Nature. Vol. V, No. 106 November 9, 1871

London: Macmillan Journals, November 9, 1871

https://digital.library.wisc.edu/1711.dl/LBXITYVRTMAPI83

THURSDAY, NOVEMBER 9, 1871

THE ORIGIN OF GENERA *

ALTHOUGH it is now two years since the publication of Prof. Cope's "fragmentary essay," as he modestly terms it, bearing the above title, it may not be out of place, in the present stage of the theory of Evolution, to give our readers some idea of its scope. It ought to be in the possession of every naturalist. Although already so condensed that anything like an analysis of it is impossible, the following tabular sketch may serve to give our readers an idea of the mode in which the Origin of Genera is treated :—

I. Relations of allied genera.
  First ; in adult age.
  Second ; in relation to their development.
    α. On exact parallelism.
    β. On inexact or remote parallelism.
    γ. On parallelism in higher groups.
    δ. On the extent of parallelisms.
II. Of retardation and acceleration in generic characters.
  First ; metamorphoses in adult age.
    α. The developmental relations of generic and specific characters.
    β. Probable cases of transition.
    γ. Ascertained cases of transition.
  Second ; earlier metamorphoses.
    δ. The origin of inexact parallelisms.
III. Relations of higher groups.
    α. Of homologous groups.
    β. Of heterology.
    γ. Of mimetic analogy.
IV. Of natural selection.
    α. As affecting class and ordinal characters.
    β. As affecting family characters.
    γ. As affecting generic characters.
    δ. As affecting specific characters.
    ε. On metaphysical species.
V. Of epochal relations.

Professor Cope considers that the laws which have regulated the successive creation of organic beings are of two kinds. The first, that which has impelled matter to produce numberless ultimate types from common origins ; the second, that which expresses the mode or manner in which the first law has executed its course, from its commencement to its determined end, in the many cases before us.

"That a descent, with modifications, has progressed from the beginning of the creation is exceedingly probable. The best enumerations of facts and arguments in its favour are those of Darwin, as given in his various important works, 'The Origin of Species,' &c. There are, however, some views respecting the laws of development on which he does not dwell, and which it is proposed here to point out.

"In the first place, it is an undoubted fact that the origin of genera is a more distinct subject from the origin of species than has been supposed.

"A descent with modification involves continuous series of organic types through one or many geologic ages, and the co-existence of such parts of such various series at one time as the law of mutual adaptation may permit.

"These series, as now found, are of two kinds: the uninterrupted line of specific, and the same uninterrupted line of generic characters. These are independent of each other, and have not, it appears to the writer, been developed *pari passu*. As a general law, it is proposed to render highly probable that the same specific form has existed through a succession of genera, and perhaps in different epochs of geologic time.

"With regard to the first law of development as above proposed, no one has found means of discovering it, and perhaps no one ever will. It would answer such questions as this. What necessary coincidence of forces has resulted in the terminus of the series of fishes in the perches as its most specialised extreme? or, of the batrachia, in the fresh-water frogs, as its ultimum? or, of the thrushes, among birds, as their highest extreme? in a word, what necessity resulted in man as the crown of the mammalian series, instead of some other organic type? Our only answer and law for the questions must be, the will of the Creator.

"The second law of modes and means has been represented to be that of natural selection by Darwin. This is, in brief, that the will of the animal applied to its body in the search for means of subsistence and protection from injuries gradually produces those features which are evidently adaptive in their nature. That, in addition, a disposition to a general variation on the part of species has been met by the greater or less adaptation of the results of such variation to the varying necessities of their respective situations. That the result of such conflict has been the extinction of those types that are not adapted to their immediate or changed conditions, and the preservation of those that are" (pp. 4, 5).

In the chapter "On the relations of nearly allied genera," he gives no less than eight "examples of exact parallelism."* We select one at random as illustrating the large number of facts he brings to bear on the subject of which he treats. "The Cervidæ of the Old World are known to develop a basal snag of the antler at the third year; a majority of those of the New World never develop it, except in abnormal cases in the most vigorous maturity of the most Northern Cariacus: while the South American Subulo retains to adult age the simple horn of the second year of Cervus. Among the higher Cervidæ, Rusa and Axis never assume characters beyond an equivalent of the fourth year of Cervus. In Dama, on the other hand, the characters are assumed more rapidly than in Cervus; its third year corresponding to the fourth of the latter. Among American deer there is the Blastocerus, whose antlers are identical with those of the fourth year of Cariacus.

"Now, individuals of the genus Cervus of the second year do not belong to Subulo, because they have not as yet their mature dentition. Rusa, however, is identical with those Cervi whose dentition is complete before they gain the antlers of the fifth year. When the first trace of a snag appears on one beam of *Cariacus virginianus*, the

* "On the Origin of Genera." By Edward D. Cope, A.M., Corresponding Secretary of the Academy of Natural Sciences of Philadelphia. Pp. 80. 1869. (Philadelphia: Merrihew and Son. London: Trübner and Co.)

* The author applies the term *exact parallelism* to the relation of genera which are simply steps in one and the same line of development; while *incomplete parallelism* is applied to that of those where one or more characters intervene in the maturity of either the lower or higher genera to destroy identity.

dentition includes the full number, but there remain $\frac{1}{3}$ milk molars much worn and ready to be shed. Perhaps the snag is developed before these are displaced. If so the Cariacus is never a Subulo; but there can be little doubt that the young Blastocerus belongs to that genus before its adult characters appear."

From the examples of inexact parallelism we select the second and eighth.

"In both perissodactylous and artiodactylous mammalia certain types develop their family character of canines at the earliest appearance of dentition; others, not till a comparatively late period of life (Equus); and the extreme genera never produce them" (p. 14).

"In most serpents the left lung is never developed; in such the pulmonary artery, instead of being totally wanting, remains as a posterior aorta bow, connected with the aorta by a ductus botalli; serpents without left lung being, therefore, identical in this respect with the embryonic type of those in which that lung exists."

Under the head of "adult metamorphoses," in the second chapter, Prof. Cope explains his law of retardation and acceleration. It consists "in a continual crowding backwards of the successive steps of individual development, so that the period of reproduction, while occurring periodically with the change of the year, falls later and later in the life-history of the species, conferring upon its offspring features in advance of those possessed by its predecessors. This progressive crowding back of stages is not, however, supposed to have progressed regularly On the contrary, in the development of all animals, there are well-known periods when the most important transitions are accomplished in an incredibly short space of time (as the passage of man through the stages of the aorta bows and the production of limbs in the Batrachia Anura); while other transitions occupy long periods, and apparently little progress is made" (p. 37).

On these and other similar grounds, the author concludes, that "the transformation of genera may have been rapid and abrupt, and the intervening periods of persistency very long. As the development of the individual, so the development of the genus" (p. 38).

To the question—Has any such transition from genera to genera ever been seen to occur? Prof. Cope answers in the affirmative, and gives eleven probable and six ascertained cases, for the details of which we must refer to pp. 42—46.

Passing for want of space over the third and fourth chapters, we arrive at the concluding one, "On Epochal Relations, or those Measuring Geological Time," which abounds in valuable matter. The comparisons of different faunæ "indicate that an inherent difference between the types of a continent exists at the present time, though the difference is subordinated to a universal distribution of the higher groups throughout the earth. Has this state of things existed for any long period, or is it the result of different progress in the same group since the human period? Thus the present fauna of Australia was preceded in the post-pliocene and pliocene by forms possessing similar peculiarities, and belonging to the same classes: that is, by herbivorous and carnivorous marsupials and monotremes, and by Varanid Sauria, all of greater size than their predecessors.

"The same fact is well known of the Neotropical region, its present peculiar Edentata having been preceded by giants of the same type in the post-pliocene and pliocene."

In the Nearctic, the later Palæarctic, and the Palæotropical regions, the existing genera were similarly represented by pre-existing types, sometimes wonderfully developed.

"Prior to these faunæ another state of things has, however existed. North America has witnessed a withdrawal of a Neotropical fauna, and the Palæarctic the retreat of an Ethiopian type. During the post-pliocene in North America, Neotropical genera were to Neartic as 12 to 29, as the record now stands. In the pliocene beds of Pikermi (Greece) antelopes, giraffes, rhinoceros, hippopotamus, huge manis, monkeys, monitors, and other genera and species of African relationship, are the prevailing forms, and still earlier a strong mingling of Nearctic and more of Neotropical types abounded in the Palæarctic" (p. 77).

We have, then, three important terms from which to derive a theory of the creation:—(1) The existing six faunæ bear in many of their parts developmental relations to one another; (2) They were preceded immediately by faunæ similar to them in each case, but more remotely by faunæ like those now in existence; and (3) the Southern Hemisphere is a geologic stage behind the Northern one in progress, as is shown by its perfection in types extinct in the Northern, and by its inferiority in modern types prevalent in the Northern.

For a fuller demonstration of the last point we must refer our readers to pp. 78, 79 of this valuable monograph.

G. E. D.

MISS NIGHTINGALE ON LYING-IN INSTITUTIONS

Introductory Notes on Lying-in Institutions. By Florence Nightingale. Pp. 110. (Longmans, Green, and Co. 1871.)

MISS NIGHTINGALE tells us the story of this book somewhat as follows:—The Committee of the Nightingale Fund, with the view of extending the usefulness of their Institution for training nurses, entered into an arrangement with St. John's House and King's College Hospital, by which a special ward was set apart for the reception of poor women in childbed, and steps were taken for training midwifery nurses to be employed among the poor in their own houses.

After the ward had been in use for several years, the Committee were made aware that there had been many deaths among cases admitted; this led to inquiry, and the ward was closed.

The Committee being still desirous of continuing this special branch of their work, Miss Nightingale deemed it advisable to inquire into the whole subject of puerperal mortality, and the result is now before us in a form which we can all understand, and we will venture to say that to the generality of readers the facts will bear the aspect of an unwelcome revelation. These facts have been drawn from the Registrar-General's reports, from reports of public institutions in the United Kingdom and over most European countries, affording relief to poor women in their need, both at home and in lying-in institutions, and also from records of private practice.

They show that, while the death-rates for all England from diseases and accidents peculiar to childbirth amount to 4·83 per 1,000, they exceed this amount whenever women pass within the walls of lying-in hospitals—increasing to 5, 6, 7, and in one instance to above 19 per 1,000. If we confine our attention to puerperal diseases, we find that, while the death-rate for all England from these is 1·61 per 1,000, it mounts up in workhouses and other lying-in establishments to 3·3, 3·9, 4·1, and 14·3 per 1·000. In King's College Hospital lying-in ward, the puerperal disease death-rate was nearly 29½ per 1,000. By using Dr. Lefort's data, which give the death-rates from all causes at home and in hospital, in various European countries, it is shown that the approximate death-rate at home is 4·7 per 1,000, while in lying-in institutions it is no less than 34 per 1,000.

Miss Nightingale discusses the causes of these immense death-rates, which, she reminds us, occur among women undergoing not a diseased, but a perfectly natural condition, among whom a death "is little short of a calamity," and "almost a subject for an inquest." We cannot enter into the discussion, but we can say distinctly what is the impression produced by the evidence. It affords another illustration of the danger of unenlightened philanthropy. Some one takes pity on poor suffering women, and forthwith builds an hospital for them or gets it built, without a thought, apparently, of what organic laws of human nature he is about to violate. Nature takes no account of his good intentions, but just goes on, as Miss Nightingale has elsewhere said, "to levy her own cess in her own way."

The practical result of the whole discussion is that lying-in establishments, as at present managed, are destructive of human life, and should be forthwith closed, and that poor women should, as a rule, be attended at home.

The case, however, is not altogether hopeless; and Miss Nightingale proceeds to show how an institution for training midwives and midwifery nurses can be planned and managed without risk. The whole secret consists in assimilating the establishment to home conditions, whatever the cost may be. The evidence shows that in such an institution there would be no more risk than at home. The difficulty, as it appears to us, would be in the cost and in the perfection of management required, which could only be attained by persons practically conversant with physiological laws. But, at the same time, there can be no question of the superior advantages for training which such an institution would afford. This portion of the book is illustrated by plans of existing hospitals, and of the proposed training school. It contains a large amount of valuable detail in small compass, well worthy the attention of the medical profession and the public at large; concluding with an appeal to women, desirous of entering on medical studies, to make this department of practice their own.

The book, as its title implies, is tentative, and there is prefixed to it a quaint dedication to "the shade of Socrates' mother," including a call for help to "the questioning shade of her son, that I, who write, may have the spirit of questioning aright, and that those who read may learn not of me but of themselves." If this Socratic spirit of "questioning aright" were more cultivated, we should have fewer philanthropic mistakes, and science would be less troubled than it has been of late by dogmatic assertions and crude speculations.

OUR BOOK SHELF

Text-Book of Geometry. Part I. By T. S. Aldis, M.A., Senior Mathematical Master, Manchester Grammar School. (Deighton, Bell, and Co.)

WE are much pleased with this book as a good text-book for teaching geometry. It is evidently the work of one who has been at the pains to consider well what are the difficulties which the average pupil encounters. It is the work, too, of one who has seen what the fault of the school teaching of geometry has hitherto been, and who is determined, as far as lies in his power, to remedy it. The evil of school-teaching has been that Euclid has been learned by rote, or when things have not been so bad as that, its propositions have been regarded too much as only abstract truths, which neither have been elucidated by, nor have been used to elucidate natural phenomena or the ordinary things of life. Mr. Aldis supplies this defect by an admirable series of examples and exercises appended to each proposition, calculated to give a practical turn to the whole study in the mind of a beginner, and to familiarise him early with the idea that he can really make use of the subject, and can give it a vast variety of application. Mr. Aldis frequently gives more than one demonstration of the same proposition. This also is very useful in teaching, inasmuch as it practically informs the pupil that the truths of geometry are independent of any particular demonstration of them, and gets him into the habit of approaching any problem from more than one point of view. The present is a first instalment. It contains pretty nearly what is in Euclid's first four books. J. S.

Populäre Wissenschaftliche Vorträge. Von H. Helmholtz. 2tes Heft. (Braunschweig: Verlag von F. Wieweg. London: Williams and Norgate.)

THIS part of Helmholtz's essays reminds us in many respects of Tyndall's lectures—in their clear and eloquent language, eminently adapted for popular comprehension, their freedom from technical expressions, except where these are unavoidable, and in the original mode in which well-known facts are dealt with and used to illustrate profound scientific truths. The work contains six lectures, of which three are devoted to recent advances in the theory of vision, one to the correlation of the physical forces, one to the conservation of force, and the last to the objects and advances of science. In the three lectures devoted to the eye, whilst extolling its perfection as an instrument in the mode in which we use it, he points out its various defects; the blind spot, the blind lines and striæ corresponding to the vessels, its incapacity to focus equally red and violet rays, the want of uniformity in its refraction as indicated by the lines that appear to proceed from a star, &c. He discusses the various colours of the spectrum, and represents this not in the mode usually adopted of a circle with segments of various sizes corresponding to the several primary colours, but as a triangle, of which green, violet, and red occupy the angles, and blue, yellow, and purple the sides, white having an eccentric position near the yellow. Violet, which he was formerly indisposed to regard as a primary colour, he again admits, and he seems inclined to advocate, as best explaining the phenomena of colour-blindness, the views of Young: that there are special nerves for perceiving red, green, and violet rays, an opinion that is less surprising in view of Brown Séquard's conclusions in regard to the number of channels for special sensations contained in the spinal cord, and which is also supported by the remarkable specialisation shown by Helmholtz himself to occur in the branches of the auditory nerve indicated by the phenomena of certain defects of hearing. The chapters on the correlation of the physical forces and the conservation of force, subjects that are now familiar to most scientific Englishmen, are very interesting, as being, to use the German phrase, amongst the original path-breaking essays on these subjects. H. P.

Notes on the Food of Plants. By Cuthbert C. Grundy, F.C.S. (London: Simpkin, Marshall, and Co., 1871.)

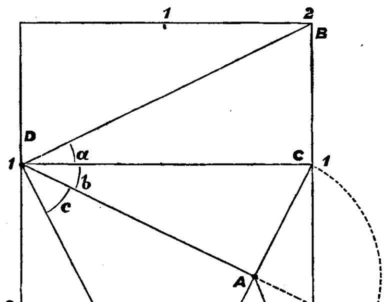
THIS is a useful elementary sketch of the form and manner in which food is obtained by plants. Faults in it there are. Thus, notwithstanding the conclusive experiments of Prillieux and Duchartre, proving that plants have no power of absorbing moisture through their leaves, and the author's own reference to this now established fact in the preface, we still find the assertion (p. 14) that "the leaves withdraw from the atmosphere aqueous vapour." The statement (p. 25) that the sap descends in dicotyledonous plants *through* the bark is not strictly correct; and a Fellow of the Chemical Society ought not to have described (p. 23) carbonic acid as "carbon dioxide combined with water." These blemishes apart, this little book may be recommended to those who desire an explanation of the mode in which vegetable organisms are built up from inorganic materials, and who are unable to devote the time to the more elaborate works of Mr. Johnson, "How Crops Grow" and "How Crops Feed." The portion relating to the effect on crops of different soils strikes us as the best.

