THURSDAY, SEPTEMBER 3, 1908.

MARINE DEPOSITS.

Les Dépôts marins. By Dr. Léon W. Collet. Pp. vii+325; with 35 figures in text and a map. (Paris: Octave Dion, 1908.) Price 5 francs.

THE publication of this volume is connected with the inauguration of a scientific work on the grandest scale. It is proposed to issue under the title of "Encyclopédie scientifique" about 1000 volumes in the French language, dealing with every branch of pure and applied science. The general director of this vast work is Dr. Toulouse, and its general secretary M. H. Pieron; but for each of the branches of science, to the number of forty, an editor specially conversant with the subject has been secured.

The volume before us is the first published of the group of works on "Océanographie physique," the editor of which is Dr. Jules Richard, of the Musée Océanographique de Monaco. Dr. Collet, who is a privat-docent in the University of Geneva, had the advantage of spending two years of study in the *Challenger* Office in Edinburgh, and the volume is appropriately dedicated to Sir John Murray. The work is professedly based on Murray and Renard's well-known *Challenger* volume, though a considerable amount of information obtained by later researches is incorporated by the author. The illustrations are taken either from the *Challenger* Report, from Dr. J. M. Flint's Bulletin on the Oceanography of the Pacific, from the Encyclopædia Britannica, or from the papers published by the author and Dr. Lee on glauconite deposits.

The book commences with a slight sketch of the work done in the subject before the despatch of the *Challenger* Expedition, and an essay on the effects of temperature in determining the distribution of plants and animals at the surface and in the deeper parts of the ocean. The classification adopted for the various deposits is that of Sir John Murray, with some slight and unimportant modifications, though the author notices the attempt of Thoulet to substitute for this grouping one on a mathematical basis—the size and chemical composition of the particles of which the deposits are built up being taken as a guide.

While the accounts of the several oozes and of the manganese nodules and other materials found in the red muds are condensed from the descriptions in the *Challenger* volume, the chapter on the glauconite deposits extends to much greater length (48 pages), and is taken, with its illustrations, from the paper published by the author and Dr. Lee in the Proceedings of the Royal Society of Edinburgh.

The fourth division of the book, dealing with the deposits of coral-origin, extends to considerable length, and includes references to recent works, such as the Funafuti Report of the Royal Society, and the results of Mr. Stanley Gardiner's work in the Indian Ocean. The author adopts the estimate of the last-mentioned naturalist as to the limit of depth of coral-reef building organisms—namely, 64 metres for corals and 120 metres for "nullipores." In discussing theoretical conclusions from the known facts concerning coral-reefs, however, the author seems to be labouring under some unfortunate misconceptions. He says:—

"Le sondage de l'atoll de Funafuti dans les îles de l'Ellice fut entrepris aux fins d'obtenir des renseignements sur le soubassement d'une île corallienne; bien que ce but n'ait pas été atteint, ce sondage n'en est pas moins très important, comme nous le verrons dans la suit" (pp. 261, 262).

It cannot, however, be sufficiently understood and remembered that the Coral-Reef Committee of the Royal Society was formed, not to seek for confirmation of any particular theory, but to obtain facts which might enable us to put the several theories to a critical test, and in this it was entirely successful. The committee included the advocates of all the different theories, and the plan and details of the expedition were agreed upon by mutual arrangement. The selection of Funafuti—as the most typical of atolls—was made by the late Admiral Sir W. Wharton (who was not a believer in Darwin's theory), and was accepted by every member of the committee. It was agreed by all that a boring of 1000 feet would be sufficient to test the several theories, even on the most liberal estimate of the depth at which reef-forming corals, &c., could flourish. The actual depth reached was 1114 feet, and the very careful study of the materials brought up was shown by Dr. Hinde to demonstrate, beyond doubt, that—setting aside subsequent chemical changes—the rock, *from top to bottom*, consists of the same organisms, in the same conditions as respects position of growth and association. Neither in the microscopical characters of the rock nor in its chemical composition was there the smallest indication in the lowest cores brought up of any volcanic, or, indeed, of any non-calcareous rock being approached. It was open, of course, to the opponents of Darwin's views to oppose the undertaking of the boring as an adequate test. But it is scarcely fair, now that the test has been applied under their own auspices, to declare that it is of no value.

The author reproduces an attempted explanation, with modifications by later authors, of the facts ascertained at Funafuti. But it must be remembered that these are new views, put forward *since* the boring was brought to such a successful termination by the skill and energy of Profs. Sollas and David and their assistants. Neither the theory of a talus nor the amended suggestion of the building up of the lower part of the atoll by the growth *in situ* of deep-sea corals, nullipores, or other organisms, receives the smallest support from the study of the cores brought up. Every unprejudiced observer of the whole course this discussion has taken must be convinced that the Funafuti boring, far from having failed in its object, was carried to a successful and useful conclusion.

In the final division of the work the author discusses some of the geological formations in the light of the results obtained by deep-sea explorations. In these he generally follows and summarises the work of M. Cayeux. In his discussion of the origin of the oolites, he makes no reference to the interesting recent researches of Mr. Wethered, nor to the early observations of De la Beche. The account of rocks formed by siliceous organisms is equally defective in many respects, and the papers of Dr. Hinde are not referred to either in the text or in the "Index Bibliographique" at the end of the volume.

Taken as a whole, therefore, the work, while it may be regarded as a very useful summary of the general results obtained, up to the present time, in the study of the ocean-floor by deep soundings, cannot be commended as an absolutely safe guide to those anxious to make their acquaintance with all the original sources of information on the subject.

THE CONTRAST BETWEEN GERMAN AND ENGLISH EDUCATION.[1]

German Education, Past and Present. By Prof. Friedrich Paulsen. English translation by Dr. T. Lorenz. Pp. xx+310. (London: T. Fisher Unwin, 1908.) Price 5*s.* net.

TO write well a short book on a vast subject is a task which only a master can accomplish. It is not too much to say that Prof. Paulsen is the only man in Europe who could have given, within such small compass, so readable and well-proportioned an account of the growth of German education from a remote past to the present time. Dr. Paulsen has style as well as profound knowledge. He knows what to leave out. He neither fatigues the reader by a superfluity of uninterpreted facts nor offends him by superficiality of treatment. Two years ago Messrs. Teubner, of Leipzig, published the original edition of this work in a slim, closely printed volume of less than 200 pages as one of the series which they are issuing under the title "Aus Natur und Geisteswelt." Now Mr. Unwin gives us the book pithily and idiomatically translated by Dr. Lorenz, and prefaced by a useful outline of the mechanics of the German educational system, and a short dictionary of English renderings of German technical terms. As it stands, it is the handiest book on the outlines of the subject in the English language.

For an English reader the weakest point in the book lies in the fact that Dr. Paulsen knows comparatively little about the history of education in England. This is not his fault. The history of English education—that most elusive, intricate, and many-provinced subject—has not yet been satisfactorily written. Piece by piece, the materials for it are being brought together by scholars like Mr. Arthur Leach, Dr. Rashdall, Prof. Foster Watson, Mr. J. E. G. de Montmorency, and Mr. Sidney Webb. But the facts are not yet known with anything like completeness as regards some critical periods in the history of our English schools. The time for synthesis and illuminating generalisation has not come. We ourselves suffer from this at every turn. Pamphleteers give us what they believe to be the meaning of our educational history, and often mislead us as completely as might a guide with a bad map. But, apart from Mr. Graham Balfour's invaluable summary of facts in his "Educational Systems of Great Britain and Ireland," there is at present no book about higher education in the United Kingdom to which a student can turn as he turns to Dr. Paulsen's "Geschichte des gelehrten Unterrichts auf den deutschen Schulen und Universitäten," to the translation of which, by the way, Dr. Lorenz may perhaps feel willing to turn his hand. The result of this gap on the student's bookshelf is that even Dr. Paulsen, in spite of the extraordinary range of his knowledge of educational developments, fails to see the significance of the sidelights which German educational history throws upon English, and which, in its turn, English educational history throws upon German.

To the practical Englishman the main question which arises in the perusal of a book like this is "What have we to learn from the history of German education? What pitfalls can the record of their experiments help us to avoid?" Now, broadly speaking, the English and German systems of education are at the present time moving in precisely opposite directions towards a point which lies somewhere in the wide space which now stretches between them. In other words, the study of German education (using that term in a broad sense) is for an Englishman a study in contrasts. Germany (again using that term with due regard to the fact that one part is as different from another as Scotland is from England) has a great respect for intellect; we are a little on our guard against it. Germany believes in scientific research as applied to industry and commerce. We are only beginning not to be contemptuous of it. Germany is fast extending the age of compulsory education through the critical years of (at any rate male) adolescence. In England and Wales, out of the half-million children who annually leave the public elementary schools at thirteen or fourteen years of age, not more than one in three ever afterwards receives in point of civic or technical education any further systematic care.

So far the score is against us. But there is another side of the account. For real independence of private judgment the atmosphere of English life is much more favourable than the German. In the healthy development of the character of girls, the German schools have much to learn from the best English, and German secondary education can offer, except in schools which have broken loose from official routine, nothing really comparable to the training of the will which is afforded by the corporate life, where it is healthy and inspiring, of a first-rate English school.

The English reader of Dr. Paulsen's sketch, which, because it is drawn by a master-hand, shows the capital features of the situation in bolder outline than might a more detailed picture, cannot but note two points of significant difference in the educational history of the two countries. For generations education has been more thought of as a necessary State function in Germany than with us. But there was a time

[1] This review was written before the lamented death of Prof. Paulsen on August 15.

when the great body of Englishmen had as strong a sense as had their contemporary Germans of the propriety of some form of general State control over educational matters. Protector Somerset had it as strongly as Duke Christopher of Württemberg, Mulcaster as Melanchthon, Milton (when at last he grappled with the political aspects of the question) as Comenius, Archbishop Sheldon as the Great Elector. But at that point the parallel stops. In Germany thenceforth the idea of education as a function of Government waxes stronger; in England it is pushed into the background, only to come seriously to the front again in the Socialist movement led by Robert Owen in the first quarter of the nineteenth century. What is the cause of this divergence? The roots of the matter lie deep in the religious and political fortunes of the two countries. But the proximate cause of the difference may perhaps be found in the fact that the idea of the autocratic State (benevolent, educational, directive) found a congenial home in the German kingdoms, but in England an unfertile soil.

The other point which strikes the reader of Dr. Paulsen's masterly little book is that Germany is apt to give over-zealous adhesion to some idea or set of ideas which may be intellectually or politically fashionable at a given period, and then at a later time to change its mood and discard its old theory with a somewhat unrestrained contempt. Both for good and for evil, German educational development is scarred with deeper rifts than the English. Our growth has been more continuous but less enthusiastic; we have been readier to blend old-fashioned things with new; we have been less often carried off our feet by some favourite idea.

Now, as we look upon German education as a whole from our different standpoint, with respect and gratitude for all that German thought, German idealism, and German laboriousness have done for us, two features stand out in it as being of cardinal importance. The first is the influence exerted upon education, even upon some parts of university education, by the bureaucratically organised State. But is it, after all, a good thing to make a youth's future prospects of professional advancement depend so closely upon his performing the prescribed tasks of a prolonged school education, which is necessarily somewhat bookish and (in spite of all Matthew Arnold's too easy assurances) examination-clouded? The other point is that a belief in the supreme value of a "circle of ideas," built up in a boy's mind by the skilful hand of a schoolmaster, is much more deep-seated in Germany than in England. Education to them, far more than to us, means a highly trained and often eloquent teacher directing from his desk the aspirations and intelligence of rows of diligent and well-disciplined youths sitting before him, each with his neat satchel of books. Let us confess our fault. We have too faint an idea of personal obligation to the organised State. We have too little conception of the power of ideas. But does not the elaborately organised educational system of Germany tend to exalt too highly the prestige of the official and bureaucratic State? And does it not entrench in fortifications of vested interest a bookish and over-intellectualised kind of education, against the claims of which brilliant but not yet effective protest is made by Prof. Dewey in America and by Prof. Armstrong among ourselves?

M. E. SADLER.

OUR BOOK SHELF.

Industrial Electrical Measuring Instruments. By Kenelm Edgcumbe. Pp. xiv+227. (London: A. Constable and Co., Ltd., 1908.) Price 8s. net.

ALTHOUGH, as the author points out in the preface, the subject of electrical measuring instruments is of great importance, there exists so far not a single work in the English language dealing comprehensively with such instruments. This book will therefore undoubtedly be welcomed by all engineers who have to deal with electrical measuring instruments, and by all who may chance to desire information as to the construction, working and maintenance of the various types of measuring instruments.

Every possible type of electrical measuring instrument, with the exception of supply meters, which has already been adequately dealt with in other books, is fully discussed from a practical point of view.

Although relays, synchronisers and lightning arresters cannot be classified as measuring instruments, they have been included, and this is certainly no disadvantage, as there is practically no information available about this class of apparatus except what is scattered in the technical papers.

The first thirty-nine pages are devoted to general principles, underlying all types of instruments, such as scales, control, pivots, springs, damping, &c., and contains much interesting information.

A good section is that dealing with resistance measurements, and a good description is given of the "Megger," one of the latest commercial instruments. The section on galvanometers is very short and contains very little information, and can in a future edition be enlarged with advantage. The various types of ammeters, voltmeters, and wattmeters are fully described, and the advantages and disadvantages compared. The section dealing with instrument transformers is specially useful, for there is little information available on this subject.

The section on recording instruments is not very complete; for instance, there is practically no description of any recording ammeter or voltmeter such as is used for traction work.

The last section, dealing with Prof. Fleming's cymometer and its application, might with advantage have been left out, first because the description given is only very brief, and secondly because it is entirely out of place in a book of this kind.

The book is clearly printed, and well illustrated by workmanlike sketches. The author, whose great practical experience in electrical measuring instruments is well known, has produced a most valuable book, containing a mine of practical information.

Many of the sections might in a future edition be somewhat enlarged and others cut out altogether, but this, of course, in no way diminishes the value of the book. It contains no misleading statements, is free from complicated mathematical formulæ, and can therefore be thoroughly recommended to all engineers who have to deal with this class of instruments. The subject of photometry is not touched upon; the author hopes to deal with this in another work.

L. C.

The Deinhardt-Schlomann Series of Technical Dictionaries in Six Languages, German, English, French, Russian, Italian, Spanish. By Kurt Deinhardt and Alfred Schlomann. Vol. iii., Steam Boilers, Steam Engines, Steam Turbines. Edited by Wilhelm Wagner. Pp. xi+1322; illustrated. (London: A. Constable and Co., Ltd., 1908.) Price 16s. net.

THE third volume of Deinhardt and Schlomann's illustrated technical dictionary cannot fail to prove of enormous value in international relations. The idea is quite original. The dictionary is in six languages, German, English, French, Russian, Italian and Spanish, and the necessity for elaborate definitions is obviated by the insertion of small sketches. The vocabularies are classified under the following heads:—A. Steam boilers: (1) fuels, (2) production of heat, (3) furnace installations, (4) transmission of heat, (5) evaporation, (6) testing of materials, (7) boiler construction, (8) types of boilers, (9) boiler fittings, (10) boiler erection, (11) feed apparatus, (12) steam superheaters, (13) management of steam boilers, (14) boiler explosions, (15) boiler inspection, (16) range of steam pipes. B. Steam engines: (17) theory, (18) machine parts, (19) condensers, (20) types of engine, (21) erection, (22) working of engines. C. Steam turbines: (23) theory, (24) steam-turbine parts, (25) turbine plant.

The volume concludes with an alphabetical index of the German, English, French, Italian and Spanish terms, showing the page and paragraph at which each word is to be found. A second index is devoted to the Russian words. The type is small but clear, and the volume is tastefully bound. Well-known experts all over the world have helped to revise the text, and the result is that Deinhardt and Schlomann's technical dictionary is undoubtedly the most accurate that has yet been published.

Das Wetter und seine Bedeutung für das praktische Leben. By Prof. Carl Kassner. Pp. vi+148; illustrated. (Leipzig: Quelle and Meyer, 1908.)

THIS book is No. 25 of the series "Wissenschaft und Bildung," edited by Dr. Paul Herre. It is published to meet the wish for information respecting the principles of weather prediction consequent upon the establishment of the public weather service in the German Empire (June, 1906). The Deutsche Seewarte, Hamburg, has issued an excellent daily weather report since February, 1876, for the whole of Germany, but the new system divides the Empire into fifteen forecast districts. The first section of Dr. Kassner's work deals with the historical development of weather prediction, and contains a concise summary of Dr. Hellmann's valuable researches into this subject, from the Babylonian era, some 4000 years B.C. This is a new departure from the usual course, and will be very useful for reference. Part ii. deals with the bases of modern weather prediction, with the preparation of weather charts, with areas of high and low barometric pressure, &c. This portion of the work, although not new, will be found most useful to students. The author has consulted all available sources of information, and presents a lucid exposition of the present state of the science; various popular notions, such as the supposed direct influence of the Gulf Stream and of the existence of icebergs on our weather (also lunar influence, part i.), are satisfactorily dealt with. Part iii. deals with the importance of weather in public and private life, and contains much that is not usually found in similar works.

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.]*

Indiscriminate Criticism.

MAY I enter a mild protest against a habit that seems to me somewhat harmful? Many people, when they obtain results which disagree with the previous work on the subject, seem to think it incumbent upon them to suggest that the disagreement comes from some flaw of reasoning or method in the work used for comparison.

This habit, especially in experimental work, where the chances against accuracy are always so great, is really a hindrance to the advancement of knowledge; "any stick is good enough to beat a dog with," and so the slight approximation to truth that there may be in the depreciated work will certainly not be enhanced by indiscriminate criticism, and may, thereby, be overlooked.

Surely it is understood that no one publishes results which disagree with those previously obtained without having a strong belief that his own work is, in one way or another, an advancement—why not, therefore, leave it at that? A saving of time and words would result.

I need scarcely say that I am not deprecating careful criticism; on the contrary, those who from facts and figures will point out where a source of error may lie confer a benefit on science. Unfortunately, people in the position to do this seem to shun the task.

What I have alluded to in the previous paragraphs may well be illustrated by examples drawn from criticism passed on researches lately published by Mr. Hartley and myself.

(1) Prof. Callendar (Proc. Roy. Soc., vol. lxxx., p. 466), when comparing our experimental results with an approximate calculation of his own, says, "the numbers in the last column appear to indicate a systematic error in the experimental numbers for strong solutions"; now it is possible, nay, probable, that there are systematic errors in the work, but it seems to me more would be done to elucidate the truth if he had either pointed out where the errors are or stated to what degree of accuracy his approximate calculation can be relied upon.

(2) Mr. O. F. Tower (Jour. Am. Ch. Soc., August, p. 1225) says, "Lord Berkeley and Mr. Hartley's results for the vapour pressures of the solutions are undoubtedly too low, showing probably that with their apparatus the air was not completely saturated," &c. This is a flagrant example; for Mr. Tower on the same page gives a table containing three sets of results, one obtained by Smits by a direct method, ours by an indirect one, and his own results, which are direct. Now Smits's numbers and ours, for the same concentration, give values (by different methods, be it noted) which are within 1 per cent. of each other, while Mr. Tower's numbers differ from the other two by nearly 10 per cent.!

As this letter is concerned with a more important matter than the accuracy of our work, I will not discuss Mr. Tower's assumption that the air was not saturated, &c.

(3) Messrs. Lincoln and Klein (Jour. Phys. Chem., xi., 1907) say,[1] when deducing the relative value of Prof. Kahlenburg's method of determining vapour pressures and ours, that Mr. Hartley and I claim an accuracy of 5 per cent. They omit, however, to state that our experimental error is (ultimately) calculated on the difference between the vapour pressures of solution and solvent, while theirs, if reduced to the same basis, would show errors of 25 per cent.

(4) Prof. Morse (Amer. Chem. Journ., vol. xxxiv., 1905, pp. 24–25) says, "we wish to speak of certain objections to a method of measuring osmotic pressures which has sometimes been practised. The attempt has been made, on several occasions, to ascertain the pressures of solutions by bringing mechanical pressure to bear upon the contents of the cell until," &c., and then goes on to raise purely imaginary objections to the method. It is interesting to

[1] Being away from home I am unable to give the exact wording.

note (*loc. cit.*, August) that Prof. Morse's latest corrected result (his own corrections) for the osmotic pressure at 0° C. of his 1·0 weight normal aqueous solution of cane sugar is 24·45 atmospheres, while ours, by the method under discussion, was 24·5 atmospheres.

Evian les Bains. BERKELEY.

The Rotation of a Crystal of Tourmaline by Plane Polarised Light.

(*Preliminary Note.*)

WHEN a beam of plane-polarised light is incident normally on a plate of tourmaline cut parallel to the optic axis, it will be absorbed or transmitted depending upon whether the axis of the tourmaline is parallel or perpendicular to the plane of polarisation of the incident light. If the arrangement is such that the light is absorbed, then in a given time a definite amount of heat energy will have passed from the source of light into the plate of tourmaline, and if, as is necessarily true, the former is at a high temperature while the latter is at a low temperature, it is plain that the entropy of the system will have been increased. The same increase in entropy would not have taken place if the orientation of the tourmaline had been such that the light had been transmitted.

Since the entropy of a system always tends to increase, it seemed to be of some interest to try what would happen if a plate of tourmaline was suspended so as to be free to rotate about an axis perpendicular to its faces, and then allowing a beam of plane-polarised light to fall on it in the direction of the axis of rotation, the arrangement being such that the plane of polarisation should make an angle of 45° with the optic axis of the tourmaline.

The experiment was tried by the author during the month of July, 1908, at the Rouss Physical Laboratory of the University of Virginia, and the results, though not absolutely conclusive, indicate that a moment acts on the tourmaline tending to set its optic axis parallel to the plane of polarisation of the incident light. In other words, the system tends to arrange itself so that as large a percentage of the light as possible shall be absorbed.

The apparatus used consisted of a fine plate of tourmaline, 1 cm. square, and 2·96 mm. thick, weighing almost exactly one gram. This was fastened to one end of a short straight copper wire to which was also fastened a small plane mirror. The system was suspended in a suitable vessel having a plane glass top and bottom, which could be exhausted, the suspension being such that the system had a period of 29·5 seconds. From the period and the moment of inertia of the system it was calculated that the moment necessary to give a deflection of 1 cm. with the scale at a distance of one metre was $2 \cdot 5 \times 10^{-5}$ dyne-cm. A beam of approximately parallel light from an arc was reflected in a vertical direction from a plane silver mirror, and was rendered plane polarised by a Nicol before entering the vessel.

Owing to the fact that the suspended system was not perfectly symmetrical with respect to its axis of rotation, it was found that the zero moved constantly in one direction when the light was allowed to fall on the tourmaline. This was undoubtedly due to radiometric action, and possibly also to the pressure of light. The motion was, however, much slower when the angle between the plane of polarisation and the optic axis of the crystal was +45° than when it was −45°. An average of twenty trials gave 150 seconds as the time to get a deflection of 30 cm. in the first case, while the time required for the same deflection when the angle was −45° was 90 seconds. These trials were all made with as high a vacuum in the vessel as could be obtained by means of the Gaede rotating mercury pump. The experiment was repeated at various pressures up to 10 or 15 cm. of mercury; the results were qualitatively the same, that is, the rate of deflection was much slower in every case when the angle mentioned above was +45° than when it was −45°, but the motion of the zero in one direction, although somewhat different at different pressures, could not be avoided.

If the cause of this motion of the zero is what was stated above, then it ought to disappear when the suspended system is made symmetrical about the axis of rotation.

These experiments were only preliminary, and during the coming winter a more careful investigation of the question will be carried out at the physical laboratory of the Johns Hopkins University. JOHN A. ANDERSON.

Physical Laboratory, Johns Hopkins University, August 15.

Access to Chemical Works.

STUDENTS of chemistry so often complain of the extreme difficulty of obtaining access to chemical works that it may be well to bear in mind that the universities and technical schools of the United Kingdom have perhaps a remedy for what is a serious obstacle to a proper study of chemistry. The amount of chemicals now consumed in educational laboratories must be enormous, and, as regards ordinary materials, quite sufficient to employ a large factory. A very large proportion comes from Germany. Let the universities start their own cooperative factory for the manufacture of the acids and salts they require. Let chemical students both have free access to it and put in part of their chemical course as workers there. The action of several universities in running their own farms for the benefit of agricultural students supplies a kind of precedent. When motives of education, patriotism, and economy point the same way, the scheme is worth consideration.

CHEMIST.

FLY FEVER IN AFRICA.

