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THURSDAY, FEBRUARY 23, 1871

THE MEDITERRANEAN ECLIPSE, 1870
II.

IN my former article under the above title, written from Venice, I gave as shortly as I could the conclusions at which I had arrived as to the results of the various Eclipse expeditions as gathered from the very imperfect information then at my disposal. Since I returned home, I have naturally become possessed of more facts, though even yet the time has not arrived for discussing all the observations as they must be discussed before an absolutely final verdict can be given.

Still, there is so much general interest taken in the recent work, that I venture to return to it at the present time, more especially as I can now print a letter from a distinguished American astronomer, giving his view of the work done, and also as I am anxious to refer to Prof. Young's article which has recently appeared in NATURE.

Prof. Peters, whose long and laborious researches on the sun are well known to all of us, thus writes in reference to my former article :—

"Its perusal has been to me a source not only of pleasure but of much instruction. You have placed on record, with great lucidity, the question as it stood before the Eclipse, and the points to be examined by the various ways of observation for bringing the question nearer to its solution. Although the unfavourable state of the weather over the entire zone of totality, as it seems, from Spain to Sicily, has greatly obstructed the execution of the plans and the extensive preparations made with the liberal aid of our respective governments; and although hitherto, of course, only imperfect, mostly verbal, information has reached us of what the parties really did succeed in obtaining—still the result that is to be drawn from the sum total, as you are showing, seems of importance. The spectroscopic, polariscopic, and telescopic observations altogether agree in demonstrating an interior portion of the corona to belong to the sun. The existence of such a *solar* stratum is sustained also by my researches on the motion of spots when near the limb, pointing to a refraction on, or rather above, the sun's surface. I concur further in your opinion that the outer, more irregular radiating portion of the corona very likely owes its origin to our atmosphere. It is highly to be regretted that our Etna parties, in elevations respectively of 3,100, 5,500 and 8,000 feet, suffered disappointment from a heavy cloud at the critical moment of totality. Their observations would have been decisive as to the local and atmospheric cause of the radiating coronal phenomenon."

One more extract before I proceed. With reference to the suggestion (based on my observations of injections into the chromosphere) contained in my article, that probably the green line seen in the spectrum of the Corona might indicate a new element lighter than hydrogen, Prof. Young, claiming priority in the suggestion, writes :—

"In *Silliman's Journal*, November 1869, I wrote, 'should it turn out that this line in the spectrum of the aurora does actually coincide with 1474, it will be of interest to inquire whether we are to admit the presence of *iron vapour* in and above our atmosphere, or whether in the spectrum of iron this line owes its origin to some foreign substance, probably some occluded gas as yet unknown, and perhaps standing in relation to the magnetic powers of that metal.'

"This is the only reference I am able to make here. In my paper published in the Proceedings of the American Association for 1869, the same thing is, I think, more forcibly expressed. I think you will find it also in my Eclipse Report in the 'Journal of the Franklin Institute' (and in my letter to NATURE last spring).

"The idea that 1474 might represent some new element occurred to me at once when I found it in the Corona, but of late I own I have more inclined to the opinion that it might possibly be a true iron line, and caused by meteoric iron dust of almost infinitesimal fineness; yet I have always felt the difficulty of supposing the complicated iron spectrum reducible to this one line."

I feel it due to Prof. Young to give this extract, though I confess I do not see that the suggestions are similar, nor do I see anything similar in the letter referred to, though I have lighted upon this passage which I had forgotten, which shows the great advance that has been made. Prof. Young last year wrote* "It is not impossible that the so-called corona may be complex. Some portion of its radiance may, *perhaps*, originate in our own atmosphere, although I do not yet find myself able to accord with the conclusions of Dr. Gould and Mr. Lockyer in this respect, and am strongly disposed to believe that the whole phenomenon is purely solar." His present views were given to the readers of NATURE three weeks ago, as in the main concurring with my own.

With reference to Prof. Young's article, I am anxious to say one word on the "sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality," witnessed by himself and Mr. Pye. I have seen this *once*, and only once, during all my observations, and Professor Young (who enjoys better atmospheric conditions than I do) has never seen it when working with the new method. Now, I hold that the new method is competent to pick up such an envelope as the one referred to by Mr. Longley, if it can pick up an uprush similarly composed; and although of course the vapours competent to give such lines are not far off, as the ordinary observations prove, I do not think they are ordinarily high enough above the level of the photosphere to be seen in this manner. That the number of lines is largely increased when the atmospheric glare is withdrawn, was proved during the American Eclipse.

But to return to the Corona, the main point of attack during the last Eclipse. Since my last article was written I have had an opportunity of inspecting copies of the beautiful photographs taken by Mr. Brothers at Syracuse, and also one of the photographs taken by the Americans in Spain. These, compared with the sketches taken at the respective stations, are very curious. In the Spanish photograph there is a very distinct "rift," or dark space in the coronal region, extending, I believe, almost to the sun, and fainter indications of two other such rifts in another region, not extending so low down in the Corona. So far as the facts have yet been before me, only one of these rifts was sketched. Now, at Syracuse Mr. Brothers also photographed rifts—three rifts; but the sketches did not record a single one. In Prof. Watson's drawing, a copy of which I have now in my possession, there is no indication whatever of them. But there is a much more important fact behind. Of course, if these rifts had been in the same positions in the two photographs, taken at stations so wide apart as Spain and Sicily, the presumptive evidence in favour of the solar nature of the Corona for a distance outside the sun equal to

* NATURE, vol. i. p. 533.

its diameter would have been overwhelming, and feeling that here was a crucial test to apply to a question which has so long been debated, but never with such interest among the workers as recently, it was with some excitement that I found myself before these two photographs some little time ago with two American astronomers of eminence, for the purpose of endeavouring to settle the question. Suffice it to say that we came to the conclusion that the rifts were not identical, that the two cameras had *not* photographed the same phenomenon, although at first there appeared to be sufficient similarity to make the matter appear doubtful, and, unfortunately, the photographs vary so much in size, and the margin of the American one is so limited, that it will be scarcely possible to make a final comparison until they are brought to a common scale, and superposed the one on the other. I do not think it is surprising that rifts should appear in both photographs, supposing a non-solar cause were at work, for the Corona between the rifts on Mr. Brothers' photograph looks like a very wide ray.

Assuming then for the present that the photographic evidence goes the way of all the other evidence—that in short, the solar corona, including all its fantastic boundaries, has been probably reduced from one, two, or three solar diameters, to six, eight, or ten minutes,—I care not which,* —let us examine some of the details of the various observations.

In Professor Watson's drawing, the intimate connection between the higher and lower levels of the chromosphere (including the portions not at present observed by the new method), comes out in a very striking way. Mr. Seabroke, at my request, made careful maps of the positions of the prominences before the totality commenced, and Professor Watson made his drawing of the Corona, independently of the positions of the prominences. On the homeward journey the map was compared with the sketch, and to use Professor Roscoe's words, "On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabroke's map, there a corresponding bulging out of the Corona was chronicled on Professor Watson's drawing, and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the sun's limb." We may remark that these coincidences show the excessive fidelity of the drawing, and make it one of the most valuable of the products of the Expedition.

On former occasions the Corona has been stated to assume a roughly four-cornered form. This was also observed in Spain last December, and seems at last explained by three drawings made by one of the American party there.

At the commencement and end of totality, when the moon unequally covered the sun, the photographs have recorded an excess of light on the Corona on the side where the limbs occur nearest in contact. I am told that this effect in one of Lord Lindsay's photographs is very striking; it is certainly so in one of Mr. Brothers'. In the drawings we have a slightly different effect. At the commencement of totality, when the western or right hand limbs were in contact, we get Fig. 1, at the end of totality the appearance recorded was Fig. 2; the picture at the middle

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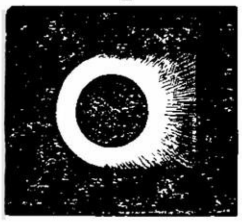


2

3

of totality compounding both these appearances, and being roughly represented by Fig. 3, in which the rectangular appearance comes out in its full strength.

A word now about the polariscopic observations. I may remark on this that it is much more easy for us to explain slight polarisation which might be atmospheric, than it is to explain, if we content ourselves with laboratory experiments, strong *radial* polarisation which must take place at the sun. If we assume that gas or vapour of considerable tenuity does not reflect light (although I think this is to assume very much for the gas or vapour *at the sun*, at all events), what is it that reflects light to us at the sun, and reflects it apparently only *above* the level of the intensely incandescent hydrogen? Certainly not solar spray. If we deny reflection to gases altogether, may it not be the continuous portion of the spectrum of the gas itself to which the light is due. But this question of polarisation is certainly one in which very much remains to be done, and it is consoling to know that the results obtained now will much facilitate the planning of the next polariscopic campaign, which, we may add, should not be deferred beyond the end of this year.

J. NORMAN LOCKYER

* I beg here to give the actual words employed by Dr Frankland and myself in the communication to the Royal Society on the subject. Speaking of the chromosphere, it was remarked "the tenuity of this incandescent atmosphere is such that it is extremely improbable that *any extensive atmosphere*, such as the corona has been imagined to indicate lies outside it"—Proc R S, Feb. 11, 1869. I never imagined that all the Corona was non-solar. Again, Proc. R.S., No. 116, 1870, discussing the American Eclipse, I state that the chromosphere includes the "radiance" observed in the American Eclipse, of which radiance Dr. Gould wrote as follows:—

"An examination of the beautiful photographs made at Burlington and Ottumwa . . . and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made during totality is not the corona at all, but is actually the image of what Lockyer has called the chromosphere. This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable. There is a diameter approximately corresponding to the solar axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity. . . . Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

PHYSICAL LABORATORIES

IN an excellent article in a late number of NATURE, Prof. Pickering has drawn attention to the importance of the practical teaching of Physics, and has shown how this is being done on an extensive scale in America. It may be interesting to trace the similarity between the methods employed by different teachers, and to show what opportunities are and have for some time past been open to students in London for the practical study of every branch of Physics.

Professors of Physics at different Universities have usually selected their best students to assist them in their private laboratories, to the mutual advantage of professor and student, but I believe that Prof. Clifton was the first to propose, more than three years ago, that a course of training in a physical laboratory should form a part of the regular work of every student of Physics.

This system was adopted and at once put in action at King's College, on a very considerable scale for a college with no endowment whatever, and has been working for now nearly three years. Two large rooms adjoining the Museum of Physical Apparatus were fitted up for a Physical Laboratory, and a third room was built for a store and battery room. Fixed tables in both large rooms are supplied with water and gas, and with pipes passing to gasholders containing oxygen and hydrogen, also with thick copper wires insulated from one another passing to the battery room, so that in electrical work the fumes from batteries are entirely got rid of.

The principal instruments have their fixed places on the tables, and a description of the measurement to be made is given to each student, and while in progress his work is examined by the professor or demonstrator. The course of study includes the subjects of pneumatics, heat, light, electricity, and magnetism, and with the regular class, a definite order in each subject is kept to as nearly as possible. When, as has sometimes been the case, there are twelve or more students beginning their laboratory course at the same time, it is necessary to deviate from the regular course, and to set some to begin with heat, some with light, and others with electricity. For some experiments, such as the determination of the relation between the pressure and volume of a gas, or the measurement of the expansion of a gas for given changes of temperature, requiring the use of the manometer and cathetometer, it is found better to have two students working together, each student making in his turn and so checking every part of the measurement or determination.

The accuracy of the results obtained has been very great, and is an evidence of the interest taken in the work by the student, and of the value of such a course of study as a mental training, to say nothing of the actual knowledge gained. Every student is required to produce fair results, and to give an account of the methods which he has employed, before he is allowed to proceed to another part of the subject. Besides the students pursuing the regular course there are several who wish to devote their attention to some one branch, such as Electricity. In this subject, after making determinations of Resistance, Strength of current, and Electromotive force with simple galvanometers, they pass to more delicate measurements with Thomson's Galvanometers and Electrometers, such as the experimental determination of equi-potential lines on a conducting surface uniting two poles of a battery, and perform all the tests and measurements required in connection with Telegraph lines and cables.

The more advanced students carry on investigations in the Laboratory, such as the measurement of the effect of heat in altering the magnetic polarity of dia-magnetic bodies, or in altering the rotation of the plane of polarisation of a beam of polarised light as it passes through sugar solutions.

Students are encouraged to combine their work in the Physical Laboratory with their work in the Mechanical Workshop, and are enabled to design and construct apparatus, and their inventive powers are exercised often with great success. From experiments with Attwood's Machine one student has designed and made for himself a new form of governor for an engine, another has designed an Inductometer for measuring the time required to produce the maximum induced current in a wire by the action of another current.

The success of the Laboratory system of teaching may be judged from the quality of the work done by the students, who are mostly from sixteen to eighteen years of age, and are always eager for the work, as well as from the fact that in the last term there were twenty-three students in the Laboratory, and the numbers are steadily increasing.

It will be seen that to the student of Electricity or any branch of Physics, every opportunity is offered of pursuing the object which lies before him, and that the advance which may be made by him is dependent only on his own exertions.

W. G. ADAMS

MORELL'S GEOMETRY

The Essentials of Geometry, Plane and Solid, as taught in French and German Schools, with shorter Demonstrations than in Euclid; adapted for Students preparing for Examination, Cadets in Military and Naval Schools, Technical Classes, &c. By J. R. Morell, formerly one of Her Majesty's Inspectors of Schools. (London: Griffith and Farran, 1871.)

A WORK with this attractive and somewhat ambitious title cannot fail to attract attention, appearing, as it does, at a moment when very commendable efforts are being made to improve the teaching of Geometry in our schools, and to prepare the public mind for an important reform which will necessarily involve the adoption of text-books more suited to modern habits of thought and inquiry than the Elements of Euclid. It will, no doubt, therefore be expectantly read by many, and the fact that the author is already widely known as a writer on philosophy and grammar, and was formerly one of Her Majesty's Inspectors of Schools, will tend to raise expectations, and will lend it an authority to which, as we shall presently see, it is by no means entitled. Our duty to students preparing for examination compels us, in fact, to warn them that this book can render them no essential service whatever, but, on the contrary, may do them incalculable mischief; and our sympathy with the praiseworthy efforts above alluded to—our desire to prevent undeserved discredit from being attached to those efforts—obliges us to dissociate this one from them, and to criticise it with all due severity.

The plan and general arrangement of the book are open to the severest criticism; we deem it unnecessary, however, to dwell thereon, for the work is so destitute of the most essential of all "Essentials of Geometry"—accuracy and clearness—that no possible rearrangement of its materials could redeem it. We shall consequently draw attention solely to the deplorable looseness of expression, indicative of hopeless inaccuracy of thought with which almost every page is disfigured.

We shall commence with quoting a few definitions, and simply italicising their most salient incongruities. Further comment will rarely be needed.

"Extension is the *space* occupied by a body" (p. 1).

"A straight line is that which has all its *points* in the same *direction*" (p. 1). Direction is, of course, not defined, nor is it stated whence the points of the line are supposed to be viewed.

"A plane angle is the *greater* or *less* inclination of two straight lines *to a common point*" (p. 5). In a note hereto the author complacently observes that "our definition accords with Euclid, Bk. i., n. 8."

"The angle A O B increases continually *in proportion* as the straight line O B takes the direction O C, O D, &c." (p. 6).

"The name adjacent angles is given to those angles that have one side common" (p. 6). The author appears to have felt that there was some insufficiency here, but instead of expunging the passage as wholly useless, he tries again in small print, and almost, though not quite, succeeds. That some haziness still clings around his conception of adjacent angles is obvious; for when we come, in the Geometry of Space, to angles between two planes, dihedral angles, we are told (p. 90) that "adjacent dihedral angles are those which have a common plane, *and whose other two sides are in one plane*." The condition here italicised is wholly unessential, and the really essential one is again omitted; viz., that the two angles should be on opposite sides of their common plane. This misconception of the nature of adjacent dihedral angles leads the author, naturally, to the following absolutely false definition:—"If two adjacent dihedral angles are equal, each one is named a dihedral right angle." We may here observe that there is deplorable confusion in this part of the work between polyhedra and polyhedral angles. Trihedral angles are generally, though not always, termed *trihedra*, dihedral angles occasionally crop up as *dihedra*, and uncouth entities such as *polyhedrals*, *dihedral sides* of a polyhedral angle (p. 94), and *polyhedral triangles* (p. 95), not unfrequently stop the way.

