THURSDAY, JULY 29, 1897.

M. FAYE ON CYCLONES.

Nouvelle étude sur les Tempêtes, Cyclones, Trombes ou Tornados. Par M. Faye, Membre de l'Institut et du Bureau des Longitudes. Pp. vii + 142. (Paris: Gauthier-Villars et fils, 1897.)

M. FAYE has quitted the position of dignified retirement to which he is entitled by his age and his scientific reputation, to do battle with the meteorologists. For the last twenty-five years, as he reminds us, he has struggled to ensure the recognition of his views concerning the origin and development of cyclones and tornados. At the advanced age of eighty-three, he again enters the arena of polemics and controversy, hoping to secure converts and supporters of the views that he has consistently held. We may offer our congratulations to the distinguished physicist on the vigour which such an undertaking demonstrates, but we could have wished that it had been exhibited in a manner better qualified to justify his reputation, and increase the number of his admirers. In the work which M. Faye has undertaken, of collecting his scattered writings and systematising the information which supports his views, he is perhaps supported by the belief that his theory is making headway. He quotes the remark of M. Sainte-Claire Deville, who, in the Academy, asserted with some warmth that M. Faye was entirely alone in his opinion, that there was not a meteorologist, not a sailor, who shared his views, and at that time the author admits that he was universally condemned, but adds, "If I were put on my trial to-day I should get some votes, though not enough to acquit me." In this opinion we think M. Faye deceives himself—the views of American and German meteorologists are as much opposed to those he holds as when M. Deville made his cutting rejoinder.

The interest which M. Faye takes in meteorology in general, and in cyclones in particular, is the outcome of the attentive study which he has given to the behaviour of sun-spots. As is well known, he is the author of a theory concerning the origin of these phenomena which was respectfully received at the time it was enunciated, and gained some adherents, but accumulated knowledge drawn either from spectroscopic or photographic observations has done nothing to support it, and probably the hypothesis would now find fewer partisans than when originally conceived. M. Faye tells us that these spots are due to downward gyrating movements produced in the currents that everywhere furrow the photosphere, and carrying with them the cooler gases of the solar atmosphere. Fully possessed with the accuracy of this notion, it is easy for him to pass to terrestrial cyclones, and to see the same mechanism operating in our atmosphere. His judgment seems to be warped by his preconceived theories, and he views a cyclone or a tornado as originating in the upper strata of the atmosphere and penetrating downwards to the earth's surface. Herein lies the fundamental difference between the theories of M. Faye and those of modern physicists. The unanimous opinion of meteorologists, he is forced to admit, is that our cyclones are not the result of downward, but of an ascending motion. The evidence that is sufficient to convince most inquirers leaves M. Faye in doubt. While all the observed phenomena of a cyclone are not fully explained, there is still room for an alternative theory. M. Faye quotes with approval Dr. Sprung's admission that the motion of translation of cyclones is not adequately explained by any accepted theory. Like a practised controversialist, he knows how to make the most of such difficulties.

"Je disais aux météorologistes : Vous cherchez en bas, au sein d'un air calme, ce qui ne peut s'y trouver : la cause de cet effroyable mouvement ? Eh bien ! levez les yeux, et voyez quels mouvements rapides existent dans les hauteurs de l'atmosphère à 2000^{m} seulement, où les couches d'air se transportent à grande vitesse sans que nous en sentions rien en bas ; c'est là qu'il faut chercher ; c'est là et non au ras du sol, qu'il faut placer l'origine du phénomène. Mais alors il faut renoncer aux trombes ascendantes, il faut reconnaître que les mouvements d'ensemble sont descendants."

Meteorologists would probably give M. Faye his 2000 metres, or something like it. The greater the saturation of the air the easier would the ascending current be maintained, and the greatest saturation would probably occur near the lowest cloud stratum. The origin of the tornado or the cyclone would probably not be "au ras du sol," but at some distance from the surface. But in the "drift" theory of translation M. Faye has no original rights. Ferrel and Loomis have equally asserted the principle of the "drift" theory, and it is difficult to see what support it lends to the "downward" motion or advances our knowledge of either the origin or the motion of cyclonic centres.

The real question at issue, and one which it would seem might be settled by observation, is one of fact. Is there an indraught of air to the cyclone vortex at or near the earth's surface and a consequent ascending current over it, or, on the other hand, an outflow from a descending current? Or, to adopt the characteristics and nomenclature with which the author sums up the discussion on tornados, and contrasts his theory with those of meteorologists, we may arrange the question thus :—

	M. Faye.
Aspiration ...	nulle.
Ascension ...	neant, l'air de la trombe est descendant par sa trompe.

	Meteorologists.
Aspiration ...	énergique, en masses énormes.
Ascension ...	totale, couches inférieures portées en haut.

The two theories are the reverse of each other ; we cannot say with the author that the meteorological theory is the reverse (contrepied) of the truth. Observation, as mentioned above, ought to be able to settle this question, and consequently a large portion of M. Faye's book is occupied with accounts of cyclones and tornados, drawn from various sources from the time of Dampier to the present day. And the author, as we think, lingers rather lovingly over the older sources of information. His sympathies and his admiration centre round Redfield and Piddington rather than Ferrel and Loomis and Blandford. We doubt if the last-named occurs once in the whole book. We gather, too, that Faye himself has never witnessed the destruction caused by the passage of a severe tornado. He has to rely entirely on the accuracy of eye-witnesses, to picture the scene as best he can, to select his facts, and to supply the explanations that best accord with his notions. This places him at a disadvantage when compared with some other of the physicists named. M. Faye is of course obliged to admit that heavy objects are sometimes carried to a distance. He sees these objects practically whisked over the surface by a succession of impulses, like a football continually kicked in the same direction. That some of these objects, transported possibly for miles, are often of a frail nature, and would be broken by rude and frequent contact with the surface, is not remembered. But if M. Faye had actually witnessed these occurrences, he could not have treated the evidence so lightly.

Then the author avails himself of another mode of escape. When the evidence in favour of an ascensional current is too strong, then the phenomena of which it is a characteristic is not that which is under consideration. Consequently we have "fausses trombes" and "pretendus cyclones" whose explanation, as generally given, the author would accept but without admitting any similarity with the more powerful manifestations. Something of disdain runs through the following passage :—

"On cite même des trombes de linge exposé sur le sol pour sécher, où l'on voit le linge, les mouchoirs, les chemises, &c., s'élever dans les airs en tournoyant pour retomber plus tard au loin sur le sol . . . ce sont de fausses trombes dont on a fait la théorie d'ailleurs parfaitement correcte."

As a general rule it is a matter of perfect indifference to the ordinary purposes of life whether we hold a correct or an incorrect theory in astronomy or meteorology. Life and commerce and navigation would go on the same whether we believed that the earth went round the sun or the sun round the earth. But in this matter of tempests and cyclones, trade and commerce can be very adversely affected if we teach an incorrect theory of their origin and motion. A captain can only hope to escape from the danger with which they threaten him by localising with some precision the situation of the inner vortex. To do this he has but one guide, the direction of the wind. The use he makes of this guide in inferring the position of the ship with reference to that of the storm centre will be materially affected by the views he holds concerning the motion of the wind in a cyclonic storm. A rule must be devised for his guidance without ambiguity, and one that can be followed without hesitation. Piddington and the older meteorologists held that the movement of the wind in a cyclone was circular. In this view they are followed by M. Faye. The result of this belief was the enunciation of the rule of eight points, expressed something in this way. With the face turned to the wind extend the right arm. In the northern hemisphere you will point in the direction of the storm centre. This rule can be supported only by ignoring a great mass of recent observations. The rule asserts that the wind blows at right angles to the radius, but it has been shown, over and over again, that in true cyclones the winds are strongly inclined inwards; not directly to the centre, but approaching it by a spiral. A more accurate rule has been deduced and is supported by weighty authorities, but not by M. Faye. In the northern hemisphere with face to the wind, the direction of the centre is from ten to eleven points to the right-hand side. To go back to the old rule of Piddington is a retrograde step, but the mischief does not end there. The distrust likely to be awakened in the mind of the seaman by the spectacle of disaccord among scientific authorities can have the most disastrous results. The ordinary seaman asks for a clear and precise rule, on which he can act without argument or question, while his whole attention is directed to the preservation of his ship. M. Faye is a great authority. His name is one to conjure with, and it is not unlikely that the rules which he quotes with approval will be copied into English books by those who compile manuals of brief and ready directions for navigation, and in this way perpetuate an evil against which a mass of scientific evidence, collected in less accessible quarters, is powerless.

THE YEW.

The Yew-Trees of Great Britain and Ireland. By John Lowe, M.D., &c. Pp. xiv + 270. (London: Macmillan and Co., Ltd., 1897.)

NOT far behind the oak stands the yew in popular estimation. Its associations, its form, its distribution, its utility, account for this. We consider it an aboriginal native, and so no doubt it is. From the Tertiary epoch to the present it has been in existence, and now it extends over the whole of the northern hemisphere from Norway to the Azores and Algeria, from Ireland to the Amur. It abounds in certain parts of the Himalayas, and we have it from the ruby mines of Burmah. In Japan a yew exists which it is hard to distinguish from our European species. From Canada to Virginia in Eastern America another species ranges, whilst on the opposite side of the American continent in California and some parts of the "Rockies" yet another is found; one, moreover, is chronicled from the mountains of Mexico. According to circumstances, and especially according to his proclivities, the botanist will range all these as separate species (Parlatore enumerates six), or as representatives of one and the same. The *Index Kewensis* quotes no fewer than ninety synonyms for these six species, a pretty good illustration of the variation among botanists! Those who visit an extensive tree-nursery, and see the large number of forms known to be seedling variations from the common yew, will be inclined to favour the idea that there is but one species. Those whose research is limited to herbaria may come to the opposite conclusion.

Naturally the yew is diœcious, the male flowers being on one tree, the female on another; but it not unfrequently happens that flowers of both sexes may be met with on the same branch. It seems natural to expect a greater amount of variation among diœcious than among monœcious trees; but it is striking to see so much variation in trees growing, as far as we can see, under the same natural environment. There are yews with red fruits and yews with yellow, golden variegations and silver-mottled leaves, long leaves, short leaves, leaves in two ranks, leaves in spiral arrangement; the latter being probably the primordial disposition, the two-ranked arrangement being more apparent than real. When the stem and branches grow erect, as they do in some varieties, particularly in that called the Irish yew, the leaves are in spiral order, and when the larva of a little fly (*Cecidomyia taxi*) has its abode in the young buds, the degenerated leaves are in spires.

In this country, at least, the yew is not generally found in association with other trees of the same or other species. Woods entirely or mainly composed of the tree are rare. One such we know, and it is duly noted in the book before us: we allude to the very remarkable grove at Cherkley Court, near Leatherhead. Here are some ninety to ninety-five acres covered with yew-trees, to the almost, if not complete, exclusion of everything else. They form a most impressive sight, and still more impressive than their numbers is the variety of their forms, their difference "in habit," as gardeners say. They are all growing under like circumstances, and yet there is this astonishing variation in outward form. There is a similar but much less extensive group at Cliveden, the gnarled roots of which, clinging on to the chalk escarpment for dear life, are very picturesque.

The isolated condition of yew-trees is no doubt due to the trees having been planted where they now are, as in churchyards, or along the roadsides.

If old yews do not exhibit the highest type of arboreal beauty, there are few trees more imposing. This, no doubt, leads to exaggerated estimates of their age. On this subject quite a literature has grown up. Dr. Lowe has a statistical chapter relating to it in the present volume, reprinted from the *Journal* of the Linnean Society; but the results are not uniform, and it is evident that further research on the comparative rate of growth of various trees is to be desired.

In the book before us the author treats the yew from almost every point of view; he has been conscientious in his work, accurate in his statements, careful in the verification of his references—in fact, he has produced a monograph which will be consulted in the future, and which will be read with interest by the lovers of trees at the present day. The book, too, is well printed, well illustrated, and provided with a sufficient index. A few amendments may be suggested—Cliveden is in Bucks, not, as stated, in Berks; the "De Candolle," so often mentioned, was "Augustin Pyramus," not "Alphonse," as might be inferred; *Gardener's Chronicle* should be *Gardeners'*; and the reference "1870–1890" is very inadequate, seeing that the periodical in question dates from 1841, and contains various references to the yew in its earlier as in its later volumes; "Helmsley" should be "Hemsley." Reference to Sargent's "Silva of North America," which contains an epitome of almost all that is known concerning the yew, whether in America or elsewhere, would be desirable in a new edition. We suspect from the date of publication that the author of the "Yew-Trees of Great Britain" could hardly have been able to consult the "Silva." The microscopical structure of the wood and leaves should also receive some attention, as it differs considerably from that of other conifers. Yews, for instance, have no resin canals.

OUR BOOK SHELF.

Untersuchungen ueber Bau Kerntheilung und Bewegung der Diatomeen. Von R. Lauterborn. Mit 1 figur im Text u. 10 Tafeln. (Leipzig: Verl. v. Wilhelm Engelmann, 1896.)

IT is some years since Bütschli declared that centrosomes could be identified during life in certain diatoms, just as Van Beneden had already described them in the segmenting eggs of *Ascaris megalocephala*. But whilst the worm has furnished the text for numberless papers and memoirs, the diatoms have been passed by and left unheeded.

Dr. Lauterborn's book will again direct attention to this neglected group, for the observations he records are so startling in themselves, and so unlike anything else with which we are as yet acquainted, that they will urgently require confirmation at the hands of other investigators.

The author has succeeded in following out many of the details of cell-division in the living cells, and this fact is calculated at once to arrest the attention of cytologists, who are all aware of the great difficulty which exists in making much out of a nucleus till it has been fixed and starved. Perhaps the most remarkable statements in the volume are those which are concerned with the origin and structure of the spindle and its relation to the centrosome. The latter structure is a spherical body lying near a depression in the nucleus, but when karyokinesis is about to begin, a second sphere is seen to lie close to the centrosome, and Lauterborn believes it has been actually derived from the latter. This second body is the rudiment of the central spindle, and it wanders about independently of the centrosome, and becomes rapidly of an elaborately complex nature. Ultimately it is found within the nucleus, whilst the centrosome is left outside. Meanwhile two masses of protoplasm become apparent in an excentric position at the ends of the barrel-shaped spindle, and these are regarded by the author as representing two fresh centrosomes. Finally the chromosomes split, become arrayed on the spindle, and are distributed to the two poles. But even in this process we meet with an anomaly, for there seem to be no mantle fibres formed, or indeed any other special mechanism by which the chromosomes find their way to their destination; they are stated to move automatically to the ends of the spindle. The cell-wall, which divides the diatom into two cells, originates in much the same way as in *Spirogyra*, beginning at the cell periphery and advancing to the centre.

The above sketch will show that these plants, of which *Surirella* has here been taken as a leading type, differ in many respects from other organisms in the mode of their cell-division. But the author by no means confines himself to the topic of karyokinesis. The structure of the protoplasm, and of its varied organised inclusions, as well as the remarkable movements exhibited by the plants themselves, all come in for a share of attention. In short, the book is one which should serve to stir up research into a group of plants which have hitherto been too much regarded as the special property of amateurs who, with the aid of expensive microscopes, delighted to count striæ and to make species.

Journal and Proceedings of the Royal Society of New South Wales for 1896. Vol. xxx. Edited by the Honorary Secretaries. Pp. xxiv + 410 + cxlviii. (London Agents: George Robertson and Co., 1897.)

THE twenty-four papers in this volume testify to scientific activity at the antipodes. In his presidential address Prof. T. W. Edgeworth David sums up the contributions of New South Wales to scientific knowledge during

1896, and discusses the structure and origin of the Blue Mountains of the colony. Mr. H. C. Russell's paper, in which he shows that the good and bad seasons follow a nineteen years' cycle, appears in the volume, with the discussion which took place upon it. Among other subjects and and authors of papers are: The Mika or Kulpi operation of the Australian Aboriginals, by Prof. T. P. Anderson Stuart; the cellular kite, by Mr. Lawrence Hargrave; an explanation of the marked difference of the effects produced by subcutaneous and intravenous injection of the venom of Australian snakes, by Dr. C. J. Martin; recent determinations of the viscosity of water by the efflux method, by Mr. G. H. Knibbs; the occurrence of precious stones in New South Wales, and the deposits in which they are found, by the Rev. J. Milne Curran; the rigorous theory of the determination of the meridian line by altazimuth solar observations, by Mr. G. H. Knibbs; an address to the engineering section of the Society, by Prof. W. H. Warren; the machinery employed for artificial refrigeration and ice-making, by Mr. Norman Selfe; and the present position of the theory of the steam engine, by Mr. S. H. Barraclough.

Many of the papers are accompanied by plates, that of the Rev. J. M. Curran being particularly well illustrated.

L'évolution régressive en biologie et en sociologie. By Jean Demoor, Jean Massart, and Prof. Émile Vandervelde. Pp. 324. (Paris: Félix Alcan, 1897.)

TO show that the laws of biology are followed in the domain of sociology has been attempted by many writers. Unfortunately, bio-sociological subjects are often taken up by naturalists who have little knowledge of social questions, or by sociologists having but a superficial acquaintance with biological realities, the result being unsound conclusions and exaggerated analogies. With the view to see the subject from different aspects, and produce a composite picture in which neither sociology nor biology is given undue prominence, the authors of this book have collaborated in its production. The result is not altogether satisfactory, for the book is really more sociological than biological, and not good at that. The general conclusions which the authors labour to prove are that evolution is at once progressive and retrogressive, that transformations of organs and institutions are always accompanied by retrogression, and that the same laws hold good in the changes of societies as well as organisms; all the actual forms undergoing transformation, and, in consequence, losing certain parts of their structure. The text of the book is the universal application of the principle of devolution, and in the exposition of it the authors have exercised their ingenuity to the utmost.

The Geographical Journal. Vol. ix. January to June 1897. Pp. viii + 748. (London: The Royal Geographical Society; Edward Stanford, 1897.)

THIS volume of the Royal Geographical Society's *Journal* contains several papers of exceptional interest, among them being Mr. W. L. Sclater's final contribution on the geographical distribution of mammals; a paper on the formation of sand-dunes, by Mr. Vaughan Cornish; Sir Martin Conway's account of his Spitsbergen expedition; two years travel in Uganda, Unyoro, and on the Upper Nile, by Lieut. C. F. S. Vandeleur; Dr. Nansen's statement of the results of his arctic expedition, and his views on the north polar problem; and a paper by the president, Sir Clements Markham, on the voyages of John Cabot. In addition to these papers, the monthly record and a number of special articles furnish a store of interesting information on geographical progress in its widest sense. Large coloured maps and other illustrations accompany the papers, and assist in making the volume valuable.

LETTERS TO THE EDITOR.

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

The Electro-Chemical Equivalent of Silver.

IN NATURE, vol. lvi. p. 259, Mr. Griffiths points out that recent comparisons of the values of the mechanical equivalent of heat, obtained by mechanical and electrical methods, suggest that the adopted value of the equivalent of silver may be in error to the extent of 1/1000. This adopted value rests, I believe, almost entirely upon experiments made by Kohlrausch, and by myself with Mrs. Sidgwick in 1882; and the question has been frequently put to me as to the limits within which it is trustworthy. Such questions are more easily asked than answered, and experience shows that estimates of possible error given by experimenters themselves are usually framed in far too sanguine a spirit.

When our work was undertaken the generally accepted number was ·01136, obtained by Kohlrausch in 1873. Mascart had recently given ·01124, subsequently corrected to ·011156. The uncertainty, therefore, at that time amounted to at least 1 per cent. The experiments of Mrs. Sidgwick and myself were very carefully conducted, and we certainly hoped to have attained an accuracy of 1/2000. So far as errors that can be eliminated by repetition are concerned, this was doubtless the case, as is proved by an examination of our tabular results. But, as every experimenter knows, or ought to know, this class of errors is not really the most dangerous. Security is only to be obtained by coincidence of numbers derived by different methods and by different individuals. It was, therefore, a great satisfaction to find our number (*Phil. Trans.*, 1884) (·011179) confirmed by that of Kohlrausch (·011183), resulting from experiments made at about the same time.

