THURSDAY, JANUARY 24, 1895.

A BAD METHOD IN TEXT-BOOKS.

Lehrbuch der Vergleichenden Anatomie. Von Arnold Lang, o. Professor der Zoologie und vergleich-Anatomie, Zurich. 4te Abtheilung: Echinodermen und Enteropneusten. (Jena: Fischer, 1894.)

PROF. ARNOLD LANG completes, by the publication of this fourth part, his treatise on the comparative anatomy of Invertebrata. As the successive parts of this work have appeared, the author has changed to a considerable degree the limits of space which he appears originally to have contemplated, and has consequently treated those groups reserved for later volumes at greater length than that which he permitted himself to occupy in the first part of the work. We note that he now proposes to rectify this inequality by the production of new editions of the earlier part of the treatise.

Prof. Lang remarks in an interesting "Nachwort," that "man hat es vielfach getadelt"—that is to say, "complaints have been very generally made"—that the names of authors are not cited in the discussions which constitute the text of his work. I confess that I am most emphatically in agreement with those who have "getadelt"; and I do not think that Prof. Arnold Lang's contention is either correct in itself or sufficient (if it were correct), when he asserts that the book would have been double the size it is, had he given an impartial account of the historical development of the knowledge of the facts which he describes, with reference to the names of the most important authors. There are, it seems to me, three good reasons for adopting the method to which Prof. Lang objects. Firstly, such a method (namely, a historical method not carried beyond the citation of the works of the chief contributors to surviving doctrine) is the most natural for the mind of the student to follow, and gives him the truest appreciation of the present condition of knowledge on any topic, and of its probable future development; secondly, such a citation of the names of really surviving authorities—that is to say, of authors whose work is at this moment admitted as being the original and approved source of observational record, or theoretical conception still holding its ground—is, as a mere matter of bibliographical reference, of far more service to the student than an indiscriminate list of memoirs at the end of a chapter (such as Prof. Lang gives) without any indication of the contents of the memoirs named; thirdly, that such a citation is the bare justice to his predecessors and contemporaries which every teacher of zoology (or other advancing science) should feel bound to accord, and should, I venture to say, gladly and scrupulously take trouble to ensure for those whose original work he has appropriated and accepted.

I confess that I feel sensible of something ungenerous and even unfair when I read the long and careful statements as to the skeletal and other systems of the Echinoderms (300 pages), illustrated by admirable copies of other naturalists' drawings, made in the present volume by Prof. Lang, without citing in his text a single name of those to whom he and the world in general are indebted for all this knowledge. After all, it may well be questioned whether any man of science is justified in making statements, even in a text-book, as though he himself had investigated and was responsible for the accuracy of these statements in virtue of his own observations on the objects described, when all the time he is simply stating what this man and that man have seen, and he has not seen, though he omits to mention the name of any of those to whom he is indebted. When such a method is adopted, one is quite unable to distinguish the individual opinions of the author from the mass of second-hand information which he pours out. A very simple introduction of the phrases, "as was first shown by X," or "according to the observations of Y," or again, "an observation which I can confirm, as it has been called in question," would alter the whole significance of Prof. Lang's discourse, and make it not only much more valuable, but much more interesting. At the same time, let me hasten to say that every one will at once accept Prof. Lang's statement, that his object in ignoring the names of zoologists has been to secure brevity. My contention is that the end has not justified the means.

Take, for instance, the so-called "apical" nervous system of the Crinoids. The discovery of this remarkable and altogether improbable nervous system by that staunch and unwearied naturalist, Dr. W. B. Carpenter, in his old age, is one of the most delightful episodes in the history of comparative anatomy. He made the discovery after he was seventy years of age, and no one believed that his interpretation of his observations was correct when he first announced it. Old as he was, he lived to see his discovery confirmed and accepted on all sides. This extraordinary nervous system is described at some length, and explained by figures in Prof. Lang's book; yet the name of Dr. Carpenter is not once mentioned in connection with it, and the figures given (p. 966) are taken from and ascribed to Beyrich (!) and Carpenter's son Herbert, who merely worked out, long after his father's complete publication, some details of the old man's discovery. On the other hand, the mere accident that the cœca of the lantern-membrane, discovered by Prof. Charles Stewart in the Cidaridæ, have no special name, results in a recognition of his interesting discovery by the title of the "Stewartschen Organen."

The unsatisfactory character of Prof. Lang's method, if we regard his book as one for reference and assistance in tracing the more recent discoveries, is well exhibited in the section of little more than one page (pp. 1036, 1037) on the axial organ of Echinoderms (dorsal organ, heart, pseudo-heart, renal organ, plastidogenous gland, ovoid gland, lymph gland). Here the space assigned by the author seems to be singularly out of proportion to that given to other topics, and we get not only the very scantiest description of the axial organ in Asterids, Ophiurids, Echinoids, and Crinoids, but no indication or reference *whatever* to ampler sources of information. Since this is really one of the critical subjects of recent investigation in Echinoderm anatomy, it is to be regretted that Prof. Lang says so little about it.

Whatever faults one may find with Prof. Lang's book, there is no doubt that it also has merits, and will be found to contain a survey of Echinoderm morphology more extensive than that accorded to the morphology of other groups by the same author, as well as some useful original diagrams, such as that of Pentremites, on p. 968.

E. RAY LANKESTER.

THE STUDY OF ROCKS.

Lehrbuch der Petrographie. Von Dr. Ferdinand Zirkel, Ord. Professor der Mineralogie und Geognosie an der Universität Leipzig. Zweite gänzlich neu verfasste Auflage. Three vols. 8vo. Pp. 2619. (Leipzig: Engelmann, 1893–94.)

THE appearance of a second edition of Prof. Zirkel's admirable "Lehrbuch der Petrographie" is an event of no little importance in the history of geological science. The part played by the author of this work in developing the methods of microscopic analysis, as applied to rocks, is too well known to require recapitulation in this place, and the long series of petrographical memoirs with which he has enriched geological literature—dealing with the rocks of the North American as well as with those of the European continent—are familiar to all students. While a great number of works treating of the microscopic study of rocks (including the author's own "Die mikroscopische Beschaffenheit der Mineralien und Gesteine") have appeared during the twenty-eight years which have elapsed since the first edition of the "Petrographie" was published, there is not one among them that quite occupies the place of that excellent treatise—with its wealth of information on the history and development of petrographical nomenclature.

The rapidity of growth of this branch of geological science during the past thirty years, is brought out in a very striking manner by a comparison of the first and second editions of this standard work. The first edition consisted of two thin volumes with an aggregate of 1241 pages; the second edition forms three bulky volumes with 2619 pages. But this is not all: it is evident to any one who peruses these volumes, that, in spite of the employment of more than double the number of enlarged pages, with much small type introduced, the author has found it impossible to discuss in all their aspects the views of previous authors with the completeness and comprehensiveness that were so remarkable in the first edition of the book. The student may be satisfied that Prof. Zirkel has overlooked little or nothing of importance in the literature of his subject; but, not unfrequently, it will be noticed that in his attempt to deal as concisely as possible with this vast mass of literature, he has made statements with respect to the views of the authors quoted, which are scarcely borne out by a reference to the memoirs themselves. While, therefore, this new edition will be invaluable in supplying ample references to petrographical literature, it will not in any way obviate the necessity of consulting the original memoirs.

As indicating the fulness with which the subject is now treated, we may mention that the number of pages dealing with "General Petrography" has increased from 171 to 634. The account given of the optical properties of minerals and of the structure of rocks, as made out by the aid of the microscope, is naturally responsible for a large part of this increase; the description of the common rock-forming minerals, which was comprised in forty pages in the first edition, now requiring no less than 291 pages. The greatest defect in this part of the work will be found in the absence of illustrations. Of this the author is fully sensible, as will be seen from a reference to his preface; but, as he justly pleads, the addition of illustrations could not fail to add to the bulk and cost of a book that has already grown to encyclopædic dimensions.

Every student of geology will naturally examine the work with the desire to learn what are the present views of so great an authority as Prof. Zirkel on the vexed subject of rock-classification and nomenclature. In the first edition of the book, our author, following the plan of most German writers upon the subject, attempted to class rocks according to their structure and mineralogical constitution, quite irrespectively of their origin, into simple crystalline rocks (ice, rock-salt, quartz-rock, limestone, &c.), compound crystalline rocks of granular and schistose character respectively, and clastic, or fragmental, rocks. In the second edition, he departs from this method, and commences the descriptive portion of the book with an account of the "Massige eruptive Erstarrungsgesteine," which occupies no less than 1292 pages. This is followed by an account of the "Krystallinische Schiefer" (275 pp.), the "Krystallinische oder nicht-klastiche Sedimentgesteine" (230 pp.), and the "Klastichen Gesteine" (125 pp.). It will thus be seen that the primitive classification into crystalline (simple and compound) rocks and clastic rocks, has been abandoned for one in which account is taken of their mode of origin.

In dealing with the great class of igneous rocks, Prof. Zirkel has also introduced some modifications of his original method. In 1866, he grouped these rocks, according to the nature of the alumino-alkaline silicate present in them, into Orthoclase-rocks, Oligoclase-rocks, Nepheline- and Leucite-rocks, Labradorite-rocks, Anorthite-rocks, and rocks without felspathic constituents. The obvious objections to this classification were: (1) That many rocks contain several distinct species of felspar, notably in their porphyritic constituents, and in their groundmass respectively; and (2) that geologists possess no simple, infallible, and easily applied test for ascertaining the exact species of felspars present in rocks. In 1873 (in his "Mikroskopische Beschaffenheit der Mineralien und Gesteine"), Prof. Zirkel abandoned this classification for the simpler division of felspar-bearing rocks into Orthoclastic and Plagioclastic. This classification, which is facilitated by the general presence of twin striation as an easily recognised distinction of the plagioclases, has now been very generally adopted by petrographers. In the work before us, however, the author divides the felspar-bearing igneous rocks into two series, distinguished by the predominance of an "alkali felspar" or of a "soda-lime felspar" respectively. It does not appear to us that anything is gained by this new departure, which will compensate for the admitted difficulty of applying the test for the discrimination of the two classes. Another change in classification which will interest English readers is that, while the separation of volcanic rocks into two series, the pre-tertiary (palæo-volcanic) and post-tertiary (neo-volcanic) is maintained, the Plutonic rocks are admitted to be of all ages.

Geologists will, alas, look in vain in this work for any indication that they may hope for a speedy termination of the terrible confusion that has so long prevailed with respect to petrographical nomenclature. On the contrary, they will find that in addition to having to reckon with the schools of Paris and Heidelberg, as they have done in the past, they will now have to take account of a third—that of Leipzig! With some of Prof. Zirkel's criticisms of contemporary palæontological literature, English and American geologists will heartily sympathise. The employment of such terms as granitite, granophyre, &c., with significations different from those given to them by the authors of th names, cannot but fail to lead to almost endless confusion, and we are glad to see that the authority of Prof. Zirkel is thrown into the scale against such principles of nomenclature being adopted; but in other cases we cannot but think that his objections to the nomenclature of other authors are not likely to be sustained by future workers in this branch of science.

Whether the confusion that now exists can be removed by any friendly discussion between the representatives of rival schools—such as those of the international committee proposed at the late Geological Congress at Zurich—time alone can show. If this be impossible, and writers in France and Germany, respectively, continue to ignore the terminology employed in other countries than their own, then it appears to us that, if science is to maintain her cosmopolitan character, only one method of escape is possible. We must follow the example of the other natural-history sciences in adopting the test of *priority* as absolute and final in our terminology of rocks. That many inconveniences must result from such a course may be readily admitted; and it will not be easy to fix upon the Linnæus of our science—or to decide upon the date at which exact petrographical literature may be supposed to have commenced. But almost any trouble and difficulty of this kind is worth encountering, if we may hope that geologists in the future will, in speaking of rocks, attain that great desideratum of "one thing—one name."

In the meanwhile, we are not ungrateful to the author of the work before us for the enormous labour and pains he has taken in wading through the great mass of petrographical literature; in furnishing us with correct statements concerning the origin and history of terms; and in placing on record the decisions he has arrived at upon many of the difficult problems that confront us. The "Lehrbuch der Petrographie" has always been a standard work of reference; and, in its new form, it has become more indispensable than ever. J. W. J.

OUR BOOK SHELF.

Pithecanthropus Erectus, eine Menschenaehnliche Uebergangsform aus Java. By E. Dubois. 4to, pp. 40, illustrated (Batavia, 1894.)

JAVA, from its geographical situation, being just one of those countries where the remains of a connecting form between man and the higher apes would be extremely likely to occur, zoologists have naturally been attracted by the title of the work before us, which proclaims in no uncertain tones that such a missing link has actually been discovered. A feeling of disappointment will, however, probably come over the student, when he finds how imperfect are the remains on the evidence of which this startling announcement is made; and when he has submitted them to a critical examination, he will probably have little difficulty in concluding that they do not belong to a wild animal at all. The specimens described are three in number, and were discovered in strata of presumed Pleistocene age near a spot called Trinil. The first of these is a last upper molar tooth, found during the drying-up of a river-bed in the autumn of 1891. A month later, the roof of a large cranium was discovered in the same bed, at a distance of only about a yard from the spot where the tooth laid. Finally, in August 1892, at a distance of some sixteen yards higher up the stream, a left femur was disinterred, which is stated to present much more human resemblances than either of the other two specimens. The bed from which this bone was derived is stated to have been the same as that from which the other two specimens were obtained. The author is confident that all are referable to a single animal; and we are content to accept this view.

Especial stress is laid on the femur as indicative of human affinities; and here again we are in agreement with the author, only we would go one step further, and say that it actually is human. As is pointed out in the text, this bone has a large exostosis below the lesser trochanter; and we believe that such slight differences as it shows from normal human femora, are due to this diseased condition. With regard to the skull, which shows a marked human facies, but an extremely small development of the brain-cavity, the absence of ridges on the calvarium clearly shows that it can belong to no wild anthropoid; and there appears every reason to regard it as that of a microcephalous idiot, of an unusually elongated type. The molar, so far as we can see from the figure, may likewise perfectly well be human.

Haeckel's "*Pithecanthropus*" may, therefore, be relegated to the position of an hypothetical unknown creature for which it was originally proposed; while the specific name "*erectus*" must become a synonym of the frequently misapplied "*sapieus.*" R. L.

The Planet Earth. An Astronomical Introduction to Geography. By R. A. Gregory, F.R.A.S. (London: Macmillan and Co., 1894.)

IT is, perhaps, one of the consequences of the antiquity of astronomy that it is not now usually presented to the youthful mind in a thoroughly scientific manner. The established truths of the science, in so far as they concern the earth's place as a planet, though once so astounding to mankind, are now so commonplace that the educational advantages of a study of the phenomena which brought them to light are frequently overlooked altogether. As in the case of geography, information rather than education appears to be the principal aim of astronomical teaching when it is not carried beyond the elementary stage which it reaches in schools; although, when properly handled, there is no subject better calculated to lead the mind into a scientific groove.

We therefore cordially welcome this attempt to indicate the lines which should be followed for a profitable study of that portion of astronomy which deals with the earth as a planet. The bald statements as to the earth's dimensions and movements, so frequently appearing in the text-books of geography, furnish the sole astronomical knowledge which many acquire; but they are, as Mr. Gregory remarks, quite inadequate. The design of the little book before us, is first to direct the students' attention to observations which they may generally make for themselves, and then to show how such phenomena can be accounted for. Thus, in the first chapter, a concise account is given of the means of naming and identifying stars, sufficient to make possible an intelligent observation of the diurnal motion of the celestial sphere; and in the next chapter, it is shown that the observations can be explained by regarding the earth as a spinning globe. The same method is followed throughout.

On the whole, the subject-matter has been judiciously selected, but a slight want of proportion is shown in introducing explanations of the phases of Mercury and Venus, while those of the moon are not referred to at all. The chapter on the determination of the size of the earth would have been a little more educational if the description of the methods employed had been accompanied by hints as to the amount of playground surveying which is possible by the use of a protractor and foot-rule.

The book forms an admirable introduction to astronomy, which stands a fair chance of fulfilling the author's hope "that this little book will help to revive the observational astronomy of pre-telescopic times." This branch of astronomical knowledge is certainly not without danger of being neglected in favour of the fascinating and rapidly-advancing study of the results obtained by the use of the camera and spectroscope. The explanations are models of clearness and accuracy, and the diagrams illustrating them are excellent. Many of them are new, and involve original ideas of the author; as, for instance, a diagram illustrating the sun's apparent path in winter and summer, and another showing the principle of Foucault's pendulum by a lecture experiment. Teachers of geography and physiography will do well to make themselves familiar with Mr. Gregory's methods.

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 Hodgkins Prizes.

THE time for the reception of treatises or essays offered in competition for the Hodgkins Fund Prizes of 10,000 dols., of 2000 dols., and of 1000 dols. respectively, closed on December 31, 1894, and all papers so offered are now in the hands of the Committee of Award.

In view of the very large number of competitors, of the delay which will be necessarily caused by the intended careful examination, and of the further time which may be required to consult a European Advisory Committee, if one be appointed, it is announced that authors are now at liberty to publish these treatises or essays without prejudice to their interest as competitors. S. P. LANGLEY.

Washington, January 10.

The Artificial Spectrum Top.

AS the spectrum top is exciting a good deal of interest at the present moment, perhaps I may be allowed to record some experiments which I have made with a view of arriving at a solution of the colour problem which it sets. I have observed the colours produced by the white light of the positive pole of the electric arc, and also by monochromatic light produced by means of my colour-patch apparatus. The top was rotated on a horizontal axis at any desired speed by means of an electromotor. The following colours were observed (No. 1, No. 2, No. 3, and No. 4 are the triple lines in order from the centre of rotation):

White light.

No. 1. Crimson.
No. 2. Olive green.
No. 3. Grey (slightly violet).
No. 4. Dark violet.

(When the yellow light of gas is used, the above results would be modified).

Red (C light).

No. 1. Red.
No. 2. Lighter red.
No. 3. Very light olive green.
No. 4. Darker olive green.

Green (Magnesium b).

No. 1. Bluish-green.
No. 2. Lighter bluish-green.
No. 3. Same as No. 2.
No. 4. Ruddy black.

Blue (near the blue lithium).

No. 1. Grass green.
No. 2. Lighter grass green.
No. 3. Same as No. 2.
No. 4. Ruddy black.

Violet (all the violet of the spectrum).

No. 1, 2, and 3. Light violet.
No. 4. Darker violet with a suspicion of red.

When a red a little below the red lithium line was employed, all the groups appeared dark red, and as in the 3 sensation theory this part and the violet are simple sensations, the results obtained in these last, were to be expected.

The next two series are interesting, observations being made in white light compounded by the mixture of two simple colours.

Mixture of red and green to make white.

No. 1. Indigo-blue.
No. 2. Reddish orange.
No. 3. Same as No. 2.
No. 4. Darker orange.

Mixture of yellow and blue to make white.

No. 1. Sky blue.
No. 2. Sage green.
No. 3. Same as No. 2.
No. 4. Bluish-black (perhaps black).

These results were confirmed by an independent observer When the rotation was reversed the same order of colours was observed, but in the reversed order. These observations seem to confirm the original opinion I had formed regarding these phenomena.

Bearing in mind that none of the observed colours in the lines are pure colours, but mixed with a certain quantity of white, and are seen on a more or less dark ground, then if the order of persistency of the three colour sensations be violet (blue), green, red, the results would be as given above. Should this be so, the velocity of rotation must alter the position of the colours seen in white light, the violet being the last to be seen on No. 1 when rotated more rapidly, and this is the case. The effect of contrast also has to be taken into account.

I have made a good many more experiments under varying conditions of position and dimensions of lines and proportions of black to white; and it seems possible that this toy, when modified, may be adapted to give valuable information as regards certain problems in colour vision. W. DE W. ABNEY.

WE have recently made a few experiments bearing on the phenomena exhibited by Mr. C. E. Benham's artificial spectrum top (see NATURE, November 29, 1894, p. 113), and the explanation of them suggested by Prof. Liveing (NATURE, December 13, 1894, p. 167), and have obtained results which we believe to be novel and of some scientific interest.

In the first place, if Prof. Liveing's explanation be correct, there seems to be no reason why the same effects should not be obtained with broad bands instead of lines, the bands being drawn in precisely the same manner as the lines upon the white half of the disc.