Leaving definitions, however, let us glance at Mr. Morell's enunciations of theorems, and the "improved shorter demonstrations" of them with which he supplies us.

On p. 3 we find the following short paragraph, into which a theorem and its demonstration are supposed to be condensed:—"Any diameter whatever divides a circumference into two equal parts; *therefore* if on superposing its two halves they did not agree, the radii of the same circumference would be unequal, which is absurd." The word *therefore* would imply that the assertion which precedes it is not itself the theorem to be demonstrated, but one which is to be employed in demonstrating something else. But what is this something else? Long reflection failed to furnish any answer to the question; it seemed but to give to the last three words a more extended significance than the author could have contemplated.

The entire paragraph remained a mystery to us, in fact, until we reached p. 25, when Mr. Morell's happy arrangement enabled him to return to the subject in these slightly modified words:—"Every diameter divides the circumference into two equal parts; *for* if on placing one *half* on the other they did not coincide, the radii of one and the same circumference would be different in length, which is absurd." Here the word *for* being substituted for *therefore*, we can no longer doubt that what precedes it *is* the theorem to be proved; the moment one *part*, however, is termed a *half* the whole question is begged; and even if this gross blunder had been avoided, the demonstration would still have been worthless, so long as the *mode* of placing one part on the other was left wholly unexplained.

The enunciation and demonstration of the two fundamental theorems of parallels are thus given on p. 21:—

"If two straight lines are parallel, and are cut by a secant, the alternate angles are equal, and also the corresponding angles. Conversely, if two straight lines cut by another form equal alternate or corresponding angles, the said lines will be parallel."

"For two alternate internal, or alternate external, or corresponding angles are both either acute or obtuse, and *consequently* equal in the case of parallels. But, if of two internal or external angles on the same side, one is acute, and the other obtuse, these angles are, *therefore*, supplementary."

Let the reader picture to himself for a moment the perplexity of the misguided student, "preparing for his examination," as he vainly strives to extract a meaning from this sheer nonsense. Distrustful of his own powers, the poor fellow will probably attribute his failure to his own incapacity, and in despair commit the precious passage to memory for the purpose of reproduction when his hour of trial shall come. The consequences of such rashness need not be stated. And yet Mr. Morell cites a respectable French geometer as an authority for this "shorter demonstration!" It was with no small curiosity that we turned to the pages of Amiot's *Éléments de Géometrie* to see how an author, who usually writes with admirable clearness, though not always with desirable rigour, could have been made responsible for such absurdity. The process was simpler than we expected; editorial scissors had simply clipped away everything worthy of the name of demonstration, and the editorial pen had garbled the feeble residue into the chaotic sentences above reproduced. We know of no epithet too severe to apply to editorial transgression of the kind here exposed.

To proceed with the painful task we have imposed upon ourselves, we have next to draw attention to two blunders, not in geometry, but in simple logic. On p. 20 we read as follows:—

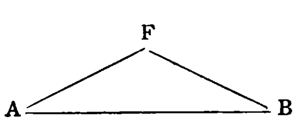
"It is often assumed as a self-evident proposition that through a given point only one parallel can be drawn to a straight line," and, in a foot-note hereto, we are told that "this is the *opposite* of Euclid's 12th axiom, similarly assumed to base upon it his theory of parallels." Had Mr. Morell read Amiot, even, with anything like attention he would have seen that this is not the case. In forming the opposite of a theorem, the new hypothesis and predicate must be made to contradict, respectively, the original hypothesis and predicate. The opposite of Euclid's 12th axiom, therefore, as Mr. Morell may now convince himself by trial, is very different from the one above quoted.

On p. 44 again, we have this marvellously short enunciation and demonstration of Euclid's Prop 8, Book I.; "Let A B C, D E F be two triangles, having A B = DE

A C = D F, and B C = E F. Then angle A = D; *for if they were unequal, sides B C and E F would be unequal. Therefore* A = D." In violation of the most obvious of all logical rules, Mr. Morell here professes to prove a theorem by the simple assertion of it in its contra-positive form! To assert that B C and E F would be unequal if A and D were unequal is *logically equivalent* to saying that A and D would be equal if B C and E F were equal, which is precisely the proposition to be proved. The truth of either of these statements involves that of the other by the necessary laws of thought, and neither can be said to require geometrical demonstration more than the other. It is true that Euclid himself (*vide* Props. 7 and 9, Book iii.) overlooked this necessary relation between contra-positive theorems. But Euclid's error was very trivial in comparison with Mr. Morell's. Euclid proved *both* theorems geometrically when a proof of one would have sufficed, but Mr. Morell unwarrantably *asserts* the truth of one, and imagines he has thereby proved the other.

Notwithstanding the fact that Mr. Morell devotes a considerable part of his book to the approximate calculation of the ratio of the circumference of a circle to its diameter, we fear that he must henceforth be classed amongst the "circle squarers of the period." For, as is well known, the solution of this vexed problem is at once reducible to that of the rectification of the circumference; and on page 62 we find it stated that "the graphic construction which *gives us the length of the circumference in a straight line* is the following." For obvious reasons we forbear to reproduce it.

The "technical classes" to whom Mr. Morell professes to have "adapted" his text-book, will, we fear, derive as little profit from its pages as the "students preparing for examination." How can it profit them, for instance, to be told that "many public buildings have as façade a front elevation like A F B?" Yet it is



with this *isolated* technical observation that Mr. Morell terminates the forty-first page of his book. Again, what earthly, not to say technical use can they make of the three very bad pictures of a surveying rod, a Gunter's chain, and a measuring tape, with which they are favoured on page 36? On page 37, Mr. Morell starts with his technical class to determine a straight line through "a winding narrow street;" but he fails utterly to bring them round the very first corner. On page 38 he proposes to show them how "to draw *a* perpendicular bisector of a given straight line in nature," but we will not disfigure this page of NATURE with a reproduction of his unintelligible method. Although readers might be amused by it, we are not in a humour to extract amusement from a work which was certainly not written for the purpose of providing it, but with the serious intention of teaching Geometry in an improved manner.

We have by no means exhausted the incongruities of the book; but we have, we think, sufficiently shown that the teaching of Geometry cannot but be vitiated by its use; that, in fact, this text-book of Her Majesty's ex-Inspector of Schools will itself bear no inspection whatever.

RODWELL'S DICTIONARY OF SCIENCE

A Dictionary of Science: comprising Astronomy, Chemistry, Dynamics, Electricity, Heat, Hydrodynamics, Hydrostatics, Light, Magnetism, Mechanics, Meteorology, Pneumatics, Sound, and Statics. Edited by G. F. Rodwell, F.R.A.S., F.C.S. (London: E. Moxon and Co., 1871.)

THERE are Dictionaries and Dictionaries. We have had occasion to expose the shortcomings of some books that are called by this name; it is a far pleasanter task to direct attention to the merits of a work like the one before us, which really deserves its title. Mr. Rodwell's "Dictionary of Science" is a repertory of facts connected with physical science, which will be invaluable to the student. From Chemistry to Chladni's Figures; from Thermo-dynamics to Turacine, scarcely a term will be met with in scientific works, of which the learner will not here find an explanation. The articles have the great advantage of being short, and presenting the salient points of each subject at a glance before the reader's eye; and that their scientific accuracy may be relied on, is guaranteed by such names (amongst others) as those of Mr. Crookes, Prof. Guthrie, and Mr. Wormell in the list of contributors. To illustrate the style of the book, we cannot do better than select two of the shorter articles. The first is on a subject which has recently been discussed in these columns:—

"*Mass.*—Mass is a term for the quantity of matter in a body. In order to measure mass, we assume that the attraction of the earth on all particles of matter is the same, and is not dependent on the nature of the matter attracted. This assumption seems to be justified by the fact that bodies of all kinds fall with equal velocity in the exhausted receiver of an air-pump. Hence we measure the mass of a body by its weight, and can only define the mass as a quantity proportional to the weight. If, then, at the same spot of the earth's surface, one body is twice as heavy as another, the mass of the first is twice that of the second. Suppose, however, that the body be weighed by a spring-balance at a certain place, and weighed again by the same instrument at another place nearer the equator, it will be found that the body is lighter at the latter place. It is found also that the acceleration due to the attraction of the earth is also less at the second place than at the first, in the same proportion. This illustrates the fact that when the mass remains the same, the weight varies as the acceleration of gravity. Hence the weight varies as the product of the mass and the acceleration of gravity, and, consequently, when suitable units are chosen, the mass of a body is equal to its weight divided by the acceleration due to gravity."

Our next extract is chemical:—

"*Alcohol.*—By this name, when standing by itself, is usually understood the second term of the series of ordinary alcohols, or vinic alcohol. It is a transparent, colourless, mobile liquid, of a specific gravity 0·7939 at 60° F.; it boils at 74·4°C. (173·1 F.); its vapour density is 1·613; its formula is $C_2 H_6 O$; it is the spirituous principle of wine, beer, and spirits, and is produced by the fermentation of sugar, which is split up into alcohol and carbonic acid. In the diluted state alcohol is sometimes called spirits of wine. It is difficult to render anhydrous; distillation alone will not produce an alcohol containing less than 9 per cent. of water, and this remaining quantity must be removed by adding something which unites with the water chemically, as quick-lime. By oxidation it is converted into aldehyde, and then into acetic acid, but other products of oxidation are obtained in less quantity; these are formic acid, acetal, acetic ether, saccharic acid, gloxal, glyoxilic acid, and glycolic acid, the final products being water and carbonic acid. When the elements of water are removed from absolute alcohol, ether is formed."

Now for a few words of criticism, or rather of suggestion. The book is not quite free from mistakes, probably in most cases printers' errors, which ought by all means to be avoided in a Dictionary, and will doubtless be corrected when a second edition is called for, which we hope may be very soon. But what we find most fault with is the cross-references, which are needlessly complicated, and often misleading. Being anxious to see what was said about the latest discoveries in Spectrum Analysis, we turned to the article under that heading, which we found to be very clear as far as it went. At the end we were referred "for further information" to five other articles; of these, "Spectra of the Elements" and "Metallic Spectra" are not to be found; "Spectra, Metallic," does occur, but simply refers us again to "Coloured Flames," under which we found only two lines relating to spectroscopy. Neither of the articles referred to gave us any further information of importance on the subject.

One other criticism on another sentence in the preface. We observe with pleasure that this is to be the first of a series of similar dictionaries, which shall embrace the other departments of Science. One, it is stated, will have for its subject the "classificatory sciences" of Botany and Zoology. Now we must protest against the use of this term as applied to the two sciences named. Zoology and Botany have their physiological as well as their systematic side, and far the more important of the two. We trust that the forthcoming dictionaries will be framed on no such narrow basis as that implied in the denunciations by Mr. Emerson and Mr. Ruskin of the pursuit of botany as "a mere science of names." When these volumes are published, some confusion may possibly arise from the title "Dictionary of Science" having been given to this work, when it should have been more correctly "Dictionary of Physical Science."

The book is however an indispensable addition to the library of every student, and we cordially recommend it to the notice of our readers.

A. W. B.

LETTERS TO THE EDITOR

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

Ocean Currents

I do not altogether agree with Mr. Johnston in concluding that his suggestion as to the influence of barometric differences on Ocean Currents was stated with insufficient clearness. His words were:—"The waters which lie under the high pressure area have a tendency to escape from under the excessive weight, towards the space over which the pressure is less;"—and about the meaning of this sentence there can be no dispute. It seems to me rather that Mr. Johnston, in his letter in NATURE for Feb. 2, is changing his ground; and whilst he formerly distinctly suggested the probability of the currents being due to mean differences of pressure stationary over certain oceanic areas, he now wishes to attribute them to the *motion* of centres of low pressure. This is quite a different thing, and is so far admissible, that what Piddington aptly called "storm-currents," attendant on tropical cyclones, are undoubtedly caused in such a way. But these storm-currents are very exceptional; within the tropics, where their excessive development renders them most noticeable, they occur only in the hurricane months, and even then only at intervals; they have clearly no connection with the continuous current which flows west all through the year, and besides the great depressions of the barometer which give rise to these, there are no others. The barometer in tropical latitudes is remarkably constant, and shows no centres of low pressure passing continually towards the west, in the direction of the Trade-winds or the Equatorial Currents. The Equatorial Currents are thus clearly not due in the slightest degree to differences of barometric pressure, either to those that are stationary, as Mr. Johnston now seems to admit, or to moving ones which exist only at rare intervals.

It is however certain that centres of low pressure, round which the air circles, do very frequently pass along in temperate latitudes, driven (it seems to me) by the prevalent west wind. In our own latitudes, and more especially in the winter months, these succeed each other at intervals of a few days, and though their action is intermittent, and for the most part peculiar to winter, I see no reason to doubt that they carry with them a species of storm-current, which does occasionally modify and even intensify the prevailing easterly drift. But it is in the highest degree improbable that the *formation* of these centres of low pressure can give rise to any appreciable currents. Even towards the centre of a cyclone, the barometric gradient does not exceed one inch in fifty miles; which, so far as its effect on the ocean is concerned, is equivalent to a difference of water-level of one-fourth of an inch in a mile; and even this can only exist if we suppose the barometric depression to be formed almost instantaneously. There is no evidence that it is so formed; and the longer it takes to arrive at its maximum, the more gradually does the water rise into the central space, and the more infinitesimal is the velocity with which it does so.

Mr. Johnston rightly corrects the slip which appeared in my former letter, of twenty miles *an hour*, instead of *a day*, and thinks that even twenty miles a day is too large an estimate of the velocity of the equatorial current of the North Atlantic; but the Admiralty chart to which he refers shows many instances of a velocity still greater; and whatever force produces the current must clearly be adequate to the production of the greatest velocity it attains. This, however, is of no consequence to my argument. The barometer decreases very steadily, regularly, and gradually, from the patch of permanent high pressure to the line; and the effect of the formation of this patch would therefore be equivalent—as I said before—to a difference of water-level of about four inches in 1,800 miles; a difference which could no more generate a current of ten miles a day than it could one of twenty; but as whatever effect it was capable of producing was produced, once for all, many ages ago, the consideration of it has no direct bearing on the currents of the present day.

I would, therefore, repeat that neither as permanently existing nor as changing with the seasons, neither in their continuance, nor in their formation, nor in their fluctuations, can the areas of high and low pressure, which Mr. Buchan has delineated, and which formed the subject of Mr. Johnston's first letter, produce an appreciable effect on the Ocean Currents; and that, since the Equatorial Currents in the several oceans follow the course of the Trade-winds, and are not affected by transitory differences of pressure which do not exist in the Trade-wind region, the movement of these differences of pressure where they do exist, cannot be considered as causing the current, or as necessary to its flow, though it may occasionally give it an increased velocity. But any effect so produced is due not to the mere existence or formation of a centre of low pressure, but to its *onward movement*; and to urge that this onward movement exercises an influence on the currents, is merely to urge, with respect to one not very important detail, the application of that principle which, in its widest sense, I have already maintained through a long chapter in my Physical Geography, and which Mr. Croll is now maintaining in the *Philosophical Magazine*—that the Ocean Currents are due entirely to the action of wind.

J. K. LAUGHTON

Royal Naval College, Portsmouth, Feb. 5

In reference to the exchange current found to exist between the Mediterranean and Atlantic, at the Straits of Gibraltar, it seems a matter for inquiry how far the rise of the ocean bed, just outside the Straits, is natural; and how far it may have been formed or increased by deposits left during the successive ages, by this undercurrent of out-set-water, from the Mediterranean Sea into the Atlantic Ocean. A. H.