It would, however, in my opinion, be rash to exclude the question of an error of 1/1000. Indeed, I have more than once publicly expressed surprise at the little attention given to this subject in comparison with that lavished upon the ohm. I do not know of any better method of measuring currents absolutely than that followed in 1882, but an ingenious critic would doubtless be able to suggest improvements in details. The only thing that has occurred to me is that perhaps sufficient attention was not given to the change in dimensions that must accompany the heating of the suspended coil when conveying the current of ½ ampere. Recent experiments upon the coil (which exists intact) show that, as judged by resistance, the heating effect due to this current is 2½° C. But it does not appear possible that the expansion of mean radius thence arising could be comparable with 1/1000. RAYLEIGH.

Terling Place, Witham.

Acetylene for Military Signalling.

IN conjunction with Captain J. E. S. Moore, I have been making some experiments on the use of acetylene in signalling lamps. We have obtained such good results with the very primitive apparatus at present employed, the light is so brilliant, and the requirements so portable, that it seems well worth considering whether acetylene could not take the place of the limelight where portability is an object.

The apparatus consists of a 5-oz. bottle, carrying a two-hole rubber cork; water drips on to the carbide from a wide glass tube, holding some 2½ oz., and furnished with a connection of rubber tube and a screw-clamp to act as regulator. The gas escapes from a straight tube to the lamp, being trapped on the way by a wider piece of tube, into which the smaller tubes are corked at either end; this makes a sufficient condenser for any water vapour. The gas tube enters the lamp through the base, and the gas burns from an ordinary 0000 Bray. The generator, when charged, weighs one pound, and after a couple of minutes, during which time the action is a little irregular, will give a steady light for thirty or forty minutes; on more than one occasion, indeed, it has run out without the clamp being touched after first adjustment. We find an ordinary lamp small for the heat produced, and have had to rivet the soldered parts; but increased ventilation would be easy to arrange. Of course for permanent work the generator would have to be arranged in metal; even then it would probably be the lightest gas-supplying arrangement, for the illumination, yet produced.

The Laboratory, Felsted, Essex. A. E. MUNBY.

Disappearance of Nitrates in Mangolds.

DURING last autumn, winter, and spring, I made a series of analyses of mangolds, determining in each case the nitrogen present as albuminoids and as nitrates.

The roots were pulled during the last week in October, and clamped in the ordinary way. Roots of the same weight—about 7 pounds—were taken for analysis on the dates shown in the table.

The albuminoid nitrogen was determined in the dry matter, by leaving two grams in contact with 4 per cent. carbolic acid, containing metaphosphoric acid, for twenty-four hours; the nitrogen determined in the insoluble portion, by Kjeldahl's method, being taken as albuminoid nitrogen.

The total nitrogen was determined by a modification of Kjeldahl's method in the dry matter.

The nitric nitrogen was determined in the juice by Schlœsing's method.

The amide and ammoniacal nitrogen was calculated from the above by difference.

The results are given in the annexed table.

Table of Results of Mangold Analyses.

Date of analysis	Total nitrogen		Albuminoid N.		Nitric nitrogen		Amide and ammon. N.	
	Per cent. in root	Per cent. of total N.	Per cent. in root	Per cent. of total N.	Per cent. in root	Per cent. of total N.	Per cent. in root	Per cent. of total N.
October 12 ..	·183	100	·055	30·0	·050	27·3	—	42·7
December 1 ..	·241	100	·113	48·4	·048	19·8	—	31·8
February 1 ...	·238	100	·075	32·1	·035	14·7	—	53·2
March 10 ...	·207	100	·061	29·3	·026	12·6	—	58·1
,, ,, ...	·204	100	·053	25·9	·028	13·5	—	60·6
April 20[1]	·277	100	·047	17·3	·023	8·3	—	74·4
,, ,, ...	·250	100	062	24·8	·022	8·8	—	66·4
May 20	·233	100	·056	24·0	·022	9·4	—	66·6

[1] This root was rotten inside and hollow.

Two questions appear to me to arise from these analyses:

(1) Is the disappearance of the nitrates due to a denitrifying action of the cells, or to bacterial action?

(2) Is not this disappearance of the nitrates during storage answerable for the fact that mangolds are more suited for food after January than in the autumn?

I purpose repeating these experiments on a larger scale next winter.

T. B. WOOD.

Agricultural Department, University of Cambridge.

Globular Lightning.

THE following appears to be an instance of so-called "Globular Lightning" (NATURE, vol. xl. pp. 296, 366, 415, &c.).

During the thunderstorm of July 20, with which the drought broke up, an elderly man, Thomas Smith, residing in this parish about half a mile from the railway station, was watching the lightning from his cottage door, between 5 and 6 p.m., when he noticed a white ball, "about the size of an egg," dancing about in the air "like rooks when at play." He watched it through the intervals between two or three lightning flashes, therefore during several seconds. After some interval (perhaps a few minutes), he still standing at the door, his wife just coming down the stairs to him, something seemed to pass between them which felt hot to their faces. Simultaneously Miss Downes, schoolmistress, sitting on the landing above the stairs, felt something hot pass her hair behind, and then in a small bedroom, with open door adjoining, a loud detonation took place; whitewash from the ceiling covered bed and floor, the wall-paper was torn, the plaster fissured, and the house filled with a "sulphurous" smell.

There is a draught up the stairs, but no apparent reason why what it brought should enter the little bedroom. The cottage stands alone, on high ground, but not the highest, and there is nothing exceptional in its construction or its surroundings.

Cockfield, Suffolk, July 22. E. HILL.

"Bicycles and Tricycles."

IN reading Mr. Boys' most interesting review of Mr. Sharp's book on "Bicycles and Tricyles" (NATURE, July 8), a few points occur to me as requiring further notice.

In Chapter xi., on bending, the *strengths* of tubes of various sections are compared by considering the modulus (Z) of each section, while Mr. Boys speaks of *stiffness*, and mentions that D tubing is 1 per cent *stiffer* than round tube of the same weight and width. I venture to think that, in stating this, Mr. Boys has overlooked the fact that, since the D section is unsymmetrical about the longer axis, the ratio of its moment of inertia to that of round tube $\left(\frac{I_D}{I_0}\right)$ is much greater than the ratio of the moduli $\left(\frac{Z_D}{Z_0}\right)$.

Thus the relative stiffness of D tube $\left(\frac{\cdot 1542}{\cdot 1250} = 1\cdot 23\right)$ is much greater than its relative strength $\left(\frac{\cdot 252}{\cdot 250} = 1\cdot 008\right)$.

These values assume the D section to be semicircular, and the tubes to be infinitely thin; when the thickness is finite, the round tube must have the thicker wall, and the mean diameter becomes less than the mean radius of the semicircle, so that the ratios become greater still.

In practice the semicircular section is seldom used, a much more square-shouldered shape being preferred, which, whilst only slightly less stiff than rectangular tube of the same weight, width and perimeter, is much less unsightly in appearance. The actual advantage of D tube over round is found by experiment to be from 30 per cent. to 40 per cent.

It may be well here to draw attention to tests of various fanciful sections (webbed, corrugated, &c.), which are published from time to time, showing great apparent advantages over plain tubing. In every case the test is made by supporting the tube at the ends, and applying the load at the centre by means of the ordinary knife-edge used for testing solid bars. The result is that the tube wall is crushed in long before the real limit of bending moment is reached, and the test merely indicates resistance to local denting. To avoid this the tube should be as long as possible, and the load distributed over a large area.

Mr. Sharp's book is the first serious effort that has been made to bring the cycle-maker into line with the rest of the engineering profession.

R. H. HOUSMAN.

Mason College, Birmingham.

I AM obliged to Mr. Housman for pointing out a slip of expression in my review of Mr. Sharp's book. I had not confused the ratio of the I's for the ratio of the D's, but merely inadvertently used the word "stiffness" in its colloquial and more extended sense, so to include resistance to forces which would seriously bend or damage, as well as to those which would produce infinitesimal bending.

While on the subject of strength or stiffness of thin tubes, it may be worth while to point out that the complete theory of bending, as applied to very thin tubes, is by no means included in the usual formulæ; and it is for this reason that properly designed experiment is essential in extreme cases.

C. V. BOYS.

"A Text-book of Histology."

MY attention has been drawn to a review of my work on histology, which appeared in your issue of May 20; and as the review appears to me to be biassed, and amounts in fact to a public attack on me as an histologist, I trust you will, in fairness, give me the opportunity of as publicly defending myself, by inserting this letter in your next number.

Not one of the charges brought against the book can be fairly substantiated. Your space will not admit of my traversing more than a few of the points raised, but these will serve as samples of the whole.

(1) Your reviewer refers to a well-known excellent atlas of histology, and contrasts the accuracy of the drawings with mine. I deny the justice of his comparison: to take instances, if any one will compare the drawings of the eye and cochlea in my text-book with those of the same structures in the atlas, he will speedily be convinced as to where the inaccuracy lies. If the descriptions in the atlas referred to contained much that was new at the time, I can only say that histology must have

been previously in a very bad pass indeed; but, as far as I know, there is very little that was new even then, except the portion of the work dealing with the lymphatics.

(2) Your reviewer appears not to have apprehended the purpose for which the book was written, viz. to help the ordinary student. To endeavour to teach him how to trace degeneration of nerve fibres "by the invaluable method described by Marchi" is laughingly absurd to any one who has had much acquaintance with histological classes.

(3) Staining in bulk is by no means omitted, the process being carefully described in the chapter on methods, and referred to again and again in the directions for preparation of sections given in the appendices.

(4) I fear your reviewer will "look in vain" again in the second edition of the book for "the methylene-blue method of Ehrlich for showing nerve endings"; for I shall not forsake the principle I have clearly laid down, that it is useless and inadvisable to preach to students what it is impossible, or at least improbable, that they can practise. The work has no ambition to be an up-to-date histologist's *vade-mecum*.

(5) To say that I state that the process of staining with silver nitrate solution "requires from a few hours to a day or two" is to give an entirely false impression. It is distinctly stated that the tissue requires to be subjected to the reagent for from ten to twenty minutes, and then exposed to daylight "for a few hours to a day or two"—a totally different thing. This is quite sufficient, I think, to demonstrate the unfairness of this part of the review.

(6) The statement in the concluding paragraph that the book is "acknowledgedly compiled from other sources" is absolutely untrue. The usual acknowledgment of indebtedness to current literature is made, and the immediate source is given of some of the formulæ; the latter, however, being as much public property as the dates in English history.

(7) Your reviewer is inconsistent in saying at one time that the drawings are "sadly lacking in accuracy," and at another that "they will rejoice the heart of the average student, who will find them just like his specimens." To say that a student would rejoice over a "gaudy" coloured, uninstructive drawing, lacking in accuracy, and having only a superficial resemblance to his specimen, is not only insulting to his intelligence, but is childish in the extreme.

In conclusion: your reviewer charges the book with inaccuracy in the drawings, and also in the text. I take this to mean that both the text and the drawings, as a whole, are inaccurate, because he does not qualify his hostility by one good word from beginning to end. I deny that he can substantiate his charge. I challenge him to do so as publicly as he has made it.

I am glad to say that your review in its unfavourableness stands alone. The rest of the press, both lay and scientific, has spoken well of the work, and I am sure the editor of NATURE will not be under the impression that that valuable paper is the only one enjoying the services of experts for scientific reviews.

ARTHUR CLARKSON.

Marischal College, Aberdeen, June 29.

IN reply to the above, I beg to assure Dr. Clarkson that the bias of which he complains is solely the result of a critical examination of his book. I have no personal knowledge of him, nor any previous reason for thinking ill of him. I will take his paragraphs in succession:—

(1) Comparison with Klein and Noble Smith's "Atlas of Histology." Dr. Clarkson's temerity in endeavouring to put his book on a par with this classical work, which teems with original observations, and the illustrations to which are drawn with the most minute attention to detail, will raise a smile on the lips of every histologist. He is particularly unfortunate in calling attention to his illustrations of the eye and cochlea, which are vastly inferior to those in the "Atlas," although in the sixteen years since that work was published there has been an enormous advance in our knowledge of the structure of these parts, and notably of the retina. I fail to find a sign of this advance either in the text or illustrations.

(2), (3), (4) That the book is intended for the "ordinary student" (I presume that by ordinary student "medical student" is intended), and does not, therefore, require (to use the author's own language) to be "up to date." Dr. Clarkson seems to be under the impression that there is a special kind of scientific knowledge desirable for medical students, and that it is therefore unfair to have judged his book by a rigid scientific standard. I, on the other hand, hold that a book which is sent to a scientific journal for review must be judged on its scientific merits, and must stand or fall upon these. And if I find two of the most valuable modern methods of investigating the structure of the nervous system omitted, and venture to point out their omission, Dr. Clarkson does not, in my judgment, improve his position by the statement that he has purposely committed this blunder, and that it is his intention to perpetuate it.

There may be a "careful description of the process of staining in bulk," but I have failed to find it. There is no mention of Heidenhain's method, which is largely used in all laboratories.

(5) Dr. Clarkson convicts himself, in having misapprehended my criticism. It is precisely the statement that after silver nitrate a tissue "requires from a few hours to a day or two" exposure in water to daylight that I animadverted upon. Every histologist knows, or should know, the detrimental effect of prolonged exposure to light of such preparations.

(6) It is a sufficient answer to this to give Dr. Clarkson's own words. He says in the preface: "The author would acknowledge his indebtedness generally to the current standard works on the subject; and especially to Prof. Stirling's 'Outlines of Histology' for many of the formulæ of reagents." To this I would, however, add that many points besides the formulæ of reagents have a singular resemblance to corresponding points in Stirling, to say nothing of the other "current standard works" to which no name is appended; and, on the other hand, if there is anything original either in the way of descriptions or methods, I at least have been unable to find it.

(7) I have not made merely a general and unsupported accusation of inaccuracy, but I have given specific instances, which might easily be multiplied were it worth the space they would occupy. Since Dr. Clarkson has in his letter made no attempt to explain these, I take it that he admits their justice, and his public challenge becomes a vain piece of bombast.

Finally, I would add that the fact that the rest of the press has spoken favourably of Dr. Clarkson's work is simply an indication that notices of such books are far too frequently drawn up in a careless and perfunctory manner. The injurious effect which such promiscuous eulogy may have upon an author is only too evident from the tone of Dr. Clarkson's letter.

THE REVIEWER.

A Phenomenal Rainbow.

A VERY beautiful rainbow was observed here on the evening of May 26 last, just before sunset. A light easterly air prevailed at the time; but the thin bank of stratus cloud upon which the bow was projected had drifted slowly across from the south-west, and now hung in the eastern sky. The sun was quite low at the time, and during the last two or three minutes before setting was shining through a thin layer of stratus which lay just above the horizon; but there was no apparent diminution in the startling vividness of colour exhibited in the arch. This extraordinary brightness, however, was not the only noticeable feature; immediately below the great arch, and contiguous to it and to each other, were four narrow arches, in which the vivid colours were repeated; these did not reach the horizon, but faded when about three parts of the way down. There was also, some distance above the main arch, a secondary bow, with the four narrow arches appearing again; but here, instead of being below, they were directly above the arch, and, of course, not so bright as the primary set. The whole appearance was curiously like some of the solar phenomena observed in the Arctic sky, and was so beautiful as to attract the attention of several working bushmen, who are not prone to fall into ecstacies over any natural wonders.

The appearance lasted about five minutes, until the sun was below the horizon; a light shower fell at the time. For some days previously the weather had been thundery and unsettled, with variable winds.

H. STUART DOVE.

Table Cape, Tasmania, June 1.

Fire-fly Light.

IN reply to Prof. S. P. Thompson (p. 126), the insect called in German *Johanniskäfer* or *Johanniswürmchen* is certainly the *Lampyris noctiluca* (glow-worm), of which only the female, which has no wings, is luminous.

But Prof. Muraoka, in *Wiedemann's Annalen*, describes, as *Johanniskäfer*, a Japanese insect which undoubtedly is not the *Lampyris noctiluca*, but a luminous flying insect, very abundant at the end of June. Therefore it would be a *Luciola*, but a little larger than our famous *Luciola italica*, which appears very numerously in all Italy at the end of June.

Gemminger and Harold mention in Japan two *Luciolæ*: *Luciola japanica*, Linn., and *Luciola chinensis*, Thunb., but no kind of *Lampyris*.

CARLO DEL LUNGO.

R. Liceo Galileo, Florence, Italy, July 11.

THE EVOLUTION OF STELLAR SYSTEMS.[1]

ABOUT a century ago Laplace presented to the world an hypothesis concerning the mechanics of the heavens, basing it on sound dynamical principles, and working it out with that genius which he alone at that time could bring to bear. This hypothesis, grand and general as it was and still is, has made his name familiar to every student of astronomy of to-day; and the equipment of a modern observatory enables us to observe more minutely the stellar systems (which he could not see, but only imagine), and wonder at his far-reaching mind in expounding such a simple scheme of evolution for them. Modern investigations have necessitated, however, a modification of Laplace's original hypothesis. In his time the view was held that figures of equilibrium of rotating bodies were necessarily surfaces of revolution about the axes of rotation, but thanks to the mathematical researches of Jacobi, Darwin, Poincaré, &c., this is found now not to be universally true. To-day, for instance, if we consider the revolution of two separate fluid masses so close to one another that they are caused to coalesce and form a rigid system, through tidal distortions, then the form of the resulting mass will be dumb-bell shaped, approximating to Poincaré's apioid. It is regarding the mutual reaction of two such bodies as these that the author of the volume under consideration has recently made mathematical investigations, and he has not limited himself to the purely mathematical side of the problem, but has extended the view to the stars in space, which according to the ideas now held are not solid bodies, but masses of matter in which tidal action can have full play. It seems exceedingly probable, he says, "that the great eccentricities now observed among double-stars have arisen from the action of tidal friction during immense ages: that the elongation of the real orbits, so unmistakably indicated by the apparent ellipses described by the stars, is the visible trace of a physical cause which has been working for millions of years. It appears that the orbits were originally nearly circular, and that under the working of the tides in the bodies of the stars they have been gradually expanded and rendered more and more eccentric."

Dr. See, in the first of the three chapters which composes this volume of more than 250 pages, gives a short historical sketch of double-star astronomy from the time (1779) of Sir William Herschel down to that of Mr. Burnham. The next three sections are devoted to the solution of several problems referring to Laplace's demonstration of the law of attraction in the planetary systems, investigation of the law of attraction in the stellar systems and the analytical solution of Bertrand's problem based on that developed by Darboux, together with the solution given by Halphen. The three following sections treat of problems which Dr. See has previously published in the *Astronomischen Nachrichten*. In the first of these he develops the theory by which, by a simple spectroscopic observation, the absolute dimensions, parallaxes, and masses of stellar systems may be immediately ascertained assuming the orbits are known from micrometrical measurement. In a later chapter he points out how this method may be applied to the best-known doubles. Those most suitably situated for such measurements of relative motion are: η Cassiopeæ, α Canis Majoris, 9 Argûs, ξ Boötis, γ Coronæ Borealis, Σ 2173, 70 Ophiuchi, β Delphini, and α Centauri. The second section gives us a means of rigorously testing the law of gravitation by comparing the observed motion in the line of sight of a companion with the theoretical value.

Sections 8–12 are devoted to a survey of the chief methods of determining the orbits of binary stars. Among these attention may be drawn to a very simple graphical process of finding the apparent orbit from the given elements. Dr. See also properly brings to the fore that admirable graphical method of solving Kepler's equation which was originally invented by J. J. Waterson, and subsequently rediscovered by Dubois. This method, which Klinkerfues describes in his treatise on theoretical astronomy, and which is used by many continental astronomers, is suited to ellipses of all eccentricities, and can be applied, by the addition of a simply determined correction, to the orbits of comets and planets, giving all the accuracy required.

As regards chapter ii. much could be written, since this part of the volume extends over 178 pages out of the 258, and is of great importance. The author has brought together the detailed researches on the motions of the forty stars whose orbits can be best determined at this epoch. For the completion of this work Dr. See has been able to obtain measurements by double-star observers which have not been previously published, and by this means he has carefully determined independently the orbits of these forty doubles, a piece of work which must have involved an immense amount of labour. In the case of each binary are given the observed measures up to date, the previously determined elements and his own elements, and a comparison of the observed with the computed places. There further follow an ephemeris up to the year 1900, and sometimes up to a later epoch, general remarks on the binary in question, and in each case a plate showing the apparent orbit and the positions of the observed companion.