IN the *Times* of June 27 there appeared an article on "fly fever" in Africa, and the suggested destruction of big game. It appears that Prof. Koch has lately suggested that the African big game should be exterminated in order to destroy the principal means of nourishment of the tsetse fly. As this fly is the carrier of the infective agent of "fly fever," its extinction would, in Koch's opinion, blot out the disease. The members of the German Society for the Preservation of Game in East Africa are, naturally, opposing tooth and nail this proposed drastic measure of Prof. Koch. They deny the truth of his conclusions, and hold that the fly disease can exist where there is no big game, and, further, that there are other methods of getting rid of the fly without destroying the game.

This question is a complicated one, and perhaps if I give a short sketch of the history of the "tsetse-fly disease" I shall best clear the way to its better understanding.

When I went to Zululand in 1894 at the request of the Natal Government, to investigate an outbreak of nagana among native cattle, I was unaware that nagana and tsetse-fly disease were one and the same. Further, I believed, with the rest of the world, that the fly disease was caused by the poison of the tsetse fly, just as an animal is killed by the poison of a snake. Soon after arriving in Zululand a parasite, the *Trypanosoma brucei*, was discovered in the blood of the affected animals, and it soon became apparent that fly disease and nagana were one and the same. By suitable experiments it was demonstrated that this trypanosome is the cause of the disease. The next fact made out was that the trypanosome could be conveyed from sick to healthy animals by the local species of tsetse fly (*Glossina morsitans*). As this tsetse fly only remained infective for forty-eight hours, it was evident that it must get the parasite somewhere, and it seemed most probable that this would be from the wild animals living in the "fly country." This was found to be the case, and the *Trypanosoma brucei* was demonstrated in the blood of the buffalo, wildebeeste, koodoo, and other big game, both microscopically and by means of inoculation experiments.

Now this is a short summary of the discovery of the cause of the "tsetse-fly disease," and one point I want to bring out is that at that time, and for some time after, there was supposed to be only one "tsetse-fly disease" and one species of tsetse fly. The disease was called "the fly disease," and the fly "the tsetse fly." Now all this is changed, and "the fly disease" is now a generic term for several diseases, and the tsetse flies are found to be made up of several more or less well-marked species.

But the taking for granted that there was only one tsetse-fly disease of course led to much confusion and many mistakes. For example, when Koch studied the fly disease in German East Africa in 1898, naturally he considered he was dealing with the Zululand disease, nagana. But was he? Lately, I have studied a trypanosome from the East Coast which causes a more or less mild disease in horses and other animals. Is it not possible that Koch was dealing with this East Coast species when he said that Masai donkeys were not susceptible to nagana? So it can be easily understood how a great number of erroneous notions have crept into the literature of this subject.

It is evident that what is true of nagana, the disease caused by *Trypanosoma brucei*, need not be true of the diseases caused by *Trypanosoma theileri, dimorphon, pecaudi, congolense, vivax, nanum*, &c. But an observer comes in contact with one of these diseases in a place where there is no big game and no tsetse fly, and he at once thinks that "the fly disease" does not depend on big game or tsetse flies. When I say that "the tsetse fly" disappears from a district when the big game are killed off, and with the extinction of the big game that "the fly disease" also disappears, I only mean that *Glossina morsitans* disappears, and that a particular "fly disease"—that called by me nagana, and caused by *Trypanosoma brucei*—becomes extinct. I do not mean that the diseases caused by *Trypanosoma dimorphon*, &c., will be blotted out by the same means.

With the exception of nagana and sleeping sickness there is little real knowledge as to how the other African trypanosome diseases are spread. That they may spread by other agencies than tsetse flies is probable, since surra spreads in India, although there are no tsetse flies in that country. It is quite possible that many of these diseases in Central Africa may not be spread by the agency of tsetse flies and may not depend on the big game as a reservoir of the virus. The cattle themselves may be the reservoir, and the disease may be spread in the herd by means of any of the common biting flies, such as stomoxys or tabanus. In sleeping sickness, so far as we know, the native himself is the reservoir of the disease.

It is therefore, in my opinion, very important that, in the first place, these trypanosome diseases should be more thoroughly studied as to their distribution, their carrying agent, and the reservoir of the virus. When this is done it may well be that, by the use of this knowledge alone, owners of stock may escape damage. Now that we know the natural history of sleeping sickness, its distribution, its carrying agent, &c., any intelligent person has only himself to blame if he contracts it.

These few sentences will show how complicated a subject "the fly disease" has become, and in what a state of confusion and chaos the classification of this family of diseases at present is.

Lastly, in regard to the suggested destruction of big game. To begin with, it may be said that civilisation and big game cannot exist together. As soon as a new country is divided off into farms, either for agricultural or stock purposes, the great mass of the wild animals must go. Take, for example, the destruction of the fences by stampeding herds of zebra, wildebeeste, or buffalo, not to speak of the probability that there is not enough food to go round. Even in exceptional cases, where the wild animal has been protected from sentimental and picturesque reasons, as in the case of the herd of hippopotami preserved until lately in Natal, a time came when the neighbouring farmers could no longer put up with their destructive habits, and they had to be destroyed. We may say, then, that when a country becomes settled and civilised, the big game go. This has occurred in Cape Colony, the Orange River Colony, Transvaal, and Natal, and will occur in Zululand when that country is opened up.

But this inevitable disappearance of wild animals before the advance of civilisation is very different from the instant carrying into effect of an international measure for the wholesale destruction of big game all over Africa. Such a measure, in the present state of our knowledge, would be quite unjustifiable, and would probably fail to a great extent in its object. *Festina lente*. Let local authorities frame regulations from time to time as the exigencies of the place demand. But there ought to be room for the next thousand years in many parts of Africa for game reserves in which all the varieties of big game may live, thereby gladdening the eye and enriching the imagination and fancy of many future generations, and delaying the day when man will have for his sole companions the domestic hen, the cow, and the motor.

DAVID BRUCE.

THE LATE HENRI BECQUEREL.

ON Tuesday, August 25, 1908, died suddenly Antoine Henri Becquerel at Croisic, in Brittany, at the comparatively early age of fifty-six.

Henri Becquerel was the third of the scientific dynasty of that name. His grandfather, Antoine César Becquerel (1788–1878), a contemporary of Faraday, was a most prolific investigator of electrical and electrochemical phenomena. He was for forty-nine years a member of the Academy of Sciences, and from 1837 until 1878 professor of physics at the Musée d'Histoire naturelle in Paris. The second Becquerel, Alexandre Edmond (1820–1891), who is known chiefly for his researches in phosphorescence, which are embodied in the two volumes of his book "La Lumière," also made important investigations on thermo-electricity and on underground temperatures. He was professor at the Conservatoire des Arts et Métiers, and succeeded his father as professor and administrator of the Musée d'Histoire naturelle.

Into this distinguished family Henri Becquerel was born on December 15, 1852. He was educated first at the Lycée Louis le Grand, and at the age of twenty entered the École polytechnique. In 1875 he entered the service of the French Government as an Ingénieur des Ponts et Chaussées. Three years later, on the death of his grandfather, when his father succeeded to the full professorship at the Musée d'Histoire naturelle (the duties of which he had discharged for some years), young Becquerel was appointed his assistant under the title of "Aide-naturaliste."

Already Henri Becquerel had begun to show his powers in original research. The *Comptes rendus* for 1875 and 1876 contain his earliest papers, researches on magnetic rotatory polarisation. These were continued in 1876 in the *Journal de Physique*; while in a fourth memoir he discussed the effect on the phenomenon of using different wave-lengths. In 1878 he announced the discovery of the magnetic rotation of the plane of polarisation of light by the influence of the earth's magnetism. During the years from 1879 to 1883 he was associated with his father in a series of joint memoirs on the temperature at the surface of the earth, and beneath the surface to a depth of 36 metres, using thermoelectric methods for the subterranean observations. In 1879 he investigated the temporary magnetic properties of cobalt and nickel, and further examined the magneto-optic rotatory power of gases. He also determined the specific magnetic properties of ozone. Then he turned to the subject of phosphorescence, which his father had studied for so many years. One of the phenomena of phosphorescence—discovered originally by no other than Goethe—was the hastening of the fading out of the light of a phosphorescent body when exposed to the red rays at the hot end of the spectrum. Becquerel saw in this fact a means of studying the distribution of the intensity of the invisible infra-red rays of the spectrum. These cannot be photographed by ordinary photographic means. The method of exploring the infra-red spectrum by the thermometer or the thermopile is too coarse to give satisfactory results. The bolometer of Langley had not yet been invented. Becquerel exposed a brightly-phosphorescing strip of prepared material—one of the sulphides of the alkaline earths, so much studied by his father—to the action of the invisible infra-red spectrum, and found it to become striated with dark and light lines and bands, according as the radiation had hastened the decay of the luminosity. These phosphorographic studies he extended to include an investigation of emission spectra, in the same region, of incandescent metallic vapours. From 1886 to 1890 he was conducting experiments on the absorption of light in crystals, and on the anomalies in this absorption in different directions.

In 1892, on the death of Edmond Becquerel, Henri became professor in the Musée d'Histoire naturelle. In 1894 he was named Ingénieur en chef des Ponts et Chaussées, and in 1895 he was given a chair at the École polytechnique. Beyond giving an account of the laws of emission of light by phosphorescent bodies, he published little in these years. But in 1896 came the chief of his scientific successes. At the close of 1895 Röntgen had described the rays of peculiar penetrating power which he had observed to be emitted from highly exhausted Crookes's tubes, rays which he discovered and investigated by their singularly effective action in stimulating the luminescence of phosphorescible bodies. Associated as these rays were, both in the tubes whence they were emitted and on the platinocyanide screens where they were received, with the phenomena of phosphorescence, the association seemed to suggest a further inquiry. Was it not possible that in the phenomena of ordinary phosphorescence and fluorescence there might also be an emission of penetrating rays? Such a query suggested itself independently to several physicists in more than one country. Henri Becquerel was the first to publish any certain facts. In the *Comptes rendus* of February 24, 1896, there is a note by him, "Sur les Radiations émises par Phosphorescence." His experiment was as follows:—A photographic dry-plate was enclosed in opaque black paper. Over it was laid a thin plate formed of encrusted crystals of the double sulphate of uranium and potassium, and the whole was exposed to the sun for several hours. On developing the photographic plate it was found that the uranium salt (which has a brief phosphorescence) had emitted radiations capable of traversing the opaque paper and of reducing the silver salts. Metallic objects such as coins, interposed, left their silhouettes printed on the photographic plate. Such was the first announcement. On March 2 came a second note, "Sur les Radiations invisibles émises par les Corps phosphorescents." He has now found that the crystals of uranium salt produce the same effect when shielded from exposure to the sun's rays, and even when kept in darkness, and concludes that the invisible radiations emitted by phosphorescence continue to act long after the temporary phosphorescence has ceased. He recognises that here is a new order of phenomena. One week later he sends a third contribution. He has discovered that, like Röntgen's rays, the radiations emitted from the phosphorescent salts can discharge an electroscope, and he begins to employ this electric test quantitatively. He also announces that these new rays can be reflected, and possibly refracted. He tries different substances as to the amount and duration of their activity, finding the uranium salts to surpass by far the alkaline sulphides and the zinc-blende preparations. By March 23 he communicates another notice, in the title of which it is significant to observe that he has dropped all reference to phosphorescence. It is called "Sur les Radiations invisibles émises par les Sels d'Uranium"; for he finds that a non-phosphorescent solution of uranium is also active. He has also been studying the absorption of these rays, and has, he thinks, confirmed their refraction. On March 30 he reads to the academy another note on the differences between the radiations of uranium and the Röntgen rays. He insists that the former can not only be reflected and refracted, but that they can show double refraction and polarisation if transmitted through tourmaline. He has also obtained them from non-phosphorescing compounds of uranium. That he was mistaken in respect of reflection, refraction, and polarisation does not detract from the merits of the great discovery. Before six months from the date of his first note he was able further to announce that metallic uranium, furnished by his friend Moissan, far surpasses its salts in activity, the first example, he declares, of a metal presenting a phenomenon of the order of an invisible phosphorescence.

The subsequent development of the new branch of physics—radio-activity—thus opened out by the discovery of the Becquerel rays is known to all students of science. In 1898 Schmidt and Mme. Curie independently observed that thorium was also radio-active. M. and Mme. Curie set out on a systematic examination of other minerals, and Mme. Curie, after finding that certain uraniferous minerals were more active than uranium itself, embarked on the laborious search which yielded her the successive discoveries of polonium and radium. Rutherford, in the Cavendish Laboratory, repeated and extended Becquerel's measurements on the electrical properties of the uranium radiations, and pushed the investigation into new regions by demonstrating the various stages of phenomena explicable only on the hypothesis of the degradation of the uranium atom and the successive evolution of new elements of transitional types. Becquerel continued to investigate the radiations, and their divisibility into three kinds which differ in penetrating properties and in the deviations which they suffer when subjected to magnetic and electric forces. In 1903 he united in a large quarto memoir of 360 pages, under the title of "Recherches sur une Propriété nouvelle de la Matière," his hitherto scattered contributions. This memoir, written with admirable lucidity of phrase and illustrated with many plates, remains a witness to his powers of investigation and scientific acumen. He had since 1889 been a Membre de l'Académie des Sciences; he was also Officier de la Légion d'Honneur; and with the reputation of his great discovery honours fell thickly upon him. He was president of the Société Française de Physique in 1897. In 1900 the Royal Society awarded him the Rumford medal. In 1903 the Nobel prize in physics was awarded to him conjointly with the Curies. In 1907 the National Academy of the United States decreed to him the Burnard medal. In 1907 he was president of the Société nationale d'Agriculture, and the Berlin Academy awarded him the Helmholtz medal. In the same year he was elected vice-president of the French Academy of Sciences, and only in June last he was elected perpetual secretary of the Academy in succession to M. Lapparent. He was a member of many foreign academies, and received honorary doctorates from the Universities of Cambridge, Oxford, Aberdeen, Manchester, and Göttingen. He was a foreign fellow of the Physical Society of London, and an honorary member of the Royal Institution, where, in March, 1902, he lectured on radio-activity. In NATURE of December 22, 1905 vol. lxxi., p. 177), in an article of the series Some Scientific Centres, by Mr. J. B. Burke, an account is given of the laboratory of the Musée d'Histoire naturelle, illustrated by a portrait of Becquerel amongst the apparatus used in his researches. Amiable and ever courteous, he was greatly endeared to all who knew him by his frank and sympathetic demeanour. He leaves one son, M. Jean Becquerel, Ingénieur des Ponts et Chaussées, who has already distinguished himself by important investigations on the absorption of light in crystals and other researches, the latest of which promises to elucidate the nature of positive electricity. He has honourably carried on the family tradition even in having been appointed assistant in the Musée d'Histoire naturelle.

THE DUBLIN MEETING OF THE BRITISH ASSOCIATION.

THE seventy-eighth annual meeting of the British Association for the Advancement of Science began yesterday, September 2, when Mr. Francis Darwin, M.A., LL.D., F.R.S., assumed the presidency and delivered his presidential address in the great hall of the Royal University of Ireland, Earlsfort Terrace, Dublin. More than 2000 members and associates are attending the meeting. In the afternoon of the same day the members met informally at the Dublin Mansion House, where the Lord Mayor, Alderman Gerald O'Reilly, bade them welcome in the name of the city.

The sectional meetings began this morning. They are mostly being held in the various schools of Trinity College, the sole college of Dublin University, which was founded some 300 years ago by Queen Elizabeth. The Educational Science Section meets in the Royal University building, which is shortly to be re-modelled for the accommodation of the new and as yet unnamed university founded by Mr. Birrell's recent Act. Other sections meet in the Royal Irish Academy, the Royal College of Science for Ireland (soon to be provided with new and handsome buildings), the historic Leinster House of the Royal Dublin Society, and the Royal Colleges of Physicians and Surgeons. A service of trams and a volunteer service of motor-cars have been arranged to facilitate the circulation of members among the various sections. The official journal, published every morning at 10, gives a list of papers to be read, and an intersectional telephone service announces the progress made with the reading of the various papers.

The examination hall in Trinity College has been fitted up as a reception-room, with the usual facilities as to postal and telegraphic business. Letters should be addressed to "British Association, Dublin." The names of persons for whom telegrams have been received are written on a blackboard at the post office. There is a liberal provision of writing, smoking, and lounge rooms, and drawing-rooms for ladies. There is an official luncheon-room in the dining hall of Trinity College, and luncheons and teas are obtainable in a marquee in the College Park.

The Royal Dublin Society and the Dublin Chamber of Commerce are offering the use of their rooms to members of the Association, and many of the clubs are giving facilities for temporary membership.

The "Handbook" to the city of Dublin and the surrounding district, prepared for the meeting and printed at the Dublin University Press, is an attractive work the production of which is creditable to the general editors, Prof. Grenville Cole and Mr. Lloyd Praeger. It contains 440 pages, numerous illustrations, and an excellent district map. Its contents deal with the geology, meteorology, botany, and zoology (the latter very fully) of the Dublin district. The history and archæology of Dublin are treated by a subcommittee of experts. A melancholy interest attaches to the sketch of the history of Dublin, by Mr. C. Litton Falkiner, late secretary to the council of the Royal Irish Academy, who lost his life mountain-climbing in Switzerland last month. A special chapter, edited by Prof. G. H. Carpenter, deals with the various scientific and other educational institutions of Dublin, and Prof. Adeney's work on Dublin industries and commerce concludes the volume, which will do much to bring the more exceptional features of the Irish capital before the scientific public in an informing and attractive manner.

E. E. FOURNIER.

INAUGURAL ADDRESS BY FRANCIS DARWIN, M.A., PH.D., LL.D., F.R.S., PRESIDENT OF THE ASSOCIATION.

BEFORE entering on the subject of my Address, I may be allowed to refer to the loss which the British Association has sustained in the death of Lord Kelvin. He joined the Association in 1847, and had been for more than fifty years a familiar figure at our meetings. This is not the occasion to speak of his work in the world or of what he was to his friends, but rather of his influence on those who were personally unknown to him. It seems to me characteristic of him that something of his vigour and of his personal charm was felt far beyond the circle of his intimate associates, and many men and women who never exchanged a word with Lord Kelvin, and are in outer darkness as to his researches, will miss his genial presence and feel themselves the poorer to-day. By the death of Sir John Evans the Association is deprived of another faithful friend. He presided at Toronto in 1897, and since he joined the Association in 1861 had been a regular attendant at our meetings. The absence of his cheerful personality and the loss of his wise counsels will be widely felt.

May I be permitted one other digression before I come to my subject? There has not been a Botanical President of the British Association since the Norwich meeting forty years ago, when Sir Joseph Hooker was in the chair, and in "eloquent and felicitous words" (to quote my father's letter) spoke in defence of the doctrine of evolution. I am sure that every member of this Association will be glad to be reminded that Sir Joseph Hooker is, happily, still working at the subject that his lifelong labours have so greatly advanced, and of which he has long been recognised as the honoured chief and leader.

You will perhaps expect me to give a retrospect of the progress of evolution during the fifty years that have elapsed since July 1, 1858, when the doctrine of the origin of species by means of natural selection was made known to the world in the words of Mr. Darwin and Mr. Wallace.

This would be a gigantic task, for which I am quite unfitted. It seems to me, moreover, that the first duty of your President is to speak on matters to which his own researches have contributed. My work—such as it is—deals with the movements of plants, and it is with this subject that I shall begin. I want to give you a general idea of how the changes going on in the environment act as stimuli and compel plants to execute certain movements. Then I shall show that what is true of those temporary changes of shape we describe as movements is also true of the permanent alterations known as morphological.

I shall insist that, if the study of movement includes the problem of stimulus and reaction, morphological change must be investigated from the same point of view. In fact, that these two departments of inquiry must be classed together, and this, as we shall see, has some important results—namely, that the dim beginnings of habit or unconscious memory that we find in the movements of plants and animals must find a place in morphology; and inasmuch as a striking instance of correlated morphological changes is to be found in the development of the adult from the ovum, I shall take this ontogenetic series and attempt to show you that here also something equivalent to memory or habit reigns.

Many attempts have been made to connect in this way the phenomena of memory and inheritance, and I shall ask you to listen to one more such attempt, even though I am forced to appear as a champion of what some of you consider a lost cause—the doctrine of the inheritance of acquired characters.

Movement.

In his book on "The Power of Movement in Plants" (1880)[1] my father wrote that "it is impossible not to be struck with the resemblance between the foregoing movements of plants and many of the actions performed unconsciously by the lower animals." In the previous year Sachs[2] had in like manner directed attention to the essential resemblance between the irritability of plants and animals. I give these statements first because of their simplicity and directness; but it must not be forgotten that before this Pfeffer[3] had begun to lay down the principles of what is now known as *Reizphysiologie*, or the physiology of stimulus, for which he and his pupils have done so much.

The words of Darwin which I have quoted afford an example of the way in which science returns to the obvious. Here we find revived, in a rational form, the point of view of the child or of the writer of fairy stories. We do not go so far as the child; we know that flowers do not talk or walk; but the fact that plants must be classed with animals as regards their manner of reaction to stimuli has now become almost a commonplace of physiology. And inasmuch as we ourselves are animals, this conception gives us a certain insight into the reactions of plants which we should not otherwise possess. This is, I allow, a very dangerous tendency, leading to anthropomorphism, one of the seven deadly sins of science. Nevertheless, it is one that must be used unless the great mass of knowledge accumulated by psychologists is to be forbidden ground to the physiologist.

Jennings[4] has admirably expressed the point of view from which we ought to deal with the behaviour of the simpler organisms. He points out that we must study their movements in a strictly objective manner: that the same point of view must be applied to man, and that any resemblances between the two sets of phenomena are not only an allowable but a necessary aid to research.

What, then, are the essential characters of stimuli and of the reactions which they call forth in living organisms? Pfeffer has stated this in the most objective way. An organism is a machine which can be set going by touching a spring or trigger of some kind; a machine in which energy can be set free by some kind of releasing mechanism. Here we have a model of at least some of the features of reaction to stimulation.

[1] P. 571.
[2] *Arbeiten*, ii. 1879, p. 282.
[3] *Osmotische Untersuchungen*, 1877, p. 202.
[4] "The Behaviour of the Lower Organisms," 1904, p. 124.

The energy of the cause is generally out of all proportion to the effect, *i.e.*, a small stimulus produces a big reaction. The specific character of the result depends on the structure of the machine rather than on the character of the stimulus. The trigger of a gun may be pulled in a variety of different ways without affecting the character of the explosion. Just in the same way a plant may be made to curve by altering its angle to the vertical, by lateral illumination, by chemical agency, and so forth; the curvature is of the same nature in all cases, the release-action differs. One of those chains of wooden bricks in which each knocks over the next may be set in action by a touch, by throwing a ball, by an erring dog, in short by anything that upsets the equilibrium of brick No. 1; but the really important part of the game, the way in which the wave of falling bricks passes like a prairie fire round a group of Noah's Ark animals, or by a bridge over its own dead body and returns to the starting-point, &c.—these are the result of the magnificent structure of the thing as a whole, and the upset of brick No. 1 seems a small thing in comparison.

For myself I see no reason why the term *stimulus* should not be used in relation to the action of mechanisms in general; but by a convention which it is well to respect, *stimulation* is confined to the protoplasmic machinery of living organisms.

The want of proportion between the stimulus and the reply, or, as it has been expressed, the unexpectedness of the result of a given stimulus, is a striking feature in the phenomena of reaction. That this should be so need not surprise us. We can, as a rule, only know the stimulus and the response, while the intermediate processes of the mechanism are hidden in the secret life of protoplasm. We might, however, have guessed that big changes would result from small stimuli, since it is clear that the success of an organism in the world must depend partly at least on its being highly sensitive to changes in its surroundings. This is the adaptive side of the fundamental fact that living protoplasm is a highly unstable body. Here I may say one word about the adaptation as treated in the "Origin of Species." It is the present fashion to minimise or deny altogether the importance of natural selection. I do not propose to enter into this subject; I am convinced that the inherent strength of the doctrine will insure its final victory over the present anti-Darwinian stream of criticism. From the Darwinian point of view it would be a remarkable fact if the reactions of organisms to natural stimuli were not adaptive. That they should be so, as they undoubtedly are, is not surprising. But just now I only direct attention to the adaptive character of reactions from a descriptive point of view.

Hitherto I have implied the existence of a general character in stimulation without actually naming it; I mean the indirectness of the result. This is the point of view of Dutrochet, who in 1824 said that the environment suggests but does not directly cause the reaction. It is not easy to make clear in a few words the conception of indirectness. Pfeffer[1] employs the word *induction*, and holds that external stimuli act by producing internal change, such changes being the link between stimulus and reaction. It may seem, at first sight, that we do not gain much by this supposition; but since these changes may be more or less enduring, we gain at least the conception of *after effect* as a quality of stimulation. What are known as *spontaneous* actions must be considered as due to internal changes of unknown origin.

