4 Logic of Discovery in Maxwell's Electromagnetic Theory*

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I

Contemporary accounts of the structure of science are broadly distinguishable into two types which I shall call the hypothetical and the historicist.[1] The hypothetical accounts restrict the operation of logic and reasoning to the deductive testing of hypotheses and banish the role of discovery to psychology or mere guesswork. The historicist accounts on the other hand describe the internal coherence of particular theories, emphasizing how the presuppositions of empirical observation itself are theory-laden and historically relative. Neither account investigates the inductive relation of observation to theory; indeed, both in a sense deny that there can be any such logical relation, the first because the only logic available is that of deduction from hypotheses, and the second because observation is already permeated by theory and therefore cannot be used as independent inductive evidence for theory.

Nevertheless, the historian of science cannot ignore the number of occasions on which inductive evidence for theories has been claimed, and inductive reasoning found acceptable. This feature of scientific inference has no doubt been oversimplified in the past, and therefore in order to investigate its character and validity it has become neces-

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sary to re-examine the historical cases where inductive inferences are claimed. It is likely that as a result of such re-examination the traditional accounts of both inductive and deductive inference in science will be found to be inadequate.

In this paper I shall investigate in particular Maxwell's explicit discussions of physical method, and their application in his electromagnetic theory. This theory has given rise to much historical literature, in which almost every style of scientific inference recognized by philosophers has been claimed to be illustrated. But in spite of this it has not been much remarked that Maxwell himself often claims that his method is the authentic Newtonian one of "deduction from experiments" without the aid of unproved hypotheses. My aim is to clarify this claim and to consider how far it is justified in his electromagnetic theory, particularly in the introduction of the electric displacement current, which looks on the face of it like a paradigm case of the "theoretical concept" of later philosophical analysis. I shall argue that, on the contrary, in his mature theory Maxwell intended it to involve no hypothetical concept, and no physical "model" as ordinarily understood, but that he attempted to justify it by a generalized method of induction and analogy.

In Maxwell's discursive methodological remarks[2] four different types of theoretical method can be distinguished: the hypothetical, the mathematical, the analogical, and "deduction from experiments."

1. The Hypothetical Method

In several investigations Maxwell explicitly adopts the classic hypothetical and eliminative method. In his early work on Saturn's rings, for example, he lists three possible mechanical hypotheses as to their form: they are solid and uniform, or not solid, or not uniform. A solid uniform ring would be unstable; the form of solid irregular ring which would be stable is inconsistent with observation; hence the hypothesis of a fluid ring remains to be investigated. And in the first paper on the dynamic theory of gases, the hypothesis of small, hard, perfectly elastic spheres is to be explored:

> If the properties of such a system of bodies are found to correspond to those of gases, an important physical analogy will be established, which may lead to more accurate knowledge of the properties of matter. If

experiments on gases are inconsistent with the hypothesis of these propositions, then our theory . . . is proved to be incapable of explaining the phenomena of gases. In either case it is necessary to follow out the consequences of the hypothesis. (*Scientific Papers,* 1:378)

In both these examples it is noticeable that the hypotheses are entirely mechanical in character, that is to say, no physical quantities are involved except those concerning masses in motion and the forces of weight, impact, pressure, and friction. When the subject matter is less obviously mechanical, however, Maxwell is more critical of the hypothetical method. In electric and magnetic science, he says, if "we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages" (1:155). Theoretical entities such as molecules and the aether should not be postulated without evidence (2:253, 315), and should not be endowed with *ad hoc* "attractive and repulsive forces whenever a new phenomenon is to be explained" (2:339, cf. 223). And in the case of chemistry, where the material systems are too small to be directly observed, the hypothetical method is only amenable to verification "so long . . . as someone else does not invent another hypothesis which agrees still better with the phenomena" (2:419). Not only was the hypothetical method regarded by Maxwell as undesirably pervasive in nineteenth-century physics, but his objections to it rest on arguments, not on timidity or prejudice.

2. The Mathematical Method

On the other hand Maxwell is not content with theories composed of purely mathematical formulae in which "we entirely lose sight of the phenomena to be explained; and though we may trace out the consequences of given laws, we can never obtain more extended views of the connexions of the subject" (1:155). In a review of Thomson's *Papers on Electrostatics and Magnetism* he complains that no one has developed Thomson's theory of vortex molecules: "Has the multiplication of symbols put a stop to the development of ideas?" (2:307), and in a paper of the same period he writes:

We must retranslate [symbols] into the language of dynamics. In this way our words will call up the mental image, not of certain operations

of the calculus, but of certain characteristics of the motions of bodies. (2:308)

Explicit criticism of the purely mathematical method does not go very deep in Maxwell's writings, but it has been pointed out interestingly by G. E. Davie that dislike of pure analysis is ingrained in the Scottish tradition in which Maxwell had his first philosophical education.[3] Maclaurin's method of geometrizing the calculus was not dead in Edinburgh, and Maxwell's own interest in physical interpretations of Euclidean geometry is exhibited in several of his early mathematical papers. We shall see later, however, that logical as well as genetic reasons for objecting to purely formalist methods in physics emerge implicitly in Maxwell's treatment of physical theory.

3. The Analogical Method

For Maxwell the middle way between the "rash assumptions" of physical hypotheses and the "analytical subtleties" of mathematical formulae consisted sometimes in a method of "physical analogy," and sometimes in a Newtonian method of deduction of forces from phenomenal motions. The first of these methods appears in the introduction to his first paper on electricity and magnetism, "On Faraday's Lines of Force" (*FL*).[4] Faced with the dilemma between hypothesis and pure mathematics,

> We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed. . . .
>
> In order to obtain physical ideas without adopting a physical theory we must make ourselves familiar with the existence of physical analogies. By a physical analogy I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other. (1:156)

Maxwell here gives four examples, which recur throughout his subsequent discussions of physical analogy: the laws of numbers on which all mathematical sciences are founded, the resemblance of form between both corpuscular theory and wave theory and the phenomena of light, William Thomson's analogy between electric and magnetic attraction and the equations of heat conduction and fluid

flow, and the analogy which Maxwell develops in *FL*, between theories of electric and magnetic action at a distance, fluid flow, and Faraday's representation in terms of lines of force.