As an illustration of one of the orbits, we may mention that of 70 Ophiuchi, as this is of special interest since the motion of the comparison indicates that a third body is probably in question. Several investigators have worked out the orbit of this double, but there seems always to have been a certain amount of dissatisfaction about the resulting ellipse. The figure shows very clearly the wavy line of motion of the observed with the computed position. Prof. Schur, who made a most rigorous investigation of this binary in 1868 and 1893, discussing 400 observations in the latter year, inspired the belief that at length a definite orbit was obtained, but subsequent comparison of the observed with the computed positions indicated that there must probably be an unseen body disturbing the elliptic motion. Prof. Burnham, who has specially searched for this third perturbatory body, has as yet failed to see it, although he has used the 36-inch Lick refractor in the attempt.

Coming now to the third and last chapter, Dr. See sums up the results of the researches on these forty binaries. A general glance at the table shows that the elements T, a, ☊, i, λ have no relation to physical causes; but, in the case of the eccentricities, "a most remarkable law" is established, which is "of fundamental importance in our theory of the origin and development of the stellar systems, and is besides of practical value to working astronomers." Perhaps the following table will best show the number of orbits corresponding to different eccentricities:—

[1] "Researches on the Evolution of the Stellar Systems. Vol. i. On the Universality of the Law of Gravitation and on the Orbits and General Characteristics of Binary Stars." By T. J. J. See, A.M., Ph.D. (Lynn, Mass, U.S.A.: The Nichols Press, 1896.)

Eccentricities between	No. of orbits.
0·0 and 0·1	0
0·1 „ 0·2	2
0·2 „ 0·3	4
0·3 „ 0·4	8
0·4 „ 0·5	9
0·5 „ 0·6	9
0·6 „ 0·7	2
0·7 „ 0·8	4
0·8 „ 0·9	2
0·9 „ 1·0	0

Mean average value of the eccentricity of the forty binaries, 0·482.

The author thus points out that binaries are distinguished from the planets and satellites in two very distinct respects, namely:

(1) The orbits are highly eccentric.

(2) The stars of a system are comparable, and frequently almost equal in mass.

Dr. See gives a series of illustrations of the orbits arranged in the order of their eccentricity, and remarks that while these are more eccentric than those of the planets and satellites, they are much less eccentric than those of the long-period comets.

The reason why these orbits came to be so eccentric the author evidently leaves to a second volume, as he says that hereafter we shall see that the orbits were originally circular.

In Dr. See's concluding remarks, he points out that these double systems stand in direct contrast to the planetary systems, in which the masses of the components are not in the proportion of two to one, or equal, but where the central body has 746 times as much mass as all of the planets combined.

It is true that investigators, as Dr. See remarks, have always approached the problems of cosmogony from the point of view of our solar system, and have not given sufficient attention to systems of the double or triple star type. This is probably owing to the fact that double star astronomy is practically very modern, and that those investigators were not aware that the telescope would reveal such innumerable systems of double and triple stars as we now know to exist in the heavens. It is further natural that we should consider our system in the first instance a common type of the celestial ones, until it is proved on the contrary to be otherwise. Indeed, such a system as ours need not be in any case an exceptional one in space; looking at a similar one in the heavens, we should most probably only be able to see the central body the sun, in consequence of the smallness of the components (the planets) revolving round it.

It seems likely that such a system would be more easily observed when in the nebulous stage, as, for instance, in those spiral nebulæ which have central nuclei very large compared with the smaller condensations scattered along the outliers.

In conclusion, we may say that we have nothing but praise for this book. By its publication double-star astronomy is greatly enriched, and every double-star observer and computer will find it a valuable addition to his library.

Not only will the exposition of the modern methods of computation of such systems add greatly to its usefulness, but a mine of valuable information regarding the previous researches on the best-known members of double-star systems is brought together in one volume.

William J. S. Lockyer.

THIERRY WILLIAM PREYER.

To our readers the announcement of the death of this distinguished physiologist will come with surprise. To those who knew Preyer it might have seemed as if he, with that appearance of overflowing vigour, might have looked forward to a long lease of life. It was otherwise, for Preyer died at the comparatively early age of fifty-six of Bright's disease.

Preyer was born in Manchester in 1841, and, after studying in London, he, like most German students, attended several universities, including Bonn, Berlin, Vienna, Heidelberg, and Paris. In 1862 he took the degree of Doctor of Philosophy and that of Doctor of Medicine in Bonn in 1865. In Bonn he was brought under the influence of Max Schultze; in Berlin Helmholtz, Du Bois, and Virchow inspired him with a desire to become a physiologist, while later he worked under Bernard in Paris.

He commenced his independent scientific career as a "privat docent" in Bonn in 1865, and shortly afterwards, in 1869, he was appointed Professor of Physiology in Jena, where his best work was done.

His energy was unbounded, his enthusiasm unquenchable, and so he set to work and had erected the well-known physiological institution in Jena, where he remained until a few years ago, when he resigned his chair, and went to Berlin, where he remained some time, and then retired to live in Wiesbaden.

Preyer's name will always remain associated with his work on hæmoglobin, a work inspired partly by the researches of the Berlin School. The many-sided view of his genius found its expression in the very diverse subjects in which he worked and wrote.

His well-known work "Die Seele des Kindes" (1882) was a study of the mental development of his own child; it amplified and extended the less extensive observations of Darwin.

In the 'seventies his researches were chiefly acoustical, and to-day there exists in the Jena Institute an extraordinary collection of acoustical apparatus which he used for his researches.

About the same time (1877) he published his researches on the cause of sleep.

His "myophysical law" was not so well received by physiologists. Many of his papers and those of his pupils are published in his "Sammlung Physiologischer Abhandlungen" (1876-80), in which will be found his most interesting observations on hypnotism and an allied subject which he called "Kataplexie." Whatever may be thought of his theory, his observations stand, and only this year Verworn, of Jena, has again taken up the subject, and published some most interesting results of "Kataplexie" in serpents. Perhaps Preyer's attention was directed to hypnotism by the works of Braid, of Manchester, which he translated.

As showing the peculiar character of Preyer, and illustrating his never-ceasing quest after something new, we have his physiology of the embryo, which has been translated into French.

Preyer had a ready pen, he was a charming and attractive lecturer, and some of his popular discourses have had a wide circulation.

One of the most pleasantly written of his works is his "Elements of General Physiology," in which he gives a rapid, bold, and characteristic sketch of the development of this subject. This work was also translated into French.

Preyer was the very personification of buoyancy and good humour, and he had an open, frank expression which won for him friends on every hand. He visited England frequently, and those who heard him discourse at the Edinburgh meeting of the British Association are not likely to forget the intense impression he made on his audience, not only by the extraordinary array of facts with which he dealt, but also by the ease and fluency with which he spoke English. Preyer had many of the gifts of an orator, and when his perfervid temperament was roused he carried his audience with him and brought conviction to their minds by the very ardour and force of discourse.

NOTES.

PROF. SCHÄFER asks us to state that members of the British Association who have arranged to go to Toronto must take this year's membership ticket with them in order to secure the railway privileges available.

GRANTS amounting to ninety-five thousand marks (4750*l.*) were awarded for scientific purposes at the last meeting of the Berlin Academy of Sciences. Among these awards were the following:—2000 marks to Prof. Engler, Berlin, for the publication of monographs on African plants; 900 marks to Dr. G. Lindau, for his studies of lichens; 1500 marks to Prof. Frech, for his geological investigations; 850 marks to Prof. Hürthle, for his studies of muscle; 800 marks to Prof. R. Bonnet, for investigations of blood-vessels; 2000 marks to Dr. Lühe, for investigations of the fauna of salt lakes in French North Africa; 300 marks to Dr. G. Brandes, for the study of parasitic Nemertines in Messina; 500 marks to Dr. R. Hesse, for the investigation of eyes of lower marine animals, especially molluscs, at the Naples Zoological Station; 1500 marks to Prof. E. Cohen, for his researches on meteorites; 1500 marks to Dr. Ludwig Wulff, to continue his investigations on artificial crystals; 35,000 marks to Prof. F. E. Schulze, in support of the publication of "Das Tierreich," by the German Zoological Society.

UPON the outer wall of the Pasteur laboratory at the École normale, Paris, a bronze medallion, reproducing with great truth the features of the great investigator, has just been placed. The medallion is the work of M. A. Patey, and it surmounts a slab of black marble bearing in letters of gold the following inscription:—

Ici fut le laboratoire de Pasteur.
1857.—Fermentations.
1860.—Générations spontanées.
1865.—Maladies des vins et des bières.
1868.—Maladies des vers à soie.
1881.—Virus et vaccins.
1885.—Prophylaxie de la rage.

This memorial was voted by the Municipal Council of Paris in December 1894 to commemorate the work done by Pasteur in the laboratory in the rue d'Ulm, now occupied by the École normale.

AN important change in the administration of the U.S. National Museum is announced in *Science*. Three sections have been organised—a section of anthropology, a section of biology and a section of geology, each having a head curator with an annual salary of 3500 dollars. Dr. W. H. Holmes has been appointed head curator of anthropology; Dr. Frederick W. True, head curator of biology; and Dr. George P. Merrill, head curator of geology. Dr. True and Dr. Merrill are already connected with the Museum, and it is expected that Dr. True will continue to act as the executive curator. Dr. Holmes, who leaves the Field Columbian Museum, Chicago, to accept this position, was formerly connected with the U.S. Geological Survey and the Bureau of Ethnology.

DR. ALFRED M. MAYER, professor of physics at the Stevens Institute of Technology since the foundation of that institute, died on July 13, aged sixty-one years. He was a Member of the National Academy of Sciences, and one of the original Fellows of the American Association for the Advancement of Science.

THE statue of Darwin that has been erected by the Shropshire Horticultural Society at the entrance to the Public Library and Museum, the former school buildings, of Shrewsbury, will, says *Natural Science*, be unveiled by Lord Kenyon, President of the Society, on August 10. The statue, which is of bronze on a granite pedestal, is the work of Mr. Horace Montford, of Shrewsbury, and is not wholly unlike the fine statue in the Natural History Museum, South Kensington.

THE Council of the British Institute of Preventive Medicine have appointed Dr. Allan Macfadyen Director of the Institute.

THE British Medical Association has awarded the Stewart Prize of 50*l.* to Dr. G. Sims Woodhead, and the Middlemore Prize of 50*l.* to Dr. Alexander Hill. Sir Walter Foster and Mr. C. G. Wheelhouse have been awarded the gold medals for distinguished merit.

CARBIDE of calcium not exceedingly 5 lbs. in quantity, when stored in separate, substantial, hermetically-closed metal vessels, containing not more than 1 lb. each, may now be kept without a licence, the Secretary of State having been advised that such small quantities might be safely exempted from the operation of the Order of Council of February 26, in which certain parts of the Petroleum Acts were applied to the substance.

THE preliminary programme of the sixteenth congress of the Sanitary Institute, to be held in Leeds, from September 14 to 18, has now been issued. The president of the congress is Dr. Robert Farquharson, M.P. The congress will include three general addresses and lectures. The sections will meet for two days each, and deal with (1) sanitary science and preventive medicine, presided over by Mr. T. Pridgin Teale, F.R.S.; (2) engineering and architecture, presided over by Mr. Lewis Angell; (3) chemistry, meteorology, and geology, presided over by Mr. William Whitaker, F.R.S. There will be six special conferences, the subjects and presidents of which will be "River Pollution," Major Lamorock Flower; "Municipal Representatives," Councillor B. Wormsley; "Medical Officers of Health," Dr. Edward Seaton; "Municipal and County Engineers," Mr. Thomas Hewson; "Sanitary Inspectors," Mr. Peter Fyfe; "Domestic Hygiene." In connection with the congress a health exhibition of apparatus and appliances relating to health and domestic use will be held. Excursions to places of interest in connection with sanitation will be arranged for those attending the congress. It appears from the programme that over three hundred authorities, including several County Councils, have already appointed delegates to the congress, and, as there are also over two thousand members and associates in the Institute, there will probably be a large attendance in addition to the local members of the congress.

AT a very numerously attended meeting of the Essex Field Club, held at Easton Lodge, near Dunmow, by the invitation of the Earl and Countess of Warwick, on Wednesday, July 21, a discussion was held for the consideration of practical methods for the protection of our native fauna and flora from the destruction and actual extermination which now threaten many interesting species. Mr. C. G. Barrett (hon. secretary to the Committee of the Entomological Society for the Protection of Insects in danger of extermination) opened the subject by a short address on "Insect protection: its necessity, means, and objects." Mr. J. E. Harting spoke with respect to birds and mammals; Prof. Boulger referred to the wholesale collecting which was exterminating many rare plants; and Prof. Meldola urged that children should be taught to respect the sacredness of life. The President, Mr. David Howard, strongly supported the pleas of the speakers, and Mr. W. Cole hinted at the injury that might be caused by legislative interference with the balance of nature. Eventually the following resolution of Prof. Boulger's was adopted unanimously, and the Club resolved to assist the scheme of the Entomological Society in every possible way: "That in view of the danger of extermination threatening many beautiful, rare and interesting plants, all lovers of nature should do their best to avoid this danger (*a*) by abstaining from wholesale collecting, collecting for merely individual private collections, needless rooting-up of specimens, attempting to cultivate wild specimens of refractory species, and purchasing such wild specimens from itinerant or other dealers; (*b*) by endeavouring to persuade others, especially school children, cottage gardeners, and dwellers in large towns, to a similar abstention." It is to be hoped that other local natural history societies will follow the example of the Essex Field Club, and bring this pressing matter prominently before their members.

THE first section of an interesting Russian expedition which was at work this spring, in Bukhara, under Colonel Kuznetsoff, has just returned to Tashkend. Although the Khanate of Bukhara is open to Europeans, and the Russian Turkestan Railway crosses it, the Khanate remains one of the least-known parts of Central Asia as regards its population and economical conditions. The new expedition had precisely these studies in view. It has visited the chief towns of Bukhara, as also the desert portion of the banks of the Amu for 190 miles, between Pata-ghissar and a lonely village Sarai, and has explored the region of Sarygor. It returns with valuable materials, and with a great number of photographs.

A SCIENTIFIC expedition was sent to Central Borneo, in 1893, by the Dutch Society for the advancement of natural history exploration in the Dutch colonies. Prof. Molengraaff went out as geologist, Dr. Hallier as botanist, and Dr. J. Büttikofer as zoologist, while Dr. Nieuwenhuis undertook the study of the anthropology and ethnography of the natives. A concise description of the field of exploration and a general statement of the zoological results is contributed to *Notes from the Leyden Museum* (vol. xix., published July 15) by Dr. J. Büttikofer. In the same publication, Dr. F. A. Jentink describes the mammals collected during the expedition.

THE most important matters which the Trustees of the South African Museum refer to in their report for the year 1896 are the reorganisation of the staff, and the completion of the new buildings recently described in NATURE (p. 31). The scientific staff of the Museum now stands as follows:—Mr. W. L. Sclater, Director and Keeper of the Department of Vertebrates; Mr. L. Peringuey, Assistant-Director and Keeper of the Department of Insects; Dr. W. F. Purcell, Assistant and Keeper of the Department of Land Invertebrates; Dr. G. S. Corstorphine, Keeper of the Department of Geology and Mineralogy; Dr. J. D. F. Gilchrist, Honorary Keeper of the Department of Marine Invertebrates. The total number of specimens added to the collections during the year was 8009, of which 900 species were new to the Museum. Evidence of increasing interest in the Museum is shown by the fact that the number of visitors in 1896 was 49,419, this being an increase of nearly eleven thousand upon the number of the previous year, and by far the highest number yet registered. Mr. Peringuey refers to the discovery, by Surgeon-Major Bruce, that the dreaded Tsetse-fly (*Glossina morsitans*) is a larviparous insect, *i.e.* it gives birth to a full-grown larva, which, very shortly after being extruded, pupates, the external skin or puparium hardening and assuming an ovate shape, auriculate at one end. This discovery, so much at variance with what is known of the life-history of kindred dipterous insects, was at first doubted, but Mr. Peringuey says he can authenticate it, for he has bred from puparia, sent to him by Sir Walter Hely-Hutchinson, the Governor of Natal, what is undoubtedly the *Glossina morsitans* of Westwood.

THE vast and sudden changes of wind-velocity occurring in stormy weather, which were revealed by Prof. Langley's observations published in 1893 in his essay on "The Internal Work of the Wind," have been made the subject of a novel investigation at the hands of Dr. Romei Martini. In the *Rendiconti del Reale Istituto Lombardo*, Dr. Martini describes a series of observations on the periodical and other variations in the level of water in wells, and has proved the existence of rapid fluctuations corresponding very closely with the fluctuations of wind-pressure during a storm, of which latter he also gives diagrams closely resembling Langley's. The old and well-known tradition, that a sudden rise or fall of water in wells may be used to predict bad weather, has thus received explanation; but the most remarkable feature is that while small and rapid fluctuations of pressure readily make their influence felt on the instrument for registering the level of the well, large variations in the barometer are much more uncertain in their action.

IN the *Monthly Weather Review*, considerable attention is now being given to kites and their use in exploring the upper air. The number in which the weather of April 1897 is discussed, contains a long monograph by Prof. C. F. Marvin dealing with the theory of the mechanics and stability of kites. In addition, the editor (Prof. Cleveland Abbe) gives some interesting historical facts relating to the early history of kites, and a controversy as to the earliest use of wire for kite-lines shows that experiments with wire were made in 1836 at Philadelphia, and in 1844 by Mr. Joshua Law in England. We also learn that just as the European meteorological bureaus have taken up sounding balloons as a means of exploration at great altitudes, the United States Weather Bureau has prepared kites to cover a great horizontal extent of territory with automatic meteorological instruments. While the European system is designed for special occasional work at 50,000 feet altitude, the American system of kites contemplates regular daily work at 5000 feet. Probably both systems will in time supplement each other.

THE current number of the *Zeitschrift für Hygiene* contains an elaborate memoir by Dr. Max Neisser, of the University of Breslau, on the correct diagnosis of diphtheria bacilli. Amongst the many interesting and important points investigated, considerable attention is directed to the production of acid in culture media by these bacilli. This has long been regarded as a characteristic feature in the growth of diphtheria bacilli; but Dr. Neisser has made a new departure in estimating the amount of acid elaborated, quantitatively. It appears that during the first nine hours no increase was observed, but at the end of the first day a considerable quantity was discovered, and that it materially increased in the course of the second day; after that period, however, no further rise was recorded. So far Dr. Neisser has only met with one variety of bacillus resembling that of diphtheria, which possesses this power of elaborating acids in such quantity. Some very interesting investigations are recorded on the rate of multiplication exhibited by diphtheria bacilli grown in serum, and then plate-cultivated in agar dishes for numerical determinations. The original number inoculated consisted of 1½ million diphtheria bacilli; after six hours this figure was transformed into 60 millions, after nine hours into 500 millions, and after twenty-four hours into 1100 millions. The growth in serum between the sixth and ninth hour after inoculation is, therefore, particularly prolific. In broth the multiplication is much slower, and at the end of twenty-four hours only about 120 millions of diphtheria bacilli were found, which, if contrasted with the 1100 millions recorded at the end of that time in blood-serum cultures, is very striking. Dr. Neisser's laborious investigations should prove of great value to all concerned in the bacterial diagnosis of diphtheria.

IN a recent number of the *Bulletin* of the Geological Society of America, Prof. Tarr describes some of the results of his study of the Greenland glaciers during the summer of 1896. In opposition to the views of other recent observers, he considers there is ample evidence of a former much greater extension of the ice. He points out that it is not safe to assume that a rugged peak has escaped ice-action, since there is plain evidence that certain peaks have been overwhelmed by the ice without losing their ruggedness. Two incidental discoveries given in this paper deserve special mention. One is the finding of recent marine shells in the boulder-clay of a moraine fifty feet above sea-level, and also in the ice itself of the glacier that had brought them there—a fact of great interest to British glacialists, in view of recent controversies. The second refers to the flora of a nunatak (Mount Schurman), regarded as but recently exposed above the ice: only light-seeded plants are said to have reached it as yet. A thorough study of the flora of nunataks would doubtless be as interesting as that of volcanic islands.

