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NATURE

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A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME V.

NOVEMBER 1871 to APRIL 1872

"*To the solid ground*
Of Nature trusts the mind that builds for aye."—WORDSWORTH

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INDEX

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 2, 1871

*RIPPLES AND WAVES**

YOU have always considered cohesion of water (capillary attraction) as a force which would seriously disturb such experiments as you were making, if on too small a scale. Part of its effect would be to modify the waves generated by towing your models through the water. I have often had in my mind the question of waves as affected by gravity and cohesion jointly, but have only been led to bring it to an issue by a curious phenomenon which we noticed at the surface of the water round a fishing-line one day slipping out of Oban (becalmed) at about half a mile an hour through the water. The speed was so small that the lead kept the line almost vertically downwards; so that the experimental arrangement was merely a thin straight rod held nearly vertical, and moved through smooth water at speeds from about a quarter to three-quarters of a mile per hour. I tried boat-hooks, oars, and other forms of moving solids, but they seemed to give, none of them, so good a result as the fishing-line. The small diameter of the fishing-line seemed to favour the result, and I do not think its roughness interfered much with it. I shall, however, take another opportunity of trying a smooth round rod like a pencil, kept vertical by a lead weight hanging down under water from one end, while it is held up by the other end. The fishing-line, however, without any other appliance proved amply sufficient to give very good results.

What we first noticed was an extremely fine and numerous set of short waves preceding the solid much longer waves following it right in the rear, and oblique waves streaming off in the usual manner at a definite angle on each side, into which the waves in front and the waves in the rear merged so as to form a beautiful and symmetrical pattern, the tactics of which I have not been able thoroughly to follow hitherto. The diameter of the "solid" (that is to say the fishing-line) being only two or three millimetres and the longest of the observed waves five or six centimetres, it is clear that the waves at distances in any directions from the solid exceeding fifteen or twenty centimetres, were sensibly unforced (that is to say moving each as if it were part of an endless series of uniform parallel waves undisturbed by any solid). Hence the waves seen right in front and right in rear showed (what became immediately an obvious result of theory) two different wave-lengths with the same velocity of propagation. The speed of the vessel falling off, the waves in rear of the fishing-line became shorter and those in advance longer, showing another obvious result of theory. The speed further diminishing, one set of waves shorten and the other lengthen, until they become, as nearly as I can distinguish, of the same lengths, and the oblique lines of waves in the intervening pattern open out to an obtuse angle of nearly two right angles. For a very short time a set of parallel waves some before and some behind the fishing-line, and all advancing direct with the same velocity, were seen. The speed further diminishing the pattern of waves disappeared altogether. Then slight tremors of the fishing-line (produced for example by striking it above water) caused circular rings of waves to diverge in all directions, those in front advancing at a greater speed relatively to the water than that of the fishing-line. All these phenomena illustrated very remarkably a geometry of ripples communicated a good many years ago to the *Philosophical Magazine* by Hirst, in which, however, so far as I can recollect, the dynamics of the subject were not discussed. The speed of the solid which gives the uniform system of parallel waves before and behind it, was clearly an absolute minimum wave-velocity, being the limiting velocity to which the common velocity of the larger waves in rear and shorter waves in front was reduced by shortening the former and lengthening the latter to an equality of wave-length.

Taking ·074 of a gramme weight per centimetre of breadth for the cohesive tension of a water surface (calculated from experiments by Gay Lussac, contained in Poisson's theory of capillary attraction, for pure water at a temperature, so far as I recollect, of about 9° Cent.), and one gramme as the mass of a cubic centimetre, I find, for the minimum velocity of propagation of surface waves, 23 centimetres per second.* The mini-

* Extract from a letter to Mr. W. Froude, by Sir W. Thomson.

* One nautical mile per hour, the only other measurement of velocity, except the French metrical reckoning, which ought to be used in any practical measurement, is 51·6 centimetres per second.

mum wave velocity for sea-water may be expected to be not very different from this. (It would of course be the same if the cohesive tension of sea water were greater than that of pure water in precisely the same ratio as the density.)

About three weeks later, being becalmed in the Sound of Mull, I had an excellent opportunity, with the assistance of Prof. Helmholtz, and my brother from Belfast, of determining by observation the minimum wave velocity with some approach to accuracy. The fishing-line was hung at a distance of two or three feet from the vessel's side, so as to cut the water at a point not sensibly disturbed by the motion of the vessel. The speed was determined by throwing into the sea pieces of paper previously wetted, and observing their times of transit across parallel planes, at a distance of 912 centimetres asunder, fixed relatively to the vessel by marks on the deck and gunwale. By watching carefully the pattern of ripples and waves, which connected the ripples in front with the waves in rear, I had seen that it included a set of parallel waves slanting off obliquely on each side, and presenting appearances which proved them to be waves of the critical length and corresponding minimum speed of propagation. Hence the component velocity of the fishing-line perpendicular to the fronts of these waves was the true minimum velocity. To measure it, therefore, all that was necessary was to measure the angle between the two sets of parallel lines of ridges and hollows, sloping away on the two sides of the wake, and at the same time to measure the velocity with which the fishing-line was dragged through the water. The angle was measured by holding a jointed two-foot rule, with its two branches, as nearly as could be judged, by the eye, parallel to the sets of lines of wave-ridges. The angle to which the ruler had to be opened in this adjustment was the angle sought. By laying it down on paper, drawing two straight lines by its two edges, and completing a simple geometrical construction with a length properly introduced to represent the measured velocity of the moving solid, the required minimum wave-velocity was readily obtained. Six observations of this kind were made, of which two were rejected as not satisfactory. The following are the results of the other four:—

Velocity of Moving Solid.	Deduced Minimum Wave-Velocity.
51 centimetres per second.	23·0 centimetres per second.
38 " "	23·8 " "
26 " "	23·2 " "
24 " "	22·9 " "
	Mean 23·22

The extreme closeness of this result to the theoretical estimate (23 centimetres per second) was, of course, merely a coincidence, but it proved that the cohesive force of sea-water at the temperature (not noted) of the observation cannot be very different from that which I had estimated from Gay Lussac's observations for pure water.

I need not trouble you with the theoretical formulæ just now, as they are given in a paper which I have communicated to the Royal Society of Edinburgh, and which will probably appear soon in the *Philosophical Magazine*. If 23 centimetres per second be taken as the minimum speed they give 1·7 centimetres for the corresponding wave-length.

I propose, if you approve, to call ripples, waves of lengths less than this critical value, and generally to restrict the name waves to waves of lengths exceeding it. If this distinction is adopted, ripples will be undulations such that the shorter the length from crest to crest the greater the velocity of propagation; while for waves the greater the length the greater the velocity of propagation. The motive force of ripples is chiefly cohesion; that of waves chiefly gravity. In ripples of lengths less than half a centimetre the influence of gravity is scarcely sensible; cohesion is nearly paramount. Thus the motive of ripples is the same as that of the trembling of a dew drop and of the spherical tendency of a drop of rain or spherule of mist. In all waves of lengths exceeding five or six centimetres, the effect of cohesion is practically insensible, and the moving force may be regarded as wholly gravity. This seems amply to confirm the choice you have made of dimensions in your models, so far as concerns escaping disturbances due to cohesion.

The introduction of cohesion into the theory of waves explains a difficulty which has often been felt in considering the patterns of standing ripples seen on the surface of water in a finger-glass made to sound by rubbing a moist finger on its lip. If no other levelling force than gravity were concerned, the length from crest to crest corresponding to 256 vibrations per second would be a fortieth of a millimetre. The ripples would be quite undistinguishable without the aid of a microscope, and the disturbance of the surface could only be perceived as a dimming of the reflections seen from it. But taking cohesion into account, I find the length from crest to crest corresponding to the period of $\frac{1}{256}$ of a second to be 1·9 millimetres, a length which quite corresponds to ordinary experience on the subject.

When gravity is neglected the formula for the period (P) in terms of the wave-length (l), the cohesive tension of the surface (T), and the density of the fluid (ρ), is

$$P = \sqrt{\frac{l^3 \rho}{2 \pi T}},$$

where T must be measured in kinetic units. For water we have $\rho = 1$, and (according to the estimate I have taken from Poisson and Gay Lussac) $T = 982^* \times \cdot 074 = 73$. Hence for water

$$P = \frac{l^{\frac{3}{2}}}{\sqrt{2 \pi \times 73}} = \frac{l^{\frac{3}{2}}}{21 \cdot 4}$$

When l is anything less than half a centimetre the error from thus neglecting gravity is less than 5 per cent. of P. When l exceeds $5\frac{1}{2}$ centimetres the error from neglecting cohesion is less than five per cent. of the period. It is to be remarked that, while for waves of sufficient length to be insensible to cohesion, the period is proportional to the square-root of the length, for ripples short enough to be insensible to gravity, the period varies in the sesquiplicate ratio of the length.

WILLIAM THOMSON

Mr. Froude having called my attention to Mr. Scott Russell's Report on Waves (British Association, York, 1844) as containing observations on some of the phenomena which formed the subject of the preceding letter to him, I find in it, under the heading "Waves of the Third Order," or, "Capillary Waves," a most interesting account of the

* 982 being the weight of one gramme in kinetic units of force-centimetres per second.

"ripples" (as I have called them), seen in advance of a body moving uniformly through water; also a passage quoted by Russell from a paper of date, Nov. 16, 1829, by Poncelet and Lesbros,* where it seems this class of waves was first described.

Poncelet and Lesbros, after premising that the phenomenon is seen when the extremity of a fine rod or bar is lightly dipped in a flowing stream, give a description of the curved series of ripples (which first attracted my attention in the manner described in the preceding letter). Russell's quotation concludes with a statement from which I extract the following:— . . . . "on trouve que les rides sont imperceptibles quand la vîtesse est moyennement au dessous de 25c. per seconde."

Russell gives a diagram to illustrate this law. So far as I can see, the comparatively long waves following in rear of the moving body have not been described either by Poncelet and Lesbros or by Russell, nor are they shown in the plan contained in Russell's diagram. But the curve shown above the plan (obviously intended to represent the section of the water-surface by a vertical plane) gives these waves in the rear as well as the ripples in front, and proves that they had not escaped the attention of that very acute and careful observer. In respect to the curves of the ripple-ridges, Russell describes them as having the appearance of a group of confocal hyperbolas, which seems a more correct description than that of Poncelet and Lesbros, according to which they present the aspect of a series of parabolic curves. It is clear, however, from my dynamical theory that they cannot be accurate hyperbolas; and, as far as I am yet able to judge, Russell's diagram exhibiting them is a very good representation of their forms. Anticipating me in the geometrical determination of a limiting velocity, by observing the angle between the oblique terminal straight ridge-lines streaming out on the two sides, Russell estimates it at $8\frac{1}{2}$ inches ($21\frac{1}{2}$ centimetres) per second.

Poncelet and Lesbros's estimate of 25 centimetres per second for the smallest velocity of solid relatively to fluid which gives ripples in front, and Russell's terminal velocity of $21\frac{1}{2}$ centimetres per second, are in remarkable harmony with my theory and observation which give 23 centimetres per second as the minimum velocity of propagation of wave or ripple in water.

Russell calls the ripples in front "forced," and the oblique straight waves streaming off at the sides "free" —appellations which might seem at first sight to be in thorough accordance with the facts of observation, as, for instance, the following very important observation of his own:—

"It is perhaps of importance to state that when, while these forced waves were being generated, I have suddenly withdrawn the disturbing point, the first wave immediately sprang back from the others (showing that it had been in a state of compression), and the ridges became parallel; and, moving on at the rate of $8\frac{1}{2}$ inches per second, disappeared in about 12 seconds."

Nevertheless I maintain that the ripples of the various degrees of fineness seen in the different† parts of the fringe are all properly "free" waves, because it follows from dynamical theory that the motion of every portion of fluid in a wave, and, therefore, of course, the velocity of propagation, is approximately the same as if it were part of an infinite series of straight-ridged parallel waves, provided that in the actual wave the radius of curvature of the ridge is a large multiple of the wave-length, and that there are several approximately equal waves preceding it and following it.

No indication of the dynamical theory contained in my communication to the *Philosophical Magazine*, and described in the preceding letter to Mr. Froude, appears either in the quotation from Poncelet and Lesbros, or in any other part of Mr. Scott Russell's report; but I find with pleasure my observation of a minimum velocity below which a body moving through water gives no ripples, anticipated and confirmed by Poncelet and Lesbros, and my experimental determination of the velocity of the oblique straight-ridged undulations limiting the series of ripples, anticipated and confirmed by Russell. W. T.

* Memoirs of the French Institute, 1829.

† The dynamical theory shows that the length from crest to crest depends on the corresponding component of the solid's velocity. For very fine ripples it is approximately proportional to the reciprocal of the square of this component velocity, and therefore to the square of the secant of the angle between the line of the solid's motion and the horizontal line perpendicular to the ridge of the ripple.

ALLBUTT ON THE OPHTHALMOSCOPE

On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys; also in certain other General Disorders. By Thomas Clifford Allbutt, M.A., M.D., Cantab. &c. (London and New York: Macmillan and Co., 1871.)

THE advances that have been made in the knowledge of the diseases of the eye since the introduction of the ophthalmoscope are now very widely known, not alone in the medical profession but to the general public. This little instrument, essentially consisting of a mirror with a hole in the centre by which a ray of light can be thrown into the interior of the eye, lighting up its recesses, and enabling, with the aid of a common hand lens, almost every portion of it to be explored, may be said to have revolutionised the surgery of the eye. Many separate and distinct types of disease have been distinguished in conditions that were formerly grouped together under the general term of amaurosis, and the ophthalmic surgeon, no longer administering, as was too often formerly the case, his remedies in rash ignorance, is now able either to infuse well-grounded hope of recovery, or to spare his patient the annoyance of protracted treatment when treatment would be hopeless. For nearly twenty years the use of the ophthalmoscope has been, as was natural, almost entirely restricted to those who devoted themselves to the study of ophthalmic diseases. Like other mechanical aids to diagnosis, as the stethoscope and laryngoscope, its employment requires practice, the opportunities for acquiring a mastery over it were till recently rare, and its value in the practice of medicine was by no means generally recognised. Within the last few years, however, several excellent surgeons and physicians, amongst whom Mr. Hutchinson, Dr. Hughlings Jackson, Dr. John Ogle, and the author of the treatise before us may be especially mentioned, have gradually begun to recognise that the ophthalmoscope may be made available not only to determine the nature of any defect of vision of which the patient may complain, but as a means of reading within certain limits changes in the conditions of the system at large, and of the nervous system in particular.

The work of Dr. Allbutt is, however, the first treatise in English that is occupied exclusively with the ophthalmoscopic appearances presented in cases of cerebral disease, or in other words with the diagnosis of nervous affections by the ophthalmoscope. Abroad he has been preceded by M. Bouchut, whilst the volumes of the "Archiv für Ophthalmologie" are a mine of original memoirs written by the best ophthalmologists in Germany on the bearings of ophthalmoscopic observations on nervous affections. To these, of course, Dr. Allbutt makes frequent reference. In no instance, however, have we noticed a servile adherence to the opinions of others, the statements he quotes being always checked by his own observations, and every page bearing the stamp of very careful and sound investigation. It is impossible with the limited space here at disposal, and it would perhaps scarcely be interesting to many of our readers, to give what the work really deserves, a *résumé* and discussion of its successive chapters; but we may here perhaps point out one or two of the principal points of interest.

