explained by fan-structure, the Witteberg series coming up along a local anticline, which has expanded southward as an overfold. Though various authors show various dips, the anticlinal view has been generally accepted. Dr. Sandberg, however, quoting the magic names of Schardt, Lugeon, and Termier, reads the structure as the downward-turned, or, shall we say, pseudo-synclinal, end of an overpushed anticline, the root of which lies away in the Zwartebergen to the south. Mr. A. W. Rogers, who is invoked by the writer, points out that a section quoted from him in support does not touch the Tygerberg, and he evidently prefers the older view for the present (Proceedings to accompany the above volume, 1907, p. liii.). But Dr. Sandberg's paper serves as a fresh indication of the keenness and vitality that prevail in South African geology.

It is evident that very few geologists now doubt that overfolds occur in the earth's crust, whereby strata are moved from their place of deposition over distances amounting to even 100 kilometres. If we grant ten miles for such movements in the north of Scotland, we do not find it unreasonable to allow seventy miles of overthrusting in the more crumpled region of the Alps (see Sir A. Geikie's remarks, Abstracts of the Proceedings of the Geol. Soc. of London, April 10, p. 67). But the admission raises serious questions as to what happens in the foundations underlying these compressed areas of the crust. Dr. Ampferer, in a lengthy and closely reasoned paper on "Das Bewegungsbild von Faltengebirgen" (*Jahrb. der k.k. geol. Reichsanstalt*, Bd. lvi., 1906, pp. 539–622), rejects the theory that localised crumpling is due to the approach of great earth-blocks in the contracting crust. He urges, moreover, that there is nothing in any complex group of rocks, such as we ordinarily find folded together in a mountain-chain, to account for the folding in that particular locality. The local structure determines the details of the architecture (p. 607), but the position of the chain on the earth must be referred to the nature of the plastic foundation, the "Untergrund." The composition of the earth's interior is by no means uniform (pp. 608 and 609), and both physical and chemical changes in it may produce considerable alterations of volume in one portion or another. These alterations are manifested at the surface, in the sensitive skin of the earth, as local subsidences or elevations. The vertical movements of the foundation lead to gravitational sliding, which affects the overlying skin (p. 601), and it is suggested that the folded mountain-chains are formed along lines of more or less intense vertical upheaval, from which the fundamental masses then flow away sideways, producing the overfolds and crumplings that we perceive upon the surface. The underflow, the "Unterströmung," is thus Ampferer's main cause of surface-folding, and changes in the living and mobile "Untergrund" determine where "Unterströmung" shall occur.

We trust that in these few words we have correctly represented Dr. Ampferer; for the paper is not an easy one, and the absence of references to the details shown in many of the diagrams renders these of little help as illustrations. Fig. 41, however, on p. 611, shows suggestively how the irregularities of the foundation or "Untergrund" may be ascertained by a comparison of the geological history of areas on the earth's surface. Areas with similar foundations may be expected to subside together or to be elevated together during geological time. It is easier to estimate the amount of subsidence that took place during any epoch than the amount of elevation, the latter being marked by no characteristic sediments; but we may hope eventually to represent the history of an area by a curve rising or falling in reference to a datum-line, the time-scale being indicated on the latter. Prof. Steinmann, in the paper above noticed, has employed similar curves for various regions in the Alps. Dr. Ampferer holds that a similarity in the curves for two or more areas would imply a general similarity in the foundation. This seems to leave out of count Mr. Osmond Fisher's suggestion of convection-currents in a liquid interior, which might produce considerable local differences in the curves; but the absence of all reference to previous workers, except a passing one to Schardt's and Lugeon's theory of overfolding, makes it uncertain how far Dr. Ampferer wishes to link himself with his predecessors. While admitting (p. 620) that his criticism has been largely of a destructive order, he feels hopeful that future research may make us better acquainted with the "Untergrund."

Dr. Ampferer's paper was written in March, 1906, but was not issued until December. Meanwhile, Mr. R. A. Daly had published a paper on abyssal igneous injection as a causal condition and as an effect of mountain-building (*American Journ. of Sci.*, vol. xxii., September, 1906, p. 195). Ampferer does not believe in geosynclinals and subsequent lateral compression; but it is precisely these that Daly sets out to explain. He urges that the underlying molten magma, which he believes to be of basaltic composition, is always ready to leap into any cracks that arise in the plastic layer above it and in the "shell of tension" in the lower portion of the crust. Cracks may arise in the plastic layer by the effects of tidal torsion on the crust, and in the shell of tension by the general contraction due to cooling. The igneous mass in the great dykes thus produced, so long as it remains liquid, exercises a hydrostatic pressure on its walls, and forces them further apart. Here we have a cause that may close up other cracks in the shell of tension, and the total lateral creep "involves a strong downward pull exerted on the shell of compression," *i.e.* that part of the crust above the level of no strain. The resulting geosynclinal area finally becomes weakened, as sediments accumulate in it and its underlying rocks are bent down into hotter regions; and then, in some way which is rather lightly dealt with, an "orogenic collapse" takes place, and a mountain-chain begins to rise. The increase in bulk of the crust by magmatic injection, which was, by the by, well pointed out by Mr. Osmond Fisher, is urged as an additional cause of its crumpling, and the hydration of its minerals gives similar assistance. The shearing apart of the shell of compression and the shell of tension "during the orogenic revolution releases the tensions still unrelieved in the underlying shell," and allows of further abyssal injection on a large scale. The location and elongation of mountain-chains and geosynclinals are all (p. 216) related to special zones of abyssal injections from the substratum. Here we see Mr. Daly looking, like Dr. Ampferer, for final causes in the "Untergrund," and with this point of agreement we must for the moment rest content.

Still more recently (March, 1907), Dr. L. Waagen publishes in the *Verhandlungen der k.k. geologischen Reichsanstalt* a general review of the relations of ocean-basins and mountain-chains. He finds the origin of folding in the subsidence of continental masses, and the continued sinking of the moving *Hinterland* of a chain may bring this region below sea-level, and so promote an interchange between continental and oceanic areas. Marine transgressions (p. 121) are thus the natural accompaniments of epochs of considerable mountain-building.

G. A. J. C.

THE SOUTH AFRICAN ASSOCIATION.

THE report of the South African Association for the Advancement of Science (Cape Town, 1907), a handsome volume of 640 pages, affords striking evidence of the large amount of attention that is being devoted to scientific research in South Africa. The report includes the presidential address delivered by Mr. T. Reunert at Johannesburg in 1905, in which year the usual sectional meetings were not held, and minutes of the proceedings of the fourth meeting of the association at Kimberley in 1906. The address of the president, Mr. G. F. Williams, who was unable to attend the Kimberley meeting in 1906, contains an interesting historical sketch of the settlement of the Cape, of the adventurous spirit of the Portuguese, of the influence of the Dutch pioneers, and of the rush of adventurers from almost every part of the world, who laid the foundation of the mining industry of to-day.

The president of Section A, Mr. J. R. Sutton, gave a valuable summary of our present knowledge of the diurnal variation of barometric pressure, which may be considered as the fundamental problem in meteorology—the rise and fall of the barometer twice a day, the precise cause of which has never been satisfactorily explained. After referring to the efforts of Herschel, Dove, and others, the author pointed out that Dr. Buchan made a material advance in distinguishing between the weight and elasticity of the air. Perhaps the most important contribution to the subject is due to Prof. J. Hann, who by classifying and generalising the harmonic elements for a great number of stations succeeded in establishing some noteworthy results. Among the various papers read in this section, some of which have been published elsewhere, we may specially mention:—(1) Anticyclones and their influence on South African weather, by Colonel H. E. Rawson. An examination of the charts published by the London Meteorological Office, and of other data, showed that the weather in South Africa is under the direct influence of the movements of two great anticyclonic systems lying to the west and east. (2) The barometer in South Africa, by Mr. R. T. A. Innes. The author states that the annual variation of the barometer consists of a well-marked single oscillation, pressure being greatest in winter and least in summer. (3) Variability of temperature in South Africa, by Mr. J. R. Sutton, as represented by three typical stations, showing the mean difference between the temperatures of one day and the next. Prof. J. Milne contributed a paper on the observation of earthquakes and other earth movements, and Prof. R. A. Lehfeldt one on accelerations of gravity at Johannesburg.

At the various sectional meetings sixty papers were read, which are published in full or in abstract. Among these, noteworthy papers dealing with matters of local interest are, in addition to those already mentioned:—Rev. H. A. Junod, on the theory of witchcraft among South African natives; Rev. F. Reuter, on northern Transvaal ethnology; Mr. A. W. Rogers, on the glacial beds of the Griqua Town series; Mr. R. H. Rastall, on the petrography of the Kimberley district; Mr. F. P. Mennell, on the Somabula diamond field of Rhodesia; Mr. J. P. Johnson, on the Stone age of South Africa; Mr. F. A. Hurley and Mr. C. D. H. Braine, on irrigation in South Africa; Mr. K. A. H. Hought, on native education in its higher branches; and Mr. T. Lowden, on the place of manual training in South African education.

Of the sectional presidential addresses, that by Mr. Sidney J. Jennings, on wastes in mining, waste of thought, waste of labour, and waste of material, should be mentioned. Nothing has proved so efficacious for the prevention of waste of thought as the free interchange of ideas made possible by the numerous societies and associations. Centralised management can also be made to perform a valuable function in preventing thought-waste. In preventing waste of labour, the fundamental difficulties lie in the untrained condition of the Kafir for manual labour, and in the ineptitude of many white overseers for transforming a semi-savage population into an industrial one. The best prevention of waste of stores is the continued thought of the management combined with the loyal and interested cooperation of the men.

An account of a discussion on university education in South Africa is contained in the report. The discussion was opened by Prof. Lyster Jameson with a brief survey of the past history and present position of university education in South Africa, followed by a summary of the more obvious remedies for the present unsatisfactory state of affairs. There is a single university, the University of the Cape of Good Hope, an examining body pure and simple. In addition to the University, several institutions calling themselves colleges have arisen. In the Transvaal the only institution seriously doing university work is the Transvaal University College, which was founded as a full faculty of mining and engineering, adding its arts department at a later date. However great the disadvantage of federation or of separation, the greatest peril to university education in South Africa lies in the excessive multiplication of institutions with poor endowment and small, underpaid and overworked staffs. The discussion was well sustained; and, in summing up, the chairman, Mr. S. J. Jennings, pointed out that in Germany and England a population of a million could support a university. Roughly speaking, a population of a million in South Africa would correspond in fee-paying capacity with a population of two millions in Germany or England. It therefore seemed within the range of possibility that South Africa could support two universities.

INTERNATIONAL MARINE INVESTIGATIONS.[1]

THIS summary of the results so far obtained by the international investigation of the North and adjacent seas is drawn up by the executive committee of the Swedish Hydrographic-biological Commission, and is the second of its kind. Being well written, illustrated by good charts and plates, and demanding no great previous knowledge from the reader, it is one of those accurate yet popular accounts which, by educating public opinion in the utility of research, possess a real public value. Its slight unevenness is probably inevitable in the rapid survey of so wide a field, and it is to be regretted that the language in which it appears will restrict so narrowly the number of its readers.

The introduction patriotically reminds us that Sweden took the initiative in cooperation in marine research when King Oscar issued invitations to the conferences of Stockholm (1899) and Christiania (1900), and states the aim of the work to be, in the terms of a resolution of the latter meeting, "to prepare for the rational exploitation of the sea on a scientific basis." The aim is thus practical; the writers proceed at once to discuss the urgent practical question which played a considerable part in securing British participation in the international scheme, namely, the over-fishing question.

The belief that the catch of fish (mainly trawl-caught fish) was greater than nature replaced had arisen, declined, and revived when the international work began. Remedies had been proposed, and, being based on insufficient knowledge of the sea, had failed. The authors unreservedly include among the failures the closure of areas to trawling and the replenishment of the sea by fish-hatcheries; they speak hopefully of the value of market statistics, recognise the recent improvements in English methods of collection, and pass to the biological attack on the problem. This section is a little disappointing. Much has been ascertained concerning intensity of fishing, migration, &c., the bearing of which on over-fishing is not clearly brought out in the text. Since, for instance, over-fishing is stated to affect plaice mainly by reducing the average size at which they are caught, any experimental evidence of a possibility of increasing the rate of growth deserves close consideration; yet the promising results of transplanting plaice from crowded "nurseries" to good feeding-grounds where growth is more rapid are very briefly dealt with.

To make any proposal for restrictive legislation before the International Council has fully sifted the evidence collected on over-fishing seems premature, and, from the representatives of a country not greatly interested in trawling, even a little out of place. The writers, however, advise that each country fix an inshore size limit independently, while no plaice should be landed from offshore grounds of less length than 28 cm., that limit to be gradually increased to 33 cm. As to the practicability of enforcing this rather complicated scheme they are silent; possibly wisely.

Numerous biological researches are described, but by far the greatest detail is accorded to hydrography. Even Prof. Petterssen's theory of the effect of ice melting is included, although, as Nansen's "Northern Waters" has shown, it is still controversial. The Baltic hydrography is perhaps the freshest section for English readers. Hydrography gained much from cooperation; the standardising of instruments and reagents removed one frequent source of wasted opportunity in earlier voyages, by making all observations more strictly comparable, while the periodic cruises of the numerous vessels employed ensured regular observations over the whole great area involved. The main result has been the discovery that European seas are flooded every autumn by Atlantic water (of 35 per mille salinity or more) which withdraws in spring, and that many fisheries depend on these movements. Such a fishery is that of the Swedish "winter herring"; the fish is abundant, and the fishery prosperous when southern bank-water, of characteristic salinity, temperature and plankton, forms a thick layer in the Baltic entrances, while in years of exceptional abundance of Atlantic water this displaces the overlying bank-water, and a "bad herring year" results. These years occasion considerable distress.

