LETTERS TO THE EDITOR

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Eozoön Canadense

I CANNOT understand Mr. T. Mellard Reade's right to fling the taunt at those who maintain the foraminiferal nature of Eozoön, that "each disputant takes up a different position, and shifts it as occasion requires."

I have never taken up any other position than this: that *the best-preserved specimens of the Canadian Eozoön exhibit an unquestionably foraminiferal structure.* I am supported in this by every British naturalist with whom I am acquainted, as specially conversant with foraminiferal organisation, viz., by Messrs. H. B. Brady, T. Rupert Jones, W. K. Parker, and Prof. W. C. Williamson; whilst the most eminent authorities in micro-mineralogy and pseudomorphic structure, viz., Messrs. David Forbes, T. Sterry Hunt, and H. Sorby, altogether disown Eozoön as a mineral.

I have further asserted, and I do not in the least "shift" my position, that *the character of the Canadian Eozoön is altogether independent of that of later ophites.* The occurrence of true Eozoic structure in the newest Tertiaries would only show that Eozoön, like Lingula, has maintained its continuity through a long succession of geological epochs. On the other hand, the occurrence of minerals presenting superficial resemblances to true Eozoönal structure, can be of no account to such as really understand the latter.

If the Skye ophite, for example, possesses a true "nummuline layer" in combination with other characteristic Eozoönal features, its presence in a formation of later "geological time than the Laurentian," furnishes no argument whatever against its organic character.

If, on the other hand, the supposed "nummuline layer" in the Skye ophite is nothing but a lamella of chrysotile, the existence of such a pseudomorph can only affect the opinions of such as are incompetent to distinguish the two by those microscopic tests on which experienced observers feel perfect reliance.

Since I do not feel called upon to expend valuable time in giving to Mr. T. Mellard Reade the instruction which he requires to qualify him for discussing this question, I now leave him to the enjoyment of his own opinion. Whenever he shall have shown, by work of his own, his competence to criticise the observations of others who have made a special study of the subject he discussed, I shall be most happy to afford him the same opportunity of forming his judgment as to the organic nature of Eozoön, by an examination of my preparations, that I have given to the many eminent naturalists, who have thus fully satisfied themselves of the justice of my conclusions.

W. B. CARPENTER

Dr. John Hopkinson on "The Overthrow of the Science of Electro-Dynamics"

As I see you have reprinted at length Dr. Hopkinson's paper with the above title, in which he criticises severely, not to say ungenerously, some papers of mine published in the *Quarterly Journal of Science* and *Chemical News*, you will think it only fair to publish my reply, in which I think I shall show that, in the course of his short paper, Dr. Hopkinson has committed mistakes at least as grave and important as any he imputes to me. Let us see if this is not the case.

Dr. Hopkinson quotes one of my articles as follows: "They (that is Joule and Scoresby) calculate the maximum theoretical power of a grain of zinc to be 158 foot-pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than $\frac{1}{4}$ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot-pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." On which he observes, "It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a-b)$ used to produce work W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electromagnets passed $\frac{1}{8}$ of an inch from them, doubtless W would be greater, but so also would $(a-b)$, and it does not follow that $\frac{W}{(a-b)}$, with which we are concerned, would be at all changed. What becomes, then, of the dogmatic assertion that the duty of a grain of zinc would be increased tenfold?"

Why he should say, "It is hardly credible, but the above looks very like a confusion between Force and Work," I know not. I cannot plead guilty to having made the slightest confusion between the two. I do think the total of the force used is *a measure* of the work produced. But Dr. Hopkinson tries to persuade us that a well-constructed engine would do no more duty than an ill-constructed one, and consequently, I presume, that the magnets might possibly be weakened *ad infinitum*, and removed to ever so great a distance, without necessarily affecting the efficiency of the engine. And then he ventures to criticise my papers as full of fallacies! I retort that it is hardly credible, but that the above looks very like a confusion between $(a-b)$ and b! In these experiments of Joule and Scoresby's the quantity of zinc used to produce work W, is represented by the authors not as $(a-b)$ but as b, and therefore the duty per grain of zinc is not $\frac{W}{a-b}$ but $\frac{W}{b}$; and when the permanent magnets are stronger, and the electromagnets are passed nearer to them, not only does W increase but b also diminishes. So that was I not justified in saying that the duty of a grain of zinc could in a better-constructed engine be probably increased tenfold? And if it be increased only twofold, or even half as much again, then, allowing for waste, I have proved my point, and disproved Joule's mechanical equivalent of heat. Might I not retort fairly on Dr. Hopkinson that the Manchester Literary and Philosophical Society "never ought to have permitted this paper to appear in their Proceedings?"

Next let us take Dr. Hopkinson's next criticism. My argument is this:—That if the doctrine of the mechanical equivalence of heat be, *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, and if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, the doctrine of the mechanical equivalent of heat cannot be universally true. To this argument Dr. Hopkinson replies that the facts I quote as paradoxes are simple deductions from the two laws of thermo-dynamics. Quite true; but this only shows that one of the laws of thermo-dynamics is inconsistent with the doctrine of the mechanical equivalence of heat. Might I not retort again on Dr. Hopkinson that "a hostile critic should at least understand the meaning of what he criticises"?

If I said anything which *seemed* to imply that a minimum of work in an engine was inconsistent with a maximum of duty, I freely retract the expression; and I also acknowledge that the argument drawn from the fire-syringe had better have been omitted. But my point was proved abundantly without it.

But still, as the maximum of work done by a battery before it is worn out is only a multiple of the maximum duty of a grain of zinc, I do think it is a *startling* thing, though not mathematically impossible, that this maximum of work should prove to be no work at all.

Perhaps you will allow me to add that I have read Sir W. Thomson's paper read before the British Association in 1852, to which your own reviewer referred me. No doubt you will think it presumptuous in me to say so, but I think that in that paper he has mixed up two totally distinct questions, namely, the cold produced by the decomposition of water into its elements at two electrodes, and the heat produced by the resistance of the film of hydrogen or oxygen or oxide, to the passage of the current. The first is a fixed determinate quantity; the second an accidental one depending on the character of the surface of the electrode, and the ease with which it throws off the film of hydrogen or oxygen. These two points affect the question, as well as the polarisation, and the specific power of retaining or transmitting heat exercised by various electro-motive combinations. M. Favre suggests the formation, sometimes, of peroxide of hydrogen; but this supposition is unnecessary, and, moreover, would not remove the difficulty, for peroxide of hydrogen is so unstable a compound, that it would soon be resolved into oxygen