In speaking of the disc of the optic nerve, Dr. Allbutt expresses himself in favour of the view of Galezowski, who is fortified by the observations of Leber, to the effect that the vascularity of the disc is to a great extent independent of that of the retina, and rather forms a part of the vascular system of the brain. The importance of this principle in enabling deductions to be drawn respecting the occurrence of intercranial disease is obvious. Proceeding on this hypothesis, Dr. Allbutt points out the changes that are visible in a large number of different affections. He draws a strong line of distinction between ischæmia and optic neuritis, conditions that have hitherto been almost invariably confounded by ophthalmic surgeons, but of which the former is produced by some cause, often of a mechanical nature, interfering with the return of the blood from the retina, whilst the latter is a true inflammation of the nerve. The diagnosis of the two in their earlier stages is very clearly and correctly laid down. At a later period both conditions pass into white atrophy, and it is not always then easy to pronounce which of the two has previously been present. His views, in regard to changes in the optic disc from intercranial disease, are clearly laid down in the following passage (pp. 129, 130):—"We find optic changes in connection with two kinds of intracranial disease in particular; the one tumour, the other meningitis. When we analyse the matter one degree further, we ascertain that, although the choked disc (ischæmia) and the inflamed nerve may co-exist with either of these kinds of disease, that nevertheless the choked disc is far more commonly found in association with tumour and hydrocephalus than the inflamed nerve. The inflamed nerve, on the other hand, is very commonly found in association with meningitis, and of meningitis not of the surface, nor of parts near any supposed vasomotor centres, but with meningitis near the centre." And with this we are disposed substantially to agree. Dr. Allbutt expresses himself in very doubtful terms in regard to the existence of tobacco amaurosis, and it certainly is extraordinary that, if really constituting an effect of the use of that leaf, it is not of more frequent occurrence amongst the Germans and Americans, who are much larger consumers than either the French or ourselves.

Our readers will see that Dr. Allbutt has, if not exactly opened up, at all events vigorously worked at, a new field of medical investigation. This field promises when duly cultivated to yield very valuable fruit; and, we are sure, the conclusion at which every candid reader will arrive, after carefully perusing it, will be that no physician should consider he has fully examined any case of cerebral disease unless he has accurately investigated the appearances presented by the eye under the ophthalmoscope. It is not to be supposed that Dr. Allbutt has by any means exhausted the subject. Many difficulties lie in the path of the most diligent inquirer. In many instances conditions of disease are seen to be present, as to the nature of which only a guess can be formed, and respecting which from forgetfulness or lack of observation on the part of the patient no history can be obtained; whilst in a multitude of cases the disease is seen only at one stage of its progress, and the physician is unable to ascertain, owing to his losing sight of his patient, the ulterior changes that take place.

Lastly, in many cases the prejudice of friends (a point to be greatly regretted) prevents the examination of the eyes after death. The fragmentary character of many of the reports of cases collected by Dr. Allbutt in his appendix is painfully evident, and leaves many hiatuses to be filled up by future research. We may, however, in conclusion, thank Dr. Allbutt for having published a work which constitutes an important step in the advancement of medicine, and will certainly form a very valuable guide to the profession at large, nor may we omit to thank the publishers for the excellent manner in which the book has been issued from the press. H. POWER

OUR BOOK SHELF

Hardy Flowers: Descriptions of upwards of thirteen hundred of the most ornamental species, and directions for their arrangement, culture, &c. By W. Robinson, F.L.S. (London: F. Warne and Co., 1871.)

MR. ROBINSON is a prolific writer, but his prolificacy (as Webster has it, if Dr. Ingleby and Dr. Latham will allow us the word) does not degenerate into mere book-making. Like its predecessors, this volume is one of practical utility both to the professional gardener and to the cultivator of flowers for their beauty. Much the greater part of the volume is occupied with a descriptive list of the most ornamental hardy flowers, with directions for their culture, suitable positions, &c.; but this is introduced by some practical hints on the general subject of gardening. That Mr. Robinson has the courage to attack some time-honoured gardening customs, will be seen from the following paragraph:—"No practice is more general, or more in accordance with ancient custom, than that of digging shrubbery borders, and there is none in the whole course of gardening more profitless or worse. When winter is once come, almost every gardener, although animated with the best intentions, simply prepares to make war upon the roots of everything in his shrubbery border. The generally accepted practice is to trim, and often to mutilate, the shrubs, and to dig all over the surface that must be full of feeding roots. Delicate half-rooted shrubs are often disturbed; herbaceous plants, if at all delicate and not easily recognised, are destroyed; bulbs are often displaced and injured; and a sparse depopulated aspect is given to the margins, while the only 'improvement' that is effected by the process is the annual darkening of the surface of the upturned earth." After this we find some pertinent and useful hints on the best mode of grouping hardy perennials, and the art of managing the rock-garden, the wild-garden, water, and boggy ground; on the culture and propagation of early flowers, and other subjects dear to the dweller in the country. Compared with the art of gardening as practised twenty years ago, we are certainly now in an altogether new and improved epoch, and Mr. Robinson is one of the pioneers to whom we are mainly indebted for the introduction of a better and more rational style. A. W. B.

Hints on Shore-Shooting: with a chapter on skinning and preserving Birds. By James Edmund Harting, F.L.S., &c. (London: Van Voorst, 1871.)

A GOOD sportsman, whether he knows it or not, must be more or less of a good naturalist, and this Mr. Harting is. His unpretending little book, therefore, certainly deserves mention here, and the more so since he has worthily won his spurs by making the group of birds most sought by the "shore-shooter" an especial subject of study. What he tells us is the result of his own observation, and is pleasantly told. What he does not tell us is whether "shore-shooting" has, with most people,—for we except him—any other *raison d'être* than the "fine-day-let's-go-and-kill-something" impulse. If not, we really do not see that there is much difference in principle between Pagham and Hurlingham.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

An Universal Atmosphere

I HAVE much pleasure in replying to Mr. Browning's question in NATURE, vol. iv. p. 487, as it is one that legitimately strikes at the root of all my speculations, and which, if unanswerable, conveys an objection that must demolish the whole structure I have endeavoured to erect in my essay on the "Fuel of the Sun."

If I am right, the atmospheres of the sun, the moon, the planets, or of any other cosmical body of known mass and dimensions, may be calculated in units of the earth's total atmosphere by the simple formula reasoned out in Chap. iii. of the above-named work, *i.e.*, by multiplying the mass of the body (expressed in units of the earth's mass) by its own square root, thus

$$x = m \sqrt{m};$$

where x is the atmosphere of the body in question expressed in units of the earth's known total atmosphere, and m is the mass of the body expressed in units of the earth's mass.

The mass of the moon being $\frac{1}{80}$ that of the earth, we get

$\frac{1}{80}\sqrt{\frac{1}{80}} = \frac{1}{715 \cdot 5416}$, or, discarding fractions, the moon's atmosphere as $\frac{1}{715}$ that of the earth. But the diameter of the moon being to that of the earth as 0·264 to unity, the lunar surface will be to that of the earth as $0 \cdot 264^2$ or 0·0697 to 1, and the lunar atmosphere will be concentrated accordingly, bringing the mean atmospheric pressure on the lunar surface to

$$\frac{\frac{1}{715}}{0 \cdot 0697} = \frac{1}{49 \cdot 8355}, \text{ or } \frac{1}{50},$$

nearly of that of the earth's mean atmospheric pressure. Such an atmosphere would support a column of mercury six-tenths of an inch in height. Mr. Browning will recognise this as about equal to the best vacuum obtainable in an old-fashioned air-pump of average defectiveness.

Such is the theoretical pressure upon every part of the moon's surface, supposing the form of the moon to be a perfect spheroid of rotation with a perfectly smooth surface. But the moon is no such regular body. It presents far more irregularities in proportion to its size than would our earth if the ocean were evaporated, and its depths laid bare so that our mountain heights should be measured from the ocean bottom. Under such conditions the bulk of even our atmosphere would occupy the ocean valleys, and very rare indeed would be the remainder that reached the mountain tops and elevated ridges of the earth. On the moon with its filmy atmosphere of only six-tenths of an inch mean pressure, the rarefaction on the high lands and mountains would be carried beyond the limits of observable refractive power under the conditions assumed—viz., of a special atmosphere merging gradually into the universal interstellar medium.

The visible edge of the moon which effects the occultation of a star must in almost every possible case be formed by the ridges and summits of the lunar mountains, in no case by the bottom of the lower valleys, for in looking horizontally across the moon's rotundity these valleys and even the maria must be foreshortened, and their lower depths walled out of the reach of our vision by the great lunar elevations. Thus the occultation of a star would occur without its previous plunging behind any outlying lunar atmospheric matter of appreciable density. We must not forget that Sir J. Herschel's calculation, which assigns one second of refraction to an atmosphere equal to $\frac{1}{1980}$ of the density of the earth, is based on the theory of a limited atmosphere with a sharp and definite boundary suddenly terminating in a vacuum.

But this rarefaction on the elevated portions of the moon demands a compensating condensation or concentration of atmospheric matter in the valleys, crater-pits, and maria. Here the pressure on the moon's surface should considerably exceed the calculated mean. This consideration suggests a very interesting question. Would such an atmosphere, say capable of supporting one inch of mercury, produce any observable effects? If I am right in regarding water as one of the constituents of the universal atmosphere, there are good reasons for supposing that it would.

The small share of water due to the moon would all be raised far above its low boiling point, early in the lunar day, by the heated lunar surface. There would be no sea, no clouds, no rain, no snow, but on the plains and in the valleys a formation of hoar-frost should occur at the lunar eventide, beginning just where the sun's rays become too oblique to maintain the temperature of the rapidly radiating lunar surface above the freezing-point.

In a note appended to Mr. Lockyer's translation of M. Guillemin's work on "The Heavens," the Rev. T. W. Webb thus corrects the author's rather positive statements concerning the total absence of a lunar atmosphere: "After all fair deductions on the score of imperfection of observation or precipitancy of inference, there are still residuary phenomena, such as, for instance, the extraordinary profusion of brilliant points which on rare occasions diversify the Mare Crisium, so difficult of interpretation, that we may judge it wisest to avoid too positive an opinion." Now the Mare Crisium is a great depression of the lunar surface close upon that edge of the moon which, to our vision, first receives and loses the solar illumination. If I am right, aqueous vapour should be suddenly forming there during the early crescent period after the new moon, and the hoar-frost should be as suddenly precipitated as this wide depression rolls towards the darkness after the full moon. In that chapter of the "Fuel of the Sun" which is devoted to the meteorology of the moon and Mercury, I have discussed some of the theoretical results of these conditions and the appearances they should present. I may here merely add that, as the temperature of any part of the moon's unmantled surface must directly and very rapidly vary with the incidence of solar radiation, all the undulating regions of the moon must at morning and evening have a very patchy temperature, the slopes towards the sun being hotter than our tropics, while the opposite side of the same hill receiving the solar rays with great obliquity, and radiating into space almost without impediment, must retain a freezing temperature, and thus the cryophorous phenomena, which Sir John Herschel describes as a possible result of the contrasted temperatures of the opposite sides of the moon, should be effected even by the shady lunar craters and contrasted hill-slopes.

On the highlands of the moon no appreciable amount of hoar-frost precipitation should take place on account of the absence of sufficient atmosphere; but on the deeper maria, wherever the conditions are the most favourable, the patchy temperature should produce patches of such precipitation. If anywhere visible, these should be seen on the Mare Crisium, on account of its proximity to the edge of the moon, for there the morning rays that strike most obliquely upon the cold slopes would be the most effectively reflected towards the earth. Not having seen any original or detailed account of the phenomena to which Mr. Webb alludes, I am unable to say whether they fulfil these theoretical conditions, but I believe that something more may be learned by means of careful observations specially directed to the elucidation of the questions I have suggested.

W. MATTIEU WILLIAMS

Woodside, Croydon, Oct. 23

Pendulum Autographs

IT may interest some of your readers to know that they can for themselves observe in the most accurate manner the motion of the compound pendulum described by Mr H. Airy* by merely attaching the ends of a fine thread to two points in the ceiling of a room, and suspending a leaden bullet by means of a second thread tied to the middle point of the former, so that the bullet may just escape the floor. Lay underneath a large sheet of white paper ruled with two dark lines at right angles to each other to correspond to the two axes of vibration. It is Mr. Airy's experiment with the hoop on an extended scale. The motion of the bullet, unimpeded by contact of pencil with paper, is graceful and accurate in the extreme.

Perhaps the most remarkable case is that in which the two points of suspension are taken about an inch apart, and the third about half an inch below them; the pendulum will now keep reversing its motion as uniformly as before, and apparently without any adequate cause, a matter of astonishment to the uninitiated spectator.

I believe the general equation to the path, including all the curves described, will be found to be $\sqrt{n}\, cos^{-1} \frac{x}{a} = \sqrt{m}\, cos^{-1} \frac{y}{b}$, where the particle starts from the point (a, b) and is attracted to the axes of X and Y by forces $= -ny$ and $-mx$ respectively.

Woolwich, Oct. 24 GEO. S. CARR

Exogenous Structures in Coal Plants

I CORDIALLY agree with your recommendation that discussion on the Exogenous Stems of the Coal Measures should cease for the present. It is evident that I shall not convince my two opponents, and they are as far as ever they were from convincing me. But I must request that in justice to me, you will allow me to enter a protest against the last paragraph of Prof. Dyer's article, in which he objects to my applying the term Protoplasmic to the cambium layer, and endeavours to show that I am two hundred years behind the age in my physiology. I cannot but think that Prof. Dyer, when he penned that paragraph, knew perfectly well in what sense I used that expression. I meant by it nothing more than is implied in the following sentence, taken from Prof. Balfour's "Manual of Botany," p. 43, which certainly does not belong to the age of Grew:—

"External to the woody layers, and between them and the bark, there is a layer of mucilaginous semifluid matter, which is particularly copious in spring, and to which the name Cambium has been given. In this are afterwards found cells, called Cambium Cells, of a delicate texture, in which the protoplasm and primary utricle are conspicuous."

W. C. WILLIAMSON

Fallowfield, Oct. 25

*** This correspondence must now close.—ED.

Classification of Fruits

IT seems from the numerous attempts that have been made that a philosophical classification of fruits is either unattainable or practically of very little value when attained. At any rate working botanists have, as a rule, discarded the majority of the carpological terms that are to be found in text-books as too cumbrous or too uncertain in their application. Among the latest attempts at simplification in the matter of the classification of fruits are those of my friends Prof. Dickson and Dr. M'Nab (see NATURE, vol. iv. p. 475). Both of these are open to some criticism on matters of detail, but I can hardly expect you to accord me space to point out what I believe to be the merits or shortcomings of their respective schemes. I should also trespass too much on your courtesy and on the patience of your readers did I enter into any engthened explanation of the following scheme, in which I have adopted to some extent the nomenclature of Prof. Dickson and Dr. M'Nab, and which I offer for consideration solely on the grounds of expediency and simplicity:—

Classification of Monothalamic Fruits

A. Ripe pericarp uniform

Fruits indehiscent	I. Nuts or Achænocarps.
Fruits dehiscent	II. Pods or Regmacarps.

B. Ripe pericarp of two or more layers of different substance *

Seeds within a hardened endocarp	III. Stone-fruits or Pyrenocarps.
Seeds embedded in pulp*	IV. Berries or Sarcocarps.

I. *Nuts or Achænocarps*

WINGLESS—	
Fruit of one carpel, or, if of more, apocarpous	Achene
Fruit of more than one carpel	
Carpels ultimately separate but indehiscent	Carcerule (Cremocarp).
Carpels inseparate	
Pericarp adherent to the seed . .	Caryopsis.
Pericarp free from the seed, within a cupule	Glans.
WINGED	Samara

II. *Pods or Regmacarps*

Fruit of one carpel	
Opening by one suture	Follicle
two sutures	Legume
transversely	Lomentum (Dichisma, *M'Nab*)
Fruit of more than one carpel	
Opening by pores or sutures . . .	Capsule (Siliqua) (Regma) (Conceptaculum) (Tryma, &c.).
Opening transversely	Pyxis.