It appeared to us of some importance to determine if this were really the case, as we thought the effects obtained with Mr. Benham's top might possibly be due to irradiation being different in amount for the different colours, while the change in colour with reversion in direction of rotation was presumably due to the black lines succeeding, upon a given part of the retina, a previously white ground in the one case and a previously black one in the other.

If this were so, by using broad bands instead of lines, one would expect to get a coloured line at each border, while the central portion remained uncoloured. Prof. Liveing's theory would lead one to expect a uniformly coloured band.

Accordingly, we prepared a disc, one half of which was blackened and the other half left white, as in Mr. Benham's top, and on the white half were described three black circular bands about one centimetre broad, with radii of about 4·7 and 10 centimetres respectively, and each with an arc of 45°. The outer band was described in an opposite sense to the two inner bands, that is to say, in such a manner that if the disc was rotating so that the order of succession for the two inner bands was black field, bands, white field, for the outer band it was white, bands, black. The disc was rotated by attaching it to the spindle of an electromotor, the speed of which could be graduated by friction against the spindle.

On rotating the disc at a slow speed, in such a direction that the order of succession for the two inner bands was black, bands, white, the following results were obtained:—The two inner bands were each bounded on both inner and outer edges by a bright red line, fading off towards the centre in a dark, somewhat reddish, ground, which became less coloured and darker as it approached the centre. The outer band presented a marked contrast to these two; the whole extent of the band itself was black, entirely free from colour, but on the white ground on both borders of the band, and apparently outside it, there appeared a brilliant coloured band, varying in colour from blue to green. On reversing the direction of rotation, the appearances were exactly reversed; the inner bands now became black bordered by green, and the outer band reddish black lined by bright red.

We do not contend that this experiment proves that the effect is one of irradiation, for it might easily be supposed that the heightened effect at the border is one of contrast with the surrounding whiter coloured field. Our next experiment, on the effect of change in the speed of rotation, also tends to negative the idea that the colours are due to irradiation, as there is no reason to think that if irradiation were the cause, there would be a change in colour with a change in the speed of rotation, as was found to be the case.

To test the effect of change in the speed of rotation, a disc of one of Mr. Benham's tops was detached, fixed to the spindle of the electromotor, and rotated in such a direction as to cause the three central bands to appear red with a slow speed of rotation. On gradually increasing the speed, a remarkable series of changes in appearance presented itself. The bright blood-red of the three inner lines gradually grew darker and duller, and then passing rapidly through a transition, the shades of which we were unable to observe, gave place to a most vivid green, which in turn, with still increasing speed, passed through another transition stage into blue, deepening into a full violet at the greatest speed we could obtain. On causing the motor to slow down, the same changes in an inverse order from violet to red were observed. These changes in colour *with the same direction of rotation* are very remarkable, and seem to us to be in direct opposition to Mr. Benham's explanation supplied with the top; for if the colours are due to a certain percentage of the etherial vibrations being cut off, this percentage will remain the same for all speeds, and there is no reason apparent why there should be a change in colour with difference in speed. Neither are they easily explainable on Prof. Liveing's theory that red is the first colour to appear, and blue the last to disappear; also, the green we got at the intermediate rate was certainly not a neutral grey or green, but a pure vivid green. Probably other observers have not used a greater speed than that of the first transition stage from red to green, which has a kind of neutral green tint.

An experiment was next made with a disc constructed similarly to that of Mr. Benham, but having white lines drawn on the black semicircle instead of black lines on the white part. On rotating this disc so that *white* lines on *black* ground succeeded the black surface, with slow rotation the colour obtained was red, but a different kind of red to the deep blood-red given by Mr. Benham's top in the case of *black* lines on a *white* ground also following a black surface, viz. a very bright red, evidently not saturated. With higher speeds there followed a light green and light blue, both evidently containing white. The difference in hue of the two series of colours seems obviously that in the case of the white-lined disc the colours are mixed with white, and in that of the black-lined are mixed with black.

Throughout the series of experiments we have tried to eliminate psychical errors as much as possible, by experimenting on persons unacquainted with the expected results.

Belfast, January 15. J. M. FINNEGAN.
B. MOORE.

The Kinetic Theory of Gases.

THE difficulty of reconciling line spectra with the kinetic theory of gases, has been referred to by Prof. Fitzgerald (NATURE, January 3, p. 221). The following considerations show that it is possible under certain suppositions to have a number of spectral rays with a very restricted number of degrees of freedom. Most of us, I believe, now accept a definite atomic charge of electricity, and if each charge is imagined to be capable of moving along the surface of an atom, it would represent two degrees of freedom. If a molecule is capable of sending out a homogeneous vibration, it means that there must be a definite position of equilibrium of the "electron." If there are several such positions, the vibrations may take place in several periods. Any one molecule may perform for a certain time a simple periodic oscillation about one position of equilibrium, and owing to some impact the electron may be knocked over into a new position. The vibrations under these circumstances would not be quite homogeneous, but if the electron oscillates about any one position sufficiently long to perform a few thousand oscillations, we should hardly notice the want of homogeneity. Each electron at a given time would only send out vibrations which in our instruments would appear as homogeneous. Each molecule could thus successively give rise to a number of spectral rays, and at any one time the electron in the different molecules would, by the laws of probability, be distributed over all possible positions of equilibrium, so that we should always see all the vibrations which any one molecule of the gas is capable of sending out. The probability of an electron oscillating about one of its positions of equilibrium need not be the same in all cases. Hence a line may be weak not because the vibration has a smaller amplitude, but because fewer molecules give rise to it. The fact that the vibrations of a gas are not quite homogeneous, is borne out by experiment. If impacts become more frequent by increased pressure, we should expect from the above views that the time during which an electron performs a certain oscillation is shortened; hence the line should widen, which is the case. I have spoken, for the sake of simplicity, as if an electron vibrating about one position of equilibrium could only do so in one period. If the forces called into play, by a displacement, depend on the direction of the displacement, there would be two possible frequencies. If the surface is nearly symmetrical, we should have double lines.

The only weight I attach to these speculations lies in the illustration it affords that a number of spectral lines does not necessarily mean an equal number of degrees of freedom. In the existence of the "electron" I firmly believe; and this necessarily implies a very restricted number of variables

ARTHUR SCHUSTER.

"Acquired Characters."

IT would appear that Prof. Lankester has not thought it worth while to read all the letters that have appeared in NATURE on the question raised by Sir Edward Fry, unless it is to be inferred from his remarks that he confines himself to the consideration of the arguments of those who have a place on the scientific Olympus of the Royal Society. In my letter, published December 6, I defended Lamarck's laws against the accusation that they were reciprocally destructive. Prof. Lankester reiterates his accusation without any further support. But this is not the whole question. In his last letter he suggests that acquired characters corresponding to Mr. Galton's definition should be taken, and an investigation made as to whether they are inherited or not in later generations. But in his former letter (November 29) he suggested very distinctly and deliberately that such an investigation was unnecessary, because the question was already settled. He has already condemned the heretic, and now consents to his trial. His words were—"Since the old character had not become fixed and congenital after many thousands of successive generations of individuals had developed it in response to environment, but gave place to a new character when new conditions operated on an individual, why should we suppose that the new character is likely to become fixed after a much shorter time of responsive existence?" To apply this once more to the case of pigment in relation to light. For thousands of generations no pigment has been developed, say, on the lower side of a flat fish, no light having fallen on it. The skin is experimentally exposed to light, and pigment appears: therefore the acquired character of absence of pigment, after thousands of generations, has produced no hereditary change, has not altered the potentialities of the tissue. The argument is fallacious, because the question of how much pigment is entirely ignored, and also the question how long the development of pigment experimentally takes. The force of the argument is entirely on the other side. Assume in this case, as Prof. Lankester does in his general argument, that the old character, the absence of pigment, is an acquired character. Then experiment has shown that this character is inherited: that is to say, the action of light obviously overcomes a resistance in producing pigment, and after years does not produce as much as on the upper side is present from the beginning. This resistance can be nothing else than heredity, the inheritance of a tendency to pigmentlessness. Therefore the acquired character is inherited. It is undeniable and indisputable that the argument propounded by Prof. Lankester proves the inheritance of acquired characters, if it is properly applied in accordance with the facts. This is on the assumption that the "old characters" are acquired. If they are not acquired, the argument has no force at all. The facts allow us to say that the tendency to pigmentlessness, or the resistance to the development of pigment on the lower side of a flounder, is certainly inherited, but whether or not it is due to the absence of light during many successive generations we do not know. As Sir Edward Fry says, if we by definition confine the term "acquired character" within the limits of an individual history, then of course an acquired character can never be inherited. The question is whether the conditions which produce a change in the individual can affect the offspring? The experimental investigation must take the following course. Suppose a given amount of stimulation X to act upon individuals in successive generations, producing in the first generation a result x. Then the question is if X remains the same, does x remain constant or not? If there is no inherited effect, then x must remain constant in all succeeding generations. If x increases by some amount, however small, and becomes $x+a$, then a is not acquired by the individual, but inherited, and it is clear that the result will go on increasing to $x+2a$, $x+3a$, and so on to $x+na$, where n represents the number of generations. In my own opinion, there is evidence that something of this kind does occur, though definite investigations are much to be desired.

Plymouth, January 11. J. T. CUNNINGHAM.

As one who has been reading the discussion in your pages on the meaning of the term "acquired characters," I may perhaps be permitted to direct attention to the history of the term. It was first used with reference not to species but to individuals. Every character of an individual is either derived from the fecundated ovum or acquired during life. This was obvious; and the question arose: Could acquired characters be transmitted? As long as the term is applied to an individual, it has that kind of precision which is desirable in all scientific terminology, namely, that it perfectly explains itself.

Glasgow, January 12. JOHN CLELAND.

Chinese Theories of the Origin of Amber.

IN my letter on "Some Oriental Beliefs about Bees and Wasps" (NATURE, vol. l. p. 30, May 10, 1894), I have traced the origin of the Chinese belief in the production of amber from bees into the presence in amber of hymenoptorous remains. Apparently developed from this belief, there is another misconception recorded by Cháng Hwá (killed 300 A.D.), whose passage on the subject reads as follows: "In 'Shinsien-chuèn,' it is said, the resins of the pine and arbor-vitæ, after remaining underground for one thousand years, are turned into *Pachyma cocos* (Fuh-ling),[1] which is turned into amber.' Notwithstanding this statement, the Mount Tai produces Pachyma, but no amber; whereas Yung-chang . . . produces amber, but no Pachyma. Another theory is that amber is made by burning the honey-combs. Which is true of these two theories is not yet decided."[2]

Of all Chinese theories propounded to account for the origin of amber, the most veracious one is given in Li Shi-Chin's work,[3] thus: "Amber originates in the resin of pines; when the pines, with their branches and knots luxuriantly growing, were heated by the sun, the resin came out of the wood; it coagulated after days and sunk underground, and after undergoing subterraneous changes, left behind the lustrous substance [which is amber]. In this condition still it has in it the tenacity of resin, so that when it is rubbed and warmed between the palms, it can pick up particles of dust. Those insects in its enclosure had cohered with it before its sinking underground."

Besides the resin of pines, the exudation from the "Fung" (*Liquidambar Maximowiczii*) is asserted by Kán Páu-Shing (lived in the tenth century A.D.) to be a nascent form of amber,[4] the opinion well coinciding with the Western idea that has given to styrax the name "Liquidambar."[5]

In "Shi-shwoh" (written in the fifth century A.D.) amber is said to be formed from the subterraneous metamorphosis of the gum of peach trees,[6] which reminds us of the simile, "Like gum from the cherry," used by Pliny in his exposition of the resinous origin of amber.[7]

Some other theories are full of absurdity. One of these holds that the dragon's blood buried underground turns to amber, and the demon's to agate.[8] Also, the etymological origin of "Hú-pèh," the Chinese name for amber, is involved in myth. In ancient times this word was written in two letters, together signifying "Tiger's Soul," which is explained in this way: "At night the tiger applies its one eye for illumination, and another for vision. When it is shot with arrow the light of the eye, which is the tiger's soul, sinks underground, and turns into a white stone. . . . Amber resembles this stone; hence the name."[9]

According to "Hwái-nán-tze" (written in the second century B.C.), "the dodder is the outgrowth of amber."[10] Almost inexplicable as this story may appear, I have found certain clues to its elucidation. Káu Yú (lived in the second century A.D.) gives "Nü-ló" (*i.e. Usnea longissima*)[11] as a synonym of "Tú-sze" (*i.e.* the dodder).[12] From this it is evident that the early Chinese have confounded Usnea with dodder—the confusion caused by the superficial resemblance and similar habitats of the two plants.[13] Now, there is a Chinese belief recorded about 240 B.C., that *Pachyma cocos* is the root of dodder,[14] which has doubtless grown out of the common occurrence upon and under the pines of the Usnea and Pachyma. And as this Pachyma had been held as an intermediary phase through which resins were to pass into amber (see above), it would seem that the story which affirms the dodder to be the outgrowth of amber, was not inconsistent with the understanding of the early Chinese theorisers. KUMAGUSU MINAKATA.

January 11.

[1] Identified thus in Dr. K. Itô's "Nihon Sambutsu-shi," part vii "Pŏh-wuh-chi," tom. iv., sub. "Yŏh-wuh.'

[3] "Pan-tsau Káng-muh," 1578, art. "Hú-pèh."
[4] *Ibid.*
[5] Loudon, "Encyclopædia of Plants," 1880, p. 798.
[6] Twan Ching-Shih, "Yú-yáng Tsáh-tsú," tom. xi.
[7] "Natural History," English translation, Bohn's edition, vol. vi., p. 401.
[8] Twan Ching-Shih. *loc. cit.*
[9] "Pan-tsau Káng-muh," *loc. cit.* and art. "Hú."
[10] Twan Ching-Shih, *ubi supr.*
[11] Identified thus in Dr. M. Miyoshi's article in the *Shokubutsugaku Zasshi*, No. 31, p. 435, Tōkyō, Dec. 10, 1889.
[12] "Lŭ-shi Chùn-tsiú," Japanese edition, N.D., tom. ix. p. 9, Káu Yú's note.
[13] Cháng Hwá appears to have well distinguished the two plants. He says, "Usnea lives upon the dodder, and the dodder upon trees." "Pŏh-wuh-chi," *loc. cit.*
[14] "Lŭ-shi Chùn-tsiú, *loc. cit.* text.

Rhynchodemus Terrestris in Germany.

IN NATURE of October 25, 1894 (p. 617), Mr. Scharff mentioned *Rhynchodemus terrestris*, as stated, in Germany, near Würzburg, by Semper. It would seem that the worm was exceedingly rare. But I found it repeatedly at several points of Saxony and Thuringia, in the mountains and in the plain, in leaved and fir wood, under moss or dead leaves. Sufficient attention would detect it without doubt in many regions. Recently Mr. Ehrmann found several specimens feeding on a dead *Arion empiricorum*. H. SIMROTH.

Leipzig.

The "Proceedings of the Chemical Society."

THE title-page and index of this periodical have just come to hand, and on the title-page occur the words, "Edited by the Secretaries." It appears to me right that authors should know the precise significance of these words, as lately determined by the Council of the Society.

Two courses appeared to be open: either to submit proofs to the authors of the abstracts of their papers sent to the Society, if any substantial (*i.e.* more than typographical) alteration had been made; in which case, the authors themselves would naturally bear the responsibility of their statements; or to throw the whole responsibility on the Editors, leaving them to make any excisions or alterations they may choose in the abstracts sent to them; or indeed, if they so think fit, entirely to rewrite them. The Council, in order to secure rapid publication, have chosen the latter alternative; and it should be understood that the abstracts are now "official"—*i.e.* the responsibility for all statements put forth rests solely on the Editors of the *Proceedings*. WILLIAM RAMSAY.

University College, London, W.C., January 14.

Philosophy and Natural Science.

WHILST feeling obliged to your reviewer's appreciation of my essay (p. 220), I am bound to rectify some very glaring discrepancies.

(1) As plainly stated in my preface, my essay has *not* obtained the Philosophical Society's prize, but only an "honourable recognition," and two fifths of the prize sum.

(2) Eighth line from bottom (p. 220), for "physical," read "*psychical*," as said in my paper (p. 30).

(3) Your reviewer makes me say: "Physical development is not the cause, but the effect of psychical development"; whereas, I have expressedly *combated* this view of Wundt's (p. 32).

(4) Neither did I say: "The modifications in the brain and nervous system throughout the animal kingdom are intelligible as resulting from psychical causes . . ." but only (p. 32) that *in many cases* the beginnings of modifications are intelligible from the psychical side—*e.g.* the modifications of many organs—resulting from sexual selection.

(5) Lastly, far from saying that the high mental position of man, on the one hand, and of ants on the other, "is independent of the structure of the nervous system," my sentence (p. 34) is: "Here, where the organic substratum (*i.e.* the brain) in both types differs even in its principal morphological features, it is most evident how occult are the processes which constitute the proper material side of psychical phenomena."

Freiburg, Badenia, January 5. DAVID WETTERHAN.

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(1) THE facts are that the Philosophical Society of Berlin offered a prize of 1000 marks for an essay on "The relation of philosophy to the empirical knowledge of nature."

The essay reviewed, only obtained 400 marks of this prize, and an honourable mention. In a hasty glance at the preface I overlooked the words "ein Antheil von vierhundert Mark," which occur in the next line to "der als Preis ausgesetzten Summe," which caught my eye.

(2) This is evidently a slip of the pen, which I regret was overlooked in proof.

(3) In my notes, jotted down as I read the pamphlet, I put Wundt's words in quotation marks, intending to point out Mr. Wetterhan's opposition thereto; but in writing the review, I unfortunately omitted the commas, and, I regret, entirely misrepresented the author's views. Perhaps I may quote from p. 32 of the pamphlet: "Man durfte Wundt's Satz, 'dass die physische Entwicklung nicht die Ursache, sondern vielmehr die Wirkung der psychischen Entwicklung ist,' zu weitgehend, und auch in seinen Konsequenz bedenklich finden." We are then referred to page 46, where we read: "Der Ausführung dagegen, welche Wundt (s.o. p. 32) jenem Prinzipe gegeben hat, vermag ich kaum eher beizustimmen, als der verwandten Ideen Schopenhauer's."

(4) It appears to me that the passage will bear the construction which I put upon it; though perhaps "throughout" the animal series is too inclusive as a rendering of "der Tierreihen."

(5) The author had been discussing the similarity of habits and instincts in ants and termites, and then remarks that there is a distinct agreement in the mental functions ("von geistigen, ja gemütlichen Funktionen") of bees with those of the higher animals. He refers to Darwin's opinion that the small brain of a bee is a more wonderful thing than the brain of a Man: and I think I was entitled to make the obviously true remark that this "mental development is independent of the structure of the nervous system." I was not quoting Mr. Wetterhan's words, but giving the general sense of the passage.

In conclusion, I must express my regret that the condensation of some of the author's remarks should have resulted in a confused expression of his views. THE REVIEWER.

SOME EARLY TERRESTRIAL MAGNETIC DISCOVERIES PERTAINING TO ENGLAND.

IT should be a source of considerable pride to British men of science that so many of the discoveries in terrestrial magnetism have been made in England. And yet, owing to the absence of a complete and carefully written history of the development of this science, probably few could enumerate all the achievements in this subject by Englishmen.

In February 1893 the writer had the good fortune to light upon a book,[1] by Will Whiston, containing matter pertaining to the terrestrial magnetism of England, which appears to have been entirely overlooked by prominent terrestrial magneticians. Owing to pressure of work, this interesting book, of which a copy was found in the Royal Library of Berlin, could not be subjected to a critical examination until the early part of 1894, when the writer called the attention of prominent Berlin investigators, such as Prof. Hellmann and Dr. Eschenhagen, to it.[2] In the meantime, Dr. W. Felgentraeger, Assistant at the Göttingen Magnetic Observatory, made an independent discovery of Whiston's book, and carefully worked up part of the material contained therein.[3] The writer has since found time to complete his examination of Whiston's contribution, and has embodied his results in a paper[4] presented by Prof. Cleveland Abbe before the Philosophical Society of Washington on November 10, 1894. In the following these results will be briefly sketched.