The precise nature and function of this kind of "physical analogy" are not altogether easy to gather from Maxwell's explicit remarks about them, and I shall consider them in more detail below. As a preliminary statement it may be said that on the one hand Maxwell is concerned to insist that the existence of a "formal" analogy of equations does not imply identity of physical process or substance, as when fluid flow is compared with heat flow, with current, and with electric induction, without any implication that these last processes in fact involve fluids in motion. The function of formal analogies is rather to aid the imagination in understanding formal relationships, and to enable transfer of mathematical results from one system to another irrespective of subject matter. But on the other hand, systems of ideas which are "really analogous in form" must be distinguished from those that are merely mathematical, and when such analogies are found, they lead "to a knowledge of both [systems], more profound than could be obtained by studying each system separately" (2:219), and "It becomes an important philosophical question to determine in what degree the applicability of the old ideas to the new subject may be taken as evidence that the new phenomena are physically similar to the old" (2:227).[5] Again, although causes of invisible processes cannot be identified with their formal analogues in observable processes, the relations between cause and effect are similar, and what we require are methods of representation so general that they express the real similarity of relations without introducing unwarranted hypothetical ideas into the expression of the cause.[6]

4. Deduction From Experiments

These general methods of representation are to be "deduced" from experiments by a method similar to that used by Newton in deriving the law of gravitational force. Echoing Newton's own statement of method, Maxwell contrasts "the true method of physical reasoning [which] is to begin with the phenomena and to deduce the forces from them by direct application of the equations of motion," with the "too frequent practice" of inventing a particular dynamic

hypothesis and deducing results, the agreement of which with phenomena "has been supposed to furnish a certain amount of evidence in favour of the hypothesis" (2:309).[7] In electrical science, however, the motions involved are not all observable, and the forces involve quantities which are not mechanical:

> When we pass from astronomical to electrical science, we can still observe the configuration and motion of electrified bodies, and thence, following the strict Newtonian path, deduce the forces with which they act on each other; but these forces are found to depend on what we call electricity. (2:419)

For hypothetico–deductivist philosophers of science, talk of "deduction from experiments" must appear logically bizarre. I do not wish here to examine the logical credentials of the expression, which has been common since Newton, except to remark that of course no one has claimed that general laws can be deduced from particular experiments without intervention of a general premise of some kind. For Newton one premise was supplied by some version of his "Rule of Philosophizing III," to the effect that "The qualities of bodies . . . which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever."[8] For Maxwell one such premise was always the conservation of energy, supplemented in his later writings by the Lagrangean formulation of the laws of mechanics. These premises were themselves regarded, however, as generalizations from experience, powerful enough to enable specific laws to be deduced for systems other than those from which they were induced.[9] Maxwell developed the method explicitly in the *Treatise,* where he proposes

> to examine the consequences of the assumption that the phenomena of the electric current are those of a moving system, the motion being communicated from one part of the system to another by forces, the nature and laws of which we do not yet even attempt to define, because we can eliminate these forces from the equations of motion by the method given by Lagrange for any connected system. . . . I propose to deduce the main structure of the theory of electricity from a dynamical hypothesis of this kind.[10]

This preliminary account of Maxwell's analogical and deductive methods leaves their distinctive character obscure. For it may be

argued that the method of physical analogy is nothing but the mathematical method in disguise, since many of Maxwell's remarks about it may be interpreted in the light of later formalist analyses of science as implying no more than similarity of abstract mathematical relations, in which no real or physical similarity between the analogous systems need exist. On the other hand it may be held that the method of deduction from experiments is essentially the hypothetical method, since it admittedly depends on general premises which are logically indistinguishable from hypotheses. I think both these arguments are mistaken, and for the same reason. For in both cases it is assumed that elementary inductive generalizations do not constitute a distinctive form of scientific inference which is distinguishable from hypothetical inference and also a necessary element in the recognition of physical analogies. I shall try to substantiate this claim, first by examining what Maxwell means by "real physical analogy," and second by showing that when such analogies are established, hypothetical theories and concepts may be avoided.

II

In later analyses of physical theory, formal analogy has sometimes been construed in terms of uninterpreted symbols related by equations (the "calculus"), for which entirely independent interpretations are given (by "correspondence rules") to transform the calculus into the analogous theories of different physical systems. If the formal analogy between two systems implied no more than this, it would be a rather weak if not trivial relation, for in the first place, the mathematical expression of the theory of a given physical system is generally not unique. It is therefore possible that in some forms of representation a formal analogy would exist between two systems, but not in other forms. In the second place, in the formalist view the theoretical calculus gives in itself no hints as to its own interpretation, and any interpretation by a set of correspondence rules which will make a given system a semantic model of a given calculus would be acceptable as a theory of that system. Thus, not only may a given system be represented by many formal theories, but a given formal theory may have many models in physical systems, some of them

mutually contradictory, as in the case of the particle and wave theories of light. It follows from both these types of non-uniquencss that there can be no analogical argument from one system to another with which it has some formal analogy. For it is not possible to distinguish the "real" formal analogy from othcrs produced by transformations of mathematical representation, or indeed to know whether any one of them can bc said to be "real," and even if one such analogy is distinguished, there are no grounds for picking particular models of the formal theory as the intended ones in the analogous systems, and arguing from the known properties of one system to further properties of the other systems.

If Maxwell's formal analogy is not that of later formalists, the question arises, what constitute for Maxwell the constraints required to ensure that transformation from one analogous system to another represents real physical analogy and not merely arbitrary similarity of form? Some light on this question is thrown by what he calls the "mathematical classification" of physical quantities (2:257). Classification into scalars, vectors, and quaternions is an important example, as is the distinction among vectors between forces, represented by magnitude along a line, and fluxes, represented by the magnitude of an area, directed normally to that area. This classification of forces and fluxes conveys the mathematical distinction between many pairs of physical quantities: length and area, temperature and heat, electric potential and current. There is also a distinction among vectors between those referring to translation and those to rotation, as in the grad (Maxwell's "slope"), div (Maxwell's "convergence"), and curl functions. These functions represent linear and vortex fluid motion respectively, and also, by analogy, electric and magnetic actions. It follows that the mathematical entities involved—scalars, vectors, forces, fluxes, translations, rotations—are not uninterpreted symbols, but have at least a spatio-temporal interpretation which is identical in all physical systems to which they apply. The "flux" of current may not be flow of a substance, but it has direction in space and quantity related to cross-sectional area, and in these respects is exactly the same as the flow of water along a tube. Translation and rotation have equally obvious spatial interpretation in all Maxwell's analogous systems; indeed, in spite of his many disclaimers that the

relata of an analogy need not themselves be at all similar, he assigns "rotation" to magnetic force precisely because it causes spatial rotation of the plane of polarized light, detected by physically turning polarization gratings in space. But what it is that causes the turning is (at least in his later papers) not specified in the case of magnetic force as it is in the case of material fluid vortex motion.