III. *Stone-fruits or Pyrenocarps*

Carpels one or more, superior	Drupe (Fibro-drupe as in *Cocos, Grewia*, sp., &c.).
Carpels one or more, adherent to, or enclosed within a fleshy receptacle . .	Pome (Sphalerocarpium, as in *Hippophae*)

IV. *Berries or Sarcocarps*

Seeds embedded in pulp	Bacca (Hesperidium) (Uva) (Pepo).

I believe that the foregoing arrangement will include most of the varieties of fruits and seed-vessels, though, as in all similar cases, exceptional forms are not readily sorted into their proper place; the fruit of such Cassias as *C. Fistula*, generally called a lomentum, is a case in point. For general purposes the varieties enclosed in brackets may well be omitted, save in the case of so well known and constantly used a term as *siliqua*, which, despite Prof. Dickson's veto, I think is too useful practically to be lightly abandoned.

MAXWELL T. MASTERS

The Berthon Dynamometer

ABSENCE from home, and many engagements, have prevented an earlier reply to "W. R.'s" letter in NATURE, October 5. In my previous communication I believe I gave the address of the inventor, to whom I thought reference might naturally be made; in order, however, to meet "W. R.'s" wish, I will explain the construction of the very simple but efficient instrument in question. It is merely a V gauge, formed of two pieces of thin brass converging at a very acute angle, and graduated along one of the edges; the divisions being viewed through a lens held in the

* See NATURE, vol. iv pp. 310, 317.

* The pericarp is here understood as including not only the ripened carpellary wall, but also any adjunct to it which in process of development may be combined with it. In the same manner the pulp may be a production from the carpel or from the seed itself.

hand simultaneously with the image of the object-glass or speculum formed by the eye-piece; the diameter of that image is given at once by the divisions to $\frac{1}{500}$ of an inch, and can be readily estimated to half that value. The arrangement mentioned by "W. R." is no doubt very convenient, and quite adequate for his purpose; but for high powers I should suppose that the comparative coarseness of the engraved lines would make itself much more felt than it is in Mr. Berthon's invention, and the balance of economy is so greatly in favour of the latter in comparison with every contrivance with which I am acquainted, that I have no hesitation in saying that it ought to be in the hands of every amateur who cares to know the magnifying power of his telescope. It may be procured for five shillings, of Mr. Tuck, watchmaker, Romsey. T. W. WEBB

New Form of Cloud

THE kind of cloud described by M. André Poëy (NATURE, Oct. 19, 1871, p. 489) is by no means new or rare if I can judge correctly from the figure and explanation. It may often be seen on the lower part of the flank of a great rain or thunder cloud, and appears to arise from the dropping or subsidence of portions of the air heavily loaded with watery particles. My own impression is that it appears when the cloud is about to break up. M. Poëy will find the cloud described in the *Philosophical Magazine* for July 1857, where the name of *droplets* is given to the form, and its position in a thunder cloud indicated by a figure. J.

Spectrum of Blood

IN my letter, published in the last number of NATURE, I am strangely enough made to say that "we must not rely on *the* spectrum." This is an error of the printer. The sentence should have been:—"I have always argued that in such inquiries we must not rely on *one* spectrum, but compare the action of various reagents." H. C. SORBY

Broomfield, Sheffield, Oct. 28

Earthquake in Burmah

I HAVE not read in your "Notes" any record of the earthquake which was felt at daylight of the 16th February last in this city, in two successive and gentle but decided shocks, doing no damage, but which, from the files of the Calcutta *Englishman* of February 18, seems to have been severe to the N.W. of this, extending through Cachar, Silchar, Gowahatty, to Calcutta and Barrackpore.

This earthquake, you will observe, is synchronous with those of the western hemisphere already recorded by you.

CHARLES HALSTED

Mandalay, Burmah, Sept. 1

A Plane's Aspect

I AGREE with Mr. Proctor that the disuse of the term "position" in geometry would be a serious misfortune; happily, however, it is not its disuse, but the prevention of its misuse which is contemplated. I cannot agree with him that "position" is a word "which no one can misunderstand," for his own letter is a striking example of its being misunderstood, either by Mr. Proctor, or by others. "Aspect and slope," he tells us, "indicate two elements, which, together, *fix* the position" of a plane. Geometers, however, certainly understand, when a plane is said to be given *in position*, that something more than its aspect and slope may be regarded as known. Parallel planes have necessarily the same slope and aspect, but surely not the same position. To be told that, because its slope and aspect are invariable, the plane of Saturn's rings has a fixed position in space, notwithstanding that the planet moves bodily in its orbit, would scarcely satisfy a student of astronomy accustomed to geometrical precision.

There can be no doubt that "position" is the true English equivalent of the German word "Lage," and that no ambiguity of the kind above indicated could attach itself to the term, had we a suitable English rendering for the word "Stellung." I do not consider the term "aspect" to be perfect as an equivalent for "Stellung," but I have no hesitation in admitting that Mr. Laughton's suggestion is happier than any previous one I can remember. Mr. Proctor declares his intention of opposing the "use of the word 'aspect' in a sense not at present assigned nor properly assignable;" but when he wrote thus, he had not seen the letter of Mr. Wilson wherein the term "aspect" is very rigidly defined to be the direction of the normal. To me this very facility with which the word "aspect" lends itself to rigid definition, is a ground of objection against it. I have never seen Stellung defined in the manner in which Mr. Wilson has defined "aspect." Von Staudt, in whose admirable writings I first met the word, introduced it thus: "Parallel planes possess something in common, which may be regarded as appertaining to each one of them, and shall be called their 'Stellung;' the 'Stellung' of a plane, therefore, is determined by any plane which is parallel thereto, and two planes have the same 'Stellung' or different 'Stellungen' according as they are parallel to, or intersect one another."

That the term "aspect" is not sufficiently elastic to permit of its taking the place of "Stellung" in the above passage cannot, I think, be well maintained by Mr. Proctor, seeing that he has not himself hesitated to use it in two widely different senses in the following passage of his letter: "I can see no reason why 'aspect' should be regarded in a new and unfamiliar aspect." The expression "aspect of a plane," whether it be retained or not as the equivalent of the "Stellung einer Ebene," appears to me, I confess, to be much too good to be claimed by Mr. Proctor as indicative solely of *the direction of the projection of the normal upon a certain plane of reference*. I would suggest, in the interest of his twelve excellent books, that he might qualify "aspect," as thus defined, by an appropriate adjective, for the term is there used in a very technical sense indeed, and is not even applicable to all planes. Although Mr. Proctor can assign, for example, a southerly aspect to the face of a roof which has a slope of 30°, he would find some difficulty in describing the aspect of a roof which has no slope at all, whereas Mr. Wilson would without hesitation pronounce its aspect to be vertical.

Athenæum Club, Oct. 31 T. ARCHER HIRST

IT is due to my friend and your correspondent, Mr. Cecil J. Monro, of Hadley, to state that, to my knowledge, he was in the habit of employing the word "aspect" in this technical sense long before the publication of Mr. Laughton's letter, and I should not be surprised to learn that other geometers have used it before Mr. Monro.

I think Mr. Proctor will find few to agree with him in his condemnation of the word so used. For myself I heartily agree with Mr. Wilson in the welcome he accords to this "old friend with a new face." C. M. INGLEBY

Highgate, N., Oct. 27

I AM glad to find, by Mr. Wilson's letter in NATURE for October 26, that the word "aspect," which I suggested, is accepted by him as satisfactory; as, in fact, the word wanted. But another correspondent in the same number, Mr. Proctor, pertinaciously insists on the superior merit of the word "position," to be used in the particular sense explained by Mr. Wilson in his former letter. In this I conceive Mr. Proctor is entirely wrong.

"In geometrical language"—I quote from Gregory's "Solid Geometry," 1845—"the position of a plane is determined by making it pass through three given points." Mr. Proctor says he "can see no reason why 'position' should be dismissed from the position it has so long occupied." No more can I. I would only call his attention to the fact that the meaning which he would assign to the word "position" is quite different from that which has been accepted, in a technical sense, by geometers, and in an everyday sense by everyday people.

Mr. Proctor's special objections to the word "aspect," rest, it seems to me, on a misconception of its meaning and familiar use. We speak of the aspect of a wall, but not of the aspect of a roof, nor of a hill. What the usage amongst builders in respect of roofs may be, I don't know, but geographers almost invariably speak of the "slope" of a hill, as, for instance, the southern slope of the Himalayas. Put into exact language, the aspect of a plane is the direction of its normal; and as parallel planes have parallel normals, any number of parallel planes have the same aspect, without reference to their position; but no two planes, parallel or not, can possibly have the same position.

The word "slope" is almost equally inadmissible; in the first place, it refers to some other plane, which is apt to cause confusion; and in the second, although all parallel planes have the same slope, any number of other planes not parallel can also have it; the word is therefore not sufficiently definite. "Tilt," a word spoken of by Mr. Proctor, as though it had been suggested, has no geometrical meaning whatever. As a substantive it is a "tent," or "awning;" it has also been sometimes used poetically as an equivalent to "tournament," and is, I believe, the familiar abbreviation of "tilt-hammer." These are its only meanings, and none of them apply to a plane.

I would only add that I do not quite see what the fact mentioned by Mr. Proctor, that he has written twelve books in the last six years—interesting as it may be from a bibliographical point of view—has to do with the matter.

Oct. 29 J. K. LAUGHTON

THREE elements are necessary to fix the position of a plane as I understand the word "position." If "aspect" and "slope" be the names of two of these, the third will be the perpendicular upon the plane from some fixed point. It is because the term "position" implies the fixedness of this third element that it is inappropriate to express my friend Mr. Wilson's meaning.

My friend Mr. Proctor will pardon me if I do not consider the question entirely settled by the fact that he has written perspicuously and explained clearly by the use of a term which fixes too much. With an improved scientific terminology, he will be able to make his next twelve books superior (if that be possible) to those he has written within the last six years.

"Aspect" and "slope" stand on the same footing, one connotes a reference to the points of the compass, the other to the horizon. Neither can be used in Mr. Wilson's sense without departing from their colloquial meaning, but it is perfectly competent for geometers to take a word from common conversation and give it a scientific meaning. Either of these words may be used in Mr. Wilson's test sentences. Parallel planes have the same slope, two slopes determine a direction, &c.

It is yet possible that some correspondent can suggest a better term, either one imported from ordinary life or one conceived for the purpose.

THE CORRESPONDENT WHO SUGGESTED "SLOPE"

Geometry at the Universities

PROF. THISELTON DYER has well pointed out a distinction which exists between the mathematical courses at Oxford and Cambridge. But his conclusion, that at Oxford "special attention to geometrical methods would pay very well," though acceptable in its way, falls far short of what I advocate. The great want at both Universities is a course of geometrical studies; and the proof that such a want exists is to be found in the fact that the geometrical treatises in use at either University, cover so very limited a range. There are not even any text-books on the geometry of the sphere, cone, cylinder, and like simple solids, or on such curves as the lemniscate, cycloid, and the simpler spirals. A few stray notes on these subjects may be found in some of the text-books, but not a thorough and systematic geometrical investigation of any of them. Geometrical treatises might with advantage range much further. A geometrical treatise on ellipsoids would be of immense use apart from its employment as a means of mental training. Geometrical treatises on paraboloids of both kinds, on hyperboloids of one sheet and of two sheets, on the various orders of ring-surfaces and screw-surfaces, and on many other tridimensional matters, would afford invaluable exercise to the student, besides having a real value to the scientific worker. I venture to express my conviction, that a course of such studies would tend to develop mathematical powers much more thoroughly even than the study of covariants and contravariants, Jacobians, Hessians, *et hoc genus omne*.

If there is one department of mathematical research in which our countrymen are fitted by their mental habitudes to distinguish themselves pre-eminently, it is precisely this neglected department of geometrical research. As it is, though we have geometricians of great power, no systematic geometrical work is done in England. Our treatises range only over the most elementary geometrical subjects, and even in discussing these subjects our writers are fain to accept the assistance of Continental geometricians. One would conceive that each of our Universities might yearly send out many who could treat of the elements of geometry without keeping a hand always on some French or German text-book.

Brighton, Oct. 27 RICHD. A. PROCTOR

DEEP-SEA DREDGING IN THE GULF OF ST. LAWRENCE

THE marine zoology of the deeper parts of the River and Gulf of the St. Lawrence has not been investigated until quite recently. This summer, under the auspices of the Natural History Society of Montreal, and in consequence of the kindness of the Hon. Peter Mitchell, Minister of Marine and Fisheries for the Dominion, (who not only gave me facilities for dredging on board Government vessels, but also caused sufficient rope to be provided for the purpose), depths of from 50 to 250 fathoms were successfully examined. The greatest depth in the Gulf, to the west of the Island of Newfoundland, as given in the Admiralty charts, is 313 fathoms. It is thought that a general sketch of the results obtained may be of interest to the readers of NATURE.

The cruise lasted five weeks, the first three of which were spent on board the Government schooner *La Canadienne*, and the remaining two on the *Stella Maris*. The area examined includes an entire circuit round the Island of Anticosti, and extends from Point des Monts (on the north shore of the St. Lawrence) to a spot about half way between the east end of Anticosti and the Bird Rocks. As these investigations were almost necessarily subordinate to the special duties on which the schooners were engaged, in several cases the same ground was gone over twice.

The bottom at great depths generally consists of a tough clayey mud, the surface of which is occasionally dotted with large stones. So far as I could judge, using an ordinary thermometer, the average temperature of this mud was about 37° to 38° Fahrenheit, at least on the north shore. In the deepest parts of the river, on the south shore, between Anticosti and part of the Gaspe Peninsula, the thermometer registered a few degrees higher. Sand dredged on the north shore in 25 fathoms also made the mercury sink to 37° or 38°.

Many interesting Foraminifera and Sponges were obtained, but as yet only a few of these have been examined with any care. A number of Pennatulæ were dredged south of Anticosti; the genus has not been previously recorded, so far as I am aware, as inhabiting the Atlantic coast of America. They were found in mud, at depths of 160 and 200 fathoms, and it seems probable that this species, at least, is sedentary, and that it lives with a portion of the base of the stem rooted in the soft mud. *Actinia dianthus* and *Tealia crassicornis* were frequent in 200 to 250 fathoms. The Echinoderms characteristic of the greater depths are a *Spatangus* (specifically distinct from the common British species), *Ctenodiscus crispatus*, *Ophioglypha Sarsii* (very large), *Ophiacantha spinulosa*, and *Amphiura Holbollii*. Marine worms, of many genera and species, were both numerous and fine. Among the more interesting of the Crustacea were *Nymphon grossipes* (?) and a species of *Pycnogonum*. Several of the last named Crustaceans were taken at a depth of 250 fathoms, entangled on a swab, fastened in front of a deep-sea lead, which was attached to the rope, a few feet from the mouth of the dredge. This circumstance tends to show that the genus is not always parasitic in its habits. The Decapods, Amphipods, &c., at least those of greatest interest, have not yet been identified. Among the most noticeable of the marine Polyzoa are *Defrancia truncata*, and what appears to be a *Retepora*. Not many species in this group were obtained in very deep water, and those procured were, for the most part, of small size. About six species of Tunicates were collected. Being anxious to have Mr. J. Gwyn Jeffreys' opinion upon the various species of Mollusca during his visit to Montreal, I studied these carefully first, and submitted the whole of them to him for examination. Twenty-four species of Testaceous Mollusca were obtained at depths of from 90 to 250 fathoms. Nearly all of these are Arctic forms, and eleven of them are new to the continent of America.