As will appear from the title of Whiston's work, the chief object was the exposition of a method for determining the longitude and latitude by means of the magnetic dip-needle, *i.e.* by means of the angle which a magnetic needle mounted on a horizontal axis, when placed in the vertical plane of passing through the magnetic meridian, makes with the plane of the horizon. It will be recalled that at that time great prizes had been offered by the English Parliament for an easy and trustworthy method of determining longitude at sea. From the very birth of terrestrial magnetism we find methods proposed for determining longitude by means of magnetic observations, and, like the problem of perpetual motion, these magnetic methods were revived every once in a while until the beginning of the nineteenth century. Owing to the irregular distribution of magnetism within the earth's surface, and on account of the many fluctuations terrestrial magnetism is subject to, these magnetic attempts to determine geographical position have been doomed to failure. They, nevertheless, have done much to promote the science of terrestrial magnetism. A striking instance of this is the book of Whiston's. The prime object of the book has failed of

[1] "The Longitude and Latitude found by the Inclinatory or Dipping Needle; wherein the Laws of Magnetism are also discover'd. To which is prefix'd an Historical Preface; and to which is subjoin'd Mr. Robert Norman's New Attractive, or Account of the first Invention of the Dipping Needle." By Will Whiston, M.A., sometime Professor of Mathematicks in the University of Cambridge. (London, 1721. 8vo, xxviii. 115, iv. and 43 pp. 2 charts and 3 cuts.)

[2] See remarks in *Physical Review*, vol. ii. No. 1, p. 72.

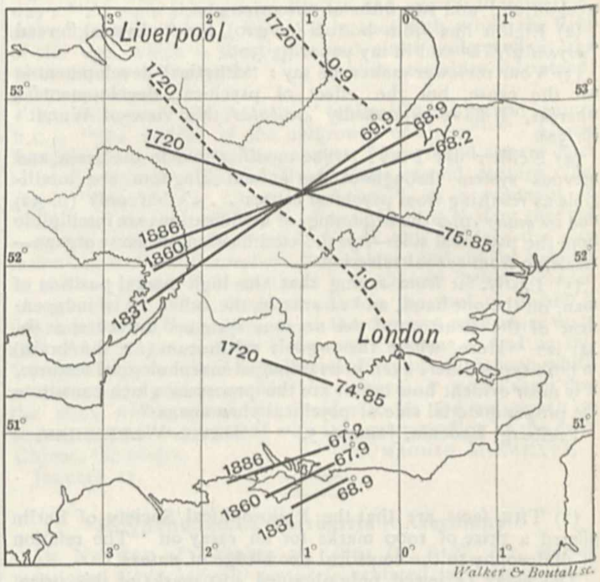
[3] "Die Isoklinenkarte von Whiston und die säkulare Aenderung der magnetischen Inklination im östlichen England." Von W. Felgentraeger. Reprint from *Nachrichten der k. Gesell. der Wiss. zu Göttingen Math. Phys. Klasse*, 1894. No. 2. 8vo, 12 pp.

[4] Entitled "The Earliest Isoclinics and Observations of Magnetic Force." (*Bull. Phil. Soc., Wash.*, vol. xii. pp. 397-410.)

its purpose,[5] yet the incidental discoveries, the importance of which the author himself did not fully appreciate, may perpetuate the name of Whiston for ever. It is most remarkable that his contributions have been, apparently, entirely overlooked.

Whiston, who, as stated in the title of his book, was at one time Professor of Mathematics at Cambridge, being Sir Isaac Newton's successor, was banished not long after assuming the chair, on account of heresy—he was a Unitarian. He was led to pursue the longitude problem magnetically through Halley's famous Isogonic Chart of 1700,[6] which came under his notice. As is well known, this chart of Halley's, giving the lines of equal magnetic declination, *i.e.* these lines on the earth's surface connecting all the places at which a magnetic needle swung horizontally has the same bearing, is the earliest published chart of its kind. In consequence, these lines have likewise been termed the "Halleyan Lines." Since then Halley's method has been effectually applied to the representation of other terrestrial phenomena, *e.g.* distribution of temperature ("Humboldt's Isotherms," 1817). Wilcke is credited as first applying Halley's method to the representation of the distribution of the magnetic *inclination*, and Wilcke's isoclinics are therefore referred to occasionally as the "Wilckean Lines." Wilcke published his chart, covering the greater portion of the earth, in 1768.[7] It appears, however, that the credit of first drawing the isoclinics should be accorded to Whiston. Wilcke nowhere states in the article cited that he for the first time has drawn the lines of equal magnetic inclination, and it is, moreover, reasonable to suppose, by his reference to Whiston's book, that he was familiar with its contents.

Whiston was led to drawing the isoclinics upon finding that the "Halleyan Lines," through "the Quickness of the Mutation of those Lines and their different Position in the rest of the World," could not be satisfactorily used for the determination of longitude. He therefore began to consider the "lines of equal dip,"[8] thinking they would answer his purpose better. To this end he collected all observations of dip made up to his time, and with their assistance drew, as far as then possible, the "lines of equal dip" upon Mr. Molyneux's terrestrial globe. Furthermore, to practically test his method, he made dip observations himself in 1719 and 1720 in various portions of England, and with their aid drew and published the *first isoclinics*, to be sure for only a small portion of the earth, viz. for southern England and north-western France. These isoclinics were laid down on two small charts (11·4 × 18·2 cm.), and are given opposite p. xxviii. of his book. The first chart is the result of dip observations made in 1719 with a needle 12 inches long; the second[9] is based upon more numerous observations made in 1720 with a $47\frac{1}{2}$-inch needle. The results with the two needles differ, on the average, by about $1\frac{1}{2}°$, the long needle giving the larger value. To counteract the error due to flexure of the long needle, Whiston placed a small "Poise of brass circular Wire," which required shifting to and fro according to dip, on the north end of the needle. Whiston believed that the longer the needle, the better the result if the needle be poised as stated. It is needless to say that experience has not borne him out in this respect. Owing to the large discrepancy between the results of the two needles, it might appear, then, that but little value can be attached to these Whistonian isoclinics. The writer finds, however, in his paper that the mean of the two results can doubtless be accepted as being within 1° of the truth. Moreover, while the *absolute* value may be impaired to the extent mentioned, the *relative* value remains intact, as the effect of the constant instrumental error would be almost entirely eliminated when considering the *relative* distribution of the dip over England. It is this latter fact that may give a value to these early isoclinics. Thus in 1720, according to Whiston, the isoclinics over England ran approximately from west-north-west to east-south-east, while to-day they go roughly from west-south-west to east-north-east. In the accompanying sketch, the mean isoclinics are shown by full lines for the epochs 1720, 1837, 1860, and 1886, as drawn by Dr. Felgentraeger in the paper cited.[3] It will be seen that between 1720 to about 1837 they shifted from year to year, *anti-clockwise;* they are now moving *clockwise*. Sabine,[10] I believe, was the first to call attention to this reversal of motion. It is hoped that this matter will receive further investigation.



Walker & Boutall sc.

Whiston comes in, however, for a still greater share in the early terrestrial magnetic discoveries. He invents, namely, a new and indirect method of determining the dip—the vibration method. He swings a magnetic needle horizontally, and determines the time of one horizontal vibration. He then swings the *same* needle mounted as a dip-needle, and again determines the time of one vibration. From the two times of vibration it is a simple matter to compute the prevailing dip. Whiston proposes this method as an approximate check upon the direct method where the angle of dip is measured at once. For example, Whiston found at London the time of one horizontal vibration of his long needle 120° from the magnetic meridian to be $60\frac{1}{4}$ seconds, which reduced to the magnetic meridian gives 42·6 seconds. The time of one vibration of the same needle, mounted as dip-needle, was found to be 22 seconds. If F is the total magnetic force, we then have the following relation:

$$F : F \cos \text{Dip} = (42{\cdot}6)^2 : (22)^2, \text{ or Dip} = 74° \; 32'.$$

Now, the mean of the results of the direct measurements with the 12-inch needle ($73\frac{3}{4}°$), and of the $47\frac{1}{2}$-inch

5 When Graham discovered, a few years after the publication of Whiston's book, that terrestrial magnetism is subject to a daily variation, Whiston perceived the inutility of his method. See "Memoirs of the Life and Writings of Mr. William Whiston. Written by himself." (London, 1749, vol. 1. p 297.)

6 Published in London in 1701, and reproduced photolithographically in Greenwich "Observations," 1869. To be soon republished in "Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus." Edited by Prof. Dr. G. Hellmann. (Berlin, A. Ascher and Co.)

7 "Sv. Vetensk. Akad. Handl.," 1768, p. 193.

8 Whiston even calls these lines also "Magnetick Parallels."

9 Reproduced in facsimile in Dr. Felgentraeger's paper (see Ref. 3). Both charts are to be given, also, in facsimile, in Hellmann's "Neudrucke," No. 4 (see Ref. 6).

10 *Proc. Roy. Soc.*, vol. xi. p. 144: "The angle of intersection of the meridian and isoclinics has been diminishing up to about 1840, when a reversal took place, and the angle is now increasing."

needle ($75\frac{1}{8}°$), is 74° 28′. Hence the agreement is quite satisfactory. We can probably say that the magnetic dip in London in 1720·0 was 74·5° ± 0·5°.

The invention of this new method of determining dip, led to still more important results. In making the dip observations with the long needle in 1720, for the purpose of his second chart, he at the same time observed the time of one horizontal vibration of the same needle, with the express purpose of determining the distribution of the force. These vibration-times are tabulated on his second chart.[11] From them the distribution of relative intensity can be determined. *These observations of Whiston's are undoubtedly the earliest relative terrestrial magnetic force observations ever made.* It is usually believed that the earliest relative intensity observations are the defective ones of Mallet (1769), or the more successful ones, but lost in shipwreck, of Lamanon (1785–87). The absolute value of these Whistonian intensity observations can, of course, not be checked; however, the relative value admits of some control. Thus the writer in his paper has reduced the observations, taking the value of the horizontal force at London as unity, and, with the aid of the reduced values, has roughly sketched the isodynamics (the broken lines in the figure) as prevailing over southern England in 1720. It will be seen that these lines of equal magnetic horizontal force have the same general trend as the isoclinics, as, indeed, they roughly should. Again, taking two stations (London and Saltfleet), which are practically in the same meridian and are distant from each other 2° in latitude, through which, consequently, the same isodynamic would not be likely to pass, it is found that the difference of the vibration-times—$60\frac{1}{4}$ and 66 seconds respectively, is in the right sense, *i.e.*, since the force varies inversely as the squares of the vibration-times, *the more northerly station, Saltfleet, gives the smaller horizontal force*, as it should. To be sure these early intensity observations are affected with a large probable error; they may, however, not deserve to be assigned to utter oblivion.

Upon the presentation of the writer's paper before the Washington Philosophical Society, Prof. Abbe became interested in the matter, and kindly called the author's attention to a later book[12] of Whiston's, a copy of which was likewise found in the Royal Library of Berlin. Whiston, in this book, gives an account of dip-observations made in various portions of the earth, with the aid of most liberal means furnished by King George and others, for the purpose of testing his magnetic method of determining geographical position. He sent "four several Dipping-Needles to Sea," and "with proper Instructions to the Masters of the Vessels" to observe the dip with both methods (direct and indirect), "to discover the State of Magnetism in the several parts of the Globe." Thus Captain James Jolly set out in July, 1722, for Archangel with one of Whiston's dipping-needles. Owing to a defect of the instrument he could observe only horizontal vibrations. Whiston says (p. 84), "he made me twenty-eight very good Horizontal Observations from the Latitude of 65 quite to Archangel." . . . "In this Space the Needle altered its Velocity very greatly, as I expected it would: And 5 Vibrations which at first were perform'd in about 280″, beyond the North Cape, came to 250″; till towards Archangel it gradually returned to about 177″." The first figure, 280″, is probably a misprint, and should be 180″. *These observations are the first to show the truth of the law that horizontal intensity decreases in approaching the magnetic pole.* Humboldt has credited Lamanon (1785–87) with the discovery of this law; it was not, however, firmly established until Humboldt's observations of 1798–1803. Furthermore, Captain Othniel Beal set out about the same time as Captain Jolly for Boston. From thence he sailed to Barbados, and thence to Charlestown, South Carolina. At all these places and at sea he made dip observations with both methods. A dip of 68° 22′ is given for Boston, and of $44\frac{1}{2}°$ for Barbados, on p. 92. *These two dips precede by fifty-eight years any dip that has hitherto become known in the United States.* The vibration-times are unfortunately not given. A third dip instrument was sent with Captain Tempest to Antigua and St. Christopher's, a fourth sent with Captain Michel to Hamburg. The results with the last two instruments had not yet been all received at the writing of the book. Whiston does not give the actual observations, but says, on p. 90, "The original Journals are all in the Hands of my great Friend and patron, Samuel Molyneux, Esq., Secretary to his Royal Highness the Prince of Wales, and Fellow of the Royal Society: which Journals, when I have compleated the rest of the Observations I hope to procure, I intend to publish entire, for the more full Satisfaction of the curious." It seems that Whiston never published these records. It is hoped that the present article will induce some one to look them up. They may possibly be a valuable find.

In conclusion, let us sum up Whiston's achievements.

(1) Whiston drew the first isoclinics (1719–20).

(2) He invented the vibration method of determining the dip.

(3) He made the first relative terrestrial magnetic intensity observations (1720).

(4) The first intensity observations (1722), revealing the law of decrease of horizontal terrestrial magnetic force with approach towards magnetic pole, were made under his instructions. L. A. BAUER.

[11] The only thing that Whiston says with respect to the method employed, is the following Passage on page 112, viz.: "The Difference of this *Strength* of the Magnetick Power, from its *Direction*, is most visible in my Second Map hereto prefix'd. Where I have all along set down the 'Seconds' wherein my Needle perform'd a single horizontal Vibration, at about 120 Degrees from the Magnetick Meridian, in most Places, whose Squares, when Allowance has been made for the different Obliquity of the several Directions as to our Horizon, will give us the different *Strength* of that Magnetick Power at those several Places; as does the angle of dip give us the different *Direction* of the same power there. Now, at first Sight, the former there appears to be irregular, and the latter regular; as is the Case also of our Terella" Why Whiston should have observed the vibration time 120° *from the magnetic meridian*, instead of across the magnetic meridian, the writer has not been able to ascertain. Whiston does not appear to have made any further use of his observations.

[12] "The Calculation of Solar Eclipses without Parallaxes . . . . with an Account of some late Observations made with Dipping-Needles, in order to discover the Longitude and Latitude at Sea." (London, 1724.)

THE TEACHING UNIVERSITY FOR LONDON.

DURING the last week very satisfactory progress has been made towards the reorganisation of the University of London as a teaching as well as an examining body. In the first place, King's College has been brought into line with the other teaching institutions of the metropolis by expressing a general assent to the recommendations of the Gresham Commission, coupled with the proviso that any Statutory Commission appointed to give effect to the Gresham Commissioners' recommendations should have power to make such modifications in the scheme as may seem to them expedient after consultation with the bodies affected—a proviso already insisted on by every teaching institution that has expressed its general approval of the scheme.

The adhesion of King's College to the views of the other teaching institutions mentioned in the Report of the Gresham Commission, was made known on the eve of the reception by Lord Rosebery of the deputation of delegates from the London colleges, and made it possible for these to present their case with the strength derived from complete accord.

On Tuesday last, Lord Rosebery received two deputations—one in the morning in favour of the Gresham scheme, in which representatives of the Senate, the

Annual Committee of Convocation, and the Committee of Graduates of the University of London; the Royal Colleges of Physicians and Surgeons; University College; King's College; Bedford College; the Medical Schools; the Theological Colleges; and the Association for Promoting a Professorial University for London took part, and a second in the afternoon, composed solely of members of Convocation opposed to the scheme.

Lord Rosebery's replies show that personally he is anxious to give effect to the Commissioners' recommendations. To the first deputation he said that the Government attach great importance to the Report of the Commission, and are fully sensible of the fact that the present time seems to offer a favourable opportunity, and one that ought not to be postponed, for the appointment of a Statutory Commission in the sense desired by those who had addressed him; while to the second, he made it clear that the opinions of the Government point in the direction of the appointment of a Statutory Commission, which would be able to receive full representations from any interests involved, and thereby be enabled to arrive at a scheme not unsatisfactory both to the present University and to the Empire at large.

Lastly, on Tuesday evening, Convocation of the University of London met, and for the first time came face to face with the question of approval or disapproval of the Commissioners' recommendations. As pointed out in a previous article (vol. l. p. 269), the power of veto possessed by Convocation under the Charter lent considerable importance to the decision arrived at, since an adverse vote might seriously retard the reorganisation of the existing University. In view of this contingency, it is highly satisfactory to record that Convocation, the last of the bodies to which the scheme has been submitted, by 157 votes to 133 resolved—"That Convocation, while desiring to express generally its approval of the proposals contained in the Report of the Royal Commission, is of opinion that power ought to be given to the Statutory Commission to vary the details of the scheme, and that it ought to be made an instruction to the Commissioners, before framing the statutes and regulations, to confer with duly accredited representatives of the Senate and of Convocation as to the modifications which may be desirable;" a previous resolution affirming that there should be one University in London, and not two, being carried by a slightly larger majority, namely, 206 votes to 175. These majorities may not be large, but they may be fairly taken to proportionately represent the opinion of the 3600 members of Convocation, since so far as any expression of opinion has been elicited by the various parties, 1165 members have expressed general approval of the Commissioners' recommendations, while 900 have indicated that in their view any teaching University for London ought to be constituted apart from the existing University. It may be earnestly hoped that with this vote the long controversy within the University has come to an end, and that all parties will now unite in the endeavour to make the new University worthy of the capital of the Empire. W. PALMER WYNNE.

NOTES.

WE are informed that Mr. G. F. Scott Elliot has arrived at Blantyre, in the Shiré Highlands, on his way home. His route from Ruwenzori has been by Karagwe and Urundi, to the extreme north of Tanganyika, which was traversed in Arab dhows to Abercorn. Thence he followed the usual route by the Stevenson Road to Lake Nyassa and the Upper Shiré.

PROF. E. WARBURG, Professor of Physics in Freiburg University, has been appointed Prof. Kundt's successor in Berlin University.

M. HAUTEFEUILLE, Professor of Mineralogy at the Sorbonne, has been elected a member of the Section de Minéralogie of the Paris Academy of Sciences.

DR. S. NAWASCHIN has been appointed Professor of Botany and Director of the Botanic Garden at Kiew, Russia.

THE death is recorded, at Berne, on December 13, of Dr. F. A. Flückiger, well known for his researches in pharmacologica botany, at the age of sixty-six.

DR. MURRAY THOMSON died on the 13th inst., in his sixty-first year. He was a Fellow of the Royal Society of Edinburgh and a Fellow of the University of Calcutta. For some years he was Professor of Experimental Science in the Government Engineering College, Roorkee, and chemical examiner for the Government in the North-Western Provinces of India. He was also the author of several medical and chemical treatises.

THE *Times* correspondent at Teheran reports that the town of Kuchan, which was destroyed by an earthquake fourteen months ago, and immediately rebuilt, was again destroyed on January 17. The extent of the damage and the loss of life are not yet known. Earthquake shocks were also felt at Meshed, but no damage was done.

WE learn that a general survey of the tides and currents on the Canadian coasts is now being commenced by the Canadian Department of Marine and Fisheries. It cannot fail to be of great use to navigation, and of especial interest to science, as the districts will include the phenomenal one of the Bay of Fundy with its 70 feet rise of tide, with which we have nothing to compare in magnitude in the British Isles.

THE Königliche Gesellschaft der Wissenschaften of Göttingen are organising a conference of delegates of scientific societies and academies, for the consideration of the relations between the variations in the intensity of gravity and the geological constitution of the earth's crust. It is intended that the congress shall take place at Innsbrück on September 5, where and when the International Geodetic Association will hold a meeting.

THE first number of the new series of *Science* has now reached us. To the editorial committee announced in our issue of December 20, should be added President T. C. Mendenhall of the Worcester Polytechnic Institute (Physics), Prof. R. H. Thurston of Cornell University (Engineering), Prof. Le Conte of the University of California (Geology), and Prof. H. F. Osborn of Columbia College (General Biology). The editorial committee, composed of the American men of science best known in England, and the contents of the first number, promise a journal that will adequately represent the progress of science in America. If in a multitude of counsellors there is wisdom, the journal should greatly advance scientific knowledge; not, however, by publishing memoirs and papers for specialists, but by promoting intercourse between students of all branches of nature.