Transformation rules between formally analogous systems do not, however, always depend on identities of spatio-temporal properties. Sometimes they take the form of identifications of dissimilar causes which nevertheless have identical effects. The most obvious example is the identification of all forms of force with mechanical force, whether produced by a mechanical or electrical or magnetic or any other physical system. All forms of force satisfy the dynamic laws of motion, and hence have the same mechanical effects. They are therefore generically identical properties of systems. A less obvious example in Maxwell's electrical theory is the identification of the potential function of electrostatic force with the "electric tension" (potential difference) causing currents in conductors. This identification deserves scrutiny, because although physically elementary, it reveals a fundamental logical structure which has been little noticed in descriptions of scientific inference.

In *FL* Maxwell introduces the theory of current electricity by reference to the researches of Ohm and Kirchhoff, which had shown that in maintaining a current, "pressure" must be different at different points of the circuit:

> This pressure, which is commonly called electrical tension, is found to be physically identical with the *potential* in statical electricity, and thus we have the means of connecting the two sets of phenomena. If we knew what amount of electricity, measured statically, passes along that current which we assume as our unit of current, then the connexion of electricity of tension with current electricity would be completed. . . . Thus the analogy between statical electricity and fluid motion turns out more perfect than we might have supposed, for there the induction goes on by conduction just as in current electricity, but the quantity conducted is insensible owing to the great resistance of the dielectrics. (1:180)

The argument is this: electric tension is the analogue of static potential and also of fluid pressure, in the sense that the experimental

laws satisfied by these three systems are of the same form. Moreover, electric tension and static potential are physically identical. By this identity Maxwell seems to mean that, given a difference of potential between two points, its physical effect depends solely on the type of material existing between the points: if it is an insulator the phenomena are those of electric induction, if it is a conductor, those of electric current.[11] In this passage in *FL* Maxwell is inclined to carry the analogy further and suggest that it indicates also identity of the unobservable processes involved, namely that both current and electric induction consist of the flow of a substance. We have here the first hint that electric induction should be treated as physically similar to current flow, a step which leads eventually to postulation of the displacement current. But it is already clear that this extension of the analogy is a shaky one, for between the whole process of induction and of current there are obvious negative analogies: conductors and insulators have very different electric properties; in particular, current generates heat, and induction so far as we know does not do so.

However, the identification of tension with potential does not depend on this further analogical extension, which is in fact abandoned in "A Dynamical Theory of the Electromagnetic Field" (*DT*):

> In the case of electric currents, the force in action is not ordinary mechanical force, at least we are not as yet able to measure it as common force, but we call it electromotive force, and the body moved is not merely the electricity in the conductor, but something outside the conductor, and capable of being affected by other conductors in the neighbourhood carrying currents. (1:539)

And, as we shall see later, even the model of current as "something flowing" in the conductor is not insisted upon in later expressions of the theory.

We now have a method of recognizing "real analogy" as contrasted with mere mathematical similarity of equations. It is simply that if two apparently distinct properties of different systems turn out to be interchangeable in appropriately different physical contexts, they are to be considered the same property. I shall call this type of inference to identity *experimental identification.* There may be *generic identification,* when it is the properties of different systems that are identified, or *substantial identification,* when the same entity is found

to be involved in apparently different systems. An example of substantial identification is Maxwell's conclusion in "On Physical Lines of Force" (*PL*), on the basis of the identity of numerical value of the velocities of transmission of transverse electromagnetic waves and of light, that the aetherial media of electromagnetism and light are one medium (1:492).

Another important example of generic identification is the assumption that all forms of energy are to be identified. This assumption is indeed essential for the method of "deduction" of generalized laws of motion from experiments. It does not presuppose that all energy is reducible to the kinetic and potential energy of matter in motion, any more than the language of forces and fluxes implies that heat or electric current are so reducible. It does imply that some general terms can be substituted in Lagrange's equations for the "kinetic" and "potential" energy of mechanical systems, in such a way that the equations are satisfied by diverse physical systems, and that the real analogues of kinetic and potential energy in these other systems are observably identifiable. The identifications are somewhat more complex than the equivalence of effects which led to the identification of electric induction and potential, for they depend on transformations of energy according to quantitative conservation rules. The generic identity of energy justifies Maxwell's claim in *DT* that, whereas all other phrases inherited from the vortex model of the earlier *PL* "in the present paper are to be considered as illustrative, not as explanatory," still, "In speaking of the Energy of the field, however, I wish to be understood literally" (1:564).

Analogical inference leading to experimental identifications goes very deep into the structure of science. Such inferences are not infallible—the identification of phlogiston in different chemical reactions, for example, turned out to be a mistake—but they are pervasive in all scientific theory, and may be supported by stronger or weaker inductive evidence. They are indeed involved even where no specific arguments for identification are adduced. Consider the identification of mechanical force due to magnets, and to electrified bodies, with mechanical force in general. No argument seems to be required to establish this, but it is not a logically necessary identification. It rests

on the interchangeability of mechanical effects of different kinds of physical systems, and the fact that "force" in all cases satisfies the laws of motion. The possibility of such identifications rests on an assumption of relative independence among physical properties, that is to say, although the logical possibilities of variety in different physical systems is unlimited, it is not supposed that interdependence within physical systems is in fact so tight that all the properties of every system have to be considered as different from all of those of every other system.

Similar considerations regarding identity throw some light on the nature of the "experiments" which form the starting point of Maxwell's (and Newton's) "deductions." In *DT* Maxwell claims to deduce his theory from Ampère's and Faraday's laws. These laws rest on such experimental identifications as that of all forces with mechanical force, magnetic force produced by permanent magnets with that produced by iron and by currents, and currents produced by batteries, with current produced by friction or electric induction. The experimental laws are not only empirically vulnerable in the usual sense of being generalizations from instances, they are also vulnerable in being analogical generalizations over instances of different kinds, leading to experimental identifications of properties without which there could be no general descriptive language with which to carry on scientific inference. It may be noted parenthetically that the occurrence of these identifications makes the task of any logic of induction doubly difficult, for not only is there the usual problem of explicating evidence for inductive generalizations, but also the stock of basic distinguishable individuals and predicates in the language is continually changing as evidence for identifications accumulates or is found to be misleading.