A VALUABLE paper has recently been published by Mr. C. S. Myers, giving "An account of some skulls discovered at Brandon, Suffolk" (*Journ. Anth. Inst.*, xxvi. p. 113). Brandon is the site of the famous flint quarries which are believed to have been worked continuously since the Neolithic period. In the vicinity are two Roman camps, and near by runs the Icknield Way, the great war- and trade-route of the Iceni in pre-Roman times. A few skulls resemble the "Neolithic" or "Long Barrow type." The skulls of brachycephalic series do not belong to the "Round Barrow type," which is quite unrepresented, but are to be allocated to a fairly widely-spread "Romano-British type." Among the elongated skulls Mr. Myers has proved the occurrence of the old "Row-Grave type" of Germany; it is a significant fact that about 372 A.D. the Alemannic Bucinobantes came from Mainz on the right bank of the Rhine, and appear to have settled, within twenty miles of Brandon, at Buckenham in Norfolk. Allied to these skulls is the long, low-crowned "Batavian type," which also occur at Brandon. Only one definitely Saxon skull was noted. The evidence points to the fact that the burial-ground whence these skulls were obtained was that of a people of mixed ethnic character belonging to a time antecedent to the Saxon Invasion; but it is probable that even then Saxon settlers were arriving in small numbers.

THE curious custom of trepanning—that is, of removing small pieces of bone from the living head—is very ancient and widely spread. In his recently-published book, "Prehistoric Problems," Dr. Munro has devoted a chapter to "Prehistoric trepanning and cranial amulets." About the same time, Drs. H. Malbot and R. Verneau published in *l'Anthropologie* (tome vii.) a memoir on the Chaouias and the trepanning of the skull in the Aurès. The Djebel-Aurès, "Mountains of the Cedars," form the south-east border of the Algerian plateau; here and in the neighbouring Djebel-Chechar is the centre of trepanning. The natives are carefully described; they belong to the Berber stem; a portion—perhaps one-eighth—are fair; thus the external, as well as the cranial, characters show them to be a somewhat mixed people. The method of trepanning is very fully described, and a native doctor showed Dr. Malbot a skull with over a dozen circular holes, two slits, and a large irregular orifice, all of which had been pierced when the man was alive! The skull, though taken from a grave, was kept hidden, and it evidently was used as an example by the local doctors. The enthusiastic French doctor says "the Chaouias respect their tombs, and on no pretext will rifle them; the love of science alone can explain this profanation on the part of our trepanner. It is the same sentiment which has led to our possession of the specimen." Dr. Malbot describes how he acquired the specimen which is now in the Museum d'histoire naturelle in Paris. The natives have recourse to trepanning for blows or wounds on the head; it does not matter how long before the blow may have been given, if only a sick person can remember that he has had one. The operation is by no means a severe one, as the people have a most remarkable recuperative constitution. A woman, tired of the conjugal yoke, has been known to call in the services of a trepanner in order to procure a divorce from her husband by producing a piece of her skull, which she affirmed had been broken by his ill-treatment.

THE useful services rendered by ladybirds in ridding fruit trees of insect pests were referred to in an article in NATURE of March 25 (vol. lv. p. 499). Further information upon the subject is given by Mr. C. L. Marlatt in the "Year-Book of the U.S. Department of Agriculture," in an article describing the various methods employed to combat the ravages of injurious insects in California, where the possibility of control of insects by introducing and fostering their natural enemies has been thoroughly tested. The very notable instance of the entire eradication of the white scale insect by the introduction from Australia of its ladybird enemy, *Vedalia cardinalis*, demonstrated the possibilities in this direction in the most striking way. This one experiment saved the State its citrus industry, and gave the greatest confidence in many quarters in this means of controlling insects, as well as incited the later action looking to the introduction of beneficial insects on a much larger scale. It led the State of California, in 1891, to grant 5000 dols. "for the purpose of sending an expert to Australia, New Zealand, and adjacent countries to collect and import into this State parasitic and predaceous insects." Mr. Albert Koebele, who had previously been instrumental in introducing *Vedalia cardinalis*, was selected for the work. His chief object was to obtain predaceous insects which might exterminate the black scale, the red scale, and the San José scale. Mr. Koebele's mission lasted upwards of a year, and during this time he imported into California probably 60,000 specimens, representing very many species, chiefly of ladybirds. Five or six of these species took hold well from the start, and two or three of them are still represented abundantly in the orchards of California, the others having practically disappeared. The important ones remaining include a very efficient predatory enemy of the black scale in the little *Rhizobius ventralis*, and two much smaller species, *R. debilis* and *R. toowoombæ*, which attack the black scale, and also the red scale and San José scale to a less extent. *Rhizobius ventralis* was easily colonised, and during the last three years has been distributed in enormous numbers to different parts of the State, 300,000 or 400,000 having been colonised in Southern California alone. This beetle is by far the most useful of the recent importations, and has already done much good; in several instances it has effected the entire eradication of the black scale in badly infested orchards. The disappearance of the scale may in some cases be due to other natural causes, but there seems to be no doubt that the chief credit belongs to the ladybirds. Once the ladybirds have established themselves in sufficient numbers, it seems best not to spray or fumigate the trees, as these treatments are very prejudical to the multiplication of this beneficial beetle.

ON Monday, August 2 (Bank Holiday), the Yorkshire Naturalists' Union will hold a meeting at Market Weighton, for the investigation of Everingham Park and surrounding woods and the low-lying Carr-land in the district.

WE have to acknowledge the receipt of "*Bulletin* No. 1 of the Free Museum of Science and Art, Department of Archæology and Palæontology, University of Pennsylvania." The object of the bulletin, which will be published four times a year, or as frequently as occasion may require, is the publication of new material acquired by the Museum, with accounts of explorations conducted by the Museum, and original investigations based upon its collections. The present issue contains articles by Dr. D. E. Brinton, entitled "The Pillars of Ben" and "The So-called 'Bow-Puller,'" besides brief notes on collections and publications.

THE following are among the papers and other publications which have come under our notice during the past few days:— "Life in Sewers," by Mr. H. A. Roechling, in the *Transactions* of the Leicester Literary and Philosophical Society (vol. iv. part iii., 1897). The paper is an instructive account of bacterial life found in the sewage itself and in sewer air, and the diseases produced by it.—A paper on the distribution and migration of Colorado birds, by Mr. W. W. Cooke, is published as *Bulletin* No. 37 of the State Agricultural College, Fort Collins, Colorado. The total number of species and varieties of birds which occur in Colorado is 360, of which 228 are known to breed. This is said to be a larger number of species than has been taken in any State east of the Mississippi, and is only exceeded by Nebraska with nearly four hundred species.—The tidal phenomena of the St. John River, New Brunswick, Canada, at low summer level, are described by Mr. A. Wilmer Duff in the *Bulletin* of the Natural History Society of New Brunswick (No. xv., 1897.) Dr. G. F. Matthew contributes to the same bulletin a long review of the scientific work of Dr. Abraham Gesner, the geologist.—The Pasteur Memorial Lecture delivered by Prof. Percy Frankland before the Chemical Society on March 25, and reported in NATURE at the time (vol. lv. p. 518), is printed in full in the July *Journal* of the Society, with an excellent portrait of Pasteur.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*) from South Africa, presented by Mr. W. H. Stather; a Brown Capuchin (*Cebus fatuellus*) from South America, presented by Mr. D. Mackintosh; a Harnessed Antelope (*Tragelaphus scriptus*, ♂) from West Africa, presented by Mr. R. B. Llewelyn, C.M.G.: a Vulpine Squirrel (*Sciurus vulpinus*) from North America, presented by Messrs. A. G. and R. Rawlins; a King Parrot (*Aprosmictus syanopygius*) from Australia, presented by Mrs. R. L. Turner; a Crowned Horned Lizard (*Phrynosoma coronatum*) from California, presented by Mr. C. H. Hastings; a Daudin's Tortoise (*Testudo daudini*) from the Aldabra Islands; a West African Python (*Python sebæ*) from West Africa, deposited; a King Vulture (*Gypagus papa*) from South America; a Bronze-winged Pigeon (*Phaps chalcoptera*) from Australia, purchased; a Peacock Pheasant (*Polyplectron chinquis*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

NEW OBSERVATIONS OF VENUS.—During the first three months of the present year, Dr. Eduardo Fontseré, of the Observatory of the Royal Academy of Sciences, Barcelona, made a series of observations of the planet Venus with a refractor of 11 cm. belonging to that observatory. An account of these observations is briefly described in the current number of the *Astronomischen Nachrichten* (No. 3430), and is accompanied by numerous figures illustrating the different surface markings that were recorded. The colour of the planet was noted as being of a yellowish green tinge, the most brilliant regions of the disc being less coloured than the others. The polar regions were not found to resemble those on Mars. Attention was paid especially to the dark and light spots on the disc, which were at times very conspicuous. We cannot here enumerate the various differences of shade observed, but we may mention the most brilliant noticed, namely, that situated near the south pole, and forming the letter X by the crossing of two arcs of circles. Dr. Fontseré classifies the bright regions into two divisions: those which are, to all intents and purposes, permanent, but of a variable nature, increasing and decreasing in relative brightness between certain limits; and those which appear like white trails always inclined towards the equator, but never parallel to it. As in the observations of Trouvelot, large deformations of the terminator were distinctly noticed, but are attributed for the most part to irradiation. The extremities of the horns were also found to be sometimes prolonged into the non-illuminated portion of the disc. The observations during these three months have indicated that, relatively to the sun, Venus has not undergone any rotation except as regards the small libration, which was exactly equal to that which corresponded to the change of geocentric latitude. The above-mentioned observations form a valuable contribution to our present knowledge concerning the telescopic appearance and time of rotation of this planet.

THE YERKES OBSERVATORY.—In the June number of the *Astrophysical Journal*, Prof. Hale brings to a conclusion his series of articles on the Yerkes Observatory and Telescope. The concluding article is elaborately illustrated, showing the telescope and dome in various stages of construction, the frontispiece illustrating the stage reached on May 11 last. We notice that the dedication of the observatory will take place on October 1 next, and it is hoped "that European men of science who propose to attend the Toronto meeting of the British Association in August, may think it desirable to take part in the formal inauguration of the Yerkes Observatory. . . . It is planned to hold a series of informal conferences on astronomical and astrophysical subjects. . . . A cordial invitation is hereby extended to all men of science who may be willing to honour the observatory by their presence on this occasion."

RESOLVING POWER OF SPECTROSCOPES.—In this column, on May 20 (p. 62), we referred briefly to Prof. Wadsworth's investigations on the question of the theoretical resolving power of optical instruments, in which he distinguished between four different cases. In this work he obtained formulæ which gave the three different resolving powers, namely, (1) p (theoretical) for a wide slit and monochromatic radiations; (2) R (limiting) for an infinitely narrow slit, but for lines of finite width $\Delta\lambda$; and (3) P (practical) for a wide slit and non-monochromatic radiations ranging for each line over a small value $\Delta\lambda$. In the current number of the *Astrophysical Journal* (vol. vi. No. 1), he now publishes tables which he has prepared, giving the values of p, R and P for values of r, which gives the value of the theoretical resolution of the instrument for an infinitely narrow slit and infinitely narrow spectral lines. In these tables r ranges from 25,000 to 1,000,000, $\Delta\lambda$ from 0·01 to 1·00 tenth metres, s (linear width of slit) from 0·005 mm. to 0·3 mm, and Ψ (angular magnitude of collimator as viewed from slit) from 1/40 to 1/10. All the values are computed for $\lambda = 5500$ tenth metres, this being the mean wave-length of the brightest part of the visible spectrum. Prof. Wadsworth adds also a complete explanation of the use of these tables, and numerous important remarks.

THE HORIZONTAL GYROSCOPE.—Attempts have several times been made to eliminate the use of the horizon when employing a sextant on board ships, by adopting mechanical or other means of determining the horizontal or vertical. Among these may be mentioned the pendulum in a collimator devised by Colonel Goulier, the mercury siphon of M. Renouf, and other ingenious devices suggested by Lejeune, Cardan, &c. None of these seems, however, to have come into practical use, and the mariner is still using the sextant in its ordinary form. Another rather novel mode of determining the horizontal is described by M. Gaspari in the *Journal de Physique* (vol. vi. p. 229). This idea was proposed by Rear-Admiral Fleuriais, and from all accounts seems to be of practical use. It consists in making a small addition to an ordinary sextant by mounting in front of the telescope and behind the small mirror a horizontal gyroscope which contains on its upper part two small plano-convex lenses, equal in all respects, placed a distance apart equal to their focal length. On the plane faces of these lenses are engraved two lines parallel to the equator of the gyroscope, and this plane contains their optical centres. The gyroscope is given a motion of rotation from 80 to 100 turns a second under some conditions. The horizontal is obtained by observing the position of the *locus* of these lines as the lenses revolve. To describe the method of working, and give an idea of the theory of the instrument, would occupy too much space in this column. We may mention, however, that the instrument has been used both on land and sea, and the officer who made the experiments "est arrivé à etablir que l'appareil est definitivement devenu pratique." With a telescope magnifiying from 3 to 4 diameters an approximation of 2′ was obtained under ordinary conditions of observation, but generally greater accuracy than this was secured.

SOME PROBLEMS OF ARCTIC GEOLOGY.

I. THE POLAR BASIN.[1]

UNTIL the return of the *Fram* from its epoch-making drift, the belief was almost universal[2] that the Arctic Ocean is a shallow, island-strewn sea; and the evidence for this view was thought to be so conclusive, that theories of Arctic geology might be safely based upon the hypothesis. Facts inconsistent with the theory were not unknown. Scoresby had let out two miles of line west of Spitsbergen, and Parry had sounded with a 500-fathom line at his furthest north, and on neither occasion was the bottom reached; Nordenskjold, in the *Sofia*, had found that the sea at 81° 32′ N. and 17° 30′ E. was 1300 fathoms deep. But this direct evidence did not shake the widespread belief in the general shallowness of the areas where no direct evidence was available. This theory was originally based on the notion that ice cannot be formed on the sea except where it is shallow; and after this view was proved to be mistaken, the old conclusion was maintained by various authorities from many different considerations. Thus Petermann, the great German Arctic geographer, deduced the extension of Greenland across the Pole to Wrangel Land near Behring Straits, from the distribution of drift-wood. It is well known that the Siberian rivers carry down vast quantities of tree-trunks, which float across the Arctic Ocean, and are cast upon its shores. But there is very little of this drift-wood in the Robeson Channel, and at the northern end of Smith's Sound. Petermann, therefore, concluded that there can be no direct sea communication between that strait and the coast of Northern Asia. While Petermann inferred the existence of a land connection between Greenland and the Asiatic islands from the discontinuity of the sea, Sir Allen Young, even as late as 1893, held that there is direct evidence of its actual existence; for the Governor of Upernivik, the most northern Danish settlement in Greenland, received from a native hunter a reindeer skin which had been branded with one of the marks used in Siberia; hence Young argued that this reindeer must have walked across from Siberia, which it could only have done along a line of continuous land, or, at least, a chain of islands. The existence of land to the north of Greenland was also maintained by Captain Tyson, the chronicler of the Hall Expedition, owing to the very moderate current that flows southward down Robeson Channel, which, it was said, could only be explained by the assumption that a northern archipelago acted as a weir. Again, Admiral Sherard Osborn contended that the sea to the north of the Parry Islands is a land-locked basin, as its characteristic icebergs never pass out through either Smith Sound or along the eastern coast of Greenland; and the occurrence of land to the north of Spitsbergen was asserted, as flocks of birds fly northward as if on their way to a safer breeding-place.

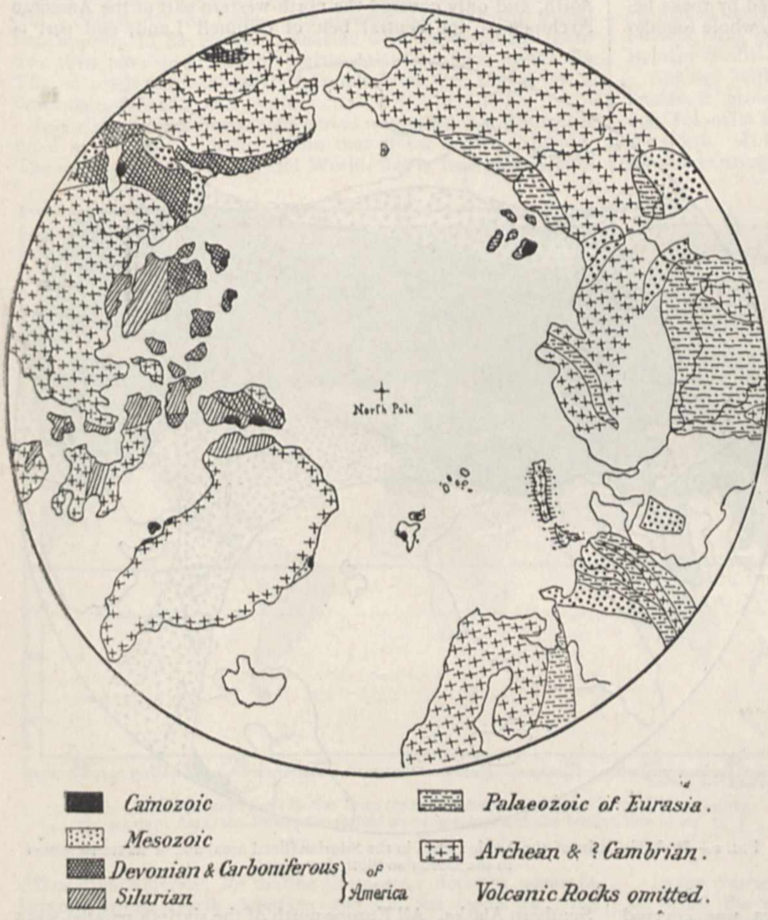

FIG. 1.—Geological Sketch Map of Polar Regions.

How firmly ingrained this view of the shallowness of the Arctic Ocean had grown, cannot perhaps be more strikingly illustrated than by the fact that the *Fram* was only supplied with apparatus for sounding in comparatively slight depths. Accordingly, geologists and biologists were permitted to introduce great changes in the size and position of the Arctic Ocean without any objections being raised. Thus Sir J. D. Hooker was allowed to call up great land areas from the Arctic deeps, to account for the apparent eccentricities of plant distribution. When Sir Chas. Lyell explained the vicissitudes of Arctic climates by a different arrangement of land and water in the North Polar regions, no *à priori* protests were made. But the sounding-line of the *Fram* has changed all that, for it has introduced into Arctic geology the theory of the permanence of oceanic basins. If that theory be true, then throughout the æons of geological time a large part of the Polar area has been occupied by a vast, deep reservoir of water. In that case many of the problems of British geology require solutions, different from those which have hitherto been deemed satisfactory.

The question whether the Polar Basin has been permanent, and, if not, at what age it has been formed, is the problem of greatest geological importance raised by the voyage of the *Fram*.

In the case of the other oceans, biological evidence is fortunately available. Thus a map of the world marked off into botanical regions, shows that in the tropics and the southern hemisphere the ocean basins separate distinct floras. Thus South America, Africa, and Australia now belong to different botanical regions, so that these three continents must have been separated from one another for a very long period. But palæo-botanical evidence shows that in Triassic times they were inhabited by the same flora; and therefore, during or immediately preceding that period, they were not so completely separated by ocean barriers as they are to-day. But in the case of the Arctic Ocean we get no such assistance from biological evidence, for one flora and one fauna extend uninterruptedly through all the Arctic lands. This uniformity may have resulted from the original development of this flora and fauna in some land around the Pole, whence they spread radially into Europe, Asia and America. But, on the other hand, it may have resulted from an east or west diffusion along the circum-arctic belt of land. That this land has not always been arranged as it is at present, is rendered almost certain by the minor differences that occur in the floras of different parts of the Arctic zone. For example, the line of separation between the American and European subdivisions of the Boreal botanical realm does not run between Greenland and Iceland, but between Greenland and the rest of the American archipelago. In fact, according to Hooker and

[1] As limits of space prevented the insertion of adequate references to authorities, it has been thought advisable to omit them altogether.
[2] The principal opponent of this view was Dr. J. Murray.