LETTERS TO THE EDITOR

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

Proof of Napier's Rules

As the following graphical construction is easily executed, representing to the eye the figure usually employed for the proof of Napier's rules of the parts of right-angled triangles in spherical geometry, it will perhaps remove difficulties from their proof for beginners, like those which Mr. W. D. Cooley's work on "Elementary Geometry" must, from his description of some interesting parts of its contents in NATURE of the 19th of October, have proposed to itself to meet, and to render at least as easily accessible as possible to the inquiring student in mathematics.



BF is a rectangular card, measuring two inches by three inches in the sides, and divided by the lines DB, DC, DA, DB′, and B′C in the directions shown in the figure, and in such a manner that the three corners of the rectangle are completely cut away by the last two, and by the first of these lines; while DC and DA are only cut or scored lightly in the card, so as to allow the remaining three triangles, DBC, DCA, DAB′, to be folded towards each other, until, DB and DB′ coinciding, they form a solid angle of three faces at the point D. The property possessed by this solid angle, that the inclination of the two faces, DCB, DCA, to each other is a right angle (the angle shown at C′ in the base, AB′C′ of the solid angle), and that the base AB′C′ of the resulting tetrahedron cuts the two faces ADC, ADB′, perpendicularly (or at right angles to their common intersection DA) in the line AC, AB′, so that the plane angle A of the plane right-angle triangle B′AC′ is also the inclination between those faces, or t_c angle of the right-angled spherical triangle formed by the intersection of a sphere, about the centre D, with the three planes meeting each other at that point, affords a ready proof of all Napier's rules, excepting that connecting the two angles of a right-angled spherical triangle, from the simple definitions of the trigonometrical "ratios" of plane angles.*

Calling the angles of the faces which meet together at the point D, as shown in the figure *a*, *b*, *c*, opposite to the spherical angles A, B, C, formed by the inclination of the other two faces to each other, these angles, and those of inclination of the faces are, respectively, the sides and angles of a right-angled spherical triangle, whose right angle is C, its hypothenuse is *c*, and the angle A, between *b* and *c* is equal to the plane angle A, of the right-angled triangle AB′C′.

Taking, firstly, as the radius, DA, equal to unity, AC (or AC′), and AB′ are the tangents of *b* and *c*; and the right-angled triangle AC′B′ gives the rule,

$$\frac{\tan b}{\tan c} = \cos A\,;\ \text{or}\ \cos A = \tan b\,.\,\cot c \qquad (1)$$

Taking, in the next place, DB, (or BB′), as the radius, equal to unity; BC (or B′C′), and B′A are the sines; and DC, DA are the cosines of the angles *a* and *c*. In the first case the right-angled triangle AB′C′ affords the ratio

$$\frac{\sin a}{\sin c} = \sin A\,;\ \text{or}\ \sin a = \sin c\,.\,\sin A\,; \qquad (2)$$

And in the second case we obtain from the right-angled triangle ADC the rule

$$\cos c = \cos a\,.\,\cos b \qquad (3)$$

The rules for the angle B, corresponding to (1) and (2) for the angle A, are simply obtained from them by transposing in them the sides and angles *a*A for *b*B; thus—

$$\cos B = \tan a\,.\,\cot c \qquad (4)$$
$$\sin b = \sin c\,.\,\sin B \qquad (5)$$

Finally, dividing (1) by (5), a rule for connecting together the two angles of the right-angled spherical triangle is found as follows:—

$$\cos A \div \sin B = \frac{\tan b}{\tan c} \div \frac{\sin b}{\sin c} = \frac{\cos c}{\cos b} = \cos a,\ \text{by (3)};$$
$$\text{or}\ \cos A = \cos a \sin B \qquad (6)$$

If, as in Napier's rules, the two sides and the differences from 90° of the two angles and of the hypothenuse arranged in their natural order round the triangle are regarded as constituting its five parts, it will be seen that all the above consequences may be included in the two rules known as Napier's rules, that the sine of the middle (that is, of any chosen) part is equal to the product of the tangents of the two adjacents, as well as to the product of the cosines of the two opposite parts.

As a rule to assist the memory, the laconic brevity and completeness of Napier's formula possess a most uniquely felicitous, and, happily for mathematicians, a not unfrequently enduring charm. But should the student desire to divest himself of their artificiality, and to retrace for himself the steps of the demonstration upon which any one example of these rules is based, he must first draw a solid tetrahedron ABCD, in which the facial angles at A, C, are as represented in the figure, but as they cannot all be correctly shown on account of the embarrassing effects of the perspective in the drawing, right angles. By having recourse to a model, on the other hand, which may very readily be cut from a card like that illustrated in the above description, and folded so as to form the solid figure required for their demonstration, all the cases of Napier's rules may be exhibited, and proved, almost as speedily, and satisfactorily to a learner's apprehension in solid geometry, as the definitions of the simple trigonometrical ratios of plane angles, and the least complicated relations connecting together the parts of plane triangles may be made intelligible to him; and that by a plain series of immediate deductions from the figure, which his familiarity with the processes of plane trigonometry will already have taught him very easily to supply.

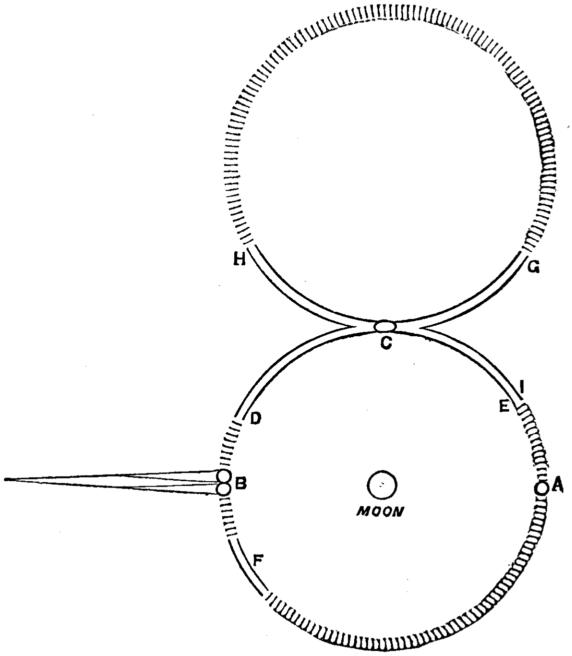
Newcastle-on-Tyne, Oct. 30 A. S. HERSCHEL

Remarkable Paraselene seen at Highfield House on October 25th, 1871

THE phenomenon first became visible at $7^h\ 12^m$ P.M., and finally vanished at $7^h\ 33$ P.M. The upper portion of a halo of

* Another similar property, with a somewhat less important application of the same tetrahedron, is described in the *Quarterly Journal of Mathematics* for October 1862, p. 306.

22° 30′ radius marked DCE, together with a detached portion of F, had the moon for its centre; at the apex of this circle was the apex of another of similar dimensions, HCG, whose centre was about 45° above the moon. On the horizontal level of the moon on either side were mock moons, AB, and immediately above the moon within the same circle was an oval mock moon, C.



Both A and C, though very apparent, were nevertheless not brilliant, the grandeur of the phenomenon centering in the double mock moon B; this was so brilliant that it attracted immediate attention, and that portion nearest the moon was sensibly orange-red. At first it appeared as one large mock moon (twice as broad as it was long), and at $7^h\ 19^m$ divided into two with a thin dark band between. Whilst the two moons (touching each other) were visible, each had a tail of 10° or more in length, and these were included within a gigantic tail of 25° long, considerably more brilliant, but colourless, and contrasting much with the orange-red of the mock moons. At the time of the phenomenon a fog spread over the valley, and overhead were strong cirri in parallel bands The temperature was 37°·2, and on the grass 30°·6.

The moon shone brightly and the sky was cloudless near her throughout the whole time. At $7^h\ 33^m$ a cloud of considerable density obscured both the moon and the phenomenon.

E. J. LOWE

Structure of Lepidodendron

I MAY, perhaps, notwithstanding the editorial injunction to the contrary, be permitted to make one remark by way of addition to what I said in my last letter on this subject. I have been favoured by Mr. J. T. Young with the inspection of some Lepidodendroid stems from the Lancashire coal-fields. These are somewhat different from any others which I have seen, and are probably similar to those Prof. Williamson is working with. At any rate they enable me to understand, what otherwise I have failed to comprehend, namely, the three structures which Prof. Williamson sees in the vascular axis of these plants. In Mr. Young's specimens a transverse section of the vascular axis exhibits (1) the investing cylinder, (2) a zone of larger scalariform vessels, (3) a central irregular mass of vertically disposed rows of scalariform cells with transversely truncate ends. Suppose the transverse septa separating these cells absorbed, as probably eventually they would have been, and the rows of cells become scalariform vessels. I see no reason therefore to lead me to alter my views upon this matter, or to look upon 2 and 3 as forming more than one central structure distinct from 1, the investing cylinder.

W. T. THISELTON DYER

Is Blue a Primary Colour?

No exception can be taken against Dr. Aitken's argument in your number for Oct. 12. The colours of the substances he experimented on could not be regarded as simple. But he does not consider how loosely all names of colours must be applied in common language. The colours of most blue pigments, especially in thin washes, no doubt contain a large proportion of green. But let the colour of the blue salvia, or that of the pigment called French blue or ultramarine (often given as the best example of true blue) be tested in conjunction with the purest yellows (even with the almost greenish yellow of the pigment called lemon-yellow) and the two will be found perfectly complementary. This is the colour of Newton's indigo rays, which he himself in his colour circle put opposite to his yellow. In fact, in good English, not only sea-greenish blues, like the colour of Newton's blue part of the spectrum, or that of the pigment called azure or cæruleum, but even the colour of the violet itself, is properly called blue. Witness Milton's "beds of violets blue." The violet of the spectrum is in truth little more than a pure blue diluted with white by reason of the fluorescence of the retina, as recent researches have shown. (See J. J. Müller's paper in *Poggendorff's Annalen*, March and April last.) I must, therefore, protest against substituting a fanciful term like violet for the good English blue, as the designation of a simple colour-sensation. It is hard enough to make artists believe that yellow is not a simple colour. To tell them the same of its complementary blue would add to their disgust, and not unreasonably.

WILLIAM BENSON

MR. AITKEN in his letter in NATURE, Oct. 12, seems to confound primary with pure colours; it is true they are pure in a certain sense, but in what sense is fully explained in Prof. J. C. Maxwell's lecture, given in NATURE, vol. iv. p. 13. All the experiments mentioned by Mr. Aitken merely prove that the blue colours we commonly see are mixed ones; but the same is the case with almost all the colours we see, while any tint of the spectrum, whether primary or not, may be had pure, *i.e.*, consisting of homogeneous light. Likewise colours which appear just the same to the eye may be made of very different components.

T. W. BACKHOUSE

A Shadow on the Sky

ON the 21st of last August, being at Zermatt, Switzerland, I witnessed from the balcony of the *salle-à-manger* of the Hotel du Mont Cervin a very remarkable appearance. The sun had recently set, and, as I was intensely enjoying the view of that extraordinary mountain, the Matterhorn, I saw its shadow thrown upon the clear sky in the most distinct manner. It was the exact figure of a cone lying obliquely, with its apex somewhat in an upward direction, and its base taking its origin from the S.S.E. side of the mountain. The cone was well defined, the edges of the shadow being sharp and regular. The moon was, from our point of view, at this time behind the Matterhorn. I immediately acquainted some gentlemen, who were at supper in the *salle-à-manger*, with this interesting appearance, and all were much struck with it. My son, Marshall Hall, had just retired to rest, having to be up at two the next morning, in order to make a new ascent in this locality; but I called him out into the garden to enjoy with me this striking scene. The deep, distinct shadow added to the weird effect always produced by this extraordinary mountain, and it so impressed me that I thought the phenomenon might be worth recording in your journal.

Brighton, Oct. 23 CHARLOTTE HALL

A Plane's Position

THIS question is becoming one *de gustibus*, and its further discussion will probably be profitless. I retain my opinion, and am content with the few who side with me. In the two finest treatises on astronomy published during the present century, Herschel's "Outlines of Astronomy," and Grant's "History of Physical Astronomy," the word position is used as I use it. Not systematically, I admit; for Herschel sometimes wrote "situation" where I should write "position." Grant in one place deals somewhat definitively with the word, for at p. 258 he writes, "The position of Saturn's ring is usually determined by the inclination of its plane to the ecliptic and the longitude of its ascending node," the longitude of this node being defined, as all astronomers know, by the *direction* of the line of nodes, not by its *actual place*.

By-the-bye, Sir John Herschel is sometimes very careful to use the words "actual place" where my critics contend that the word "position" would be sufficiently definitive.

It seems overlooked that I pointed out in the beginning that "position" was often but erroneously used as synonymous with "place." It is not my fault if this error appears in the technical use of the word "position" in some mathematical treatises. I say again with Colonel Mannering, *Abusus non tollit usum*—"The abuse of anything doth not abrogate the lawful use thereof." It was a *lapsus calami* of mine to say that "position" could not be misunderstood. It could be, for it has been misused.

Prof. Hirst is quite right in saying I should be unable to describe the aspect of a horizontal plane. I should not think of trying to. He says, however, that Mr. Wilson would unhesitatingly pronounce its aspect *vertical*. (Does it look vertically *up* or vertically *down?*) What would Mr. Wilson assign—unhesitatingly or otherwise—as the aspect of the "prime vertical"?

Has a true plane (as distinguished from a plane face of a solid) one aspect or two? It has one *position* or *situation*, and one *place* or *location*, but I conceive that it has two aspects.

Mr. Laughton seems quite unaware of Sir J. Herschel's repeated use of the word "tilt."

His comment on my remark about the books which I have written is unworthy. He must surely perceive that I only sought to indicate how much occasion I had had to consider the subject of plane-position; more occasion, I think, than any of my critics, save Prof. Hirst, the weight of whose opinion I recognise fully, though I cannot agree with him. But I have *not* felt free to use the word "position" so systematically as I should wish, precisely because of its misuse to indicate *place*. I have only been able to use it where there could be no fear of that wrong meaning being assigned to it.

As I claim no credit for the invention of any word for indicating plane-position, and as I could not take from Mr. Laughton that which is not his—the credit for Hamilton's word "aspect"—perhaps I may be permitted to say that if I am "pertinacious" (as Mr. Laughton asserts) there is nothing personal in my pertinacity. It is not my custom to admit that I am wrong when I consider that I am right.

[My objections to the word "aspect" are confirmed by Mr. Wilson's letter. I wrote that the word could not be used in the sense indicated, "unless a new and artificial meaning were assigned to it." Mr. Wilson obligingly proves this by assigning to it just such a meaning. "The aspect of a plane is the direction of its normal," it would seem. Now no special objection need be urged against this definition, if it is to be confined rigidly within the limits of mathematical text-books. The definition is strange and artificial no doubt; but it is nothing new to see the familiar and natural banished from such works. As a writer on astronomy, however, I must decline to accept the proposed usage, which seems to me altogether objectionable. If I write respecting the celestial equator-plane that "its position is at right angles to the polar axis of the heavens," I find that I am understood; but I am sure my readers would be very much perplexed if I wrote that "the aspect of the equator-plane is the direction of the polar axis." Again, I should be understood, I think, if I said that "the positions of two hour-planes determine the direction of the polar axis," or that "the directions of the polar axis and the vertical determine the position of the meridian-plane." But if I wrote "aspect" where I have here written "position," I scarcely know what my readers would think.

By the way, what would be the "aspect" of the meridian-plane according to the proposed usage? Would it be "east" or "west"? The normal to that plane would lie east and west; but we could not hear of an "east-and-west" aspect without thinking of certain "clear stories towards the south-north, lustrous as ebony."

I am bound to point out, however, though I may seem to weaken my position by doing so, that a very eminent authority long since used the word "aspect" in the sense suggested by Mr. Laughton. In one of his well-known "Letters to a Lady," on quaternions, Sir W. R. Hamilton uses the words "position," "slope," "ledge," and "aspect," to express the relations which I have called respectively "place," "slope," "aspect," and "position." (See Nichol's "Cyclopædia of the Physical Sciences," 2nd edition, p. 708.) I apprehend, however, that he lays no special stress on this verbiage. He had used the word "position" for "place," and this left him without any word to indicate position. Besides, his illustrative plane is the surface of a desk, and a surface may be conceived to have an aspect definable by the direction of its normal, but a geometrical plane is two-faced.] *

This is my last letter on the present subject—unless one of your correspondents should employ arguments showing me to be in error, in which case I shall crave two lines of your space to admit as much. RICHD. A. PROCTOR

Brighton, Nov. 3

P.S.—Let it be noticed that the question is not how the word "position" has been used by some, but how it ought to be used by all.