We can now summarize the method which Maxwell claims is a preferable alternative to both the hypothetical and mathematical methods, and spell out exactly how it is distinguished from them. First it should be clear by now that the methods of analogy and of deduction from experiments are not separate methods but aspects of a single method. The view that this is not so is due to a confusion between the method of analogy and the postulation of a hypothetical

physical model, such as the mechanical vortex aether model in Maxwell's early electromagnetic theory. This model may exhibit some formal analogy with electromagnetic phenomena, but it is quite clear that even in *PL* Maxwell thought that it failed to be a satisfactory theory: "I do not bring it forward as a mode of connexion existing in nature" (1:486). The method of physical analogy, however, is different. It begins with two or more existing physical systems which are related in two ways: 1) they satisfy formally analogous mathematical laws, and 2) there are sufficient experimental identifications of their entities and/or properties to constitute a real physical analogy between the systems, and permit analogical inference from one system to the other. Under these conditions it is possible, without specifying in detail the character of unobservable entities or causes, to use the general laws representing the formal analogy of the systems in deducing the particular form of the laws and their particular effects for particular systems. For example, in the case of Newton's theory, experimental identifications of the properties of various mechanical systems as constituting analogues related by the laws of motion enables the particular form of the law of gravitation to be deduced from the observable laws of planetary motion. Both experimental identifications and the general laws of motion are necessary here, in order to permit laws derived directly from observable forces and motions to be transformed into laws concerning forces-at-a-distance, and bodies whose masses cannot be discovered by the usual operational means of scales and spring balances.

Enough has been said to indicate why the method of analogy and deduction from experiments is not a purely formal mathematical method. But it may be asked whether it is not after all just a particular form of the hypothetical method. It can indeed be represented in terms of the hypothetico–deductive schema, but there are the following crucial differences between it and the kind of hypothetical speculations to which Maxwell objects.

1. The general laws constituting the formal analogy between systems are themselves derived by direct inductive generalization from experiments; in other words they are "experimental laws" rather than hypotheses.

2. No hypothetical concepts or entities are postulated, since there

is no specification of the detailed character of unobservable causes except in so far as this is justified by experimental identifications and analogical argument using the general laws.

3. A logic of induction to laws and analogy between systems replaces the "guess and test" method of the hypothetico–deductive account.

III

I shall now examine Maxwell's "Dynamical Theory" of 1864 as an application of the analogical method. In this paper Maxwell abandons the hypothetical vortex model, and claims that his conclusions, particularly the equation for the displacement current, are "deduced from experimental facts." It would be at least charitable to suppose that this claim is meant seriously, and that Maxwell now believes himself to have a valid and non-hypothetical inference from experiments to his electromagnetic equations, even though we might prefer to describe this inference as inductive or analogical rather than deductive.

Maxwell begins by arguing the substantial identity of the electromagnetic medium and the light aether, in contrast to *PL,* where this identity is held to follow from the postulates of the theory. The phenomena of light and heat, he says, give reason to believe "that there is an aetherial medium filling space and permeating bodies, capable of being set in motion and of transmitting that motion from one part to another" (1:528). This medium must carry energy, since heat and light radiation take time to traverse it, and the conservation of energy through time is assumed. It is also assumed that the energy is mechanical, in the sense that it is carried as the energy of motion of the material aether, and of its "elastic resilience":

> We may therefore receive, as a datum derived from a branch of science independent of that with which we have to deal, the existence of a pervading medium, of small but real density, capable of being set in motion. . . . (1:528)

Later on in the paper, even this minimal specification of the material aether is withdrawn, and all that remains of the properties of the medium is whatever can be identified respectively with the kinetic

and potential energy terms in Lagrange's generalized equations of motion.

The next step in *DT* is to consider the evidence from motion of conductors and dielectrics relative to a magnetic field. When a material body is moved across magnetic lines of force, whether by the motion of the body or change of the lines, its ends tend to become oppositely electrified, and it may even experience chemical decomposition. By the same inference that led to the identification of electric tension and potential, the electromotive force is defined as that single property which causes the observable polarization, whether in conductors, dielectrics, or electrolytes. Maxwell goes on to consider the action of the electromotive force in dielectrics:

> (11) . . . when electromotive force acts on a dielectric it produces a state of polarization of its parts similar in distribution to the polarity of the parts of a mass of iron under the influence of a magnet, and like the magnetic polarization, capable of being described as a state in which every particle has its opposite poles in opposite conditions.* (1:531)

The footnote is a reference to the work of Faraday and Mossotti on polarization of material dielectrics.

On the face of it, Maxwell's argument in this section (11) is an appeal to the formal analogy of electric and magnetic polarization developed experimentally by Faraday, and mathematized by Poisson and Mossotti. After reading Faraday, Mossotti had realized that Poisson's theory of magnetic polarization could be directly applied to dielectrics by, as Maxwell put it, "merely translating it from the magnetic language into the electric, and from French into Italian" (2:258). But this formal analogy in itself is hardly what Maxwell would call a "real physical analogy," sufficient to justify inference to the displacement current; indeed, the known relations between electricity and magnetism constitute rather a negative than a positive analogy.

In the paragraph following the one just quoted, however, Maxwell sketches Mossotti's own representation of dielectrics, which involved more than the skeletal mathematical analogy with magnetic polarization. The physical content of Mossotti's hypothesis depended on another analogy, namely that between the behavior of dielectrics and conductors in an electric field. Consider a large parallel plate con-

denser in which the plates are separated by free space. Its capacity is defined as the ratio of charge on one of the plates to potential difference between the plates, and is constant for constant geometric configuration of the plates. If macroscopic uncharged conductors are now introduced between the plates they are found to be polarized by the electric field, and the net effect on the potential distribution is found to be a decrease in the potential difference between the plates for a given charge, or in other words, an increase in the capacity of the resulting condenser, the amount of the increase depending on the size and position of the small conductors. Now this increase in capacity is exactly what occurs when a material dielectric is inserted between the plates, the amount of increase being characteristic of the geometry and dielectric constant of the dielectric medium. Mossotti's hypothesis is that material dielectrics consist of microscopic conducting particles insulated from each other by space or aether, where the dielectric constant of each specific material depends on the unobservable geometric configuration of its microparticles. In this hypothesis there are properly speaking no material dielectrics; the only dielectric (insulator) that exists is aether. It therefore provides a simple, physically realizable macromodel for microscopic processes, and may be said to compare favorably in inductive status both with the formal analogy of magnetic and electric potential and with Maxwell's vortex model.