Nathorst, Greenland botanically belongs to Europe, and not to America. Though this throws doubt on the permanence of the separation of Greenland and North-west Europe, it does not prove any change in the Arctic Ocean inconsistent with the theory of the permanence of ocean basins. As we, therefore, cannot prove that the resemblances between the inhabitants of the Arctic lands on opposite meridians have been established by migration across the Arctic Ocean, instead of around it, we cannot hope for much help from biological evidence in determining the age of the Arctic Basin.

We are therefore compelled to rely on the facts of the stratigraphical geology of the Arctic regions, of which a short outline is accordingly advisable. This is illustrated by the accompanying sketch map (Fig. 1). The rocks of the Arctic regions belong to the following systems: the Archean, Cambrian, Silurian, Devonian, Carboniferous, Triassic, Jurassic, Cretaceous, Lower or Middle Tertiary, and Pleistocene.

The largest part of the Arctic land is occupied by rocks belonging to the Archean system. They form the whole foundation of Greenland, and occur in Baffin's Land, Labrador and the eastern part of British North America; westward they plunge below the Devonian and Cretaceous rocks of the Mackenzie River Basin, and reappear in the Yukon River and in Alaska. They occupy an enormous extent of Siberia, reappearing at intervals beneath Palæozoic rocks and Pleistocene tundras: they form the backbone of the Ural mountains, and of their northern continuation the islands of Nova Zemlya; west of the White Sea they cover nearly the whole of Finland, the Kola Peninsula, and Scandinavia, and a ridge of them extends up Western Spitsbergen, and forms most of North-East Land and its off-lying islands.

The age of the next series is somewhat uncertain. Its members overlie the Archean rocks unconformably, while they are always earlier than any fossiliferous beds with which they may be associated. The series consists of red sandstones and coarse conglomerates, with quartzites and dolomites. The rocks are regularly bedded, and are often horizontal; but they may be violently contorted and roughly cleaved. This series does not form huge continental blocks like the Archean, but occurs as a belt which may at one time have been continuous around the Arctic Ocean. Representatives now occur in northern Norway and Spitsbergen; in eastern, western and southern Greenland, in Labrador, the basin of the Coppermine River, and at one or two places on the Siberian coast. Fossil remains occur occasionally, but none have yet been described which settle the age of the series. But from stratigraphical considerations this series is probably of Lower Cambrian, or possibly Torridonian age.

In the next system representative fossils are abundant, and they show us that in Silurian times a large part of the Arctic area was covered by a sea, whose waters were warmer than those of the present Arctic Ocean. The Silurian rocks occur in belts. One belt runs down Smith's Sound, and then, bending westward, crosses the islands of North Devon, North Somerset, and Victoria Land. So that the Silurian Sea apparently covered most of the American Archipelago, and extended up two gulfs, of which one ran across Baffin's Land, and another up Hudson's Bay. But most of Arctic North America was then above the sea. The shore line of the Silurian Arctic Ocean skirted the American coast as far west as Cape Parry; thence it swept northward, and it is not until we reach the Indigirka River and the new Siberian Islands that we again find exposures of Silurian rocks. In the basins of the Indigirka, Lena, and Yenesei the Silurian limestones occupy a wide extent of country; but approaching Europe the land again extended northward, and it is even possible that there was no direct communication between the Silurian Arctic Ocean and the seas that then covered parts of England and occupied the basin of the Baltic. If any connection existed, it probably occurred as a strait from the Gulf of Finland to the White Sea.

In the succeeding Devonian period the Arctic Sea was larger: one arm of it ran up the basin of the Mackenzie River, and covered a wide tract in British North America. In North-eastern Europe a similar inroad of the sea had occurred; for marine Devonian deposits are known from Nova Zemlya and the flanks of the Ural Mountains, and they cover a large part of the district of Timan and the Petchora Basin.

The accompanying sketch map (Fig. 2) illustrates the probable limits of the Arctic Sea in the Silurian period, and the extent of its transgression in the Devonian.

In addition to the marine Devonian deposits, others are known which were probably formed on land or in inland seas; but more satisfactory evidence of land conditions occurs in the Carboniferous period, when the extent of the land was probably much greater. In Arctic America the sea had withdrawn to the north, and only covered the north-western part of the American Archipelago, the central belt of Grinnell Land, and part of

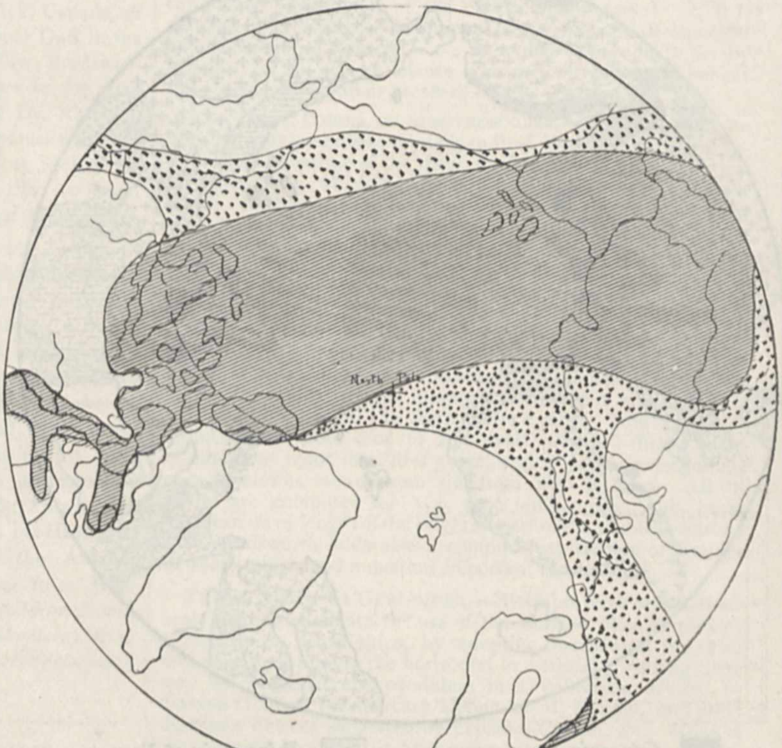

FIG. 2.—Probable limits of the Arctic Ocean in the Silurian (lined area) and of its transgression in the Devonian (dotted area).

Southern Alaska. All Europe north of the sixtieth parallel was a land area, except for a gulf in the Timan region of North-eastern Russia: Nova Zemlya was then an island chain, while most of Spitzbergen was submerged. But the advance of the sea in this area was more than counterbalanced by its recession from Eastern Siberia.

The Triassic Arctic Ocean was probably smaller than that of the Carboniferous period. The retreat of the sea from the American Archipelago, which had been gradually taking place throughout Palæozoic times, was now complete, but marine Triassic rocks in Arctic America are known from Alaska. In the Old World the best-known Triassic deposits are the barren red sandstones of various parts of north-western Europe; but a sea, inhabited by a very rich and interesting fauna, then occupied the Mediterranean area, covered most of Switzerland, and stretched eastward across the Balkan Peninsula into Russia, and possibly into India. At the same time a great Triassic sea lay to the north of Europe and Asia; it covered Spitsbergen and, probably, also Franz Josef Land, and skirting the Eurasian coast-line as far as the Lena, then spread southward into the Amur Valley; it thus reached the sea of Okhotsh, whence one coast ran southward across Japan, and the other eastward to Alaska. From many Arctic localities these Triassic rocks are rich in fossils; but the fauna of the Triassic Arctic Ocean is so different from that of the contemporary Mediterranean Sea, that it is doubtful whether there was any direct connection between them.

After the close of the Trias there is a considerable gap in the annals of the Arctic Ocean. When the record is resumed in the middle and upper parts of the Jurassic period, we find that the sea has either again grown very considerably, or has materially shifted its position. Thus the sea, instead of ending near Spitsbergen, has encroached to Greenland on the west, and extended southward to the Lofoten Islands, to southern Sweden and to England, France and Germany; and further east a series of gulfs ran southward up the valleys of the Petchora, Obi, Yenesei, and the Lena. The Jurassic Arctic Ocean, therefore, appears to have been connected with whatever sea there may then have been in the North Atlantic; but, unlike its Triassic predecessor, it was separated from the Pacific by a broad belt of land.

In the succeeding Cretaceous period we get the last geological proof of an Arctic Ocean before that of the existing period. The sea had receded in the Old World, but it had gained considerably in America; for marine Cretaceous deposits occur in Central British North America, and the sea seems to have entered that area by the Mackenzie River depression. In the Tertiary era all the positive evidence relates to land conditions, excepting some obscure fossils in one or two localities, and some patches of Miocene shell-beds bordering the Pacific, as on the shores of the sea of Okhotsh. The Arctic Ocean therefore appears to have shrunk very considerably, and the land areas to have once again broken up the basin of the northern sea.

A general summary, therefore, of the facts of Arctic geology show that the Arctic Ocean has varied greatly, both in position and size. The Arctic Basin is at the present time bounded by a rim of land which is supported by five great blocks of Archean rocks; these blocks form, respectively, Scandinavia and North-western Russia; Labrador and North-eastern British America; Greenland; Alaska and North-eastern Asia; and the Archean block of Central Siberia. These Archean blocks were each more or less completely surrounded by bands of sedimentary rocks. At least two of them have never been below sea-level; and there is no satisfactory evidence to prove that the other blocks have been submerged, at least, since Middle Palæozoic times. In fact, the geological history of the Arctic Basin is the record of the alternate enlargement and diminution of the Arctic seas by the elevation and depression of parts of the bands of sediments, which surround the Archean blocks. The blocks themselves are of great geological antiquity, and the successive earth movements have been moulded upon them. As the main nuclei of the great land masses of the Arctic regions are therefore of vast antiquity, it may be thought only reasonable to assume an equally great age for the central ocean basin. But if we look at a map of the Polar regions showing the strike of the rocks and the trend of the mountain chains, we see that these all run north and south, and end abruptly on the margin of the Polar Basin. This meridianal trend occurs in the branch of the Rocky Mountains that forms the western boundary of the Mackenzie River, in the Archean axes of Boothia and Melville Peninsula, in the strike of the rocks of Northern Greenland and Western Spitzbergen. In Asia it is particularly well shown by the Ural and Verkhanoysk Mountains and their respective geological continuations, Nova Zemlya and the New Siberian Islands, and by the parallel chains between the Lena and Behring Straits.

Analogy with similar truncated mountain lines elsewhere renders it probable that all the mountain ranges, having what von Tol calls a "Ural orientation," once extended further to the north. If they did so, they would have effectually broken up the existing Polar Basin. At the present time our knowledge is insufficient for a final conclusion. But the evidence of the historical geology, physical structure and earth movements of Arctic lands are all consistent with the origin of the Arctic Basin as a great area of subsidence (a "senkungsfeld" of Suess) later than the deposition of the lower Tertiary plant beds. The geological facts attest such great geographical changes in that region, that geologists are not at present bound to abandon helpful explanations, which are in themselves probable and are in harmony with the geological evidence, simply because they may be inconsistent with the permanence of the Arctic Basin.

J. W. GREGORY.

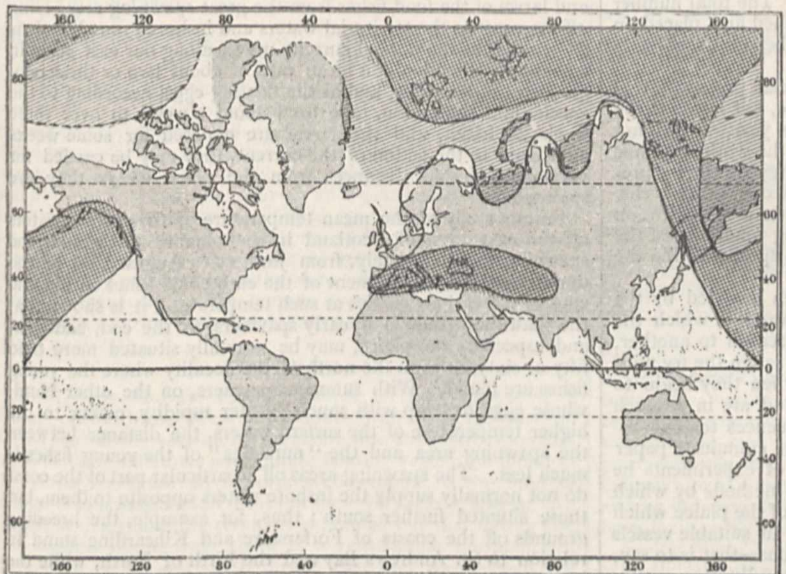

FIG. 3.—The Northern Seas in the Trias (cross-hatched) and Upper Jurassic (lined tint). [In Eastern Asia, the lined area should not come south of the broken line in lat. 75°.]

SCIENTIFIC INVESTIGATIONS OF THE SCOTTISH FISHERY BOARD.

THE third part of the Fifteenth Annual Report of the Fishery Board for Scotland, dealing with the principal scientific investigations conducted under the auspices of the Board in 1896, has just been published. The work done may be judged from the subjoined summary, which is abridged from the general statement prefixed to the detailed reports on the investigations carried out.

In the course of the year, the investigations which were carried on under the supervision of the Scientific Superintendent, Dr. Wemyss Fulton, were prosecuted on the same general lines as in previous years, and have resulted in further extensions of knowledge respecting the life-history and habits of the food fishes, and by the physical conditions and changes in the sea which bear upon fishery problems. Special attention was given to certain hydrographical questions concerning the circulation of the water in the North Sea and the adjacent parts of the North Atlantic. In addition to such inquiries, the hatching and artificial propagation of some of the important food fishes have been continued at the Board's Marine Hatchery at Dunbar.

One of the most important results of the work has been to show that the food fishes which form the basis of the fishing industry—such as plaice, cod, haddock, ling, turbot, &c.—do not spawn on the east coast within the three-mile limit, as was previously supposed. On the other hand, it is not known at what distances from the shore or in what precise localities the spawning areas are chiefly situated. It would obviously be of great advantage to obtain accurate information on this subject, and to be able to map out on a chart the regions where the various species of the food fishes spawn.

THE INFLUENCE OF BEAM TRAWLING.

The results of the trawling experiments carried on in 1896, together with the tables embodying the details of the observations, are given in a special report. The trawling experiments have for the present been suspended in the Firth of Forth and St. Andrews Bay, where they were most systematically and regularly conducted for a number of years. The general results, so far as concerns the most important subject of the experiments in these waters, the increase or decrease in the abundance of the food fishes since beam trawling was prohibited, showed that while the relative numbers of most of the round-fishes, such as cod and haddock, and the unimportant flat-fishes, the dabs, had slightly increased, there was a decrease among the more valuable flat-fishes, the plaice and lemon sole, a circumstance probably due to the increased trawling in the offshore areas where these fishes spawn.

THE HATCHING AND REARING OF FOOD FISHES.

In previous reports, detailed descriptions were given of the methods and processes adopted at the Dunbar Hatchery in connection with the artificial propagation of marine food fishes. Operations have been conducted for the most part with the valuable flat-fishes, especially the plaice, but also the turbot, sole, and lemon sole, and also on a lesser scale with certain round-fishes, such as the cod and haddock. The total number of the various species which have been hatched and placed on the fishing grounds since the work was begun in 1894 is 92,920,000.

During the current season (1897) the artificial propagation of plaice is being proceeded with on a large scale, but owing to the earlier publication of the annual report this year, it was not possible to give a statement of the results of the work, which is still in progress. As mentioned in last year's report, the fry produced in the hatchery are being transferred to certain sea-lochs, which are, to a large extent, cut off from free communication with the open sea, and observations are being made to test the results on the relative abundance of the same species within the areas selected.

The hatching work has hitherto been much impeded by the want of suitable ponds or enclosures of sea-water in which the adult spawners could be retained from one season to another, and by means of which it would be possible to retain the fry until towards the close of the post-larval stage, when they begin to assume the form and habits of the adult, and are in a much better condition to successfully meet the influences tending towards their destruction. The present report contains a paper by Mr. Harald Dannevig, giving the results of experiments he has made with the view of ascertaining the methods by which this may be accomplished. Some of the fry of the plaice which were hatched in the establishment were kept in suitable vessels of unfiltered water, to which tow-net collections—that is to say, the gatherings of minute organisms found naturally in sea-water—were added. By this means the fry were reared through their post-larval stages, until they had undergone their transformation into little plaice and settled on the bottom. Their food consisted to a small extent of diatoms, and chiefly of minute crustaceæ and larval molluscs.

These experiments point to a method by which the utility of artificial propagation might be considerably extended, namely, by retaining the fry for a few weeks in suitable enclosures of sea-water before they are transferred to the sea.

THE CURRENTS IN THE NORTH SEA AND THEIR RELATION TO FISHERIES.

In recent years, the attention of a number of investigators has been directed to the hydrography of the North Sea, and several inquiries and series of observations have been made with the object of determining its principal physical conditions with especial relation to the movement of its waters. During the last two years and a half some thousands of drift-bottles have been thrown into various parts of the North Sea, about five hundred of which have been recovered, and from careful comparison of the course taken, combined with a study of the prevailing winds throughout the period, it has been possible to ascertain the general circulation of the surface water. The results are given in a paper in the present report by Dr. Wemyss Fulton, who shows (1) that surface water passes into the North Sea from the Atlantic round the north of Scotland and in the neighbourhood of the Orkney and Shetland Isles, and then moves southwards along the east coast of Scotland and England to the neighbourhood of the Wash; (2) that it then travels in an easterly direction towards the coast of Denmark, and then northwards to the Skagerak, which it may or may not enter, and finally passes northwards along the west coast of Norway, at least as far as the Loffoden Islands.

Drift-bottles were found scattered along a stretch of about 1700 miles of coast, in Scotland, England, Holland, Germany, Denmark, Sweden, and Norway, between 53° and 69° N. latitude.

A detailed study of the winds prevailing during the time the experiments were in progress, based upon over 12,000 observations, appears to show that this circulation of the surface water in the North Sea is principally due to the preponderance of south-westerly and westerly winds, which tend to heap up the surface water on the western or continental coasts, when, as it cannot escape southwards owing to the shallows and the narrow orifice of the Channel, it passes to the north; but subsidiary influences may aid the movement. For some weeks last winter, owing to prolonged gales and strong winds, first from a south-easterly and then from a north-easterly direction, the circulation was reversed, the surface water passing rapidly northwards along our east coast, from Norfolk to the Shetlands.

The main object of the experiments was to determine the part taken by the surface currents in transporting the floating eggs and larvæ of the food fishes from the great spawning areas lying off the coast to the territorial waters and inshore grounds. It is shown that as the normal current moves along our east coast in a southerly direction at a mean rate of about two or three geographical miles a day, and as the floating eggs, according to the species and the season, take from about a week to over three weeks to hatch, and the larvæ are exposed for some weeks additional to the action of the current, they may be carried for very considerable distances from the place where they are spawned.

From a study of the mean temperature of the surface waters off the east coast of Scotland in each month throughout the spawning season, namely, from January to August, and of the duration of the development of the embryonic fishes within the eggs of the various species at such temperature, it is shown that the spawning grounds of early spawners, as the cod, haddock, and especially the plaice, may be normally situated more than fifty or sixty miles to the north of the locality where the young fishes are found. With summer spawners, on the other hand, whose eggs develop with much greater rapidity, owing to the higher temperature of the surface waters, the distance between the spawning area and the "nurseries" of the young fishes is much less. The spawning areas off a particular part of the coast do not normally supply the inshore waters opposite to them, but those situated further south; thus, for example, the breeding grounds off the coasts of Forfarshire and Kincardine stand in relation to St. Andrews Bay and the Firth of Forth, while the breeding grounds situated to the east of the latter stand in relation to the coasts of Berwickshire and Northumberland.