Jan. 19

The Frost

HAVING seen notices in your journal about the severity of the late frost, I beg to state that its duration and severity have been most remarkable here, and unequalled, as far as my knowledge extends, for many years back. My instruments are standard ones, which have been recently compared at Kew, and placed in my garden quite detached from buildings, and facing the N.E. at 4ft. elevation. I append a table of the observations, which may be interesting to some of your readers:—

Dec. 1870.	Shade max.	Shade min.	Exposed min.	Jan. 1871.	Shade. max	Shade. min	Exposed. min
20	47·8	30	30	1	27·5	19·8	18·2
21	30	22·8	22	2	26	24	24
22	26·8	15	15	3	33	21·5	21·5
23	29	1·5	0	4	31	14	13
24	24	−2	−3·5	5	39	23	23
25	28	+3	+3	6	42·2	31	31
26	33	24	23	7	45	30	29·5
27	31	24	23·5	8	40	28·3	28·3
28	32·2	27	27	9	40	25·3	25
29	32·8	10·5	10	10	40	28	28
30	31·6	15	14·5	11	36	30·5	29
31	32·8	15·2	14·3	12	35·8	20·5	20
				13	35·6	26·5	26

The frost was succeeded by a heavy gale of wind and a deluge of rain; in four days 2¼ inches fell, one inch being between 9 A.M. 17th, and 9 A.M. on the 18th. This, together with the melting of the snow, inundated the valley of the Medway round us for miles. The greatest cold I ever registered here was on January 4, 1867, being 5° below zero. The highest shade temperature I have recorded was 100°·5 on July 22, 1868, which was the hottest summer ever experienced.

Tunbridge G. H. FIELDING

Caves near St. Asaph

IT will interest archæologists to know that new caves are being opened by Mr. Townshend Mainwaring in the neighbourhood of St. Asaph, and that already we have much additional evidence brought to light as to the early inhabitants of that part of the country. In one which appears to run downward into the cliff at Carregwen, near Galltfænan, remains of various animals have been found in brown cave-earth, among them one which has been determined to be that of a reindeer, by Mr. Dawkins, who is further of opinion that it has been gnawed by a wolf or hyæna. This is very interesting, as the cave is high up in the face of a precipice, and with the present physical geography the larger animals could not get into that cave except by being carried there; so that we have here either cave-earth containing remains of such a remote antiquity that the gorge below has been considerably altered since its accumulation, or we have the ancient abode of carnivorous beasts able to carry the large animals into their den.

In Brysgill, Mr. Mainwaring has met with greater success. From the rubbish and tumble under the rock shelter outside the mouth of a large cave, he has obtained a fine bone scraper ground to a sharp edge, several flint flakes and bones of man, horse, ox, sheep, hog, &c. Inside the cave, immediately under the recent mould, there is a broken stalagmite floor, associated with which were human bones and the flint flakes, and cores. At about two feet below the broken stalagmite floor, the bones of a horse were found in undisturbed brown earth. Here we have evidently the home of some of our troglodytic ancestors who manufactured their flakes in the cave from flint which they may have procured from the drift not far off.

This is only one of a number of most promising looking caves to which Mr. Dawkins some time ago called attention, and it is to be hoped that, with so many residents in the neighbourhood interested in scientific investigation, we may have them all systematically explored, and not lose any bit of important evidence from the want of observation at the time of discovery.

T. McK. HUGHES

The Primary Colours

ONE more proof that violet is a primary. Place a hand prism between the eye and the sunlight so as to show the prismatic colours. Then hold a sheet of yellow glass between the prism and the light, and observe the result. The reds and yellows are scarcely altered, the greens are very greatly intensified, the blues and violets are altogether extinguished. If violet had really any red in it, the yellow glass, which does not stop the red rays, would change the violet to red, or would show at least some *trace* of red where the violet had been. Instead of this, the violet is totally stopped out, and the space which it occupied left dark. Wherever the secondary *pink* appears, this is changed to red by the stopping of the violet rays. The increased strength and brilliancy of the green shows clearly also the primary character of this colour. It is usually much weakened in the spectrum by mixture with the far-spreading violets; when this is removed it comes out in full splendour. I commend this little experiment to amateurs; it is simple and interesting. The same effect is produced by throwing the coloured spectrum on to a white wall, and holding the yellow glass between the prism and the wall.

Leicester, Feb. 20 FREDERICK T. MOTT

Californian Oaks

IN NATURE, No. 68, p. 313, you did me the honour to quote a paper of mine in reference to the edible qualities of some of the Californian oak acorns. You will, however, allow me to state that, though this is true of some species, such as *Quercus lobata* Nee, which was the one I chiefly referred to in the passage quoted, yet that the acorns of others have a decidedly injurious effect, or are inedible. For instance, it is very commonly believed by the *rancheros* that the acorns of *Q. Kelloggii* Newb. give rise among pigs to a peculiar disease of the kidneys, while the acorns of a new species from Southern Oregon—which I shall shortly describe in a work now in the press—(*Q. echinoides* mihi) are so very bitter that no animal but the black bear (*Ursus Americanus*) will eat them, and it only when pressed by hunger. On the other hand the acorns of *Quercus Orstediana* (mihi), another as yet undescribed species, are so nutritious, that though the species never grows to a greater size than a small shrub, the produce of forty or fifty such bushes will fatten a hog. Again, there is a difference of quality among the edible species. The "digger" Indian, who is quite a *connoisseur* in acorns, makes a difference; for while the interior tribes prefer those of *Q. lobata*, those living near the coast chiefly affect *Q. sonomensis* Benth. Though pigs fatten freely on the acorns of *Quercus Garryana* Dougl., and in California on those of its ally, *Q. Douglasii* Hook., yet I never knew the Indians either in Vancouver Island or in California eat the acorns of either species, while those of *Q. agrifolia* Nee, *Q. chrysolepis* Liebm., *Q. densiflora* Hook. and Arn., *Q. Sadleriana* mihi (nondescript species), &c., are not, so far as I am aware, eaten by any animal but squirrels. The fruit of *Castanopsis chrysophylla* Doug., a plant allied to the oaks and chestnuts, is, however, in great favour with the black bear. I have eaten the acorns cooked in the manner described in the extract, and—I suppose in common with other naturalists in the less explored parts of North-west America—have been forced by hunger to search for the acorns which *el carpentero* (*Melanerpes torquatus*) stores away for its use in the spongy bark of *Torreya*, *Sequoia*, *Pinus*, and various other trees, yet notwithstanding the sauce which famine gave to my appetite, I must confess that they were by no means palatable. This may, however, have been prejudice, for the Ancient Britons—who were by no means savages in the ordinary acceptation of the term—ate the acorns of *Quercus robur*, the common oak of this country. How they cooked them we are not informed. I presume, however, that it was not in so *recherché* a style as practised by some aboriginal friends of Mr. Paul Kane, the artist—a full description of which those curious in North American Indian *cuisine* will find in that gentleman's book descriptive of his journey across the American continent. ROBERT BROWN

Edinburgh, Feb. 20

THE ECLIPSE PHOTOGRAPHS

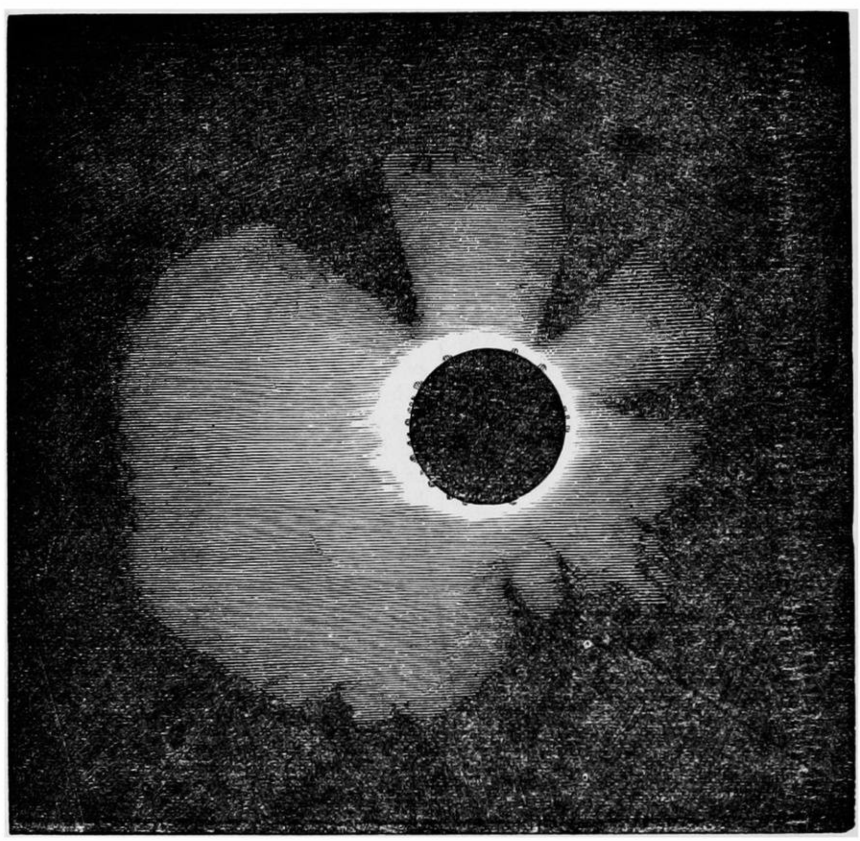
THE accompanying woodcut is a copy of a drawing made from the negative No. 5, taken at Syracuse during the Eclipse of the sun on Dec. 22 last. When viewed by transmitted light, the negative shows chiefly the portions indicated by the unshaded parts, and the red prominences; some parts of the first light shade can be seen, but the outer rays are altogether invisible. When, however, the plate is viewed by reflected light, the whole of the detail is distinctly seen. The negative was the last one taken; four others were exposed for the corona, but owing to the presence of cloud very little detail is visible.

It will be noticed that there is more of the corona shown on the west side of the moon than on the east, north, or south. This feature is shown on all the plates, so that there can be no question that there was more coronal light on the west side of the moon than at the other points. In explanation of the great display of the outer rays (I use the term *rays* for want of a better—perhaps *outer light* would be more correct, for there is no indication of lines or rays on any of the plates), I had supposed that the east side might have been partially covered with cloud; but in conversation with Prof. Eastman I found that he was observing for the reappearance of the sun, and he is quite certain that there was no cloud at the time the photograph was taken—that is, at about thirteen seconds from the end of totality. Mr. Fryer also is equally certain that there was no cloud. The plate was exposed eight seconds. It will be noticed also that the prominences are more numerous on the side where the corona is brightest.

Various opinions have been expressed as to the quality of the light of the corona. The effect we saw was that of moonlight, but not of the *full* moon, excepting the brilliant light close to the moon's limb, which is equal to the brightest moonlight, and I think its action on the sensitive plate confirms this opinion.

A point of much interest to be noticed is, that the light of the corona had been considered to be much less active than it really is; eight seconds were sufficient to produce on the plate an effect of light extending beyond the moon's limb, at least one and a half millions of miles.

I leave it to others to account for the cause of the great gaps or rifts in the corona; also their identity in position with those shown in the photograph taken by the American photographers at Cadiz. The identity of one of the rifts



THE LATE ECLIPSE, AS PHOTOGRAPHED AT SYRACUSE

is absolutely fixed by the two prominences between which it appears in the photographs, and this one gives the relative places of the others.

When the two photographs are compared, there is an apparent difference in the places of the rifts with respect to their angular position on the moon's circumference. How this difference arises I am not prepared to say, as I have no information as to how the American picture was taken, and there is no mark on the transparency which has been lent to me by Prof. Young, to indicate the north point. In the engraving from my photograph the top is the north.

It is perhaps necessary to say that it is quite impossible to represent in an engraving on wood the delicate detail of the corona. The cut fairly gives the main features, but it is *hard* when compared with the original; the contrast should not be so great; the ground should not be perfectly black; and the effect should not be produced by *lines*. No woodcut has ever yet accurately represented the phenomena of the eclipsed sun.

When the photograph No. 5 is combined in the stereoscope with the one taken about one minute earlier, stereoscopic relief is produced—the corona is distinctly seen beyond the moon. It may be thought that this is merely the effect of contrast, but I believe it is really due to the change in the position of the moon. No such relief is seen when two copies of the same photograph are combined stereoscopically.

In order to see the woodcut with the best effect, it should be placed at a few feet distance from the observer, so as to lose all trace of the lines of the engraving; the effect is then very accurately given of the corona as seen by the unaided eye.

A. BROTHERS

THE LATE EAST INDIA COMPANY'S MUSEUM—A ZOOLOGIST'S GRIEVANCE

THE late East India Company in their former palace in Leadenhall Street were in possession of a valuable Zoological Museum. It contained specimens in all departments of science, received from the Company's Oriental dominions. These had been contributed by public servants, attached as naturalists to various missions, or had been given by gentlemen of the civil and military services to the Court of Directors. Amongst the contributors to the East India Museum, it will be sufficient to mention the names of Dr. F. Buchanan Hamilton, Dr. Horsfield, Sir T. Stamford Raffles, Col. Sykes, Dr. Wallich, Mr. M'Clelland, Dr. Falconer, Mr. Griffith, and Mr. Hodgson, to prove that the collection was one of no ordinary merit. The Zoological importance of the East India Company's Museum was further augmented by the preparation and publication by, or under the superintendence of, the late Dr. Horsfield, of several catalogues. Of these may be particularly mentioned that of the Mammalia, published in 1851, and that of the Birds in 1854 and 1858, the second part of which bears likewise the name of Mr. Frederick Moore, then assistant-keeper of the Company's Museum, as joint author, on its title-page.

When the East India Company became extinct, and the premises in Leadenhall Street were vacated, the Museum was removed to Fife House, Whitehall, but was very imperfectly exhibited there, a large portion of the contents (the more bulky specimens in particular) being kept stowed away in boxes. When naturalists who wanted to consult specimens remonstrated at their inaccessibility, they were told that this was a mere temporary arrangement, and that when the magnificent buildings of the new India Office were completed, special accommodation would be assigned to the Museum, and there would be ample space for everything. At length the time arrived. The new India Office, with its suites of *salons*, assembly rooms, waiting rooms, and apartments of every description, was finished and opened. Fife House was demolished, and everything that it contained was removed to the new establishment. But when space was required for the Museum it was discovered that the only rooms assigned to this purpose were three or four chambers in the uppermost story, which would not contain a tenth part of the collection. Dr. Forbes Watson, the present chief of this department, has thought it right to devote these to the exhibition of a fine series of specimens illustrative of the arts and manufactures of British India, and we are by no means disposed to find fault with his decision on this subject. But it is the duty of the Government, we maintain, either to provide proper space for the Zoological collections also in the New India Office, or to transfer them to some other Institution, where they may be at least accessible to the scientific student. These Zoological collections contain a large number of typical specimens, without reference to which it is impossible in many cases to ascertain the identity of the species. Some of these typical specimens have, we believe, been handed over to the British Museum, but a number of them still remain in the collection, packed away, we are told, in the same cases in which they were originally removed from Leadenhall Street. This is, we maintain, a great and crying scandal, though as only a few working Zoologists are injured thereby, it is difficult to excite popular feeling upon the subject. In taking over the goods and chattels of the former Company, the India Office must certainly be held to have accepted the corresponding liabilities. Amongst these, it cannot be denied, was that of keeping, at least safe from destruction and in a state accessible to the scientific student, the specimens which the servants of the former Company amassed at such an expenditure of time and toil. If, as we are told, the new India Office is already so short of space that it is not possible to find room for them within its precincts, it is very simple to obtain the necessary accommodation elsewhere. We have good reason to know that Naturalists working on various branches of Indian Zoology are frequently brought to a standstill by the impossibility of access to this important collection, and we trust, therefore, that some steps will be taken to remedy the evil

P. L. S.

THE METAMORPHOSES OF INSECTS *

THIS very handsomely got-up volume is illustrated by 40 full-page engravings, many of which are exquisite landscapes as well as representations of insects in their various stages; and by about 200 excellent woodcuts in the text, from which we have selected a few specimens as samples of the rest. The subject of insect transformations presents us with so many curious examples of instinct, and such strange eccentricities of structure and habits, as to be especially adapted to attract the attention of the young, and to lead them to study this most fascinating branch of Natural History. The name of M. Emile Blanchard, and the high scientific reputation of Prof. Duncan, are a sufficient guarantee that the facts are accurately stated. In the introductory portion of the work, the main features of the external structure and internal anatomy of insects are exhibited by such large and clear illustrations as to be easily comprehended, the changes in the nervous system, from the larva to the perfect insect, being particularly well shown. The nature of metamorphosis and its different kinds are then explained, and a series of chapters is devoted to each order of insects, beginning with the Lepidoptera and ending with the Crustacea.

