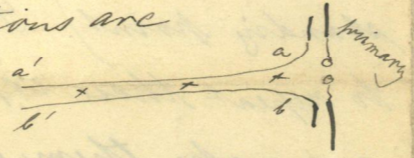


its own waves excited. This is quite true, but you can avoid complications arising from this, if either you make the wire very long, about 20 waves lengths or if you change its length until the nodes are best pronounced. Then ~~its~~ ^{its} the rate of vibration coming into play will be the same as that of the primary and of the receiver. ~~That~~ I always took care to have these simplifications of ^{arranged} theory. 3) I should advise Mr Thorpe ^{always} to make use of two wires. ^{not any but} As the wave is conducted in the air between them, the conditions are much poorer in this case than with one wire.



4) I myself never observed any difference of rate in different wires from the thickest to the thinnest, nor can I say anything that should look like an explanation. But if the observation proves real I should attach the greatest interest to it. With a vibrator and a receiver in resonance, with a long pair of wires and with the observation of three nodes I think there could be no doubt left as to reality. I thank you very much and shall be thankful for any communication of former results. With best compliments to Mr Trouton and Mr Thorpe I am Yours most truly
H. Hertz

Bonn, July 20, 1891

26/10

Dear Prof. Fitzgerald,

I am very much indebted to you for the notice you kindly gave me about Mr. Thorpe's experiments. Permit me firstly to state my own present opinion about the matter ⁱⁿ question. The only reason I have to believe that there be any difference in the rate of propagation in air and wires ^{is} the result of my own experiments. The contrary ^{was} made probable ^{to me} 1) by the theory 2) by the fact that for short waves I could not find any difference 3) that I could ^{not} detect any influence of the size or form of the section of the wires 4) by the experiment of Sarazin and de la Rive, who got the same velocities. I think that the odds are entirely in favour of the latter view and though I do not think possible that I made so large a mistake in the

observation; I think it possible, that special reasons spring from the form and size of the room in which I worked displaced the nodes so that I was deceived. I was confirmed in this view when during the last Easter holidays I tried to repeat the experiments in my present lecturing room. This is a very beautiful room but the elevated seats forced prevented me to choose a direction which was symmetrical to the walls, I was forced to make use of directions of propagation inclined to the walls. Now under these circumstances the nodes were so badly pronounced, that I could not make observations which could compete with the much better ones I made formerly, and that even I could not with certainty say if the observations agreed better with Sarasin's and de la Rive's or with my own. So I gave it up and acquiesced with the idea that a hall 12 mtr square is much too small which is not absolutely empty

to make proving experiments with waves 3-4 mtr long and that ^{the proof of} ~~certainly~~ theory was better taken from short waves, which proved its rightness.

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Now this all of course becomes very much changed if the results of Mr. Thorpe to prove to be exact. Again some mysterious difference would come in. So I think the very greatest interest is attached to the observation (especially from my own personal standing point). So I think I will be necessary to repeat these experiments over and over again and make them quite sure. Permit me to make some observations. 1) You say that the resonator picks out its own proper vibration. This is quite true, but what does it matter, if from the beginning you get it to resonate with the primary. Then its own vibration is the vibration of the primary too. With a little exercise this is easily done and all theory is infinitely more simpler than if from the beginning you have two rates of vibration in play. 2) You say that possibly the wave conducting wire may resonate too and have