The summary closes with appendices, some of which, semi-diplomatic documents now apparently published for the first time, are worth careful perusal by all interested in fishery legislation and research. One, written by Prof. Petterssen in reply to a question from the English Board of Agriculture and Fisheries, as to the probability of practical results shortly appearing, is especially interesting. Prof. Petterssen mentions the confusion of ideas and opinions that, owing to lack of knowledge of the sea, prevailed before the international work began, describes the results attained and the value set on cooperation by the investigators, and, speaking of the protection of immature fish, he makes the noteworthy remark, "International measures of this kind must be founded on strong and indisputable evidence. . . . Such evidence can only be the outcome of a joint investigation of the total area in question, executed by the best specialists of every nation concerned." These words constitute now, as they did three years ago, a weighty defence of international cooperation in fishery research.

[1] "Resultaten af den Internationella Hafsforkningens arbete under åren 1902–1906, och Sveriges andel däruti. By G. Ekman, O. Pettersen, F. Trybom. Pp. 164. (Stockholm: Isaac Marcus, 1907.)

THE TRANSVAAL DEPARTMENT OF AGRICULTURE.

WE have received from the director a copy of the annual report of the Transvaal Department of Agriculture for the year 1905–6. The department was formed soon after the close of the war, and was placed under the charge of Mr. F. B. Smith, who had been trained at Cambridge and had gained experience as an agriculturist at Wye College, of which he was for some years the vice-principal. On his arrival in the Transvaal, Mr. Smith gathered round him a band of zealous and competent workers, and organised the new department on American lines, assigning the work to a number of "divisions." Each of these, while independent and under the charge of separate heads, was kept in close touch with the work of the other divisions through the director of the department and his office staff.

The report for the past year gives a *résumé* of the work on which the new department is now engaged, which should prove of interest not only to those directly concerned, but to many in our own country who may wish to know what the trained agriculturist can do to assist the development of the colonies. The most obviously beneficial work of the department is that of the veterinary division, which was formed partly to investigate the numerous diseases which threatened the live stock of the colony at the close of the war, but chiefly to check the spread of disease by treating diseased animals and by administering acts regulating their movements. The need for this type of work may be inferred from the fact that during the year 726 outbreaks of contagious disease were dealt with, 140,000 animals inspected, and 660,000 examined for soundness at the ports or on the borders of the Transvaal before being admitted into the country.

The acts regulating the movement of diseased animals have caused stock-owners some inconvenience and have been the subject of occasional complaints, but they have succeeded in a remarkable way in improving the health of the live-stock. For example, the disease known as East Coast fever, which at the close of the war was a serious menace to the cattle of the colony, has been overcome, and large areas have been altogether freed from it. In 1904–5 about 8000 cattle died of this disease; in 1905–6 the number was reduced to 800. As an instance of the protective measures adopted by the department, we may cite the case of rabies. The Transvaal is free from this disease, but it is found in Rhodesia, and in the hope of preventing its introduction a strip of country fifty miles wide, along the northern border of the Transvaal, has been entirely cleared of dogs.

A large part of the time of the chief of the division of botany is taken up by consultative work. Information upon new crops, weeds, poisonous plants, forest trees, &c., is in constant demand, and, apart from interviews and attendances at shows, this work alone involves the writing of some 3000 letters per annum. A herbarium is being formed. Some progress has been made in crossing and selecting maize, but it is remarked that, owing to the pressure of other work, plant-breeding has not hitherto received the attention it deserves. An important section of the work of the division is that which deals with plant pathology. A pathologist was recently appointed by the department, and the number of diseases which he has already observed is referred to in the director's report as "amazing." Special attention has been directed to the rusts, and five have been so far identified, viz. *Puccinia graminis* on wheat and barley, a second form of *P. graminis* on oats, *P. triticini* on wheat, *P. coronifera* on oats, and *P. maydis* on maize. Some attention has been directed to disease-resistant varieties, and stress is laid on the fact that a cereal which may be immune to the attacks of one rust may be very susceptible to infection by another; the practical conclusion is drawn that every effort should be made to obtain disease-resisting varieties, and that the continued growing year after year of the same variety of any cereal should be avoided as much as possible.

The chemical division has been engaged in an examination of soils, and attention is directed to the fact that the soils of the Transvaal are generally well supplied with potash, but are deficient in phosphoric acid, lime, and organic nitrogen. In conjunction with the veterinary division, the chemist has carried out an investigation into the composition of the bone of animals suffering from osteo-porosis, and he finds that affected bones are deficient in total ash, lime, and phosphoric acid. The normal proportion of nitrogen to total ash is about 1 : 14; in diseased animals the proportion is approximately 1 : 11.

The "division of publications" issues a quarterly journal, each number of which extends to some 300 pages; there are two editions, an English of some 8000 copies, and a Dutch of about 2000 copies. The journal contains original articles, notes from the various divisions, extracts from foreign journals and Government circulars, market prices, customs returns and other figures of interest to farmers. In addition to the journal, this division publishes leaflets and bulletins; among the latter, those written by members of the veterinary division upon the common diseases of the live stock of the colony have been of most importance.

It is satisfactory to learn that the work of the department commended itself to the Public Service Commission which inquired into the working of all branches of the Civil Service. The commission report emphasises the importance to the Transvaal of agricultural research, and goes on to state that it "has been impressed by the zeal, devotion, and business-like methods which characterise the Department at present, and that it finds itself unable to suggest any improvements in the organisation, or in the distribution of the business."

THE ARC AND THE SPARK IN RADIO-TELEGRAPHY.[1]

THE discovery by Heinrich Hertz between 1887 and 1889 of experimental means for the production of electric waves, and Branley's discovery that the conductivity of metallic particles is affected by electric waves, form the foundation on which, in 1896, Signor Marconi built up his system of wireless telegraphy.

Many of the early investigators certainly had glimpses of a future system of being able to transmit messages without connecting wires, for as early as 1892 Sir William Crookes predicted in the *Fortnightly Review* the possibility of telegraphy without wires, posts, cables, or any of our costly appliances, and said, granting a few reasonable postulates, the whole thing comes well within the realms of possible fulfilment.

Two years later Sir Oliver Lodge gave his memorable lecture on the work of Hertz, and carried the matter a step nearer the practical stage.

There will not be time to dwell to-night on the early history of the art and its development. It will be necessary, however, to explain some of the fundamental properties of signalling by means of Hertzian waves in order to be able to bring out clearly the relative advantages and disadvantages of the two rival methods now in practical use for producing Hertzian waves for wireless telegraphy.

The fundamental part of the transmitting apparatus may be said to consist of a long conductor generally placed vertically, in which an alternating or oscillating current is set up by some suitable means. Such a conductor radiates energy in the form of Hertzian waves at right angles to itself into space, in very much the same way that an ordinary candle sends out light in all directions. This radiation, though it is strictly in the nature of light, is invisible to our eyes, as the frequency is too low.

If we set up any other conductor approximately parallel to the first, there will be produced in this second conductor alternating or oscillating currents having the same frequency as those in the first conductor, and which can be detected by suitable instruments.

The simplest and one of the earliest methods for producing Hertzian waves for use in wireless telegraphy consisted in charging up, by means of an induction coil, a vertical insulated conductor, which was allowed to discharge itself to earth by means of a spark taking place between its lower end and another conductor which was connected to earth. To detect the Hertzian waves Marconi employed an improved form of the Branley filings tube, which is known as the coherer.

In order to transmit messages the radiation is started and stopped so as to form short and long signals, or dots and dashes of the Morse code, out of which the whole alphabet is built up in the well-known way.

As I have already stated, the radiation takes place round the vertical conductor approximately equally in all directions. Suppose that I set up my transmitting apparatus here in Leicester, a receiving station set up either in Nottingham, Derby, Rugby, or Peterborough would be able to receive the message equally well. Should I wish to send a message from here to Nottingham at the same time that Derby wishes to speak to Rugby, then the receiving station at Nottingham would receive both the message from Leicester which it should receive, and the message from Derby which it was not required to receive.

To get over this difficulty, known as "interference," a large number of devices have been patented. The most successful in practice is syntony, or tuning; in this method each station has allotted to it one definite frequency or tune, and the apparatus is so arranged at each station that it will only be affected by messages which are radiated by other stations on its own frequency or tune, and not by any other radiations. To take a musical analogy, supposing I had somebody who was either deaf to all notes of the piano except, say, the middle "C," or had such a musical ability that he could tell at once when I struck the middle "C," then I could transmit to that person a message in the ordinary Morse code by playing on the middle "C," and that person, whom I shall call Mr. C., would not take any notice of the fact that I might also be playing on the notes D, E, F, G, &c., but Mr. C. would confine his attention entirely to what is being done with the middle "C." It is conceivable that I might find a series of persons or train them so that they could each pick out and hear one note only of the piano, irrespective of what was being played on the other notes or of any other noises that were taking place. Taking an ordinary seven-octave piano, and neglecting for a moment the black notes, this would give me fifty-six distinct notes on which I could transmit messages; so that, transmitting from Leicester, I might send messages simultaneously to fifty-six different towns.

[1] Discourse delivered at the Leicester meeting of the British Association on Friday evening, August 2, by Mr. W. Duddell, F.R.S.

The number of possible simultaneous messages depends on the number of octaves there are on the piano used, and on how close together the different notes are which can be used without producing confusion. For instance, it might be quite easy to train someone to distinguish with certainty between "C" and "E," and pick out signals on "C" at the same time that signals are being sent on "E." It is certainly more difficult to do this with two notes that are closer together, say "C" and "D," and still more difficult if the half-tones are used as well. The problem, therefore, in wireless telegraphy is to arrange the receiving apparatus so that it can hear, or perhaps I should say, more accurately, so that it can only see, notes of one definite frequency or pitch, and not be affected by any other notes, even though of but slightly different pitch. Another requirement to obtain good working is that we should use as little power as possible at our transmitting station consistent with obtaining enough power in our receiving instruments to work them with certainty.

I have a mechanical model to illustrate how we are able to make our receiving instruments very sensitive to one frequency, and only slightly affected by frequencies which differ but slightly from its proper frequency.

The transmitter in the model consists of a disc that can be rotated slowly at any speed I like, with a pin fixed eccentrically on its face. This pin can be connected to a vertical wire which moves up and down as the disc rotates. I shall assume that the movement of this wire corresponds with the movement of the electricity in the vertical conductor. As a receiving apparatus I have a pendulum, and representing the ether between the transmitter and receiver I have an elastic thread connecting the pin in the disc to the pendulum.

When I set the disc rotating slowly the elastic thread is alternately stretched out and relaxed, and the pendulum is a little affected. If I gradually increase the speed of the disc at one definite speed it will be found that the pendulum is set into violent oscillation, and by observation it will be found that when this is the case the disc makes one complete revolution in exactly the same time that the pendulum would make one complete swing if left to itself; that is to say that the disc and the pendulum make the same number of swings per second or have the same frequency; in music they would be said to be in tune with each other. If instead of allowing the disc to rotate continuously I allow it to make only half a dozen revolutions, then the pendulum will be affected, but much less strongly. The greater the number of revolutions the disc makes up to a certain maximum number, the more the pendulum will be caused to swing.

Instead of starting and stopping the disc, I can keep the disc rotating, and start and stop the pulls on the elastic thread by moving the pin in the face of the disc in and out from the centre, which produces a movement which much more nearly corresponds with the actual current in the vertical wire as used in spark telegraphy.

It is necessary here to explain the relationship that exists between the wave-length, the frequency, and the velocity of propagation of Hertzian waves. The waves travel with, as far as we know, the same velocity as light, namely, 300,000,000 metres, or 186,000 miles, per second. Between these quantities we have the relationship that the product of the wave-length by the frequency is equal to the velocity of propagation, or, as I have already mentioned, the velocity of light.

The wave-lengths which are of practical use in wireless telegraphy at the present time range between 100 and 3000 metres, though, of course, it is quite possible to use for special purposes wave-lengths outside these limits. The corresponding frequencies in practical use are therefore between 3,000,000 and 100,000 complete periods per second. We require, therefore, to produce in the vertical conductor alternating or oscillating currents of any frequency within this range, and to have a sufficient number of oscillations following one another without interruption to allow of good syntony being obtained.

There are three methods of producing these currents—namely, the alternator, the spark, and the arc methods.

There are great difficulties in the way of constructing an alternator to give such high-frequency currents, and I can best illustrate this by taking an example. Suppose that it is required to build an alternator to work at the lowest frequency, namely, 100,000 periods per second, and let us assume that we can drive this alternator by means of a turbine at the high speed of 30,000 revolutions per minute. This alternator could not have a diameter much above 6 inches for fear of bursting; and, as it makes 500 revolutions per second, it would have to generate 200 complete periods for each revolution, so that the space available for the windings and poles for one complete period will be less than 1/10 inch, a space into which it is quite impossible to crush the necessary iron and copper to obtain any considerable amount of power. In spite of the small space that we have allotted to each period, as there are 100,000 periods per second, the speed of the surface of the moving part works out at more than 500 miles per hour. A small alternator has been built to give more than 100,000 frequency, but the amount of power it produced was extremely small. Several experimenters have stated lately that they have built alternators giving these high frequencies and a considerable amount of power, but, so far as I am aware, there is no trustworthy data available as to the design of these machines.