Among the more remarkable forms in the first-named order are the Psychidæ, small moths the females of which are not only without wings, but have neither legs nor antennæ. The female *Psyche* is, in fact, a mere helpless egg-bag, which never quits the case or covering in which it was bred. The males are small delicate moths with bodies covered with long silken hairs, and with dusky semi-transparent wings. The larvæ live in cases made of silk or vegetable tissue, bits of straw, stick, or leaves, and they carry these cases just as snails do their shells.

The ravages of the Tineidæ and the curious cases of *Coleophora* and *Gelechia* are illustrated by figures after Stainton; while the cut on p. 331 represents the beautiful pink or violet net-work cocoons in which some Brazilian species suspend themselves by slender threads.

The parasitic Hymenoptera forming the families Ichneumonidæ, Chalcididæ, and Proctotrupidæ are well described, and a quotation from this chapter will exhibit the style in which the book is written :—

"These parasites are very pretty and elegantly-formed insects when in the adult form, and are gifted with great agility and restlessness; but in their early condition they cannot move, having no locomotive organs, and their structures are so soft that they are destroyed with the greatest ease. The larvæ look like worms or maggots, and do not attain a great perfection of development during their growth. All the parasites seek out a caterpillar, a larva, or an insect which suits their purpose, in order to lay an egg within its body. The larva which is born from this egg is nourished by the blood and fat of the victim, whose vital organs it does not touch or injure in any way; for were it to die, the parasite would come to an end also. It is only when the larva is nearly full grown, and is about to undergo its metamorphosis into a pupa, that it appears to know that the life of the victim is not likely to be of much further use. It then devours the internal organs of the unfortunate insect, and undergoes its transformation. The skin of the victim protects some of the pupæ of its destroyers after all the inside has been eaten. Nearly all, if not quite all, insects are subject to the attacks of parasitic Hymenoptera. Fine, smooth, and brightly coloured caterpillars often have a black spot upon their skin, and this is the healed wound of the ovipositor of one of the parasites. Sooner or later the creature is sure to die, and

* "The Transformations or Metamorphoses of Insects (Insecta, Myriapoda, Arachnida, and Crustacea)." Being an adaptation for English readers of M. Emile Blanchard's "Metamorphoses, Mœurs, et Instincts des Insectes." By P. Martin Duncan, F.R.S., Professor of Geology in King's College, London. (Cassell, Petter, and Galpin)

it never reaches the stage of growth when it can lay eggs or reproduce its kind, for before this time the growing larvæ within destroy it, as it were, by slow consumption. Some affected caterpillars die soon, others nearly reach their full growth, and a few undergo their transformation into the chrysalis state before death. It is, therefore, not an uncommon thing for a butterfly-collector, who hopes to see a fine moth disengage itself from its pupal covering, to be disappointed by the appearance of several little parasitic Hymenoptera that had been living within the chrysalis he has been keeping."

One of the most curious recent discoveries among beetles is that which was published by Schiödte, in 1864, of viviparous Staphylinidæ. These are about the tenth of an inch long, and are found in the nests of the *Termites* of Brazil. They are distinguished by the swollen development of the abdomen, which is carried in a most peculiar manner, being turned up and allowed to rest on the back of the insect. The enormous distension of this part of the body is due to the fact that the beetles do not lay eggs, but produce living larvæ, and they are the only beetles that do so. It is supposed that the hairs which

FIG. 1.—THE CHRYSALIS AND THE FEMALE PERFECT INSECT OF *Psyche graminella.*

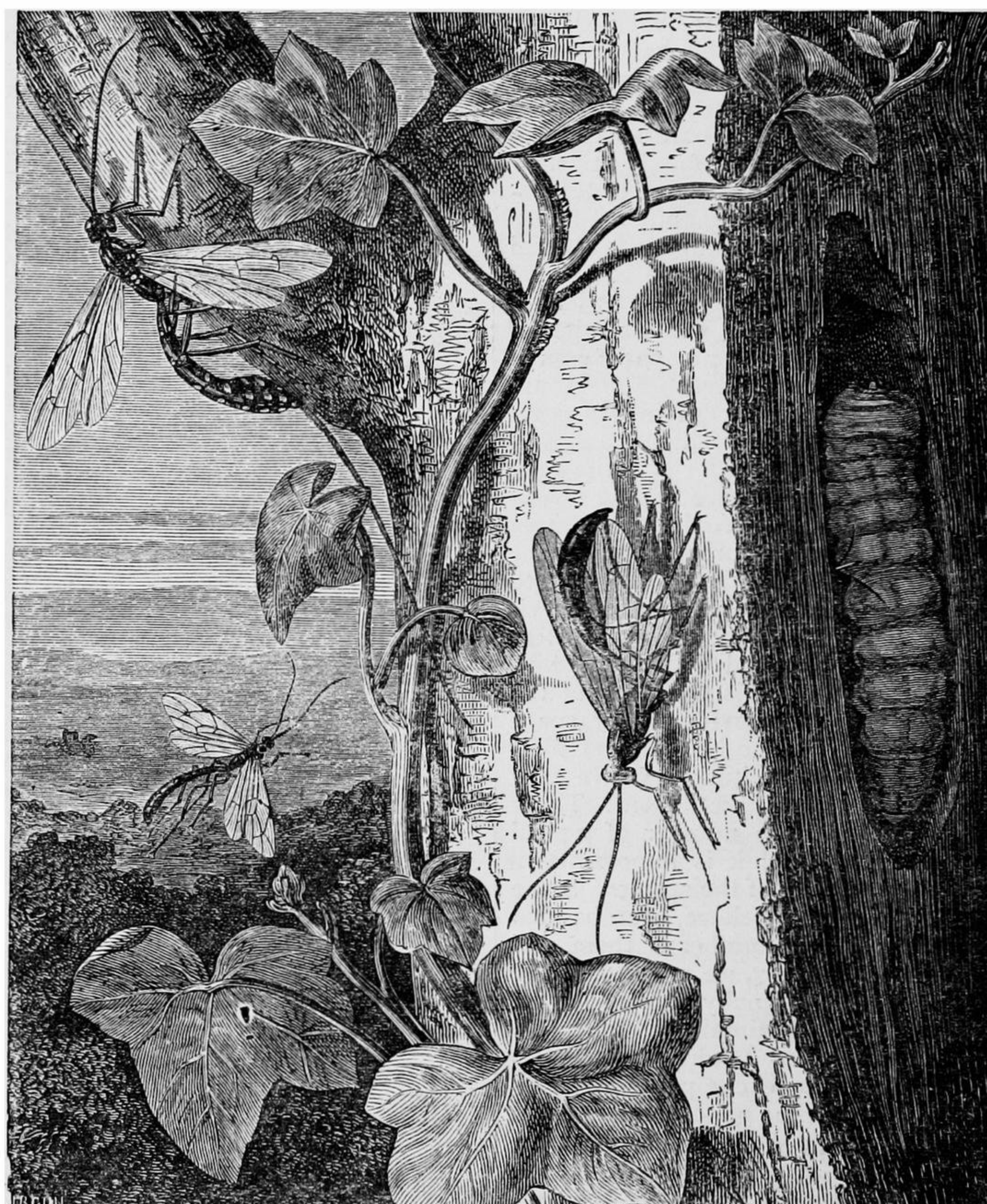


FIG. 2.—ICHNEUMONS

Ephialtes manifestator. The male is flying on the left, and the female is introducing an egg into the body of a larva. Another *Ephialtes, Rhyssa persuasoria*, a female, is on the branch to the left hand.

cover some parts of the abdomen are furnished with a peculiar secretion that is liked by the white ants among which they live.

The extraordinary economy of many of the Diptera or flies is exhibited in a variety of beautiful cuts, one of the best of which represents the metamorphoses of *Stratiomys chameleo.* This fly frequents flowers in order to prey upon other insects, but its larva lives in stagnant water, and is a long, hard-skinned creature, whose small head is furnished with two minute hook-like mandibles. The terminal segments of the body are gradually narrowed, and can be elongated like a sliding telescope, and the slender extremity terminates in two small orifices crowned with hairs. This larva swims about in shallow water, and when it wants to breathe it sticks up the end of its body and respires through the two small holes at the apex. When the larva is mature it floats on the surface, the pupa being formed within the skin, which serves at once as a cocoon and as a boat, from which in due time the brightly-coloured and active fly escapes to its aërial existence.

In the last chapter a short but clear account is given of the recent discoveries as to the metamorphoses of Crustacea, from the works of Spence Bate, Fritz Müller, Darwin, and others.

In the original work reference is chiefly made to common French or Continental insects, some of which are natives of our own country, while many are not found here. In adapting the work for English readers, it would have been well to have stated in every case whether the insect mentioned was an English one. The great carpenter bee

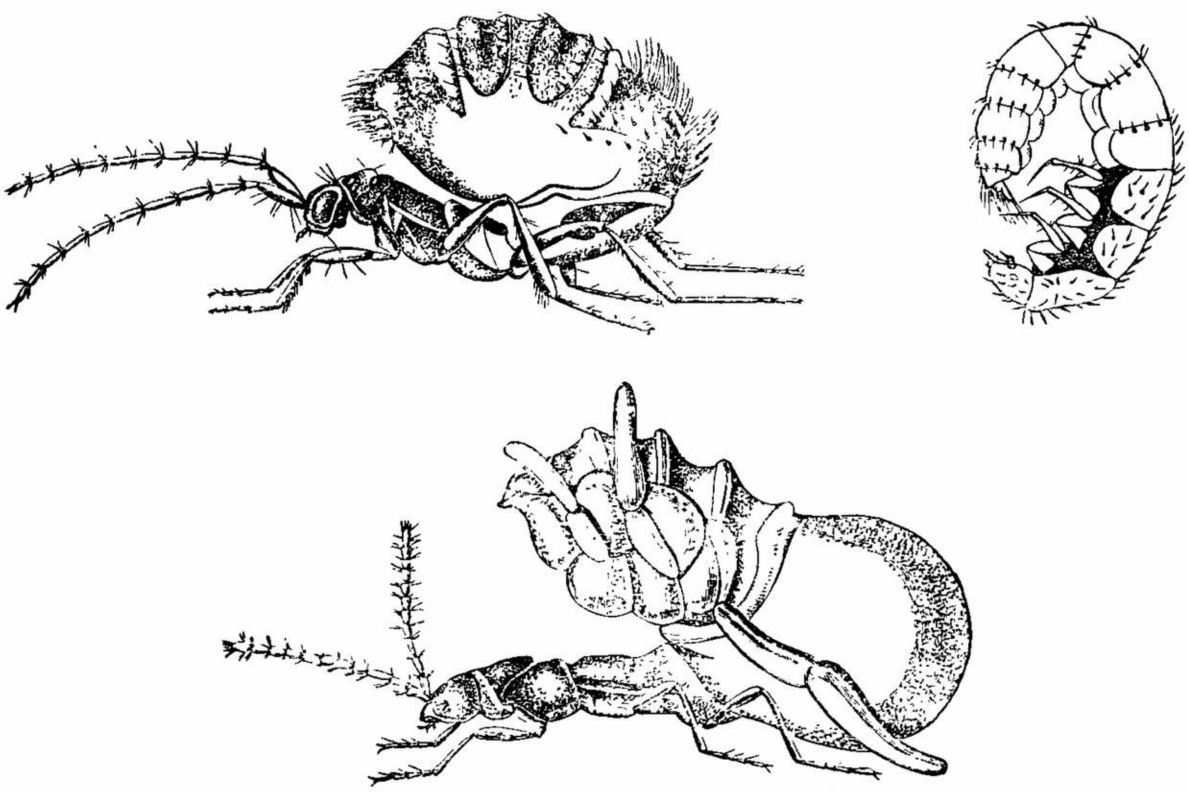


FIG. 3.—VIVIPAROUS *Staphylinidæ*. (After Schiödte.)

Corotoca melantho and larva. *Spiractha Eurymedusa*.

The upper figures are those of *Corotoca*. The turning up of the hinder parts of the body is very evident in the engraving.

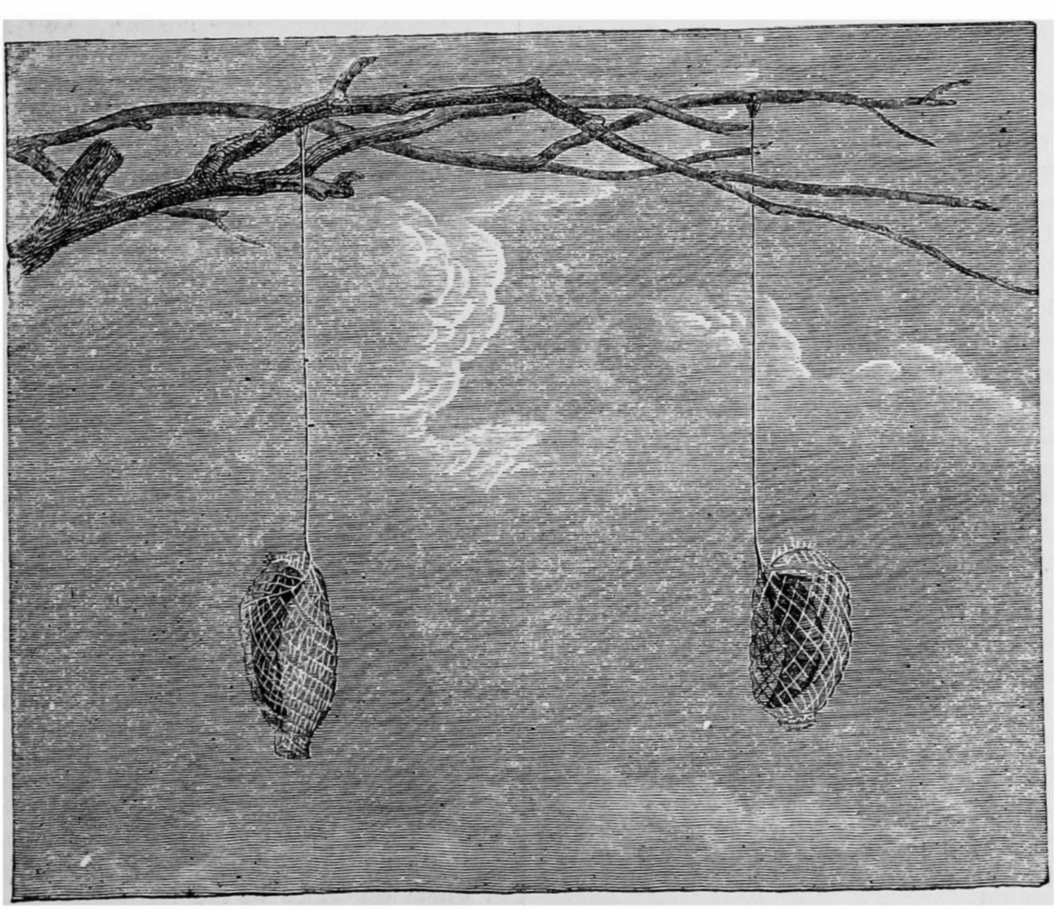


FIG. 4.—COCOONS OF BRAZILIAN *Tineina*.

(*Xylocopa violacea*), for example, is described as "not uncommon;" but the reader is not told that, although common in France, it is unknown in England. More simple and familiar language might also have been occasionally used; but these are small defects in so useful and attractive a work, which is just the thing for a present to an intelligent and inquiring country schoolboy.