The following are some of the most interesting of the deep-water Lamellibranchiata:—*Pecta grœnlandicus* of Chemnitz, but not of Sowerby; * *Arca pectunculoides* Scacchi; *Yoldia lucida* Loven; *Y. frigida** Torell; *Neœra arctica** Sars; *N. obesa** Loven. Among the novelties in the Gasteropoda of the same zone are the subjoined:—*Dentalium abyssorum* Sars; *Siphonodentalium vitreum* Sars; *Eulima stenostoma* Jeffreys; *Bela Trevelyana;* Chrysodomus (Sipho) Sarsii.** Three Brachiopods occur in the Gulf, of which *Rhynchonella psithacea* and *Terebratella Spitzbergensis* are found in about 20—50 fathoms, and *Terebratula septentrionalis* in from 100—250. A few rare shells were obtained in comparatively shallow water; among them an undescribed *Tellina* (of the section *Macoma*), a new *Odostomia*, and *Chrysodomus (Siphho) Spitzbergensis** Reeve. Nor were even the Vertebrata unrepresented; from a depth of 96 fathoms off Trinity Bay, a young living example of the Norway "Haddock" (*Sebastes Norvegicus*) was brought up in the dredge. And off Charleton Point, Anticosti, in 112 fathoms, on a stony bottom, two small fishes were also taken; one, a juvenile wolf-fish (*Anarrhicas lupus*) the other a small gurnard, a species of *Agonus*, probably *A. hexagonus* Schneid.

The similarity of the deep-sea fauna of the St. Lawrence to that of the quaternary deposits of Norway, as described by the late Dr. Sars, is somewhat noticeable. *Pennatulœ*, *Ophiura Sarsii*, *Ctenodiscus crispatus*, several Mollusca, &c., are common to both; but on the other hand, the absence of so many characteristic European invertebrates on the American side of the Atlantic should be taken into consideration. The resemblance between the recent fauna of the deeper parts of the St. Lawrence, and that of the Post-pliocene deposits of Canada, does not seem very close, but our knowledge of each is so limited that any generalisations would be premature.

J. F. WHITEAVES

* I am indebted to Mr. Jeffreys for the identification of species to which an asterisk is attached. He corroborates also my determination of the remainder.

THE REDE LECTURE AT CAMBRIDGE

ONE of the indirect results of university reform has been the establishing at Cambridge of the Rede Lecture, one of the highest intellectual treats of the whole year, as will at once be acknowledged when the names of the distinguished persons who have delivered it since its establishment in 1858 are known—viz., Professors Owen, Phillips, Max Müller, Willis, Ansted, Airy, Tyndall, Miller, Ruskin, Huggins, General Sabine, Sir W. Thomson, and Mr. Norman Lockyer. For many years past there had been certain lecturers at various colleges, whose duty it was to deliver lectures on mathematics, philosophy, rhetoric, and logic; but in 1858 the endowments for these lectures (originally given in 1524 by Sir Robert Rede, Chief Justice of the Common Pleas in the reign of Henry VII.) were amalgamated, and the result has been the delivery once a year of the Rede Lecture by some distinguished man of science chosen by the Vice-Chancellor for the time being. Such is the history of the benefaction; but it must now be added that as the remains of this distinguished man lie in a village church in Kent, that of Chiddingstone, near Eden Bridge, in which parish he lived and died, without a memorial or inscription of any kind over his grave, it is proposed to do for him what Cicero did for the unhonoured grave of Archimedes, and an effort is, therefore, being made to mark his place of burial by erecting a window of stained glass in the chancel that he built. The cost of the memorial, with suitable inscription, cannot be less than 160*l.*, but nearly 70*l.* has been raised by subscriptions from the distinguished persons who have delivered the lecture, and by other friends, members of the university and otherwise—viz., the Earls of Powis, Derby, and Strathmore, the Vice-Chancellor, the Masters of Jesus and Clare Colleges, the Provost of King's, Professors Selwyn and Sedgwick, Mr. Beresford-Hope, M.P., Sir John Lubbock, M.P., the Public Librarian, Rev. W. H. Latham, and J. Brocklebank, with many others; but the amount thus subscribed, together with the local effort, is inadequate for the full completion of the memorial, and it is hoped that there will be some others who will be willing to help on the work. Mr. Norman Lockyer, F.R.S., the present holder of the office of Rede Lecturer, has kindly consented to receive subscriptions at 6, Old Palace Yard, Westminster.

It is proposed to erect the following inscription, from the pen of Professor Selwyn, who will receive any subscription forwarded to him at Cambridge.

IN PIAM MEMORIAM
ROBERTI REDE MILITIS
CAPITALIS JVSTICIARII
DOMINI REGIS HENRICI VII.
DE COMMVNI BANCO
QVI HOC SACELLVM
ÆDIFICAVIT
GRATI AC MEMORES
BENEFICIORVM
CANTABRIGIENSES SVI
HANC FENESTRAM
PONI CURAVERVNT

THE CONJOINT EXAMINATION SCHEME*

THE proposition carried at the last meeting of the Council of the College of Surgeons clears away, we suppose, the last difficulty in the way of an amalgamation between the Colleges of Physicians and Surgeons for the purposes of examination and of issuing diplomas. It is remarkable that the College of Surgeons should have come back to the original proposal, though it was at first demurred to and given to a committee for consideration. The College of Physicians, at its Comitia on Thursday, finally agreed to this proposal; and it now only remains for the General Medical Council to give its consent under the Medical Act of 1858, so as to allow of the fusion in question.

In order to get at the practical working of the proposed scheme of division of fees, we may take the present income of the College of Surgeons from the membership diploma, adding 10*l.* for each diploma issued to represent the additional fee to include the College of Physicians. The sum produced by the membership diploma during the last financial year was close upon 8,000*l.*; and if we add 10*l.* for each of the 291 diplomas issued, we have in round numbers the sum of 11,000*l.* The proposed scheme is, that one-half of this should be devoted to all the expenses of the examinations, and that the remaining moiety of 55,000*l.* should be divided into thirds. One-third is to go to the support of the Museum of the College of Surgeons and its unendowed professorships, one-third for the maintenance of the *personnel* of the College of Physicians, and one-third similarly to that of the College of Surgeons. This will give the Hunterian Museum and each of the Colleges some 1,800*l.* a year apiece, irrespective of other sources of income. With this income, it will, we imagine, be perfectly possible to carry on satisfactorily the establishments in Pall Mall and Lincoln's Inn Fields, if due economy be observed and proper supervision exercised over the subordinate officials. The Hunterian Museum will be upon a somewhat shorter allowance than heretofore; but if this prove insufficient, Parliament must be appealed to for a grant in favour of what the Council of the College of Surgeons properly characterises as an "institution of national as well as professional importance."

* Reprinted from *The Lancet.*

SIR RODERICK MURCHISON

THE life of a scientific man is for the most part uneventful, and perhaps to the world at large uninteresting. That he was born, lived a certain number of years, and died, are often the chief facts chronicled of the man himself. Of his work and of the influence of his work men are willing to read, but for the story of his life, with its quiet everyday monotony, they care little. Yet it is true, at least of the higher type of mind, that the story of the man's life and the history of the work he accomplished are inseparably connected, and are each necessary for the understanding of the other. There arise, too, ever and anon instances when the man was not merely a man of science, but one whose scientific career formed as it were a nucleus round which many other and often divergent interests gathered. Such a man's life is sometimes linked in so many ways with that of the society in which he lived, that its chronicle becomes in some degree the history of his time. And such a man was Roderick Impey Murchison. By no means standing on the highest platform of scientific intellect, a patient gatherer of facts rather than a brilliant generaliser from them, he yet gained by common consent in the commonwealth of science the position of a king, under whom men of all ranks, and even men of far higher ability and attainment than his own, were not only willing but delighted to serve. He held a place which no other man of science left among us now fills. It was not merely his achievements in geology, memorable as these were, which gave him that proud pre-eminence, nor did he owe anything to success in other branches of science, for he seldom travelled beyond what he knew to be his proper domain, nor to graces of literary style, on which men of slender acquirements often float

THE LATE SIR RODERICK I. MURCHISON

into popularity. He wrote only on geological and geographical subjects, and in a solid matter-of-fact way not likely to attract readers who did not previously care for his subjects. It was his personal character, his noble-heartedness, his indomitable energy, his tact and courtesy, the dignity and grace which he never failed to show even to opponents, and the social position which his family and fortune gave him, and which enabled him greatly to extend the respect shown in society to science and scientific men, —it was these causes which largely went to make Sir Roderick's influence what it was. A narrative, to do him justice, should tell how these causes came into play, and how, combined with the regard which he could always claim for his solid contributions to science, they placed him so high in the scientific circle in which he moved.

Murchison was born on February 19, 1792, at his father's little estate of Tarradale, in Eastern Ross-shire. He used to speak with fondness of the fact that he first saw the light amid those old palæozoic sandstones, conglomerates, and schists, on which he was afterwards to rest part of his title to fame. Yet it was not among the wilds of Ross that he acquired a love for rocks. He was removed from his birthplace at an early age, and taken into Dorsetshire, and though when still a child he was brought back into Scotland, and remained with his mother at Edinburgh for a short while, it was in England that he spent most of his boyhood, and where he was educated. At the age of fifteen he obtained a commission in the 36th Regiment of Foot, and served in the Peninsula under Sir Arthur Wellesley. He carried the colours at the Battle of Vimiera, and went through much hardship in the retreat of Corunna. At the end of the war, after having become a captain of dragoons, he quitted the army, and marrying the daughter of General Hugonin, settled in England. So ended what he used to call the military episode of his life. Next came the fox-hunting period, when his activity of disposition found vent in the excitement of the chase, into which he threw himself heart and soul. He might have continued a merry, hearty, sporting, country gentleman, but for the influence of his wife, who was fond of natural history pursuits, and the advice of Sir Humphrey Davy, who, meeting him at the house of Mr. Morritt, of Rokeby, and seeing in him promise of something better than fox-hunting, advised him to attend the Lectures of the Royal Institution. Sir Roderick used to tell an interesting anecdote of that early beginning of his scientific career. He was attending the lectures of (if we remember) Dr. Brande, when one day the lecturer's place was taken in his absence by a pale thin lad, his assistant, who gave the lecture and experiments in so admirable a manner as to be received at the end with a hearty round of applause. It was Michael Faraday, and this was his first public appearance.

After gaining considerable knowledge from public lectures and private instruction, Sir Roderick's active mind sought as early as possible to study Nature in the field. Geology was the branch of science which suited best a nature so fond of out-of-door life as his. He had made the acquaintance of William Smith, the father of English Geology, from whose own lips he had learned the order of succession which the marvellous patience and ingenuity of that pioneer of the science had made out for the rocks of England and Wales, and indeed, as was afterwards found, for the rocks of all the world. In the year 1825, when he was thirty-three years of age, he wrote his first-published paper, "A Geological Sketch of the North-western Extremity of Sussex and the adjoining parts of Hants and Surrey." From that time onwards for nearly half a century he continued to furnish accounts of his observations in the field. Beginning, as was natural, with the district in which he lived, he soon extended his researches even as far as his own native Highlands, then step by step over the Continent of Europe, even as far as the confines of Asia. He has published more than 100 memoirs on British and Continental Geology, besides numerous addresses to scientific societies, and in addition to upwards of twenty memoirs in conjunction with other authors. To all this mass of work must be added what he published in separate volumes—his great "Silurian System," his splendid volumes on "Russia," and the successive editions of his "Siluria."

Of the incidents of his life during its scientific period it is not necessary here to say much, nor to try to count up the honours showered on him from all parts of the world. There was hardly a scientific Academy anywhere which had not enrolled him among its associates, and to the dignities conferred on him by his own Sovereign, were added others conferred by Emperors and Kings abroad. His time was largely passed in London, where he took an active share in scientific work. But every year he made a tour either in this country or on the Continent, and added to our knowledge of the geological structure of the districts which he visited. Sometimes these tours were prolonged, and in the case of his Russian campaign he was absent for two or three years from England.

At the time when Murchison broke ground as a geologist, the science of geology had entered a new phase of its history. The absurd system of Werner, though still upheld by high authority in this country, was daily losing ground, and the simple and obvious classification of William Smith on the one hand, and the doctrines of Hutton on the other, were guiding all the younger intellects of the day. Murchison's tact is nowhere more conspicuous than in his choice of a field for the exercise of his patient energy of research. He saw that the old Wernerian notion of "transition" rocks was doomed, and that it would be a task well worthy of his time and toil to unravel the succession of these rocks, and try to introduce into them the same order and consistency which Smith had shown to mark the Secondary series of England. He chose for the scene of his researches the border country of England and Wales, where these old rocks are well displayed, and after five years of unremitting labour he produced his "Silurian System"—a work, which, though dealing only with the rocks of a limited tract of Britain, yet first unfolded the earlier chapters of the history of life upon our globe. The classification he adopted, though of course necessarily subject to local variation and change, has been found to hold true on the great scale over the whole world.

This work laid the foundation of Sir Roderick's fame. In his subsequently published "Siluria," which has gone through several editions, he recast the original work, introducing much detail regarding the extension of Silurian and older palæozoic rocks into other countries; but while in the later publication, the results given were necessarily often the work of other observers—the "Silurian System" remains a monument of the unaided labour of a mind quick in observation, sagacious in inference, patient in the accumulation of data, and full of that instructive appreciation of the value of facts not yet understood, which is near of kin to genius.

It would be beyond the limits of this journal to offer an adequate outline of Sir Roderick's scientific work. He was distinctly and specially a geologist. His early attachment to palæozoic rocks never waned, and though now and then he was led to make and record observations on later formations, he always returned to the older deposits as his natural inheritance and domain. He was not a palæontologist, but no geologist could use more skilfully than he the data furnished by palæontology. This faculty he acquired at the beginning of his career, and it marked all his work in the field both at home and abroad. It enabled him to apply to distant countries the principles which he had so successfully used in his own. Perhaps the leading idea of his scientific life should be regarded as the establishment of the order of succession among rocks. This was what he did in the Silurian region originally, and what he always endeavoured to ascertain in every district to which choice or accident might lead him. He had a singularly quick eye for the geological structure of a country. No one who travelled with him through a hilly tract, and, after listening to his rapid inferences, has gone actually over the ground to see, could fail to be struck with the accuracy with which he seized on some of the leading features, and from these deduced the general arrangement of the rocks. It was in this way, and by the use of palæontological evidence, that he was enabled to arrive at one of the most brilliant generalisations he ever achieved, when he brought order and intelligibility into the chaos of the so-called primary rocks of his own Scottish Highlands—a deduction which is, perhaps, destined to bear fruit of which he never dreamed, in the still obscure subject of metamorphism.

Sir Roderick Murchison's early training in geology was acquired at a time when men believed in periodic cataclysms, by which the surface of the globe was destroyed and renewed. He never could, and he never seemed seriously to try, to shake himself free from the influence of that training. Though he modified his views as years went on, he remained a member, and indeed in this country the leader, of the Cataclysmic School. The upholders of a long line of successive creations and of the former greater intensity of all geological causes have lost in him one of their ablest, staunchest, and most influential associates.

To the world at large, however, it was not from his geological work chiefly that Murchison was known. His generalisation as to the probable gold-bearing nature of the Australian quartz-country, and as to the probable aspect of the interior of Africa, are probably familiar to most people. But in later years what has especially brought his name into prominence is the chivalrous devotion with which he has maintained one might almost say the national belief in the welfare of Dr. Livingstone. Yet this is only a sample, though one which has come more publicly before us, of the tenacious friendship and active benevolence which have always marked him. As President of the Geographical Society—a society which is in a sense his own creation—he had frequent opportunities of befriending not only the cause of geography but the personal well-being of travellers, and he never failed to use them. The geographers have good cause to lament the death of their chief.