If it should prove possible to construct alternators for these very high frequencies, we shall be able to obtain a sufficient number of consecutive oscillations of the current in the aërial of definite frequency to enable very sharp syntony to be obtained. Not only will this greatly reduce interference troubles in wireless telegraphy, but such alternators will be of the greatest value for wireless telephony.

The earliest method of producing high-frequency oscillations was proposed by Lord Kelvin, who pointed out that if a Leyden jar or condenser be allowed to discharge through a circuit possessing self-induction or electrical inertia, then under certain conditions the discharge of the jar is oscillatory, that is to say, that the electricity flows backwards and forwards in the circuit several times before the jar or condenser becomes finally discharged. I think that perhaps the best way to make this matter clear is by demonstrating experimentally with an oscillograph the nature of the discharge of a condenser and how it is affected by the resistance and self-induction in the circuit. As a mechanical analogy one may look upon the charged condenser as a weight attached to a spring which has been pulled away from its position or rest. To discharge the condenser we let go the weight, and it begins to oscillate backwards and forwards, and, after making a greater or less number of oscillations, finally comes to rest. The number of oscillations per second will depend upon the strength of the spring and the mass of the weight, which correspond with the capacity and self-induction in our electrical circuit. The number of oscillations before the weight finally comes to rest is determined by the friction which tends to stop the weight, or by the resistances and other losses in the electrical circuit.

In practice the aërial conductor acts as a Leyden jar or condenser. It is charged with electricity and allowed to discharge, the current oscillating backwards and forwards in the aërial during the discharge. In many installations Leyden jars or condensers are electrically connected to the aërial, so that the oscillations taking place in them are transmitted to the aërial. Any remarks, therefore, that I may make as to the oscillations which may be set up in condensers apply equally well to the oscillations in the aërial in wireless telegraphy.

For wireless telegraphy it is usual to charge the condenser or aërial by means of an induction coil or an alternator to a very high voltage, and it is allowed to discharge by means of a spark between the two electrodes which form the ends, so to speak, of a gap in the electrical circuit. As long as the pressure is low the spark gap is a perfect insulator; when the pressure becomes high enough, the air between the electrodes breaks down and a spark passes the gap, becomes conducting, and allows the condenser to discharge. The property of the spark gap of passing almost instantaneously from a condition of being an insulator for electricity to being an extremely good conductor for electricity is of the utmost value in the spark method of wireless telegraphy. The more perfectly the spark gap insulated before the discharge takes place, and the more perfectly it conducts after the discharge has taken place, the better it is for our purpose.

If I take two electrodes sufficiently far apart in air, and gradually raise the electrical pressure between them, the first indication that anything is going to happen is the formation of fine violet aigrettes on the more pointed or rougher parts of the electrodes. This is known as the brush discharge. By gradually raising the pressure, this brush discharge extends further out into the air, until finally the air between the two electrodes becomes so strained that it breaks down and the real spark passes.

The long thin spark that occurs in this case is not very suitable for wireless telegraphy, as its resistance is too high. Ordinary lightning flashes are good examples of long sparks on a very large scale. If instead of working with the electrodes far apart they are placed nearer together, and if the electrical pressure is supplied from a very powerful source, then directly the spark passes it forms a thick discharge having the appearance of a flame in which the nitrogen of the air is actually being burnt; a process which, it is hoped, in the future may have immense importance in the supply of artificial nitrates for agriculture. This flame-like discharge has a low electrical resistance, but has the effect that it so heats or modifies the air that it is difficult to get the air to insulate again, after one discharge, ready for the next.

If a large quantity of electricity is discharged through the spark gap, and if the spark lasts a very short time compared with the interval between successive sparks, then a highly-conducting spark can be obtained, as well as a good insulation between the sparking terminals when no discharge is passing.

In order to help to bring the gap back to its insulating condition after each discharge, many devices are employed, such as subdividing the spark into several shorter sparks, cooling the electrodes, blowing air across the spark gap, &c. When the condenser or antenna discharges through the spark gap, oscillations are set up which radiate Hertzian waves.

In practice in wireless telegraphy it is difficult to obtain a large number of oscillations during each discharge as corresponding with each oscillation; the antenna radiates energy. A large number of oscillations means, if we keep amplitude of each the same, that we are radiating a large quantity of energy. Besides this radiated energy, which is useful for transmitting messages, there is also energy wasted in heat in the spark gap, in the conductors, in the glass or other insulation of the condensers. It is this useless part which we require to make as small as possible.

I have lately had an opportunity to determine how many oscillations actually take place in a certain wireless transmission. The experiment was made by photographing the spark as seen in a mirror rotated at a very high speed, and it was found that each spark consisted of nine or ten complete oscillations.

If all the oscillations had been of equal strength or amplitude, and if the receiving circuit had been similar to my pendulum in my mechanical model, then there would be very little to be gained by increasing the number of oscillations. As the oscillations die away in the spark method, two or three times this number would probably be required for the best effect. As a matter of experiment, very good tuning was obtained with the wireless transmission referred to above.

As an example of the sharpness of tuning obtainable by the spark method, the following test carried out on the Lodge-Muirhead installation at Hythe may be of interest.

The station at Hythe had to receive messages from Elmer's End at a distance of fifty-eight miles over land, in spite of the fact that the Admiralty station at Dover, only 9¼ miles distant, was transmitting as powerfully as it could, in order to produce interference, and that the regular communications were going on in the Channel between the shipping. It was found possible with a difference of wave-length of 6 per cent. to cut out the interference from the Dover station.

In the arc method of producing continuous oscillations we employ, as before, a condenser and self-induction; but, instead of charging the condenser to a high voltage and allowing it to discharge by means of oscillations which die away, and then repeating the process over and over again, we actually maintain the condenser charging and discharging continuously without any intermission, so that we practically obtained a high-frequency alternating current in the aërial.

To impress the difference on your minds, I have an incandescent lamp, which I switch on and off rapidly about ten times, and then after a short time I repeat the same flickering of the light, and so on. The flickering of the light corresponds with the oscillations in the ordinary spark method, and the time spaces between the flickers represent the times during which the condenser or antenna is being charged ready to produce a fresh series of oscillations. In practice we may have as many as, say, a couple of hundred discharges of the condenser a second, and during each discharge we may get, say, ten complete oscillations, each oscillation lasting one-millionth of a second, if the wave-length is 300 metres; thus the total time that the condenser is discharging is only one one-hundred-thousandth of a second, or the five-hundredth part of the interval of time between two successive discharges. My lamp here flickers about five times per second, and makes ten flickers before it goes out; the total time that it is flickering is two seconds, and the time before it should start to flicker again to correspond with the practical wireless case is therefore 1000 seconds, or rather more than a quarter of an hour. If now I represent continuous oscillations, such as are obtained by the arc method with this lamp, I shall simply keep the lamp flickering continuously, and there will be no intervals whatever.

The arc method of producing continuous oscillations is founded on my musical arc. In order to explain this I must demonstrate some of the properties of the direct-current arc. If I vary the current flowing through the arc very slowly and note the potential difference corresponding with each value of the current, keeping everything else constant, I obtain a curve generally spoken of as the characteristic of the arc. These curves under different conditions have been very thoroughly investigated by Mrs. Ayrton.

With the carbon arc between electrodes in air the voltage decreases very rapidly when the current is gradually increased, starting from very low values. As the current becomes larger the rate of decrease of the voltage becomes less and less until it is, comparatively speaking, quite small, with a current of ten or twelve amperes. With the arc between metal electrodes similar results are obtained, except that the discontinuity in the curves, called the hissing point by Mrs. Ayrton, takes place at very small currents, generally well below one ampere.

With arcs burning in hydrogen, Mr. Upson has found that the curves are generally much steeper for the larger values of the current than for the corresponding arcs burning in air. This point is of great importance as explaining the value of the hydrogenic atmosphere used by Poulsen and referred to later.

In general, I may therefore say for the above arcs that increase in current through the arc is accompanied by decrease of the potential difference between its electrodes, and *vice versâ* decrease of the current causes increase in the potential difference; on the other hand, certain arcs, such as the arc between cored carbons, behave in an opposite manner, that is to say, current and potential difference increase and decrease together.

I demonstrated in 1900 that if I connect between the electrodes of a direct-current arc (or other conductor of electricity for which an increase in current is accompanied by a decrease in potential difference between the terminals) a condenser and a self-induction connected in series, I obtain in this shunt circuit an alternating current. I called this phenomenon the musical arc. The frequency of the alternating current obtained in this shunt circuit depends on the value of the self-induction and the capacity of the condenser, and may practically be calculated by Kelvin's well-known formula.

Besides the condition that an increase of current must be accompanied by a decrease in potential difference, it is necessary that the relative decrease in potential difference produced by a given increase in current, that is to say, the steepness of the characteristic, shall exceed a certain minimum value which depends on the losses in the shunt circuit. It is also necessary that an increase in current shall be accompanied by a decrease in potential difference, even when the current is varied very rapidly.

Let us consider what takes place when I connect this shunt circuit to an arc. At the moment of connection a current flows from the arc circuit into the condenser circuit, which tends to reduce the current flowing through the arc. This reduction of the current through the arc tends to raise the potential difference between its terminals, and causes still more current to flow into the condenser circuit, and I now have a condenser charged above the normal voltage of the arc. The condenser, therefore, begins to discharge through the arc, which increases the arc current and decreases the potential difference, so that the condenser discharges too much; the reverse process then sets in; the condenser becomes successively overcharged and undercharged, due to the fact that, instead of the potential difference between the terminals of the arc remaining constant and allowing the condenser to settle down with its proper corresponding charge, the potential difference actually decreases when the condenser is discharged and increases when it is charging, so as to help to keep up the flowing backwards and forwards of the current indefinitely.

The oscillograph wave forms show what is going on very clearly, and they show that in general the swing of the current in the condenser circuit attains such a magnitude that when the condenser is charging it takes the whole of the current away from the arc, so as to make the arc, although burning on a direct current circuit, a pulsatory arc. The pulsation of the current through the arc causes the vapour column to grow bigger and smaller, and the light to vary. When the vapour column grows bigger and smaller it displaces the air around it and produces a note the pitch of which is determined by the frequency of the current in the shunt circuit.

The values of the capacities of a series of condensers have been calculated by Kelvin's formula to give the frequencies corresponding with a musical octave, and the nearest values in an ordinary laboratory box of condensers have been taken and connected to a keyboard. The result shows how nearly Kelvin's law is obeyed.

With this apparatus I can demonstrate the importance of tuning in electrical circuits, and perform electrically some experiments which I have already performed mechanically earlier this evening. I use the large coil which forms the self-induction in the circuit shunting the arc as a transmitting circuit for wireless telegraphy by the magnetic induction or Preece method; and I have a receiving circuit consisting of a coil of wire connected to a small lamp, and not connected in any way to the transmitting circuit. At a certain short distance between the transmitting coil and the receiving coils, the indicating lamp lights if I cause my arc to sound *any* one of the notes of the octave, and so produce an alternating current of corresponding frequency in the transmitting coil. If I now tune the receiving circuit, by connecting a condenser in it, the lamp on the receiving circuit will light at about five times the distance; but it will only light when one definite note is sounded by the arc. These are the two distinct advantages of tuning, namely, greater distance and syntony, or responding to only one definite note.

For wireless telegraphy by means of Hertzian waves, based on my arc method, we require much higher frequencies in the shunt circuit. If we attempt to obtain this higher frequency from the ordinary arc burning between solid carbons in air, we find that above a certain limit the oscillations will no longer take place. This is due to the fact that we are varying the current through the arc at this higher frequency too quickly for an increase in current to be accompanied by a decrease in potential difference. I have demonstrated that if I only vary the current through the ordinary current arc sufficiently rapidly, then an increase in current is accompanied by a proportionate increase in the potential difference, and the arc behaves just like an ordinary resistance. If we work with very small current arcs, we can obtain high-frequency musical arcs burning in air either between carbon or metal electrodes.

In a paper read before the International Electrical Congress at St. Louis in 1904, Mr. Poulsen showed that, by placing the arc in a flame, it was possible to obtain higher frequencies than when the arc was burning in air. Following this up, Mr. Poulsen came to the conclusion that the best results were obtained when the arc was burning in hydrogen, or a gas containing hydrogen; and he further added a magnetic field around the arc somewhat similar to that which has been previously used by Elihu Thomson.

The arc burning in coal gas in a powerful transverse magnetic field was used by Poulsen in his early experiments to produce the high-frequency current necessary for wireless telegraphy between Lyngby and Esbjerg, in Denmark. This apparatus has been further improved, and is now employed by the Amalgamated Radio-Telegraph Company in their station at Cullercoats and the other stations that they are erecting.

In both the arc and the spark methods of wireless telegraphy we employ a high-frequency alternating current in the aërial conductor. The essential difference between the two methods lies in the fact that with the spark method our alternating current in the aërial conductor first increased to a maximum value and then dies away rapidly, making only a limited number of oscillations, whereas in the arc method the oscillations are maintained continuously of unvarying amplitude.

With the arc method we are further able to choose the number of consecutive oscillations which make up each signal sufficiently great to obtain the very best syntony. On the other hand, improvement in the arrangement and construction of the apparatus for the spark method has so increased the number of oscillations corresponding with each spark that it may be that we shall be able to obtain a sufficient number in each train to give as good syntony by this method as that obtained with the arc method.

The arc method seems eminently suitable for very high speeds of working. As the oscillations are quite continuous, we can cut them up into groups to form the dots and dashes of the Morse alphabet, just as if we were working with a continuous current such as is used on land lines, so that there seems no reason why as high a speed of working should not be obtained from the arc method of wireless telegraphy as is obtainable by automatic signalling on land lines; for it is to be noted that the dot or shortest signal of the Morse alphabet, even at a speed of three or four hundred words per minute, will last long enough to consist of many hundreds of oscillations of the current in the aërial, so that there will be plenty of oscillations in the group forming the dot to give good syntony.

