

14/17 Paington 4. 2. 95

Dear Fitzgerald. Thanks for yours 1st, after reading it. I am inclined to think you are the virtual author of a good bit in his memoir. What I expected more than has come was, I think, ~~his~~ ^(Lamont) own fault. It was this way. We had a little correspondence near the end of '93 about wave surface, and he then mentioned that he was working hard at a new theory of the ether, rotational; had read my remarks in "Forces etc" paper about great difficulties in the way as regards conductors, etc; ~~didn't think it had~~ ^{but he thought it had} extraordinary merits, ~~but thought~~. In reply I told him it was an excellent analogy for dielectrics, but I did not make it go further. Break down in conductors. I also referred him to 2. m. Theory. He said in reply he didn't think anything of that [failure as regard induction] & something equivalent. Anyhow, I got the idea from his confidence that he was going to show us how to do it, and this was confirmed by his Abstract, which seemed very comprehensive & did not carry the idea that it was only tentative. So when I got the memoir I certainly was disappointed. But God forbid that I should find fault because he has not solved the problem; or that he has only written a tentative paper. What I should like to see would be a little more candour about the illusions of the rotational ether to satisfy electrical requirements. He seems to make believe that he does account for electric and electro-magnetic forces, from the properties of his initially assumed medium. I should rather say that it is proved (by his own work Stern's interpretation) that the rotational ether cannot do it in the way supposed (i.e. D a twist) without auxiliary hypotheses of a nature not yet discovered, & perhaps impossible of attainment.

If your remark about "mattocks" who seem to despise anything tentative" has any reference to an affair of mine, I am not to sure that Dr. tentativeness is not a good deal greater than mine, and in addition I have been perfectly candid in correcting errors.

The backbone of my despised Part 3 consists of an extension to B's for in general, and generalized, of the remarkable properties previously found for the zeroth order, with a conclusion, however. The Transformations from one order to another I do by means of two fundamental formulae

$$z^x = \sum_i \frac{x^i}{L^i} \quad (\text{got in Part I})$$

x positive, & any real + or - number; and a new companion formula

$$z^{-x} = \sum_i \frac{x^i}{L^i \cos \pi} \quad 14/(19)$$

with some restrictions on x and i . I got the second one by general reasoning, and proved that it was the missing link required to harmonize the B's for. Since then I have recited the second formula in different ways. So I have no particular reason to doubt any Part 3, or to think it unworthy of publication. I feel sure that the rigorous ~~method~~^{if not got already} must come to the same results if they go on at it long enough. I didn't send you the paper as you suggested, for a set of reasons. I had to wait for it first. I suggested that "if the decision of the Editing Com. was irreversible" I'd like to withdraw it, & in your notes got it back without comment. Since then I have kept it to see if I could make nonsense of it. Also, like D. Smidler waiting for something to turn up; quite independent the something. Also, because I don't like putting another Doc. to the great expense of publishing a 50 or 60 page paper (Proceedings etc) when they haven't had the previous parts. How can I say it is important enough for that, when the rigorists are against me? The R. Soc. in the proper place, if anywhere. Perry suggested the ~~Royal~~ Phys. Soc., but the Phys. Soc. is a physical society. So I let it slide for the present. This long personal explanation brings me to L again, in this way. Perry sent me a letter from him, in wh. I was very surprised to find him on the side of the rigorists. Something like this:— When H. keeps to operators in problems in physics he

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is within his rights; but another don't like his doctrine of divergent series, so when he writes about them without first reading up what they have said about them, he deserves [or shd expect] to be set upon!

Suppose I apply that to D's paper! Electricians don't like his doctrine about the circuital nature of D, or that D can be not circuital, and still be the curl of a vector, and if he chooses to write about these things without first reading up what electriicians have already done, he deserves to be set upon - However, I am not going to do it. I'd score the action. But I think he shd be a little more modest when his own work is so tentative, and displays such a remarkable want of rigor. E.g. - from the potential energy fm he finds the condⁿ of equilibrium, viz. $\text{curl } E = 0$ (equivalently), so says he, therefore $E = -\nabla \text{potential}$, the electrostatic post. But he does not see that the condition $\text{curl } E = 0$, together with his datum $\text{div } D = 0$, has only the solution $E = 0$. That is, circuital disturbances alone can be produced by unipolar force, and when left alone, they dissipate & leave $D = 0$. That is the true argument, I believe, so it is not all snobbism of Cambridge who are rigorists, evidently. I might similarly object to some other failures in his logic, but never mind that, when it has to be found how to get a charge on a body, or even an atom, and yet have $D = \text{curl } Z$. I don't say D circuital. Your twisted threads glued together won't do, I think. If the glue is melted they will untwist. Besides, glue or no glue, it does not make a continuous deformation of a medium, & that is what is wanted. Z is the Displ^t in a medium. It is rotationally elastic. How keep up, or purely produce a radial system of displacement? ~~the other~~ I suppose L. will say by disruption ^{of the other}. But that does not help I think. The medium must be continuous after disruption, & the difficulty begins again. That is, I assert that you cannot have $D = \text{curl } Z$ outside a body and also $E = -\nabla P$.

and I cannot at present conceive any sort of constraint at the body itself, or the electron, that will get over this difficulty, which seems to be fundamental. I shall be glad to know your opinion here, for it involves the total rejection of the rotational ether when D is twist, except in the limited case of one by-collateral analogy for dielectrics unchanged. There are equal difficulties with B a twist, I believe. As we come to what I have before arrived, that we shd. not regard the actual bodily motions of the ether as representing D, B, or E, H, but consider the latter something more inward, supported by and transmitted through the ether. This does account for the mechanical forces, though no doubt there are plenty of difficulties left. It is a dynamical theory too; it is made not more, but less dynamical, by defining D or B to be so and so. You lose generality, and you introduce the special difficulties involved in the assumption.

If you have a double electron \bar{O}^+ with a bond, which is conducting, & with an impressed force in the bond, (or expiring) then perhaps you might do something in changing a body, though I fancy you will come to great difficulties.

Of course you will not trouble to answer, if you are busy. It amuses me to scribble sometimes, when I can't settle to work. Yours sincerely Oliver Heaviside.

16/19