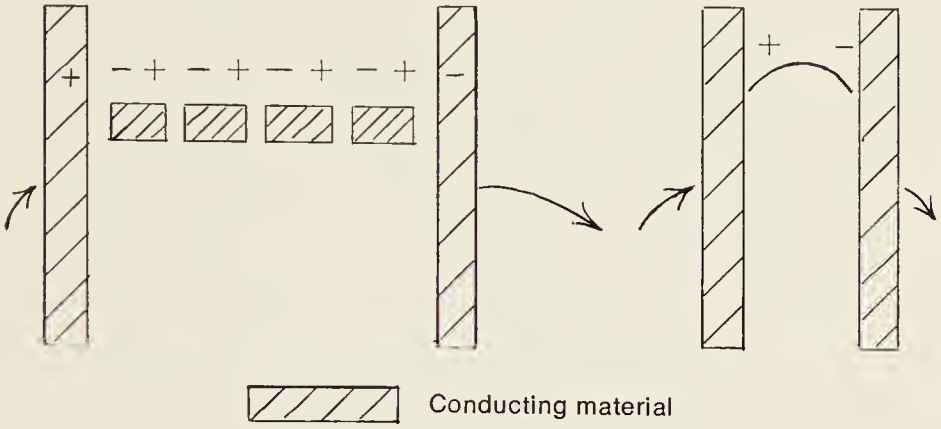
Maxwell does not mention the details of Mossotti's hypothesis in *DT*,[12] but he may have regarded it as implicitly strengthening his analogy between conducting currents and displacement currents in material dielectrics. It causes no difficulty so long as it is restricted to material dielectrics, but trouble arises as soon as, following Faraday, Maxwell begins to treat the aether itself as a dielectric. He is bound to take some such step, since his view is that energy processes are going on in the medium between conductors and magnets, whether this medium is free space or filled with material insulator. What more natural, then, than to identify the structure of the aether with that of material dielectrics, and to postulate a charge polarization of its parts under the influence of electromotive force by analogy with that of material dielectrics? The cavalier manner in which Maxwell passes in *DT* from "real" electric current and "real" dielectric

polarization to displacement current and polarization in aether suggests that he has used this analogy quite uncritically, and regards it as needing no further support. After an interlude chiefly devoted to Ampère's and Faraday's investigations of the mechanical effects of closed currents, he moves straight on to the "General equations of the electromagnetic field," and immediately asserts, "Electrical displacement consists in the opposite electrification of the sides of a molecule or particle of a body which may or may not be accompanied with transmission through the body. . . . The variations of the electrical displacement must be added to the currents p, q r to get the total motion of electricity" (1:554). Maxwell subsequently substitutes this "total motion" for the ordinary conduction current j in Ampère's experimental relation for closed circuits, so as to obtain (in our notation) $\text{curl}\, H = (4\pi/c)j + (1/c)\dot{D}$, and uses this equation, without further comment, to refer both to material dielectrics and to the aether (1:557).

After so much insistence on deduction from experiments, the brevity of this most crucial part of the argument comes as something of a shock, and not surprisingly, many critics found it unconvincing. It is worth noticing, however, that Maxwell might at this stage have supplemented the analogical argument by being more explicit about his interpretation of the nature of the displacement current. His unwillingness to specify the character of the cause here prevented him from examining the inductive force that might have been given to the argument by analogies that lay close at hand, as I shall now show.

It was soon noticed by Maxwell's successors that the implicit interpretations of electric charge and current in his work are highly ambiguous and sometimes confused.[13] Broadly speaking, two distinct interpretations have been recognized, which I shall call simply the first interpretation and the second interpretation. The first interpretation regards electric charge as an incompressible substance satisfying a fluid continuity equation, whose motion in conductors constitutes current and which acts at a distance; the second regards charge as an epiphenomenon of the polarization of the aetherial medium, which becomes detectable when lines of force meet interfaces between insulators and conductors. The first interpretation is related to the traditional action at a distance view of electric force;

the second is derived from Faraday's view of the primacy of the polarization of the medium. There is no doubt, as several commentators remarked, that Maxwell's own use of the term "displacement current" caused confusion at the outset, because it falsely suggests that he is consistently adopting some variety of the first interpretation.



First Interpretation with material dielectric particles indicated

Second Interpretation with no material dielectric between plates

Certainly the *prime facie* interpretation to be given to Maxwell's words in paragraph (11) is the first. Unfortunately, this makes the argument depend on an analogy between material dielectrics and aether which becomes increasingly implausible the more it is scrutinized. For in Mossotti's hypothesis, the aether is just that which intervenes between the conducting particles of the material dielectric, and if particles carrying moving charges are now postulated of aether the question arises, what intervenes between the aether particles? At best the model reduces to one of action at short distances within the aether. There are admittedly places where Maxwell seems to allow this possibility, but it fits uneasily with his general insistence on the propagation of action through a medium.[14] A further physical difficulty of the first interpretation arises from the fact that charge is regarded as an incompressible fluid whose motion produces the total current of conduction and displacement, moving in closed circuits

without sources or sinks.[15] In other words both current and electric induction are transmitted instantaneously, an assumption that is conceptually awkward in a theory of the electromagnetic aether in which wave-disturbances are found to travel with the velocity of light.

The first interpretation, then, does not satisfactorily bridge the gap between material dielectrics and aether in the argument to the displacement current, both because it rests on no close analogy between dielectrics and aether, and also because it leads in itself to implausible physical consequences. When we consider the second interpretation, however, in which charge depends upon lines of force, many of the difficulties of the first interpretation disappear, but at the cost of reducing the plausibility of another part of the analogical argument.

As we have seen, Maxwell argues in many places that energy of electric polarization resides in aether in some fashion which reveals itself to observation as a tendency to motion along the direction of the electric lines of force. On the second interpretation, the charge on a conductor is just the interruption of a line of force by a conducting surface, not some *thing* whose presence produces the lines of force. It is a misunderstanding to regard this as a circular definition of charge or to regard the first interpretation as somehow closer to observation than the second,[16] for both models are interpretations of what is observed. We do not first "observe" charges as particles, and then define lines of force; we observe motions of macroscopic bodies which have been treated in a certain way: by friction, by motion in the neighborhood of magnets, by being connected to a battery, and so on. All these observable situations are reinterpretable directly into the second interpretation without intervention of the idea of charged particles in motion. The cause of electric lines of force in aether is not the presence of substantial charges; the cause is friction, moving magnets, batteries, etc. Wherever a static or moving charge is mentioned in the first interpretation, it can be consistently reinterpreted as a state of the lines of force, at rest or in motion, in the second interpretation.