Turning to the spark method for high working speeds, we find a difficulty in that the dot of the Morse alphabet must at least occupy the average time required to charge the condenser or aërial and produce one spark, and preferably sufficiently long for several. We are therefore obliged in the spark method to use a high rate of sparking for high-speed signalling. This difficulty has not become very serious with the present low speeds of sending. When we come to use considerable amounts of power to transmit messages over long distances, and we also require a high speed of working, the practical difficulty in constructing apparatus suitable for sufficiently rapid sparking will become serious.

Mr. Marconi in 1905 claimed to have already reached a speed of 100 words per minute by the spark method, and lately there has appeared in the technical Press examples of high-speed signalling by the British Post Office over a distance of fifteen miles in which readable signals were received at a speed of seventy words per minute.

Turning to the receiving end, almost all the receivers that have been used in the spark method can be equally well used for the arc method; for it must be remembered that the transmission in either case is affected by Hertzian waves traversing space, and that the only fundamental difference consists in the number of oscillations in each train of waves. It must be noted, however, that in those methods in which a telephone receiver is used it is necessary to break up the continuous oscillations of the arc method into groups succeeding one another sufficiently rapidly to produce an audible sound in the receiver; for in the spark method the sounds we hear in the receiver correspond with the succession of impulses of the diagram, one for each spark at the transmitter. This chopping up of the continuous wave-train so as to produce audible signals in the receiving apparatus can be done either at the transmitting end or in the receiving apparatus. An example of this latter method is Poulsen's ticker.

The question whether receiving apparatus can be arranged so as to receive messages from stations equipped with the spark apparatus and from stations equipped with the arc apparatus is a matter of enormous importance at the present moment in view of the probable ratification of the Berlin Convention, which imposes an obligation on all commercial stations to inter-communicate without regard to the make or system of transmitting apparatus employed. I am of the opinion that there will be no difficulty in carrying this into effect provided that the stations using the spark method send out long trains of waves, as they should do to obtain syntonic working, which is also called for by the Berlin Convention.

An extremely interesting development which is now progressing rapidly owing to the possibility of producing continuous oscillations by the arc method is wireless telephony. Suppose that we can vary the intensity of the oscillations in a manner corresponding with the vibrations of the air which constitute sound and speech, then we should obtain at the receiving stations a train of Hertzian waves the amplitude of which varies in a corresponding way; by allowing these waves to act on a telephonic receiver which is sensitive to the intensity of the waves we shall obtain in the telephone a reproduction of the sounds. This has actually been carried into effect by employing an ordinary microphone to modify the current through the transmitting arc so as to vary the intensity of the oscillation current produced, and by employing what is known as a point-detector and a telephone at the receiving station.

Another method which may be used consists in causing the microphone to vary the frequency of the oscillations of the generator, and by arranging the receiver so that it is more or less strongly affected according to the frequency of the received waves.

I am informed that experiments have been made in wireless telephony in Berlin by the Amalgamated Radio-Telegraph Company between their stations in Mathieustrasse and Weissensee, 6·5 km. apart, with good results, and that it is now proposed to equip the stations at Oxford and Cambridge for the further perfecting of this application.

It is greatly to be desired that wireless telephony may develop rapidly, as it seems to me that for the purpose of communicating with ships wireless telephony will have great advantages over wireless telegraphy.

I am deeply indebted to Mr. Colson for all the facilities that he has placed at my disposal, and to his engineers for their assistance, which has enabled me to carry out the experiments in the lecture; and I have also to thank the Tramway Department for the special supply of current.

THE BRITISH ASSOCIATION.

SECTION K.

BOTANY.

OPENING ADDRESS BY PROF. J. B. FARMER, M.A., F.R.S., PRESIDENT OF THE SECTION.

CUSTOM has decreed that those who are charged with the responsibilities that to-day fall to my lot should endeavour to address themselves to the consideration of matters such as they may deem to be of advantage to others, or, at any rate, of interest to themselves. It is not, perhaps, always easy to combine these two courses, and if I choose the less altruistic one I experience the smaller compunction in doing so because the undisturbed repose that most Addresses enjoy when they have been decently put away between the covers of our Annual Report seems to indicate that an attempt to express the passing thought, however ephemeral its interest, may not be the worst introduction to the business of the advancement of our science.

Any attempt to give a survey of the progress and present position of botanical science, even were so large a task at all within my power, has almost ceased to be necessary, owing to the enterprise which has so admirably provided for its adequate fulfilment elsewhere. I propose, therefore, to try to put together, in a form as intelligible as I can, the result of reflections on some of the aspects of botany that are often not seriously regarded; perhaps because they belong rather to the nebulous region of speculation than to the hard (and sometimes dry) ground of accepted fact.

I am by no means blind to the risks incurred in venturing on such a course, but I believe that a glance directed, however imperfectly, towards some of the less obvious sides of our science may not be altogether futile, even though the attempts should evoke the criticism:

Dum vitat humum, nubes et inania captat.

The problems that confront us as botanists are far more numerous and far more complex than formerly. We are attached to a science that is rapidly growing, and this rapid advance is carrying with it a process of corresponding differentiation. Some years ago a danger arose, even within this Association, that we might have replaced differentiation, that quality which distinguishes the higher organisms, by a process of fission which is more characteristic of the lower ranks of life.

The products of the threatened fission would doubtless have pursued divergent paths, and the botanist of to-day would have been the poorer for it. He would have been lost to physiology, and all that physiology implies. Happily that danger was averted, and, to our lasting advantage as members of the botanical organism, our science escaped disruption, and physiological investigation still continues both to inspire, and to be aided by, other branches of botanical research. A physiological conception of morphological phenomena is the one that to me seems to afford the broadest outlook over our territory. It serves to check a tendency towards mere formalism on the one hand and to correct the not less baneful effects of a superficial teleology on the other. Both are real dangers, and we have all encountered examples of them.

In rating highly the value of maintaining a physiological attitude of mind towards the phenomena presented by the vegetable kingdom, one is mainly influenced by the logical necessity which such a position carries with it of constantly attempting to analyse our problems, as far as may be possible, into their chemical and physical components. It seems to me that this is the only really profitable method that we can bring to bear on the difficulties that lie before us, because in using it we are constantly forced to consider the *causes* which have led to the final result. Of course I am well aware that to some minds the very attempt to apply such a method beyond a very limited range may appear futile, or at least premature. But the goal of all scientific inquiry lies in the ultimate ascertaining of cause and effect, and only with this knowledge can we hope to get control over the results.

Chemistry and physics each present to their followers problems far more elementary than those with which we have to grapple; but the explanation of the great advances which these two branches have made lies essentially in the fact that an analysis of the factors involved has enabled the investigator intelligently to interfere with, and so to control, the mode of presentation of the reacting bodies to each other. And our own special problems, whether we confine ourselves to the simpler ones, or whether we approach the obscurer matters of organisation, heredity, and the like, are assuredly susceptible of a similar method of treatment. We can never expect to get further than to be able to modify the mode of presentation to each other of the materials that interact to produce what we call the manifestations of life; but the measure of our achievement will depend on the degree in which we are successful in accomplishing this.

Indeed, until we have analysed the nature of the reacting bodies, and also especially the particular conditions under which the reactions themselves are conducted, we are avoiding the first steps in the direction of ultimate success. At present, when we desire to know the taxonomic value of this or that character, we are perforce largely guided by purely empirical considerations. We find, for example, that a particular structure is very constant through a group of species otherwise closely resembling each other, and we rightly (but quite empirically) regard the possession of that character as a valuable indication of affinity within that alliance. But the very same feature in other groups may be highly variable, and lack all importance amongst them for systematic purposes. It may be, and very probably is, optimistic to look forward to the time when we shall know *why* the character is good in one, and worthless in another, alliance. But when we do, I am convinced that the reason will be found to lie in chemical and physical causes. We are very ignorant as yet of the details, but we can nevertheless even now form a fair guess at their general nature.

In this connection I would venture to express the opinion that much real harm is done by the toleration of an uncritical habit of mind, all too common, as to the significance of structures which are regarded as adaptive responses to stimuli of various sorts. It is *not* enough to explain the appearance of a structure on the ground of its utility; properly speaking, such attempts, so far from providing any explanation, actually tend to bar the way of inquiry just where scientific investigation ought to commence.

That many of the responses to such stimuli are of a kind to render the organism "adapted" to its environment no one, of course, will dispute; but to put forward the *adaptedness* as an explanation of the process is both unscientific and superficial. The size and the spherical shape of duckshot are admirably adapted to the purposes for which duckshot is used; but this affords no insight into the necessary sequence of cause and effect, which makes the melted lead assume the characters in question as it falls down the shot-tower.

But many people still find consolation and satisfaction in an anthropomorphic and somewhat slipshod application of a kind of doctrine of free-will to matters that really call for rigorous examination into the causes which, under given conditions, must inevitably and of necessity bring about their definite result.

One of the commonest responses to the stimulus of wounding in the higher plants is the formation of a layer of cork over the injured and exposed tissue. No one can deny that this is a reaction of great utility, checking as it does the undue evaporation of water and the entrance of other parasitic organisms. And yet I suppose that no one would go so far as seriously to maintain that the obviousness of these advantages satisfactorily explains *why* the cork layer is produced. It seems to me that an investigation of the real underlying conditions which govern such a modified reaction would be of immense value, and that the information we might gain therefrom as to the nature of the chemical processes involved would prove to be of first-rate importance in tracking to their sources some of the factors that influence the course of carbohydrate metabolism within the cell. Again, we know how easy it is to produce colour-changes in the leaves of certain plants—*e.g.*, rhubarb—by severing the vascular bundles, and thereby interfering with the process of translocation. Overton has shown how the accumulation of soluble carbohydrates within the leaf of such a plant as Hydrocharis modifies the metabolic processes within the cells. Thus in bright light, under conditions of cold sufficient to arrest starch formation, but not enough to stop photosynthesis, a red-coloured substance makes its appearance in the cell, and this again disappears on raising the temperature, so that the accumulation of soluble carbohydrates diminishes. The red colour which is associated with the change may possibly by absorbing the heat ray aid in restoring metabolism to its "normal" course; but such a teleological explanation is not of general application, and gives no real insight into the nature of the processes involved. The well-known laboratory method, which we owe to Klebs, of inducing Eurotium to enter on a sexual phase by keeping it at a temperature of 26° C. is another example of the same order. The particular reaction that occurs in each of these instances is that which necessarily results under the specified conditions, and no other course of chemical change is possible.

In the last-mentioned example, Eurotium acts in a way similar to that of drought, only the result is more quickly produced. This perhaps indicates that we are dealing with a definite series of changes which are inhibited by the presence of too much available nutriment supplied at a temperature too low to enable it to be sufficiently rapidly altered within the organism so as to give rise to the specific substance, which is more directly responsible for the ascogonial phase of the life-history. Something of an analogous character is probably effective in the formation of "fairy-rings," so typical of the growth of certain agarics. This appearance of fairy-rings may be easily reproduced in artificial cultures of moulds by appropriate means. Thus if the nutriment agar be kept fairly dry, so that the rate of diffusion of soluble materials is slowed down, it is found that concentric zones of sterile and sporiferous hyphæ regularly alternate with each other. An explanation of this behaviour, which seems most probable, is that the hyphæ, after they have been growing over the substratum for a certain distance, have acquired sufficient raw material to provide for the building-up of the substance which stimulates spore-production. When this has taken place the substance so elaborated is used up and spore-production ceases until a fresh supply of material, under the conditions of the experiment, has been formed to act in its turn as a new stimulus. This suggestion is supported by the interference with the circular form of zones that can be brought about by artificially interfering with the rate of diffusion of the supply of nutriment in the jelly. The rhythmical alternation of sterile and fertile zones seems to prove that *quantity* of elaborated material is an essential factor in the process, just as in the stimulation of a motile organ the stimulus itself has to reach a certain minimal intensity in order to cause a movement.

The parallelism between the nutritive, *i.e.*, the chemical, stimulus in the case of the fungus and the minimal time-stimulus required to provoke geotropic movement is very striking. For it will be remembered that there is evidence in the latter instance also of the occurrence of a definite chemical change as the result of the disturbance of normal gravitational relations. This finds expression in the accumulation of homogentisinic acid as the result of the formation of an anti-oxidative substance which arrests the complete disruption of tyrosin in the cells. Whether this is the immediate cause of the geotropic movement, or merely a concomitant of it, we cannot settle at present. But it is of the highest interest to know that chemical change is initiated as a result of the external gravitational impulse, even when the latter is of too short duration to produce an actual geotropic movement. And although we may not at present be able to identify the exact material which is directly concerned in these stimulatory or formative processes, we have, as it seems to me, irresistible evidence in favour of its real existence. It is more than mere analogy that leads us to believe that the various kinds of galls, for example, that may be formed on an oak leaf owe their formation to the specific interference of the secretion of the grub with the higher metabolic processes going on in the cells of the leaf.

I have alluded to the different conditions under which given reagents may interact, and these may in turn very materially affect the final result by modifying the course of the reaction itself. We are coming to realise the fact that the physical conditions of the cellular constituents exercise an important influence on the course of chemical activity manifested within their range. We all know what an important part water plays in ordinary chemical reactions, but the water question assumes a special prominence when the reactions are going on in a colloidal matrix, or rather in a mixture of colloids, such as the various proteins that occur in the cell. Questions of rates of diffusion, physical absorption, and the like have to be taken into account; and beyond all these there remain the series of remarkable electrical relations which the proteins exhibit, as well as those changes in surface-tension that are, in part at least, connected with them.

