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XXXII. On static electrical inductive action

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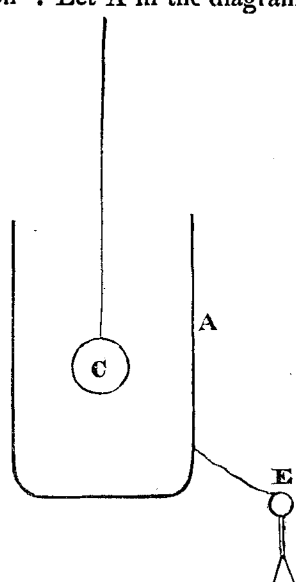
so much concerned with how a particle must be displaced relatively to the *medium*, as with how it must be displaced relatively to *the front of the wave*. And the confounding of these two is (as I said before) the cause of Mr. Earnshaw's difficulties and the explanation of the inapplicability of his objections.

XXXII. *On Static Electrical Inductive Action. By* MICHAEL FARADAY, *Esq., D.C.L., F.R.S.*

To R. Phillips, Esq., F.R.S.

DEAR PHILLIPS,

PERHAPS you may think the following experiments worth notice; their value consists in their power to give a very precise and decided idea to the mind respecting certain principles of inductive electrical action, which I find are by many accepted with a degree of doubt or obscurity that takes away much of their importance: they are the expression and proof of certain parts of my view of induction*. Let A in the diagram represent an insulated pewter ice-pail ten and a half inches high and seven inches diameter, connected by a wire with a delicate gold-leaf electrometer E, and let C be a round brass ball insulated by a dry thread of white silk, three or four feet in length, so as to remove the influence of the hand holding it from the ice-pail below. Let A be perfectly discharged, then let C be charged at a distance by a machine or Leyden jar, and introduced into A as in the figure. If C be positive, E also will diverge positively; if C be taken away, E will collapse perfectly, the apparatus being in good order. As C enters the vessel A the divergence of E will increase until C is about three inches below the edge of the vessel, and will remain quite steady and unchanged for any lower distance. This shows that at that distance the inductive ac-

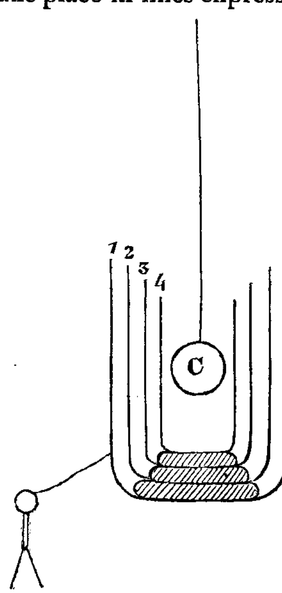


* See Experimental Researches, Par. 1295, &c., 1667, &c., and Answer to Dr. Hare, Philosophical Magazine, 1840, S. 3. vol. xvii. p. 56. viii.

tion of C is entirely exerted upon the interior of A, and not in any degree directly upon external objects. If C be made to touch the bottom of A, *all* its charge is communicated to A; there is no longer any inductive action between C and A, and C, upon being withdrawn and examined, is found perfectly discharged.

These are all well-known and recognised actions, but being a little varied, the following conclusions may be drawn from them. If C be merely suspended in A, it acts upon it by induction, evolving electricity of its own kind on the outside of A; but if C touch A its electricity is then communicated to it, and the electricity that is afterwards upon the outside of A may be considered as that which was originally upon the carrier C. As this change, however, produces no effect upon the leaves of the electrometer, it proves that the electricity *induced* by C and the electricity *in* C are accurately equal in amount and power.

Again, if C charged be held equidistant from the bottom and sides of A at one moment, and at another be held as close to the bottom as possible without discharging to A, still the divergence remains absolutely unchanged, showing that whether C acts at a considerable distance or at the very smallest distance, the amount of its force is the same. So also if it be held excentric and near to the side of the ice-pail in one place, so as to make the inductive action take place in lines expressing almost every degree of force in different directions, still the sum of their forces is the same constant quantity as that obtained before; for the leaves alter not. Nothing like expansion or coercion of the electric force appears under these varying circumstances.



I can now describe experiments with many concentric metallic vessels arranged as in the diagram, where four ice-pails are represented insulated from each other by plates of shell-lac on which they respectively stand. With this system the charged carrier C acts precisely as with the single vessel, so that the intervention of many conducting plates causes no difference in the amount of inductive effect. If C touch the inside of vessel 4, still the leaves are unchanged. If 4 be taken out by a silk thread, the leaves perfectly collapse; if it be introduced again, they open out to the same degree as before. If 4 and 3 be connected by a wire let down between them by a silk thread, the leaves remain the same, and so they still remain if 3 and 2 be connected by a similar wire; yet all the electricity originally on the carrier and acting at a considerable distance, is now on the outside of 2, and acting through only a small non-conducting space. If at last it be communicated to the outside of 1, still the leaves remain unchanged.

Again, consider the charged carrier C in the centre of the system, the divergence of the electrometer measures its inductive influence; this divergence remains the same whether 1 be there alone, or whether all four vessels be there; whether these vessels be separate as to insulation, or whether 2, 3 and 4 be connected so as to represent a very thick metallic vessel, or whether all four vessels be connected.

