THURSDAY, FEBRUARY 9, 1871

THE POWER OF NUMERICAL DISCRIMINATION

It is well known that the mind is unable through the eye to estimate any large number of objects without counting them successively. A small number, for instance three or four, it can certainly comprehend and count by an instantaneous and apparently single act of mental attention. The limits of this power have been the subject of speculation or experiment among psychologists, and Sir William Hamilton thus sums up almost the whole of what is known about it :—

"Supposing that the mind is not limited to the simultaneous consideration of a single object, a question arises, How many objects can it embrace at once? . . . . I find this problem stated and differently answered by different philosophers, and apparently without a knowledge of each other. By Charles Bonnet, the mind is allowed to have a distinct notion of six objects at once; by Abraham Tucker the number is limited to four; while Destutt Tracy again amplifies it to six. The opinion of the first and last of these philosophers appears to me correct. You can easily make the experiment for yourselves, but you must beware of grouping the objects into classes. If you throw a handful of marbles on the floor, you will find it difficult to view at once more than six, or seven at most, without confusion; but if you group them into twos, or threes, or fives, you can comprehend as many groups as you can units, because the mind considers these groups only as units; it views them as wholes, and throws their parts out of consideration. You may perform the experiment also by an act of imagination." (Lectures, vol. i. pp. 253-4)

This subject seemed to me worthy of more systematic investigation, and it is one of the very few points in psychology which can, as far as we yet see, be submitted to experiment. I have not found it possible to decide conclusively in the manner Hamilton suggests, whether 4 or 5 or 6 is the limit, nor do imaginative acts of experiment seem likely to advance exact knowledge. Probably the limit is not really a definite one, and it is almost sure to vary somewhat in different individuals.

I have investigated the power in my own case in the following manner. A round paper box $4\frac{1}{2}$ inches in diameter, lined with white paper, and with the edges cut down so as to stand only $\frac{1}{4}$ inch high, was placed in the middle of a black tray. A quantity of uniform black beans was then obtained, and a number of them being taken up casually were thrown towards the box so that a wholly uncertain number fell into it. At the very moment when the beans came to rest, their number was estimated without the least hesitation, and then recorded together with the real number obtained by deliberate counting. The whole value of the experiment turns upon the rapidity of the estimation, for if we can really count five or six by a single mental act, we ought to be able to do it unerringly at the first momentary glance.

Excluding a few trials which were consciously bad, and some in which the number of beans was more than 15, I made altogether 1,027 trials, and the following table contains the complete results :—

Estimated Numbers.	Actual Numbers.												
	3	4	5	6	7	8	9	10	11	12	13	14	15
3	23												
4		65											
5			102	7									
6			4	120	18								
7			1	20	113	30	2						
8					25	76	24	6	1				
9						28	76	37	11	1			
10						1	18	46	19	4			
11							2	16	26	17	7	2	
12								2	12	19	11	3	2
13										3	6	3	1
14										1	1	4	6
15											1	2	2
Totals..	23	65	107	147	156	135	122	107	69	45	26	14	11

The above table gives the number of trials in which each real number was correctly or incorrectly guessed; thus in 120 cases 6 was correctly guessed; in 7 cases it was mistaken for 5, and in 20 for 7. So far as my trials went, there was absolute freedom from error in the numbers 3 and 4, as might have been expected; but I was surprised to find that several times I fell into error as regards 5, which was wrongly guessed in 5 per cent. of the cases. Abraham Tucker thus appears more correct as to my power than the other philosophers.

But in reality the question is not to be so surely decided by the trial of the few first numbers, as by endeavouring to obtain some general law pervading the whole series of trials. Calculating the average error of estimation in the case of each number, without regard to the direction of the error, we get the following numbers :—

3	4	5	6	7	8	9	10	11	12	13	14	15
0	0	·06	·18	·27	·44	·41	·65	·81	·73	1·08	1·21	1·27

These numbers vary pretty regularly in an apparently linear manner, except that in the case of the numbers 9 and 12, the result is too small. The error is simply proportional to the excess of the real number over $4\frac{1}{2}$, or obeys a law expressed in the formula (n being the real number)—

$$\text{error} = m \times (n - 4\tfrac{1}{2})$$

When we calculate the constant m for each number it comes as follows :—

5	6	7	8	9	10	11	12	13	14	15
·112	·122	·110	·127	·091	·117	·125	·098	·127	·128	·121

These numbers are sufficiently equal to enable us to take the average 0·116 as a good result, and the formula then becomes—

$$\text{error} = 0{\cdot}116 \times (n - 4\tfrac{1}{2})$$

or approximately—

$$\text{error} = \frac{n}{9} - \frac{1}{2}$$

This is a purely empirical law, the meaning or value of which I cannot undertake to explain. The most curious point is that it seems to confirm my previous conclusion that my own power of estimating the number *five* is not perfect. The limit of complete accuracy, if there were one, would be neither at 4 nor 5, but half-way between them; but this is a result as puzzling as one of the uninterpretable symbols in mathematics, just, for instance, like the factorial of a fractional number. But I give it for what it may be worth.

When we take into account the direction of the errors, the results are as follows:—

5	6	7	8	9	10	11	12	13	14	15
+ ·06	+ ·09	+ ·05	0·0	− ·05	− ·27	− ·46	− ·51	− ·85	− ·93	− 1·27

Thus there is a clear tendency to over-estimate small numbers and to under-estimate large ones. There is an evident inclination towards those medium numbers which most frequently recurred: how far this discredits the experiments I cannot undertake to say, but it is an instance of that inevitable bias in mental experiments against which it is impossible to take complete precautions.

My conclusion that the number five is beyond the limit of perfect discrimination, by some persons at least, is strongly supported by the principles of rhythm. All the kinds of time employed by musicians depend upon a division of the bar into two or three equal parts, or into multiples of these. Music has, indeed, been composed with the bar divided into five equal parts, but no musicians have yet been found capable of performing it (Rees' Cyclopædia, RHYTHM). Short runs, indeed, consisting of five or even seven equal notes, are not unfrequently employed by the best musicians, but it is to be doubted whether the ear can grasp them surely. I presume it is beyond doubt that 6, 8, 9, or more equal notes in a bar are always broken up by the hearer, if not by the performer, into periods of 2, 3, or 4. Quinary music, even if it could be executed, would be ill appreciated by the hearers, and, though all the powers of the human mind may be expected to progress in the course of ages, quinary rhythm belongs to the music of the distant future.

W. STANLEY JEVONS

BURMEISTER'S FAUNA ARGENTINA

FEW districts of the world are so rich in well-preserved remains of an extinct fauna of remarkable and interesting character as the neighbourhood of the city of Buenos Ayres. The immense alluvial plain of the Argentine Republic is the burial-place of the Megatherium, the Mylodon, the Glyptodon, the Macrauchenia, the Toxodon, and many other strange forms of ancient life, whose bones are ever and anon restored to light by the crumbling away of the soft banks of the great rivers which flow into the estuary of the Plata. So abundant, indeed, are they that, as remarked years ago by Darwin, any line whatever drawn across the Pampas would probably cross the skeleton of some extinct animal.

Collections of these fossils have at various times been sent to several European museums, and much information has been published upon the nature of the animals to which they belonged, but these observations have been generally made upon imperfect or fragmentary materials. The fortunate circumstance of the able and energetic German naturalist, Dr. Hermann Burmeister, formerly Professor of Zoology in the University of Halle, having taken up his residence at Buenos Ayres, and having been appointed Director of the Public Museum of that city, has been the occasion of a systematic and elaborate elucidation of the ancient fauna of this important district.

This has been brought about mainly by the publication of a richly illustrated serial quarto work, entitled "Anales del Museo Publico de Buenos Aires," the special object of which is to describe and figure the new or little-known objects preserved in that establishment. This work, which was commenced in 1864, appears at irregular intervals, but has already reached its sixth number, the first five of which constitute the first volume, the sixth, which is just published, being the commencement of a second volume. Dr. Burmeister is the sole author, and we have no hesitation in saying that it promises to be one of the most important contributions yet made to the knowledge of Mammalian zoology, for to this class is the publication mainly restricted. The parts already before us contain not only far more complete descriptions and detailed figures than have hitherto been given, of many of the extinct forms mentioned above, but it has also several admirable anatomical memoirs on rare or little-known living forms, especially of the Cetacea which occur in the estuary of the great river Plata, and in the adjoining part of the Atlantic Ocean, a field of research hitherto almost unexplored.

As this work, being written in the Spanish language, is not so well known in this country as it deserves to be, we propose to lay before our readers a summary of the contents of the volume already completed, from which they will be able to judge of the richness and variety of the material which has been at the author's disposal, and of the excellent use that has been made of it in his experienced hands.

After a history of the foundation and progress of the public museum of Buenos Ayres, and a general essay on palæontology, a detailed description is given of the skeleton of *Macrauchenia patachonica*. The first discovered remains of this very remarkable animal, which is about the size of a camel, were found by Mr. Darwin in 1834 at Port St. Julian on the Patagonian coast, and presented by him to the Museum of the Royal College of Surgeons of London, where they are now preserved. They were described by Professor Owen in the appendix to the "Voyage of the *Beagle*" (1840). Since that time but little addition was made to our knowledge of the species (although some bones of a smaller animal of the same genus, discovered by Mr. D. Forbes in Bolivian copper mines, have been described by Professor Huxley), until the lamented Bravard commenced the description, in a work to be entitled "Fauna fósil del Plata," of a comparatively perfect skeleton, which was contained in the museum of Buenos Ayres; but as he did not recognise its identity with Owen's Macrauchenia, he gave it the name of *Opisthorhinus falconeri*. The premature death of Bravard in the earthquake which destroyed the greater part of the town of Mendoza, prevented the publication of this work; but three of the plates, which had already been executed, containing figures of the skull with nearly complete dentition, and many of the vertebræ and limb bones, form the first three plates of the present work. To these, Dr. Burmeister has added another containing views of the pelvis and some more vertebræ, and an elaborate description of the whole of the known bones, finally concluding that the zoological position of the genus is among the imparidigitate or perissodactyle Ungulata, between the Horse and the Tapir.

After some remarks on the humming-birds described by Azara, a preliminary notice is next given on the different species of Glyptodon, or gigantic extinct Armadillo, in the museum. Three species are distinguished as well established, viz., *G. spinicaudus*, *G. clavipes*, and *G. tuberculatus*; a fourth, *G. pumilio*, smaller than the other, is founded on a portion of a lower jaw. Some general observations on the osteology of the genus are added.

The next portion of the work is called "Fauna Argentina, Part I., Fossil Mammals." It commences by a geological description of the fossiliferous region, and then follows a list of the fossil mammals of the "diluvian" deposits, with some remarks upon each. This list comprises, of Carnivora, *Machairodus neogæus*, of which the museum possesses a nearly complete skeleton discovered in 1844 near Lujan, about fifty miles west of Buenos Ayres. A short account of the osteological character of this interesting specimen is given, and a full and illustrated description is promised in one of the future numbers of the work. We have also *Felis longifrons*, *Canis protalopex*, *Canis avus*, *Mephitis primeva*, *Ursus bonærensis*. With regard to Marsupialia, it is singular that no remains of this group have hitherto been found in the diluvial deposits of the region in question, although, as is well known, they are not infrequent in the Brazilian caves explored by Lund. The list of fossil rodents includes *Myopotamus bonærensis*, *M. antiquus*, *Ctenomys bonærensis*, *Lagostomus angustidens*, and *Cavia breviplicata*. In the Edentata, the district is of course especially rich. Notices are given of *Megatherium americanum*, *Mylodon giganteus*, *M. gracilis*, *M. robustus*, *M. darwinii*, and a plate is devoted to the illustration of details of the osteology of these two genera, especially the hitherto little known sternum, sternal ribs, and hyoid bones. Then follow *Scelidotherium leptocephalum*, *S. cuvieri*, *Megalonyx meridionalis*, and *M. jeffersoni*. The genus *Glyptodon*, now divided into several sections, is represented by the following species: *G.* (*Panochthus*) *clavicaudatus*, *G.* (*P.*) *tuberculatus*, *G. clavipes*, *G.* (*Hoplophorus*) *asper*, *G.* (*H.*) *elongatus*, *G.* (*H.*) *lævis*, with several species but incompletely known. Numerous details are given of the osteology of these animals, with three plates of illustrations, one containing a view of a complete skeleton of *G. asper*. Under the head of Pachydermata, the teeth of two extinct species of horse, *E. curvidens* and *E. devillei* are described and figured, further notes are added on *Macrauchenia*, together with a complete view of the skeleton, and a restored outline of the animal with a slender elongated pendant proboscis, and some valuable details are given on the genus *Toxodon*, for the first knowledge of which, as in the case of *Macrauchenia*, we are indebted to Darwin's collection, described by Owen. Three species are now distinguished, viz., *T. burmeisteri*, *T. owenii*, and *T. darwinii*, appropriately commemorating the three distinguished naturalists by whose labours the history and affinities of this singular form have been made known. Three plates are devoted to the illustration of this genus. The list concludes with *Mastodon humboldtii*, to the osteology and dentition of which one plate is assigned.

The next and concluding section of the volume is entitled "Fauna Argentina, Part II., Mammifera pinnata." It is devoted to an account of the marine mammalia of the republic. Of the Pinniped Carnivora, *Otaria jubata* and *O. falklandica* are mentioned. The Manati is stated not to occur in the Argentine rivers. The Cetacea are represented by *Pontoporia blainvillii*, *Delphinus microps*, *D. obscurus*, *D. cymodoce*, *Lagenorhynchus cæruleo-albus*, *Orca magellanica*, *Phocæna spinipinnis*, *Globiocephalus grayii*, *Epiodon australe*, *Balænoptera bonæriensis*, *B. patachonica*, *Sibbaldius antarcticus*, and *Megaptera burmeisteri*.

A very detailed description is given of the external characters and anatomy of a newly discovered species of Ziphius, named by Dr. Burmeister *Epiodon australe*, which is not only valuable as being one of the most complete and fully illustrated accounts we possess of the structure of any Cetacean, but especially as the members of the particular group to which this one belongs are all exceedingly rare, or, at all events, have a remarkable habit of keeping out of the way of naturalists, and, consequently, are less known than almost any other section of the Mammalia. Descriptions, more or less detailed, are also given of the following new species:—*Globiocephalus grayii*, and *Orca magellanica*, both founded on characters of the skulls, which are figured. The former has much larger and thicker teeth than any other members of the genus to which it is referred. *Phocæna spinipinnis* is characterised not only by the numerous and regularly placed horny tubercles on the anterior edge of the dorsal fin (which are also frequently found, though in a more rudimentary condition, in the European porpoise) but also by the peculiar form of that fin, the anterior edge being concave, and by the conformation of the skull.

Not less valuable than the anatomical description of *Epiodon australe* is the article which concludes the work, which is a full and excellently illustrated account of the external characters and anatomy of a very singular and aberrant form of dolphin, hitherto but imperfectly known, called *Pontoporia blainvillii*. This animal is one of the smallest of the Cetacea, being but five or five and a-half feet long when adult. It inhabits the estuary of the river Plata and the adjoining parts of the ocean, but it is not truly fluviatile, like the *Inia* of the Amazon and the *Platanista* of the Ganges, to which two forms it presents some structural affinities.

We trust that the brief outline which we have given of the contents of the first volume of these "Anales" will be sufficient to show that it is a book indispensable to every good scientific library, and, in conclusion, we wish to express our cordial hope that the inhabitants and government of Buenos Ayres will continue zealously to carry on the creditable work they are doing for science in keeping up and augmenting their valuable museum, and that Dr. Burmeister will long continue to be the exponent of its treasures.

W. H. FLOWER

RECENT PETROGRAPHICAL LITERATURE

I.

Lehrbuch der Mineralien und Felsartenkunde. Von Dr. F. Senft. Jena. (London: Williams and Norgate.)