It is shown that the southward drift of the floating eggs and larvæ of the plaice is in agreement with the migratory movement of the adults and growing fish, which is in the opposite, or northerly, direction.

The easterly surface drift from the neighbourhood of the Dogger Bank also tends to explain the enormous aggregation of immature flat-fishes in the great bight between the north coast of Holland and the coast of Denmark. The southerly drift is not improbably related to the movements of the herring shoals during summer and autumn, but the connection has not yet been thoroughly investigated.

THE LIFE-HISTORIES AND DEVELOPMENT OF THE FOOD FISHES.

Prof. M'Intosh describes the life-histories of the cod, haddock, and whiting from very early stages. It is shown that, while the spawning grounds of the cod are offshore, the eggs and larvæ are wafted inshore, or that the post-larval stage is attained in the former region, the young fish moving shorewards subsequently, when from about half an inch to three-quarters of an inch in length. During June and July they frequent the shallow rock-pools at ebb-tide in company with the green-cod or saithe, and as they grow older many of them pass outwards again to the off-shore waters. Young haddocks have a different distribution from the young cod, and are found in the deeper water at a distance from shore, as appears also to be the case with the younger stages of the whiting. The appearance and diagnostic characters of the various stages, which have frequently been difficult to distinguish in the past, are described very fully, and are illustrated by a series of figures.

THE DISTRIBUTION OF PELAGIC EGGS.

Mr. A. T. Masterman furnishes a review of the work done by the *Garland* in connection with the distribution of the pelagic eggs of food fishes in the years 1890–96, with special reference to the determination of the spawning areas and seasons of the various species and the direction taken by the eggs after they are shed. The observations made in the Firth of Forth and St. Andrews Bay throughout the above period are brought together and compared, lists being furnished of the principal species dealt with. The more important conclusions drawn from the study of the distribution of the pelagic eggs agree with those previously derived from the investigations into the distribution of spawning adults, namely, the season at which the various species spawn, and the place where the eggs are shed. It is shown that the more valuable forms, such as the cod, haddock, plaice, coal-fish, turbot and ling, spawn outside the three-mile limit, the floating eggs appearing first at the seaward stations and being gradually drifted in; on the other hand, less important species, such as the gurnard, flounder, and dabs, spawn within the limit as well as beyond it, and the sprat spawns principally within the limits of the Firth of Forth.

THE REARING OF LARVAL AND POST-LARVAL PLAICE AND OTHER FLAT-FISHES.

In connection with the artificial propagation of the food fishes a series of experiments were made by Mr. H. Dannevig in the rearing of the young fishes derived from the artificially fecundated eggs, which have yielded results of scientific interest. The natural food of the early post-larval plaice has been determined, and also the duration of the larval and post-larval periods It was found that the larvæ from plaice eggs which were fertilised on April 28 and hatched on May 10, took eight days to absorb the yolk and enter on the post-larval stage, and other thirty-four days, or forty-two days from the date of hatching, before they settle permanently on the bottom as typical little flat-fishes. The changes during their development are described and illustrated in a plate.

MARINE DIATOMS.

Mr. George Murray, F.R.S., Keeper of the Department of Botany of the British Museum, conducted on board the *Garland* during part of the year an investigation into the distribution and reproduction of diatoms and minute floating vegetation found in the sea, which form an important constituent of the food of minute crustaceans and of fishes in their very early stages. In the paper describing the results it is pointed out that during the first months of the year there is a remarkable prevalence of diatom life in the sea off both the east and west coasts, the quantity diminishing towards the end of March, and thereafter remaining at a fairly constant minimum. The part taken by these minute vegetable forms in furnishing food for crustacea and young fishes is described, as well as the reproductive processes of the diatoms, respecting which the observations have been of great scientific importance.

Prof. Cleve, of Upsala, Sweden, also contributes a paper to the report, describing the characters and distribution of the diatoms and minute plant-life collected by tow-nets in the Faroe-Shetland channel during the expedition of H.M.S. *Research* to that region in August last year. The chief object of the inquiry was to determine, by comparison of the abundance and distribution of minute floating organisms, the movements of the water towards and from the North Sea.

THE INVERTEBRATE FAUNA.

Mr. Thomas Scott contributes to the report a paper describing the invertebrate fauna, as well as the fishes, of Loch Fyne, and furnishing lists of all the species which have been found in that loch, together with notes of their distribution. An account is also given of the parasites of the common copepod, *Calanus finmarchicus*, which forms an important constituent of the food of fishes. In another paper Mr. Scott gives the results of his continued investigations on the invertebrate fauna of the inland waters of Scotland, including that of several lochs in Cantyre, Bute, and Forfarshire, as well as of Shetland, in the examination of which he was assisted by Mr. Robert Duthie. Through these investigations some important additions have been made to the fresh water fauna of Scotland. In a third paper the invertebrate fauna, collected by tow-nets used on board H.M.S. *Research* in the Faroe-Shetland Channel, in August, is described, notes being furnished showing the distribution of the various species obtained.

PHYSICAL INVESTIGATIONS.

In addition to the regular determinations of the temperature and density of the sea water at various stations by the *Garland*, special physical investigations were made last year in the Faroe-Shetland Channel and in Loch Fyne. By the courtesy of Admiral Wharton, the Hydrographer to the Admiralty, a series of temperature observations were taken in the former area by the officers of H.M.S. *Research*, under the command of Captain Moore, and a large number of samples of water were secured from various depths for the subsequent determination of the density. Mr. H. N. Dickson has prepared a special paper on the subject, which is printed in the present report. The work was undertaken with the view of forming part of the continued hydrographic survey of the North Sea and the North Atlantic, instituted as an international scheme with Sweden, Denmark, Norway, and Germany in 1893, and the observations made on board H.M.S. *Research* form an important contribution to the subject. The observations in Loch Fyne and the Firth of Clyde were made by Dr. H. R. Mill in April and September, and are dealt with in another paper. They serve to confirm the previous conclusions as to the circulation of the waters in Loch Fyne.

THE PHYSIOLOGY OF THE EMOTIONS.[1]

THE respiratory movements have wide-reaching effects. They not only lead to the flow of air to and from the lungs, but they profoundly influence the circulation of the blood and lymph; they also affect the functions of the abdominal and pelvic viscera by rhythmically compressing and dislocating them. Now, these movements are liable to constant modification in the physiological acts of talking, shouting, singing, laughing, crying, sighing, and yawning (as also in the occasional and semi-pathological acts of sneezing, coughing, vomiting, and hiccoughing), and it therefore follows that these acts are more far-reaching in their effects than would at first sight appear, and hence are worthy of our careful study. This will the more readily be granted when it is added that they affect the body, not only by modifying the respiratory movements and thus producing the effects already mentioned, but by involving the expenditure of a considerable amount of neuro-muscular energy, and by inducing definite psychic phenomena which themselves have their physical accompaniments.

Seeing, then, how far-reaching are the effects of these several acts, and remembering how large a part they play in normal life, we may safely conclude that they influence the functions of the body beneficially, and that an undue interference with them is injurious. One is apt to forget how strong is the instinct to shout and sing, laugh and cry. It is especially noticeable in the savage and in the child. If these instincts are unduly repressed in the child he is sure to suffer. Crying should certainly be restrained within limits, but there can be no doubt that it is primarily physiological, not only favouring the proper expansion of the lungs and accelerating the circulation, but deadening the effects of pain and relieving nerve tension (especially in woman). Rosbach thinks it not improbable that many evils which manifest themselves in later life, such as chlorosis, contracted chest, and the phthisical habit, "may take their origin in the practice of mothers to stop their infants from screaming by soothing them to sleep in their arms or by stupefying rocking in the cradle." (Von Ziemssen's "General Therapeutics," vol. iii. p. 581). It is well known that children show a strong instinct to chatter and sing the first thing in the morning, and it should be allowed full vent as far as is practicable. The shouting which young people indulge in during their play is quite remarkable and is manifestly physiological. The same tendency to shout is observed in young adults, especially among the poorer classes in holiday time. Though from the physiological point of view justifiable, and even

[1] "The Therapeutical Aspects of Talking, Shouting, Singing, Laughing, Crying, Sighing, and Yawning." (By Dr. Harry Campbell. (Abridged from *The Lancet*, July 17.)

beneficial, the noises they make are certainly not always pleasing, especially to the sensitive nerves of the cultured, amongst whom this instinct is consequently suppressed, though whether altogether to the gain of the individual is questionable.

The various acts referred to will now be considered individually.

TALKING.

As regards the psychic aspect of talking, thought becomes much more vivid if it finds expression, whether in speech, writing, music, or artistic production, than if it remains unexpressed. The physical effects of thought are more pronounced in talking than in writing. The cortical nervous discharges underlying it send a stream of energy towards the muscles involved in speech and gesture, and both voice and gesture can be modified to convey subtle shades of thought and feeling which cannot find expression in writing. The very expression of these refinements enhances the vividness and intensity of mentation. Talking is for this reason stimulating, and its influence in this respect is in a measure proportional to the gesture accompanying it. Few things are more calculated to stimulate the body, to rouse it from lethargy, than "animated" conversation. In talking, as in laughing, shouting, singing, and crying, inspiration is short, while expiration is prolonged, the exit of air being checked partly by obstruction in the glottis and partly, perhaps, by the action of the inspiratory muscles. The actual amount of work done in talking is far more than might at first sight be supposed, and should always be taken into account in reckoning up the quantity of exercise taken during the day. The amount of talking done by barristers, politicians, and others enables them to dispense largely with exercise as ordinarily understood; for not only do they in this way expend a considerable amount of muscular energy, but they experience the manifold advantages of active respiratory movements continued for long periods together; indeed, I believe talking to be distinctly conducive to longevity. That talking involves a considerable expenditure of energy is shown by the exhaustion which it induces in those who are nervously run down. Such are often greatly exhausted, even after a moderate day's talking. This exhaustion is due to mental as well as to muscular expenditure; indeed, in the very neurasthenic the bare process of thought may be an effort and the mere effort to think may alone cause exhaustion; and if such is the case, how much more likely is the putting of thought into speech to do so, seeing that, apart from the muscular expenditure involved in speech, thought is so much more intense when spoken than when unexpressed. Talking is a beneficial exercise in heart disease, especially in those forms in which the blood tends to be dammed back upon the lungs. The good effect is here doubtless due to the increased amplitude of the respiratory movements and to the help thus afforded to the pulmonary circulation. It is for this reason that I always encourage talking in those suffering from passive engorgement of the lungs. "The breathlessness due to dilatation of the heart," observes Sir William Broadbent, "is often relieved by exercise of the voice. I have met with numerous instances in which a clergyman has climbed into the pulpit with the utmost difficulty, and has not only preached a sermon comfortably, but has been all the better for it" (*The Lancet*, April 4, 1891, p. 798). The good result, I take it, in these cases is attributable to the deep inspirations required by the loud voice necessary to fill a large building.

SHOUTING.

The psychic accompaniment of shouting is essentially emotional. Emotion is not only expressed, but sustained, and, indeed, intensified by it. Thus the shouting of children at play, itself the outcome of exuberant emotion and pent-up neuromuscular energy, enhances the emotional outburst. In like manner the hurrahs of an applauding multitude, the cry of the huntsman, the war-whoop of the savage, the yells of an attacking force, may so exalt emotionality as to induce a condition bordering on ecstacy. A further effect of shouting is to dull sensibility, the emotional exaltation which it provokes, and the voluminous discharge of neuro-muscular energy accompanying it, inducing a corresponding depression in the sensorial sphere. It is on this principle that groaning, and still more the shriek of acute agony, bring relief. The mere sound produces a similar effect by violently energising the acoustic centres.[1] The shouting and gesticulation which accompany an outburst of passion act physiologically by relieving nerve tension; and, indeed, as Hughlings Jackson has suggested, swearing may not be without its physiological justification. Passionate outbursts are generally succeeded by a period of good behaviour, and, it may be, improved health. One frequently notices this in children, and I have also observed it in the adult. It is possible that the outbursts of irritability observed in disease, as, for instance, in gout, have their physiological as well as their pathological aspect. As regards the modifications in the respiratory movements caused by shouting, the important practical point to notice is that they are increased in depth. Hence shouting favours the development of the lungs and accelerates the circulation of blood and lymph.

SINGING.

Singing, like shouting, is more emotional than intellectual, the degree of emotion called forth depending upon the extent to which the individual throws himself into the spirit of the song. The nature of the attendant emotion varies of course considerably, and there is a corresponding variability in its physical correlatives: if the theme of the song be joyous the proper rendering of it is highly stimulating. From the medical standpoint singing is a most important exercise, both by virtue of its influence on the emotions, on the respiratory movements, and on the development of the lungs. The good average health enjoyed by professional singers is in large measure attributable to the mere exercise of their calling.[2] Such therapeutic importance do I attach to singing that I recommend it wherever opportunity affords. It is especially useful in defective chest development and in chronic heart disease. Oertel speaks enthusiastically of the beneficial influence of singing on the general health, and especially on the lungs, and he refers to the fact that almost all eminent singing masters can tell of serious cases of lung disease which have been cured by their method of singing. He thinks there can be no doubt that weak chests of various kinds can be greatly improved by it, and he would even appear to include phthisis.

LAUGHTER.

The psychic accompaniment of laughter being joyous emotion, its effect is stimulating, and it has been truly said that the man who makes us laugh is a public benefactor. Its beneficial effect on the body is illustrated by the saying, "Laugh and grow fat." The expiratory act in laughter is greatly prolonged, and, the glottis being partly closed, intra-pulmonary tension is increased; and thus in excessive laughter there may be considerable impediment to the flow of blood through the lungs, as shown by the turgid head and neck. This disadvantage—for in most cases of heart disease it is a disadvantage—is far more than compensated for by other effects, foremost among which must be reckoned the deep inspirations which separate the individual paroxysms.

CRYING.

In thinking of the term "crying," one must distinguish between the mere shedding of tears, and weeping accompanied by sobbing. In the one the effects are limited, while in the other the entire body may be convulsed. I have already referred to the beneficial effects of crying in children. The crying of the infant is peculiar. Expirations are prolonged sometimes for as much as half a minute, and are interrupted by short inspirations. During the expirations the glottis is contracted, and the intra-pulmonary pressure rises considerably. Not only is the pulmonary circulation thereby greatly impeded, as shown by the swollen veins of the head and neck, but bronchial mucus, flatus, and other noxious matters are evacuated. The paroxysm is succeeded by rapid deep respirations, which restore the equilibrium of the circulation. Women likewise often derive benefit from "a good cry"—the profuse flow of tears lessens blood-pressure within the cranium; the voluminous discharge of nerve energy relieves nerve tension; the sobbing movements of respiration influence in a very decided and doubtless beneficial way the circulation and the movements of the abdomino-pelvic

[1] A famous quack extracts his patient's teeth to the blare of trumpets and the boom of the big drum.

[2] The splendid chest development of public singers is, of course, not entirely attributable to the constant exercise of the voice, since no one can attain a high excellence without having a good chest development in the first instance. It must also be observed that every singer who attains to fame is careful to lead a healthy life.

viscera; while the widespread contraction of the muscle system has probably also a good effect. How pronounced are the dynamic effects induced by completely abandoning oneself to a fit of crying is shown by the exhaustion which it entails. It is partly through this exhaustion that crying induces sleep; we hear of "crying oneself to sleep," though this must be but a very crude explanation of the phenomenon. The tendency of women to cry should, of course, be kept within proper bounds, but certainly harm may result from its complete suppression, as Tennyson recognises in the line—

"She must weep or she will die."

It is said that women who are able to find relief in tears keep their youth longer than those who repress them. The internal cankering action "like a worm i' the bud" of pent-up emotion is not only a beautiful poetic conceit, but a profound physiological truth. In short, strong emotion should receive expression—"give sorrow words."

SIGHING.

"A sigh is a deep thoracic respiration, with retraction of the abdomen."[1] The retraction of the abdominal muscles leads to a compression of the splanchnic veins. This compression is probably increased by slight descent of the diaphragm. The blood is thus pressed out of those veins into the right heart, and the flow into this chamber is further favoured by the deep inspiration which also aids the circulation through the lungs. A more common cause of sighing I believe to be shallow breathing, however induced. Thus sadness and a sense of weariness or boredom are wont to be attended by shallow breathing, and in all of them sighing is frequent. In consequence of this shallow breathing, blood-aeration lags behind, and the blood tends to accumulate in the right heart and systematic veins. The sigh benefits by promoting the aeration of the blood and quickening the pulmonary circulation, and it is for similar reasons that sighing is apt to occur during a state of "breathless attention"—when the attention, *i.e.*, is so strained that one forgets, as it were, to breathe adequately.

YAWNING.

There can be little doubt that one of the objects of yawning is the exercise of muscles which have been for a long time quiescent, and the acceleration of the blood and lymph flow which has, in consequence of this quiescence, become sluggish. Hence its frequency after one has remained for some time in the same position—*e.g.* when waking in the morning. Co-operating with this cause is sleepiness and the shallow breathing which it entails. This factor, as well as muscle-quiescence, is apt to attend the sense of boredom which one experiences in listening to a dull sermon. Hence it is that the bored individual is apt to yawn. As in the case of sighing, the deep breath which accompanies the act of yawning compensates for the shallow breathing, which is so apt to excite it.

[1] L. Hill: *Journal of Physiology*, vol. xv. p. 48.

ON THE ASCENT OF WATER IN TREES.[2]

WITHIN the last few years the problem of the ascent of water has entered on a new stage of existence. The researches which have led to this new development are of such weight and extent that they might alone occupy our time. It will be necessary therefore to avoid, as far as possible, going into ancient history. But it will conduce to clearness to recall some of the main stepping-stones in the progress of the subject.

The two questions to be considered are: (1) What is the path of the ascending water? (2) What are the forces which produce the rise?

(1) The first question has gone through curious vicissitudes. The majority of earlier writers assumed that the water travelled in the vessels. This was not, however, a uniform view. Cæsalpinus, 1583, seems (Sachs' "History of Botany," English Trans., p. 451) to have thought that water moved by imbibition in the "nerves." Malpighi and Ray held that the vessels serve for air, and the wood fibres for the ascent of water. Hales ("Vegetable Staticks," p. 130), who believed in the "sap-vessels" as conduits, speculated on the passage upwards of water between the wood and the bark. Also (*loc. cit.* p. 19), that water may travel as vapour not in the liquid state. In the present century Treviranus (Sachs' "History"), 1835, held that water travelled in vessels; De Candolle, 1832, that the intercellular spaces were the conduits. In Balfour's "Manual of Botany," 1863, vessels, cells, and intercellular spaces are spoken of as transmitting the ascending water.

[2] A paper read before Section K of the British Association at the Liverpool meeting, by Francis Darwin, F.R.S. (Revised January 20, 1897).

The change in botanical opinion was introduced by the great authority of Sachs,[1] who took up Unger's view[2] that the transpiration current travels in the thickness of the walls as water of imbibition.

Then followed the reaction against the imbibitionists—a reaction which has maintained its position up to the present time. Boehm, who had never adopted the imbibition theory, must have the credit of initiating this change; his style was confused and his argument marred by many faults, but the reaction should in fairness be considered as a conversion to his views, as far as the path of the travelling water is concerned. Nevertheless, it was the work of others who principally forced the change on botanists—*e.g.* von Höhnel (*Pringsheim's Jahrb.* xii., 1879), Elfving (*Bot. Zeitung*, 1882), Russow (*Bot. Centr.* xiii., 1883), R. Hartig ("Ueber die Vertheilung," &c., *Untersuchungen aus dem Forst. Bot. Inst. zu München*, ii. and iii.), Vesque (*Ann. Sc. Nat.* xv. p. 5, 1883), Godlewski (*Pringsheim's Jahrb.* xv., 1884), and others.

(2) The second question has a curious history, and one that is not particularly creditable to botanists generally. It has been characterised by loose reasoning, vagueness as to physical laws, and a general tendency to avoid the problem, and to scramble round it in a mist of *vis à tergo*, *capillarity*, *Jamin chains*, *osmosis*, and *barometric pressure*.

An exception to this accusation (to which I personally plead guilty) is to be found in Sachs' imbibition theory, in which, at any rate, the barometric errors were avoided, though it has difficulties of its own, as Elfving has pointed out.