I CANNOT agree with Mr. Wilson that "aspect" is *exactly* the word wanted. The same wall has *two* aspects; if a southern, then also a northern aspect on the other side. In fact the word seems adapted, according to its common usage, to express the "sense" (*sens*), as well as the direction of the plane's normal, whereas I take it that the word sought for should express the direction only without connoting the "sense."

I think a word sometimes used by geologists would be, if we dare use it, exactly the word. As they speak of the *lie* of strata, defined (with respect to the horizon) by its two elements, *strike* and *dip*, so geometers might well speak of the *lie* of a plane; but would our English language permit us to say that "two lies determine one direction," and "two directions determine one lie"? I fear the moral connotation of the word, although an etymological accident, is too ugly.

If we are reduced to coin a new word, I would suggest that the Latin root "pand" (spread), would afford for a plane the fitting analogue of the root "reg" (rule, make straight), for a line, and so the word "dispansion" would be the analogue of "direction." "Parallel planes have the same dispansion." "Two dispansions determine one direction, and two directions determine one dispansion." Will not the neatness of this mode of expressing Mr. Wilson's test propositions atone for the strangeness of the word?

The word "aspect," however, is too good to be rejected from geometrical science, though I believe its chief use will be found beyond the domain of pure geometry. Should it not be appropriated to cases where the plane presents different aspects to the portions of space on either side of it? For instance, if two bodies revolve in the same or parallel planes, their orbits might be said to have the same or contrary aspects, according as the bodies revolve in the same or contrary directions, and so the positive aspect of a planet's orbit would determine, not only the "lie" or "dispansion" of the plane of the orbit, but also the direction of revolution in that orbit. So, too, the statement that all the planetary orbits have nearly the same aspect, would imply not only that their planes nearly coincide, but also that they all revolve in the same direction. I cannot help thinking that Mr. Proctor would find his account in adopting this sense of the word "aspect" in his astronomical writings, especially since he might, as Dr. Hirst suggests, retain the word where he has hitherto employed it, by simply qualifying it with an appropriate adjective. (Would the adjective "azimuthal" satisfy him?

May I conclude with a question which I have often wished to propound? What is the proper English equivalent for the French "*sens*"? English mathematicians generally seem shy of using the word "sense," while, to use the word "direction" as well for the "*sens*" as the "direction" of a line, is very awkward and inconvenient. The difficulty, I imagine, is the same as appears to me almost fatal to the word "lie" proposed above, namely, that the proposed technical use diverges too widely from the familiar use of the word. Is not the superior flexibility of the German language in the formation of new terms in part due to a less degree of fastidiousness in this respect?

Harrow, Nov. 6 ROBT. B. HAYWARD

AFTER all, I fear the word "aspect" is not quite the right thing. What is wanted is a word to express "plane-direction;" something in the plane, and not looking out from it. And I am not sure that the compound word "plane-direction," which is not ambiguous nor colloquial, will not be better even than "aspect."

We should then have axioms on planes analogous to those on straight lines: that planes may have the same or different plane-directions: that intersecting planes have different plane-directions; and conversely.

Parallel planes will be defined as those which have the same plane-direction.

* The matter between brackets was written on October 27.—ED.

With this word it is easier to state the theorems, "two line-directions determine one plane-direction," and its reciprocal, than with the other. "Two directions determine one aspect," is hard.

If this discussion has not gone on too long perhaps some of your correspondents will criticise this suggestion and compare it with "aspect." It is desirable that the best word possible should be chosen. J. M. W.

Science and Art Examinations

THE subject of Science and Art Examinations by the Department of Science and Art is one which really requires looking up, and I wish to make one or two suggestions and remarks as to the mode of examination.

In the first place, take the examination itself. The candidates make their appearance at the appointed time and place. Their forms are given them, and their places assigned to them. Now the candidate is told to write on both sides of the form, thus leaving no back pages on which to do his rough calculation. Blotting-paper in 1870 was not allowed; but in 1871 the Department fixed a sheet to the bottom of each form in such a position that it was very difficult to make use of it; much time—time that was of the utmost consequence to the candidate—being lost in doing so. This, of course, stopped him from doing so much work, and so lessened his chance of success. This may be all very well for the Department so far as it affects grants on results; but what about the unfortunate student who is made the victim of this very arbitrary custom?

Then again for the questions set. In all the papers the questions set were very difficult. "The Department" having, without any notice, raised the standard of examination, the subjects of questions set in the first stage of mathematics were placed in the syllabus a stage higher, viz., the second stage. Then in chemistry (inorganic) the standard was considerably raised. The questions in this subject are very unfair in the opinion of many persons who have seen them. Take the following:—

"HONOURS 1871

"Describe the process of manufacturing sulphuric acid, as carried on in an alkali works, illustrating the various chemical changes by equations, and, as far as possible, the constitution of the compounds formed by graphic formulæ."

Now about the sulphuric acid part, or about the equations, I have nothing to say; but when the question requires a knowledge of graphic formulæ I protest against it. Graphic formulæ are not in sufficient use to warrant their introduction into an examination—thus enforcing their general adoption whether right or wrong; and I do not think the examiner should be allowed to enforce his peculiar views—the views taken by himself and a few other chemists—into the great system of Science examination in the country, thus compelling it to be learnt by any person wishing to compete.

Now for the results. The results of the examinations for 1871 are very unsatisfactory, and a very high ratio is shown of failures, and second classes to first classes obtained. This, of course, must lessen the amount of money to be paid on results by the Department, and a report was circulating a short time ago, to the effect that "The examiners, after having made their reports, had the papers returned to them, with an instruction to reduce the number of successful candidates, as an intimation had been given by a right hon. gentleman that the amount of grant due upon those papers must be reduced 20,000*l*. The examiners were thus obliged to eliminate half the names from their lists." The question was asked by Mr. Dixon, M.P., in the House of Commons, whether this was or was not true, and Mr. Forster, M.P., denied it. But, previous to that, a provincial local secretary, hearing the rumour, wrote to ask the Department if it were true, and received a reply saying it was true, and that instead of the amount being 20,000*l*. it was 40,000*l*. (The Department's letter can be produced.) Now I would suggest that the Department reform these matters referring to the forms, blotting-paper, questions, and results, and that if they do not do so that the House of Commons take the matter up and do justice to Science teachers and students. HENRY UHLGREN

New Zealand Forest Trees

IN the last number of NATURE is a paragraph relating to some New Zealand woods, which the writer observes are "deserving of a better fate than to be cut down wholesale and used as firewood." Five timber trees are mentioned, of which the native names only are given.

Knowing that it is the province of NATURE to give as accurate information as possible on all points with which it deals, I send you the botanical names of four of these New Zealand trees. The Rimu or red pine is probably *Dacrydium cupressinum* Soland, a tree 80 or more feet high, the fleshy cup of the fruit of which is eatable. *D. laxifolium* Hk. fil., a small creeping bush, is also known occasionally as Rimu. The Mataii or black pine is *Podocarpus spicata* Br., likewise a large tree, and having an eatable fruit. The Totara is *Podocarpus totara* A. Cunn., a tree about 60 feet high, producing a durable and close-grained wood much valued in the islands, and, like the others, having an eatable drupe. These trees are all more or less abundant in the Northern and Middle islands, and all belong to the natural order Coniferæ, though we are told in the paragraph referred to that "none of them are Coniferæ."

The Rata, "that wonderful vegetable production forming itself out of numberless vines," &c., is referable to some species of *Metrosideros*. *M. robusta* A. Cunn, and *M. florida* Sm., are both known as Rata, but the hard and very dense wood usually known under that name is mostly derived from *M. robusta*. This, however, is not a climbing plant, but an erect tree 50 or 60 feet high; therefore the plant referred to in the paragraph before us is probably *M. florida*. The Makia I do not know, but its extreme hardness would seem to indicate it as belonging to the same order as the last, namely the Myrtaceæ.

JOHN R. JACKSON

Kew, Nov. 7

The Glacial Drift at Finchley

A FURTHER examination of the railway cutting at the Finchley and Hendon Station shows that the glacial beds now revealed there have a greater thickness and range than I at first imagined. On Saturday last I visited the place in company with Dr. Hicks, of Hendon, a gentleman well-known for his researches in the Cambrian formation. Above the blue clay, and right up within a few inches of the vegetable soil, we found drift fossils. With an interruption here and there from the underlying London clay, these chalky glacial beds, consisting of blue (Oxford?) clay, blueish clay with flints, marl, sand, and gravel (in no regular descending order), have an average thickness of 30 feet. They are open for about 500 yards, and they might perhaps be traced farther north-west, towards the Dollis Brook Viaduct. Dr. Hicks and I afterwards visited Mr. Plowman's Manor brick-fields, a little south-east of the railway station; here too we found fossils in the brick-earth.

From what has transpired during the last few weeks, it would seem that the Muswell Hill deposit need no longer figure in geological literature as an outlier, at a long distance from the general deposit; and Londoners may in future find glacial drift without much difficulty about Highgate, Finchley, Whetstone, and Barnet. I am indebted to Professor Morris for the information that the Great Northern Cemetery at Barnet lies almost wholly in the glacial clay. The forthcoming Survey memoir upon the drift in this district is looked for by London geologists with much interest. HENRY WALKER

100, Fleet Street, E.C., Nov. 7

*ON THE ORIGIN OF INSECTS**

THE metamorphoses of this group have always seemed to me one of the greatest difficulties of the Darwinian theory. In most cases the development of the individual reproduces to a certain extent that of the race, but the motionless, imbecile, pupa cannot represent a mature form. Fritz Müller considers that the wingless Blattidæ probably most closely represent the original insect stock; Haeckel is inclined rather to the Pseudo-Neuroptera. I feel great difficulty in conceiving by what natural process an insect with a suctorial mouth like that

* Abstract of a paper read before the Linnean Society, Nov 2, 1871, by Sir John Lubbock, Bart., M.P., F.R.S.

of a gnat or butterfly could be developed from a powerfully mandibulate type like the Orthoptera, or even from the Neuroptera. M. Brauer has recently suggested that the interesting genus *Campodea* is, of all known existing forms, that which probably most nearly resembles the parent insect stock. He considers that the grub form of larva is a retrograde type, in which opinion I am unable to concur, though disposed to agree with M. Brauer on the first point. M. Brauer in coming to this conclusion relies partly on geological considerations; partly on the fact that larvæ, more or less resembling *Campodea*, are found among widely different groups of insects. I think there are other considerations which offer considerable support to this view. No one, so far as I know, has yet attempted to explain, in accordance with Mr. Darwin's views, such a life history as that, for instance, of a butterfly, in which the mouth is first mandibulate and then suctorial. A clue to the difficulty might, I think, be found in the distinction between developmental and adaptive changes, to which I called the attention of the Society in a previous memoir. The larvæ of insects are by no means mere stages in the development of the perfect animal. On the contrary, they are subject to the influence of Natural Selection, and undergo changes which have reference entirely to their own requirements and condition. It is evident then that, while the embryonic development of an animal in the egg gives us an epitome of its specific history, this is by no means the case with species in which the immature forms have a separate and independent existence. Hence, if an animal when young pursues one mode of life, and lives on one kind of food, and subsequently, either from its own growth in size and strength, or from any change of season, alters its habits or food, however slightly, immediately it becomes subject to the action of distinct forces; Natural Selection affects it in two different, and it may be very distinct, manners, gradually leading to differences which may become so great as to involve an intermediate period of change and quiescence.

There are, however, peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva and suctorial as an imago. From this point of view, however, *Campodea* and the *Collembola* (*Podura*, &c.) are peculiarly interesting. There are among insects three principal types of mouth, firstly, the mandibulate, secondly, the suctorial, and thirdly, that of *Campodea*, and the *Collembola* generally, in which the mandibles and maxillæ are attached internally, and though far from strong, have some freedom of motion, and can be used for biting and chewing soft substances. This type is intermediate between the other two. Assuming that certain representatives of such a type found themselves in circumstances which made a suctorial mouth advantageous, those individuals would be favoured by Natural Selection in which the mandibles and maxillæ were best calculated to pierce or prick, and their power of lateral motion would tend to fall into abeyance, while, on the other hand, if powerful masticatory jaws were an advantage, the opposite process would take place.

There is yet a third possibility—namely, that during the first portion of life the power of mastication should be an advantage, and during the second that of suction, or *vice versâ*. A certain kind of food might abound at one season and fail at another; might be suitable for the animal at one age and not at another: now in such cases we should have two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions. It will not be denied that the ten thousand variations in the mouth parts of insects have special reference to the mode of life, and are of some advantage to the species in which they occur. Hence no believer in Natural Selection can doubt the possibility of the three cases above suggested, and the last of which seems to explain the possible origin of species which are mandibulate in one period of life and not in another. The change from the one condition to the other would no doubt take place contemporaneously with a change of skin. At such times we know that, even when there is no change of form, the temporary softness of the organs often precludes the insect from feeding for a time, as, for instance, is the case in the silkworm. When, however, any considerable change was involved, this period of fasting would be prolonged, and would lead to the existence of a third condition, that of pupa, intermediate between the other two. Since other changes are more conspicuous than those relating to the mouth, we are apt to associate the pupa state with the acquisition of wings, but the case of the Orthoptera (grasshoppers, &c.) is sufficient proof that the development of wings is perfectly compatible with continuous activity. So that in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth, although in many cases no doubt the result is accompanied by changes in the legs, and in the internal organisation. It is, however, obvious that a mouth like that of a beetle could not be modified into a suctorial organ like that of a bug or a gnat, because the intermediate stages would necessarily be injurious. Neither, on the other hand, for the same reason could the mouth of the Hemiptera be modified into a mandibulate type like that of the Coleoptera. But in *Campodea* and the *Collembola* we have a type of animal closely resembling certain larvæ which occur both in the mandibulate and suctorial series of insects, and which possesses a mouth neither distinctly mandibulate nor distinctly suctorial, but constituted on a peculiar type capable of modification in either direction by gradual changes without loss of utility.

If these views are correct, the genus *Campodea* must be regarded as a form of remarkable interest, since it is the living representative of a primæval type from which not only the *Collembola* and *Thysanura* but the other great orders of insects have all derived their origin.

CHARLES BABBAGE

DIED THE 20TH OF OCTOBER, 1871

THERE is no fear that the worth of the late Charles Babbage will be over-estimated by this or any generation. To the majority of people he was little known except as an irritable and eccentric person, possessed by a strange idea of a calculating machine, which he failed to carry to completion. Only those who have carefully studied a number of his writings can adequately conceive the nobility of his nature and the depth of his genius. To deny that there were deficiencies in his character, which much diminished the value of his labours, would be useless, for they were readily apparent in every part of his life. The powers of mind possessed by Mr. Babbage, if used with judgment and persistence upon a limited range of subjects, must have placed him among the few greatest men who can create new methods or reform whole branches of knowledge. Unfortunately the works of Babbage are strangely fragmentary. It has been stated in the daily press that he wrote eighty volumes; but most of the eighty publications are short papers, often only a few pages in length, published in the transactions of learned societies. Those to which we can apply the name of books, such as "The Ninth Bridgewater Treatise," "The Reflections on the Decline of Science," or "The Account of the Exposition of 1851," are generally incomplete sketches, on which but little care could have been expended. We have, in fact, mere samples of what he could do. He was essentially one who began and did not complete. He sowed ideas, the fruit of which has been reaped by men less able but of more thrifty mental habits.

It was not time that was wanting to him. Born as long ago as the 26th of December, 1792, he has enjoyed a working life of nearly eighty years, and, though within the last few years his memory for immediate events and persons was rapidly decaying, the other intellectual powers seemed as strong as ever. The series of publications which constitute the real record of his life commenced in 1813 with the preface to the Transactions of the Analytical Society, a small club established by Babbage, Herschel, Peacock, and several other students at Cambridge, to promote, as it was humorously expressed, the principles of pure D-ism, that is, of the Leibnitzian notation and the methods of French mathematicians. Until 1822 Mr. Babbage's writings consisted exclusively of memoirs upon mathematical subjects, which, however little read in the present day, are yet of the highest interest, not only because they served to awaken English mathematicians to a sense of their backward position, but because they display the deepest insight into the principles of symbolic methods. His memoir in the "Cambridge Philosophical Transactions" for 1826, "On the Influence of Signs in Mathematical Reasoning" may be mentioned as an admirable example of his mathematical writings. In this paper, as in many other places, Mr. Babbage has expressed his opinion concerning the wonderful powers of a suitable notation in assisting the human mind.