THE future historian of Geology who shall describe the rise and progress of the science in England, will find material for one of his most curious and interesting chapters in tracing out the causes which checked the growth, and finally all but extinguished the very existence of petrographical study in this country. While in all that relates to stratigraphical geology, we have kept well ahead of other nations, and have been quite abreast of them in palæontology, we have allowed petrography, or the study of rock-species, to fall into disuse. For half a century we have been content to make shift with vague, incorrect names invented in the infancy of the science; our progress in this respect since the early days of M'Culloch, Boué, and Jamieson, having been simply *nil*. More especially is this true of our nomenclature of igneous rocks. While we have unravelled the complicated stratigraphical structure and relations of these rocks with unrivalled labour and detail, we have left aside the questions touching their mineralogical ingredients and chemical composition, and their classification as mineral compounds. English petrography does not exist; what we have in its stead is an indefinite obsolete grouping of rocks patched up with occasional borrowings from the Continent. And yet, strange to say, it is in England that the most important step in modern petrography has originated. Sorby's application of the microscope to the study of rocks has opened a new era in the science, and our good friend Sorby himself is regarded as a kind of demi-god in the eyes of our German brethren of the hammer. But even his wand, though it has raised up a new army of zealous petrographers on the Continent, has, as yet, failed to quicken the dry bones of English petrography Mr. David Forbes is our *spes altera Romæ*. Our waiting eyes have been turned to his laboratory in York Place, Portman Square, for years past. His materials are vast, his enthusiasm great, and his intention fixed, to retrieve the honour of English petrography. May his shadow never be less until long after his wishes and our hopes are fully realised!

They manage things petrographical very differently in Germany. There the study of rocks is introduced into the curriculum of schools and universities. It is treated of in many excellent text-books. It is eagerly pursued by zealous investigators from Berlin to Vienna. The great paper of Mr. Sorby, published here thirteen years ago, has done much to quicken this research by showing that the older methods were in many respects untrustworthy. These methods were based primarily upon chemical analysis. But such analysis, while it reveals the ultimate chemical constitution of the rock, may not explain its mineralogical composition. The various stages of the metamorphism of the component minerals are thereby often lost sight of. Hence two rocks, having by analysis approximately the same chemical composition, may differ materially from each other in mineralogical composition. It is here that, as Sorby showed, the microscope comes in to our aid, and shows what the different mineral ingredients of the rock are, how far they have respectively undergone alteration, how they are built into each other so as to form the rock-mass, and under what conditions they may originally have been formed. This important addition to the methods of research has so powerfully affected petrography, that this branch of science must be regarded as at present in a transition state. Many of the groups of rocks in the nomenclature now in vogue in Germany will require reconsideration. More especially is revisal needed in those based upon subdivisions of the triclinic felspars. Petrographers are now coming to see that, in a vast number of cases, it is not possible to discriminate the particular species of felspar in a rock, further than as belonging to the othoclase or plagioclase division. In this separation the microscope becomes of essential importance.

A small pile of German petrographical literature has accumulated on our table, and we propose in this and a subsequent paper to notice the more important works. The first volume that comes to hand is another publication of that most voluminous writer, Dr. Ferdinand Senft, Professor of Natural Science, Eisenach. He seems to issue a goodly octavo every year, though possibly the past year's political events may have interrupted his labours for 1870. The present work is entitled "A Text-book of Mineralogy and Petrography," and contains some 700 pages. One would have thought that the Doctor had hardly left himself room for such a book as this, when we remember not only his former special treatises on the subject, but his text-book of "Forstlicher Naturkunde," one of the volumes of which is devoted to geognosy, soils, and chemistry. And yet the book differs materially from any of his former works, and, if we mistake not, is likely to be at least quite as useful. It is not designed to be an elaborate methodical text-book, but one in which the teacher and pupil will find all the material they require for a successful and methodical study of minerals and rocks, and also one which will prove sufficient for the student in his early inquiries, even without the help of a master. The author has had peculiar advantages for the compilation of such a book. In an interesting preface he tells us that for a quarter of a century he has been engaged in teaching these subjects to the two higher classes in a school, and he details the method of instruction which his experience has found to be successful. He had used the best mineralogical treatises as text-books in his classes, but had always found them too difficult for use in schools. Accordingly in 1860 he brought out a little "School Text-book of Mineralogy and Geognosy," which, having been out of print since 1866, he has remodelled and enlarged into the present work.

The general plan of the book is like that of the ordinary German text-books, only the first or mineralogical division occupies about five times more space than that devoted to petrography. It is of the latter that we have at present to speak. Retaining the usual grouping of crystalline and fragmental rocks, the writer gives a clear and succinct account of each subdivision and species. His plan for the specific details somewhat resembles that in his earlier work on the Classification of Rocks, but with some improvements. Under each species of rock, a clear but brief description of its leading features is given in large type, then follows an equally concise account of its varieties, transitions, mode of weathering, geological occurrence, and geographical distribution. The notes on the weathering of the different rocks, and the general remarks on that subject in the introductory part of the petrographical section, go some way to supply a want which every beginner soon discovers to exist in other manuals. The same commendation may be given to the descriptions of the various kinds of *débris* and soil formed by the decay of rocks.

As a school-book, the present volume seems likely to prove useful. The arrangement into short subdivisions, each clearly marked in the mode of printing, and treating each rock in the same method, will facilitate the progress of a class, and give precision to the inquiries of a beginner. Were there only any general taste for such pursuits in our own country, we might hope to see the book translated and adapted for use in our colleges and schools. At the same time, we are bound to notice what appears a very serious defect in the volume. The author has ignored recent microscopical research, and instead of giving that method a distinct and prominent place in his account of the investigation of rocks, he contents himself with the old "dry way" and "wet way" of analysis. In so doing, he tacitly confesses himself to be behind his time. His compilation, useful as it is, will, we hope, ere long be superseded by another, when petrography has had time to compose itself again into something like clearness and symmetry. In the mean time, the student who wishes to go more fully into the matter, will still find Zirkel's "Lehrbuch" his best guide, though even that valuable manual is fast getting out of date; owing to the great progress which the last few years have witnessed in this branch of geology. That progress has been largely shared in by Zirkel himself, as will be shown in a subsequent paper.

ARCH. GEIKIE

OUR BOOK SHELF

Der Zoologische Garten. Zeitschrift für Beobachtung, Pflege und Zucht der Thiere. Herausgegeben von Dr. F. C. Noll. XI. Jahrgang, 1870. (Frankfort a. M.) (London: Williams and Norgate.)

THE "Zoological Garden" is, as its name imports, a periodical especially devoted to all that is connected with the maintenance of animals in what are commonly called "Zoological Gardens." Having been founded some ten years ago by the Zoological Society of Frankfort-on-the-Main, it more especially relates to the affairs and condition of the small but well-arranged garden belonging to that Society, which is situated in the environs of that free and ancient city. It may appear somewhat surprising that a journal devoted to a subject of apparently so limited an extent can achieve sufficient circulation to command success. But the number of zoological gardens, aquarium houses, and similar establishments in Germany, has considerably increased of late years, and their institution in nearly all the principal cities of the Fatherland has been very favourably received, so that it is easy to understand that a considerable amount of popular interest in these subjects has been excited. Hamburg, Cologne, Dresden, Berlin, Hanover, and Munich, have all flourishing establishments of this description, and although the Zoological Garden founded some years ago in Vienna has come to an untimely end, yet in every part of what is now the new Empire of Germany the prospects of such institutions seem to be extremely favourable. But our "Zoological Garden" by no means entirely confines its attention to animals in captivity. It likewise contains many excellent articles relating to the habits of birds and beasts in a state of nature, so as to embrace many of the well-known attractions of a popular magazine of Natural History. Occasionally also more strictly scientific articles, such as that of Professor Pagenstecher on the Anatomy of the Cape Hunting Dog (*Lycaon pictus*) in the numbers for July and August last year, are given, so that the result is a zoological miscellany of a very various character. The woodcut illustrations are, it is true, perhaps not always in the highest style of art, but we have seen many inferior in English popular works of Natural History, and they have generally the merit of being tolerably correct. To such of our readers therefore as are growing weary of the "Zoologist," and cannot appreciate the learning of the "Annals," we recommend a trial of the "Zoological Garden," it being pre-supposed, of course, that they understand the language in which it is written (which in these days is a matter of course!). The subscription-price is very moderate, amounting only to about 8*s*. per annum for the twelve numbers, and the journal is regularly forwarded through the post to this country.

P. L. S.

The Marvels of the Heavens. By Camille Flammarion. From the French, by Mrs. Norman Lockyer. With 48 Illustrations. (London: R. Bentley, 1870.)

THE French certainly have the art, which we have not, of putting science in an attractive form to the popular mind—attractive, and yet not at the expense of scientific accuracy. Good service is, therefore, done by the translation into easy and graceful English of works like this by M. Flammarion. From the very commencement he carries the reader with him by his enthusiasm. Instead of starting with a bare statement of facts—that the Sun is the centre of the solar system, that it is so many hundred thousand miles in diameter, and has this, that, and the other planet revolving round it at such and such distances, he takes his reader out with him, as it were, to behold the heavens on a starry night; explains how it is that we see the sun only during a portion of the twenty-four hours; and speaks of the arrangement of the stars in clusters and nebulæ. Then he descends from the stars as a whole to a particular one, the Sun, and proceeds to describe in detail the solar system. And, throughout, the subject is treated with a graceful fancy and a wealth of illustration which make it very charming. Old Greek myths and fables of the astrologers, quotations from Byron and Lamartine, from Bryant and Victor Hugo, anecdotes of the value of astronomical knowledge, are brought in to point the moral and adorn the tale, and never appear to come amiss, or to be beside the mark. We must say a word about the illustrations, which are extremely good. We have never seen anything that so well recalls to our mind the appearance of the heavens through a powerful glass as Fig. 21, a part of the constellation of the Swan, as seen through the telescope; on the opposite page is placed, by way of contrast, the same seen by the naked eye. Author, translator, and artist have combined to produce a book which ought to be in the hands of every one who desires an introduction to "The Marvels of the Heavens." B.

Geology. By Prof. John Morris, F.G.S., and Prof. T. Rupert Jones, F.G.S. First series. Heads of Lectures on Geology and Mineralogy, in several courses from 1866 to 1870, at the Staff and Cadet Colleges, Sandhurst, by T. Rupert Jones, F.G.S. (London: Van Voorst, 1870.)

THIS book can hardly be called a Manual of Geology; it is rather the *avant-coureur* to the book which is to be written presently. It is a series of outlines for a course or courses of lectures, furnishing in a brief and concise form the heads or texts for any number of geological discourses which a Science teacher may be called upon to give: or the student may take it as his guide to the main lines and branches of geological study, along which he may have to pursue his readings in preparing himself, either alone or with the assistance of a "coach," to pass his "B.Sc.," or other examination at any one of the Universities. Now-a-days, when a man has to coach up so many different subjects in so short a time, it is obvious that, the more handy and concise a book is, the more useful will it be in helping to the desired end. One thing more seems to us to be needed in order to render this book of practical utility to the *uninitiated*; it is, to give, under each head, references to the authors (with chapter and verse) whom the student or teacher should consult, to gather more fully what is here only hinted at, often in but six words or less. To those who are already read up in Geology and Palæontology, the book is a most useful form of "Remembrancer," containing besides numberless facts — the key-notes to whole discourses on the earth and its past history.

H. W.

LETTERS TO THE EDITOR

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

The Cretaceous Period

IN the number of NATURE for January 19 appeared a letter from Prof Wyville Thomson, in which he attempts to justify the opinion which has been concisely summed up in the words "we are still living in the Cretaceous epoch," a statement emphatically contested in the lately published "Students' Elements of Geology," by Sir Charles Lyell.

Prof. Thomson begins by supposing that the term Cretaceous epoch is considered by geologists as an undefined period of time which may very well be elastic enough to reach up to the present day; and, in this misapprehension of the exact meaning of geological terms, betrays the source of the whole misunderstanding. This accounts for an unnecessary digression about axes of elevation and raising the floor of the Atlantic to the surface; as if the possibility of part of that ocean having remained as such since the chalk period had ever been denied. It is known with certainty that a large area of Cretaceous sea existed where the Atlantic does not extend, but beyond our observations on dry land it is perfectly useless to speculate how far the present distribution of land and water coincides with that which obtained when *Belemnitella mucronata* was a living animal. Moreover the possibility of perfectly continuous and conformable series of deposits from the chalk period upwards being present somewhere at the bottom of the sea, has not received an atom of confirmation in the late deep-sea dredgings, and if true, would be no reason for laying aside a very serviceable classification based on other than purely stratigraphical considerations.

In speaking of organic remains, Prof. Thomson persists in misunderstanding the reasons on which the title "Cretaceous" has been founded, and so clearly defined in the controverted paragraph of the Student's Elements, p. 263. He cannot gainsay the fact that so many genera of Cephalopoda have completely disappeared, and, indeed, grants only a generic and specific resemblance between certain fossils and recent deep-sea forms; which is obviously no adequate reason for the assumption that we now live in the Cretaceous epoch. It seems also to have been forgotten that our knowledge of this period is not confined to chalk strata, but is derived also from the examination of sand and clay formations in England, as well as those on the Continent and America, which are recognised as Cretaceous from their containing the same species of mollusca which characterise our chalk. Further, as the only organisms common to the Atlantic sand and the chalk are the Globigerina and other Bathybius, which are also common to the Indian and Pacific Oceans, any inferences drawn from their occurrence in the Atlantic are equally applicable to the two latter cases, and equally so to Eocene seas where Globigerinæ flourished along with Nummulites.

In fact the whole question is shortly this: calcareous mud has been dredged up from the bottom of the Atlantic, often from great depths. This mud resembles chalk so much as to leave no doubt but that the older deposit was produced under identically the same physical circumstances and organic conditions (as far as concern the matrix of Globigerinæ, Coccoliths, &c., but exclusive of all imbedded remains) as the newer formation. The outward resemblance of the two rocks, and the fact of the new deposit having been found so near where the old one accumulated, together with the recurrence of a form of Encrinite and a few Echinoderms, have imposed on Dr. Thomson, and led him to a conclusion which has no justification in facts. Had this eminent naturalist been an equally experienced geologist, he would have seen that, with the recurrence of outward conditions, it was but natural to expect a return of some old forms of life, as is the real state of the case.

L. L.

A QUARTER of a century ago, when first I began to study geology, it appeared to me that a predominance was given to the more recent rocks, such as the Pleistocene, Miocene, Eocene, Cretaceous, and the like, to which they were not entitled, when ranked as periods alongside such great groups as the Carboniferous the Silurian, the Cambro-silurian, and the Cambrian. The more I have since daily learned confirmed me in this opinion, therefore it is with great interest that I have watched the discussion on the valuable suggestions of Drs. Thomson and Carpenter. Perhaps I may be allowed to join in the controversy, and to draw attention to some facts connected with the Carboniferous period, which seem to add weight to the arguments put forward by those gentlemen. Before proceeding further, I ought to state that in the Carboniferous period I include the so-called Old Red sandstone or Devonian period, on account of their being interpenetrated, dovetailed, and graduated one into the other in the S.W. of Ireland.

In the counties Limerick and Clare, well marked divisions occur in the Carboniferous period, of which the following is an epitome:—

11. Coal-bearing rocks.
10. Flag series.
9. Lower coal-measure shales.
8. Upper limestone.
7. Calp (shaly orgillaceous limestone, with shales and sometimes sandstones. Between the calp and the upper limestone a cherty zone may occur, but it is not constant).
6. Upper chert zone.
5. Amorphous limestone (Fenestrella beds).
4. Lower chert zone.
3. Lower shaly limestone.
2. Lower limestone shale.
1. Grit, &c. (Old Red sandstone, or Devonian period).

Certain fossils do range through all these divisions, while others will occur in all the similarly, or nearly similarly, circumstanced rocks; nevertheless, the major part of, if not all, these divisions have their groups of fossils. In some, a fossil first appears, in others it dies out, so that any one well acquainted with the rocks in the field can tell from the assembly of fossils to what division of the Carboniferous period the rock belongs.

