LETTERS TO THE EDITOR.

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Dynamical Units.

WITHIN the moderate dimensions of a letter it is hard to give due weight to every aspect of a complicated matter, and while trying to emphasise one side I have somewhat overstated the case, as is evident from the way in which Prof. Lodge has taken me up. I was only considering the teaching of elementary dynamics to engineering students. I do not object to a teacher explaining that inertia is such an important constant property of matter, that equality of inertia is our *definition* of equal quantities of matter. What I do object to is, a common inversion of this, by which equality of inertia is *explained* by saying that the quantities of matter are equal. In addition, I urge that teachers of elementary *dynamics* should call what is usually called mass, inertia, so as constantly to bring before the student the fact that this is *the* property with which the dynamics of motion deals. I do not plead guilty in this to confusing the issues. The issues of Prof. Perry's review have been overlaid with a discussion as to one of the greatest advances of modern physics, namely the possibility of representing physical quantities by algebraic symbols; but I was trying to recall the original issue, as to the way dynamics should be taught to engineering students. Babes must be treated babyishly, and as long as engineering students are what they are now, and have to attend a variety of lecturers, and read engineering books as they are, I agree with Prof. Perry in recommending that the engineer's unit of inertia be used by their teachers. I have already explained that a multiplicity of units is a very minor difficulty to those who have once grasped what it is that is being measured, but I do think it confuses them, while getting these ideas, for one teacher to use one system, and another another system, and for each teacher to call the system of the others by hard names. GEO. FRAS. FITZGERALD.

Definite Variations.

MR. F. A. BATHER, in the January number of *Natural Science*, has some remarks on Prof. Cope's "Factors of Evolution" which seem to call for further comment. The case is cited of sheep taken from Ohio to Texas losing the fine quality of their wool, and this definite variation, due to environment, being apparently inherited and cumulative, in spite of selection by the breeders of those lambs which least present the new character. Such facts as these are not new, and it seems to me that they represent simply a phase of atavism. On July 23, 1890, I was present at a meeting of the Royal Horticultural Society, at Chiswick, and heard Mr. E. J. Lowe give an interesting address on ferns. In the course of it, he told how he had a great number of varieties of the hart's-tongue fern, which, on changing his place of residence, he moved into new and poorer soil. They all reverted to the typical form, and it was not until they were again transplanted to good soil that they consented to exhibit their varietal characters! Now in the case of the sheep, the fine wool of the Ohio breed is not a specific character, but a varietal one produced under domestication; and it is not surprising, therefore, that removal to a locality less favourable, and, perhaps, more resembling that of the original type of the species, should produce reversion. But it is probable that, as in the case of Mr. Lowe's ferns, the varietal character could be made to reappear by transference to the former kind of environment. The precise explanation of such facts as these may probably be found in Dr. Weismann's principle of germinal selection, which has surely been more or less understood for a long time. The sheep is born with two or three distinct possibilities, as to its wool; one locality favours one of these possible developments, one another. It is a case parallel to that of an amphibious *Ranunculus*, which can be made to assume one form or the other, according to the terrestrial or aquatic environment.

It is worth while to add, that here in New Mexico, one frequently sees small, usually pale yellowish-brown, horses, with extremely well-marked leg-stripes. These are descendants of the horses which ran wild in former years over this country; and there can be little doubt, I think, that they represent an atavistic variety.

While it is probable that really new variations are equally in all directions, practically the variation of most organisms is remarkably definite, because so largely atavistic. And these definite atavistic variations may be perpetuated, in new combinations, in new races. It is precisely this which gives rise to "kaleidoscopic characters" in a group. A character may appear here and there, and species may be represented by different combinations of the same characters, as words are composed of combinations of the same letters.

To cite an illustrative instance, the wings of bees present frequently one marginal, three submarginal, and three discoidal cells. The submarginals may be reduced to two, or even to one, and the discoidals to two. All sorts of combinations, as to the number, shape, and size of these cells will be found, but the marginal will *not* be found lacking, nor the first discoidal or first submarginal absent, nor will the number of submarginals be found increased.[1] Really new variations, as new ones running out to form a second marginal or a fourth submarginal, appear in slight degree, but doubtless sufficiently to afford material for selection, under new environment; but the old and common variations may occur suddenly, so that there may even be a radical difference between the opposite wings of the same specimen. Very rarely, a remarkable sport will occur, not in accordance with our expectations, but these are much too rare to have selective value. In other hymenoptera, as the sawflies, the range of common variation is quite different; but still the variations to be looked for in each family are not miscellaneous, but run along certain well-recognised lines. To show that new, not atavistic, variations take definite lines is another matter, and I do not believe it can be done.

T. D. A. COCKERELL.

Mesilla, New Mexico, U.S.A., February 18.

The Coral Reef at Funafuti.

THE report on the coral reef at Funafuti that was read to the Royal Society on February 11, will doubtless be of very great interest to all who have studied the very difficult problems concerning the origin of reefs and atolls.

At the same time, many will wonder why Prof. Sollas characterises the boring as a failure. Scientific expeditions very rarely accomplish all that is anticipated or even expected of them, but they are not necessarily failures in consequence. It is true that the borings at Funafuti could not be carried to a depth of more than 105 feet, and that the structure they revealed was not "what a field geologist might have anticipated"; but they revealed the very important fact that underlying a coral reef of 50 feet in thickness, there was a stratum of sand containing a few coral blocks.

It is perhaps premature to consider, until further details are published, whether this fact supports the views of Mr. Murray or his followers; but what is perfectly clear at once, is that it lends no support to the well-known subsidence theory.

I think it is of importance to call the attention of the scientific public to this at once, because, after dismissing the boring as a failure, Prof. Sollas gives the results of the soundings made in the neighbourhood of the island by H.M.S. *Penguin*, and concludes by the statement that, in his opinion, these soundings support Darwin's theory of coral atolls.

I should not like at this stage to take upon myself the responsibility of saying they do not, but I should like to ask, after the negative evidence afforded by the borings, upon what grounds Prof. Sollas bases his opinions. SYDNEY J. HICKSON.

Owens College, Manchester, February 20.

Two Unfelt Earthquakes.

REFERRING to Prof. John Milne's interesting communication on "Two Unfelt Earthquakes," asking for information as to whether these disturbances have been instrumentally recorded elsewhere, Dr. Copeland requests me to say that an examination of the photographs of the oscillation-curve of the bifilar pendulum at this observatory shows several disturbances on February 7, the first of the dates mentioned by Prof. Milne. These are as follows:—At 7.37 a.m. an abrupt movement of the pendulum towards the north; from 8.24 to 8.41 a.m. a distinct reduction in the intensity of the colour of the photographic

[1] *Melipona* and *Trigona* are exceptional, and in many ways depart widely from the normal type, so that they hardly come within the range of typical bee-modification.