It is impossible to resist the belief that a closer study of the physico-chemical changes that accompany a nuclear division will yet throw much light on the mechanics of this wonderful process. Indeed, we already possess some data which are serving as starting-points for further investigation, and they have placed some of the known facts in a very suggestive light.

It has often been urged as a reproach against the histological methods employed in the study of the cell that all such investigations can, after all, only give information as to the character of coagulations or precipitations. Of course this is perfectly true; but provided we have sufficiently good grounds for enabling us to feel confident that the precipitation or coagulation faithfully maps out the positions originally occupied by the respective colloids during life, there is no real force in the objection. No one would call in question the accuracy of a photographic negative on the ground that after development it no longer consisted of the actual substances which had been formed in the film by the exposure to the action of light. All that is required is that the deposited silver shall accurately express the limits of, and be proportionate in amount to, the alteration in the composition of the salt which was produced when the plate was exposed in the camera.

Much of the general detail of a nuclear division can be followed even in the living cell, and we therefore possess direct as well as indirect means of testing the degree of accuracy with which the fixed preparation represents the original pattern of distribution of the colloids within the cell. No one who has studied the behaviour of artificially prepared mixtures, the colloidal proteins and nucleins after "fixing" and staining them, can entertain reasonable doubts as to the substantial identity of the structures visible in a well-fixed cytological preparation with those present during life. For the substances, even in these artificial mixtures, keep remarkably distinct, as indeed Fischer showed some years ago.

Few things are more striking than the remarkable series of evolutions passed through by the linin, and by the chromosomes which finally emerge from it during the progress of a mitosis. We have clear evidence that the nucleus at this period is the seat of rapid chemical change. The process of distribution of the nuclein within the linin is sufficient proof in itself of this. But we have also, I believe, evidence of physical disturbances of an electrical nature which accompany, and indeed in a measure determine, the course of mitosis. This is indicated, not only by the movements that proceed within the nucleus, and concern the linin and chromosomes, but also by the remarkable alterations in surface-tension exhibited by the nuclear membrane.

It is well known that at a certain stage of the heterotype division, for example, the chromosomes move to the periphery of the nucleus, and each one is removed as far as possible from every other chromosome. At this stage, to which Haecker has given the name of "diakinesis," the nucleus reaches its maximal size. Diakinesis is not the only stage in which there is an indication of repulsion between the elements of the chromatic linin. Measurements prove that all such periods of repulsion are also marked by an increase of nuclear size which is transitory, and either disappears or alters in a synchronous fashion with them. These phases of enlargement have been generally regarded as directly connected with the intake of liquid by the nucleus, due to a hypothetical change in osmotic conditions. But, so far as I am aware, no satisfactory explanation has yet been given as to why, or how, the supposed increase of osmotically active molecules within the assumed semi-permeable nuclear membrane could be effected. On the other hand, an enlargement of the surface-membrane of the nucleus would necessarily follow on the migration towards it of chromosomes or other bodies carrying similar electrical charges. For the induced charge in the particles of the membrane would of course weaken its coherence, for the same reason that the free chromosomes repel and move away from one another.

There is evidence to show that the proteins *are* able to carry such charges, and this is a matter of the highest importance as affording a clue to many other processes in which changes of surface-tension play a part, besides those connected with nuclear division.

Not the least of the many remarkable properties exhibited by the proteins lies in their capacity of taking on either a positive or a negative charge of electricity. A clear proof of this was afforded by the beautiful experiments of Billitzer, who showed that, when so charged, the colloid moves as a whole towards one pole or the other on sending a current through the liquid in which it was suspended. At first sight it may not be easy to understand how it is possible for a colloid to receive and retain a charge under the conditions which obtain either in the solution or in the cell. It must, however, be remembered that the liquid contains electrolytes in solution also, and any disturbance in the equilibrium of the products of ionic dissociation will be accompanied by corresponding differences of potential. The most reasonable explanation of the phenomenon in question seems to be that the colloids are unequally permeable to the ions, whereby there comes to be a preponderance of one or the other group associated with the proteins. Perhaps this should be connected with the remarkable though still imperfectly understood property of adsorption which is characteristic of many colloids.

Much, however, still remains to be done before a complete survey of the electrical changes that are associated with mitosis can be made. We especially desire more complete information on the nature of the chemical processes which are involved. For it is obvious that the physical changes must ultimately be connected with the transformation of materials which goes on so energetically at these recurrent periods of nuclear activity. We do not yet know how or why the chromosomes that have been dispersed at diakinesis should again congregate on the spindle prior to their final separation. Possibly this is to be connected with the signs of disturbance in the extra-nuclear cytoplasm, which in its turn finds expression in the differentiation of the achromatic spindle. The character of this body has long aroused the suspicion that its existence is to be attributed to electrical causes. The more recent work serves to indicate that this suspicion was well founded.

The more complete study of the chemistry and physics of karyokinesis is certain to prove valuable for another reason. The successive changes which the nuclei of both animals and plants exhibit when they are undergoing division are so remarkably similar that it seems exceedingly probable that the processes actually involved may turn out to be relatively simple, at any rate in their broader features. I mean that they probably belong to what we might term the lower grade of metabolic problems. For the great uniformity of the process as a whole, complex though it undoubtedly is, hardly suggests direct relations as existing between it and those more specialised forms of metabolism on which the properties of specific form, and such like characters, depend. This view of the matter is not in any way weakened by the fact that the materials providing for the multiplication of nuclei have themselves passed through the very highest stages of anabolic construction. There are, indeed, some grounds for believing that the composition of the higher proteins is distinctly specific for different groups of organisms; but apart from this it is difficult to resist the conviction that, in so far as its essential constituents are concerned, the nucleus is the seat of a complex organisation which is superadded to its chemical composition. But this conception of the nucleus does not affect the position of the lower-grade chemical changes, with their physical accompaniments which are periodically rendered apparent during the rhythmical series of changes that culminate in the division of the nucleus. It is true that there are some who refuse to admit the necessity of what I might perhaps call architectural complexity in protoplasm. They prefer to regard all the phenomena of organisation and heredity as the outcome of dynamical, rather than of structural, conditions. It seems to me that it is impossible to reconcile such a view with the known facts respecting the inheritance of characters, and that we are driven to postulate the existence of material units which are to either responsible for the sum of the characters represented in any individual. There are grounds for believing that their entities, whatever be their nature, are doubled, and then equally distributed to the two daughter cells at every ordinary nuclear division; and thus the properties of organisation are preserved and transmitted over and above the flux of chemical change.

Most people who have concerned themselves with cytological studies agree that the salient features of karyokinesis strongly emphasise the probability of a conservation of definite material; and that an extremely accurate distribution of it occurs where two daughter cells arise from a parent cell by division. And this inference is greatly strengthened by what occurs, more or less immediately, in connection with the formation of the sexual cells. The origin of these in all the higher animals and plants, as is well known, can invariably be traced to a nuclear division of remarkable complexity. In this, the so-called heterotype division, the special feature consists in the sorting-out of the nuclear constituents originally furnished by the two parents of the individual. This sorting or distribution takes place in such a way that each of the two daughter nuclei which arise as the result of the division receives only half the total number of chromosomes previously contributed by the two parents. The essential point of interest lies in the fact that the process does not consist in the mere halving of nuclear *substance*, but in the distribution of nuclear *constituents*. When two sexual cells which have been formed in this way unite to give rise to a new individual, the total number of nuclear chromosomes is again made good; but the resulting nuclear constitution will not exactly resemble that of either parent. That such is really the case is borne out by innumerable experiments that have been made by breeders. Furthermore the extensive investigations on the results of crosses, both in animals and plants, have confirmed the view that particular characters can be treated as entities. For they are distributed amongst the posterity of the original parents in proportions that closely approximate to mathematical expectation. In this distribution the separate characters behave independently. For instance, the green colour and round form of peas are two characters which may occur in the same or in different individuals. The numerical proportions in which they *will* appear can be foretold with a considerable degree of accuracy.

With these facts before us—and many others could be adduced, all pointing in the same direction—it is not easy to resist the conviction that within the nucleus there must exist material entities which are severally responsible for the appearance of the characteristic traits of any given individual. The question is, What conception can we form as to their nature, and how are they able to produce the observed results? It is not necessary to discuss the evidence that the chromosomes, or the materials of which they are composed, play a most important part in connection with development. All the work of the last decades has tended to emphasise their importance in the transmission of hereditary qualities, and this is equivalent to admitting that they contain factors that determine the path of development, and are responsible for the production, from the egg, of the form and structure of the adult.

Now it is certain that it is not the *chromosome-substance acting as a whole* which is effective in those processes summed up in the term Ontogeny. It might be, and until recently was, thought that in those plants in which there is a marked alternation of generations a definite relation existed between the number of the chromosomes and the particular stage of the life-history. The double number was supposed to be essential for the sporophyte, whilst the halved number was similarly regarded as causally related with the appearance of the gametophyte or prothallial generation.

But Loeb and others had already shown that the eggs of echinoderms might be stimulated to parthenogenetic development by means other than fertilisation, and Wilson found that such larvæ only contained the half number of nuclear chromosomes, as, indeed, was only to be expected. But the idea of a close parallelism between chromosome number and the alternative phases of the life-history was so deeply rooted that the full significance of Wilson's discovery was not at once grasped. The comparative neglect was, perhaps, partly justified, inasmuch as the larvæ could not be reared. It may, however, be incidentally remarked that no one, so far as I am aware, has yet succeeded in raising the *normal* echinoderm larva beyond the pluteus stage.

The investigation of cases of apospory that occur in the pteridophytes have proved that no causal relation can exist between the number of the chromosomes and the characters that distinguish the gametophyte and the sporophyte respectively. For the sporophyte may give rise to the gametophyte aposporously without any reduction, whilst the various types of apogamy with which we are now acquainted exhibit all gradations between a coalescence of more or less differentiated nuclei and the complete absence of all semblance of nuclear fusion. In the latter case, when the sporophyte springs from a gametophyte that has itself arisen after nuclear reduction, the sporophyte continues to retain the smaller number of chromosomes normally associated with the other generation only.

We thus have a complete proof that a single sexual cell which has undergone reduction in the number of its chromosomes retains, in so far as its architectural configuration is concerned, the capacity of giving rise to a plant possessed of the full complement of characters belonging to the species. But this, after all, is only what the facts of heredity might have led us to anticipate. For, whilst we are ignorant of the fundamental *significance* of the sexual fusion of the gametes, one of its most obvious *results* consists in the duplication of the primordia of the specific characters in the cells of the individual thus produced. This statement is not only in accord with results of experiments in breeding, but it is also in harmony with the essential features of the heterotype mitosis; and no other satisfactory interpretation of the latter series of phenomena has yet been found.

Furthermore, the facts of Mendelian dominance clearly show that each parent, through the gametes to which it gives rise, contributes an independent organisation responsible for at least some of its own distinctive characters, as well as those which distinguish the species. Consequently, when two gametes fuse, the embryo will be provided with a duplicate stock of agents or primordia which determine the appearance of its own specific and individual characters. These will not always be similar in the two parents, and when this is the case it often happens that the offspring resembles one parent only in respect of a particular feature. Nevertheless the results of further breeding show that the corresponding, but apparently lost, character only is latent, for it reappears in a proportion—and often a fixed proportion—of the individuals of the succeeding generations. In such an example, where both agents or primordia are present, one of them lies dormant, whilst the dominant one alone influences the course of metabolic processes, and thus brings about the appearance of the character itself. The dormant primordium can be transmitted as such through many generations, betraying its existence in each by the occurrence of individuals in which it finds its perfect expression. This happens when the opposite dominant agent or primordium has been removed from some of the gametes by the sorting-out process during the heterotype mitosis to which I have already alluded.

The particulate character of inheritance seems, as many writers have pointed out, to demand a structural organisation for its basis; and the units or primordia of which the latter is composed must be relatively permanent, inasmuch as heredity itself is so stable. The agents or primordia themselves probably act by definitely influencing the course of chemical reactions that proceed within the living protoplasm, somewhat after the fashion of the ferments. But whether this influence on the course of metabolism is to be attributed more directly to the chemical or the physical aspect of the organisation must, of course, remain an open question, though I incline to the latter alternative on grounds which I have already indicated.

The processes of the higher metabolism offer suggestive analogies with those reactions for which the ferments are responsible. In contemplating them one can hardly fail to be struck by the orderly way in which ferment succeeds ferment on an appropriate medium. Each one produces its own special change, which it is unable to carry further itself, but it thereby provides a substratum suitable for its successor. Starting, for example, with a complex substance like cane sugar, we see it acted on by a series of ferments, each the result of protoplasmic differentiation, and each one carrying the process of disintegration a little further, but strictly limited in its power to act, and only able to take the change on to a definite stage.

Everyone who has experimented with plants with the view of inducing the formation of some structure foreign to the species or individual by artificial means must have become impressed by the great difficulty of getting into touch, so to speak, with the higher metabolism at all. It is often easy enough to divert the life-history into either the vegetative or the reproductive channel, as every gardener is more or less consciously aware, and as Klebs has conclusively shown in his remarkable series of carefully conducted experiments. But even here it is sometimes difficult exactly to hit off the conditions requisite to ensure the production of one or other of the various phases of the life-history. There are many fungi, for example, which are believed to represent vegetative stages of Ascomycetes or Basidiomycetes, but it has not yet been found possible to ascertain the conditions that would cause them to form the highest fructifications. Even in simpler instances a similar difficulty is sometimes encountered. Thus *Bispora moniliforme*, a mould that often occurs on the wood and stumps of oak or hornbeam, is not readily cultivated as the Bispora form, whether it be grown on wood or on various nutritive media. The usual result of raising it under artificial conditions is to obtain a luxuriant crop of Eurotium-like mould. But the Bispora form can be reproduced from such a culture by growing it in strong solutions of cane sugar under certain conditions, all of which are not as yet understood.

