cially by some indication of such piles of raw material for discussion as have been accumulated by the Fishery Departments of many governments. A compilation of this sort depends for its value on its completeness, as the reason for adopting one theory or classification rather than another must be the outcome of an attempt to weigh evidence. After a brief discussion of the conditions of life, there follow sections on the life-districts of the ocean, Hæckel's classification of marine organisms, a concise discussion of the influence of light, temperature, salinity, tides, waves, and currents on marine life, and a short statement of the flora and fauna of the littoral, shallow water, estuarine, open sea, deep sea, and oceanic archipelago divisions, concluding with a few pages on the geological changes of ocean basins.

It would be premature to express an opinion of Prof. Walther's contemplated work. The sketch he gives of its plan stimulates interest and curiosity, and we can heartily congratulate him on the orderly way in which he has collected and laid down the building-material, while we wish him success in his labours.

LETTERS TO THE EDITOR.

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Correlation of Solar and Magnetic Phenomena.

THE opinion of Mr. Whipple, quoted at p. 2 of NATURE for November 2, to the effect that the solar outburst observed by Messrs. Carrington and Hodgson on September 1, 1859, was not the cause of the coincident magnetic perturbations, corresponds to my own conclusion in regard to the matter based upon evidence of an altogether different character. There was a recurrence of strong magnetic perturbations and auroras twenty-seven days later than the great magnetic storms of August 28 and September 1, 1859, thus following the general rule which is found to apply in such cases, there being a well-marked periodicity of such outbreaks at this precise interval corresponding to the time of a synodic rotation of the sun. Such recurrence manifestly could not exist if outbreaks upon the sun were able to produce terrestrial magnetic effects indifferently in all locations. In order that there may be recurrence at the synodic period the magnetic effects must proceed from the sun at some particular angle exclusively, and fortuitous outbursts elsewhere, no matter how violent, must fail to have any perceptible effect. In the estimation of the writer there is no point more important in connection with solar physics than the determination of this period and this angle with the greatest accuracy possible.

M. A. VEEDER,

Lyons, N.Y., December 26, 1893.

MY letter in NATURE (vol. xlix. page 30), amongst other interesting communications, has brought one from Mr. Lawrence (vol. xlix. page 101) and the accompanying letter from Dr. Veeder. Mr. Lawrence's graphic account well describes the circumstances attending the manifestation of 1882 (November 17). The magnetic disturbance which broke out at 10 a.m. on that day set us all on the look-out for aurora in the evening. Neither were we disappointed; the display was remarkable. But the question in this case, as with the Carrington-Hodgson and Young instances, is still whether the solar and magnetic phenomena were directly related or simply coincident. This cannot be said to be determined, and nothing less than proof, in so important a matter, will serve. Better to advance surely if slowly towards truth, rather than accept too hastily evidence that is incomplete. We must remember that on the occasion of the solar disturbance seen by Trouvelot, the magnets were especially quiet, not only at the time but also before and after. But any explanation of these phenomena must include all cases. The position of things, as stated in my first letter, referred to above, still I consider holds, qualified only by the circumstance that instead of one presumed case of direct relation, three are now adduced, with a fourth case (the Trouvelot observation), which unquestionably was not accompanied by magnetic disturbance.

If we further consider that, since the year 1859, when attention became distinctly drawn to this question, there has occurred magnetic movement, equal to and in very many cases far exceeding that accompanying the Carrington-Hodgson observation, on some 400 different days, we see on how slight a foundation the presumption for direct relation, that is of a nature more or less instantaneous in action, exists, although the general relation between the two classes of phenomena remains undoubted.

Dr. Veeder, from his own point of view, supports the contention that the 1859 solar outburst cannot be taken as causing the accompanying moderate magnetic movement; indeed there was far greater movement some three days previously, and again early on the morning of the following day; but in regard to his affirmation that there exists a well-marked periodicity in magnetic outbursts corresponding to the period of the sun's rotation, whilst this in a limited sense may be in some degree true, I cannot say that my personal acquaintance with magnetic records during very many years enables me at present to accept such conclusion as a general one, or indeed what as a consequence follows, that anything really depends on the position in rotation which the sun occupies relatively to the earth.

The whole subject is, however, exceedingly interesting, and various considerations arise. One bearing on the present question may be mentioned. Great terrestrial magnetic disturbances are evidently in character cosmical, produced, it would seem, or stimulated, by some external cause. For it has been shown (Proc. Roy. Soc. vol. lii. p. 191) that, on occasions of unusually sudden magnetic disturbance, the commencement of disturbance, at places so widely separated on the earth's surface as Greenwich, Pawlowsk, and Bombay, is simultaneous within a much smaller limit of time than had before been supposed. Such sudden simultaneous action would thus appear to indicate an impulse, solar or otherwise, from without, but whether one distinctly solar, or in what other way produced, is a question yet to be determined.

WILLIAM ELLIS.

Greenwich, January 6.

The Mendip Earthquake of December 30-31, 1893.