But the most hopeful change in botanical speculation began with those naturalists who, concluding that no purely physical causes could account for the facts, invoked the help of the living elements in the wood. To Westermaier (*Deutsch Bot. Ges.* Bd. i., 1883, p. 371) and Godlewski (*Pringsheim's Jahrb.* xv., 1884) is due the credit of this notable advance, for whether future research uphold or destroy their conclusions, it claims our sympathy as a serious facing of the problem by an ingenious and rational hypothesis.[3]

We may pass over the cloud which arose to witness for and against these theories, and proceed at once to Strasburger's great work (*Leitungsbahnen*, 1891), in which, with wonderful courage and with the industry of genius, he set himself to work out the problem *de novo*, both anatomically and physiologically. In my opinion it is difficult to praise too highly this great effort of Strasburger's.

Strasburger's general conclusion is now well known. He convinced himself that liquid can be raised to heights greater than that of the barometric column in cut stems, in which the living elements have been killed. Therefore, the cause of the rise could not be (1) barometric pressure, (2) nor root pressure, (3) nor could it be due to the action of the living elements of the wood. His conclusions may be stated as follows:—

(*a*) The ascent of water is not dependent on living elements, but is a purely physical phenomenon.

(*b*) None of the physical explanations hitherto made are sufficient to account for the facts.

Strasburger has been most unjustly depreciated, because his book ends in this confession of ignorance. I do not share such a view. I think to establish such distinct, though negative, conclusions would be, in this most nebulous of subjects, an advance of great value. Whether he has established these conclusions must of course be a matter of opinion. To discuss them both would be to go over 500 pages of Strasburger's book, and will not here be attempted. Conclusion (*a*) that the ascent is not dependent on living elements must, however briefly, be discussed, because it is here that the roads divide. If we agree with Strasburger, we know that we must seek along the physical

[1] *Physiol. Végétale* (French Trans.), 1868, p. 235, and more fully in the *Lehrbuch*. Sachs also partially entertained Quincke's well-known suggestion of movement of a film of water on the surface of vessels.

[2] *Sitz. k.k. Akad. Wien*, 1868. Dixon's and Joly's paper in the *Annals of Botany*, September 1895, gives evidence in favour of a certain amount of movement of the imbibed water.

[3] It is of interest to note that Hales, in speaking of the pressure which he found to exist in bleeding trees, says: "This force is not from the root only, but must also proceed from some power in the stem and branches." ("Veg. Staticks," 1727, p. 110.)

line; if we differ from him, we are bound to seek for the missing evidence of the action of the living elements.

Schwendener's Criticism.—Perhaps the best plan will be to consider the most serious criticism that has been published of Strasburger's work, namely Schwendener's paper "Zur Kritik," &c. (*K. Preuss. Akad.* 1892, p. 911).

Schwendener objects that although a continuous column of water cannot be raised by air pressure to a greater height than that of the barometric column, yet when broken into a number of columns, as in the case of a Jamin chain, that a column considerably over 10 m., even as much as 13 or 14 m., of water can be suspended. This, though not fatal to Strasburger's conclusions, is no doubt a serious criticism. For if 13 m. can be supported, some of Strasburger's experiments are inconclusive. He finds that a branch can suck up a poisonous fluid to over 10 m., and, as above explained, argues that all ascent above that height, not being due to barometric pressure or to the living elements (since the wood is poisoned), is for the present inexplicable. But, if Schwendener is right, the effect above 10 m. may have been due to atmospheric pressure. Askenasy (*loc. cit. infra*, 1895, p. 6) objects to Schwendener that the supposed action cannot be continuous. By repeating the diminution of air pressure at the upper end, the movement of water becomes less and less, and sinks to almost nothing. Askenasy adds, moreover, that the amount of water which could be raised according to Schwendener's theory would be very small.

One difficulty about Schwendener's theory is that the result depends on the length of the elements of which the chain is made up (such element being a water column, *plus* an air bubble.) In his paper "Ueber das Saftsteigen" (*K. Preuss. Akad.* 1886, p. 561), he finds that the elements of the chain in *Fagus* equal in round numbers 0·5 mm. In his paper (*K. Preuss. Akad. Sitz.* 1893, p. 842), "Wasserbewegung in der Jamin'schen Kette," he finds the element in *Acer pseudo-platanus* = 0·9 mm., in *Acer platanoides* and *Ulmus effusa* = 0·2. But the calculation (1892, p. 934) is based on the existence of a chain in which the water columns are each 10 mm. in length, a condition of things which he allows does not occur in living trees.

But even if we allow Schwendener to prove theoretically the possibility of a Jamin chain being raised to a height much greater than that of a barometric column, I do not think he invalidates Strasburger's position. Schwendener's idea necessitates the travelling of a Jamin chain as a whole, *i.e.* the translation not only of water, but of air bubbles. But this cannot (as Strasburger points out) apply to his experiments on conifers, in which the movement of air to such an extent is impossible ("Ueber das Saftsteigen," *Hist. Beiträge*, v. 1893, p. 50). And for the case of dicotyledonous woods, Strasburger has shown that the movement of air is excluded by the fact that transverse walls occur in the vessels at comparatively short distances. In Aristolochia the sections may be as long as 3 m., but in ordinary woods, according to Adler (as quoted by Strasburger), we get: Alnus, 6 cm.; Corylus, 11 cm.; Betula, 12 cm.; Quercus, 57 cm.; Robinia, 69 cm. These facts seem impossible to reconcile with Schwendener's views.

Action of the Poisonous Fluids in Strasburger's Experiments.—The question whether the living elements are killed in Strasburger's experiments is of primary importance in the problem.

Schwendener does not criticise it at length; he seems to assume (*Zur Kritik, loc. cit.*, 1892, p. 935)—as far as I can understand—that since the death of the tissues extends gradually from the cut end upwards, there are living cells in the upper part which may still be effective. He also doubts "whether the cells were always killed at once." The first objection of Schwendener's may or may not be sound, but in any case it does not (as Strasburger points out) account for the experiment (*Hist. Beitr.* v. p. 12) in which an oak stem was poisoned by picric acid, and three days afterwards was placed in fuchsin-picric. The second reagent had to travel in tissues already killed with picric acid, yet a height of 22 m. was reached.

The question whether the reagents kill the cells in Strasburger's experiments does not lend itself to discussion. It is difficult to see how they should escape, and we have Strasburger's direct statement that the living tissues were visibly killed. It must not be forgotten that in some of his experiments the death of the tissues was produced by prolonged boiling, not by poisons (*Leitungsbahnen*, p. 646). Thus the lower 12 m. of a Wistaria stem were killed in this way, yet liquid was sucked up to a height of 108 cm. In the *Histolog. Beitr.* v. p. 64, he has repeated his air-pump experiment, using a boiled yew branch, and found that eosin was sucked up from a vessel in which almost complete vacuum was established, so the action of living elements and of atmospheric pressure was excluded.

On the whole, the balance of evidence is, in my judgment, against the belief that the living elements are necessary for the rise of water. In other words, I think we should be justified, from Strasburger's work, in seeking the cause of ascent in the action of purely physical laws.

Strasburger's general argument from the structure of wood.—It seems sometimes to be forgotten that, apart from the physiological or experimental evidence, there is another line of argument founded on the structure of wood. Strasburger's unrivalled knowledge allows him to use this argument with authority, and he seems to me to use it with effect. Thus (*Hist. Beitr.* v. p. 17) he points out that though in coniferous wood the action of the living elements in pumping water is conceivable, yet this is far from being universally the case. He points out that in the monocotyledons such theories meet with almost unconquerable difficulties. This is, he says, especially the case in Dracæna. He goes on to point to difficulties in the case of such dicotyledons as Albizzia. The case may perhaps best be put in the generalised manner that Strasburger himself employs (*loc. cit.* p. 20). If the living elements are of such importance as Godlewski, Westermaier, and Schwendener hold, we ought not to find these difficulties; we ought rather to find structural peculiarities pointing distinctly to the existence of such functions. For instance, we ought to find the tracheal water-path actually interrupted by living elements, which might act like a series of pumping stations one above the other. It should, however, be remembered that if we deny the importance of the medullary rays and other living elements in raising water, we ought to be able to point more clearly than we can at present to the function of the medullary rays and to structural adaptations to these functions.

The work of Dixon and Joly and of Askenasy.—I now pass on to the recent work in which Strasburger's indications to search along a purely physical line have been followed. In the paper of Dixon and Joly (*Proc. Roy. Soc.*, vol. lvii. 1894, No. 340), the suggestion was for the first time made that the raising of water to the tops of trees depends on the quality which water possesses of resisting tensile stress. To most botanists the existence of this quality is a new idea. To believe that columns of water should hang in the tracheals like solid bodies, and should, like them, transmit downwards the pull exerted on them at their upper ends by the transpiring leaves, is to some of us equivalent to believing in ropes of sand. The idea is more fully treated in the *Phil. Trans.* vol. clxxxvi., and in the *Annals of Botany*, vol. viii. The same leading idea occurred independently to Askenasy, who has published it in the *Verhand. a. d. naturhist. med. Vereins Heidelberg*, N. F., Bd. v., 1895; and N. F., Bd. v., 1896.

Askenasy has earned the gratitude of his botanical readers by giving some of the evidence which demonstrates the existence of this property of water.[1] A tube a metre in length was filled by Donny with water, and the remaining space was as far as possible freed from air. When the tube was placed vertically, the water-column at the upper end hung there, and could not be made to break or free itself from the glass by violent shaking. Berthelot filled a thick-wall capillary tube completely with water at 28°–30° C.; it was allowed to cool to 18°, so that the space left by the shrinking of water was filled with air. It was then sealed up and again warmed to 28°–30°, so that the air was dissolved in the water. When it was allowed to cool again it retained its volume, filling the tube completely. A slight shake, however, allowed the water to break and return to its proper volume at 18° with the appearance of a bubble of air. In this experiment the water contained air, yet it seems to have been until recently assumed by some physicists that to show cohesion, water must be air-free. If this were the case, the application of the principle to plants would be impossible. Dixon and Joly have, however, proved that this is not so, and this forms an important part of their contribution to the subject.

They also[2] investigated the amount of tension which water under these circumstances will bear, and found it about equal to

[1] He gives reference to Donny, *Poggendorff's Annalen*, 67 Bd. (143. Bd. d. g. R.), 1846, p. 562; Berthelot, *Annales de Chimie et de Physique*, S. 3, t. 30, 1850, p. 232; Worthington, *Proc. Roy. Soc.* vol. l., 1892, p. 423.

[2] *Phil. Trans.* vol. 186, p. 570. With ethyl alcohol Worthington records a tension of 17 atmospheres. See *Proc. R. Soc.*, vol. l.

seven atmospheres. If, therefore, the leaves at the top of a tall tree can exert the requisite upward pull on the water in the trunk, it seems certain (if no other condition in the problem interfere) that the pull can be transmitted to the level of the ground. This opens up the question whether the leaves can exert this traction on the water in the tracheals, and what is equally important, Are there any factors in the problem incompatible with the theory?

(1) *The sucking force of the leaves.*—In Dixon and Joly's first paper (*Phil. Trans.* pp. 563, 567) they assume that tractional force is given by the meniscuses "formed in the membranous *réseau* of the evaporating cell-walls," as well as possibly by the osmotic action of the cells of the mesophyll. We shall take these theories in order. Our knowledge of the cell wall does not allow us to believe in the existence of pores visible with even the highest powers of the microscope. Dixon's more general expression (*Proc. Roy. Irish Acad.* Jan. 13, 1896, p. 767) "surface tension forces developed in the substance of the walls of the evaporating cells," is therefore preferable. But Askenasy seems to me to state the matter much more conveniently by using the term "imbibition" (*loc. cit.* 1895, p. 10). The force with which vegetable membranes, *e.g.* the thallus of Laminaria, absorb water, has been demonstrated by Reinke and others, and the existence of such a force is familiar to botanists.

Both Askenasy (*loc. cit.*) and Dixon and Joly (*Annals of Bot.*, September 1895) have pointed out that the force of imbibition, or the surface tension forces, as the case may be, can exert a tractional effect on the water in the tracheals, when the turgescence of the mesophyll has been destroyed. But Askenasy in his original paper (1895), Dixon in the January 1896 paper, and again Askenasy in his second paper (March 1896) have also considered the imbibitional or surface tension forces in connection with the turgescent cell. It must clearly be understood that this does not remove imbibition from the problem. The sun's heat causes the evaporation of the water with which the walls of the mesophyll cells are imbibed; this water is replaced by imbibition from the cell-sap. The concentration of the cell-sap so produced maintains the osmotic force of the cell, which again exerts suction on the water on the tracheals.[1]

I have now given, in its simplest form, the modern theory of the rise of water. Apart from the main idea, it combines the points of several familiar views. Imbibition becomes a factor of paramount importance, though not in the way that Sachs employs it. The suspended threads of water remind us of Elfving's capillary theory, while the living element factor is represented by the turgescent mesophyll cells.

Resistance.—It is not possible to discuss the question whether the tractional forces in the leaf are sufficient for the work imposed on them until we know what is the resistance to the passage of water through wood. For it is clear that the work done by the leaf includes not only the lifting of a given column, but the overcoming of the resistance to its flow.

The resistance to the flow of the transpiration current is in want of further investigation. Janse (*Pringsheim's Jahrb.* xviii., 1887, p. 1) has discussed the question, and points out (*loc. cit.* p. 36) that two kinds of resistance must be reckoned with. The first (which he calls statical) is illustrated by means of a cylinder of Pinus wood fixed to the short arm of a J tube filled with water, when it was found that in five days the level of water in the long arm was only one mm. above that in the short arm.[2] That is to say, when time enough is given, the resistance is practically nothing. Janse has also investigated the resistance to the passage of water flowing through wood at the rate of an ordinary transpiration current. His method seems to me open to criticism, but this is not the place to give my reasons. His experiments give a wide range of results. With *Pinus strobus* a pressure of water equal to ten times the length of the wood was required to force water through at a pace equal to the transpiration current. In Ginkgo the pressure was twenty-one times the length of the wood. Strasburger (*Leitungsbahnen*, p. 779) has repeated Janse's experiment, and finds a column "several times the length of the object" necessary. Nägeli ("Das Mikroskop," 2nd edit. p. 385) found that 760 mm. of mercury were needed to force water through fresh coniferous wood at the rate of $\frac{1}{20}$ mm. per second, *i.e.* at 180 mm. per hour. If we allow one metre per hour as a fair transpiration rate (Sachs' "Arbeiten," ii. p. 182), we get a pressure of 5 atmospheres required to produce such a flow. To return to Janse's experiments: even if we assume that the resistance (expressed in water) = 5 times the length, it is clear that with a tree 40 m. in height, the resistance of 20 atmospheres has to be overcome. This would not be a pressure greater than that which osmotic forces are able to exert, but when we come to a tree of 80 m. in height, and a resistance of 40 atmospheres, the thing becomes serious.[1] A great difficulty in the question of resistance is that the results hitherto obtained are (though here I speak doubtfully) much greater than those obtained by physicists for the resistance of water flowing in glass capillaries. Until this discrepancy is explained, it is rash to argue from our present basis of knowledge.[2]

Is the osmotic suck sufficient?—The osmotic force of a turgescent cell is usually measured by its power of producing hydrostatic pressure within the cell. Thus, De Vries ("Untersuchungen über d. mechanischen Ursachen der Zellstrecken," 1877, p. 118) investigated the force necessary to extend a plasmolysed shoot to its original length; Westermaier (*Deutsch. Bot. Ges.* 1883, p. 382) the weight necessary to crush a tissue of given area; Pfeffer (*Abh. k. Sächs. Ges.* 1893) the pressure exerted by growing roots; Krabbe (*K. Akad. Berlin* (*Abhandlungen*), pp. 57, 69, 1884) the pressure under which cambium is capable of maintaining its growth.

The figures obtained by these naturalists have a wide range; it may be said that the hydrostatic pressure varies between 3 and 20 atmospheres.

Another method is to ascertain the osmotic strength of the cell-sap in terms of a KNO_3 solution, and calculate the pressure which such a solution can produce. According to Pfeffer (Pfeffer, *Phys.* i. p. 53), 1 per cent. KNO_3 with artificial membrane gives a pressure of 176 cm. = 2·3 atmospheres. De Vries (*Pringsh. Jahrb.* xiv. p. 527) calculates that in a cell, a 0·1 equivalent solution (practically = 1 per cent.) gives a pressure of 3 atmospheres. We may therefore take it as between 2·5 and 3 atmospheres. Now, De Vries found that beetroot requires 6–7 per cent. KNO_3 to plasmolyse it; this would mean 15–21 atmospheres. I do not know what is the greatest pressure which has been estimated in this way. Probably Wieler's (*Pringsh. Jahrb.* xviii. p. 82) estimate of the pressure in the developing medullary ray cells of *Pinus sylvestris* at 21 atmospheres is the highest. It is clear that investigation of the osmotic capacity of leaves for high trees is wanted, also investigations of the variation in osmotic power produced by varying resistances in the flow of the current. The experiments of Pfeffer and others[3] show that the osmotic strength of cell-sap is capable of great adaptation to circumstances—cells respond by increased turgescence to various stimuli. Whether they can respond sufficiently to account for the ascent of water is another question.

My own opinion is that the question of resistance to the flow of water is a difficulty which the authors of the modern theory have not sufficiently met. Unless it can be shown that the resistance to the flow of water in wood is less than that indicated by existing researches, we must face the fact that we do not at present know of osmotic forces which we can suppose capable of raising water to a greater height than 40 metres.

Continuity of the water in the tracheals.—The theory we are considering apparently requires that there shall be continuous columns of water from leaf to root, because a break in the column means a collapse of the machinery. This seems at first sight a fair assumption, though I doubt its complete correctness. It is in any case worthy of discussion. It has been constantly insisted on by Sachs and others that at the time of most active transpiration the vessels contain air, and not water. It is therefore a violent disturbance of our current views to believe in continuous columns of water.

For evidence on this point we are chiefly indebted to Strasburger. It is a remarkable fact that he should, without any theory to encourage such a view, have come to the conclusion that approximate continuity of water columns is a condition of primary importance, and that he should have made out the cognate fact that the whole of the *alburnum* need not be simul-

[1] Sachs' "Text Book," edit. iv., Eng. Tr., p. 679, describes evaporation taking place in the cell wall, which makes good the loss by imbibition.

[2] Strasburger (*Leitungsbahnen*, p. 777) observed equilibrium established a good deal quicker.

[1] Schwendener's experiments, *K. Preuss Akad.* 1886, p. 579, do not particularly bear on this question.

[2] It is possible that the rate of the ascending water is much less than is usually assumed. Thus Schwendener (*K. Preuss. Akad.* 1886, p. 584) calculates from an observation of v. Höhnel that the transpiration current in the stem of a tall beech was only 2 metres per day.

[3] Pfeffer, "Abhand. der k. Sächs," *Ges.* xx. p. 300; Eschenhagen, *Untersuchungen aus d. Bot. Inst. z. Tübingen*, 1889; Stange, *Bot. Zeit.*, 1892.

taneously occupied by a transpiration current; parts of it may be so occupied, while parts of it are filled with air, and do not function as water-ways. This is a valuable contribution to knowledge, and to the adherents of the new theory it is priceless; the very existence of their hypothesis may depend on it.

Strasburger's statements and reasoning are by no means accepted by every one; for instance, Schwendener refuses to take them seriously (*K. Preuss. Akad.*, 1892, p. 931).

Strasburger has microscopically examined the condition of the tracheals as regards air.[1] He found in the spruce fir in July "almost no air bubbles" in the wood of the current year, but air in considerable quantity in four-year-old wood. In the same month *Pinus Salzmanni* (*Laricio*) showed scattered bubbles in the spring wood of last year, and more in the autumn wood. In a larch there were only very occasional bubbles in the two last years' wood. In the silver fir the current year's wood was practically free from air: the air increased in the inner rings. *Tsuga canadensis* had no air in this year's wood, only a little in last year's, and an increasing quantity in the older rings, the fifth being very rich in air. In February, *Pinus strobus* had hardly any air in this year's wood, and the silver fir was all but free from it in the youngest ring. Robinia in July had the youngest wood almost air-free. *Ficus elastica* and *spuria*, various *Acacias*, and willows gave vessels not entirely free from air, but nearly so. He concludes (*loc. cit.* p. 688) that the path of the transpiration current is not absolutely free from air. The younger wood, which especially functions as the water-carrier, is the most free.