ON Friday, the 11th inst., the Physical Society of London, in response to an invitation from Prof. Carey Foster, visited the new physical laboratories of University College. Before the commencement of the regular meeting in the lecture theatre (a report of which will be found in another column), the large number of members present went over the laboratories and practical class-rooms. There are three large rooms solidly built on the ground, and devoted to the use of the more advanced students, and of those engaged in original research. They are in a separate building apart from the main structure, and were specially built for physical work. Above one of them is the optical room, while within the main building there are two large basement rooms, one used chiefly for electrical and magnetic measurements, and one reserved for the practical classes. On the floor above are the lecture theatre and smaller classroom, apparatus room, chemical room, &c. The laboratories are lighted in the main with electric light, the direct current, supplied by the St. Pancras Vestry, being also used to charge a set of about fifty accumulators. A collection of apparatus was on view, more especially that designed for educational experiments, and used by students in the practical classes. Some of the pieces shown were of historical interest, among them being various instruments designed and used by Ritchie, who was formerly Professor of Natural Philosophy in the College.

ON Thursday last, January 17, the French Society of Aerial Navigation inaugurated the lectures to be delivered to the pupils of the newly-established school of aeronautics. During an address, Prof. Cornu, who was in the chair, said that he was glad that the Academy of Sciences had always exhibited an interest in aerial navigation. In 1782, a programme was drawn up of experiments to be conducted with the help of balloons. In 1794, there was established at Meudon the first aeronautical school, and the first captive balloons were made. In 1802, Gay-Lussac and Biot made the first scientific ascents, which remained almost unequalled until sixty years afterwards, when Glaisher took his aerial travels. The first dirigible balloon was constructed in 1870. Later, Paul Bert investigated the condition of human life at high altitudes. With these facts before them, the pupils of the French aeronautical school were reminded that their efforts would always be supported by the Academy. Aeronautics had been always popular in France, and had rendered good services to science and to the country.

SOME forgotten pages of photographic history were brought before the Brixton Camera Club, by Mr. W. H. Harrison, on January 15. It was pointed out that the many photographic researches of Foucault have been so completely overlooked that in scarcely any recent photographic history is mention to be found of more than his name, if so much as that. Foucault's early experiments in photographing the spectrum upon daguerreotype plates are interesting, and his results were not complicated by the presence of collodion, gelatine, or other colloids. In those early days photographers were so anxious to improve processes and to quicken them for purposes of portraiture, that perhaps these researches in the higher branches of photography interested them little, and soon afterwards were forgotten entirely, so that the name of Foucault as a pioneer of photography has practically passed out of the literature of the subject for nearly a generation. H. Bayard, the first to exhibit a selection of photographs to the general public, in July 1839, has also been much neglected in modern photographic literature. By means of his process, direct positives could be obtained without the intervention of a negative. Bayard's process seems likely to initiate useful modifications at the present time, in the easy production of reversed negatives. Sometimes it is now facetiously said that the best place for backings to prevent halation is on the front of the plate, meaning the use of a thick coating of emulsion: perhaps on Bayard's principle something of the kind may hereafter be done in a more literal sense. It should be remembered that there are two kinds of halation, and that one of them is due to reflection among the particles of silver haloid in the film itself. Perhaps something of especial benefit in astronomical photography may hereafter be evolved from Bayard's principle. Who knows but that it may hereafter lead to the production of a new class of dry plates which can be freely exposed to light, and, when required for use, sensitised by an alcoholic or other volatile liquid containing a haloid salt, to enable the re-drying to be effected quickly?

DR. A. MACDONALD, of the U.S. Bureau of Education, has sent us a number of statistics, showing the sensibility to pain, by pressure, in hands of individuals of different classes, sexes, and nationalities. So far as they go, the results indicate that the majority of people are more sensitive to pain in their left hand than in the right. Women appear to be more sensitive to pain than men, but of course it does not necessarily follow that women cannot endure more pain than men. American professional men are more sensitive to pain than American business men, and also than English or German professional men. The labouring classes are much less sensitive to pain than the non-labouring classes, and the women of the lower classes are much less sensitive to pain than those of the better classes. The general conclusion is that the more developed the nervous system, the more sensitive it is to pain. It is worth remark that, while the thickness of tissue on the hand has some influence, it has by no means so much as one might suppose, *à priori*; for many with thin hands require much pressure before experiencing any pain.

AN auriferous quartz-vein has been met with near Douglas in the Isle of Man. This seems to be the first recorded discovery of gold in that island, though, in view of its presence in the very similar districts of Merioneth and Wicklow, it is not in any way surprising.

Electrical Discovery is the title of a new fortnightly journal, in which it is intended to publish information on electrical patents filed in the British Patent Office. The *Official Journal* furnishes short abstracts of such inventions, but these do not appear until after the period for opposing the grants of the patents has expired. The new journal is designed to supply the need, by giving electricians early abstracts of all patent specifications and amendments relating to electricity. Digests and reports of patent cases of interest to electricians will also be included, and an index of articles on electrical subjects.

A FEW years ago it was practicable for persons of moderate income to subscribe to all the periodicals devoted to engineering and allied sciences, and to keep abreast with the contents. Now the number of such journals is so great, and so many are the memoirs and works bearing on engineering, that engineers, like the rest of us, are feeling the need of an index of their literature. Suggestions for the construction of such an index are given by Prof. G. D. Shephardson in the *Transactions* of the American Institute of Electrical Engineers for November 1894, and the following number contains a report of a long discussion on the subject. It is a satisfactory sign of the development of the scientific side of engineering, that electrical, mechanical, hydraulic, civil, and mining engineers want an index to their literature.

THE great Andalusian earthquake of December 25, 1884, as is now well known, produced slight disturbances of the magnetic curves at Lisbon, Parc St. Maur, Greenwich, and Wilhelmshaven. Two astronomical clocks were also stopped at the observatory of San Fernando, near Cadiz. From the times so recorded, the French Commission appointed to study the earthquake obtained values which seemed to show that the velocity of the earthquake wave diminished as it radiated outwards. In two recent papers (*R. Accad. dei Lincei, Rend.* iii. 1894, pp. 303–310, 317–325), Dr. G. Agamennone reconsiders the problem. He shows that the apparent diminution of velocity would disappear if the time at Cadiz were a minute too late. And an error of this kind, he remarks, is possible, for the times given for the magnetic observatories correspond to the beginning of the movement, whereas the clocks at Cadiz would be stopped during a later phase. Assuming the velocity uniform, he finds it to be 3·15 ± ·19 kilometres per second, a value which agrees very closely with some recent determinations.

THE light of the Blue Grotto of Capri, as well as that of the so-called red and green grottoes (*grotto rosso* and *grotto verte*) has been spectroscopically tested by Dr. H. W. Vogel, and is described in the current number of *Wiedemann's Annalen*. The most striking fact about the Blue Grotto was the occurrence of an absorption band between the Fraunhofer lines C and E of the solar spectrum, which does not occur in ordinary water. In addition to this, the red and the orange were extinguished as far as the D line. The same spectrum was exhibited by the water in front of the grotto. The "green grotto" is a rocky tunnel filled with bluish-green sea-water. The rocky walls show green reflections in the interior, produced by the impact of the bluish light from the water upon the yellow stones. But the absorption band noted in the Blue Grotto is here entirely absent. From the top of the cliffs, patches of azure-blue water could be seen surrounded by green. They all showed the absorption band, and retained their position permanently, so that they are probably due to some local cause. The "red grotto" does not show a trace of red light.

ATTENTION was recently drawn in our columns to an interesting observation made by Dr. Ostroumoff, of Sebastopol, on the power possessed by the Copepod *Pontellina mediterranea*, of jumping in the air upon the surface film of water. It appears from several further communications that this peculiar habit is also possessed by several other Copepods, viz. *Pontella atlantica* (M. Edw.), according to Dahl (*Verh. deutsch. Zool. Gesell.* 1894, p. 64), and *Pontella securifer* (Brady), according to an observation made by Captain Hendorff, who states that he several times saw this Copepod leap quite a foot high from the water in which it was contained. Herr Mrázek, in recording Captain Hendorff's observations (*Zool. Anz.* No. 415, p. 5), also mentions the additional case of a Schizopod having the same habits. This phenomenon, however, can be easily observed in the case of British Schizopods, and appears to be the result of abnormal conditions rather than a natural habit. Herr Mrázek does not support Dr. Ostroumoff in his view of the connection between this habit and the process of exuviation; he regards the movements in question as either purposeless and sportive, or for the sake of effecting escape from enemies. The latter view certainly receives support from the somewhat analogous case of the flying fish.

WE have received an elaborate paper read before the Congress of Scandinavian Naturalists at Copenhagen, by Dr. Ernst Abery, on the transmission of yellow fever. Much uncertainty and difference of opinion exist as to the means by which this disease is distributed, some authorities asserting that it has a purely local malarial origin, and cannot be imported into a place; whilst others are equally convinced that it can be imported, but is not transmissible directly from one individual to another. Dr. Abery, who has made a special study of the subject, has gathered together the principal facts about the dissemination of yellow fever, which are admitted by various authorities, and seeks to connect them together and explain them by a theory of his own. He accepts for this purpose the presence of a particular microbe specific to yellow fever, and regards it as capable of existing in different forms, such as spores and rodlets, and endowed with correspondingly different characters and degrees of virulence. By means of this theory, Dr. Abery explains many otherwise puzzling phenomena, and has produced an excellent working hypothesis. Unfortunately, however, it must remain only a hypothesis, for so far no microbe specific to yellow fever has been discovered and accepted; but possibly the author intends attacking this aspect of the question next.

THE new volume of *Memoirs* (Zapiski) of the Caucasian Geographical Society (vol. xvi.) is again one of exceptional interest. It contains, first, a series of botanical papers on the flora of Northern Caucasia, by I. Akinfieff, together with an account of a journey of the same author in Ossetia and Svanetia; three papers, by N. M. Alboff, on the vegetation of Western Transcaucasia, on new species found in Abkhasia, and on the Abkhasian ferns; and an abridged translation of several papers, by Dr. E. Dieck, on the flora of Western Transcaucasia; the series thus making a very valuable addition to our present knowledge of the flora of Caucasus. And next, the same volume contains a series of papers devoted to the still imperfectly known parts of the central section of the Main Caucasus ridge. M. N. Zhukoff contributes a paper on the glaciers of North-east Svanetia, with a new and very interesting map (1·3 miles to the inch) of a wide glacier region; A. V. Pastukhoff describes his ascensions of the Shah-dagh and the Ararat, as well as his visits to some of the high-level villages, of which Kurush, situated at a height of 8175 feet, is the highest in Caucasia—the paper being accompanied by small maps and photographs of the Kichen-dagh, the Nesen-dagh, and the Kurush village; M. and Mme. Rossikoff contribute two papers on the glaciers and Alpine lakes of the Central Caucasus main chain, giving exact measurements of the speed of motion of several glaciers; and another Alpinist, N. Dinnik, gives a description of Mount Oshten and the surrounding parts of the province of Kuban, which is full of very interesting geographical and botanical data. And, finally, two papers, by MM. Shalikoff and Andronikoff, are geographical and statistical descriptions of two districts of the government of Tiflis—Ksan and Uraveli. An index of all the papers contained in the hitherto published volumes of both the *Memoirs* (Zapiski) and the *Bulletin* (Izvestia) of the Caucasian Geographical Society, is also a most welcome feature of the present volume.

A SECOND edition of Dr. G. V. Poore's instructive "Essays on Rural Hygiene" has been published by Messrs. Longmans, Green, and Co. The new edition includes more than fifty additional pages.

THE Rose Polytechnic Institute, Terre Haute, Indiana, has published a bulletin on "Physical Units," by Prof. Thomas Gray. The bulletin comprises a concise and admirable collection of definitions of fundamental and derived units.

NO. 2 of the *Botanisches Centralblatt* for 1895 contains an exhaustive bibliography of the colouring matters of plants, by Dr. Hermann Ritter Schrötter-Kristelli, together with some new observations on the occurrence of carotin.

A WORK entitled "Molecules and the Molecular Theory of Matter," by A. D. Risteen, will be published in February by Messrs. Ginn and Co. The work is intended to be a popular exposition of the molecular theory of matter as it is held by the leading physicists of to-day. The subject is treated from a physical standpoint.

WHEN a book twenty years of age blossoms into a second edition, it is hardly necessary to say that the original must have undergone a thorough revision. This is the case with Prof. Alfred Newton's little "Zoology," published in 1874, among a series of manuals of elementary science, by the Society for Promoting Christian Knowledge. The new edition takes in much of the zoological work done during the past two decades, thus rendering it one of the cheapest, handiest, and best broad introductions to the study of zoology.

THE publication is announced of the first number of a *Phycotheca Boreali-americana*, by Messrs. F. S. Collins and Isaac Holden and Dr. W. A. Setchell. The work will include all families of Algæ, both freshwater and marine (except, for the present, Characeæ, Desmidieæ, and Diatomaceæ), from the Arctic Ocean to the Isthmus of Panama, including the West Indies. Each number will include fifty species, and the price will be five dollars. Subscriptions and offers of contributions are to be addressed to Mr. Frank S. Collins, 97 Dexter Street, Mass., U.S.A.

FROM a note in the *Botanical Gazette* we learn that the fox-tail grass or squirrel-tail grass, *Hordeum jubatum*, is a serious pest to stock in the Western States of America. The barbed awns break up into pieces, penetrate the gums, especially near the teeth, producing swelling, and ultimately suppuration, of the gums, and ulceration of the jaw-bones and teeth, the latter being so loosened as to drop out. If the animal continues to eat hay containing this grass, the disease progresses till the bony tissue of the jaws is disarranged, the ulcers extend to all parts of the jaw-bone, and it becomes distorted and enlarged. The marrow-filled interior is changed into great cavities filled with the broken awns. This condition may continue till the cavities extend entirely through the jaw, and the tightly-packed awns protrude till they may be pulled out with forceps or fingers.

THE completeness of the series of Eocene and Cretaceous strata exposed along the great central river system of Alabama, is well known to American geologists. A report on the geology of the coastal plain of Alabama, just issued by the Geological Survey of that State, shows that, on account of the fine exposures along the river banks, and the great number and perfection of the fossils, the region presents the most complete and varied series of Eocene and Cretaceous strata known in the United States. All that relates to Cretaceous, Tertiary, and Post-Tertiary formations in the vicinity of the Alabama and Tombigbee rivers, is described in the first part of the report. The second part contains all the data of practical value concerning the various phosphatic marls, greensands, &c., occurring in the region surveyed, and the third part includes a number of geological details referring to the different counties of the State. Prof. E. A. Smith, the State Geologist, and those who have assisted him, may be congratulated upon the publication of this important account of the stratigraphy of the Cretaceous and Tertiary formations of the Gulf region of Alabama.

AN inorganic mode of preparing hydrazine, N_2H_4, is described by Dr. Duden, of the Jena University Laboratory, in the latest issue of the *Berichte*. Hitherto the numerous methods of formation of this important hydride of nitrogen, described by its discoverer, Prof. Curtius, and his assistants, and by Thiele and von Pechmann, have all been based upon the decomposition of more or less unstable organic compounds of the diazo, nitrosamine, or nitramine types. Dr. Duden has succeeded in an inorganic synthesis by use of a singular compound, discovered by Davy and further investigated by Raschig and by Divers and Haga, which is produced by the action of sulphurous acid upon potassium nitrite. This compound, whose composition is represented by the formula $K_2SO_3 . N_2O_2$, is now found to yield hydrazine upon careful reduction in alkaline solution. Divers and Haga showed that the ordinary products of reduction of the compound with sodium amalgam in concentrated alkaline solution are mainly potassium hyponitrite KNO and hydrogen potassium sulphite $HKSO_3$, smaller quantities of hydroxylamine and ammonia being likewise produced. Dr. Duden finds that if the reduction with sodium amalgam, or zinc dust and ammonia or soda, is carried out at a low temperature, the solution produced possesses very strong reducing properties, and after acidification deposits the salt of hydrazine corresponding to the acid employed. The freshly prepared compound of potassium sulphite and nitric oxide is suspended in water cooled by ice, and sodium amalgam is gradually added with further extraneous cooling by means of a freezing mixture, until the liquid is found to strongly reduce Fehling's solution and yields, after acidification and warming to expel sulphur dioxide, a precipitate of benzalazine upon the addition of benzaldehyde. The benzalazine so obtained is found to exhibit all the properties of the compound as described by Prof. Curtius; it melts at 93° and yields numbers on analysis exactly agreeing with the formula $(C_6H_5CHN)_2$. With sulphuric acid it yields hydrazine sulphate $(N_2H_4)_2 . H_2SO_4$, identical in melting point (256°) and all other properties with that derived from the organic methods of preparation. The formation of hydrazine from the compound of nitric oxide and potassium sulphite would appear to occur in two stages, an intermediate reduction compound being first produced of analogous constitution to Davy's salt in accordance with the following equation:

$$\left.\begin{matrix}KSO_3\\KO\end{matrix}\right\rangle N . NO + 6H = \left.\begin{matrix}KSO_3\\H\end{matrix}\right\rangle N . NH_2 + H_2O + KOH.$$

A further reaction then occurs between the intermediate compound and the alkali, with production of potassium sulphate and hydrazine

$$\left.\begin{matrix}KSO_3\\H\end{matrix}\right\rangle N . NH_2 + KOH = K_2SO_4 + H_2N . NH_2.$$

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*, ♀) from India, presented by Mr. H. Ralls; a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Mr. Fred Bissmire; a Dusty Ichneumon (*Herpestes pulverulentus*), a White-throated Monitor (*Varanus albigularis*) from South Africa, presented by Mr. J. E. Matcham; a Derbian Wallaby (*Halmaturus derbianus*) from Australia, presented by Mr. Joseph Palmer; a Jackal Buzzard (*Buteo jacal*) from South Africa, presented by Mr. E. Wingate; a Yellow-headed Conure (*Conurus jendaya*) from South-east Brazil, a Brown-throated Conure (*Conurus æruginosus*) from South America, presented by Mrs. Hankey; three Eroded Cinixys (*Cinixys erosa*), two Home's Cinixys (*Cinixys homeana*) from West Africa, presented by Mr. J. Banks Elliot; a Lesueur's Water Lizard (*Physignathus lesueuri*) from Australia, deposited; a Rosy-billed Duck (*Metopiana peposaca*), a Garden's Night Heron (*Nycticorax gardeni*) from South America, purchased.

OUR ASTRONOMICAL COLUMN.

THE PERSEID METEORS.—This well-known shower of meteors should be of the greatest interest to the mathematician, as well as to the observer. At Pulkova, in 1893, the shower was observed from July 22 to September 12, and a discussion of the 563 paths recorded forms the subject of an interesting paper by Dr. Bredichin (*Bull. Imp. Acad. Sc.*, St. Petersburg, September 1894). It has long been known that the Perseids are not as other regularly recurring meteor showers, and Dr. Bredichin explains their peculiarities by supposing that they are not produced by a swarm of meteorites, in the true sense of the word, but by particles circulating in different orbits. These orbits have widely differing inclinations, and the other elements also show striking departures from each other. One gets a good idea of the system which is suggested, by imagining a bundle of materialised orbits crossing the earth's orbit at the points which the earth occupies during August, and for some days before and after. The particles with long periods correspond to the meteors at the beginning of the showers, and to the primitive position of the comet from which the meteors are supposed to be derived, which, having a moderate period, has left these meteors behind. The position of the node of the comet is not symmetrical with reference to the nodes of the meteors, but appears to be nearer the beginning than the end of the showers. This continuity of the phenomena for so long a time seems to confirm the idea of a variety of periods for the particles, and indicate also the repeated omission of the meteors from the body of the comet. If the forces of disruption were only those which have ordinarily been considered, the meteors would be dispersed in a long thin stream along the length of the orbit, as in the case of the November meteors; but since the orbits of the meteors are variously inclined to that of the comet, another force, acting transversely to the plane of the orbit, must be admitted as an important factor. The anomalous phenomena of the tails of some comets—a subject with which Dr. Bredichin is already closely associated—and the energetic emissions which have been noted in several comets, including that which is connected with the Perseids, serve to demonstrate the possibility of such an action as that which he supposes to have taken place in the case of this swarm.