It may be said that in speaking of the "indirectness" of the response to stimuli we are merely expressing in other words the conception of release-action; that the explosion of a machine is an indirect reply to the touch on the trigger. This is doubtless true, but we possibly lose something if we attempt to compress the whole problem into the truism that the organism behaves as it does because it has a certain structure. The quality of indirectness is far more characteristic of an organism than of a machine, and to keep it in mind is more illuminating than a slavish adherence to the analogy of a machine. The reaction of an organism depends on its past history; but, it may be

[1] "Physiology," Engl. edit., i. p. 11.

answered, this is also true of a machine the action of which depends on how it was made, and in a less degree on the treatment it has received during use. But in living things this last feature in behaviour is far more striking, and in the higher organisms past experience is all-important in deciding the response to stimulus. The organism is a plastic machine profoundly affected in structure by its own action, and the unknown process intervening between stimulus and reaction (on which the indirectness of the response depends) must have the fullest value allowed it as a characteristic of living creatures.

For the zoological side of biology a view similar to that of Pfeffer has been clearly stated by Jennings[1] in his admirable studies on the behaviour of infusoria, rotifers, &c. He advances strong arguments against the theories of Loeb and others, according to which the stimulus acts directly on the organs of movement; a point of view which was formerly held by botanists, but has since given place to the conception of the stimulation acting on the organism as a whole. Unfortunately for botanists these movements are by the zoologists called *tropisms*, and are thus liable to be confused with the geotropism, heliotropism, &c., of plants: to these movements, which are not considered by botanists to be due to direct action of stimuli, Loeb's assumptions do not seem to be applicable.

Jennings's position is that we must take into consideration what he calls "physiological state, *i.e.*, 'the varying internal physiological conditions of the organism, as distinguished from permanent anatomical conditions.'" Though he does not claim novelty for his view, I am not aware that it has ever been so well stated. External stimuli are supposed to act by altering this physiological state; that is, the organism is temporarily transformed into what, judged by its reactions, is practically a different creature.

This may be illustrated by the behaviour of Stentor, one of the fixed infusoria.[2] If a fine jet of water is directed against the disc of the creature, it contracts "like a flash" into its tube. In about half a minute it expands again and the cilia resume their activity. Now we cause the current to act again upon the disc. This time the Stentor does not contract, which proves that the animal has been in some way changed by the first stimulus. This is a simple example of "physiological state." When the Stentor was at rest, before it received the first current of water, it was in state 1, the stimulus changed state 1 into state 2, to which contraction is the reaction. When again stimulated it passed into state 3, which does not produce contraction.

We cannot prove that the contraction which occurred when the Stentor was first stimulated was due to a change of state. But it is a fair deduction from the result of the whole experiment, for after the original reaction the creature is undoubtedly in a changed state, since it no longer reacts in the same way to a repetition of the original stimulus.

Jennings points out that, as in the case of plants, spontaneous acts are brought about when the physiological state is changed by unknown causes, whereas in other cases we can point to an external agency by which the same result is effected.

Morphological Changes.

Let us pass on to the consideration of the permanent or morphological changes and the stimuli by which they are produced, a subject to which, in recent years, many workers have devoted themselves. I need only mention the names of Vöchting, Goebel, and Klebs among botanists, and those of Loeb, Herbst, and Driesch among zoologists, to remind you of the type of research to which I refer.

These morphological alterations produced by changes in environment have been brought under the rubric of reaction to stimulation, and must be considered as essentially similar to the class of temporary movements of which I have spoken.

The very first stage in development may be determined

[1] H. S. Jennings, "Contributions to the Study of the Behaviour of the Lower Organisms." Carnegie Institution, 1904, p. 111.
[2] Jennings, "Behaviour of the Lower Organisms," 1906, p. 170.

by a purely external stimulus. Thus the position of the first cell-wall in the developing spore of Equisetum is determined by the direction of incident light.[1] In the same way the direction of light settles the plane of symmetry of Marchantia as it develops from the gemma.[2] But the more interesting cases are those where the presence or absence of a stimulus makes an elaborate structural difference in the organism. Thus, as Stahl[3] has shown, beech leaves developed in the deep shade of the middle of the tree are so different in structure from leaves grown in full sunlight that they would unhesitatingly be described as belonging to different species. Another well-known case is the development of the scale-leaves on the rhizome of Circæa into the foliage leaves under the action of light.[4]

The power which the experimenter has over the lower plants is shown by Klebs, who kept *Saprolegnia mixta*, a fungus found on dead flies, in uninterrupted vegetative growth for six years; while by removing a fragment of the plant and cultivating it in other conditions the reproductive organs could at any time be made to appear.[5]

Chlamydomonas media, a unicellular green alga, when grown in a 0·4 per cent. nutrient solution continues to increase by simple division, but conjugating gametes are formed in a few days if the plant is placed in pure water and kept in bright light.[6] Numberless other cases could be given of the regulation of form in the lower organisms. Thus Sporodinia grown on peptone-gelatine produces sporangiferous hypha, but on sugar zygotes are formed. Again, *Protosiphon botryoides*, if grown on damp clay, can most readily be made to produce spores by transference to water either in light or in darkness. But for the same plant cultivated in Knop's solution the end can best be obtained by placing the culture in the dark.[7] Still these instances of the regulation of reproduction are not so interesting from our point of view as some of Klebs' later results.[8] Thus he has shown that the colour of the flower of *Campanula trachelium* can be changed from blue to white and back again to blue by varying the conditions under which the plant is cultivated. Again, with Sempervivum[9] he has been able to produce striking results—*e.g.*, the formation of apetalous flowers with one instead of two rows of stamens. Diminution in the number of stamens is a common occurrence in his experimental plants, and absolute loss of these organs also occurs. Many other abnormalities were induced, both in the stamens and in other parts of the flowers.

There is nothing new in the character of these facts;[10] what has been brought to light (principally by the work of Klebs) is the *degree* to which ontogeny is controllable. We are so much in the habit of thinking of the stable element in ontogeny that the work of Klebs strikes us with something of a shock. Most people would allow that change of form is ultimately referable to changed conditions, but many of us were not prepared to learn the great importance of external stimuli in ontogeny.

Klebs begins by assuming that every species has a definite *specific structure*, which he compares to chemical character. Just as a substance such as sulphur may assume different forms under different treatment, so he assumes that the specific structure of a plant has certain potentialities which may be brought to light by appropriate stimuli. He divides the agencies affecting the structure into external and internal conditions, the external being supposed to act by causing alterations in the internal conditions.

It will be seen that the scheme is broadly the same as that of Pfeffer for the case of the movement and other temporary reactions. The internal conditions of Klebs correspond also to the "physiological state" of Jennings.

From what has gone before, it will be seen that the

[1] Stahl, *Ber. d. Bot. Ges.*, 1885, p. 334.
[2] Pfeffer, in Sachs' *Arbeiten*, i. p. 92.
[3] *Jenaische Zeitschr.*, 1883, p. 162.
[4] Goebel in *Bot. Zeitung*, 1880.
[5] *Willkürliche Entwick.*, p. 27.
[6] Klebs, *Bedingungen*, 1896, p. 430.
[7] *Biol. Centralbl.*, 1904, pp. 451–3.
[8] *Jahrb. f. wiss. Bot.*, xlii. 1906, p. 162.
[9] *Abhandl. Naturforsch. Ges. zu Halle*, xxv., 1906, pp. 31, 34, &c.
[10] See the great collection of facts illustrating the "direct and definite action of the external conditions of life" in "Variation of Animals and Plants," ii. 271.

current conception of stimulus[1] is practically identical whether we look at the phenomena of movement or those of structure. If this is allowable—and the weight of evidence is strongly in its favour—a conclusion of some interest follows.

If we reconsider what I have called the indirectness of stimulation, we shall see that it has a wider bearing than is at first obvious. The "internal condition" or "physiological state" is a factor in the regulation of the organism's action, and it is a factor which owes its character to external agencies which may no longer exist.

The fact that stimuli are not momentary in effect but leave a trace of themselves on the organism is in fact the physical basis of the phenomena grouped under memory in its widest sense as indicating that action is regulated by past experience. Jennings[2] remarks: "In the higher animals, and especially in man, the essential features in behaviour depend very largely on the history of the individual; in other words, upon the present physiological condition of the individual, as determined by the stimuli it has received and the reactions it has performed. But in this respect the higher animals do not differ in principle, but only in degree, from the lower organisms. . . ." I venture to believe that this is true of plants as well as of animals, and that it is further broadly true not only of physiological behaviour, but of the changes that are classed as morphological.

Semon in his interesting book, "Die Mneme,"[3] has used the word *Engram* for the trace or record of a stimulus left on the organism. In this sense we may say that the internal conditions of Pfeffer, the physiological states of Jennings, and the internal conditions of Klebs are, broadly speaking, *Engrams*. The authors of these theories may perhaps object to this sweeping statement, but I venture to think it is broadly true.

The fact that in some cases we recognise the chemical or physical character of the internal conditions does not by any means prevent our ascribing a *mnemic* memory-like character to them, since they remain causal agencies built up by external conditions which have, or may have, ceased to exist. Memory will be none the less memory when we know something of the chemistry and physics of its neural concomitant.

Habit illustrated by Movement.

In order to make my meaning plain as to the existence of a *mnemic* factor in the life of plants, I shall for the moment leave the morphological side of life and give an instance of habitual movement.

Sleeping plants are those in which the leaves assume at night a position markedly different from that shown by day. Thus the leaflets of the scarlet-runner (Phaseolus) are more or less horizontal by day and sink down at night. This change of position is known to be produced by the alternation of day and night. But this statement by no means exhausts the interest of the phenomenon. A sensitive photographic plate behaves differently in light and darkness; and so does a radiometer, which spins by day and rests at night.

If a sleeping-plant is placed in a dark room after it has gone to sleep at night, it will be found next morning in the light-position, and will again assume the nocturnal position as evening comes on. We have, in fact, what seems to be a habit built by the alternation of day and night. The plant normally drops its leaves at the stimulus of darkness and raises them at the stimulus of light. But here we see the leaves rising and falling in the absence of the accustomed stimulation. Since this change of position is not due to external conditions it must be the result of the internal conditions which habitually accompany the movement. This is the characteristic *par excellence* of habit—namely, a capacity, acquired by repetition, of reacting to a fraction of the original environment. We may express it in simpler language. When a series of actions are compelled to follow each other by applying a series of stimuli they become organically tied together, or *associated*, and follow each other automatically, even when the whole series of stimuli are not acting. Thus in the formation of habit *post hoc* comes to be equivalent to *propter hoc*. Action B automatically follows action A, because it has repeatedly been compelled to follow it.

This may be compared with Herbert Spencer's[1] description of an imaginary case, that of a simple aquatic animal which contracts its tentacles on their being touched by a fish or a bit of seaweed washed against it. If such a creature is also sensitive to light the circumstances in which contraction takes place will be made up of two stimuli—those of light and of contact—following each other in rapid succession. And, according to the above statement of the essential character of associative habit, it will result that the light-stimulus alone may suffice, and the animal will contract without being touched.

Jennings[2] has shown that the basis of memory by association exists in so low an organism as the infusorian Stentor. When the animal is stimulated by a jet of water containing carmine in suspension, a physiological state A is produced, which, however, does not immediately lead to a visible reaction. As the carmine stimulus is continued or repeated, state B is produced, to which the Stentor reacts by bending to one side. After several repetitions of the stimulus, state C is produced, to which the animal responds by reversing its ciliary movement, and C finally passes into D, which results in the Stentor contracting into its tube. The important thing is that after many repetitions of the above treatment the organism "contracts at once as soon as the carmine comes in contact with it." In other words, states B and C are apparently omitted, and A passes directly into D, *i.e.*, into the state which gives contraction as a reaction. Thus we have in an infusorian a case of short-circuiting precisely like the case which has been quoted from Herbert Spencer as illustrating association. But Jennings' case has the advantage of being based on actual observation. He generalises the result as the "law of the resolution of physiological states" in the following words: "The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times." He goes on to point out that the operation of this law is seen in the higher organisms, "in the phenomena which we commonly call memory, association, habit-formation, and learning."

In spite of this evidence of mnemic power in the simplest of organisms, objections will no doubt be made to the statement that association of engrams can occur in plants.

Pfeffer, whose authority none can question, accounts for the behaviour of sleeping plants principally on the more general ground that when any movement occurs in a plant there is a tendency for it to be followed by a reversal—a swing of the physiological pendulum in the other direction. Pfeffer[3] compares it to a released spring which makes several alternate movements before it settles down to equilibrium. But the fact that the return movements

[1] With regard to the terminology of stimulation, I believe that it would greatly simplify matters if our classification of causal conditions could be based on the relation of the nucleus to the rest of the cell. But our knowledge does not at present allow of more than a tentative statement of such a scheme. It is now widely believed that the nucleus is the bearer of the qualities transmitted from generation to generation, and the regulator of ontogeny. May we not therefore consider it probable that the nucleus plays in the cell the part of a central nervous system? In plants there is evidence that the ectoplasm is the sensitive region, and, in fact, plays the part of the cell's sense-organ. The change that occurs in the growth of a cell, as a response to stimulus, would on this scheme be a reflex action dependent for its character on the structure of the nucleus. The "indirectness" of stimulation would then depend on the reception by the nucleus of the excitation set up in the ectoplasm, and the secondary excitation reflected from the nucleus, leading to certain changes in the growth of the cell.

If the nucleus be the bearer of the past history of the individual, the scheme here sketched would accord with the adaptive character of normal reactions and would fall into line with what we know of the regulation of actions in the higher organisms. Pfeffer ("Physiology of Plants," Eng. trans., iii. 10) has briefly discussed the possibility of thus considering the nucleus as a reflex centre, and has pointed out difficulties in the way of accepting such a view as universally holding good. Delage ("L'Hérédité," 2nd edit., 1903, p. 88) gives a good summary of the evidence which induces him to deny the mastery of the cell by the nucleus. Driesch, however ("Analytische Theorie der organischen Entwicklung," 1894, p. 81), gives reasons for believing that the cytoplasm is the receptive region, while the nucleus is responsible for the reaction, and it is on this that he bases his earlier theory of ontogeny.

[2] P. 124 (1904).

[3] "Die Mneme, als erhaltendes Prinzip im Wechsel des organischen Geschehens," von Richard Semon, 1[te] Auflage, 1904, 2[te] Auflage, 1908. It is a pleasure to express my indebtedness to this work, as well as for the suggestions and criticisms which I owe to Prof. Semon personally.

[1] "Psychology," 2nd edit., 1870, vol. i. p. 435.

[2] "Behaviour of the Lower Organisms," 1906, p. 289.

[3] See Pfeffer, *Abhandl. K. Sächs. Ges.*, Bd. xxx. 1907. It is impossible to do justice to Pfeffer's point of view in the above brief statement.

occur at the same time-intervals as the stimuli is obviously the striking feature of the case. If the pendulum-like swing always tended to occur naturally in a twelve hours' rhythm it would be a different matter. But Pfeffer has shown that a rhythm of six hours can equally well be built up. And the experiments of Miss Pertz and myself[1] show that a half-hourly or quarter-hourly rhythm can be produced by alternate geotropic stimulation.

We are indebted to Keeble[2] for an interesting case of apparent habit among the lower animals. *Convoluta roscoffensis*, a minute worm-like creature found on the coast of Brittany, leads a life dependent on the ebb and flow of the sea. When the tide is out the Convoluta come to the surface, showing themselves in large green patches. As the rising tide begins to cover them they sink down into safer quarters. The remarkable fact is that when kept in an aquarium, and therefore removed from tidal action, they continue for a short time to perform rhythmic movements in time with the tide.

Let us take a human habit, for instance that of a man who goes a walk every day and turns back at a given mile-post. This becomes habitual, so that he reverses his walk automatically when the limit is reached. It is no explanation of the fact that the stimulus which makes him start from home includes his return—that he has a mental return-ticket. Such explanation does not account for the point at which he turns, which as a matter of fact is the result of association. In the same way a man who goes to sleep will ultimately wake; but the fact that he wakes at four in the morning depends on a habit built up by his being compelled to rise daily at that time. Even those who will deny that anything like association can occur in plants cannot deny that in the continuance of the nyctitropic rhythm in constant conditions we have, in plants, something which has general character of habit, *i.e.*, a rhythmic action depending on a rhythmic stimulus that has ceased to exist.

On the other hand, many will object that even the simplest form of association implies a nervous system. With regard to this objection it must be remembered that plants have two at least of the qualities characteristic of animals—namely, extreme sensitiveness to certain agencies and the power of transmitting stimuli from one part to another of the plant body. It is true that there is no central nervous system, nothing but a complex system of nuclei; but these have some of the qualities of nerve cells, while intercommunicating protoplasmic threads may play the part of nerves. Spencer[3] bases the power of association on the fact that every discharge conveyed by a nerve "leaves it in a state for conveying a subsequent like discharge with less resistance." Is it not possible that the same thing may be as true of plants as it apparently is of infusoria? We have seen reasons to suppose that the "internal conditions" or "physiological states" in plants are of the nature of engrams, or residual effects of external stimuli, and such engrams may become associated in the same way.

There is likely to be another objection to my assumption that a simple form of associated action occurs in plants—namely, that association implies consciousness. It is impossible to know whether or not plants are conscious; but it is consistent with the doctrine of continuity that in all living things there is something psychic, and if we accept this point of view we must believe that in plants there exists a faint copy of what we know as consciousness in ourselves.[4]

I am told by psychologists that I must define my point of view. I am accused of occupying that unscientific position known as "sitting on the fence." It is said that, like other biologists, I try to pick out what suits my purpose from two opposite schools of thought—the psychological and the physiological.

What I claim is that, as regards reaction to environment, a plant and a man must be placed in the same great class, in spite of the obvious fact that as regards complexity of behaviour the difference between them is enormous. I am not a psychologist, and I am not bound to give an opinion as to how far the occurrence of definite actions in response to stimulus is a physiological and how far a psychological problem. I am told that I have no right to assume the neural series of changes to be the cause of the psychological series, though I am allowed to say that neural changes are the universal concomitants of psychological change. This seems to me, in my ignorance, an unsatisfactory position. I find myself obliged to believe that the mnemic quality in all living things (which is proved to exist by direct experiment) must depend on the physical changes in protoplasm, and that it is therefore permissible to use these changes as a notation in which the phenomena of habit may be expressed.

1 *Annals of Botany*, 1892 and 1903.
2 Gamble and Keeble, *Q. J. Mic. Science*, xlvii. p. 401.
3 "Psychology," 2nd edit., vol. i. p. 615.
4 See James Ward, "Naturalism and Agnosticism," vol. i., Lecture X.

Habit illustrated by Morphology.

We have hitherto been considering the mnemic quality of movements; but, as I have attempted to show, morphological changes are reactions to stimulation of the same kind as these temporary changes. It is indeed from the morphological reactions of living things that the most striking cases of habit are, in my opinion, to be found.

The development of the individual from the germ-cell takes place by a series of stages of cell-division and growth, each stage apparently serving as a stimulus to the next, each unit following its predecessor like the movements linked together in an habitual action performed by an animal.

My view is that the rhythm of ontogeny is actually and literally a habit. It undoubtedly has the feature which I have described as preeminently characteristic of habit, viz., an automatic quality which is seen in the performance of a series of actions in the absence of the complete series of stimuli to which they (the stages of ontogeny) were originally due. This is the chief point on which I wish to insist—I mean that the resemblance between ontogeny and habit is not merely superficial, but deeply seated. It was with this conclusion in view that I dwelt, at the risk of being tedious, on the fact that memory has its place in the morphological as well as in the temporary reactions of living things. It cannot be denied that the ontogenetic rhythm has the two qualities observable in habit—namely, a certain degree of fixity or automaticity, and also a certain variability. A habit is not irrevocably fixed, but may be altered in various ways. Parts of it may be forgotten or new links may be added to it. In ontogeny the fixity is especially observable in the earlier, the variability in the later, stages. Mr. Darwin has pointed out that "on the view that species are only strongly marked and fixed varieties, we might expect often to find them still continuing to vary in those parts of their structure which have varied within a moderately recent period." These remarks are in explanation of the "notorious" fact that specific are more variable than generic character—a fact for which it is "almost superfluous to adduce evidence."[1] This, again, is what we find in habit: take the case of a man who, from his youth up, has daily repeated a certain form of words. If in middle life an addition is made to the formula, he will find the recently acquired part more liable to vary than the rest.

Again, there is the wonderful fact that, as the ovum develops into the perfect organism, it passes through a series of changes which are believed to represent the successive forms through which its ancestors passed in the process of evolution. This is precisely paralleled by our own experience of memory, for it often happens that we cannot reproduce the last learned verse of a poem without repeating the earlier part; each verse is suggested by the previous one and acts as a stimulus for the next. The blurred and imperfect character of the ontogenetic version of the phylogenetic series may at least remind us of the tendency to abbreviate by omission what we have learned by heart.

In all bi-sexual organisms the ontogenetic rhythm of the offspring is a combination of the rhythms of its parents. This may or may not be visible in the offspring; thus in the crossing of two varieties the mongrel assumes the character of the prepotent parent. Or the offspring

1 "Origin of Species, 6th edit., p. 22.

may show a blend of both parental characters. Semon[1] uses as a model the two versions of Goethe's poem—

"Ueber allen Gipfeln, ist Ruh, in allen { Wäldern, hörest du, keinen Hauch." / Wipfeln, spürest du, kaum einen Hauch." }

One of these terminations will generally be prepotent, probably the one that was heard first or heard most often. But the cause of such prepotency may be as obscure as the corresponding occurrence in the formation of mongrels. We can only say that in some persons the word "allen" releases the word "Wäldern," while in others it leads up to "Wipfeln." Again, a mixture of the terminations may occur leading to such a mongrel form as: "in allen Wäldern hörest du kaum einen Hauch." The same thing is true of music; a man with an imperfect memory easily interpolates in a melody a bar that belongs elsewhere. In the case of memory the introduction of a link from one mental rhythm into another can only occur when the two series are closely similar, and this may remind us of the difficulty of making a cross between distantly related forms.

Enough has been said to show that there is a resemblance between the two rhythms of development and of memory; and that there is at least a *prima facie* case for believing them to be essentially similar. It will be seen that my view is the same as that of Hering, which is generally described as the identification of memory and inheritance.[2] Hering says that "between the *me* of to-day and the *me* of yesterday lie night and sleep, abysses of unconsciousness; nor is there any bridge but memory with which to span them." And in the same way he claims that the abyss between two generations is bridged by the unconscious memory that resides in the germ-cells. It is also the same as that of Semon and to a great extent as that of Rignano.[3] I, however, prefer at the moment to limit myself to asserting the identity of ontogeny and habit, or, more generally, to the assertion in Semon's phraseology, that ontogeny is a mnemic phenomenon.

Evolution, in its modern sense, depends on a change in the ontogenetic rhythm. This is obvious, since if this rhythm is absolutely fixed, a species can never give rise to varieties. This being so, we have to ask *in what ways* the ontogenetic rhythm can be altered. An habitual action, for instance, a trick learned by a dog, may be altered by adding new accomplishments; at first the animal will persist in finishing his performance at the old place, but at last the extended trick will be bonded into a rhythm of actions as fixed as was the original simpler performance. May we not believe that this is what has occurred in evolution?

We know from experiment that a plant may be altered in form by causes acting on it during the progress of development. Thus a beech tree may be made to develop different forms of leaves by exposing it to sunshine or to shade. The ontogeny is different in the two cases, and what is of special interest is, that there exist shade-loving plants in which a structure similar to that of the shaded beech-leaf is apparently typical of the species, but on this point it is necessary to speak with caution. In the same way Goebel points out that in some orchids the assimilating roots take on a flattened form when exposed to sunlight, but in others this morphological change has become automatic, and occurs even in darkness.[4]

Such cases suggest at least the possibility of varieties arising as changes in or additions to the later stages of ontogeny. This is, briefly given, the epigenetic point of view.

But there is another way of looking at the matter—namely, that upheld by Galton and Weismann. According to this view ontogeny can only be changed by a fundamental upset of the whole system—namely, by an alteration occurring in its first stage, the germ-cell, and this view is now very generally accepted.