I SUBMIT the following notes for the use of any of your readers who may be collecting information on the subject:—

So far as I can judge, from statements obtained directly from inhabitants of the locality, and from the experiences of various persons, recorded in the *Shepton Mallet Journal* of January 5, the movements in this earthquake occurred chiefly along the south flank of the Mendip Hills between Shepton Mallet on the east-south-east, and Draycott (near Cheddar) on the west-north-west. The shock extended as far southwards as Evercreech and West Pennard; it reached as high up as Priddy, which is near the axis of the hills, and was also noticed at Chewton, several miles distant on the northern flank.

The force of the shocks appears to have been very irregularly distributed, in some houses the movements being quite alarming, while in others not far distant they were trifling though unmistakable. Some persons failed to hear the sound, which was very evident to others. Persons out of doors heard the sound most distinctly, even when they felt no shock.

A lady at Shepton Mallet, who had previously experienced an earthquake in New Zealand, recognised at once what was occurring, but was not in any way alarmed. She says that her bed began suddenly to shake or rock, and as suddenly ceased. She was also conscious of a movement of the whole house, and in the sharper shock heard the furniture rattle; but she did not observe any rumbling. Another lady in the same house noticed particularly the "funny unusual sort of noise." Again, in the same house a man describes the movement as resembling a wave moving from east to west. A school master and mistress got up under the impression that the water-heating apparatus had burst. At West Compton a lady in a farmhouse thought from the sound and movement "that some one was about the house, or that a barrel of cider had burst." At Westbury-below-Wells the shock was sharp enough to cause alarm.

The policeman on duty at Shepton Mallet very naturally referred the sound to the direction of the Midland Railway, which runs high on the hills in such a way that the rumble of its trains is heard at a great distance. It is well known that we have but little certainty in localising sounds, especially if of indefinite character, and that we usually refer them to positions whence we expect them.

The area in which the earth-movements seem to have been

most felt, corresponds with a series of outlying masses of carboniferous limestone, which are separate from the main mass of carboniferous limestone of the Mendip anticlinal. Whether these are parts of another anticlinal, or owe their position to faulting, I do not know. Westward of Wells these outliers form little knolls, as at Draycott and Westbury. Further east, in the area between Wells, Shepton Mallet, and West Compton, they form a group of prominent hills, whose valleys are occupied by later formations. If such outliers exist east of Shepton Mallet, they are deeply hidden by the oolitic strata.

Evercreech and West Pennard lie off this carboniferous limestone, but it extends beneath the valley in which these villages lie. Priddy is on the main anticlinal of the Priddy Mendip hills, and Chewton is separated from all the foregoing by the exposure of Old Red Sandstone. It would be interesting to know how far the Old Red Sandstone shared the movements; but information is likely to be scanty, as the sandstone forms a bleak and sparsely inhabited region.

F. J. ALLEN.

Mason College, Birmingham, January 6.

Quaternionic Innovations.

THAT Prof. Tait should not be able to do justice to those who prefer to treat vectors as vectors, and quaternions as quaternions, instead of commingling their diverse natures, with the result, in the latter case, of confusion of physical ideas (and geometrical also, for of course geometry is itself ultimately a physical science, having an experiential foundation), is naturally to be expected. He does not know their ways, either of thinking or of working, as is abundantly evident in all that he has written adversely to Prof. Willard Gibbs and others. It is, however, a little strange, in view of Prof. Tait's often expressed conservatism regarding Quaternions, that he should tolerate *any* innovations therein, such as Mr. MacAulay has introduced. The latter may perhaps take this as a compliment to his analytical powers, which compel the former's admiration, and toleration of his departures from quaternionic usage. For myself, I welcome any quaternionic innovations that may (ultimately) tend in the direction of the standpoint assumed by Prof. Gibbs and others, and foresaw some two years since (when a very bulky manuscript came to me for my opinion) that there would be some quaternionic upstirring.

Prof. Gibbs has already pointed out how the development of Quaternions has involved first the elimination of the imaginary, and next the gradual elimination of the quaternion! Now there is a capital illustration of this innate tendency in Prof. Tait's review (*NATURE*, December 28, 1893), where, on p. 194, he explains by an example the meaning of a startling innovation of Mr. MacAulay's. Put it, however, in vectorial form, and let us see what it comes to then. Take the case of a stress and the force to correspond (which is a little easier than Prof. Tait's example, though not essentially different). Let ϕ be a stress operator (pure, for simplicity), so that $\phi\mathbf{N}$, or $\mathbf{N}\phi$, is the stress per unit area on the $\mathbf{N}$ plane, $\mathbf{N}$ being any unit vector. Now we know, by consideration of the stresses acting upon the faces of a unit cube, that the $\mathbf{N}$ component of the force $\mathbf{F}$ per unit volume is the divergence of the stress vector for the $\mathbf{N}$ planes. That is,