Dixon and Joly quote Strasburger's results, which they consider sufficiently favourable to their views. They rely, in addition, on the impermeability of wet cell-walls to air, isolating the conduits in which air has appeared; and on the possibility that the air may be redissolved under root-pressure (*Phil. Trans.* p. 572), an idea well worth testing.

I think Strasburger's facts are not so favourable to their theory as these authors believe; in the same way it seems to me that Askenasy is rash in saying[2] that the tracheals in many cases contain continuous columns of water. It is true that this statement does not affect the validity of his general argument, since he faces the undoubted occurrence of air bubbles in many cases. This is undoubtedly necessary, and, fortunately, we can once more turn to the *Leitungsbahnen*. Strasburger states that he has seen water creep past the air bubbles (*Leitungsbahnen*, pp. 704, 709; see also "Hist. Beitr." v. p. 76) in coniferous tracheids. The best evidence for this seems to be the fact mentioned (*ibid.* p. 79), that the part of a single tracheid in front of an air bubble gets red with absorbed eosin, though the neighbouring tracheids are colourless. This clearly suggests the creeping round the bubble which Strasburger believes in. Schwendener (*Zur. Kritik*, &c., p. 921) has been unable to confirm Strasburger's microscopic observations, and, moreover, denies the physical possibility of the phenomena. I am unable to judge of the validity of Schwendener's theoretic objections, and must leave this point. It is a question of great importance whether it is possible that, on the breaking of a column of water, a film of water remains surrounding the air bubble, and capable of holding the two columns together. If this is impossible, we must suspend our judgment until we know more of the contents of the tracheals.

To sum up this part of the subject, we may believe that the tracheals in their youngest condition may contain water in continuous columns, since the cambium cells from which they arise certainly contain fluid. But we know also that this condition is not absolutely maintained, since Strasburger has shown that the young wood contains air, though in small quantity. We must, therefore, believe either (1) that the transpiration current is able to travel past the air bubbles, or (2) that tracheals partly filled with air may again become continuous water-ways by solution of the air. If we adopt the first alternative, we must believe that the film of water between the bubble and the wall of the vessel is able to bear such a tensile stress that it can serve to link the column above with the column below the bubble. But this is analogous to trusting a rope so nearly cut through that only a few threads remain intact. With regard to the second alternative, we have, at least, indications from Strasburger's work that a tracheal partly filled with air does not necessarily remain permanently functionless (see *Leitungsbahnen*, p. 692).

[1] *Leitungsbahnen*, p. 683 *et. seq.*; Russow in 1882 (*Bot. Centr.*) vol. xiii. 1883) observed similar facts in the distribution of water and air.

[2] *Verhand. Naturhist. med. Vereins Heidelberg*, 1895, p. 15.

The isolation of the tracheals.—There are a number of points connected with the structure and properties of wood which ought to be considered in relation to the modern theories. Want of space forbids my doing more than referring to two of them.

The resistance which the wetted cell-wall offers to the passage of undissolved air is a point on which many writers have laid stress. It is clear that on any theory of the movement of water in the tracheals, it is essential that air should not filter into the water-way. This necessity is not, however, stronger in the case of the modern theories we are considering. The pressure tending to fill the tracheals with air from outside cannot be greater than atmospheric pressure, and since the wetted cell-walls of gymnospermous wood can resist the passage of air under a pressure of about an atmosphere,[1] we need not fear criticism of the theory on this ground. The above remarks seem, however, to be needed in face of the frequently recurring statement that wet wood membranes are impermeable to free air. Schwendener has some good remarks on this head (*Zur Kritik*, p. 943).

Strasburger has called attention to the important subject of the localisation or isolation of vessels, or of certain lines of tracheids. When this is possible we may have one set of tracheals containing continuous water columns, while neighbouring ones contain air at negative pressure (see *Histolog. Beiträge*, v. p. 87). This is especially important in connection with the Dixon-Joly-Askenasy theory, since, if there were no such isolation, a functioning tracheal containing a continuous column of water would give up its water to one which was not functioning. In other words, the inactive tracheals would, by negative pressure, suck water from the active ones. In the coniferous trees, the young wood is cut off by the absence of pits in the tangential walls[2] from free communication with the older wood, where air is more frequent.

In the same way the valve-like closure of the pits by the aspiration of the pit membrane, comes to be a subject of much importance.

At present I merely wish to show by a couple of examples the necessity of a complete study of the minute structure of wood in relation to the modern theories. It is, at least, a hopeful fact for Messrs. Dixon, Joly, and Askenasy that we cannot point to anything in the anatomy of wood which is absolutely inconsistent with their views. Finally, with regard to the question at large, whether we are friends or opponents of Messrs. Dixon, Joly, and Askenasy's theory, the broad facts remain that water has the power of resisting tensile strength, and that this fact must henceforth be a factor in the problem. There are difficulties in the way of our author's theory, but it is especially deserving of notice that many of these difficulties are equally serious in the case of any theory which excludes the help of the living elements of the wood, and assumes a flow of water in the tracheals. The authors have not only suggested a *vera causa*, but have done so without multiplying difficulties. There is, therefore, a distinct balance in their favour.

Huxley, quoting from Goethe, makes use of the expression *thätige Skepsis*. It is a frame of mind highly appropriate to us in the present juncture, if we interpret it to mean a state of doubt whose fruit is activity, and if we translate activity by experiment.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

MRS. RANDALL, of Massachusetts, has bequeathed to Radcliff College (the Harvard annex for women) 20,000 dollars; an equal amount to the Prospect Union, and 70,000 dollars to the Foxcroft Club of Harvard.

THE Council of University College, Liverpool, on the recommendation of the Senate and Medical Faculty, have converted the Medical Lectureship of Hygiene into a Professorship of Public Health, and have appointed Dr. E. W. Hope, Medical Officer of Health to the Corporation of Liverpool, to the chair.

[1] *Leitungsbahnen*, p. 722. Nägeli and Schwendener, *Das Mikroskop*, 2nd edit. p. 367, give 225 cm. of mercury.

[2] Strasburger discusses, in this connection, the existence of tangential pits in the autumnal wood (see *Leitungsbahnen*, p. 713).

THE London Polytechnic Council, a joint committee comprising representatives of the Council of the City and Guilds of London Institute, the Technical Education Board of the London County Council, and the central governing body of the City Parochial Foundation, adopted the following resolution at a specially convened meeting on Friday last:—"The London Polytechnic Council having had under their consideration the London University Commission Bill, and being satisfied that the students of London polytechnic institutes pursuing a course of study approved by the University under one or more of the recognised teachers of the University will enjoy equal facilities with students of a school of the University in graduating at the University, expresses its approval of the Bill and its hope that the Bill may be passed during the present Session, as affording a satisfactory solution of the London University question."

THE following are among recent appointments:—W. W. Watts, assistant geologist on the Geological Survey of the United Kingdom, to be assistant professor in Geology at the Mason College, Birmingham; Prof. R. C. Woodward to be president of the University of South Carolina; Dr. C. E. Beecher to be University professor of historical geology at Yale University; Dr. L. V. Pirsson to be professor of physical geology in the Lawrence Scientific School; Dr. F. E. Hull to be professor of physics in Colby University; Prof. William A. Rogers to be professor of physics in Alfred University at Alfred, N.Y.; Dr. Jaeger and Dr. Brodhun to be professors at the Reichsanstalt at Charlottenburg; Dr. Ignaz Zakezewski to be full professor of experimental physics at the University at Lemburg; Dr. H. Finger to be assistant professor of organic chemistry at the Polytechnic Institute in Darmstadt; Dr. A. L. Foley to be professor of physics in the University of Indiana; Dr. R. J. Aley to be professor of mathematics, and Mr. E. B. Copeland to be assistant professor of botany in the same University; Mr. T. I. Pocock to be assistant geologist on the Geological Survey of the United Kingdom; Dr. W. F. Hume and L. Gorringe to be assistants on the Geological Survey of Egypt.

THE London University Commission Bill passed through the House of Lords on Tuesday. The Duke of Devonshire, in moving the second reading on Friday last, said the Bill was substantially the same as the one which passed through the House with little discussion last Session, but for which, unfortunately, time did not allow full consideration in the other House. Certain modifications had been introduced which were the result of communications which had been in progress during almost the whole of the Session between those interested, and there was reason to hope that the difficulties which prevented the Bill from passing into law had been removed, and that it would pass now as practically an unopposed measure. He was sorry to say very considerable time had elapsed since Lord Cowper's Commission reported, and during that time a very great change had taken place in the higher education of the City of London, and this had caused the necessity for certain alterations of procedure in the Bill. Almost the only point discussed in the House last year was that which affected, or was supposed to affect, denominational colleges and principally King's College. The agreement which was arrived at last year had been embodied in somewhat different terms in the present Bill, and he believed it was now practically accepted by King's College and the principal bodies concerned, and was now not objected to by Lord Herschell, who took a strong line of opposition last year. The Earl of Kimberley said as he was a member of the Senate of the University he was aware of the circumstances to which the noble Duke had alluded, and which led to the introduction of this amended Bill. He did not think it was necessary or desirable to go into the provisions now; he would confine himself to saying the present form was the result of very careful consideration, he might say a compromise between the different interests. There was very sanguine hope, and he sincerely trusted it would be fulfilled, that the Bill in its present form was probably in the only form that would be acceptable to all parties concerned, and he hoped it might pass. The text of the Bill is printed in the *Times* of Saturday, July 24, from which we learn that the gentlemen who are to fill the statutory commission under the Bill are—Baron Davey, the Bishop of London, Lord Lister, Sir William Roberts, Sir Owen Roberts, Prof. Jebb, and Mr. E. H. Busk, the Chairman of Convocation of London University.

SCIENTIFIC SERIALS.

Wiedemann's Annalen der Physik und Chemie, No. 5.—Polarisation capacities, by C. M. Gordon. These were determined by a new arrangement of Wheatstone's bridge, in which a known capacity and resistance are introduced into one branch, and an electrolytic cell of unknown capacity and resistance in the other. A minimum is obtained in the telephone when the capacities are inversely as the resistances in the remaining branches. The author found that for small currents the polarisation is a reversible process, and that the counter E.M.F. is rigidly given by Kohlrausch's equation $E = \frac{1}{c}\int idt$. The best results were obtained with electrodes of "platinised" platinum.—Effect of concussion and heat upon magnetism, by Carl Fromme. Concussion exerts a well-defined effect upon magnetisation, which is independent of that produced by deformation, and of the magnetic history of the material. It acts directly upon the molecular magnets, probably by producing groupings of them, which reduce the magnetic moment and lower the susceptibility. The effects of concussion may be imitated by sending an alternating current through the magnetising coil and gradually reducing it to zero. Also, in the case of iron wires, by making them vibrate transversely.—Röntgen rays, by A. Voller and B. Walter. As exhaustion is increased, less heat is developed in a discharge tube. The production of heat gives way before the production of Röntgen rays. The vacuum may be regulated by heating a small quantity of caustic potash in a side tube. The heating is best done by a small coil of wire carrying a current, wound on the outside of the side tube. The refractive index of diamond for X-rays does not differ from unity by more than 0·0002. This gives a limiting value for the wave-length of these rays. It is 1 $\mu\mu$, or the 600th part of that of the D line, assuming, of course, that the waves are transverse.—Co-efficient of thermal expansion of the white marble of Carrara, by I. Fröhlich. This is important, in view of its frequent use for inductance standards. Between 15° and 100° the mean coefficient of linear expansion is 0·000012.—Change of length of wooden rods with moisture and heat, by H. Stadthagen. Deal rods cut along the fibre were impregnated with linseed oil and painted with shellac varnish. The impregnation was carried out under a pressure of $1\frac{1}{2}$ atmospheres and a temperature of 65°. It was found that the process does not make the rods independent of moisture, since the smaller pores remain accessible to it. The coefficient of expansion for 1 per cent. of relative humidity is 0·00001. The American method of compression at 200° under 14 atmospheres would probably yield better results.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 19.—M. A. Chatin in the chair.—The election of M. Virchow as Foreign Associate, in the place of the late M. Tchebichef, was approved by the President of the Republic.—Establishment of a uniform state in a pipe of large rectangular section, by M. J. Boussinesq.—Researches on the state in which elements other than carbon are found in cast iron or steel, by MM. Ad. Carnot and Goutal. The attack of the metal by appropriate solvents shows that silicon is present chiefly as the silicide FeSi. If manganese is present, however, the silicon combines with it in preference to iron. Sulphur behaves similarly, all the manganese apparently being turned into sulphide before any iron sulphide is formed. Phosphorus and arsenic show remarkable differences in their behaviour towards the solvent employed (a solution of potassium-copper chloride), the phosphorus being combined with the iron forming Fe_3P, while the arsenic is uncombined and simply dissolved in the casting.—Note relating to a memoir by M. D. Eginitis on the climate of Athens, by M. Lœwy.—Ephemeris of the periodic comet of D'Arrest, by M. G. Leveau.—On the quadratic integrals of dynamics, by M. P. Painlevé.—On the integration of systems of partial differential equations of the first order of several unknown functions, by M. Jules Beudon.—On surfaces referred to their lines of zero length, by M. Eugène Cosserat.—On a practical method of setting out gear teeth, by M. L. Lecornu.—On the phenomenon of the electric arc, by M. A. Blondel. In the experiments described, the passage of the current across the carbon poles was only broken for 1/200th of a second, during which the back E.M F. of the poles was opposed to a single cell. The effect of the cooling of the carbons was thus eliminated, and the conclusion is drawn from the experiments, that the arc behaves exactly like an ordinary resistance, and presents no counter electromotive force comparable in magnitude to the difference of potential of the carbon poles.—On the action of electric charges upon the discharging power given to air by the X-rays, by M. Emile Villari.—On the properties of gases traversed by the X-rays, and on the properties of luminescent or photographic bodies, by M. G. Sagnac. A connection is traced between the rapidity of discharge of a conductor by the gases exposed to the X-rays, and the luminescence of the same gases.—The penetration of metals by the Röntgen rays, by M. Radiguet.—On the spectrum of carbon, by M. A de Gramont. A method is described for obtaining the spectrum of carbon free from foreign lines. Short intense sparks are passed through an alkaline carbonate, kept in a pasty state by a red-hot platinum spiral, the whole being placed in an atmosphere of dry carbonic acid or hydrogen. The spectrum obtained was identical with that given by Siberian graphite, with the exception of a doubtful ray in the red exhibited by the latter. Retort carbon, in spite of careful purification, gave numerous rays attributable to impurities such as calcium, barium, and iron.—Action of copper hydrate upon solutions of silver nitrate, by M. Paul Sabatier. The precipitation of cupric nitrate solutions by silver oxide appears to give rise to a basic nitrate of copper and silver.—Hydrobenzamide, amarine, and lophine, by M. Marcel Delépine. A thermochemical paper.—New syntheses with cyanosuccinic ether, by M. L. Barthe.—On some combinations of phenylhydrazine and metallic nitrates, by M. J. Moitessier.—On the aloïns, by M. E. Léger.—The function of auto-intoxication in mechanism of the death of animals deprived of their subrenal capsules, by M. D. Gourfein.—Nuclear purification at the commencement of ontogenesis, by M. L. Cuénot.—Variations of the lower fungi under the influence of the medium, by M. Julien Ray.—On the germination of grains of Leguminosæ containing parasitic larvæ, by M. Edmond Gain.

AMSTERDAM.

Royal Academy of Sciences, June 26.—Prof. van de Sande Bakhuyzen in the chair.—Prof. Bakhuis Roozeboom, on melting-lines of systems of two and three organic substances.—Mr. Hamburger read a paper on a method of separation and quantitative determination of diffusible and non-diffusible alkali in serous fluids.—Prof. van Bemmelen made, on behalf of F. Schreinemakers, a communication concerning an inquiry into equilibriums in systems of three components in which two liquid phases occur.—Prof. Kamerlingh Onnes presented a paper, by Mr. E. van Everdingen, on the Hall effect and the increase of magnetic resistance in bismuth; and, on behalf of Mr. A. van Eldik, measurements of the capillary ascent of the liquid phase of a mixture of two substances in equilibrium with the gaseous phase.—Prof. Haga presented, on behalf of Dr. C. H. Wind, a contribution entitled "On the influence of the dimensions of the source of light in Fresnel's diffraction phenomena, and on the diffraction of X-rays" (second paper). In this paper the theory developed in the first paper was applied to the case of a narrow rectangular screen for obstacle. The shadow must consist principally of a nucleus, surrounded by maxima, or else—if the screen is very narrow—an illuminated space in the middle between minima, again followed by maxima. The distance of these maxima and minima from each other renders it possible to estimate the wave-length. Experiments with rays of light, as well as with X-rays, yielded diffraction images as expected; and from this it follows—at any rate, that in the case of X-rays—λ is very small.—Prof. Franchimont, on the action between methylnitramine and potassium nitrate in an aqueous solution at the ordinary temperature. The principal products are potassium nitrate, nitrogen and methylalcohol, besides dimethylnitramine and isodimethylnitramine. Secondary products are, among others, a little carbonic acid and a very volatile substance with a strong isonitril smell. The principal reaction is regarded by the author as an addition of methylnitramine to nitrous acid, followed by a decomposition of the product into nitric acid and diazomethyl hydrate; the latter then yields nitrogen and methylalcohol, and at the same time methylates a small portion of the methylnitramine. The author further states that all acid and all neutral aliphatic nitramines, and also nitro-urea, when treated with zinc in a solutlon of acetic acid, to which α naphhtylamine, dimethylaniline, aniline, metaphenylenediamine, &c., yield colouring matters, and that these reactions closely resemble those of nitrous acid, though an examination of the colouring matters themselves gives rise to doubt whether they are due to those reactions.—Prof. van der Waals presented: (*a*) On behalf of Prof. C. A. J. A. Oudemans, a paper in which the author publishes the finding of some fungi, hitherto unknown and injurious to agriculture, as *Brachyspora pisi* on the leaves of green peas (*Pisum sativum*), *Marsonia secalis* on the leaves of rye (*Secale cereale*), *Hendersonia grossulariæ* on the leaves of the gooseberry (*Ribes grossularia*), and *Fusicladium fagopyri* on the leaves of buckwheat (*Fagopyrum esculentum*). The author further points out that the names *Helminthosporium gramineum*, Eriksson, *Helm. teres*, Sacc., and *Helm. gramineum*, Rabh., are synonymous; and that the last-mentioned, being the oldest, ought to be retained; and finally describes a new genus of *Verpa*, growing in Java on refuse of *Indigofera tinctoria*, from which the colouring matter has been extracted, and which plant is eaten by the Javanese; the author calls this genus *Verpa indigocala*. (*b*) On behalf of Prof. Lorentz, a paper by Dr. C H. Wind, on the dispersion of magnetic rotation of the plane of polarisation, with a note added by Prof. Lorentz. (*c*) On behalf of Dr. P. Zeeman, a paper on doublets and triplets in the spectrum produced by external magnetic forces (ii.).

BOOKS RECEIVED.

BOOKS.—A Course of Practical Chemistry: M. M. P. Muir. Part I. Elementary (Longmans).—Organic Chemical Manipulation: Dr. J. T. Hewitt (Whittaker).—Geographical Journal, Vol. ix. (Stanford).—A System of Medicine: edited by Dr. T. Clifford Allbutt, Vol. 3 (Macmillan).—The Potentiometer and its Adjuncts: W. C. Fisher (*Electrician* Company).—Cuirassés et Projectiles de Marine: E. Vallier (Paris, Gauthier-Villars). Les Huiles Minérales: F. Miron (Paris, Gauthier-Villars).—Physikalisches Praktikum: E. Wiedemann and H. Ebert, Dritte Verbesserte und Vermehrte Auflage (Braunschweig, Vieweg).—La Cure d'Altitude: Dr. P. Regnard Paris, Masson).

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