ALFRED R. WALLACE

NOTES

WE understand that the Home Secretary has intimated to Dr. Lyon Playfair, member for the Edinburgh University, that he will submit the name of Mr. Geikie, F.R.S., to the Queen as first professor in the new Chair of Geology in that university. This has been done at the express recommendation of Sir Roderick Murchison, who, as already announced, has given the sum of 6,000*l.* to found the chair. In spite of her great mineral wealth, Scotland has no school of applied science like the State-supported establishments in London and Dublin. It is matter for congratulation, therefore, that the first appointment to the first Chair of Geology established in Scotland should have been given to one whose position as director of the Geological Survey in that part of the United Kingdom will enable him to act effectively in the teaching of the practical applications of geology.

THE trustees of the British Museum have wisely not departed from precedent in appointing Mr. W. Carruthers, F.L.S., F.G.S., to the Keepership of the department of Botany, in the room of Mr. J. J. Bennett, lately resigned. Mr. Carruthers has been Mr. Bennett's senior assistant for eleven years, during which time he has done much to render the extensive botanical collections of the Museum readily accessible and useful to the public. Mr. Carruthers is widely known for his numerous and important papers on Vegetable Palæontology, in which he has greatly contributed to a knowledge of the structure and affinities of extinct forms of vegetation.

ACCORDING to a constant practice strictly adhered to for almost two centuries, at the first sitting in January of the French Académie des Sciences, the vice-president for the preceding year fills the office of president, and keeps it for twelve months. But M. Coste having left Paris before the siege, the change in the presidency could not take place. The members present, to the number of thirty-one, elected the vice-president for 1871, who will be president for 1872. M. Faye was elected and proclaimed by M. Lionville, who was then in office. The election took place with the thermometer at – 6° C. No fire was allowed to be lighted, in consequence of the scarcity of fuel.

DR. W. B. CARPENTER recently delivered his Lecture, "On the Temperature and Life of the Deep Sea," at the Hulme Town Hall, Manchester, and it is reprinted as one of the penny series of "Science Lectures for the People," to which we recently referred in terms of commendation.

THE report of the commissioners appointed by the Lord-Lieutenant and General Governor of Ireland in October 1868 to inquire into and report on the Artificial Cultivation and Propagation of Oysters, and the methods used in some parts of Great Britain and France for these purposes, has just been printed. It forms an octavo blue-book of nearly 200 pages, and is illustrated with woodcuts and ten plates. We hope in a future number to give a short notice of the recommendations offered in this report.

WE understand that the library of the late Dr. Matthiessen, including a large selection of valuable works on chemistry and physics, is to be sold to-morrow, at Puttick and Simpson's Auction Rooms, in Leicester Square.

PROF. KARL KOCH, of Berlin, proposes the establishment in that city of a Dendrological Garden, to be specially devoted to the cultivation of species and varieties of all kinds of foreign trees and shrubs. Plans of the proposed garden were shown at the last meeting of the Council of the Royal Horticultural Society.

M. JANSSEN is now at Bordeaux preparing his memoir on the Eclipse, for presentation to the French Académie des Sciences.

THE Astronomer Royal for Ireland has just published the first part of a series of astronomical observations and researches made at Dunsink, the Observatory of Trinity College, Dublin. This part contains results of observations made with the South Refractor from June 1868, to October 1869, and consists of 88 quarto pages and three plates, printed at the expense of the Board of Trinity College. We purpose, in an early number, to give a short account of the College Observatory and of the equatorial erected for the fine 11¾in. object glass presented by Sir James South to the college.

THE following is a list of the third series of lectures arranged by the Sunday Lecture Society to be delivered at St. George's Hall, Langham Place, on Sunday afternoons, at half-past three o'clock :—Feb. 26—Mr. Moncure D. Conway, on "The Past and Present of New England : its Early History, Physical Features, Literary and Religious Development, and Sketches of leading thinkers—Emerson, Theodore Parker, &c." March 5—Jon A. Hjaltalin, of Iceland, on "Iceland : its Physical Features, Volcanoes, Hot Springs, &c., the Manners and Customs of its Inhabitants." March 12—W. G. Clark, M.A., Vice-Master of Trinity College, and late Public Orator, Cambridge, on "Protestantism." March 19—J. Norman Lockyer, F.R.S., on "The Total Eclipse of Dec. 22." March 26—T. Spencer Cobbold, M.D., F.R.S., on "The General Structure and Development of Ferns." April 2—Edward Maitland, B.A., on "Jewish Literature and Modern Education ; or the Use and Mis-use of the Bible in the School-room." April 16—W. K. Clifford, M.A., on "The History of the Sun : an Explanation of Laplace's Nebular Hypothesis, and of Recent Controversies in regard to the Time which can be allowed for the Evolution of Life." April 23—Prof. J. S. Blackie, on "War ; its Causes, Character, and Consequences."

AT the sitting of the 23rd January, the French Academy learned the death of M. H. Regnault, son to M. Regnault, the learned member of the section for physics. This young man, a painter of rising fame, had been killed when attacking the woods of Buzenval on the 19th of the same month. A deliberation of the assembly was taken to congratulate M. Victor Regnault, the father, then at Geneva. The body having been found was buried in the Père La Chaise before a deputation from the Académie des Sciences, Académie des Beaux Arts, and Académie Française. The proceedings were recorded in the *Comptes Rendus.*

DURING the week ending 18th February upwards of 3,500 British objects, consisting of sculpture, pottery, woollens, and educational works and appliances, were delivered at the buildings of the International Exhibition of 1871, besides foreign objects from Bavaria, Belgium, and Saxony.

WE are informed that public meetings on the subject of Science and Art Education have recently been held in many places in the West of England. The well-known Royal Albert Museum at Exeter, with its Science and Art classes, museum, and free library, has done much towards attracting attention to the value of scientific knowledge in this part of England. The Plympton Grammar School, which has just been reorganised under a new scheme, will include the teaching of chemistry and drawing as a regular part of the school course.

PROF. PHILLIPS is busily engaged at Oxford upon his new work on the "Physical Geography and Geology of the Thames Valley." This volume will contain numerous plates of fossils, illustrative of the various beds to be met with in the district, all of which are drawn by the learned Professor himself. In addition to these the numerous drawings of the magnificent series of Ceteosaurian remains lately acquired by the Oxford Museum will cause this volume to be eagerly looked forward to by every geologist and palæontologist

THE offer to build a Museum at Hereford in connection with the Woolhope Naturalists' Field Club has been very gratefully accepted by the Corporation of that town, and there is every prospect of Mr. Rankine's generous scheme being fully carried out.

THE British Museum has lately acquired two fine specimens of the most gigantic of known crabs. This species measures 10 feet between the tips of the claws, but has a comparatively small body, triangular in shape, somewhat convex. The claws are thin, and about 6 feet in length, including the pincers. Its habitat is Japan, where it is, we believe, eaten for food, and it seems strange that specimens should be so scarce in Europe. It was described and figured by Kæmpfer, in 1763, in his account of Japan, and is now named *Macrochira Kæmpferi* (Dettaan) in commemoration of that eminent naturalist. One specimen has also been acquired for the Edinburgh Museum.

AT the sitting of the 30th January the French Academy learned the death of M. Gustav Lambert, who was engaged in preparing an expedition to the North Pole when war broke out, and who had been badly wounded at Montrelaut when fighting at the head of a company of National Guards which he commanded. M. Élie de Beaumont read a memoir sent four years ago by the enterprising intended explorer. Before leaving Paris for his last fight, he had sent to the Défense Nationale a memoir on the means of communication between Paris and the Provinces. This memoir will be published when peace shall be restored.

THE large series of fossils in the Oxford Museum is in process of re-arrangement, and the space formerly occupied by the Physical Science Apparatus, removed into the Physical Laboratory, is now devoted to large collections of fossils hitherto undisplayed for want of room. Chief among these is to be noticed the magnificent series of Ceteosaurian remains from Enslow Bridge, which is the finest collection of gigantic reptilian remains to be seen in any museum in the world. The greatest care and ingenuity have been displayed in reconstructing these immense bones, many 4ft. or 5ft. long, from the broken and fragmentary state in which they were originally found.

THE splendid meteor noticed by Mr. Wilson, in our last number, as having been observed at Rugby, was also very clearly seen in the neighbourhood of Worcester a few minutes past nine o'clock. It is described as falling towards the S.W., and bursting like a rocket, when it left a trail of light behind it for some time after its disappearance. No audible explosion was heard. The atmosphere was clear, and there were only a few clouds in the sky.

M. BIENAYMÉ has published in the *Comptes Rendus* a rectification of a catalogue of the articles written by Cauchy, the celebrated mathematician, formerly a member of the French Institute. Some of the articles attributed to Cauchy were written by Cournot. This list is of some importance, as Cauchy's works are under publication at the expense of the French Academy, according to a vote which was taken a few months before the war broke out.

WE have received the first report of the Cheltenham College Natural History Society, which we commend to the notice of similar youthful societies, for the modest earnestness that pervades it. The achievements of the society for the first year of its existence are summed up as follows :—"Botany—Decidedly well worked; best point of the Society. Entomology—Also well worked; sadly deficient in *Coleoptera*. Zoology—Practically reduced to Ornithology; so far, so good. Geology—Little attempted; less done." In so rich and interesting a county, geologically speaking, as Gloucestershire, we hope the Society will have next year a very different tale to tell. We notice that in order to encourage the pursuit, the Council offers a prize for the best original collection of local fossils; but trust this will not tend to promote the habit of collecting for mere collecting's sake—the bane of young naturalists. From the general tone of the pursuit of Natural History in the College, however, we hope for better things. It is interesting to find that such subjects are discussed at their meetings as the theories respecting the Aurora, and the causes of the November Showers of Meteors; and although the papers on "The Flowers of Virgil's Eclogues" and "The Flowers of Virgil's Georgics" are stated to have been "scarcely suited to the requirements of a scientific meeting," we doubt not they will have invested the old poems with a new interest in the eyes of both the writer and his hearers. Some of the papers are, as might be expected, crude and inaccurate; the one on "Butterflies at Home and Abroad" requires revision; and the statement that the Killarney bristle-fern, *Trichomanes radicans*, has been gathered by one of the members in Cornwall, must be received with caution. Although this would not be "the only spot in England where that fern has yet been found" (there being old records of its having been gathered in Derbyshire, and it having recently been unquestionably met with in North Wales), and there is no inherent improbability in its growing in Cornwall, yet the specimen stated to have been gathered "not half an inch high," can scarcely have been satisfactorily determined to be this rare fern. Seedling ferns of all kinds are extremely difficult to distinguish, and when growing in damp places, frequently simulate the filmy appearance of the Irish fern. We shall watch with great interest the progress of the Cheltenham College Natural History Society.

DR. C. L. SPIEGEL'S "Cultur-historische Tafeln," published at St. Petersburg, gives in parallel columns the men distinguished in each epoch from the earliest times to the present in politics and war, technical or mechanical discoveries, medicine, divinity, natural science and mathematics, philosophy, law, history, belles-lettres, and the fine arts; in Greece, Italy, Spain and Portugal, France, Great Britain, America, Sweden, Norway and Denmark, Holland, Germany, Russia and Turkey, Asia and Africa. It is printed in German.

THE register taken by M. Renan at Montsouris Observatory shows that the minimum temperature in December was $-11{\cdot}7°$ C. in the garden, and $-12{\cdot}8°$ on the roof. This minimum is not exceptionally low for the district. The feature of the temperature for the month was not so much the number of degrees under freezing-point as the continuity of cold. Nine days only exhibited a mean temperature above the freezing-point. The temperature for the whole month was about 8° F. lower than the mean temperature of Paris for December.

WILLKOMM AND LANGE'S "Prodromus Floræ Hispanicæ," published at Stuttgart, is now brought down to the third part of the second volume, including the natural orders to the end of the Gamopetalæ or Corolliforæ.

FREIHERR LUDWIG VON HOHENBUHEL-HEUFLER has reprinted from the Proceedings of the Zoological and Botanical Society of Vienna an interesting contribution to botanical history, "Franz von Mygind, der Freund Jacquins." Mygind was born at Broust, in Jutland, in 1710, and after his education at the University of Copenhagen, and a short stay at St. Petersburg, took up his residence at Vienna, and devoted his attention to botany. He speedily became an intimate friend of Jacquin's, and corresponded with the most eminent scientific men of the day, including Priestley, Sir Joseph Banks, L'Héritier, and Gmelin. He did great service in investigating the flora of Austria, paying the expense of two Alpine expeditions by Wulfen. He died in 1789.

THE MICROSCOPE

IMPROVEMENTS IN THE LENSES OF MICROSCOPES

FOR some time, people in England have been content to let the improvement of the optical powers of the microscope remain entirely in the hands of the makers, believing, apparently, that Mr. Lister had effected all in his suggestions and improvements that could be desired. Dr. Royston Pigott, an able mathematician, formerly Fellow of St. Peter's College, Cambridge, and a Doctor of Medicine of that University, was not, however, inclined to look at the matter in this way, and for many years has been working and experimenting with a view, first, to test the accuracy of our best object-glasses, and, secondly, to suggest means for their improvement. It should be remembered that Oberhauser, Nachet, and especially Hartnack, on the Continent, not satisfied with the old system of combinations for object-glasses, and not having the benefit of Lister's researches, have made excellent objectives on a totally different system, and during the last few years the last-named maker has carried his system of "immersion lenses" to such a point of excellence as really to surpass the best glasses on Lister's system, in definition, penetration, working distance, and illumination. Those who do not admit the excellence of these objectives, which are now used by nearly all German histologists, have probably seen older glasses, made at a time when Hartnack had not reached his best. It is worth stating, now that the Parisian opticians are inaccessible, that Gundlach of Berlin has succeeded in making excellent glasses of high power at astonishingly small prices, some of his 1-12ths and 1-16ths immersion 1-16ths (so-called), being admirable in their performance. They are not, however, equal to Hartnack's glasses, which, though costing far less than what similar English glasses cost, yet are more expensive than Gundlach's. It is only fair to all parties concerned to state that the terms 1-8th, 1-12th, 1-16th, &c., as now applied to an object-glass, appear to have no definite meaning, but depend on the caprice of the maker, since the magnifying power of glasses, with the same fraction assigned to them, differs enormously.

To return to Dr. Royston Pigott. He found the usual means of testing an object-glass by trying if it gave some particular appearance with a "test object," such as the Podura-scale, very unsatisfactory, since we have no certainty to begin with as to what is the true appearance of such an object. He therefore examined minute images of objects of which he knew the true form, such as a watch-face or thermometer-scale, forming these images by aid of mercurial globules and the condenser properly adjusted below the microscope-field. By this means he has found that object-glasses corrected so as to show dark, sharply marked spines (like !!!) on the Podura-scale—a favourite test-object with our microscope-makers—give false, blurred, and distorted appearances with his known images, and on making such corrections of the objective as to show the known images in their true form, he finds that the Podura-scale, examined with the corrected objective, is not really marked at all, as supposed, but is beset with a series of bead-markings, which by intersection, when improperly defined, give the curious appearance like notes of exclamation. This important discovery of the falsity of our high powers (1-8th to 1-16th), has led Dr. Royston Pigott to pay more attention to the lower powers, and he finds that though you may not get so much actual amplification, you yet get a truer effect, and greater clearness of detail, by employing very carefully made low powers (1-2nd to 1-5th) and increasing the magnifying power at the other end of the microscope, *i.e.*, the eye-piece. We have in this way seen the beaded structure of the scales of Podura more satisfactorily than with very high objectives even when corrected so far as they would admit, and we may say the same of some Diatom-valves *e.g.*, *Pl. formosum*. It would be most important to know how far such a change of combination would be useful in histological work.

The general upshot of Dr. Royston Pigott's investigations appears to be that it is desirable to shift the burden hitherto cast almost wholly upon the objective, to the other parts of the instrument. We should be content with an objective as high as a fifth, or even less. A very deep eye-piece is to be used; and to correct residuary aberrations of the objective, and at the same time amplify, Dr. Pigott has introduced an important adjustable combination *between the eye-piece and the object-glass*. There seems to be considerable reason for the step proposed by Dr. Royston Pigott. Just as great results were obtained in passing from the single lens or combination to the compound microscope of eye-piece and objective, so by adding distinct integral factors to these two, such as Dr. Pigott's "aplanatic searcher," we may obtain excellences quite impossible by any amount of attention bestowed on the objective alone, or only with difficulty reached by long labour, leading to very high price for high powers.