I take it we shall agree that the properties of structure and form are to be interpreted as the necessary result of the action of particular substances on the protoplasm, and that these cause it to assume those definite attributes which we term specific on account of their constancy through a larger or smaller range of individuals. But this constancy of form must then be the result of a corresponding definiteness in the series of changes undergone by the raw materials supplied as food in their upward transformations; each stage in the process limits the possible range of those that follow, as in the case of the ferments to which I have alluded; and thus it becomes increasingly difficult to modify the final result.

In this way we may see, perhaps, an explanation of the circumstance that in amphibious plants the particular structure, whether adapted for land or water, that will arise in conformity with the environment is irrevocably determined long before the organs themselves are sufficiently developed to be exposed to the direct influence of the conditions to which they are supposed to be specially adapted.

Now it is a matter of common knowledge that the formative processes can be, and sometimes *are*, disturbed with the most surprising results. I may again refer to the fungal or insect galls as examples that will be familiar to everyone. It appears to me that these exceptional developments are of extraordinary importance in relation to any endeavour to probe the mysteries of organisation. The very difficulty experienced in imitating the effect of the insect's secretion strongly emphasises the specialised nature of the particular substance which is able to modify the "normal" reactions of the plant. The latter are dependent on the way in which the organic apparatus determines the fashion of the molecular presentations, so that, as I have said, the course of the reactions themselves become increasingly limited in their range. Now as regards the manner in which the secretion of the insect operates, it seems clear that it can produce no permanent change in the organising apparatus of the protoplasm, since the growth is at once arrested on the removal or death of the insect. But whether the influence is one that more directly affects the physical state of the apparatus for the time being, or whether it acts more directly by introducing new substances into the final chemical reactions, are questions which are plainly worth investigation, but at present certainly do not admit of an answer.

Another example of interference with the developmental processes is afforded by the well-known "lithium larva," which was discovered by Herbst to arise when the eggs of some species of sea-urchins are allowed to segment in sea-water that has been altered by the addition of lithium salts. The monstrosity produced under these conditions was just as constant and specific in character as are the different galls which can be induced to develop on an oak leaf by the corresponding species of insect.

Extending these considerations a little further, one sees that what we call disease also falls into the same category. For disease represents the necessary outcome of a disturbance, however introduced, into the course of metabolism, which diverts it from the "normal" channels. Pathology has long recognised that the explanation and the consequent control of disease lies, ultimately, in the correct appreciation of the cellular reactions as the result of their experimental study. We cannot pride ourselves on the advances that have been made in the study of plant pathology as yet. Our remedies are commonly of the crudest kind, and we have only recently begun to take serious count of the facts of organisation in the scientific attempt to breed races of plants immune from the attack of certain diseases. The results that have already been obtained, both abroad and by Biffen and others in this country, are full of hope at the present time. The study of the causes of immunity along scientific lines ought assuredly to form a fruitful field of investigation in the near future.

From what we already know it seems clear that the proximate causes of immunity may be diverse in character, and may consist in very different reactions in different cases. It may be that the response becomes expressed in a modification of the carbohydrate metabolism, leading to the formation of an excluding layer of cork; or it may lie in the direction of those substances, as yet so little understood, the anti-toxins; or, again, it may be due to still other and even less apparent causes. But whatever the true nature of the response, it will have to be investigated for individual cases, and its secrets will only be unlocked when the chemical and physical processes involved in its operation are understood.

In making these remarks I dare say I may be accused of putting forward an impossible ideal, or at any rate one that is impracticable of attainment. I am not very much concerned about that. Progress is only to be made by trying to penetrate further than we can at present see, and I believe we have gained enough insight into the chemistry and physics of the living processes to warrant us in hoping that we shall penetrate a good deal deeper still. But if we are to ever unravel the tangle, it can only be by applying such methods as have been successful in dealing with material things elsewhere.

For the problems that rise up before us are seen, as we become able to get at close quarters with them, to resolve themselves more and more into questions of chemistry and physics. I believe that it is only by the help of these elder branches of science that the accurate formulation, to say nothing of the final solution, of the problems will be achieved. A recent writer has suggested that life is not the cause of the reactions underlying the phenomena of life. Nevertheless the reactions that go on in the living body are obviously guided as to the particular directions they take by the apparatus or mechanism of the individual organism. When the conditions for the manifestation of life, and all that it implies, are satisfied, what will be produced depends partly on the structure of the apparatus itself (*i.e.*, on the hereditary organisation), partly on the nature of the substances fed into the apparatus, and finally on the physical conditions under which it is working. It is probably along the last two lines that investigation will continue to be pursued with more immediate profit; but the goal will not be finally reached until we have solved the problem as to the nature of organisation itself.

SECTION L.

EDUCATIONAL SCIENCE.

Opening Address by Sir Philip Magnus, B.Sc., B.A., M.P., President of the Section.

The Application of Scientific Method to Educational Problems.

Notwithstanding the fact that the greater part of my life has been spent in educational work, in teaching, in examining, in organisation, and in the investigation of foreign systems of instruction, I have experienced considerable difficulty in selecting, from the large number of subjects that crowd upon me, a suitable one on which to address you as President of a Section of the British Association devoted to educational science.

At the outset I am troubled by the title of the section over which I have the honour to preside. I cannot refrain from asking myself the question, Is there an educational Science, and if so, what is its scope and on what foundations does it rest? The object of the British Association is the advancement of Science, and year by year new facts are recorded in different branches of inquiry, on which fresh conclusions can be based. The progress of past years, whether in Chemistry, Physics or Biology, can be stated. Can the same be said, and in the same sense, of Education? It is true that the area of educational influence is being constantly extended. Schools of every type and grade are multiplied, but is there any corresponding advance in our knowledge of the principles that should govern and determine our educational efforts, or which can justify us in describing such knowledge as Science? If we take Science to mean, as commonly understood, organised knowledge, and if we are to test the claim of any body of facts and principles to be regarded as Science by the ability to predict, which the knowledge of those facts and principles confers, can we say that there exists an organised and orderly arrangement of educational truths, or that we can logically, by any causative sequence, connect training and character either in the individual or in the nation? Can we indicate, with any approach to certainty, the effects on either the one or the other of any particular scheme of education which may be provided? It is very doubtful whether we can say that educational science is yet sufficiently advanced to satisfy these tests.

But although education may not yet fulfil all the conditions which justify its claim to be regarded as a science, we are able to affirm that the methods of science, applicable to investigations in other branches of knowledge, are equally applicable to the elucidation of educational problems. To have reached this position is to have made some progress. For we now see that if we are ever to succeed in arriving at fixed principles for guidance in determining the many difficult and intricate questions which arise in connection with the provision of a national system of education, or the solution of educational problems, we must proceed by the same methods of logical inquiry as we should adopt in investigating any other subject-matter.

In order to bring Education within the range of subjects which should occupy a place in the work of this Association, our first efforts should be directed towards obtaining a sufficient body of information from all available sources, past and present, to afford data for the comparisons on which our conclusions may be based. One of the five articles of what is known as the Japanese Imperial Oath states, "Knowledge shall be sought for throughout the whole world, so that the welfare of the Empire may be promoted"; and it may certainly be said that, as the welfare of our own Empire is largely dependent on educational progress, a wide knowledge of matters connected with Education is indispensable, if we are to make advances with any feeling of certainty that we are moving on the right lines.

There can be no doubt that of late years we have acquired a mass of valuable information on all sorts of educational questions. We are greatly indebted for much of our knowledge of what is being done in foreign countries to the Reports of different Commissions, and more particularly to those special reports issued from the Board of Education, first under the direction of my predecessor in this Chair, Prof. Sadler, and latterly of his successor at the Board, Dr. Heath. But much of the information we have obtained is still awaiting the hand of the scientific worker to be properly coordinated and arranged. A careful collation of facts is indispensable if we are to deduce from them useful principles for our guidance, and unfortunately we in this country are too apt to rest content when we have provided the machinery for the acquisition of such facts without taking the necessary steps to compare, to coordinate, and to arrange them on some scientific principle for future use. Within the last week or two a Bill has passed through several stages in Parliament for requiring Local Authorities to undertake the medical inspection of school children, but, unless the medical inspectors throughout the country conduct their investigations on certain well-considered lines laid down for them by some Central Authority, we shall fail to obtain the necessary data to enable us to associate educational and physical conditions with a view to the improvement of the training given in our schools.[1] On the other hand, although I personally am sceptical as to the results, we have reason to believe that the inquiry recently undertaken into the methods adopted here and elsewhere for securing ethical as distinct from specifically religious training will be so conducted as to give us not only facts, but the means of inferring from those facts certain trustworthy conclusions.

The consideration of Education as a subject capable of scientific investigation is complicated by the fact that it necessarily involves a relation—the relation of the child or adult to his surroundings. It cannot be adequately considered apart from that relation. We may make a study of the conditions of the physical, intellectual, and ethical development of the child, but the knowledge so obtained is only useful to the educator when considered in connection with his environment and future needs, and the means to be adopted to enable him, as he grows in physical, intellectual, and moral strength, to obtain a mastery over the things external to him. Education must be so directed as to prove the proposition that "Knowledge is Power." It can only be scientifically treated when so considered. Education is imperfectly described when regarded as the means of drawing out and strengthening a child's faculties. It is more than this. Any practical definition takes into consideration the social and economic conditions in which the child is being trained, and the means of developing his faculties with a view to the attainment of certain ends.

It is in Germany that this fact has received the highest recognition and the widest application, and for this reason we have been accustomed to look to that country for guidance in the organisation of our schools. We have looked to Germany because we perceived that some relation had been there established between the teaching given to the people and their industrial and social needs; and further, that their success in commerce, in military and other pursuits was largely due to the training provided in their schools. Unmindful of the fact that Education is a relation, and that consequently the same system of education is not equally applicable to different conditions, there were many in this country who were only too ready to recommend the adoption of German methods in our own schools. Experience soon showed, however, that what may have been good for Germany did not apply to England, and that, in educational matters certainly, we do well to follow Emerson, who, when addressing his fellow citizens, declared: "We will walk on our own feet; we will work with our own hands, and we will speak our own minds." Still, the example of Germany and the detailed information which we have obtained as to her school organisation and methods of instruction have been serviceable to us.

Whilst all information on educational subjects is valuable, I am disposed to think that in our efforts to construct an educational science we may gain more by inquiring what has been effected in some of the newer countries. Wherever educational problems have been carefully considered and schemes have been introduced with the express intention and design of training citizens for the service of the State and of increasing knowledge with a view to such service, those schemes may be studied with advantage. Thus we may learn much from what is now being done in our Colonies. Their efforts are more in the nature of experiments. Our Colonies have been wise enough not to imitate too closely our own or any foreign system. They have started afresh, free from prejudice and traditions, and it is for this reason that I look forward with interest to the closer connection in educational matters of the Colonies with the mother country, and I believe that we shall gain much knowledge and valuable experience from the discussions of the Federal Conference which has recently been held in London, and which, I understand, is to be repeated a few years hence.

But valuable as are the facts, properly collated and systematically arranged, which a knowledge of British and foreign methods may afford us in dealing scientifically with any educational problem, it is essential that we should be able to test and to supplement the conclusions based on such knowledge, whenever it is possible, by direct experiments, applicable to the matter under investigation. We have not yet recognised the extent to which experiments in education, as in other branches of know-

[1] Since this was written the President of the Board of Education has stated in the House of Commons that "it was the intention of the Board, if the Bill now before Parliament passed, to establish a medical bureau, which would guide and advise the local authorities as to the nature of the work they would have to do under the Act."

ledge, may help in enabling us to build up an educational science. Some years since there was established in Brussels an *École modèle* in which educational experiments were tried. I visited the school in the year 1880, and I could easily point to many improvements in primary education which found their way from that school through the schools of Belgium and France to our own country, and, indeed, to other parts of the world. From a special Report on Schools in the North of Europe, recently published by the Board of Education, we learn that in Sweden the value of such experiments is fully recognised. We are told that in that country "it was early felt that the uniformity in State Schools was of so strict a kind that some special provision should be made for carrying out educational experiments," and experiments in many directions have been made, mainly in private schools, which receive, however, special subventions from the State. We gather from the same Report that the State regards the money as well earned "if the school occasionally originates new methods from which the schools can derive profit." I venture to think that experimental schools might with advantage be organised under the direction of some of our larger local authorities. The children would certainly not suffer by being made the subjects of such experiments. The intelligent teaching which they would receive—for it is only the most capable teachers who should be trusted with such experiments—would more than compensate for any diminution in the amount of knowledge which the children might acquire, and indeed such experimental schools might be conducted under conditions which would ensure sound instruction. Many improved methods of teaching are constantly advocated, but fail to be adopted because there is no opportunity of giving them a fair trial. As a general rule it is only by the effort of private individuals or associations that changes in system are effected, and teachers are enabled to escape from the old grooves on to new lines of educational thought and practice. It is not difficult to refer to many successful experiments. The general introduction into our schools of manual training was the direct result of experiments carefully arranged and conducted by a Joint Committee of the City Guilds and the late London School Board. Experiments in the methods of teaching Physical Science, Chemistry, and Geometry have been tried, with results that have led to changes which have revolutionised the teaching of those subjects. The age at which the study of Latin should be commenced with a view to the general education of the scholar has been the subject of frequent trial. I would like to see such experiments more systematically organised, and I am quite certain that the curriculum of our rural and of our urban schools would soon undergo very considerable changes, if the suggestions of competent authorities could receive a fair trial under conditions that would leave no manner of doubt as to the character of the results.