COMET 1894 I (DENNING) AND BRORSEN'S COMET.—Dr. Hind contributes to the current number of the *Astronomische Nachrichten* (No. 3271) a very interesting note as to the identity of Denning's comet with that discovered by Brorsen. To investigate the question, he has found, with M. Schulhof's elements for Denning's comet and Dr. Lamp's elements for Brorsen's comet, that the distance of the orbits would be 0·0367 in Longitude 285° (1894·0), and that in April 1881 the comets approached one another within a distance of 0.138. On this account, he says, during the comet's recession from perihelion, might not Brorsen's comet have met with a catastrophe, causing disintegration and the return of a portion of it to perihelion, in a somewhat different orbit, in Denning's comet of last year?

Dr. E. Lamp has also considered this question of identity, and, in referring to Dr. Hind's note, writes that the similarity of the two orbits is very striking, and that, in the beginning of 1881, the two bodies must have been very close to one another near the point of intersection of the two orbits. With the same elements as used by Dr. Hind, he finds the point of intersection of the orbits in Longitude 284° 47′ and South Latitude 1° 57′. The places of the two bodies are then as follows:—

	Denning.	Brorsen.
	° ′	° ′
True anomaly	154 22	169 7
Ecc. anomaly	123 22	147 25
Radius vector	5·240	5·218

The point of nearest approach in the orbit of Denning's comet occurs in a position 5′ behind, and in the Brorsen's orbit 4′ before, the actual place, the distance between these points being 0·022 radii of the earth's orbit. Dr. Lamp suggests that, by decreasing Schulhof's value of the mean daily motion by about 28″, the comets would thus be brought together. The question, however, is in a very undecided state, but astronomers will await with interest the results of Dr. Lamp's investigation as to whether the comets furnish an instance of a mere approach or of a real physical connection.

STARS HAVING PECULIAR SPECTRA.—Prof. E. C. Pickering states in the *Astronomische Nachrichten*, No. 3269, that an examination of photographs of stellar spectra, taken at the Arequipa Station of the Harvard College Observatory, has led to the discovery of four new variable stars in Centaurus, Lupus, Pavo, and Microscopium, and ten other objects with spectroscopic peculiarities. Of these, the spectra of five are classified as Type IV.; two appear to belong to Type V.; one (R.A. 18h. 38·4m. Decl. −27° 55′) is a nebula; one has H^{β} bright; and the photographic spectrum of the remaining object contains no blue light. To show how difficult it is to draw any sharp distinction between nebulæ and bright line stars, we quote the concluding paragraph of Prof. Pickering's communication. "The photographic spectra of faint gaseous nebulæ and stars of the fifth type closely resemble each other, and can only be distinguished by the wave-length of the principal bright line. In gaseous nebulæ this line (5007) is of greater wave-length than H_{β}, while in stars of the fifth type, the line 4688 is of shorter wave-length. A superposition of a chart and spectrum plate of the star whose approximate position for 1900 is R.A. = 15h. 10m. Decl. −45° 17′, which has been announced as a star of the fifth type (*Astronomische Nachrichten*, vol. 135, p. 195), shows that this object is in reality a gaseous nebula."

NITROGEN FIXATION IN ALGÆ.

IN NATURE of March 29, 1894, Prof. Marshall Ward gave a clear and excellent *résumé* of certain aspects of the question of nitrogen fixation in plants. Since the publication of that article, fresh and most important additions have been made to the subject.

Last May, P. Kossowitsch published an account of his experiments on Algæ in respect to their nitrogen-fixing powers (*Bot. Zeitung*, May 16, 1894), and a short account of this contribution should form an appropriate supplement to Prof. Ward's paper.

In 1888, Prof. Frank, of Berlin, had stated his opinion that Algæ possessed the power of free nitrogen fixation.

In 1892, Messrs. Schloesing and Laurent published an account of their classical researches dealing with many plants, among which Algæ also found a place. Their experiments with these forms range in two series. In the first they found that if they kept soil, covered with Algæ and containing bacteria of certain kinds, under observation for some time, an increase in nitrogen was perceptible. On the other hand, if they prevented the formation of Algæ, although the same bacteria remained, there was no noticeable addition to the nitrogen of the system. In the second set of experiments, in which different Algæ were employed, no nitrogen fixation could be perceived. It was evident from this that either particular kinds of Algæ only have "fixing" powers, or that suitable bacteria were not simultaneously present in the second case, and that Algæ can only fix with the additional aid of these micro-organisms.

In the following year, Koch and Kossowitsch devoted their attention to the subject, and went over much the same ground as Laurent and Schloesing, confirming their results, and adding new facts, the value of which, however, was somewhat enhanced by the algal cultures never consisting of any single species alone, but of several intermingled. Accordingly when, in 1894, Kossowitsch set himself the task of determining whether Algæ in themselves possess the power of assimilating free atmospheric nitrogen or not, the first obstacle he had to overcome was the difficulty of finding a method by which he could obtain a single algal species in absolute purity. This was ultimately effected by growing the Algæ on gelatinous silicic acid permeated with a nutritive solution, and subsequently on sterilised sand also containing food solution. The steps by which the isolation was effected were slow and beset with difficulties, which sprang up in the most unexpected manner, and the pages of Kossowitsch's memoir which deal with this subject possess a separate and great interest of their own; space, however, will not permit that the matter be detailed here. Having obtained the Algæ in a state of purity, the next step was to transfer them to the apparatus in which their nitrogen-fixing powers were to be tested.

This consisted of a central air-tight vessel connected with a series of U-tubes, which were blown into bulbs at certain intervals. These bulbs contained strong sulphuric acid. The whole apparatus was sterilised, and the Algæ under consideration sown upon a sterilised nutritive substratum in the central vessel. Air freed from all traces of nitrogen compounds was blown into the vessel through the U-tubes, the sulphuric acid in which killed any organisms which might be contained in this air.

The Alga which was first experimented on was Cystococcus (or an extremely similar form). Every precaution was taken in introducing this into the apparatus.

Using a nutritive solution perfectly free from all nitrates, it was seen that the Algæ refused to show any signs of growth; it was clear, therefore, that at least to start development a trace of nitrate must be added to the sand. The addition of other nitrogen compounds was found to be useless, and accordingly a small and accurately measured quantity of a nitrate was mixed with the food solution in the central vessel. The whole apparatus thus fixed up was placed in the light, and left for some weeks. At first rapid increase in the Algæ was noticeable, but after the lapse of about three weeks things evidently came to a standstill.

The addition of more nitrate-free nutritive solution gave no result; but if only the merest trace of a nitrate were added, there was an immediate resumption of activity.

These facts in themselves are very good proof of the inability of Cystococcus to fix atmospheric nitrogen; but to make matters doubly sure, a careful chemical analysis was made. This showed that there was no increase in nitrogen during all the weeks the Algæ had been flourishing, and that accordingly no iota of the stream of free nitrogen which had been constantly passing through the apparatus had been "fixed." So far, then, the first Alga which had been put to the test of experiment showed itself incapable of utilising atmospheric nitrogen.

Kossowitch now turned to fresh experiments, choosing algal cultures of sometimes one, sometimes several species taken together; to all of these he added simultaneously soil-bacteria of mixed sorts. The apparatus employed was very nearly the same as that above described. In these experiments he desired to test the supposition of Berthelot and Winogradsky, who considered the presence of certain organic substances to be favourable to the fixation of nitrogen; he accordingly arranged his experiments in five pairs, both members of each couple having identical conditions, except that in the one a small quantity of sugar (dextrose) was added to the nutritive solution, whilst in the other no organic compound was present. One set was arranged with Cystococcus and soil-bacteria, and the results obtained showed that in the absence of organic materials a small but yet noticeable increase in the nitrogen of the system had taken place (from 2·6 mg. to 3·1 mg.) Where sugar had been previously added, however, there were three times as much nitrogen after the experiment as before. In a second pair of cultures the Alga Stichococcus and certain bacteria were used, but here in no case, either with or without sugar, was there any increase in nitrogen. This shows that Stichococcus has in itself no power of nitrogen fixation.

Another couple contained a mixture of several Algæ, Nostoc, Cylindrospermum, &c., and certain soil-bacteria. In this instance a very large fixation of nitrogen took place, both where sugar was present and where not; in fact, in the former case the nitrogen was increased more than nine-fold.

All these observations shed much light upon the question of the relations existing between Algæ, micro-organisms, and atmospheric nitrogen. They show:—

(1) That at least two Algæ—Cystococcus and Stichococcus—possess no "fixing" powers in themselves.

(2) That many Algæ, taken together with certain micro-organisms of the soil, do possess the power of assimilating atmospheric nitrogen.

(3) That this power is much increased by the addition of such organic substances as sugar.

It should be noticed that among the ten cultures used in the second set of experiments, only two contained definitely isolated algal species, viz. the cases of the two cultures of Cystococcus and soil-bacteria.

It was just in this instance, moreover, that it had been shown that the Alga itself had no capacity for fixing atmospheric nitrogen. Accordingly, there could be little doubt that it was through the agency of the micro-organisms that the "fixation" had taken place in these latter cultures.

The experiments of Laurent and Schloesing had shown that if in a culture of Algæ and bacteria endowed with "fixing" powers, the Algæ were destroyed, the bacteria lost partly, if not entirely, this capacity, which the mixture had possessed. This pointed clearly to the fact that there was some close relationship existing between the Algæ and micro-organisms.

There are many facts which seem to indicate the nature of this relationship.

Berthelot found that the nitrification of the soil only took place as long as organic compounds were present; if these were exhausted, the nitrifying process ceased. Gautier and Drouin also showed the importance which organic compounds have with respect to nitrification. Kossowitsch's own experiments, in which the advantage of adding sugar to the culture was shown, also point in the same direction.

From such observations as these, Kossowitsch concludes that the relationship which the Algæ bear to the micro-organisms is one connected with the organic food supply of these latter; he thinks that the Algæ, furnished with nitrogen by the bacteria, assimilate carbohydrate material, part of which goes to their own maintenance, but part also to that of the micro-organisms. It is, therefore, in his belief, an instance of symbiosis in which each supplies the wants of the other. There are many facts, partly the result of his own observations, partly the result of those of others, which uphold this view. If the mixed culture be placed in the light, there is a far more noticeable nitrogen increase than when in darkness. Again, if a rich supply of carbon dioxide gas be provided, this is marked by a decided rise in nitrogen-fixing powers. Both these conditions are such as are known to influence carbohydrate assimilation in chlorophyll-containing organisms; but all experience is antagonistic to the view that light should be beneficial to the vital activity of the bacteria, and there are only one or two exceptional instances (Nitromonas, &c.) in which carbon dioxide can be directly assimilated by these micro-organisms.

Moreover, in the cases where the bacteria are brought into immediate contact with the Alga, as in those species of Algæ which are enveloped in a gelatinous covering wherein the micro-organisms become embedded, nitrogen fixation appears to be greatly aided, and the addition of sugar to the culture has no such marked effect as in the instances where non-gelatinous Algæ are employed. The explanation of this seems to be that the bacteria embedded in the gelatinous sheath are amply provided with carbohydrate food without the addition of sugar, which, therefore, comes more or less as a superfluity.

All this seems to justify Kossowitsch's view of the part played by the Algæ in the fixation of nitrogen; it appears to show that they have an indirect, but none the less important, influence upon the process.

This is roughly the extent of Kossowitsch's article; it has been impossible to give here its details, the bare outlines of his researches could alone be mentioned, but it is hoped that sufficient has been said to show the importance of his work, perhaps even to indicate the interest which every page of his memoir possesses, dealing as it does with one of the most fascinating branches of vegetable physiology.

RUDOLF BEER.

THE COMMERCIAL SYNTHESIS OF ILLUMINATING HYDROCARBONS.[1]

THE direct combination of carbon and hydrogen in the electric arc is a true case of synthesis, and if we could form acetylene in this way in sufficiently large quantities, it would be perfectly easy to build up from the acetylene the whole of the other hydrocarbons which can be used for illuminating purposes. For instance, if acetylene be passed through a tube heated to just visible redness, it is rapidly and readily converted into benzol; at a higher temperature naphthalene is produced, whilst by the action of nascent hydrogen on acetylene, ethylene and ethane can be built up. From the benzol we readily derive aniline, and the whole of that magnificent series of colouring matters which have gladdened the heart of the fair portion of the community during the past five-and-twenty years, whilst the ethylene produced from acetylene can be readily converted into ethyl alcohol, by consecutively treating it with sulphuric acid and water, and from the alcohol, again, an enormous number of other organic substances can be produced, so that acetylene can, without exaggeration, be looked upon as one of the great keystones of the organic edifice, and, given a cheap and easy method of preparing it, it is hardly possible to foresee the results which will be ultimately produced.

In 1836, it was found that when making potassium, by distillation from potassic carbonate and carbon, small quantities of a bye-product, consisting of a compound of potassium and carbon, was produced, and that this was decomposed by water with liberation of acetylene; whilst Wöhler, by fusing an alloy of zinc and calcium with carbon, made calcic carbide, and used it as a source from which to obtain acetylene by the action of water.

Nothing more was done until 1892, when Macquenne prepared barium carbide by heating at a high temperature a mixture of barium carbonate, powdered magnesium, and charcoal, the resulting mass evolving acetylene, when treated with water; whilst, still later, Travers made calcic carbide by heating together calcic chloride, carbon, and sodium. None of these processes, however, gave any commercial promise, as the costly nature of the potassium, sodium, magnesium, or calcium-zinc alloy which had to be used, made the acetylene produced from the carbide too expensive.

Whilst working with an electric furnace, and endeavouring by its aid to form an alloy of calcium from some of its compounds, Mr. T. L. Willson noticed that a mixture containing lime and powdered anthracite, under the influence of the tem-

[1] Abstract of a paper by Prof. Vivian B. Lewes, read before the Society of Arts, Wednesday, January 16.

perature of the arc, fused down to a heavy semi-metallic mass, which having been examined, and found not to be the substance sought, was thrown into a bucket containing water, with the result that violent effervescence of the water marked the rapid evolution of a gas, the overwhelming odour of which enforced attention to its presence, and which, on the application of a light, burnt with a smoky, but luminous flame.

Investigation into the cause of this phenomenon soon showed that in a properly constructed electric furnace, finely ground up chalk or lime, mixed with powdered carbon in any form, whether it were charcoal, anthracite, coke, coal, or graphite, can be fused with the formation of a compound known as calcic carbide, containing 40 parts by weight of the element calcium, the basis of lime, and 24 parts by weight of carbon, and that, on the addition to this of water, a double decomposition takes place, the oxygen of the water combining with the calcium of the calcic carbide to form calcic oxide or lime, whilst the hydrogen unites with the carbon of the calcic carbide to form acetylene, the cost of the gas so produced bringing it not only within the range of commercial possibilities for use *per se*, but also the building up from it of a host of other compounds, whilst the production of the calcic carbide from chalk and from any form of carbon, renders us practically independent of coal and oil, and places in our hands the prime factor by which nature in all probability produces those great underground storehouses of liquid fuel upon which the world is so largely drawing to-day.

Calcic carbide is a dark grey substance, having a specific gravity of 2·262, and, when pure, a pound of it will yield on decomposition 5·3 cubic feet of acetylene. Unless, however, it is quite fresh, or means have been taken to carefully protect it from air, the outer surface gets slightly acted upon by atmospheric moisture, so that in practice the yield would not exceed five cubic feet. The density and hardness of the mass, however, protects it to a great extent from atmospheric action, so that in lumps it does not deteriorate as fast as would be expected, but in the powdered condition it is rapidly acted upon.

The acetylene made from it, when analysed by absorption with bromine, the analysis being also checked by determining the amount present by precipitation of silver acetylide, gives 98 per cent. of acetylene and 2 per cent. of air, and traces of sulphuretted hydrogen, the presence of this impurity being due to traces of sulphate of lime—gypsum—in the chalk used for making it, and to pyrites in the coal employed.

Acetylene is a clear, colourless gas with an intensely penetrating odour which somewhat resembles garlic, its strong smell being a very great safeguard in its use, as the smallest leakage would be at once detected; indeed, so pungent is this odour, that it would be practically impossible to go into a room which contained any dangerous quantity of the gas.

This is an important point to remember, as the researches of Bistrow and Liebreich show that the gas is poisonous, combining with the hæmoglobin of the blood to form a compound similar to that produced by carbon monoxide; whilst the great danger of the latter gas is that having no smell, its presence is not detected until symptoms of poisoning begin to show themselves, so that no fear need be apprehended of danger from this source with acetylene.

Acetylene is soluble in water and most other liquids, and at ordinary temperature and pressure—60° F. and 30 inches of mercury—10 volumes of water will absorb 11 volumes of the gas; but as soon as the gas is dissolved, the water being saturated takes up no more. Water already saturated with coal-gas does not take up acetylene quite so readily, whilst the gas is practically insoluble in saturated brine—100 volumes of a saturated salt solution only dissolving 5 volumes of the gas. The gas is far more soluble in alcohol, which at normal temperature and pressure takes up six times its own volume of the acetylene, whilst 10 volumes of paraffin under the same conditions will absorb 26 volumes of the gas. It is a heavy gas, having a specific gravity of 0·91.

When a light is applied to acetylene, it burns with a luminous and intensely smoky flame, and when a mixture of one volume of acetylene with one volume of air is ignited in a cylinder, a dull red flame runs down the cylinder, leaving behind a mass of soot, and throwing out a dense black smoke. When acetylene is mixed with 1·25 times its own volume of air, the mixture begins to be slightly explosive, the explosive violence increasing until it reaches a maximum with about twelve times its volume of air, and gradually decreases in violence until, with a mixture of one volume of acetylene to twenty of air, it ceases to be explosive.

The gas can be condensed to a liquid by pressure, Andsell finding that it liquefied at a pressure of 21·5 atmospheres, at a temperature of 0° C., whilst Cailletet found that at 1° C. it required a pressure of 48 atmospheres, the first-named pressure being probably about the correct one. The liquid so produced is mobile, and highly refractive, and when sprayed into air, the conversion of the liquid into the gaseous condition absorbs so much heat that some of the escaping liquid is converted into a snow-like solid, which catches fire on applying a light to it, and burns until the solid is all converted into gas and is consumed.

In my researches upon the luminosity of flame, I have shown that all the hydrocarbons present in coal-gas and other luminous flames are converted by the baking action taking place in the inner non-luminous zone of the flame into acetylene before any luminosity is produced, and that it is the acetylene which by its rapid decomposition at 1200° C. provides the luminous flame with these carbon particles, which, being heated to incandescence by various causes, endow the flame with the power of emitting light. The acetylene, being in this way proved to be the cause of luminosity, one would expect that in this gas we have the most powerful of the gaseous hydrocarbon illuminants; and experiment at once shows that this is the case.

Owing to its intense richness, it can only be consumed in small flat-flame burners, but under these conditions emits a light greater than that given by any other known gas, its illuminating value calculated to a consumption of 5 cubic feet an hour being no less than 240 candles.

Illuminating Power of Hydrocarbons for a Consumption of 5 cubic feet of Gas.

	Candles.
Methane	5·2
Ethane	35·7
Propane	56·7
Ethylene	70·0
Butylene	123·0
Acetylene	240·0

It is stated that the carbide can be made at about £4 a ton; and if this be so, it should have a great future, as a ton will yield 11,000 cubic feet of the gas. The lime left as a bye-product would be worth 10*s*. a ton, and the gas would cost at this rate 6*s*. 4½*d*. per 1000 cubic feet, and in illuminating value would be equal to London coal gas at 6*d*. a thousand. Its easy production would make it available for illuminating purposes in country houses, whilst its high illuminating value should make it useful for enriching poor coal gas.

CHEMICAL CHANGES BETWEEN SEA-WATER AND OCEANIC DEPOSITS.[1]

THE numerous analyses of sea-water by Forchhammer previous to 1865, and the later analyses by Dittmar, from samples collected during the *Challenger* Expedition, show that while the *salinity*—*i.e.* the amount of dissolved salts contained in 100 parts of sea-water—varies greatly in different regions of the ocean, still the composition of these dissolved salts—*i.e.* the ratio of the constituents of sea-salts—remains practically the same in all the superficial waters of the ocean. Consequently, it is only necessary to determine the chlorine in a definite weight of water to ascertain at once the respective quantities of the other salts present in the sample. Dittmar's examination of the *Challenger* waters has, however, shown that lime is slightly, although distinctly, more abundant in samples of sea-water collected in greater depths than in those samples collected nearer the surface of the ocean, and Dittmar's tables showing the difference between the chlorine calculated from the specific gravity and the chlorine found by analysis[2] point to differences in the composition of the sea-salts; but the observations are

[1] Abstract of a Paper read before the Royal Society of Edinburgh on March 7. 1892, by Dr. John Murray and Robert Irvine, and published in *Trans.*, vol. xxxvii. part 2, No. 23.