The section of Limerick and Clare is only local, and will not be found in many places in Ireland. To the S.W. and S. in Cork and Kerry, some of the divisions die out, while others thicken, so that eventually in S.W. Cork and Kerry the limestones have entirely disappeared. To the N., N.E., and E. of Clare other changes take place. Moreover, they are not as gradual or regular as those to the S.W., and more especially if the rocks are followed towards the N.E. In north Galway the divisions in the limestone have disappeared, while interstratified with the limestone, in some place evidently high up (the representation of division No. 8) will be found sandstone, and conglomerates in aspect exactly similar to the so-called Old Red sandstone, and when fossiliferous containing similar fossils. Farther N.E. in the county Leitrim, the divisions are very similar to those in Clare, while still farther N.E. the different divisions become mixed, and in the coal-fields of Ulster will be found rocks similar to the coal measures, the Old Red sandstone, and the Carboniferous limestone, all interstratified one with another.

Neither are these rocks without breaks. In West Cork there is a continuous sequence either from the upper Silurian rocks, or rocks immediately above them to the coal measures; while in Kerry, there are in these rocks two well-marked breaks or unconformabilities, and in Limerick there may be another, as, at the only junction exposed, between the lower-coal-measure-shales and the upper-limestone, the limestone appears to have been denuded prior to the shales having been deposited. By the above it is shown that the condition under which the Carboniferous rocks were deposited, in the area that is now Ireland, are very varied in character, and if Great Britain were also included, other marked changes could be pointed out; as, for instance, in South Staffordshire, where there seem to have been subaërial accumulations going on during a great part of the Carboniferous period, as suggested by the vast thickness of the coal. If, in such a mere speck as Ireland, these great changes took place in the Carboniferous period, what must have been the vast changes on the whole earth's surface?

To me there appears to be a certain analogy between "the Carboniferous period" and the "Cretaceous period of Thomson and Carpenter." Both are great limestone periods. and both will have their coal beds. But what is more remarkable, Carpenter suggests that the geologist of future ages will separate the rocks forming in the "cold area" from the rocks forming in the "warm area," and put them in separate formations on account of the great difference in their mineral constituents and their assembly of fossils, while the greater number of the field geologists of the present age make two periods of the rocks of the Carboniferous period (viz., Carboniferous period and Devonian period), although the rocks of both groups are found to interpenetrate one another, to dovetail into each other, and gradually to blend one into the other, both in lithological character and fossil contents.

Will any palæontologist take it on himself to say that there is a greater difference between the fauna of the Atlantic chalk and the chalk of England, than there is between the lower and upper divisions of the rocks of the Carboniferous period? Of course the Atlantic chalk is not to be represented only by the low forms found in the deep-sea soundings, as they do not represent its entire fauna. Years ago the late Mr. Salter pointed out at Glengariff, Co. Cork, that more fossils occur at changes of strata than elsewhere; such as the uppermost limits of a series of argillaceous or arenaceous beds, or at the top of a bed, if grits and shales alternate. This I have since found to be a good general rule, more especially when subordinate beds appear in a group. In Limerick, as well as other places in Ireland, masses of limestone may be without fossils, or, at least, conspicuous fossils; but if subordinate beds appear, such as the cherty zones, the aspect of affairs immediately changes, and, as a general rule, the rocks immediately subjacent to such changes are almost entirely made up of fossils and their *débris*. Similar changes are not only possible, but also most probable, in the Atlantic chalk. However, they are not likely to be proved in our day. But as in the limestone, so in the Atlantic chalk, in such places the mass of the fossils belonging to the latter ought to be found.

In considering such a question as the present, I would suggest that such fragile accumulations as those of the Kainozoic epoch ought to be considered of only minor importance; as most of them would be denuded away as the land sank, while those that chanced to remain would only form very subordinate strata. Moreover, Edward Forbes long since suggested that both from palæontological and petrological considerations, it might be better if the division between the Mesozoic and Kainozoic epochs were obliterated. Furthermore it has to be borne in mind that while in new strata very minute breaks can be detected; in old strata, like the Carboniferous period, it would be nearly impossible; and most of the great advocates for the minute division of the newer rock would not allow them in the old, as they explain everything they cannot understand by a "fault."

Connemara, Jan. 29 G. HENRY KINAHAN

Eozoön Canadense

THE organic nature of Eozoön Canadense may, I trust, be regarded as established conclusively by the evidence which has been adduced by Dr. Carpenter, Dr. Hunt, and myself, and I think I am safe in saying that it is accepted by all or nearly all those best qualified to judge. Since, however, the doubts expressed by your correspondent, Mr. Reade, may be shared by many who have not had full opportunity to satisfy themselves on the subject, I think it may be useful once more to direct attention to the facts serving to answer the objections which he has stated, and which, on more full consideration of the questions involved, I trust he may abandon.

Your correspondent objects: First, That the supposed Liassic serpentine or ophiolite of Skye shows structures similar to those of Eozoön. In answer to this it is not necessary to have recourse to the supposition that creatures similar to Eozoön have continued to exist up to the Liassic age, since, as Dr. Hunt has shown,* there is reason to doubt the accuracy of the observations which refer this rock to the Lias; and, further, Profs. King and Rowney, in a recent paper on Eozoön in the Proceedings of the Royal Irish Academy,† have figured this supposed Eozoön, and have thus shown that the portions of it which they consider similar in structure to the Canadian specimens do not possess such structure. I would not, in any Canadian specimen, accept such appearances as those represented in their figure as the Eozoön. This objection is therefore wholly irrelevant.

He objects: Secondly, That Eozoön occurs only in metamorphic rocks, and usually mineralised by serpentine. To this I answer: (1.) It unfortunately happens that Eozoön is a fossil of the Laurentian period, and that the rocks of this age are in a more or less metamorphic state in every part of the world where they are known. When we shall have found unaltered Laurentian rocks it will be time to inquire if this fossil occurs in them, and in what state of preservation. (2.) I have elsewhere shown that the chambers and canals of Eozoön are filled not only with serpentine but with other mineral substances, as Loganite, Pyroxene, and Calcite. There is thus, as Sir William Logan affirmed previous to the discovery of the minute microscopic structure, no connection between the forms of the supposed organism, and the mineral substances in connection with which they appear.

* Silliman's Journal, March 1870.
† Proc. R.I.A., July 1869.

In the third place, in order to be enabled to make the assertion above referred to, your correspondent "disposes of" the Tudor specimen, which, as compared with the others examined, occurs in a comparatively unaltered sediment. With regard to this specimen, I affirm, and the published figures show: (1) that it presents the characteristic features of Eozoön, more especially resembling the specimens from the Calumet and from Perth; (2) that other specimens found in the same locality confirm its determination as Eozoön; (3) that the matrix containing the Tudor specimens is a coarse limestone not more metamorphic than many Silurian beds holding fossils. I have, however, to state that the recent explorations of Mr. Vennor, of the Geological Survey, seem to show that the beds which afforded the Tudor specimen, though unconformably underlying the Lower Silurian, overlie the highly metamorphic Lower Laurentian of the district, and, therefore, instead of being, as heretofore supposed, comparatively unaltered Lower Laurentian, they may prove to be even as late in age as the Cambrian. It is in these rocks that the worm-burrows which I observed some time ago occur.*

Fourthly, he alleges imitative forms which Profs. King and Rowney consider to be "identical with the thing itself." Now, imitative forms are not unknown to palæontologists. I have seen rill-marks figured as fossil leaves, and trails of worms and other mere markings, as fossil plants of various kinds; and many dendritic crystallisations are wonderfully like mosses and algæ. I have on my table at this moment a curious group of rounded concretions of black oxide of manganese in a coal-formation sandstone, which I received a few days ago from a very judicious collector, who believed that it was an undescribed fruit. But such things do not invalidate the evidence of real fossils. It is to be observed, however, that while it is extremely easy to assert that such imitative forms are identical with fossils, and even to make this appear plausible in descriptions and drawings, careful examination of actual specimens, with attention to chemical conditions and modes of occurrence, may be necessary in order to draw the proper lines of distinction. In the case of Eozoön, the imitative form has neither been shown to unite the general arrangement, microscopic structure, and mode of occurrence of the fossil, nor perfectly to resemble it in any one of these respects.† In so far as my own comparisons have extended, I am prepared to demonstrate the difference between all such crystalline, dendritic, and concretionary forms, and the Canadian Eozoön.

Your correspondent merely confines himself to general assertions and to starting difficulties. His authorities, Profs. King and Rowney, in the paper above referred to, have ventured on the more dangerous ground of constructive criticism; and have endeavoured to explain the way in which they suppose Eozoön to have been produced. In doing so they have been obliged to resort to an extravagant and complex theory of pseudomorphism, which I fancy most of the palæontologists will throw down in despair of comprehending it, and which I am sure any competent mineralogist or chemical geologist who studies it, will reject as much more trying to his faith than anything required to explain the occurrence and preservation of Eozoön as a fossil.

Lastly, your correspondent desires further investigations with reference to the questions involved in the organic character of Eozoön. It may satisfy him to be informed that Dr. Hunt and I have just sent to Dublin a reply to the objections of Profs. King and Rowney, in their paper above referred to; and that I have for some time been pursuing investigations of Primordial and Silurian fossils akin to Eozoön either in structure or mode of preservation. When these investigations are completed, I hope to show that Eozoön has several foraminiferal successors in the older palæozoic rocks of Canada, and that fossils of various kinds occur in those rocks infiltrated with mineral matters in a manner not dissimilar from that observed in the Laurentian Eozoön. J. W. DAWSON

McGill College, Montreal, Jan. 18

Natural Science at Cambridge

"M.A." will best satisfy himself as to the grounds for the sentence which appeared in NATURE for January 12, to which he

* Journal of Geological Society, xxii. 608.
† Messrs. Rowney and King themselves virtually admit this.

demurs, respecting the willingness in Cambridge to award fellowships for merit in Natural Science, by making inquiries of the tutors of the several colleges. This I trust he will do, and if he takes the opportunity of impressing upon them the advantage of following the example set by Trinity of absolutely offering a fellowship or fellowships as the reward of great proficiency in Natural Science, he will be doing a great service, for that is unquestionably an important desideratum. At the same time, I would ask him to take the trouble to ascertain whether there have recently been in Cambridge any persons of great proficiency in Natural Science to whom fellowships have not been awarded. A close and fair examination of the matter will, if I mistake not, prove to him that the colleges have not been backward in this way in rewarding real merit in Natural Science; that there is in some colleges not only a willingness but an anxiety to do this; and that the arena is opening for Natural Science to enter the lists against Classics and Mathematics, with the prospect of a fair adjudication of fellowship prizes. "M.A." will do a further great service if he can turn his wide acquaintance with the members of various colleges to account by inducing the colleges to offer more Scholarships for proficiency in Natural Science, instead of limiting them so much, as is at present done, to students who have not commenced a University career. The stimulus thus afforded to the study of Natural Science by undergraduates would have the effect of producing a greater number of candidates deserving fellowships, and the more frequent award of fellowships to them.

THE WRITER
Feb. 4 OF THE ARTICLE IN QUESTION

Prismatic Structure in Ice

DURING the late frost and subsequent thaw, I have watched the ice as far as was in my power to see whether its demeanour bore out Mr. Langton's explanation of the prismatic structure (NATURE, vol. iii. p. 105) in reply to my communication (Id. vol i. p. 481). At the time when I received his letter, I felt unable to accept the compromise which he proposed, and the result of these last investigations has been to confirm my previous opinion. Let me, however, first explain away a slight misconception into which I have led him. In using the words "*severe* frost," I spoke as an Englishman, and used the epithet relatively, without thinking how it might be understood by one accustomed to a colder climate. All I meant was a frost severe enough to form ice more than an inch or so thick.

I will first give you the result of my observations, and then proceed to answer the questions which Mr. Langton proposes.

On December 26, while skating for some hours on a pool in Hagley Park, Rugeley, I examined the ice carefully, but could detect no trace of the prismatic structure. The air bubbles, generally more abundant towards the lower side of the ice, were irregularly dispersed, and not in any way arranged in vertical lines, and the ice had its usual fracture. On Tuesday, January 3, and the following day, I again skated on the same pool, and could not ascertain that any noteworthy change had taken place during my absence in such parts of the ice as were free from snow. Then came a thaw, on the first day of which I visited the pool; the sloppy state of the surface, owing to the melted snow, made examination difficult; but on breaking the ice, I detected occasional traces of the prismatic structure. On returning in the afternoon of January 7, I found that a slight frost during the morning had been sufficient to make the ice safe, though the surface was still wet in places. Now, however, there was a marked change in its appearance; in many spots the delicate surface-reticulation caused by the prismatic structure could be detected; small air-bubbles, as it seemed to me, were more numerous, and very many of them were arranged in vertical lines, which when produced, met the angles of the surface-polygons. In a few cases they were not in vertical but in sloping lines; here it was evident that from some accidental cause, the sides of the prisms had not been at right angles to the surface of the ice; this, however, was rare. Everything that I saw convinced me that the lines of bubbles as a rule were the consequence of the prismatic structure, not the cause of it. The quantity of snow and sleet that subsequently fell made it impossible again to examine the ice satisfactorily before I left the neighbourhood, though I visited the spot more than once. Yesterday and to-day I have been examining the ice on a pond in the Botanic Garden here, which is gradually melting away. It exhibits almost everywhere the delicate prismatic structure which I described last year. On withdrawing fragment after fragment, one to two inches thick, from different parts of the pond, I found the surface covered with a most delicate reticulation, and the edges crenulated, as though the whole were a model of a sheet of columnar basalt. The distance between opposite angles of the surface-polygons was generally from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch. Here and there the ice-slab was partially or wholly perforated, the surface-water having penetrated at the junction of a set of joints, and dissolved away more or less of the adjacent columns; in not a few cases these apertures were polygonal, whole columns having perished. There were, of course, many air-bubbles; but I could see nothing to lead me to suppose that they had caused the structure; thousands more would have been required than I could discover, for in many places where the structure was very perfectly exhibited there was hardly a bubble to be seen.

With regard to the first of Mr. Langton's questions, "Does ice contract on approaching 32° F.?" Jamin ("Cours de Physique," vol. ii. p. 108) states that it does; and though he does not mention either the amount of contraction or the point of minimum density, the former, from his illustrative diagram, appears not inconsiderable. (2.) "Do air-bubbles form from the first in vertical lines?" I should say not, as a rule. (3.) "Is there any indication, as the winter advances, of a re-arrangement of the bubbles, as that they run into each other, and get more and more ranged in vertical lines?" None that I can discover. (4.) "Is there any indication in the earlier stages of the ice that after a night's hard frost the cracks seen on its surface spread through its substance?" I have not observed any. The fifth question I have not at present the means of answering; but that does not materially affect the point in debate.

I may add that while visiting the Schafloch Glacière, during the past summer, I noticed that, near the entrance of the cave, the ice everywhere showed prismatic structure, but that near the extremity, where the temperature was at the time about 30° F., there was little, if any trace of it.

One word in conclusion to this long letter, on a point of practical importance. I am now convinced that this minute prismatic structure is the rule rather than the exception in a thaw, though it has hitherto been overlooked; and that it is the chief cause of the "rottenness" in ice. When it has been set up, a slab of ice 5 or 6 inches square, and full one inch thick, can be broken across as easily as if it were a cake of bread; and slabs little larger will snap in two when dropped flat on level turf from a height cf less than eighteen inches. Almost every thaw brings its melancholy tale of persons drowned while skating on ice which, though thick, proves on trial to be "rotten." I have no doubt that in every one of these cases it had become prismatic. Therefore all who are about to venture on the ice, when a thaw is setting in, should look carefully for the signs of this structure, and if it is present keep their distance.