Of the man himself, what he was as he lived and moved among us, his loss is too recent to permit us justly to speak. We can only think of him as the stately courteous old gentleman, carrying even to the last that military bearing which dated from the days of Wellesley and Moore, kindly and thoughtful in his kindliness—a man whose friendship, once given, even ingratitude and injustice could not wholly alienate. He was not without some of the littlenesses of humanity, but they were so transparent, and often even so child-like, that we forget them in the recollection of all the goodness of heart and strength of head and nobility of nature which have gladdened us for so long, but which are now only subjects of tender remembrance.

ARCH. GEIKIE

HOMOPLASY AND MIMICRY

ALL students of the remarkable phenomenon of superficial resemblances in the animal and vegetable kingdom will be glad that Prof. Dyer has published an extension of the paper which he read on this subject at the Edinburgh meeting of the British Association. It is especially satisfactory that he has abandoned the very objectionable term "pseudomorphic," and substituted that of "homoplastic," a very much better term, because it simply expresses a fact without committing one to any theory. There are, however, one or two points in his paper of last week, on which I should wish to be allowed to comment.

Prof. Dyer holds that the distinction between "mimicry" in animals and "homoplasy" in plants, is "sufficiently obvious," the difference assigned being, apparently, that in the one case it takes place between species found in the same locality, in the other between species unconnected geographically. I doubt, however, whether facts will warrant this distinction. The most remarkable instances of "mimicry" among animals hitherto published are, undoubtedly, in the case of species inhabiting the same area; but I am inclined to think that, when attention is called to the subject, others will be found between animals not so associated, though these instances would naturally not attract so much observation. And secondly, homoplasy in plants does frequently occur in species occupying the same area. The statement reported to have been made by Prof. Dyer at Edinburgh that "the resembling plants are hardly ever found with those they resemble," would scarcely be borne out by a careful investigation. The real objection to the terms "mimicry" and "imitation" is that they seem to imply a *conscious* effort at convergence, which will hardly be conceded in the case of Lepidoptera any more than of Ferns. The substantial difference betweeen the two is that, in the case of animals, the resemblance appears to be protective, while in the case of plants, there is seemingly no such benefit arising from it; but this is a difference in result and not in the nature of the phenomenon itself. I fail to see that the objections to the use of these terms in the case of plants do not equally apply to animals; we have no reason to suppose that the two sets of phenomena are not produced by similar causes.

Prof. Dyer states, and no doubt truly, that the external resemblances of plants may frequently be traced to the effect of similar external conditions, and quotes in support Mr. E. R. Lankester's view with regard to animals. But in assuming that this explanation will account for all such phenomena if fully investigated, I think too much is assumed. Cases of homoplasy are referable to two distinct classes—resemblances in general habit, and resemblances of particular organs. The former, as in the case of the homoplasy between a *Cactus* and a *Euphorbia* or a *Stapelia*, or between a *Kleinia* and a *Cotyledon*, are no doubt due to the operation of similar external conditions of climate and soil. But in the second class this explanation wholly fails.

As illustrations of the kind of resemblance I mean, I may refer to the two collections of "mimetic plants" exhibited by Mr. W. W. Saunders at the two last *soirées* of the Linnean Society, a list of which will be found in NATURE for May 26, 1870, and May 4, 1871. The extraordinary resemblance in the markings of the leaves in plants thus grouped together, might well deceive the most experienced botanist. To account for this homoplasy on the ground of similar external conditions, is to start a mere hypothesis, without any facts to warrant it. A still more curious series of resemblances occurs in the case of fruits than of leaves, so close that it has deceived botanists of the experience of the elder Hooker, Bentham, and Kunth into placing species in a genus with which they have no structural affinity whatever. I have in my mind in particular two samaroid fruits, both from the forests of Brazil, so absolutely identical in external facies, that distinction is quite impossible without dissection, and yet belonging to exceedingly remote orders. I will not, however, say more on this point, as it would be impossible to appreciate the closeness of the homoplasy without drawings, which I hope shortly to be able to publish. The singular part of this resemblance is, that, as far as we know, it is never protective. In our Bee-orchis we have what might well have been assumed *primâ facie* to be a case of protective resemblance, the flower being so fashioned in order to attract bees to assist in its fertilisation. It is remarkable, however, that the Bee-orchis is one of the few plants that appear to be perpetually self-fertilised, never being visited by insects. It is just possible that we have an instance of protective or rather beneficial resemblance of scent in the case of the carrion-like odour of the flowers of *Stapelia*, which attracts blue-bottle and other flies.

In a paper read at the recent meeting of the American Association for the Advancement of Science, by Prof. E. D. Cope, I find the following thoughtful remarks:—"Intelligence is a conservative principle, and will always direct effort and use into lines which will be beneficial to its possessor. Thus, we have the source of the fittest, *i.e.*, addition of parts by increase, and location of growth-force directed by the will, the will being under the influence of various kinds of compulsory choice in the lower, and intelligent option among higher animals. Thus intelligent choice may be regarded as the originator of the fittest, while natural selection is the tribunal to which all the results of accelerated growth are submitted. This preserves or destroys them, and determines the new points of departure on which accelerated growth shall build."

Biologists generally are, probably, hardly prepared to apply the terms "intelligence" and "will" to the vegetable kingdom; but the use of the term "vegetable life" seems to me to imply of necessity that there are powers at work in the economy of the plant, as of the animal, which it is vain to attempt to reduce to manifestations of the forces which govern the inorganic world.

ALFRED W. BENNETT

NOTES

In our present number we give a portion of Prof. T. Sterry Hunt's Address at the Indianapolis meeting of the American Association, and propose in following numbers to reprint some of the more important papers read at the meeting. The next meeting will be held at San Francisco, and the following officers were elected for the meeting of 1872: President, Prof. J. Lawrence Smith, of Louisville; Vice-President, Prof. Alex. Winchell, of Ann Arbor; Permanent Secretary, Prof. Joseph Lovering, of Cambridge; General Secretary, Prof. E. S. Morse, of Salem; Treasurer, William S. Vaux, of Philadelphia; Auditing Committee, Dr. H. Wheatland, of Salem, and Prof. H. L. Eustis, of Cambridge; Standing Committee, *Ex Officio*, Messrs. Smith, Winchell, Lovering, Morse, Vaux, Gray, Barker, Putnam. Committee from the Standing Committee to arrange for next meeting, Profs. J. L. Smith, Asa Gray, Joseph Lovering, in connection with a committee from the Association at large, consisting of Profs. J. L. Smith, J. D. Whitney, and O. C. Marsh.

The Senate of the University of London on Wednesday last week exercised for the first time its privilege, under the Public Schools Act, of appointing a member of the governing body of Rugby and Charterhouse Schools. To Charterhouse it appointed Mr. Busk, F. R. S., President of the Royal College of Surgeons, thus recognising the claims of science in the direction of education. To Rugby it nominated Dr. Temple, Bishop of Exeter (late head master of Rugby).

The Inaugural Meeting of the Newcastle College of Physical Science on Tuesday last week was a great success. The Council decided unanimously, on the application of upwards of seventy ladies, to make no distinction of sex in the admission of pupils, placing all on a footing of exact equality. The total number of students admitted up to the time of the inaugural ceremonial, was fifty-one. In contrast to this we may note that last week the governing body of the University of Edinburgh rejected by a small majority Dr. Alexander Wood's motion, "That, in the opinion of this Council, the University authorities have, by published resolutions, induced women to commence the study of medicine at the University; that these women, having prosecuted their studies to a certain length, are prevented from completing them for want of adequate provision being made for their instruction; that this Council, without again producing any opinion on the advisability of women studying medicine, do represent to the University Court, that, after what the Senatus and Court have already done, they are at least bound in honour and justice to render it possible for those women who have already commenced their studies to complete them."

According to M. Le Verrier, Prof. Alluard of Clermont-Ferrand has obtained a grant of the necessary funds for establishing his long-projected observatory on the summit of the Puy-de-Dome.

Fathers Secchi and Denza and M. Diamilla Müller are engaged in organising a series of researches in the Mont Cenis tunnel, for the purpose of ascertaining what variations gravity and magnetism may undergo there.

The Mayor and other inhabitants of the town of Belfast lately expressed their sense of Prof. Wyville Thomson's many efforts for the encouragement of Science, and for the improvement and gratification of the working classes, in a suitable address, accompanied by a valuable service of plate.

The *Bulletin Astronomique* gives the following observations of Tuttle's comet. From M. Borrelly of Marseilles:—October 12, Marseilles M.T., $16^h 29^m 19^s$, R.A. $9^h 9^m 44^s{\cdot}68$, Decl. $+ 44° 16' 15''{\cdot}1$. The comet had the appearance of a diffuse nebulosity badly defined; it appeared elongated in the direction N.W. by S.E.; it was feeble but of moderate extent, about $2' 20''$. The approximate correction of Mr. Hind's ephemeris, given by this first observation, is $\Delta\alpha = + 0^m{\cdot}5$, $\Delta\delta = + 1° 3'$. From MM. Loevy and Tisserand of Paris:—October 14, Paris M. T. $12^h 36^m 12^s{\cdot}2$, R A. $9^h 14^m 35^s{\cdot}29$, polar distance, $47° 12' 13''{\cdot}1$. The comet resembled a whitish nebulosity, diffuse, and of irregular form. Its diameter was about $3'$; the light scarcely that of a star of the 13th magnitude.

Dr Hooker, of Kew, has placed the Lichens which he collected during his Morocco expedition in the hands of the Rev. W. A. Leighton, of Shrewsbury, for examination and determination.

The first Servian Agricultural Exhibition was opened with great ceremony at Belgrade, on October 2.

In addition to the announcements last week, the following works are in preparation:—From Edward Stanford:—The Laws of the Winds Prevailing in Western Europe, by W. Clement Ley, with charts, diagrams, &c., Part I.; Notes on the Geography of North America, Physical and Political, intended to serve as a text-book for the use of elementary classes; Notes on the Geography of South America, intended to serve as a text-book for the use of elementary classes. The following additional volumes are also announced to Weale's Series, published by Lockwood and Co.:—Analytical Geometry and Conic Sections, by J. Hann, new edition, entirely re-written by J. R. Young, numerous diagrams; Treatise on the Construction of Iron Bridges, Girders, Roofs, and other Structures, by Francis Campin, C.E., numerous illustrations; Drawing and Measuring Instruments, by J. F. Heather, M.A., numerous woodcuts; Optical Instruments, by J. F. Heather, M.A., numerous woodcuts; Surveying and Astronomical Instruments, by J. F. Heather, M.A., numerous woodcuts; Physical Geology, partly based on Portlock's "Rudiments of Geology," by Ralph Tate, numerous woodcuts; Historical Geology, partly based on Portlock's "Rudiments of Geology," by Ralph Tate; Emigrants' Guide to Tasmania and New Zealand, by James Baird, B.A.; Workman's Manual of Engineering Drawing, by J. Maxton, seven plates and nearly 325 woodcuts; Mining Tools, for the Use of Mine Managers, Agents, Students, &c., by W. Morgans; Atlas to the above, containing 235 illustrations.

A new horticultural Magazine is announced to be shortly commenced, with the title of *The Garden*, under the editorship of Mr. W. Robinson, F.L.S., author of "Hardy Flowers," "Alpine Flowers for English Gardens," &c.

A complete geological and statistical history of Australia by C. E. Meinicke, with a magnificent coloured map by A. Petermann, appears as a supplementary number of Petermann's "Mittheilungen."

The Ven. Archdeacon Pratt has reprinted a lecture on "The Descent of Man in Connection with the Hypothesis of Development," delivered at the Dalhousie Institute, Calcutta, on July 28, in which the Darwinian doctrine of evolution is vigorously combated.

The High Wycombe Natural History Society has resolved upon a new course of action suggested by the fact that its meetings had become pleasant social gatherings rather than in any way furthering the pursuit of natural science. In future the meetings will be held at the house of the President, the Rev. T. H. Browne, F.G.S., and will partake more of the nature of classes for the study of certain subjects. A loss in the number of members is expected, but it is hoped that those who remain will benefit by the change. Other local societies would do well to adopt a somewhat similar arrangement. The Quarterly Magazine of the above body is discontinued.

ASSISTANT-SURGEON VERCHERE, of the Indian Army, has suggested, says the *Medical Times and Gazette*, that some experiments should be made with reference to meteorological influences on sickness and health. Medical Meteorology in India is still all but an unknown science, and, as at present studied, is useless to medical practitioners. The long range of "readings" tells us nothing; but a register of the effects of meteorological conditions on the men selected for the purpose, with all conditions of exposure, &c., taken into account, and compared with the average sickness of a corps for the same period, would teach us more in a few months than yards of meteorological tables. We understand that the Sanitary Commissioners of India are favourable to the proposal of Mr. Verchere, and it will therefore, probably, be carried out.

THE *Wigtonshire Free Press* says that the foundation of a lake dwelling has been discovered by Mr. Charles Dalrymple, Kineliar Lodge, Aberdeenshire, on a small circular island at the south end of the Black Loch, Castle-Kennedy. On removing the surface soil, a circle of stones was discovered, the diameter of which was between 50 and 60 feet. On digging deeper through the stratum of forced earth and stones, three feet thick, what appeared to be a different and older layer of soil was reached. Among this black earth were found wood ashes, bits of calcined bones, and flat stones placed contiguously. Immediately below the stones, at the depth of a few inches, an artificial flooring was discovered, formed of the trunks of oak and alder trees. At this point the level of the loch was reached, and the influx of water prevented further excavations in a downward direction. In 1865-6, by the draining of Dowalton Loch, in the same county, several lake-dwellings were exposed; in the spring of this year, when the White Loch of Castle-Kennedy, which is now in connection with the Black Loch by a short canal, was being dragged with a net for trout, the net brought up a canoe of ancient make. In all likelihood it was the ferry-boat, or one of several perhaps, used by the lake-dwellers.

THERE is a volcanic eruption going on in the Hawaian Islands at Maunalva, but its exact site has not been recognised. From Kowa the lava was seen at night to rise to a height of several hundred feet in a column. The eruption is supposed to be near the locality of that of 1868, while others think it is nearer the summit of the mountain, on the scene of the great eruption of 1859. On September 6, an eruption took place on the southern slope of Maunalva.

THE Constantinople earthquake is now known to have originated in the southern region of the island of Scio, where it began strongly, growing weaker towards its northern circumference. At the Dardanelles it was much sharper than at Rodosto, while at Boorgas, on the Black Sea, it was very slight, and further on at Varna was not felt.

ANOTHER small place to be marked soon as a big one is Chimbote on the coast of Peru. Its harbour, the finest in the South Pacific, can shelter the navies of the world. It was a great town in the times of the Incas, as remains of a colossal aqueduct will show. Near it are coal mines. It has been abandoned and neglected on account of the difficulties of access, but a railway is now to be constructed to the fertile interior at a cost of 6,400,000*l*.

To the map of Bolivia must be added the small town of Calama in the new mining district of Caracolas.

EXPERIMENTAL farms are now being extended in the Madras presidency—a most essential step for agricultural improvement and practical instruction.

THE Government of Madras has been ordered to furnish special information on the Neilgherry nettle fibre plant.

ON the 5th July a most destructive typhoon attacked Hiogo, in Japan.

IN the same presidency, in the Parambalore district, a man-eating tiger has appeared, and killed four men, so that the Government has taken him into consideration, and placed a price of 30*l*. on his head.

THE Island of Gorgona, off the coast of Choco, is much complained of by ship captains for its electric storms, and its irregular currents. It has held this reputation since the time of Pizarro.

A VALUABLE discovery of workable lead ore is announced from Jersey.

THE latest report from Tasmania in regard to the experiments for introducing salmon and trout into that country, shows that while the success of the cultivation of both is extremely probable, the existence of trout of large size is unmistakeable.