As early as 1812 or 1813 he entertained the notion of calculating mathematical tables by mechanical means, and in 1819 or 1820 began to reduce his ideas to practice. Between 1820 and 1822 he completed a small model, and in 1823 commenced a more perfect engine with the assistance of public money. It would be needless as well as impossible to pursue in detail the history of this undertaking, fully stated as it is in several of Mr. Babbage's volumes. Suffice it to say that, commencing with 1,500*l.*, the cost of the Difference Engine grew and grew until 17,000*l.* of public money had been expended. Mr. Babbage then most unfortunately put forward a new scheme for an Analytical Engine, which should indefinitely surpass in power the previously-designed engine. To trace out the intricacies of negotiation and misunderstanding which followed would be superfluous and painful. The result was that the Government withdrew all further assistance, the practical engineer threw up his work and took away his tools, and Mr. Babbage, relinquishing all notions of completing the Difference machine, bestowed all his energies upon the designs of the wonderful Analytical Engine. This great object of his aspirations was to be little less than the mind of a mathematician embodied in metallic wheels and levers. It was to be capable of any analytical operation, for instance solving equations and tabulating the most complicated formulæ. Nothing but a careful study of the published accounts can give an adequate notion of the vast mechanical ingenuity lavished by Mr. Babbage upon this fascinating design. Although we are often without detailed explanations of the means, there can be little doubt that everything which Mr. Babbage asserted to be possible would have been theoretically possible. The engine was to possess a kind of power of prevision, and was to be so constructed that intentional disturbance of all the loose parts would give no error in the final result.

Although for many years Mr. Babbage entertained the intention of constructing this machine, and made many preparations, we can hardly suppose it capable of practical realisation. Before 1851 he appears to have despaired of its completion, but his workshops were never wholly closed. It was his pleasure to lead any friend or visitor though these rooms and explain their contents. No more strange or melancholy sight could well be seen. Around these rooms in Dorset Street were the ruins of a life time of the most severe and ingenious mental labours perhaps ever exerted by man. The drawings of the machine were alone a wonderful result of skill and industry; cabinets full of tools, pieces of mechanism, and various contrivances for facilitating exact workmanship, were on every side, now lying useless.

Mr. Babbage's inquiries were not at all restricted to mathematical and mechanical subjects. His work on the "Economy of Manufactures and Machinery," first published in 1832, is in reality a fragment of a treatise on Political Economy. Its popularity at the time was great, and, besides reprints in America, translations were published in four Continental languages. The book teems with original and true suggestions, among which we find the system of Industrial Partnerships now coming into practice. It is, in fact, impossible to overpraise the work, which, so far as it goes, is incomparably excellent. Having assisted in founding the Statistical Society of London in 1834, Mr. Babbage contributed to their Transactions a single paper, but as usual it was a model research, containing a complete analysis of the operations of the Clearing House during 1839. It was probably the earliest paper in which complicated statistical fluctuations were carefully analysed, and it is only within the last few years that bankers have been persuaded by Sir John Lubbock to recognise the value of such statistics, and no longer to destroy them in secret. In this, as in other cases, many years passed before people generally had any notion of the value of Mr. Babbage's inquiries; and there can be little doubt that, had he devoted his lofty powers to economic studies, the science of Political Economy would have stood by this time in something very different from its present pseudo-scientific form.

Perhaps the most admirable of all his writings was the Ninth Bridgewater Treatise, an unexpected addition to that well-known series, in which Mr. Babbage showed the bearing of mathematical studies upon theology. This is one of the few scientific works in which the consistency of natural laws with breaches of continuity is clearly put forth. That Power which can assign laws can set them aside by higher laws. Apart from all particular theological inferences, there can be no question of the truth of the views stated by Babbage; but the work is hardly more remarkable for the profundity of its philosophy than for the elevated and eloquent style in which it was written, although as usual an unfinished fragment.

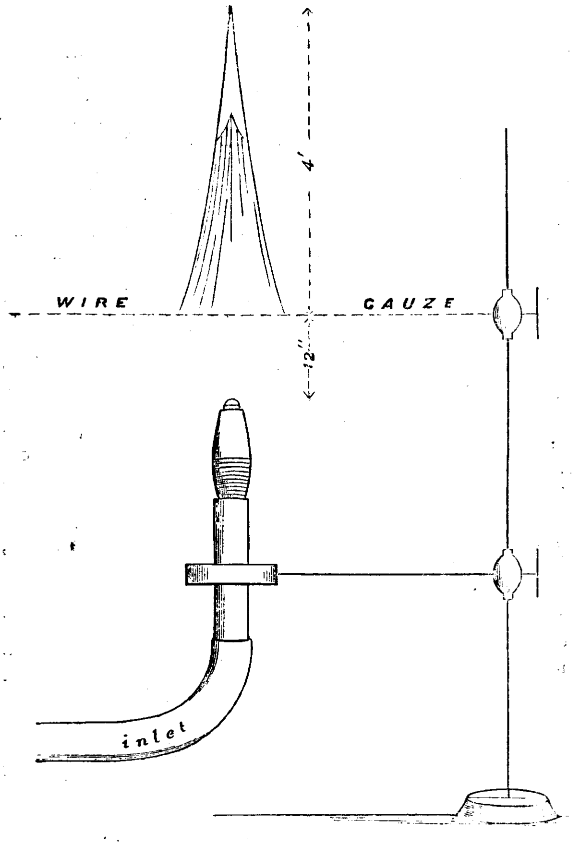
Of all Mr. Babbage's detached papers and volumes, it may be asserted that they will be found, when carefully studied, to be models of perfect logical thought and accurate expression. There is, probably, not a sentence ever penned by him in which lurked the least obscurity, confusion, or contradiction of thought. His language was clear, and lucid beyond comparison, and yet it was ever elegant, and rose at times into the most unaffected and true eloquence. We may entertain some fear that the style of scientific writing in the present day is becoming bald, careless, and even defective in philosophic accuracy. If so, the study of Mr. Babbage's writings would be the best antidote.

Let it be granted that in his life there was much to cause disappointment, and that the results of his labours, however great, are below his powers. Can we withhold our tribute of admiration to one who throughout his long life inflexibly devoted his exertions to the most lofty subjects? Some will cultivate science as an amusement, others as a source of pecuniary profit, or the means of gaining popularity. Mr. Babbage was one of those whose genius urged them against everything conducive to their immediate interests. He nobly upheld the character of a discoverer and inventor, despising any less reward than to carry out the highest conception which his mind brought forth. His very failures arose from no want of industry or ability, but from excess of resolution that his aims should be at the very highest. In these money-making days can we forget that he expended almost a fortune on his task? If, as people think, wealth and luxury are corrupting society, should they omit to honour one of whom it may be truly said, in the words of Merlin, that the single wish of his heart was "to give them greater minds"?

A NEW FORM OF SENSITIVE FLAME

MR. PHILIP BARRY, of Cork, has sent the following account of a new and very beautiful sensitive flame to Prof. Tyndall:—"It is in my experience the most sensitive of all sensitive flames, though from its smaller size is not so striking as your vowel flame. It possesses the advantage that the ordinary pressure in the gas mains is quite sufficient to develop it. The method of producing it consists in igniting the gas (ordinary coal gas) not at the burner but some inches above it, by interposing between the burner and the flame a piece of wire gauze.

"With a pressure of $\frac{7}{10}$ths at the burner, I give a sketch of the arrangement I adopted, the space between burner and gauze being two inches. The gauze was about seven



inches square, resting on the ring of the retort-stand—ordinary window-blind wire-gauze 32 meshes to the lineal inch. The burner was Sugg's steatite pin-hole burner, the same as used for vowel flame.

"The flame is a slender cone about four inches high, the upper portion giving a bright yellow light, the base being a non-luminous blue flame. At the least noise this flame roars, sinking down to the surface of the gauze, becoming at the same time almost invisible. It is very active in its responses, and being rather a noisy flame, its sympathy is apparent to the ear as well as the eye.

"To the vowel sounds it does not appear to answer so discriminately as the vowel flame. It is extremely sensitive to A, very slightly to E, more so to I, entirely insensitive to O, but slightly sensitive to U.

"It dances in the most perfect manner to a small musical snuff box, and is highly sensitive to most of the sonorous vibrations which affect the vowel flame, though it possesses some points of difference."

NOTES

THE following telegram has been received from the English Government Eclipse Expedition:—"On board the *Mirzapore*, Malta, Saturday, November 4. We have arrived here in safety. All the members of the Eclipse Expedition are quite well, no thanks, however, to the weather, which during the voyage has been very bad. It was so bad that there was no possibility of practising with the instruments. Last night Mr. Lockyer, at the request of all on board the *Mirzapore*, gave a scientific lecture with experiments. You may form some idea of the novel character with which the lecture was invested when I state that it was blowing half a gale at the time."

SIR RODERICK MURCHISON has appointed Professor Archibald Geikie, of Edinburgh, his literary executor, and has left him a legacy of 1,000*l.* The Professor will write Sir Roderick's life, for which the deceased baronet had collected ample materials. Sir Roderick has also bequeathed to each of the professors at Jermyn Street a little remembrance of 100*l.* To the institution itself he has left the diamond snuff-box and the magnificent Siberian avanturine vase, mounted on a porphyry pedestal, presented to him by the late Emperor of Russia. He has not been unmindful of the scientific societies with which he has been so long connected. To the Geological and Geographical Societies he has bequeathed legacies of 1,000*l.* each, for the purpose of furthering the cause of science by rewarding men of science by prizes or otherwise as may be deemed proper. To old associates with him in his work he has likewise left legacies as expressions of his regard. Besides that to Mr. Geikie, sums of 350*l.* are appropriated for Prof. John Morris, Prof. T. Rupert Jones, Mr. Trenham Reeks, and Mr. Bates, and a sum of 100*l.* to Mr. C. W. Peach. We believe also that in the event of the failure of some of the heirs designated in the will, considerable sums are to go to various charitable and scientific institutions.

IN addition to the appointments to the governing bodies of the public schools, made by the Senate of the University of London, which we announced last week, the Council of the Royal Society has made the following:—Prof. P. M. Duncan, for Charterhouse; Prof. Tyndall, for Harrow; Prof. Henry J. Smith, for Rugby; Sir James Paget, Bart., for Shrewsbury; and the Rev. Prof. Price, for Winchester School.

PROF. P. M. DUNCAN, F.R.S., of King's College, has been appointed Lecturer on Geology to the India Civil Engineering College, Cooper's Hill.

WE learn from the *Pall Mall Gazette* that a mixed Committee has been appointed by the authorities of the War-Office, to conduct an inquiry into the safety of gun-cotton, and to make the necessary experiments. The committee will also be required to collect evidence with regard to its value as an explosive agent; and generally to pronounce as to the suitability and safety of the material for use in torpedoes, breaching stockades, mining, &c. The Committee consists of Colonel Younghusband, R.A., president; Colonel Milward, R.A., Colonel Gallwey, R.E., Lieutenant-Colonel Nugent, R.E., Captain Field, R.N., Dr. Odling, F.R.S., Mr. H. Bauerman, and Mr. G. Bidder, C.E. The question of the safety of the new explosive "Lithofracteur," which a German firm is anxious to be permitted to make in this country, has also been referred to the same Committee.

MR. G. M. SEABROKE, the Temple Observer at Rugby, states, in a letter to the *Times*, for the information of those who possess telescopes of moderate aperture, that Encke's comet is now within their reach. It has been examined at the Rugby Observatory with an $8\frac{1}{4}$ in. aperture, and was very plainly seen. It has somewhat the shape of a fan, and there is a marked condensation on the eastern side, being the leading portion of the comet. It would probably now be seen with a much smaller aperture than that mentioned above, and, as it is approaching us, small telescopes will probably soon show it.

THE German Astronomical Society has recently held its triennial meeting at Stutgart, under the presidency of Prof. Otto Struve. The gathering was eminently a social one; after papers read in the morning, they adjourned for excursions in the afternoon, one day visiting the birth-place of Kepler, a small town about an hour by rail from Stutgart. The inhabitants, who have recently erected a bronze statue to their great fellow townsman, decorated it with flowers for the occasion.

THE Scientific Societies have now mostly commenced their winter session. The greater number held their first meeting either last or during the present week. The first meeting of the Royal Society for the season is on November 16.

THE Annual General Meeting of the five Academies which constitute the Institute of France was held on the 25th of October, the anniversary of the day on which the Institute was established by the famous Directory suppressed by the first Napoleon. The third Napoleon, by an Imperial decree, changed the day of the anniversary meeting from that instituted by the Republic to his *fête* day, the 15th of August. Last year the meeting was not held, and on the present occasion the original date has been resumed. The presidency of the Institute is filled each year by the president of one of the five academies in rotation, the Académie des Sciences, Académie Française, Académie des Sciences Morales et Politiques, Académie des Beaux Arts, and Académie des Inscriptions et Belles Lettres. This year it is occupied by M. Jules Simon, president of the Académie Française, to which belong M. Thiers himself and four of his colleagues in the Government, including M. Simon. The annual address for the Académie des Sciences was delivered by General Morin, and dealt chiefly with military science, especially with the inventions of the great artillery officer General Piobert.

MR. J. J. MURPHY delivered the opening address to the Belfast Natural History and Philosophical Society for the current session. It was occupied chiefly with a *résumé* of the most important fresh applications of applied science during the year.

MR. RUTHERFORD, of New York, the most eminent American amateur astronomer, and especially known for his magnificent photographs of celestial bodies, has lately presented to Mr. Brothers, the English astronomical photographer, three superb negatives of the moon—one representing her in the first quarter, one when full, and one in the third quarter; and it is proposed to publish these in a volume containing about one hundred pages of descriptive letterpress. The work will also contain a map of the moon, as we see her, and a chart, on the stereographic projection, showing the true shape and the relative dimensions of all the chief lunar features. The letterpress, map, and stereographic chart will be prepared by Mr. Proctor; the photographs by Mr. Brothers. The work will be got out on a magnificent scale, and sold at a guinea and a half to subscribers.

MESSRS. TRÜBNER announce the proposed publication of a new magazine, *The Pioneer*; a monthly journal of Sociology, Psychology, and Biology. The great aim which the *Pioneer* has in view will be "the expression of truly philosophic principles, and their application to human progress and welfare. The opinions of all will be treated with respect when expressed with the clearness and force arising from strong conviction." The subjects of "Psychic Force" and Anthropology are especially alluded to in the prospectus as coming within the range of the proposed serial.

THE Geological Expedition to the Rocky Mountain region under the charge of Dr. Hayden, to which we have already made briet allusion, according to *Harper's Weekly*, had reached Fort Hall, Idaho, on the 18th of September. After completing the survey of the Yellow Stone Valley, the party left Fort Ellis on the 5th of September, passing down Gallatin Valley to the Three Forks, and thence by the Jefferson to its very source, exploring many of its branches, and pursuing a direction nearly parallel to that which the party had traversed in the June previous. The valleys of the Gallatin, Madison, and Jefferson forks of the Missouri, with all the little branches, were found occupied by industrious farmers and miners—a contrast quite striking to the doctor, who, twelve years ago, in exploring that same region, met with not a single white inhabitant. The Rocky Mountain Divide was crossed at the Horse Plain Creek, from which the party passed over into Medicine Lodge Creek, following this down into the Snake River Plain. An interesting fact observed was the occurrence of two species of trout in great quantities in streams such as Medicine Lodge, Camas, and other creeks all sinking into the plains after a course of from fifty to seventy-five miles. The trout appeared to be of the same two species in all, although the waters had no apparent connection. The party expected to leave Fort Hall, and to proceed to Fort Bridger by way of Soda Springs, Bear Lake, and Evanston, and there to disband the scientific corps returning to the East.

IN a very important paper on the "Estimation of Antimony," published in the *Chemical News*, Hugo Tamm calls the attention of chemists to a new phenomenon which the author describes under the name of "Hygraffinity." This phenomenon was discovered in a peculiar compound of antimony—bigallate of antimony. This compound is totally insoluble in water, and yet it possesses a powerful affinity for moisture, which it absorbs rapidly from the air after being dried at the temperature of 100° Cent. Most powders and precipitates, as it is well known, dried at that temperature, absorb moisture on exposure to the atmosphere, but this is a purely physical phenomenon due to porosity. On the contrary, in the case of gallate of antimony, chemical affinity is at work, and this precipitate, after exposure to the air for two or three hours, actually absorbs two equivalents of water. In a word, this insoluble substance has as much affinity for moisture as deliquescent salts. But one of the most curious features in connection with this extraordinary phenomenon is that on being dried at 100° Cent., bigallate of antimony loses the two equivalents of water which it had absorbed from the air, and that on being left exposed once more to the atmosphere, it reabsorbs the same amount of moisture. This interesting experiment may be repeated indefinitely.