T. G. BONNEY

St. John's College, Cambridge, Jan. 17

Coming Home from Sicily

THE Eclipse Expedition has gone forth and returned, having on the whole been most successful in its observations. But never, perhaps, was science pursued under greater difficulties. The hot haste in which all the necessary preparations and arrangements had to be made and perfected before departure is already well-known; the combination of circumstances which impeded our locomotion on the outward journey to Sicily; the slight mishap on the Brenner, and the unfortunate loss of the beautiful *Psyche* have been noised abroad, but the return journey was not accomplished without mishaps, although nothing has been said about them. The stars in their courses fought against Sisera not more relentlessly than did the elements against us.

The Eclipse over, with all possible speed we packed up our instruments and started for Naples, being anxious to hasten homewards. We reached Messina on Monday, 26th December, with the full intention of sailing immediately. But a sirocco prevailed, which snapped the telegraph cable to Naples, and prevented our finding out anything about the movements of the steamer. Daily we expected the arrival of one from some quarter or other which might take us back, but we hoped against hope that each day would be our last in Sicily. It was not until late on Friday afternoon, Dec. 30th, that our hopes were answered, and we weighed anchor.

In due time we reached Naples (after a rough passage), and there our party divided. Between Naples and Rome the communication was open, but, on arriving at Rome, we found very evident traces of the recent inundations, which are said to express the indignation of Providence or the delight of Father Tiber at the downfall of the Pope's temporal power. The regular route from Rome to Florence *viâ* Foligno and Perugia, was no longer available, as the flood had carried away most of the bridges on the railway, so we were compelled to take the other and very circuitous route *viâ* Civita Vecchia, Pisa, and Empoli. From Florence, after a delay of two days, during which large quantities of snow fell, we made an attempt to reach Bologna on January 9th, but our ill-luck still followed us, and we had to return whence we had come.

By the advice of some Italian friends, we decided to wait a few days at Florence before making a second attempt to cross the Apennines. Luckily we succeeded the next time in crossing the mountains and leaving Lombardy without any great inconvenience, for by this time the four feet of snow which had covered the country everywhere had been cleared from the rails. We experienced no further difficulty until we arrived at Brenner, where, after remonstrances on our part, we were turned out in a heavy snowstorm to find our way as best we could to the nearest hotel. The next morning we found that the cause of the delay was an avalanche, which, descending a very short time before our train came up, and carrying with it in its downward course trees and rocks, had effectually blocked up the line of rails. Vague rumours reached the hotel that no trains would pass for a week, that two battalions of Austrian soldiers were cutting a way through the snow, and that the avalanche was 200 yards in length, 80 feet in height, and extending across the gorge from side to side. We had no means of verifying these statements, as the telegraph wires were broken, and the officials were evidently as much in the dark as ourselves. Twenty-four hours after us the train bearing the Indian mails from Brindisi came up to the same spot, the passengers were treated as we had been, and ordered to turn out at midnight in happy ignorance of the cause of the delay. The mails were, however, sent up as near as possible to the obstruction, and thence carried over the tops of the mountains for a distance of eight miles, to a train which was waiting on the further side. The cold was intense at Brenner, and the depth of the snow confined us to the hotel. The weather, however, was only such as might be expected on the Alps, but we had good reason to fear from famine, as each successive train brought up regularly from Verona its freight of passengers, and discharged them all at Brenner, until the two hotels were full and overflowing. On the evening of the third day, as we sat at dinner, tidings came that the line was once more clear, and that a train would probably start that evening for Munich; so, thankful to quit the dull monotony of Brenner, we left by the night train, and after a delay of two days in passing through the disturbed parts of Germany, we arrived safely in England, having spent eight days *en route* from Florence. W. A. HARRIS

St. Michael's Mount

IN the last number of NATURE there appears a letter from Mr. R. A. Peacock, of Jersey, in which he attempts to prove that St. Michael's Mount, in Cornwall, was insulated in the eleventh century. To do this, he quotes the passage from Domesday relating to the lands held by the church of St. Michael, which he translates—"Keiwal holds the church of St. Michael," &c. Now this land, Mr. Peacock says, was 240 acres, but the area of the Mount is now only 30 acres, so that there are 210 acres missing, therefore it could not at that time have been an island, because, in the eleventh century, it contained at least eight times as much land as it does at present.

Unfortunately for this theory, the passage which Mr. Peacock translates "Keiwal holds the church of St. Michael" is really "The church of St. Michael holds Treiwal, or Treuthal," as it is called on p. 11, which is a manor in the parish of St. Hillary, Cornwall. H. MICHELL WHITLEY

Penarth, Truro, Feb. 6

The Zodiacal Light

IN NATURE for January 26, in the course of an interesting account of the Augusta Eclipse Expedition, by Prof. W. G. Adams, of King's College, there is a short allusion to the Zodiacal Light, which can hardly fail to be looked on by many as being, both on account of the author and the occasion, authoritative as well as important and instructive:—"At about 6^h 30^m on Monday evening (runs the article in question) we saw a brilliant display of the zodiacal light, consisting of brilliant pink streamers, stretching perpendicularly to the horizon, the planet Jupiter being just on the most brilliant streamers. Towards the north and round the horizon there were also streamers," &c.

Until assured by the author that the word "zodiacal" is not a misprint from something else, it is hardly worth while for me to point out in detail that the above description mentions almost everything which does *not* belong to the true zodiacal light, and nothing which does belong to or characterise it as hitherto known amongst astronomers. C. PIAZZI SMYTH

15, R. Terrace, Edinburgh, Feb. 1

The Reign of Law

THE following is an extract from a letter I lately received from a friend of mine who is on the Geological Survey of India:—

"The Duke of Argyll and his Council have determined that the leave-rules, which are good enough for the natives, are good enough for us, although they are not sufficiently good for educated Europeans, such as the Staff Corps and Civil Service, and this they have resolved on, in spite of the Indian Government. If we could only get fair leave and pension rules, the same as men of the same rank and education receive in the other services, I do not think I should be tempted to give up field-work."

May I be allowed to inquire whether this is a new illustration of "The Reign of Law?" TANTALUS

Misadventures in Conchology

MY experience seems to me curious; is it unique? I don't complain, I simply inquire.

1. As channel of communication for a foreign friend, I sent copies of his valuable conchological work to three public libraries in Great Britain. I know they were received. They have never been acknowledged.

2. I sent a complete set of shells, of a specially interesting field, to a foreign collection; they were delivered by a friend. They have never been acknowledged.

3. I sent (at his request) to a man of science a number of the rarer shells of a district. He acknowledged them through his clerk.

4. To a dealer at his urgent request and offer of exchanges, I sent a quantity (some hundreds) of shells. He sent me in return less than half what he had promised, selected from my list at his fancy.

5. To a scientific man in America, at his earnest request and offer of exchange, I sent a set of the shells of a district. No answer whatever.

6. I sent a unique specimen of a shell to a foreign conchologist for examination. I have never heard more of it.

7. At the earnest request of a dealer offering exchange, my brother sent a great number of Scotch glacial shells. His letter of request for desiderata was returned unopened.

8. A friend near me has more than once sent shells to German collectors, but has the same sad tale of packets sent, and no promised returns made.

Are these experiences exceptional, or is conchology fatal to conscience, or are *all* men liars? VALLE

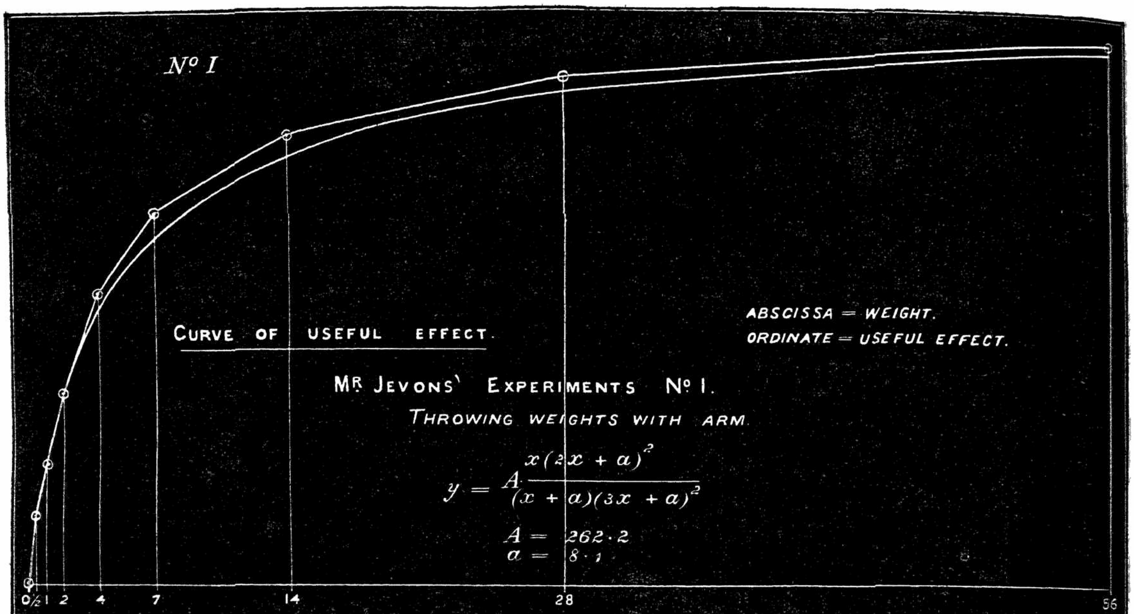
ON THE NATURAL LAWS OF MUSCULAR EXERTION

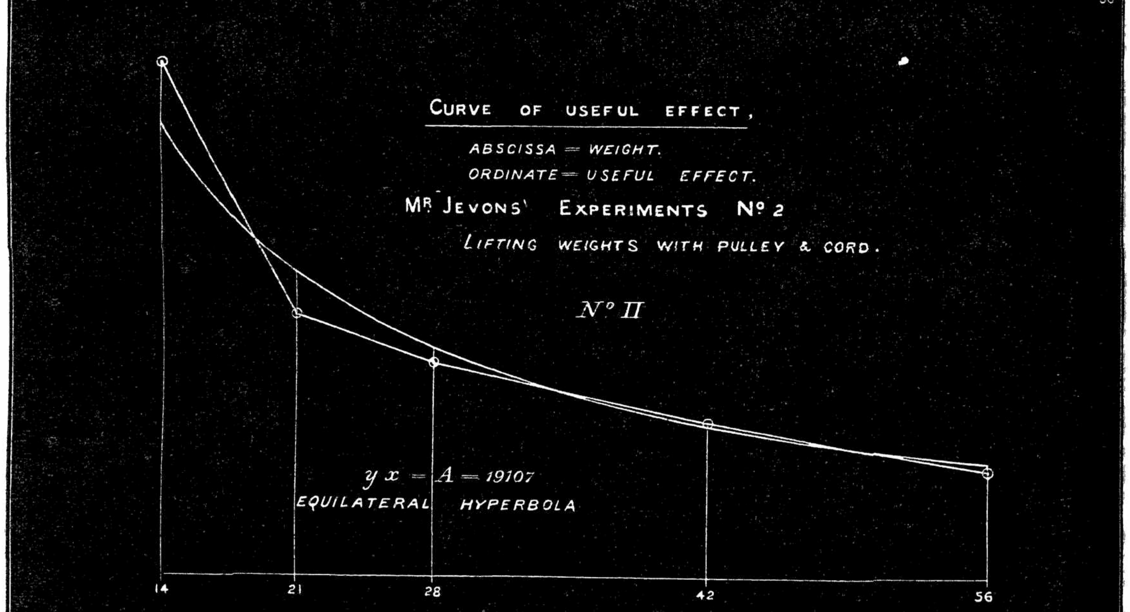
II.

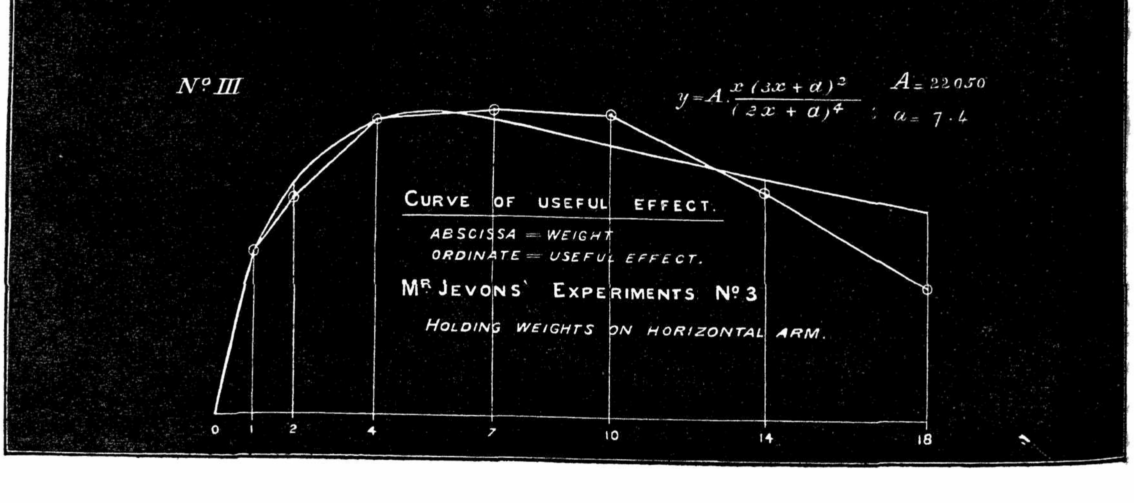
HAVING shown* that Mr. Jevons' first and third sets of experiments illustrate laws 1 and 2 of muscular action, it remains for me to apply the same laws to his second set of experiments, and to show that they also illustrate these laws.

Before doing so, a few words may be said on the subject of the *maximum* of useful effect. I have shown, from theory, that a *maximum* of useful effect is obtained in holding out weights horizontally in the hand, by using a weight which is 73 per cent. of the weight of the arm

* See NATURE, vol. ii. p. 324.


Nº I
Curve of useful effect.
Abscissa = weight.
Ordinate = useful effect.
Mr. Jevons' Experiments Nº I.
Throwing weights with arm.
$y = A\frac{x(2x+a)^2}{(x+a)(3x+a)^2}$
A = 262·2
a = 8·1
0 ½ 1 2 4 7 14 28 56


Curve of useful effect,
Abscissa = weight.
Ordinate = useful effect.
Mr. Jevons' Experiments Nº 2
Lifting weights with pulley & cord.
Nº II
$yx = A = 19107$
Equilateral hyperbola
14 21 28 42 56


Nº III
$y = A.\frac{x(3x+a)^2}{(2x+a)^4}$
A = 22050 ; a = 7·4
Curve of useful effect.
Abscissa = weight
Ordinate = useful effect.
Mr. Jevons' Experiments Nº 3
Holding weights on horizontal arm.
0 1 2 4 7 10 14 18

itself, and this result agrees with the experiments. In throwing weights with the arm Mr. Jevons found that there was no true maximum of useful effect, and that it increases with the weight used.

It can be shown from the equations used by me, which are deduced from Law 1, that this result might have been predicted from theory.

