LETTERS TO THE EDITOR

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Dr. Carpenter and Dr. Mayer

WITH reference to Dr. Tyndall's communication of last week, in which I most unexpectedly found a private note of my own placed before your readers, I should be obliged by your allowing me to state :—

1. That the idea of "Correlation," as originally entertained by Mr. Grove, and applied by myself to physiology more than twenty years ago, most unquestionably included that of the *quantitative equivalence* of the convertible forces, as will appear from the following passage in my memoir of 1850 (Phil. Trans. p. 731) :—"The idea of correlation also involves that of a certain *definite ratio* between the two forces thus mutually interchangeable, so that the measure of force B, which is excited by a certain exertion of force A, shall, in its turn, give rise to the same measure of force A as that originally in operation." And further I urged the *precise relation* observable between the vital activity of plants and cold-blooded animals, and the amount of heat they receive from external sources, as a ground for the belief that heat has the same relation to the organising force as it has to electricity (pp. 747-750).

2. In crediting Dr. Mayer therefore with the independent (and in my own case the previous) enunciation of the "Correlation" doctrine, I most certainly meant to include the notion of *quantitative equivalence*. Whether the quantities be or be not expressed in number seems to me a matter of secondary importance. WILLIAM B. CARPENTER

University of London, Dec. 26

The "North British Review" and the Origin of Species

THE writer of the article on the "Origin of Species," which was published in the *North British Review* for June 1867, has corrected in your periodical for November 30 an unimportant error which occurs in a certain paragraph of that article. There is, however, it appears to me, a much more serious error in the same paragraph, which vitiates his arithmetical calculations throughout, and leads him to an erroneous conclusion.

The paragraph in which this error occurs is quoted at length in Mr. Mivart's work on "The Genesis of Species." It may therefore be worth while to point out the oversight alluded to.

The error arises from the writer's assuming that in a race which remains constant in numbers, only one individual out of each family, *i.e.*, out of the offspring of one female, will on an average survive to produce young. This assumption is not true; for since only one half of the race, namely the females, bring forth young, it follows that two out of each family must, on the average, survive to have offspring, namely, one male and one female. Each of these will transmit its peculiarities to its descendants.

I will now quote the writer's words, putting within brackets the necessary corrections.

He says, "A million creatures are born; 10,000 survive to produce offspring. One of the million has twice as good a chance as any other of surviving; but the chances are 50 to 1 against the gifted individual being one of the 10,000 survivors." Further on he says, "Let us consider what will be its influence on the main stock if preserved. It will breed and have a progeny of, say 100; now this progeny will, on the whole, be intermediate between the average individual and the sport. The odds in favour of one of this generation of the new breed will be, say, $1\frac{1}{2}$ to 1, as compared with the average individual; the odds in their favour will therefore be less than that of the parent, but owing to their greater number the chances are that about $1\frac{1}{2}$ of them would survive [*about* 3 *of them*, for without any advantage two would on an average survive.] Unless these breed together, a most improbable event, their progeny would again approach the average individual; there would be 150[300] of them, and their superiority would be, say in the ratio of $1\frac{1}{4}$ to 1; the probability would now be that nearly two [$6 \times \frac{5}{4}$, or nearly 8] of them would survive, and have 200 [750] children with an eighth superiority. Rather more than 2 [15] of these would survive; but the superiority would again dwindle, until after a few generations it would no longer be observed, and would count for no more in the struggle for life than any of the hundred trifling advantages which occur in the ordinary organs."

The writer thus concludes that the advantage derived by inheritance from the sport will ultimately die out. The true conclusion is, that the advantage never dies out, but only becomes distributed through the whole race; and, moreover, that the sum of the advantages of all the favoured individuals, when added together, is greater than the original advantage, and becomes greater and greater every successive generation, though it tends to a limit at which it never actually arrives. Thus, representing the original advantage by unity, the advantage in the next generation is $1\frac{1}{2}$, in the next $1\frac{7}{8}$, and so on.

If now the same kind of sport arise independently, (*i.e.* not by inheritance from some previous sport) say once in every generation, and is preserved, say once in every fifty generations, the advantages derived by inheritance from these sports will accumulate and become distributed throughout the whole race. Hence in the course of an immense number of generations they must produce a decided effect upon the character of the race.

Thus, though any favourable sport occurring once, and never again, except by inheritance, will effect scarcely any change in a race, yet that sport, arising independently in different generations, though never more than once in any one generation, may effect a very considerable change. These conclusions are opposed to those which the writer of the article is endeavouring to establish.

Leeds Grammar School A. S. DAVIS

Prof. Tait on Geological Time

As I have lately found, under the signature of Prof. Tait, in the well-known *Révue Scientifique*, several statements that would doubtless have been challenged had they appeared in any English scientific journal, and of which the following are specimens :—"Sir W. Thomson has already demonstrated, by three complete and independent physical proofs, the impossibility of admitting the existence of such periods"—"Each one (of Sir W. Thomson's arguments) would suffice to upset at once the pretensions of Lyell and Darwin"—"Professor Huxley's attempt has completely failed;" and as in the new edition of Juke's Geology Sir W. Thomson's demonstration is stated at some length, while an adverse argument used by Jukes is omitted, I venture to ask that you will allow me a few words on the subject, since I treated the matter at length two years ago in *Scientific Opinion*, and, so far as I am aware, my arguments remained unanswered.

1. Does not the conclusiveness of all Sir W. Thomson's arguments depend upon the assumption of the universality of the principle of dissipation of energy? But to assume this is to assume that uniformitarianism is false. The whole question is therefore begged in the premisses, as must be the case in mathematical arguments.

2. As Mayer categorically denies the universality of the said principle, by what right does Sir W. Thomson entitle it a "principle of natural philosophy," and therefore state that uniformitarians are "directly opposed to the principles of natural philosophy"? As in the opinion of the French Academy, and of many eminent English and German savants, Mayer is one of the first physicists in Europe, I think it cannot be assumed with Prof. Tait that, "as regards method, Mayer and his supporters are little in advance of the Middle Ages," though undoubtedly Mayer is very different from Sir W. Thomson.

3. By what process does Sir W. Thomson discover "universal principles?" His universal principle regarding the origin of life "true through all space and all time," affords an opportune answer to this question. I would simply refer to Mr. Ray Lankester's article on that principle (NATURE, No. 97, p. 368), and ask if any one can discover a more satisfactory foundation for the *universal* principle of dissipation. From long study of Sir W. Thomson's reasonings, I conclude that he will reject any evidence for spontaneous generation, in consequence of the "universal principle" he has assumed on that question.

4. In Section A of the last British Association, Sir W. Thomson supported his argument regarding the form of the earth (controverted in your pages by Mr. Croll), by referring to existing mountains five miles high (see *Athenæum* report). His audience must have understood that these mountains are primæval, as otherwise the argument would have had no meaning. But as this is the reverse of the truth, I cannot help saying that Sir W. Thomson appears to consider himself entitled, not merely to invent principles, but also to invent facts. I know no conclusions of