Dr. Pigott has, during the past year, published some account of his researches in the *Quarterly Journal of Microscopical Science*, and has communicated papers to the Royal Society, one of which is about to appear in the Philosophical Transactions.

Naturally at first the makers in London and the Microscopical Society were sorely tried by Dr. Pigott's exposure of the Podura-scale, but we hear, as one good result already obtained, that Messrs. Powell and Lealand have constructed a new 1-8th both dry and immersion, with great care, which is declared to be the best glass yet made. It has been proposed to form a committee for the purpose of examining carefully as to penetration, definition, and angular aperture, the best glasses of our English makers, the best American glasses, and the best of Hartnack's, Gundlach's, and others; the glasses being mounted similarly, with private marks only for recognition, so as to prevent all possibility of prejudice on the part of the committee. Were this done, the result, whichever way it tended, would be eminently satisfactory. Of this the writer is sure, that many persons—even eminent microscopists—have made up their minds about the qualities of foreign objectives, without having seen any, or only very poor examples, and then when a really fair specimen of such a glass is placed before them, they exclaim with astonishment "Why this is the finest glass I have ever seen." We shall be glad to receive suggestions or assistance, in carrying out the proposed comparison of objectives. Dr. Royston Pigott has expressed his willingness to aid in such an undertaking.

E. R. L.

REPORT ON DEEP-SEA RESEARCHES

Carried on during the months of July, August, and September, 1870, in H.M. Surveying Ship "Porcupine" *

By W. B. CARPENTER, M.D., F.R.S., AND J. GWYN JEFFREYS, F.R.S.

THE equipment of the *Porcupine* for the previous Expedition had been found so complete and satisfactory, that nothing more was considered necessary to prepare her for the work of the present season than the overhauling of her gear, and the manufacture of new dredges, sieves, and other apparatus, on the patterns of those which had already proved most serviceable. We had the advantage of the same excellent commander, now promoted to the rank of staff-captain, with his able staff of officers; and we would take this opportunity of again expressing our deep sense of obligation to them all for their hearty co-operation in our scientific work, and for the unvarying personal kindness by which our voyage was rendered a most agreeable one. A considerable part of the crew, also, consisted of the same steady and experienced men. The Meteorological Department supplied eight of the protected Miller-Casella thermometers, including the two with the performance of which we had been so thoroughly satisfied last year; and we usually employed one of these in conjunction with one that had not been used in the previous Expedition.

At the request of the Committee, Mr. Siemens undertook to devise an apparatus for testing the depth of sea-water to which light, or at least the actinic rays, can penetrate. The foundation of the apparatus which he constructed for this purpose is a horizontal wheel with three radii, each of them carrying a glass tube in which a piece of sensitised paper is sealed up. The rotation of this wheel round a vertical axis brings each of the tubes in succession out of a dark chamber in which it ordinarily lies, exposes it to light in an uncovered space, and then carries it into darkness again. This movement is produced by a spring; but it is regulated by a detent that project from the keeper of an electro-magnet, which is made and unmadeby the completion or breaking of a circuit that connects it with a galvanic battery. When the magnet is made, it lifts the keeper with its projecting detent; and this allows the wheel to be carried by the spring through one-sixth of its rotation, whereby the first of the tubes is brought out into the open space. There it remains until the circuit is broken, whereby the magnet is unmade; the keeper then falls, and the wheel is allowed to move through another sixth

* Extracted from the Proceedings of the Royal Society.

of a rotation, so as to carry on the tube into the dark chamber. A repetition of the making and unmaking of the magnet brings out the second tube, and shuts it up again; and another repetition does the like with the third tube. This apparatus, with a deep-sea lead attached to it, is suspended by an insulating cable that contains the wires whereby it is connected with the battery in the vessel. Being lowered down to any desired depth, the circuit is completed, the magnet made, and one of the tubes exposed for as long a time as may be wished; the circuit is then broken, the magnet unmade, and the tube shut up again. The second tube may be exposed for a longer time in the same place, or the apparatus may be lowered to a greater depth, at which the experiment may be repeated; and the third tube may then be dealt with in like manner.—The committee having been satisfied with the performance of Mr. Siemens' apparatus, it had been arranged that trial should be made of it, and also of his Differential Thermometer, now provided with an improved Galvanometer; and he had undertaken to send out a qualified assistant to take charge of these instruments during the Mediterranean Cruise. The declaration of war between France and Germany, however, unfortunately interfered with this arrangement; the assistant (a German) being recalled to his own country, and no other competent person being available on a short notice. Under these circumstances it was thought better that the Differential Thermometer should not be sent out; but it was hoped that such a trial might be given to the Photometric Apparatus as should at any rate determine whether satisfactory results might be anticipated from its use, or whether any modifications in its construction might be needed. The apparatus was sent out to Gibraltar under charge of Dr. Carpenter, and was got into working order by his son and himself in Gibraltar Harbour. It proved, however, that the action of sea-water on the bearings,—increased as this was by the galvanic current arising out of the contact of iron and brass in them,—so embarrassed its mechanical arrangements, that no fair trial could be made of its photometric efficiency. But the experiment served the important purpose of showing the weak points of the apparatus; and neither Mr. Siemens nor Dr. Carpenter entertains any doubt that it may be so reconstructed as to answer the purpose for which it was devised.

The work of this year's Expedition was divided, according to the plan originally marked out, into two Cruises: the First to examine the Deep-sea bottom between Falmouth and Gibraltar; the Second to make the like examination of the western basin of the Mediterranean between Gibraltar and Malta, and to determine its Physical and Biological relations to the Atlantic,—with special reference to the Gibraltar Current. The First Cruise was under the scientific direction of Mr. Gwyn Jeffreys, who was accompanied by a young Swedish naturalist, Mr. Josua Lindahl, of the University of Lund, as Zoological Assistant; whilst Mr. W. L. Carpenter, as before, took charge of the Chemical department,—his special work, on this occasion, being the determination, by volumetric analysis, of the proportion of chlorine in samples of Atlantic water taken from the surface, the bottom, and from intermediate depths, so as to serve as a basis of comparison with similar determinations of Mediterranean water. In the Second Cruise it had been arranged that Dr. Carpenter and Prof. Wyville Thomson should co-operate as before; but the latter being unfortunately prevented by serious illness from taking part in it, the whole charge of this Cruise rested with Dr. Carpenter. He was fortunately able to retain the assistance of Mr. Lindahl; and the chemical work was continued (as in the Third Cruise last year) by Mr. P. H. Carpenter; Mr. Laughrin throughout acted as dredger and sifter.

[For the exceedingly interesting and valuable zoological details of the various dredging-hauls, we must refer our readers to the report itself.]

Throughout the whole of this cruise the temperature of the *sea-bottom* was taken by the protected Miller-Casella Thermometers in nearly every sounding. As, for the reason already mentioned, no extreme depths were sounded, and as the general rate of the diminution of temperature on the margin of the North-Atlantic basin seemed to have been established by the serial soundings taken in the expedition of the preceding year, it was not thought necessary to repeat these; more especially as the variety of depths at which the *bottom*-temperature was ascertained gave adequate data for comparison with the results then correlated. It will be shown hereafter that this comparison leads to some very interesting conclusions, fully confirming the view advanced in the last report as to the slow northward movement of an upper stratum of warm water 700 or 800 fathoms in depth, and of the southward movement of the whole deeper stratum, bringing water of an almost icy coldness from the Arctic basin into the temperate and even the intertropical zone.

During the whole of this expedition the temperature of the *surface* of the sea was ascertained and recorded every two hours, both by day and by night; as were also the readings of the Dry and Wet-bulb Thermometers, which were placed in a small penthouse on deck, in which they were freely exposed to the surrounding air, but secluded from direct or reflected solar heat. The temperature of the *surface-water*, from the time of our leaving the British Channel in lat. 48° N. to our turning the corner of Cape St. Vincent in lat. 36° 50′ N., increased at a rate which bore a pretty regular proportion to the Southing. Thus, at the "chops of the Channel," it averaged 62° for five days; whilst, by the time we approached Cape St. Vincent, it had gradually risen to above 69°. After passing that point, however, we found both the *surface* and the *bottom*-temperatures to present certain variations, which, though not considerable in themselves, proved to be of great interest when taken in connection with the peculiar condition of the *embouchure* of the Strait of Gibraltar. These points, however, will be more fitly discussed hereafter; and we shall now only notice a sudden *rise* in surface-temperature of about 3° which showed itself as we turned the corner of Cape St. Vincent and entered the *north side* of the embouchure; and a sudden *fall* of nearly 6° (to 66°·4) which was encountered when we entered the mid-stream of the narrower part of the Strait as we proceeded towards Gibraltar.

In the course of the first portion of the cruise between Falmouth and Lisbon (beyond which point Mr. W. L. Carpenter was unable to proceed), thirty-six quantitative determinations were made by volumetric analysis, of the amount of chlorine in as many samples of Atlantic water, taken (1) from the surface, (2) from the bottom at various depths, and (3) from various intermediate depths. The greater part of these, as will be shown hereafter, indicated a very close conformity to a uniform standard of density, as indicated by a specific gravity of 1·0268, and a Chlorine proportion of 11·84 per 1000;* the chief departures being observable in the *lower* density of the *deepest* waters, and in the occasional *excess* of density in the surface-waters. The former is doubtless attributable to the fact that the *deepest* water is essentially *Polar*, and therefore derives its more dilute character from that source. The latter we are inclined to attribute to the influence of slight concentration by evaporation.

Second Cruise.—Leaving Gibraltar early in the morning of Monday, Aug. 15, we steamed out into the middle of the Strait, for the purpose of commencing our experiments on the Gibraltar current. The point selected by Capt. Calver (Chart of Strait of Gibraltar, Station 39) lay midway between Point Carnero, which forms the south-eastern boundary of Gibraltar Bay, and Jebel Musa or Apes Hill, which lies opposite to it, at a distance of only 8 geographical (9¼ statute) miles, on the African coast, the Strait being here nearly at its narrowest; and it was also that at which the greatest depth (510 fathoms) was indicated by the soundings marked on the Chart. With this depth our own sounding, which gave a bottom at 517 fathoms, agreed very closely; and having thus at once found the position most advantageous for our work, that position was precisely determined by angles taken by sextant from the ship between conspicuous objects on the shore. The *bottom*-temperature obtained in the first sounding was between 5° and 6° *higher* than that which had been met with at corresponding depths on the bed of the Atlantic about 100 miles to the westward; whilst the *surface*-temperature was *lower* by from 1°·3 to 2°, as will be seen by the following comparative statement:—

	Station.	Depth.	Surface temperature.	Bottom temperature.
			°	°
Strait of Gibraltar	39	517	70·0	55·5
Atlantic	31	477	71·3	50·5
Atlantic	32	651	71·5	50·0
Atlantic	33	554	72·0	49·7
Atlantic	34	414	71·7	50·0

This striking difference led us to take a set of *serial* soundings at intervals of 50 fathoms, and these gave a result, which, though it appeared anomalous at the time, was afterwards fully explained, and proved to be of unexpected import. The temperature fell, at 50 fathoms from the surface, to 56°; at 100

* The proportion here adopted,—the number of Grammes of Chlorine to 1000 Cubic Centimetres of water, is that employed by Prof. Forchhammer in his elaborate Memoir on the Composition of Sea Water (Phil. Trans. 1865).

fathoms it was 55°·7; at 150 it was 55°·5; and from that depth to the bottom, at 517 fathoms, there was *no further descent.* Now, it will be shown hereafter that the thermal condition, which here so much surprised us by its contrast with that of the Atlantic waters, is that universally met with in the Mediterranean, the temperature of which, whatever may be its surface-elevation, falls to within 1° Fahr. above or below 56° at a depth of 50 fathoms, to a degree lower at 100 fathoms, and then remains uniform down to the greatest depth (1,743 fathoms) at which we examined it. And it thus appears that whilst the *surface*-water in this part of the Strait is certainly derived from the Atlantic, the *deeper* water, partaking of the thermal condition which so remarkably characterises that of the Mediterranean basin, may be fairly regarded as belonging to the latter.

This inference is in harmony with another fact ascertained on the same occasion, viz., the great excess in salinity shown by water brought up from the depth of 250 fathoms over the water of the surface. Whilst the specific gravity of the latter was found to be 1·0271, that of the former was 1·0293; and whilst the proportion of chlorine in the latter was 20·034 per 1,000, it was 21·775 in the former. Now in these particulars the surface-water agreed well with what had been found to be the condition of the water of the Atlantic, whilst the water at 250 fathoms agreed equally well with what proved to be the condition of the water at the like depth in the adjacent part of the Mediterranean. We were not a little surprised, however, to find that the water taken from the *bottom* (517 fathoms) was of much *less* density, as indicated both by specific gravity and by chlorine percentage, than that of the *intermediate* stratum; its specific gravity being 1·0281, and its proportion of chlorine 21·465. This apparent anomaly (the existence of which was confirmed by observations made on our return voyage) pointed to the existence of an out-current in the *intermediate* stratum as the probable explanation of the overlaying of the lighter by the heavier water. The specific gravity of the *bottom*-stratum closely corresponded, as we subsequently found, with that of the bottom-water over the deepest part of the area of the western basin of the Mediterranean.

These data having been obtained by the examination of the several parts of the vertical column at one and the same point, and this point being in the centre nearly of the narrowest part of the Strait, and at the deepest part of the channel, we proceeded to test the *actual movement* of water on the surface and at different depths beneath it.

The rate of *surface*-movement was easily determined. The precise position of the ship having been ascertained in the manner already stated, a small flat basket, presenting no such elevation above the water as would cause it to be influenced in any considerable degree by a moderate wind,* was sent adrift, so as to be freely carried along by the current; it was allowed to float for a determinate time, throughout which it was followed by the ship, and when it was taken up at the expiration of that time, the place of the ship was again ascertained as before. The space between the two points being then determined trigonometrically, the rate of the flow per hour, and its precise direction, could be readily calculated. Thus on the morning of August 15th the float was followed by the ship for fifteen minutes, during which it was found to have moved 4,377 feet in the direction E. by S. ¾ S., or at the rate of 2·88 miles per hour.

For the determination of the *movement of the water at different depths below the surface*, a current-drag had been constructed by Capt. Calver on a plan suggested by his previous experience; which had led him to the conclusion that a submerged basket lined with sail-cloth, which of course fills itself with water, presented a better resisting surface than any vessel of wood or metal. Such a basket being made the basis (so to speak) of the apparatus, its resisting surface was augmented by fixing two pairs of arms at right angles to one another across its upper end, and stretching a piece of sail-cloth between each arm and the side of the basket, which device caused a uniform resisting surface to be presented to the current whatever the manner in which the sails might meet it. To the lower part of this "drag" a couple of sinkers, of 112lb. each, were attached; and the whole apparatus was supported by cords meeting in a ring above it, to which the suspending line was secured.

* It is obvious that the movement of the ship itself would be liable to be considerably affected by even a slight breeze, on account of the large surface of resistance presented by its transverse section (especially by its paddle-boxes) above the water. This would cause its drift to be *more* rapid than the curren, if the direction of the wind should be *with* that of the current, and *less* rapid if the wind should be opposed to it.