The same type of change may conceivably occur in memory or habit, that is, the rhythm as a whole may be altered by some cause acting on the nerve-centres connected with the earlier links of the series. The analogy is not exact, but such an imaginary case is at least of a different type from a change in habit consisting in the addition of a new link or the alteration of one of the latest formed links. If we were as ignorant of the growth of human actions as we are of variation, we might have a school of naturalists asserting that all changes in habit originate in the earliest link of the series. But we know that this is not the case. On the other hand, I fully admit that the structure of an ovum may in this way be altered, and give rise to a variation which may be the starting-point of a new species.

But how can a new species originate according to an epigenetic theory? How can a change in the latter stages of ontogeny produce a permanent alteration in the germ-cells? Our answer to this question will depend on our views of the structure of the germ-cells. According to the mnemic theory they have the quality which is found in the highest perfection in nerve-cells, but is at the same time a character of all living matter—namely, the power of retaining the residual effects of former stimuli and of giving forth or reproducing under certain conditions an echo of the original stimulus. In Semon's phraseology germ-cells must, like nerve-cells, contain engrams, and these engrams must be (like nerve-engrams) bonded together by association, so that they come into action one after another in a certain order automatically, *i.e.*, in the absence of the original stimuli.

This seems to me the strength of the mnemic theory—namely, that it accounts for the preformed character of germ-cells by the building up in them of an organised series of engrams. But if this view has its strength, it has also its weakness. Routine can only be built up by repetition, but each stage in ontogeny occurs only once in a lifetime. Therefore if ontogeny is a routine each generation must be mnemically connected with the next. This can only be possible if the germ-cells are, as it were, in telegraphic communication with the whole body of the organism; so that as ontogeny is changed by the addition of new characters, new engrams are added to the germ-cell.

Thus in fact the mnemic theory of development depends on the possibility of what is known as somatic inheritance or the inheritance of acquired characters. This is obvious to all those familiar with the subject, but to others it may not be so clear. Somatic inheritance is popularly interesting in relation to the possible inherited effects of education, or of mutilations, or of the effects of use and disuse. It is forgotten that it may be, as I have tried to show, an integral part of all evolutionary development.

Weismann's Theory.

Everyone must allow that if Weismann's theory of inheritance is accepted we cannot admit the possibility of somatic inheritance. This may be made clear to those unfamiliar with the subject by an illustration taken from the economy of an ant's nest or beehive. The queen[1] on whom depends the future of the race is cut off from all active experience of life: she is a mere reproducing machine, housed, fed, and protected by the workers. But these, on whom falls the burden of the struggle for life and the experience of the world generally, are sterile, and take no direct share in the reproduction of the species. The queen represents Weismann's germ-plasm, the workers are the body or soma. Now imagine the colony exposed to some injurious change in environment; the salvation of the species will depend on whether or no an improved pattern of worker can be produced. This depends on the occurrence of appropriate variations, so that the queen bee and the drones, on whom this depends, are of central importance. On the other hand any change occurring in the workers, for instance, increased skill due to practice in doing their work or changes in their structure due to external conditions, cannot possibly be inherited, since

1 "Die Mneme," 2nd edit., pp. 147, 221, 303, 345.

2 Everyone who deals with this subject must take his stand on the foundation laid by Hering in his celebrated address given at Vienna in 1870 and reprinted in No. 148 of Ostwald's *Exakt Klassiker*. The passage quoted (p. 14) is from Samuel Butler's translation of Hering in "Unconscious Memory," 1880, p. 110. Butler had previously elaborated the view that "we are one person with our ancestors" in his entertaining book "Life and Habit," 1878, and this was written in ignorance of Hering's views.

3 "Sur la Transmissibilité des Caractères acquis," Paris, 1906.

4 Goebel's "Organography of Plants," part ii., p. 285.

1 Nor do the drones share the activity of the workers.

workers are absolutely cut off from the reproduction of the race. According to Weismann, there is precisely the same bar to the inheritance of somatic change.

The racial or phyletic life of all organisms is conceived by him as a series of germ-cells the activity of which is limited to varying, and the survival of which in any generation depends on the production of a successful soma or body capable of housing, protecting, and feeding the germ-cell. Most people would *a priori* declare that a community where experience and action are separated must fail. But the bee's nest, which must be allowed to be something more than an illustration of Weismann's theory, proves the contrary.

It is clear that there must be war to the knife between the theory of Weismann and that of the somatists—to coin a name for those who believe in the inheritance of acquired characters. A few illustrations may be given of the strength of Weismann's position. Some trick or trivial habit appears in two successive generations, and the son is said to inherit it from his father. But this is not necessarily a case of somatic inheritance, since according to Weismann the germ-plasm of both father and son contained the potentiality of the habit in question. If we keep constantly in view Weismann's theory of continuity, the facts which are supposed to prove somatic inheritance cease to be decisive.

Weismann has also shown by means of his hypothesis of "simultaneous stimulation"[1] the unconvincingness of a certain type of experiment. Thus Fischer showed that when chrysalids of *Arctia caja* are subjected to low temperature a certain number of them produce dark-coloured insects; and further that these moths mated together yield dark-coloured offspring. This has been held to prove somatic inheritance, but Weismann points out that it is explicable by the low temperature having an identical effect on the colour-determinants existing in the wing-rudiments of the pupa, and on the same determinants occurring in the germ-cells.

It does not seem to me worth while to go in detail into the evidence by which somatists strive to prove their point, because I do not know of any facts which are really decisive. That is to say, that though they are explicable as due to somatic inheritance, they never seem to me absolutely inexplicable on Weismann's hypothesis. But, as already pointed out, it is not necessary to look for special facts and experiments, since if the mnemic theory of ontogeny is accepted the development of every organism in the world depends on somatic inheritance.

I fully acknowledge the strength of Weismann's position; I acknowledge also most fully that it requires a stronger man than myself to meet that trained and well-tried fighter. Nevertheless, I shall venture on a few remarks. It must be remembered that, as Romanes[2] pointed out, Weismann has greatly strengthened his theory of heredity by giving up the absolute stability and perpetual continuity of germ-plasm. Germ-plasm is no longer that mysterious entity, immortal and self-contained, which used to suggest a physical soul. It is no longer the aristocrat it was when its only activity was dependent on its protozoan ancestors, when it reigned absolutely aloof from its contemporary subjects. The germ-plasm theory of to-day is liberalised, though it is not so democratic as its brother sovereign Pangenesis, who reigns, or used to reign, by an elaborate system of proportional representation. But in spite of the skill and energy devoted to its improvement by its distinguished author, Weismannism fails, in my opinion, to be a satisfactory theory of evolution.

All such theories must account for two things which are parts of a single process but may logically be considered separately: (i) The fact of ontogeny, namely, that the ovum has the capacity of developing into a certain more or less predetermined form; (ii) The fact of heredity—the circumstance that this form is approximately the same as that of the parent.

The doctrine of pangenesis accounts for heredity, since the germ-cells are imagined as made up of gemmules representing all parts of the adult; but it does not account for ontogeny, because there seems to me no sufficient reason why the gemmules should become active in a predetermined order unless, indeed, we allow that they do so by habit, and then the doctrine of pangenesis becomes a variant of the mnemic theory.

[1] I borrow this convenient expression from Plate's excellent book, "Ueber die Bedeutung des Darwin'schen Selectionsprincips," 1903, p. 81.

[2] "An Examination of Weismann," 1893, pp. 169, 170.

The strength of Weismann's theory lies in its explanation of heredity. According to the doctrine of continuity, a fragment of the germ-plasm is, as it were, put on one side and saved up to make the germ-cell of the new generation, so that the germ-cells of two successive generations are made of the same material. This again depends on Weismann's belief that when the ovum divides, the two daughter cells are not identical; that in fact the fundamental difference between soma and germ-cells begins at this point. But this is precisely where many naturalists whose observations are worthy of all respect differ from him. Weismann's theory is therefore threatened at the very foundation.

Even if we allow Weismann's method of providing for the identity between the germ-cell of two successive generations, there remains, as above indicated, a greater problem —namely, that of ontogeny. We no longer look at the potentiality of a germ-cell as Caliban looked on Setebos, as something essentially incomprehensible ruling the future in an unknown way—"just choosing so." If the modern germ-cell is to have a poetic analogue it must be compared to a Pandora's box of architectonic sprites which are let loose in definite order, each serving as a master builder for a prescribed stage of ontogeny. Weismann's view of the mechanism by which his determinants—the architectonic sprites—come into action in due order is, I assume, satisfactory to many, but I confess that I find it difficult to grasp. The orderly distribution of determinants depends primarily on their arrangement in the ids, where they are held together by "vital affinities." They are guided to the cells on which they are to act by differential divisions, in each of which the determinants are sorted into two unequal lots. They then become active, *i.e.*, break up into biophores, partly under the influence of liberating stimuli and partly by an automatic process. Finally the biophores communicate a "definite vital force" to the appropriate cells.[1] This *may* be a description of what happens; but inasmuch as it fails to connect the process of ontogeny with physiological processes of which we have definite knowledge, it does not to me seem a convincing explanation.

For myself I can only say that I am not satisfied with Weismann's theory of heredity or of ontogeny. As regards the first, I incline to deny the distinction between germ and soma, to insist on the plain facts that the soma is continuous with the germ-cell, and that the somatic cells may have the same reproductive qualities as the germ-cells (as is proved by the facts of regeneration); that, in fact, the germ-cell is merely a specialised somatic cell and has the essential qualities of the soma. With regard to ontogeny, I have already pointed out that Weismann does not seem to explain its automatic character.

The Mnemic Theory.

If the mnemic theory is compared with Weismann's views it is clear that it is strong precisely where these are weakest—namely, in giving a coherent theory of the rhythm of development. It also bears comparison with all theories in which the conception of determinants occurs. Why should we make elaborate theories of hypothetical determinants to account for the potentialities lying hidden in the germ-cell, and neglect the only determinants of the existence of which we have positive knowledge (though we do not know their precise nature)? We know positively that by making a dog sit up and then giving him a biscuit we build up something in his brain in consequence of which a biscuit becomes the stimulus to the act of sitting. The mnemic theory assumes that the determinants of morphological change are of the same type as the structural alteration wrought in the dog's brain.

The mnemic theory—at any rate that form of it held by Semon and by myself—agrees with the current view, viz., that the nucleus is the centre of development, or, in Semon's phraseology, that the nucleus contains the

[1] "The Evolution Theory," Eng. trans., i. 373 *et seq.*

engrams in which lies the secret of the ontogenetic rhythm. But the mode of action of the mnemic nucleus is completely different from that of Weismann. He assumes that the nucleus is disintegrated in the course of development by the dropping from it of the determinants which regulate the manner of growth of successive groups of cells. But if the potentiality of the germ nucleus depends on the presence of engrams, if, in fact, its function is comparable to that of a nerve-centre, its capacity is not diminished by action; it does not cast out engrams from its substance as Weismann's nucleus is assumed to drop armies of determinants. The engrams are but cut deeper into the records, and more closely bonded one with the next. The nucleus, considered as a machine, does not lose its component parts in the course of use. We shall see later on that the nuclei of the whole body may, on the mnemic theory, be believed to become alike. The fact that the mnemic theory allows the nucleus to retain its repeating or reproductive or mnemic quality supplies the element of continuity. The germ-cell divides and its daughter cells form the tissues of the embryo, and in this process the original nucleus has given rise to a group of nuclei; these, however, have not lost their engrams, but retain the potentiality of the parent nucleus. We need not therefore postulate the special form of continuity which is characteristic of Weismann's theory.

We may say, therefore, that the mnemic hypothesis harmonises with the facts of heredity and ontogeny. But the real difficulties remain to be considered, and these, I confess, are of a terrifying magnitude.

The first difficulty is the question how the changes arising in the soma are, so to speak, telegraphed to the germ-cells. Hering allows that such communication must at first seem highly mysterious.[1] He then proceeds to show how by the essential unity and yet extreme ramification of the nervous system "all parts of the body are so connected that what happens in one echoes through the rest, so that from the disturbance occurring in any part some notification, faint though it may be, is conveyed to the most distant parts of the body."

A similar explanation is given by Nägeli. He supposes that adaptive, in contradistinction to organic, characters are produced by external causes; and since these characters are hereditary there must be communication between the seat of adaptation and the germ-cells. This telegraphic effect is supposed to be effected by the network of idioplasm which traverses the body, in the case of plants by the intercellular protoplasmic threads.

Semon faces the difficulty boldly. When a new character appears in the body of an organism, in response to changing environment, Semon assumes that a new engram is added to the nuclei in the part affected; and that, further, the disturbance tends to spread to all the nuclei of the body (including those of the germ-cells), and to produce in them the same change. In plants the flow must be conceived as travelling by intercellular plasmic threads, but in animals primarily by nerve-trunks. Thus the reproductive elements must be considered as having in some degree the character of nerve-cells. So that, for instance, if we are to believe that an individual habit may be inherited and appear as an instinct, the repetition of the habit will not merely mean changes in the central nervous system, but also corresponding changes in the germ-cells. These will be, according to Semon, excessively faint in comparison to the nerve-engrams, and can only be made efficient by prolonged action. Semon lays great stress on the slowness of the process of building up efficient engrams in the germ-cells.

Weismann[2] speaks of the impossibility of germinal engrams being formed in this way. He objects that nerve-currents can only differ from each other in intensity, and therefore there can be no communication of potentialities to the germ-cell. He holds it to be impossible that somatic changes should be telegraphed to the germ-cell and be reproduced ontogenetically—a process which he compares to a telegram despatched in German and arriving in

[1] E. Hering in Ostwald's *Klassiker der exakten Wissenschaften*, No. 148, p. 14; see also S. Butler's translation in "Unconscious Memory," p. 119.

[2] Weismann, "The Evolution Theory," 1904, vol. ii. p. 63; also his "Richard Semon's 'Mneme' und die Vererbung erworbener Eigenschaften," in the *Archiv für Rassen- und Gesellschafts-Biologie*, 1906. Semon has replied in the same journal for 1907.

Chinese. According to Semon,[1] what radiates from the point of stimulation in the soma is the primary excitation set up in the somatic cells; if this is so, the radiating influence will produce the same effect on all the nuclei of the organism. My own point of view is the following. In a plant (as already pointed out) the ectoplasm may be compared to the sense-organ of the cell, and the primary excitation of the cell will be a change in the ectoplasm; but since cells are connected by ectoplasmic threads the primary excitation will spread and produce in other cells a faint copy of the engram impressed on the somatic cells originally stimulated. But in all these assumptions we are met by the question to which Weismann has directed attention—namely, whether nervous impulses can differ from one another in *quality*?[2] The general opinion of physiologists is undoubtedly to the opposite effect—namely, that all nervous impulses are identical in quality. But there are notable exceptions, for instance, Hering,[3] who strongly supports what may be called the qualitative theory. I am not competent to form an opinion on the subject, but I confess to being impressed by Hering's argument that the nerve-cell and nerve-fibre, as parts of one individual (the neuron), must have a common irritability. On the other hand there is striking evidence, in Langley's[4] experiments on the cross-grafting of efferent nerves, that here at least nerve impulses are interchangeable and therefore identical in quality. The state of knowledge as regards afferent nerves is, however, more favourable to my point of view. For the difficulties that meet the physiologist—especially as regards the nerves of smell and hearing—are so great that it has been found simpler to assume differences in impulse-quality, rather than attempt an explanation of the facts on the other hypothesis.[5]

On the whole it may be said that, although the trend of physiological opinion is against the general existence of qualitative differences in nerve-impulses, yet the question cannot be said to be settled either one way or the other.

Another obvious difficulty is to imagine how within a single cell the engrams or potentialities of a number of actions can be locked up. We can only answer that the nucleus is admittedly very complex in structure. It may be added (but this not an answer) that in this respect it claims no more than its neighbours; it need not be more complex than Weismann's germ-plasm. One conceivable simplification seems to be in the direction of the pangenes of De Vries. He imagines that these heritage-units are relatively small in number, and that they produce complex results by combination, not by each being responsible for a minute fraction of the total result.[6] They may be compared to the letters of the alphabet which by combination make an infinity of words.[7] Nägeli[8] held a similar view. "To understand heredity," he wrote, "we do not need a special independent symbol for every difference conditioned by space, time, or quality, but a substance which can represent every possible combination of differences by the fitting together of a limited number of elements, and which can be transformed by permutations into other combinations." He applied (*loc. cit.*, p. 59) the idea of a combination of symbols to the telegraphic quality of his idioplasm. He suggests that as the nerves convey the most varied perceptions of external objects to the central nervous system, and there create a coherent picture, so it is not impossible that the idioplasm may convey a combination of its local alterations to other parts of the organism.

Another theory of simplified telegraphy between soma and germ-cell is given by Rignano.[9] I regret that the

[1] Semon, "Mneme," ed. i. p. 142, does not, however, consider it proved that the nucleus is necessarily the smallest element in which the whole inheritance resides. He refers especially to the regeneration of sections of Stentor which contain mere fragments of the nucleus.

[2] I use this word in the ordinary sense without reference to what is known as *modality*.

[3] "Zur Theorie der Nerventhätigkeit," Akademische Vortrag, 1898 (Veit, Leipzig).

[4] Proc. R. Soc., 1904, p. 99. *Journal of Physiology*, xxiii. p. 240, and xxxi. p. 365.

[5] See Nagel, "Handbuch der Physiologie des Menschen," iii. (1905), pp. 1-15.

[6] De Vries, "Intracellular Pangenesis," p. 7.

[7] I take this comparison from Lotsy's account of De Vries's theory. Lotsy, "Vorlesungen über Deszendenztheorien," 1906, i. p. 98.

[8] Nägeli's "Abstammungslehre," 1884. p. 73.

[9] For what is here given I am partly indebted to Signor Rignano's letters.

space at my command does not permit me to give a full account of his interesting speculation on somatic inheritance. It resembles the theories of Hering, Butler, and Semon in postulating a quality of living things, which is the basis both of memory and inheritance. But it differs from them in seeking for a physical explanation or model of what is common to the two. He compares the nucleus to an electric accumulator which in its discharge gives out the same sort of energy that it has received. How far this is an allowable parallel I am not prepared to say, and in what follows I have given Rignano's results in biological terms. What interests me is the conclusion that the impulse conveyed to the nucleus of the germ-cell is, as far as results are concerned, the external stimulus. Thus, if a somatic cell (A) is induced by an external stimulus (S) acting on the nucleus to assume a new manner of development, a disturbance spreads through the organism, so that finally the nuclei of the germ-cells are altered in a similar manner. When the cellular descendants of the germ-cells reach the same stage of ontogeny as that in which the original stimulation occurred, a stimulus comes into action equivalent to S as regards the results it is capable of producing. So that the change originally wrought in cell A by the actual stimulus S is now reproduced by what may be called an inherited stimulus. But when A was originally affected other cells, B, C, D, may have reacted to S by various forms of growth. And therefore when during the development of the altered germ-cell something equivalent to S comes into play, there will be induced, not merely the original change in the development of A, but also the changes which were originally induced in the growth of B, C, D. Thus, according to Rignano, the germ-nucleus releases a number of developmental processes, each of which would, according to Weismann, require a separate determinant.

If the view here given is accepted, we must take a new view of Weismann's cases of *simultaneous stimulation, i.e.*, cases like Fischer's experiments on *Arctia caja*, which he does not allow to be somatic inheritance. If we are right in saying that, the original excitation of the soma is transferred to the germ-cell, and it does not matter whether the stimulus is transferred by "telegraphy," or whether a given cause, *e.g.*, a low temperature, acts simultaneously on soma and germ-cell. In both cases we have a given alteration produced in the nuclei of the soma and the germ-cell. Nägeli used the word *telegraphy* to mean a dynamic form of transference, but he did not exclude the possibility of the same effect being produced by the movement of chemical substances, and went so far as to suggest that the sieve tubes might convey such stimuli in plants. In any case this point of view [1] deserves careful consideration.

Still another code of communication seems to me to be at least conceivable. One of the most obvious characteristics of animal life is the guidance of the organism by certain groups of stimuli, producing either a movement of seeking (positive reaction [2]) or one of avoidance (negative reaction). Taking the latter as being the simplest, we find that in the lowest as in the highest organisms a given reaction follows each one of a number of diverse conditions which have nothing in common save that they are broadly harmful in character. We withdraw our hands from a heated body, a prick, a corrosive substance, or an electric shock. The interesting point is that it is left to the organism to discover by the method of trial and error the best means of dealing with a sub-injurious stimulus. May we not therefore say that the existence of pleasure and pain simplifies inheritance? It certainly renders unnecessary a great deal of detailed inheritance. The innumerable appropriate movements performed by animals are broadly the same as those of their parents, but they are not necessarily inherited in every detail; they are rather the unavoidable outcome of hereditary but unspecialised sensitiveness. It is as though heredity were arranged on a code-system instead of by separate signals for every movement of the organism.

It may be said that in individual life the penalty of failure is pain, but that the penalty for failure in ontogenetic morphology is death. But it is only because pain is the shadow cast by Death as he approaches that it is of value to the organism. Death would be still the penalty of creatures that had not acquired this sensitiveness to the edge of danger. Is it not possible that the sensitiveness to external agencies by which structural ontogeny is undoubtedly guided may have a similar quality, and that morphological variations may also be reactions to the edge of danger. But this is a point of view I cannot now enter upon.

It may be objected that the inheritance of anything so complex as an instinct is difficult to conceive on the mnemic theory. Yet it is impossible to avoid suspecting that at least some instincts originate in individual acquirements, since they are continuous with habits gained in the lifetime of the organism. Thus the tendency to peck at any small object is undoubtedly inherited; the power of distinguishing suitable from unsuitable objects is gained by experience. It may be said that the engrams concerned in the pecking instinct cannot conceivably be transferred from the central nervous system to the nucleus of the germ-cells. To this I might answer that this is not more inconceivable than Weismann's assumption that the germ-cell chances to be so altered that the young chicken pecks instinctively. Let us consider another case of what appears to be an hereditary movement. Take, for instance, the case of a young dog, who in fighting bites his own lips. The pain thus produced will induce him to tuck up his lips out of harm's way. This protective movement will become firmly associated with, not only the act of fighting, but with the remembrance of it, and will show itself in the familiar snarl of the angry dog. This movement is now, I presume, hereditary in dogs, and is so strongly inherited by ourselves (from simian ancestors) that a lifting of the corner of the upper lip is a recognised signal of adverse feeling. Is it really conceivable that the original snarl is due to that unspecialised stimulus we call pain, whereas the inherited snarl is due to fortuitous upsets of the determinants in the germ-cell?

I am well aware that many other objections may be advanced against the views I advocate. To take a single instance, there are many cases where we should expect somatic inheritance, but where we look in vain for it. This difficulty, and others equally important, must for the present be passed over. Nor shall I say anything more as to the possible means of communication between the soma and the germ-cells. To me it seems conceivable that some such telegraphy is possible. But I shall hardly wonder if a majority of my hearers decide that the available evidence in its favour is both weak and fantastic. Nor can I wonder that, apart from the problem of mechanism, the existence of somatic inheritance is denied for want of evidence. But I must once more insist that, according to the mnemic hypothesis, somatic inheritance lies at the root of all evolution. Life is a gigantic experiment which the opposing schools interpret in opposite ways. I hope that in this dispute both sides will seek out and welcome decisive results. My own conviction in favour of somatic inheritance rests primarily on the automatic element in ontogeny. It seems to me certain that in development we have an actual instance of habit. If this is so, somatic inheritance must be a *vera causa*. Nor does it seem impossible that memory should rule the plasmic link which connects successive generations—the true miracle of the camel passing through the eye of a needle—since, as I have tried to show, the reactions of living things to their surroundings exhibit in the plainest way the universal presence of a mnemic factor.

We may fix our eyes on phylogeny and regard the living world as a great chain of forms, each of which has learned something of which its predecessors were ignorant; or we may attend rather to ontogeny, where the lessons learned become in part automatic. But we must remember that the distinction between phylogeny and ontogeny is an artificial one, and that routine and acquisition are blended in life.[1]

[1] See Semon, *Archiv f. Rassen- und Gesellschafts-Biologie*, 1907, p. 39.
See Jennings, "Behaviour of the Lower Organisms."

[1] This subject is dealt with in a very interesting manner in Prof. James Ward's forthcoming lectures on the "Realm of Ends." Also in his article on "Mechanism and Morals" in the *Hibbert Journal*, October, 1905 p. 92; and in his article on "Psychology" in the "Encyclopædia Britannica," 1886, vol. xx. p. 44.