[2] Dittmar, "*Challenger* Report on the Composition of Ocean Water," "Phys. Chem. *Chall.* Exp.," part 1, p. 43.

relatively so few, these differences so slight, so mixed up with observational errors, and so irregular in their geographical and bathymetrical distribution, that they cannot be said to indicate any general law other than a greater quantity of lime in deep water.

But there is abundant evidence that great changes in chemical composition take place in the substances deposited on the floor of the ocean, and, with the view of throwing some light on the manner in which these changes are brought about, it occurred to us to examine the composition of the sea-water associated or mixed up with marine deposits on the floor of the ocean, and especially with that variety of marine deposits known as Blue Mud.

The depth at which a fine blue mud may form in the sea, depends entirely on the depth of water and the extent of the basin; or, in other words, on the height and length of the waves.[1] In harbours it may be deposited not deeper than 1 or 2 fathoms, while along the western coasts of Scotland and Ireland, which are exposed to the waves of the wide and deep Atlantic, the true mud-line may be situated at a depth of about 150 or 200 fathoms.

In this paper we state the results of our investigation into the composition of the sea salts in samples of water enclosed in the blue muds from Granton Harbour and Quarry, near Edinburgh, at Queensferry, N.B., and other places. The water was obtained by filling a canvas bag with the mud and collecting the water which filtered through, the first portions being rejected.

The Specific Gravity of the filtrate was about normal for inshore water, ranging from 1024 to 1026.

Chemical Composition of the Sea-water Salts in Mud-water.—On comparison with normal sea-water salts, the sulphates were greatly reduced, while the alkalinity (combined carbonic acid) was correspondingly increased, sometimes rising to ten times above the normal.

When a portion of the clear water filtered from the harbour muds was boiled, a precipitate was thrown down in a crystalline form, consisting of carbonates of lime and magnesia in the following proportions:—

$CaCO_3$	73·30
$MgCO_3$	26·70
	100·00

Before boiling, the water had an alkalinity of 0·7760 grms. per litre, while after boiling it showed an alkalinity of only 0·2200 grms., thus proving that the alkalinity was mainly due to the formation and presence of these carbonates rendered soluble by free carbonic acid.

Saline and albumenoid ammonia ranged from 4 to 80 parts, and 1 to 5 parts per million, respectively. Lime was much less, and magnesia slightly less than the normal. The chlorine and the total bases were higher than normal water of equal density. Bicarbonate of manganese was present in the water up to 1 part in 16,000. Normal sea-water contains no manganese.

The following table gives a comparison of the composition of normal and mud-waters:—

	Average sea-water.[2]	Mud-water.
Sodium chloride, NaCl	77·758	79·019
Magnesium ,, $MgCl_2$	10·878	11·222
Magnesium bromide, $MgBr_2$ ...	0·217	0·220
Magnesium sulphate, $MgSO_4$ ...	4·737	3·232
Potassium ,, K_2SO_4 ...	2·465	2·506
Ammonium ,, $(NH_4)_2SO_4$	...	0·206
Magnesium carbonate, $MgCO_3$ ...	...	0·729
Calcium ,, $CaCO_3$ ...	0·345	2·686
Calcium sulphate, $CaSO_4$ ...	3·600	...
Manganous carbonate, $MnCO_3$ ...	...	0·180
	100·000	100·000

[1] See Murray and Renard, "*Challenger* Report on Deep-Sea Deposits," p. 185. (London, 1891.)
[2] See Dittmar, *op. cit.* pp. 137-138 and 203.

It will be seen that the total salts of the mud-water are low in proportion to the chlorine, consequently the D value—that is, the density minus 1000 divided by the chlorine—will be lower than normal water. In normal water the D value is 1·457, in mud water 1·430.

The reactions that take place in Blue Muds seem to be the following, and may be distinctly proved from the analyses of mud-water, as well as from a consideration of the whole subject. During the process of decomposition it appears that the greater part of the oxygen for the oxidation of the carbon and hydrogen of the organic substances in the blue muds is derived from the sulphur salts of the alkaline and earthy alkaline metals in sea-water, which, in the first instance, are reduced to the form of sulphides. These sulphides, owing to their instability, especially in the presence of free or loosely-combined carbonic acid, are decomposed as formed. The sulphur thus reduced from the sulphates may in part, on passing as hydrosulphuric acid into the water immediately above the mud, become oxidised back again into sulphuric acid, which in turn, decomposing the carbonate of lime always present in the water (or in the deposit), would re-form sulphates.

This oxidation is effected but slowly, as the following laboratory experiments show:—

Exp. I.—A solution of hydrosulphuric acid (H_2S) in pure water, which at first gave no precipitate with barium chloride, after standing a month, gave a distinct precipitate of barium sulphate, showing that the hydrosulphuric acid had been oxidised into sulphuric acid (SO_3).

Exp. II.—A solution of hydrosulphuric acid in sea-water was exposed to the air till complete oxidation had taken place. On titration the sea-water had lost its alkalinity, the sulphuric acid being proportionately increased.

Exp. III.—Hydrosulphuric acid was passed into water holding carbonates of calcium and magnesium in suspension, and resulted in a yellowish solution of the sulphides of calcium and magnesium, carbonic acid being expelled. The sulphides so formed were in turn decomposed by excess of carbonic acid, with evolution of hydrosulphuric acid, bicarbonates being formed, the reaction apparently depending on which acid is in excess.[1]

A certain part of the sulphides, or it may be of hydrosulphuric acid, derived from the soluble sulphides by the action of the free carbonic acid present in the mud, reduces the ferric oxides of the deposit, forming sulphide of iron, which so long as it is not exposed to the action of oxygen, remains stable, being in this respect unlike sulphide of manganese.[2] The sulphide of iron gives the characteristic blue-black colour to the great majority of the blue muds, especially where there is abundance of organic matter. It is by this process that sulphur is continually being abstracted from sea-water and locked up in marine deposits, which may finally be converted into blue-coloured shales, schists, and marls.[3] In these rocks the crystalline pyrites (FeS_2) has evidently its origin in the processes of death and decay going on at the time of their deposition at the sea-bottom, the sulphur of the sulphide of iron being derived from the sulphates of the sea-water, and not from the sulphur of the organisms, as generally supposed. This decomposition seems to be due to the action of bacteria in causing putrefactive changes in dead organic matter. We have found that if sea-water containing putrescible organic matter be sterilised by boiling, and thereafter care be taken to prevent the ingress of bacteria to this cooled liquid, the changes above indicated do not take place. Apparently the organic matter must be broken down by bacteria into its component elements, which in the nascent condition are capable of reducing the sulphates to a lower form of combination. The bisulphide of iron in the coal measures has without doubt a similar origin.

[1] See also *Comptes rendus*, tom. lxxxiii. pp. 58 and 345 (1876). Note by Naudin and Montholon; also Sainte Claire Deville, *Leçons sur la Dissociation*, 1864.
[2] Irvine and Gibson, *Proc. Roy. Soc. Edin.* p. 37, 1891.
[3] The black or dark blue colour of many shales and schists is due principally to the presence of iron, either combined with silica as silicate, or more rarely in the condition of carbonate or oxide. These shales, schists, &c., contain organic matter in a state of decomposition. In the older rocks its condition nearly approaches that of graphitic carbon; in a dark schist from Argyllshire only 0·91 per cent. of *graphitic* organic matter was found. In the more recent formations the organic matter, if in sufficient quantity, may give rise to the formation of petroleum. See paper by Dr. J. J. Jahn, *Jahrbuch der K.K. Geolog. Reichsanstalt*, 1892, Bd. 42, Heft 2.

The principal reactions which occur in mud-waters may be explained by the following formulæ:—

$$(1)\ RSO_4 + 2C = 2CO_2 + RS,$$

where R is an earthy alkaline metal.

$$(2)\ RS + 2CO_2 + H_2O = H_2S + RCO_3CO_2.$$
$$(3)\ RS + RCO_3CO_2 + H_2O = 2RCO_3 + H_2S.$$

On the hydrosulphuric acid meeting with ferric oxide (Fe_2O_3) present in the surface layer of these blue muds the following reaction occurs:—

$$(4)\ Fe_2O_3 + 3H_2S = 2FeS + S + 3H_2O.$$

Part of the sulphur is thus fixed in the mud, and part, if there be not sufficient iron in the mud, may escape into the water above, where, meeting oxygen, it will be converted into sulphuric acid (H_2SO_4), and return into RSO_4. The products RCO_3CO_2 in (2), and RCO_3 in (3), or the bicarbonate and carbonate of the metal are found in the water strained from the mud.

The increase of alkaline ammoniacal salts points, however, to a further reaction, by which carbonate of lime is increased in a slight degree, for as ammonium carbonate [$(NH_4)_2CO_3$] is formed by the decomposition of the albuminoids present, the sulphates in the sea-water by this means are decomposed, sulphate of ammonia [$(NH_4)_2SO_4$] and earthy carbonates being the result.

In the red muds and clays, either from the abundance of oxygen in the superincumbent waters, from the ochreous matter present in the mud or clay, or from the organic matter being small in quantity, the sulphide of iron is either not formed, or is after formation soon oxidised into ferric hydrate, which then gives its characteristic red colour to these deposits.

It may be accepted as the rule that muds containing a large amount of organic matter relatively to the iron present invariably partake of the characteristic blue-black colour, whilst if organic matter be low in amount, or altogether absent, the black sulphide is either not formed at all, or is oxidised into peroxide of iron.

Our attention has been recently drawn to a most interesting paper, read before the British Association, Edinburgh (1892) meeting, by N. Andrussow, on the "Russian Exploration of the Black Sea."[1]

The condition of the water in the Black Sea below the 100-fathom line, in which hydrosulphuric acid and sulphides exist in great abundance, is due to the same action as that now being carried on so widely in the formation of the blue muds on the ocean floor, viz. the deoxidation of the sulphates in the water by organic matter, and not, as stated in Andrussow's paper, as simply the decomposition-products after death of a great number of organisms. But a compound or double reaction appears in this instance to be taking place, viz.—

Firstly, on those portions of the bottom within a moderate distance from the shore, ordinary blue mud containing sulphide of iron (in large amount) is being deposited.

Secondly, in the deep water, especially far from the shore, below a depth of 100 fathoms where the oxygen has been used up, the hydrosulphuric acid, not having enough iron present in the floating mud to combine with, or to fix it as sulphide of iron (FeS), is found in the free condition. At the same time there must be a large quantity of free or loosely-combined carbonic acid in the water, the result of the deoxidation of the sulphates by organic matter, which naturally would decompose the sulphides at their inception (or as these are formed). That this is probably the case appears from the fact that in the greater depths of the Black Sea, far from land, there exists a large deposit of mud consisting principally of carbonate of lime, precipitated from its waters, which hold in solution lime and other salts as well as hydrosulphuric and carbonic acids. In the laboratory experiments noted above we have the rationale of these conditions. The deep water in the Black Sea may be thus in a state of continual change, the alkalinity in this case being due either to sulphides or carbonates in so far as carbonic acid or hydrosulphuric acid predominates, and not wholly to carbonic acid as in the open ocean, where sulphides cannot remain permanent owing to the constant excess of oxygen present; but it is evident that, since the light grey mud consists principally of carbonate of lime, the carbonic acid must, it may be on account of the pressure or temperature, have had the advantage over the hydrosulphuric acid.

[1] "On Deep-Sea Research in the Black Sea," giving the results of an expedition (under the superintendence of Colonel J. B. Spindler) sent out by the Russian Government in 1890 and 1891. These results have already been partly published in the preliminary transactions of the Russian Geographical Society (in Russian), the physical results in German by Prof. Woicjk off in *Petermann's Mitteilungen*. An abstract of Andrussow's paper has been published in the Royal Geographical Society's *Journal*, January 1893, giving a very fair epitome of the various points dealt with.

METEOROLOGICAL WORK IN AUSTRALIA.[1]

THE object of the present paper is to place before the Association a brief and succinct account of meteorological work in Australia. Mr. Russell has already told us, in his interesting paper on astronomical and meteorological workers, read before the Association at its first meeting in Sydney in 1888, what had been done in the early days of the mother colony, and brings the history up to the year 1860, or immediately following the commencement of the active work of the new observatory completed in 1858, an establishment with which he has been associated during the past thirty-four years, and over which he has presided since his appointment as astronomer in 1870, on the death of Mr. Smalley in July of that year.

It is unnecessary that I should travel over the same ground. My intention is to carry on the history of which Mr. Russell has already given us the opening chapter. Indeed, as regards meteorology but little had been done before the advent of Mr. Scott, the first Director of the Sydney Observatory, in 1858, who, Mr. Russell tells me, established twelve meteorological stations, two of which, Brisbane and Rockhampton, were in Queensland, then forming part of New South Wales. Each station was equipped with a standard barometer, dry and wet bulb thermometers, maximum and minimum thermometers, and a rain gauge.

Meteorological stations had previously—in 1840—been established at South Head, Port Macquarie, and Port Phillip, Victoria being then under the Government of New South Wales. The observations at South Head were kept up, but, I fear, not in a very satisfactory or systematic manner, for fifteen years, or until 1855. At Port Phillip and Port Macquarie they are said to have been discontinued after six years. During Mr. Smalley's tenure of office several stations started by his predecessor, for some reason or other, probably owing to his bad health, were closed or allowed to fall into disuse. These were, however, speedily re-established by Mr. Russell: and I may here mention, as showing the active manner in which that gentleman has prosecuted the work commenced by Mr. Scott, that he has now, in addition to the Sydney Observatory, thirty-five meteorological stations, having barometers, dry and wet bulb thermometers, maximum and minimum thermometers, and rain gauges; 139 stations furnished with thermometers and rain gauges; and 1063 stations having rain gauges.

The Sydney Observatory is equipped with continuous self-recording barograph and thermograph, pluviometer and anemograph, made after Mr. Russell's own designs, besides underground thermometers at depths of 10 feet, 5 feet, 2 feet 6 inches, and 1 inch; an evaporation tank, or atmometer, &c.; a record, combined with the valuable astronomical work being done, worthy of the oldest colony of the group, which had already gained distinction in its promotion of science by the Dawes Point Observatory, erected in 1788, and the celebrated Paramatta Observatory, established in 1821 by Sir Thomas Brisbane.

In Mr. Tebbutt, Mr. Russell has found a most valuable coadjutor. That gentleman has not only carried out an extensive series of astronomical observations entirely at his own cost, but also furnished his observatory with a complete meteorological outfit.

In Victoria there were only broken records of rainfall, temperature, and weather, made chiefly by New South Wales officials in Melbourne, from 1840 to about 1849, and of rainfall up to 1851. In 1854 observations of barometer and temperature for astronomical purposes only, and of rainfall, were made at the Williamstown Observatory, then in charge of Mr. R. L. J. Ellery. Meteorological observations were also made at Melbourne by Mr. Brough Smyth, of the Crown Lands De-

[1] Abstract of a Paper read before the Australasian Association for the Advancement of Science, by Sir C. Todd, K.C.M.G., F.R.S.

partment, from 1856 to the end of February 1858, when Prof. Neumayer, now Director of the Nautical Observatory at Hamburg, commenced systematic observations at the new Magnetic and Meteorological Observatory, at Flagstaff Hill, Melbourne. Dr. Neumayer also established several observing stations at the lighthouses on the coast, and at a few places inland.

On the retirement of Dr. Neumayer in 1863, the Magnetic and Meteorological Department was transferred to the present Astronomical Observatory, then just erected, and placed under the direction of the astronomer, Mr. Ellery, in whose hands the institution soon became what it is to-day—not only a credit to the colony which founded it, but second to none in the southern hemisphere. He threw all his energy and skill as a physicist into his work, and early introduced photographic and other systems, by which we obtain continuous records of all variations of terrestrial magnetism, barometric pressure, and changes of temperature, electrical states of the atmosphere, and the direction and force or velocity of the wind, besides thermometers sunk at various depths (3 feet, 6 feet, and 8 feet) to determine the temperature of the ground; while, as regards astronomy, we have only to visit the observatory to see that it possesses some of the finest instruments in the world.

Besides the Melbourne Observatory, he has established meteorological stations of the second order at Portland, Cape Otway, Wilson's Promontory, Gabo Island, Ballarat (Mount Pleasant), Bendigo, Echuca, Sale (at the School of Mines), and twenty-three stations of the third order, besides 515 rainfall stations judiciously distributed throughout the colony.

In South Australia, thanks to the late Sir George Kingston, father of the present Premier, we have a continuous record of the rainfall in Adelaide from 1839, which that gentleman maintained until 1878.

Meteorological observations, more or less complete, were made at the Survey Office for a number of years, or until I took up the work in November 1856, when the observatory records commenced under my direction as Government Astronomer.

Since May 1860, all the observations have been made at the West Terrace observatory. For several years I had no assistant, and having a growing telegraph department to look after and control, the area of my work was necessarily restricted, and I laboured under many disadvantages; but I early established meteorological stations at Clare, Kapunda, Strathalbyn, Goolwa, Robe, and Mount Gambier, and placed rain gauges at the different telegraph offices. I also introduced the system of publishing daily reports of the weather and rainfall from all stations at the head telegraph office in Adelaide.

We have now meteorological stations, having standard or Board of Trade barometers, dry and wet bulb thermometers, maximum and minimum thermometers, and rain gauges, at Port Darwin, Daly Waters, Alice Springs, Charlotte Waters, Strangways Springs, Farina, Port Augusta, Yongala, Clare, Kapunda, the Agricultural College at Roseworthy, Mount Barker, Strathalbyn, Eucla, Fowler's Bay, Streaky Bay, Port Lincoln, Cape Borda, Robe, Mount Gambier, and Cape Northumberland, and 370 rain gauges; at the lighthouses at Cape Borda and Cape Northumberland, and at the telegraph offices at Port Darwin and Alice Springs, the observations are taken every three hours, night and day; at other stations at 9h. a.m., 3h. p.m., 9h. p.m.; whilst at Alice Springs there is a large evaporation tank similar to the one at the observatory, which it may be convenient here to describe.

It consists, first, of a brick tank, lined with cement; internal measurement, 4ft. 6in. square and 3ft. 2in. deep. Inside this tank is another, made of slate, 3ft. square and 3ft. deep, leaving an intervening space between it and the larger tank of 7in. Both tanks are filled to the same level, or to within 3in. or 4in. of the top, fresh water being added as required. The evaporation is measured by a graduated vertical rod, which is carried by a float placed in a vertical cylinder of copper 4in. in diameter (perforated at the bottom) standing in the inner tank. The rod is graduated to $\frac{1}{10}$ of an inch, and is read off by means of a fixed vernier to $\frac{1}{100}$ of an inch. A rain guage is placed by the side of the tank, and both the evaporation and the rainfall are read at 9 a.m. and 9 p.m.

In Tasmania, the Imperial Government established a magnetic and meteorological observatory at Hobart, as part of an international scheme, in charge of Captain Kay, and systematic meteorological observations were conducted from 1841 to 1854, hourly readings being taken until the end of 1848. The results were published, together with the magnetic observations, in four large quarto volumes with a short but interesting and instructive article by the late Prof. Dove, then director of the meteorological stations in Prussia. Similar observatories were established at Greenwich, St. Helena, Cape of Good Hope, and Toronto, besides places in Europe, and by Russia in Asia.

From the beginning of 1855, the Imperial Observatory being closed, meteorological observations at Hobart were carried on by the late Mr. Francis Abbott until about the year 1880, when the Government took up the work, which was entrusted to the late Captain Shortt, R.N., who died in 1893. Captain Shortt proved a valuable coadjutor, and established eight other observing stations, besides a number of rain gauges in various parts of the island, of which there are now about fifty-nine.

In Western Australia, a meterological observatory was established by the Government in connection with the Surveyor-General's office, the work being entrusted to Mr. M. A. C. Fraser, in 1876, since which continuous records have been published. Prior to the date mentioned we have rain and temperature records at Perth from 1860 to 1869, taken by Mr. H. Knight. At present Mr. Fraser has fifteen meteorological stations, exclusive of Perth, and ninety-one rain gauges. At Perth there is a self-recording barometer, selected by me when in England in 1886. The observations in this colony are very valuable, extending, as they do, from the south coast well into the tropics at Wyndham, Cambridge Gulf.