COAL has been found in large quantities on the banks of a stream flowing into the Godavery, about 224 miles from Juggianet, and ninety-six from Budrachellum. It is close to the surface, and it is extremely probable that fresh deposits will be found in the adjacent British territory.

IT is to be noted that on the night of the 21st of August a very severe earthquake was felt at Callao, in Peru, at 8.32 P.M. The undulations were from N.W. to S.E. The shock was of fifteen seconds' duration. It was also felt severely at Cero, Azul, and Pisco. The sea, which previously had been unusually calm, suddenly became very rough, and a strong southerly wind set in. For two days the sea remained very rough at Cero Azul. The observations were confirmed by the steamship *Colon*. The shock severely shook the ship while it lasted. It was felt six miles to the westward of Chala Point at 8.50 P.M. ship time, and the sea almost immediately thereafter became agitated.

MEASURES are being taken by the Chilian Congress to prohibit the destruction of timber, particularly in the neighbourhood of springs. The timber districts of the provinces of Llanquihue, Valdivia, Chiloe, and of the Magellan territory are exempted from the law.

COCOS Island, in lat. 5° 30′ N. in the Pacific Ocean, about 600 miles west of the Columbian coast, has now for some years been occasionally occupied by treasure seekers on a legend of a treasure buried by buccaneers. At present it is again abandoned, but it is alleged a new expedition is organised. The island is not flat, as stated in many newspapers, but is volcanic, and 2,000 feet high. It is covered with timber and scrub, and being visited by frequent and heavy rains is always green. The place is riddled with shafts, some 150 feet deep. It produces nothing eatable.

THE valuable timber so abundant in the North Island of New Zealand is deserving of a better fate than to be cut down wholesale and used as firewood. The rimu, or red pine, is most valuable for furniture and all ornamental work; the matai, or black pine, is more brittle and heavy than the other, but will take a most beautiful polish; whilst the totara, another so-called pine (for they are none of them Coniferæ), is easily worked both green and dry. There is also the rata, "that wonderful vegetable production forming itself out of numberless vines, which first receive their support from some full-grown tree, then enclose it in a deadly embrace, and gradually expel the remains of their foster parent as their own growing demands for space require to be satisfied, then finally uniting themselves form a solid tree, with all the characteristics of bark, sap and heart, roots, trunk, and branch." This rata is almost the toughest wood known, and is used in many places for the cogs of wheels, &c. Besides these there are many others, especially the makia, which when thoroughly dry would turn or break the edge of the best axe ever produced in Sheffield, which are now only cut down for firewood as occasion requires.

*THE GEOGNOSY OF THE APPALACHIANS AND THE ORIGIN OF CRYSTALLINE ROCKS**

IN coming before you this evening my first duty is to announce the death of Prof. William Chauvenet. This sad event was not unexpected, since, at the time of his election to the presidency of the Association, at the close of our meeting at Salem in August 1869, it was already feared that failing health would prevent him from meeting with us at Troy, in 1870. This, as you are aware, was the case, and I was therefore called to preside over the Association in his stead. In the autumn of 1869, he was compelled by illness to resign his position of Chancellor of the Washington University of St. Louis, and in December last died at the age of fifty years, leaving behind him a record to which Science and his country may point with just pride. During his connection of fourteen years with the Naval Academy at Annapolis he was the chief instrument in building up that institution, which he left in 1859 to take the chair of Astronomy and Mathematics at St. Louis, where his remarkable qualities led to his selection, in 1862, for the post of Chancellor of the University, which he filled with great credit and usefulness up to the time of his resignation. It is not for me to pronounce the eulogy of Prof. Chauvenet, to speak of his profound attainments in astronomy and mathematics, or of his published works, which have already taken rank as classics in the literature of these sciences. Others more familiar with his field of labour may in proper time and place attempt the task. All who knew him can however join with me in testifying to his excellences as a man, an instructor, and a friend. In his assiduous devotion to scientific studies he did not neglect the more elegant arts, but was a skilful musician, and possessed of great general culture and refinement of taste. In his social and moral relations he was marked by rare elevation and purity of character, and has left to the world a standard of excellence in every relation of life which few can hope to attain.

In accordance with our custom it becomes my duty in quitting the honourable position of President, which I have filled for the past year, to address you upon some theme which shall be germane to the objects of the Association. The presiding officer, as you are aware, is generally chosen to represent alternately one of the two great sections into which the members of the Association are supposed to be divided, viz., the students of the natural-history sciences on the one hand, and of the physico-mathematical and chemical sciences on the other. The arrangement by which, in our organisation, geology is classed with the natural history division, is based upon what may fairly be challenged as a somewhat narrow conception of its scope and aims. While theoretical geology investigates the astronomical, physical, chemical, and biological laws which have presided over the development of our earth, and while practical geology or geognosy studies its natural history, as exhibited in its physical structure, its mineralogy and its palæontology, it will be seen that this comprehensive science is a stranger to none of the studies which are included in the plan of our Association, but rather sits like a sovereign, commanding in turn the services of all.

As a student of geology, I scarcely know with which section of the Association I should to-day identify myself. Let me endeavour rather to mediate between the two, and show you somewhat of the two-fold aspect which geological science presents, when viewed respectively from the stand-points of natural history and of chemistry. I can hardly do this better than in the discussion of a subject which for the last generation has afforded some of the most fascinating and perplexing problems for our geological students; viz., the history of the great Appalachian mountain chain. Nowhere else in the world has a mountain system of such geographical extent and such geological complexity been studied by such a number of zealous and learned investigators, and no other, it may be confidently asserted, has furnished such vast and important results to geological science. The laws of mountain structure, as revealed in the Appalachians by the labours of the brothers Henry D. and William B. Rogers, of Lesley and of Hall, have given to the world the basis of a correct system of orographic geology,† and many of the obscure geological problems of Europe become plain when read in the light of our American experience. To discuss even in the most summary manner all of the questions which the theme suggests, would be a task too long for the present occasion, but I shall endeavour to-night in the first place to bring before you certain facts in the history of the physical structure, the mineralogy and the palæontology of the Appalachians; and in the second place to discuss some of the physical, chemical, and biological conditions which have presided over the formation of the ancient crystalline rocks that make up so large a portion of our great eastern mountain system.

I. THE GEOGNOSY OF THE APPALACHIAN SYSTEM.—The age and geological relations of the crystalline stratified rocks of eastern North America have for a long time occupied the attention of geologists. A section across northern New York, from Ogdensburg on the St. Lawrence to Portland in Maine, shows the existence of three distinct regions of unlike crystalline schists. These are the Adirondacks to the west of Lake Champlain, the Green Mountains of Vermont, and the White Mountains of New Hampshire. The lithological and mineralogical differences between the rocks of these three regions are such as to have attracted the attention of some of the earlier observers. Eaton, one of the founders of American geology, at least as early as 1832, distinguished in his Geological Text-book (2nd edition) between the gneiss of the Adirondacks and that of the Green Mountains. Adopting the then received divisions of primary, transition, secondary and tertiary rocks, he divided each of these series into three classes, which he named carboniferous, quartzose, and calcareous; meaning by the first schistose or argillaceous strata such as, according to him, might include carbonaceous matter. These three divisions in fact corresponded to clay, sand, and lime-rocks, and were supposed by him to be repeated in the same order in each series. This was apparently the first recognition of that law of cycles in sedimentation upon which I afterwards insisted in 1863.* Without, so far as I am aware, defining the relations of the Adirondacks, he referred to the lowest or carboniferous division of the primary series the crystalline schists of the Green Mountains, while the quartzites and marbles at their western base were made the quartzose and calcareous divisions of this primary series. The argillites and sandstones lying still farther westward, but to the east of the Hudson River, were regarded as the first and second divisions of the transition series, and were followed by its calcareous division, which seems to have included the limestones of the Trenton group; all of these rocks being supposed to dip to the westward, and away from the central axis of the Green Mountains. Eaton does not appear to have studied the White Mountains, or to have considered their geological relations. They were, however, clearly distinguished from the former by C. T. Jackson in 1844, when, in his report on the geology of New Hampshire, he described the White Mountains as an axis of primary granite, gneiss, and mica-schist, overlaid successively, both to the east and west, by what were designated by him Cambrian and Silurian rocks; these names having, since the time of Eaton's publication, been introduced by English geologists. While these overlying rocks in Maine were unaltered, he conceived that the corresponding strata in Vermont, on the western side of the granitic axis, had been changed by the action of intrusive serpentines and intrusive quartzites, which had altered the Cambrian into the Green Mountain gneiss, and converted a portion of the fossiliferous Silurian limestones of the Champlain valley into white marbles.† Jackson did not institute any comparison between the rocks of the White Mountains and these of the Adirondacks; but the Messrs. Rogers in the same year, 1844, published an essay on the geological age of the White Mountains, in which, while endeavouring to show their Upper Silurian age, they speak of them as having been hitherto regarded as consisting exclusively of various modifications of granitic and gneissoid rocks, and as belonging "to the so-called primary periods of geologic time."‡ They however considered that these rocks had rather the aspect of altered palæozoic strata, and suggested that they might be, in part at least, of the age of the Clinton division of the New York system; a view which was supported by the presence of what were at the time regarded by the Messrs. Rogers as organic remains. Subsequently, in 1847,§ they announced that they no longer considered these to be of organic origin, without however retracting their opinion as to the palæozoic age of the strata. Reserving to another place in my address the discussion of the geological age of the White Mountain rocks, I proceed to notice briefly the

* Address of Prof. T. Sterry Hunt on retiring from the office of President of the American Association for the Advancement of Science; abridged from the "American Naturalist."
† Amer. Jour. Sci., II. xxx. 406.

* Amer. Jour. Sci., II. xxxv. 166.
† Geology of New Hampshire, 160-162.
‡ Amer. Jour. Sci., II. i. 411.
§ Ibid, II. v. 116.

distinctive characters of the three groups of crystalline strata just mentioned, which will be shown in the sequel to have an importance in geology beyond the limits of the Appalachians.

1. *The Adirondack or Laurentide Series*—The rocks of this series, to which the name of the Laurentian system has been given, may be described as chiefly firm granitic gneisses, often very coarse-grained, and generally reddish or grayish in colour. They are frequently hornblendic, but seldom or never contain much mica, and the mica-schist (often accompanied with staurolite, garnet, andalusite, and cyanite), so often characteristic of the White Mountain series, are wanting among the Laurentian rocks. They are also destitute of argillites, which are found in the other two series. The quartzites, and the pyroxenic and hornblendic rocks, associated with great formations of crystalline limestone, with graphite, and immense beds of magnetic iron ore, give a peculiar character to portions of the Laurentian system.

2. *The Green Mountain Series.*—The quartzo-feldspathic rocks of this series are to a considerable extent represented by a fine-grained petrosilex or eurite, though they often assume the form of a true gneiss, which is ordinarily more micaceous than the typical Laurentian gneiss. The coarse-grained, porphyritic, reddish varieties common to the latter are wanting to the Green Mountains, where the gneiss is generally of pale greenish and grayish hues. Massive stratified diorites, and epidotic and chloritic rocks, often more or less schistose, with steatite, dark-coloured serpentines and ferriferous dolomites and magnesites, also characterise this gneissic series, and are intimately associated with beds of iron ore, generally a slaty hematite, but occasionally magnetite. Chrome, titanium, nickel, copper, antimony, and gold are frequently met with in this series. The gneisses often pass into schistose micaceous quartzites, and the argillites, which abound, frequently assume a soft, unctuous character, which has acquired for them the name of talcose or nacreous slates, though analysis shows them not to be magnesian, but to consist essentially of a hydrous micaceous mineral. They are sometimes black and graphitic.

3. *The White Mountain Series.*—This series is characterised by the predominance of well-defined mica-schists, interstratified with micaceous gneisses. These latter are ordinarily light-coloured from the presence of white feldspar, and though generally fine in texture, are sometimes coarse-grained and porphyritic. They are less strong and coherent than the gneisses of the Laurentian, and pass, through the predominance of mica, into mica-schists, which are themselves more or less tender and friable, and present every variety, from a coarse gneiss-like aggregate down to a fine-grained schist, which passes into argillite. The micaceous schists of this series are generally much richer in mica than those of the preceding series, and often contain a large proportion of well-defined crystalline tables belonging to the species muscovite. The cleavage of these micaceous schists is generally, if not always, coincident with the bedding, but the plates of mica in the coarser-grained varieties are often arranged at various angles to the cleavage and bedding-plane, showing that they were developed after sedimentation, by crystallisation in the mass, a circumstance which distinguishes them from rocks derived from the ruins of these, which are met with in more recent series. The White Mountain rocks also include beds of micaceous quartzite. The basic silicates in this series are represented chiefly by dark-coloured gneisses and schists, in which hornblende takes the place of mica. These pass occasionally into beds of dark hornblende-rock, sometimes holding garnets. Beds of crystalline limestone occasionally occur in the schists of the White Mountain series, and are sometimes accompanied by pyroxene, garnet, idocrase, sphene, and graphite, as in the corresponding rocks of the Laurentian, which this series, in its more gneissic portions, closely resembles, though apparently distinct geognostically. The limestones are intimately associated with the highly micaceous schists, containing staurolite, andalusite, cyanite, and garnet. These schists are sometimes highly plumbaginous, as seen in the graphitic mica-schist holding garnets in Nelson, New Hampshire, and that associated with cyanite in Cornwall, Conn. To this third series of crystalline schists belong the concretionary granitic veins abounding in beryl, tourmaline, and lepidolite, and occasionally containing tinstone and columbite. Granitic veins in the Laurentian gneisses frequently contain tourmaline, but have not, so far as is yet known, yielded the other mineral species just mentioned.*

II. THE ORIGIN OF CRYSTALLINE ROCKS.—We now approach the second part of our subject, namely, the genesis of the crystalline schists. The origin of the mineral silicates, which make up a great portion of the crystalline rocks of the earth's surface, is a question of much geological interest, which has been to a great degree overlooked. The gneisses, mica-schists, and argillites, of various geological periods do not differ very greatly in chemical constitution from modern mechanical sediments, and are now very generally regarded as resulting from a molecular re arrangement of similar sediments formed in earlier times by the disintegration of previously existing rocks not very unlike them in composition; the oldest known formations being still composed of crystalline stratified deposits presumed to be of sedimentary origin. Before these the imagination conceives yet earlier rocks, until we reach the surface of unstratified material, which the globe may be supposed to have presented before water had begun its work. It is not, however, my present plan to consider this far off beginning of sedimentary rocks, which I have elsewhere discussed.*

Apart from the clay and sand-rocks just referred to, whose composition may be said to be essentially quartz and aluminous silicates, chiefly in the forms of feldspars and micas, or the results of their partial decomposition and disintegration, there is another class of crystalline silicated rocks which, though far less important in bulk than the last, is of great and varied interest to the lithologist, the mineralogist, the geologist, and the chemist. The rocks of this second class may be defined as consisting in great part of the silicates of the protoxyd bases, lime, magnesia, and ferrous oxyd, either alone, or in combination with silicates of alumina and alkalies. They include the following as their chief constituent mineral species:—Pyroxene, hornblende, olivine, serpentime, talc, chlorite, epidote, garnet, and triclinic feldspars, such as labradorite. The great types of this second class are not less well defined than the first, and consist of pyroxenic and hornblendic rocks, passing into diorites, diabases, ophiolites and talcose, chloritic and epidotic rocks. Intermediate varieties resulting from the association of the minerals of this class with those of the first, and also with the materials of non-silicated rocks, such as limestones and dolomites, show an occasional blending of the conditions under which these various types of rocks were formed.