Again, the notions of incompressibility and conduction of charge acquire new interpretations. There is no motion of particulate or fluid charges along a conductor, and nothing corresponding to pressure along it either; therefore there is no problem about instantaneous

transmission. When a condenser is charged, the second interpretation identifies this process with the establishment of a system of tubes of force between the condenser plates, and between these and other conductors if there are any present in the space. As the charging process takes place, the tubes of force move into position at right angles to their length, so that they become established at the surfaces of all terminating conductors simultaneously. There is no instantaneous action from one condenser plate to the other; it is an action of the battery or other charging device, in which energy takes time to reach the conductors from the battery through the field. There is also implicit in the second interpretation a radical shift in the notion of a current carrying conductor. This is no longer conceived on the model of material particles or fluids in pipes, but as a closed surface on the outside of which lines of force terminate, and along which they move, thus producing the effects of moving charge on the second interpretation. If this conception had had an observable model to replace fluids in pipes, the analogical argument to Mossotti's hypothesis, and hence to the displacement current, might even have been said to be complete.

It cannot be pretended that Maxwell conceived anything like a detailed picture of the second interpretation in *DT*. Its full consequences were indeed not presented until the papers of Poynting in 1884 and 1885 and Heaviside in 1885–1887, which showed that Maxwell's equations entail a continuous energy flow in the field where electric and magnetic actions are taking place.[17] In this they are only following out consistently Maxwell's original insight that energy is present in the field. In "On the Transfer of Energy in the Electromagnetic Field," Poynting writes:

> If we believe in the continuity of the motion of energy, that is, if we believe that when it disappears at one point and reappears at another it must have passed through the intervening space, we are forced to conclude that the surrounding medium contains at least a part of the energy, and that it is capable of transferring it from point to point.
>
> According to Maxwell's theory, currents consist essentially in a certain distribution of energy in and around a conductor, accompanied by transformation and consequent movement of energy through the field. (p. 343)

A conduction current then *may be said to consist* of this inward flow of energy with its accompanying magnetic and electromotive forces, and the transformation of the energy into heat within the conductor. (p. 351, my italics)

Poynting recognizes, however, that the inductive evidence for the theory is not increased by the new interpretation:

We can hardly hope, then, for any further proof of the law [of energy transfer] beyond its agreement with the experiments already known until some method is discovered of testing what goes on in the dielectric independently of the secondary circuit. (p. 361)

And in the *Note* written four years after *DT* Maxwell himself reacts to the hiatus in the experimental argument for the displacement current by requiring the same direct verification:

the current produced in discharging a condenser is a complete circuit, and might be traced within the dielectric itself by a galvanometer properly constructed. I am not aware that this has been done, so that this part of the theory, though apparently a natural consequence of the former, has not been verified by direct experiment. The experiment would certainly be a very delicate and difficult one. (2:139; cf. *Treatise on Electricity*, Section 607)

The same assessment of the logical situation was later made by Helmholtz and Hertz. Indeed, as the first page of his Introduction to *Electric Waves* makes clear, Hertz's own experiments were prompted by the offer of a prize by the Berlin Academy for experimental investigation of the relationship between electromagnetic forces and dielectric polarization which would complete just this deductive argument from experiment. In 1887 Hertz claimed to have shown the magnetic effects for some dielectrics, but despaired of verifying them directly for air. His eventual discovery of the finite propagation of electromagnetic waves in air is in fact derived from his earlier attempts at this direct verification for air.[18]

IV

It must be concluded that neither in the first nor in the second interpretation can Maxwell's analogical inference to the displacement current be made cogent.[19] A more fundamental question about his

method remains to be asked, however, namely whether any such argument could be made to work in the light of objections stemming from the historicist or "meaning variance" account of science. The conclusion, "There is a displacement current in aether," the historicist will claim, means something quite different in the two interpretations; how then can it be said to be inferred without theoretical assumptions from the experimental facts? Consistently with Maxwell's discussion of formal analogy and deduction of generalized equations from experiments, the reply would seem to be this: the conclusion "There is a displacement current in aether," understood as asserting the occurrence of an extra term in Ampère's equation, does not depend for its experimental meaning or validity upon either interpretation of current. It is not an assertion of the nature of the unobservable cause of observable magnetic effects due to a changing electric field, it is a generalized expression of the laws relating these effects with other observables, namely the macroscopic set-up which constitutes the changing electric field. The displacement current has in this respect a status similar to that of the energy of aether, whose relations with observables are expressed by general laws of motion without specification of the kind of energy involved in mechanical or other microscopic terms.

This reply leaves two further points to be clarified. First, if the assertion of the displacement current is intended to be independent of either interpretation, what force could the attempted analogical arguments have, even if they were successful? For the arguments certainly do depend upon adopting one or the other interpretation. It should be noticed, however, that if either or both of the analogical arguments had worked, the corresponding interpretation would not be objectionably hypothetical, because it would have rested on observable analogies of behavior of just the same kind as those leading to identifications of the forms of energy, and of electric induction and potential. There would also have been no need to regard the interpretations as alternatives with respect to the argument for the displacement current term in Ampère's equation, for an argument from one set of observed analogies is surely strengthened by an argument from another set just in so far as the arguments lead to the same conclusion, even though in other respects they may lead to

different conclusions.[20] An inference to the sine law of refraction, for example, is stronger in virtue of being an inference in both the corpuscular and the wave theories.

The second point is more fundamental. The historicist has doubtless been waiting throughout the last two paragraphs to object that the whole force of his analysis has been missed by talking about the "observables" which are said to be related by the generalized "neutral" displacement current equation. These observables themselves, he will maintain, are pervaded by theoretical interpretation. Here there is, however, an ambiguity. If it is being asserted that there are theoretical assumptions hidden in descriptions of experimental observations, such as those concerning electric potential for example, this has already been taken account of. These theoretical assumptions have been specified above in terms of empirically vulnerable experimental identifications which underlie not only the experimental laws assumed, but also the descriptive predicates in terms of which they are expressed. In the latter case the experimental identifications may not rest on any explicit argument; they are directly recognized as acceptable uses of whatever descriptive vocabulary is currently in use. No undesirable regress is thereby created, because some empirically vulnerable identifications of "the same property again" must be presupposed in the use of any descriptive language. It does not follow, of course, that some such identification may not later have to be brought to consciousness, questioned, and possibly abandoned in the light of further evidence, as was the case with the tacit identification of electric current with "something flowing" (presupposed in the very metaphor of "current").

