

of structure consisting of small parts of canals filled with the dark colouring matter of the limestone," and in only "a few rare instances" are detected "with a higher power in the margin of some of the septa traces of the fine tubulation characteristic of the chamber wall of Eozoön." 3rd. It is almost an isolated example, and the measure of the metamorphism of its matrix together with its character—organic or otherwise—(from its generally doubtful nature as shown by Dr. Dawson's own description) could only be ascertained and settled by independent inquiry.

I may also here observe that other cases of "chambers and canals stated to be injected with calcite appear to me to be of a no more reliable character. If, indeed, we accept the Tudor specimen and Madoe fragments as Eozoön, why refuse this term to the Skye specimens which apparently possess the true features (chamber casts and nummuline layer) in a much more marked manner? As regards imitative forms, Dr. Dawson and myself are in agreement, excepting that I must contend, from all the facts we are acquainted with, that *all* Eozoönal forms are imitative, and not merely those that the exigencies of the discussion demand looked at from the organic standpoint.

Respect for your space prevents me going into further details, but I may be permitted to suggest that the truth of the matter in no way hinges upon the possibility of comprehending the constructive pseudomorphic theories advanced by King and Rowney. In what way though, excepting by pseudomorphism, I would ask, does Dr. Dawson account for the presence of the *imitative forms* which he thinks have confused other observers? And if we believe pseudomorphism to have originated them, why is it so trying to our faith to consider Eozoön Canadense to have been formed in the same manner? The fact is, chemical geology and the replacement and alteration of minerals—occurring as they do in the forms of other minerals—have been little studied by palæontologists, or probably Eozoön might have remained "unconstructed" to this day. It is well known that not only do minerals assume by replacement the crystalline forms proper to the mineral replaced, but also amorphous shapes filled with one mineral may be replaced by another without in any way destroying the original form.

There can be no doubt that a little knowledge of this kind would have infused the necessary caution, and have prevented anyone accepting as a fossil that which required the invention of a method of chemical deposition (excepting in these pages) hitherto unknown in nature. Serpentine marble, as we may ascertain from all sources, is the typical Eozoönal rock, and, though the minerals filling the so-called chambers and canals may be Loganite or pyroxene, in addition to the serpentine, they are intimately related in a pseudomorphic point of view. Loganite and serpentine, as I have before stated, are both products of alteration. The organic hypothesis demands that we should consider the infilling to be, as contended for by Dr. T. S. Hunt, the *same mineral originally supposed to be deposited therein*. If this be so, then what becomes of the meaning of metamorphism? This to me is a trial of faith greater even than the acceptance of King and Rowney's "constructive criticisms."

I await with interest the publication of the papers sent to the Royal Irish Academy by Dr. Dawson and his colleague, Dr. Hunt, and trust they may contain new matter for consideration, as my only object has been to elicit further evidence, if there be any, in favour of the organic hypothesis.

Feb. 17

T. MELLARD READE

#### Ocean Currents

In reference to Mr. Laughton's letter in NATURE of the 23rd of February, I must admit that the question of the movement of barometric depressions was not introduced into my first letter, for the reason that I did not anticipate the objection of a state of equilibrium which he raised, since the average differences of pressure only were dealt with; but I do not see that it necessitates a change of ground to show how this difficulty is met by the variations which occur in the region of lower average pressure, and how these changes themselves, taking place in a certain line of movement, might affect the surface currents of the ocean; and I am not disposed to accept the sweeping rejection of the whole power of differences in the atmospheric pressure, permanent or moving, which is contained in the last paragraph of the letter referred to, until more extended observations shall show what directions the great movements of changing pressure take in passing over the parts of the ocean which lie outside of the trade wind regions.

The action of a barometric depression in moving over the sea differs entirely from that of the winds in this, that by the former the level of a large area of the ocean surface may be raised and carried along with the depression round which the winds blow, whilst by the force of the latter the waters can only be drifted at the same level before the wind.

I have been particularly careful to suggest difference of atmospheric pressure only as a supplementary power in the production of ocean streams, not as a chief one, and it was indeed the partiality of the various theories of the causes of currents which led me to open the subject at all.

That the trade winds have a very large share in originating the Gulf Stream is undoubted, but that they can account for the whole of the phenomena of ocean currents, as Mr. Laughton maintains, appears more than doubtful.