The useful effect is proportional to

$$w \times R,$$

or to

$$w \times l^2,$$

which, by equation (2), becomes

$$w \times \frac{a^2}{l^2} v^2;$$

this expression varies as the following, by equation (3)

$$w \times \frac{(2w + x)^2}{(3w + x)^2} v^2;$$

and since, by equation (1) or Law 1, v^2 varies inversely as $w + x$; we obtain finally

$$\text{useful effect} = \frac{Aw(2w + x)^2}{(w + x)(3w + x)^2} \qquad (13)$$

The condition necessary to make this expression a maximum is

$$w(2w + x)(9w + 7x) = (w + x)(3w + x)(6w + x);$$

which reduces to the quadratic equation

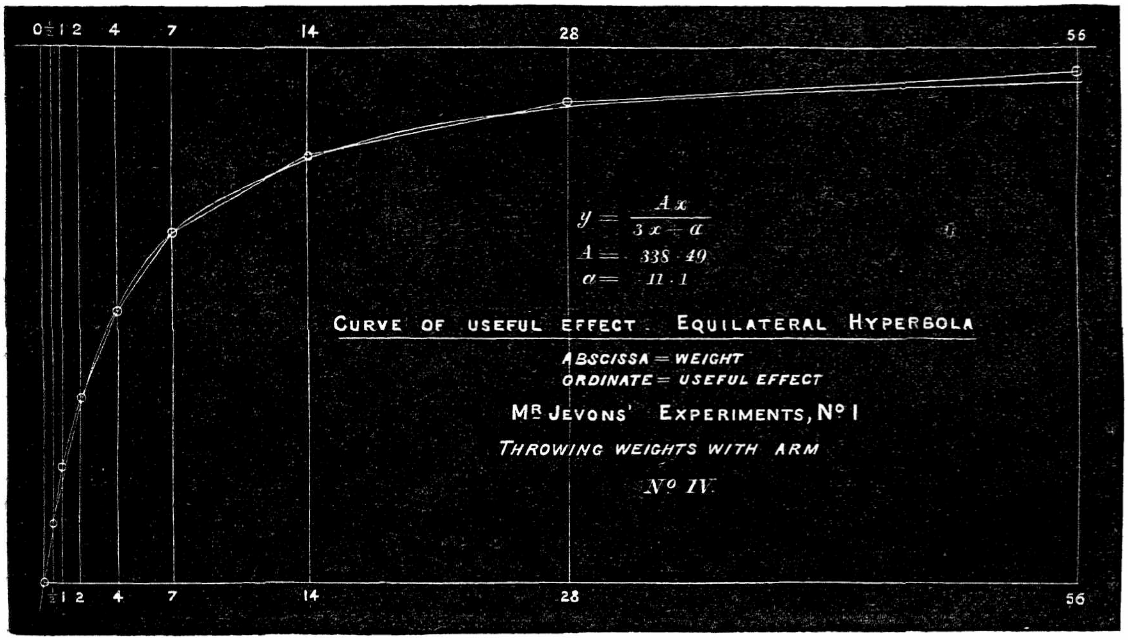
$$4w^2 + 3xw + x^2 = 0 \qquad (14)$$

This equation has imaginary roots, viz.:

$$w = x\left(\frac{-3 \pm \sqrt{-7}}{8}\right)$$

Hence there exists no real value for the weight thrown which will make the *useful effect* a maximum.

Mr. Jevons' second set of experiments consisted in raising and lowering various weights by a pulley and cord, through the convenient range of the arm, and noting the number of times the weights were raised, the rapidity of the motion being maintained constant.



The results of these experiments are

Weight.	No. of times raised.
56 lbs.	5·7
42 „	11·9
28 „	23·0
21 „	37·6
14 „	110·0

It is easy to see, on theoretical grounds, that the weight (x) of the arm will disappear from the equations that represent the work done in these experiments; for in raising the weights the work done is proportional to

$$(w - x)\, n,$$

and in lowering the weights the work done is

$$x\, n.$$

These two, added together, give for the total

$$\text{Work done} = w\, n.$$

I have verified this anticipation of theory by introducing x into the *ten* equations furnished by the *five* simultaneous values of w and n, and I find that the mean value of x turns out to be

$$x = 0{\cdot}18\text{lb.},$$

a value which, if the experiments were absolutely accurate, ought to become zero.

The rate of work is proportional to w, and multiplying this by the work done, in accordance with Law 2, we find

$$w^2 n = A \qquad (15)$$

This equation gives the following values of A, corresponding to the five simultaneous values of w and n:

A	w
17875	56 lbs.
20992	42 „
18074	28 „
16582	21 „
21560	14 „

The mean of these values is

$$A = 19017.$$

Solving equation (15) for n, we find

$$n = \frac{A}{w^2}. \qquad (16)$$

Substituting for A and w, in this equation, we find the following comparison of theory and observation:

w	n (observed).	n (calculated).	Difference.
56 lbs.	5·7	6·0	$-$ 0·3 ($\frac{1}{19}$)
42 "	11·9	10·8	$+$ 1·1 ($\frac{1}{11}$)
28 "	23·0	24·2	$-$ 1·2 ($\frac{1}{19}$)
21 "	37·4	43·1	$-$ 5·5 ($\frac{1}{7}$)
14 "	110·0	97·0	$+$13·0 ($\frac{1}{8}$)

This table is satisfactory, as the differences are less than possible errors of observation.

The *useful* effect admits of no maximum in these experiments; for it is represented by wn, and

$$w \times wn = A;$$

therefore—

$$wn = \frac{A}{w}; \qquad (17)$$

which represents an equilateral hyperbola; wn becoming infinite when the weight is zero.

The agreement between Laws 1 and 2 and Mr. Jevons' experiments, may be best shown by means of the accompanying tables and diagrams, which represent the curves of useful effect, calculated from Laws 1 and 2.

First Set of Experiments.

Throwing weights.—It follows from equation (5) that the useful effect

$$= wR = A \frac{w\,(2\,w + x)^2}{(w + x)\,(3\,w + x)^2} \qquad (18)$$

Substituting in this equation, the values

$$A = 262·2$$
$$x = 8·1$$

we obtain the following table:—

Useful Effect No. 1.

w	$w\,R$ (observed).	$w\,R$ (calculated).
0	0	0
$\frac{1}{2}$	13·2	13·6
1	23·6	23·8
2	39·2	38·2
4	58·6	55·6
7	74·3	70·1
14	90·4	86·4
28	101·6	98·5
56	108·1	106·5

In the accompanying curve No. 1, the abscissa is the weight, and the ordinate the useful effect; and the centres of the small circles represent the actual observations.

Second Set of Experiments.

Lifting weight with pulley and cord.—In these experiments the useful effect wn may be at once calculated from equation (17), from which the following comparison is made, and Diagram No 2 constructed.

Substituting for A, its value 19,017, we find—

Useful Effect No. 2

w	wn (observed).	wn (calculated).
14 lbs.	1554	1358
21 "	790	905
28 "	644	679
42 "	500	453
56 "	319	340

The curve represented in Diagram 2 is a portion of an equilateral hyperbola, whose abscissa is the weight, and its ordinate the useful effect.

Third Set of Experiments.

Holding weights on hand extended horizontally.—The useful effect in these experiments may be calculated from equation (11).

$$\text{Useful effect} = w\,t = A\,\frac{w\,(3w + x)^2}{(2w + x)^4}. \qquad (11)$$

Substituting for A and x their values

$$A = 22050$$
$$x = 7·4$$

we obtain the following comparison, and construct Diagram No. 3.

Useful Effect No. 3.

w	wt (observed).	wt (calculated).
0	0	0
1	321	306
2	438	468
4	592	590
7	612	594
10	603	547
14	455	479
18	266	421

In Diagram No. 3, as before, the abscissa of the curve represents the weight, and the ordinate denotes the useful effect.

The equations of the three curves which represent the useful effect in Mr. Jevons' three sets of experiments, when expressed in Cartesian co-ordinates, are as follows:—

$$y = A.\frac{x\,(2x + a)^2}{(x + a)\,(3x + a)^2} \qquad \text{No. 1}$$

$$y\,x = A \qquad \text{No. 2}$$

$$y = A\,\frac{x\,(3x + a)^2}{(2x + a)^4} \qquad \text{No. 3}$$

It will be interesting, in conclusion, to explain why the simpler empirical formula, used by Mr. Jevons, coincides so nearly with the more complex formula deduced from theory.

According to Mr. Jevons' empirical formula, the useful work done in throwing weights is

$$\frac{B\,w}{2w + x}$$

where B = 231·4; and, according to my formula, deduced from theory, the useful work is

$$\frac{A w\,(2w + x)^2}{(w + x)\,(3w + x)^2}$$

where A = 262·2.

These two expressions, algebraically considered, can never become identical, but may become nearly so, if

$$(w + x)\,(3w + x)^2 = K\,(2w + x)^3, \qquad (19)$$

in which K is a co-efficient nearly constant.

Expanding both sides we have

$$9\,w^3 + 15\,x\,w^2 + 7\,x^2\,w + x^3$$
$$= K\,(8\,w^3 + 12\,x\,w^2 + 6\,x^2\,w + x^3);$$

from which we obtain four equations to determine the best value of K, viz.:—

$$9 = 8\,K \qquad K = 1·125$$
$$15 = 12\,K \qquad K = 1·250$$
$$7 = 6\,K \qquad K = 1·166$$
$$1 = K \qquad K = 1·000$$

Mean . . 1·135

The co-efficients of the two sides of equation (19) will be most nearly equal, when $K = 1·135$.

The constants used by Mr. Jevons and myself give

$$K = \frac{A}{B} = \frac{2622}{2314} = 1·133,$$

which is about the value required by the preceding considerations.

The curve denoted by Mr. Jevons' equation is an equilateral hyperbola, the co-ordinates being parallel to the asymptotes, and the origin taken upon the curve itself.

It is easy to see, from an inspection of Diagram No. 1, that a portion of such an equilateral hyperbola would coincide nearly with the true curve, and with the points determined by experiment.

Addendum.—Mr. Robert B. Hayward, of Harrow, has kindly called my attention to an error in my application of Law 1 to Mr. Jevons' first series of experiments in which different weights were thrown by hand. In applying Law 1 to these experiments, I assumed, in equations (1, 2, 3, 4), that the total work stored up in a revolving body is that of a body of equal weight, moving with the velocity of the centre of *oscillation*. I should have stated that it is the work of a body of equal weight moving with the velocity of the centre of *gyration*. If this error be corrected, the four equations become much simpler in form, and lead directly to the empirical formula used by Mr. Jevons, and also furnish a curve which corresponds very well with the experiments.

Let k denote the radius of gyration, and let the other symbols remain as before; we find the following equations, which are to be used instead of the first four already given.

$$(w + x)\, v^2 = \text{Const.} \qquad (1)$$

$$v = V\frac{k}{a} \qquad (2)$$

$$\frac{k^2}{a^2} = \frac{1}{3} \cdot \frac{3w + x}{w + x} \qquad (3)$$

$$(3w + x) \times R = A \qquad (4)$$

Mr. Jevons' eight experiments furnish twenty-eight equations to determine the value of x, and the mean of all the values is

$$x = 11{\cdot}1\text{lb.}$$

This value of x, when substituted in the eight equations furnished by (4), gives us the following values of A.

w	A
56 lbs.	329·5
28 „	351·8
14 „	364·2
7 „	338·9
4 „	337·5
2 „	318·9
1 „	325·0
½ „	342·1
Mean	338·49

If the mean values of A and x be used in equation (4), we find the following comparison of theory and observation:—

w	R (*observed*).	R (*calculated*).	*Difference.*
56 lbs.	1·84 feet	1·89 feet	−0·05 feet
28 „	3·70 „	3·56 „	+0·14 „
14 „	6·86 „	6·37 „	+0·49 „
7 „	10·56 „	10·55 „	+0·01 „
4 „	14·61 „	14·65 „	−0·04 „
2 „	18·65 „	19·79 „	−1·14 „
1 „	23·05 „	24·01 „	−0·96 „
½ „	27·15 „	26·86 „	+0·29 „

The curve of useful effect, deduced from equation (4) is

$$wR = \frac{Aw}{3w + x}.$$

This curve represents an equilateral hyperbola, whose equation, referred to its asymptotes as axes of co-ordinates, is

$$xy + 417{\cdot}4 = 0.$$

Diagram No. 4 shows this hyperbola, while the centres of the little circles show the several experiments.

This hyperbola, derived from the property of the centre of gyration, represents the observations very well; and it nearly coincides with the more complex curve derived from the centre of oscillation, shown in Diagram No. 1.

Mr. Jevons' empirical formula corresponds at once to equation (4), provided the quantity (q) be used to denote *one-third* of the weight of the arm instead of *one half*. His formula becomes, multiplying above and below by 3,

$$R = \frac{347{\cdot}1}{3w + 11{\cdot}7}$$

The weight of the arm and shoulder used in throwing weights, viz. 11·1lb. or 11·7lb., is considerably greater than the weight of the simple arm, 7·4lb., a result quite consistent with the fact that the scapular muscles move with the arm in this kind of exertion.

S. HAUGHTON

NOTES

AT a preliminary meeting held at King's College on Monday last, the Rev. Dr. Barry, Principal, in the chair, it was resolved that a fund for a Memorial to the late Dr. Miller, in connection with King's College as having been for thirty years the principal sphere of his labours, be at once raised by subscriptions in the College, and from the general public. It was further resolved to form a committee for the purpose of carrying out this resolution, and as soon as the committee is complete a meeting will be summoned to adopt such further measures as are necessary.

WE are glad to hear that Prof. Frankland has accepted the Presidency of the Chemical Society for the ensuing session.

A CIRCULAR has been issued by the Science and Art Department of the Committee of Council on Education, South Kensington, directing that in future all teachers certificated by the Education Department shall be allowed to earn payments on the results of the instruction of their pupils in Stage I. of Mathematics, according to the rules of the Science Directory, without having previously passed the qualifying examination required by § XXXIII. The Department has also issued the following amended programme of the examinations for the diplomas of the Royal School of Naval Architecture and Marine Engineering, which is to come into force at the examination to be held in April 1872, the examination in April 1871, being held according to the programme contained in the existing Directory of the School:—"Diplomas will be given to all persons, whether they have received their instruction at the school or not, who pass the final examinations of the school, provided that they give satisfactory evidence of having gone through the course of practical work recommended by the Council of the Institution of Naval Architects. These diplomas will be of two grades, according to the success of the candidate in the examination, the title of the higher grade being Fellow, and of the lower, Associate, of the Royal School of Naval Architecture." These examinations will be held annually towards the end of April. Candidates who have not been students of the school will be required to produce certificates that they have been engaged for three years, at least, in—(1) practical wood or iron ship-building in a dockyard; or (2) practical engine and boiler building in a dockyard, or in the works of a marine engineer; or (3) practical work as a draughtsman in a dockyard (or marine engine works), during which the candidate must have himself gone through the complete formation of the design of a ship (or of a marine engine), with the whole of the calculations included in it. Such candidates will also be required to give references as to character and good conduct before they are admitted to the examination. All such candidates must apply to the Secretary, Science and Art Department, South Kensington, W., not later than the 15th March in each year.

IN our notice last week of the attainments of the Senior Wrangler, we should have included the statement which we made some time since, that Mr. Hopkinson took the degree of D.Sc. at the University of London, in 1870, in two distinct branches, viz. Mathematics and Physical Optics, this being the first similar instance on record. Dr. Hopkinson has also attained the additional distinction which so often falls to the lot of Senior Wranglers, that of First Smith's Prize-man. The Second Smith's Prize has been awarded to Mr. Temperley, of Queen's College, bracketed fourth wrangler. It is stated that Dr. Hopkinson, being disqualified as a Nonconformist from sharing in the emoluments of the University, intends following his father's profession of engineer.

THE following are the Council and Officers of the new Anthropological Institute of Great Britain and Ireland:—President: Sir John Lubbock, Bart., F.R.S.; Vice-Presidents: Prof. Huxley, F.R.S., Prof. Busk, F.R.S., Mr. John Evans, F.R.S., Dr. Charnock, Dr. Barnard Davis, F.R.S., Mr. George Harris; Director: Mr. C. Staniland Wake; Treasurer: Mr. J. W. Flower, F.G.S.; Council: H. G. Bohn, Colonel Fox, Dr. Hyde Clarke, W. Blackmore, W. Boyd Dawkins, F.R.S., R. Dunn, David Forbes, F.R.S., T. M'K. Hughes, Dr. A. Campbell, S. E. Bouverie Pusey, W. C. Dendy, Sir D. Gibb, Bart., Dr. R. King, Capt. Bedford Pim, R.N., Rev. D. I. Heath, Dr. J. Beddoe, Dr. George Harcourt, Joseph Kaines, F. G. H. Price, and C. Robert des Ruffières. Secretary: J. F. Collingwood; Sub-editor of Journal: F. W. Rudler. A Special General Meeting of the Anthropological Society will be held at the rooms, No. 4, St. Martin's Place, on the 14th February, at half-past seven o'clock, for the purpose of authorising the trustees of the Society to transfer its funds and effects to the trustees of the "Anthropological Institute of Great Britain and Ireland," in pursuance of an agreement for union under that name between the Anthropological and the Ethnological Societies of London. On the same evening, at eight o'clock, will be held the first ordinary meeting of the Anthropological Institute, when Sir John Lubbock, Bart., President, will read a paper "On the Development of Relationships."