The great engine of natural selection is taunted nowadays, as it was fifty years ago, with being merely a negative power. I venture to think that the mnemic hypothesis of evolution makes the positive value of natural selection more obvious. If evolution is a process of drilling organisms into habits, the elimination of those that cannot learn is an integral part of the process, and is no less real because it is carried out by a self-acting system. It is surely a positive gain to the harmony of the universe that the discordant strings should break. But natural selection does more than this; and just as a trainer insists on his performing dogs accommodating themselves to conditions of increasing complexity, so does natural selection pass on its pupils from one set of conditions to other and more elaborate tests, insisting that they shall endlessly repeat what they have learned and forcing them to learn something new. Natural selection attains in a blind, mechanical way the ends gained by a human breeder; and by an extension of the same metaphor it may be said to have the power of a trainer—of an automatic master with endless patience and all time at his disposal.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY W. N. SHAW, SC.D., LL.D., F.R.S., PRESIDENT OF THE SECTION.

IT is with much misgiving that I endeavour to discharge the traditional duty of the President of a Section of the British Association. So many other duties seem to find a natural resting-place with anyone who has to reckon at the same time with the immediate requirements of the public, the claims of scientific opinion, and the interests of posterity, that, unless you are content with such contribution towards the advancement of the sciences of mathematics and physics as my daily experience enables me to offer you, I shall find the task impossible.

With a leaning towards periodicity perhaps slightly unorthodox I have looked back to see what they were doing in Section A fifty years ago. Richard Owen was President of the Association, William Whewell was President of Section A for the fifth time.

At the meeting of 1858 they must have spent some time over nineteen very substantial reports on researches in science, which included a large section of Mallett's facts and theory of earthquake phenomena, magnetic surveys of Great Britain and of Ireland, and, oddly enough, an account of the self-recording anemometer by Beckley; perhaps a longer time was required for fifty-seven Papers contributed to the Section, but very little was spent over the Presidential Address, for it only occupies two pages of print. My inclination towards periodicities and another consideration lead me to regard the precedent as a good one. That other consideration is that Section A has always more subjects for discussion than it can properly dispose of; and, in this case, discipline, like charity, might begin at home.

Since the Section met last year it has lost its most illustrious member and its most faithful friend. Lord Kelvin made his first contribution to Section A at Cambridge in 1845, on the elementary laws of statical electricity; he was President of the Section in 1852 at Belfast for the first of five times. I have looked to see what suggestion I could derive from his first essay in that capacity. I can find no reference to any Address in the published volume. I wish I had the courage to follow that great example.

Lord Kelvin's association with Section A was so constant and so intimate that it requires more than a passing word of reference. There is probably no student of Mathematics or Physics grown into a position of responsibility in this country but keeps among his treasured reminiscences some words of inspiration and of encouragement from Kelvin, spoken in the surroundings which we are once more met to inaugurate. I refer to those unrecorded acts of kindness and help because they were really a striking characteristic of Section A. Their value for the amenity as well as for the advancement of science it would be difficult to overestimate. I could not, even if time permitted, hope to set before you an adequate appreciation of Kelvin's contributions to Science as illustrated by his communications to this Section, and in this place it is not necessary. But I cannot pass over that feature of his character without notice.

Closely following on the loss of Kelvin came the death of Sir Richard Strachey, a personal loss to which it is difficult to give expression. I am not aware that he had much to do with Section A. I wish, indeed, that the Section had seen its way to bring him more closely into touch with its proceedings. He was President of Section E in 1875, and, by appointment of the Royal Society, he was for twenty-two years Chairman of the Meteorological Council. I had the good fortune to be very closely associated with him during the last ten years of his life, and to realise the ideas which lay behind his official actions and to appreciate the reality of his services to science in the past and for the future.

These losses unfortunately do not stand alone. Only last year Sir John Eliot received the congratulations of all his fellow-workers upon the publication of his Climatological Atlas of India as representing the most conspicuous achievement of orderly, deliberate, purposeful compilation of meteorological facts for a special area that has yet been seen. He was full of projects for a handbook to accompany the atlas, and of ideas for the prosecution of meteorological research over wide areas by collecting information from all the world and enlisting the active cooperation of the constituent parts of the British Empire in using those observations for the advancement of science and the benefit of mankind. He died quite suddenly on March 18, not young as years go, but quite youthful in the deliberate purpose of manifold scientific activities and in his irrepressible faith in the future of the science which he has adorned.

The Section will, I hope, forgive me if I put before them some considerations which the careers of these three men suggest. Kelvin, a mathematician, a natural philosopher, a University Professor, some part of whose scientific work is known to each one of us. He was possessed with the notion that Mathematics and Natural Philosophy are applicable in every part of the work of daily life, and made good the contention by presenting to the world, besides innumerable theoretical papers, instruments of all degrees of complexity, from the harmonic analyser to an improved water-tap. It was he who transfigured and transformed the mariner's compass and the lead-line into instruments which have been of the greatest practical service. It was he who, when experimental science was merely a collection of facts or generalisations, conceived the idea of transfiguring every branch of it by the application of the principles of natural philosophy, as Newton had transfigured astronomy. The ambition of Thomson and Tait's "Natural Philosophy," of which only the first volume reached the stage of publication, is a fair index of Kelvin's genius.

Strachey, on the other hand, by profession a military engineer, a great administrator, head of the Public Works Department in India, deeply versed in finance and in all the other constituent parts of administration, by his own natural instinct demanded the assistance of science for every branch of administration. In promoting the development of botany, of meteorology, of geodesy, and of mathematics, he was not administering the patronage of a Macænas, but claiming the practical service of science in forestry, in agriculture, in famine relief, in public works, and in finance. You cannot gauge Strachey's services to science by the papers which he contributed to scientific societies, if you leave out of account the fact that they were really incidents in the opening of fresh channels of communication between scientific work and the public service.

And Eliot, as Meteorological Reporter to the Government of India, an accomplished mathematician (for he was second wrangler and first Smith's prizeman in 1869), a capable and devoted public servant, the medium by which Strachey's ideas as regards the use of meteorology in administration found expression in the Government of India, who caught the true perception of the place of science in the service of the State, and made his office the indispensable handmaid of the Indian administration. These three men together, who have all passed away within a space of three months, are such representative types of scientific workers, complementary and supplementary, that a similar combination is not likely to occur again. All three indispensable, yet no two alike, except in their enthusiasm for the sciences for the advancement of which Section A exists.

To these I might indeed add another type, the private contributor to the physical exploration of the visible universe, of which Ireland furnishes so many noble examples; and in that connection let me give expression to the sense of grievous loss, to this Association and to Science, occasioned by the premature death of W. E. Wilson, of Daramona, a splendid example of that type.

In the division of the work of advancing the sciences of mathematics and physics and their application to the service of mankind, I am reminded of Dryden's somewhat lopsided comparison of the relative influence of music and song in his Ode to St. Cecilia's Day. If I may be pardoned for comparing small things with great, the power of Timotheus' music over Alexander's moods was hardly less complete than Kelvin's power to touch every department of the working world with his genius. But I may remind you that, after a prolonged description of the tremendous influence of Timotheus upon the victorious hero, the poet deals in one stanza with his nominal subject:—

> " At last divine Cecilia came,
> Inventress of the vocal frame;
> The sweet enthusiast, from her sacred store,
> Enlarged the former narrow bounds
>
> With nature's mother-wit, and arts unknown before.
>
> Let old Timotheus yield the prize,
> Or both divide the crown;
> He raised a mortal to the skies,
> She drew an angel down."

I doubt if any of my hearers who knew Strachey by sight would recognise in him the scientific reincarnation of St. Cecilia, but it is none the less true that he was preeminent among men in inventing the means of drawing angels down and using their service for the attuning of common life to a scientific standard. It may be equally hard for those who knew him to look upon Eliot as a vocal frame, for of all his physical capacities his voice was the least impressive; and yet it is not untrue to say that he was conspicuously a medium by which the celestial harmonies of the physical sciences were brought into touch with the practical life of India through his work, which is represented by a considerable number of the twenty volumes of Memoirs of the Indian Meteorological Service.

I do not indulge in this poetic extravagance without some underlying reason. Speaking for the physics of the atmosphere, there is a real distinction between these three sides of scientific work. To some is given the power of the mathematician or the physicist to raise the mortal to the skies, to solve some problem which, if not in itself a meteorological one, still has a bearing, sooner or later to be discovered and developed, upon the working of atmospheric phenomena. It is easy enough to cite illustrious examples: among notable instances there recur to my mind Rayleigh's work on the colour of the sky and Pernter's meteorological optics; papers by Ferrel and others on the general circulation of the atmosphere; Kelvin and Rayleigh on the elastic oscillations of the atmosphere; the papers by Hagen, Helmholtz, Oberbeck, Margules, Hertz, and Von Bezold on the dynamics and thermodynamics of the atmosphere, collected and translated by Cleveland Abbe; the work on atmospheric absorption by Langley and the theoretical papers on radiation by Poynting; those on condensation nuclei by Aitken and Wilson, and the recent work on atmospheric electricity, including the remarkable paper by Wilson on the quiet transference of electricity from the air to the ground.

But these things are not of themselves applied to the meteorology of everyday life. It is, in a way, a separate sense, given to few, to realise the possibilities that may result from the solution of new theoretical problems, from the invention of new methods—to grasp, in fact, the idea of bringing the angels down. And, in order that the regular workers in such matters may be in a position constantly to reap the advantages which men of genius provide, the vocal frame must have its permanent embodiment. For the advancement of science in this sense we require all three—the professor with academic freedom to illuminate with his genius any phenomenon which he may be pleased to investigate, the administrator, face to face with the practical problems in which science can help, and the living voice which can tune itself in harmony with the advances of science and in sympathy with the needs of the people whom it serves.

The true relations of these matters are not always apparent. Eliot, bringing to the work of the Indian Meteorological Office a mind trained in the mathematical school of which Kelvin was a most conspicuous exponent, achieved a remarkable success, with which perhaps my hearers are not familiar.

In this country there is a widespread idea that meteorology achieves its object if by its means the daily papers can give such trustworthy advice as will enable a cautious man to decide whether to take out his walking-stick or his umbrella. Some of us are accustomed to look upon India as a place of unusual scientific enlightenment, where governments have a worthy appreciation of the claims of science for recognition and support. But Eliot was never tired of telling me that it was the administration of India, and not the advancement of science, that the Indian administrators had in view; and among his achievements the one of which he was most proud was that the conduct of his office upon scientific lines during his tenure had so commended itself to the administrators that his successor was to be allowed three assistants, with special scientific training, in order that the State might have the benefit of their knowledge.

It is, of course, easy to suggest in explanation of this success that the Department of Public Works of India cannot afford to be unmindful of the distribution of rainfall, and that there is an obvious connection between Indian finances and Indian droughts; but it is a new fact in British history that the application of scientific considerations to the phenomena of rainfall is of such direct practical importance that meteorological information is a matter of consequence to all Government officials, and that meteorological prospects are a factor of finance. Imagine his Majesty's Chancellor of the Exchequer calling at 63 Victoria Street to make inquiries with a view to framing his next Budget, or taking his prospects of a realised surplus from the Daily Weather Report. Yet in India meteorology is to such an extent a public servant that such proceedings would not excite remark.

To have placed a scientific service on such a footing is, indeed, a notable success. Again, I rely upon Eliot when I say that that success is only to be achieved by being constantly on the watch to render service wherever service can be rendered. There is a difference between this attitude and that which has for its object the contribution of an effective paper to a scientific publication; in other words, it must be frankly recognised that the business of the scientific departments of government is not to raise an occasional mortal to the skies, but to draw down as many angels as are within reach. I was much surprised, when Eliot wished to develop a large scheme for meteorological work on a wider scale, that he made his appeal to the British Association as Chairman of the Sub-section for Cosmical Physics at Cambridge, and thereby to the Governments of this country and the Colonies. He felt that he could only urge the Indian Government to join, and he did so successfully, so far as India would be directly benefited thereby, however important the results might be from a purely scientific point of view. Strange as it may appear to some, it was to this country that he looked for assistance, on the plea of the increase of knowledge for its own sake, or for the sake of mankind at large.

I am disposed, therefore, to carry your thoughts a little further, and rely on your patience while I consider another aspect for the process of drawing down the angels from the mathematical and physical sky, a process which is sufficiently indicative of the functions of a State scientific department. Viewing the world at large, and not merely that part of it with which we are ourselves immediately concerned, such departments deal with celestial physics in astronomy, with the physics of the air in meteorology and atmospheric electricity, with the physics of land and water in physical geography and geology, seismology and terrestrial magnetism, oceanography and hydrography. It is for the practical applications of these sciences to the service of the navigator, the fisherman, the husbandman, the miner, the medical man, the engineer, and the general public that there is an obvious public want.

Let me carry you with me in regarding these departments, primarily, as centres for establishing the growth of science by bringing it to bear upon the practical business of life, by a process of regular plantation, and not the occasional importation of an exotic scientific expert. I shall carry you with me also if I say that the gravest danger to such scientific institutions is the tendency to waste. I use the term "waste" not in its narrowest but in its most liberal sense, to include waste of money, waste of effort, waste of scientific opportunity. I do not regard it as a waste that such a department should be unable to emulate Timotheus' efforts. Any aspiration in that direction is, of course, worthy of every encouragement, but the environment is not generally suitable for such achievements. I do, however, regard it as waste if the divine Cecilia is not properly honoured, and if advantage is not taken of the fullest and freest use of the newest and best scientific methods, and their application in the widest manner possible.

I speak for the Office with which I am connected when I say its temptations to waste are very numerous and very serious. It is wasteful to collect observations which will never be used; it is equally wasteful to decline to collect observations which in the future may prove to be of vital importance. It is wasteful to discuss observations that are made with inadequate appliances; it is equally wasteful to allow observations to accumulate in useless heaps because you are not sure that the instruments are good enough. It is wasteful to use antiquated methods of computation or discussion; it is equally wasteful to use all the time in making trial of new methods. It is wasteful to make use of researches if they are inaccurate; it is equally wasteful to neglect the results of researches because you have not made up your mind whether they are accurate or not. It is wasteful to work with an inadequate system in such matters as synoptic meteorology; it is equally wasteful to lose heart because you cannot get all the facilities which you feel the occasion demands.

It is the business of those responsible for the administration of such an office to keep a nice balance of adjustment between the different sides of activity, so that in the long run the waste is reduced to a minimum. There must in any case be a good deal of routine work which is drudgery; and if one is to look at all beyond the public requirements and public appreciation of the immediate present, there must be a certain amount of enterprise and consequently a certain amount of speculation.

Let me remark by the way that there is a tendency among some of my meteorological friends to consider that a meteorological establishment can be regarded as alive, and even in good health, if it keeps up its regular output of observations in proper order and up to date, and that initiative in discussing the observations is exclusively the duty of a central office. That is a view that I should like to see changed. I do not wish to sacrifice my own privilege of initiative in meteorological speculation, but I have no wish for a monopoly. To me, I confess, the speculation which may be dignified by the name of meteorological research is the part of the office work which makes the drudgery of routine tolerable. For my part I should like every worker in the Office, no matter how humble his position may be, somehow or other to have the opportunity of realising that he is taking part in the unravelling of the mysteries of the weather; and I do not think that any establishment, or section of an establishment, that depends upon science can be regarded as really alive unless it feels itself in active touch with that speculation which results in the advancement of knowledge. I do not hesitate to apply to other meteorological establishments, and indeed to all scientific institutions that claim an interest in meteorology, the same criterion of life that I apply to my own office. It is contained in the answer to the question, How do you show your interest in the advancement of our knowledge of the atmosphere? The reply that such and such volumes of data and mean values measure the contribution to the stock of knowledge leaves me rather cold and unimpressed.

But to return to the endeavour after the delicate adjustment between speculation and routine, which will reduce the waste of such an institution to a minimum; experience very soon teaches certain rules.

I have said elsewhere that the peculiarity of meteorological work is that an investigator is always dependent upon other people's observations; his own are only applicable in so far as they are compared with those of others. Up to the present time, I have never known anyone take up an investigation that involved a reference to accumulated data without his being hampered and harassed by uncertainties that might have been resolved if they had been taken in time. I shall give you an example presently, but, in the meantime, experience of that kind is so universal that it has now become with us a primary rule that any data collected shall forthwith be critically examined and so far dealt with as to make sure that they are available for scientific purposes—that is, for the purposes of comparison. A second rule is that as public evidence of the completion of this most important task there shall be at least a line of summary in a published report, or a point on a published map, as a primary representation of the results. Such publication is not to be regarded as the ultimate application of the observations, but it is evidence that the observations are there, and are ready for use.

You will find, if you inquire, that at the Office we have been gradually lining up these troops of meteorological data into due order, with all their buttons on, until, from the commencement of this year, anyone who wishes to do so can hold a general review of the whole meteorological army, in printed order—first-order stations, second-order stations, rainfall stations, sunshine and wind stations, sea temperatures and other marine observations—on his own study table, within six months of the date of the observations, upon paying to his Majesty's Stationery Office the modest sum of four shillings and sixpence. For all the publications except one the interval between observation and publication is only six weeks, and as that one has overtaken four years of arrears within the last four years, I trust that by the end of this year six weeks will be the full measure of the interval between observation and publication in all departments. This satisfactory state of affairs you owe to the indefatigable care and skill of Captain Hepworth, Mr. Lempfert, and Mr. R. H. Curtis, and the members of the staff of the Office who work under their superintendence. I need say little about corresponding work in connection with the Daily Weather Report, in which Mr. Brodie is my chief assistant, although it has received and is receiving a great deal of attention. The promptitude with which the daily work is dealt with hardly needs remark from me, though I know the difficulties of it as well as anyone. If I spend only one long sentence in mentioning that on July 1, 1908, the morning hour of observation at twenty-seven out of the full number of twenty-nine stations in the British Isles was changed from 8 a.m. to 7 a.m., and the corresponding post-offices, as well as the Meteorological Office, opened at 7.15 a.m. in order to deal with them, so that we may have a strictly synchronous international system for Western and Central Europe, and thus realise the aspiration of many years, you will not misunderstand me to mean that I estimate the task as an easy one.

The third general rule is that the effectiveness of the data of all kinds, thus collected and ordered, should be tested by the prosecution of some inquiry which makes use of them in summary or in detail. It is here that the stimulating force of speculative inquiry comes in; and it is in the selection and prosecution of these inquiries, which test not only the adequacy and effectiveness of the data collected, but also the efficiency of the Office as contributing to the advance of knowledge, that the most serious responsibility falls upon the administrators of Parliamentary funds.

Scientific Shylocks are not the least exacting of the tribe, and there have been times when I have thought I caught the rumination :—

> *Shy.* Three thousand ducats? 'tis a good round sum!
> *Bas.* For the which, as I told you, Antonio shall be bound.
> . . . . . .
> *Shy.* Antonio is a good man?
> *Bas.* Have you heard any imputation to the contrary?
> *Shy.* No! no, no, no, no. . . . Yet his means are in supposition: he hath an argosy bound to Tripolis, another to the Indies; I understand moreover, upon the Rialto, that he hath a third in Mexico, a fourth for England, and other ventures he hath squandered abroad. But ships are but boards, sailors but men. There is the peril of water, winds, and rocks. . . . Three thousand ducats.

We at the Meteorological Office are very much in Antonio's position. Our means of research are very much in supposition: four observatories and more than four hundred stations of one sort or another in the British Isles; an elaborate installation of wind-measuring apparatus at Holyhead; besides other ventures squandered abroad; an anemometer at Gibraltar, another at St. Helena; a sunshine recorder at the Falkland Isles, half a dozen sets of instruments in British New Guinea, and a couple of hundred on the wide sea. The efforts seem so disconnected that the rumination about the ducats is not unnatural.

And you must remember that we lack an inestimable advantage that belongs to a physical laboratory or a school of mathematics, where the question of the equivalent number of ducats does not arise in quite the same way. The relative disadvantage that I speak of is that in an office the allowance for the use of time and material in practice and training disappears. All the world seems to agree that time or money spent on teaching or learning is well spent. In the course of twenty years' experience at a physical laboratory, and in examinations not a few, I have seen 𝕸 and 𝕳 or the wave-length of sodium light determined in ways that would earn very few ducats on the principle of payment by results; but, having regard to the psychological effect upon the culprit or the examiner, the question of ducats never came in. Wisely or unwisely public opinion has been educated to regard the psychological effect as of infinite value compared with the immediate result obtained. But in an office the marks that an observer or computer gets for showing that he "knew how to do it," when he did not succeed in doing it, do not count towards a "first class," and we have to abide by what we do; we cannot rely on what we might have done. Consequently our means in supposition, spread over sea and land, are matters of real solicitude. In such circumstances there might be reason for despondency if one were dependent merely upon one's own ventures and the results achieved thereby. But when one has the advantage of the gradual development of investigations of long standing, it is possible to maintain a show of cheerfulness. When Shylock demands his pound of flesh in the form of an annual report, it is not at all uncommon to find that some argosy that started on its voyage long ago "hath richly come to harbour suddenly." There have been quite a number of such happy arrivals within the last few years.

I will refer quite briefly to the interesting relations between the yield of barley and cool summers, or the yield of wheat and dry autumns, and the antecedent yield of eleven years before, which fell out of the body of statistics collected in the Weekly Weather Report since 1878. The accomplished statisticians of the Board of Agriculture have made this work the starting-point for a general investigation of the relation between the weather and the crops which cannot fail to have important practical bearings.

Let me take another example. For more than a full generation meteorological work has been hampered by the want of a definite understanding as to the real meaning in velocity, or force, of the various points of the scale of wind-estimates laid down in 1805 by Admiral Beaufort for use at sea, and still handed on as an oral tradition. The prolonged inquiry, which goes back really to the report upon the Beckley anemograph already referred to, issued quite unexpectedly in the simple result that the curve

$$p=0{\cdot}0105B^3$$

(where p is the force in pounds per square foot, and B the arbitrary Beaufort number) runs practically through nine out of the eleven points on a diagram representing the empirical results of a very elaborate investigation. The empirical determinations upon which it is based are certainly not of the highest order of accuracy; they rely upon two separate investigations besides the statistical comparison, viz., the constant of an anemometer and the relation of wind-velocity to wind-pressure, but no subsequent adjustment of these determinations is at all likely to be outside the limits of an error of an estimate of wind-force; and the equation can be used, quite reasonably, as a substitute for the original specification of the Beaufort scale, a specification that has vanished with the passing of ships of the type by which it was defined. This result, combined with the equation $p=0{\cdot}003V^2$, which has been in use in the Office for many years, and has recently been confirmed as sufficiently accurate for all practical purposes by Dr. Stanton at the National Physical Laboratory and Monsieur Eiffel at the Eiffel Tower, places us upon a new plane with regard to the whole subject of wind-measurement and wind-estimation.

Results equally remarkable appear in other lines of investigation. Let me take the relation of observed wind velocity to barometric gradient. You may be aware that in actual experience the observed direction of the wind is more or less along the isobars, with the low pressure on the left of the moving air in the northern hemisphere; and that crowded isobars mean strong winds. Investigations upon this matter go back to the earliest days of the Office.

There can be no doubt that the relation, vague as it sometimes appears to be upon a weather chart, is attributable to the effect of the earth's rotation. In order to bring the observed wind velocity into numerical relation with the pressure-gradient Guldberg and Mohn assumed a coefficient of surface "friction," interfering with the steady motion. The introduction of this new quantity, not otherwise determinable, left us in doubt as to how far the relation between wind and pressure distribution, deducible from the assumption of steady motion, could be regarded as a really effective hypothesis for meteorological purposes.

Recent investigations in the Office of the kinematics of the air in travelling storms, carried out with Mr. Lempfert's assistance, have shown that, so far as one can speak of the velocity of wind at all—that is to say, disregarding the transient variations of velocity of short period and dealing with the average hourly velocity, the velocity of the wind in all ordinary circumstances is effectively steady in regard to the accelerating forces to which it is subject. This view is supported by two conclusions which Mr. Gold has formulated in the course of considering the observations of wind velocity in the upper air, obtained in recent investigations with kites. The first conclusion is that the actual velocity of wind in the upper air agrees with the velocity calculated from the pressure distribution to a degree of accuracy which is remarkable, considering the uncertainties of both measurements; and the second conclusion affords a simple, and I believe practically new, explanation upon a dynamical basis of the marked difference between the observed winds in the central portions of cyclones and anti-cyclones respectively, by showing that, on the hypothesis of steady motion, the difference of sign of the effective acceleration, due to curvature of path and to the earth's rotation respectively, leads to quite a small velocity and small gradient as the limiting values of those quantities near anti-cyclonic centres.

This conclusion is so obviously borne out by the facts that we are now practically in a position to go forward with the considerable simplification which results from regarding the steady state of motion in which pressure gradient is balanced by the effective acceleration due to the rotation of the earth and the curvature of the path, as the normal or ordinary state of the atmosphere.