This "current-drag" having been transferred to a boat, was lowered down by a couple of men placed in her, to the desired depth; and the boat was then left entirely free to move, being lightened by the return of the men into the ship. The motion of the boat would be the composite result of (1) the action of wind (if any) upon the tranverse section of the part of the boat above the water; (2) the action of the surface-current upon the transverse section of the immersed part of the boat; (3) the action of the upper current upon the suspending line; and (4) the action of the current in which the "drag" is suspended, upon the drag itself. Putting aside the *first* of these agencies, which will be of very little account if (as in the experiment now narrated) the boat be small and the breeze be light, it is obvious that the relative influence of the *second* and *third* to that of the *fourth* will depend upon the proportion between the surfaces presented by the boat, the line, and the "drag" respectively, and the strength of the current acting upon each. The surface given to the "drag" being larger than that of the boat and line taken together, the force acting on the "drag" will dominate, if it hang in an opposing current superior, equal, or even somewhat inferior in rate to that which acts on the boat and line, so that the boat would be carried along by the drag *against* the surface stream at a rate proportioned to the excess. If, again, the rate of the under-current should be greatly inferior to that of the surface, its action upon the "drag" might still be sufficient to neutralise that of the surface-current upon the boat and line, and the boat would then remain *stationary*, or nearly so. A still further reduction in the rate of the op-

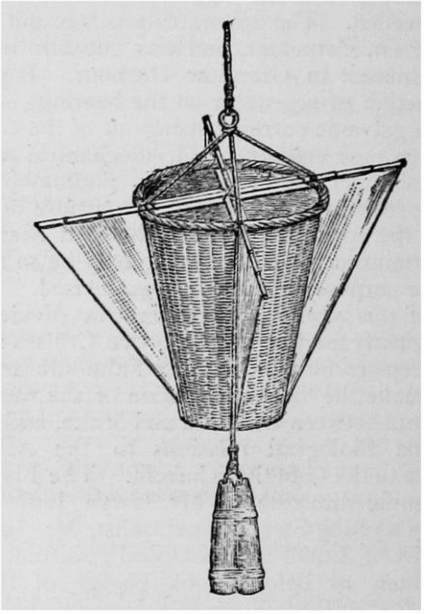


Fig. 1—Cu. rent Drag

posing under-current would make its action upon the "drag *less* powerful than that of the surface-current upon the boat and suspending-line, and the boat would then move *with* the surface-current but at a rate of which the great retardation would indicate an antagonistic force beneath. Supposing again, the water of the stratum in which the "drag" is suspended to be stationary, the action of the surface-current upon the boat and line would be opposed by the resistance offered by the deeper water to the movement of the drag; and the retardation of the movement of the boat would be less, though still considerable.—If, again, the stratum in which the "drag" is suspended should itself be moving in the direction of the surface-current, but at a reduced rate, there will still be a resistance to the movement of the "drag" at the more rapid rate of the surface-current; and this resistance will produce a proportional retardation in the motion of the boat. Finally, if the stratum in which the "drag" is suspended, with the intermediate stratum through which the suspending line passes, move at the same rate with the surface-current, the motion of the boat with the whole suspended apparatus will have the same rate as that of the simple float.

Putting these respective cases conversely, it may be affirmed (1) that if the boat, having the "current-drag" suspended from it, should move *with* the surface-current and *at the same rate*, the stratum in which the "drag" hangs may be presumed to have a motion nearly corresponding with that of the surface-current; (2) that if the rate of movement of the boat *with* the surface-current should be *retarded*, a diminution of the rate of the stratum in which the drag hangs to a degree *exceeding* the retardation of the movement of the boat, may be safely predicated; (3) that when this retardation is so considerable that the boat moves *very slowly* in the direction of the surface-current, it may be inferred that the stratum in which the drag is suspended is either stationary, or has a slow movement in the opposite direction; (4) that if the boat should remain *stationary*, a force must be acting on the "drag" which is equal, and in the *contrary direction*, to that of the upper current upon the boat and suspending line; so that the existence of a counter-current is indicated, having a rate as much *less* than that of the surface-current as the resisting surface presented by the "drag" is greater than that offered by the boat and upper part of the suspending line; (5) that if the boat should move in . direction *opposed* to that of the surface-current, motion is indicated in the stratum in which the "drag" hangs, which will correspond in *direction* with that of the boat, and which will *exceed* it in *rate*, the effect of the "drag" upon the boat being partly neutralised by the antagonistic drift of the surface-current.

Now our first set of experiments with the "current-drag" gave the following results:—

(1.) The surface-movement being first tested in the manner already described, its rate was found to be 2·88 nautical miles per hour, and its direction E. by S. ¾ S. The wind was W. by N., with a force of 4.

(2.) The "drag" having been lowered down to a depth of 100 fathoms, the rate of movement of the boat from which it was suspended was reduced to 1·550 mile per hour, or *rather more than half* the surface-movement. Its direction was E. ½ S. Taking into account the action of the wind and surface-current on the boat, it may be safely affirmed that at 100 fathoms the rate of the current was reduced to *less than one half.*

(3.) The drag having been lowered down to a depth of 250 fathoms, the boat remained nearly stationary, its *rate* of movement being reduced to 0·175 mile per hour, while its direction (S.E. ¼ E.) was slightly altered to the southward, though still easterly. From this we felt ourselves justified in inferring that the 250-fathoms' stratum had a movement in the *reverse direction*, acting on the current-drag with a force almost sufficient to neutralise the action of the upper stratum on the boat and suspending line. And this inference is strengthened by the extraordinary density of the water of this stratum, and is fully justified by the results of the experiments which we made on our return-voyage.

While these experiments were in progress, we had the pleasure of seeing the Channel Fleet, which was expected to meet the Mediterranean Fleet at Gibraltar, come in sight beyond Cape Tarifa; its approach having been indicated, long before even the tops of the masts of the vessels composing it showed themselves above the horizon, by the number of separate puffs of smoke which the experienced eye of our Captain enabled him to distinguish. As soon as all possibility of doubt was removed by the appearance of the masts, Capt. Calver communicated "Fleet in sight" by signals to the Admiral in Gibraltar Harbour; our position being such that *we* could be seen by him, though the Fleet could not. In due time, the massive hulls of the ironclads rose above the horizon, and whilst we continued at our work, all passed us in sailing order, at a distance of not more than a couple of miles, the ill-fated *Captain* being the chief object of interest. A few hours later, the *Monarch*, which had been detained for repair, but whose passage had been made in a shorter time by the free use of her steam-power, came in sight, and passed on in solitary grandeur to join the fleet, now united in Gibraltar Bay.

The whole of our first day having been consumed without our being able to work the "current-drag" in the deepest stratum, we anchored for the night near Point Carnero, with a view to resuming our experiments on the following morning. We then ran out to a spot almost precisely identical with that which had been our starting-point on the previous day; and commenced as before, by testing the rate and direction of the surface-movement. Its rate proved rather slower, being 2·40 miles per hour, instead of 2·88; and its direction was E. by N., instead of E. by S. ¾ S. Both differences seemed to be accounted for by the difference in the force and direction of the wind; which, having been W. by N. with a force of 4 on the previous day, was now W. ½ S. with a force of only 2. The "drag" was then lowered to a depth of 400 fathoms; but our expectation that it would there encounter a westerly (or outward) current sufficiently strong to carry the boat in that direction in spite of the antagonistic movement of the easterly (or inward) surface-current, was not verified on this occasion; for the boat slowly drifted in an E. ⅓ N. direction, its rate being 0·650 mile per hour. Whether this result should be taken to indicate a stationary condition of the deep stratum, or a slight movement in either direction, can scarcely be affirmed with positiveness; but from the indication afforded by the specific gravity of the water taken up from this depth, it would seem probable that the general movement of this stratum was at this time rather *westerly*, or in conformity with that which we attributed to the intermediate stratum, though at a slower rate. It will be shown hereafter that a decisive proof of such a movement was obtained on a subsequent occasion.

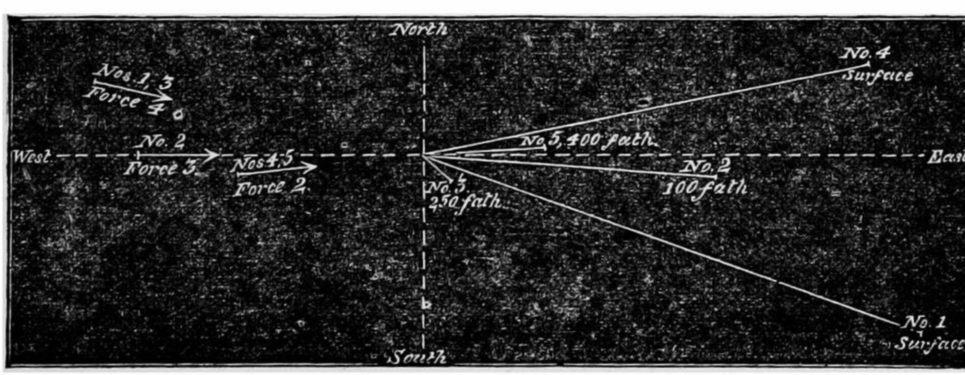


FIG. 2 - Rate (per hour) and Direction of Movement of Surface-Float, and of Current-Drag at different Depths; with Force and Direction of Wind

Thinking it expedient to postpone the further prosecution of this inquiry until our return-voyage,—when we should be able to repeat our experiments, not only at this narrow end of the Strait where it enters the Mediterranean Basin, but also at that shallowest portion to the westward, where the Strait opens out into the Atlantic,—we put steam on before mid-day, and entered the basin of the Mediterranean, directing our course in the first instance to the spot (lat. 36° 0′ N., long. 4° 40′ W.) at which the sample of bottom-water had been obtained by Admiral Smyth, which, when analysed by Dr. Wollaston, was found to possess the extraordinary specific gravity of 11·288, and to yield a percentage of 17·3 of salt.* As we were within sight of both shores, and could distinguish several remarkable mountain-summits which were accurately laid down on our charts, the bearings of these enabled the situation of the ship to be determined with great precision; and Captain Calver undertook to place her within a mile of the point at which Admiral Smyth's observation had been taken. Having reached this, we took our first sounding in the Mediterranean, and awaited the result with no little interest. The depth proved to be 586 fathoms, or 84 fathoms less than that given by Admiral Smyth's sounding; but as the latter was not taken on the improved method now adopted, and as its correctness may have not improbably been affected by the strength of the easterly current which is here very perceptible, the discrepancy can scarcely be considered as of any real account as showing that the two points were otherwise than nearly coincident.† The

* Phil. Trans. for 1829 p. 29; and Admiral Smyth's "Mediterranean," pp. 128—130.

† Thus Admiral Smyth states ("Mediterranean," p. 159) the depth in mid-channel between Gibraltar and Ceuta to be 950 fathoms; whereas it is now known to be but little more than 500 fathoms. "A little farther to the eastward," he says, "there is no bottom with 1,000 fathoms of line up-and-down (*upwards of 1,300 payed out*);" whereas the greatest depth as far east as Malaga Bay is now known not to exceed 750 fathoms. These errors are noticed in no invidious spirit, but merely to prevent their perpetuation. Admiral Smyth doubtless made the very best use of the means at his disposal; but a far more satisfactory method has now entirely superseded that formerly adopted.

specimen of bottom-water brought up by our bottle was found to have a specific gravity of 1·0292, whilst that of the surface-water was 1·0270. The volumetric determination of the chlorine gave 21·419 per 1,000 for the bottom-water, as against 20·290 per 1,000 for the surface-water. A decided excess of salt is thus indicated in the bottom-water, as compared on the one hand with the surface-water of the same spot, and on the other, with the bottom-water of the Atlantic, which had been generally found to show a rather smaller proportion of chlorine than the surface-water. But this excess is extremely small in comparison with that indicated by Dr. Wollaston's analysis. For, assuming his factor of ·134 as representing, when multiplied by the excess of specific gravity above that of distilled water, the total per-centage of salt, that per-centage is only 3·91, instead of being 17·3, as stated by Dr. Wollaston.

This result accorded so closely with that obtained by Dr. Wollaston himself from the analysis of two other samples of bottom-water taken up by Admiral Smyth, the one in long. 1° E. from a depth of 400 fathoms, and the other in long. 4° 30′ E. from a depth of 450 fathoms, as well as with our own determinations of the specific gravities and chlorine per-centages of a great number of samples taken in different parts of the Western basin of the Mediterranean, that we cannot hesitate in regarding it as representing the *ordinary* condition of the bottom-water at this spot. And it seems to us far more probable that the sample furnished by Admiral Smyth to Dr. Wollaston had been concentrated by evaporation in a badly-stopped bottle in the three years during which it had remained in Admiral Smyth's possession, than that any extraordinary discharge of salt from a brine-spring at the bottom (a sort of *Deus ex machinâ* invoked by Admiral Smyth to account for the occurrence) should have given rise, in the spot at which his sounding was taken, to an exceptional condition of which no indication whatever was presented in our own.

The temperature-phenomena presented at this Station proved of singular interest. The *surface*-temperature, 74°·5, was higher than any that had been encountered on the Atlantic side of the Straits, even in a latitude half a degree farther south; and the observations which had been regularly taken every two hours showed that it had increased nearly *ten degrees* as we proceeded eastwards from Station 39, between 10 A.M. and 2 P.M. A part of this increase was doubtless due to the heating effect of the midday sun; but as the temperature of the air had not increased quite *six* degrees during the same time, and as it will be shown hereafter by a comparison of the diurnal averages of the surface temperature of the Mediterranean with those of the Atlantic, that the latter are at least four or five degrees higher than the former, it may be fairly assumed that at least half the increase was due to the passage from the colder Atlantic water of the mid-channel into the warmer water of the Mediterranean basin, the temperature of the latter being even here somewhat reduced by the inflow of the former.—The *bottom*-temperature was found to be here 55°; and this corresponded closely with that which we had met with in the Strait, while it was at least 5° higher than had been obtained at corresponding depths on the Atlantic side. Being desirous of determining the rate of its diminution, we took *serial* soundings at intervals of 10 fathoms down to 50, and then at 100 fathoms; with the following remarkable result:—

	°	°
Surface	74·5	
		diff. 5·2
10 fathoms	69·3	
		diff. 4·3
20 ,,	65·0	
		diff. 2·0
30 ,,	63·0	
		diff. 1·3
40 ,,	61·7	
		diff. 2·0
50 ,,	59·7	
		diff. 4·6
100 ,,	55·1	
		diff. 0·1
586 ,, (bottom)	55·0	

Thus there was a fall of 9°·5 in the first 20 fathoms; of 5°·3 in the next 30 fathoms; of 4°·6 in the next 50 fathoms; whilst from 100 fathoms to the bottom at 586 fathoms there was no further descent.

Whilst we were prosecuting these inquiries, we found ourselves surrounded—the surface of the sea being extremely calm—by great numbers of the beautiful floating *Velellæ*, which are occasional visitors to our own coast, accompanied by the *Porpitæ*, which are more exclusively restricted to warmer seas. With these was a great abundance of a small species of *Firola*, about 0·4 inch in length, the extreme transparence of which enabled every part of its organisation to be readily studied microscopically, its nervous system being specially distinguishable. Of this very interesting *Heteropod*, a full description will be hereafter published by Dr. Carpenter.

The result obtained by our first temperature-sounding in the Mediterranean, was fully borne out by that of the temperature-soundings taken during three subsequent days; which show an extraordinary uniformity of *bottom*-temperature at depths from 162 to 845 fathoms *:—

Station. No.	Depth, in faths.	Surface-temp. °	Bottom-temp. °
41	730	74·5	55·0
42	790	74·0	54·0
43	162	74·7	55·0
44	455	70·0	55·0
45	207	72·7	54·7
46	493	73·5	55·5
47	845	69·5	54·7

It will be observed that the surface temperature varied between 69°·5 and 74°·5; and that whilst the highest temperatures were shown at Stations near the African coast, the lowest presented itself between Cape de Gat and Cartagena. Now the Gibraltar inflow is very sensibly felt at Cape de Gat, where the current usually runs at the rate of a mile an hour; and of the strength of this current we had unpleasant experience. For on the 19th of August, as we were crossing from Station 46 towards the Spanish coast, we encountered a strong N.E. breeze, which, meeting the current, worked up a considerable swell; this prevented us from taking even a temperature-sounding on that day, and gave our ship a peculiar twisting or screwing movement, from which we were glad to escape by the subsidence of the breeze during the following night. During this day the surface-temperature of the sea came down from the average of 72°·2 which it had maintained on the 18th, to 66°·9. Had the weather been calm, we might have attributed this reduction to the colder Gibraltar in current; but as the average temperature of the air also fell from 73°·8 to 69°·8, and as the strong N.E. breeze must have had a cooling effect upon the surface of the sea, we should have deemed it probable that the reduction of surface-temperature was due at least as much to the latter as to the former of these causes, had it not been that a set of serial soundings which we took at Station 47 showed that the reduction extended very far down, as will be apparent on comparing the following results with those previously given:—

	° Fahr.
Surface	69·5
10 fathoms	59·0
20 ,,	57·5
30 ,,	56·5
40 ,,	55·7
50 ,,	55·3
100 ,,	54·7
845 ,, (bottom)	54·7

It will be seen hereafter that the observations made on our return voyage gave more distinct evidence of the cooling influence of the Gibraltar in-current.