THE Anniversary Meeting of the Royal Astronomical Society will be held to-morrow at three, and that of the Photographic Society on Tuesday next at eight.

THE following lectures on Comparative Anatomy are to be delivered in the Theatre of the Royal College of Surgeons on Mondays, Wednesdays, and Fridays, at four o'clock, commencing on Friday, Feb. 17, by W. H. Flower, F.R.S., Hunterian Professor of Comparative Anatomy and Physiology. Eighteen lectures on the Characters, Structure, Functions, and Modifications of the Teeth and Allied Organs in the Mammalia. Essential characters and structure of teeth. Development and succession of teeth. Classification and nomenclature of teeth—Dental formulæ. Modifications of the characters of the teeth in the different groups of the mammalia. Teeth of Man. Teeth of *Simiina*—Old-World Monkeys and New-World Monkeys. Teeth of *Lemurina.* Teeth of terrestrial *Carnivora*—Dogs and allied forms; Cats and allied forms; Bears and allied forms. Teeth of *Pinnipedia*—Sea-Bears and Seals; Walrus. Teeth of *Insectivora*—Hedgehogs, Moles, Shrews, &c.; Galeopithecus. Teeth of *Chiroptera*—Frugivorous Bats, Insectivorous Bats, Blood-sucking Bats. Teeth of *Rodentia*—Hares, Guinea Pigs, Porcupines, Rats, Squirrels, &c. Teeth of *Cetacea*—Odontocetes or Toothed Whales; Dolphins, Porpoises, Narwhal, Sperm-whale, Ziphius, and allied forms; Zeuglodonts; Mystacocetes or Whalebone Whales, rudimentary Teeth; Structure and function of baleen or whalebone. Teeth of *Ungulata*—Perissodactyles: ancient and modern forms, Palæotherium, Horse, Rhinoceros, Tapir; Artiodactyles: Pigs, Hippopotamus, Anoplotheridæ, Camels, Chevrotains, and Pecora (Deer, Giraffe, Antelopes, Sheep, Goats, and Oxen). Teeth of Hyrax, of Toxodon, of Typotherium, and other anomalous forms. Teeth of *Proboscidea*—Elephant, Mastodon, Dinotherium. Teeth of *Sirenia*—Dugong and Manatee. Teeth of *Edentata*—Sloths, Anteaters, Armadillos. Teeth of *Marsupialia*—Opossums, Thylacine, Dasyures, Perameles, Phalangers Kangaroos, Wombats; Fossil Marsupials. Value of dental characters in drawing inferences as to the affinities and habits of extinct animals. Horny teeth of *Monotremata*—Ornithorhynchus. The lectures are free to all who take an interest in the subject.

INTELLIGENCE has been received of the death of Dr. F. A. G. Miquel, Professor of Botany at the University of Utrecht and Director of the Botanic Gardens at Leyden, one of the few remaining distinguished systematic botanists on the Continent, and greatly esteemed by all scientific visitors to Holland for his generous hospitality and kindly nature. He was the author of several monographs, and had written largely on the botany of the Netherland-Indies, especially Java and Surinam; and also on that of Japan, which has been much worked out by Dutch naturalists; but will be best known by his magnificent "Annales Musei botanici Lugduno-batavi," in four folio volumes, with splendid illustrations. He was one of the foreign members of the Linnean Society of London.

A STATEMENT having appeared in the *Globe* that the M.A.'s of the University of London have made a representation to the authorities that the examinations for degrees are too stringent, and that it is probable that the standard will be lowered; we are authorised to state that the report is entirely incorrect; no such representation has been made, and no proposal for lowering the standard of the degrees is under the consideration of the Senate of the University.

IN consequence of a report made to the Senate of the University of London by the Examiners in Physiology, setting forth the insufficiency of the present *viva voce* examination as a test of the practical acquaintance of candidates with histology and practical physiology, an important alteration has been made in the regulations of the First Bachelor of Medicine Examination. The alteration will take effect in the year 1872 and subsequent years, and the Registrar has been directed to notify to the various medical schools in connection with the University, that the Candidates for the first Examination in Bachelor of Medicine will be required to pass a practical examination in Histology. The requisite provision is to be made in the University building for carrying out a plan which has been suggested by the Examiners, and approved of by the Senate, and of which the following outline is given by the *British Medical Journal*:—The candidates are to be examined in convenient batches, placed in a suitable room, fitted up with microscopes, glasses, reagents, needles, scissors, and razors. Each student is to have placed before him a few characteristic mounted specimens, and portions of fresh tissue or tissues prepared for minute examination, all numbered and carefully selected, and to be given three hours in which to examine and report upon the specimens presented to him, as well as to mount certain specimens of particular organs or tissues, as may be required by the Examiners. In this way, it is felt, the possession of mere book-knowledge by the candidate will be at once disclosed, and the physiological examination will be made a real and satisfactory one.

A ZOOLOGICAL RECORD ASSOCIATION has been established for the purpose of continuing the *Record of Zoological Literature* (an annual volume, containing an abstract of, and an index to, all that has been done in zoology during the previous year), which has been held in such high esteem by working zoologists that for some time past the British Association has been induced to vote an annual grant of 100*l.* in its support. Owing, no doubt, to the fact of its utility not being sufficiently known to the public, the undertaking has not hitherto proved a financial success. The new Association, which includes, we understand, all the leading zoologists of the country, hopes to have better luck, and in a few days it is expected that its programme will be before the world. Mr, Stainton, F.R.S., is the Secretary.

THE New York Association for the Advancement of Science has recently elected officers for the ensuing year, and the *American Chemist* notes that on looking over the list it finds that, among the names of fifteen officers, it does not recognise a single man of science. Six are clergymen, three are physicians. Though it is pleasant to see these men anxious to promote the cause of science, a little sprinkling of scientific men would tend to increase the confidence of the public in the discussions of the society.

THE *American Chemist* states that Prof. William Griffis, of Rutgers College, has gone to Japan as Professor of Chemistry and Natural Philosophy in the University of Japan.

THE following scientific appointments to American Colleges have recently been announced:—Mr. George C. Swallow, to the chair of Agriculture in the University of Missouri; Mr. Charles H. Wing, to the Professorship of Applied Chemistry, Dr. Alonzo M. Chase, as Instructor in Analytical Chemistry, and the Rev. J. J. Brown, to the chair of Physics, in Cornell University; Mr. T. H. Staver has been elected Professor of Agricultural Chemistry at Harvard University; and Mr. Albert H. Chester has been appointed to the Childs Professorship of Agricultural Chemistry in Hamilton College.

THE Legislature of Colorado, at its last session, appropriated thirty-nine hundred dollars for a building for a school of mines. The corner-stone of that building was laid on Monday, August 15, 1870, by Bishop Randall, in the presence of a large assembly of people.

THE *Scientific American* states that the Board of Trade of the city of Buffalo has obtained a franchise and organised a company to be styled the Oxyhydrogen Gas Company, having for its object the introduction of the oxyhydrogen gas light into that city. A committee of investigation has visited the oxygen gas works in New York, and with the information thus obtained we are informed that the work is to proceed at once. It would appear that Buffalo is to be the first city in America to adopt this splendid light. The experiment is an important one, and its success will be watched with considerable interest by gas-consumers in this country and America.

IT may be interesting to record that during the two mid-winter months of December 1870 and January 1871, the average temperature of the air at Blackheath was, according to Mr. Glaisher's tables, exactly 5° F. below the mean of the last fifty years. On 46 days the temperature was below the average, and above the mean on only 16 days, the mean for the 24 hours having been below the freezing-point on 27 days. The lowest average was on December 25th, when it fell as low as 18·9°, or 18·7° below the mean of the last fifty years. During nearly the whole of January the weather was also exceptionally warm in the neighbourhood of London; in the midland and eastern counties, the depression would be considerably greater.

THE *American Chemist* for January announces that it commences its existence with that number as an independent journal, devoted to Theoretical, Analytical, and Technical Chemistry. The articles, both extracted and original, among which is a curious one on Assaying one hundred and thirty years ago, promise a supply of varied and useful information. It proposes to present its readers with abstracts of all papers published in America or elsewhere, which involve chemical principles in their discussion.

THE *British Medical Journal* states that two additional cases illustrating the efficacy of the injection into the veins of ammonia in snake-bite, on the plan of Professor Halford, are published in the Melbourne papers. They are reported with great decision of statement as to the immediate benefits conferred, but without the scientific precision of detail which is needed, and which will, we hope, be forthcoming.

A MEETING was held in the Town Hall, Stranraer, on Friday, the 5th January, to discuss the advisability of establishing cheese factories in the county on the American system, and a committee was appointed to inquire into the details regarding the management of the Derbyshire cheese factories, and to report at a future meeting.

M. LITTRÉ has received a letter from M. Gambetta, praising him highly for his invaluable works, and nominating him as Professor of History to the Bordeaux Polytechnic School. The letter was published by public authority in the *Moniteur*.

THE new fuel invented for Paris is more substantial than had been supposed. It is the asphalt used for coating the side-ways of the streets. The total length of *trottoirs* is about two million yards, the breadth two yards, and the thickness half an inch, so that the cubic measurement of asphalt ready for use is 555,550 cubic yards. It is mixed with about half of its weight of sand, which reduces the real weight of asphalt to 277,777 cubic yards. It is difficult to burn asphalt without being suffocated with smoke. But all these drawbacks can be met successfully by scientific skill and ability. M. Le Troost, directorof the Grenelle Gas Works, has erected furnaces for using tar as fuel in the distillation of tar. Tar is sufficient without the help of any other fuel to prepare gas for ordinary purposes or for inflating balloons. These furnaces were working successfully from the beginning of November, and this success has probably led to the idea of using asphalt as fuel for ordinary purposes.

ONE rather singular invention for remedying the actual want of fuel in private houses became very popular in Paris during the siege. They prepare cylinders of clay impregnated with bituminous substances; these combustible cylinders are used like the ordinary charcoal which is necessary in Parisian cookery. The earthy matters, of which the proportion is not greater than necessary, remain in the furnaces like ashes left by the combustion of charcoal. It is stated that it is proposed to continue the use of this kind of artificial fuel.

THE *Academy* states that the great attraction of the geological portion of the St. Petersburg Museum consists in one entire gallery of mammoth remains, probably including many nearly entire skeletons, the bones of all of which are in a remarkably fresh state of preservation. Some years since (owing to the exertions of Sir Antonio Brady), an almost entire mammoth's skull, with tusks of colossal proportions, was obtained at the brick-pits at Ilford, in the Thames Valley brick-earth, and now adorns the geological gallery of the British Museum. Since the appointment of M. Dupont to the post of keeper in the Brussels Museum, the almost entire skeleton of a mammoth, found some time since, but neither repaired nor mounted, has been diligently mended up bit by bit, till they are able now to boast the possession of the best example of *Elephas primigenius* out of St. Petersburg. One is struck with the comparative height and slenderness of the mammoth's skeleton as compared with the modern elephant, and it seems probable that he was a more active and a lighter-built beast than the Asiatic or African species. It is to be hoped that the British Museum collection will ere long be enriched by the addition of a skeleton of the Siberian mammouth. The execution of the fortifications around Antwerp has led to the discovery, not only of elephants' and mastodons' remains, but of a most wonderful series of cetacean bones. These are now arranged in a fine gallery in the Brussels Museum, which, under the direction of M. A. de Borre, bids fair to be one of the most attractive of Continental institutions.

SHOCKS of earthquake were felt in Scinde on the 12th November.

ON the 24th October at 1.40 A.M. a smart shock of earthquake was felt at Yokohama, with a deviation from E. to S.W. The shock was remarkable for long duration.

THE NATURE OF THE EARTH'S INTERIOR*

IN a previous discourse on volcanoes,† I directed attention to the phenomena of volcanic action, specially considered in relation to the part which such igneous or internal forces have played in determining the grand features of the external configuration of the sphere upon which we live.

If now we follow up this subject still further, it will naturally lead to an inquiry into the nature of the internal substance of the globe itself, within which the foci of such agencies must be situated; quite independent of this, however, I have little doubt but that many of you must at some time or other have already asked yourselves the question—What does the central mass of the earth beneath us consist of?

The answer which, in the first instance, would, I imagine, be most likely to suggest itself to your minds, would be that it consisted of solid stone, such as you see forming the body of its mountains, the foundations of its continents, and the rock basins which contain its seas. The belief in such an hypothesis would, however, be rudely shaken by the first personal experience of the shock of an earthquake, the sight of a volcano in eruption, or the consideration of the immense faults which have dislocated many parts of the solid land; since, so far from disposing us to regard the ground under us as entitled to the appellation of the *terra firma*, so commonly used by the ancients, the study of such phenomena could not but suggest grave doubts in our minds as to whether the earth was after all anything like so solid or stable as we at first sight felt inclined to imagine.

But very little inquiry into the subject is necessary, however, to convince any one of the great difficulties in the way of obtaining a satisfactory answer to this question, and to prove that at present we do not have at our command sufficient data or evidence to enable us to arrive at a thoroughly conclusive solution of this most interesting problem.

As the rapid advances made by the natural sciences are, however, daily adding to our information on this subject, and thus enabling us to correct or modify our previous deductions, so as to form a more and more trustworthy opinion on the nature of those parts of our globe, which, from their position, must always remain inaccessible to our powers of direct observation, I have imagined that a short sketch of the present state of our knowledge concerning the probable constitution of the interior of the earth might prove interesting.

In treating this subject, we will first consider what has already been done in the way of direct examination of the earth's substance in depth; yet when it is remembered that the mean diameter of our planet is some 7,912 miles, whilst the greatest depth hitherto attained by man's direct exploration has not even yet reached one mile from the surface downwards; this disproportion appears so enormous as to render it self-evident, in the pursuit of this inquiry, especially as regards the more central portions of the earth, that we must in the main rely upon data furnished by calling in the aid of the natural sciences. The direct examination of the exterior of the earth, even when restricted to this depth, does nevertheless furnish us with many important data from which to start in this to a great degree speculative inquiry, and to some of these we shall now direct attention.

It must first of all be remembered that all the rocks which we meet with at the surface, and which compose so much of the solid exterior of our globe as is actually known to us, may be arranged under two principal heads, viz., the volcanic or endogenous rocks, *i.e.* those formed within the body of the earth itself, and the sedimentary or exogenous, *i.e.* those rocks formed, or rather reconstructed upon its surface, out of the *débris* of previously existing rocks arranged in beds or strata by the mechanical action of water.

It was long taken for granted by geologists that the lowest sedimentary strata, in their normal or in a more or less altered condition, rested directly upon granite, which was for a long time regarded as the foundation upon which they, in the first instance, had been deposited, since this rock was looked upon as the oldest of all, and as representing the primeval or original surface covering of the earth. Later researches, however, have proved this hypothesis to be untenable, since it appears that no instance of a granite has as yet been met with in nature, which if followed up, does not at some place or other break through, and alter or disturb more or less, the stratified rocks in immediate contact with it, so that it naturally follows that such stratified rocks must have preëxisted on the spot, or in other words, that they were older in geological chronology than the granite which came to disturb them.

In the present state of geology, however, it is utterly impossible for us to point out any variety of rock whatsoever as the one which may have served as a foundation upon wh ch the oldest sedimentary rocks were originally deposited, in fact the oldest rocks which we know of at present are themselves sedimentary rocks (mostly in an altered condition) belonging to the Laurentian series in Canada, and as yet it has not been found out what these sedimentary beds may in their turn rest upon, *i.e.*, what is actually below them.