The existence of the under outflowing current of the Mediterranean, corresponding to the inflowing surface stream, has been abundantly proved by the recent Government expedition under Dr. Carpenter. If this current be due to the action of the winds alone we should expect to find the direction of the upper and under streams reversed with a change of wind to opposite points east or west in the Strait. But the observations on the direction of the winds for six years at Gibraltar show that westerly winds (from N.W., W. and S.W.) prevail there for 198 days in the year, and easterly winds (N.E., E., and S.E.) for 144 days; further it happens that in the months of July, August, and September, in one of which the observations on the outflowing under-current were made, east winds prevail in the ratio of two days to one day of west wind. Experiments similar to that used to determine the presence of an under-current in the Strait of Gibraltar, have been made in the open ocean, and Maury (at p. 206 of the 9th edition of his work) quotes an instance of an apparatus constructed of a block of wood, sunk by weights to 500 fathoms, and attached by a line to a small floating barrel, having moved off "against wind and sea, and surface current." The members of the late German Arctic expeditions have observed that where the warmer blue waters moving from south-westward meet the impure waters of the Spitzbergen and east Greenland current, there is a definite line of demarcation which would indicate that the Atlantic water here dips down beneath the specifically lighter water of the ice-bearing current, a conclusion which is supported by the increase of temperature with the depth beyond this point.

Such under-currents can in no way be primarily caused by the action of the winds, and if difference of temperature and density must be called in to account for them, it must be admitted that these causes have to do with the upper streams also.

I would take this opportunity to correct a statement made in my second letter, of a probable movement of a barometric depression across the British Isles at the rate of ninety miles an hour. The depression there referred to appears to have had an oblong form, the longer axis moving nearly parallel to the length of the British Isles from N. to S., so that the record of its passage took place at Valentia and at Aberdeen within a short interval of time, thus giving an apparently great rate of travelling. But I have the authority of the Secretary of the Meteorological Office for the facts given beneath, which prove that a rate of depression movement of upwards of seventy English miles an hour may take place. On the 16th of December, 1870, a minimum reading of the barometer was registered at Valentia at 2<sup>h</sup> 45<sup>m</sup> P.M.; at Kew, at 9<sup>h</sup> 30<sup>m</sup> P.M.; at Yarmouth, at 10<sup>h</sup> P.M., where the mercury remained at the same level for four hours. The interval of time between the registrations at Valentia and Yarmouth is 7<sup>h</sup> 15<sup>m</sup>; the distance between these places is 520 miles. It seems probable also that the centre of the depression moved directly from Cork to Yarmouth, for the wind records prove that it passed north of Falmouth, and south of Holyhead and of Valentia.

KEITH JOHNSTON, JUN.

#### Perpetual Motion

PROBABLY your sense of justice will induce you to insert some very brief remarks on your review of my article in the *Quarterly Journal of Science*. The tone of the review is a penalty which all who venture to impugn commonly accepted theories must be prepared to submit to. Heresy in science meets with as little mercy as heresy in theology. I confess that in one sense of the word I am consciously a perpetual-motionist, but not in the sense of believing that any merely mechanical contrivance can produce

perpetual motion. That there are forces in nature which can and do produce it, is a matter of daily, yearly, and secular experience. If I am a perpetual-motionist in this sense, I am in good company. You will find that Sir W. Thomson, in the *Philosophical Magazine* for February 1854, described a machine by which a steam-engine or water-wheel could produce thirty-five times the heat commonly considered as equivalent to the force used; or the corresponding amount of cold. At that time, then, two years after his paper read to the British Association (to which you refer me), he certainly did not hold such an opinion with regard to the mechanical equivalent of heat as to exclude the possibility of such an engine.

The final judgment of the question I confidently leave to time and facts. When any of the "grand founders of a rapidly progressive science" can spare time from their investigations to refute my fallacies, I shall gladly retract them. H. HIGHTON

### The Spectrum of the Aurora

IN the sketch appended to my letter on this subject in last week's *NATURE*, I notice that the engraver has made the line at 4.1 much too sharp and definite in both spectra. It really shades off rapidly to the more refrangible side, at least in the spectrum of the vacuum tube, and possibly also in that of the aurora. Though much the brightest line in the auroral spectrum, it is not the most conspicuous in that of the tube, but the relative brightness of lines frequently varies much at different temperatures. The band at 8 in the auroral spectrum is also represented too narrow. Those who have practical experience with the spectroscope will appreciate the great difficulty of representing faint spectra correctly in a woodcut.

HENRY R. PROCTER

### Science Teaching for the People

THE subject of Science Teaching in our elementary schools having been ably brought forward by Mr. Henry Ullyett in a recent number of your Journal, and the scientific instruction under the Science and Art Department, South Kensington, having been at various times the subject of consideration in it, I venture to ask for a short space in your columns in order to submit the following proposition for the consideration of your readers, many of whom, have, probably, special opportunities of coming to a correct judgment on the point. The question I wish solved is this: Is the spread of scientific education, under the auspices of the Science and Art Department, likely to be best promoted by the whole of the Department's assistance to any one town being dispensed by a single committee, by whom a central school shall be provided, of which all other schools established, or that may be established, in various districts of the town, shall be considered only as branches, and be subject to the control of the central committee, on whose books the names of all students would be borne, and through the one secretary of which all the returns and other communications to and from the Department would have to pass?