The distinctions just drawn between the two great divisions of silicated rocks are not confined to stratified deposits, but are equally well marked in eruptive and unstratified masses, among which the first type is represented by trachytes and granites, and the second by dolerites and diorites. This fundamental difference between acid and basic rocks, as the two classes are called, finds its expression in the theories of Phillips, Durocher, and Bunsen, who have deduced all silicated rocks from two supposed layers of molten matter within the earth's crust, consisting respectively of acid and basic mixtures; the trachytic and pyroxenic magmas of Bunsen. From these, by a process of partial crystallisation and eliquation, or by commingling in various proportions, those eruptive rocks which depart more or less from the normal types are supposed by the theorists of this school to be generated.† The doctrine that these eruptive rocks are not derived directly from a hitherto uncongealed nucleus, but are softened and crystallised sediments, in fact that the whole of the rocks at present known to us have at one time been aqueous deposits, has, however, found its advocates. In support of this view, I have endeavoured to show that the natural result of forces constantly in operation tends to resolve the various igneous rocks into two classes of sediments, in which the two types are, to a great extent, preserved. The mechanical and chemical agencies which transform the crystalline rocks into sediments, separate these more or less completely into coarse, sandy, permeable beds on the one hand, and fine clayey impervious muds on the other. The action of infiltrating atmospheric waters on the first and more silicious strata, removes from them lime, magnesia, iron-oxyd, and soda, leaving behind silica, alumina, and potash—the elements of granitic, gneissic, and trachytic rocks. The finer and more aluminous sediments, including the ruins of the soft and easily abraded silicates of the pyroxene group, resisting the penetration of the water, will, on the contrary, retain their alkalies, lime, magnesia, and iron, and thus will have the composition of the more basic rocks.‡

A little consideration will, however, show that this process, although doubtless a true cause of differences in the composition of

* Hunt, Notes on Granitic Rocks; Amer. Jour. Sci. III. i. 182.

* Amer. Jour. Sci., II. l. 25.
† Hunt on Some Points of Chemical Geology, Quar. Jour. Geol. Soc., XV. 489.
‡ Quar. Jour. Geol. Soc., XV. 489; also, Amer. Jour. Sci., II. xxx. 133.

sedimentary rocks, is not the only one, and is inadequate to explain the production of many of the varieties of stratified silicated rocks, such are serpentine, steatite, hornblende, diallage, chlorite, pinite, and labradorite, all of which mineral species form rock masses by themselves, frequently almost without admixture. No geological student will now question that all of these rocks occur as members of stratified formations. Moreover, the manner in which serpentines are found interstratified with steatite, chlorite, argillite, diorite, hornblende, and feldspar rocks, and these, in their turn, with quartzites and orthoclase rocks, is such as to forbid the notion that these various materials have been deposited, with their present composition, as mechanical sediments from the ruins of pre-existing rocks; a hypothesis as untenable as that ancient one which supposed them to be the direct results of plutonic action.

There are, however, two other hypotheses which have been proposed to explain the origin of these various silicated rocks, and especially of the less abundant, and, as it were, exceptional species just mentioned. The first of these supposes that the minerals of which they are composed have resulted from an alteration of previously existing minerals, often very unlike in composition to the present, by the taking away of certain elements and the addition of certain others. This is the theory of metamorphism by pseudomorphic changes, as they are called, and is the one taught by the now reigning school of chemical geologists, of which the learned and laborious Bischof, whose recent death science deplores, may be regarded as the great exponent. The second hypothesis supposes that the elements of these various rocks were originally deposited as, for the most part, chemically formed sediments, or precipitates; and that the subsequent changes have been simply molecular, or, at most, confined in certain cases to reactions between the mingled elements of the sediments, with the elimination of water and carbonic acid. It is proposed to consider briefly these two opposite theories, which seek to explain the origin of the rocks in question respectively by pseudomorphic changes in pre-existing crystalline rocks, and by the crystallisation of aqueous sediments, for the most part chemically-formed precipitates.

Mineral pseudomorphism, that is to say, the assumption by one mineral substance of the crystalline form of another, may arise in several ways. First of these is the filling up of a mould left by the solution or decomposition of an imbedded crystal, a process which sometimes takes place in mineral veins, where the processes of solution and decomposition can be freely carried on. Allied to this, is the mineralisation of organic remains, where carbonate of lime or silica, for example, fills the pores of wood. When subsequent decay removes the woody tissue, the vacant spaces may, in their turn, be filled by the same or another species.* In the second place, we may consider pseudomorphs from alteration, which are the result of a gradual change in the composition of a mineral species. This process is exemplified in the conversion of feldspar into kaolin by the loss of its alkali and a portion of silica, and the fixation of water, or in the change of chalybite into limonite by the loss of carbonic acid and the absorption of water and oxygen.

The doctrine of pseudomorphism by alteration as taught by Gustav Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others, leads them, however, to admit still greater and more remarkable changes than these, and to maintain the possibility of converting almost any silicate into any other. Thus, by referring to the pages of Bischof's *Lehrbuch der Geognosie*, it will be found that serpentine is said to exist as a pseudomorph after augite, hornblende, olivine, chondodrite, garnet, mica, and probably also after labradorite, and even orthoclase. Serpentine rock or ophiolite is supposed to have resulted, in different cases, from the alteration of hornblende-rock, diorite, granulite, and even granite. Not only silicates of protoxyds and aluminous silicates are conceived to be capable of this transformation, but probably also quartz itself; at least, Blum asserts that meerschaum, a closely related silicate of magnesia, which sometimes accompanies serpentine, results from the alteration of flint, while, according to Rose, serpentine may even be produced from dolomite, which we are told is itself produced by the alteration of limestone. But this is not all—feldspar may replace carbonate of lime, and carbonate of lime feldspar, so that, according to Volger, some gneissoid limestones are probably formed from gneiss by the substitution of calcite for orthoclase. In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine, or more directly from granite, granulite, or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite, till we are ready to exclaim in the words of Goethe:—

"Mich ängstiet das Verfängliche
Im widrigen Geschwätz,
Wo Nichts verharret, Alles flieht,
Wo schon verschwunden was man sieht,"

which we may thus translate:—"I am vexed with the sophistry in their contrary jargon, where nothing endures, but all is fugitive, and where what we see has already passed away."

By far the greater number of cases on which this general theory of pseudomorphism by a slow process of alteration in minerals, has been based, are, as I shall endeavour to show, examples of the phenomenon of mineral envelopment, so well studied by Delesse in his essay on pseudomorphs,* and may be considered under two heads:—first, that of symmetrical envelopment, in which one mineral species is so enclosed within the other that the two appear to form a single crystalline individual. Examples of this are seen when prisms of cyanite are surrounded by staurolite, or staurolite crystals completely enveloped in those of cyanite, the vertical axes of the two prisms corresponding. Similar cases are seen in the enclosure of a prism of red in an envelope of green tourmaline, of allanite in epidote, and of various minerals of the pyroxene group in one another. The occurrence of muscovite in lepidolite, and of margarodite in lepidomalene, or the inverse, are well-known examples, and, according to Scheerer, the crystallisation of serpentine around a nucleus of olivine is a similar case. This phenomenon of symmetrical envelopment, as remarked by Delesse, shows itself with species which are generally isomorphous or homœomorphous, and of related chemical composition. Allied to this is the repeated alternation of crystalline laminæ of related species, as in perthite, the crystalline cleavable masses of which consist of thin alternating layers of orthoclase and albite.

Very unlike to the above are those cases of envelopment in which no relations of crystalline symmetry nor of similar chemical constitution can be traced. Examples of this kind are seen in garnet crystals, the walls of which are shells, sometimes no thicker than paper, enclosing in different cases crystalline carbonate of lime, epidote, chlorite, or quartz. In like manner, crystalline shells of leucite enclose feldspar, hollow prisms of tourmaline are filled with crystals of mica or with hydrous peroxyd of iron, and crystals of beryl with a granular mixture of orthoclase and quartz, holding small crystals of garnet and tourmaline, a composition identical with the enclosing granitic veinstone.† Similar shells of galenite and of zircon, having the external forms of these species, are also found filled with calcite. In many of these cases the process seems to have been first the formation of a hollow mould or skeleton-crystal (a phenomenon sometimes observed in salts crystallising from solutions), the cavity being sometimes filled with other matters. Such a process is conceivable in free crystals found in veins, as for example, galenite, zircon, tourmaline, beryl, and some examples of garnet, but is not so intelligible in the case of those garnets imbedded in mica-schist, studied by Delesse, which enclosed within their crystalline shells irregular masses of white quartz, with some little admixture of garnet. Delesse conceives these and similar cases to be produced by a process analogous to that seen in the crystallisation of calcite in the Fontainebleau sandstone; where the quartz grains, mechanically enclosed in well-defined rhombohedral crystals, equal, according to him, sixty-five per cent. of the mass. Very similar to these are the crystalloids with the form of orthoclase, which sometimes consist in large part of a granular mixture of quartz, mica, and orthoclase, with a little cassiterite, and in other cases, contain two thirds their weight of the latter mineral, with an admixture of orthoclase and quartz. Crystals with the form of scapolite, but made up, in a great part, of mica, seem to be like cases of envelopment, in which a small proportion of one substance in the act of crystallisation, compels into its own crystalline form a large portion of some foreign material, which may even so mask the crystallising element that this becomes overlooked, as of secondary importance. The substance which, under the name of houghite, has been described as an altered spinel, is found by analysis to be the mixture of völlknerite with a variable proportion of spinel, which in some specimens, does not exceed eight per cent., but to which, nevertheless, these crystalloids appear to owe their more or less complete octohedral form.‡

(To be continued.)

* Hunt on the Silification of Fossils, Canadian Naturalist, N. S., I. 46.

* Annales des Mines, V. xvi. 317-392.
† Report Geol. Survey of Canada, 1866, p. 189.
‡ Rpt. Geol. Sur. of Canada, 1866, pp. 189, 213. Amer Jour. Sci., III. i. 188.

INSTRUCTIONS FOR OBSERVERS, AT THE ENGLISH GOVERNMENT ECLIPSE EXPEDITION, 1871

II.—Polariscopic Observations

THE chief points to which observers of polarisation should direct their attention appear to be :—

A. What is the nature of the outlying corona?

B. Can the radial polarisation of the circumsolar corona be traced down to the photosphere, or, if not, how low?

C. Is secondary atmospheric polarisation traceab'e? and if so, does the plane change during totality?

A. We might suppose this to be due—
(1) to circumsolar matter (though at a great distance from the sun) reflecting light,
(2) to circumsolar matter in the state of self-luminous gas,
(3) to circumlunar matter diffracting and, to a certain extent, reflecting light (most improbable),!
(4) to lofty atmospheric haze or cloud, of excessive tenuity, diffracting light.

The light ought to be, for
(1) strongly and radially polarised,
(2) unpolarised,
(3 and 4) insensibly or all but insensibly polarised.

Hence polarisation observations would only serve to discriminate between (1) on the one hand, and (2), (3), or (4) on the other.

From the faintness of the object and its considerable extent, the naked eye, armed with a polariscope, might be best. If a telescope be used, it should be of quite low power, and the aperture as large as the breadth of the pupil multiplied by the magnifying-power.

Suppose the polariscope be Savart's, the quartz plates being thick enough (if the naked eye be used) to give bands as narrow as, say, 20′ diameter.

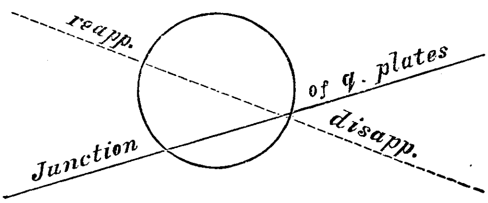

Fig. 1.

Let the observer rotate the polariscope till the bands, if any, seen on the dark moon disappear; then, *without rotating the instrument round its axis*, let him *incline* the axis so as to point at the outlying corona in different directions round the sun, and notice whether the bands spring into existence; and if so, let him sweep *round* the sun, noticing what lies *outside* the clearly circumsolar corona of 5′ or so height, and let him notice particularly by estimation the direction, relatively to the bands, of the radius vector of the region where they are most vivid, or, better, the azimuth of both radius and bands. He should also specify, *provided he can do so with certainty*, whether the bands were black-centred or white-centred. He should also state in his account, and verify the statement by an observation made at leisure before or after totality, whether his Savart is constructed (or set) so as to have the hands *parallel or perpendicular* to the principal plane of the Nicol.

A very useful adjunct to a Savart's polariscope would be a glass reflector, or else a tourmaline, placed so as to cover a small segment of the field of view near the edge. On account of the possible difficulty of illuminating the reflector in the peculiar circumstances of a total eclipse, a tourmaline would seem to be preferable. It should be placed for the naked eye at the least distance of distinct vision—for a telescope, in or in front of the eye-piece, where a real image is formed so as to be seen distinctly —the axis of the tourmaline being parallel to the edge or chord of the segment, and the bands being set perpendicular to this chord. In the event of rotation during the observation, the whole should be rotated together. The question whether the bands are bright-centred or dark-centred, which, in the case of slight polarisation, is difficult to decide, would thus be replaced by the simpler question, whether the bands in the field were of the same character as in the segment (*i.e.*, bright being a prolongation of bright, and dark of dark) or of opposite character.

The observer should previously have practised on the blue sky, rotating his Savart till the bands disappear, and noticing to what degree they are brought back by small changes of pointing without rotation, so as to be prepared for what he is liable to from secondary atmospheric polarisation during totality.

Should only very feeble bands be seen in the outer corona, such as might possibly be attributable to atmospheric polarisation operating through small changes of pointing, it would be well for control to rotate the instrument *a little* till bands are fairly visible on the disc of the moon, and notice whether on passing to the outer corona, *in whatever direction*, the bands, instead of being reinforced, tend rather to be drowned in white light. Should luminous beams or dark rifts be seen in the outer corona, so as to exhibit contrast of light and shade in close proximity, a good opportunity will be afforded of testing whether the light of the outer corona is polarised or not. If it be polarised, then on rotating the Savart, so as to make the bands cut at various indications the boundary of light and shade, the bands will in certain azimuths of the Savart be stronger on the luminous than on the dark side of the edge of the beam or rift. If it be unpolarised, then, whatever be the azimuth of the Savart, the bands will be rather drowned in white light than reinforced on passing from the dark to the luminous side of the edge.

But Savart's and other colour-polariscopes, which are admirable for detecting a slight polarisation in light which is not particularly feeble, break down when the difficulty arises from the feebleness of the light rather than the slightness of the polarisation. In such cases a simple double-image prism, with a diaphragm-tube, is better. Unless those who have seen total eclipses can decide from trial (suppose on the clear sky after sunset, or at night when illuminated by the moon), combined with

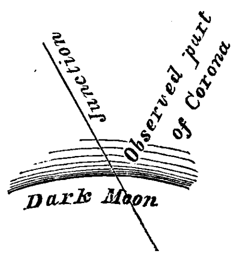

Fig. 2.

their memory of the degree of illumination of the outer corona, it might be well that the observer should be provided with and should try both instruments.

B. For this a telescope will be required with a magnifying power of, say, 16 or 20. A biquartz seems the best instrument, placed at the common focus of the eye-piece (which should be positive) and objective, and combined with a Nicol's prism, or, if it can be procured, a thoroughly good tourmaline. A tourmaline might be placed over the eye-hole, whereas a Nicol might have to be placed in the body of the eye-piece, which, however, is no particular disadvantage if properly done.