As therefore we have not as yet been able to reach down to investigate directly any rocks lower in the geological series than those pertaining to the Laurentian formation, we will now turn to the volcanoes, in order to examine the mineral products which they bring up for our consideration, from depths vastly beyond those which we can ever hope to reach directly. What volcanoes teach us with regard to the nature of the earth's interior, at a depth from which they derive their supply of mineral matter, may be summarised as follows:—

That at this depth, the earth's substance exists in a state of perfect molten liquidity, forming as it were a sea of molten rock, analogous in character to the eruptive rocks which have broken through the earth's crust in former times. Secondly, that the mineral products ejected from volcanoes are very similar to one-another in chemical and mineral constitution, no matter from what part of the globe they may emanate from. And, lastly, that from the same volcanic orifice, and during the same eruption, lavas of two totally different classes may be emitted, viz., the light, acid, or trachytic lava, analogous to the granites, felsites, &c., of the oldest period, and the heavy basic or pyroxenic lava, all but identical with the dark basaltic or trappean rocks commonly met with as dykes, &c., disturbing most of the different sedimentary formations.

Another deduction from the study of volcanic phenomena, indicating that at a certain depth below their surface they must be in connection with a continuous sea of molten lava, is based upon the influence which the moon appears to have on volcanic eruptions; this opinion seems to have been confirmed by the observations of Prof. Palmieri made during the last eruption of Vesuvius, on which occcasion he reported that distinct tidal phenomena could be recognised, thereby indicating that the moon's attraction occasioned tides in the central zone of molten lava, in quite a similar manner as it causes them in the ocean. A further corroboration of this view is seen in the results of an examination of the records of some 7,000 earthquake shocks which occurred during the first half of this century, compiled by Perry, and which, according to him, demonstrate that earthquakes are much more frequent in the conjunction and opposition of the moon than at other times, more so when the moon is near the earth than when it is distant, and also more frequent in the hour of its passage through the meridian.

Returning now to the more direct examination of the superficial parts of the earth, we find that the results of mining operations have also thrown considerable light, not only on the mineral nature of the rocks encountered in depth, but also upon some of their physical conditions. A numerous set of experiments made in deep mines in various parts of the world, often far distant from one another, has most conclusively proved that the temperature of the earth, at least as deep down from the surface as has been explored by man, increases in direct ratio as we descend towards its centre. Other observations on the temperature of the water from deep-seated and hot springs, and from artesian wells, fully confirm the experiments made in mines, and show that the temperature of the water furnished by them also becomes higher in proportion to the depth of the source from which it is derived.

As might naturally be expected, the interference of local causes renders it a matter of considerable difficulty to determine the true mean general rate of such increase in temperature of the earth's substance downwards; still, in the main, observers all agree in placing it at somewhere between $1\frac{1}{2}°$ and $2\frac{1}{2}°$ degrees F. for every hundred feet in depth, so that we cannot be far wrong, if for our purpose we estimate it at 2° F. for every hundred feet in depth, or a rate which amounts to 121° for each geographical mile nearer the earth's centre. Since no facts are at the present time known which can in any way invalidate the supposition that this or a somewhat similar rate of increase in temperature holds good in still greater depths, it is perfectly correct and justifiable reasoning to assume that such is actually

* Substance of a Lecture delivered for the Sunday Lecture Society on January 29.

† See NATURE, vol. ii. p. 283.

the case, and therefore a simple calculation will show that at a depth of about twenty-five geographical miles from the surface downwards a temperature of about 3,000° F. should be attained, which would represent a heat at which iron melts, or one sufficient to keep lava in a state of perfect molten liquidity at the surface of the earth. As it must be remembered, however, that at this depth the substance of the earth would be exposed to the pressure of the superincumbent mass, and as it has been shown by experiment that many substances become more refractory—*i e.*, require a greater degree of heat to melt them—when exposed to pressure than when at the surface, the above calculation will have to be modified considerably in order to meet this condition. Unfortunately, we have not as yet sufficient data at command to enable us to estimate the true ratio in which the melting points of such rocks would become elevated by pressure; yet we may safely take it for granted, after allowing far more than the maximum rate of increase, deduced from the experiments of Bunsen and Hopkins, that we should not require to sink so deep again in order to attain a temperature fully sufficient to keep such substances in a state of fusion, or, in other words, this deduction necessitates the supposition that the solid rock crust of the earth cannot, at the utmost, be more than 50 miles in thickness.

If we now reason from the above data as premisses, it will follow as a natural consequence that our globe must in reality be a sphere of molten matter surrounded by an external shell or crust of solid matter, of very insignificant thickness when compared to the diameter of the entire globe itself; or, in other words, this deduction represents exactly such a state of things as would ensue in the event of a sphere of molten matter becoming consolidated on its exterior by the cooling action of the external atmosphere; and the figure of the earth itself, which is an ellipsoid of revolution, *i.e.*, a sphere somewhat flattened in at the poles, but bulging out at the equator, being that which a plastic mass revolving round its own axis would assume, is regarded by natural philosophers in general as all but conclusive evidence, that the earth at an early period of its history must have been in a fluid condition.

Although the doctrine that the earth is a molten sphere surrounded by a thin crust of solid matter was all but universally taught by geologists, there have of late years been brought forward several arguments to the contrary, which apparently are more in favour of its being a solid or nearly solid mass throughout, and these arguments are fully entitled to our consideration, as our object is not to defend any particular theory, but to arrive as nearly as we can at the truth. I will, therefore, in the first place proceed to scrutinise all which has been brought forward in opposition to the older hypothesis, and then to consider whether any other explanation yet advanced is more in accordance with the facts of the case.

First of all we have to answer the question as to whether it is possible for such a thin crust to remain solid, and not at once to become melted up and absorbed into the much greater mass of molten matter beneath it? This would doubtless be the case, if the central fluid mass had any means of keeping up its high temperature, independently of the amount of heat it actually possessed when it originally assumed the form of an igneous globe. This question, however, in reality answers itself in the negative, since it is evident that no crust could even commence to form on the surface, unless the sphere itself was at the moment actually giving off more heat from its outer surface to the surrounding atmosphere than it could supply from its more central parts, in order to keep the whole in a perfectly fluid condition, so that when once such a crust, however thin, had formed upon the surface, it is self-evident that it could not again become melted up or re-absorbed into the fluid mass below.

This external process of solidification due to refrigeration would then continue going on from the outside inwards, until a thickness of crust had been attained sufficient to arrest or neutralise (owing to its bad conductibility of heat) both the cooling action of the surrounding air and the loss of more heat from the molten mass within; and thus a stage would soon be arrived at when both these actions would so counterbalance one another that the further cooling down of the earth could be all but arrested: a condition ruling at the present time, since the earth-surface at this moment, so far from receiving any or more than a minute amount of heat from the interior, appears to depend entirely, as regards its temperature, upon the heat which it receives from the sun's rays.

We have next to consider the argument that, if the earth's exterior was in reality only such a thin covering or crust, like the shell of an egg, to which it has often been likened, that such a thickness would be altogether insufficient to give to it that stability which we know it to possess, and that consequently it could never sustain the enormous weight of its mountain ranges, such as, for example, the Himalayas of Asia or the Andes of America, which are, as it were, masses of rock piled up high above its mean surface-level.

At first sight, this style of reasoning not only appears plausible, but even seems to threaten to upset the entire hypothesis altogether. It requires but little sober consideration, however, to prove that it is more, so to speak, sensational in character than actually founded on the facts of the case; for it is only requisite for us to be able to form in our minds some tangible idea of the relative proportion which the size of even the highest mountain bears to that of the entire globe itself, to convince us, if such a crust could once form and support itself, that it could with ease support the weight of the mountains also. The great Himalaya chain of mountains rises to a maximum altitude of 31,860 feet, or six miles above the level of the sea; and if the earth could be seen reduced in scale down to the size of an orange, to all intents and purposes it would look like an almost smooth ball, since even the highest mountains and deepest valleys upon its surface would present to the eye no greater inequalities in outline than the little pimples and hollows on the outside of the skin of an ordinary orange. If this thin crust of the earth can support itself, it is not at all likely to be crushed in by the comparatively speaking insignificant weight of our greatest mountain chains, for in point of fact it would be quite as unreasonable to maintain such a supposition, as to declare that the shell of a hen's egg would be crushed in by simply laying a piece of a similar egg-shell upon its outside.

That a very thin spheroidal crust or shell enclosing a body of liquid matter such as an ordinary fowl's egg, does possess in itself an enormous degree of stability and power to resist pressure from without, is easily demonstrated by merely loading a small portion of its surface with weights as long as it does not give way under them. Even when placed on its side (or least strong position) it was found that a portion of the shell only one quarter of an inch square would sustain several pounds weight without showing any symptoms of either cracking or crushing; or, in other words, this simple experiment indicates that if the external crust of the earth was but as thick and strong in proportion as an egg-shell, it would be fully capable of sustaining masses equal in volume and weight to many Himalayas piled up one atop of another, without any danger whatever to its stability.

The next argument which has been advanced against the probability of the major part of the earth's substance being in a fluid condition, is one based altogether upon astronomical considerations. It having been demonstrated when two clocks are set agoing, the pendulums of which are similar to one another in all respects except that whilst the bob of the one is solid, that of the other is hollow and filled with mercury, that the latter will swing somewhat faster, and consequently the clock gain time upon the former. The late Mr. Hopkins, of Cambridge, applied this observation to the consideration of movements of the earth in space, and by a very elaborate course of mathematical reasoning and calculation, demonstrated that the earth, if not quite solid, must be nearly so, since according to his results, if it was merely a comparatively thin shell filled with liquid matter, the ratio of certain of its movements (the precession or nutation) would differ considerably from what they are actually known to be, and these conclusions appeared to be confirmed by the subsequent calculations of Sir William Thomson and Archdeacon Pratt. Although grave doubts suggested themselves as to the correctness of the values used in these calculations for two of their most important elements, viz., the condensing action of pressure and the expanding action of the very high temperatures within the globe—both of which have not as yet been determined with any certainty, and although it might also be surmised that the conditions of a pendulum bob of polished glass filled with heavy slippery mercury swinging at the end of a rod must be extremely different from those of a nearly spherical globe filled with viscid sticky lava revolving around its own axis; still geologists felt themselves quite unable to answer the arguments of the astronomers and mathematicians, and since none of them appeared to be sufficiently versed in either astronomy or mathematics to be able to submit the method of reasoning or the calculations to any strict scrutiny, they felt themselves, reluctantly no doubt, compelled to bow to the decision of such eminent authorities.

So stood the matter until the summer of 1868, when, fortunately for the advancement of this inquiry, M. Delaunay, now Director of the Observatory at Paris, an authority equally eminent as a mathematician and an astronomer, was induced to undertake the reconsideration of this problem; a labour which has not only resulted in altogether reversing the above decision, and demonstrating the complete fallacy of the premisses upon which so much elaborate reasoning had been expended, but which proved conclusively by experiment that a sphere filled with liquid matter would, under circumstances such as are present in the case of the earth, behave in precisely the same manner as an entirely solid one, and, consequently, that the fact of the earth being either solid or liquid in its interior could neither have any influence whatever on the rate of precession or nutation, nor be of any use as a means of deciding as to the real or approximative thickness of the earth's crust.

It may also be added that the conclusions arrived at by Mr. Hopkins, even when supported by Sir William Thomson and Archdeacon Pratt, were not universally acquiesced in; the celebrated German physicist, Helmholtz, amongst others, was not satisfied as to their correctness, and in opposition to the deductions of Sir William Thomson that the earth's crust must be some 1,000 miles in thickness, we have the entirely opposite conclusions of Mr. Henessey, whose calculations tend to show that the earth's crust cannot be less than eighteen miles or more than 600 miles in thickness. We may now, however, fairly conclude that all the objections as yet advanced from an astronomical point of view against the theory of the fluid condition of the interior of our planet, have been invalidated or explained away.

The only other argument in favour of internal solidity is one which bases itself upon the law, announced upon theoretical considerations by Professor Thomson in 1849, that the fusing points of bodies must become more elevated when subjected to pressure, or, in other words, that under the influence of pressure, bodies will require more heat to melt them.

Starting from this, Bunsen argued that the earth could not be other than solid to the core, since the enormous pressure accumulated at the centre would cause its internal substance to become so infusible that it could not remain in a molten state. To a certain extent this law was corroborated by the experimental researches of Bunsen and Hopkins, made upon some of the easily fusible substances like wax, spermaceti, paraffin, and sulphur; but as far as the later experiments went, it was not confirmed either in the case of metallic substances, nor did it appear to hold true with other than the more easily compressible bodies.

In the case of the earth, therefore, the conclusions of Bunsen cannot be accepted, since we have to deal with materials to which, as yet, this law has not been proved to apply; still, assuming, as seems most probable, that the materials composing the earth's mass do become to some extent more and more infusible according as they approach nearer to its centre, it must, on the other hand, be remembered that this would be more or less neutralised by the expansion which these substances would undergo from the action of the internal heat; and as incontrovertible evidence has been produced to prove that the temperature of the earth downwards from the surface increases in direct ratio with the depth, it seems most probable that the combined effects of expansion and elevated temperatures would more than counteract any tendency to solidification due to the effects of pressure.

Having now taken into consideration the various objections which have been urged against the theory of the earth's internal fluidity, as well as devoted some consideration to the opposing view of its solidity, it will be noticed, if we pass in review some of the distinctive features of the two hypotheses, that the former theory is a legitimate deduction from the data afforded by the direct study of the earth itself, whereas the latter, on the contrary, instead of making the explanation of the earth's phenomena its starting point, devotes itself almost exclusively to the task of proving that it could not be fluid.

Thus, how is it possible, if the earth's mass be solid throughout, to account for the great upheavals or sinkings down of large portions of the rock formations which compose its external crust? Do not these phenomena lead to the direct inference that the external crust cannot, by any possibility, rest in depth upon an unyielding mass of matter in the solid state, but that it must necessarily be superposed upon some more or less fluid substance which by its mobility can, when some one portion of the crust above sinks down, become displaced, and so make room for it by elevating, or, as it were, floating up some other part of the same?

In like manner the hypothesis that the earth is essentially solid necessitated that the phenomena of volcanoes should be explained upon the supposition that they had their sources in numerous small isolated local basins of molten rock scattered over the surface of the globe; a view which is altogether inconsistent with the results of chemical and mineralogical investigation, which proves that the ejected products are identical in constitution even if taken from volcanic vents the most distant from one another, nor does such a theory attempt to explain the tidal phenomena of volcanic outbursts and earthquakes previously alluded to.

So far, therefore, as we have gone into this subject, we may regard the balance of evidence as proving that at a depth of about fifty miles or less from the surface, there exists a continuous zone of molten rock or lava, such as is brought up to the surface by volcanic eruptions. Let us now consider how deep this zone or stratum of molten matter is likely to extend, and also what forms the more central mass of the earth below it.

In order to answer these questions we must look to other than direct evidence, and first of all must inquire whether the consideration of the mean density, or in other words the actual weight, of the earth itself, can throw any light upon these abstruse points. The consideration of the attraction which bodies exert upon one another in the ratio of their magnitude, has enabled the physicist to effect the at first sight apparently impracticable task of determining the entire weight of the earth itself, but it is out of our province to describe the mode of doing so, and we must content ourselves by accepting as a fact the results of such investigations, which prove that the total weight of our planet is as near as possible $5\frac{1}{2}$ times the weight of a similar globe of pure water. Knowing now that the mean density, or specific gravity, as it is also called, of the earth, is $5\frac{1}{2}$, and also from direct experiment that the mean density of the entire solid rock forming its external crust cannot be higher than $2\frac{1}{2}$, or less than half that of the entire sphere, it naturally follows that the central parts must be very much more heavy in order to account for so high a mean figure as $5\frac{1}{2}$, and it has been calculated that if we suppose that the earth was composed of three concentric portions of equal thickness, each in turn increasing in density towards the centre in arithmetical progression, we should then have an outer circle of specific gravity $2\frac{1}{2}$, or as heavy as rock, an intermediate zone of 12, or as heavy as quicksilver, and a central nucleus of about twenty times the density of water, or as heavy as gold.