$$\mathbf{F}\mathbf{N} = \nabla\phi\mathbf{N}, \quad (1)$$

for any direction of $\mathbf{N}$. I employ my usual notation for the benefit of readers (now becoming numerous) who, though they cannot follow the obscure quaternionic processes, can understand the plainer ones of pure vector algebra. Now, may we remove the vector $\mathbf{N}$ (which is any one of an infinite number of vectors) and write

$$\mathbf{F} = \nabla\phi \text{ or } = \phi\nabla \quad (2)$$

simply, as the complete expression for the force? Certainly we may. For, in full, we have

$$\nabla = i\nabla_1 + j\nabla_2 + k\nabla_3, \quad (3)$$

$$\phi = \phi_1.i + \phi_2.j + \phi_3.k \text{ or } = i.\phi_1 + j.\phi_2 + k.\phi_3, \quad (4)$$

where ∇_1 , &c. are the scalar components of the vector ∇ (not a quaternion, of course) and ϕ_1 , &c. are the vector stresses on the planes of i , &c., so that $\phi_1 = \phi i$, &c. Direct multiplication gives at once

$$\nabla\phi = \nabla_1\phi_1 + \nabla_2\phi_2 + \nabla_3\phi_3, \quad (5)$$

which is $\mathbf{F}$. We may also write it $\phi\nabla$, because ϕ is pure.

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On the other hand, when ϕ is rotational, let its conjugate be ϕ' , then instead of (1) we have

$$\mathbf{F}\mathbf{N} = \nabla\phi'\mathbf{N}, \quad (6)$$

and therefore

$$\mathbf{F} = \nabla\phi' = \phi\nabla. \quad (7)$$

Here if ϕ is given by the first expansion in (4), ϕ' is given by the second.

Now there are several things that deserve to be pointed out about the above, which should be compared with Prof. Tait on p. 194. First, that the result $\mathbf{F} = \phi\nabla$, irrespective of pureness, or $\mathbf{F} = \nabla\phi$ also when the stress is pure, when got quaternionically seems to be a great novelty to Prof. Tait, and to give him a "severe wrench," involving a "dislocation" and a "startling innovation." Perhaps, however, it is only Mr. MacAulay's peculiar way of arriving at the result, that Prof. Tait is alluding to. Moreover, secondly, in the vector algebra of Willard Gibbs and others the use of equation (2) or of (7) to express the force complete, by removal of the intermediate vector $\mathbf{N}$, is neither new, nor does it involve any straining of the intellect, for it is actually a part of the system itself, done naturally and in harmony with Cartesian mathematics. See Gibbs's "Elements of Vector Analysis" (1881-4) for the direct product of ∇ and ϕ . (Also for the skew product, a more advanced idea; it, too, is a physically useful result.) Thirdly, note how very differently the same thing presents itself to Prof. Tait according as it is clothed in his favourite quaternionic garb or in vectorial vestments. In the latter case it is either unnoticed or is contemptible; in the former, it may be a novel and valuable improvement.

I do not think that Prof. Tait does justice to Mr. MacAulay in making so much of a trifle such as passes unnoticed or unappreciated in the previous work of others. There is, I know, much more in Mr. MacAulay's mathematics than Prof. Tait has yet fathomed. For my own part, I like to translate it into vectors, not merely because it is then in a form I am used to, and is plainer, but also because the true inwardness of these processes involving linear operators is properly exhibited by the dyadical way of viewing them in conjunction with vectors, without the forced and unnatural amalgamation with quaternions, and the attendant obscurities. This seems to me to be particularly true in physical applications. I should not be writing this note were it not for the misconceptions that Prof. Tait indulges in about what he does not know, viz. vector algebra apart from quaternions. At the same time, to avoid possible misunderstanding, I disclaim any hostility to Mr. MacAulay's quaternionic innovations, although I must agree with Prof. Tait as to the "singular uncouthness" of some of his expressions in their present form. I hope he may be able to see his way to do his work vectorially. It will be more amenable to innovations, I think, without mental wrenches. At any rate he is a reformer, and not afraid to innovate when he thinks fit.

OLIVER HEAVISIDE.

Paignton, Devon, December 30, 1893.

The Second Law of Thermodynamics.

I APOLOGISE to Mr. Bryan for unintentionally reading into the Report, Article 17, what he did not intend to be there. I understand now that according to his view conservative systems are not alone to be included in the Clausian proof.

My point, however, is (or was) that they ought to be excluded, at all events when there is only one controllable co-ordinate v , because (1) in conservative systems the virial equation gives a relation between T and v , so that only one of them is independent. That, I submit, is true in fact. And (2) the second law, I said, requires two independent variables. That, however, is a question of definition, and if Mr. Bryan were to take the equation

$$\int \frac{\partial Q}{T} = 0$$

for a complete cycle, whatever be the nature of the system, as a definition of the second law, I see no valid objection to that definition.

I admit, and did admit, that for a conservative system, moving in a complete cycle,

$$\int \frac{\partial Q}{T} = 0,$$