In order to describe the concept of displacement current neutrally as expressing a relation between observables, it must be assumed that in both interpretations of current the same experimental identifications are assumed in expressing the observables. It follows that if the historicist objector wishes to claim that no distinction can be made between observables which are neutral relative to the two interpretations and the interpretations themselves, he is in conflict with the present account. Without entering into this dispute in detail, it may be remarked that the historicist's mistake here lies in his assump-

tion that the only alternative to "radical meaning variance" between theories is a radical distinction between theories on the one hand and an absolutely neutral observation language on the other. In the present account an intermediate position is adopted: theory is not to be conceived, as in the hypothetico–deductive account, as an external hypothesis *imposed,* however intimately, upon independent observations; it is rather to be understood as *constituted by* the fundamental experimental identifications which control both the inference to general laws, and the basic descriptive language in which the laws are expressed. In this, the present account agrees with the historicist. But it does not follow in this account that all theories are self-contained and incommensurable, as some historicists have maintained, for some experimental identifications span more than one theory, and can be used as the relatively neutral ground of comparison between theories.

In summary, then, I have tried to show how Maxwell's method of analogy and deduction from experiments can be developed in such a way as to provide an alternative, not only to the hypothetical and mathematical accounts from which he himself wished to distinguish it, but also to the historicist account which would deny both the concept of deduction from experiments, and also, more fundamentally, the independent status of experiments themselves.

NOTES

1. The distinction is familiar in recent literature: the hypothetical (or hypothetico-deductive) account is found in such writers as Braithwaite, Carnap, Hempel, Nagel, and Popper; the historicist account (though not under that name) in Feyerabend, Hanson, and Kuhn. I have dubbed the latter view "historicist" because it depends largely upon detailed analyses of historical examples, and tends in various ways toward a conventionalist or historically relativist view of science and its methods. I do not mean to imply, however, that this characterization exhausts the valuable insights to be found in these writers, nor that they hold anything like a common view. For example, Hanson, unlike Feyerabend and Kuhn, was prepared to argue for the existence of a logic of discovery

along lines similar to those I shall pursue here: cf. his "Is There a Logic of Discovery?", *Current Issues in the Philosophy of Science,* ed. H. Feigl and G. Maxwell (New York: Holt, Rinehart & Winston, 1961), p. 20.

2. These remarks occur scattered throughout his writings. Some of the most perceptive are found in an early paper written for the Apostles' Club at Cambridge in 1856: "Analogies. Are There Real Analogies in Nature?", *Life of James Clerk Maxwell,* L. Campbell and W. Garnett (London, 1884), p. 347; and in the first pages of "On Faraday's Lines of Force [1856]" *(FL), Scientific Papers,* ed. W. D. Niven (Cambridge, 1890), 1:155. More extensive methodological discussions, especially of the significance of the generalized Lagrangean formulation of mechanics, are to be found in a series of papers from 1870 to 1879, for example, "Address to the Mathematical and Physical Sections of the British Association" (2:215), "On the Mathematical Classification of Physical Quantities" (2:257), "On the Proof of the Equations of Motion of a Connected System" (2:308), "Action at a Distance" (2:311), "On the Dynamical Evidence of the Molecular Constitution of Bodies" (2:418), "Thomson and Tait's Natural Philosophy" (2:776).

3. G. E. Davie, *The Democratic Intellect* (Edinburgh, 1961), p. 192 ff. I owe this reference to the unpublished Ph.D. thesis of P. M. Heimann, "James Clerk Maxwell, His Sources and Influence" (Leeds, 1970).

4. *Scientific Papers,* 1:155.

5. Maxwell's own discussions of physical analogy have been considered by Joseph Turner in two pioneering papers: "Maxwell's Method of Physical Analogy," *Brit. J. Phil. Sci.* 6 (1955):266; and "Maxwell on the Logic of Dynamical Explanation," *Phil. Sci.* 23 (1956):36. Turner does not recognize, however, the function of analogies in providing inductive arguments in Maxwell or elsewhere. Indeed, he explicitly denies (but without giving reasons) that the method of physical analogy permits inference from one physical system to another which is similar to it in some respects ("Maxwell's Method," p. 238). In this he seems to differ from Maxwell himself, as the remark just quoted from Maxwell in the text indicates.

In another analysis of Maxwell's method of analogy, "Model and Analogy in Victorian Science, Maxwell's Critique of the French Physicists," *Journ. Hist. Ideas* 30 (1969):423, Robert Kargon has, almost alone among recent commentators, noticed that the analogical method is regarded by Maxwell as inductively preferable to the hypothetical and the mathematical. He has, however, interpreted analogy as a second best to the method of deduction from experiments, when the latter is applicable. In this he differs from the interpretation given later in this paper, where an intimate relation is discerned between the analogical and the deductive methods.

6. The most explicit discussion of the last point occurs in *An Ele-*

mentary Treatise on Electricity, ed. W. Garnett (Oxford, 1881), Section 64, and especially Section 113:

> The effects of the current of conduction on the electrical state of *A* and *B* are of precisely the same kind as those of the current of convection. . . . In the case of the convection of the charge on the pith ball we may observe the actual motion of the ball. . . . But in the case of the current of conduction through a wire we have no reason to suppose that the mode of transference of the charge resembles one of [the] methods [of convection] rather than another. All that we know is that a charge of so much electricity is conveyed from *A* to *B* in a certain time.

7. The claim to deduction from experiments occurs not only in Maxwell's more discursive methodological writings, but also throughout his mature electromagnetic theory, for example: "the laws of the distribution of electricity on the surface of conductors have been analytically deduced from experiment" (*FL*, *Scientific Papers*, 1:155), "the conclusions arrived at in the present paper are independent of this hypothesis of motions and strains in the aether, being deduced from experimental facts." ("A Dynamical Theory of the Electromagnetic Field [1864]" *(DT)*, *Scientific Papers*, ed. W. D. Niven [Cambridge, 1890], 1:564.) "I propose . . . to state [the electromagnetic theory] in what I think the simplest form, deducing it from admitted facts." ("Note on the Electromagnetic Theory of Light [1868]" (*Note*), Ibid., 2:138.) Cf. also Maxwell's correspondence, at the time of the publication of "On Physical Lines of Force [1861/2]" (*PL*), Ibid., vol. 1 and *DT*, in Campbell and Garnett, pp. 246, 255.