IN the *Comptes Rendus* for August and in the *Philosophical Magazine*, M. Angström gives an analysis of the spectra which are observed in connection with hydrogen, and criticises the conclusions of M. Wüllner "that hydrogen has no less than four and oxygen no less than three distinct spectra." He explains that the spectrum lines of hydrogen (as observed by Plücker in rare hydrogen) spread out in disruptive discharges when the tension of the gas is increasing, and end by uniting so as to form a continuous spectrum. With regard to M. Wüllner's second spectrum of hydrogen, he points out that it is no other than the spectrum observed by M. Berthelot and ascribed by him to *acetylene*. Also, by a comparison of wave-lengths for sulphur and for M. Wüllner's third hydrogen-spectrum, he shows this to be in all probability the spectrum of sulphur. M. Angström also points out the close agreement between one of the oxygen spectra of M. Wüllner and the spectrum of oxide of carbon, and his tables show also a very close agreement between another of these oxygen spectra and the spectrum of chlorine, and concludes that neither oxygen nor hydrogen has more than one spectrum.

PROF. YOUNG has communicated to the *Philosophical Magazine* a catalogue of more than a hundred bright lines in the spectrum of the chromosphere, in which the observed lines are referred to the scales of Kirchhoff's and of Angström's maps. Of the seventy new lines which are given in this list, there are two which are proved to belong to the chromosphere, and not to be due to the exceptional elevation of matter to heights where it does not properly belong. No less than twenty of these lines are due to the metal titanium, and show the presence of titanium vapour in the prominences and chromosphere.

THE cultivation of beet-root sugar in France has now risen to an industry of the first importance. It employs more than 400 manufactories, and the process of manufacture is each year brought to a higher state of perfection. There are in France three or four journals specially devoted to subjects connected with the manufacture, its cultivation, its sale, the machinery required, the chemistry of the process, &c.

THE Fourth Annual Report is published of the Trustees of the Peabody Museum of American Archæology and Ethnology at Cambridge, U.S.A. Two important series of explorations have been carried out in the course of the past year on behalf of the Museum, by the Rev. F. O. Dunning in Eastern Tennessee, and by Dr. Berendt in Central America, resulting in valuable acquisitions to its collections. The Museum has also been enriched during the year by the gift of the "Charles Hammond Collection" from the towns of Chatham and Rochester, Cape Cod, and by a very valuable series of about 125 objects from the conservator of the Christy collection in London, consisting of original specimens and casts from Les Eyzies, La Madelaine, and Le Moustier, in the department of Dordogne, France. The Report is accompanied by a set of comparative measurements of crania from Peru, presented by Mr. Squier, of those from the mounds of Kentucky obtained by Mr. Lyon, and from the mounds of Florida.

THE Annual *Conversazione* of the Royal Society of Victoria was held on August 14, when the president, Mr. R. L. J. Ellery, delivered an address, in which he referred especially to the scientific results of the eclipse of last winter, and the preparations making in Australia for observing the eclipse of next month, to Prof. Heis's observations on the correspondence of auroral phenomena in the southern and northern hemispheres, to Dr. von Mueller's botanical researches in the colony, to the very important subject, economically, to the colony of the preservation of meat, and to Prof. Tyndall's germ theory of disease.

THE Report is published of the Annual Meeting of the Academy of Sciences of Vienna, held on the 30th of May, 1871, containing a review of the proceedings of the various departments of the Academy during the past year. The Academy has also issued its "Almanack," with list of home, foreign, and honorary members.

A SUPPLEMENT to the Sixth and Seventh Annual Report of the "Verein für Erdkunde" at Dresden, by D. Abendroth, contains a very interesting series of maps, illustrating the extent of geographical knowledge of the world possessed at different periods from A.D. 1350 to 1566.

A WORK has come out in Holland which particularly interests those who are engaged in the treatment of sewage manure. It is by M. J. A. C. Eschauzler, and gives all the results of the centuries of experience in the Netherlands. It is copiously illustrated.

WE are informed that the German translation of Tylor's "Primitive Culture" is not by Dr. Spengel, but conjointly by Herr Spengel and Herr Poske.

A NEW class for civil engineering has been formed in the Presidency College, Calcutta.

THE Madras Government has allowed 200*l*. for the expense of bringing the Assistant Government Astronomers to England to learn celestial photography.

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

II.

THE characteristic examples already given of symmetrical and asymmetrical envelopment are cited from a great number of others which might have been mentioned. Very many of these are by the pseudomorphists regarded as results of partial alteration. Thus, in the case of associated crystals of andalusite and cyanite, Bischof does not hesitate to maintain the derivation of andalusite from the latter species by an elimination of quartz; more than this, as the andalusite in question occurs in a granite-like rock, he suggests that itself is a product of the alteration of orthoclase. In like manner the mica, which in some cases coats tourmaline, and in others fills hollow prisms of this mineral, is supposed to result from a subsequent alteration of crystallised tourmaline. So in the case of shells of leucite filled with feldspar, or of garnet enclosing epidote or chlorite or quartz, a similar transformation of the interior is supposed to have been mysteriously effected, while the external portion of the crystal remains intact. Again the aggregates of tinstone, quartz and orthoclase having the form of the latter, are, by Bischof and his school, looked upon as results of a partial alteration of previously formed orthoclase crystals. It needed only to extend this view to the crystals of calcite enclosing sand-grains, and regard these as the result of a partial alteration of the carbonate of lime. There is absolutely no proof that these hard crystalline substances can undergo the changes supposed, or can be absorbed and modified like the tissues of a living organism. It may, moreover, be confidently affirmed that the obvious facts of envelopment are adequate to explain all the cases of association upon which this hypothesis of pseudomorphism by alteration has been based. Why the change should extend to some parts of a crystal and not to others, why in some cases the exterior of the crystal is altered, while in others the centre alone is removed and replaced by a different material, are questions which the advocates of this fanciful hypothesis have not explained. As taught by Blum and Bischof, however, these views of the alteration of mineral species have not only been generally accepted, but have formed the basis of the generally received theory of rock-metamorphism.

Protests against the views of this school have, however, not been wanting. Scheerer, in 1846, in his researches in Polymeric Isomorphism,† attempted to show that iolite and aspasiolite, a hydrous species which had been looked upon as resulting from its alteration, were isomorphous species crystallising together, and, in like manner, that the association of olivine and serpentine in the same crystal, at Snarum in Norway, was a case of envelopment of two isomorphous species. In both of these instances he maintained the existence of isomorphous relations between silicates in which 3HO replaced MgO. He hence rejected the view of Gustav Rose that these serpentine crystals were results of the alteration of olivine, and supported his own by reasons drawn from the conditions in which the crystals occur. In 1853 I took up this question, and endeavoured to show that these cases of isomorphism described by Scheerer entered into a more general law of isomorphism pointed out by me among homologous compounds differing in their formulas by nM_2O_2 (M = hydrogen or a metal). I insisted, moreover, on its bearing upon the received views of the alteration of minerals, and remarked, "The generally admitted notions of pseudomorphism seem to have originated in a too exclusive plutonism, and require such varied hypotheses to explain the different cases, that we are led to seek for some more simple explanation, and to find it, in many instances, in the association and crystallising together of homologous and isomorphous species."‡ Subsequently, in 1860, I combated the view of Bischof, adopted by Dana, that "regional metamorphism is pseudomorphism on a grand scale," in the following terms:—

"The ingenious speculations of Bischof and others, on the possible alteration of mineral species by the action of various saline and alkaline solutions, may pass for what they are worth, although we are satisfied that by far the greater part of the so-called cases of pseudomorphism in silicates are purely imaginary, and, when real, are but local and accidental phenomena. Bischof's notion of the pseudomorphism of silicates like feldspars and py-

* 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."

† Pogg. Annal., lxviii. 319.

‡ Ibid.

roxenes, presupposes the existence of crystalline rocks, whose generation this neptunist never attempts to explain, but takes his starting-point from a plutonic basis."

I then asserted that the problem to be solved in regional metamorphism is the conversion of sedimentary strata, "derived by chemical and mechanical agencies from the ocean waters and pre-existing crystalline rocks into aggregations of crystalline silicates. These metamorphic rocks, once formed, are liable to alteration only by local and superficial agencies, and are not, like the tissues of a living organism, subject to incessant transformations, the pseudomorphism of Bischof."*

I had not, at that time, seen the essay by Delesse on pseudomorphs already referred to, published in 1859, in which he maintained views similar to those set forth by me in 1853 and 1860, declaring that much of what had been regarded as pseudomorphism had no other basis than the observed associations of minerals, and that often "the so-called metamorphism finds its natural explanation in envelopment." These views he ably and ingeniously defended by a careful discussion of the whole range of facts belonging to the history of the subject.

My own expression of opinion on this question, in 1853, had been privately criticised, and I had been charged with a want of comprehension of the question. It was, therefore, with no small pleasure, that I not only saw my views so ably supported by Delesse, but read the language of Carl Friedrich Naumann, who in 1861 wrote to Delesse as follows, referring to his essay just noticed :—

"You have rendered a veritable service to science in restricting pseudomorphs to their true limits, and separating what had been erroneously united to them. As you have remarked, envelopments have, for the most part, nothing in common with pseudomorphs, and it is inconceivable that they have been united by so many mineralogists and geologists. It appears to me, moreover, that they commit an analogous error when they regard gneisses, amphibolites, &c., as being, all of them, the results of metamorphic epigenesis, and not original rocks. It is precisely because pseudomorphism has been so often confounded with metamorphism that this error has found acceptance. I only admit a pseudomorph where there is some crystal the form of which has been preserved. There are very many metamorphic substances which are, in no sense of the word, pseudomorphs. Had the name of crystalloid been chosen instead of pseudomorph, this confusion would certainly have never found its way into the science. I think, with you, that the envelopment of two minerals is most generally explained by a contemporaneous and original crystallisation. Secondary envelopments, however, exist, and such may be called pseudomorphs or crystalloids, if they reproduce exactly the form of the crystal enveloped, whether this last still remains, or has entirely disappeared."†

It is unnecessary to remark that the view of Delesse and Naumann—viz. : that the so-called cases of pseudomorphism, on which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallisation—is identical with the view suggested by Scheerer, and generalised by myself long before, when, in 1853, I sought to explain the phenomena in question by "the association and crystallising together of homologous and isomorphous species."

Later in 1862, I wrote as follows :—

"Pseudomorphism, which is the change of one mineral species into another, by the introduction or the elimination of some element or elements, presupposes metamorphism (*i.e.*, metamorphic or crystalline rocks), since only definite mineral species can be the subjects of this process. To confound metamorphism with pseudomorphism, as Bischof and others after him have done, is therefore an error. It may be further remarked, that, although certain pseudomorphic changes may take place in some mineral species, in veins and near the surface, the alteration of great masses of silicated rocks by such a process is as yet an unproved hypothesis."‡

Thus this unproved theory of pseudomorphism, as taught by Bischof, does not, even if admitted to its fullest extent, advance us a single step toward a solution of the problem of the origin of the various silicates, which, singly or intermingled, make up beds in the crystalline schists. Granting, for the sake of argument, that serpentine results from the alteration of olivine or labradorite, and steatite or chlorite from hornblende, the origin of these anhydrous silicates, which are the subjects of the supposed change, is still unaccounted for. The explanation of this shortsightedness is not far to seek ; as already remarked, Bischof, although a professed neptunist, starts from a plutonic basis. When the epigenic origin of serpentine and its related rocks was first taught, these were regarded as eruptive and unstratified, and it was easy to imagine intruded masses of dioritic and feldspathic rocks, which had become the subjects of alteration. As, however, the progress of careful investigation in the field has shown the stratified character of these serpentines, diallage-rocks, steatites, &c., and their intercalation among limestones, argillites, quartzites, gneisses, and mica-schists, and even among feldspathic and hornblendic strata, we are forced to reject, with Naumann, the notion of their epigenic derivation, and to regard them as original rocks.

This view brings us face to face with the problem of metamorphism as defined by me in 1860* (see *ante*). We must either admit that these crystalline schists were created as we find them, or suppose that they were once sands, clays, marls, &c. ; in a word, sediments of chemical and mechanical origin, which by a subsequent process have been consolidated and crystallised. Whence, then, come these silicates of magnesia, lime, and iron, which are the sources of serpentine, hornblende, steatite, chlorite, &c.? This is the question which I proposed in that same year, when, after discussing the results of my examinations of the tertiary rocks near Paris containing layers of a hydrous silicate of magnesia related to talc in composition, among unaltered limestones and clays, I remarked that it is evident "such silicates may be formed in basins at the earth's surface, by reactions between magnesian solutions and dissolved silica ;" and, after some further discussion, said, "further inquiries in this direction may show to what extent certain rocks composed of calcareous and magnesian silicates may be directly formed in the moist way."† Subsequently, in a paper on "The Origin of some Magnesian and Aluminous Rocks," printed in the "Canadian Naturalist" for June 1860,‡ I repeated these considerations, referring to the well-known fact that silicates of lime, magnesia, and iron-oxyd are deposited during the evaporation of natural waters, including those of alkaline springs and of the Ottawa River. Having described the mode of occurrence of the magnesian silicate sepiolite, in the Paris basin, and the related quincite, containing some iron-oxyd and disseminated in limestone, I suggested that while steatite has been derived from a compound like sepiolite, the source of serpentine was to be sought in another silicate richer in magnesia ; and, moreover, that chlorite, unless the result of a subsequent reaction between clay and carbonate of magnesia, was directly formed by a process analogous to that which (according to Scheerer) has, in recent times, caused the deposition from waters of neolite, a hydrous alumino-magnesian silicate, approaching to chlorite in composition,§ "the type of a reaction which formerly generated beds of chlorite in the same way as those of sepiolite or talc." Delesse, subsequently, in 1861, in his essay on Rock-Metamorphism, insisted upon the sepiolites or so called magnesian marls, as probably the source of steatite, and suggested the derivation of serpentine, chlorite, and other related minerals of the crystalline schists, from deposits approaching these marls in composition.‖ He recalled, also, the occurrence of chromic oxyd, a frequent accompaniment of these magnesian minerals, in the hydrated iron ores of the same geological horizon with the magnesian marls in France. Delesse did not, however, attempt to account for the origin of these deposits of magnesian marls, in explanation of which I afterwards verified Bischof's observations on the sparing solubility of silicate of magnesia, and showed that silicate of soda, or even artificial hydrated silicate of lime, when added to waters containing magnesian chlorid or sulphate, gives rise, by double decomposition, to a very insoluble magnesian silicate.¶

To explain the generation of silicates like labradorite, scapolite, garnite, and saussurite, I suggested that double aluminous silicates allied to the zeolites might have been formed, and subsequently rendered anhydrous. The production of zeolitic minerals observed by Daubrée at Plombières and Luxeuil by the action of a silicated alkaline water on the masonry of ancient Roman baths, was appealed to by way of illustration. It had

* Amer. Jour. Sci., II. xxx. 135.
† Bull. Soc. Geol. de France, II. xviii. 678.
‡ Descriptive Catalogue, Crystalline Rocks of Canada, p. 80, London Exhibition, 1862 ; also Dublin Quar. Jour., July 1863, and Amer. Jour. Sci., II. xxxvi. 218.