It would seem, therefore, that if our knowledge of the facts and principles of education is not yet sufficiently organised to enable us to determine *a priori* the effect on individual or national character of any suggested changes, education is a subject that may be studied and improved by the application to it of scientific method, by accurate observation of what is going on around us, and by experiments thoughtfully conducted. This is the justification of the inclusion of the subject among those that occupy the attention of a separate section of this Association. Our aim here should be to apply to educational problems the well-known canons of scientific inquiry; and, seeing that the conditions under which alone any investigation can be conducted are in themselves both numerous and complicated, it is essential that we should endeavour to liberate, as far as possible, the discussion of the subject from all political considerations. Such investigations are necessarily difficult. We have to determine both statically and dynamically the physical, mental, and moral condition of the child in relation to his activities and surroundings, and we have further to discover how he is influenced by them, how he can affect them, and the character of the training which will best enable him to utilise his experiences, and to add something to the knowledge of to-day for future service.

Notwithstanding the undoubted progress which we have made, it cannot be denied that in this country there still exists a large amount of educational unrest, of dissatisfaction with the results of our efforts during the last thirty years. This is partly due to the fact that there is much loose thinking and uninformed expression of opinion on educational questions. No one knows so little as not to believe that his own opinion is worth as much as another's on matters relating to the education of the people. In this way statements, the value of which has not been tested, pass current as ascertained knowledge, and very often ill-considered legislation follows. In this country, too, the difficulty of breaking away from ancient modes of thought is a great drawback to educational progress. Suggestions for moderate changes, which have been most carefully considered, are deferred and decried if they depart, to any great extent, from established custom, and the objection to change very often rests on no historical foundation. Occasionally, too, the change proposed is itself only a reversion to a previous practice, which was rudely broken by thoughtless and unscientific reformers. The opposition which was so long raised to the establishment of local universities was largely due to want of knowledge on the subject; and certainly the creation, some seventy years ago, of a teaching University in London was actually hindered through a mere prejudice, which broader views as to the real purposes of University teaching and fuller information on the course of University development would have removed.

There never was a time perhaps when it was more necessary than now that education should be regarded dispassionately, apart from political bias, as a matter of vital interest to the people as a whole. Education nowadays is a question which affects not only the life of a few privileged, selected persons, but of the entire body of citizens. The progress that has been made during the last few years in nationalising our education has been very rapid. It may be that it has been too rapid, that sufficient thought has not been given to the altered social and industrial conditions which have to be considered. We have witnessed a strong desire and a successful effort to multiply Secondary and Technical Schools and to open more widely the portals of our Universities. The object of the desire is good in itself. As the people grow in knowledge the demand for higher education will increase; but the serious question to be considered is whether the kind of education which was supplied in schools, founded centuries ago to meet requirements very different from our own, is equally well adapted to the conditions which have arisen in a state of society having other needs and new ideals. Very rightly our students in training for the profession of teachers are expected to study the writings of Locke, Rousseau, Milton, Montaigne, and others; but many are apt to overlook the fact that these writers had in view a different kind of education from that in which modern teachers are engaged, and that their suggestions, excellent as many of them are, were mainly applicable to the instruction to be given by a tutor to his private pupil, and had little or no reference to the teaching of the children of the people in schools expressly organised for the education of the many. Only recently have we come to realise that a democratic system of education, a system intended to provide an intellectual and moral training for all citizens of the State, and so organised that, apart from any consideration of social position or pecuniary means, it affords facilities for the full development of capacity and skill wherever they may occur, must be essentially different in its aims and methods from that under which many of us now living have been trained. It has also been brought home to us that the marvellous changes in our environment, in the conditions under which we live and work, whether in the field, the factory, or the office, have necessitated corresponding changes in the education to be provided as a preparation for the several different pursuits in which the people generally are occupied. Yet, notwithstanding these great forces which have broken in upon and disturbed our former ideals, forces the strength and far-reaching effects of which we readily admit, we still hesitate to face the newly arisen circumstances and to adapt our educational work to its vastly extended area of operation and to the altered conditions and requirements of modern life.

When I say we hesitate to face the existing circumstances I do not wish to be misunderstood. As a fact, changes are continually being discussed, and are from time to time introduced into our schools. But such modifications of our existing methods are generally isolated and detached, and have little reference to the more comprehensive measures of reform which are now needed to bring our teaching into closer relation with the changed conditions of existence consequent on the alterations that have taken place in our social life and surroundings.

Four years ago, it will be remembered, a committee of this section was appointed to consider and to report upon the "Courses of Experimental, Observational, and Practical Studies most suitable for Elementary Schools." That committee, of which I had the honour to be chairman, presented a report to this section at the meeting of the Association held last year at York. The general conclusion at which they arrived was that "the intellectual and moral training, and indeed to some extent the physical training, of boys and girls between the ages of seven and fourteen would be greatly improved if active and constructive work on the part of the children were largely substituted for ordinary class teaching, and if much of the present instruction were made to arise incidentally out of, and to be centred around, such work." It is too early, perhaps, to expect that the suggestions made in that report should have borne fruit, but I refer to it because it illustrates the difference between the spasmodic reforms which from time to time are adopted, under pressure from bodies of well-meaning representatives of special interests, and the well-considered changes recommended by a committee of men and women of educational experience who have carefully tested the conclusions at which they have arrived.

There can be no doubt that, as regards our elementary education, there is very general dissatisfaction with its results, since it was first nationalised thirty-seven years ago. Our merchants and manufacturers and employers of labour, our teachers in secondary and technical schools all join in the chorus of complaint. They tell us that the children have gained very little useful knowledge and still less power of applying it. There is enough in this general expression of discontent to give us pause and to make us seek for a rational explanation of our comparative failure. The inadequacy of the results attained to the money and effort that have been expended is in no way due to any want of zeal or ability on the part of the teachers, or of energy on the part of school boards or local authorities. They have all discharged the duties which were imposed upon them. It is due rather to the fact that the problem has been imperfectly understood, that our controlling authorities have had only a vague and indistinct idea of the aim and end of the important work which they were charged to administer. If we look back upon the history of elementary education in this country since 1870, we cannot fail to realise how much its progress has been retarded by errors of administration due very largely to the want of scientific method in its direction. It is painful to reflect, for instance, on the waste of time and effort, and on the false impressions produced as to the real aim and end of education, owing to the system of payment on results, which dominated for so many years a large part of our educational system. We must remember that it is only within the last few decades that education has been brought within reach of all classes of the population. Previously it was for the few; for those who could pay high fees; for those who were training for professional life, whether for the Church, the Army, the Navy, Law, or Medicine, or for the higher duties of citizen life. This had been the case for centuries, not only in this country, but in nearly all parts of the civilised world. If we read the history of education in ancient Greece or Rome, or mediæval Europe, we shall see that popular education, as now understood, was unknown. All that was written about education applied to the few who got it, and not to the great mass of the people engaged in pursuits altogether apart from those in which the privileged classes were employed. Trade and manual work were despised, and were considered degrading and unworthy of the dignity of a gentleman. I need scarcely say that these social ideas are no longer held. The fabric of society is changed, and we have to ask ourselves whether the methods of education have been similarly changed, whether they have been wisely and carefully adapted to the new order of things. What is it that has really happened? Is it not true that we have annexed the methods and subjects of teaching which had been employed during many centuries in the training of the few and applied them to the education of the people as a whole—to those who are engaged in the very callings which were more or less contemned? Surely it is so, and the results are all too manifest. We have applied the principles and methods of the secondary education of the Middle Ages to our new wants, to the training of the people for other duties than those to which such education was considered applicable, and it is only within the last few years that we have begun to see the error of our ways. In the report of your committee, to which I have referred, it is pointed out that the problem of primary education has been complicated by the introduction of the methods which for many years prevailed in secondary schools, and at a meeting of the National Education Association, held only a few weeks since, it was truly said: "In this country secondary education preceded primary by several centuries, and so the nation now finds itself with the aristocratic cart attempting to draw the democratic horse."

Let it not be supposed that in the days not so far distant, yet stretching back into the remote past, the people as a whole were uneducated. This was not so. But we have to widen the meaning of education to include the special training which the people then received—an education that was acquired without even the use of books. It cannot for one moment be said that the artisans, the mechanics, the farm hands, male and female, were wholly uneducated in those far-off days. In one sense possibly they were. Very few of them could read or write. But from earliest childhood they had received a kind of training the want of which their descendants have sadly felt in the cloistered seclusion of the modern elementary school. They were brought face to face with Nature. They learned the practical lessons of experience; and as they grew up their trade apprenticeship was an education which we have been trying vainly to reproduce. They gained some knowledge of the arts and sciences, as then understood, underlying their work. Their contact with their surroundings made them thoughtful and resourceful, for Nature is the most exacting and merciless of teachers. The difficulties they had to overcome compelled them to think, and of all occupations none is more difficult. They were constantly putting forth energy, adapting means to ends, and engaging in practical research. In the field, in the workshop, and in their own homes boys and girls acquired knowledge by personal experience. Their outlook was broad. They learned by doing. It is true that nearly all their occupations were manual, but Emerson has told us, "Manual training is the study of the external world."

Compare for a moment this training with that provided in a public elementary school, and you cannot be surprised to find that our artificial teaching has failed in its results, that our young people have gained very little practical knowledge, and that what they have gained they are unable to apply; that they lack initiative and too often the ability to use books for their own guidance, or the desire to read for self-improvement. We seem to have erred in neglecting to utilise practical pursuits as the basis of education, and in failing to build upon them and to evolve from them the mental discipline and knowledge that would have proved valuable to the child in any subsequent occupation or as a basis for future attainments. We have made the mistake of arresting, by means of an artificial literary training, the spontaneous development of activity, which begins in earliest infancy and continues to strengthen as the child is brought into ever closer contact with his natural surroundings. We have provided an education for our boys which might have been suitable for clerks; and, what is worse, we have gone some way, although we have happily cried a halt, to make our girls into "ladies," and we have run some risk of failing to produce women.

If we are to correct the errors into which we have drifted, if we are to avert the consequences that must overtake us through having equipped our children for their life-struggle with implements unfitted for their use, we must consider afresh the fundamental ideas on which a system of elementary education should be based. Instead of excluding the child from contact with the outer world we must bring him into close relationship with his surroundings. It was given to man to have dominion over all other created things, but he must first know them. It is in early years that such knowledge is most rapidly acquired, and it is in gaining it that the child's intellectual activities are most surely quickened.

It is unfortunate that we failed to realise this great function of Elementary Education when we first essayed to construct for ourselves a national system. The three R's, and much more than that, are essential and incidental parts of Elementary Education. But what is needed is a *Leitmotif*—a fundamental idea underlying all our efforts and dominating all our practice, and I venture to think that that idea is found in basing our primary education on practical pursuits, on the knowledge gained from actual things, whether in the Field, the Workshop, or the Home.

Instead of fetching our ideas as to the training to be given in the people's schools from that provided in our old grammar schools, we should look to the occupations in which the great mass of the population of all countries are necessarily engaged, and endeavour to construct thereon a system with all such additions and improvements as may be needed to adapt it to the varied requirements of modern life. By this process—one of simple evolution adjusted to everyday needs—a national system of education might be built up fitted for the nation as a whole—a system founded on ideas very different from those which, through many centuries, have governed the teaching in our schools. In the practical pursuits connected with the Field, the Workshop, and the Home, and in the elementary teaching of science and letters incidental thereto, we might lay the foundation of a rational system of primary education.

These three objects—the Field, the Workshop, and the Home—should be the pivots on which the scheme of instruction should be fixed, the central thoughts determining the character of the teaching to be given in rural and urban schools for boys and girls. It was Herbart who insisted on the importance of creating a sort of centre around which school studies should be grouped with a view to giving unity and interest to the subjects of instruction. I have elsewhere shown how a complete system of primary education may be evolved from the practical lessons to be learned in connection with out-door pursuits, with workshop exercises and with the domestic arts, and how, by means of such lessons, the child's interest may be excited and maintained in the ordinary subjects of school instruction, in English, arithmetic, elementary science, and drawing. In the proposals I am now advocating I am not suggesting any narrow or restricted curriculum. On the contrary, I believe that, by widening the child's outlook, by closely associating school work with familiar objects, you will accelerate his mental development and quicken his power of acquiring knowledge. I would strongly urge, however, that the child should receive less formal teaching, that opportunities for self-instruction, through out-door pursuits, or manual exercises, or the free use of books, should be increased, so that as far as possible the teacher should keep in view the process by which in infancy and in early life the child's intelligence is so rapidly and marvellously stimulated. Already we have discovered that our unscientific attitude towards primary education has caused us to overlook the essential difference between the requirements of country and of town life, and the training proper to boys and girls. Our mechanical methods of instruction, as laid down in codes, make for uniformity rather than diversity, and we are only now endeavouring, by piecemeal changes, to bring our teaching somewhat more closely into relation with existing needs. But the inherent defect of our system is that we have started at the wrong end, and, instead of evolving our teaching from the things with which the child is already familiar, and in which he is likely to find his life's work, we have taken him away from those surroundings and placed him in strange and artificial conditions, in which his education seems to have no necessary connection with the realities of life.