8. I. Newton, *Mathematical Principles of Natural Philosophy*, tr. Motte-Cajori (Berkeley and Los Angeles: University of California Press, 1934), p. 398. In this quotation I have omitted the significant and difficult phrase "which admit neither intensification nor remission of degrees," for which, see J. E. McGuire, "The Origin of Newton's Doctrine of Essential Qualities," *Centaurus* 12 (1968):233. This paper and the same author's "Atoms and the 'Analogy of Nature': Newton's Third Rule of Philosophizing," *Studies in Hist. Phil. Sci.* 1 (1970):1, contain detailed analyses of Newton's Rules. For the logic of "deduction from phenomena," see the important series of papers by J. Dorling, "Maxwell's Attempts to Arrive at Non-speculative Foundations for the Kinetic Theory," *Studies in Hist. Phil. Sci.* 1 (1970):229; "Einstein's Introduction of Photons: Argument by Analogy of Reduction from the Phenomena?", *Brit. J. Phil. Sci.* 22 (1971); "Henry Cavendish's Deduction of the Electrostatic Inverse Square Law from the Result of a Single Experiment," presented to a Cambridge Seminar, 1970; and "The Validity of Deduction from the Phenomena," read to the British Society for the Philosophy of Science, 1970.

9. But see Section II below, where "generalization from experience"

is taken to include problematic "experimental identifications" which may give richer content to the terms of Lagrange's equations compared with those of Newton. On this question see an interesting article by T. K. Simpson, "Some Observations on Maxwell's *Treatise on Electricity and Magnetism*," *Studies in Hist. Phil. Sci.* 1 (1970):249, which appeared too late for me to take explicit account of it in this paper.

10. J. C. Maxwell, *A Treatise on Electricity and Magnetism*, 1st ed. (London, 1873), Section 552.

11. This argument nearly becomes explicit in *Elementary Treatise*, Section 5: "Definition.—*Whatever produces or tends to produce a transfer of Electrification is called an Electromotive Force.*"

12. He describes it in *Treatise on Electricity*, Section 62, where he says it "may be actually true," provided there are no dielectrics with constant less than that of vacuum. He does not explicitly "reject Mossotti's hypothesis," as stated erroneously by Fitzgerald in "M. Poincaré and Maxwell," *Scientific Writings of G. F. Fitzgerald*, ed. J. Larmor (Dublin and London, 1902), p. 284.

13. See for example P. Duhem, *Les Théories électriques de J. C. Maxwell* (Paris, 1902); O. Heaviside, "Electromagnetic Induction and Its Propagation [1885–7]," *Electrical Papers* (London, 1892), 1:434, 477; H. Hertz, *Electric Waves*, 1892, tr. D. E. Jones (London, 1893), Introduction; H. Poincaré, *Electricité et Optique* (Paris, 1901), p. viii and *passim.*; J. J. Thomson, "Report on Electrical Theories," *Report of the 55th Meeting of the British Association* (Birmingham, 1886), p. 125 ff. Among more recent commentators, see A. O'Rahilly, *Electromagnetic Theory* (New York, 1965), first published as *Electromagnetics* (London, 1938), pp. 76 ff.; J. Bromberg, "Maxwell's Displacement Current and his Theory of Light," *Arch. Hist. Ex. Sci.* 4 (1967/8):218; and Maxwell's Electrostatics," *Amer. J. Phys.* 36 (1968):142; P. M. Heimann, "Maxwell and the Modes of Consistent Representation," *Arch. Hist. Ex. Sci.* 6 (1970):171; and "Maxwell, Hertz and the Nature of Electricity," *Isis* 62 (1971):149–157.

14. "[I endeavour] to explain the action between distant bodies without assuming the existence of forces capable of acting directly at *sensible* distances" (1:527, my italics).

15. Cf. *Treatise on Electricity*, Section 61. Since curl $B = (4\pi/c)j + (1/c)\dot{D}$, and any div curl function is identically zero, div $(4\pi/c)j + (1/c)\dot{D}$) is everywhere zero.

16. A. O'Rahilly remarks with approval: "The ordinary working physicist remains convinced that a current really consists of something travelling along the wire. He remains frankly sceptical in face of the paradoxical hypothesis that it is everywhere except in the wire" (*Electromagnetic Theory*, p. 277). J. H. Poynting, in "On the transfer of

energy in the electromagnetic field," *Phil. Trans.* 175 (1884):360, is more sensitively aware of the paradigm switch required to understand the second interpretation:

> It is very difficult to keep clearly in mind that this 'displacement' is, as far as we are yet warranted in describing it, merely a something with direction which has some of the properties of an actual displacement in incompressible fluids or solids. . . . It seems to me then that our use of the term is somewhat unfortunate, as suggesting to our minds so much that is unverified or false, while it is so difficult to bear in mind how little it really means.

17. J. H. Poynting, "Transfer of Energy" and "On the Connexion Between Electric Current and the Electric and Magnetic Inductions in the Surrounding Field," *Phil. Trans.* 176 (1885):277; O. Heaviside, "Electromagnetic Induction." Maxwell's first explicit statement of the second interpretation is in *Treatise on Electricity*, Section 111:

> The electrification therefore at the bounding surface of a conductor and the surrounding dielectric, which on the old theory was called the electrification of the conductor, must be called in the theory of induction the superficial electrification of the surrounding dielectric. According to this theory, all electrification is the residual effect of the polarization of the dielectric. . . .
>
> In the phenomenon called the electric current the constant passage of electricity through the medium tends to restore the state of polarization as fast as the conductivity of the medium allows it to decay. Thus the external agency which maintains the current is always doing work in restoring the polarization of the medium, which is continually becoming relaxed, and the potential energy of this polarization is continually becoming transformed into heat.

18. For an analysis of the significance of Hertz's experiments in relation to Maxwell's theory, see S. d'Agostino, "Hertz's Discovery of Electromagnetic Waves," forthcoming.

19. J. Bromberg seems to be mistaken in her suggestion that in the second interpretation Maxwell has "cut [his equations] off from the physical ideas [the first interpretation] upon which he had founded them" ("Maxwell's Electrostatics," p. 151), if this implies that the first interpretation has some inductive cogency which the second lacks. At best it may be said that Maxwell's first understanding of the meaning of the displacement current was probably in terms of the first interpretation. But in the absence in most commentators of any distinction between the meaning of different models and their inductive force, it is difficult to know how to interpret judgments such as Bromberg's.

20. This logical point about a multiplicity of analogies gives a rational ground for Maxwell's *pluralism* of interpretations, an aspect of his physics which is remarked upon by d'Agostino, "La Pensée scientifique de Maxwell et le développement de la théorie sur champ électromagnéti-

que dans le mémoire 'On Faraday's lines of force'," *Scientia* 103 (1968):7. It is also enough to put in their right perspective Duhem's many accusations of *contradiction* in Maxwell's work (*Les Théories électriques*, p. 101). A pluralism of models does not imply a self-contradictory theory.

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