Again, if in place of the metallic vessels 2, 3, 4, a thick vessel of shell-lac or of sulphur be introduced, or if any other variation in the character of the substance within the vessel 1 be made, still not the slightest change is by that caused upon the divergence of the leaves.

If in place of one carrier many carriers in different positions are within the inner vessel, there is no interference of one with the other; they act with the same amount of force outwardly as if the electricity were spread uniformly over one carrier, however much the distribution on each carrier may be disturbed by its neighbours. If the charge of one carrier be by contact given to vessel 4 and distributed over it, still the others act through and across it with the same final amount of force; and no state of charge given to any of the vessels 1, 2, 3, or 4, prevents a charged carrier introduced within 4 acting with precisely the same amount of force as if they were uncharged. If pieces of shell-lac, slung by white silk thread and excited, be introduced into the vessel, they act exactly as the metallic carriers, except that their charge cannot be communicated by contact to the metallic vessels.

Thus a certain amount of electricity acting within the centre of the vessel A exerts exactly the same power externally, whether it act by induction through the space between it and A, or whether it be transferred by conduction to A, so as absolutely to destroy the previous induction within. Also, as to the inductive action, whether the space between C and A be filled with air, or with shell-lac or sulphur, having above twice the specific inductive capacity of air; or contain many concentric shells of conducting matter; or be nine-tenths filled with conducting matter, or be metal on one side and shell-lac on the other; or whatever other means be taken to vary the forces, either by variation of distance or substance, or actual charge of the matter in this space, still the amount of action is precisely the same.

Hence if a body be charged, whether it be a particle or a mass, there is nothing about its action which can at all consist with the idea of exaltation or extinction; the amount of force is perfectly definite and unchangeable: or to those who in their minds represent the idea of the electric force by a fluid, there ought to be no notion of the compression or condensation of this fluid within itself, or of its coercibility, as some understand that phrase. The only mode of affecting this force is by connecting it with force of the same kind, either in the same or the contrary direction. If we oppose to it force of the contrary kind, we may *by discharge* neutralize the original force, or we may *without discharge* connect them by the simple laws and principles of static induction; but away from induction, which is *always of the same kind*, there is no other state of the power in a charged body; that is, there is no state of static electric force corresponding to the terms of *simulated* or *disguisèd* or *latent* electricity away from the ordinary principles of inductive action; nor is there any case where the electricity is *more latent* or *more disguised* than when it exists upon the charged conductor of an electrical machine and is ready to give a powerful spark to any body brought near it.

A curious consideration arises from this perfection of inductive action. Suppose a thin uncharged metallic globe two or three feet in diameter, insulated in the middle of a chamber, and then suppose the space within this globe occupied by myriads of little vesicles or particles charged alike with electricity (or differently), but each insulated from its neighbour and the globe; their inductive power would be such that the outside of the globe would be charged with a force equal to the sum of *all* their forces, and any part of this globe (not charged of itself) would give as long and powerful a spark to a body brought near it as if the electricity of all the particles near and distant were on the surface of the globe itself. If we pass from this consideration to the case of a cloud, then, though we cannot altogether compare the external surface of the cloud to the metallic surface of the globe, yet the previous inductive effects upon the *earth* and its buildings are the same; and when a charged cloud is over the earth, although its electricity may be diffused over every one of its particles, and no important part of the *inductric* charge be accumulated upon its under surface, yet the induction upon the earth will be as strong as if all that portion of force which is directed towards the earth *were* upon that surface; and the state of the earth and its tendency to discharge to the cloud will also be as strong in the former as in the latter case. As to whether lightning-discharge begins first at the cloud or at the earth, that is a matter far more difficult to decide than is usually supposed *; theoretical notions would lead me to expect that in most cases, perhaps in all, it begins at the earth. I am,

My dear Phillips, ever yours,

Royal Institution, M. Faraday.
4th Feb. 1843.

XXXIII. *On the Electrical Origin of Chemical Heat.* *By* James P. Joule, *Esq.*†

In a paper‡ which I read on the 2nd of last November before the Literary and Philosophical Society of this town, I endeavoured to account for the heat evolved by the combustion of certain bodies, on the hypothesis of its arising from resistance to the conduction of electricity between oxygen and the combustibles at the moment of their union. Taking this view of phænomena, I showed that the heat evolved by the union of two atoms is proportional to the electromotive force of the current passing between them, in other words, to the intensity of their chemical affinity.

In that paper I gave the results of my own experiments, and I apprehended that my numbers were below the truth on account of the simplicity of my apparatus. On comparing them, however, with the experiments of Dulong, which were conducted in a manner very well calculated to prevent loss of heat, I now find that they agree so well with the results of that very accurate philosopher as to show that the method I adopted of carrying on the combustion in the inner of two glass jars, while the heat evolved was measured by water placed between them, was not unworthy of reliance. In the following table I give the results of Dulong's experiments reduced to degrees Fahrenheit acquired by a pound of water.

* Experimental Researches, Par. 1370, 1410, 1484.

† Read before the British Association at Manchester, 25th June 1842; and now communicated by the Author.

‡ Published in the Phil. Mag. S. 3. vol. xx. p. 98.