In Queensland, as has already been stated, meteorological stations were started at Brisbane and Rockhampton by Mr. Scott, the first Government Astronomer of New South Wales. I do not know the exact date, but Mr. Scott arrived in the colony in 1858, and retired in 1862. The instruments were transferred to Queensland on its separation from the parent colony, and for some years the duties of meteorologist devolved on Mr. Edmund MacDonnell, who established several observing stations and a number of rain gauges.

In 1887, Mr. Wragge was appointed, who—with the great ability and energy which characterise him, and which had brought him so much renown in starting, I believe at his own expense, the high level observatory at Ben Nevis, where he conducted the work under difficulties which would have deterred most men—soon effected a complete revolution. Beginning his work on January 1, 1887, he speedily equipped stations of the several orders all over the colony, along the coast round to the Gulf of Carpentaria, and inland to the very western boundary of the colony. He classified his stations under five orders, according to the completeness of their equipment.

Following the example of Mr. Ellery, Mr. Russell, and myself, Mr. Wragge commenced the system of publishing daily reports of weather and rainfall, and a synoptic map similar to the map we had for some time been issuing in Adelaide. He also co-operated with us in publishing forecasts of the probable weather during each ensuing twenty-four hours, with this addition, that he issued forecasts not only for Queensland, but also for the other Australian colonies; and, as these latter were made without regard to those published at an earlier hour by the several local authorities, it has occasionally happened that the two forecasts for the same colony differed from each other. I will not venture an opinion as to the desirableness of this independent action, beyond remarking that supposing the judgment and qualifications of the other meteorologists to be equally good, their local experience, and the possession of more detailed information in regard especially to prognostics, clouds, &c., gives them an advantage, and their forecasts should be of equal value, and be more frequently justified. I regret that Mr. Wragge's collected observations have not yet been published—from causes, it may be presumed, beyond his control—in such detail as he himself would wish, and which, in the interests of science, we all desire. This is to be regretted, as his stations are so distributed as to represent the climate of all parts of that large colony.

Besides the stations in Queensland, Mr. Wragge tells me he has supplied instruments for two stations of the first order in New Guinea, for one in New Caledonia, one in Fiji, and one in Norfolk Island, and two others of the second order in New Guinea.

In New Zealand, I learn from Sir James Hector, that from 1853 meteorological reports were included in the yearly volume of statistics issued by the Registrar-General, but the observations were of irregular character, and possessed little value until 1859, when the work was taken up in a more systematic manner. Observers were appointed at Wanganui, Auckland, Napier, New Plymouth, Wellington, Nelson, Christchurch, and Dunedin, each being supplied with a set of standard instruments. The service appears to have been placed, in the first instance, under the supervision of Dr. Knight, the Auditor-General, but in 1867 it was transferred to Dr. (now Sir James) Hector, under whose skilful management great improvements were introduced. The principal stations are supplied with mercurial Fortin barometers, dry and wet bulb and self-registering maximum and minimum thermometers, solar and terrestrial radiation thermometers, Robinson's anemometers, and rain gauges. The height of every barometer above sea-level has been ascertained, and every reading, as in the other colonies, is reduced to sea-level and 32° F.

At present there are eight stations, viz. Te Aroha, Taranaki, Russell, The Bluff, Wellington, Lincoln, Hokitiki and Dunedin, equipped as above, except Te Aroha, which has an aneroid; and seventy-nine rain stations.

To facilitate the transmission of daily weather reports, Sir James Hector has prepared a series of isobaric maps, which fairly represents all the different types of weather. These maps are numbered in consecutive order, and stereotyped copies are supplied to each station, so that all that is necessary is for the head office to telegraph to each office the number of the map to be posted up for the information of the public. In the same manner typical maps of the pressure in Australia have been prepared, with the assistance of Mr. Russell, of Sydney. The reports from a few selected stations, a brief description of the weather, and the number of the map, are daily exchanged between Wellington and Sydney (representing Australia); the New Zealand reports being transmitted by telegraph to the head office in each of the other colonies.

Spread throughout the colonies we have 357 meteorological stations, more or less completely equipped, and 2575 rain gauges.

It will be seen that, excepting the magnetic and meteorological observatory at Hobart, established in 1841, which was an Imperial institution, systematic observations under the auspices of the Colonial Governments date, speaking approximately, from about 1858, a date which closely coincides with that given by Prof. Waldo (1860) as marking a definite epoch in the development of the modern science of meteorology. The investigation of the law of storms by Buys Ballot, Dove, and others, and the researches of Ferrel, then just commenced, on the theory of atmospheric motions, cleared the way to further advances; and, later on, the utilisation of the electric telegraph, which is to the meteorologist what the telescope is to the astronomer, in extending his field of view over large areas of the earth's surface, enabled the observer to mark and watch the birthplace of storms, track their course and rate of translation. The same means informed him of the general distribution of pressure, and, knowing the laws governing the circulation of air currents round regions of high and low barometers, he soon felt himself justified in issuing warnings of coming gales and the probable state of the weather some hours in advance. He was no longer confined to his own particular locality, laboriously compiling statistics and studying local prognostics; he could look far around him, see storms a thousand or more miles distant, and tell people with a considerable amount of confidence when they might be expected and what would be their force. This is the great function of modern meteorology. But, like everything else, it took time. It required money from the State, which was not always readily forthcoming; it required, moreover, a complete and extensive organisation of skilled observers, all working on the same lines and with the same objects in view. It had also to win the confidence of a sceptical public, which still placed confidence in quack weather prophets, who could tell them what the weather would be all the year through, according to the phases of the moon. Confidence, we are told, is a plant of slow growth. So it is, and so it should be if progress is to be made on a sound, solid, lasting basis.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The University Lecturer in Geography (Mr. Yule Oldham) will during the present term give a course of lectures on the History of Geographical Discovery, in the Lecture-theatre of the Chemical Laboratory on Thursdays at noon, beginning on Thursday, January 24.

The Council of the Royal Geographical Society offer in the present academical year a Studentship of £100, to be used in the geographical investigation (physical or historical) of some district approved by the Council. Candidates must be members of the University of not more than eight years' standing from matriculation, who have attended the courses of lectures given in Cambridge by the University Lecturer in Geography. Applications should be addressed to the Vice-Chancellor not later than the last day of the full Lent term, March 15, 1895.

Prof. Ewing, F.R.S., has been appointed Chairman of the Examiners for the Mechanical Sciences Tripos.

The Gamble Prize for 1894 has been awarded to Miss Isabel Maddison, for her essay on "Singular solutions of differential equations of the first order."

THE first annual meeting of the Association of Technical Institutions was held on Friday last. In the course of an address, Mr. W. Mather, M.P., the President for the ensuing year, remarked that, so far as the pecuniary facilities conferred by the Technical Instruction Acts were concerned, local authorities had the means of annually bestowing on technical education in England and Wales (1) from grants under the Local Taxation Act, about £780,000; (2) from a penny rate levied on the total rateable value of the whole country, £664,500; (3) grants from the Department of Science and Art, about £355,000. The total amount available is thus, in round numbers, £1,800,000 per annum. To this must be added the voluntary aid given to technical schools and institutions. Among the resolutions adopted by the meeting was one for the appointment of a sub-committee to consider the best methods by which reform could be effected in the present system of examination in practical chemistry adopted by the Department of Science and Art, and to confer with other committees appointed with a similar object; and another to the effect that the result of examinations should not form the sole basis for the calculation of the grant in aid of science classes, but that there should also be a variable grant dependent on the report of the inspector on the equipment and arrangement for efficient instruction.

SOCIETIES AND ACADEMIES.

LONDON.

Mathematical Society, January 10.—Major Macmahon, R.A., F.R.S., President, in the chair. The Chairman gave a short obituary sketch of Mr. A. Cowper Ranyard, in the course of which he pointed out that that gentleman had only been a *pro tem.* secretary with the late Mr. G. C. De Morgan.—The secretaries elected at the first meeting of the Society, January 16, 1865, were Messrs. H. Cozens Hardy and H. M. Bompas. Mr. Hardy resigned at the second meeting (February 20), and Mr. W. Jardine was elected in his room.—The following communications were made:—Note on the expansion of functions, by Mr. Edward T. Dixon. The author had long thought that so fundamental a theorem as the expansion of a function in Taylor's series ought to be demonstrable from first principles in a simple maner which should be applicable to complex as well as to real quantities. The main feature in the proof he proposed was that the series was regarded not as the expansion in terms of powers of the increment of the variable, but rather as the expansion in terms of the values of the successive differential coefficients of the function for the given initial value of the variable. If two functions were equal for a given value of the variable, they would remain equal while the variable varied in any specified manner, so long as their rates of change remained equal and finite. The two sides of the equation known as Taylor's theorem were such functions; and the author explained how the limitations to the application of Taylor's theorem followed directly from his way of regarding the expansion. He also showed how the same line of argument applied to the case of complex variables, and how in that case also the limitations could easily be deduced.—Electrical distribution on two intersecting spheres, by Mr. H. M. Macdonald. In Maxwell's "Electricity and Magnetism," vol. i. §§ 165, 166, the problem of the distribution of electricity induced by an electrified point placed between them on two planes cutting at an angle which is a submultiple of two right angles, and the inverse problem of the conductor formed by two spherical surfaces cutting at such an angle (the angle referring to the dielectric) is solved by the method of point images. This method is inapplicable when the (dielectric) angle is not a submultiple of two right angles, as has been shown by W. D. Niven, *L.M.S.* vol. xii. p. 27. The only other case which has been hitherto solved is, the author thinks, that of the spherical bowl (Lord Kelvin, "Papers on Electrostatics and Magnetism," p. 178). In the paper by W. D. Niven, mentioned above, an attempt is made to deduce the capacity of such a conductor from the solution of a functional equation for a particular value of one of the variables, but the result obtained does not seem in the case of the spherical bowl to agree with Lord Kelvin's. The results obtained by the author also differ from those given by Niven. The object of the paper is to obtain the solution in the general case. To effect this, the functional image of a point placed between two planes intersecting at any angle is obtained in the form of a definite integral. In the next few paragraphs the reduction of this integral to known forms is effected in certain cases, and it is shown that the integration can be performed when the angle of intersection is any submultiple of four right angles; the case in which it is reducible to elliptic functions is also discussed. In § 5, the functional image of a line of uniform density parallel to the intersection of the planes is deduced. In § 7, the potential due to a freely-charged conductor bounded by two spherical surfaces cutting at any angle is obtained and some particular cases discussed. The capacity of such a conductor is obtained in § 8, in finite terms, and some particular cases are discussed in § 9; one of the most interesting of these is the capacity of a hemisphere, which is found to be nearly one and a quarter times that of the complete sphere, showing that the sharp edge acts somewhat like a condenser. Some cases are mentioned in the last paragraph which could be deduced from the results of the preceding ones.—The Dynamics of a Top, by Prof. A. G. Greenhill, F.R.S. To construct a model of the articulated deformable hyperboloid described by M. Darboux in Note xix. to Despeyrous' "Cours de Mécanique," t. ii., which shall realise the motion of the axis of a Top, the ratio of the axes of the focal ellipse must be taken equal to the modulus k of the associated elliptic functions. The parameters a and b of the two elliptic integrals of the third kind corresponding respectively to the lowest and highest vertical positions of the Top, which give the azimuth ψ, will be of the form

$$a = pK'i \text{ and } b = qK'i + K,$$

where p and q are real proper fractions. Then two points P and Q must be taken on the focal ellipse whose excentric angles, measured from the minor axis, are given by

$$\text{am}\{(1 - p + q)K', k'\} \text{ and } \text{am}\{(1 - p - q)K', k'\};$$

and if the tangents HI and HJ are drawn at Q and P, intersecting in H, these tangents make angles

$$\text{am}\{(p + q)K', k'\} \text{ and } \text{am}\{(p - q)K', k'\}$$

with the minor axis. The parallel tangents OC and OG being drawn, intersecting in O, the design of the model is completed, in Henrici's manner, by drawing any other two pairs of tangents to the focal ellipse; the tangents OG and OC being replaced by a pair of rods, representing the generators through O, HI and HJ representing the parallel generators through H, the other pairs of tangents representing the connecting generators, all freely jointed at the points of crossing. If OG is held fixed in a vertical position, the point H opposite to O is constrained to move in a fixed horizontal plane; and now if H is moved along a herpolhode of parameter $a + b$, the generator OC will imitate the motion of the axis of a Top. Starting with the hyperboloid flattened in the plane of its focal ellipse, and H at a maximum distance from OG, the axis OC is in its lowest position; and as H moves along the herpolhode to its minimum distance from OG, the axis rises to its highest position, when the hyperboloid becomes flattened in the plane of its focal hyperbola. If the herpolhode has points of inflexion, the path of a point C in the axis OC will be looped; since the motion in azimuth of OC vanishes as H passes through a point of inflexion. If $p - q = 1$, the point Q lies at the end of the minor axis of the focal ellipse; the path of C now has cusps. In the spherical pendulum the points H and O lie on the pedal of the focal ellipse with respect to the centre; this gives a geometrical interpretation of the equation

$$p'a = \pm p'b,$$

discussed in Halphen's "Fonctions elliptiques," i. p. 110. The vector OH represents the resultant angular momentum of the Top; and the tangent to the path of H is thus perpendicular to the vertical plane GOC. When the momental ellipsoid at O of the Top is a sphere, then OH represents also the resultant angular velocity. But in the general case, when the momental ellipsoid at O is a spheroid, with axis OC, the resultant angular velocity is represented by the vector OI to a point I fixed in the generator HQ; also H and I describe equal curves with respect to OC in parallel planes perpendicular to OC, which are herpolhodes of parameter $a - b$. Since I can be joined to a fixed point in the opposite generator OG by a rod of fixed length, we have Darboux's theorem that the motion of the Top can be realised by rolling a herpolhode of parameter $a - b$, on a fixed sphere. The connection of the motion of a Top with herpolhodes has also been discussed by Dr. Routh, *Q.J.M.* xxiii. p. 34. As an application, consider Halphen's algebraical herpolhode

$$(\xi^2 + b^2)(\eta^2 + b^2) = a^4$$

or

$$\tfrac{1}{4}r^4 \sin^2 2\theta + b^2 r^2 + b^4 - a^4 = 0,$$

produced by rolling the hyperboloid of two sheets

$$\frac{x^2}{-a^2} + \frac{y^2}{-b^2} + \frac{z^2}{a^2} = 1$$

on a fixed plane at a distance b from the centre.

In the associated motion of the Top, $p + q = \frac{1}{2}$; the focal ellipse of the articulated hyperboloid is given by

$$\frac{x^2}{0} + \frac{y^2}{\frac{1}{4}b^2\left(\frac{a^2}{b^2} - 1\right)^2} + \frac{z^2}{\frac{1}{4}b^2\left(\frac{a^2}{b^2} + 1\right)^2} = 1;$$

the coordinates of H and O in its plane are given by

$$y^2 = \frac{a^2}{8}\left(\frac{a^2}{b^2} - 1\right),\quad z^2 = \frac{a^2}{8}\left(\frac{a^2}{b^2} + 1\right);$$

at P $y^2 = \dfrac{b^4}{8a^2}\left(\dfrac{a^2}{b^2} - 1\right)^3,\quad z^2 = \dfrac{b^4}{8a^2}\left(\dfrac{a^2}{b^2} + 1\right)^3$; at Q

$$y^2 = \frac{b^8}{8a^6}\left(\frac{a^2}{b^2} - 1\right)^3\left(\frac{a^2}{b^2} + 2\right)^2,\quad z^2 = \frac{b^8}{8a^6}\left(\frac{a^2}{b^2} + 1\right)^3\left(\frac{a^2}{b^2} - 2\right)^2.$$

The motion of the axis of the Top is given by

$$\sin^2\theta \cos 2\psi = 4\sqrt{2}\,\frac{ab^2}{(a^4 + 8b^4)^{\frac{1}{4}}}\sqrt{\left\{\frac{a^4 + 2b^4}{a^2\sqrt{(a^4 + 8b^4)}} - \cos\theta\right\}}.$$

$$\sin^2\theta \sin 2\psi = \left\{\frac{a^2}{\sqrt{(a^4 + 8b^4)}} - \cos\theta\right\}$$
$$\sqrt{\left\{\frac{a^2 + 4b^2}{\sqrt{(a^4 + 8b^4)}} + \cos\theta \,.\, \frac{a^2 + 4b^2}{\sqrt{(a^4 + 8b^4)}} + \cos\theta\right\}}.$$

Thus for instance, if $a^2 = 2b^2$, $k = \frac{1}{3}$; the point Q is at an end of the minor axis of the focal ellipse, and the curve described by C has cusps. If $a^2 = 3b^2$, $k = \frac{1}{2}$; Halphen's herpolhode has points of inflexion where $r^2 = \frac{16}{3}b^2$, and r^2 varies between $4b^2$ and $8b^2$; the equation of the focal ellipse is

$$\frac{x^2}{0} + \frac{y^2}{b^2} + \frac{z^2}{4b^2} = 1,$$

and the coordinates of H and O are $\pm\frac{1}{2}\sqrt{3}\,b$, $\pm\frac{1}{2}\sqrt{6}\,b$.—Some properties of generalised Brocard circles, by Mr. J. Griffiths. —On fundamental systems for algebraic functions, by Mr. H. F. Baker.

