

ON ELECTROMAGNETIC INDUCTION.

BY S. J. BARNETT.

1. In a recent article¹ on electromagnetic induction and relative motion published in this journal, and in a preliminary paper² published in the *Physikalische Zeitschrift*, I referred incidentally and briefly, and of necessity adversely, to some work on unipolar induction³ by Mr. E. H. Kennard, who has now published three articles⁴ in reply, the last in the May number of this journal. Although, in my opinion, Mr. Kennard's fallacies are quite apparent to anyone familiar with this part of electrical theory, it seemed advisable to make a reply⁵ to the first of these articles on its appearance; and a final reply is now necessary.

2. In each of the three articles Mr. Kennard either states or implies that my chief criticism of his work is on account of his treatment of the dielectric on the moving line hypothesis. My chief criticism of Mr. Kennard's work has always been that even if his moving line theory were absolutely correct when applied to the ordinary case of unipolar induction in which the complete field-producing agent rotates, his sweeping conclusion from his experiments would be entirely without justification. For in his experiments the iron core of the electromagnet rotated alone while the magnetizing coil remained fixed like the condenser; yet his calculation of the effect to be expected on the moving line hypothesis assumes that on this hypothesis all the lines in his experiments would move with the iron. But it is quite impossible to say what fraction of the lines, on this hypothesis, would adhere to the iron and move, and what fraction would adhere to the coil and remain at rest. For the same reason, viz., that a part of the field-producing system was at rest, instead of all being in motion, the experiments are without value in connection with the matter of relativity.

3. In treating the problem of unipolar induction in a fixed dielectric

¹ PHYSICAL REVIEW, 35, 1912, p. 323.

² Phys. Zeit., 13, 1912, p. 803.

³ Phil. Mag., 23, 1912, p. 937.

⁴ Phys. Zeit., 13, 1912, p. 1155, and 14, 1913, p. 250; PHYSICAL REVIEW (2), 1, 1913, p. 355.

⁵ Phys. Zeit., 14, 1913, p. 251.

Poincaré,¹ Abraham, and myself have each assumed that throughout the field (points within the rotating magnet not being included) the electric density is proportional to the divergence of the *total* electric intensity. Mr. Kennard says that this was the assumption made by Hertz,² but that it is not always allowable on the theory of Lorentz. These statements are both true. But it is also true and well known that in fields such as that under discussion, in which the intensities are steady and the ether and matter at rest, *Lorentz's theory and Hertz's theory and Maxwell's theory are completely identical* so far as the total intensity, displacement, and density are concerned; and that in this case the assumption referred to is not only allowable but necessary. Mr. Kennard says that this assumption "may easily be shown to be incompatible with the moving line theory," and attempts to prove his assertion as follows: He writes the equation

$$\operatorname{div} f = \operatorname{div} (E + e) \quad (1)$$

in which E , e , and f are the field, motional, and total intensities, and applies the equation to a point in a dielectric of free ether at which the electric density, and therefore $\operatorname{div} f$, according to Poincaré, Abraham, and myself, are zero. This gives

$$\operatorname{div} e = -\operatorname{div} E. \quad (2)$$

Mr. Kennard then *assumes, without justification*, that $\operatorname{div} E = 0$. Then he calculates $\operatorname{div} e$ and finds that it does not vanish—a particular case of a well-known relation to which he has repeatedly called attention. Hence he concludes that (1) and (2) cannot be correct, and therefore that I and the others are wrong. The trouble, however, is only with the false assumption $\operatorname{div} E = 0$.

4. To come to the next point, which I did not realize that my treatment could have left in obscurity, *I have certainly assumed that moving lines of induction act on the ether precisely as on a material dielectric.*³

¹ Poincaré assigns to the dielectrics, magnet, and other conductors velocities which may vanish in particular cases. His treatment of the subject on Lorentz's theory is not correct for the case in which the velocities of the dielectrics differ from zero; and his conclusion that so far as experiment can show "the theory of Lorentz leads to the same results as that of Hertz" in the general case is erroneous. When the dielectrics move the two theories give quite different results; but on each theory the result is independent of the hypothesis adopted with reference to the seat of the electromotive force.

² Mr. Kennard's statement that Hertz "rejected the motional intensity" cannot be accepted.