I cannot forbear to add one more instance of an argosy which has richly come to harbour so lately as this summer. You may be aware that Kelvin was of opinion that the method of harmonic analysis was likely to prove a very powerful engine for dealing with the complexities of meteorological phenomena, as it has, in fact, dealt with those of tides. In this view Sir Richard Strachey and the Meteorological Council concurred, and an harmonic analyser was installed in the Office in 1879, but subsequently numerical calculation was used instead. A considerable amount of labour has been spent over the computation of Fourier coefficients. Not many great generalisations have flowed from this method up to the present time. I have no doubt that there is much to be done in the way of classifying temperature conditions, for climatic purposes, by the analysis of the seasonal variations. A beginning was made in a paper which was brought to the notice of the Association at Glasgow. The most striking result of the Fourier analysis we owe to Hann, who has shown that, if we confine our attention to the second Fourier coefficient of the diurnal variation of pressure—that is, to the component of twelve-hour period—we get a variation very marked in inter-tropical regions, and gradually diminishing poleward in both hemispheres, but synchronous in phase throughout the 360 degrees of a meridian. The maximum occurs along all meridians in turn about 10 a.m. and 10 p.m. local time. This semi-diurnal variation with its regular recurrence is well known to mariners, and we have recently detected it, true to its proper phase, in the observations at the winter quarters of the *Discovery*; small in amplitude indeed—about a thousandth of an inch of mercury—but certainly identifiable.

The reality of this variation of pressure, common to the whole earth, cannot be doubted, and, so far as it goes, we may represent it (if indeed we may represent pressure differences as differences in vertical heights of atmosphere) as the deformation of a spherical atmosphere into an ellipsoid, with its longest axis in the Equator pointing permanently 30° to the west of the sun. Its shortest axis would also be in the Equator, and its middle axis would be along the polar axis of the earth. Somehow or other this protuberance remains fixed in direction with regard to the sun, while the solid earth revolves beneath it. Whatever may be the cause of this effect, obviously cosmical, and attributable to the sun, at which it indirectly points, its existence has long been recognised, and further investigation only confirms the generalisation. It is now accepted as one of the fundamental general facts of meteorology.

Prof. Schuster, for whose absence from this meeting I may venture to express a regret which will be unanimous, has already contributed a paper to the Royal Society pointing out the possible relations between the diurnal variations of pressure and those of terrestrial magnetic force. Going back again to the ubiquity of the application of the relation of pressure and wind, in accordance with the dynamical explanation of Buys Ballot's law, we should expect the effect of a pressure variation that has its counterpart in that of terrestrial magnetism to be traceable also in wind observations.

Mr. J. S. Dines has just given me particulars of the discovery of that effect in the great air-current, the variations of which I have called the pulse of the atmospheric circulation—I mean the south-east Trade Wind, the most persistent atmospheric current in the world. It is difficult as a rule to get observers to pay much attention to that current, because it is so steady; but in 1891 the Meteorological Council set up an anemometer at St. Helena, in the very heart of the current, and we have just got out the results of the hourly tabulations. When the observations for the hours 1 to 24 are grouped separately for months, so as to give the vector resultants for each hour and for each month, it appears that there is a conspicuous semi-diurnal variation in the current, which shows itself as a closed polygon of vector variations from the mean of the day.

The month of April gives the most striking diagram of the twelve. It displays the superposition of two practically complete dodekagons, one a large one, completing its cycle from 6 a.m. to 6 p.m., the other a small one, for 6 p.m. to 6 a.m. The resultant wind for the whole day is very nearly south-east, and practically remains so for all the months of the year, the monthly variation of resultant wind being confined to a change of velocity from about fourteen miles per hour in May to about twenty-one miles per hour in September.

If, instead of combining the south and east components to form a vector diagram, we plot their variations separately, the semi-diurnal variation in each is plainly marked; and the calculation of its constants shows that its amplitude is about three-quarters of a mile per hour both in the south and rather less in the east component. The easterly increment has its maxima at 10 a.m. and 10 p.m., and at these hours the phase of the variation of the southerly component is nearly opposite. Thus, to correspond with the semi-diurnal variation of pressure, there is a semi-diurnal variation in the Trade Wind at St. Helena, which is equivalent to the superposition upon the resultant wind of a north-easterly component of about one mile per hour amplitude, with maxima at 10 a.m. and 10 p.m., the hours when the ellipsoidal deformation of the spherical atmosphere is passing over the locality.

I have only dealt with one month. I believe that when all the results that flow from this simple statement can be put before you, you will agree with me that the argosy which the Meteorological Council sent out in 1891 has indeed richly come to harbour.

Let me digress to say a word in illustration of the principle I laid down that, if one would avoid waste in meteorological work, the observations must be examined forthwith and so far discussed that any ambiguities may be cleared up.

After some years of wear at St. Helena the persistent rubbing of the south-east part of the spiral metallic pencil upon the metallic paper wore away the metal and left a flat place. This got so bad that the instrument had to come home for repairs, and when it was set up again, after a year's absence, the average direction of the Trade Wind differed by two points from the averages of most, but not of all, of the previous years. So far as we know, the orientation has been attended to, as before, and yet it is hardly possible to resist the suggestion that the anemometer has been set slightly differently. We are now making very careful inquiries from the observer; but, in the meantime, it seems to me that there is a great opportunity for a competent mathematical physicist to help us. Dynamical explanations of the Trade Winds have been given from the time of Halley. Let me offer as a simple question in the mathematical physics of the atmosphere whether a variation of two points in the direction of the south-east Trade Wind between the years 1903 and 1905 can be regarded as real, and, if not, which of the two recorded directions is the correct one?

It would be appropriate for me to add some words about the results of last year's work upon the upper air, in which we have had the valuable cooperation of the University of Manchester. These results have disclosed a number of points of unusual interest. But we are to have an opportunity of considering that subject in a discussion before the Section, and I need not deal with it here. I must, however, pause to give expression of the thanks of all meteorologists to Prof. Schuster for his support of the Manchester University station at Glossop Moor. I may remind you that this generous contribution for the advancement of science on the part of Prof. Schuster is in addition to the foundation of a readership in mathematical physics at Manchester and a readership in dynamical meteorology, now held by Mr. Gold at Cambridge.

I have said enough to show that the speculative ventures of official meteorologists are not all failures, and I will only add that if any mathematician or physicist would like to take his luck on a meteorological argosy he will be heartily welcomed. Part of the work will be drudgery; he must be prepared to face that; but the prospects of reaching port are reasonably good, so much so, indeed, that such a voyage might fairly lead to a claim for one of the higher academical degrees.

Up to now I have been dealing with the adjustment of official scientific work to reduce waste to a minimum, in so far as it lies within the control of those responsible for an office. I turn now to an aspect of the matter in which we require the assistance of others, particularly of the British Association.

The most serious danger of waste in a busy office is that it should carry on its work without an adequate knowledge of what is being done in advancing science and improving methods elsewhere. I speak myself for the Meteorological

Office alone, but I believe that the responsible officials of any scientific Government department will agree with what I say.

Year by year some Timotheus "with his sounding flute and tuneful lyre" performs some miracle by the application of reasoning to the phenomena of Nature. Only last year you heard Prof. Love in his presidential address treat of the mundane question of the shape of the earth and etherealise the grim actualities with the magic of his spherical harmonics. Year by year, in every one of the subjects in which the practical world is immediately interested, active students, whether public officials, academic officials, or private enthusiasts, not only keep alight the sacred flame but occasionally add to its brilliance; and all the new knowledge, from whencesoever it comes, ought to be applied to the service of the State.

The actual volume of original contributions on these subjects is by no means inconsiderable. You are all aware that, some years ago, the Royal Society initiated a great international enterprise for the compilation of a catalogue of scientific literature. I have been looking at the fifth annual issue of the volume on meteorology, including terrestrial magnetism. I may remark that the catalogue is quite incomprehensibly eclectic as regards official literature, but let that pass. I find that, in the year that closed with July, 1907, 1042 authors (not counting offices and institutions as such) presented to the world 2131 papers on meteorology, 229 on atmospheric electricity, and 180 on terrestrial magnetism. This will give some idea of the annual growth in these subjects, and may convince you that, after all allowance is made for duplicate titles, for papers of no importance, and for mere sheets of figures published for purposes of reference, there remains a bulk of literature too large for any single individual to cope with if he has anything else to do.

If instead of confining ourselves to what can be included in meteorology alone we extend our view over the other allied sciences, it would be necessary to take in other volumes of the international catalogue, and there would be some overlapping. I have taken instead the volume of the "Fortschritte der Physik" for 1906, which deals with "Kosmische Physik." It is edited by Prof. Assmann, who adds to his distinction as head of the Royal Prussian Aëronautical Observatory of Lindenberg that of an accomplished bibliographer. In this volume are given abstracts or titles of the papers published during the year which can be regarded as worthy of the attention of a physicist. An examination of the volume gives the following numbers of the papers in the different sections:—

		Papers
Astro-Physics		222
Meteorology		1122
Atmospheric Electricity		135
Geophysics:		
Geodetics	105	
Seismology and Volcanic Phenomena	256	
Terrestrial Magnetism and Aurora	108	
Currents, Tides, and Waves	46	
Inland Hydrography	117	
Ice, Glaciers, and Ice Age	139	
Other papers	126	
	—	897
Total		2376

I need hardly say that these 2376 papers are not all English; in some of the sections few of them are in that language, and fewer still are British. If British students, official and unofficial, are to make the most of the operation of drawing the angels down, they need help and cooperation in dealing with this mass of literature, in winnowing the important from the unimportant, and in assimilating that which makes for the real progress of the practical application of science. This is the more necessary for these subjects because there is no organised system of academic teaching, with its attendant system of text-books. In a subject which has many university teachers it might reasonably be supposed that any important contribution would find its way into the text-books, which are constantly revised for the use of students; and yet in his presidential address to the Royal Society in November of last year, Lord Rayleigh felt constrained to point out that, for the advance of science, although the main requirement is original work of a high standard, that alone is not sufficient. "The advances made must be secured, and this can hardly be unless they are appreciated by the scientific public." He adds that "the history of science shows that important original work is liable to be overlooked and is, perhaps, the more liable the higher the degree of originality. The names of T. Young, Mayer, Carnot, Waterston, and B. Stewart will suggest themselves to the physicist, and in other branches, doubtless, similar lists might be made of workers whose labours remained neglected for a shorter or longer time."

If this is true of physics how deplorably true it is of meteorology. If I allow a liberal discount of more than 50 per cent. from the numbers that I have given, and estimate the number of effective contributions to meteorology as recognised by the "International Catalogue" at a thousand, which agrees pretty well with that given by the "Fortschritte der Physik," and if I were to ask round this room the number of these papers read by anyone here present, I am afraid the result would be disheartening. Many of us have views as to the way in which the study of meteorology ought to be pursued, but the views are not always based on an exhaustive examination of the writings of meteorologists. Few of us could give, I think, any reasonable idea of the way in which it is being pursued by the various institutions devoted to its application, and of the progress which is being secured therein. Meteorological papers are written by the hundred, and whether they are important or unimportant, they often disregard what has been already written in the same or some other language, and are themselves in turn disregarded. I do not think I should be doing any injustice if I applied similar remarks to some of the other subjects included in the table which I have quoted. How many readers are there in this country for an author in terrestrial magnetism, atmospheric electricity, limnology, or physical oceanography? But, if the papers are not read and assimilated, the advancement of science is not achieved, however original the researches may be.

By way of remedy for the neglect of important papers in physics, Lord Rayleigh suggests that teachers of authority, who, from advancing years or from some other reason, find themselves unable to do much more work in the direction of making original contributions, should make a point of helping to spread the knowledge of the work done by others. But what of those subjects in which there are no recognised teachers? and in this country this is practically the case with the subjects which I have mentioned. It is true that many of them are made the occasion of international assemblies, at which delegates or representatives meet. But such international assemblies are of necessity devoted, for the most part, to the elaboration of the details of international organisation, and not to the discussion of scientific achievements. The numbers attending are, equally of necessity, very restricted.

The want of opportunity for the discussion of progress in these sciences is specially lamentable, because in its absence they lose the valuable assistance of amateur workers, who might be an effective substitute for the students of an academic study. In no subject are there more volunteers, who take an active part in observing, than in meteorology; but how few of them carry their work beyond the stage of recording observations and taking means. The reason is not lightly to be assigned to their want of capacity to carry on an investigation, but far more, I believe, to the want of knowledge of the objects of investigation and of the means of pursuing them.

Among the agencies which in the past have fostered the knowledge of these subjects, and stimulated its pursuit, there stand out prominently the annual meetings of this association. It was the British Association which in 1842 re-founded the Kew Observatory for the study of the physics of the atmosphere, the earth, and the sun. It was the British Association which promoted the establishment of magnetic observatories in many parts of the earth, and in the early 'sixties secured the most brilliant achievements in the investigation of the atmosphere by means of balloons. I know of no other opportunity of anything like the same potentialities for the writers of papers to meet with the readers, and to confer together about the progress of the sciences in which they are interested. But its potentialities are not realised. Those of us who are most anxious for the spread of the application of mathematics and physics to the phenomena of astronomy, meteorology, and geophysics have thought that this opportunity could not properly be utilised by crowding together all the papers that deal with such subjects into one day, or possibly two days, so that they can be polished off with the rapidity of an oriental execution. In fact, the opportunity to be polished off is precisely not the opportunity that is wanted. There are some of us who think that a British Association week is not too long for the consideration of the subjects of which a year's abstracts occupy a volume of six hundred pages, and that, if we could extend the opportunity for the consideration of these questions from one or two days to a week, and let those members who are interested form a separate committee to develop and extend these subjects, the British Association, the country, and science would all gain thereby. I venture from this place, in the name of the advancement of science, to make an appeal for the favourable consideration of this suggestion. It is not based upon the depreciation, but upon the highest appreciation of the service which mathematics and physics have rendered, and can still render, to the observational sciences, and upon the well-tried principle that close family ties are strengthened, and not weakened, by making allowance for natural development.

The plea seems to me so natural, and the alternatives so detrimental to the advancement of science in this country, that I cannot believe the Association will turn to it a deaf ear.

NOTES.

We deeply regret to have to announce the death, at the age of seventy-one, of M. E. E. Mascart.

We much regret to have to record the death of the Earl of Rosse, F.R.S., which took place on Saturday last.

The death is announced of Mr. F. Kynaston Barnes, formerly assistant constructor of the Navy and surveyor of dockyards. He was the author of many papers in the Transactions of the Institution of Naval Architects, joint author, with Prof. Rankin, of "Shipbuilding," and joint editor for a number of years, with Lord Brassey, of "The Naval Annual." Mr. Barnes, who at the time of his death was in his eighty-first year, was the inventor of the present method of calculating the stability of ships, which is known as "Barnes's method," and was the designer of the *Nile* and the *Trafalgar*.

The death is announced of M. J. F. Nery Delgado, president of the Geological Survey of Portugal. M. Nery Delgado was also inspector-general of mines and a member of the Lisbon Royal Academy of Sciences.

The death is announced of Mr. James D. Hague, the eminent American mining geologist, at the age of seventy-two. He became manager of the Lake Superior copper mines in 1863, and participated in the early development of the Calumet and Hecla mine. His most important work was his report on the mining industry of the fortieth parallel, published in 1870.

A citizens' committee has been formed to arrange for the entertainment of the British Association in Canada next year, and various Western Governments and cities will be requested to cooperate. The programme, so far, provides for a trip through the west, and one through the mountains to the Pacific coast. Transportation facilities are being arranged, and a number of distinguished guests from Canada and the United States will be invited. Provision will also be made for a limited number of ladies.

According to the *Times*, the Liverpool School of Tropical Medicine is making arrangements to send an expedition to Jamaica to investigate tropical diseases there and the insect life of the island, which is responsible for carrying disease. It is intended to send Mr. Robert Newstead, the lecturer in economic entomology and parasitology of the Liverpool School of Tropical Medicine, in the first week of November to undertake the investigation of the ticks there responsible for certain diseases in animals, and of disease-bearing insects. It is possible that he may be accompanied by a medical research investigator, whose duties would be to investigate indigenous diseases of the island.

A Reuter message from Simla, dated August 28, states that the servant of Dr. Sven Hedin has reached Leh, Kashmir, reporting that the explorer was four marches from Gartok twenty-five days before, and was in good health. A message from the *Times* correspondent at Simla, dated August 31, reports that Dr. Sven Hedin is expected at Simla next week. A letter dated Gartok, August 1, is the first direct news heard from the explorer for several months.

It is stated in *Science* that the department of meridian astrometry of the Carnegie Institution, in charge of Prof. Lewis Boss, of the Dudley Observatory at Albany, N.Y., where the work of the department is carried on, is dispatching an expedition to the Argentine Republic to establish a branch observatory there. This observatory will be established at San Luis, about 500 miles west from Buenos Aires. This town, of about 10,000 inhabitants, is located near the eastern edge of the Andean plateau at an elevation of about 2500 feet. It is reported to have a fine climate with remarkably clear skies. The principal instrument will be the Olcott meridian circle of the Dudley Observatory, which will be set up in its new location for the purpose of making reciprocal observations upon stars already observed at Albany, together with observations upon all stars from south declination to the south pole that are brighter than the seventh magnitude, or which are included in Lacaille's survey of the southern stars made at the Cape of Good Hope in 1750. It is estimated that the work of observation in Argentina will last three or four years. The object of these observations is to gather material for facilitating the construction of a general catalogue of about 25,000 stars, in which will be contained accurately computed positions and motions of all the stars included in it.

According to a Reuter message from Berlin, a wireless telegram has been received from the steamer *Kaiserin Auguste Victoria* stating that Dr. Polis, the director of the meteorological observatory at Aachen, is continuing his experiments in transmitting meteorological observations at sea between New York and England by means of wireless telegraphy. Dr. Polis is reported to have succeeded in receiving weather reports from America at a distance of 800 nautical miles from the American coast, while reports from Europe were picked up at a distance of 1200 nautical miles from the English coast. Daily weather charts were drawn up by using reports from passing ships, which indicated the state of the weather on the Atlantic Ocean over an extent of 800 nautical miles. A message sent on August 27 to the *Kaiserin Auguste Victoria* from Aachen, *via* Ireland, took three hours to reach the ship.

The summary of the weather for the closing week of August issued by the Meteorological Office shows that the rainfall was everywhere largely in excess of the average, the total for the week exceeding 2 inches in several districts, and amounting to 2·30 inches in the south-west of England. Over the western portion of the kingdom, as well as in the south of England, rain fell each day. The mean temperature was nowhere very different from the average, and notwithstanding the heavy rain there was a slight excess of bright sunshine. The aggregate rainfall for the summer, as comprised by the thirteen weeks ending August 29, was everywhere in defect of the average except in the south and north-west of England. In the east of Scotland, the north-east of England, the south of Ireland, and in the Channel Islands the deficiency of rain was more than 2 inches. The rainy days were also deficient, except in the north of Scotland, where there was a deficiency of bright sunshine, all other districts showing a larger amount of sunshine than usual. In London, June was the only summer month with a deficiency of rain, and the total excess for the three months is 0·6 inch, the aggregate measurement being 7·14 inches; the wettest month was July, with a rainfall measuring 3·42 inches.

MAJOR W. A. J. O'MEARA, R.E., C.M.G., has been appointed by the President of the Board of Trade an additional British delegate to the International Conference on Electrical Units and Standards, which is to meet in London on October 12 next.

PROF. C. O. WHITMAN, who has for the past twenty years been director of the Marine Biological Laboratory, Wood's Hole, Mass., has resigned that position, and the assistant director, Prof. F. R. Lillie, of the University of Chicago, has been elected in his stead.

AT the celebration of the jubilee of the British Ornithologists' Union, which is to take place in London in December next, gold medals will be presented to each of the four original members—Dr. F. Du Cane Godman, F.R.S., Mr. P. S. Godman, Mr. W. H. Hudleston, F.R.S., and Dr. P. L. Sclater, F.R.S.

HIS MAJESTY THE KING has accorded his patronage to the Royal Society of Medicine, and has intimated his intention to sign the roll of the society in the autumn. During the past three months the Society of Anæsthetists and the Society for the Study of Disease in Children have joined the Royal Society of Medicine as sections for the study of the subjects in which they are especially interested.

THE fifty-third annual exhibition of the Royal Photographic Society of Great Britain will be held in London from September 17 to October 24.

ACCORDING to *Science*, the assistants of Prof. Novarro, of Genoa, have decided to endow a Novarro prize to be awarded for work in general pathology.

NOTICE is given by the council of the Royal Society of Arts that the next award of the Swiney prize (consisting of a cup of the value of 100*l.* and money to the same amount) will be awarded in January next. The award will be for a work on medical jurisprudence. Any person desiring to submit a work in competition, or to recommend any work for the consideration of the judges, should do so by letter, addressed to the secretary of the Royal Society of Arts.

THE New York Academy of Medicine offers a prize of 200*l.* for the best essay on "The Etiology, Pathology, and Treatment of the Diseases of the Kidney." The papers submitted must reach the academy on or before October 1, 1909.

THE Academy of Sciences of Stockholm has undertaken the publication of the scientific works of Swedenborg, and vol. i. of the series, dealing with geology, and containing a number of Swedenborg's letters, has recently been issued. Vol. ii. will contain treatises on chemistry, physics, and mechanics, and vol. iii. treatises on cosmology. Four further volumes are planned, and will deal respectively with the brain and general physiology.

THE provisional programme of the Incorporated Institution of Automobile Engineers for the session 1908–9 has been issued, and comprises the following items:—On October 14 the presidential address by Mr. Dugald Clerk, on some problems of the motor-car; on November 11 a paper by Mr. B. Hopkinson, on a complete test of a modern petrol engine—power, thermal and mechanical efficiency, exhaust products at various powers and speeds; and on December 9 a paper entitled "How the Weight of the Motor-car is made up" will be read by Mr. Mervyn O'Gorman. The following papers and discussions have been arranged for the general meetings of the institution in 1909:—Mr. F. H. Royce, causes of wear in motor machinery; Mr. G. H. Baillie, carburettor experiments; Mr. Horatio Ballantyne, the chemistry of petrol; Mr. Bertram Blount, on specifying the quality of petrol; Mr. F. R. S. Bircham, the use of small internal combustion engines for marine work; Mr. L. A. Legros, transmission; Mr. E. H. Cozens-Hardy, motor cabs; Dr. W. Watson, F.R.S., petrol engine experiments; and a discussion on valve setting, introduced by Mr. Max R. Lawrence.

A RECENTLY issued consular report from Tahiti states that among the innovations in agriculture to which the soil of some of the uninhabited valley lands of Tahiti and of other neighbouring islands would be propitious is the planting of rubber, which, it is believed, would give excellent results. The variety which appears to be specially recommended for the valleys of Tahiti is the *Castilloa elastica*, which has been experimented upon on a small scale with such encouraging results that a local company has been floated for the purpose of planting rubber on an extensive scale.

A DESCRIPTIVE account of the new aëroplane of Mr. Henry Farman appears in *La Nature*, and is abstracted by the Paris correspondent of the *Times*. The apparatus differs entirely in construction from Mr. Farman's two previous machines. Instead of having double planes, connected by ties and stays, which are regarded as offering undue resistance to the air, the new machine has, on either side, three wing-like single planes, giving it the appearance which has suggested its name—the *Flying Fish*. The body, made of ash, has, indeed, the exact shape of a long and slender fish, tapering backwards with a gentle dropping curve. It is 46 feet long, square in section, and comes somewhat sharply to a point in front, where a plate of aluminium supports the shaft of the propeller. Its four members are connected by wooden ties and steel stays, producing a girder of perfect rigidity. The machine is mounted on two wheels placed well forward under the motor. The steel framework which carries these wheels, as well as that of a third wheel placed near the tail, is provided with strong spiral springs intended to reduce the shock of alighting on the ground. The six rectangular "wings" are fixed towards the head of the machine, and are each 8 feet 8¼ inches long and 1 foot 3½ inches wide. They consist of wooden frames rising towards the extremity, slightly curved and tapering on the same lines as the body of the "fish" itself. They are covered by a double layer of thin fabric. The second plane on each side is placed somewhat lower than the first, and the third somewhat lower than the second. At the tail are two similar but rather shorter planes, the hindermost of which is movable on its axis, and acts as a horizontal rudder for regulating the height of flight. At the end of all comes the vertical cellular rudder of direction. It is mounted on a pivot fixed in the solid wooden shoe, which terminates the body of the machine, and it is prolonged forward over the back of the "fish" for nearly half the total length by a triangular extension of the same material as the planes. The whole body is covered with material, but in front of the pilot's position sheets of mica take the place of the stuff, so that his view may not be obstructed. The total bearing surface is 24 square metres. A novelty has been introduced into the steering apparatus. The wheel, which resembles that of a motor-car, is mounted vertically and acts normally upon the vertical rudder of direction, but when moved horizontally it acts by means of a lever on the horizontal rudder. The *Flying Fish* is fitted with a new 35-horsepower motor, especially constructed by the Renault Company. This motor has eight cylinders, arranged in a V, is air-cooled by two fans, and weighs 130 kilograms. There is an aluminium carburettor and a diminutive magneto. The motor is connected directly with the two-bladed propeller. The whole aëroplane, including the pilot, weighs 650 kilograms.