This increase of density has sometimes been erroneously represented as entirely due to the effects of the enormous pressure of the superincumbent mass; but this supposition is quite untenable, since the tendency of all the numerous experiments made in this direction has been to prove that no substances can be compressed or condensed to an indefinite extent, since what may be termed their approximative maximum density is soon attained, beyond which the effects of pressure become so much smaller and smaller in proportion to the force applied, that at last the further condensation effected by still greater pressure is all but inappreciable. Besides this, it must not be forgot that the crust of the earth is a species of dome like the shell of an egg, which supports itself without resting or floating upon its fluid centre; and further that the earth's high internal heat, by causing the materials which compose it to expand, must also counteract the effects of superincumbent pressure, so that when all these facts are taken into due consideration, it appears quite evident that the materials which actually form the mass of the interior must be infinitely denser than any of the rocks met with on the surface, and that they must be metallic in their nature, since no other bodies are known which could at all fulfil these conditions of density.

If now we suppose that the earth's interior is composed of a series of concentric zones or layers made up of substances which are of more and more dense nature as they are situated nearer the centre, and that the external one is rock of a density of 2·5, a calculation will show that the centre or nucleus will be about 10, or as heavy as silver. If now we suppose that the zone of molten lava, which we have already concluded must exist at a depth of about 50 miles below the surface, has a density of 3, or say even 4, to give the fullest allowance for the condensing effects of superincumbent pressure, then we should find by calculation that this zone could not extend deeper than about 400 miles, since below this depth the matter would be so heavy that its density can only be explained on the supposition that it is made up of metallic compounds, and as the density of the still lower zones would continue to increase up to the very centre of the earth, the inference is that the whole of this great central mass situated at a distance of some 450 miles or less below the surface, is actually formed of metals and their compounds.

Whether this great central metallic nucleus is fluid or solid may next be inquired into. According to Bunsen's theory previously alluded to, it ought to be solid, for owing to the enormous pressure to which it would be exposed, the solidification of the molten sphere should first commence at the centre. This view would be quite correct if the earth was composed of highly compressible non-metallic materials; but since this is not the case, and since, as before alluded to, the experimental data already obtained indicate that neither the metallic nor the less compressible substances become more refractory in proportion to the increase of pressure, we are more justified in assuming that the central nucleus also must be in a fluid condition, and the more so, not only because we know that metallic compounds are as a rule infinitely more fusible than rock silicates, but also as the well known high temperature of the earth's interior would, by its expanding action, tend to counteract the effects of the pressure

In summing up this inquiry, the balance of evidence appears to me to be decidedly in favour of the hypothesis that the interior of our earth is a mass of molten matter arranged in concentric layers or zones according to their respective densities, and the whole enclosed within a comparatively thin external crust or shell.

DAVID FORBES

SCIENTIFIC SERIALS

THE *American Naturalist* for January opens with a long paper by Prof. J. S. Newberry "On the Ancient Lakes of Western America: their Deposits and Drainage," which is stated to be a chapter from Dr. Hayden's forthcoming "Sun-pictures of the Rocky Mountains." Prof. Newberry states that the wonderful collection of fossil plants and animal remains brought by Dr. Hayden from the country bordering the Upper Missouri has been shown, by his observations and the researches of Mr. Meek, to have been derived from deposits made in extensive fresh water lakes, lakes which once occupied much of the region lying immediately east of the Rocky Mountains, but which have now totally disappeared. The sediments that accumulated in the bottom of these old lakes show that in the earliest periods of their history they contained salt water, at least that the sea had access to them, and their waters were more or less impregnated with salt, so as to be inhabited by oysters and other marine or estuary mollusks. In due time the continental elevation which brought all the country west of the Mississippi up out of the widespread Cretaceous sea raised these lake-basins altogether above the sea-level, and surrounded them with a broad expanse of dry land. Between these lakes were the areas of dry land covered with luxuriant and beautiful vegetation, and inhabited by herds of elephants and other great mammals, such as could only inhabit a well-watered and fertile country. Prof. Newberry's explanations throw much light on that remarkable feature of the western side of the great continent, the canons formed by the rivers, like the stupendous one of the Colorado, nearly 1,000 miles in length and from 3,000 to 6,000 feet in depth, with almost perpendicular sides. The Rev. A. P. Peabody contributes an account of the Chinese in San Francisco; Mr. H. Willey, a paper on Lichens under the microscope, with wood-cuts which very well illustrate their mode of vegetation and reproduction; and Dr. A. P. Barnard, a description of a new form of binocular for use with high powers of the microscope. The shorter articles and *Natural History Miscellany* contain, as usual, much interesting information.

The *Journal of Botany* for February commences a series of papers which will be very useful to systematic botanists; an alphabetical catalogue of the new genera and species of plants published during 1870 in the English botanical and gardening journals, not including the "Journal of the Linnean Society." The present number only carries the list down to *Dracontium*. Mr. J. G. Baker continues his monograph of the genus *Xiphium*, and Dr. Hance contributes an article on the so-called "olives" of Southern China, which he states to be produced by two species of *Canarium*, trees from twenty to thirty feet high, largely grown in the neighbourhood of Whampoa. The stones are beautifully and elaborately carved by the Chinese as ornaments, and, when set in gold, form exceedingly handsome brooches or bracelets. Two articles of special interest to systematists are Prof. Dyer and Dr. Trimen on *Polygonum nodosum*; and Mr. W. P. Hiern on the form and distribution over the world of the Batrachian (or aquatic) section of *Ranunculus*. There is also the usual section of short Notes and Queries.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, February 2.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected Fellows: R. J. Friswell, R. F. Humiston, M.D., A. H. Mason, I. R. Justin. Prof. Frankland, F.R.S., read a paper "On the Development of Fungi in Potable Water." He began by alluding to the experiments Dr. Heisch had made some months back with waters contaminated with sewage matter. When to such waters some sugar was added, very soon a kind of fermentation ensued, and a rich fungoid growth made its appearance. Prof. Frankland has now repeated and extended these experiments and arrived, with one or two exceptions, at the same results. But in the course of his researches he encountered some reactions which revealed to him that the presence of sewage matter in saccharic water is in itself not sufficient to produce fungoid growth, but that the presence of phosphates in some form is indispensable to such production. Prof. Frankland further found that the germs which give rise to the development of fungi need not necessarily come from sewage contamination, but that they may be derived from the atmosphere. Finally, he found that animal charcoal does not remove those germs. Dr. Frankland thinks that the sugar test of Dr. Heisch for the detection of traces of sewage contamination may be turned into a very delicate reagent for the detection of minute quantities of phosphates; for when these defy the power of the usual laboratory tests, they yet are capable of feeding those germs and thus giving rise to the fungoid growth. From all his observations Prof. Frankland drew the following conclusions:—1. Potable water mixed with sewage, urine, albumen, and certain other matters, or brought into contact with animal charcoal, subsequently develops fungoid growth, and other organisms, when small quantities of sugar are dissolved in them and they are exposed to a summer temperature. 2. The germs of these organisms are present in the atmosphere, and every water contains them after momentary contact with the air. 3. The development of these germs cannot take place without the presence of phosphoric acid, or a phosphate or phosphorus in some form of combination. Water, however much contaminated, if free from phosphorus, does not produce them. A German philosopher has said "ohne Phosphor kein Gedanke." The above experiments warrant the alteration of this dictum to "ohne Phosphor gar kein Leben."

Anthropological Society January 31.—Dr. Charnock, President, in the chair. A paper was read by Mr. Joseph Kaines, on some of the Racial Aspects of Music. The author, in a very brief glance at the characters of the music of the various races of men on the globe, drew particular attention to a striking anthropological fact—namely, that the music of the people of the north-east of Europe, unlike that of all the rest, was pervaded by a settled melancholy. He sought to account for this phenomenon physically and psychically. He drew attention to the climatal and general physical conditions under which the peoples of the north-east of Europe live, and suggested that, in the constant war with Nature, and the endeavour to modify Nature's laws, they acquired a gravity, awe, and sadness, of which the peoples of the sunny south knew nothing, as their music showed, Nature having used them more kindly. The author contrasted the biographies (as well as the music) of the German and Italian composers, and showed that the men differed as widely; sadness and sorrow marking the one, brightness and gladness characterising the other. He commented upon the introspectiveness of the northern peoples, and the rapt attention and morbid analysis they give to the great problems of Life, Death, God, and Immortality; and stated that the contemplation of these and such sublime mysteries saddened and brightened by turns all their thoughts and impressions. It was curious to note that even the dance tunes and popular airs of the Germans, Norwegians, and Swiss, as has been remarked by Mr. H. F.

Chorley, the eminent musical critic, were in a minor key. "Joyousness," contined the author, "is a plant that does not flourish in the bleak north. It flowers and blossoms perennially in the south, because there the air is balmy and soft. There the skies are always bright, and beneath man's feet the earth is fruitful though untilled. There Nature uses her children kindly, and even "prepares for them a table in the wilderness.'" The author remarked incidenatlly that not music only, but the other arts of expression—architecture, sculpture, and the mythologies of the north of Europe—were imbued by the same melancholy spirit. He concluded by a few observations on the character of ancient Roman, modern Anglican, and dissenting Church music. The following gentlemen took part in the discussion: Mr. Mackenzie, Dr. Hyde Clarke, Mr. Bendir, Dr. Blake, Mr. Lewis, Mr. Wake, Captain Brine, Mr. W. R. Cooper, Mr. Quaritch, and the Chairman. The President announced that this was the last ordinary meeting of the Anthropological Society, an amalgamation with the Ethnological Society having been carried out by the delegates appointed for that purpose at the general meeting of this Society on January 17th. The new society was to be styled "The Anthropological Institute of Great Britain and Ireland."

Linnean Society, February 2.—Mr. G. Bentham, President, in the chair. The President announced the death of one of the corresponding members of the Society, Prof. Miquel, of Leyden; and also that the Council had agreed to recommend the election of Prof. O. Heer, of Zurich, to fill the vacancy in the list of foreign members caused by the death of Prof. Unger, of Vienna.—"Natural History of Deep-sea Soundings between Galle and Java," by Captain Chimmo. The ooze dredged up from a depth of 2,300 fathoms, where the temperature was found to be 35° F., consisted to the extent of 90 per cent. of organic matter, Foraminifera, chiefly Globigerinæ, together with Polycistinæ, with a few broken sponge-spicules. In the shallow water near Sumatra, the animal life had decreased to only about five per cent. of the ooze, the Globigerinæ having entirely disappeared. The water brought up from great depths was found to contain a large proportion of salts in solution, which crystallised out immediately on exposure to the air. Mr. Busk remarked on the great interest and importance of the observation of the low temperature of the deep water in a latitude within a few degrees of the equator, strongly confirming the conclusions as to a general circulation of the water between the equator and the poles drawn from similar observations in the Atlantic.

Victoria Philosophical Institute, January 30.—The Rev. J. H. Titcomb read a paper on "Archæology, with some of its Parallels and Contrasts;" it was a general review of the whole subject, and also showed how the resources of nature had been made use of by improving the arts. The discussion was carried on by the Chairman, Captain F. Petrie, Mr. V. Newton, the Rev. Mr. Heard, Mr. Shiffard, and Mr. Row.

EDINBURGH

Botanical Society, November 10, 1870.—Sir Walter Elliot, president, in the chair. The president delivered an opening address, in which he congratulated the Society on its continued prosperity, having now entered on its thirty-fifth year of existence. He reviewed the rise and progress of naturalists' field clubs in Britain, and concluded by giving short biographical notices of the members whom the Society had lost by death during the past year.—"Experiments on the Transpiration of Leaves," by Dr. W. R. M'Nab.—"On the Laws of Growth in Plants," by Col. T. B. Collinson.

December 8.—Mr. Alexander Buchan in the chair. The following communications were read:—"Botanical Excursions in July and August 1870, with pupils," by Prof. Balfour.—"Notice of some new and rare mosses collected on Ben Lawers," by Dr. Stirton. This was a continuation of a paper read by Dr. Stirton last session, recording the recent discovery of several new species of mosses on Ben Lawers, with notes as to place of growth, &c., of the rarer species found on that mountain.—"On the varieties of *Hieracium stoloniflorum* of Waldst. and Kit. at different seasons," by Prof. Balfour.—Prof. Dickson exhibited a plant of the Chinese primrose, having stamens and style of the same length (short), although in this species, as in the other dimorphic primroses, they are usually of different lengths. This form is interesting, inasmuch as in an abnormal cowslip, described some years ago by Mr. John Scott, the stamens and style, although of the same length, were both long.

BOOKS RECEIVED

ENGLISH.—The Text-book of Science; Algebra, and Trigonometry: W. N. Griffin (Longmans and Co.)—Strange Dwellings: Rev. J. G. Wood (Longmans and Co.).—The Sun: Ruler Fire, Light, and Life of the Planetary System: R. A. Proctor (Longmans and Co).—The Schools for the People: G. Bartley (Bell and Daldy).

DIARY

THURSDAY, FEBRUARY 9.

ROYAL SOCIETY, at 8.30.—The Effect of Exercise on the Bodily Temperature: Dr. Allbutt.—Observations of the Eclipse at Oxford, Dec. 22, 1870: Prof. J. Phillips, F.R.S.—On the Problem of the In- and Circum-scribed Triangle: Prof. Cayley, F.R.S.—On the Unequal Distribution of Weight and Support in Ships, and its Effects in Still Water, in Waves, and in exceptional Positions on Shore: E. J. Reed, C.B.
SOCIETY OF ANTIQUARIES, at 8.30.—On Documents illustrating the Position of the Prior and Convent of Canterbury *sede vacante*: J. B. Sheppard.—On the hitherto undescribed Expedition of the Emperor Augustus into Britain: W. H. Black, F.S.A.
LONDON MATHEMATICAL SOCIETY, at 8.—On a Problem in the Calculus of Variations: Prof. Cayley, V.P.—On Surfaces of Negative Deficiency: Prof. Cayley, V.P.
LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff, M.A., F.C.S.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

FRIDAY, FEBRUARY 10.

ROYAL ASTRONOMICAL SOCIETY, at 3.—Anniversary Meeting.
ROYAL INSTITUTION, at 9.—On Some Fallacies connected with Ships and Guns: E. J. Reed, C.B.
QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, FEBRUARY 11.

ROYAL INSTITUTION, at 3.—Laws of Life revealed in History: Rev. W. H. Channing.

SUNDAY, FEBRUARY 12.

SUNDAY LECTURE SOCIETY, at 3.30.—The Entozoa of Man and Animals in relation to Public Health and the Sewage Question: Dr. Cobbold, F.R.S.

MONDAY, FEBRUARY 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.
LONDON INSTITUTION, at 4.—On the First Principles of Biology: Prof. Huxley. (Educational Course.)

TUESDAY, FEBRUARY 14.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.
PHOTOGRAPHIC SOCIETY, at 8.—Anniversary Meeting.

WEDNESDAY, FEBRUARY 15.

SOCIETY OF ARTS, at 8.—On the Commerce of India: Dadabhai Navroji.
METEOROLOGICAL SOCIETY, at 7.
ROYAL SOCIETY OF LITERATURE, at 8.30.
ANTHROPOLOGICAL INSTITUTE, at 7.30.—General Meeting.—Ordinary Meeting at 8.
LONDON INSTITUTION, at 7.—On Alizarine and other Colouring Matters: W. H. Perkin, F.R.S. (Conversazione.)

THURSDAY, FEBRUARY 16.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
LINNEAN SOCIETY, at 8.—On Tremellineous Fungi and their Analogues: L. R. and C. Tulasne.—Bryological Remarks: S. O. Lindberg, M.D.
CHEMICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 9.—On the Wolf-Rock Lighthouse: James N. Douglass.

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ERRATUM.—Page 275, second column, line 22 from bottom, for "ardent" read "ordeal."