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(b) 1

My dear Fitzgerald, I believe you do not care to be "Professor" d, so I drop it at once. I duly rec'd your batch of papers and note of the 25<sup>th</sup>.

When I receive proof of paper sent to Phil. at Xmas I will make due reference to your work in connection with moving charge. I have read your papers (except the big one) with much pleasure, and particularly admire your candour on difficult points. Some writers shut them over, and are besides, so very indistinct that you don't know what they are doing, & doubt whether they know themselves. My own practice (generally) is to leave the unsettled points out altogether; but as regard what I have established, to be dogmatic, & take the chance of having blundered. By idiocy I suppose you mean that you were in an unsettled state of mind & didn't see your way clearly. You were misled by Mr's statement of identity of results, by German & his methods. But I think he only meant as regards phenomena to be got by assuming displacement ignorable. I was misled another way, viz by his description of the way current begins in a wire, in his chapter on the General Equations. It is nothing like it. But it put me on the wrong track for years. As regards your speculations concerning the waves set up by periodic current, which lead you to the stationary vibrations in space, I had, not the same, but similar difficulties, which I only completely surmounted ~~first~~ in 1888; before that I was going round and round without catching the missing link. Which was to make the field perfectly clear & common-sense-like. At one time I actually formulated a rigid demonstration that disturbances emitted from a conductor alone in space, must, given time enough, all come back to it. This was in 1886. But I never published any failures.

Can you tell me whether you were the first to make use of the twice-integral of  $H^{\alpha\beta\gamma}$ , such that  $\text{curl } H = CE$ , as an auxiliary function? I thought I introduced it. I have applied it to conductors as well as dielectrics, & shown that it is necessary to be used in order to represent electric energy in terms of potentials. It is quite as useful as Maxwell's A; but I never use it except (like A) as an auxiliary function. I never made any progress till I knew all.

the potentials overboard, and made E and H the objects of attention, with  $\text{curl } E = -\mu H$  as companion to Maxwell's equation connecting H and electric current; that done, I did, (is outside) in a week or two, nearly all I have since published, & a great deal unpublished. And now Sir W. is falling foul of my curls, & tells me they are useless in physical applications! He will get his elastic solid theory at his head, I am afraid.

Regarding Hertz. I only knew of his investigation thro' Deudemann's account in the Electrician. From it I see that Hertz is not a Maxwellian though he is learning to be one. By a Maxwellian I mean one who follows Maxwell as interpreted by O.H. All others are not Maxwellians, of course, according to my definition. I have certainly not worked out the problem you mention, in an exact manner. The fact is, Hertz's waves are very complex indeed near their source. Now if Hertz had been a Maxwellian, he would have used a double wire, instead of a single one, in his wave experiments. His waves along wires (he says through or in wires) are bounded by the walls of the room & obstacles, and are very unsuitable for testing the "speed of the current". He got about  $\frac{2}{3}$  of the result in free space. I have asked Lodge, & he tells me Hertz is wrong on this point, & will back it up, whilst he (L.) gets (approximately) the theoretical result. But it does not certainly follow that H. is wrong, on account of the great lateral dimensions of his waves compared with their length. 14/1

I ~~do~~ have considerable reluctance to investigate such a complex problem as H's except present. I ~~do~~ like to do the expts myself first, & so get to know all the peculiarities, & then, guided by theory to simplify if possible. Though not <sup>properly</sup> an experimentalist, I know the importance of details in experimenting. I was, by the way, very close to getting <sup>some of</sup> H's results myself some years ago, though I did not know it then.

You may have perhaps seen in the Elc that I have got the complete soln. of the problem of a moving charged sphere (of no res.) and in a finite simple form, at any speed not exceeding light.

It was a surprise to me that it ~~do~~ reduce to simplicity. I had it in the form of an inf. series of harmonics, & brought it to finiteness in order to cure Sir W. of his notion about pressural waves. If this does not answer, there is the elastic solid to fall back upon. From my perusal of your papers I am sure you will be interested in

my solution. In fact you say how desirable it would be to have a complete investigation. But a point to which I don't like to direct your attention is, What is the value when the speed of motion exceed that of light? The same solution will do identically, provided we ignore the unreasonableness. I have been quite unable to come to any real solution, & have been obliged to conclude that there is none, that is, no steady (<sup>possible</sup> steadily moving) state <sup>1</sup> after the speed of light is passed.

On the way to this solution I have been obliged to point out serious errors in J.J.T. He is wrong as regards the self-energy, the mutual energy, and the forces! And why? All on account of the vector-potential.

Yours sincerely  
Oliver Heaviside.

P.S. If you have any time to spare at any time, I could recommend you to read Sections XL to XLVII of my E.M.C. & its P. in the E&E June 1887 and later; it is the Royal Road to E.mag.waves. It is incomplete, but there is enough to show what it is like. (2d.)

Can you tell me if <sup>a</sup> Lorenz or Lorentz of Copenhagen is living & what address would find him. I don't like to send him a book, as, if not too old, he would probably read it.

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