* Amer. Jour. Sci., II. xxx. 135.
† Ibid., II. xxix. 284 ; also II. xl. 49.
‡ Ibid., II. xxxii. 286.
§ Pog. Annal., lxxi. 288.
‖ Etudes sur le Metamorphisme, 4to, pp. 91. Paris, 1861.
¶ Amer. Jour. Sci., II. xl. 49.

there been shown by Daubrée that the elements of the zeolites had been derived in part from the waters, and in part from the mortar, and even the clay of the bricks, which had been attacked, and had entered into combination with the soluble matters of the water to form chabazite. I, however, at the same time pointed out another source of silicated minerals, upon which I had insisted since 1857, viz., the reaction between silicious or argillaceous matters and earthy carbonates in the presence of alkaline solutions. Numerous experiments showed that when solutions of an alkaline carbonate were heated with a mixture of silica and carbonate of magnesia, the alkaline silicate formed acted upon the latter, yielding a silicate of magnesia, and regenerating the alkaline carbonate; which, without entering into permanent combination, was the medium through which the union of the silica and the magnesia was effected. In this way I endeavoured to explain the alteration, in the vicinity of a great intrusive mass of dolerite, of a gray Silurian limestone, which contained, besides a little carbonate of magnesia and iron-oxyd, a portion of very silicious matter, consisting apparently of comminuted orthoclase and quartz. In place of this, there had been developed in the limestone, near its contact with the dolerite, an amorphous greenish basic silicate, which had seemingly resulted from the union of the silica and alumina with the iron-oxyd, the magnesia, and a portion of lime. By the crystallisation of the products thus generated it was conceived that minerals like hornblende, garnet, and epidote might be developed in earthy sediments, and many cases of local alteration explained. Inasmuch as the reaction described required the intervention of alkaline solutions, rocks from which these were excluded would escape change, although the other conditions might not be wanting. The natural associations of minerals, moreover, led me to suggest that alkaline solutions might favour the crystallisation of aluminous silicates, and thus convert mechanical sediments into gneisses and mica-schists. The ingenious experiments of Daubrée on the part which solutions of alkaline silicates, at elevated temperatures, may play in the formation of crystallised minerals, such as feldspar and pyroxene, were posterior to my early publications on the subject, and fully justified the importance which, early in 1857, I attributed to the intervention of alkaline silicates in the formation of crystalline silicated minerals.*

While, however, there is good reason to believe that solutions of alkaline silicates or carbonates have been efficient agents in the crystallisation and molecular re-arrangement of ancient sediments, and have also played an important part in the local alteration of sedimentary strata which is often observed in the vicinity of intrusive rocks, it is clear to me that the agency of these solutions is less universal than was once supposed by Daubrée and myself, and will not account for the formation of various silicated rocks found among crystalline schists, such as serpentine, hornblende, steatite, and chlorite. When I commenced the study of these crystalline strata, I was led, in accordance with the almost universally received opinion of geologists, to regard them as resulting from a subsequent alteration of palæozoic sediments, which, according to different authorities, were of Cambrian, Silurian, or Devonian age. Thus in the Appalachian region, as we have already seen, they have, on supposed stratigraphical evidence, been successively placed at the base, at the summit, and in the middle of the Lower Silurian or Champlain division of the New York system. A careful chemical examination among the unaltered palæozoic sediments, which in Canada were looked upon as the stratigraphical equivalents of the bands of magnesian silicates in these crystalline schists, showed me, however, no magnesian rocks except certain silicious and ferruginous dolomites. From a consideration of reactions which I had observed to take place in such admixtures in presence of heated alkaline solutions, and from the composition of the basic silicates which I had found to be formed in silicious limestones near their contact with eruptive rocks, I was led to suppose that similar actions, on a grand scale, might transform these silicious dolomites of the unaltered strata into crystalline magnesian silicates.

Further researches, however, convinced me that this view was inapplicable to the crystalline schists of the Appalachians; since, apart from the geognostical considerations set forth in the previous part of this paper, I found that these same crystalline strata hold beds of quartzose dolomite and magnesian carbonate, associated in such intimate relations with beds of serpentine, diallage, and steatite, as to forbid the notion that these silicates could have been generated by any transformations or chemical re-arrangement of mixtures like the accompanying beds of quartzose magnesian carbonates. Hence it was that already, in 1860, as shown above, I announced my conclusion that serpentine, chlorite, and steatite had been derived from silicates like sepiolite, directly formed in waters at the earth's surface, and that the crystalline schists had resulted from the consolidation of previously formed sediments, partly chemical and partly mechanical in their origin. The latter being chiefly silico-aluminous, took, in part, the forms of gneiss and mica-schists, while from the more argillaceous strata, poorer in alkali, much of the aluminous silicate crystallised as andalusite, staurolite, cyanite, and garnet. These views were reiterated in 1863,* and further in 1864, in the following language, as regards the chemically-formed sediments: "steatite, serpentine, pyroxene, hornblende, and in many cases, garnet, epidote, and other silicated minerals are formed by a crystallisation and molecular re-arrangement of silicates generated by chemical processes in waters at the earth's surface."† Their alteration and crystallisation were compared to that of the mechanically formed feldspathic, silicious, and argillaceous sediments just mentioned.

(*To be continued.*)

* Proc Roy. Soc., May 7, 1857. Amer. Jour. Sci., II. xxiii. 438, and xxv. 289 and 435.

THE RELATIONS BETWEEN ZOOLOGY AND PALÆONTOLOGY‡

MY distinguished predecessor, the late Prof. E. Forbes, appears to have been the first who undertook the systematic study of marine zoology with reference to the distribution of marine animals in space and in time. After making himself well acquainted with the fauna of the British seas to the depth of about 200 fathoms by dredging, and by enlisting the active co-operation of many friends, among whom we find MacAndrew, Barlee, Gwyn Jeffreys, William Thompson, and many others, entering enthusiastically into the new field of natural history inquiry; in the year 1841, Forbes joined Captain Graves, who was at that time in command of the Mediterranean Survey as naturalist. During about eighteen months he studied with the utmost care the conditions of the Ægean and its shores, and conducted upwards of 100 dredging operations at depths varying from 1 to 130 fathoms. In 1843 he communicated to the Cork meeting of the British Association an elaborate report on the mollusca and radiata of the Ægean Sea, and on their distribution as bearing on geology. Three years later, in 1846, he published in the first volume of the "Memoirs of the Geological Survey of Great Britain," a most valuable memoir upon the connection between the existing Fauna and Flora of the British Isles and the geological changes which have affected their area, especially during the epoch of the northern drift. In the year 1859 appeared the "Natural History of the European Seas," by the late Prof. Edward Forbes, edited and continued by Robert Godwin-Austen. In the first hundred pages of this little book Forbes gives a general outline of some of the more important of his views with regard to the distribution of marine forms. The remainder of the book is a continuation by his friend Mr. Godwin-Austen, for before it was finished an early death had cut short the career of the most accomplished and original naturalist of his time. I will give a brief sketch of the general result to which Forbes was led by his labours, and I shall have to point out that, although we are now inclined to look somewhat differently on certain very fundamental points, and, although recent investigations with better appliances and more extended experience have invalidated many of his conclusions, to Forbes is due the credit of having been the first to treat these questions in a broad philosophical sense, and to point out that the only means of acquiring a true knowledge of the *rationale* of the distribution of our present fauna is to make ourselves acquainted with its history, to connect the present with the past. This is the direction which must be taken by future inquiry:—Forbes as a pioneer in this line of research was scarcely in a position to appreciate the full value of his work. Every year adds enormously to our stock of data, and every new fact indicates more and more clearly the brilliant results which are to be obtained by following his methods, and by emulating his enthusiasm and his indefatigable industry. Forbes believed implicitly, along with nearly all the leading naturalists of his time, in the immutability

* Geol. of Canada, pp. 577—581.
† Amer. Jour. Sci., II. xxxvii. 266, and xxxviii. 183.
‡ Abstract of Opening Lecture on Natural History delivered at the University of Edinburgh, Nov. 2, by Prof. Wyville Thomson, F.R.S.

of species. He says:—"Every true species presents in its individuals certain features, specific characters, which distinguish it from every other species: as if the Creator had set an exclusive mark or seal on each type." He likewise believed in specific centres of distribution. He held that all the individuals composing a species had descended from a single progenitor, or from two, according as the sexes might be united or distinct, and that, consequently, the idea of a species involved the idea of the relationship in all the individuals of common descent; and the converse, that there could by no possibility be community of descent except in living beings which possessed the same specific characters. He supposed that the original individual or pair was created at a particular spot where the conditions were suitable for its existence and propagation, and that the species extended and migrated from that spot on all sides, over an area of greater or less extent, until it met with some natural barrier in the shape of unsuitable conditions. No specific form could have more than a single centre of distribution. If its area appeared to be broken up, a patch not in connection with the original centre of distribution occurring in some distant locality, it was accounted for by the formation, through some geological change, after the first spread of the species, of a barrier which cut off part of its area, or by some accidental transport to a place where the conditions were sufficiently similar to those of its original habitat to enable it to become naturalised. No species once exterminated was ever re-created, so that in those few cases in which we find a species abundant at one period over an area, absent over the same area for a time, and recurring at a later period, it must be accounted for by a change in the conditions of the area which forced the emigration of the species, and a subsequent further change which permitted its return. Forbes defined and advocated what he called the law of "representation." He found that in all parts of the world, however far removed, and however completely separated by natural barriers, where the conditions of life are similar, species, and groups of species, occur, which, although not identical, resemble one another very closely; and he found that this similarity existed likewise between groups of fossil remains and between groups of fossils and groups of recent forms. Admitting the constancy of specific characters, these resemblances could not be accounted for by community of descent, and he thus arrived at the generalisation that in localities placed under similar circumstances, similar, though specifically distinct, specific forms were created. These he regarded as mutually representative species. Our acceptance of the doctrines of "specific centres" and of "representation," or at all events the form in which we may be inclined to accept them, depends greatly upon the acceptance or rejection of the fundamental dogma of the immutability of species, and on this point there has been a very great change of opinion within the last ten or twelve years—a change certainly due to the remarkable ability and candour with which the question has been discussed by Mr. Darwin and Mr. Wallace. I do not think that I am speaking too strongly when I say that there is now scarcely a single competent general naturalist who is not prepared to accept some form of the doctrine of evolution. There are no doubt very great difficulties in the minds of many of us in conceiving that, commencing from the simplest living being, the present state of things in the organic world has been produced solely by the combined action of "atavism," the tendency of offspring to resemble their parents closely, and "variation," the tendency of offspring to differ individually from their parents within very narrow limits; and many are inclined to believe that some law, as yet undiscovered, other than the "survival of the fittest" must regulate the existing marvellous system of extreme and yet harmonious modification. Still, it must be admitted that variation is a *vera causa*, probably capable, within a limited period, under favourable circumstances, of converting one species into what, according to our present ideas, we should be forced to recognise as a different species; and such being the case, it is perhaps conceivable that during the lapse of a period of time—still infinitely shorter than eternity—variation may have produced the entire result. The individuals composing a species have a definite range of variation strictly limited by the circumstances under which the group of individuals is placed. Except in man and in domesticated animals, in which it is artificially increased, this individual variation is usually so slight as to be inappreciable except to a practised eye; and any extreme variation which passes the natural limit in any direction clashes in some way with surrounding circumstances, and is dangerous to the life of the individual. The normal or graphic line, or "line of safety," of the species, lies midway between the extremes of variation. If at any period in the history of a species, the conditions of life of a group of individuals of the species are gradually altered; with the gradual change of circumstances the limit of variation is contracted in one direction and relaxed in another, it becomes more dangerous to diverge towards one side, and more desirable to diverge towards the other, and the position of the lines limiting variation is altered. The normal line, the line along which the specific characters are most strongly marked, is consequently slightly deflected, some characters being more strongly expressed at the expense of others. This deflection, carried on for ages in the same direction, must eventually carry the divergence of the varying race far beyond any limits within which we are in the habit of admitting identity of species. But the process must be, so to speak, infinitely slow. It is difficult to form any idea of ten, fifty, or a hundred millions of years; or of the relation which such periods bear to changes taking place in the organic world. We must remember, however, that the rocks of the Silurian system, overlaid by ten miles thickness of sediment, entombing a hundred successive faunæ, each as rich and varied as that of the present day, are themselves teeming with fossils fully representing all the existing classes of animals except the very highest. If it is possible to imagine that this marvellous manifestation of eternal power and wisdom involved in living nature can have been worked out through the law of "descent with modification" alone, we shall certainly require from the physicists the very longest row of cyphers which they can afford. Now, although the admission of a doctrine of evolution must affect greatly our conception of the origin and *rationale* of so-called specific centres, it does not practically affect the question of their existence, or of the laws regulating the distribution of species from these centres by migration, by transport, by ocean currents, by elevations or depressions of the land, or by any other causes at work under existing circumstances. So far as practical naturalists are concerned, species are permanent within their narrow limits of variation, and it would introduce an element of infinite confusion and error if we were to regard them in any other light. The origin of species by "descent with modification" is as yet only a hypothesis. During the whole period of recorded human observation, not one single instance of the change of one species into another has been detected, and, singular to say, in successive geological formations, although new species are constantly appearing, and there is abundant evidence of progressive change, no single case has as yet been observed of one species passing through a series of inappreciable modifications into another.

ON THE OBJECTS AND MANAGEMENT OF PROVINCIAL MUSEUMS *

ALTHOUGH every intelligent person knows more or less what these institutions are, and what they ought to be, there is probably no subject, connected with the modern means of education in natural science, concerning which so much misconception or ignorance is manifested and tolerated as in the Management and Objects of our Provincial Museums. The majority of them throughout England present such examples of helpless misdirection and incapacity as could not be paralleled elsewhere in Europe. Some noteworthy exceptions there are. But generally the managers or guardians of local museums are precisely of this unfit class, and seem to have no more notion of their charge than as mere curiosity-shops, and even display less intelligence than is shown in such shops, where the cupidity or shrewdness of the dealer induces him at least to take due care of, and give a local habitation and a name to, his wares. But in the provincial museums even this care and tittle of information is pertinaciously withheld, and the visitors are left to do the best they can amid the surrounding bewilderment. This is commonly made up of a most puzzling jumble of heterogeneous miscellanies, arranged, or rather scattered, with an equally sovereign contempt for the convenience or instruction of the public, and indeed all in such admired disorder as may most plainly show how Chaos is come again and Confusion can make his masterpiece, and how every specimen added to the heap only tends to increase or perpetuate the miserable derangement. It looks as

* Abstract of an Address to a Meeting of the East Kent Natural History Society, at Canterbury, Oct. 12, 1871, by its Vice-President and Honorary Secretary, George Gulliver, F.R.S.

if the presiding local genius had set his wits to work in order to prove how much time and money might be most effectually expended with the least profit to a knowledge of the natural history, or any history, of the neighbourhood; and indeed for exemplifications of the solution of this knotty point we have too commonly only to appeal to the museum of the place. Instead of methodical illustrations of the natural history and antiquities of the district, we are likely to find a few good things overlaid by such a rabble-rout, such a multifarious and disorderly medley of outlandish and queer odds and ends, as are rather fitted for a laughing-stock than a sober exposition of science. Thus we are met at once in the hall and saloons by such incongruous lots as effigies of double women, elephants' teeth, nose-rings, brain-stones, tomahawks, stuffed alligators, moccasins, New Zealanders' heads, cockatoos, canoes, Babylonish bricks, cocoa nuts, boas, javelins, lions and tigers, calumets, matchlocks, palm-branches, shields, monkey-stones, sugar-canes, Roman cement, Oliver Cromwell's watches, Panama hats, fabricated elephants, walking-stick insects, and numberless other eccentric things of this motley and confounded order. The garniture of Romeo's apothecary's shop, or the countryman's museum on the barn door, would be more instructive or intelligible and less ridiculous or perplexing.

It might be painful or appear invidious to inquire minutely by what means or under whose misconduct so many provincial museums have sunk into their present disgraceful confusion and uselessness; especially as it is little creditable to the intelligence of that community under the tolerance or approval of which this reproachful state of things exists. If the fault be attributed to the apathy or something worse among the majority of the ratepayers, it is one that the friends of popular government should hasten to correct. However this may be, it is enough for us to know that this notorious evil has increased, is increasing, and ought to be diminished; it will otherwise remain a foul blot on and a costly nuisance to the places under such unprofitable infliction. Hence every naturalist and antiquarian, every intelligent and honest member of the community, should be ready to lend his hand cordially to the good work of reform in this direction; more especially as soon as the truth is realised that the difficulty is by no means insuperable, but may be easily removed, is a consummation devoutly to be wished, and would involve no addition to the customary and regular expense. The remedies are sufficiently obvious, and to point out how they should be used, after having described the disorder and the necessity for them, is the object of the present observations. To this end we have in the first place to consider what is desirable and practicable. To instruct ourselves and the rising generation, by means of local museums, in the elements of natural history generally, and in the local examples of it particularly, is obviously both practicable and desirable. For the first purpose, when indigenous specimens are wanting we must get exotic ones; and these should be limited to such typical examples only as are absolutely necessary for the elucidation of fundamental or comprehensive facts; for which purpose anatomical preparations, whether botanical or zoological, are chiefly, but not exclusively, to be esteemed. On the other hand, all and every species belonging to the district should be preserved and displayed so far as they admit it; partly for the knowledge they display of the science, but principally for the information they afford of the natural history of the locality. Antiquarian objects should be treated in a similar spirit. Thus would be collected at one view, or at least under one roof, much of that important knowledge which is within the means and scope of any country museum, so that every visitor to it might easily find therein both pleasure and profit in natural science in general and in the natural features of the locality in particular. The museum would then also be in a condition to fulfil one of its leading offices, as a centre for the meetings, lectures, and conversations on the natural history and antiquities of the district, and in this mode be available for contributions in furtherance of the special objects of local societies, and likely thus to add to the general stock of knowledge. And happily, this is now being regularly ventilated and popularised in such useful publications as the *Zoologist*, the *Field*, and *Land and Water*. When will the *Times* discover the fair and fertile field of instruction in the Provincial Museums, now lying waste for want of culture? NATURE, in a recent notice of certain donations to the Ludlow Museum, has shown a judicious sense of the subject.