Is it not better that the schools established in various districts of a town, say in connection with each elementary school, should each have their separate organisation of committee and secretary, at least in so far as the teachers connected with each are different? In this town, in common with a great many others, the latter plan has been the rule, but a suggestion has now been made that all these committees should be amalgamated, together with those of the art-classes also, the plea being that it might lead to the erection of a central building for the purposes of an art and science school.

Now, however necessary for art it is that there should be a central building for the provision of higher instruction than can be given in the night classes, I cannot see that there is anything in the study of science that demands greater facilities than can easily be provided by any district school, and the possession of which is indeed required by the Science Directory before any science school receives the approval of the Department.

Why, then, the science committees should be asked to unite in this town alone, by which a most dangerous precedent would be established, I cannot understand, for I do not believe that such a course would be beneficial to the town at large, while it would be very prejudicial to the interests of the existing district schools, and of any persons wishing to commence teaching in future.

Perhaps some of your readers will favour us with their views on the question I have stated, which I venture to think involves a principle of very general interest to the science schools of the kingdom.

A MEMBER OF A SCIENCE COMMITTEE

### A Rare Moth

It may be interesting to know that the rare and beautiful moth, *Psephenophila Galii*, appeared somewhat plentifully in the neighbourhood of Derby during the past summer. I have in my possession a fine male and female which were captured at Long Eaton, some few miles from Nottingham, whilst flying in company over a bed of geraniums. Is not this the first recorded instance of their appearance in this locality? *Zenzera Esculi* also appeared in unusual abundance in the same garden, upwards of fifty specimens being taken by one person. W. H. G.

### Measurement of Mass

WILL you allow me a few words in explanation of a sentence in my last letter, which has strangely been misunderstood by Prof. Everett. In defending the system which makes the standard pound a unit of force on the ground that although not the most philosophical, it simplifies the conception of mass which is always difficult for beginners, I said, "The assumption of a hypothetical force of gravity not dependent on latitude, seems to stand on the same footing as the employment of a mean solar day," meaning, of course, that just as we assume (for convenience) that a solar day is the same length at whatever period of the year we take it, so we may assume a mean force of gravity (the actual force of gravity in latitude 45°) which is the same all over the earth. Such an assumption will enable us to explain the unfamiliar notion of mass by the familiar one of weight, and when it does become necessary to take into account the variation in the force of gravity at different points of the earth's surface, the correction is easily made.

Prof. Everett seems to think that I suppose that the average length of the apparent solar day is not the same at all places on the earth. Will you allow me to quote the following passage from the original in support of my first assertion, that Prof. Everett's tacit assumption that everybody knows what mass is, is less likely to lead to clear ideas than the explanation given by Deschanel. He says—"Un corps a une masse plus grande qu'un autre lorsque la même force lui imprime une vitesse plus petite, et réciproquement, . . . si nous considérons en particulier le poids d'un corps, on aura, entre ce poids, la masse et l'accélération de la pesanteur, la relation fondamentale,

$$P = Mg$$

Cette formule nous montre que dans le même lieu le poids est proportionnel à la masse, parce que  $g$  a la même valeur pour tous les corps. Il n'en est pas de même quand on passe d'un lieu à un autre; mais comme après toutes les variations sont extrêmement petites, en réalité la masse et le poids sont deux quantités toujours sensiblement dans le même rapport. Toutefois il faut se rappeler que ces deux expressions correspondent à des notions distinctes, et, abstraction faite de toute évaluation numérique, la masse d'un corps est quelque chose qui lui est propre et qui est indépendante du poids. La pesanteur n'existerait pas qu'il n'en serait pas moins vrai qu'une sphère de plomb a une masse plus grande qu'une sphère de liège de même diamètre. Nous reconnaissons ce fait ordinairement à ce que la poids de la première sphère est plus grande que celui de la seconde; mais à défaut de la pesanteur, l'emploi de toute autre force pourrait nous conduire au même résultat."

W. M. W.

### PHOTOGRAPHS OF THE ECLIPSE

PERMIT me to call your attention to the position of the woodcut illustrating my remarks on the Eclipse Photographs. The south point is where the north should be. As what I have now to say refers to the picture I shall feel obliged if you will permit its reinsertion in its true position.\* With reference to the power of the light of the Corona, I used the word *actinic*, not *active* as printed.

The readers of *NATURE* may perhaps be glad of the opportunity to compare for themselves tracings of the American and of my own photographs, which I now give in outline in illustration of remarks in your second article

\* This vexing mistake was due to a blunder of the printer in reversing the block after it had been placed on the machine. Its re-insertion this week will rectify the mistake.—Ed.