To the August number of the *Contemporary Review* Dr. Alfred Russel Wallace has contributed a fighting article on the present position of Darwinism, in which it is urged that neo-Lamarckism, the mutation theory of de Vries, and Mendelism in no wise affect the truth and stability of the natural-selection doctrine. Neo-Lamarckism is dismissed with the statement that since, according to Mr. W. L. Tower, there is no evidence "to show the inheritance of acquired somatic characters or their incorporation in the germ-plasm," the fundamental assumption of the theory is false. As regards the mutation-doctrine, it is pointed out that whereas sudden structural "jumps" are common among cultivated plants and domesticated animals, in wild nature they are exceedingly rare, and would inevitably be speedily swamped in the course of evolution. This implies the existence in cultivation and domestication of some "provocative" factor which is lacking, or latent, in nature, and this, again, strikes at the root of the Mendelian doctrine as explanatory of the origin of species. "The claims of the Mutationists and Mendelians," writes Dr. Wallace in unequivocal language, "as made by many of their ill-informed supporters, are ludicrous in their exaggeration and total misapprehension of the problem they profess to have solved." On the other hand, it is admitted by the critic that Mendelism may, and probably will, have a certain value in explaining the transmission of disease and other matters connected with heredity.

"THE Rate of Growth of the Reef-building Corals" forms the title of a small pamphlet, by Mr. F. Wood Jones, published by Messrs. John Bale, Sons and Danielsson, Ltd., of Oxford House, Great Titchfield Street. These notes, which were made during a fifteen months' residence on the Keeling-Cocos Atoll, claim to have put the evidence as to the rate of coral-growth in a more definite form than has hitherto been the case. It is pointed out that, in order to be of value, observations must extend over a long period, as corals are subject to great seasonal and individual variation in their rate of increase, while there is likewise great difference in this respect between the branching and the massive groups. On the average, it appears that branching corals grow about 3·7 inches in a twelvemonth, while the massive species increase their diameter by about 1/37 of their original circumference in 100 days. In other words, a coral 37 inches in diameter will measure 38 inches across in a little more than three months. An estimate of the rate of growth of the branching species made by Dr. Guppy is practically identical with the author's results.

THE nature and causes of dwarf faunas are discussed at some length by Prof. H. W. Shimer in the July issue of the *American Naturalist*. Instances are given of the occurrence of such dwarfed invertebrate aquatic faunas in several parts of the world, while extinct faunas of the same type are likewise noticed. The chief agency in their production seems to be variation of environment, such as a large infusion of fresh water into a more or less isolated sea. Two types of dwarf faunas occur, one in which the individuals of different species are smaller than the normal, and the other in which individuals are normal, but all the species are small owing to the weeding-out of the larger ones. Dwarfing may show itself by the premature development of senile features or by the retention of juvenile characteristics (owing to slow development) throughout life.

CAPTAIN STANLEY FLOWER, in a very interesting article published in the *Zoologist* for August, discusses the ordinary prices paid to dealers for various species of wild animals (inclusive of mammals, birds, and reptiles). The prices quoted are restricted to transactions which have taken place during the last dozen years, and are solely based on the author's personal experiences. The highest-priced animal mentioned in the list is the giraffe, which ten years ago could not be purchased for 1000*l.*, although its value has now fallen to 400*l.* or 500*l.* We believe, however, that equally high prices have been paid for rhinoceroses. On the other hand, for its size, the brown bear is one of the cheapest of all wild animals, a specimen having changed hands for 4*l.* We should like to know the estimated value of a living sea-otter.

A NOTE on the utilisation of the "khair" forests in eastern Bengal and Assam has been published as Forest Pamphlet No. 1 issued by the Government of India. The author, Mr. P. Singh, adduces evidence for disregarding the belief that the wood of the "khair" tree, *Acacia catechu*, is devoid of catechin when it grows in moist localities. He also indicates the methods for preparing the dye-material cutch and for extracting the catechin in the preparation of "katha" or "kath," a product that finds favour among the native population as a chewing substance.

AT the meeting of the American Philosophical Society held at Philadelphia in April, Mr. J. W. Harshberger read a paper on the leaf structure of the sand-dune plants of Bermuda. On the upper beach *Cakile aequalis* is a characteristic plant, and *Ipomoea pes-caprae* is luxuriant. Associated with the latter on the dunes are *Scaevola Plumieri*, *Tournefortia gnaphalodes*, and *Juniperus bermudiana*. *Conocarpus erectus* and *Stenotaphrum americanum* also grow on the dune slopes. Various devices for preventing undue loss of water are described. *Sisyrinchium bermudianum* bears the stomata in deep cavities, in the leaves of *Lantana involucrata* they lie in depressions fringed with hairs; Conocarpus secretes gum in the cells, and *Borrichia arborescens* depends upon a dense covering of hairs. The paper is printed in the first quarterly number of this year's Proceedings.

THE fortieth volume of Engler's "Botanische Jahrbücher," beginning with a part published in May, 1907, was concluded with the fifth part, published in May last.

Two fascicles of the contributions to the flora of Africa are included in the volume, in which the most general article is a revision of the African genera and species of the order Flacourtiaceæ, that has been prepared by Dr. E. Gilg. A phytogeographical study based on an exploration of the mid-Amazon is presented by Mr. E. Ule. The expedition, primarily undertaken to obtain information with regard to rubber trees and their distribution, has yielded much botanical treasure, and the author gives an elaborate description of the floras of the various tributaries. In the pages of the "Beiblätter" will be found the proceedings of the Society of Systematic Botanists at their meetings in Hamburg (1906) and Dresden (1907). An important paper was read at the Dresden meeting by Prof. O. Drude on mapping methods in connection with botanical surveys. Colours are used for certain broad, distinctive formations, such as moors and swamps or coniferous forest; on these are superposed special signs and letter combinations indicative of plant associations.

UNDER the title of "Classification palethnologique," the eminent French anthropologist, M. A. de Mortillet, publishes a pamphlet intended to provide a scheme for the seriation of early art from prehistoric times down to the age of Charlemagne. His plan of grouping is founded on typical specimens, the terminology being based on the names of those Continental sites at which the most characteristic examples have been discovered. The prehistoric period, or age of Stone, falls into three subgroups:—Éolithique, including Thenaysien and Puycournien; Paléolithique, with its subdivisions, Chelléen, Acheuléen, Moustérien, Solutréen, and Magdalénien; Néolithique, confined to Robenhausien. Similarly, the protohistoric time divides itself into an age of Bronze and of Iron, the former represented by the Tziganien period subdivided into the Morgien and the Larnaudien. The age of Iron falls into three periods, Gaulois, Romain, and Mérovingien, the first divided into Hallstattien and Marnien, the second into Lugdunien and Champdolien, the third including Wabénien. Each period is illustrated by excellent drawings of typical specimens, with descriptions and details of provenance. The scheme will be of much use in classifying the objects of human art in Continental museums, to which the survey is largely confined.

Two important communications on the subject of stone implements appear in *Man* for July and August. In the earlier number the Rev. H. G. O. Kendall describes a collection of Neolithic microliths from Welwyn, in Hertfordshire, and other sites in the Quaternary gravels of Essex. Many of the specimens are carefully chipped, and were probably used as boring tools. Those at Welwyn were found at a depth of 12 feet in some thin layers of gravelly sand. They seem to be analogous to the so-called "pygmy" flints discovered by Mr. R. A. Gatty at Scunthorpe, in Lincolnshire, which were described in *Man* (February, 1902), and closely resemble specimens found by Mr. A. C. Carlleyle in the Indian Vindhya range. In the August number of the same periodical Mr. C. G. Seligmann describes a collection of quartz implements from Ceylon, found in various parts of the island in sites varying from a height of a few hundred feet above sea-level to about 4000 feet. The range of their distribution indicates that at one time there must have been a considerable population using tools of this kind. They are found in places at present occupied by the Veddahs, and Mr. Seligmann accepts the view of the brothers Sarrasin that they may be attributed to this race. The caves in which they were discovered seem to have been seized by the Sinhalese some two thousand years ago, when they expelled the Veddah occupants. In later days the Veddahs re-occupied these sites. These recent discoveries seem to indicate a closer connection between the two races than is usually realised.

THE report on the work of the Survey Department, Egypt, in 1907, shows that good progress is being made in the various branches of its useful and far-reaching operations. Among these are included, *inter alia,* (1) the topographical survey comprising the 1 : 10,000, 1 : 50,000, and 1 : 250,000 series of maps; (2) the cadastral survey, which prepares maps on large scales, showing property boundaries and the land registers which accompany them. These sheets are utilised for the production of maps on smaller scales, but since the country is changing very rapidly in parts, owing to perennial irrigation, barrage, and reclamation, the cadastral sheets have usually to be revised in the field. (3) The geological survey; Captain Lyons states that during the past ten seasons' work the general outline of the geological structure of the country has been laid down in considerable detail, and that a geological map of the country on the scale of 1 : 1,000,000 is now in hand. In addition, the department tests the gas and water supplies of Cairo, and analyses materials supplied to various departments to see if they are in conformity with specification; it also superintends the meteorological stations in Egypt and the Sudan, and the preparation of observations for publication. We notice several important additions to the meteorological work, *e.g.* the publication of daily synoptic charts for the Mediterranean and adjacent parts (to which we have before referred), the discussion and immediate utilisation of observations made in Cyprus (by arrangement with the Meteorological Committee), and the exploration of the upper air. Although the latter service was only begun in July, 1907, some very valuable results have already been obtained.

WE have received for notice eight further volumes of the water supply and irrigation papers issued by the Department of the United States Geological Survey. These relate to the geology and water resources of districts along the Mississippi and Hudson Bay, in California, north-west of the Pacific, Nebraska, and Beaver Valley, Utah. The information contained is comprehensive and useful locally, but there is nothing of a special character that calls for further notice here. The methods of gauging the streams and the instruments employed, which are described and illustrated, have been already dealt with in previous articles.

IN the July number of the *National Geographic Magazine* Mr. A. H. Sylvester, of the United States Geological Survey, gives an interesting account, illustrated by admirable photographs, of "our noblest volcano," Mount Hood, which rises to a height of 11,225 feet in the State of Oregon. It is an almost perfect volcanic cone, the fourth in height of the snow peaks of the Pacific North-West, being surpassed only by Rainier, Shasta, and Adams. It was built up of andesitic lavas which were ejected from a single summit crater. Recently the volcano has displayed signs of renewed activity. Prof. Russell, in his book on "American Volcanoes," gave a picture taken in 1882 of a so-called fumarole on the south slope, which has since that time apparently become inactive; but steam has recently been observed to issue from fissures on Crater Rock, and something resembling a glow was noticed at the same point in 1907. It is interesting to note that this activity was synchronous with changes observed in the Bogaslof group of volcanic islands off the Alaskan coast.

THE *Rivista Geografica Italiana*, No. 6 of 1908, contains a short note on the remarkable eruption of Etna on April 29 last. This was preceded by violent earthquakes, and accompanied by the opening of a fracture of more than a kilometre in length and from 20 to 50 metres in breadth. Several parasitic cones of small size were formed along it, and about 500,000 cubic metres of lava poured out, but the fissure was only partially obscured by erupted material, and remained conspicuous after the eruption had ceased. Although this eruption was violent while it lasted, and although the interval separating it from the next preceding eruption was more than fifteen years, or about two and a half times the average during the last 150 years, the eruption was of very short duration, commencing at 5.20 a.m. on April 29 and ceasing at 5.40 p.m. on April 30, but practically lasting for only about seventeen hours.

THE report of the Meteorological Committee for the year ending March 31 last contains much useful reading for those interested in the development of meteorological science, and shows that great efforts are being made both from practical and theoretical points of view. Many useful publications have been issued during the year, to some of which we have already referred; among those still in the press we may specially mention:—(1) meteorological results for the western portion of the Atlantic anticyclone, by Dr. R. H. Scott; (2) seasons in the British Isles since 1878; and (3) summary of hourly values at four observatories, 1879–1908. The most important point to be noted in connection with the periodical publications is the revision of the form of the monthly weather report, which gives summaries from all stations in connection with the office, either directly or through the meteorological societies and other bodies, and includes a rainfall map contributed by Dr. H. R. Mill. This change is based on the principle that the value of the observations is much enhanced by prompt publication, and now extends to all branches of the work; *e.g.* the marine department, under the able superintendence of Commander Hepworth, issues elaborate monthly pilot charts for the Atlantic and Indian Oceans, which include the latest intelligence of use to seamen received by cable from the Canadian and Indian Meteorological Services. In view of the importance of a homogeneous system of weather telegraphy in western Europe, the committee has changed the hour of reports from 8h. to 7h. a.m.; the additional expense of the earlier opening of the telegraph offices gives rise, however, to a serious question of ways and means. The use of wireless telegrams and the investigation of the upper air are among the many other important matters engaging the earnest attention of the committee.

THE June number of *Terrestrial Magnetism and Atmospheric Electricity* contains a short article on the work of the magnetic survey yacht *Galilee* from the pen of the director, Dr. L. A. Bauer. During the three years' voyages of the *Galilee* a complete magnetic survey of the Pacific Ocean was made with scarcely a hitch in the programme originally sketched out for it. The experience gained on board has led to the conclusion that for future work a vessel must be specially constructed, and the Carnegie Institution has undertaken to defray the cost of a new wooden sailing vessel, the *Carnegie*, 155 feet long, with auxiliary power (125 horse-power) provided by a gas engine, built, so far as possible, of non-magnetic materials, so that the outstanding magnetic effect of the ship will be less than the errors of observation. It is hoped that the ship will be ready next year, when a survey of the Atlantic will be commenced.

IN a further article in the same magazine Dr. Bauer points out that the recent attempts to represent the magnetic state of the earth by means of spherical harmonics have not led to results of which any practical use can be made, owing to the wide divergence between the calculated and the observed values for any point. This he puts down to the distribution of areas of irregularity of varied amounts and extents over the earth, and the difficulty of representing their effects analytically without calculating a prohibitive number of terms. He concludes that the time has come to halt in our attempts to calculate more terms, and to fix on a small number as representing the principal features of the magnetic state of the earth with sufficient accuracy, and to deal with each of the residuals separately.

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES IN SEPTEMBER:—

Sept. 7. Venus. Illuminated portion of disc = 0·431; 27th = 0·568.
9. 7h. 56m. to 8h. 56m. Moon occults τ^2 Aquarii (mag. 4·3).
10. 16h. 9m. to 16h. 56m. Moon occults 30 Piscium (mag. 4·7).
11. 7h. 45m. Saturn in conjunction with Moon (Saturn 2° 33′ N.).
13. 11h. 42m. Minimum of Algol (β Persei).
14. 9h. Venus at greatest elongation (46° 2′ W.).
15. 13h. 37m. to 14h. 40m. Moon occults ϵ Tauri (mag 3·7).
,, Saturn. Outer minor axis of outer ring = 5″·32.
16. 8h. 31m. Minimum of Algol (β Persei).
,, 16h. 12m. to 17h. 29m. Moon occults o Tauri (mag. 4·8).
20. 19h. Venus in conjunction with Moon (Venus 5° 0′ S.).
22. 12h. 19m. Jupiter in conjunction with Moon (Jupiter 3° 45′ S.).
,, 22h. 59m. Sun enters Libra; Autumn commences.
29. 19h. Saturn in opposition to the Sun.

OBSERVATION OF PHŒBE, SATURN'S NINTH SATELLITE.—From a note in No. 4270 of the *Astronomische Nachrichten* (p. 362, August 21) we learn that photographs of Saturn's ninth satellite, Phœbe, were obtained at Greenwich, with the 30-inch reflector, on July 31, August 1, 2, and 3. Provisional measures of the position-angle and distance of the satellite, about 63° and 39′ respectively, are given for each date. On August 3 the satellite was at, or very near, eastern elongation, so that these positions, in combination with those determined at western elongation about October 30, 1907, will furnish valuable data for the determination of the mass of Saturn.

THE PARALLAX OF 61 CYGNI.—From meridian observations, made with the small meridian-circle of the Astronomical Institute of Heidelberg Observatory, Herr Giorgio Abetti has determined the parallax of the well-known double star 61 Cygni, and publishes a preliminary communication of his results in No. 4270 of the *Astronomische Nachrichten*. These preliminary results give a somewhat lower value than previous determinations, the respective parallaxes of the preceding and following components being $+0''·24 \pm 0''·05$ and $+0''·22 \pm 0''·05$.

PROMINENCES AT THE SUN'S POLES.—In No. 7, vol. xxxvii., of the *Memorie della Società degli Spettroscopisti Italiani* (p. 107) Father Fenyi discusses, at some length, the occurrence and appearance of large prominences near the solar poles. The discussion embraces the question as to the epoch of the sun-spot period at which such prominences are most frequently seen, and it is shown that their maximum takes place some months after the sun-spot maximum. Among other conclusions, Father Fenyi finds that there is a periodical sharp maximum to which it is desirable that further attention should be paid. He also shows that the estimation of the heliographic latitude of the sun's polar cap from the continuous observations of the positions of prominences in regard to the limb is not satisfactory, as a prominence 60″ in height will remain visible and simply appear to oscillate during a whole rotation. It is suggested, however, that satisfactory determinations of the polar rotation in high latitudes could be made by observing these prominences, whereas the sun-spot observation method cannot be applied and the spectroscopic method is unsatisfactory.

OBSERVATIONS OF VARIABLE STARS.—The periods and light-changes of several variable stars are discussed in Bulletins Nos. 15 and 16 of the Laws Observatory, University of Missouri. No. 15 is devoted to the discussion of 395 observations of the Algol variable RW Monocerotis (24, 1907) made during the period October, 1907, to April, 1908, and a period of 1·9 d. is deduced, the light-changes taking place in 7h. 34m.

The observations of the long-period Algol variable RZ Ophiuchi are discussed in Bulletin No. 16, and a period of 261·8 d. is found to satisfy them. The other variables, for which only preliminary announcements are given, are RS Boötis, 43.1907 Draconis, 44.1907 Ursæ Majoris, and SW Andromedæ (5, 1907).

THE INFLUENCE OF THE EARTH'S ROTATION ON THE COURSES OF RIVERS.—In a paper published in the Transactions of the New Zealand Institute (vol. xxxix., pp. 207–213) Dr. F. W. Hilgendorf discusses some very careful observations made by himself of the possible influence of the earth's rotation on the course of the rivers which flow over the Canterbury Plains, New Zealand. These plains, being of a very homogeneous structure, afford an excellent site for the testing of "Ferrel's law" concerning the deflecting force of the earth's rotation, and Dr. Hilgendorf succeeds in showing that this deflecting force has, in all probability, been an effective factor in the modification of the banks of the rivers which flow in a N.E.–S.W. direction across the Canterbury plains.

A POSSIBLY UNDISCOVERED FORM OF SOLAR RADIATION.—In No. 5 of the *Comptes rendus* (p. 318, vol. cxlvii., August 3) M. E. Durand-Gréville discussed the secondary twilight and dawn which are observed in the Alps and at other great altitudes, and suggested that reflection of the sunlight from the temperature-reversing layer of the atmosphere, discovered by M. Teisserenc de Bort, might account for these phenomena; but in No. 7 of the *Comptes rendus* (August 17) M. Deslandres offers an alternative suggestion. It is that, in addition to the solar radiations which traverse our atmosphere and those ultra-violet radiations which are known to be absorbed by it, there may be others, in the further ultra-violet, to which the atmosphere may be transparent or which are able to produce a phosphorescence which would account for the secondary illumination of the mountain sides, &c., after the passing of the ordinary twilight. He further suggests a method whereby the existence of such radiations may be demonstrated.

WELSH ASTRONOMICAL TRADITIONS.

I HAVE put together some notes, compiled out of the flotsam and jetsam of Welsh tradition bearing on the continuity of the astronomy of the stone monuments, with the view of finding out how far such traditional materials will enable us to reconstruct, with the aid of the testimony of the monuments themselves, the story of the megalithic period in Britain, the period or periods of the avenue, circle, and cromlech.

The Testimony of the Bards.

I have already in these columns claimed for the Gorsedd a continuity of bardic tradition of the greatest value. A more careful study of isolated bardic utterances shows us the bard-astronomer at work in the same capacity as the priest-astronomer of the megalithic period.

There are two utterances attributed to the bard Taliesin which strongly suggest the use of stars as heralds of sunrise or as clock stars. In such utterances the note of antiquity is the bard's assumption of exclusive knowledge of astronomical phenomena. He challenges others to tell him "what hour in the small of the day (meinddydd) that Cwy was born?" Who Cwy was I know not, but the expression should be remembered in discussing Welsh solar heroes. Again, the bard speaks contemptuously of some who "do not know the point of separation between dewaint (the midnight watch, 1 to 3) and gwawr (dawn)."

It should be remembered that the body of tradition we are discussing was once common to the inhabitants of Wales as Goidelic or Irish before it became Welsh. The Irish bard Amairgen speaks still more definitely of his indispensableness:—

> "Who foretells the ages of the moon (but I)?
> Who teaches the spot where the sun rests (but I)?"

The sun rests at the solstice. People from the earliest times would have noted as much. But the spot—who but the bard knew the solstitial alignment? The words take us back not only to a period before the popular use of a calendar, but also to the time when the almanac for the year was fixed by direct observation of the solstice sun on the horizon; not that observation of the solstice along the horizon is in itself a proof of antiquity, for a farmer in the parish from which I write still uses that ancient method; but what is curious is the bard's assumption of exclusive credit for the information.

The leading astronomers of bardic tradition are mythical personages. I have elsewhere shown how the leading saints of Wales were regarded as astronomers. But the leading astronomers were the associates of gods, if not gods themselves. "The three sublime astronomers of the Isle of Britain:—Idris the Giant, Gwydion the son of Don, and Gwyn the son of Nudd. So great was their knowledge of the stars, and of their nature and situation, that they could foretell whatever might be desired to be known to the day of doom."

Idris is commemorated in the name of the Merionethshire mountain, Cadair Idris (Idris's Chair). The Milky Way is called Caer Gwydion (Gwydion's Encampment). His mother was a goddess. Gwyn, the son of Nudd, is spoken of as the King of the Fairies. His father seems to have been the Welsh Neptune.

So the remotest antiquity and the place of highest importance is given to astronomy in Welsh or British tradition.

Holed Stones.

These are rather rare monuments. I have notes of some in Wales, and I expect, with the growth of interest in the astronomical study of such monuments, that more will be brought to light. As Cornish and Scottish tradition shows, such stones were used as charms, a fact which largely explains their present rarity.

I have not been able to find out the origin of a familiar Cardiganshire expression. When one makes a vain attempt to make another person understand or heed what is told him, the speaker or a friend makes the remark, "You might as well say Carreg a Thwll (Stone and Hole) to him." This cryptical Welsh expression is the name of the famous holed stone of Cornwall, Men-an-Tol, so that the Welsh colloquial Carreg a Thwll may reasonably be supposed to be the holed stone of the megalithic period.

A holed stone figures prominently in one of our oldest written tales, namely, the tale of Math, son of Mathonwy. The stone was on the bank of the river Cynvael in Ardudwy, a part of Merionethshire, and it was called Llech Gronw, "the Stone of Gronw or Goronwy." Gronw loved, and was loved by, the wife of Llew Llew Gyffes, "Llew of the Un-erring Hand." The woman induced her husband to tell her how he might be slain, pretending the most affectionate concern in such an event. He told her gladly, "Not easily can I be slain, except by a wound. And the spear wherewith I am struck must be a year in the forming. And nothing must be done towards it except during the sacrifice on Sundays. I cannot be slain within a house, nor without. I cannot be slain on horseback nor on foot." "Verily," said she, "in what manner then canst thou be slain?" "I will tell thee," said he. "By making a bath for me by the side of the river, and by putting a roof over the cauldron, and thatching it well and tightly, and bringing a buck, and putting it besides the cauldron. Then if I place one foot on the buck's back, and the other on the edge of the cauldron, whosoever strikes me thus will cause my death."