At most of these stations we explored the bottom by means of the dredge, with results much less profitable than we had anticipated. Except near the coast, on either side, where the ground was rocky and unequal, the bottom was found everywhere to consist of a tenacious mud, composed of a very fine yellowish sand mixed with a bluish clay—the former predominating in some spots, the latter in others. Large quantities of this mud were laboriously sifted, often without yielding anything save a few fragments of shells, or a small number of *Foraminifera*; and in no instance was it found to contain any considerable number of living animals of any description. Our disappointment at this unexpected paucity of life was not small; and it was destined, as will hereafter appear, to continue through the whole of our dredging exploration of the deeper portions of the Mediterranean basin. The operation of dredging in the shallower portions nearer shore was rendered difficult by the rocky nature of the bottom, on which the dredge continually "fouled;" and after the loss of two more dredges and a considerable quantity of rope, Capt. Calver came to the conclusion that the "tangles" only should be used where the inequality of the soundings indicated danger to the dredge. Now the "tangles," whilst gathering

* This uniformity, as we have since learned, had been previously observed by Captain Spratt, in his Soundings in the Eastern Basin of the Mediterranean; but owing it seems probable to the want of "protection" in his thermometers, he had set the uniform temperature too high, namely 59°. (See his "Travels and Researches in Crete," vol ii. Appendix II.)

Polyzoa, Echinoderms, Crustacea, and the smaller corals, sometimes even better than the dredge, pick] up but few shells; and hence our collection of Mollusca is altogether a scanty one. Nevertheless, many of the types we did obtain were of considerable interest. Thus at Station 45, at a depth of 207 fathoms, we got *Turbo Romettensis*, Seguenza, MS. (Sicilian fossil); *Scalaria plicosa* (Sic. foss.); *Odostomia obliquata*, Ph.; *Philine*, two undescribed species; and an interesting coral (*Dendrophyllia corrugosa*).

SCIENTIFIC SERIALS

THE *Geological Magazine* for January (No. 79) commences with an article by Mr. A. H. Green, on a subject which has lately attracted much attention, namely, the "Geological Bearings of the recent Deep-Sea Soundings." Mr. R. Tate contributes an article on the Invertebrate Fossils of the Lias, which includes a useful table of the known Liassic genera, showing the number of species in each found in Britain and on the Continent, with observations on the distribution of the genera, and descriptions of some new species. Mr. J. Clifton Ward has a paper on the Development of Land, illustrated by references to the geological history of Italy and England. In an article on the Transport of Wastdale Crag Blocks, Mr. Croll ascribes the distribution of those puzzling boulders to the action of land-ice, and adduces evidence in support of the opinion that the great ice-covering of Scotland overlapped the high grounds of the North of England. Mr. Hopkinson characterises a new genus of Graptolites, under the name of *Dicellograpsus*; it includes five species hitherto referred to *Didymograpsus*.

The February number of the same journal opens with a note on the Diamond-fields of South Africa, by Prof. T. Rupert Jones, in which the author endeavours to correlate the scattered and scanty information that we possess on the geology of that region. MM. Brady and Crosskey furnish lists of fossil Ostracoda obtained from the post-tertiary deposits of various localities in Canada and New England, with descriptions of six new species, illustrated by an excellent plate. From the Rev. Osmond Fisher, we have some interesting notes on phenomena connected with denudation observed in the so-called coprolite pits near Haslingfield in Cambridgeshire; and from Mr. Woodward a note on the new British Cystidean, *Placocystites Forbesiamus* of De Koninck, which is identified, on the authority of Mr. Billings, with his *Ateleocystites Huxleyi*. The numbers also contain the usual notices, reviews, and smaller communications.

OF the *Württembergische Naturwissenschaftliche Jahreshefte* we have received the twenty-sixth volume, published in 1870, and including, besides the ordinary general notices of the Proceedings of the Natural History Society of Württemberg, some important papers in various departments of science.—Dr. Samuel Bartsch contributes a notice of the Rotifera observed by him in the neighbourhood of Tübingen, which he preludes with remarks on the anatomy and physiology of those animals. The species are not described, but the genera are characterised, and the author contributes valuable remarks on some of them. He establishes two new families: the Longisetæ, for the genera *Distemma*, *Rattulus*, *Furcularia*, and *Monocerca* of authors, and a new genus, *Monommata*, including *Notommata tigris* and *longiseta*; and the Loricata, for the reception of Ehrenberg's *Euchlanidota* and *Brachionœa*.—Prof. Fraas describes the progress of the geological investigation of Württemburg, and M. C. Deffner, the very curious structure of the Buchberg, an outlier of the Jura, near Bopfingen.—The most important geological paper is a notice of the fauna of Steinheim, relating principally to the remains of Miocene mammals and birds from that locality. This fauna includes, together with numerous well-known species some new forms of great interest, among which may be noticed especially, a species of *Colobus*, described by the author as *C. grandævus*; a new generic type allied to the badgers, and described as *Trochotherium cyamoides*; and species of *Chæropotamus* and *Tapirus*. The fauna is considered by the author to have its nearest existing representative in South-Eastern Asia and the great islands of the Indian Archipelago, and he regards the deposit as following in order of time, the "Langhian stage" of Carl Mayer, which includes the deposits of Weissenau, Oppenheim, Radoboj, and others in Germany; and of Arquato, Superga, Malta, and others in the South of Europe. This valuable memoir is illustrated with six plates.—Prof. C. W. Baur reports on recent geodetic surveys made in Württemberg, for the purpose of a European measurement of a degree.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 9.—"On the Problem of the In-and-Circumscribed Triangle," by A. Cayley, F.R.S.—The problem of the in-and-circumscribed triangle is a particular case of that of the in-and-circumscribed polygon; the last-mentioned problem may be thus stated—to find a polygon such that the angles are situate in and the sides touch a given curve or curves. And we may in the first instance inquire as to the number of such polygons. In the case where the curves containing the angles and touched by the sides respectively are all of them distinct curves, the number of polygons is obtained very easily and has a simple expression; it is equal to twice the product of the *orders* of the curves containing the several angles respectively into the product of the *classes* of the curves touched by the several sides respectively, or say it is equal to twice the product of the orders of the angle-curves into the product of the classes of the side-curves. But when several of the curves become one and the same curve, and in particular when the angles are all of them situate in and the sides all touch one and the same curve, it is a much more difficult problem to find the number of polygons. The solution of this problem when the polygon is a triangle, and for all the different relations of identity between the different curves, is the object of the present memoir, which is accordingly entitled "On the Problem of the In-and-Circumscribed Triangles;" the methods and principles are, however, applicable to the case of a polygon of any number of sides, the method chiefly made use of being that furnished by the theory of correspondence.

Anthropological Institute of Great Britain and Ireland, February 14.—Sir John Lubbock, Bart., M.P., F.R.S., President, in the chair. The President stated that this was the first meeting of the "Anthropological Institute of Great Britain and Ireland," since the union of the late Ethnological and Anthropological Societies under that name, and having vacated the chair in favour of Professor Huxley, proceeded to read a paper, "On the Development of Relationships." After some preliminary observations on the character of the family among the lower races of men, and the preponderance of the tribal tie, Sir John proceeded to discuss the conclusions drawn by Mr. Morgan from the valuable schedules of relationships collected by him and published by the Smithsonian Institution, especially with reference to his theory that the similarity between the Mohawk and Tamil systems indicated any Ethnological affinity between those races, a conclusion which Sir John was unable to accept. He then proceeded to show how in his opinion that similarity had arisen, and traced up the gradual development of correct ideas on the subject of relationships from the system of the Sandwich Islanders, which is the lowest on record, step by step to that of the Karens; showing that in each system there are points which can only be explained on the hypothesis of its development from a still ruder condition. He then compared these actually existing systems with those which would be produced by a retrogression of social customs, and showed that the systems of the lower races all indicate progress, and there are no instances of the existence of such a system as would arise in the case of degradation. He also laid stress on the fact that the social system is invariably in advance of the nomenclature of relationships, another evidence of progress as opposed to degradation. He showed that even in some European nations we have traces of earlier lower condition, and that therefore in the systems of relationships we have an interesting proof of the social progress of man, and the gradual development of family ties.—Mr. Hodder M. Westropp exhibited a Worked Flint Implement, said to have been found in a barrow, Ashby Down, near Ventor, Isle of Wight.—The Chairman announced that the meetings of the Institute would, during the remainder of the Session, be held on Mondays, commencing on Monday, 6th March.

Chemical Society, February 16.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected fellows:—W. D. Herman, W. W. Houlder, G. Lockyer, jun., W. J. Lockyer, J. E. Mayer, R. Meldola, M. M. P. Muir, W. J. Reynolds, W. Smith, T. E. Thorpe. The following papers were read:—"On the production of wood spirit," by E. T. Chapman. The author began by remarking that the difficulties of obtaining correct information regarding this production are very great. The manufacturers are too jealous to disclose their "secrets." The woods chiefly employed in wood distilling are oak, beech, birch, thorn, crab or apple, hazel, alder, and ash. Great preference is given to holly and yew, whilst poplar, elm, and the whole of the coniferous order are avoided. The ovens or retorts are of various forms, and either of cast or of wrought iron, protected outside by brickwork against the dire effects of the fire. The condensers of iron and copper are constructed so as to permit the ready passage of very large volumes of gas, and to admit frequent cleaning. The temperature at which the distillation is conducted influences the amount of the products; as a rule, greater heat yields more of the so-called naphtha, and a lower temperature more acetic acid. The liquid products of distillation which form two layers, are differently treated according to the object the manufacturer has in view. After having given a lengthy description of the various products obtained in wood distilling, the author went on to say that some easier and more exact methods of estimating the commercial values of the various products ought to be introduced.—"On the effects of pressure on the absorption of gases by charcoal," by John Hunter. Very numerous experiments lead to the observations—first, that the amount of absorption increases with the pressure to which the gas is exposed; and secondly, the same change of pressure produces about the same amount of increase in the quantity of each gas absorbed.—"On the solubility of the phosphates of bone-ash in water holding carbonic acid," by E. Warington.

CAMBRIDGE

Philosophical Society, February 13.—"On the Great Trigonometrical Survey of India," by Colonel J. T. Walker, R.E. The author carefully described the process of carrying out the survey and the instruments in use, pointing out the various difficulties which were experienced, and the mode in which they were overcome. He also gave an account of the earlier efforts in this field and pointed out the importance of careful survey, and the mode by which accuracy in geodelic investigations was secured. In conclusion he discussed the mathematical difficulties which presented themselves, and a theorem which had been found useful in the reduction of observations.

NORWICH

Naturalists' Society, January 3.—The chairman read a paper by Mr. W. M. Crowfoot, on Spontaneous Generation. After distinguishing between the *origin* of life and the *nature* of life, two very distinct subjects, which have at all times been more or less confounded with one another, Mr. Crowfoot proceeded to review briefly the history of the theories concerning the nature of life as propounded by Hippocrates, Paracelsus, &c.; he then gave a condensed history of the views concerning the origin of life, and of the experiments of Redi, Needham, Pasteur, Huxley, and Bastian, and a *résumé* of the discussion which has been recently carried on in the columns of NATURE and elsewhere; concluding with some practical remarks on the nature of epidemic diseases, and the important results which may arise from such discussions. The thanks of the meeting were voted to Mr. Crowfoot for his very interesting paper, which was followed by an animated discussion.

DUBLIN

Royal Geological Society, January 11.—Rev. Maxwell Close in the chair. Edward Hull, F.R.S., read a paper on the Geological Age of the Ballycastle Coalfield, and its relations to the Carboniferous rocks of the west of Scotland.—Mr. John Leech read a paper on the moving bog of Castlerea, Co. Roscommon.

February 8.—Dr. Reynolds in the chair. The annual report of council, and statement of accounts for the year 1870 were submitted, and the following were elected as officers and council for 1871:—President, the Earl of Enniskillen, F.R.S.; Vice-presidents, Colonel Meadows Taylor, J. Emerson Reynolds, Sir Robert Kane, F.R.S., Rev. H. Lloyd, Provost T.C.D., F.R.S.; Sir Richard Griffith, Bart., LL.D.; Treasurers, William Andrews, and Samuel Downing, LL.D.; Secretaries, Rev. S. Haughton, M.D., F.R.S., and Alexander Macalister, M.D.; Council, Alphonse Gages, B. B. Stoney, W. Frazer, George Dixon, Alexander Carte, M.D., W. H. S. Westropp, C. R. C. Tichborne, Rev. Maxwell Close, Francis M. Jennings, Ramsay H. Traquair, M.D., R. Callwell, John Barker, M.D., John Ball Greene, Edward Hull, F.R.S., William H. Baily.—Mr. J. Scott Moore read a paper on a moulded piece of quartz, and exhibited a remarkable specimen of dendritic markings in granite.—A paper was read from Mr. G. H. Kinahan on foliation.

PERTHSHIRE

Society of Natural Science, February 2.—Dr. Buchanan White, president, in the chair. Mr. C. Fleckstein read a paper upon the Zoology and Botany of the Ancients. The paper, which was of general interest, was confined almost entirely to a consideration of the knowledge of natural history possessed by those nations termed *par excellence* the *ancients*, viz., the Greeks and Romans, and related chiefly to the researches of Aristotle and Pliny. Mr. W. Herd read a paper upon the Lepidoptera of Moncrieffe Hill and its neighbourhood. It was illustrated by specimens of the insects mentioned, and contained the results of Mr. Herd's own observations of the habits of the less common species found in the district selected.

DIARY

THURSDAY, FEBRUARY 23.

ROYAL SOCIETY, at 8.30.—On the Mutual Relations of the Apex-Cardograph and the Radial Sphygmograph Trace: A. H. Garrod.—On the Thermo Electric Action of Metals and Liquid: G. Gore, F.R.S.
SOCIETY OF ANTIQUARIES, at 8.30.—On the Topography of Jerusalem, with special reference to the results obtained by the Palestine Fund Committee: Thomas Lewin, M.A., F.S.A. (Second paper.)
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.
LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff, M.A., F.C.S.

FRIDAY, FEBRUARY 24.

ROYAL INSTITUTION, at 9.—On Rumford's Scientific Discoveries: W. Mattieu Williams.
QUEKETT MICROSCOPICAL CLUB, at 8.
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, FEBRUARY 25.

ROYAL INSTITUTION, at 3.—Socrates: Prof. Jowett.

MONDAY, FEBRUARY 27.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.
LONDON INSTITUTION, at 4.—On the first Principles of Biology: Prof. Huxley. (Educational Course.)
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, FEBRUARY 28.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.

WEDNESDAY, MARCH 1.

ROYAL MICROSCOPICAL SOCIETY, at 8.—Notes on the Microscopical Examination of Waters for Domestic Use: J. Bell.
SOCIETY OF ARTS, at 8.—On the Patent Laws and their Administration, with a view to the Adoption of Practical Amendments: A. V. Newton.
ROYAL SOCIETY OF LITERATURE, at 8.30.
LONDON INSTITUTION, at 4.—On the first Principles of Biology: Prof. Huxley. (Educational Course.)
ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, MARCH 2.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
LINNEAN SOCIETY, at 8.—On the Tamil names of Plants: Rev. S. Mateer. —Contributions towards a knowledge of the *Curculionidæ*: H. P. Pascoe.
CHEMICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.
LONDON INSTITUTION, 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers.

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ERRATUM.—Page 266, second column, lines 15, 16, for "three times" read "g times."