The problem of primary education is to teach by practical methods the elements of letters and of science, the art of accurate expression, the ability to think and to control the will; and the ordinary school lessons should be such as lead to the clear apprehension of the processes that bring the child into intimate relation with the world in which he moves. During the last few years the importance of such teaching has dimly dawned upon our educational authorities, but, instead of being regarded as essential, it has been treated as a sort of *extra* to be added to a literary curriculum, already overcrowded. What is known as manual training is to some extent encouraged in our schools, but it forms no part of the child's continuous education. It is still hampered with conditions inconsistent with its proper place in the curriculum, and is uncoordinated with other subjects of instruction. Moreover, no connecting link has yet been forged between the teaching of the Kindergarten and workshop practice in the school. We speak of lessons in manual training as something apart from the school instruction, as something outside the school course, on the teaching of which special grants are paid. Twenty or thirty years ago people used to talk about "teaching technical education," and from this unscientific way of treating the close connection that should exist between hand-work and brain-work our authorities have not yet freed themselves.

It is true we have long since passed that stage when it was thought that the object of instruction in the use of tools was to make carpenters or joiners; but, judging from a report recently issued by the Board of Education, it would seem that it is still thought that the object of cookery lessons to children of twelve to fourteen years of age is the training of professional cooks. Until the Board's inspectors can be brought to realise that the aim and purpose of practical instruction in primary schools, whether in cookery or in other subjects, is to train the intelligence through familiar occupations, to show how scientific method may be usefully applied in ordinary pursuits, and how valuable manipulative skill may thus be incidentally acquired, it does not seem to me that they themselves have learned the most elementary principles of their own profession. An anonymous teacher, writing some weeks since in the *Morning Post*, said: "The cookery class can be made an invaluable mental and moral training ground for the pupils, the most stimulating part of primary education. It teaches unforgettable lessons of cleanliness and order, of quickness and deftness of movements. The use of the weights and scales demands accuracy and carefulness, and the raw materials punish slovenliness or want of attention with a thoroughness which the most severe of schoolmasters might hesitate to use. Practical lessons in chemistry should form an important feature of each class. . . . The action of heat and moisture on grains of rice provides an interesting lesson on the bursting of starch cells, and the children's imagination is awakened by watching the hard isolated atoms floating in milk change slowly to the creamy softness of a properly made rice pudding. The miraculous change in the oily white of egg when it is beaten into a mountain of snowy whiteness gives them interest in the action of air and its use in cookery."

Can the teaching of grammar or the analysis of sentences provide lessons of equal value in quickening the intelligence of young children?

I must add one word before passing from this suggestive illustration of the value of scientific method in the treatment of educational questions. We live in a democratic age, and any proposed reform in the teaching of our primary schools must be tested by the requirement that the revised curriculum shall be such as will provide not only the most suitable preparatory training for the occupations in which four-fifths of the children will be subsequently engaged, but will, at the same time, enable them or some of them to pass without any breach of continuity from the primary to the secondary school. There must be no class distinctions separating the public elementary from the State-aided secondary school. The reform I have suggested is unaffected by such criticism. The practical training I have advocated, whether founded on object-lessons furnished by the Field, the Workshop, or the Home, would prove the most suitable for developing the child's intelligence and aptitudes and for enabling him to derive the utmost advantage from attendance at any one of the different types of secondary schools best fitted for his ascertained abilities and knowledge. The bent of the child's intellect would be fully determined before the age when the earliest specialisation would be desirable. No scheme of instruction for primary schools can be regarded as satisfactory which is not so arranged that, whilst providing the most suitable teaching for children who perforce must enter some wage-earning pursuit at the age of fourteen, or at the close of their elementary school course, shall at the same time afford a sound and satisfactory basis on which secondary and higher education may be built. And I hold the opinion, in which I am sure all teachers will concur, that a scheme of primary education pervaded by the spirit of the Kindergarten which, by practical exercises, encourages observation and develops the reasoning faculties, and creates in the pupil an understanding of the use of books, would form a fitting foundation for either a literary or a scientific training in a secondary school.

I have purposely chosen to illustrate the main subject of this address by reference to defects in our primary instruction, because the success of our entire system of education will be found, year by year, to depend more and more upon the results of the training given in our public elementary schools. We have scarcely yet begun to realise the social and political effects of the momentous changes in our national life, consequent on the first steps which were taken less than forty years ago to provide full facilities under State control and local management for the education of the people.

At present all sorts of ideas are afloat which have to be carefully and scientifically considered. The working classes have to be further and somewhat differently educated, in order that they may better understand their own wants and how they are to be satisfied. We have placed vast powers in the hands of local bodies, popularly elected, powers not only of administration, for which they are well adapted, but powers of determining to a very great extent, by the free use of the rates, the kind of instruction to be given in our schools, and the qualifications of the teachers to impart it. Moreover, these local bodies have shown, in many instances, a distrust of expert advice and a desire to act independently as elected representatives of the people, which cannot fail for some time at least to lead to waste of effort and of means. It was said years ago, when the centre of our political forces received a marked displacement, that we must educate our masters. Our masters now, both in politics and education, are the people, and it is only, I believe, by improving their education that we can enable them to understand the essential difficulties of the problems which they are expected to solve, and can induce them to rely, to a greater extent than they do at present, on the results of the application to such problems of scientific method, founded on the fullest information obtainable from historical and contemporary sources.

I might have illustrated my subject by reference to the acknowledged chaotic condition of our secondary education. In the report of the Board of Education published in December last we read: "While the development of secondary education is the most important question of the present day, and is the pivot of the whole education as it affects the efficiency, intelligence, and well-being of the nation, yet its present position may be described as 'chaos.'" The "chaos" by which the present position of our secondary education is here described is intimately connected with the questions relating to primary education, which I have been engaged in considering. If we construct a system of primary education which serves equally for children of all classes, apart from social conditions—a system educationally sound, both as a preparation for immediate wage-earning pursuits and for more advanced and somewhat more specialised training in a secondary school—many of the difficulties which confront the Board of Education, and which are largely of an administrative order, would disappear. The difficulties are in part dependent on the question of curriculum, to the discussion of which a day will be devoted during the present meeting.

University education in this country, and indeed in other countries, has also suffered much from the hands of the unscientific reformer. In Germany, owing to many causes, the higher education has made considerable advances during the past century; but, even in that country, a more critical study of the development of University education and a truer recognition of the twofold function of a University might have prevented the early separation in distinct institutions and under separate regulations of the higher technical from University instruction. Only within recent years has France retraced her steps and returned to the University ideal of seven centuries ago. But perhaps the climax of unscientific thinking was reached in the scheme, happily abandoned, of founding a new University in Dublin on the lines suggested by Mr. Bryce in his now famous speech of January last.

Our conception of the functions of a University has undergone many violent changes. Between the ideal of the University of London prior to its reorganisation and that of a mediæval University, in which students were never plucked, obtaining their degrees whether they did their work well or badly, there have been many variations; but I think it may be said that, recently at any rate, we have come to realise the fact that our Universities, to fulfil their great purpose, must be schools for the preparation of students for the discharge of the higher duties of citizenship and professional life, and Institutions for the prosecution of research, with a view to the promotion of learning in all its branches, and that examinations for degrees, necessary, as they undoubtedly are, as tests of the extent of a student's acquired knowledge, must be regarded as subordinate to these two great functions.

I will not detain you longer. I have endeavoured to show under what limitations education may lay claim to be included among the sciences, and how a knowledge of the history of education and the application of the methods of scientific inquiry may help in enabling us to solve many of the intricate and complicated questions which are involved in the establishment on a firm foundation of a national system of education. I have taken my illustrations mainly from the reform of elementary, or, as I prefer to call it, primary education, and I have sought to indicate some of the errors into which we may fall when we fail to apply to the consideration of the problem the same principles of inductive inquiry as are employed in all investigations for the attainment of Truth.

I believe that this Section of the British Association has the opportunity of rendering a great service to the State. Numerous educational societies exist, in which questions of importance are discussed, and all, perhaps, do useful work. But none is so detached from separate and special interests; none stands so essentially apart from all political considerations; none is so competent to discuss educational problems from the purely scientific standpoint as are the members of this Association. If, in the remarks I have offered, somewhat hastily prepared under the pressure of many different kinds of work, I have contributed anything to the solution of a problem the difficulty and national importance of which all will admit, I shall feel that I have not been altogether unworthy of the honour of occupying this Chair.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

LIVERPOOL.—Mr. Percy E. Newbury has been appointed professor of Egyptology in the University.

LONDON.—University College:—With the assistance of the Chadwick trustees, arrangements have been completed to hold a new course on school hygiene, including lectures, demonstrations, and practical work, beginning on October 16. The course will be given by Prof. Henry Kenwood and Dr. H. Meredith Richards. It is designed to meet the requirements of school teachers, school lecturers, and those qualifying to become school inspectors and school medical officers. A certificate of proficiency will be granted to those who qualify themselves.

A programme of the courses of study in the departments of pure and applied mathematics and astronomy of University College, London, has just been issued. In it are to be found full particulars as to the courses in the departments for students preparing for honours in the subjects above referred to, also of the facilities for research in the college. The programme may be obtained on application to the secretary of the college.

OXFORD.—Owing to the great increase in the number of students at Ruskin College, the staff is being increased. Prof. Lees Smith has retired from the position of vice-principal, and has been elected director of studies and chairman of the executive committee. He will for the present combine this with his duties in the newly created chair of economics and public administration at University College, Bristol, and with his work at the London School of Economics. Mr. Charles S. Buxton, of Balliol College, has been appointed vice-principal. Mr. H. S. Furniss, of Hertford College, has been appointed lecturer in economics. The plans for the new building are now under consideration. The building will be erected on the site of the temporary buildings adjoining Worcester College, and will accommodate 100 students. For this purpose the college will require about 20,000*l.*

A FURTHER 2000*l.* has been given by Sir Donald Currie towards the equipment fund of Queen's College, Belfast, bringing up his contributions to the sum of 22,000*l.*

THE calendar of the Merchant Venturers' Technical College, Bristol, has just been issued, and gives particulars of the courses of instruction at the institution and much other information. Although the main building of the college was partially destroyed by fire, the work of the institution has not been crippled, owing to the Bristol Education Committee having placed at the disposal of the governors large buildings planned to accommodate more than 1000 pupils. These buildings, temporarily known as the Castle Branch of the Merchant Venturers' Technical College, Castle Green, have been fitted with the necessary lecture theatres, laboratories, and workshops.

AN amendment to the Education Administrative Provisions Bill, recommending that power should be given to local authorities to make periodic anthropometric records of children which would afford definite information as to the physical condition and development of the children, was moved last week in the House of Commons by Sir Philip Magnus, who referred to the resolution upon the subject adopted at the joint meeting of the Anthropological and Educational Sections of the British Association, and the report upon such measurements conducted by the Glasgow School Board, which has just been issued by the Scotch Education Department; but upon the President of the Board of Education saying that clause 13 of the Bill as it stands gives the necessary powers the amendment was not carried. It is well, perhaps, to emphasise the fact that local authorities possess under the new Act the necessary powers to institute a system of scientific measurements.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 12.—M. Bouquet de la Grye in the chair.—Integral curves of differential equations: Georges **Rémoundos.**—The accidents arising during the manipulation of compressed oxygen, and on an arrangement permitting of their avoidance: Georges **Claude.** The ordinary forms of regulator for reducing the pressure contain a piece of ebonite, and this is liable to catch fire and even to inflame the steel cylinder. The author describes a simple modification of the regulator by which any local elevation of temperature in the neighbourhood of the ebonite is avoided.—A dynamo designed for wireless telegraphy: P. **Villard.** This dynamo has been designed so that the voltage curve is analogous to that of a Ruhmkorff coil, and, in addition, allows the time interval between the successive sparks to be regulated mechanically. The nature of the voltage curve produced has been studied by means of the oscillograph, curves from which are reproduced in the paper. Experiments have been made with this instrument both in the laboratory and in the field, and for equal motive power the results are much superior to those obtained with a coil. The dynamo is also useful for the production of X-rays.—The maximum of phosphorescence: J. **de Kowalski** and C. **Garnier.** A discussion of some recent results on the same subject by L. Bruninghaus.—The cause of the beating of the heart: H. **Kronecker.** Serum from the blood of a calf was subjected to diffusion in a current of flowing water so that the amount of sodium chloride was reduced to 0·6 per cent. This fluid was used to replace the blood in the cardiac cavities of the frog, toad, and tortoise, and it was found possible to suppress completely the beats of the heart for one hour. Any stimulus applied during this period of arrest caused either a strong pulsation or a group of pulsations. The effects of other solutions are also recorded. The conclusion is drawn from these experiments that the heart does not beat automatically, but requires stimulants of a chemical nature to act on the nervous plexus of the heart.—The reaction of tuberculin in leprosy (subcutaneous, dermic, and conjunctival inoculations): Charles **Nicolle.**—Observations on the Eocene and Oligocene in Hampshire: Jean **Boussac.**—The results of observations of the intensity of gravity at the island of Booth-Wandel, Grahamsland, by the Antarctic expedition of Dr. J. Charcot: M. **Matha.** An account is given of the experimental method used and the accuracy attained. The value of g found, 982·439, is higher than the value calculated from the formula of Defforges, $g = 978{\cdot}106\,(1 - 0{\cdot}005243 \sin^2 A)$, by 0·116 cm. This difference is in full accord with the results of Foster in the same regions.—The paroxysms of Stromboli: A. **Riccò.**

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