The woman's paramour toiled for a whole year making the spear, and one day Llew was induced by his wife to stand in the position by the cauldron he had indicated. "Thereupon Gronw rose up from the hill which is called Bryn Cyfergyr, and he rested on one knee, and flung the poisoned dart and struck him (Llew) on the side, so that the shaft started out, but the head of the dart remained in." Llew was, as we would say, mortally wounded, but nobody dies in the Mabinogion tales. They simply become transformed. Llew reappeared to return exactly the same compliment to Gronw. The latter begged of Llew to allow a stone to be placed between him and the marksman. "Then Llew flung the dart at him, and it pierced the slab and went through Gronw likewise, so that it pierced through his back."

At first sight the whole story might be regarded as a clumsy attempt at explaining the holed stone and its name. But what of the very curious details of this fabulous Order of the Bath?

The following considerations offer themselves. The typical wizard in Welsh tradition orders his body to be buried neither within nor without the church. "Standing with one foot in the grave" is a familiar expression. The picture of a man standing with one foot on the cauldron and the other on a buck's back brings to my mind another legendary picture of a man visiting a cave, where hidden treasures lie, on a cow's back. It was necessary for him to enter into the cave and return without dismounting, and he was allowed to pick up as much of the treasure as he could grab at on that bovine ride. The two Welsh words "bwch," "buck," and "buwch," "cow," are similar enough to become confused in such tales.

I would assume that the holed stone Llech Gronw was a solstitial sight-line from a circle or cromlech to the point on the horizon where the marksman stood to throw the dart. Such a sight-line was of no use except once a year, as the sun would only call once a year at that point. The spear was a year in the making. Sundays may be regarded as the moon's quarters, and though a solstitial cromlech or alignment was of use only once a year, there is evidence of a weekly ceremony of some sort at such monuments. The "curate" in charge of an astronomical monument had, like other curates, his work to do all the year round.

A holed stone between a specified hill-point and a covered structure—we have here the complete apparatus for observation from an *allée couverte*. To the archæologist, however, the holed stone is now a telescope reversed—that is, it is of use not to observe sunrise so much as to find hidden parts of the original alignment. Sir Norman Lockyer, in "Stonehenge," has shown that "holed stones were arrangements for determining alignments," and he tells us how an ancient stone alignment was traced from a circle to holed stones at Tregaseal (p. 282).

The utility of the holed stone as a path-finder, so to speak, has been proved by Dr. Baudouin, in France. At the third "Prehistoric" congress of France last year (reported in NATURE, October 24, 1907, p. 649), he showed that "menhirs were indicators of megalithic sepultures." "By using two certain holed stones as indicators, he was enabled to discover an *allée couverte* which was buried under the soil, and had until then remained undiscovered. This 'find,' made with remarkable scientific precision, was received by numerous foreign congressists as a striking example of the value of a theory which many of them still ignore."

The best oral tradition of a cave with sleeping warriors guarding treasure I have been able to collect at first hand is widely known at Devil's Bridge, Cardiganshire. All the story, which I have dove-tailed out of five versions, would occupy more space than I can reasonably expect to have here. Though it is known that the cave is situate on a woody slope on the farm of Tynycastell on the south side of the river Rheidol, no one living has seen it. But tradition has it that the mouth of the cave is sometimes seen from some point on Penrhiw farm on the north or opposite side of the valley. If my recollection is correct, that farm is north-east, and on the sky-line, from the traditional site of the cave, and if a careful search were made for some alignment or indicator at Penrhiw, possibly the hidden cave might be re-discovered. At any rate, the fact that the mouth of the cave used to be seen from Penrhiw indicates that the cave was a summer solstice *allée couverte*. This is further confirmed by the tradition that the mouth of the cave was to be seen from Penrhiw only once a year. These inferences as to the solstitial use of the cave are amply confirmed by the most definite local tradition.

This was the cave where the visitor had to enter and leave on a cow's back. Another version has it that there were three steps into the cave. When the visitor stood on the first step, a sleeping warrior inside awoke; when he stood on the second step, the warrior laid his hand on his sword; and when he stood on the third, the warrior cut off with his sword the lighted head of the candle which the visitor held in his hand. Another version simply gives it that when the visitor entered a warrior awoke and asked, "A ddaeth y tri bore'n un?" (Have the three mornings become one?) For about five years I kept troubling our folklore authorities for an explanation of that strange question, and quite in vain. Sir Norman Lockyer solved the conundrum by return of post. The three mornings in one are the three days of the solstice, when the sun stands still, and because of that apparent stationariness the three days were reckoned as one.

I believe that the well-known tradition at Devil's Bridge of a demon or hero imprisoned in a rock is a part, if not the leading part, of the cave legend of Tynycastell close by. He is popularly believed to be working his way out of a rock a "fathom" deep with "an ounce hammer and a clog nail," or, as a more degenerate tradition has it, "an ounce hammer and a carpet-tack."

So much about a living tradition of a solstitial cromlech in actual use, and a tradition which has apparently survived the cromlech itself. By the bye, the "cave" is never a tomb. Its occupants are always alive, alive not as mere ghosts. Mr. T. H. Thomas has observed that "wraiths of human guardians, or inhabitants, are rare about cromlechs, while they are remarkably numerous about tumuli." While the cromlech in most instances seems to have been converted into a tomb, it is interesting to note that its folklore associations must have become fixed before such a conversion, and agree with and elucidate the astronomical interpretation of the use of the cromlech.

JOHN GRIFFITH.

THE PRAGUE EXHIBITION.

FOR commercial purposes, the kingdom of Bohemia is divided into five districts, each under an officially recognised chamber of commerce. The district of the Prague Chamber of Commerce is the largest and most important of these. It has a population of two millions, a third of the population of the kingdom, and includes many of the chief centres of industry. The organisation of the Jubilee Exhibition at Prague is due to the enterprise of the committee of the Prague Chamber of Commerce. From every point of view the exhibition is a success. It is attracting a daily attendance of from 10,000 to 60,000 persons, who come from all parts of the world. The exhibition occupies an enclosure in a beautiful, well-wooded park on the outskirts of the city. In the effective design and picturesque arrangement of its buildings it is a model of what such an exhibition should be.

As would be expected, the character of the exhibition is definitely commercial; but pure art is not entirely neglected, and the orchestral concerts of the Bohemian Philharmonic Association are of a high order. No fewer than eighteen imposing pavilions have been specially built, and in addition use is made of a permanent building, erected originally for the exhibition of 1891. These buildings are devoted to the City of Prague, the chamber of commerce, machinery, the cities of the district, ceramic and glass industries, schools, goldsmiths' work, food, metal work, leather industries, chemical industries, graphic arts, photography, commerce, and temporary exhibits. Speaking generally, the exhibits in all departments show a high standard of design, finish, and workmanship. In view of the fact that many of the industries mentioned have only been established in Bohemia in recent years, the high quality of the manufactures is certainly remarkable.

Not less remarkable is the increase in the amount of the manufactures. In 1891 one building was sufficient to include a representative exhibit of the manufactures of the district; now it is difficult to compress such an exhibit into nineteen buildings. Trade returns confirm the impression which this comparison suggests.

An inquiry naturally suggests itself into the causes of this development. Recent years have certainly seen a great awakening of national life in Bohemia, but this alone would not be sufficient to account for the commercial prosperity of the country. A better explanation is to be found in the system of technical education which is being successfully worked in Bohemia. The scope and character of this system is admirably displayed in a well-arranged exhibit. Undoubtedly the keynote of the system is "specialisation," a word which has no terrors for the Bohemian, who is surprised that the exhibits from some of the special schools—such, for example, as that from a school for training barbers—should cause amusement to the English visitor; but, nevertheless, the fundamental principle of the system seems to be sound. A boy's trade is fixed, and whether or not he is apprenticed, he is trained definitely for the selected trade, without too much insistence on theoretical principles. If the boy is apprenticed he is obliged to attend an industrial continuation school during his apprenticeship. In these schools there are from six to ten hours of instruction per week, the lessons being given in the afternoons or early in the evening and on Sunday mornings. The course lasts from two to four years. If the boy is not apprenticed he is able to attend one of the special schools (*Fachschulen*) in which courses are provided, definitely taking the place of the apprenticeship. The industries included in the scope of these schools are lace-making, wood-carving, carpentry, cabinet-making, textile industries, basket-making, iron and steel work, engineering, masonry, glass-making, hardware goods, electro-engineering, locksmiths, musical instruments, jewellery, precious stone setting, machine embroidery, watch-making, and gun-making.

Another interesting type of school is the general handicraft school, to which boys of twelve are admitted. The aim is to give the boys a better preparatory training for a trade than is possible in the ordinary elementary school. Great stress is laid on drawing, and the boys are given practical instruction in the preparation of materials for wood and metal work. An experiment on these lines is at present being conducted by the London County Council. There are also higher industrial schools for well-prepared pupils who require a higher training for art, chemical or textile industries, building or engineering. In the larger centres of population there are central industrial institutes, where work of the nature of research is carried on.

The Austrian system of technical education is of special interest at the present time, because there is undoubtedly a tendency in England towards a higher degree of specialisation in the work of technical schools. It is being realised that an efficient system of technical education cannot be organised by the erection all over the country of technical institutes of the same type, with similar classes and laboratories, staffed by the same type of teacher. Some years ago it was discovered that mathematics could be taught for the practical purposes of engineers in a practical way, without much insistence on abstract principles, and since then a good deal of thought has been given to the special educational requirements of the several industries. Moreover, the educative value of the technical processes themselves is being more fully recognised. Mr. W. R. Lethaby, professor of design at the Royal College of Art, in a paper read to the International Drawing Congress on August 3, deprecated the "elaborate approaches to a practical subject" at present in vogue. "The great end," he said, "was production, the great thing was the trade, the craft, and sufficient culture could be hung up to any sufficient trade. . . . All proper education was the opening up of a necessary and beneficent life occupation." This expresses in the clearest way the principle which appears to underlie the Austrian system of technical education. The principle may be stigmatised as utilitarian; but anyone who doubts the practical success of the system will be well advised to examine the exhibits at the Prague Exhibition.

T. LL. H.

THE IMPROVEMENT OF AGRICULTURAL PLANTS.[1]

IN the *Bulletin de la Société d'Encouragement pour l'Industrie nationale* for May, M. Schribaux gives an account of the methods adopted for obtaining new varieties of agricultural plants. These methods fall into three groups:—(1) careful watch is kept for "sports," *i.e.* for plants which, for no obvious reason, differ from the others; (2) variation is induced by altering the conditions of growth; (3) suitable plants are "crossed."

The first method is necessarily haphazard, since sports can obviously not be predicted; it has, however, proved very useful in the past, and has yielded many valuable varieties of potatoes, of fruit trees, &c. The second method promises very interesting results, for some plants respond quickly to changes in their surroundings. M. Schribaux sowed in a garden soil the seeds of the wild carrot, an annual with a woody root. In two generations a certain number had become biennials, with a fleshy root like the cultivated carrot. M. Blaringhem adopted quite a different method with maize. Plants were cut down just as the ear was beginning to develop, *i.e.* at the time of maximum vital activity; 76 per cent. of those surviving developed abnormally. Some were permanently altered; thus a late Pennsylvania maize was converted into an early variety.

Another instance of great practical importance is furnished by the vine. After struggling long and vainly against Phylloxera, the French vine-growers have made up their minds to live with it. M. Viala visited America and brought back some vines which had become so differentiated from those growing in France that they withstood the attacks of the pest. Unfortunately, they would not grow on calcareous soils, but became very chlorotic, and further search was made. Vines were in the end discovered capable of withstanding Phylloxera and of growing on calcareous soils; these have solved the problem for the French grower. Perhaps the case of the sugar-beet is most interesting. The grower requires roots containing a large percentage of sugar, a low proportion of the accompanying salts, and capable of resisting adverse conditions. The selection is made, in the first instance, on the basis of the sugar content. A large number of roots can be rejected by simple inspection, for high sugar content is correlated with certain external features; the other roots are examined chemically, since it is found that removal of a portion for this purpose does not interfere with subsequent growth. The very best are then cut up into a number of pieces to be grafted into other roots; they produce seed, which is sown, and yields roots for further selection. M. Schribaux states that a single root has yielded sixty-four pieces, each capable of producing seed! It is not surprising that the percentage of sugar has gone up from 11 per cent. in 1870 to 16 per cent. or 18 per cent. to-day.

There is evidence, however, that the process will not go on indefinitely, for roots containing more than 18 per cent. of sugar cease to vegetate properly. Sir W. T. Thiselton-Dyer discusses this aspect of the question in the Journal of the Board of Agriculture for April, taking the potato as an illustration. Like the sugar-beet, the potato has been the subject of continual selection, and the end result is a highly artificial tuber of great commercial value but difficult of cultivation. The practical man speaks of degeneration, but Thiselton-Dyer does not consider this to be the case. He points out that the potato has been induced to load itself with starch far in excess of any natural requirement of the plant, and suggests that too much is being demanded of the plant, and the machinery for the processes of growth has reached its breaking point. "We can control nature in altering the constitution of a plant; but eventually a barrier is reached beyond which it is impossible to go."

It is often found difficult to fix the new varieties obtained by selection. Even when asexual reproduction is possible as in the case of trees and potatoes, the variation frequently does not remain permanent, and many promising varieties have disappeared. When reproduction is by seed

[1] (1) *Bull. de la Société d'Encouragement pour l'Industrie national* May, 1908.
(2) *Journal of the Board of Agriculture.* April, 1908.
(3) *Journal of Agriculture of South Australia*, January, 1908.

it is still more difficult to fix a variety; this is abundantly proved by the difficulty of improving wheat. A single ear is selected because it possesses some desirable property; the seed from it is sown; an ear is selected showing the same property, and the process is continued for several generations. "Pedigree" seed is thus obtained, but it rarely remains true; the farmer has to renew his stock periodically from the raiser, who keeps on the selection process. The work done on the selection of seed wheats at the Roseworthy Agricultural College is described in the Journal of Agriculture for South Australia; it is hoped in this way to obtain strains which will keep their character for two or three seasons, and prove much more profitable than the seed wheat now in use. There is no question that a good deal can be done by selection, especially in South Australia, where, we are told, little or no attention has been given to the matter, and the best grain is sometimes sold and the worst kept for seed. But it is now clear that the only safe method for the improvement of crops grown from seed is to breed on Mendelian lines, as Biffen is doing at Cambridge, and South Australia would do well to breed, as well as to select, seed wheat.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Robert Forsyth Scott, fellow and senior bursar, has been elected master of St. John's College in place of the late Rev. Dr. Charles Taylor.

PROF. D. J. HAMILTON, F.R.S., has, in consequence of ill-health, resigned the chair of pathology in the University of Aberdeen to which he was appointed in 1882.

MR. W. GALLOWAY DUNCAN, of Dundee, has been appointed head of the Government Engineering School, Dacca, Bengal.

THE Senate of the University of Bombay has, according to the Allahabad *Pioneer Mail*, decided to include a test in science for all candidates for a degree.

THE jubilee of the University Museum at Oxford will be celebrated on October 8. Honorary degrees will be conferred upon Prof. Arrhenius and Dr. Vernon Harcourt, F.R.S., and a bust will be unveiled of Prof. W. F. R. Weldon, who died in April, 1906.

THE Year-book of the Michigan College of Mines, 1907–8, shows that the college is better equipped and more prosperous than at any previous period since its foundation in 1885. There are now 253 students, their average age being 22½ years. The concentration of effort on training men for the field of mining, the situation of the college in the heart of the copper-mining region of Lake Superior, together with its special methods of instruction, have brought to the institution a large measure of success. Considerable range is allowed in selecting the courses or subjects which shall compose a particular student's curriculum, and the Record of Graduates, published as a separate pamphlet, giving their occupations, affords interesting evidence of the success attained.

IT is now recognised that the teaching of hygiene and physical exercises to pupils in both primary and secondary schools is of equal importance to their education in other branches of knowledge. In primary schools it is of special importance, as the opportunity for games is often absent in large towns. For this reason the Board of Education makes a knowledge of the methods of teaching and the aims of physical education one of the necessary parts of the equipment of a primary-school teacher. With this qualification is associated the requirement of a knowledge of hygiene, particularly in relation to schools and school children. For the last ten years a systematic course for women has been carried on at the South-Western Polytechnic, Chelsea. This training has been so successful that the course, originally designed for two years, has developed now into one of three years. The governors of the Chelsea Polytechnic are now instituting a similar course for men, and for this purpose they have engaged a teacher of gymnastics on Ling's Swedish system. In the first instance a course of one year for men will be provided, and it is hoped to obtain students who have passed already two years in training colleges, as well as university graduates with an initial equipment of general and elementary scientific knowledge. Such students, after a year devoted mainly to the study of hygiene, physiology, gymnastic exercises, and the part of anatomy bearing on physical training with study of the theory of movements, should be in a position to take charge of physical education in schools and to take their proper positions as teachers of usual subjects.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 5.—"On the Nature of the Streamers in the Electric Spark." By Dr. S. R. **Milner.** Communicated by Prof. W. M. Hicks, F.R.S.

(1) The streamers in the inductive spark consist of metallic vapour, the atoms of which are charged, and the motion of the vapour towards the centre of the spark gap is mainly due to the action of the electric force of the spark on the charged atoms. The chief evidence in support of this consists in a number of photographs in which the streamers move back again towards the poles as the oscillating electric field of the spark reverses its direction.

(2) Very great differences were found in the appearances of the streamers which correspond to the different lines of the same metal. The streamers may be divided in this respect into three classes, between which there is in most sparks a sharp distinction.

(*a*) Blurred streamers, which are often partly masked by the whole spark gap being filled with their light. These invariably correspond to lines prominent in the arc. (*b*) Sharply defined streamers, which appear throughout the whole time during which the electrical discharge lasts. These correspond to pure spark lines, *i.e.* lines which are not present in the arc under ordinary conditions. (*c*) A third class of streamers show very brightly at the first oscillation, but fade away so rapidly that they appear for only one or two oscillations, even when the other lines, initially no brighter, show ten or twelve. These lines are very sensitive to the influence of self-induction in the circuit; they are very bright in the condensed spark without inductance, but disappear from the spectrum altogether when a moderate inductance is inserted.

By studying the duration of the lines in the inductionless spark, the difference between the three classes of streamers is found to be solely a question of the duration of the luminosities of the metallic lines to which they correspond, the arc lines having a long, the spark lines a short, and the "condensed spark" lines a very short, duration.

(3) No other difference than this one of the durations of the lines has been discovered in the character of the streamers. The photographs obtained show clearly that the velocities of the streamers corresponding to the different lines in the same spark are the same, in spite of the different character of the streamers.

April 30.—"The Supersaturation and Nuclear Condensation of certain Organic Vapours." By T. H. **Laby.** Communicated by Prof. J. J. Thomson, F.R.S.

(1) The least expansion, which causes condensation in air initially saturated with an organic vapour and ionised by Röntgen rays, has been determined for five esters, six acids (formic to iso-valeric), and iso-amyl alcohol.

(2) In the case of acetic acid the expansion required was greater for feeble Röntgen rays than for more intense ones.

(3) The supersaturation, S, existing at the end of each of the expansions mentioned in (1) has been calculated, and also for four alcohols and chloroform from Przibram's experiments.

(4) The acids are found to have the largest values of S and the alcohols the least. The isomers examined have the same value for S with one exception. In the case of the alcohols, ethyl to iso-amyl, a fairly regular decrease in S accompanies the addition of a CH_2 group.

(5) The existing theory of condensation on ionic nuclei has been given, values of S have been calculated from it, and compared with S deduced from the observed expansions. The agreement in the case of acetic, propionic, *n*-butyric, and iso-butyric acids, and methyl alcohol is very close.

(6) The expansion and supersaturation necessary for condensation on the natural nuclei have been determined for the same (dust-free) vapours. In the case of formic, acetic, and butyric acids a distinctly greater expansion is required to catch the natural nuclei than that required for the ionic nuclei produced by Röntgen rays.

(7) As the expansion was increased the number of drops usually increased continuously with it, so that the fog point was ill-defined, except in the case of tertiary amyl alcohol.

(8) Ethyl acetate, methyl butyrate, propyl acetate, acetic acid, and iso-amyl alcohol were found to condense for a smaller expansion on the positive nucleus than on the negative. Water is the only known substance for which the negative ionic nucleus is more efficient than the positive.

(9) On bubbling air through methyl, ethyl, and iso-amyl alcohols, ethyl acetate, propyl acetate, methyl butyrate, chloroform, and ethyl iodide they became negatively electrified. This was the sign of the electrification to be expected from Prof. Thomson's double layer theory of the relative efficiency of ionic nuclei. Acetic acid was not in agreement with the theory, for it became positively charged on bubbling.

PARIS.

Academy of Sciences, August 24.—M. Bouquet de la Grye in the chair.—A problem relating to the theory of orthogonal systems and to the method of the mobile trihedron: Gaston **Darboux.**—Methylation in the ethylene series from the point of view of volatility: Louis **Henry.** The replacement of the hydrogen atoms attached to carbon in ethylene glycol by the methyl group causes a lowering of the boiling point, although the replacement of the hydrogen atoms in ethylene has the contrary effect. This is probably due to the association of the molecules in the alcohols, the coefficient of association becoming less as the number of hydrogen atoms substituted is increased. The boiling point is raised when methyl groups are introduced into ethylene chloride, and the ethylene oxides behave in a similar manner.—Twilight illuminations: Ernest **Esclangon.** A discussion of the colour effects observed at Bordeaux during twilight, the causes of which still remain unexplained.—Observations of the sun made at the Observatory of Lyons during the second quarter of 1908: J. **Guillaume.** Observations were made on sixty-five days, and tables are given showing the number of spots, their distribution in latitude, and the distribution of the faculæ in latitude.—The theory of asymptotic lines: A. **Demoulin.**—The zeros of the integrals of a class of differential equations: Georges **Rémoundos.**—The variation of two ruled surfaces: M. **Haag.**—Liquid helium: H. Kammerlingh **Onnes.** Details are given of the methods adopted for the liquefaction of 200 litres of helium on July 10. This experiment required the use of 75 litres of liquid air and 20 litres of liquid hydrogen. Owing to the extremely small capillarity of liquid helium, the surface of the liquid meets the side of the containing vessel like a knife blade, and the formation of the first quantity of liquid escaped observation. The helium remained in the liquid state for two hours; its density was 0·154, and its boiling point, determined with a helium thermometer, 4°·3. The critical temperature is probably about 5°, and the critical pressure not much above 2·3 atmospheres. The helium was not solidified when the pressure was reduced to less than 10 mm. of mercury.—The action of chloride of arsenic on cobalt: F. **Ducelliez.** This reaction gives rise to cobalt chloride, together with an arsenide of cobalt. At a temperature of 1000° C. Co_3As_2 is formed, at 600° C. to 800° C. CoAs, at about 450° C. the product is Co_2As_3: below 400° C. $CoAs_2$ is slowly formed.—The action of arsenic trichloride upon nickel and arseno-nickels: Em. **Vigouroux.** The arsenides Ni_3As_2, NiAs, and $NiAs_2$ were obtained by varying the conditions of the experiment.—The two methods of preparation of monomethylamine: Maurice **François.** In a previous paper the author has described a method of separating methylamine from small proportions of ammonia by means of yellow mercuric oxide. To prepare the pure amine by this process it is requisite that ammonia should be the sole impurity, and the present paper gives a comparative study of the methods of Hoffmann (bromine and acetamide) and Brochet and Cambier (ammonium chloride and formaldehyde) from this point of view. The methylamine hydrochloride prepared by the latter method proved to be very impure; but the product of Hoffmann's reaction was quite suitable for further purification by mercuric oxide.—The mode of growth of the Morille (*Morchella semilibera*): Louis **Matruchot.**—The influences of the external conditions on the development and sexuality of the prothallus of Polypodiaceæ: G. **Perrin.**—Physiological study on the development of fruits and seeds: W. **Lubimenko.** Perforation of the pericarp causes an atmosphere with a lower percentage of carbon dioxide to surround the ovule, resulting in arrested development.—A disease of the oak: MM. **Griffon** and **Maublanc.** This disease, which since the spring has attacked the oaks in a great part of France, is due to a white mould of the genus Oidium. It spreads rapidly, and has caused great damage.—The minute structure of the sporozoites of *Plasmodium relictum*: Edmond **Sergent** and Etienne **Sergent.**

CONTENTS.