Physical Society, January 11.—Extra meeting, in the physical laboratories of University College (by invitation of Prof. Carey Foster).—Prof. Rücker, President, in the chair.—Prof. Ramsay read a paper, by himself and Miss Dorothy Marshall, on the measurement of latent heats of vaporisation of various organic liquids. The liquid to be examined is placed in a small flask with a narrow neck, and within this is a platinum wire which has its two ends fused through the bottom, so as to be capable of conveying an electric current. The flask is completely enclosed in a jacket, which is filled with the vapour of liquid of the same kind. Before the current is turned on, the vapour jacket is kept going for some time, so that the liquid in the flask is raised just to its boiling point, but no appreciable evaporation takes place. As soon as the current is turned on, boiling commences, and all the heat developed in the wire is expended in producing evaporation. By weighing the flask before and after, the mass of liquid vaporised is determined. So far the authors have only used the method for comparative determinations. Two arrangements of the kind described are placed side by side, and the same current is sent through their two wires, which are joined in series and have approximately equal resistances. The ratio of amounts of heat expended on the two liquids divided by the ratio of the masses vaporised, is equal to the ratio of their latent heats. The determinations made by this method agree well with those of other observers; but the authors' object is to obtain values correct within about per cent. for a large number of liquids, rather than a highly accurate value for any one substance. In reply to Mr. Griffiths, the authors stated that the platinum wire was found to rise about 20° above the temperature of the liquid, and Mr. Griffiths said that his experience had been similar. He did not see why a very high degree of accuracy should not be obtainable by the method. Prof. Rücker expressed his admiration for the work, and thought it justified by the fact that the results accorded more nearly with theory than those of other observers.—Mr. Eumorfopoulos read a paper on the determination of thermal conductivity and emissivity. In the first series of experiments described, two bars of the same material and polish, and each of uniform circular section, are heated, each at one end, until the distribution of temperature has become steady. By means of two thermo-joints (one on each rod) a series of isothermal points are then found. According to the ordinary theory, if the two bars agree in temperature at a given pair of points, they will also agree in temperature at distances x_1 and x_2 measured respectively from these points, where x_1 and x_2 are connected by the relation $x_1/x_2 = \sqrt{(r_1/r_2)}$, r_1 and r_2 being the radii of the rods. This relation was not found to hold good for the rods examined. In all cases x_1/x_2 was further removed from unity than the ordinary theory would require. One conclusion was that the formula usually adopted in such cases could not be used for the comparison of conductivities, unless the radii of the rods compared are equal, and their surfaces in the same condition. To settle the question, three brass rods were chosen, and their absolute conductivities compared by Ångström's method. The emissivity was found to vary considerably with the radius, being greater the thinner the rod; moreover, the value of the emissivity deduced from the first sine term of the Fourier's series was in each case found to be about 1·2 times as great as that deduced from the constant term.—Mr. A. W. Porter then read a paper on the influence of the dimensions of a body on the thermal emission from its surface. The ordinary assumption is that whether a body is *in vacuo* or surrounded by air, the "emissivity" (*i.e.* the amount of heat passing outwards from unit area per second per degree excess of temperature) is independent of the size of the body. Results obtained experimentally by Péclet for cylinders and spheres of different sizes, show that the emissivity depends materially upon the size of the body. Péclet's formulæ for cylinders and spheres surrounded by air show that for each of these forms the rate of emission per unit surface, exclusive of the radiation effect, may be represented by a constant *plus* a term inversely proportional to the radius. The author examines the results of supposing the loss to follow only in part the law of radiation, the remainder being assumed to follow the law of conduction. He thus arrives at a formula

$$e = h + \frac{c}{a(\log R - \log a)};$$

where e is the emissivity, a the radius of the rod, R the radius of a hollow cylinder which surrounds the bar, and above which the excesses of temperature are reckoned; while h and c are constants. This formula has been compared with experimental results of Ayrton and Kilgour, of MacFarlane, of Bottomley, and of Péclet, and has also been directly checked by experiments on a brass rod when surrounded by water-jackets of different radii, as well as on the same bar unjacketed. The author finds the agreement to be much closer than is the case on the theory of constant emissivity, or with empirically deduced formula of Ayrton and Kilgour, and he concludes that the enclosing boundary is as important a factor in determining emissivity as the size of the body itself. Prof. Carey Foster thought that in demonstrating the influence of the enclosure Mr. Porter had established an important point. Prof. Ayrton agreed as to the importance of the influence of the enclosure. He urged that in such experiments as those of Mr. Porter and Mr. Eumorfopoulos, the conductivity and emissivity, which were functions of the temperature, should not be assumed constant along the bar. Mr. Trotter objected to the use of the term emissivity as including loss of heat by contact with the air in addition to the loss by radiation. Mr. Griffiths said that in some of his experiments, where a wire conveying an electric current was immersed in a liquid in order to heat the latter, the rise of the temperature of the wire above that of the liquid was found to be nearly independent of the diameter of the wire. Mr. Eumorfopoulos said that in each case his comparison had been between portions of bars in which the range of temperature was the same. Moreover, the variation of emissivity and conductivity with temperature, as found by other observers, would be quite insufficient to account for his results. Mr. Porter said that the term emissivity had come to be accepted as referring to all heat lost at the surface of a body, whether by radiation or by conduction and convection. In that sense he had used the term. Prof. Rücker thought that emissivity, in this sense, was not a good term, but to change now would probably only make greater confusion.—Mr. G. U. Yule then gave a brief outline of his paper, on the passage of an oscillator wave-train through a plate of conducting dielectric. By a conducting dielectric the author means a substance whose conductivity and dielectric capacity are both of importance in the case under discussion; and the paper is mainly an investigation of the following problem: a train of plane electromagnetic waves falls at normal incidence on an infinite parallel sided plate of conducting dielectric, whose thickness is finite, and at the first face of the plate, the amplitude of the vibration vector in the incident train is zero up to a certain instant, and then becomes equal to an harmonic function of the time, multiplied by an exponential function with negative index: to find what proportion of the energy of the whole incident train is reflected back, what proportion is transmitted through the plate, and what proportion absorbed. At successive incidences of reflected and re-reflected wave-trains upon the two bounding faces of the plate, the amplitudes and phase-changes of reflected and transmitted portions have to be taken into account, and the resulting infinite series of terms have to be summed. The analysis is very long, but the results obtained are exact. Curves are given, showing (for special numerical values of the constants of the problem) the quasi-periodic variation of the amounts of energy transmitted and reflected, as the thickness of the plate is increased from zero up to a high value. Other curves are given showing the effect of varying the dielectric constant and the conductivity of the plate, and the difference between a "damped" and an "undamped" wave-train in regard to intensity of reflected and transmitted portions. The author compares his calculated results with measurements obtained in the case of oscillator waves travelling along a double-wire circuit about 100 metres in length; the wires at the middle of the circuit being run through a jar containing distilled water, alcohol, or a very dilute electrolyte.—The necessary corrections, however, are difficult and uncertain, and the author has not found it possible to deal with them in a satisfactory way.—A letter from Dr. E. H. Barton was read, emphasising the necessity for taking into account the damping in the oscillator-train, and at the same time pointing out why, in his opinion, the corrections applied by Mr. Yule were inadequate and failed to yield intelligible results. Prof. Rücker congratulated Mr. Yule on his work, and on the importance of the results he had obtained. In returning the thanks of the Society to Prof. Carey Foster for the invitation to meet in University College, he expressed the pleasure he had felt in observing the extent and completeness of the laboratories. Hitherto London had been behind the provinces in this matter, and it was gratifying to find that students in London had now such opportunities for practical instruction in physics. The papers which had been read at that meeting were a proof that good use was being made of the laboratories for the purposes of research. The educational experiments they had seen in the laboratories were excellently devised, and he hoped that many of them would become a part of the regular course of instruction in the country. Prof. Foster briefly replied.

Chemical Society, Dec. 20, 1894.—Dr. H. E. Armstrong, President, in the chair.—The following papers were read:—An improved form of barometer, by J. N. Collie. The author has devised a portable barometer presenting several new features.—The constituents of *Piper ovatum*, by W. R. Dunstan and H. Garnett. *Piper ovatum* is a West Indian medicinal plant; the authors have separated from it a toxic alkaloidal substance, which they term piperovatine, $C_{16}H_{21}NO_2$; it seems likely to be of service in therapeutics.—Note on the active constituent of the Pellitory of medicine, by W. R. Dunstan and H. Garnett. The Pellitory of medicine (*Anacyclus pyrethrum*) contains an active substance, which they name pellitorine; it closely resembles piperovatine, and is possibly identical with it.—The determination of some high temperature freezing-points by means of platinum-resistance pyrometers, by C. T. Heycock and F. H. Neville. The authors give the results of freezing-point determinations of a number of metals and salts.—The preparation of adipic acid and some of its derivatives, by W. H. Ince. Contrary to the statements of Arppe and Malaguti, adipic acid is not produced in the action of nitric acid on sebacic acid or beef suet. The author has prepared α-monobromadipic acid in a state of purity, and has obtained α-hydroxyadipic acid.—The action of hydrogen chloride on the oxides of calcium, barium, and magnesium, by V. H. Veley. Dry hydrogen chloride does not act on quick-lime or magnesia at ordinary temperatures; at higher temperatures action occurs. Baryta is acted on at all temperatures by the dry gas.—Latent heat of fusion, by H. Crompton.—Metallic tartrasenites, by G. G. Henderson and A. R. Ewing. Arsenious oxide dissolves in hot solutions of acid tartrates, giving tartrasenites; the sodium salt, $C_4H_4O_6AsONa$, $2\frac{1}{2}H_2O$, is stable and crystalline.—Note on the interaction of hydrogen sulphide and bismuth haloid compounds, by M. M. P. Muir and E. M. Eagles.

Zoological Society, January 15.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—Mr. P. Chalmers Mitchell exhibited and gave an account of a tibia and other bones of an extinct bird of the genus *Æpyornis* from Central Madagascar, which had been lent to him for exhibition by Mr. Joseph H. Fenn. With these bones was associated a skull of a species of *Hippopotamus*.—Prof. G. B. Howes exhibited and made remarks on the photograph of an embryo of *Ornithorhynchus*.—The Secretary exhibited, on behalf of Mr. R. Lydekker, a life-sized drawing of *Idiurus zenkeri*, a new and remarkably small form of flying squirrel from West Africa, recently described at Berlin.—Lord Lilford sent, for exhibition, the skin of a duck, believed to be a hybrid between the Mallard (*Anas boschas*) and the Teal (*Querquedula crecca*), that had been caught in a decoy in Northamptonshire.—The Rev. T. R. R. Stebbing exhibited a specimen of a species of *Peripatus* from Antigua.—Mr. Frederick Chapman gave an account of some Foraminifera obtained by the Royal Indian Marine Survey's s.s. *Investigator* from the Arabian Sea near the Laccadive Islands. The author described the forms found in the samples sent him. As many as 277 species and varieties were enumerated, some of which were new to science. Several of the species, which were here recorded for the first time from recent soundings, had been previously known from the Pliocene deposits of Kar Nicobar. One of the forms found in these recent deposits, viz., *Amphistegina radiata* (F. and M.), was described by the author as showing the presence of interseptal canals, a structure which had hitherto appeared to be restricted to Nummulites and allied forms. Examples of embryonic forms of the same species were also noted as being present in the peripheral chambers of the adult shell.—A communication was read from Mr. P. R. Uhler containing an enumeration of the Hemiptera-Homoptera of the Island of St. Vincent, West Indies. This paper had been based on specimens submitted to Mr. Uhler by the joint Committee of the Royal Society and British Association for the exploration of the Lesser Antilles.—A communication from Mr. T. D. A. Cockerell contained a description of a new species of the family *Coccidæ* belonging to *Lichtensia*, a genus new to the fauna of the Nearctic region. The species was named *L. lycii*.—Mr. Sclater read some notes on the recent occurrence of the Barbary Sheep in Egypt. A flock of these sheep had visited the eastern bank of the Nile above Wady Halfa in the summer of 1890.—A second paper by Mr. Sclater contained some notes on the recent breeding of the Surinam Water-Toad (*Pipa americana*) in the Society's reptile-house.

Entomological Society, January 16.—The sixty-second annual meeting; Mr. Henry John Elwes, President, in the chair.—An abstract of the treasurer's accounts, showing a good balance in the Society's favour, having been read by Mr. W. F. H. Blandford, one of the auditors, the secretary, Mr. H. Goss, read the report of the Council. It was then announced that the following gentlemen had been elected as officers and Council for 1895:—President, Prof. Raphael Meldola, F.R.S.; treasurer, Mr. Robert McLachlan, F.R.S.; secretaries, Mr. Herbert Goss and the Rev. Canon Fowler; librarian, Mr. George C. Champion; and as other members of the Council, Mr. George T. Bethune-Baker, Mr. Walter F. H. Blandford, Dr Frederick A. Dixey, Mr. Henry J. Elwes, Mr. Charles J. Gahan, Prof. Edward B. Poulton, F.R.S., Dr. David Sharp, F.R.S., and the Right Hon. Lord Walsingham, F.R.S. It was also announced that Prof. Meldola, the new President, would appoint Lord Walsingham, Mr. Henry J. Elwes, and Prof. Edward B. Poulton, Vice-Presidents for the session 1895-96. The outgoing President then delivered an interesting address on the geographical distribution of Lepidoptera. He remarked that though a great deal had been written of late years on the geographical distribution of plants, mammals, birds, fishes, and reptiles, comparatively little had yet been done by entomologists to show how far the natural divisions of the earth's surface which have been established for other classes were applicable to insects. Perhaps the proportion of known as compared with unknown insects was still too small, and the classification of the known species still too uncertain, to allow anything like the same methods to be applied to insects that had been used for mammals by Dr. Wallace, F.R.S., for birds by Dr. Sclater, F.R.S., and Dr. Bowdler Sharpe, and for plants by Sir J. Hooker, F.R.S., Mr. Thiselton Dyer, F.R.S., and Mr. W. B. Hemsley. The President enumerated the genera of the Rhopalocera, and pointed which of them were characteristic of the various regions and sub-regions into which the world had been divided by the zoologists and botanists above mentioned. He also exhibited specimens typical of these regions and sub-regions. The President then alluded to the prosperous condition of the Society, and to the increase in its numbers and income. Reference was also made to various Fellows of the Society and other entomologists who had died during the year, special mention being made of Herr H. T. Christoph, Mr. J. Jenner-Weir, Dr. F. Buchanan White, M. Lucien F. Lethierry, Pastor Wallengren, Dr. Jacob Spånberg, Major-General Carden, Dr. Hearder, and Mr. Wellman.—A vote of thanks to the President and other officers of the Society having been passed, Mr. Elwes, Mr. McLachlan, Mr. H. Goss, and Canon Fowler replied, and the proceedings terminated.

Royal Meteorological Society, January 16.—Mr. R. Inwards, President, in the chair.—The Council, in their report, reviewed the work done by the Society during the past year, and also stated that additional accommodation had been provided to meet the growing needs of the library. Forty-five new Fellows had been elected during the year. Mr. Inwards, in his presidential address, dealt with the subject of "weather fallacies," which he treated under the head of saints'-day fallacies, sun and moon fallacies, and those concerning animals and plants. He also referred to the almanac makers, weather prophets and impostors, who have from time to time furnished the world with fit materials for its credence or its ridicule.—Mr. C. Harding read a paper on the gale of December 21-22, 1894, over the British Isles. This storm was one of exceptional severity, especially over the northern portions of England and Ireland and in the south of Scotland. It developed energy very quickly, and travelled with great rapidity. The self-recording anemometers show that the greatest violence of the wind occurred at Fleetwood, where the velocity was 107 miles in the hour between 8.30 and 9.30 a.m. on the 22nd; and for four consecutive hours the velocity exceeded 100 miles. This is the greatest force of wind ever recorded in the British Isles, and is 10 miles an hour in excess of the highest wind velocity in the great storm of November 16-20, 1893. At Holyhead the wind in squalls attained the hourly velocity of 150 miles between 10 a.m and noon on the 22nd. The strongest force was mostly from the north-westward. Much destruction was wrought both on sea and land, and there was a heavy loss of life.

PARIS.

Academy of Sciences, January 14.—On a method of verification, applicable to the calculation of series in astronomical problems, by M. Poincaré.—On autumn cultivations for green manures, by M. P. P. Dehérain. The author insists on the importance of autumn cultivations for subsequent digging in, for two main reasons: (1) nitrates are retained by the roots of growing plants, which would otherwise be lost in the drainage waters; (2) if buried at the proper time, the decomposition of the vegetable matter affords a considerable amount of useful fertilisation.—Experimental researches on the critical point of liquids holding solid substances in solution, by M. Raoul Pictet. From the results obtained, it appears that either solid bodies become gaseous and mix with other gases at temperatures below their points of fusion and under considerable partial pressures of their own vapours, or the solid bodies present are dissolved in droplets momentarily formed in many places in the mass of gas above the critical temperature of the solvent. In the latter case, a solid deposit should be formed on superheating the vapours. This point has to be investigated.—The treatment of vines, infested by phylloxera, with peat-moss impregnated with a mineral oil, by M. F. de Mély. Details are given of a process which appears to effectually clear off the pest without injuring vegetation.—On a method of drawing a right line by the aid of jointed links, by M. Raoul Bricard.—M. J. Janssen called the attention of the Academy to the contents and scope of the *Annuaire du Bureau des Longitudes*.—A letter from the *Königliche Gesellschaft der Wissenschaften* of Göttingen was read inviting the Academy to send delegates to Innsbrück, to take part in a meeting for the consideration of the problem of investigating the variation of the intensity of gravity with the geological character of the crust of the earth.—On the application to differential equations of methods analogous to those of Galois, by M. Jules Drach.—On the determination of the equations of continuous finite groups, by M. E. Vissiot.—On the law of transmission of energy between the source and the conductor, in the case of a permanent current, by M. Vaschy.—On the production of cathode rays, by M. Joseph de Kowalski. (1) The production of the so-called cathode rays does not depend on the discharge from metallic electrodes across a rarefied gas. (2) They are produced chiefly where the primary illumination attains a considerable intensity; that is, where the density of the current lines is very considerable. (3) Their direction of propagation is that of the current lines at the place where the rays are produced, from the negative to the positive poles.—On the *entraînement* of luminous waves by matter in motion, by M. G. Foussereau.—On some properties of silver sulphide, by M. A. Ditte. The double sulphides, $4Ag_2S.K_2S.2H_2O$ and $3Ag_2S.Na_2S.2H_2O$, are described, and a method for their preparation given.—On the preparation of amorphous silicon, by M. Vigouroux. The preparation is carried out by heating to about 540° a perfectly dry mixture of silica, magnesium, and magnesia. The silicon, by the usual treatment with acids, is obtained as a pulverulent, maroon-coloured substance.—On the protomorphic state: sulphides of zinc and manganese, by M. A. Villiers.—On some sensitive reactions of amido-benzoic acids, by M. Oechsner de Coninck.—On a class of nitriles, by M. Albert Colson.—On the constitution of hexamethylenetetramine, by MM. R. Cambier and A. Brochet.—On ethylenic methylal, by M. Louis Henry.—New researches on pectase and on the pectic fermentation, by MM. G. Bertrand and A. Mallèvre. Pectase exists in solution in the cellular sap of acid fruits, just as in carrot roots. There is no insoluble pectase. In acid fruits, its action is only apparent after neutralisation.—On the influence exercised by the nervous system and the internal pancreatic secretion on histolysis. Facts illustrating the mechanism of normal glycæmia and sugar diabetes. A note by M. M. Kaufmann.—The Pleistocene of the valley of Chambéry, by MM. J. Révil and J. Vivien.—Remains of striped hyænas from the quatenary of Bagnères-de-Bigorre (Hautes-Pyrénées), by M. Édouard Harlé.—On the quatenary phosphorites from the region of Uzès, by M. Charles Depéret.—An anemometer with multiple-electrical indications and automatic orientation, by M. Jules Richard.

BERLIN.

Physiological Society, December 21, 1894.—Prof. du Bois Reymond, President, in the chair.—Prof. Waldeyer gave a lengthy account of the most recent researches on the formative structures of the nervous system, laying special stress on the following statements. The entire nervous system consists of single elements which may most conveniently be called "neurons," each of which is composed of a nerve-cell and its processes. These processes are, on the one hand, protoplasmic "dendrites" which rapidly become branched, and, on the other hand "neurites" or "axons," which give off collateral branches, soon become medullated, and end in fine branchings, as also do the collaterals. Each nerve-cell has only one "axon." The dendrites convey impulses to the cell, the neurites or axons convey impulses from the cell. All nerve-fibres, both dendrites and neurites, end freely in fine branchings. Every physiological path of conduction, whether from the periphery to the central nervous system, or *vice versâ*, consists of two or more neurons, never of one. Conduction in the neurons is always longitudinal. Impulses are transmitted from one neuron to the other only by means of the free endings of the terminal branches. The lecture was illustrated by a series of schematic diagrams and some preparations.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—L'Industrie des Araneina: W. Wagner (St. Pétersbourg).—Summer Studies of Birds and Books: W. Warde Fowler (Macmillan).—Over de Bevruchting der Bloemen: J. MacLeod (Gent, Vuylsteke).—Lens-Work for Amateurs H. Orford (Whittaker).—Steel Works Analysis: Prof. J. O. Arnold (Whittaker).—Handbook for Hertfordshire, Bedfordshire, and Huntingdonshire (Murray).—Calcareous Cements: G. R. Redgrave (Griffin).—An Elementary Text-Book of Metallurgy: Prof. A. H. Sexton (Griffin).—Electrical Engineering: W. Slingo and A. Brooker, new edition (Longmans).—A Popular Treatise on the Physiology of Plants: Dr P. Sorauer, translated by Prof. Weiss (Longmans).—Whence Comes Man, from "Nature" or from "God"?: A. J. Bell, new edition (Isbister).—Why does Man Exist?: A. J. Bell, new edition (Isbister).—A Collection of Appliances and Apparatus for the Prevention of Accidents in Factories, 2nd edition (Dulau).—Elektrophysiologie: Prof. W. Biedermann, Erste Abthg. (Jena, Fischer).—Allgemeine Physiologie: Dr. Max Verworn (Jena, Fischer).—Manuals of Elementary Science: Zoology: Prof. A. Newton, new edition (S.P.C.K.).—Manuals of Health: Air, Water, and Disinfectants: Dr. C. M. Aikman (S.P.C.K.).

PAMPHLETS.—Elementary Practical Chemistry: J. T. Hewitt and F. G. Pope (Whittaker).—Latent Heat of Steam and Absolute Zero: W. Donaldson (Waterlow).

SERIALS.—L'Anthropologie, tome v. No. 6 (Paris).—Quarterly Review, January (Murray).—Archives of Surgery, January (West).—Journal of Anatomy and Physiology, January (Griffin).—Botanische Jahrbücher, Neunzehnter Band, 4 Heft (Leipzig).—Royal Natural History. Part 15 (Warne).—Rendiconto dell'Accademia delle Scienze Fisiche e Matematische, serie 2ª, Vol. viii, Fasc. 11°, e 12° (Napoli).—Bulletins de la Société D'Anthropologie de Paris. Nos. 5-7 (Paris).—Bulletins of the Rose Polytechnic Institute. No. 1: Physical Units: Prof. T. Gray (Terre Haute, Ind.).

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