But how are you to get the desirable specimens, and what are you to do with them? Most of those wildernesses miscalled Museums already possess a large quantity of objects only awaiting and inviting intelligent attention. This will consist in a careful preparation, display, and description of them. After having been separately grouped under their respective kingdoms—the mineral, vegetable, and animal—they must be arranged according to the method of their natural relations, in their respective classes, orders, families, genera, and species; then accurately numbered, ticketed, and catalogued. Thus the otherwise chaotic mass of particular facts will fall into an orderly method, and be always ready to convey an accurate knowledge to visitors. Still further illustrations will be requisite, especially as regards fundamental and comprehensive phenomena, by preparations to display the essential characters at least of the classes and orders, and of the anatomy and physiology of the members thereof; and one or two careful dissections will be commonly sufficient for this purpose in each order. And now will arise the question, Who is to do all this work? Certainly neither by nor under the direction of "incorporations" of aldermen quite incapable of it can we expect any effectual labour of the kind. But with proper encouragement students of the different departments will, from a pure love of the subjects, not only be found to perform all this but probably more, and without the least expectation of any pecuniary reward. They will surely add important preparations and other objects to the collection, whenever it becomes manifest that such contributions will be duly appreciated and cared for; indeed, with regard to at least one Museum very zealous and skilful naturalists have only been prevented from giving such desirable aid by a knowledge that their work would simply be "missing," smothered, or destroyed, amid the carelessness and the maze of misplaced rubbish there undergoing a like fate, and most significantly and effectually warning them, and others like them, what they have to expect. Fortunately minerals and antiquities are commonly less perishable.

Having discussed what is desirable and practicable, we come to that which is neither one nor the other. And having somewhat irreverently adverted to the rubbish of so many Provincial Museums, a further explanation may be necessary, and the more so as this very accumulation of jumbled and useless materials is the sad *bête noire* of these collections, and so vigilantly intrusive as to force admission and predominance against all reasons of fitness or utility. Any disorderly materials when hurtful by being out of place fall into the character of rubbish, just as any plant is a weed when encroaching injuriously on the legitimate crop. In their proper place they may be very valuable; such they might be in the great general collection of the British Museum, or in a botanical garden. But nobody in his senses can suppose that it is either desirable or practicable for a provincial society to attempt an imitation of the vast and boundless metropolitan institution. This would be simply out of the question, and calculated only to provoke a smile, except peradventure among the guardians of the local museums. Indeed, with all the excellent arrangement, the army of properly paid experts, and immense space and appliances, the British Museum has become so crowded and unwieldy, especially for reference and use concerning British products, that some steps for an extrication of them from the surrounding masses of exotic things has become necessary. But the guardians of the Provincial Museum will reasonably ask, Granting that we have so much rubbish, what are we to do with it? Sell it if you can, or give it away; but by all means get rid of it, and that swiftly; to which end a bonfire might be the best thing. And having thus learned by experience the noxiousness of such rubbish, most resolutely and remorselessly refuse any quarter to it in future. At present this sort of lumber only occupies space and involves expense that might and ought to be employed for more useful and legitimate purposes; and how and why has already been mentioned. At the execution of the sentence many a wailing throe will out, some natural tears be shed, for the o'erfraught heart will speak. The very civil and complacent local genius will meekly plead for his idols, telling you how he loves them, and how some other equally wise and more potent individuals hold the same faith; and above all that the visitors to his temple have ever regarded all those very things with an admiration and delight amounting to veneration. He will refuse to be comforted by your assurance that what he says is no doubt very true, though Punch and Judy and Madame Tussaud may be almost as delightful if not quite as good in their way; but that your way is to show how the Provincial Museum may be made not to suppress or degrade but to develope and elevate the taste of the multitude; and that after all a good museum will sooner or later become more popular than a bad one.

THE SCOTTISH SCHOOL OF GEOLOGY*

I.

FOR the first time in the history of University Education in Scotland, we are to-day met to begin the duties of a Chair specially devoted to the cultivation of Geology and Mineralogy. Though Science is of no country nor kin, it yet bears some branches which take their hue largely from the region whence they sprang, or where they have been most closely followed. Such local colourings need not be deprecated, since they are both inevitable and useful. They serve to bring out the peculiarities of each climate, or land, or people, and it is the blending of all these colourings which finally gives the common neutral tint of science. This is in a marked degree true of Geology. Each country where any part of the science has been more particularly studied, has given its local names to the general nomenclature, and its rocks have sometimes served as types from which the rocks of other regions have been classified and described. The very scenery of the country, reacting on the minds of the early observers, has sometimes influenced their observations, and has thus left an impress on the general progress of the science. As we enter to-day upon a new phase in the history of Geology among us here, it seems most fitting that we should look back for a little at the past development of the science in this country. There was a time, still within the memory of living men, when a handful of ardent original observers here carried geological speculation and research to such a height as to found a new, and, in the end, a dominant shool of Geology.

In the history of the Natural Sciences, as in that of Philosophy, there have been epochs of activity and then intervals of quiescence. One genius, perhaps, has arisen and kindled in other minds the flame that burned so brightly in his own. A time of vigorous research ensued, but as the personal influence waned, there followed a period of feebleness or torpor until the advent of some new awakening. Such oscillations of mental energy have an importance and a significance far beyond the narrow limits of the country or city in which they may have been manifested. They form part of that long and noble record of the struggle of man with the forces of nature, and deserve the thoughtful consideration of all who have joined or who contemplate joining in that struggle. I propose on the present occasion to sketch to you the story of one of these periods of vigorous originality, which had its rising and its setting at Edinburgh—the story of what may be called the Scottish School of Geology. I wish to place before you, in as clear a light as I can, the work which was accomplished by the founders of that school, that you may see how greatly it has influenced, and is even now influencing, the onward march of the science. I do this in no vainglorious spirit, nor with any wish to exalt into prominence a mere question of nationality. Science knows no geographical or political limits. Nor, though we may be proud of what has been achieved for Geology in this little kingdom, can we for a moment shut our eyes to the fact that these achievements are of the past, that the measure of the early promise at the beginning of this century has been but scantily fulfilled in Scotland, and that the state of the science among us here, instead of being in advance, is rather behind the time. And thus I dwell now on the example of our predecessors, solely in the hope that, realising to ourselves what that example really was, we may be stimulated to follow it. The same hills and valleys, crags and ravines, remain around us which gave these great men their inspiration, and still preach to us the lessons which they were the first to understand.

The period during which the distinctively Scottish School of Geology rose and flourished may be taken as included between the years 1780 and 1825—a brief half-century. Previous to that time Geology, in the true sense of the word, can hardly be said to have existed. Steno, indeed, more than a hundred years before, had shown, from the occurrence of the remains of plants and animals imbedded in the solid rocks, that the present was not the original order of things, that there had been upheavals of the sea into dry land and depressions of the land beneath the sea, by the working of forces lodged within the earth, and that the memorials of these changes were preserved for us in the rocks. Seventy years later, another writer of the Italian school, Lazzaro Moro, adopting and extending the conclusions of Steno, pointed to the evidence that the surface of the earth is everywhere worn away, and is repaired by the upheaving power of earthquakes, but for which the mountains and all the dry land would at last be brought beneath the level of the waves.

But none of these desultory researches, interesting and important though they were as landmarks in the progress of science, bore immediate fruit in any broad and philosophic outline of the natural history of the globe. Men were still trammelled by the belief that the date and creation of the world and its inhabitants could not be placed further back than some five or six thousand years, that this limit was fixed for us in Holy Writ, and that every new fact must receive an interpretation in accordance with such limitation. They were thus often driven to distort the facts or to explain them away. If they ventured to pronounce for a natural and obvious interpretation, they laid themselves open to the charge of impiety and atheism, and might bring down the unrelenting vengeance of the Church.

Such was the state of inquiry when the Scottish Geological School came into being. The founder of that school was James Hutton—a man of a singularly original and active mind, who was born at Edinburgh in 1726, and died there in 1797. Educated for the medical profession, but possessed of a small fortune, which gave him leisure for the pursuit of his favourite studies, he eventually devoted himself to the study of Mineralogy. But it was not merely as rare or interesting objects, nor even as parts of a mineralogical system, that he dealt with minerals. They seemed to suggest to him constant questions as to the earlier conditions of our planet, and he was thus gradually led into the wider fields of Geology and Physical Geography. Quietly working in his study here, a favourite member of a brilliant circle of society, which included such men as Black, Cullen, Adam Smith, and Clerk of Eldin, and making frequent excursions to gather fresh data and test the truth of his deductions, he at length matured his immortal "Theory of the Earth," and published it in 1785. Associated with Hutton, rather as a friend and enthusiastic admirer than as an independent observer, was John Playfair, Professor of Natural Philosophy in this University, by whose graceful exposition the doctrines of Hutton were most widely made known to the world. His classic "Illustrations of the Huttonian Theory" is one of the most delightful books of science in our language—clear, elegant, and vivacious—a model of scientific description and argument, which I would most earnestly recommend to your notice. Sir James Hall, another of this little illustrious band, had one of the most inventive minds which have ever taken up the pursuit of science in this country. His merits have never yet been adequately realised by his countrymen, though they are better appreciated in Germany and in France. He was in fact the founder of Experimental Geology, since it was he who first brought geological speculation to the test of actual physical experiment. This he accomplished in a series of ingenious researches, whereby he corroborated some of the disputed parts of the doctrines of his master, Hutton. These were the three chief leaders of the Scottish school; but to their number, as worthy but less celebrated associates, we must not omit to add the names of Mackenzie, Webb Seymour, and Allan.

It would lead me far beyond the allotted hour to attempt any adequate summary of the work achieved by each of these early pioneers of the science. It will be enough for my present purpose if I try to sketch to you what were the leading characteristics of this Scottish School, and what claim it has to be remembered, not by us only, but by all to whom Geology is the subject either of serious study or of pleasant recreation.

Born in a "land of mountain and flood," the geology of the Scottish School naturally dealt in the main with the inorganic part of the science, with the elemental forces which have burst through and cracked and worn down the crust of the earth. It asked the mountains of its birthplace by what chain of events they had been upheaved, how their rocks, so gnarled and broken, had come into being, how valleys and glens had been impressed upon the surface of the land, and how the various strata through which these wind had been step by step built up. It encountered no rocks, like those which had arrested the notice of the early Italian geologists, charged with fossil shells, and corals, and bones of fish, such as still lived in the adjoining seas, and which at once suggested the former presence of the sea over the land. Neither did it meet with deposits showing abundant traces of ancient lakes, and rivers, and land-surfaces, each marked by the presence of animal and plant remains, like those which set Steno and Moro thinking. The rocks of Scotland are as a whole unfossiliferous. It was, therefore, only with the records of physical events, unaided by the testimony of organic remains, that the Scottish geologists had to deal. Their task was to unravel the

* A Lecture delivered at the opening of the clas of Geology and Mineralogy in the University of Edinburgh, by Archibald Geikie, F.R.S., Nov. 6, 1871.

complicated processes by which the rocky crust of the earth has been built up, and by which the present varied contour of the earth's surface has been produced,—to ascertain, in short, from a study of the existing economy of the world, what has been the history of our planet in earlier ages.

Hitherto, while men had been accustomed to believe that the earth was but some 6,000 years old, they sought in the rocks beneath and around them evidence only of the six days' creation or of the flood of Noah. Each new cosmological system was based upon that belief, and tried in various ways to reconcile the Biblical narrative with fanciful interpretations of the facts of Nature. It was reserved for Hutton to declare, for the first time, that the rocks around us can never reveal to us any trace of the beginning of things. He too first clearly and persistently proclaimed the great fundamental truth of Geology, that in seeking to interpret the past history of the earth as chronicled in the rocks, we must use the present economy of nature as our guide. In our investigations, "no powers," he says, "are to be employed that are not natural to the globe, no action to be admitted of except those of which we know the principle. Nor are we to proceed in feigning causes when those appear insufficient which occur in our experience."* This was the guiding principle of the Scottish School, and through their influence it has become the guiding principle of modern Geology.

There were two directions in which Hutton laboured, and in each of which he and his followers constantly travelled by the light of the present order of nature—viz., the investigation of (1) changes which have transpired beneath the surface and within the crust of the earth, and (2) changes which have been effected on the surface itself.

1. That the interior of the earth was hot, and that it was the seat of powerful forces, by which the solid rocks could be rent open and wide regions of land be convulsed, were familiar facts, attested by every volcano and earthquake. These phenomena had been for the most part regarded as abnormal parts of the system of nature; by many writers, indeed, as well as by the general mass of mankind, they were looked upon as Divine judgments, specially sent for the punishment and reformation of the human species. To Hutton, pondering over the great organic system of the world, a deeper meaning was necessary. He felt, as Steno and Moro had done, that the earthquake and volcano were but parts of the general mechanism of our planet. But he saw also that they were not the only exhibitions of the potency of subterranean agencies, that in fact they were only partial and perhaps even secondary manifestations of the influence of the great internal heat of the globe, and that the full import of that influence could not be understood unless careful study was also given to the structure of the rocky crust of the earth. Accordingly, he set himself for years patiently to gather and meditate over data which would throw light upon that structure and its history. The mountains and glens, river-valleys and sea-coasts of his native country, were diligently traversed by him, every journey adding something to his store of materials, and enabling him to arrive continually at wider views of the general economy of nature. At one time we find him in a Highland glen searching for proofs of a hypothesis which he was convinced must be true, and, at their eventual discovery, breaking forth into such gleeful excitement that his attendant gillies concluded he must certainly have hit upon a mine of gold. At another time we read of him boating with his friends Playfair and Hall along the wild cliffs of Berwickshire, again in search of confirmation to his views, and finding, to use the words of Playfair, "palpable evidence of one of the most extraordinary and important facts in the natural history of the earth."

As a result of his wanderings and reflection, he concluded that the great mass of the rocks which form the visible part of the crust of the earth was formed under the sea, as sand, gravel, and mud are laid there now; and that these ancient sediments were consolidated by subterranean heat, and, by paroxysms of the same force, were fractured, contorted, and upheaved into dry land. He found that portions of the rock had even been in a fused state; that granite had erupted through sedimentary rocks; and that the dark trap-rocks or "whinstones" of Scotland were likewise of igneous origin.

When the sedimentary rocks were studied in the broad way which was followed by Hutton and his associates, many proofs appeared of ancient convulsions and re-formations of the earth's surface. It was found that among the hills the strata were often on end, while on the plains they were gently inclined; and the inference was deduced by Hutton that the former series must have been broken up by subterranean commotions before the accumulation of the latter, which was derived from its *débris*. He conjectured that the later rocks would be found actually resting upon the edges of the older. His search for, and discovery of, this relation at the Siccar Point, on the Berwickshire coast, are well described by his biographer Playfair, who accompanied him, and who, dwelling on the impression which the scene had left upon him, adds: "The mind seemed to grow giddy by looking so far into the abyss of time; and while we listened with earnestness and admiration to the philosopher who was now unfolding to us the order and series of these wonderful events, we became sensible how much farther reason may sometimes go than imagination can venture to follow." Sir James Hall afterwards, by a series of characteristically ingenious experiments, showed how the rocks of that coast-line may have been contorted by movements in the crust of the earth under great superincumbent pressure.

Hutton was the first to establish the former molten condition of granite, and of many other crystalline rocks. He maintained that the combined influence of subterranean heat and pressure upon sedimentary rocks could consolidate and mineralise them, and even convert them into crystalline masses. He was thus the founder of the modern doctrines of metamorphism regarding the gradual transformation of marine sediments into the gnarled and rugged gneiss and schist of which mountains are built up. Let me quote the eulogium passed upon this part of his work in an essay by M. Daubrée, which eleven years ago was crowned with a prize by the Academy of Sciences at Paris:—"By an idea entirely new, the illustrious Scottish philosopher showed the successive co-operation of water and the internal heat of the globe in the formation of the same rocks. It is the mark of genius to unite in one common origin phenomena very different in their nature." "Hutton explains the history of the globe with as much simplicity as grandeur. Like most men of genius, indeed, who have opened up new paths, he exaggerated the extent to which his conceptions could be applied. But it is impossible not to view with admiration the profound penetration and the strictness of induction of so clear-sighted a man, at a time when exact observations had been so few, he being the first to recognise the simultaneous effect of water and heat in the formation of rocks, in imagining a system which embraces the whole physical system of the globe. He established principles which, in so far as they are fundamental, are now universally admitted."