³ This is also an implicit assumption in the work of Poincaré and Abraham. For in the case under discussion the total intensity f differs from the field intensity E (whenever there is a difference) only by the motional intensity e . As another discussion of unipolar induction manifestly based on the same assumption should be mentioned that of S. Valentiner (Phys. Zeit., 6, 1905, p. 10), which, as far as it goes, is essentially identical with my own.

Mr. Kennard says that this is not consistent with current electrical theory as he knows it. It is nevertheless old and sound doctrine, universally accepted since the days of Maxwell.¹ It is involved in the fundamental relation between the electromotive force around a closed circuit in the ether at rest and the rate of change of magnetic flux through the circuit—a change produced, according to Maxwell, by the *motion* of the lines of induction across the circuit, since the lines of induction are closed curves—coupled with the relation between intensity and displacement. Among many obvious examples, one whose mere mention should be sufficient is the case in which the energy of a magnet set into motion is transformed into electromagnetic energy and transferred to an adjacent electric circuit in accordance with Poynting's theorem. As to Mr. Kennard's ideas on the diametrical opposition of ether to matter, etc., no remarks seem necessary.

5. The calculation of the charge on the condenser in my experiments to be expected on the moving line hypothesis when the field-producing agent rotates and the condenser remains fixed (Case II.) Mr. Kennard proceeds to make by *assuming* that the charge would be the same as when the condenser rotates and the field-producing agent remains at rest (Case I.). His reason for this appears to be the false assumption that the moving lines would have no effect on the ether. Had Mr. Kennard taken proper account of the ether, he would have obtained the charge *zero*, exactly as if the lines had been assumed to remain fixed. Because his calculation gave a charge independent of the magnitude of the dielectric constant, he stated in a previous paper, referring to my criticism that he had not taken proper account of the dielectric, that the trouble could not be with his treatment of the dielectric. But the neglected ether is an important part of the dielectric.

6. In his concluding paragraph Mr. Kennard says that he has elsewhere called attention to the fact that my conclusion as to relative motion rests in part on an inference.

Now it follows immediately and necessarily from the experiments of Faraday, Lorentz, Rayleigh and others on the motion of conductors in magnetic fields, together with the experiments of Blondlot, H. A. Wilson, and myself on the motion of insulators in magnetic fields, that if the condenser, whatever the magnitude of the constant of its dielectric, rotates while the agent producing the magnetic field remains fixed (Case I.), the condenser receives a charge equal to the product of the capacity as it would be with ether alone as dielectric by the rate at which the

¹ See, for example, Heaviside's *Electromagnetic Theory*, I., § 48; Lodge's *Modern Views of Electricity*, p. viii and §§ 114, 115; and S. Valentiner, *loc. cit.*

short-circuiting wire cuts across magnetic flux; and that if the material part of the dielectric is air, as in my own recent experiments, it is of no consequence whether the air moves with the conductors or not.

In the first of my original papers I referred only to the fact of the condenser's becoming charged; and in the second I referred, in addition, only to the "experiments of Faraday and others" as establishing the fact. Probably I should have gone into greater detail. This I have done in the *Physikalische Zeitschrift*, 14, 1913, p. 251; and in *Science*, January 17 and February 21, 1913.

The brief statement of fact, without reference to authority, in my first paper drew from Mr. Kennard the criticism to which he refers. He said that I had failed to give experimental proof of my statement. References to the experimental work having been given later, however, Mr. Kennard now objects to the experiments of Blondlot, Wilson, and myself on insulators on account of sliding contacts, stationary connecting wires, absence of a conducting screen, etc. These objections are entirely inconsequential and irrelevant and I shall not consider them further. No one will object to the repetition of any or all of these experiments, either modified or unmodified, by anyone who is sufficiently interested in them; but it is quite certain what the results will be.

7. In conclusion it seems desirable to consider briefly what happens on the theories of Hertz, Einstein, and Lorentz in each of the two principal experiments involved in the discussion of relative motion. As above, the case in which the condenser rotates will be referred to as Case I.; that in which the condenser remains at rest, as Case II.

On the theory of Hertz, the condenser is uncharged in Case I. and also in Case II.

On the theory of Einstein, the condenser is charged as indicated in § 6 in Case I., and charged in the same way in Case II.

On the theory of Lorentz, the condenser is charged as indicated in § 6 in Case I., and is uncharged in Case II.

Lorentz's theory is thus the only one which is consistent with both sets of experiments.

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