Let it be ascertained by previous trial how much a Nicol must be turned from the position in which the two halves are purple alike to make the tints contrast more vividly. Say it is 30°. Suppose the observer on the line of central shadow, so that the limits of disappearance and reappearance will be on opposite ends of a diameter. The biquartz and Nicol have been relatively set so that the line of junction is in the plane of polarisa ion of light extinguished by the Nicol, turn them together before totality 30° (or whatever other angle may have been fixed on) to either side of the diameter of disappearance, and, pointing the telescope to the place of disappearance (Fig. 1), await totality without dazzling the eye. The moment the sun is covered, apply the eye to the telescope, and notice whether there is a vivid contrast of colour right and left of the line of junction of the quartz plates *all the way down to the dark moon* (Fig. 2), or only in the higher parts of the circumsolar corona.

Be ready to repeat the observation before reappearance, with the telescope pointed to the place of reappearance; and meanwhile, if time permits, repeat Prazmouski's observation by pointing the telescope, without rotation of the analyser, so that the line of junction bisects the moon, and noticing whether the semicircles of the corona are purple alike where they abut on the junction, and what is the order of colours in the semicircle on receding from the junction. A record as to which is which of the two halves of the biquartz should be carefully preserved.

Should secondary atmospheric polarisation be so strong as to throw doubt on the results (which may be judged of by noticing the light on the dark moon), it would be well to rotate the analyser till the two halves seen on the dark moon are purple alike, and then alter the pointing of the telescope, and repeat Prazmouski's observation.

It will be observed that the same general principles apply to the elimination of atmospheric polarisation, whether the polariscope employed be a Savart's polariscope, a polariscope with quartz wedges, or a biquartz polariscope.

C. This is of little intrinsic interest, its chief use being to clear up possible doubts as to the results obtained by the observers of A and B. Should there be an observer not otherwise employed, he might be deputed to observe the direction of the Savart's bands on disappearance, both on the dark moon and the surrounding sky, and whether this direction changes during totality. Also it should be specified in which pair of opposite quadrants they were black-centred and in which white-centred. Should this be found impossible or uncertain (the instrument being unprovided with the adjunct mentioned above), the Savart might be used as a simple Nicol by turning it end for end, so that the quartz plates are next the eye; and with this the plane of polarisation might be roughly determined by means of the azimuth of the principal plane of the Nicol when the light most nearly disappears.

Should registration of the azimuth be attempted, the Savart would be fixed so as not to be reversible. In that case the observer might be provided with a double-image prism and diaphragm-tube for separate use in case of need.

Stoppage of stray light in a telescope designed for polarisation

The want of this appears to have occasioned some difficulty at the last eclipse.

The simplest way is by a stop, with a hole just large enough to contain the image of the object-glass. Such exists in the erecting eye-piece, where an image of the object-glass is formed in the body of the eye-piece. It exists too, in a Gregorian or Cassegrainian telescope, where the stoppage is imperative. But in an ordinary refracting telescope, with an inverting eye-piece, the eye-hole (from certain motives of convenience) is larger than in front of (*i. e.* nearer the object-glass than) the bright circle, or image of the object-glass; and unless the tube is sufficiently provided with stops, when a faint object near a bright one is looked at, light from the bright, reflected from the inside of the tube, is liable to enter the field of view. Large instruments are provided with stops; but I fancy smaller instruments are sometimes turned out without them. This should be looked to.

The observer may test the correctness of stopping by taking out the eye-piece, inserting a paper disc with a central hole of the size of the field-glass, turning the instrument nearly but not quite to a bright object, as well as to points more distant from the bright object, and noticing whether the side of the tube, even when viewed in a direction grazing the edge of the hole, is properly dark, so that only the edges of the stops are seen.* On the other hand, the stops should not obstruct a clear view of the object-glass as seen through the hole representing the field-glass, or they will render the outer portions of the object-glass useless.

General Remarks

I consider the observation recommended by Mr. Ranyard (see NATURE, Aug. 24, 1871), very important, IF, after what Prazmouski and Ranyard have done, the point be still deemed doubtful. Prazmouski's observation seems to have been beautifully devised and executed, but carelessly described. It is only by conjecture that I can make sense and harmony with what is known, out of his observations as described by himself. But I think that Mr. Ranyard has at least shown that our conjectural interpretation of Prazmouski's observation is the right one; and if so, the point seems settled.

It is for this reason that, in lieu of No. 3, first half, I proposed something new. What becomes of the magnesium, &c., which the spectroscope reveals low down in the gigantic puffs which the sun emits? The hydrogen must surely carry the magnesium, &c., with it to the higher regions, though the magnesium, &c., would soon be condensed, and so would not be detected by the spectroscope. These substances would exist in the form of an exceedingly fine haze or dust. I use the two words, "haze" to denote a filmy cloud of molten "dust" of solid matter. This haze or dust is capable of detection, and, according to my interpretation, *has* been detected, by polarisation; and it is interesting to know how low down it can be detected. Mr. Stoney's speculations as to layers are utterly inapplicable here, as they imply a state of tranquillity quite unlike what we now know to exist, at any rate in connexion with the puffs.

I don't know why, in the second half of No. 3, Mr. Ranyard prescribes placing the line of junction *across* a sector or rift, *if* by that he means *turning* the eye-piece carrying the quartz plates so that the line is perpendicular with the corona to the sector. It would be more likely to yield results if it cut it obliquely, as represented for the corona in Fig. 2. But probably he only means pointing the telescope so that the junction cuts the rift. If the observer notices contrasting colours, he may *then* proceed to determine the plane of polarisation. G. G. S.

* If reflection occurs from the part of the tube so near the eye as not to appear *within* the field, it will not signify much.

SCIENTIFIC SERIALS

THE *Journal of the Quekett Microscopical Club.* No. 16. October 1871. "Microscopic Work and Conjectural Science," being the address of the President (Lionel S. Beale, M.B., F.R.S.), for the year 1871. This address is chiefly occupied in combating the method, presumed to have been adopted, of depreciating one kind of scientific investigation in order to elevate another, and attacks without ceremony those who would elevate physical science to the disparagement of microscopical observation.—"On the Examination of the Surface Markings of Diatoms by the Oxy-calcium Light," by N. E. Green. The writer of this paper details his examination of such diatoms as Isthmia, Biddulphia, Triceratium, Pleurosigma, &c., as opaque objects by high powers, as one-sixth Ross and one-twelfth Gundlach, through the agency of the oxy-calcium light. The conclusion at which he has arrived is, that the markings on all the above, except Pleurosigma, resemble "craters," the surface "being studded with rows of small shallow craters, the sharp edges of which projected slightly above, while the centres seemed to be below the surface." In Pleurosigma a different structure of the surface was observed. "The lime light brought out most distinctly the bead-like character of its markings; they stood out in bold relief like rows of Indian corn."—The Inaugural Address of the South London Microscopical and Natural History Club, by R. Braithwaite, M.D., F.L.S., is principally devoted to suggestions on the vast field for observation at the disposal of the microscopist.—"On Nucleated Sporidia," by M. C. Cooke, M.A. After describing the general structure which prevails in the genus Peziza of Ascomycetous Fungi, the writer details his method of mounting sections for the microscope in pure glycerine. The nucleated sporidia, so prevalent in this genus, are affirmed to be so affected by this method that in a short time all traces of the nuclei are lost, and the object of the paper is to indicate the doubtful value of nucleated sporidia in specific characters. The true nature of such nuclei and their uses are said to be obscure.

IN the *Revue Scientifique*, Nos. 13—18, are many valuable articles. Further reports are given of the proceedings of the Edinburgh meeting of the British Association, and a translation of Prof. T. Sterry Hunt's address to the Indianopolis meeting of the American Association. We have also a memoir of M. Lartet by M. G. de Mortillet; Helmholtz's paper on the rapidity of propagation of electro-dynamical actions; report of M. Chauveau's lectures on the physiology of virulent maladies; a lecture by M. Claude Bernard on the method and principle of physiology; a translation of P. Secchi's paper on the solar protuberances from the *Atti dell' Academia ponitificia de nuovi Lincei*; a biographical sketch of Haidinger by M. Fouqué; reports of the proceedings of the various scientific institutions in France and Belgium; and translations of lectures delivered at the Royal Institution, University of Edinburgh, &c., by Prof. Tyndall, Dr. Carpenter, Dr. Laycock, and others.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, October 23.—The greater part of the communications read at this meeting were devoted to chemical subjects. Of mathematical papers only one was presented—namely, a continuation of M. Chasles' memoir on the determination of a series of groups of a certain number of points on a geometrical curve.—A note was read by M. J. Bertrand on the mathematical theory of dynamical electricity, and a memoir by M. E. Mathieu on the integration of equations to the partial differences of mathematical physics.—M. du Moncel presented some observations relating to a recent communication by M. Ruhmkorff upon some experiments in magneto-electric induction, in which he claimed to have already ascertained and published facts analogous to those of the German author—M. P. A. Favre read a continuation of his thermic researches upon the electrolysis of the hydracids.—A fifth letter from Father Secchi on the various aspects of the protuberances and other remarkable parts of the surface of the sun was read, in which he describes the results of simultaneous observations made by himself at Rome, and by M. Tacchini at Palermo.—M. Secchi also presented a note on a new method of observing the eclipses and passages of Venus, by means of a spectroscopic apparatus modified by having at a distance of about 20 centimetres in front of the spectroscope, an additional prism having its refringent angle parallel to the fissure.—The chemical papers were as follows:—a theory of simple reactions limited by inverse action, and an application of the same to the transformations of phosphorus, by M. J. Lemoine.—Researches in chemical statics, by M. Stas, containing a discussion of the phenomena which occur in the precipitation of dilute solutions of salts of silver by hydrochloric, hydrobromic, and hydriodic acids, and by chlorides, bromides, and iodides. This paper contains some results of great importance in the analysis of bodies containing silver.—The conclusion of the second part of M. Berthelot's investigation of the ammoniacal salts.—A note on the transformation of glucoses into monatomic and hexatomic alcohols, by M. G. Bouchardat, communicated by M. A. Wurtz. The author acts upon the glucoses by means of an amalgam of sodium. He describes its action upon glucose and sugar of milk.—A note on the hexabromide and hexachloride of silicium, by M. C. Friedel, also presented by M. A. Wurtz; and a note on the method of determining the gases evolved by an explosion of nitroglycerine, by M. L. L'Hote, presented by General Morin. From the researches of the last-mentioned author it appears that 1 gramme of nitroglycerine produces 284 cub. centim. of gas, containing by volume 45·72 of carbonic acid, 20·36 of binoxide of nitrogen, and 33·92 of nitrogen.—M. Elie de Beaumont called attention to some specimens of native phosphate of lime from Caylux and Cajare, and noticed the importance of these deposits for agricultural purposes. M. Combes also remarked upon this subject.—M. Chapelas presented a note on a remarkable meteor observed during the night of the 19th October.

PHILADELPHIA

Academy of Natural Sciences, May 9.—The President, Dr. Ruschenberger, in the chair.—Prof. Cope demonstrated some anatomical points of importance in the classification of some of the Siluroids of the Amazon, noticing first those which have no swimming-bladder, but having the post-temporal bone pierced in a sieve-like manner, forming minute tympana; these he characterised as *Otocinclus*. Others having huge swim-bladders, gun-boat style of shape. No adipose fin; the back naked. No lyre plate, indicated as *Zathorax*. A third, body protected by bony shields above. No adipose fin; the scapular arch dermoössified and lyre-shaped below; indicated as *Physopyxis lyra*. A fourth, shielded all over its sides, with the under lip turned back, genus *Corydoras*. A fifth, where the under lip is separated, except at the ends, forming loops, named *Brochis*. In the sixth, where the lips are separated from the beard distally forming chin beards, indicated as *Dianema*.

May 16.—Dr. Carson, Vice-President, in the chair.—"Remains of Mastodon and Horse in North Carolina."—Prof. Leidy exhibited two photographs, received from Prof. W. C. Kerr, State Geologist of North Carolina, representing some remains of *Mastodon americanus* found in that State. One of the specimens represented is that of the greater part of the lower jaw of a mature male, retaining both incisor tusks and the last two molar teeth. The latter, with their angular lobes separated by deep angular and nearly unobstructed valleys, are quite characteristic of the species. The incisors are an inch and three-fourths in diameter. The last molar has four transverse pairs of lobes and a well-developed heel. The penultimate molar has three transverse pairs of lobes. The specimen was obtained from gravel overlying the miocene marl near Goldsboro', Lenoir Co., N.C. An isolated last lower molar of the same species, represented in company with the jaw, was obtained in Pitt Co.—Prof. Leidy also exhibited a specimen of an upper molar teeth, which Mr. Timothy Conrad had picked up from a pile of miocene marl at Greenville, Pitt Co., N.C. He suspected, from its size and intricacy in the folding of the enamel of the islets at the middle of the triturating surface, that the tooth belonged to the post-pliocene *Equus complicatus*, and was an accidental occupant of the miocene marl. It may, however, belong to a Hipparion of the miocene period, but the imperfection of the specimen at its inner part prevented its positive generic determination.

BOOKS RECEIVED

ENGLISH.—A Manual of the Anatomy of Vertebrated Animals: Prof. Huxley (Churchills).—A Synonymic Catalogue of Diurnal Lepidoptera: W. F. Kirby (Van Voorst).—Description of an Electric Telegraph: Sir Francis Ronald (Williams and Norgate).—Spiritual and Animal Magnetism: Prof. J. G. Zerffi (Hardwicke).—An Elementary Treatise on Statics: J. W. Mulcaster (Taylor and Francis).

FOREIGN.—(Through Williams and Norgate.)—Verhandlungen des internationalen Congress für Alterthumskunde u. Geschichte zu Bonn.

DIARY

THURSDAY, NOVEMBER 2.

LINNEAN SOCIETY, at 8.—On the Origin of Insects: Sir John Lubbock, Bart., F.R.S.—Notes on the Natural History of the Flying Fish: Capt. Chimmo.—On a Chinese Gall, allied to the European Artichoke Gall: A. Müller, F.L.S.

CHEMICAL SOCIETY, at 8.—On Anthraflavic Acid: W. H. Perkin.

LONDON INSTITUTION, at 7.30.—On Michael Faraday; the Story of his Life: Dr. J. H. Gladstone, F.R.S.

FRIDAY, NOVEMBER 3.

GEOLOGISTS' ASSOCIATION, at 8.—On the Old Land Surfaces of the Globe Prof. Morris.

MONDAY, NOVEMBER 6.

LONDON INSTITUTION, at 4.—On Elementary Physiology (II.): Prof. Huxley, LL.D., F.R.S.

ANTHROPOLOGICAL INSTITUTE, at 8.—On the Order of Succession of the several Stone Implement Periods in England: J. W. Flower, F.G.S.—Notes on some Archaic Structures in the Isle of Man: A. L. Lewis.

TUESDAY, NOVEMBER 7.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—On the Religious Belief of the Assyrians: H. Fox Talbot.

HACKNEY SCIENTIFIC ASSOCIATION, at 7.30.—Conversazione.

ZOOLOGICAL SOCIETY, at 9.—Report on Recent Additions to the Society's Menagerie: The Secretary.—On the Recent Ziphoid Whales, with a description of the Skeleton of *Berardius arnouxii*: W. H. Flower, F.R.S.—On the Habits of the Nose-horned Viper (*Vipera nasicornis*): Herbert Taylor Ussher, C.M.Z.S.

WEDNESDAY, NOVEMBER 8.

GEOLOGICAL SOCIETY, at 8.—Notes on the Diamond Gravels of the Vaal, in South Africa: G. W. Stow.—On the Geology of the Diamond Fields of South Africa: Dr. John Shaw.—Notes on some Fossils from the Devonian Rocks of the Witzenberg Flats, Cape Colony: Prof. T. Rupert Jones.

THURSDAY, NOVEMBER 9.

LONDON MATHEMATICAL SOCIETY, at 8.—On the Partition of an Even Number into two Primes: J. J. Sylvester, F.R.S.

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