(*To be continued*)

* Hutton's "Theory of the Earth," i. p. 160; ii. p. 549.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, clix., for July, opens with a concluding communication "On the constitution of the twice substituted benzenes," by E. Ador and V. Meyer. The authors converted sulphanilic acid into bromobenzine-sulphonic acid, and fused the potassium salt of this acid with potassic hydrate. The dihydroxylbenzine produced was found to be resorcin; Meyer and others have proved that resorcin belongs to the 1 : 4 series, and therefore sulphanilic acid must also be regarded as containing the SO_3H and NH_2 in the places 1 and 4 respectively. Sulphanilic acid treated with nitrous acid yields a diazo-derivative $C_6H_4N_2SO_3$, this on boiling with water is converted into phenol-sulphonic acid, which was found to be identical with Kekule's paraphenolsulphonic acid. At the end of the communication, a valuable table of the twice substituted benzines, showing the place of attachment of the second substituted group is given; it however differs in some respects from the arrangement of other chemists. Ernst and Zwenger have prepared ethyl and amyl gallates by passing hydrochloric acid through a boiling solution of gallic acid in the anhydrous alcohols; at present they have not succeeded in preparing the methyl gallate.—A very exhaustive paper follows "On some substances crystallised from microcosmic salt and from borax," by A. Knop, in which the crystallisation of phosphostannic, phosphozirconic, and phosphoniobic acids from microcosmic salt, and of stannic acid, zirconic acid, noria, and niobic acid from borax are thoroughly discussed.—Lieben and Rossi have prepared "normal valeric acid" by the action of boiling alcoholic potash on butyl cyanide, they find that the valeric acid thus obtained does not agree in properties with either of the acids already known. They have also prepared normal amylic alcohol from the above acid, by heating the calcic valerate with calcic formiate, the valeric aldehyde being converted into amylic alcohol by the action of sodium amalgam. The alcohol obtained boiled at 137°, which is somewhat higher than that of the ordinary alcohol. The normal amylic chloride, bromide, iodide, and acetate have been prepared, all of which possess boiling points higher than those of the compounds obtained from the fermentation alcohol. Normal caproic acid was prepared from amyl cyanide in the same manner as the valeric acid previously described.—A translation of Rossi's paper "On the synthesis of normal propyl alcohol from ethyl alcohol," and also of T. Smith's paper "On the estimation of the alkalies in silicates" follow.—Tollens continues with the seventh contribution on the allyl group, the subject of which is the conversion of allyl alcohol into propyl alcohol; this is accomplished by treating allyl alcohol with solid potash, the temperature being gradually raised to 155°, hydrogen being evolved in the reaction; it was found extremely difficult to purify the propyl alcohol; to obtain conclusive evidence it was converted into propionic acid; some six or eight other bodies are formed in this reaction, such as formic acid, propionic acid, and other higher compounds.—Rinne and Tollens have succeeded in preparing allyl cyanide from the bromide by the repeated action of potassic cyanide, and have converted it into crotonic acid by the action of alcoholic potash; the crotonic acid obtained fused at 72°, and possessed all the properties of crotonic acid as made from allyl cyanide prepared from mustard-oil. By the oxidation of allyl alcohol by chromic acid the authors have obtained formic acid, and small quantities of acrylic acid, no acetic acid being produced.—Fittig contributes a paper "On the alleged dibasic nature of gluconic and lactic acids," being a reply to Hlasiwetz's paper on this subject, Fittig himself considering them monobasic.—The continuation of a paper "On the action of Sulphurous Acid on Platinic Chloride," by K. Birnbaum, follows, several new and complicated salts of this series have been obtained; the reactions seem to proceed in two stages, first a reduction to platinous chloride takes place, and then the substitution of Cl by HSO_3; thus by the action of hydric ammonic sulphite on ammonic chloroplatinate a body of the composition $Pt. \begin{matrix} Cl \\ HSO_3 \end{matrix} \begin{matrix} NH_4 \\ H \end{matrix} So_3 + 4\ H_2O$ is obtained.—This number concludes with two short papers by J. Myers. The first is "On the temperature of decomposition of sulphuretted hydrogen," this is placed between 350° and 400°, probably nearer the lower temperature; the second paper is "On sulphuretted hydrogen containing arsenic." Sulphuretted hydrogen, as usually prepared from impure sulphuric acid and ferrous sulphide, contains a gaseous arsenic compound, probably arsenetted hydrogen; the two gases do not react on each other at ordinary temperatures, but when they are heated to the boiling point of mercury, a deposit of arsenious sulphide takes place. The arsenetted hydrogen is probably produced by the action of nascent hydrogen on the arsenic compound existing in the sulphuric acid.

SOCIETIES AND ACADEMIES

London

Royal Microscopical Society, November 1.—W. Kitchen Parker, F.R.S., president, in the chair. Dr. Braithwaite, F.L.S., contributed further remarks on the structure of the Sphagnaceæ or bog-mosses. Confining himself principally to the characters for grouping the numerous species into sub-genera, he advocated the system adopted by Dr. Lindberg of Stockholm, based upon those yielded by the form of the leaves investing certain portions of the stem and divergent branches.—Mr. W. Saville Kent, British Museum, read a paper on Prof. James Clark's Flagellate Infusoria with description of new species. In his communication, Mr. Kent announced the discovery among others of Prof. Clark's minute "collared" types (*Codosiga*, *Bicosœca*, &c.), first made known to the scientific world through the Memoirs of the Boston Society of Natural History for 1866, but not since corroborated by any European naturalist. Of the eleven species noticed by Mr. Kent, five were identified by him with American forms; the remaining six, while referable to corresponding genera, offering well-marked specific distinctions. The whole series are of exceedingly minute size, requiring a magnifying power of 800 diameters and upwards for the recognition of their structural peculiarities, the chief interest attached to them being their striking resemblance to the ultimate cell particles lining the incurrent cavities of sponges, as clearly shown by Prof. Clark in the calcareous, and since demonstrated by Mr. Carter in the siliceous groups. Mr. Kent expressed his dissent from Prof. Clark's views in regard to the nutritive functions of *Monas* and other Flagellata, in the course of his investigations, he having observed the former to engulf food at any portion of its periphery, after the manner of *Amœba*, while in the collar-bearing species, it was intercepted at any portion within the area circumscribed by the base of that organ, there being in no case a distinct mouth as assumed by Prof. Clark. In the discussion that ensued, Mr. Kent assented to the President's suggestion, that the Flagellata, in the possession of one or more lash-like appendages, represented a higher type of organisation than the Foraminifera, and other Rhizopodous Protozoa; and expressed his opinion that the Spongiadæ, as a class, combined the structural characters of the ordinary Rhizopoda and lower Infusoria, having superadded to this a skeletal and aggregated type of organisation essentially their own. Mr. C. Stewart affirming to having observed an appearance of three flagellate appendages to certain cells of *Leucosolenia botryoides* under a magnifying power of about 300 diameters, Mr. Kent accepted his statement as further corroboration of the existence of a membranous collar, which, under an insufficient degree of magnification, presents the aspect attested to by Mr. Stewart. The entire series of Infusorial forms recorded in Mr. Kent's communication were obtained by him from a pond on the estate of Mr. Thos. Randle Bennett, Wentworth House, Stoke Newington.

Entomological Society, November 6.—Prof. J. O. Westwood, F.R.S., vice-president, in the chair. Mr. Davis exhibited a collection of larvæ of Lepidopterous and other insects, beautifully preserved by inflation. Mr. Bond exhibited examples of *Zygæna esculans*, a new British moth, captured by Dr. Buchanan White in Braemar, and *Catocala Fraxini*, recently captured in the Regent's Park; also a singular variety of *Chœrocampa elpenor*, in which the central portion of each fore-wing was hyaline.—The Rev. A. Matthews sent for exhibition specimens of *Throscus carinifrons* and *Cryphalus tiliæ*, new, or recently discovered, British Coleoptera.—Mr. M'Lachlan exhibited *Bittacus apterus* from California, recently described by him in the *Entomologists' Monthly Magazine*.—Mr. Howard Vaughan exhibited the dark form of *Triphæna orbona*, from Scotland, known as *T. Curtisii*, and Mr. Lewis made some remarks on the synonymy of this form. Mr. Vaughan also exhibited a nearly black variety of *Arge Galathea*, captured in Kent by Mr. Tarn.—Mr. Miller exhibited an enormous oak-gall from America; also impregnated and unimpregnated eggs of *Libellula flaveola*.—Prof. Westwood exhibited numerous examples of *Formica herculeana*, a gigantic ant not hitherto known as British, found in the proventriculus of an example of *Picus martius*, said to have been shot near Oxford; from the perfect condition of the ants and of the bird which had devoured them, he fully believed in the genuineness of the bird as a British example, an opinion which was not shared by some of the members present. Prof. Westwood also exhibited two male examples of *Papilio Crino* from Ceylon, in one of which some of the veins of the wings were coated with brown hairs, a usual character with the males of some species of *Papilio*, but which had not hitherto been observed in that of *Crino*.—Mr. F. Smith exhibited a *Noctua*, apparently belonging to the genus *Aplecta*, which had been taken alive by Mr. Gwyn Jeffreys at sea, 220 miles from Nova Scotia.—Baron Chandois communicated notes commenting upon Mr. Wollaston's remarks respecting *Eurygnathus parallelus*, a Madeiran beetle described by him, and maintaining its distinctness from *E. Latreillei*.—Mr. Briggs read a paper "On *Zygæna Trifolii* and allied forms," detailing the result of his observations during many years, and arriving at the conclusion that two distinct forms or species had hitherto been confounded in Britain under the name of *Trifolii*.

Linnean Society, November 2.—Mr. G. Bentham, president, in the chair. Sir John Lubbock, Bart., read a paper "On the Origin of Insects," an abstract of which will be found in another column. An interesting discussion followed, in which Mr. George Busk, Mr. A. R. Wallace, Mr. M'Lachlan, Mr. Stainton, and Mr. B. Lowne, took part.—Captain Chimmo, "Notes on the Natural History of the Flying Fish." The author considers that he has established that during flight there is an extra consumption of oxygen by the fish, as shown by an increase of temperature. He finds that life is maintained for a period of from seven to nine minutes out of the water, and states that the fish possesses the power of changing the direction of its course during flight, using its tail as a rudder.

CHESTER

Society of Natural Science, October 25.—President, Rev. Canon Kingsley; treasurer, Mr. Kinsman; hon. secretary, Mr. Manning. The society is divided into three sections: (1) botany, (2) geology, (3) zoology; and numbers nearly 200 members. Mr. Alfred O. Walker read a paper on "Objects and Organisation of Local Natural History Societies."

GLASGOW

Geological Society, October 19.—Mr. Edward A. Wünsch, vice-president, in the chair. The Annual Report and abstract of the accounts for past year showed the society to be in a flourishing condition.—Mr. James Thomson, F.G.S., read a paper "On the Plagiostomous Fishes of the Coal Measures," particularly *Orthacanthus Dechenii* Goldfuss. He observed that Prof. Agassiz, in his "Poissons Fossiles" published in 1837, described the genus *Diplodus* (sp. *gibbosus* and *minutus*) from specimens, chiefly of dissociated teeth, found in the English coal-fields. Subsequently, a well-preserved fish was discovered in Bohemia, and described in 1847 by Goldfuss, who named it *Orthacanthus Dechenii*. In 1848, Prof. Beyrich, of Berlin, described the same fish, and named it *Xenacanthus Dechenii*, founding on the fact that the spine had a greater similarity to *Pleuracanthus* than to *Orthacanthus*. At the meeting of the British Association in Glasgow in 1855, Sir Philip Egerton, from discoveries that had been made in the interval, pointed out that the spines of *Pleuracanthus* and the teeth of *Diplodus* belonged in fact to the same fish. The specimens from which Sir Philip proved this to the Association were obtained from Carluke and Edinburgh. In 1867 Prof. Kner went carefully over the remains of such fishes in the museums of Dresden, Berlin, Breslau, and Vienna. Although none of the specimens found in these museums were complete, yet in some of them he found the teeth of *Diplodus minutus* of Agassiz in position, and from the external aspect of the fossils he accepted Goldfuss's generic name, *Orthacanthus Dechenii*. The specimen which Mr. Thomson now exhibited had been for many years in his collection, and had been provisionally named *Pleuracanthus minutus*. After a careful examination, however, of the microscopic structure both of the teeth and the shagreen, he could find no relation between the structure of *Pleuracanthus* and that now exhibited. In the meantime he accepted Prof. Kner's identification, but thought it possible that the discovery of better-preserved specimens would show that the difference of structural character might be due to difference of sex, as he had found to be the case in the recent rays' jaws of *Raia clavata*, both male and female, with the teeth in position, exhibited in support of this view.

PARIS

Academy of Sciences, October 30.—M. P. A. Favre read a continuation of his researches upon the thermal phenomena of electrolysis, containing an account of his investigations upon alkaline bases and sulphates; M. Wurtz presented the continuation of a paper, by M. G. Salet, on the spectra of phosphorus and of the compounds of silicium; and M. Le Verrier communicated a note by M. Diamilla-Müller, on a series of simultaneous magnetic observations which it is proposed to make in various parts of the surface of the globe, on the 15th of October, 1872. This note is accompanied by a table of the absolute magnetic declinations calculated for the above date, at a great number of places in all parts of the eastern hemisphere.—MM. Dumas and Chevreul and General Morin discussed the right of Daguerre to be regarded as the inventor of photography, and asserted the prior claims of Niepce de Saint-Victor.—M. Faye read the conclusion of his memoir on the history and present state of the theory of comets.—M. Delaunay presented a note by M. G. Leveau, giving the elements of the planet Hera (103).—A note was read by M. Barbe, on the uses of dynamite.—M. E. M. Raoult read a note on the transformation of dissolved cane-sugar into glucose, under the influence of light. The exposure lasted from May 12 to October 20.—M. Berthelot communicated the third part of his investigations of the ammoniacal salts, in which he discussed the reciprocal actions of the salts of ammonia and of the other alkalies.—A note was read by MM. A. Scheurer-Kestner and C. Meunier, on the composition and heat of combustion of two Welsh coals (from Bwlf and Powel.)—M. Daubrée communicated a paper on the deposit in which phosphate of lime has lately been discovered in the departments of Tarn-et-Garonne and the Lot.—M. A. Damour presented a note on an idocrase from Arendal, in Norway, containing an analysis of the mineral, and also an analysis of a garnet from Mexico.—M. E. Blanchard communicated a note by M. S. Jourdain, on the reproduction of *Helix aspersa*, in which the author described the arrangement of the reproductive organs and the mode in which their products are brought together.

BOOKS RECEIVED

ENGLISH.—The Letters of J. B. Jukes: Edited by his Sister (Chapman and Hall).—A Handbook of the Mineralogy of Cornwall and Devon: J. H. Collins (Longmans).—A Manual of Anthropology, or Science of Man: C. Bray (Longmans).—Note-book of Practical and Solid Geometry: J. H Edgar (Macmillan).—The Admiralty Manual of Scientific Inquiry, 4th edition: Rev. R. Main (J. Murray).—Proceedings of the South Wales Institute of Engineers; Vol. vii, Nos. 2-4.—Insects at Home, being a popular account of British Insects: Rev. J. G. Wood (Longmans).

AMERICAN.—Three and Four place Tables of Logarithmic and Trigonometric Functions: J. M. Peirce (Boston, Ginn Brothers).—Seaside Studies in Natural History; Marine Animals of Massachusetts Bay, Radiates: Elizabeth C. Agassiz and Alexander Agassiz (Boston, J. R. Osgood and Co.)

FOREIGN.—(Through Williams and Norgate)—Lehrbuch der anorganischen Chemie: Dr. Th. Ph. Büchner; 1te Abtheilung.—Wöhler's Grundriss der organischen Chemie: Dr. R. Fittig; 8te Auflage.—Die Zielpunkte der physikalischen Wissenschaft: E. Hagenbach.—Astronomische Tafeln u. Formeln: Dr. C. F. W. Peters.

DIARY

THURSDAY, NOVEMBER 9.

LONDON MATHEMATICAL SOCIETY, at 8.—On the Partition of an Even Number into two Primes: J. J. Sylvester, F.R.S.—General Meeting; Election of Council and Officers.

SUNDAY, NOVEMBER 12.

SUNDAY LECTURE SOCIETY, at 4.—Education in India: Jiram Row.

MONDAY, NOVEMBER 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—On Elementary Physiology (III.): Prof. Huxley, F.R.S.—Nervous Matter; its Structure and Properties: Prof. Huxley, F.R.S.

THURSDAY, NOVEMBER 16.

LONDON INSTITUTION, at 7.30.—The Influence of Geological Phenomena on the Social Life of the People: Harry G. Seeley, F G.S.

ROYAL SOCIETY, at 8.30.

LINNEAN SOCIETY, at 8.—On the Floral Structure of Impatiens fulva, &c.: A. W. Bennett, F L S.—Remarks on Dolichos uniflorus: N. A. Dalzell.—Floræ Hongkongensis Supplementum: H. F. Hance, Ph. D.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*