

WESTERN UNIVERSITY OF PENNSYLVANIA.

April 26th, 1899.

7/58

Dear Professor Fitzgerald,

Your very kind letter of the 13th. inst. is received, and I thank you also for the paper on vortex motion which accompanied it. I am very much interested in it, as I have finally got an exact and precise determination of the motions etc. concerned in electricity and magnetism, and all that now remains is the details by which they are actually accomplished, and in this your paper is very suggestive. I am going to ask you to add one more to the favours to which I am indebted to you by letting me ~~have~~ have the names of a few of the papers you have written on this subject and which are not in the Phil Mag. I have a publisher who gets such papers for me very promptly, but I have the impression that some of your most valuable work is published in proceedings which I do not see, but which I could easily secure if I had the titles and the year, approximately, of publication. Personally I like the idea of a vortex ether (which I believe is due to you) and I am glad to see, from your note, that there is some hope of working with it by the use of your two general vectors, and without having to go into the actual detailed mechanism. If we had to do this I am afraid progress would be rather slow, and most people would be inclined to let it alone.

With reference to the obscurities in my paper <sup>to</sup> which you have been good enough to call ~~to~~ my attention, I am very glad to have had your valuable criticism before publication, as these points would ~~be~~ undoubtedly have been prejudicial to the paper unless given the fuller explanation which I subjoin, in the form of an <sup>note</sup> ~~appendix~~ to the paper. I have thought it best not to give the explanation in the place where the method of determining the density of the ether is given, as it would be putting detail in a place where I wish things to be rather brief, and as the work itself, in spite of its obscurity, is entirely correct, it will be sufficient if I give the clue to any difficulties in the <sup>note</sup> ~~appendix~~. In this, however I would be guided entirely by your advice, and if you think best, would give the details ~~in a note~~ at the place where the determination is made.

I hope that if you consider the paper suitable for publication, (which I hope may be the case, as I may say that if it appears soon I am in hopes of securing an appropriation for my department, on the strength of it, from some of our manufacturers here,) I shall be able to have a few proofs of it, as I notice that in some cases at least the ~~language~~ construction is rather poor, and there is at least one misprint, as you quote my ~~value~~ value for the density of the ~~ether as 1.6 when it should be 0.6, or about 2/3 the density of water.~~  
ether as 1.6 when it should be 0.6, or about 2/3 the density of water. This must have added considerably to the obscurity of my reasoning.

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As this determination of the density of the ether, for the first time, to any degree of accuracy, is a rather important thing, I think I shall, at some time in the future go more into it and make some experiments by which I think I shall be able to get a closer value.

It will interest you to know that the velocity of a volt per cm. comes out of the same order as the actual ionic velocities. I am looking this up, as I have apparently found a relation which explains the different ionic velocities. I am also looking for gyrostatic effects in electric phenomena, like Maxwell looked for them in magnetic, (Maxwell, vol. 2, 575.) I think I have got hold of some good things, but am not ready to swear to them yet, as it is so easy to be deceived in such matters, especially if the apparatus is not very good. However I have great hopes.

I note what you say about the plates. I suppose that the experimental difficulties would be too great. I knew it had been tried, as I tried it myself some seven or eight years ago, and found out then, on looking up the matter, that it was not new. But I had thought that with great pressures, such as would be obtainable at the Eiffel tower the results might be more accurate. However, in any case, I doubt if it is worth the trouble.

*is the phenomenon but not the application of it - the direction of the force.*

You may be interested to know that we are putting up a big observatory here. We will be considerably better fitted out for astrophysical work than either Yerkes or Lick; for instance we will be able to get original negatives of the sun 18 inches in diameter, which can of course be afterwards enlarged, and with our spectroscopic apparatus will be easily able to measure planetary rotations with about twenty times the accuracy now obtainable, or within a very small fraction of a kilometer per second. I have been working to get Wadsworth here, and think I have succeeded, though a good many of the directors wanted Kaiser or Rünge, either of whom would no doubt be good men, and available, but I consider Wadsworth much better. We will have a part of it up before the eclipse here next year. Our telescope will be only thirty inches, but the auxiliary apparatus will be much superior to anything yet produced.

I am sending you a lecture given before the Franklin Institute last week on Electromagnetic Mechanism, which I think takes up the question in a rather new way, by tracing out the energy in the diamagnetic.

I trust that your family is in good health again, and that you have managed to get a little rest during the holidays. How I envy you your spring! Ten days ago it was snowing. Today it is 92 in the shade and practically midsummer, except that the week old foliage is still green. I am, Sincerely and respectfully yours,

Reginald A. Fessenden.

P.S. I enclose P.M. order for my own paper. Which I had almost forgotten.